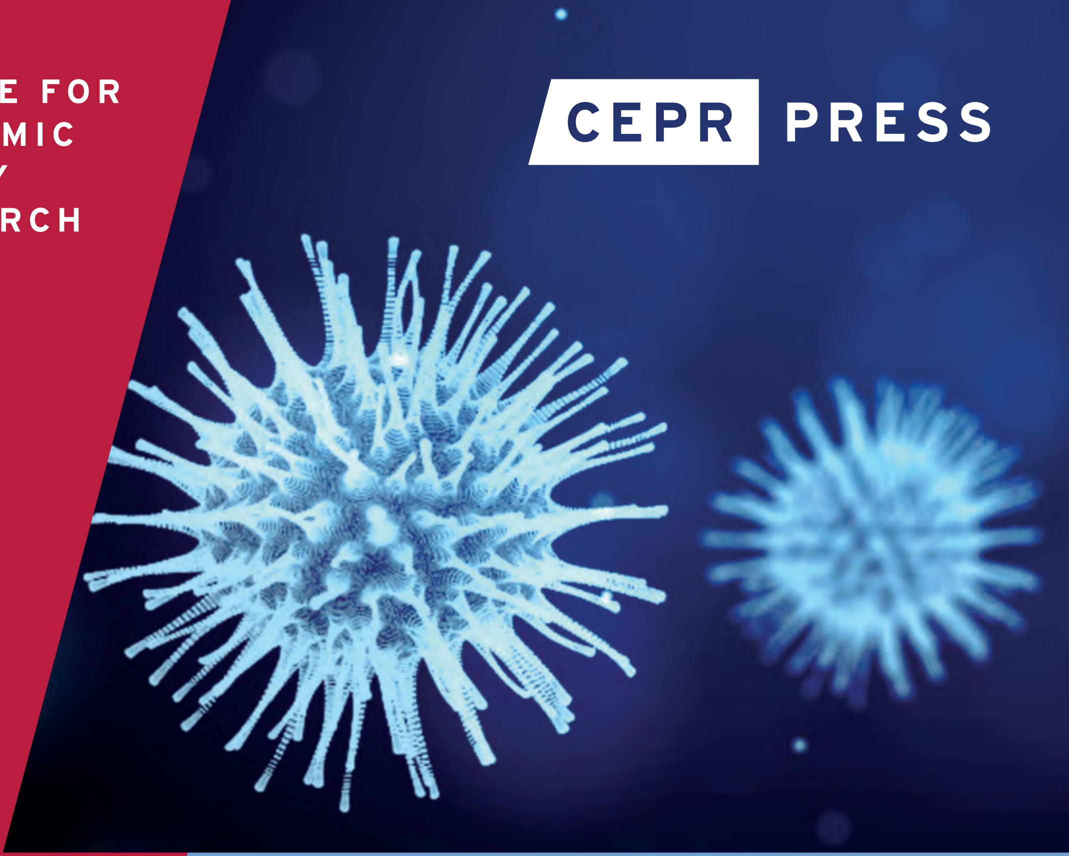
CENTRE FOR ECONOMIC POLICY RESEARCH

CEPR PRESS



COVID ECONOMICS

VETTED AND REAL-TIME PAPERS

ISSUE 50
25 SEPTEMBER 2020

Covid Economics
Vetted and Real-Time Papers

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

Founder: Beatrice Weder di Mauro, President of CEPR
Editor: Charles Wyplosz, Graduate Institute Geneva and CEPR

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

The Centre for Economic Policy Research (CEPR)

The Centre for Economic Policy Research (CEPR) is a network of over 1,500 research economists based mostly in European universities. The Centre's goal is twofold: to promote world-class research, and to get the policy-relevant results into the hands of key decision-makers. CEPR's guiding principle is 'Research excellence with policy relevance'. A registered charity since it was founded in 1983, CEPR is independent of all public and private interest groups. It takes no institutional stand on economic policy matters and its core funding comes from its Institutional Members and sales of publications. Because it draws on such a large network of researchers, its output reflects a broad spectrum of individual viewpoints as well as perspectives drawn from civil society. CEPR research may include views on policy, but the Trustees of the Centre do not give prior review to its publications. The opinions expressed in this report are those of the authors and not those of CEPR.

Editorial Board

Ethics

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

Submission to professional journals

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

American Economic Review
American Economic Review, Applied Economics
American Economic Review, Insights
American Economic Review, Economic Policy
American Economic Review, Macroeconomics
American Economic Review, Microeconomics
American Journal of Health Economics
Canadian Journal of Economics
*Econometrica**
Economic Journal
Economics of Disasters and Climate Change
International Economic Review
Journal of Development Economics
*Journal of Econometrics**
Journal of Economic Growth
Journal of Economic Theory
*Journal of the European Economic Association**
Journal of Finance
Journal of Financial Economics
Journal of International Economics
*Journal of Labor Economics**
Journal of Monetary Economics
Journal of Public Economics
Journal of Public Finance and Public Choice
Journal of Political Economy
Journal of Population Economics
Quarterly Journal of Economics
*Review of Corporate Finance Studies**
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*.

Covid Economics
Vetted and Real-Time Papers

Issue 50, 25 September 2020

Contents

Covid Economics Issue 50, 25 September 2020

Editor's foreword
Issue 50: Where do we stand?

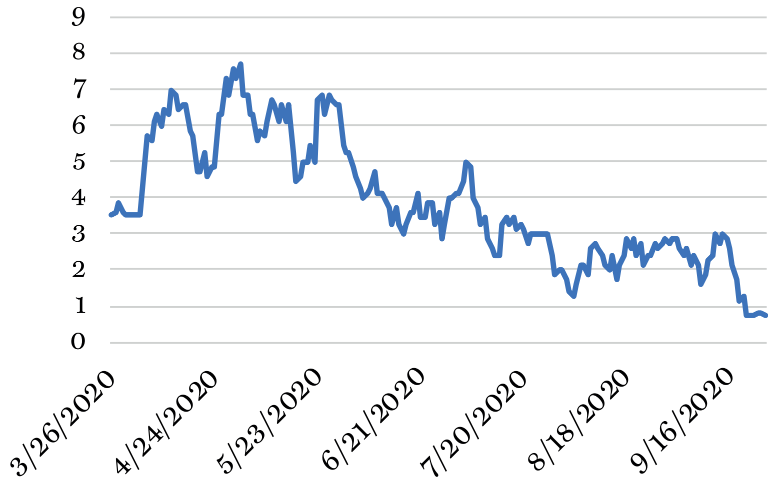
Charles Wyplosz, Editor of *Covid Economics*

Issue 1 came out on 3 April 2020. Six months later, with Issue 50, *Covid Economics* has received 726 submissions, 332 of which have been accepted. Submissions have come from all five continents – a fitting reminder of the worldwide reach of the pandemic, but also an optimistic indication that economists have worked hard at trying to contribute to the global efforts to mitigate its impact.

Until the outbreak, the field of the economics of pandemics was very limited. The 50 issues of *Covid Economics* reveal the amazing progress achieved since then. It has built upon every area of specialisation, combining theory and empirics. Hopefully, our vetting of papers for immediate posting has contributed to this effort. The average time between submission and editorial decision has been 1.97 (standard deviation = 2.17) days. On average, it has taken 5.28 (standard deviation = 2.87) days from submission to publication.

As expected, this research activity is now slowing down, as shown in the figure below. From the start, we decided that the frequency of publication would be endogenous to the number of papers accepted. At the peak, we posted three issues per week, featuring some 20 papers. We are now down to about one issue per week.

Figure 1 Number of daily submissions (7-day moving average)



Covid Economics 50, 25 September 2020: 1-2

Over time, as knowledge expanded, we have become more selective. Initially, every well-crafted paper was adding value. Nowadays, we evaluate papers relative to existing knowledge. We intend to continue for a while. Eventually, when decreasing returns have become strong enough, we will conclude that *Covid Economics* has served its purpose.

This adventure has been made possible by CEPR's decision to create *Covid Economics*, to mobilise internal resources, and to make it freely available online. The readiness of the Editorial Board members to vet papers with unusual speed has also been crucial. Thanks to all of them.

Charles Wyplosz
Editor

Covid Economics 50, 25 September 2020: 1-2

CEPR PRESS

Covid Economics Issue 50, 25 September 2020

Corona politics: The cost of mismanaging pandemics

Helios Herrera,[1] Maximilian Konradt,[2] Guillermo Ordoñez[3] and Christoph Trebesch[4]

Date submitted: 15 September 2020; Date accepted: 17 September 2020

The Covid-19 pandemic is a major test for governments around the world. We study the political consequences of (mis-)managing the Covid crisis by constructing a highfrequency dataset of government approval for 35 countries. In the first weeks after the outbreak, approval rates for incumbents increase strongly, consistent with a global "rally around the flag" effect. Approval, however, drops again in countries where Covid cases continue to grow. This is especially true for governments that do not implement stringent policies to control the number of infections. Overall, the evidence suggests that loose pandemic policies are politically costly. Governments that placed more weight on health rather than short-term economic outcomes obtained higher approval.

Covid Economics 50, 25 September 2020: 3-32

1 Warwick University and CEPR.
2 The Graduate Institute, Geneva.
3 University of Pennsylvania.
4 Kiel Institute for the World Economy and CEPR.

CEPR PRESS

Covid Economics 50, 25 September 2020: 3-32

1 Introduction

The Covid-19 pandemic is a major, common test for leaders around the world. The challenges faced were similar for all leaders, but the political responses varied substantially. Some governments, like those of Australia or Argentina, enforced stringent measures early on, right after the virus broke out, while others, like those of Brazil, Sweden or the United States, opted for looser policies. Partly because of such different policy responses and their timing, there are considerable differences in the scale and dynamics of infection rates. Europe initially saw a rapid increase in Covid case numbers in March 2020, followed by a quick decline. In contrast, the United States and Brazil continued to witness high case numbers throughout the spring and summer of 2020, with further increases recently.

In this paper, we study the political consequences of (mis-)managing the Covid-19 crisis in a cross-country setting. We ask: How does a governments' handling of the pandemic affect its political approval, and, thus, its reelection chances? Do governments get punished politically if they fail to respond strongly/promptly or if they see infections and fatalities raise? And what does the public care more about: good or bad news about infection case numbers, or news on the economy? So far, the expanding literature on Covid-19 has paid only limited attention to these questions, even though they are of central importance for one of the most widely debated policy trade-offs of our time: public health vs economic health. From a political economy perspective, it is crucial to understand what type of news affects the public during the pandemic, namely how the public evaluates governments' pandemic policy over time.

This paper is the first broad-based study on how the Covid crisis affected government approval ratings around the world. We construct a new, high-frequency polling dataset, which consists of surveys on leaders' approval and voting intentions on a weekly basis for 35 countries. This includes 20 advanced economies and 15 emerging market economies for which frequent, high-quality polling data was available (the countries in our sample account for 65% of global GDP). We then investigate how Covid infection and fatality numbers affect approval rates over time, while controlling for government policies (using weekly Oxford "stringency index") as well as for economic activity (using weekly mobility and electricity data). The high-frequency panel structure of our dataset is key, as it allows to capture the dynamics of leaders' approval, as opposed to studies that rely on (one-time) election results, more standard survey data or dynamics in a single country.

Our core finding is that lLeaders are punished, in terms of political approval, when Covid infections accelerate. This result is intuitive at first glance, but has important caveats, as we explain below. In our baseline model, a one standard deviation increase in the growth rate of Covid cases in a week (that is, a 60% increase in the weekly growth rate) is associated with a 3.6 percent decline in approval rates compared to the pre-pandemic approval level, after controlling for economic activity and Covid fatalities as well as country and time fixed effects. For a leader, for instance, with a 50% approval rate before the start

CEPR PRESS

of the outbreak, this implies a weekly decline in approval by 1.8 percentage points.[1]

Over time, the differences become substantial. Three months after the virus breaks out, governments in countries with low case growth record changes in approval (or voting intentions) that are 20 percent higher, while those with high case growth see no gains, on average. Put differently, governments that manage to limit infection numbers, gain 7 percentage points in approval (i.e. they move from an average pre-pandemic approval level of 40% to 47%) compared to the group of high-case growth countries, which do not experience change in approval relative to pre-pandemics. This is a sizable difference, given that elections are typically decided by a few percentage points in voter support.[2]

Importantly, not all governments are punished by high case numbers. Only governments that fail to impose strict countermeasures when experiencing an increase in cases see a decline in approval. More specifically, we find that at high levels of policy stringency, growing case numbers are no longer associated with a decline in political support. In sum, the relationship between case growth and approval is only significant when rising infection numbers coincide with loose policies. This suggests that leaders are evaluated by their policy choices, and not only by the consequences of the pandemic.

In addition, the detrimental effect of infections and loose policies on political support does not set in immediately. We document a rally-around-the-flag effect at the start of the Covid crisis (following the language of Mueller (1970)). In the initial weeks of the outbreak, most governments see their approval increase significantly, regardless of the policies and their outcomes. Only after some time governments see their approval erode when cases grow and policies are loose. Indeed, as we show, approval rates revert most strongly in countries where the public assesses the government's response as "insufficient" (based on novel data from a cross-country survey on Covid-19 by Fetzer et al. (2020)). This is further support for our finding that mismanaging the Covid crisis comes at a high political cost, at least after an initial "grace period".

Somewhat surprisingly, we find that approval rates do not react to indicators of economic activity. High-frequency measures of economic activity are not a significant predictor of political support in this pandemic.[3] Why government approval reacts so strongly to changes in infections but not to changes in economic activity is not clear from our data. We do not have a direct way to test why the public assigns so much weight on infection cases. One interpretation is preference-based, meaning that during a pandemic the public cares most about health outcomes and less so about economic outcomes. This is also consistent with the finding that the public supports governments that take a tough policy stand.

[1] We report our main findings based on relative changes compared to the pre-pandemic approval levels. Our results also hold in absolute terms, based on the percentage point difference in approval: In a similar baseline model, at weekly frequency, a one standard deviation increase in the growth rate of Covid infections is associated with a 1.3 percentage point fall in approval rates.

[2] See, e.g., Snowberg, Wolfers, and Zitzewitz (2007). This is also a large difference across countries: in a worldwide survey by Gallup of 2019 Survey, leader approval rates differed by a maximum of 25 percentage points, ranging from 46% (for Angela Merkel) to 21% (for Jair Bolsonaro, Benjamin Netanyahu and Hassan Rouhani).

[3] As standard variables of economic activity, such as GDP, are not available at weekly frequency, we use proxies that have also been used in related work, such as workplace visits or electricity usage.

Covid Economics 50, 25 September 2020: 3-32

Another interpretation is that the public expects that the economy will not fare well anyways until the pandemic is tamed. In this view, tough policies that bring down infections are a precondition for good economic outcomes in the medium and long-run (in line with the evidence from the Spanish flu from Correia, Luck, and Verner (2020)). Indeed, there is growing evidence that individuals react to high infection numbers by restricting their movements, so looser policies do not necessarily imply more economic activity (consistent with Farboodi, Jarosh, and Shimer (2020)). A quick "reopening" is thus far from guaranteed to result in a quick economic rebound.

Related Literature. The main distinguishing feature of this paper is establishing the dynamic relationship between infection numbers during a pandemic and political approval, using representative high-frequency panel data across countries worldwide. Our analysis ties into several strands of the literature. First, we contribute to the small body of work on the political consequences of the Covid-19 crisis. Bol et al. (2020) analyze an online survey for 15 European countries and find that public support for incumbents increases in response to lockdown policies. De Vries et al. (2020) show similar evidence using survey data from France, Germany, Poland and Spain, while Giommoni and Loumeau (2020) find that incumbents with stronger lockdowns retain a higher vote share in France's municipal election. These papers have in common that they study the cross-sectional variation in the data, typically in a limited set of countries. We provide a broader, international perspective and track approval over time, on a weekly basis, which allows us to study the political dynamics in different phases of the pandemic. Our panel dataset facilitates country comparisons and helps to exploit the unique feature of the Covid-19 crisis, namely that governments worldwide were affected by the pandemic in similar ways and often at the same time.

Our paper also relates to research on the policy trade-offs that leaders face in times of crises. It is well established that economic performance shapes election outcomes (e.g. Lewis-Beck and Stegmaier 2000; Duch and Stevenson 2008), and incumbent leaders have been shown to influence the economic cycle as elections near (e.g. Drazen (2000)). Boin and Hart (2003) were among the first to document that, during crises, politicians balance public safety concerns against economic and political concerns. Our result point to the dominant role of health outcomes in shaping public opinion during a global pandemic crisis.

In recent months, some leaders were particularly keen on re-opening the economy at the potential cost of public health. Most prominently, Donald Trump has pushed repeatedly for a rapid re-opening of the economy. Frieden (2020) points to political factors that led some leaders to follow the advice of public health experts and others to ignore them. Along this line, Pulejo and Querubín (2020) document that leaders with upcoming elections impose less strict policy measures. Taking a different direction, Besley and Dray (2020) point to the role of free media in holding policy makers accountable for their policy actions. We complement these papers, and provide more systematic evidence how policymakers are evaluated based on case numbers and policies imposed.

Covid Economics 50, 25 September 2020: 3-32

Lastly, our paper is related to the literature asking how crises and policy decisions shape the formation of public perceptions, and how the public evaluates policies in particularly distressing times. Earlier research on public health crises has focused on Ebola (e.g. Campante, Depetris-Chauvin, and Durante 2020) and HIV/Aids (Mansour, Rees, and Reeves 2020). Leaders are evaluated based on their policy decisions, as changes in approval are tied to whether voters perceive responses as adequate, and changes in sentiment can persist over time (Bechtel and Hainmueller 2011). This type of analysis is usually difficult given that crises affect only few countries at the same time, or because data are only available at annual frequency. Here we make use of a high-frequency cross-country dataset on political, policy and economic variables, and exploit the global commonality of distress. The Covid crisis provides a unique laboratory with a common shock to many leaders worldwide, then providing a "common exogenous shock" to study.

The remainder of the paper is organized as follows. We start by presenting the novel dataset on government approval and the main explanatory variables, before we outline the empirical strategy. Then, we establish an empirical link between the growth rate of Covid infections and changes in government approval, that further depends on the strictness of countries policy stances. We supplement our results with evidence from an international survey and then conclude.

2 Data and empirical strategy

This section presents the data and outlines the empirical strategy. We assemble a comprehensive cross-country dataset covering government approval, the strictness of government response measures, economic activity, as well as infection and fatality numbers at a daily or weekly frequency. The dataset covers the time span between January and July 2020, meaning that we start shortly before the Covid 19 crisis spread globally in February and March 2020. Summary statistics, sources and definitions of the main variables used for the empirical analysis are provided in Table 1.

2.1 Data: Approval, pandemic policies, and economic activity

We construct a new, high-frequency global dataset on government approval for 35 countries, including 20 advanced economies and 15 emerging market economies since January 2020. Conceptually, we follow the strategy of Herrera, Ordoñez, and Trebesch (2020), who compile similar data at annual frequency to study "political booms" (rapid increases in approval rates) and find that these predict financial crises. For the construction of the sample, we include all countries for which reliable polling data at high frequency are available. Our main source, Wikipedia, is both convenient and reliable, since it lists polling results from a broad range of organizations and firms in each country, most importantly by Gallup, Ipsos and their regional sub-branches.

We first collect available data across polling sources and then build a weekly average,

Covid Economics 50, 25 September 2020: 3-32

which is the same approach used by Politico (formerly pollofpolls.eu), who aggregate data on voting intentions in Europe, as widely cited in the press.[4] Indeed, we find a high correlation between our data and Politico's data for those time series made available to the public. We further complement the data using polls from Morning Consult Political Intelligence.[5]

Where available, we use data on leader and/or government approval, focusing on executive approval, i.e. the approval of the prime minister in parliamentary systems or that of the president in presidential systems (we disregard approval for presidents that have no or limited executive power, e.g. in parliamentary democracies). If leader approval series were not available, we use voter support for the government by adding vote share intentions for all coalition parties that are in office. In the few cases where both series were available, we find leader approval and voter support to co-move strongly. For almost all countries in our sample we have data at weekly frequency and in some cases close to daily frequency (e.g. in the US, Italy or Germany). More details on our sample and sources are shown in Appendix Table A1.[6]

Table 1: Main variables

Variable	Definition	Source(s)	Mean	Median	Min.	Max.	St. Dev.
Δ *Approval*	% change relative to pre-pandemic level	Wikipedia, Morning Consult	10.3	4.6	-30	80	19.3
Δ *Cases*	New confirmed Covid infections log growth rate	ECDC	0.4	0.1	0	3.3	0.6
Δ *Deaths*	New confirmed Covid fatalities, per 100,000 Population	ECDC, United Nations	4.8	1.5	0	33	7.4
Δ *Activity*	% change in workplace visits, relative to 2020 median before February 7, 10-Day MA	Google Community Mobility Reports	-35.6	-35.3	-74	0	16.6
Δ *Activity* (alternative)	% change in electricity usage, relative to 2020 median before February 7, 10-Day MA	Entso-E, U.S. EIA	-20.4	-20.9	-37	14	8.2
Stringency	Stringency of Government Response, Index (0-100)	Blavatnik School of Government (Oxford)	67.2	72.2	0	100	18.5

Based on the constructed dataset, our main variable of interest is the percentage change in government approval over the course of the pandemic. More specifically, we focus on changes in approval relative to the pre-Covid outbreak, i.e. compared to country-specific pre-pandemic approval rate. Following standard practice in the literature, we define the

[4]Also the Executive approval project dataset (executiveapproval.org) averages across available sources, providing data at quarterly frequency and for leader approval only.

[5]Coronavirus Outbreak Tracker, Morning Consult (07.07.2020),https://morningconsult.com/form/coronavirus-outbreak-tracker/

[6]During the sample period some countries held national government elections that potentially alter power dynamics, Ireland (8 February 2020) and Slovakia (29 February 2020). Both elections led to turnover and new coalitions, but both took place in February, so that the new government was in power from the start of the pandemic. In both cases, we treat the new coalition parties as incumbents for the entire sample period (i.e. starting in January). The results are stable when including Ireland and Slovakia from March 2020 only.

Covid Economics 50, 25 September 2020: 3-32

outbreak of the Covid pandemic in a given country as the day on which the 100th infection is reported. By focusing on changes relative to pre-pandemic levels, we capture those changes in approval that are linked to a governments' handling of the pandemic itself. In order to make absolute statements on effect sizes, we also provide results based on percentage point changes in approval relative to the pre-pandemic level.

Data on Covid infections and fatalities are taken from the European Centre for Disease Prevention and Control (ECDC), which gathers data from health authorities worldwide.[7]. To normalize the death rate by population we use data from the United Nations (2019 World Population Prospects).

To capture differences in government responses to the pandemic, we use data from the Oxford Government Response Tracker by Hale et al. (2020)[8]. The aggregate "Stringency Index" summarizes eight policy dimensions, including measures on school closing, restrictions on public gatherings or travel bans. The index varies between 0 (no stringent policies in place) to 100 (strictest possible policies) and has been used already in studies about the pandemic (e.g. Deb et al. 2020). Data start on January 21, 2020, at daily frequency, for all countries in our sample.

For a high-frequency measure of economic activity, we use data on workplace visits by Google's Covid-19 community mobility reports (Aktay et al. 2020).[9] The data are constructed from mobile phone apps such as Google Maps and measured in terms of average daily changes relative to the median value between January 3rd and February 7th 2020, for a given day of the week. Though likely imperfect at measuring economic activity (for instance due to shifts to working at home) it is the most frequently employed real-time activity proxy in recent studies about the pandemic (e.g. Alon et al. 2020). The data are available at daily frequency for all but two (Iceland and Russia) countries in our sample, starting February 8. We smooth the data using a 10-day moving averages. As an alternative proxy for economic activity we use electrical power consumption, for which we obtain data at daily frequency for 19 European countries and the United States.[10]

2.2 Empirical strategy

In this section, we describe the empirical strategy to investigate the relationship between Covid infections and government approval. In the main specifications we use weekly data

[7]Geographical distribution of Covid-19 Cases worldwide, ECDC, accessed 07.07.2020, https://www.ecdc.europa.eu/en/publications-data/download-todays-data-geographic-distribution-covid-19-cases-worldwide.

[8]Accessed 07.07.2020, https://www.bsg.ox.ac.uk/research/research-projects/coronavirus-government-response-tracker.

[9]"Covid-19 Community Mobility Reports", Google, accessed 07.07.2020, https://www.google.com/covid19/mobility/

[10]"Transparency Platform",European Network of Transmission System Operators for Electricity, accessed 07.07.2020, https://www.entsoe.eu/; "Electric Grid Monitor", U.S. Energy Information Administration, accessed 07.07.2020, https://www.eia.gov/beta/electricity/gridmonitor/

Covid Economics 50, 25 September 2020: 3-32

and estimate panel ordinary least squares (OLS) models of the following form:[11]

$$\Delta\, Approval_{it} = \beta_1\, \Delta Cases_{it} + \beta_2\, \Delta Deaths_{it} + \beta_3\, \Delta Activity_{it} + \theta_i + \delta_t + \epsilon_{it}\,,$$

where $\Delta Approval_{it}$ is the *percentage change* in approval of leader/government in country i as of week t relative to the approval at the start of the outbreak, $\Delta Cases_{it}$ is the weekly percentage change in new Covid infections, $\Delta Deaths_{it}$ is the weekly percentage change of Covid fatalities per 100,000 Population, $\Delta Activity_{it}$ is the weekly percentage change in workplace visits, θ_i and δ_t are country and time fixed effects, and ϵ_{it} is an error term. As an alternative dependent variable, we use the *change of approval (in percentage points)* of leader/government in country i as of week t. As a robustness check, we also estimate the baseline model including all explanatory variables as one week lags.

One challenge to our analysis is the heterogeneity in the timing of Covid outbreaks across countries. Italy and South Korea, for example, had more than 100 registered cases by late February, while other countries, like Brazil, India or New Zealand, passed this mark only in mid or end March. We address this issue by indexing the time dimension of our panel to start on the week of the Covid outbreak in a given country (i.e., the 100th confirmed infection). As a result, we include Italy from the last week of February onward, while for New Zealand we start in the last week of March. To control for time effects, we include monthly time fixed effects for every four weeks after the sample start. Our results are robust when using month-calendar fixed effects.

3 Covid infections and government approval

3.1 Preliminary evidence: Infection growth reduces approval

We start by showing descriptive evidence on the development of political approval across countries during the Covid-19 crisis. Most countries see increased public support, with a few exceptions that lost government approval, such as Brazil, the United States and United Kingdom. On average, governments increased their approval rates by 16 percent between February and July 2020. Figure 1 graphs the percentage change for the 35 countries in our sample in descending order. It shows considerable heterogeneity: The ranking of "winners" is led by Australia, whose prime minister Morrison saw his approval level rise by 90 percent. The governments with the strongest drop in leader approval are Romania, followed by Japan and the United States, where President Trump saw his approval rating drop by around 17 percent.

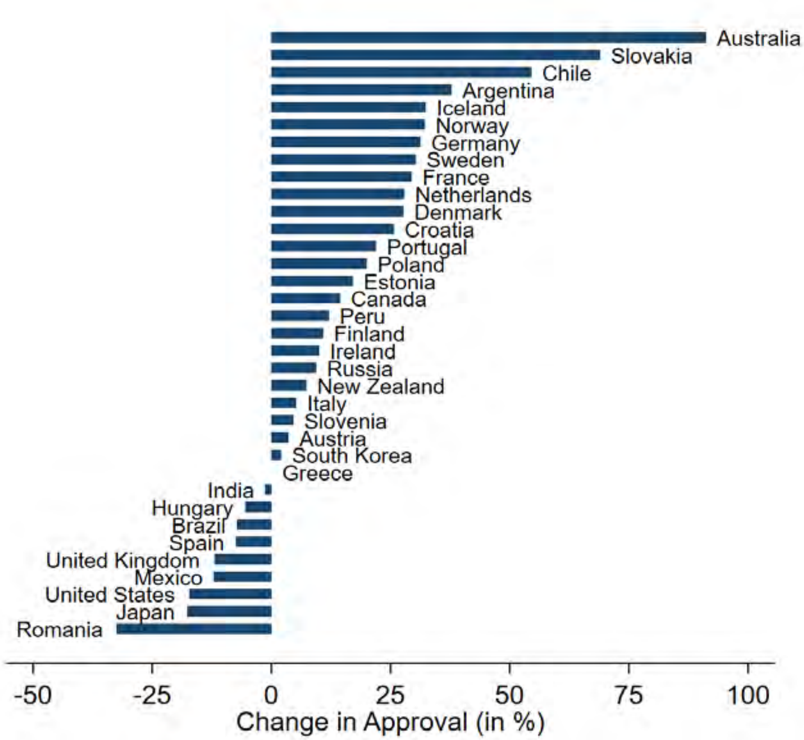
We provide similar visual evidence based on percentage point changes in Figure A1 in Appendix C, which compares absolute changes in approval, irrespective of the initial pre-pandemic level. Quantitatively, the changes in approval vary between gains of 31 (Australia) and losses of 25 (Romania) percentage points, with an average gain of 4.8

[11] The main results are robust to using alternative specifications based on daily or monthly data (see Table A3 in the Appendix).

CEPR PRESS

Covid Economics 50, 25 September 2020: 3-32

Figure 1: Government approval during the Covid-19 crisis, February - July 2020



Notes: This figure shows the percentage change in government approval from February to July 2020 for the 35 countries in our sample. The approval data build on newly collected dataset combining political polls on leader approval and polls on voting intentions for the coalition government parties (see Table A1 in Appendix A for details.

percentage points of approval between February and July 2020. These cross-country differences are substantial, given that elections are often decided by few percentage points in most countries.

We now show the dynamics of approval over time and link this with data on Covid-19 infection by country. Panel A of Figure 2 tracks the evolution of government approval over the course of the pandemic as am average for all countries. Panel B then splits the sample into countries with high case growth and those with low case growth. For this purpose, we calculate the average growth rate of Covid infections since the 100th case for each country and then split the sample at the median rate. Among others, this approach classifies Russia, Sweden, Brazil, the United Kingdom and United States as countries with high case growth. The grey shaded areas represent 90 % confidence bands (grey areas).

The graph points to a rally-around-the-flag effect (Mueller 1970), at least initially. During the first four weeks after the outbreak approval increases strongly, on average, for all countries and under all policies. The gain in popularity is less pronounced for the group of high case growth countries, but still sizable, with an increase of about 8 percent. However, in this group, approval quickly starts to decline again after the initial rally. At the end of week 13, high case growth countries are back to their pre-pandemic approval level. In contrast, governments in countries with low case growth do not see a drop in approval numbers. By week 13, their approval level is still 20 percent higher than their pre-pandemic level (this corresponds to an average increase of 7 percentage points, i.e. from an average pre-pandemic level of 41 to 48, see A2 in Appendix C) . The difference between the two groups is both quantitatively large and statistically significant.

3.2 Regression result: Infection growth reduces approval

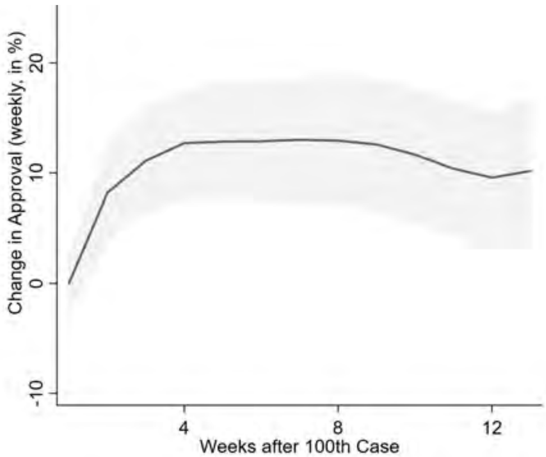
Next, we move to a more systematic analysis of the relationship between the growth rate of Covid infections and changes in approval. Table 2 reports regression results for our sample of countries indexed to the day of the 100th case. We start by including only case growth and a constant as explanatory variables, with country fixed effects (Column 1). The coefficient is negative and significant at the 1% level, confirming the previous graphical evidence.

In Columns 2 to 4, we add changes in death, economic activity and the Stringency Index one-by-one. The coefficient on case growth remains statistically significant at slightly varying sizes across specifications. The coefficient of new Covid-19 fatalities enters with the expected, negative sign, although it is barely statistically significant, only at a 10% level. More surprisingly, economic activity also has a negative sign, and is statistically significant in Column 3. This suggests that a stronger downturn is associated with gains in approval. We interpret this as the mirror image of the relationship depicted in Column 4, namely the statistically significant relationship between stringency and approval. Indeed, there is a high negative correlation between activity and stringency (-0.73) over our sample period. Taken together, Columns 3 and 4 suggest that leaders that enforce strict policies,

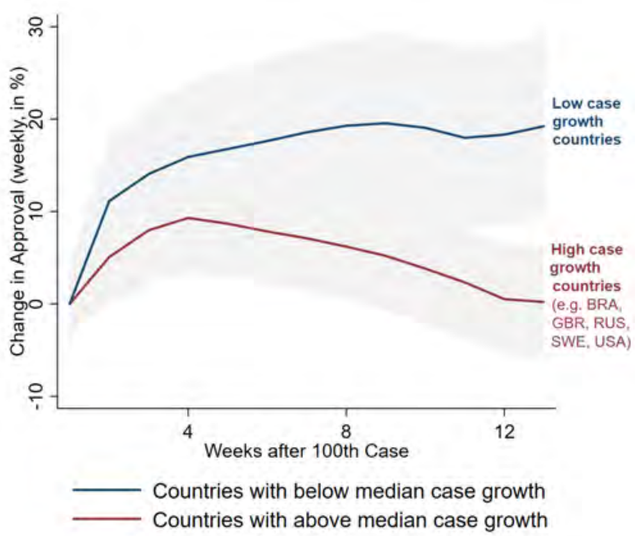
Covid Economics 50, 25 September 2020: 3-32

Figure 2: Government approval during the Covid-19 crisis

Panel A: Government approval, all countries



Panel B: Approval in countries with high and low case growth



Notes: This figure shows the percentage change in government approval on a weekly basis after the outbreak of the Covid-19 pandemic. Panel A averages across all 35 countries in our sample. Panel B splits the sample into two groups: Countries with below median case growth during the sample and countries with high case growth. The shaded grey areas show 90 percent confidence bands. The figure is based on an indexed sample, starting at the week of the 100th reported case in a given country. The data are smoothed using 3-week moving averages.

Covid Economics 50, 25 September 2020: 3-32

Table 2: Government approval and Covid-19 infections

	Contemporaneous explanatory variables							1-week lags
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δ Cases	-4.48***	-6.92***	-5.69***	-4.28**	-7.47***	-5.93***	-4.75**	-5.75***
(new Covid-19 infections)	(1.60)	(2.17)	(1.73)	(1.59)	(2.42)	(1.75)	(2.23)	(1.66)
Δ Deaths		-0.47*			-0.46*	-0.48	-0.29	-0.50
(new Covid-19 fatalities)		(0.25)			(0.26)	(0.33)	(0.29)	(0.31)
Δ Activity			-0.17***		-0.09	-0.21		-0.11
(change in mobility, Google)			(0.05)		(0.13)	(0.13)		(0.11)
Stringency government response				0.13**	0.06			
(Oxford index)				(0.05)	(0.13)			
Δ Activity							-0.27**	
(change in total electricity usage)							(0.13)	
Constant	12.47***	15.85***	7.96***	3.87	9.46*	6.33	10.12**	13.30**
	(0.73)	(2.06)	(1.78)	(3.21)	(5.30)	(7.04)	(4.36)	(6.45)
Observations	523	523	468	523	468	468	305	461
Countries	35	35	33	35	33	33	20	33
R2	0.09	0.13	0.15	0.12	0.18	0.20	0.16	0.16
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	No	No	No	No	No	Yes	Yes	Yes

Notes: The dependent variable is the change in government approval compared to the pre-pandemic level at weekly frequency. The main explanatory variable is the log growth rate of new Covid-19 infections. In Columns 1-7, the explanantory variables are included contemporanously, Column 8 uses 1-week lags. All results are estimated from an indexed sample, starting at the week of the 100th reported case in a given country. All regressions include robust standard errors clustered on country. Significance levels denoted by *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

and as a result or at the same time experience a sharp downturn in activity, benefit in terms of public support. Column 5 reports results when including all three variables jointly, with country fixed effects and a constant. The coefficient of case growth stays statistically significant, but the remaining explanatory variables lose their significance.

Column 6 shows our preferred specification, controlling for activity and Covid-19 fatalities and including country and time fixed effects. The effect of case growth is statistically significant at the 1% level. Quantitatively, a one standard deviation increase in the weekly growth rate of Covid infections is associated with a weekly decline in government approval of 3.6% (namely, $0.61 \times -5.93 = -3.6$) relative to the pre-pandemic level.

To give an example on the size of the effect: President Trump had an approval level 41.5% at the start of the pandemic, which rose to 42.5 during the first four weeks after the outbreak. Our results suggest that a one standard deviation increase in case growth in week 5 would have resulted in a fall in the approval rate by 1.5 percentage points in the next week ($41.5 \times -3.6 = -1.5$), to a new level of 41.

We estimate the same model using an alternative, electricity-based proxy of economic activity in Column 7, which is available for 20 of the 35 countries. The results in this reduced sample remain robust, with the effect of case growth slightly smaller but still significant at the 5 % level. In Column 8, we re-estimate the specification in Column 6, but including all explanatory variables as one week lags in order to capture delayed reactions

Covid Economics 50, 25 September 2020: 3-32

Covid Economics 50, 25 September 2020: 3-32

in popularity. The results are almost unchanged.

In Appendix B, Table A2, we show that we obtain similar results when using approval changes in percentage points (this is, instead of using as dependent variable $(A_t - A_0)/A_0$ as in these tables we use $A_t - A_0$, where A_t is the approval in week t and A_0 the approval at the outbreak). Our results are also robust to excluding countries without weekly polls and when using a daily or monthly data frequency (see Table A3 in Appendix B).

3.3 Dynamic effects: the impact of infection growth increases over time

We now analyze the evolution over time, especially comparing periods of policy tightening (first weeks after the outbreak) with periods of policy loosing (second phase). Figure 3 sheds light on the dynamics between approval and the explanatory variables. The black line in Panel A plots the correlation coefficient between Covid case growth and changes in political approval over time, while the two thin, dotted lines represents 90 % confidence bands (dotted lines, based on bootstrapped standard errors). As can be seen, the correlation is positive early on and becomes more negative over time.

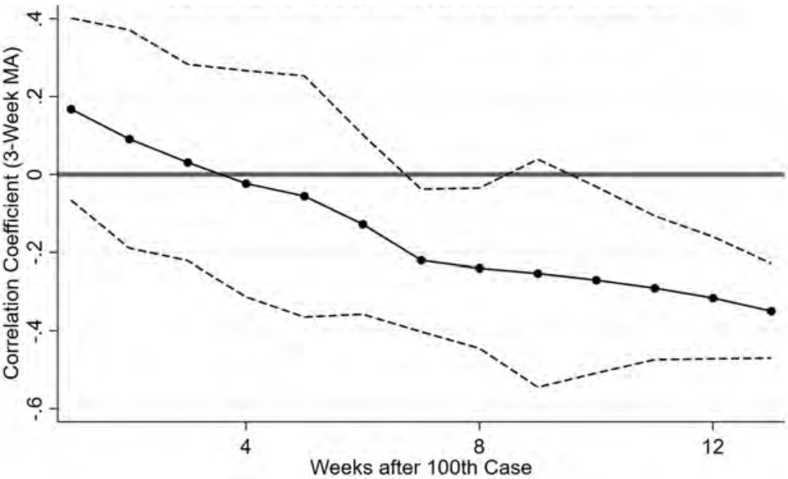
Panel B compares this to the correlation coefficients with (i) changes in stringency and (ii) economic activity. The correlation between approval and stringency is positive early on (as high as 0.35), but falls to around zero after the 6th week. Changes in workplace visits are positively correlated with changes in approval, however the coefficients are small in magnitude. In comparison, the negative correlation between Covid case growth and approval is more sizable and has a stronger time trend. This suggests not only an initial ”rally after the flag” effect, in which the public seems to grant a “truce period” at the outbreak, but also that the public becomes increasingly impatient with the increase in infections as time goes by.

A more formal analysis of the time varying correlation between case growth and changes in approval is illustrated in Figure 2. Specifically, we expand the baseline model in Column 6 of Table 2 (also shown in Column 1 of Table 3) by including time dummies for what can be termed the ”first” and ”second” phase of the Covid-19 pandemic. The first phase is characterized by rapidly increasing case numbers and policy tightening and typically spans the first 8-10 weeks after the outbreak. The second phase, is then a period of lower or at least more stable case numbers and gradual policy loosening in most countries. To be conservative, we choose a 10-week cut-off, since by then most countries saw decreasing case numbers, but the results are stable when shifting the cut-off forward or backward by 2 weeks.

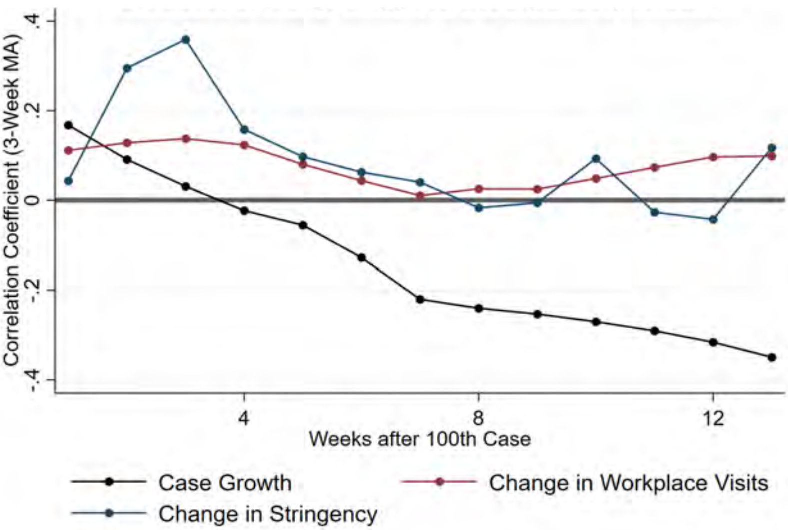
Column 2 of Table 3 includes country fixed effect only. We find that the coefficient of case growth during the first phase roughly corresponds to those obtained for the whole sample. For the second phase, however, the coefficient increases more than threefold. Both coefficients are statistically significant at the 1 and 5 percent confidence level, respectively. Next, we include time fixed effects (Column 3). Both coefficients remain statistically significant and of roughly similar size. If anything, the difference in magnitude becomes

Figure 3: Correlation of approval rates and main explanatory variables

Panel A: Correlation of approval and Covid-19 case growth



Panel B: Correlation of approval with case growth, economic activity and stringency



Notes: This figure shows correlation coefficients (3-week moving averages) of changes in approval and the main explanatory variables. Correlation coefficients are computed based on the cross section of countries in a given period. Panel A plots the correlation coefficients of case growth and approval, including 90 percent confidence bands based on bootstrapped standard errors. Panel B compares the correlation of changes in approval with changes in stringency, activity and case growth. The figure is based on an indexed sample, starting at the week of the 100th reported case in a given country.

Covid Economics 50, 25 September 2020: 3-32

CEPR PRESS

Table 3: The dynamic effect of case growth on approval

	(1)	(2)	(3)
Δ Cases	-5.93***		
(new Covid-19 infections)	(1.75)		
First Phase × Δ Cases		-7.82***	-5.79***
		(2.17)	(1.80)
Second Phase × Δ Cases		-28.50**	-30.49**
		(12.09)	(11.58)
Δ Deaths	-0.48	-0.39	-0.46
(new Covid-19 infections)	(0.33)	(0.28)	(0.34)
Δ Activity	-0.21	-0.14*	-0.21
(change in mobility, Google)	(0.13)	(0.07)	(0.14)
Constant	6.33	12.31***	5.86
	(7.04)	(2.95)	(7.58)
Observations	468	468	468
Countries	33	33	33
R2	0.20	0.19	0.21
Country FE	Yes	Yes	Yes
Time FE	Yes	No	Yes

Notes: The dependent variable is the change in government approval compared to the pre-pandemic level at weekly frequency. The main explanatory variable is the log growth rate of new Covid-19 infections. Column 1 shows the preferred specification from Table 2. Columns 2 and 3, case growth is interacted with time dummies for first phase (up to the 10th week after the outbreak) and second phase (after the 10th week). The non-interacted dummies are included in the model, but their coefficients are omitted from the table. All regressions include robust standard errors clustered on country. Significance levels denoted by *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Covid Economics 50, 25 September 2020: 3-32

CEPR PRESS

even more pronounced after adding time fixed effects. The coefficient for case growth clearly becomes more negative over time. Results based on percentage point changes are reported in Columns 4 and 5 of Table A2 in Appendix B.

The growing impact of case growth on approval is consistent with the notion that governments receive the benefit of the doubt early on, but are punished when Covid-19 case numbers do not drop or even increase again later on.

3.4 Interaction effects: conditioning on policy stringency

In this section, we analyze the conditional effect of government actions on the relationship between case growth and approval. The goal is to interpret the reaction of the public to an increase in infections when considering how leaders are acting. For that purpose, we augment the baseline model in Column 6 of Table 2 (also shown as Column 1 in Table 4), by including the Stringency Index and by including an interaction term of case growth and stringency in the regression (Column 3). The interaction coefficient turns out to be positive and statistically significant at the 1% level, suggesting that the effect of case growth declines (i.e. becomes less negative) at high levels of stringency.[12]

Figure 4 builds on Column 3 and illustrates this relationship graphically, by showing the effect of case growth on approval at different levels of policy stringency, as well as 90 percent confidence bands (dotted lines). Case growth is significant only for index levels below 80. At the median level of stringency (at 61), case growth has a coefficient of -6, such that a one standard deviation increase in the weekly growth of cases is associated with a 3.6 percent decline in approval (namely, $0.61 \times -6 = -3.6$) relative to the pre-pandemic level. However, at lower index values (loose policies) the coefficient for case growth strongly increases in size. For example, at a stringency index value of 40 the coefficient decreases to -10, such that a one standard deviation increase in weekly case growth is associated with a 6.1 percent decrease in approval (namely, $0.61 \times -9.5 = -5.8$).

Figure 4 thus lends support to the idea that the number of Covid infections influence approval only in countries which mismanage the pandemic and fail to implement forceful containment measures. To provide further perspective on this, Figure 4 shows the Stringency Index for all countries in our sample since the day the 100th case was reported. As can be seen, there is remarkable co-movement in terms of the government responses, in particular during the first few weeks. Since outbreak timing differs across regions, some countries already had stringent policies in place before the 100th case was confirmed. From the 8th week onwards, policy stances begin to diverge more strongly, with some countries deciding to loosen measures earlier than others.

Even though stringency and economic activity are highly correlated in average, in the second stage they do not move as close. As an example, Croatia and Portugal had roughly similar policy stances and experienced economic downturns of equal magnitude at the beginning of April (Stringency levels were at 96 and 82 respectively, activity declined by 55

[12] Results based on percentage point changes are shown in A2 in Appendix B, and are consistent with these results.

Covid Economics 50, 25 September 2020: 3-32

Covid Economics 50, 25 September 2020: 3-32

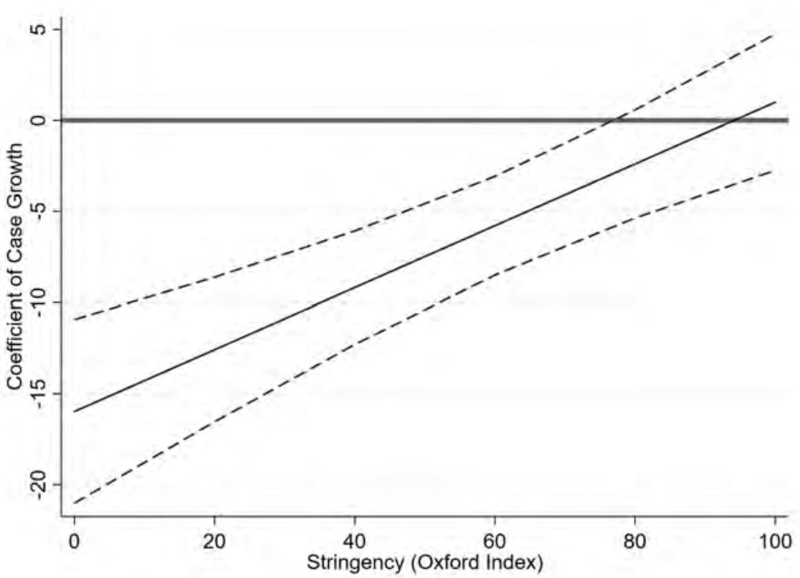
Table 4: Interaction effects between case growth and stringency

	(1)	(2)	(3)
Δ Cases	-5.93***	-5.57***	-15.97***
(new Covid-19 infections)	(1.75)	(1.65)	(3.07)
Δ Deaths	-0.48	-0.48	-0.43
(new Covid-19 fatalities)	(0.33)	(0.32)	(0.31)
Δ Activity	-0.21		
(change in mobility, Google)	(0.13)		
Stringency government response		0.14*	-0.01
(Oxford index)		(0.08)	(0.07)
Δ Cases × Stringency government response			0.17***
			(0.04)
Constant	6.33	4.70	14.05**
	(7.04)	(6.26)	(5.91)
Observations	468	523	523
Countries	33	35	35
R2	0.20	0.17	0.22
Country FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes

Notes: The dependent variable is the change in government approval compared to the pre-pandemic level at weekly frequency. The main explanatory variable is the log growth rate of new Covid-19 infections. All results are estimated from an indexed sample, starting at the week of the 100th reported case in a given country. All regressions include robust standard errors clustered on country. Significance levels denoted by *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Covid Economics 50, 25 September 2020: 3-32

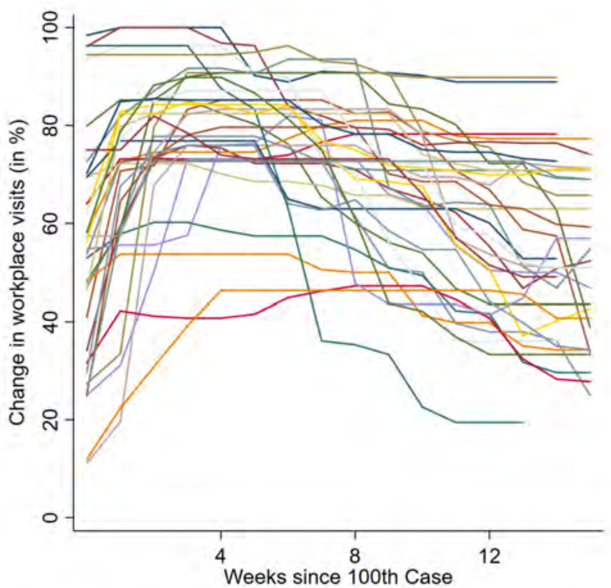
Figure 4: Interaction between case growth and stringency



Notes: This figure shows the effect of case growth on approval for different levels of policy stringency (based on Column 3 of Table 3). The dotted lines show 90 percent confidence bands.

Covid Economics 50, 25 September 2020: 3-32

Figure 5: Variation in government responses (stringency index)



Notes: This figure shows the variation of government responses (Stringency index) over time across the 35 countries in our sample. The figure is based on an indexed sample, starting at the week of the 100th reported case in a given country.

% and 60 %). Until the end of June, Croatia underwent rapid re-opening, dropping all the way to 36 in terms of stringency, whereas Portugal barely loosened policies and remained at a level of about 70. However, the economic recovery in the two economies, based on the number of workplace visits, was of identical size, gaining 35 percentage points each until the end of June. Across countries, the correlation between policy stringency is -0.72 in the first phase (before the 10th week after the pandemic outbreak) and -0.62 in the second phase (after the 10th week). Figure A3 in the Appendix provides further visual evidence on the cross-country patterns of economic activity over time.

To summarize, since case growth is associated with losses in approval only when response measures are loose, strategies which err on the side of caution seem to pay off politically.

4 (Mis-)managing the Covid crisis: survey evidence

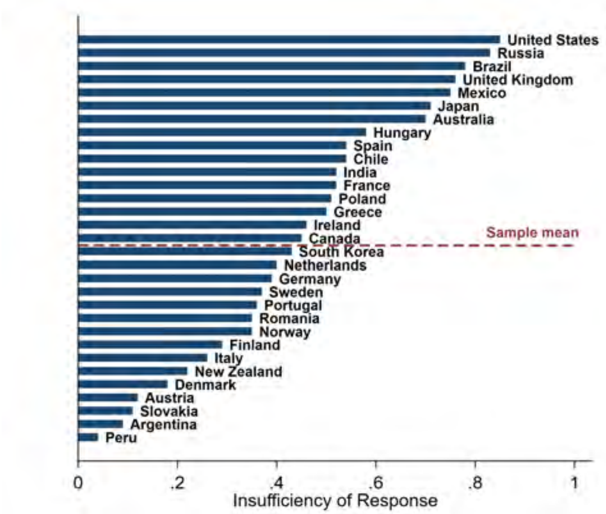
In this section we add further, more direct but also less comprehensive, evidence on the political consequences of mismanaging the Covid crisis. In particular, we consider how the population evaluates the governments's Covid management based on an international survey conducted by Fetzer et al. (2020).[13] The survey includes responses for more than

[13]Accessed on 10.07.2020, https://covid19-survey.org/index.html

100,000 individuals between March 20th and April 6th who, among other questions, were asked to evaluate their respective governments' handling of the Covid crisis (*"Do you think your governments' response to the current Coronavirus outbreak is appropriate, too extreme or not sufficient?"*). Participants responses were captured on a 5-point scale. The results are reported as percent of respondents that judge the governments' response as "insufficient".

Figure 6 illustrates the survey results for the 31 countries in our sample that are also covered by the Fetzer et al. (2020) dataset. The picture shows a large heterogeneity across countries. On the one side of the spectrum, in Argentina and Peru, only around 10 % of participants were dissatisfied with the government response to Covid. In contrast, more than 80% of participants from the United States and Russia thought that the government response was insufficient.

Figure 6: Covid-19 survey: Insufficient government reaction (in %)



Notes: This figure shows the results from an international survey of Fetzer et al. (2020) and covers the 31 countries in our sample that were included in that survey. The bars show the percent of respondents in each country that judge the governments' handling of the Covid crisis as "insufficient".

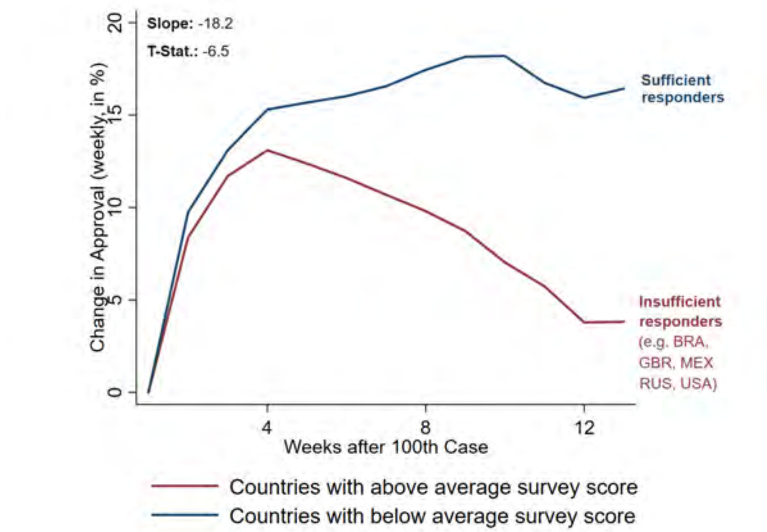
Next, we split the sample of 31 countries with available data at the average level of "insufficiency" (above or below 44%). Based on this, we form two country groups and then compare approval rates over time by adding in our own dataset.

Figure 7 shows the result for the two groups. Similar to Figure 2, there is a strong initial increase in approval in the full sample until around the 4th week, with rates increasing by between 12.5 and 15 percent. Thereafter, the average approval of countries belonging to the "sufficient responders" continues to increase (reaching a 17 percent gain after 13 weeks), while the approval rate of "insufficient responders" collapses. At the end of the

Covid Economics 50, 25 September 2020: 3-32

sample period, the difference between the two groups amounts to more than 10 percentage points, on average.

Figure 7: Government approval and sufficiency of Covid-19 response



Notes: This figure shows the percentage change in government approval on a weekly basis after the outbreak of the Covid-19 pandemic for two groups of countries based on Fetzer et al. (2020). The sample is split at the average survey score across countries, with "sufficient responders" being countries in which less than 44% judged the government response as insufficient, while "insufficient responders" are countries with a score below that average. The figure is based on an indexed sample, starting at the week of the 100th reported case in a given country. The data are smoothed using 3-week moving averages. The statistics in the upper left corner show the coefficient and t-statistic of a simple regression model of weekly approval on the survey score and an intercept.

We also confirm the negative relationship between survey response and popularity more formally, using a simple regression model of weekly approval on the survey score and an intercept. As shown in the upper left corner of Figure 7, the slope coefficient is negative and statistically significant at the 1% level, suggesting that a higher survey scores (i.e. more respondents judging the response as "insufficient") is associated with less approval. Taken as a whole, this result implies that governments which are badly evaluated in their handling of the Covid crisis, lose out substantially in terms of popularity.

5 Conclusion

This short paper studies the political approval consequences of the Covid 19 pandemic, focusing on case numbers and the policy response of governments. We construct a comprehensive, high-frequency dataset on government approval and document a robust, and quantitatively sizeable, relationship between the weekly growth rate of Covid infections

Covid Economics 50, 25 September 2020: 3-32

and changes in government approval. However, this effect only holds when government policies are loose, not when strict measures are imposed to counter the spread of the virus. Based on a cross-country survey, we further show that the pandemic responses are key for approval:governments that are badly evaluated in managing the pandemic, are those that do worst in terms of approval.

On the one hand, the cross-country scope of our dataset allows a comparison of political support for leaders across the world facing a common, synchronous and novel shock: a pandemic. On the other hand, the high-frequency dimension of our dataset allows keeping track of the dynamics of political support in how governments deal with this unique common challenge.

The evidence we present sheds new light on the policy trade-offs that politicians face in a pandemic. In a nutshell, governments that placed more weight on health outcomes versus economic outcomes gained political support. Moreover, this effect increases over time. At the initial stages of the pandemic, leaders are granted the benefit of the doubt, but this "token of trust" fades quickly: after about 4 weeks, growing case numbers increasingly hurt political approval, especially if no stringent policies were in place.

In sum, this paper highlights what the public feels most strongly about: the growth of infections (rather than the resulting deaths or other factors), especially when unaccompanied by efforts to curb them with stringent policies, even at the expense of economic activity.

Covid Economics 50, 25 September 2020: 3-32

References

Aktay, Ahmet, Shailesh Bavadekar, Gwen Cossoul, John Davis, Damien Desfontaines, Alex Fabrikant, Evgeniy Gabrilovich, Krishna Gadepalli, Bryant Gipson, Miguel Guevara, et al. 2020. "Google COVID-19 community mobility reports: Anonymization process description (version 1.0)." *arXiv preprint arXiv:2004.04145.*

Alon, Titan M, Minki Kim, David Lagakos, and Mitchell VanVuren. 2020. "How Should Policy Responses to the COVID-19 Pandemic Differ in the Developing World?" *Covid Economics* 22:1–46.

Bechtel, Michael M, and Jens Hainmueller. 2011. "How lasting is voter gratitude? An analysis of the short-and long-term electoral returns to beneficial policy." *American Journal of Political Science* 55 (4): 852–868.

Besley, Timothy, and Sacha Dray. 2020. "The Political Economy of Lockdown: Does Free Media Make a Difference?" *mimeo.*

Boin, Arjen, and Paul't Hart. 2003. "Public leadership in times of crisis: mission impossible?" *Public Administration Review* 63 (5): 544–553.

Bol, Damien, Marco Giani, André Blais, and Peter John Loewen. 2020. "The effect of COVID-19 lockdowns on political support: Some good news for democracy?" *European Journal of Political Research, forthcoming.*

Campante, Filipe R, Emilio Depetris-Chauvin, and Ruben Durante. 2020. "The Virus of Fear: The Political Impact of Ebola in the US." *NBER Working Paper No. 26897.*

Correia, Sergio, Stephan Luck, and Emil Verner. 2020. "Pandemics Depress the Economy, Public Health Interventions Do Not: Evidence from the 1918 Flu." *mimeo.*

Deb, Pragyan, Davide Furceri, Jonathan D. Ostry, and Nour Tawk. 2020. "The effect of containment measures on the COVID-19 pandemic." *Covid Economics* 19:53–86.

De Vries, Catherine E, Bert N Bakker, Sara Hobolt, and Kevin Arceneaux. 2020. "Crisis Signaling: How Italy's Coronavirus Lockdown Affected Incumbent Support in Other European Countries." *mimeo.*

Drazen, Allan. 2000. *Political Economy in Macroeconomics.* Princeton University Press.

Duch, Raymond M, and Randolph T Stevenson. 2008. *The Economic Vote: How Political and Economic Institutions Condition Election Results.* Cambridge University Press.

Farboodi, Maryam, Gregor Jarosh, and Robert Shimer. 2020. "Internal and External Effects of Social Distancing in a Pandemic." *Covid Economics* 9:22–58.

Fetzer, Thiemo, Marc Witte, Lukas Hensel, Jon Jachimowicz, Johannes Haushofer, Andriy Ivchenko, Stefano Caria, Elena Reutskaja, Christopher Roth, Stefano Fiorin, et al. 2020. "Global behaviors and perceptions in the COVID-19 pandemic." *NBER Working Paper No. 27082.*

Covid Economics 50, 25 September 2020: 3-32

Frieden, J. 2020. “The Political Economy of Economic Policy.” *Finance & Development* 57 (2): 4–9.

Giommoni, Tomasso, and Gabriel Loumeau. 2020. “Lockdown and voting behaviour: A natural experiment on postponed elections during the COVID-19 pandemic.” *Covid Economics* 41:69–99.

Hale, Thomas, Anna Petherick, Toby Phillips, and Samuel Webster. 2020. “Variation in government responses to COVID-19.” *Oxford Blavatnik School of Government Working Paper*, vol. 31.

Herrera, Helios, Guillermo Ordoñez, and Christoph Trebesch. 2020. “Political Booms, Financial Crises.” *Journal of Political Economy* 128 (2): 507–543.

Lewis-Beck, Michael S, and Mary Stegmaier. 2000. “Economic determinants of electoral outcomes.” *Annual Review of Political Science* 3 (1): 183–219.

Mansour, Hani, Daniel I Rees, and James M Reeves. 2020. “Voting and Political Participation in the Aftermath of the HIV/AIDS Epidemic.” *NBER Working Paper No. 27504*.

Mueller, John E. 1970. “Presidential popularity from Truman to Johnson.” *The American Political Science Review* 64 (1): 18–34.

Pulejo, Massimo, and Pablo Querubín. 2020. “Electoral Concerns Reduce Restrictive Measures During the COVID-19 Pandemic.” *NBER Working Paper No. 27498*.

Snowberg, Erik, Justin Wolfers, and Eric Zitzewitz. 2007. “Partisan Impacts on the Economy: Evidence from Prediction Markets and Close Elections.” *The Quarterly Journal of Economics* 122 (2): 807–829.

Covid Economics 50, 25 September 2020: 3-32

CEPR PRESS

Appendix A Data and Sources

Table A1: Sample Summary

Country	Approval data on leader	Incumbent leader or government coalition parties	Number of polls in 2020	Source(s)
Australia	Yes	Scott Morrison	27	MorningConsult
Argentina	Yes	Alberto Fernández	19	Inteligencia Analítica, Synopsis
Austria	No	People's Party, The Greens	26	Politico, Wikipedia (various)
Brazil	Yes	Jair Bolsonaro	19	Wikipedia (various)
Canada	Yes	Justin Trudeau	26	Wikipedia (various)
Chile	Yes	Sebastián Piñera	27	Cadem
Croatia	No	Christian Democratic Union, Liberal Democrats	20	Wikipedia (various)
Denmark	No	Social Democrats	35	Wikipedia (various)
Estonia	No	Centre Party, Social Democratic Party, Estonia 200	35	Wikipedia (various)
Finland	No	Social Democratic Party, Left Alliance, Centre Party, People's Party, Green League	14	Wikipedia (various)
France	Yes	Emmanuel Macron	32	Wikipedia (various)
Germany	No	Christian Democratic Union, Social Democratic Party	88	Wikipedia (various)
Greece	No	New Democracy	28	Wikipedia (various)
Hungary	No	Fidesz, Christian Democratic People's Party	17	Wikipedia (various)
Iceland	No	Independence Party, Left-Green Movement, Progressive Party	14	Wikipedia (various)
India	Yes	Narendra Modi	27	MorningConsult
Ireland	No	Fine Gael, Fianna Fáil, Green Party	17	Wikipedia (various)
Italy	No	5-Star Movement, Democratic Party	128	Wikipedia (various)
Japan	No	Liberal Democratic Party	30	Wikipedia (various)
Mexico	Yes	Andrés Manuel López Obrador	27	MorningConsult
Netherlands	No	People's Party for Freedom and Democracy, Christian Democratic Appeal, Democrats 66, Christian Union	29	Wikipedia (various)
New Zealand	No	Labour Party, New Zealand First, Greens	9	Wikipedia (various)
Norway	No	Conservative Party, Liberal Party, Christian Democratic Party	47	Wikipedia (various)
Peru	Yes	Martín Vizcarra	7	IPSOS, Pulso Peru
Poland	No	United Right	70	Wikipedia (various)
Portugal	No	Socialist Party	20	Wikipedia (various)
Romania	No	National Liberal Party	13	Wikipedia (various)
Russia	No	United Russia	25	Wikipedia (various)
Slovakia	No	Freedom and Solidarity, For the People, Christian Union, We are Family	22	Wikipedia (various)
Slovenia	No	Democratic Party, Democratic Party of Pensioners, Modern Centre Party, New Slovenia	25	Wikipedia (various)
South Korea	Yes	Moon Jae-in	30	Wikipedia (various)
Spain	No	Socialist Workers' Party, United We Can	59	Wikipedia (various)
Sweden	No	Social Democratic Party, Green Party	31	Wikipedia (various)
United Kingdom	Yes	Boris Johnson	43	Wikipedia (various)
United States	Yes	Donald Trump	114	Wikipedia (various)

Covid Economics 50, 25 September 2020: 3-32

CEPR PRESS

Appendix B Robustness

Table A2: Government approval and Covid-19 infections (Percentage point results)

	Baseline model		1-week lags	Time interaction		Policy interaction	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δ Cases	-2.11***	-1.77*	-2.21***			-2.07***	-6.43***
(new Covid-19 infections)	(0.71)	(0.87)	(0.66)			(0.72)	(1.32)
Δ Deaths	-0.19	-0.12	-0.19	-0.16	-0.18	-0.18	-0.16
(new Covid-19 fatalities)	(0.13)	(0.11)	(0.13)	(0.11)	(0.13)	(0.12)	(0.12)
Δ Activity	-0.07		-0.05	-0.05**	-0.08		
(change in mobility, Google)	(0.04)		(0.04)	(0.02)	(0.05)		
Δ Activity		-0.08*					
(change in total electricity usage)		(0.04)					
First Phase × Δ Cases				-2.75***	-2.02***		
				(0.85)	(0.72)		
Second Phase × Δ Cases				-15.94***	-16.59***		
				(5.55)	(5.52)		
Stringency government response						0.04	-0.02
(Oxford index)						(0.03)	(0.03)
Δ Cases × Stringency government response							0.07***
							(0.02)
Constant	2.04	3.69**	4.27*	4.00***	1.76	2.17	6.09***
	(2.34)	(1.54)	(2.15)	(1.00)	(2.53)	(2.18)	(2.21)
Observations	468	305	461	468	468	523	523
Countries	33	20	33	33	33	35	35
R2	0.17	0.15	0.15	0.18	0.19	0.14	0.20
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	No	Yes	Yes	Yes

Notes: The dependent variable is the change in government approval in percentage points at weekly frequency. The main explanatory variable is the log growth rate of new Covid-19 infections. Columns 1-2 show the baseline model, with mobility and electricity-based actitivty proxies, respectively. Column 3 includes all explanatory variables as 1-week lags. In Column 4, the stringency level is included instead of economic activity. Column 5 interacts the level of policy stringency and case growth. In Columns 6-7, case growth is interacted with time dummies for first phase (up to the 10th week after the outbreak) and second phase (after the 10th week of the outbreak). The non-interacted dummies are included in the regressions but omitted from the table. All results are estimated from an indexed sample, starting at the week of the 100th reported case in a given country. All regressions include robust standard errors clustered on country. Significance levels denoted by *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Covid Economics 50, 25 September 2020: 3-32

CEPR PRESS

Covid Economics 50, 25 September 2020: 3-32

Table A3: Robustness regression results

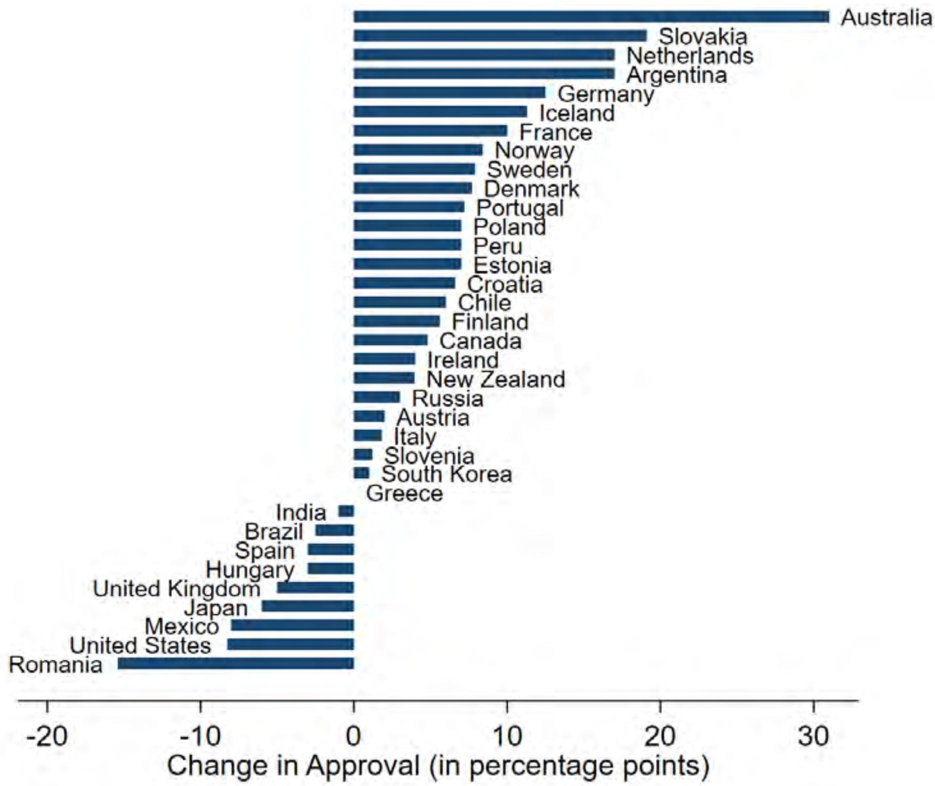
	Excluding countries without weekly polls	Daily data	Monthly data
	(1)	(2)	(3)
Δ Cases	-6.44***	-24.64***	-8.02**
(new Covid-19 infections)	(1.87)	(8.17)	(3.24)
Δ Deaths	-0.49	-0.23	-0.13
(new Covid-19 fatalities)	(0.34)	(0.17)	(0.22)
Δ Activity	-0.22	-0.16	0.65***
(change in mobility, Google)	(0.15)	(0.11)	(0.22)
Constant	7.76	6.23	75.29***
	(7.91)	(5.77)	(20.51)
Observations	430	3560	130
Countries	30	33	33
R2	0.21	0.22	0.46
Country FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes

Notes: The dependent variable is the weekly change in government approval. The main explanatory variable is the log growth rate of new Covid-19 infections. Column 1 is based on weekly data and excludes countries without weekly polls (New Zealand, Peru, Iceland and Finland), columns 2 and 3 use daily and monthly data, respectively. All results are estimated from an indexed sample, starting at the week of the 100th reported case in a given country. All regressions include robust standard errors clustered on country. Significance levels denoted by *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

CEPR PRESS

Appendix C Backup Figures

Figure A1: Government approval during the Covid-19 crisis (change in percentage points)



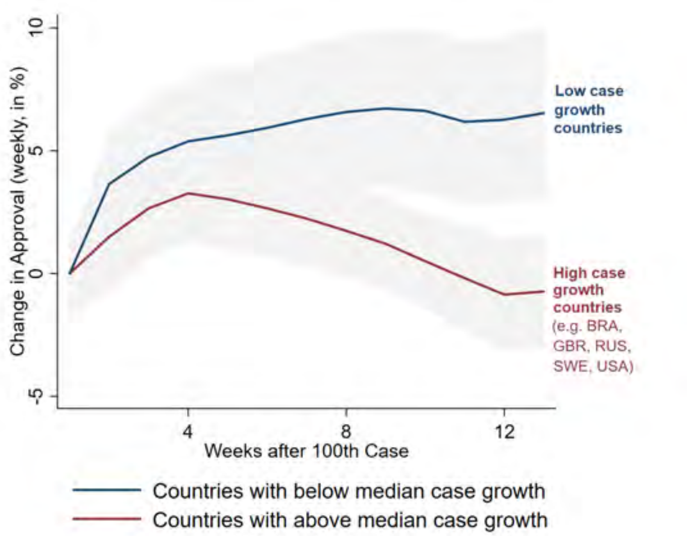
Notes: This figure shows the percentage point change in government approval from February to July 2020 for the 35 countries in our sample. We construct a measure of government approval on a weekly basis using a combination of approval rates of the incumbent leader approval and voting intentions for the coalition government parties (see Table A1 in Appendix A for details.

Covid Economics 50, 25 September 2020: 3-32

CEPR PRESS

Covid Economics 50, 25 September 2020: 3-32

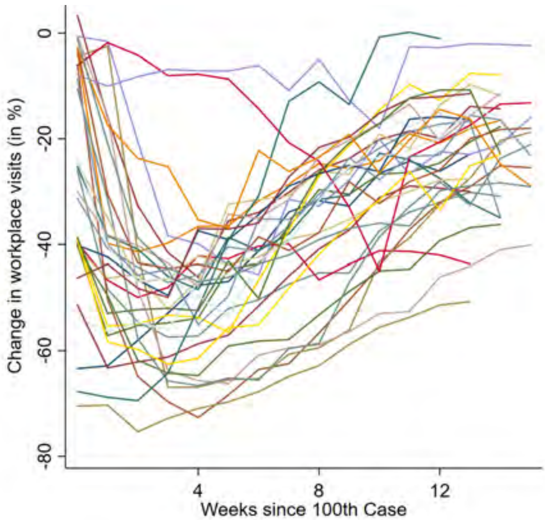
Figure A2: Dynamics of approval: high vs. low case growth (using percentage points)



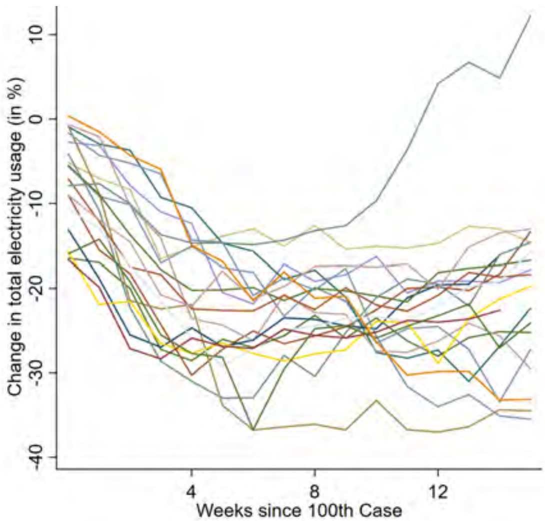
Notes: This figure shows the change in government approval (in percentage points) on a weekly basis after the outbreak of the Covid-19 pandemic. The sample is split into two groups: Countries with below median case growth during the sample and countries with high case growth. The shaded grey areas show 90 percent confidence bands. The figure is based on an indexed sample, starting at the week of the 100th reported case in a given country. The data are smoothed using 3-week moving averages.

Figure A3: Economic activity: the Covid recession by country

Panel A: Activity based on workplace visits



Panel B: Activity based on electricity usage



Notes: This figure shows the variation of changes in economic activity over time. Panel A is based on workplace visits (Google mobility) for the 33 countries in our sample with available data. Panel B is based on total electricy usage and available for 20 countries in our sample. All figures are based on an indexed sample, starting at the week of the 100th reported case in a given country.

Covid Economics 50, 25 September 2020: 3-32

CEPR PRESS

Covid Economics Issue 50, 25 September 2020

The Covid pandemic in the market: Infected, immune and cured bonds[1]

Andrea Zaghini[2]

Date submitted: 17 September 2020; Date accepted: 18 September 2020

By focusing on the cost conditions at issuance, I find that not only the Covid-19 pandemic effects were different across bonds and firms at different stages, but also that the market composition was significantly affected, collapsing on investment-grade bonds, a segment in which the share of bonds eligible to the ECB corporate programmes strikingly increased from 15% to 40%. At the same time the high-yield segment shrunk to almost disappear at 4%. In addition to a market segmentation along the bond grade and the eligibility to the ECB programmes, another source of risk detected in the pricing mechanism is the weak resilience to pandemic: the premium requested is around 30 basis points and started to be priced only after the early containment actions taken by the national authorities. On the contrary, I do not find evidence supporting an increased risk for corporations headquartered in countries with a reduced fiscal space, nor the existence of a premium in favour of green bonds, which should be the backbone of a possible "green recovery".

Covid Economics 50, 25 September 2020: 33-73

1 The author would like to thank Florian Heider, Philipp Hartmann, Peter Hoffmann and Simone Manganelli for helpful comments and suggestions. The views and opinions expressed in this paper are those of the author and do not necessarily agree with the official policy or position of the ECB..

2 Lead Economist, European Central Bank.

CEPR PRESS
Covid Economics 50, 25 September 2020: 33-73

1 Introduction

In September 2019, the ECB announced a new wave of corporate asset purchases at a monthly pace of €20 billion to reinforce the accommodative impact of the policy rates and ease euro-area corporations' financing conditions. In particular, following the experience of the first Corporate Sector Purchase Programme (CSPP), the purchases of bonds of eligible quality were expected not only to have a direct impact on targeted bonds, concerning both prices and quantities (Todorov 2020), but also to trigger the portfolio rebalancing channel, the mechanism through which also bonds of lower quality, typically associated with small and medium enterprises (SMEs), are able to benefit of a reduced cost of issuance (Zaghini 2019). In addition, given a sufficient time span, other channels could kick in and involve corporations with no access to the bond market by relaxing banks' lending constraints (Grosse-Rueschkamp et al. 2019)

Four months into the programme the economic, financial and social outlook completely changed in the euro area and worldwide due to the outburst and spreading of the Covid-19 pandemic. The ECB promptly acted by launching on 18 March, 2020 a new temporary asset purchase programme (PEPP, Pandemic Emergency Purchase Programme) on the much larger scale of 120 billion per month to counter the serious risks to the monetary policy transmission mechanism and the economic outlook for the euro area posed by the escalating diffusion of the virus.

The Covid-19 is an infectious disease brought about by a Corona virus which causes a severe acute respiratory syndrome with a deadly rate strongly depending on the age of the infected person. The disease was first identified in December 2019 in Wuhan, China and it rapidly spread from January 2020 around the world. The first official case recorded in the US is dated 21 January, 2020, while the first case in Europe is recorded in France just three days later. The World Health Organization (WHO) declared it a "public health emergency of international concern" on January 30, 2020. Up to that date financial market reactions were muted, almost entirely ignoring the spreading of the virus and behaving as if the diffusion would not have had any implication for the economic activities.

Market reactions started only after the first significant set of interventions against the spreading of the virus in Italy (the first strongly hit European country). On 23 February, 2020 the Italian government announced a decree imposing the quarantine of more than 50,000 people from 11 municipalities in Northern Italy. The Italian military and law enforcement agencies were instructed to secure and implement the lockdown. The quarantine zones identified as the centres of the two main clusters were called the Red zones. From then on an escalation of similar and stronger decisions were taken all over Europe and the world. In the 30 days starting from February 24, 2020 stock markets collapsed and volatility surged. The US S&P 500 Index lost one third of its value and the Euro Stoxx fell by 37%. Falls of comparable magnitude were recorded around the world: Brazil (46%), Japan (31%), Hong Kong (25%). The same dramatic development involved also the bond market, with the yield of both investment grade (IG) and high yield (HY) bonds skyrocketing in the euro area above the peaks reached during the sovereign debt crisis in 2012. Only in late March, when central banks stepped in providing liquidity to banks and restarting or increasing the purchases of both sovereign and corporate bonds, did financial markets change direction.

Not all bond segments were equally hit by the changing market conditions and not all bond segments equally recovered when the outlook improved. In order to assess which bonds suffered most from the Covid-19 pandemic and whether the ECB measures were effective in tackling the shock, in the first part of the paper I propose an empirical assessment of the two ECB corporate programmes (CSPP and PEPP) on corporations' cost of funding, by providing an answer to the following questions: Were the programmes able to involve targeted and non-targeted bonds (via the portfolio rebalancing channel) before, during and after the Corona virus spreading? Were the ECB

Covid Economics 50, 25 September 2020: 33-73

purchases able to shelter the different euro-area bond market segments from the deteriorating market conditions as of end February 2020? Did eligible bonds enjoy a reduced cost of placement in both good and bad times?

In order to answer the above-mentioned questions I refer to the yield on bonds at issuance, i.e. I look at the developments in the primary bond market, which is the place where the cost of funding is set for the issuing corporations (Sironi 2003; Zaghini 2019). Indeed, while secondary market prices can be thought of as the market assessment of a possible new placement in that moment, they do not change the face value of the already issued bonds: in other words, they do not change the cost for the issuing entity. Instead, the single originating trade on the primary market exactly defines the corporation's commitment and the actual funding cost.

While the impact of monetary policy measures on the price of secondary trades is fast, due to the large market liquidity and a time-continuos trading system, the adjustment on the primary bond market takes longer. For instance, while the initial effect of a policy measure is usually recorded in secondary market indexes on the same day of the announcement, it will show up later on primary placements. This is due not only to the fact that new placements occur at discrete points in time and are often agreed upon much in advance, but also to a much larger heterogeneity of issuers and bonds in the primary market. Thus, to assess the effect of the two ECB programmes, it is important to have a sufficient time span after the announcement and the start of the purchases and a clear timeline to work with.

In addition to the choice of focusing at the primary placements, another aspect of care of the paper is the identification of the correct sample of eligible and non-eligible bonds. First, the markets in which the ECB actively purchases at issuance (Eurosystem market, from now on) is a particular subset of the world market which has to be constructed by looking at the bond level. Second, the eligibility criteria, while making all HY bonds non eligible, further distinguish among IG bonds, since not all of them are eligible for

Covid Economics 50, 25 September 2020: 33-73

purchase even when placed in the Eurosystem market. Both issues are often not clearly tackled in the empirical literature.

By focusing on the markets in which the ECB programmes are active and looking at the yield on new bond placements by both IG and HY corporations, I find that the ECB purchases have successfully sheltered the eligible bond segment from the first significant deterioration in market conditions occurred between late February 2020 and mid March 2020: the cost at issuance being estimated to be smaller in the range 60-80 basis points than non-eligible bonds. On the contrary, the ECB purchases were not effective afterwards (from the second half of March to end May 2020): the further worsening that took place after the PEPP announcement was felt by eligible bonds in the same way as all other IG bonds. Both results hold even when restricting the sample to the set of issuers that were able to place both eligible and non-eligible bonds.

In addition, there is no evidence of the working of a portfolio rebalancing channel in favour of HY bonds, the spread with respect to eligible bond having dramatically increased to above 200 basis points from around 40 basis points before the start of the purchases. On the contrary, the HY issuance has shrunk to almost disappear in the second half of March 2020. Thus, a consequence of the crisis is that the bond market almost entirely collapsed to IG bonds only, with the share of eligible bonds growing to around 40%. This in turn has a relevant policy implication: corporations getting a downgrade to the HY segment (the so called "fallen angels") may not be able to issue bonds any more, since neither the ECB nor the other investors are willing to purchase in that segment.

In the second part of the paper I take into account other possible sources of price discrepancies among bonds and corporations by testing three (mutually non exclusive) hypotheses about the bond pricing. In particular, I first test whether market investors require a disaster-premium on the most vulnerable corporations as found by Pagano et al. (2020) for the US stock

Covid Economics 50, 25 September 2020: 33-73

market. In their study, they check for differences in the return performance due to pandemic resilience, where the latter is defined as reliance on technologies and/or organizational structures that are robust to social distancing. They find that not only more resilient companies outperformed less resilient during the Covid outbreak, but also that similar cross-sectional return differentials emerged before the crisis. In their opinion this in turn suggests a growing awareness of pandemic risk well in advance of its materialization ("pre-disaster learning model"). Relying on the same measure of dependence on physical human interaction provided by Koren and Pető (2020), I instead find that there was not pandemic awareness before the Covid spreading, and that it took time to learn during the crisis and eventually differentiate according to resilience to human interaction. Using Pagano et al. (2020) terminology, the result thus suggests that the Eurosystem bond market behaved according to the "unpriced–disaster risk model".

Another possible source of concerns regarding the different ability of corporations in facing the adverse environment brought about by the Corona virus is due to the different fiscal room available to governments. Indeed, countries with reduced fiscal space might not be able to implement adequate measures aimed at supporting the economy and preventing a large number of firms going bankrupt. However, it turns out that the corporations headquartered in the countries most hit by the sovereign debt crisis in 2010-2012 (Greece, Italy, Portugal and Spain) did not face, *coeteris paribus*, an additional cost of financing on the Eurosystem bond market neither during the early lockdown phase nor afterwards.

The policy decisions taken now to address the pandemic crisis are likely to have lasting effects on the global economy and the way business activities are carried on. At the same time, the commitments to a CO2 reduction and the transition to a low carbon environment taken within the 2015 Paris agreements are still to be achieved. Thus, from many sources claims are voiced that a "green recovery" through sustainable energy investments could

Covid Economics 50, 25 September 2020: 33-73

Covid Economics 50, 25 September 2020: 33-73

help economies out of the crisis and give the most needed push to the transition to a sustainable economy (Bleischwitz 2020; IMF 2020a; Moore 2020). Given that green bonds are among the most suitable financial instruments to finance green projects, I test whether they enjoy a reduced cost of placement. It turns out that the 315 green bonds placed in the Eurosystem market since January 2019, do not show any additional (positive or negative) cost at issuance.

The remainder of the paper is organized as follows. Section 2 describes the CSPP and PEPP features. Section 3 deals with the construction of the dataset. Section 4 depicts the evolution of the funding cost on the primary bond market. Section 5 introduces the econometric approach. Section 6 discusses the empirical results about the effects of the ECB programmes. Section 7 deals with the additional features of the pricing mechanism. Section 8 draws the conclusions and derives the policy implications.

2 The Eurosystem corporate market

The ECB set the conditions for corporate eligibility under the first CSPP programme on April 2016 and since then they were just marginally updated. Thus, when on September 2019 a new wave of (corporate) purchases was announced, the criteria were already known in the financial markets. In addition, also the PEPP purchases follow the same conditions as the CSPP.[1]

The eligibility criteria are listed below and concern both the bond and the issuer:

- the bond must be eligible as collateral for Eurosystem credit operations;
- the bond must be denominated in euro;

[1]The most relevant change to the eligibility framework concerns the expansion of the purchases to non-financial commercial paper, which was announcend together with the PEPP on 18 March, 2020. For further details see the ECB press releases:
https://www.ecb.europa.eu/press/pr/date/2016/html/pr160421_1.en
https://www.ecb.europa.eu/press/pr/date/2020/html/ecb.pr200318_1~3949d6f266.en

Covid Economics 50, 25 September 2020: 33-73

- the bond must have a minimum first-best credit assessment of at least BBB- or equivalent (obtained from an external credit assessment institution);
- the bond must have a minimum (remaining) maturity of six months and a maximum (remaining) maturity of less than 31 years;[2]
- the issuer must be a corporation established in the euro area, defined as the location of incorporation of the issuer;
- the issuer must not be a credit institution nor have any parent undertaking which is a credit institution.

From the credit assessment criterion it emerges that the ECB relies on a slightly different definition of IG bonds with respect to the one used by market investors. Indeed, in the paper I label IG bonds all bonds which fulfill the ECB requirement of a first-best credit assessment of at least BBB-, even though this definition is not exactly matched by the financial investors' definition of IG bond, which usually requires the mean or the median rating to be at least BBB-. While it is argued that this difference made bonds between the two thresholds, or even just slightly below, to behave in the same way (Li et al. 2019), it also happened that after the first CSPP announcement, rating upgrades were mostly noticeable for bonds initially located below, but close to, the eligibility frontier (Abidi et al. 2019).[3]

In addition, from the joint working of the above-listed criteria, it turns out that not all IG bonds are eligible, regardless of the definition adopted. Provided that the other criteria are fulfilled, when an IG firm incorporated in

[2] After 18 March, 2020 the ECB can purchase marketable debt instruments that have an initial maturity of 365/366 days or less with a minimum remaining maturity of at least 28 days. The six-month minimum remaining maturity requirement continues to apply for marketable debt instruments with an initial maturity of at least 367 days.

[3] The bonds for which the two criteria do not coincide in the sample used are only 51 worldwide and 24 in the Eurosystem market. Including them in either the IG or HY segment does not change neither qualitatively nor quantitatively the results of the paper.

Covid Economics 50, 25 September 2020: 33-73

the euro area issues euro-denominated bonds they are eligible, but the same firm may well issue bonds in currencies other than euro, which are not eligible under the ECB programmes. For instance, the German company BMW AG issued in 2019 bonds in 8 different currencies, but only those denominated in euro were eligible for purchase. Another interesting case is that of IG extra-euro area companies which issue via a financial subsidiary incorporated in the euro area. The Swiss Zurich Insurance, for example, cannot issue eligible bonds neither in the euro area or elsewhere, but it may do so when the bond is placed via the subsidiary Zurich Finance DAC, which is incorporated in Ireland.[4] These examples suggest that the eligibility criteria are of utmost relevance in guiding the correct construction of the sample for the analysis of the effects of the ECB purchases, concerning both the eligible set of bonds and the control group.

In order to have access to the universe of issued bonds, I rely on one of the most used data provider as concerns the primary market: DCM Analytics by Dealogic. Not considering commercial paper and neglecting issuers in the industry groups of Government, Development Banks and Multilateral Agencies, Export Credit Agencies I have a universe of 15,581 bonds placed all over the world in the 17 months from 1 January, 2019 to 31 May, 2020 for which both the ISIN code and the yield at issuance are available. However, the ECB purchases the eligible bonds in a much smaller sub-set which includes only the 19 domestic euro-area markets and the generic European market. Thus, to frame this *ad hoc* market, I initially consider the first two letters of the ISIN code of each bond, which uniquely identify the market of issuance. For instance, all bonds with an ISIN code starting with "FR" or "DE" are placed in the domestic markets of France and Germany, respectively. At the

[4]Note that the country of nationality is the country in which the main company business is carried out. However, mostly for tax purposes, the place of official incorporation may be different. The Cayman Islands and Bermuda are the most frequent tax heavens for euro-area companies, while Ireland, Luxembourg and the Netherlands are the favourite euro-area countries of incorporation by companies of foreign nationality.

Covid Economics 50, 25 September 2020: 33-73

same time the broader common European bond market is identified by ISINs starting with "XS". The bonds selected according to this criterion amount to 4,494.

Since there are no nationality restrictions to issue in any of the above-mentioned markets, this implies that while they mainly include bonds issued by euro-area corporations, they also allow for foreign extra-euro area placements from both other European countries and the rest of the world. At the same time, corporations with a euro-area nationality can find it convenient to issue bonds in markets outside the euro area. Coming back to BMW AG example, over the period under analysis, the German car maker issued also in Canada, Switzerland and the US. Given that these bonds are a relevant alternative for euro-area corporations I include them in the overall sample. Note that in the latter case, the ISIN starts with a couple of letters different from the ones already selected (CA for Canada, CH for Switzerland, US for the United States...) and the bond cannot be purchased under the CSPP/PEPP rules. By adding the 539 placements in all other world markets by companies with a euro-area nationality/incorporation I have a final sample of 5,033 bonds, which I label the Eurosystem market.

As concerns the eligible placements, I rely on the proprietary data from the ECB: excluding commercial paper, in the period under analysis 540 new bond tranches fulfilling all the eligibility criteria were placed in the Eurosystem market.

Notwithstanding the restrictions used to construct it, the Eurosystem market is an open and international market. Looking at the parent level, there are 1,281 corporations - which issued through 1,590 issuers - placing at least one bond over the period January 2019 - May 2020, for a total of 2.3 trillions euro (Table 1). While around one third of them shows a euro-area nationality (423 parents for almost half of the bonds), they belong to 71 different countries. In particular, there are 264 issuers from China, 115 from the UK and 111 from the US. A similar picture applies to the total value placed: more than half of the total is due to euro-area corporations (52%), followed by the US, China and the UK (11.5%, 9.5% and 7%, respectively). Also important is the role played by Canada, Switzerland and the other EU countries which together account for another 7.6% of the total market size. As already explained above, it is not surprising to see that not all eligible bonds are placed by euro-area parents: there are 69 bonds issued through euro-area incorporated subsidiaries by parents whose nationality is not in a euro-area country (mainly US, UK and Switzerland).

Table 1. The Eurosystem market by country

	Parents	Issuers	Bonds	Eligible bonds	Value	Value %
Australia	19	26	71	0	21	0.9
Canada	13	15	67	0	47	2.1
China	264	323	646	0	214	9.5
euro area	423	549	2,485	471	1,175	52.0
Hong Kong	34	48	87	4	28	1.3
India	21	23	30	0	11	0.5
Japan	18	26	71	0	27	1.2
Norway	13	19	81	0	33	1.5
other EU	56	66	294	2	85	3.8
RoW	108	122	232	1	109	4.8
Russian Federation	24	28	49	0	22	1.0
South Korea	22	25	38	0	11	0.5
Switzerland	22	26	62	17	39	1.7
United Arab Emirates	18	18	50	2	17	0.8
United Kingdom	115	140	334	12	159	7.0
United States	111	136	436	31	261	11.5
Total	**1,281**	**1,590**	**5,033**	**540**	**2,260**	**100**

This Table presents some summary statistics of the Eurosystem bond market by country. Parents, Issuers, Bonds, Eligible Bonds are reported in units; Value is the amount placed in the market in billions euro. Value % is the percentage of the amount placed by each country. Sources: DCM Analytics, ECB.

In the next sections I describe the evolution over time of the Eurosystem market and how the amounts placed and the bond yields were affected by the outburst and diffusion of the Corona virus.

Covid Economics 50, 25 September 2020: 33-73

3 A disease among bonds

The Covid-19 pandemic has paralyzed the global economy in early 2020, when considering both each single domestic economy and the international trade flows. The rapid spread of the virus has required drastic measures to be taken by governments all over the globe, ranging from social distancing and the banning of public events to shutdowns, lockdowns and restrictions on most economic activities. These needed measures are however the driving factor behind the sharp decline in economic activity recorded in the first two quarters of 2020. It is now acknowledged that the pace of this contraction is faster and its magnitude greater than seen in the Great Recession (IMF 2020b).

Market reactions to news about the virus have been surprisingly quiet up to the last week of February 2020. Indeed, up to the 23th of February the implications of the virus spreading have been largely underestimated. There is a fast-growing body of research looking at the responses of stock markets to the Covid-19 pandemic, which in not conclusive about whether stock markets were able to incorporate all available information. Indeed, while they initially ignored the pandemic, stock markets strongly reacted from the 24th of February to the news of virus diffusion, closely following the additional news of the spreading of the virus. Up to then, country-specific characteristics appeared to have had little influence, if any, on stock market responses (Capelle-Blancard and Desroziers 2020). After the intervention of almost all central banks from mid March 2020, prices rebounded all around the world and in some instances markets even completely recovered in a few months the losses from the start of the year. Several studies suggest that stock markets were effective, in this second phase, in discounting the most vulnerable firms: those who were more financially fragile, subject to the disruption of international value chains, or less resilient to social distancing (Alburque et al. 2020, Baker et a. 2020, Ding et al. 2020, Fahlenbrach et al. 2020, Pagano et al. 2020, Ramelli and Wagner 2020).

Covid Economics 50, 25 September 2020: 33-73

To frame the development over time in the Eurosystem bond market described in the previous Section, I follow the chronology outlined above and link it to the ECB policy measures. In particular, I rely on five sub-periods: i) a "Calm period" from 1 January, 2019 to the announcement of the new wave of corporate purchases (12 September, 2019); ii) a "CSPP announcement" period from 13 September, 2019 up to the day before the actual implementation of the programme (31 October, 2019); iii) a "CSPP purchases" period of corporate purchases from 1 November, 2020 up to 23 February, 2020; iv) a period of "Covid lockdowns" which starts the day after the announcement by the Italian government of the first lockdowns in the Red zones in Northern Italy and when financial markets started to react (24 February, 2020), and ends when the ECB announced the PEPP, the new larger asset purchase programme related to the pandemic (18 March, 2020); v) a "PEPP period" from the day after the announcement of the programme to 31 May, 2020 (the last day of available data).

Table 2 Corporate bond issuance by periods

	Bonds	Bonds per week	Eligible bonds	Eligible %	Other IG %	HY %
Calm period	2,612	72	209	8.0	73.6	18.4
CSPP announcement	598	85	54	9.0	66.4	24.6
CSPP purchases	1,129	75	82	7.3	69.2	23.6
Covid lockdowns	101	32	11	10.9	77.2	11.9
PEPP	593	59	184	31.0	63.7	5.2
Total	**5,033**	**70**	**540**	**10.7**	**70.7**	**18.6**

This table shows the evolution of the primary bond placements by subperiods: Calm period (1/1/2019 - 12/9/2019); CSPP announcement (13/9/2019 - 31/10/2019); CSPP purchases (1/11/2019 - 23/2/2020); Covid lockdowns (24/2/2020 - 18/3/2020); PEPP (19/3/2020 - 31/5/2020). Bonds, Bonds per week and Eligible bonds are reported in units, Eligible%, Other IG% and HY% are reported in percentage points. Sources: DCM Analytics, ECB.

Table 2 reports the development over time of bond placements when di-

Covid Economics 50, 25 September 2020: 33-73

CEPR PRESS

viding the whole time frame into the suggested five sub-periods and taking into account the three bond segments of interest: eligible bonds, other IG bonds which are not eligible, HY bonds (which are never eligible). Not surprisingly, the evolution of bond issuance strongly reflect the changing market conditions: there is a large positive announcement effect of the CSPP, a dramatic drop of placements during the lockdown period, and a partial rebound in the last period, characterized by the announcement and deployment of the PEPP and by the introduction of additional extraordinary measures by the ECB itself, the European Union and the single governments.[5]

Instead, a maybe less expected and most evident change concerns the relative market composition, especially during the last two periods, not only when distinguishing between eligible and non-eligible bonds, but also when taking into account just the bond grade (IG vs HY). While decreasing in absolute terms, during the Covid lockdowns, there was a large increase in the share of IG placements (both eligible and other IG) with respect to HY bonds, which halved their share from 24% to 12%. In the final PEPP period, the share of IG bonds continued growing reaching 95%, with the eligible bonds increasing to almost one third of all placements and the other IG bonds recovering in terms of items placed (from 24 to 38 bonds per week). The number of HY bonds instead dropped even further to reach a historical minimum of three bonds placed per week (from an already low level of 4 per week in the previous period).

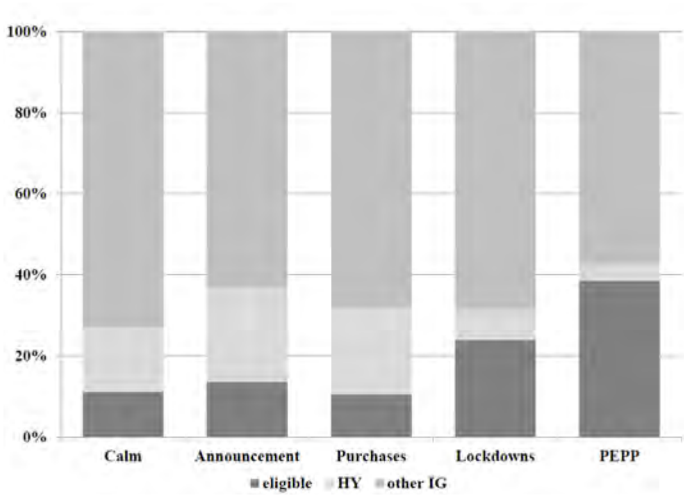
Even when looking at the total value placed (Figure 1), the drop in the market share of the HY segment is striking: after having increased to almost one fourth of total bond issuance after the CSPP announcement, most likely due to the market expectation of the triggering of the portfolio rebalancing channel, which led to a significant increase of HY issuance over the first wave of ECB corporate purchases (June 2016 - December 2018), it remained

[5] For an analysis of the policy measures taken to sustain banks' lending conditions after the pandemic outbreak in the euro area see Altavilla et al. (2020).

Covid Economics 50, 25 September 2020: 33-73

strong in the period of actual CSPP purchases, but in just three weeks it went down to 8% during the Covid lockdowns and to a mere 4% in the last period. Without any doubt, the segment most hit by the Corona virus is the HY segment.

Figure 1 Evolution of market shares by bond segments



This Figure depicts the percentage market shares from the total amount placed in the Eurosystem market of the three bond segments of IG eligible bonds, other IG non-eligible bonds and HY bonds over five consecutive time periods: Calm period (1 January, 2019 - 12 September, 2019); CSPP announcement (12 September, 2019 - 31 October, 2019), CSPP purchases (1 November, 2019 - 23 February, 2020); Covid lockdowns (24 February, 2020 - 18 March, 2020); PEPP (19 March, 2020 – 31 May, 2020). Sources: DCM Analytics and ECB.

While it is true that, regardless of the market grade, euro-area corporations in nearly all sectors faced sharp declines in revenue since the February lockdowns, with the vast majority of them witnessing also continuing costs of business, market investors were also aware that HY issuers – usually SMEs – could have faced stronger drains on the cash buffers, increased demand for credit and most likely increased missed payments or default on existing debt. This translated into the "flight to safety" to IG bonds recorded in the two

Covid Economics 50, 25 September 2020: 33-73

last periods under analysis.

Together with the reported significant changes in market shares, the Eurosystem market was characterized by large swings in bond prices. Since one aim of the paper is to assess the effectiveness of the ECB programmes in lowering and sheltering the cost of bond issuance for euro-area corporations, in the next Section I introduce the cost measure of bonds' placement and describe its evolution over time, highlighting the difficulties to be faced when dealing with the price of bonds in the primary market.

4 The cost of bonds at issuance

As the measure of the cost of financing in the primary bond market, I rely on the asset swap (ASW) spread, which is the difference between the bond yield and the yield of an asset swap contract of similar characteristics taken as the risk-free benchmark. In particular, an asset swap contract is a synthetic instrument which allows an investor to swap the payments on a bond (i.e. coupons) to a floating rate payments (risk free rate plus the ASW spread), while maintaining the original credit exposure to the fixed rate bond. In the euro area, it is supposed to perform better than the spread with respect to sovereign bonds, especially in periods of high volatility and when the flight to quality phenomenon pushes the yield of the (German) sovereign benchmark below the fundamentals (De Santis 2018).

Figure 2 depicts the average weekly values of the ASW spread for the bonds placed in the Eurosystem market. The graph is telling about the changing market conditions, which are broadly in line with the chronology of the pandemic crisis and fit fairly well the time partition into five subperiods adopted in the previous Section. For the HY and other IG bonds, the increase in the cost of placement starts during the Covid lockdowns, while for the eligible bonds the negative effect is postponed in the early weeks after the PEPP announcement. For the three segments it is clear that the cost

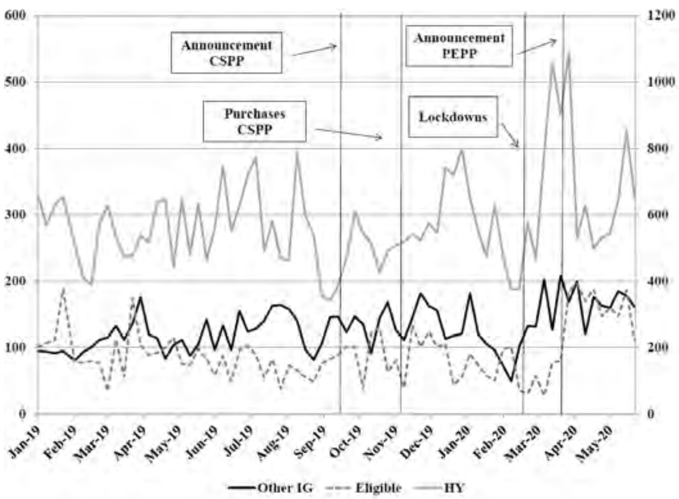
Covid Economics 50, 25 September 2020: 33-73

Covid Economics 50, 25 September 2020: 33-73

conditions reached in the last period, while lower that in the previous period, are still higher than before the pandemic crisis.

Another aspect which is immediately evident is the irregular development of the ASW spread over time, regardless of the market segment. This is due to the fact that in each week the bond issuance differs not only with respect to the amount placed but also with respect to the characteristics of the bonds and the issuers. Since the ASW spread strongly depends on the latter characteristics, it is not possible to compare even two consecutive weekly averages as they were in a *coeteris paribus* condition.[6]

Figure 2 ASW spread by market segments (basis points)



This Figure depicts the weekly average ASW spreads (in basis points) on bonds placed in the Eurosystem market for the three bond segments of IG eligible bonds, other IG non-eligible bonds and HY bonds. Sources: DCM Analytics and ECB.

Actually, the issuance on the primary bond market is not a continuous-

[6]This should be compared, for instance, with the much smoother development of secondary market price indexes (as the iBoxx index), which are instead constructed on bonds with the same characteristics and trading volumes.

Covid Economics 50, 25 September 2020: 33-73

time activity as the trading in the secondary market and the placement conditions are usually agreed upon well before the actual market issuance. Hence, it is not surprising that the effects of a monetary policy measures take more time to show in primary market volumes and prices than secondary trades (De Santis and Zaghini, 2019). For instance, even though showing a declining trend afterwards, the peak of the ASW spread is recorded for both the HY and the eligible segment not in the same week but in the week after the announcement of the PEPP. Thus, in order to correctly isolate the effect of the ECB programmes, in the next Section I propose a fully-fledged econometric approach that allow to assess whether they had an impact on the cost of bond placements and, if any, on which bonds.

5 The econometric approach

I develop the analysis of the impact of the CSPP and PEPP programmes on bond yields at issuance by building on the econometric approach proposed by Sironi (2003) and framed for the euro area by Zaghini (2019). The spread with respect to a risk-free asset is determined by the two main sources of risk of bond features and issuer characteristics:

$$spread_i = \alpha_0 + \sum_k \beta_k V_{i,k}^{bond} + \sum_l \beta_l V_{i,l}^{issuer} + FE_i + \varepsilon_i \tag{1}$$

where $spread_i$ is a generic yield spread of bond i with respect to a risk-free asset, V_k^{bond} are the K variables tracking the bond features, V_l^{issuer} are the L variables characterizing the issuing corporation and FE_i are the fixed effect by country and time, constructed by sets of dummy variables.

Relying on ASW spread as the yield spread measure and given that the announcement of the CSPP in September 2019 and that of the PEPP in March 2020 can be easily considered exogenous shocks, an extension of (1) to a difference-in-differences (DID) framework, in which the treated group is

Covid Economics 50, 25 September 2020: 33-73

the segment of eligible bonds, can be analytically implemented as:

$$ASW_i = \alpha_0 + \alpha_1 eligible_i + \delta_0 Post_i + \delta_1 Post_i * eligible_i + \\ + \sum_k \beta_k V_{i,k}^{bond} + \sum_l \beta_l V_{i,l}^{issuer} + FE_i + \varepsilon_i \tag{2}$$

where ASW_i is the ASW spread at origination on bond i, $eligible_i$ is a dummy which takes 1 if the bond is eligible and 0 otherwise, $Post_i$ is a time step dummy which takes 1 after the date of the relevant policy measure (CSPP announcement, CSPP start of purchases, PEPP announcement, PEPP start of purchases) and 0 otherwise.

However, the extremely volatile market conditions and the significant changes over time in the control sample may weigh on the DID approach, which is based on the rigid comparison of time periods and market segments. Thus a different approach is also envisaged.

The working hypothesis of equation (1) is that once the model is saturated by using a broad set of control variables and fixed effects to take into account all possible sources of systematic difference between bonds and issuers, the constant represents the overall market conditions. Since the aim of the exercise is to assess how the ECB monetary policy stance (i.e., the two corporate purchase programmes) has affected the market conditions and given that market conditions depend on several factors other than the monetary policy stance, I rely on control variables to clean as much as possible the constant and time dummies to shape it according to the chronology of the events. Therefore, I first introduce a broad set of market stress indicators, which help to clean the constant and isolate the monetary policy stance, then I rely on a set of time dummies appropriately tailored on the CSPP and PEPP chronology to assess their impact on the financing conditions.

Analytically:

$$ASW_i = \sum_j \alpha_j Time_{i,j} + \sum_j \delta_j eligible_i * Time_{i,j} + \sum_k \beta_k V_{i,k}^{bond} + \tag{3}$$

$$+ \sum_l \beta_l V_{i,l}^{issuer} + \sum_z \beta_z V_{i,z}^{market} + FE_i + \varepsilon_i$$

where ASW_i is again the ASW spread at origination on bond i, and V_z^{market} are the additional Z variables which take into account the financial stress at the time bond i was issued. Note that also the value of all other regressors is taken at time t (the exact issuance day). Therefore, for each bond i, the regressors' value is fixed at time t (the day of the bond placement), even if regressors take different values over time for the same issuer. Thus the model has a cross-section structure and the estimation procedure can be thought of as equivalent to a standard pooled OLS panel estimation in which the issuance date is just another characteristic of bond i and can be taken into account by a set of J time dummies ($Time_j$). Finally, in order to assess whether the effect of ECB purchases was constant over time and whether it did spill over to other than eligible bonds, I interact the variable tracking the eligible bonds with the time dummies. By looking, period by period, at the coefficient on this interaction it is possible to follow the evolution over time of the direct (additional) effect of the programme on the eligible segment.

Note, that a useful feature of the cross-section approach is that it allows a much larger selection of bonds and issuing institutions than a time series analysis. Indeed, many bonds, especially from smaller issuers, are not constantly priced and traded in the secondary market and thus can not be employed in a time series approach. Even when secondary market quotes exist, prices are most of the times not coupled with actual trades. By focusing on the primary market, I then avoid the market distortions due to the scarce liquidity of many euro-area bonds in secondary trades (Bao et al. 2011, Dick-Nielsen et al. 2012, Wang and Wu 2015).

Covid Economics 50, 25 September 2020: 33-73

Covid Economics 50, 25 September 2020: 33-73

I saturate the model using a broad set of controls and dummy variables to take into account all possible sources of systematic difference among bonds and issuers. The selection of the regressors is based on the traditional drivers of the risk premium.[7] In particular, as regards the bond features $\left(V_k^{bond}\right)$, the variables taken into account are: the time to maturity at origination, the amount issued (single tranche), the currency of denomination, the coupon frequency and the type of deal (fixed, floating or zero-coupon).[8]

The set V_l^{issuer} characterizing the issuer includes a measure of the creditworthiness of the corporation, the general industry sector and the business nationality.[9] As for the creditworthiness, I rely on the rating provided by the three most important rating agencies: Moody's, Fitch and Standard&Poors. Given the likely non linear relation between the probability of default and the rating, I use a set of dummy variables, one for each rating grade.[10]

In the set V_z^{market} of variables tracking the financial stress, there are three market indices at the daily frequency: the VSTOXX index, which is a measure of the equity market volatility in the euro area (computed relying on both call- and put-implied volatilities from the DJ Euro STOXX 50 index); the CISS bond index (Composite Indicator of Systemic Stress), which is

[7] The literature on the topic is abundant, the interested reader is referred to the seminal contributions by Elton et al. (2001), Collin-Dufresne et al. (2001), Campbell and Taksler (2003).

[8] Note that standard measures of bond-specific liquidity used when analysing secondary market spreads (e.g., the number of trades per day or the bid-ask spreads), cannot be used when dealing with the bonds issued on the primary market, since just the features concerning the originating trade are available.

[9] The 31 sectors are: Aerospace, Agribusiness, Alcoholic Beverages, Auto/Truck, Bank, Chemicals, Computers & Electronics, Construction/Building, Consumer Products, Defense, Dining & Lodging, Finance other, Food & Beverage, Forestry & Paper, Healthcare, Holding Companies, Insurance, Leisure & Recreation, Machinery, Metal & Steel, Mining, Oil & Gas, Professional Services, Publishing, Real Estate/Property, Retail, Telecommunications, Textile, Tobacco, Transportation, Utility & Energy.

[10] The rating of the issuer is first linearized between 1 (CC/Ca) and 20 (AAA/Aaa), so that when the same bond receives more than one assessment from Moody's, Fitch and Standard&Poors they can be averaged. Then the average is transformed into a set of dummy variables. I also add a dummy tracking the firms whose rating is not available at all.

CEPR PRESS

Covid Economics 50, 25 September 2020: 33-73

the systemic stress indicator for the euro-area financial markets proposed by Hollo et al. (2012); the iTraxx Europe index (the average of 125 equally-weighted single-name European CDS spreads), which should capture market-wide variation in CDS spreads due to changes in fundamental credit risk, liquidity, and CDS market-specific shock (Acharya et al., 2014). In addition, also at the daily frequency, I include the index of macro news for the US and the euro-area provided by Citi, the index of economic policy uncertainty (EPU) by Baker et al. (2016) for the US and the UK, the nominal effective exchange rate of the euro computed by the ECB with respect to the 19 main trading partners of the euro area.

Finally, to deal with possible idiosyncratic shocks affecting the different currencies in which the bonds are issued (17 in the Eurosystem market), I use the interaction of currency and quarter dummies. Instead, to take into account idiosyncratic shocks affecting countries and sectors, I rely on the interaction of sector and country dummies.

6 The effects of the ECB purchases

A set of DID regressions as in (2) is run over different time horizons, each characterized by a policy measure. In particular, I start by referring to the CSPP announcement period (13 September, 2019 to 31 October, 2019) as the treatment period for eligible bonds, then, step by step, I enlarge the treatment period to take into account also the four months of CSPP purchases (up to 23 February, 2020), the PEPP period divided into announcement (up to 27 March, 2020) and purchases (up to 31 May, 2020). The starting day of each sample is 1 January, 2019. In addition, an *ad hoc* sample is made on the PEPP alone: the treatment period starts with the PEPP announcement and ends on 31 May, 2019, thus the control period includes also the CSPP.

The top panel of Table 3 reports the coefficients of a basic DID regression without any time or market controls in addition to the $Post_i$ dummy. While

Covid Economics 50, 25 September 2020: 33-73

the coefficient on the treated group suggests significantly better placement conditions for the eligible bonds segment in the range of 32-37 basis points, the coefficient on the interaction term is never significantly different from zero. Thus, independently on how large we consider the period of intervention on the corporate market by the ECB, the effect on the treated group is not different from the rest of the bond market. This is true also when considering just the PEPP programme alone.

Table 3 DID regressions with expanding treatment periods

	CSPP annoucement	CSPP purchases	PEPP annoucement	PEPP purchases	PEPP alone
			No time controls		
Eligible	-37.3074 ***	-31.9823 ***	-33.1325 ***	-35.3689 ***	-38.0881 ***
	(12.8421)	(11.8215)	(11.8912)	(10.9614)	(10.2496)
Post policy measure	2.9066	3.1768	1.0382	1.0274	91.7344 ***
	(12.9660)	(12.5209)	(12.4537)	(12.0676)	(18.6289)
Eligible * Post policy measure	-10.1610	-8.9940	12.4942	17.5044	20.7542
	(17.2969)	(11.8269)	(12.4521)	(10.4913)	(13.5370)
			Market stress controls		
Eligible	-37.1995 ***	-31.4685 ***	-32.2773 ***	-34.4198 ***	-37.8565 ***
	(12.9506)	(11.8137)	(11.6992)	(10.7913)	(10.2320)
Post policy measure	14.4016	7.5375	13.8970	13.9018	47.2332
	(17.3553)	(16.1396)	(15.7995)	(14.0675)	(34.0735)
Eligible * Post policy measure	-10.9057	-9.5302	-4.9538	3.4526	19.994
	(17.2747)	(11.7591)	(11.1385)	(9.6367)	(13.7867)
			Weekly time dummies		
Eligible	-37.0507 ***	-31.2237 ***	-32.0642 ***	-35.5666 ***	-38.1781 ***
	(12.7328)	(11.6313)	(11.5746)	(9.1263)	(10.3315)
Eligible * Post policy measure	-9.2473	-8.2534	-4.0988	3.8268	18.7114
	(18.6218)	(12.4246)	(11.7441)	(9.9573)	(14.0040)
Bond and Issuer controls	YES	YES	YES	YES	YES
Currency*Time dummies	YES	YES	YES	YES	YES
Sector*Country dummies	YES	YES	YES	YES	YES
No. observations	3,210	4,339	4,499	5,033	5,033

This Table reports the estimated coefficients from DID regressions as of equation (2) with different (expanding) treatment periods. The dependent variable is the ASW spread. Robust standar errors (in parentheses) are clustered by time and issuer. From column 1 to column 4, Post policy measure is a dummy which takes 1 from 13 September, 2019 and 0 before, and ends on: 31 October, 2019 (column 1); 23 February, 2020 (column 2); 27 March, 2020 (column 3); 31 May, 2020 (column 4). In column 5 it takes 1 from 18 March, 2020 to 31 May, 2020 and 0 otherwise. The top panel shows the results when no time controls are included, the middle panel when daily market controls are added, the lower panel when weekly time dummies are added.

Covid Economics 50, 25 September 2020: 33-73

The evidence reported for the basic DID regressions is confirmed when introducing control variables which should help distentangling the effect of the ECB from the changing market conditions. Both the middle panel (in which the daily market stress indicators are introduced) and the lower panel of Table 3 (in which weekly time dummies are introduced) show that the coefficient on the interaction term is never significantly different from zero, even when considering the PEPP programme alone. At the same time it is also confirmed a structural advantage of the eligible bonds segment in the range of 31-38 basis points.

As already mentioned, the strong heterogeneity in markets conditions and the large changes in the control sample may make less effective the DID approach over the selected horizon. Thus I also employ the approach described by equation (3), which allows more flexibility and *ad hoc* time partitions.

Table 4 reports in the top panel the estimated α_j coefficients showing the evolution of the market conditions for the segment of non-eligible bonds. For the ease of interpretation the coefficient concerning the calm period before the CSPP is left out. In this way the remaining α_j coefficients show the change with respect to that initial period. Focusing on the first column, it is clear that neither the CSPP announcement nor the relatively long period of CSPP purchases changed the market conditions for the bonds non eligible to the programme: the estimated coefficients are not significantly different from zero. Instead, the three weeks of early lockdowns brought about a sudden worsening in the placement conditions of 51 basis points, which is also economically relevant given that the unconditional mean of the ASW spread up to then was 104 basis points. Then, the conditions further deteriorated up to 88 basis points over the last period characterized by the PEPP announcement and deployment, suggesting that the financing conditions in the bond market did not return to pre-Covid levels, at least for the segment of non-eligible bonds.

CEPR PRESS

Covid Economics 50, 25 September 2020: 33-73

Table 4 Funding conditions by market segments over time

	Eurosystem	IG	HY	Within-sample	Euro area	Full Sample
			Time dummies			
Before CSPP	-	-	-	-	-	-
CSPP Announcement	8.6827	-0.6155	-27.9643	-5.9968	4.4453	11.8464
	(14.8278)	(9.4865)	(45.0384)	(12.7412)	(9.8483)	(15.4699)
CSPP Purchases	0.7051	7.6973	-18.9453	-7.7105	-12.5523	7.1026
	(20.8524)	(13.0567)	(60.6006)	(14.4567)	(15.9197)	(20.1536)
Lockdowns	51.0090 *	32.7781 *	204.330 ***	35.333 *	38.615 **	58.187 ***
	(30.4278)	(18.9191)	(76.1315)	(19.2371)	(18.2413)	(27.7427)
PEPP	88.560 **	54.773 *	253.907 *	64.131 *	67.752 **	74.303 ***
	(28.6115)	(29.2759)	(136.3086)	(37.2563)	(34.0831)	(43.9767)
			Time dummies * eligible dummy			
Before CSPP	-34.8474 ***	-24.9812 ***	-300.0121 ***	-20.7396 **	-36.7159 ***	-36.4820 ***
	(10.7901)	(7.6347)	(54.7626)	(9.2560)	(11.6518)	(10.4201)
CSPP Announcement	-49.2711 ***	-23.6884 *	-279.7134 ***	-34.4415	-51.4715 ***	-42.9733 **
	(17.0261)	(14.3966)	(57.1777)	(28.3854)	(17.7316)	(17.0772)
CSPP Purchases	-39.3293 ***	-33.0614 ***	-258.5415 ***	-11.8015	-37.2862 ***	-39.7651 ***
	(14.5120)	(9.4943)	(57.3654)	(15.0052)	(15.4796)	(12.8164)
Lockdowns	-73.8511 ***	-59.4791 ***	-428.7544 ***	-62.1189 ***	-80.1138 ***	-70.2426 ***
	(21.3213)	(17.1419)	(80.3393)	(17.6157)	(22.1263)	(16.5381)
PEPP	-18.7177	-0.3173	-383.6322 ***	11.6822	-21.6967	-12.0678
	(14.3060)	(11.3287)	(82.1507)	(16.5514)	(15.0192)	(13.0764)
Bond and Issuer controls	YES	YES	YES	YES	YES	YES
Financial stress controls	YES	YES	YES	YES	YES	YES
Currency*Time dummies	YES	YES	YES	YES	YES	YES
Sector*Country dummies	YES	YES	YES	YES	YES	YES
Issuer dummies	NO	NO	NO	YES	NO	NO
No. observations	5,033	4,096	1,477	861	4,494	15,573
R^2	0.777	0.720	0.834	0.849	0.776	0.671

This Table reports the estimated coefficients α_j (top panel) and δ_j (lower panel) from regressions as of (3) with different samples. The dependent variable is the ASW spread. Robust standard errors (in parentheses) are clustered by issuer and time. In column (1) the sample is made by the Eurosystem market; in column (2) by IG bonds only (both eligible and non-eligible); in column (3) by eligible bonds and HY bonds (thus, it does not include IG non-eligible bonds); in column (4) by bonds placed by corporations which issued both eligible and non-eligible bonds; in column (5) by bonds placed in the international euro-area market and the 19 domestic markets; in column (6) by all placements available around the world.

How did eligible bonds comparatively cope is instead shown in the lower panel of Table 4, which reports the estimated δ_j coefficients. They measure the additional cost in term of placing conditions faced by eligible bonds in each period. A first circumstance that emerges is that even before the CSPP announcement, eligible bonds benefited from a discount on the spread at issuance of 35 basis points. This is most likely due to the fact that eligible

Covid Economics 50, 25 September 2020: 33-73

bonds have, *coeteris paribus*, the additional property of always being eligible as collateral in the ECB refinancing operations. The lower spread increased after the announcement of the new wave of ECB purchases, remained strong over the period of actual corporate purchases and peaked to 74 basis points during the weeks of early lockdowns, when the presence of a constant buyer as the ECB was most beneficial for this market segment. Somewhat surprisingly, the spread turned non statistically significant in the last PEPP period.

In order to further investigate the latter circumstance, Table 4 shows also the estimates when two different control samples are employed: IG and HY bonds only, respectively. Indeed, the evidence reported in Section 3 (Table 2 and Figure 1) suggests that the composition of the bond market changed a lot both in absolute and relative terms, this in turn may influence the estimations based on the full sample.

As expected, the deterioration in the financing conditions witnessed by the segment of IG bonds is much smaller than the whole sample (column 2, top panel). During the early lockdowns the ASW spread increased by 32 basis points in the segment of IG non-eligible bonds and further deteriorated to 55 basis points in the last period. However, the behavior of the market conditions for the segment of eligible bonds is not dissimilar from when employing the whole sample (column 2, lower panel). Before the Covid pandemic they enjoyed a smaller ASW spread in the range of 23-33 basis points, which peaked to almost 60 basis points during the early lockdowns and then disappeared.

Column (3) in Table 4 shows instead the (somewhat unfair) comparison of the changes in financing conditions for the set of HY bonds and eligible bonds (which are all IG bonds). The estimated deterioration with respect to the calm period is flabbergasting: during the Covid pandemic and the following PEPP period the ASW spread of HY bonds increased, *coeteris paribus*, by 204 and 254 basis points, respectively. At the same time the set of eligible bonds benefited from a better environment of around 400 basis points.

All in all, the estimations in the first three columns of Table 4 suggest that during the most critical phase of the Covid spreading the ECB purchases (under the already existing CSPP programme) were effective in shielding the eligible bond segment from a stronger deterioration in the financing conditions. That happened in a context of diminishing placements, especially by HY issuers, hinting, in turn, to a strong preference for eligible bonds even within the IG segment. Then, in the last period, characterized in addition to the PEPP by a set of policy interventions of unprecedented magnitude by the EU and the single governments, and by a rebound in the issuance activity (mainly from IG corporations), the peculiarity of a negative premium on the yield of eligible bonds vanished (it was maintained only with respect to the few HY bonds placed). This circumstance is even more surprising given the increased market segmentation by which the share of the eligible bonds grew to close to 40% of the total market value. A possible interpretation goes through the working of the portfolio rebalancing channel within the IG segment. Given that under the PEPP purchases a large share of the market was "engaged" by the ECB demand, investors started purchasing other (similar) bonds within the same IG segment, generating an endogenous increase in demand for non-eligible bonds, which was able to offset the price advantage of eligible placements.

Unfortunately, given the increased market uncertainty and volatility, the rebalancing did not extend to the HY segment, in which bond placements almost disappeared. The bond market collapsed to IG bonds only, strongly affecting the ability of many corporations to issue debt on the market. From this unexpected change in the market composition stems a relevant policy implication: once a corporation is downgraded to HY, it has almost no possibility of placing a bond, since it loses the eligibility to ECB programmes and the other investors are focused on "safe" IG bonds only.

In a further exercise I take a within-sample perspective and rely on the set of corporations which could, and actually did, issue both eligible and non

Covid Economics 50, 25 September 2020: 33-73

eligible bonds. In this way the bonds in the two samples are all issued by the same set of companies and thus are subject to the same underlying default risk. At the parent level there are 86 such issuers, which placed 861 bonds, 309 of which were eligible, 243 non eligible but placed in euro-area markets, and 307 non eligible placed in foreign markets. They are large international corporations (25 of them from extra-euro area countries), which placed bonds over the period under analysis via 225 different issuers.

From column (4) it turns out that while the R-squared of 0.85 suggests a good fit of the model, the relatively limited number of observations results in larger standard errors. However, the main results remain: i) the market worsening started in the early lockdowns period but further deteriorated up to May 2020; ii) eligible bonds enjoyed a large spread reduction in the lockdowns period, which vanished afterwards.

In addition, by looking at the amount placed, it emerges that the 86 corporations under analysis first increased the bond financing via eligible bonds from a share of 33% before the ECB corporate purchases to 48% during the CSPP programme, but then significantly diminished it to 31% over the lockdown period, in a way missing the sheltering provided by the ECB steady demand. Eventually, they strongly returned to the eligible segment by placing up to 56% of their total issuance in the last PEPP period.

Columns (5) and (6) of Table 4 provide some robustness checks of the results. In column (5) the sample employed does not include the 539 bonds, which still being part of the Eurosystem market, were placed outside the euro-area international market and the 19 domestic markets, while in column (6) the whole set of available bonds placed around the world is used. While both sets of estimates are in line with those stemming from the Eurosystem market, an additional indication comes from column (6): the further deterioration recorded in the financing conditions in the period up to May 2020 is a worldwide phenomenon. Thus, over the two months and a half from 19 March, 2020 to 31 May, 2020, which followed the three most critical weeks in which most of the anti-virus measures were taken (24 February, 2020 - 18 March, 2020), the bond market was not able to recover to the pre-crisis conditions, even at the global level.

7 Features of the pricing mechanism

While in the previous Section the empirical evidence suggests a market segmentation along the bond grade and the eligibility to the ECB programmes, in what follow I investigate whether other additional characteristics at the firm and bond level have been taken into account by market participants in the wake of the pandemic outburst. In particular, I test three (mutually non exclusive) hypotheses about the bond pricing: 1) that the Pagano et al. (2020) pre-disaster learning model of US stock returns applies also to the Eurosystem bond market; 2) that corporations from countries with less fiscal room of manoeuvre are penalized when issuing bonds; 2) that a "green recovery" has taken place and it is financed via green bonds.

The empirical approach I use is again via regression (3). However, instead of using the CSPP eligibility as the distinguishing characteristic, I create *ad hoc* dummy variables which take 1 when bond i (or the corporation issuing bond i) shows the characteristic under investigation and 0 otherwise. For instance, in column (1) of Table 5 the distinguishing characteristic is that the issuing corporation is a non-financial corporation (NFC). As in Table 4, the top panel shows the changes in the financing conditions for the "control" group, which now is the set of bonds not showing the selected characteristic, while the lower panel reports the additional effect of the distinguishing characteristic. From column (1), it turns out that being a NFC did not imply any additional spread on the yield at issuance up to the last period, when instead an additional cost of 34 basis points is estimated.

Covid Economics 50, 25 September 2020: 33-73

Covid Economics 50, 25 September 2020: 33-73

Table 5 Sources of risk in the bond pricing

	NFCs	Vulnerable firms	GIPS	Green bonds
		Time dummies		
CSPP Announcement	7.8502	8.3752	8.1701	8.0476
	(13.5561)	(13.1579)	(14.6418)	(13.1535)
CSPP Purchases	-0.7314	2.3554	3.9495	-0.5975
	(20.7992)	(15.1551)	(20.7595)	(15.5634)
Lockdowns	40.051 *	45.869 *	45.654 *	44.520 *
	(22.2955)	(24.1443)	(29.6956)	(23.9318)
PEPP	77.060 **	86.248 **	85.066 **	87.833 **
	(40.6245)	(42.5309)	(41.8490)	(41.8579)
		Time dummies * factor dummy		
CSPP Announcement	3.5918	11.755	-28.204	-15.631
	(15.3459)	(25.3017)	(26.1886)	(17.1817)
CSPP Purchases	1.4389	0.6803	-56.238 ***	13.198
	(13.9599)	(39.7786)	(19.3115)	(19.2812)
Lockdowns	16.428	10.327	-454.74	10.330
	(44.0507)	(30.2085)	(380.8605)	(22.6239)
PEPP	33.737 ***	31.240 **	-14.163	6.3449
	(12.9728)	(14.4998)	(22.2659)	(17.5704)
Bond and Issuer controls	YES	YES	YES	YES
Financial stress controls	YES	YES	YES	YES
Currency*Time dummies	YES	YES	YES	YES
Sector*Country dummies	YES	YES	YES	YES
No. observations	5,033	5,033	5,033	5,033
R^2	0.777	0.776	0.777	0.776

This Table reports the estimated coefficients α_j (top panel) and δ_j (lower panel) from regressions as of (3) with different factor dummies which take 1 when the factor is positively held by bond$_i$ and 0 otherwise. The dependent variable is the ASW spread. Robust standard errors (in parentheses) are clustered by issuer and time. In column (1) the factor dummy is made by NFCs; in column (2) by NFCs more sensible to social distancing according to Koren and Peto (2020) measure; in column (3) by corporations from countries with less fiscal space (Greece, Italy, Portugal, Spain); in column (4) by green bonds.

While NFCs faced a stronger increase than banks and other non-financial corporations in the cost of issuance in the PEPP period, were all NFCs treated in the same way, or was the market able to distinguish those more affected by the measure taken to tackle the Covid pandemic? In order to answer the latter question I rely on a measure of pandemic resilience proposed by Koren and Pető (2020) and used in Pagano et al. (2020) for their assessment of the US stock returns. Indeed, to measure the consequences of

Covid Economics 50, 25 September 2020: 33-73

social distancing on firms, recent research in labor economics has developed several indicators of the extent to which jobs can be done from home or rely on face-to-face interaction and physical proximity. Among them, Koren and Pető (2020) construct three types of industry-level measures of human interactions, depending on whether these are due to internal communication (teamwork), external communication (customers), or physical proximity to others (presence). In particular, they also provide an aggregate measure of "communication" intensity and construct an industry-level measure of the percentage of employees affected by social distancing regulations due to their occupations being communication-intensive and/or requiring close physical proximity to others. Based on the latter measure, I construct a dummy which takes 1 for the firms more affected by social distancing (top tercile) and 0 otherwise.

Column (2) in Table 5 shows again that the δ_j coefficient concerning the last period is positive and statistically significant. In addition, the magnitude is very similar to column (1) suggesting in turn that the whole effect on NFCs is due to firms sensitive to social distancing. The market was thus able to distinguish among firms in the last period, penalizing those which, by business model, were most affected by the pandemic and the measures taken to limit its spreading. However, this evidence does not fully match the findings and the interpretation suggested by Pagano et al. (2020) for the US stock market: in the Eurosystem bond market there is no evidence of a pandemic awareness before the Covid crisis, since all the δ_j coefficients before the last PEPP period are not statistically significant, including the Covid lockdown period itself. It thus seems that not only the Eurosystem market is characterized by the "unpriced–disaster risk model" (using Pagano et al. (2020) terminology), but also that in the first phase of price adjustment (the Covid lockdown period) all corporations were treated in the same way, confirming the finding by Capelle-Blancard and Desroziers (2020) of a (somewhat puzzling) stock market behavior that did not match the corporations'

Covid Economics 50, 25 September 2020: 33-73

fundamentals.

An additional source of concern which emerged in the aftermath of the first containment measures against the Covid spreading is due to the unfortunate circumstance that, at least at the beginning, the countries most exposed to the pandemic were those with less fiscal space (Italy and Spain, in particular). Indeed, a higher prevalence of the pandemic is expected to have more adverse effects on the economy and hence trigger a stronger policy response, provided that sufficient budgetary room is available (Alberola et al. 2020). This in turn implies that countries with reduced fiscal space might not be able to implement adequate measures aimed at preventing that the temporary pandemic disruptions could inflict a permanent damage to the economy.[11]

In column (3) in Table 5 I test the hypothesis that the corporations from the countries with less fiscal space faced a higher financing cost after the Covid spreading due to an expected higher default rate linked to the lack of resources from their governments. In particular, I refer to the four euro-area countries which were most involved in the sovereign debt crisis in 2010-2012: Greece, Italy, Portugal and Spain (GIPS).[12] It turns out that in both the Covid period and the PEPP period – the time span in which the market started to asses the pandemic risk on corporations – the additional δ_j coefficients are not statistically significant.[13] It thus seems that the possibility of a different involvement by euro-area government was not taken into account

[11] Note that the Recovery Fund proposal by the two French and German governments to create a fund at the EU level to deal with the recovery in the countries most hit by the pandemic is dated only 18 May, 2020, within the sample but almost at the end of it.

[12] I do not include Ireland in the group of countries with less fiscal space, notwithstanding it was involved in the sovereign debt crisis, for two main reasons. First, the fiscal outlook has significantly improved from the 2010-2012 period; second, Ireland is the country of incorporation of many foreign financial subsidiaries, thus making difficult disentangling the domestic risk from the foreign risk.

[13] While non statistically significant, the very large and negative coefficient estimated over the lockdowns period is due to the extremely small number of placements in that period: only two bonds were issued by corporations from the GIPS group.

Covid Economics 50, 25 September 2020: 33-73

by the bond market. Instead, it emerges that GIPS countries benefited a lot from the increased and stable ECB demand over the months of CSPP purchases (56 basis points).

The third possible source of differences among firms and bonds that I investigate is related to the idea that a "green recovery" through sustainable energy investments could help governments out of the crisis and toward a low carbon environment (IEA 2020, NGFS 2020). From the one hand, the policy steps needed to come out of the crisis are likely to have lasting effects on the global economy and shape societies for decades to come; from the other hand, the commitments to a CO2 reduction and the transition to sustainable economy taken within the 2015 Paris agreements are still to be achieved. One way to finance sustainable energy investment is via green bonds. Green bonds are debt instruments, whose proceeds are committed to the financing of low-carbon, climate-friendly projects. In addition, they are a very good candidate to satisfy the appetite of investor attending to environmental concerns. Indeed, a rapidly increasing number of investors are taking into account climate change in their investment decisions, with survey and anecdotal evidence suggesting that also non-pecuniary motives, specifically pro-environmental preferences, may motivate the holding of green assets (Bolton and Kacperczyk 2020, Krueger et al. 2020). The empirical evidence gathered so far suggests that the pricing of green bonds include a (negative) premium for some categories of issuers. For instance, Zerbib (2019) finds that for the whole group of green bond issuers the premium is significantly different from zero, even though very limited in magnitude (around 2 basis point). Fatica et al. (2019) suggest instead that non-financial corporations and especially supranational institutions benefit of a much larger yield spread (22 and 80 basis point, respectively). At the same time they find that financial corporations do not enjoy any negative yield differential. Flammer (2019) reports that also stock markets seem to respond positively to the announcement of green bond issuance, and documents a significant increase in firms' environmental performance afterwards, suggesting that green bonds are effective in improving companies' environmental footprint.

In the Eurosystem market 315 green bonds were placed over the time horizon under analysis, 49 of which eligible for CSPP and PEPP purchase. They are taken into account by a dummy variable and results from regression (3) are reported in Table 5, column (4). The evidence is clearly against a different treatment in the pricing of green bonds: all the δ_j coefficients are not statistically significant. This in turn suggests that euro-area investors were not moved by environmental concerns before the Covid pandemic, and that a price advantage in favour of green projects related to a "green recovery" is still absent in the bond market.

8 Conclusions

The empirical evidence provided in the paper shows that not only the effects of the Covid pandemic in the euro-area bond market were different across bonds and firms, but also that the market composition was significantly affected by the Corona virus. In particular, after the first weeks of early lockdowns (late February to mid March 2020) the market collapsed to investment-grade bonds only, a segment in which the share of bonds eligible to the ECB corporate programmes (CSPP and PEPP) has strikingly increased to 40% from a mere 15% before the crisis. At the same time the share of high-yield bonds shrunk to almost disappear at 4%.

As for the financing cost, as happened to the stock markets around the world, also the euro-area bond market was not much affected by the news about the Corona virus diffusion up to last week of February 2020, when the first severe lockdown measures were taken in Europe. The cost at issuance suddenly increased in all market segments and regardless of the business sector of the issuing corporation. However, it is estimated that the bonds eligible to the ECB corporate programme (CSPP) benefited from a more muted

Covid Economics 50, 25 September 2020: 33-73

impact of around 40 basis points. This advantage instead disappeared in the period starting from mid-March 2020, characterized by the ECB launch of a extraordinary purchasing programme (PEEP) and policy measures of unprecedented magnitude by the domestic governments and the European Union. This evidence can be explained by two circumstances: i) the change in the market composition; ii) the working of the portfolio rebalancing channel. Concerning the former, the flight-to-safety phenomenon moved financial agents away from the more risky assets (HY bonds) and towards the safer IG segment, thus making the bond market more homogeneous (also all eligible bonds are all IG). Regarding the latter, after the starting of the purchases under the PEPP, a large share of the market became unavailable because of the ECB demand, thus investors rebalanced their portfolio towards similar assets: IG bonds which were non eligible to the ECB purchases. This in turn generated an endogenous surge in the demand for non-eligible bonds which, increasing the bond price, reduced the cost at issuance and offsetted the difference with respect to eligible bonds.

While from the perspective of IG corporations the ECB intervention can be considered effective in protecting their bonds from the sudden deterioration in price conditions, the expected second-round effect through the portfolio rebalancing channel did not materialize for the HY bonds for which the cost at issuance has skyrocketed after the Covid spreading. The difficulties in financing on the bond market may further increase in the euro area since once downgraded to HY, a issuer has almost no possibility of placing a bond, since neither the ECB (due to the loss of the eligibility status) nor other investors are willing to purchase. Indeed, credit rating agencies (CRA), as the pandemic diffusion aggravated, started to downgrade euro-area corporations at a fast pace pushing several bonds in the junk segment (HY). These developments have been strongly affecting the ability of corporations to issue (more) debt on the market.

A possible measure to withstand the unexpected market composition change would be to introduce a waiver of the minimum credit quality requirement for bonds placed by euro-area non-financial corporations that have suffered a rating downgrade into the HY segment (the so called "fallen angels") in the wake of the Covid pandemic. The waiver could replicate the pre-crisis frozen rating or "grandfathering" already applied by the ECB for the general collateral eligibility on 22 April, 2020. Such a measure would not even be new for a corporate bond market, since already on April 9, 2020, the FED announced the purpose to purchase HY corporate debt by fallen angels as part of a larger rescue package (PMCCF and SMCCF- Primary and Secondary Market Corporate Credit Facility, respectively) for businesses and municipalities hit hardest by the crisis (Gilchrist et al. 2020). In order to be eligible, corporate securities must have been rated at least BBB- by two or more CRAs as of March 22, 2020. Moreover, while the new rating will be in the HY segment, it is expected not to be too far from the IG threshold at the time of issuance (at least BB-).

Another source of risk detected in the bond pricing mechanism is the weak resilience to pandemic crises, where resilience is defined as the reliance on business models based on technologies and organizational structures that are robust to social distancing (those, for instance, that can adapt extensively to teleworking or do not require close contact with customers). Relying on an indicator proposed by Koren and Pető (2020), I find that the premium requested on bonds issued by non-resilient corporations (around 30 basis points) started to be statistically significant only in the period after the first wave of actions taken by the national authorities to withstand the virus spreading. This in turn suggests that before the spreading of the Corona virus, the market was not taking into account the possibility of a pandemic nor the possible negative consequences of the measures to be implemented in the wake of a pandemic crisis (unpriced–disaster risk model).

On the contrary, I do not find evidence supporting the pricing of an increased risk for corporations headquartered in countries with a reduced fiscal

CEPR PRESS

space, nor the existence of a premium in favour of green bonds, i.e. those bonds that are placed with the aim to finance sustainable energy projects, which should be the backbone of a possible "green recovery" out of the crisis.

Covid Economics 50, 25 September 2020: 33-73

Covid Economics 50, 25 September 2020: 33-73

References

[1] Abidi, N., Falagiarda M. and Miquel-Flores I. (2019), "Credit rating dynamics: evidence from a natural experiment", ECB Working Paper No. 2274.

[2] Acharya, V., Drechsler I. and Schnabl P. (2014), "A Pyrrhic Victory? Bank Bailouts and Sovereign Credit Risk", Journal of Finance, Vol.69, No. 6, pp.2689–2739.

[3] Alberola, E., Arslan Y., Cheng G. and Moessner R. (2020), "The fiscal response to the Covid-19 crisis in advanced and emerging market economies", BIS Bulletin No.23.

[4] Albuquerque, R. A., Koskinen Y., Yang S. and Zhang C. (2020), "Resiliency of environmental and social stocks: an analysis of the exogenous COVID-19 market crash", The Review of Corporate Finance Studies, forthcoming.

[5] Altavilla, C.,Barbiero F., Boucinha M. and Burlon L. (2020), "The great lockdown: pandemic response policies and bank lending conditions", ECB Working Paper No. 2465.

[6] Baker, S R, Bloom N., Davis S.J., Kost K., Sammon M. and Viratyosin T. (2020), "The unprecedented stock market reaction to COVID-19", Covid Economics: Vetted and Real-Time Papers 1, CEPR.

[7] Bao, J., Pan J. and Wang J. (2011), "The illiquidity of corporate bonds". Journal of Finance, Vol.66, pp. 911-946.

[8] Bolton, P. and Kacperczyk M. (2020), "Do Investors Care about Carbon Risk?", NBER Working Papers No. 26968.

[9] Campbell J.Y. and Taksler G.B. (2003), "Equity Volatility and Corporate Bond Yields", Journal of Finance, Vol.58, pp. 2321-2350.

[10] Capelle-Blancard, G. and A Desroziers A. (2020), "The stock market is not the economy? Insights from the COVID-19 crisis", Covid Economics: Vetted and Real-Time Papers, CEPR.

[11] Collin-Dufresne, P., Goldstein R.S. and Spencer Martin J. (2001), "The Determinants of Credit Spread Changes ", Journal of Finance, Vol.56, pp. 2177-2207.

[12] De Santis, R.A. (2018), "Unobservable country bond premia and fragmentation", Journal of International Money and Finance, Vol. 82, pp. 1-25.

[13] De Santis, R.A. and Zaghini A. (2019), "Unconventional monetary policy and corporate bond issuance", ECB Working Paper No. 2329.

[14] Dick-Nielsen, J., Feldhutter P. and Lando D. (2012), "Corporate bond liquidity before and after the onset of the subprime crisis", Journal of Financial Economics, Vol.103, pp. 471-492.

[15] Ding, W, Levine R., Lin C. and Xie W.(2020), "Corporate Immunity to the COVID-19 Pandemic", NBER Working Paper No. 27055.

[16] Elton, E., Gruber M., Agrawal D. and Mann C. (2001), "Explaining the rate spread on corporate bond", Journal of Finance, Vol.56, pp. 247-277.

[17] Fahlenbrach, R, Rageth K. and Stulz R.M. (2020), "How Valuable is Financial Flexibility when Revenue Stops? Evidence from the COVID-19 Crisis", NBER Working Papers 27106.

[18] Fatica, S., Panzica R. and Rancan M. (2019), "The pricing of green bonds: are financial institutions different?", JCR Working Paper paper No.2019/7.

[19] Flammer, C. (2019), "Green bonds: Effectiveness and implication for public policy", NBER WP No. 25950.

Covid Economics 50, 25 September 2020: 33-73

[20] Gilchrist, S., Wei B., Yue V.Z. and Zakrajšek E. (2020), "The FED takes on corporate credit risk: An analysis of the efficacy of the SMCCF", NBER Working Papers 27809.

[21] Grosse-Rueschkamp, B., Steffen S. and Streitz D. (2019), "A Capital structure channel of monetary policy", Journal of Financial Economics, Vol. 133, pp. 357-378.

[22] Hollo, D., Kremer M., Lo Duca M. (2012). "CISS - A composite indicator of systemic stress in the financial system", ECB Working Paper No.1426.

[23] IEA (2020), "Sustainable recovery", International Energy Agency, Paris.

[24] IMF (2020a), "Greening the Recovery", Fiscal Affairs Special Series on Fiscal Policy to Respond to COVID-19, International Monetary Fund, Washington DC.

[25] IMF (2020b), "World Economic Outlook: The Great Lockdown", International Monetary Fund, Washington DC.

[26] Koren, M., and Pető R. (2020), "Business disruptions from social distancing" Covid Economics Vetted and Real-Time Papers 2, CEPR.

[27] Krueger, P., Sautner Z. and Starks L.T. (2020), "The Importance of Climate Risks for Institutional Investors", Review of Financial Studies, Vol.33, pp. 1067–1111.

[28] Landier, A and Thesmar D. (2020), "Earnings Expectations in the COVID Crisis", NBER Working Paper No. 27160.

[29] Li, F., Mercatanti A., Mäkinen T. and Silvestrini A. (2019), "A regression discontinuity design for categorical ordered running variables applied to central bank purchases of corporate bonds," Banca d'Italia Working Paper No. 1213.

[30] Moore, J. (2020), "Progressing Sustainability in the Context of Covid-19: Grasping the Opportunity", European Environment and Sustainable Development Advisory Councils, The Hague.

[31] NGFS (2020), "Statement on the Need for a Green Recovery", Central Banks and Supervisors Network for Greening the Financial System, Paris.

[32] Pagano, M., Wagner C. and Zechner J. (2020), "Disaster Resilience and Asset Prices", CEPR Discussion Paper 14773/Covid Economics: Vetted and Real-Time Papers 21, CEPR.

[33] Ramelli, S. and Wagner A.F. (2020), "Feverish stock price reactions to COVID-19", The Review of Corporate Finance Studies, forthcoming.

[34] Todorov, K. (2020), "Quantify the Quantitative Easing: Impact on bonds and corporate debt issuance", Journal of Financial Economics, Vol.135, pp. 340-358.

[35] Wang, J. and Wu C. (2015), "Liquidity, credit quality, and the relation between volatility and trading activity: Evidence from the corporate bond market", Journal of Banking and Finance, Vol.50, pp. 183-203.

[36] Zaghini, A. (2019), "The CSPP at work: Yield heterogeneity and the portfolio rebalancing channel", Journal of Corporate Finance, Vol.56, pp. 282-297.

[37] Zerbib, O.D. (2019), "The effect of pro-environmental preferences on bond prices: Evidence from green bonds", Journal of Banking and Finance, Vol.98, pp.39-60.

CEPR PRESS

Covid Economics **Issue 50, 25 September 2020**

The income gradient in mortality during the Covid-19 crisis: Evidence from Belgium[1]

André Decoster,[2] Thomas Minten[3] and Johannes Spinnewijn[4]

Date submitted: 16 September 2020; Date accepted: 18 September 2020

We use population-wide data from linked administrative registers to study the distributional pattern of mortality before and during the Covid-19 pandemic in Belgium. Excess mortality is only found among those aged 65 and over. For this group, we find a significant negative income gradient in excess mortality, with excess deaths in the bottom income decile more than twice as high as in the top income decile for both men and women. However, given the high inequality in mortality in normal times, the income gradient in all-cause mortality is only marginally steeper during the peak of the health crisis when expressed in relative terms. Leveraging our individual-level data, we gauge the robustness of our results for other socioeconomic factors and find that conclusions about the income gradient in excess mortality based on aggregate data can be misguided.

Covid Economics 50, 25 September 2020: 74-100

1 We thank Statbel, and especially Patrick Lusyne and Lien Tam Co for help with the data. Chlo´e de Meulenaer, Sebastian Ernst and Stijn Van Houtven provided excellent research assistance. We gratefully acknowledge funding by ERC (grant #716485) and by the Belgian Ministry of Social Security (2020-DGSTRAT-Studie Covivat).
2 KU Leuven.
3 London School of Economics.
4 London School of Economics and CEPR.

Covid Economics 50, 25 September 2020: 74-100

I Introduction

The Covid-19 pandemic affects everyone, but not everyone is affected equally. An important concern is that the burden of the Covid-19 crisis falls disproportionately on people with low income or socioeconomic status. A burgeoning literature studying the economic impact of the Covid-19 crisis and the associated policy measures on employment, earnings, and consumption (e.g., Adams-Prassl et al. (2020), Bachas et al. (2020), Chetty et al. (2020)) documents substantial differences depending on socioeconomic status. In parallel, many research efforts have focussed on the inequality of the health impact of the pandemic. While a rapidly growing literature suggests that socioeconomic factors are important determinants of Covid-19-related mortality (e.g., Chen, Waterman and Krieger (2020), Drefahl et al. (2020), Jung et al. (2020) and Williamson et al. (2020)), a strong income and/or socioeconomic gradient in health outcomes and in mortality in particular has been present prior to the arrival of the coronavirus. Indeed, one seemingly perennial finding documented in many countries is that mortality rates are higher among individuals with lower socioeconomic status (e.g., Chetty et al. (2016), Mackenbach et al. (2019)). An outstanding question is how the Covid-19 crisis has affected this relation between income or socioeconomic status and mortality in particular.

To answer this question we use population-wide data drawing from several administrative registers in Belgium. Belgium has been hit particularly hard by the first wave of the pandemic, noting the highest per capita death toll of any country by 30 May 2020. We use the mortality registers updated until June 2020 and linked to income registers as well as other demographic and socioeconomic information. This allows us to measure the income and socioeconomic gradient in mortality at the individual level, which we compare during the height of the Covid-19 health crisis - from March until May 2020 - with the corresponding months from 2015 to 2019.

A first advantage of our counterfactual analysis comparing all-cause mortality during and before the crisis is that we can provide evidence of the unequal burden of mortality due to the Covid-19 pandemic and relate it to the "usual" inequality in mortality in Belgium. While we do not directly observe Covid-19-related deaths, our measure of excess mortality corresponds well with Covid-19 mortality measured by Belgian health authorities in that period (see also Molenberghs et al. (2020)). The left column of Table 1 lists a large number of papers papers documenting the income or socioeconomic gradient of Covid-19-related deaths, mostly finding stark differences in mortality across different socioeconomic groups. But, importantly, these studies cannot infer how the Covid-19 pandemic has affected inequality in all-cause mortality. A few studies have looked at excess mortality directly, as listed in the right column of Table 1.

A second advantage of our data is that we can measure income and mortality at the individual level and therefore separate income effects from confounding location effects. The rows of Table 1 show that all but one paper analyze the relationship between Covid-19-related mortality and socioeconomic status measured at the municipality or another location-specific level in various countries. While most studies find a negative association, some indicate a more ambivalent relationship.[1] An important limitation of studies that use aggregate measures, however, is that they do not measure the direct link between individuals' socioeconomic status and mortality. By looking at area-level measures, these effects may confound various local factors like access to and quality of care, population density or local policy responses. Drefahl et al. (2020) are a notable exception and they finding a negative association using individual income data from Swedish registries, but they look at Covid-19 related mortality only.

There is thus a gap in the literature studying the relationship between individual-level measures of socioeconomic status and excess mortality during the Covid-19 crisis. Our paper aims to fill this gap and provides three main sets of results:

First, we find no significant excess mortality for people under 65 in Belgium during the Covid-19 crisis, like in several other countries (see EuroMOMO (2020)), neither do we find a meaningful change in the income gradient of all-cause mortality for this demographic group compared to the baseline years. The ratio between mortality among the bottom income and the top income decile stayed around 5 for men and 4 for women. In light of the earlier evidence on the unequal incidence along the income distribution in this age group of both Covid-19-related mortality (e.g., Drefahl et al. (2020)) and the underlying risk factors (e.g., Raifman and Raifman (2020), Wiemers et al. (2020)), this may come as an unexpected result.

Second, our results show that the Covid-19 pandemic significantly affected the mortality of individuals aged and over, and that the excess mortality for this age group declines significantly with income. For example for men, we estimate 326 excess deaths out of 100,000 in the bottom income decile compared to 131 in the top income decile. Importantly, the income gradient in mortality is strongly negative in normal times too. As a result, expressed in relative terms, the income gradient in all-cause mortality is only marginally steeper during the peak of the health crisis. We compare different measures for judging the inequality in all-cause mortality that the Covid-19 pandemic brought to this subpopulation, but can only reject lower-than-normal mortality inequality during the its peak. We study individuals living in nursing homes separately as the excess mortality during the Covid-19 pandemic has been particularly high for this subgroup, but we do not find any income gradient in mortality before or during the Covid-19 crisis for them. Overall, our results for this age group are confirmed when looking into other socioeconomic factors. We find strong educational gradients in excess mortality, as elderly who did not complete primary school experienced higher increases in mortality rates (30.47%) than elderly with higher

[1] Brandily et al. (2020), for instance, investigate excess mortality across municipalities in France, and find a negative income gradient, with excess mortality in the poorest municipalities twice as large as in other municipalities. In contrast, Jung et al. (2020) investigate the relationship between Covid-19 mortality and poverty across US counties and find that poverty and mortality are positively related in areas of low population density. In areas of high population density, however, they find a U-shaped relationship. Knittel and Ozaltun (2020) also analyze the county-level relationship between Covid-19 mortality and poverty in the US but find no correlation. They even find a *positive* relationship between mortality and median home value.

Covid Economics 50, 25 September 2020: 74-100

Table 1: Findings on the Association Between Socioeconomic Status (SES) and Mortality during the Covid-19 Crisis

	Covid-19 Mortality	Excess Mortality
Individual-level measure of SES	Drefahl et al. (2020) *Negative association* - Sweden	
Aggregate measure of SES	Abedi et al. (2020) *Negative association* - US Ashraf (2020) *Negative association* - World Chen and Krieger (2020) *Negative association* - US Desmet and Wacziarg (2020) *Mixed results*[a] - US Jung et al. (2020) *Mostly negative association*[b] - US Kim and Bostwick (2020) *Negative association* - US Knittel and Ozaltun (2020) *No/positive association*[c] - US Office for National Statistics (2020) *Negative association*[d] - UK Sá (2020) *Mixed results*[e] - UK Tubadji, Webber and Boy (2020) *Negative association* - UK Williamson et al. (2020) *Negative association* - UK	Brandily et al. (2020) *Negative association* - France Calderón-Larrañaga et al. (2020) *Negative association* - Sweden Chen, Waterman and Krieger (2020) *Negative association* - US

Notes: This table classifies the existing applied work on the relationship between SES and Covid-19-induced mortality into four quadrants, depending on the measure of mortality and SES used. Noted under each reference are the observed relationship between SES and Covid-19-induced mortality, as well as the country, in which the study was conducted. For papers that did not find a clear association, we provide further details below.

[a]Desmet and Wacziarg find that a higher level of Covid-19 mortality in a county is associated with poverty but uncorrelated with median household income. They also find it to have a non-monotonic relationship with the level of educational attainment.

[b]The authors find a U-shaped relationship between Covid-19 mortality and SES in counties with high population density and a negative relationship in counties with low population density.

[c]Knittel and Ozaltun find no correlation between Covid-19 death rate and poverty rate but find a positive correlation between Covid-19 death rate and median home value.

[d]The authors look at both Covid-19 mortality and all-cause mortality, but do not examine excess mortality.

[e]Sá finds no simple correlation between deprivation and Covid-19 mortality. Regression results show Covid-19 mortality to be higher in more deprived areas, although the relationship disappears when controlling for self-reported health.

Covid Economics 50, 25 September 2020: 74-100

Covid Economics 50, 25 September 2020: 74-100

education (21.91%). The increase in mortality has also been higher among Italian-, Turkish- and Polish-born residents than among Belgian-, German- and Dutch-born residents.

Third, we separate the inequality in mortality at the household level and at locally averaged levels. As mentioned, most prior work has been constrained by data availability and only considers differences in income aggregated at some local level. We find that our estimates of the income gradient using household income are robust to the inclusions of municipality or municipality-year fixed effects. This indicates that the relationship between individual income and mortality is not confounded by local factors, not even during the Covid-19 crisis. At the same time, we find that the income gradient of mortality is stronger when using income measured at the municipal instead of the household level, but this difference only appears during the Covid-19 crisis. This indicates that conclusions made about the individual socioeconomic factors of mortality can be misguided if based on aggregate data.

The paper proceeds as follows. Section II discusses the data and context. Section III present our main results, starting with the income gradients of all-cause and excess mortality and discussing the inequality implications, then studying other socioeconomic factors and the role of location effects. Section IV concludes.

II Data and Setting

Our study focuses on Belgium, which has been faced with a high count of Covid-19-related deaths per capita. The introduction of the Covid-19 virus in Belgium has mostly been attributed to the return of ski tourists from Italy and Austria after the national holiday week from February 22 until March 1, 2020. In response to the quick surge of Covid-19 infections that followed, a nationwide lockdown was imposed from March 18. This was slowly phased out starting with the opening of garden stores and DIY stores on the 18th of April, followed by the staggered opening of selected sectors (May 4), retail stores (May 11), and cafes and restaurants (June 8). At the same time, there was a staggered loosening of the restrictions on the number of close social contacts citizens could maintain with individuals from other households, going from 2 (May 4), to 4 (May 11) and 10 (June 8). These policy measures during the first months of the Covid-19 crisis were set at the federal level with arguably limited variation at the local level.

To study mortality across the income distribution, we link administrative data on mortality from the national register with data on income from tax records. We also link this to data from other population-wide registers, including the 2011 census. Below, we discuss the different data sources, which have been linked and made available through the Belgian Statistical Institute (Statbel).

Mortality in Belgium. Using the mortality records from the national register, Appendix Figure A.1 shows the dramatic increase in daily deaths in March to May 2020 following the onset of the Covid-19 pandemic. To investigate the effect of the Covid-19 pandemic and associated policy responses on mortality, we consider its impact on all-cause mortality and define excess mortality as the difference in mortality between 2020 and the average mortality in the corresponding period from 2015 to 2019. Positive excess mortality in 2020 primarily occured from 16 March to 27 May, with a record number of 314 excess deaths recorded on 10 April. We therefore take the March-May period as the relevant period to compare mortality between 2020 and the baseline years.[2] We note that total excess mortality in Belgium in this period is 8,195, which matches the official number of deaths of 9,467 counted in that period by Belgian health authorities reasonably well.[3]

Income. The income data originate from IPCAL, an administrative database that is drawn from personal income tax records. We use total net taxable income, which refers to income before tax, after social security contributions have been paid and costs deducted. It is a general definition of income, and includes labour income, unemployment benefits, sickness benefits and pensions.[4] Income data retrieved from tax declarations are contingent upon the tax legislation. Since capital income is subject to a liberating withholding tax, and some important benefits, such as child benefits, or the living wages (*leefloon*) are exempt from personal income tax, these income components are not included. We aggregate personal income over households[5] to obtain household income.

Demographic and Socioeconomic Variables. Most of the demographic information (age, country of birth, gender, municipality) originates from the national registries in Demobel. We also have an indicator for whether an individual is residing in a nursing home (*woonzorgcentra*) from Statbel. Economic sector and education level originate from the 2011 census. Municipality-specific information on per capita income and density comes from Statbel.

II.A Excess Mortality by Age

Figure 1 contrasts mortality rates by age during the months March-May in 2020 and the corresponding period in 2015-2019. Panel A provides a clear visual picture of excess mortality across different ages, indicating how concentrated it has been among the elderly. Panel B zooms in on individuals aged 0 to 50 and shows that there

[2]Mortality was significantly higher than in the previous five years continuously between 21 March and 12 May, between 22 and 25 May, and between 19 and 21 June. Hence, with the exception of significant excess mortality between 19 and 21 June, all significant excess mortality is measured within the period March-May.

[3]We do find a 13% discrepancy between excess mortality and the official death count. Potential reasons for this discrepancy might be the decrease in other-cause mortality in the study period or the over-counting of Covid-19 death toll by Belgian health authorities. Famously, all deaths with suspected involvement of Covid-19 were counted as Covid-19 deaths in Belgium. This has been actively portrayed as one of the reasons why the published death toll of Covid-19 in Belgium is one of the highest in the world.

[4]Pension income in Belgium is complex, and our data source based on taxable income captures annual pension income imperfectly. Pensions of the dominant 'first pillar' (the social security benefits) are a direct function of prior labor earnings and are mostly observed in the data. However, the treatment of the occupational pensions (the 'second pillar') and the personal private savings (the 'third pillar') is more problematic. Not only are these benefits only partly taxable in highly complex schedules, but tax payers can opt for the payment of this pension as a once-off lump sum amount. We find nevertheless that the correlation between our income measure when retired and earlier in life is quite strong, as evidenced by a high correlation of 0.63 between income decile at age 55 and income decile at age 65 for the same individual.

[5]The household indicators in our data indicate households from the socioeconomic Demobel database. We can construe household income for all individuals using this indicator, with the exception of individuals living in collective households such as nursing homes whom we rank based on personal income.

Covid Economics 50, 25 September 2020: 74-100

CEPR PRESS

Covid Economics 50, 25 September 2020: 74-100

was no significant excess mortality for people of those ages. Panel C zooms in on individuals aged 51-80 and shows that significant excess mortality only shows up for individuals aged 65+. These findings may seem surprising, as the Belgian health authorities (Sciensano) counted several hundred deaths in the 45-64 age bracket. However, these deaths have not lead to significant excess mortality. This pattern has been documented before in Belgium (Molenberghs et al. (2020)) as well as in other countries (EuroMOMO (2020)).[6] Clearly, returning to panel A, excess mortality is highest for individuals aged 80+.

Panel D considers nursing home residents separately and shows a substantial increase in mortality for nursing home residents aged 70+. Interestingly, this increase seems rather uniform for all ages above 70, which might be due to the selection of individuals less able to care for themselves into nursing homes, so that health status does not vary as much between older and younger nursing home residents compared to the general population. Our calculations suggest a heavy toll on nursing homes especially, as we estimate that in March to May 3.6% of all residents of nursing homes in Belgium have died due to the Covid-19 pandemic.

Overall, we find important differences in excess mortality in Belgium across the age distribution during the Covid-19 crisis. Based on the different patterns in excess mortality, our results in Section III distinguish between individuals aged 40-64, individuals aged 65+ not living in nursing homes (or other collective households), and individuals aged 65+ who are living in nursing homes. Appendix Table A.1 provides summary statistics for the three samples. One way to aggregate the mortality effects throughout the age distribution is to calculate the *period* life expectancy, which is the life expectancy of an individual based on the age-specific mortality rates in a given period (e.g., Chetty et al. (2016)). While the mortality rates increased the most for the elderly, changes in the mortality rates of the elderly have a smaller impact on life expectancy measures than changes among younger age groups. Based on the mortality rates in the baseline years, the period life expectancy in 2020 was 79.09 for men and 83.40 for women. Using the mortality rates between March-May of 2020 instead, the period life expectancy would be 1.87 years shorter for men, and 1.83 years for women.[7]

III Income Gradient of Mortality Rates

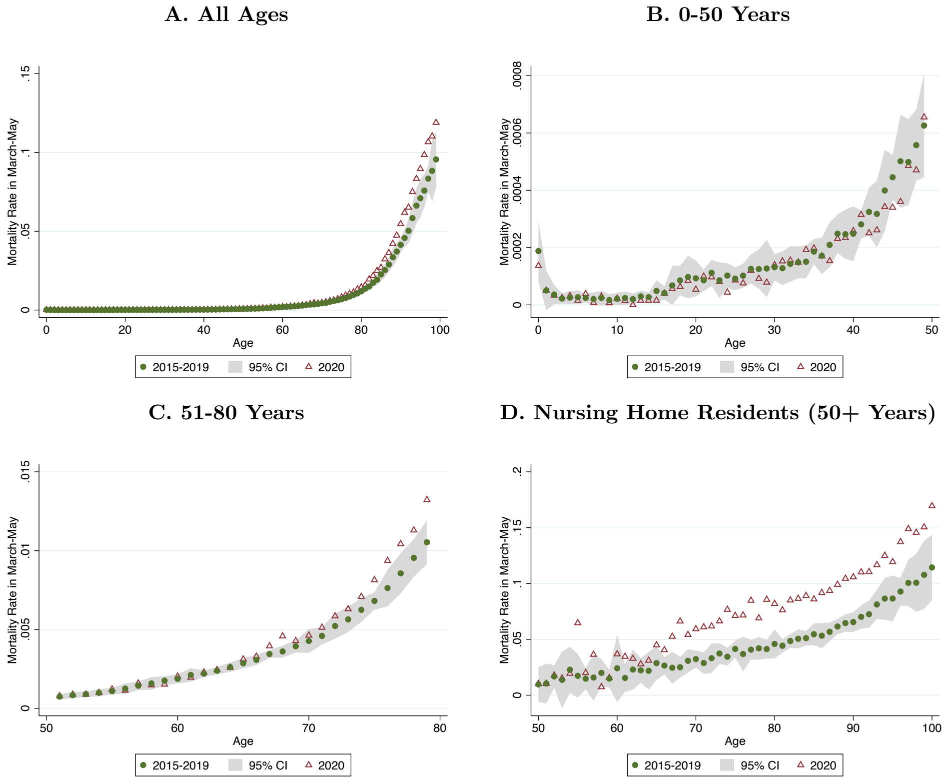
We now turn our analysis to the socioeconomic correlates of mortality and how their relationship changed during the Covid-19 crisis. Our main focus is on the income gradient of mortality rates and in particular on the comparison of the income gradient during the Covid-19 crisis with that of the baseline years. Since income - and socioeconomic status more broadly - is central to equity considerations, a large literature has studied the

[6] Only a few European countries, such as Spain and the UK, experienced significant excess mortality for people under 65.

[7] The period life expectancy for March-May 2020 is calculated in two steps. First, the 2015-2019 yearly mortality rates for each age-gender group are scaled with the P-score+1 obtained in March-May 2020, where the P-score is the estimated excess mortality divided by the baseline mortality within that group. Second, these scaled mortality rates are used to calculate life expectancy at birth for men and women separately.

Covid Economics 50, 25 September 2020: 74-100

Figure 1: Mortality Rates in March-May By Age



Notes: These figures show the average mortality rate by age in March-May of 2015-2019, with a 95% confidence interval, and in March-May of 2020. Panels A-C show mortality rates for all Belgian inhabitants, excluding people living in collective households, or households with more than 10 individuals. Panel D shows mortality rates for nursing home residents according to the classification of Statbel.

importance of health inequality along this dimension. Importantly, income gradients by themselves do not allow one to draw any causal conclusions regarding the effect of income on health outcomes, either before or during the Covid-19 crisis. However, comparing the income gradients before and during the crisis sheds lights on how the crisis has affected health inequality along this dimension.

III.A Income Gradient Before vs. During the Covid-19 Crisis

To calculate the mortality-income gradient, we rank individuals based on their household income and calculate mortality rates for different income quantiles. In particular, for every year t, we rank individuals within their age-gender group based on their lagged household income in year $t-3$ and assign a decile based on these rankings.

Covid Economics 50, 25 September 2020: 74-100

This means that the yearly deciles will be based on an individuals' lagged household income relative to all other individuals of the same gender and age in Belgium. We use a 3 year lag so that we observe lagged income for all years, including 2020, but by using lagged income we also reduce the potential response of income to health shocks (see Chetty et al. (2016)) and in particular the response of income itself to the Covid-19 crisis.[8]

Figure 2 shows mortality rates for men and women of different age groups, both in the control years (2015-2019) and in 2020, across deciles. The slope of the income gradient, either using a linear or loglinear regression specification, corresponds to two commonly used inequality measures in the literature (see Mackenbach and Kunst (1997) and Moreno-Betancur et al. (2015)): the SII or Slope Index of Inequality and the RII or Relative Index of Inequality respectively. Denoting mortality for decile d by $m(d)$, SII measures the difference $m(1) - m(10)$, and is often expressed in deaths per 100,000, whereas RII is defined as the ratio $m(1)/m(10)$ or as the percentage change in mortality across the income scale. Appendix Table A.2 reports the slope estimates and the imputed inequality indices for each of the income gradients.[9]

The top panels of Figure 2 focus on individuals between 40-64 years old. The income gradient is already strong and negative in the baseline years. For men, the mortality rate is estimated to be 5.3 times higher in the bottom income decile than in the top income decile. The same holds for women, be it somewhat less outspoken with a corresponding RII of 3.9. The negative income gradient in mortality rates is a persistent finding that underlies the substantial differences in life expectancy between low- and high income individuals (e.g., Chetty et al. (2016)). Importantly, the figure shows that for this age group the mortality rates during the Covid-19 months are indistinguishable from those of the control months. While we documented above that there is no average excess mortality in this age group, the income gradients confirm that this is also true for individuals in different income groups.

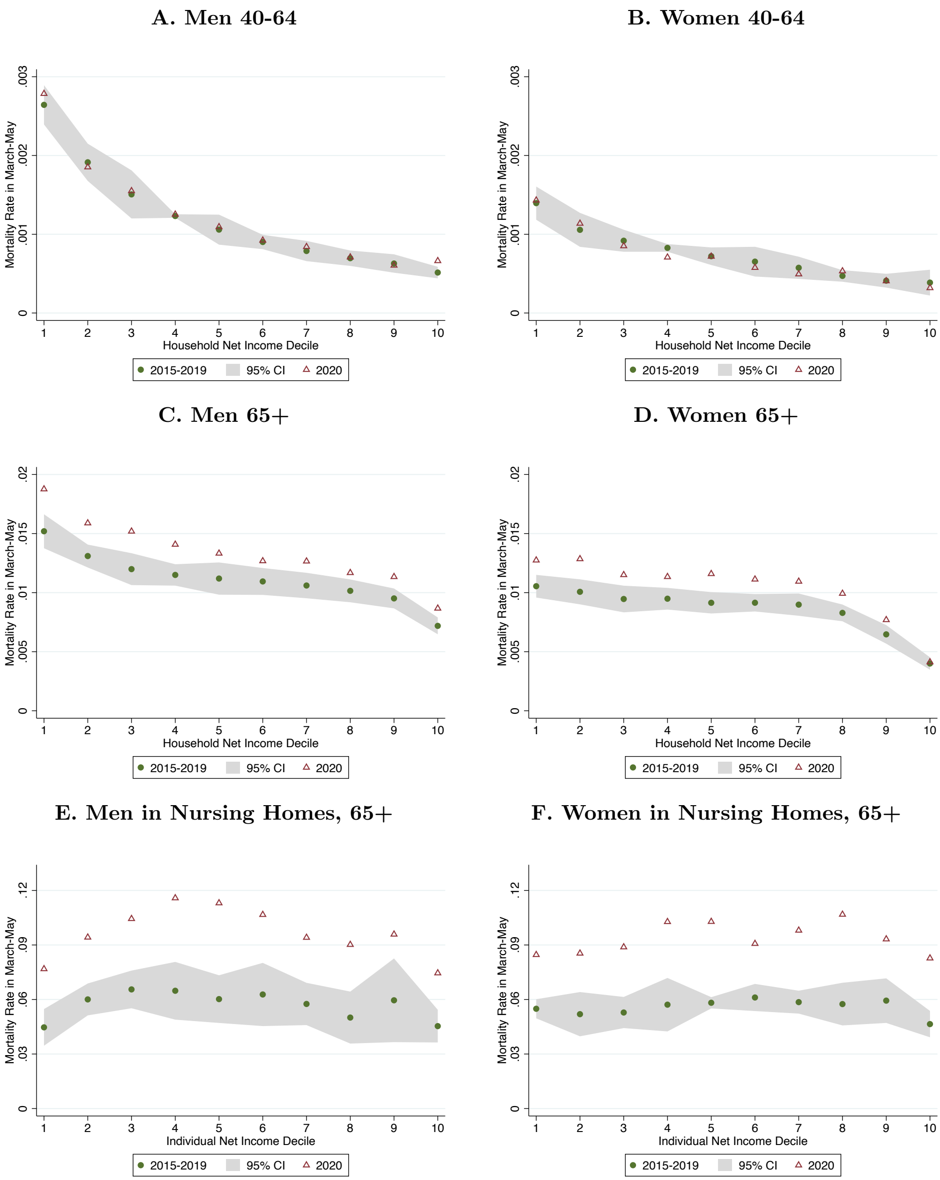
The middle panels of Figure 2 show a very different picture for the elderly. In the baseline years, the income gradient is again strongly negative. Compared to the younger age groups in panels A and B, the gradient is stronger when expressed in absolute terms, but smaller when expressed in relative terms (see Table A.2 in Appendix). More importantly, the mortality rates jump significantly during the Covid-19 months and they do so in each of the income groups of this age group. The SII increases substantially for men and women. For example for men, the estimated difference in deaths of 596 per 100,000 individuals between the bottom and top income decile during the baseline years increases to a difference of 791 deaths during the coronavirus period. However, expressed in relative terms, the increase in the income gradient has been more modest. The estimated RII increases from 1.8 to 1.9 for men and from 2.1 to 2.3 for women.

[8] Calculating household income deciles based on one year only is appropriate, as we find that individuals' household income deciles remain relatively stable over time, a finding corroborated in Chetty et al. (2016). Importantly, we find that the high correlation between individuals' income deciles continues after retirement.

[9] In particular, with the estimated coefficient of the loglinear regression equal to β, we estimate the mortality ratio between the first and the tenth decile to be equal to $\frac{1}{(1+\beta)^9}$.

Covid Economics 50, 25 September 2020: 74-100

Figure 2: Mortality Rates in March-May By Gender/Age/Income



Notes: These figures show the average mortality rate by income decile in March-May of 2015-2019, with a 95% confidence interval, and in March-May of 2020. Panels A-D show mortality rates for all Belgian individuals, excluding people living in collective households or households with more than 10 individuals. Panels E and F show mortality rates for Belgians inhabitants aged 65 or older and living in nursing homes. To control for differential selection into nursing homes depending on age, the results in Panels E and F are residualized on age.

Finally, the bottom panels show the mortality rates for individuals in nursing homes, who are excluded from the other panels. Interestingly, we do not find a clear income gradient in mortality rates for individuals in the baseline years. As is well known, the mortality increased most starkly for this group, but it did so uniformly across different income groups.

III.B Distributional Pattern of Excess Mortality

Several studies have analyzed the relationship between Covid-19-related mortality and socioeconomic status, arguing that the incidence of the pandemic falls disproportionally on low-income individuals. Our analysis of income-gradients - before and during the Covid-19 crisis for different groups - nuances this view and provides a new perspective. We already noted above that different pictures emerge when presenting the gradient by means of absolute (SII) or relative (RII) differences across the income scale. Both measures correspond to a different 'inequality equivalence' when looking at changes, the SII being invariant to equal absolute changes in mortality rates due to Covid-19, whereas the RII is invariant to equal percentage changes across the income scale. The 'choice' of presenting excess mortality as an absolute difference or as a relative change between the baseline years and the Covid-19-period then boils down to the choice of an absolute or relative perspective for the income gradient.

The top panels of Figure 3 show the excess mortality - expressed in absolute terms - for each household income decile in the male and female subpopulation older than 65. Consistent with the earlier observation that the SII increased during the Covid-19 crisis, excess mortality, measured in absolute differences, is decreasing with income. The differences are substantial. Using the estimated linear income gradients in Appendix Table A.2, the estimated excess mortality is 326 out of 100,000 in the bottom decile vs. 131 in the top decile for men. The corresponding numbers are 269 vs. 96 for women.[10] This corroborates the argument that the mortality incidence of the Covid-19 crisis falls disproportionately on lower income households. The nuance is that the difference in excess mortality by income is entirely driven by the elderly. In the younger age group the negative income gradient in all-cause mortality has basically remained the same, while in the group of nursing home residents there has been no meaningful relationship between income and mortality, neither before nor during the Covid-19 crisis.

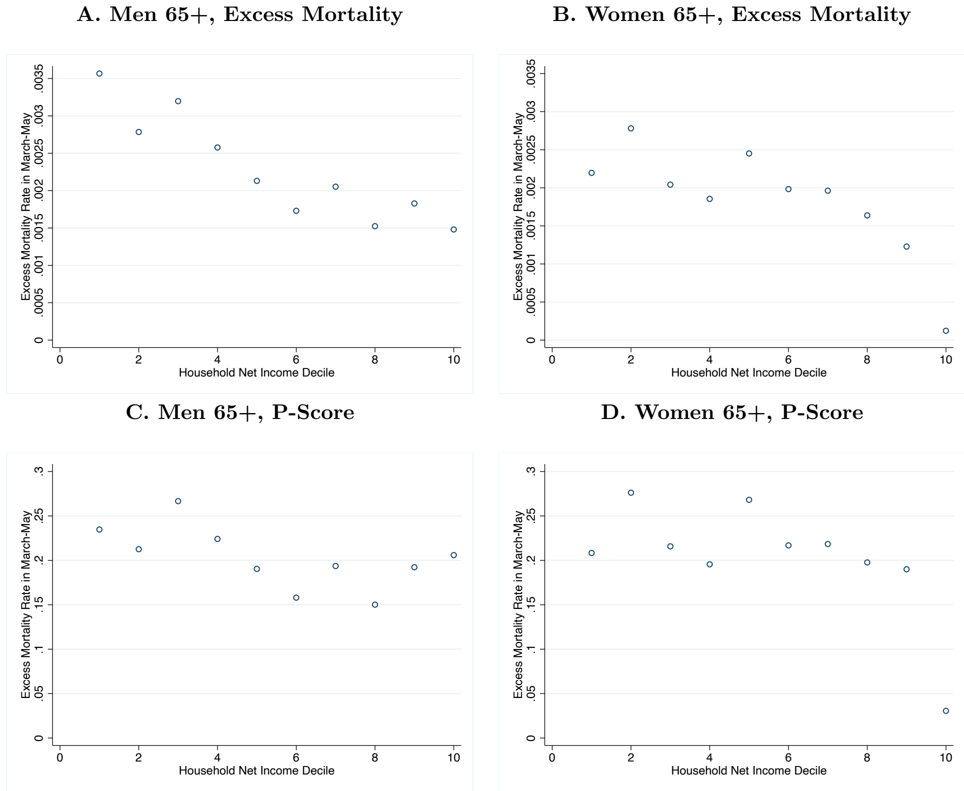
The bottom panels of Figure 3 show the excess mortality relative to baseline mortality - commonly referred to as P-scores (see Aron and Muellbauer (2020)) - for each income decile within the same subgroups. The relationship between the relative mortality increase and income is less precise and less pronounced overall. This corresponds to the small and insignifant change in the RII for both men and women, providing a new perspective

[10]The difference in observed excess mortality is even larger, especially for women, as the observed excess mortality in the top decile is an outlier.

Covid Economics 50, 25 September 2020: 74-100

Covid Economics 50, 25 September 2020: 74-100

Figure 3: Income Gradient in Absolute and Relative Excess Mortality March-May 2020



Notes: Panels A-B plot the excess mortality rate by income decile in March-May 2020 for individuals aged 65 or older, excluding people living in collective households, or households with more than 10 individuals. Panels C-D show the excess mortality fraction (P-score) for the same groups of individuals, where the P-Score is defined as excess mortality in 2020 divided by average mortality in 2015-2019 within the associated group.

on how much inequality has increased due to the Covid-19 crisis.

In principle it should not come as a surprise that choosing to use relative measures like the RII or absolute measures like the SII, can lead to different conclusions. Whereas many authors conclude that the best way out of this uncomfortable choice of measure is to present several of them - illustrated by the numerous other measures described in Mackenbach and Kunst (1997) - others point to the inescapable need to depart from the purely descriptive stance. They plead in favor of making the implicit value judgements in the chosen inequality measure explicit by following a more axiomatic route, inspired by the development in inequality or poverty measurement in the economic discipline.[11] Especially in the health economics context, this more axiomatic approach has been

[11] This is most markedly pronounced in the title of the paper by Kjellsson, Gerdtham and Petrie (2015) 'Lies, Damned Lies, and Health Inequality Measurements. Understanding the Value Judgements'. The descriptive nature of measures like SII or RII on the contrary, is revealed by labelling the estimated coefficients of the underlying regressions as the *least false parameter* (Moreno-Betancur

fruitful in unveiling the impact of using bounded variables (like mortality, which is bounded between 0 and 1), or the attractiveness of specific axioms, like the 'mirror axiom'. The latter imposes that, whether one chooses to measure inequality in terms of an 'attainment' (e.g. 'surviving'), or in terms of 'shortfall' (e.g. 'dying'), one should obtain the same inequality ordering in distributional comparisons.[12] When following the index proposed by Erreygers (2009), satisfying the mirror axiom, we again conclude that inequality has increased during the Covid-19 months (see Appendix Table A.2).

Besides the different normative perspectives, the obvious reason why the choice of measure matters so much empirically is the simple fact that mortality rates are so unequal during the baseline years. Framed differently: due to the strong baseline income gradient of mortality, the impact of the Covid-19 crisis on inequality is less clear cut. While it has not decreased by either of our measures, how much it has increased critically depends on the measurement of inequality.

III.C Household vs. Municipality-level Income Effects

Our results so far show that an individual's mortality is highly correlated with his or her household income and that this correlation increased further in the first months of the Covid-19 crisis. An individual's income is, however, related to many other factors, in particular the location that he or she lives in. The pandemic has struck differently across locations with differences in the inflow and propagation of infections, in access to hospitals and in response to the outbreak of the pandemic. To separate the effect of individuals' income from where individuals live, we study the robustness of the income gradient when controlling for municipality effects.

The first three columns from Table 2 report the estimates from a regression of individual mortality over the March-May period on log household income, allowing the relation to differ in the Covid-19 year 2020. The sample contains all individual-year observations between 2015 and 2020 for individuals alive at the start of that year. The first column confirms the negative gradient between mortality and log household income, which becomes stronger in 2020. The regression specification in the second column adds municipality fixed effects and the specification in the third column adds municipality times year fixed effects. The estimates of the income effects remain very similar across the three specifications.[13] The invariance of the estimates when using only within-municipality variation indicates that the income gradient of mortality in Belgium is not driven by location effects.

et al. (2015) p.519), emphasising that these parameters not necessarily correspond to an estimate of a "true" model underlying the data.

[12] As shown by Erreygers (2009) and Erreygers and Van Ourti (2011), imposing the mirror axiom drastically reduces the choice of inequality measures to measures which are 'absolute' instead of 'relative', i.e. inequality is unaffected by equal additions or subtractions of the outcome variable across the income scale. The fact that one cannot satisfy scale invariance, when imposing the mirror principle is easily seen from the fact that a distributional change which keeps the ratio's $m(i)/m(j)$ constant cannot simultaneously keep the ratio $(1-m(i))/(1-m(j))$ constant, where we use the example of mortality rates bounded between 0 and 1.

[13] Note that when regressing mortality on income quartiles instead, again allowing for an interaction with a year 2020 dummy, we obtain a similar invariance to the inclusion of municipality fixed effects and municipality-time fixed effects, as shown in Appendix Table A.3.

Covid Economics 50, 25 September 2020: 74-100

Table 2: Household Income Gradient vs. Municipality Income Gradient

	Dependent Variable: Mortality in March-May (0/1)			
	(1)	(2)	(3)	(4)
Year 2020	0.02022***	0.01941***		0.04087***
	(0.00152)	(0.00155)		(0.00527)
Log Household Income	-0.00419***	-0.00415***	-0.00416***	
	(0.00006)	(0.00006)	(0.00006)	
Year 2020 X Log Household Income	-0.00173***	-0.00165***	-0.00160***	
	(0.00015)	(0.00015)	(0.00015)	
Log Average Municipality Income				-0.00419***
				(0.00020)
Year 2020 X Log Average Municipality Income				-0.00395***
				(0.00054)
Constant	0.05270***	0.05238***	0.05572***	0.05099***
	(0.00058)	(0.00059)	(0.00055)	(0.00197)
Municipality FE	NO	YES	YES	NO
Municipality-Time FE	NO	NO	YES	NO
Observations	12,156,397	11,619,381	11,619,381	11,700,540
Adjusted R-squared	0.00069	0.00080	0.00084	0.00013

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: This table regresses mortality in March-May on a year 2020 dummy and on log income, as well as their interactions for individuals aged 65 or older, excluding people living in collective households, or households with more than 10 individuals. Only observations from years 2015-2020 were included. Column (1) uses log household income as the income variable. Column (2) adds fixed effects for every municipality in Belgium, and Column (3) adds the interactions between these municipality fixed effects and a year 2020 dummy. Column (4) uses log per capita municipality income as the income variable.

Importantly, this is also true for the income gradient in excess mortality during the Covid-19 crisis. Overall, the income gradients seem to reflect a relation between mortality and income itself rather than the effects of where individuals with different income live.

Most research studying the relationship between income or other socioeconomic factors and mortality during the Covid-19 crisis has been limited by data availability and needed to rely on aggregate measurements at different geographic levels (see Table 1). While the geographic inequality in the incidence of Covid-19 and how this correlates with income at the local level is important by itself, one should be cautious when drawing any inference about the role of individual socioeconomic determinants. This is particularly relevant in this context, as we have shown that the relationship between income and excess mortality is fully driven by the elderly.

We illustrate this in Table 2 by comparing the estimates when using log household income (column 1) and log municipality income (column 4). During the baseline years, the relationship between mortality and income is exactly the same when measured at the household level and the municipality level. A one percent increase in either measure decreases the mortality rate by .42 percentage points. However, this negative effect becomes much more pronounced during the Covid-19 crisis when using municipality income than when using household income. In the former case, it almost doubles (to .84 percentage points), while in the latter case, it increases by less than half (to .60 percentage points). This indicates that, during the Covid-19 crisis, the excess mortality was substantially worse in low-income municipalities compared to what the differences in household income can explain. This also implies that we would drastically overestimate the importance of socioeconomic factors at the individual level for excess mortality when using income measured at the municipality level.

III.D Other Socioeconomic Determinants

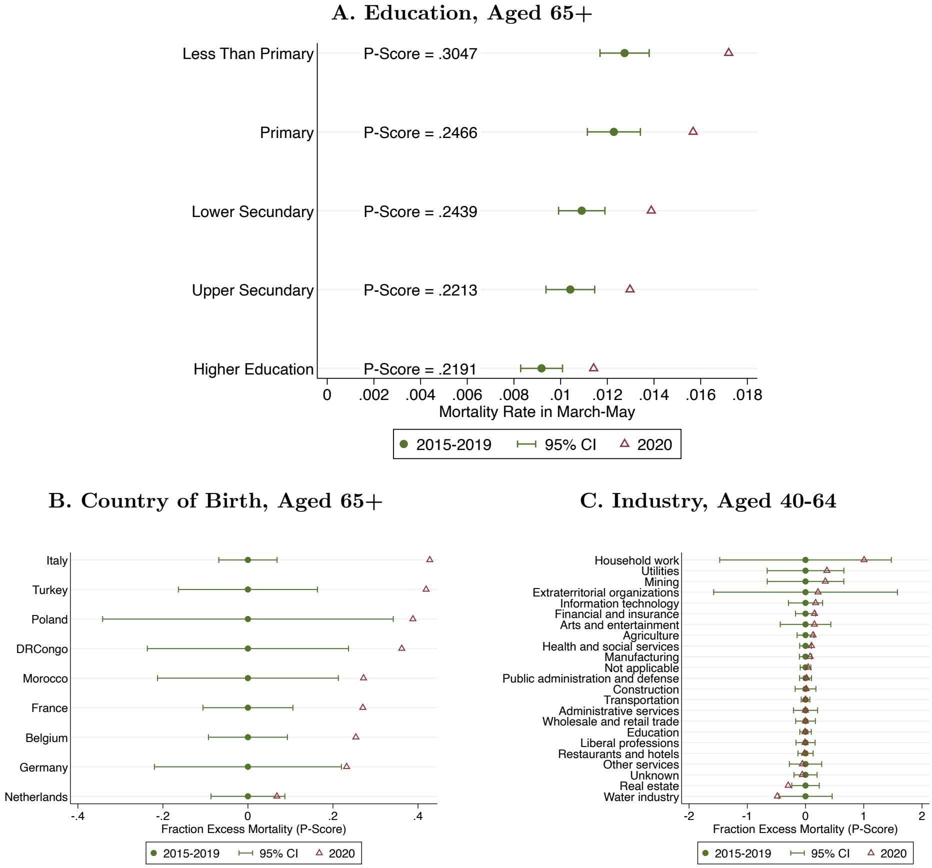
An important strand of the literature on socioeconomic differences in health points to education as the go-to indicator of socioeconomic status. The reason for this is both pragmatic and fundamental. Education is often known in survey data, and as education is obtained early in life, it is a arguably less endogenous to health than income as a socioeconomic indicator. Panel A of Figure 4 clearly shows how, for the elderly, the negative educational gradient in mortality becomes stronger during the Covid-19 crisis and the change is more pronounced than for the income gradient. Indeed, we find a negative educational gradient in excess mortality during the Covid-19 pandemic, both when expressed in absolute and relative terms. The mortality rate was 30.47% higher in March-May 2020 compared to the baseline years for elderly who did not complete primary school, while for elderly who completed higher education the increase was smaller at 21.91%. For individuals under 65, just like for the income-gradient, the relationship between education and mortality remains largely unchanged during the Covid-19 pandemic, as shown in Appendix Figure A.2.

We briefly consider two other socioeconomic factors in Figure 4. First, several authors have documented the large burden of the pandemic on minorities in the US and UK (Bertocchi and Dimico (2020), Gross et al. (2020), McLaren (2020), Chowkwanyun and Reed Jr (2020), Price-Haywood et al. (2020) and Chen and Krieger (2020)). While we do not observe race in our data, we do observe country of birth. Panel A of Figure 4 shows the relative increases in mortality (P-values) for Belgian residents aged 65+ by country of birth for the 9 most represented countries as country of birth among the elderly in Belgium. The mortality increases among Belgian residents born in Italy (42.77%), Turkey (41.91%) and Poland (38.80%) are larger than among those born in Belgium (25.39%), Germany (23.21%) and Netherlands (6.80%). While this suggests a divide between countries with Western and non-Western background, the mortality increase among those born in Morocco (27.19%) aligns

Covid Economics 50, 25 September 2020: 74-100

Covid Economics 50, 25 September 2020: 74-100

Figure 4: Excess Mortality by Education, Country of Birth and Industry



Notes: Panel A shows mortality rates (with 95% confidence intervals) in March-May 2015-2019 and March-May 2020 by educational level for individuals aged 65 and older. The excess mortality in percentages (P-Score) is also indicated on the figure. Panel B shows excess mortality fractions in March-May 2020 and 95% confidence intervals for 2015-2019 by country of birth for individuals aged 65 and older. Panel C shows excess mortality fractions in March-May 2020 and 95% confidence intervals for 2015-2019 by industry for individuals aged 40-64. Samples in all panels exclude individuals living in collective households, or households with more than 10 individuals. Average mortality rates (also used in the computation of the P-score) are the weighted average of mortality rates by age, where population-based weights are taken for each age. Such a calculation makes sure that there is no influence of age-related composition differences between origins on the plotted mortality rate differences or P-scores.

CEPR PRESS

Covid Economics 50, 25 September 2020: 74-100

with the second group.[14] Second, while we do not observe the occupation of workers, we do observe the industry they work in. Focusing on individuals between 40-64 years old, Panel C of Figure 4 shows substantial dispersion in the relative increases in mortality across industries, but for none of the industries is the difference between the mortality rate during the Covid-19 crisis and the years before highly significant. This is not too surprising given the lack of significant excess mortality in that age group as a whole. Interestingly, the only sector where we do find marginally significant positive excess mortality is the health and social services sector (10.06%), where workers have arguably been more exposed to the virus.

IV Discussion

This paper relates high-quality individual data on mortality to socioeconomic factors and contributes to a better understanding of the impact of the pandemic on the socioeconomic gradient of mortality. We showed that there exists a significant and negative income gradient in excess mortality during the Covid-19-period in Belgium for the elderly. However, this - strongly negative - gradient is comparable to the gradient in all-cause mortality in non-pandemic times. The Covid-19 crisis might stall the trend of narrowing absolute (but not relative) mortality inequality, as documented recently for European countries in Mackenbach et al. (2019).

The reasons for potential socioeconomic differences and thus a socioeconomic gradient in incidence and mortality of Covid-19 are heavily debated. Apart from the higher likelihood of high-income individuals to import the virus due to international travel, as observed in Pluemper and Neumayer (2020), several papers hint at higher transmission rates among individuals with low socioeconomic status once the illness is widespread within a country. Brandily et al. (2020) mention poor housing conditions and higher occupational exposure as the most likely mechanisms causing the higher burden for the poor in France, while McLaren (2020) stresses the importance of higher transit exposure among the less well off. Papageorge et al. (2020) argues that individuals of lower socioeconomic status typically have less flexible work arrangements and a lack of outside space at home, which in turn are correlated with less protection against a pandemic.

Due to the specific data-availability in the Belgian case, our current analysis faces important limitations. First, by using mortality data, we cannot separate the income gradient in infection (e.g., due to differences in employment or social contacts) from the one in case fatality risk (e.g., due to an income gradient in Covid-19 mortality risk factors). Clearly, linking the available data on hospitalizations, prior health diagnoses and test results would allow important progress to be made. Second, by considering mortality, we potentially miss out on important differences in morbidity, physical health, and mental well-being. Again, linking the available data

[14] When looking at excess mortality fractions for individuals aged 40-64 by country of birth in Appendix Figure A.2, we generally find few groups with significantly positive excess mortality. One exception is the high and significant P-score of 52.79% for 40-64 aged Congolese-born individuals.

from health records or surveys would allow researchers to provide a more comprehensive picture of the unequal consequences of the ongoing crisis.

Covid Economics 50, 25 September 2020: 74-100

References

Abedi, Vida, Oluwaseyi Olulana, Venkatesh Avula, Durgesh Chaudhary, Ayesha Khan, Shima Shahjouei, Jiang Li, and Ramin Zand. 2020. "Racial, Economic, and Health Inequality and COVID-19 Infection in the United States." *Journal of Racial and Ethnic Health Disparities.*

Adams-Prassl, Abi, Teodora Boneva, Marta Golin, and Christopher Rauh. 2020. "Inequality in the impact of the coronavirus shock: Evidence from real time surveys." *Journal of Public Economics*, 189: 104245.

Aron, Janine, and John Muellbauer. 2020. "Measuring excess mortality: the case of England during the Covid-19 Pandemic." INET Oxford Working Paper No. 2020-11.

Ashraf, Badar Nadeem. 2020. "Socioeconomic conditions, government interventions and health outcomes during COVID-19." *Covid Economics*, 37: 141–162.

Bachas, Natalie, Peter Ganong, Pascal J Noel, Joseph S Vavra, Arlene Wong, Diana Farrell, and Fiona E Greig. 2020. "Initial Impacts of the Pandemic on Consumer Behavior: Evidence from Linked Income, Spending, and Savings Data." National Bureau of Economic Research Working Paper 27617.

Bertocchi, Graziella, and Arcangelo Dimico. 2020. "COVID-19, Race, and Redlining." *Covid Economics*, 38(4): 129–195.

Brandily, Paul, Clément Brébion, Simon Briole, and Laura Khoury. 2020. "A Poorly Understood Disease? The Unequal Distribution of Excess Mortality Due to COVID-19 Across French Municipalities." *medRxiv.*

Calderón-Larrañaga, Amaia, Davide L Vetrano, Debora Rizzuto, Tom Bellander, Laura Fratiglioni, and Serhiy Dekhtyar. 2020. "High excess mortality during the COVID-19 outbreak in Stockholm Region areas with young and socially vulnerable populations." *medRxiv.*

Chen, Jarvis T., and Nancy Krieger. 2020. "Revealing the unequal burden of COVID-19 by income, race/ethnicity, and household crowding: US county vs ZIP code analyses." *Harvard Center for Population and Development Studies Working Paper Series*, 19(1).

Chen, Jarvis T., Pamela D. Waterman, and Nancy Krieger. 2020. "COVID-19 and the unequal surge in mortality rates in Massachusetts, by city/town and ZIP Code measures of poverty, household crowding, race/ethnicity, and racialized economic segregation." Harvard Center for Population and Development Studies (HCPDS) Working Paper Volume 19, Number 2.

Chetty, Raj, John N Friedman, Nathaniel Hendren, Michael Stepner, and The Opportunity Insights Team. 2020. "How Did COVID-19 and Stabilization Policies Affect Spending and Employment? A New Real-Time Economic Tracker Based on Private Sector Data." National Bureau of Economic Research Working Paper 27431.

Chetty, Raj, Michael Stepner, Sarah Abraham, Shelby Lin, Benjamin Scuderi, Nicholas Turner, Augustin Bergeron, and David Cutler. 2016. "The Association Between Income and Life Expectancy in the United States, 2001-2014." *JAMA*, 315(16): 1750–1766.

Chowkwanyun, Merlin, and Adolph L Reed Jr. 2020. "Racial health disparities and Covid-19—caution and context." *New England Journal of Medicine*.

Desmet, Klaus, and Romain Wacziarg. 2020. "Understanding Spatial Variation in COVID-19 across the United States." National Bureau of Economic Research.

Drefahl, Sven, Matthew Wallace, Eleonora Mussino, Siddartha Aradhya, Martin Kolk, Maria Brandén, Bo Malmberg, and Gunnar Andersson. 2020. "Socio-demographic risk factors of COVID-19 deaths in Sweden: A nationwide register study." Stockholm Research Reports in Demography.

Erreygers, G., and T. Van Ourti. 2011. "Measuring socioeconomic inequality in health, health care and health financing by means of rank-dependent indices: A recipe for good practice." *Journal of Health Economics*, 30: 685–694.

Erreygers, Guido. 2009. "Correcting the concentration index." *Journal of Health Economics*, 28: 504–515.

EuroMOMO. 2020. "Graphs and maps." https://www.euromomo.eu (accessed September 11, 2020).

Gross, Cary P, Utibe R Essien, Saamir Pasha, Jacob R Gross, Shi-Yi Wang, and Marcella Nunez-Smith. 2020. "Racial and Ethnic Disparities in Population-Level Covid-19 Mortality." *Journal of General Internal Medicine*, 1–3.

Jung, Juergen, James Manley, Vinish Shrestha, et al. 2020. "Coronavirus Infections and Deaths by Poverty Status: Time Trends and Patterns."

Kim, Sage J., and Wendy Bostwick. 2020. "Social Vulnerability and Racial Inequality in COVID-19 Deaths in Chicago." *Health Education & Behavior*, 47(4): 509–513. PMID: 32436405.

Kjellsson, G., U-G. Gerdtham, and D. Petrie. 2015. "Lies, damned lies, and health inequality measurements: understanding the value judgments." *Epidemiology*, 26: 673–680.

Covid Economics 50, 25 September 2020: 74-100

Knittel, Christopher R, and Bora Ozaltun. 2020. "What Does and Does Not Correlate with COVID-19 Death Rates." National Bureau of Economic Research Working Paper 27391.

Mackenbach, J.P., and A.E. Kunst. 1997. "Measuring the magnitude of socio-economic inequalities in health: an overview of available measures illustrated with two examples from Europe." *Soc Sci Med.*, 44: 757–771.

Mackenbach, J. P., J. R. Valverde, M. Bopp, H. Bronnum-Hansen, G. Costa, P. Deboosere, R. Kaledienne, K. Kovács, M. Leinsalu, P. Martikainen, G. Menvielle, M. Rodriguez-Sanz, and W. J. Nusselder. 2019. "Progress against inequalities in mortality: register-based study of 15 European countries between 1990 and 2015." *European Journal of Epidemiology*, 34: 1131– 1142.

McLaren, John. 2020. "Racial Disparity in COVID-19 Deaths: Seeking Economic Roots with Census data." National Bureau of Economic Research.

Molenberghs, Geert, Christel Faes, Jan Aerts, Heidi Theeten, Brecht Devleesschauwer, Natalia Bustos Sierra, Toon Braeye, Francoise Renard, Sereina Herzog, Patrick Lusyne, Johan Van der Heyden, Herman Van Oyen, Pierre Van Damme, and Niel Hens. 2020. "Belgian Covid-19 Mortality, Excess Deaths, Number of Deaths per Million, and Infection Fatality Rates (8 March - 9 May 2020)." *medRxiv*.

Moreno-Betancur, M., A. Latouche, G. Menvielle, A.E. Kunst, and G. Rey. 2015. "Relative index of inequality and slope index of inequality: a structured regression framework for estimation." *Epidemiology*, 26: 518–27.

Office for National Statistics. 2020. "Deaths involving COVID-19 by local area and socioeconomic deprivation: deaths occurring between 1 March and 31 July 2020."

Papageorge, Nicholas W, Matthew V Zahn, Michèle Belot, Eline van den Broek-Altenburg, Syngjoo Choi, Julian C Jamison, and Egon Tripodi. 2020. "Socio-demographic factors associated with self-protecting behavior during the Covid-19 pandemic." *Covid Economics*, , (40): 1–45.

Pluemper, Thomas, and Eric Neumayer. 2020. "The COVID-19 Pandemic Predominantly Hits Poor Neighborhoods, or does it? Evidence from Germany." *medRxiv*.

Price-Haywood, Eboni G, Jeffrey Burton, Daniel Fort, and Leonardo Seoane. 2020. "Hospitalization and mortality among black patients and white patients with Covid-19." *New England Journal of Medicine*.

Raifman, Matthew A., and Julia R. Raifman. 2020. "Disparities in the Population at Risk of Severe Illness From COVID-19 by Race/Ethnicity and Income." *American Journal of Preventive Medicine*, 59(1): 137–139.

Sá, Filipa. 2020. "Socioeconomic Determinants of COVID-19 Infections and Mortality: Evidence from England and Wales." Institute of Labor Economics (IZA) Policy Paper 159.

Tubadji, Annie, Don J Webber, and Fred Boy. 2020. "The determinants of the differential exposure to COVID-19 in New York City and their evolution over time." *Covid Economics*, , (13): 50–70.

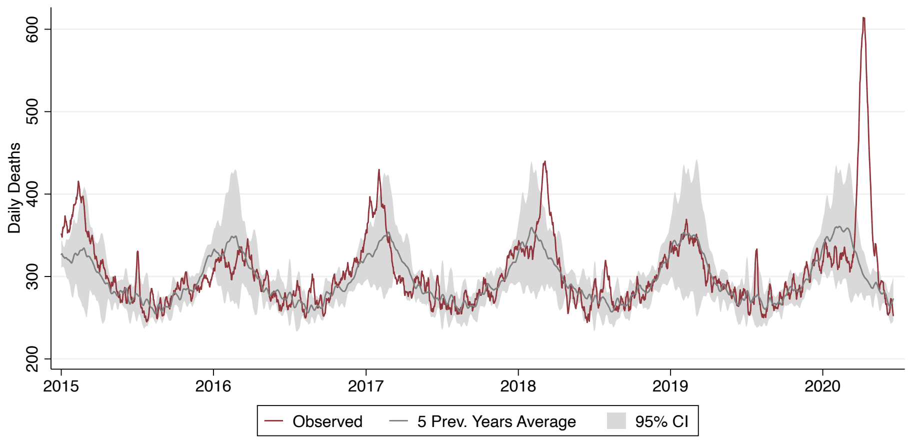
Wiemers, Emily E, Scott Abrahams, Marwa AlFakhri, V. Joseph Hotz, Robert F Schoeni, and Judith A Seltzer. 2020. "Disparities in Vulnerability to Severe Complications from COVID-19 in the United States." National Bureau of Economic Research Working Paper 27294.

Williamson, Elizabeth J., Alex J. Walker, Krishnan Bhaskaran, Seb Bacon, Chris Bates, Caroline E. Morton, Helen J. Curtis, Amir Mehrkar, David Evans, Peter Inglesby, Jonathan Cockburn, Helen I. McDonald, Brian MacKenna, Laurie Tomlinson, Ian J. Douglas, Christopher T. Rentsch, Rohini Mathur, Angel Y. S. Wong, Richard Grieve, David Harrison, Harriet Forbes, Anna Schultze, Richard Croker, John Parry, Frank Hester, Sam Harper, Rafael Perera, Stephen J. W. Evans, Liam Smeeth, and Ben Goldacre. 2020. "Factors associated with COVID-19-related death using OpenSAFELY." *Nature*, 584(7821): 430–436.

Covid Economics 50, 25 September 2020: 74-100

A Appendix Tables and Figures

Figure A.1: Covid-related Spike in Deaths in March-May 2020



Notes: This plot shows the daily 7-day moving averages of the number of deaths recorded in Belgium. Also plotted is the average daily 7-day moving average of mortality in the 5 previous years, together with 95% confidence intervals.

CEPR PRESS

Table A.1: Summary Statistics

	40-64	65+	Nursing Home Residents, 65+
	Mean	Mean	Mean
Demographics			
Male	49.97%	44.77%	24.97%
Age	52	75	86
Died in March-May 2020	0.10%	1.19%	9.64%
Education Level			
Missing	11.60%	8.01%	13.44%
Less Than Primary	1.52%	5.23%	7.60%
Primary	6.09%	20.45%	33.90%
Lower Secundary	16.95%	26.80%	23.98%
Upper Secundary	33.15%	20.79%	12.75%
Higher Education	30.69%	18.72%	8.34%
Household Income			
Mean	48,409	34,487	
p10	14,710	15,480	
Median	46,420	29,700	
p90	86,730	61,850	
Personal Income			
Mean	26,389	19,495	18,653
p10	10,440	8,380	12,870
Median	24,090	17,490	17,040
p90	50,000 (capped)	33,420	26,910
Municipality			
Per Capita Income	18,501	18,732	18,477
Number of Residents	66,343	64,415	72,120
Observations	3,740,619	2,130,114	100,829

Notes: This table shows summary statistics for three subsamples of Belgian citizens in 2020. Household and personal income are measured in 2017. Nursing home residents' household income is not included as in our data residents within one nursing home are counted as belonging to the same household. Municipality per capita income and number of residents are measured in 2017.

Covid Economics 50, 25 September 2020: 74-100

Table A.2: Regression and Inequality Estimates

	Aged 40-64		Aged 65+		Aged 65+ in Nursing Homes	
A. Slope Estimates	Men	Women	Men	Women	Men	Women
Linear Regression						
2015-2019	-0.00021	-0.00010	-0.00066	-0.00055	0.00152	-0.00084
	(0.00003)	(0.00001)	(0.00008)	(0.00011)	(0.00106)	(0.00041)
2020	-0.00020	-0.00011	-0.00088	-0.00075	0.00286	-0.00015
	(0.00004)	(0.00002)	(0.00009)	(0.00016)	(0.00202)	(0.00067)
Log-linear Regression						
2015-2019	-0.169	-0.139	-0.061	-0.077	0.030	-0.015
	(0.01)	(0.006)	(0.008)	(0.020)	(0.020)	(0.007)
2020	-0.158	-0.152	-0.067	-0.089	0.031	-0.002
	(0.015)	(0.012)	(0.007)	(0.025)	(0.020)	(0.007)
B. Inequality Measures						
Slope Index of Inequality (SII)						
2015-2019	185	93	596	499	-1368	758
2020	184	100	791	672	-2572	131
Relative Index of Inequality (RII)						
2015-2019	5.30	3.86	1.76	2.05	0.76	1.15
2020	4.71	4.43	1.86	2.31	0.76	1.02
Erreygers-index						
2015-2019	0.0014	0.0007	0.0044	0.0037	-0.0100	0.0056
2020	0.0013	0.0007	0.0058	0.0049	-0.0189	0.0010

Notes: This table provides information on the distributional pattern of mortality in 2015-2019 and in 2020. Panel (A) provides slope estimates and associated standard errors from a linear and log-linear regression of mortality rates on income deciles for both periods separately. Panel (B) shows several measures to evaluate the inequality in mortality in both periods. The calculation of SII - expressed per 100,000 - and RII are based on the estimated slopes in Panel (A).

Covid Economics 50, 25 September 2020: 74-100

Table A.3: Income Gradient Within Municipalities

	Dependent Variable:		
	Mortality in March-May (0/1)		
	(1)	(2)	(3)
Year 2020	0.0027***	0.0027***	
	(0.0002)	(0.0002)	
Income Q2	-0.0015***	-0.0014***	-0.0014***
	(0.0001)	(0.0001)	(0.0001)
Income Q3	-0.0020***	-0.0019***	-0.0019***
	(0.0001)	(0.0001)	(0.0001)
Income Q3	-0.0044***	-0.0044***	-0.0044***
	(0.0001)	(0.0001)	(0.0001)
Year 2020 X Income Q2	-0.0004	-0.0003	-0.0002
	(0.0002)	(0.0002)	(0.0002)
Year 2020 X Income Q3	-0.0009***	-0.0008***	-0.0007***
	(0.0002)	(0.0002)	(0.0002)
Year 2020 X Income Q4	-0.0014***	-0.0013***	-0.0012***
	(0.0002)	(0.0002)	(0.0002)
Constant	0.0117***	0.0117***	0.0121***
	(0.0001)	(0.0001)	(0.0001)
Municipality FE	NO	YES	YES
Municipality-Time FE	NO	NO	YES
Observations	12,156,006	11,619,004	11,619,004
Adjusted R-squared	0.0003	0.0005	0.0005

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

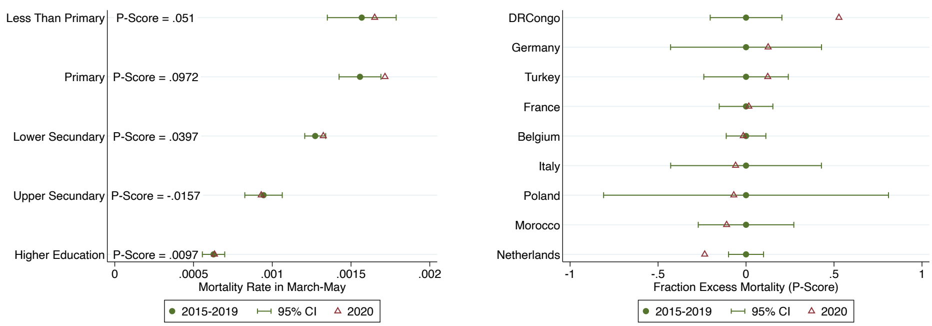
Notes: This table regresses mortality in March-May on a year 2020 dummy and on household income quartile dummies, as well as their interactions for individuals aged 65 or older, excluding people living in collective households, or households with more than 10 individuals. Only observations from years 2015-2020 are included. Column (2) adds fixed effects for every Belgian municipality. Column (3) adds the interactions between these municipality fixed effects and a year 2020 dummy.

Covid Economics 50, 25 September 2020: 74-100

Covid Economics 50, 25 September 2020: 74-100

Figure A.2: Excess Mortality by Education and Country of Birth

A. Education, Aged 40-64 **B. Country of Birth, Aged 40-64**



Notes: Panel A shows mortality rates (with 95% confidence intervals) in March-May 2015-2019 and March-May 2020 by educational level for individuals aged 40-64, excluding people living in collective households, or households with more than 10 individuals. Panel B shows excess mortality fractions in March-May 2020 and 95% confidence intervals for 2015-2019 by country of birth for individuals aged 40-64 and older, excluding people living in collective households, or households with more than 10 individuals. Calculation of mortality rates and P-score is similar as in Figure 4.

CEPR PRESS

101

Covid Economics Issue 50, 25 September 2020

Integrating health and economic parameters to optimize COVID-19 mitigation strategies[1]

Alik Sokolov,[2] Yichao Chen,[3] Jonathan Mostovoy,[4] Andrew Roberts,[5] Luis Seco[6] and V. Kumar Murty[7]

Date submitted: 17 September 2020; Date accepted: 18 September 2020

Managing the COVID-19 effectively requires a series of mitigating steps that change over time as function of external conditions and previous mitigating steps. Modeling this effectively requires a multi-disciplinary integrative approach that combines epidemiological, economic and social considerations within a unified modeling environment. Our research focuses on incorporating elements from classical utility theory in combination with control theory and machine learning to better model the dynamics of the non-linear trade-offs inherent in managing the pandemic. We postulate a theoretical formulation on how these trade-offs can be modeled, and demonstrate empirical results to elucidate the limiting factors in finding efficient solutions.

Covid Economics 50, 25 September 2020: 101-112

1 Alik Sokolov contributed equally to this work with Yichao Chen.
2 University of Toronto.
3 University of Toronto.
4 University of Toronto.
5 Smith School of Business, Queens University.
6 University of Toronto.
7 Fields Institute for Research in Mathematical Sciences.

CEPR PRESS

Covid Economics 50, 25 September 2020: 101-112

1 Introduction

The COVID-19 pandemic has unequivocally impacted nearly every individual, household, business, and organization on a global scale, with wide-ranging consequences for which there is little historical precedent to draw from. Alongside the direct impact of COVID-19 infections, there is a multitude of economic, social and health trade-offs that require consideration in determining the optimal mitigation strategies in dealing with the pandemic. It is our hope that the integrative approach we propose can bring together the many perspectives that already exist in the growing body of literature surrounding the pandemic.

In particular, modeling efforts for COVID-19 have largely been focused on epidemiological models, and the field has evolved rapidly to account for the complexity of projecting the disease forward through multiple modified SEIR approaches [1] [2], as well as other approaches to case load forecasting [3]. The SEIR approaches are supplemented by studies that model the impact of various mitigation strategies [4] [5] [6], and even quantify optimality of mitigation strategies [7], but such studies are largely focused solely on the epidemiological perspective. The academic community has not focused as heavily on economic as epidemiological forecasting, although many differing studies exist on impact on single / unique policies [8] [9] [10]. There are also in-depth economics studies focusing on the impact already incurred [11], but these again are primarily focused on the economic impact exclusively. Similar in-depth studies are available in other geographies [12] [13], but even globally there is a distinct lack of integrative modeling. Several studies also focus on the interactions between social, economic and epidemiological factors [14] [15], as well as economic and health factors [16], but these perspectives are yet to be fully integrated in a quantitative framework. Given this substantial gap in the literature, we put forward a modeling approach to explicitly analyze the multi-variate relationship between COVID-19 case-load numbers, economic impacts, and mitigation actions. We also showcase some immediate results by simulating sequences of mitigation actions and their epidemiological and economic impacts simultaneously. Lastly, we make our simulation environment publicly available for research purposes similar to prior efforts in the field [17], [18] to encourage cross-disciplinary collaboration.

2 Problem Set Up

The COVID-19 pandemic's impact involves both a significant health cost and a broader social cost. In this paper, we elucidate modeling approaches to understanding the economic and social impact of the pandemic under different mitigation actions, and over a prolonged period of time. One of the difficulties of modeling the combined effect of these factors are the different time scales under which social and economic costs are incurred. Mitigation actions that relax restrictions and increase effective reproductive number, R_{eff}, carry a shorter-term economic benefit compared to the long-term costs realized to the exponential progression of case load realized when effective R_{eff} exceeds 1.

For our initial problem set up, we concentrate on modeling the trade-off between two primary factors: number of cases, and unemployment. For each of these factors, we introduce a forecast model to estimate the consequences of mitigation actions on this factor. We refer to the forecast models for case load and unemployment as F_c and F_e, respectively. For all models, we consider a fixed time horizon T. We represent each set of actions at time t as a vector $A_t = (a_1, a_2, \ldots, a_n)$, where each action a_i can be either a categorical or continuous variable (to represent partial adherence or a restriction being partially in place). In our empirical implementation, we consider the following mitigation actions: office closures, school closures, mask mandates, retail and restaurant closures, and limiting mass gatherings. We consider the sequence (A_t) of all feasible action choices across T steps as $(A_t) := (A_1, A_2 ... A_T)$.

We also condition our forecast model on the policy choices at each time t, in addition to the naturally autoregressive structure of the forecast models. This allows us to simulate the progression of cases and economic conditions over time as autoregressive processes over the number of cases, combined with the sequence of mitigation actions, and enables us to define our problem as an optimization over all possible action sequences (A_t).

This formulation also has the advantage of having the Markov Property: both our economic and epidemiological processes are functions of each respective process and the mitigating actions at the previous time step only. That is, the expected value at time t is a function of factors at time $t-1$ only:

CEPR PRESS

$$\mathbb{E}(C_t|C_{t-1}) = \mathbb{E}(C_t|C_{t-1}, C_{t-2}, ...C_1);$$

$$\mathbb{E}(E_t|E_{t-1}) = \mathbb{E}(E_t|E_{t-1}, E_{t-2}, ...E_1);$$

Since this is also true for each choice of actions A_t, the overall dynamics can be projected forward as:

$$C_t = F_c(A_{t-1}, C_{t-1});$$
$$E_t = F_e(A_{t-1}, E_{t-1});$$

3 Optimization Formulation

A natural way to approach this problem is to frame it as an optimization problem, where we maximize our overall utility as described above based on a sequence of policy choices. The trade-offs between economic outcomes are well known prior to COVID, and direct [19] as well as indirect [20] relationships have been exhibited between health and economic outcomes. A typical approach to modeling these trade-offs is through modeling utility using equivalence scales [21]; this is the approach we adopt, as described in detail below.

The final requirement for our optimization set up to be well-defined is a definition of a utility function that can model the impact of both the economic and epidemiological projections. We take an approach similar to a recent Stanford study that examined the trade-off between mortality and consumption [22], and make some adjustments to the formulation presented in this study to adapt it better to the Canadian situation, as well as incorporate some additional assumptions to make our model more realistic.

First, we define the contribution to our utility function from unemployment and number of cases as U_e and U_c, respectively. We consider the contribution from case load by estimating the average number of life expectancy years lost per case; we also use the fact that, based on US EPA numbers, people are willing to trade of $\nu = 6$ years of consumption for a year of life expectancy. As such, the case load contribution to our utility function is defined as:

$$U_c(t) = \nu * C_t * (\Delta LE_{morbidity} + r_{mortality} * \Delta LE_{mortality}).$$

We use the following estimates to quantify the case load contribution:

- $\nu = 6$
- The average life expectancy loss due to mortality $LE_{mortality} = 14$
- Average mortality $r_{mortality}$ of 0.8% per case, as per [1]
- As accurate estimates for a morbidity impact are lacking [23], we take the average life expectancy loss due to morbidity per case as 1 year (this is approximately equal to the mortality impact in our model)
- C_t is calculated using the series of projections $C_t = F_c(A_{t-1}, C_{t-1})$ up to time T

We assume a similar trade-off between unemployment and mortality as with consumption, and take the unemployment contribution to the utility function directly as:

$$U_e(t) = E_t * Population,$$

Where E_t is again computed using our our series of projections $E_t = F_e(A_{t-1}, E_{t-1})$. Our final utility function U is then taken simply as:

$$U(T) = \sum_{t=1}^{T} [U_e(t) + U_c(t)]$$

Covid Economics 50, 25 September 2020: 101-112

Covid Economics 50, 25 September 2020: 101-112

There are multiple ways to frame this problem; one approach is to frame it as a utility maximization problem, e.g. as a Mixed Integer Problem (MIP), where the sequences of k binary or continuous choices are viewed as a $T \times k$ matrix, and a global utility optimizing solution is found. However, the global optimization approach carries several disadvantages, namely computational constraints, as well as potential inflexibility in adding various constraints and sources of uncertainty depending on the specific optimization framework chosen. As such, we opt for a stochastic control formulation as a more natural way to frame the optimization problem. The formulation and some of its key advantages is described below.

4 Stochastic Control Formulation

We design our optimization approach to be model-agnostic in the sense that the optimization format is well-formulated regardless of the form of the forecast functions F_c and F_e, as long as the assumptions described above are satisfied. To the extent that is the case, we can then then compute our expected utility recursively (and along the entire path) as:

$$U(T) = \sum_{t=0}^{T-1} [U_e(F_{e,t+1}(A_t, F_{e,t})) + U_c(F_{c,t+1}(A_t, C_t))]$$

We are now ready to formulate the optimization problem as a stochastic control problem, and propose a solution using reinforcement learning. Such an approach has the advantage of being solvable using standard deep learning tools [24], and such approaches have demonstrated many successes in recent years [25], [26]. At every step t we compute a value function $\pi : A \to \mathbb{R}$, such that π maximizes a learned value-function Q at each timestep, and $Q(t, A_{t+1}, F_{c,t}, E_{c,t})$ gives an estimated value of the action at each step t based on the current projections and all possible actions in A_t. The goal is to learn the value of Q at each time step as an expected contribution to terminal utility $U(T)$, and then use π to compute the optimal path. That is, the optimal sequence of actions is computed using π as:

$$\pi_{t+1} = \arg\max_{A_{t+1}} Q(t, A_{t+1}, F_{c,t}, E_{c,t})$$

This gives us all the ingredients needed to frame this problem as a reinforcement learning (RL) problem [27]. We created a simulation environment to enable this task, and share our immediate results in further sections, as we find our initial Monte-Carlo simulations in and of themselves extremely insightful. We also make public the simulation environment we showcase in detail in subsequent sections.

5 Simulation Environment Implementation

To enable the integrative reinforcement learning approach, we created a simulation environment [3] that can be used for agent training, but also has immediate standalone utility as a way to elucidate the dynamics of the integrative model. As described, the simulation environment aligns economic and epidemiological forecast, together with a sequence of mitigation actions, and allows us to simulate the disease spread, progression of economic conditions, and ultimate utility based on various scenarios of given mitigation actions.

The actions we model in our simulation environment are reflective of 5 key groupings of government policies: Closing Office Buildings, Limiting Mass Gatherings, Mask Mandates, School Closings, and Retail and Restaurant Shutdown. The calibration of these models (described below), as well as their alignment to our proposed actions (described in the Materials and Methods section), allows us to analyze a large number of simulated scenarios based on sequences (A_t) of these actions.

5.1 Epidemiological Autoregressive Forecast

We use the modelling framework of COVID-19 transmission developed in the work of Jianhong Wu's research group [1].

[3] Source code is provided at https://github.com/ychaochen/Optimize_COVID_19_Mitigation_Strategies

Covid Economics 50, 25 September 2020: 101-112

The population is divided into susceptible (S), exposed (E), asymptomatic infectious (A), infectious with symptoms (I), and recovered (R) compartments according to the epidemiological status of individuals. We also include diagnosed and isolated (D), isolated susceptible (S_q), and isolated exposed (E_q) compartments based on control interventions. In this framework q denotes the quarantine fraction, i.e. the proportion of individuals exposed to the virus are traced and isolated. The quarantined individuals can either move to the compartment E_q or S_q, depending on whether transmission occurred (with probability β), while the other proportion $1-q$ moves to the exposed compartment E once infected, or stay in the compartment S otherwise.

We assume the following dynamics as per [1], with parameter values given in Table 1:

$$\begin{aligned}
S' &= -(\beta c + cq(1-\beta))S(I+\theta A)/N + \lambda S_q \\
E' &= \beta c(1-q)S(I+\theta A)/N - \sigma E \\
I' &= \sigma \varrho E - (\delta_I + Alpha + \gamma_I)\, I \\
A' &= \sigma(1-\varrho)E - \gamma_A A \\
S_q' &= (1-\beta)cqS(I+\theta A)/N - \lambda S_q \\
E_q' &= \beta cqS(I+\theta A)/N - \delta_q E_q \\
D' &= \delta_I I + \delta_q E_q - (Alpha + \gamma_D)\, D \\
R' &= \gamma_I I + \gamma_A A + \gamma_D D
\end{aligned}$$

Throughout the time horizon of policy making, we assume that the mitigation actions $\mathcal{A}_t$ affect the contact rate $c(A)$ and transmission probability $\beta(A)$, while the other parameters in the epidemiological model are held constant. $c(A)$ and $\beta(A)$ are calculated at each time step t based on the mitigation actions currently in place; our approach to creating reasonable estimates for these functions can be found in the appendix.

5.2 Economic Forecast

The unemployment forecast models the impacts of job loss in Canada across industries due to COVID-19. We gathered data from StatsCan, in association with market estimates to find the aggregate impacts that each government policy had on unemployment since February 2020. We describe our process for estimating the unemployment rate as a function of possible actions A_t below.

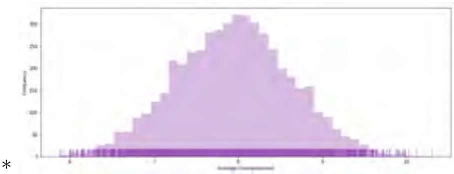
As a first step, we utilized StatsCan economic data on unemployment from months February to June of 2020 to calculate the difference in employment between pre-pandemic projections and post-pandemic actuals.

We then proceeded to align these estimates to the mitigation actions we consider. We did this by first estimating the impact of each mitigation action on unemployment in each specific sector, and then re-aggregating the sector impacts to generate a single Ontario-level estimate of the change in unemployment due to each individual action a_i. The details for these calculations are provided in Table 2, and the final estimates used are detailed in Table 3.

The motivation behind this model was to demonstrate the distinct impact that each policy had on unemployment since February 2020. The policies implemented by the Government of Canada to reduce contact rates and flatten the COVID-19 curve had strong effects on the employment market. In the model we highlight the direct impacts that each policy had individually, to gain a better perspective of which policies were more destructive than others.

5.3 Scenario Exploration

In order to validate our simulation environment and generate immediately actionable insight into our problem space, we performed 10,000 Monte-Carlo simulations over randomly generated possible sequences of actions. We began sampling our policies from an initial mitigation action vector of $(1, 1, 1, 0, 0)$, corresponding to Mask Mandates, Office Building and Public Gathering restrictions being in place, and School and Restaurant



*

Figure 1: Distribution of Average Unemployment Rate %

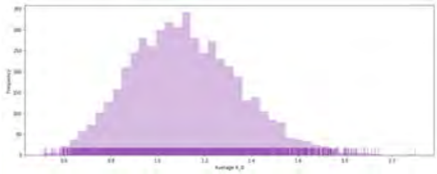


Figure 2: Distribution of Average Unemployment Rate %

Figure 3: Visualizing the Efficient Frontier of Mitigation Actions

/ Retail restrictions being relaxed. We permuted each of the action choices randomly every 14 days, keeping the restriction unchanged with a probability of 15%, leaving the action unchanged 85% of the time. We did this to make sure we were sampling over "reasonable" policy choices.

One of our goals in creating the simulation environment and defining the probability distribution of actions A_t sampled at each time-step was to ensure that we were exploring a wide range of possible scenarios. Scenarios on the extreme ends of mitigation have been explored [28] [29], and our aim was to develop a solid understanding of the dynamics of our problem over a similarly wide range of scenarios.

Figure 1 shows the distribution of average unemployment over the 365-day simulations; the upper and lower bounds of the distribution shows that we were exploring a wide range of economic scenarios, with extreme scenarios (high or low unemployment) being less frequent. Likewise, Figure 2 shows we cover a wide range of case load progression scenarios; some scenarios keeping R_{eff} consistently below 1, while other simulate less restrictive policies

5.4 Efficient Frontier of Mitigation Actions

Given the extreme time sensitivity for developing better tools for modeling the impact of mitigation actions, one of the immediate outcomes of our research was to understand how close to an optimal solution we can get with our MCMC-like sampling approach.

By plotting the average unemployment against cumulative case load at time T for each simulation (Figure 3), we see a pattern reminiscent of the efficient frontier in Markowitz portfolio theory. Under the assumptions described in our paper, the most efficient solutions in the order of magnitude of 10^4 cumulative additional cases

Covid Economics 50, 25 September 2020: 101-112

Covid Economics 50, 25 September 2020: 101-112

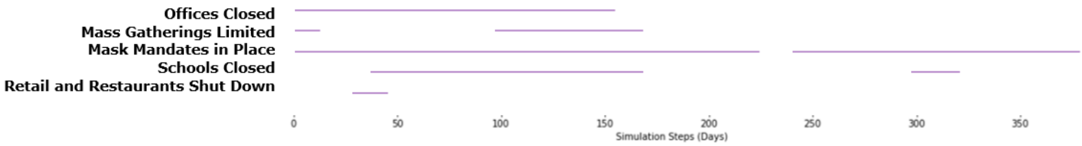


Figure 4: An Efficient Sequence of Mitigation Actions

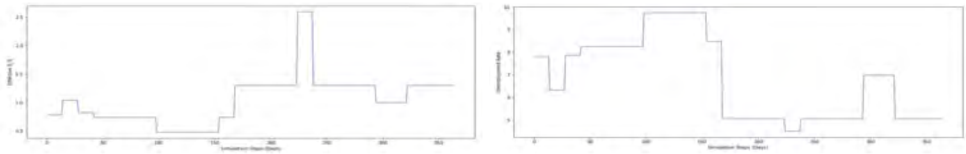


Figure 5: Unemployment Rate and R_{eff} Progression for an Efficient Scenario

are within approximately the 7% average unemployment range; though refining the simulation environment as described in the Next Steps section will improve our estimates, we believe the approach to analyzing the inherent trade-off to be sound. There is a wide distribution in how *efficient* scenarios are, in the sense that some showcase a much better trade off between case load and economic cost. This is intuitive given our previous discussion on the varying cost-efficiency of interventions, and the relative cost-effectiveness of temporary restrictions. We also demonstrate that there appears to be a phenomenon similar to the efficient frontier found in portfolio theory, with a boundary of efficient scenarios; as with portfolio theory, the choice of the *best* scenario is dependent on the choice of utility function. Lastly, although we do find some scenarios that are much more *efficient* than others (highlighted in blue in Figure 3), there is also significant white-space left around some of the best scenarios. This points to the benefit of going beyond the simulation in looking for an optimal solution, again outlined in the Next Steps section.

5.5 An Efficient Scenario

Figure 4 showcases the progression of the best scenario found through our runs. The mitigation actions are visualized as horizontal lines: the restrictions associated to each action are in place whenever the line is drawn. These results do not construe a recommendation, but rather a way to demonstrate the dynamics of a possible learned solution under a set of simplified assumptions. We expect the form of a "best" solution will change significantly as our simulation environment becomes increasingly refined, as outlined in the "Next Steps" section.

Figure 4 shows the sequence of government actions that resulted in the lowest utility given our inputs (described in the Materials and Methods) and chosen formulation of utility function. The scenario described keeps Office Buildings closed the first 154 days of the simulation, limits Mass Gatherings for days 1-14 and 98-168, schools closed for days 42-168, and then again for days 294-322. Mask mandates are in place continuously with the exception of a small break for days 224-238; this corresponds to the peak in R_{eff} during this time that can be seen in Exhibit 5, while retail in restaurants restrictions are lifted with the exception of days 28-42.

As expected given our formulation of forecasting functions F_c and F_e, effective R_{eff} and Unemployment % are effectively mirrored in each time step, as can be seen in Figure 5. This still creates complex dynamics for the trade-off between case load progression and economic costs, as relaxing policies for a short period of time is relatively inexpensive as long as effective R_{eff} is subsequently brought down in a timely manner.

There is a number of interesting observations to be made from the qualities of this scenario. Firstly, there are two decisions that appear strongly favored: mask mandates being in place, and retail and restaurant restrictions being relaxed. This is an indication of better economic trade-offs for the mitigation potential these actions provide, which could change as our forecasting models are refined further or if different assumptions are used (see Materials and Methods). Relaxing policies is also favored near the end of the simulation, as there is not enough time for the case exponential growth to case load to become overwhelming. An improvement to account for this may be to run the simulation for variable time periods, or at least long enough where mass production of a vaccine becomes highly likely. In addition, the best sequence of policy choices we found relaxes and restores restrictions intermittently. Of course, a more realistic assumption would be to include implementation costs for each action to discourage rapid oscillations. We discuss this modification and other possible improvements in the Next Steps section.

6 Next Steps

The next steps we propose for this work our two-fold. We first consider some possible improvements to the simulation environment. To encourage the academic community to help increase the realism of the dynamics modeled, we also make our initial simulation environment publicly available. The second set of improvements we propose are in numerical optimization approaches, particularly those that learn defensive strategies under various kinds of uncertainty.

The three primary avenues of improvement to our simulation environment are in enhancing the epidemiological and economic forecast models, adding additional kinds of uncertainty (e.g. forecast uncertainty or timing of a potential vaccine), as well as adding additional costs and constraints (such as refining the formulation of the utility function, and adding switching costs for changing restrictions). In addition, special care will need to be given to modeling the complexities of long-term social [30], [14] and economic [31] consequences, and accounting for risks such as unforeseen economic shocks [32]. Our environment is scalable to make the changes required for these considerations, and it is our hope that by pooling more academic and government expertise we can rapidly add these enhancements.

The simulation environment we created is useful as a standalone simulation tool for exploring various scenarios and integrative model dynamics, but also as a reinforcement learning training environment. The reinforcement learning approach to integrative optimization has several advantages:

- Robust to incorporating sources of uncertainty, e.g. by adding random error to the predictions made by the epidemiological SEIR models or economic forecasts
- Computationally scalable: stochastic gradient descent training for the reinforcement learning agent can allow us to freely scale up the complexity and hence the realism of the simulation environment
- Learns defensive strategies (i.e. strategies that work well under different ranges of estimates for R_{eff}, forecasting model errors, vaccine timing, etc.)

As we continue working on enhancing the modeling environment and improving the optimization approaches, our main goal is to encourage additional cross-disciplinary collaboration, as no individual field of study has the completeness of expertise necessary to solve these complex problems. We hope that the work put forward here can accelerate the efforts towards integrating the epidemiological, economic, and social perspectives both in Canada and abroad.

Our model relies on two primary sets of input data the epidemiological forecast F_c, and the economic forecast, F_e. The economic impact of mitigating actions is estimated using historical unemployment data as collected by StatsCan, and the epidemiological model is modeled as a SEIR model, with transmission rates depending on each action A by estimating the contact rate $c(A)$ and transmission probability $\beta(A)$

6.1 Epidemiological Model Constants

Table 1 constants were used for the base SEIR model in this paper, as an adaptation of the model postulated in [1].

6.2 Estimating c(A) and $\beta(A)$

We make use of external research and bottom-up analysis to estimate how each of the mitigation actions A_t we considered impacts the average contact rate c and average transmission probability β. As a simplifying

Covid Economics 50, 25 September 2020: 101-112

CEPR PRESS

Covid Economics 50, 25 September 2020: 101-112

Table 1: Parameters of Epidemiological Model

Parameters	Definition	Value	Source
$c(A)$	Policy specific contact rate	see Section 7.2	estimated
q	Quarantine fraction	0.3721	cited
$\beta(A)$	Policy specific probability of Transmission per contact	see Section 7.2	estimated
σ	Transition rate of exposed individuals to the infected class	1/5	cited
λ	Rate at which the quarantined uninfected contacts were released into the wider community	1/14	cited
ρ	Probability of having symptoms among infected individuals	0.7036	cited
δ_I	Transition rate of symptomatic infected individuals to the quarantined infected class	0.1344	cited
δ_q	Transition rate of the quarantined exposed individuals to the quarantined infected class	0.1237	cited
γ_I	Recovery rate of symptomatic infected individuals	0.1957	cited
γ_A	Recovery rate of asymptomatic infected individuals	0.139	cited
γ_D	Recovery rate of quarantined diagnosed individuals	0.2	cited
$Alpha$	Disease-induced death rate	0.008	cited
θ	Modification factor of asymptomatic infectiousness	0.0275	cited

assumption, we assume that β is impacted by Mask Mandates only, while c is impacted by the other 4 possible actions: School Closures, Restaurant and Retail Closures, Office Building Closures, and Limiting Mass Gatherings.

Given various studies show the effective transmission rate reduction provided by masks of up to 79% [33], we reduce β by 50% provided the Mask Mandates action is in place, to account for partial compliance.

In terms of reducing c based on each mitigation action in place, we take out baseline rate of contacts at $c_{total} = 15$, where no preventative measures are in place. We then estimate the reduction in c from each of the four remaining actions, and utilize the following contact rate change per mitigating action:

- School Closures: reduction of 3.5 contacts / day
- Restaurant and Retail Closures: reduction of 3 contacts / day
- Office Building Closures: reduction of 2.5 contacts / day
- Limiting Mass Gatherings: reduction of 3 contacts / day

These estimates were made by estimating the total number of contacts associated with their respective activities, under usual circumstances. For example, to estimate the number of contacts due to school and university attendance, we consider sub-populations of Ontarians such as school students, university students, teachers, etc. and estimate how many extra contacts per day arise due to these activities. With these estimates, we have a minimum effective R_{eff} of 0.3 with all preventative actions in place, and a maximum effective R_{eff} of 2.6 with all actions relaxed.

6.3 Estimating $F_e(A)$

We take a similar "bottom-up" approach to estimating the economic impact $F_e(A)$. we make a simplifying assumption of considering the impact of each set of action only at time t, i.e. not considering any autoregressive or momentum effects for our economic model. In order to determine the overall impact of each action on unemployment, we proceed in 3 key steps. To start, we estimate the overall impact from strict lockdown by looking at the overall unemployment differential between March and June data, based on StatsCan data. We then estimate the relative impact of each set of actions on industry as provided via the StatsCan industry classification, as can be seen in Table 2. The numbers in the table represent the relative weights used in dis-aggregating the unemployment impact each set of actions had on aggregate unemployment.

Table 2: Impact of Mitigation Actions on Unemployment

Industry	Closing Office Buildings	Limiting Mass Gatherings	Mask Mandates	School Closings	Retail and Restaurants Shutdown
Agriculture	0.1	0.1	0.1	0.3	0.4
Forestry, fishing, mining	0.1	0.25	0.1	0.25	0.3
Utilities	0.25	0.2	0.1	0.3	0.15
Construction	0.25	0.1	0.1	0.4	0.15
Durables	0.2	0.2	0.2	0.2	0.2
Non-durables	0.2	0.2	0.2	0.2	0.2
Wholesale trade	0.2	0.15	0.05	0.3	0.3
Retail trade	0.1	0.2	0.05	0.25	0.4
Transportation and warehousing	0.2	0.25	0.05	0.3	0.2
Finance and insurance	0.4	0.1	0.05	0.3	0.15
Real estate and rental and leasing	0.45	0.25	0.1	0.1	0.1
Professional, scientific and technical services	0.45	0.1	0.05	0.25	0.15
Business, building and other support services	0.4	0.1	0.05	0.25	0.2
Educational services	0.15	0.2	0.1	0.45	0.1
Health care and social assistance	0.2	0.2	0.2	0.3	0.1
Information, culture and recreation	0.2	0.4	0.1	0.2	0.1
Accommodation and food services	0.1	0.2	0.05	0.3	0.35
Other services (except public administration)	0.1	0.3	0	0.3	0.3
Public administration	0.25	0.25	0.1	0.3	0.1
Unclassified Unemployment	0.25	0.25	0	0.25	0.25

Lastly, we apply these weights to the unemployment impact in each industry to recover the estimated overall unemployment contribution of each set of actions. The total contributions used for each action are summarized in Table 3.

Table 3: Unemployment Impact Estimates per Mitigating Action

Mitigating Action	Unemployment Rate Impact Estimate
School Closures	1.93%
Restaurant and Retail Closures	1.54%
Office Building Closures	1.28%
Limiting Mass Gatherings	1.50%
Mask Mandates	0.55%
Total	**6.80%**

Covid Economics 50, 25 September 2020: 101-112

References

[1] B. Tang, F. Scarabel, N. L. Bragazzi, Z. McCarthy, M. Glazer, Y. Xiao, J. M. Heffernan, A. Asgary, N. H. Ogden, and J. Wu, "De-escalation by reversing the escalation with a stronger synergistic package of contact tracing, quarantine, isolation and personal protection: Feasibility of preventing a COVID-19 rebound in Ontario, Canada, as a case study," *Biology*, vol. 9, 5 2020.

[2] S. He, Y. Peng, and K. Sun, "SEIR modeling of the COVID-19 and its dynamics," *Nonlinear Dynamics*, 06 2020.

[3] S. Sengupta, S. Mugde, and G. Sharma, "COVID-19 pandemic data analysis and forecasting using machine learning algorithms," *medRxiv*, 2020.

[4] D. Acemoglu, V. Chernozhukov, I. Werning, and M. D. Whinston, "Optimal targeted lockdowns in a multi-group SIR model," Working Paper 27102, National Bureau of Economic Research, May 2020.

[5] A. R. Tuite, D. N. Fisman, and A. L. Greer, "Mathematical modelling of covid-19 transmission and mitigation strategies in the population of Ontario, Canada," *CMAJ*, vol. 192, no. 19, pp. E497–E505, 2020.

[6] N. H. Ogden, A. Fazil, J. Arino, P. Berthiaume, D. N. Fisman, G. A. L., A. Ludwig, A. R. Ng, Victoria Tuite, P. Turgeon, L. A. Waddell, and W. Jianhong, "Modelling scenarios of the epidemic of COVID-19 in Canada," *The Canada Communicable Disease Report*, vol. 46-6, 06 2020.

[7] C. J. Jones, T. Philippon, and V. Venkateswaran, "Optimal mitigation policies in a pandemic: Social distancing and working from home," Working Paper 26984, National Bureau of Economic Research, April 2020.

[8] A. Martin, M. Markhvida, S. Hallegatte, and B. Walsh, "Socio-economic impacts of COVID-19 on household consumption and poverty," *Economics of Disasters and Climate Change*, 06 2020.

[9] R. Abouk and B. Heydari, "The immediate effect of COVID-19 policies on social distancing behavior in the United States," *medRxiv*, 2020.

[10] V. Guerrieri, G. Lorenzoni, L. Straub, and I. Werning, "Macroeconomic implications of covid-19: Can negative supply shocks cause demand shortages?," Working Paper 26918, National Bureau of Economic Research, April 2020.

[11] A. Agarwal, A. Alomar, A. Sarker, D. Shah, D. Shen, and C. Yang, "Two burning questions on COVID-19: Did shutting down the economy help? can we (partially) reopen the economy without risking the second wave?." arXiv:2005.00072, 2020.

[12] J. Huang, H. Wang, M. Fan, A. Zhuo, Y. Sun, and Y. Li, "Understanding the impact of the COVID-19 pandemic on transportation-related behaviors with human mobility data," in *Proceedings of the 26th ACM SIGKDD International Conference on Knowledge Discovery Data Mining*, KDD '20, (New York, NY, USA), p. 3443–3450, Association for Computing Machinery, 2020.

[13] S. Flaxman, S. Mishra, A. Gandy, H. J. T. Unwin, T. A. Mellan, H. Coupland, C. Whittaker, H. Zhu, T. Berah, J. W. Eaton, M. Monod, P. N. Perez-Guzman, N. Schmit, L. Cilloni, K. E. C. Ainslie, M. Baguelin, A. Boonyasiri, O. Boyd, L. Cattarino, L. V. Cooper, Z. Cucunubá, G. Cuomo-Dannenburg, A. Dighe, B. Djaafara, I. Dorigatti, S. L. van Elsland, R. G. FitzJohn, K. A. M. Gaythorpe, L. Geidelberg, N. C. Grassly, W. D. Green, T. Hallett, A. Hamlet, W. Hinsley, B. Jeffrey, E. Knock, D. J. Laydon, G. Nedjati-Gilani, P. Nouvellet, K. V. Parag, I. Siveroni, H. A. Thompson, R. Verity, E. Volz, C. E. Walters, H. Wang, Y. Wang, O. J. Watson, P. Winskill, X. Xi, P. G. T. Walker, A. C. Ghani, C. A. Donnelly, S. Riley, M. A. C. Vollmer, N. M. Ferguson, L. C. Okell, S. Bhatt, and C.-R. T. Imperial College, "Estimating the effects of non-pharmaceutical interventions on COVID-19 in Europe," *Nature*, vol. 584, pp. 257–261, 06 2020.

[14] G. Bonaccorsi, F. Pierri, M. Cinelli, A. Flori, A. Galeazzi, F. Porcelli, A. L. Schmidt, C. M. Valensise, A. Scala, W. Quattrociocchi, and F. Pammolli, "Economic and social consequences of human mobility restrictions under covid-19," *Proceedings of the National Academy of Sciences*, vol. 117, no. 27, pp. 15530–15535, 2020.

[15] L. Alfaro, E. Faia, N. Lamersdorf, and F. Saidi, "Social interactions in pandemics: Fear, altruism, and reciprocity," Working Paper 27134, National Bureau of Economic Research, May 2020.

[16] S. Aum, S. Y. T. Lee, and Y. Shin, "Inequality of fear and self-quarantine: Is there a trade-off between GDP and public health?," *Covid Economics*, vol. 14, pp. 143–147, 2020.

Covid Economics 50, 25 September 2020: 101-112

[17] M. G. Bellemare, Y. Naddaf, J. Veness, and M. Bowling, "The arcade learning environment: An evaluation platform for general agents," *Journal of Artificial Intelligence Research*, vol. 47, p. 253–279, Jun 2013.

[18] C. Beattie, J. Z. Leibo, D. Teplyashin, T. Ward, M. Wainwright, H. Küttler, A. Lefrancq, S. Green, V. Valdés, A. Sadik, J. Schrittwieser, K. Anderson, S. York, M. Cant, A. Cain, A. Bolton, S. Gaffney, H. King, D. Hassabis, S. Legg, and S. Petersen, "DeepMind Lab." arXiv:1612.03801, 2016.

[19] F. Vahid Shahidi, C. Muntaner, K. Shankardass, C. Quiñonez, and A. Siddiqi, "Widening health inequalities between the employed and the unemployed: A decomposition of trends in canada (2000-2014)," *PLOS ONE*, vol. 13, pp. 1–22, 11 2018.

[20] M. Dilmaghani, "Financial unhealthiness predicts worse health outcomes: evidence from a sample of working Canadians," *Public Health*, vol. 144, pp. 32 – 41, 2017.

[21] L. Kools and M. Knoef, "Health and consumption preferences; estimating the health state dependence of utility using equivalence scales," *European Economic Review*, vol. 113, pp. 46–62, 04 2019.

[22] R. E. Hall and P. J. Jones, Charles I.and Klenow, "Trading off consumption and COVID-19 deaths," *Federal Reserve Bank of Minneapolis Quarterly Review*, vol. 42, 6 2020.

[23] M. A. Stoto and M. K. Wynia, "Assessing morbidity and mortality associated with the COVID-19 pandemic." Health Affairs Blog, June 25, 2020. DOI: 10.1377/hblog20200622.970112, 2020.

[24] A. Bachouch, C. Huré, N. Langrené, and H. Pham, "Deep neural networks algorithms for stochastic control problems on finite horizon: numerical applications." arXiv:1812.05916, 2018.

[25] D. Silver, T. Hubert, J. Schrittwieser, I. Antonoglou, M. Lai, A. Guez, M. Lanctot, L. Sifre, D. Kumaran, T. Graepel, T. P. Lillicrap, K. Simonyan, and D. Hassabis, "Mastering chess and shogi by self-play with a general reinforcement learning algorithm," *CoRR*, vol. abs/1712.01815, 2017.

[26] O. Vinyals, I. Babuschkin, W. M. Czarnecki, M. Mathieu, A. Dudzik, J. Chung, D. H. Choi, R. Powell, T. Ewalds, P. Georgiev, J. Oh, D. Horgan, M. Kroiss, I. Danihelka, A. Huang, L. Sifre, T. Cai, J. P. Agapiou, M. Jaderberg, A. S. Vezhnevets, R. Leblond, T. Pohlen, V. Dalibard, D. Budden, Y. Sulsky, J. Molloy, T. L. Paine, C. Gulcehre, Z. Wang, T. Pfaff, Y. Wu, R. Ring, D. Yogatama, D. Wünsch, K. McKinney, O. Smith, T. Schaul, T. Lillicrap, K. Kavukcuoglu, D. Hassabis, C. Apps, and D. Silver, "Grandmaster level in StarCraft II using multi-agent reinforcement learning," *Nature*, vol. 575, pp. 350–354, 11 2019.

[27] M. L. Littman, "Markov games as a framework for multi-agent reinforcement learning," in *Proceedings of the Eleventh International Conference on International Conference on Machine Learning*, ICML'94, (San Francisco, CA, USA), p. 157–163, Morgan Kaufmann Publishers Inc., 1994.

[28] S. Kaushal, A. S. Rajput, S. Bhattacharya, M. Vidyasagar, A. Kumar, M. K. Prakash, and S. Ansumali, "Estimating hidden asymptomatics, herd immunity threshold and lockdown effects using a COVID-19 specific model." arXiv:2006.00045, 2020.

[29] K. Kopecky and T. Zha, "Mitigation efforts versus herd immunity," *Federal Reserve Bank of Atlanta's Policy Hub*, 03 2020.

[30] N. Montemurro, "The emotional impact of COVID-19: From medical staff to common people," *Brain, Behavior, and Immunity*, vol. 87, pp. 23–24, 07 2020.

[31] S. R. Baker, N. Bloom, S. J. Davis, and S. J. Terry, "COVID-induced economic uncertainty," Working Paper 26983, National Bureau of Economic Research, April 2020.

[32] J. M. Barrero, N. Bloom, and S. J. Davis, "COVID-19 is also a reallocation shock," Working Paper 27137, National Bureau of Economic Research, May 2020.

[33] Y. Wang, H. Tian, L. Zhang, M. Zhang, D. Guo, W. Wu, X. Zhang, G. L. Kan, L. Jia, D. Huo, B. Liu, X. Wang, Y. Sun, Q. Wang, P. Yang, and C. R. MacIntyre, "Reduction of secondary transmission of SARS-CoV-2 in households by face mask use disinfection and social distancing: a cohort study in beijing, china," *BMJ Global Health*, vol. 5, no. 5, 2020.

Covid Economics 50, 25 September 2020: 101-112

Covid Economics Issue 50, 25 September 2020

The US housing market during COVID-19: Aggregate and distributional evidence[1]

Yunhui Zhao[2]

Date submitted: 23 September 2020; Date accepted: 24 September 2020

Covid Economics 50, 25 September 2020: 113-154

Using zip code-level data and nonparametric estimation, I present eight stylized facts on the US housing market in the COVID-19 era. Some aggregate results are: (1) growth rate of median housing price during the four months (April-August 2020) since the Federal Reserve's unprecedented monetary easing has accelerated faster than any four-month period in the lead-up to the 2007-09 global financial crisis; (2) the increase in housing demand in response to lower mortgage interest rates displays a structural break since March 2020 (housing demand has increased by much more than before). These results indicate either the existence of "fear of missing out" or COVID-induced fundamental changes in household behavior. In terms of distributional evidence, I find that the increase of housing demand seems more pronounced among the two ends of the income distribution, possibly reflecting relaxed liquidity constraints at the lower end and speculative demand at the higher end. I also find that the developments in housing price, demand, and supply since April 2020 are similar across urban, suburban, and rural areas. The paper highlights the potential unintended consequences of COVID-fighting policies and calls for further studies of the driving forces of the empirical findings.

1 I am grateful for the helpful discussions with Tim Christensen (NYU), Valefrie Cerra (all IMF, unless otherwise specified), Nigel Chalk, Kris Gerardi (Atlanta Fed), Deniz Igan, Heedon Kang, Divya Kirti, Vladimir Klyuev, Petya Koeva Brooks, Wojciech Maliszewski, Rui Mano, Junghwan Mok, Andrea Presbitero, Yannick Timmer, Daria Zakharova, and Jeromin Zettelmeyer, as well as for the research assistance provided by Jay Kaihao Cai (June 2020) and Yi Zhu (July-August 2020). The paper has incorporated helpful comments received at the IMF inter-departmental webinar in August 2020 and during the formal departmental review stage. The views expressed here are those of the author(s) and do not necessarily represent the views of the IMF, its Executive Board, or IMF management.

2 Economist, IMF.

I. Introduction

On March 3, 2020, the Fed cut the benchmark rate by 50 basis points to 1-1.25 percent; on March 15, 2020, in another bold action, the Fed cut the rate to 0-0.25 percent. On June 10, 2020, the "dot plot" indicated that the rate would be kept in the range of 0-0.25 percent through 2022 and Chairman Powell announced that the purchases of MBS would continue, triggering the 30-year fixed mortgage rate to hit a record low of 2.97%. On July 16, the rate dropped again and marked the seventh record low this year. At the same time, house prices have surged. According to the Case-Shiller National Home Price Index, real house prices rose 45 percent from February 2012 through May 2020, the latest data. Numerous news articles and anecdotal evidence suggest that the US housing market is experiencing a surprising boom. On July 31, Nobel laureate Robert Shiller remarked in *New York Times*: "*That is a remarkable record, considering that the United States is grappling with the coronavirus pandemic, a major recession and social upheaval*."

Against this backdrop, this paper presents some stylized facts on the US residential housing market during the COVID-19 crisis, using zip code-level data and nonparametric estimation. It aims to draw attention to some unintended aggregate and distributional effects of the COVID-fighting policies and serves as a first step for further studies, particularly if the preliminary evidence points to a build-up of the systemic risk. Indeed, one finding of the paper is that the year-on-year growth rate of the median price per square foot in the recent four months (April-August 2020) has accelerated *faster* than any four-month period in the lead-up to the 2007-09 global financial crisis. Moreover, this is the case across urban, suburban, and rural areas.

To understand these findings and their policy implications, it is useful to lay out the various driving forces of the housing market developments. On the one hand, COVID-19 is an unprecedented negative shock to households' incomes (both the current and the expected incomes due to massive layoffs), leading to a significant downward pressure on the housing price via the depressed demand. On the other hand, housing price can be higher during COVID-19 because of (a) higher housing demand induced by the central bank's unprecedented monetary easing, part of which is speculative housing demand ("fear of missing out") and part of which is the demand from previously liquidity-constrained low-income households (who were borrowing

Covid Economics 50, 25 September 2020: 113-154

up to the debt-service limits before the monetary easing)[2]; (b) lower housing supply due to stay-at-home orders (and sellers' reluctance to supply the houses out of COVID infection fears).. Hence, it remains unclear *a priori* which force would dominate.

These mechanisms can result in some unintended consequences. For example, excessively high mortgage borrowing and leverage may emerge, sowing *seeds* for the next crisis similar to the subprime mortgage crisis during 2007-09. This is particularly the case if it is the households with low or volatile incomes who have leveraged up more. In addition, inequality may worsen because those who buy houses and benefit from the ultra-easy monetary and financial conditions have high/stable incomes. Indeed, the median age of US homebuyers is 47, while the median age of people working in restaurants—a person-to-person contact service industry that suffered significant COVID-induced job losses—is 29, so a relatively small number of potential near-term homebuyers lost their jobs (*Financial Times*, June 22, 2020).

The rest of the paper is structured as follows. Section II reviews related literature; Section III describes the data and methodologies; Section IV presents the results, including the aggregate evidence, distributional evidence, and results after accounting for the urban status of the zip code, the composition change in housing supply, and the behavioral change of households; Section V concludes and discusses some policy implications. The appendix collects some technical details.

II. Literature Review

The paper contributes to four strands of literature. First, it contributes to a small emergent literature about the housing markets during COVID-19. Ling, Wang, and Zhou (2020) study the effect of COVID-19 on the US *commercial* real estate prices and find a negative relation between the growth of COVID-19 cases and the risk-adjusted return of firms' individual commercial property holdings. The first such study on the US *residential* real estate market is done by D'Lima, Lopez, and Pradhan (2020), who find no aggregate pricing effect but find a significant decrease in listings in the shutdown and re-opening periods. On the Chinese

[2] These two forces also reinforce each other: The higher housing prices caused by the speculative housing demand (particularly from high-income households) push up the overall housing price index in the country; this induces low-income households to also expect higher future housing prices and thus accelerate their house-purchase timelines (in an attempt to avoid buying houses at higher prices in the future).

Covid Economics 50, 25 September 2020: 113-154

residential real estate market, Bayoumi and Zhao (2020) document that the upward trend of the housing price has not been reversed by the COVID-19 shock, and highlight the role of a "slow-moving" structural vulnerability—financial market incompleteness—using a DSGE model.

This paper also studies the US residential real estate market, and complements D'Lima, Lopez, and Pradhan (2020) along four dimensions: (a) the former focuses on the effects of Fed's monetary easing rather than the government's shutdown policies as in the latter, and attempts to shed light on the unintended effects of monetary easing during any period of time, including a pandemic (which occurs infrequently); (b) the former focuses more on housing demand by making use of a novel indicator,[3] and the latter focuses more on housing supply; (c) the former also studies the distributional effect (across different *income groups*), complementing the latter (which finds differential effects across different *property types*); and finally (d) the former uses nonparametric estimation and structural break models, and the latter uses difference-in-difference models.

Second, the paper contributes to the economic history literature about the effects of health crises on housing markets. Ambrus, Field, and Gonzalez (2020) study the impact on housing prices of a cholera epidemic in a neighborhood of 19th-century London. They find that a decade after the epidemic, housing prices were significantly lower just inside the catchment area of the water pump that transmitted the disease, and that such differences persist over the following 160 years. Francke and Korevaar (2020) study the plague in Amsterdam and cholera in Paris between the late 16th century and 1811. They find large reductions in rents and house prices within the affected areas during the first six months of an epidemic; however, both cities quickly reverted to their initial price paths. More recently, Wong (2008) finds a 1-3 percent decrease in prices for properties in Hong Kong that were directly affected by the 2003 SARS epidemic.

Third, the paper contributes to an extensive literature on the aggregate and distributional effects of monetary easing and low interest rates. For example, on *aggregate* effects, Acharya and others (2011) point out the important role of low interest rates, relaxed lending standards,

[3] Relatedly, Bolhuis and Cramer (2020) study the impact of demographic trends on housing demand in the US. Their empirical analysis finds that since the housing trough in 2012, larger homes (and those in neighborhoods with a higher share of baby boomers) underperform substantially in terms of price growth, home sales, and liquidity. In the next two decades, as more and more baby boomers look to downsize, more than a quarter of the US homes occupied by their owners will likely hit the market (Romem, 2019).

Covid Economics 50, 25 September 2020: 113-154

CEPR PRESS

and government guarantees in the US mortgage market in driving the 2007-09 housing crisis.[4] McDonald and Stokes (2013) find that the Fed's low interest rate policy during 2001–04 was a cause of the subsequent housing price bubble, by applying Granger causality analysis to a panel dataset for 20 US cities during 1987-2010. By contrast, Merrouche and Nier (2010) (using cross-country analysis) and Dokko and others (2011) (using a common statistical approach) find that this is not the case and point to other factors such as inadequate regulation and excessive credit provision.

On *distributional* effects, Domanski, Scatigna, and Zabai (2016) find that changes in wealth inequality in five European countries and the US since 2009 are driven by movements in equity and house prices (which are in turn driven by monetary easing). Moreover, they find that house price increases alone tend to reduce wealth inequality, and equity price increases tend to increase it. Using a DSGE model of the euro area, Hohberger, Priftis, and Vogel (2020) find that expansionary conventional policy and QE shocks tend to mitigate income and wealth inequalities. And with a larger sample (a panel of 32 advanced and emerging market countries over the period 1990–2013), Furceri, Loungani, and Zdzienicka (2018) find that contractionary monetary policy shocks on average raise income inequality, and that the effect is asymmetric (policy tightening raises inequality more than easing reduces it) and depends on the state of the business cycle.

Finally, the paper contributes to a growing literature about the general economic effects of COVID-19 and government policies. Regarding *short-term aggregate* effects, Deb and others (2020) find that containment measures are associated with a very large decline of economic activity, amounting to a loss of about 15 percent in industrial production over a 30-day period; Chen and others (2020) find that deterioration of economic conditions preceded the introduction of non-pharmaceutical interventions (NPIs, such as social distancing) and a gradual recovery started before formal reopening. Regarding *medium-term aggregate* economic effects, Correia, Luck, and Verner (2020) analyze monthly data across US cities during the 1918 Flu Pandemic,

[4] Relatedly, several studies examine the excessive risk-taking induced by the US government's mortgage guarantee. Keys and others (2010), and Keys, Seru, and Vig (2012) study the *extensive* margin, that is, banks relax the lending criteria and extend mortgage loans to risky borrowers. Zhao (2019) and Zhao (2020) study the *intensive* margin, that is, banks raise the mortgage interest rate and/or the leverage ratio of the same borrower (for the same borrower, a higher interest rate and/or a higher leverage ratio means a larger repayment burden, and thus, a higher default risk).

Covid Economics 50, 25 September 2020: 113-154

and find that NPIs are associated with better economic outcomes in the medium term.[5] Regarding the *distributional* effects and the designs of the COVID-related government policies, Glover and others (2020) study optimal mitigation and redistribution policies, and find that the young who work in the sector partially shuttered would lose the most, especially when it is costly to soften the distributional consequences via public transfers; Furceri and others (2020) find that major epidemics in the last two decades have led to increases in multiple measures of inequality in affected countries, despite being much smaller in scale than COVID-19; Bronka, Collado, and Richiardi (2020) find that the UK's COVID rescue package would have a progressive effect and reduce poverty rate by 1.1 percentage points; Gerardi, Loewenstein, and Willen (2020) analyze the effects of a streamlined refinance program that allows borrowers to refinance without providing employment or income documents; Goodman and Magder (2020) discuss the renter direct payment program that aims at assisting renters who are disproportionally affected by the pandemic.[6]

III. Data, Summary Statistics, and Methodologies

A. Data Description

Since housing data in most countries are inadequate and infrequent, this study uses data from the US. But the results can shed light on other countries as well. Specifically, the housing data are from the zip code-level monthly panel databases released in the realtor.com library, covering most zip codes in the US from July 2017 to August 2020. Two databases are used, including the "realtor.com residential listings database" for median listing price per square foot, etc., and the "realtor.com market hotness index" for housing demand.[7] In particular, the housing demand (or

[5] Barro, Ursua and Weng (2020) also quantify the medium-to-long-term effects by analyzing annual data for 48 countries. They find that the 1918 Flu Pandemic lowered real GDP by 6-8% in the typical country, which is suggested to be the upper bound of the effects of COVID-19. Aum, Lee, and Shin (2020) study both short-term and medium-term effects, and find that a longer lockdown eventually mitigates the GDP loss as well as flattens the infection curve.

[6] Other studies on inequality and distributional effects include Chetty and others (2020), Schmitt-Grohé, Teoh, and Uribe (2020), Palomino, Rodríguez, and Sebastian (2020), and Galletta and Giommoni (2020). Some other studies on government's COVID interventions analyze the corporate credit channel, such as Acharya and Strafen (2020), Elenev, Landvoigt, and Van Nieuwerburgh (2020), and Greenwald, Krainer, and Paul (2020).

[7] No data on property types (single-family, condo, etc.) are available. However, since the study uses the *median* growth rates (across all zip codes) of the *median* price per square foot (across all properties in a given zip code), the lack of information on property types does not seem to be a major concern. The data

Covid Economics 50, 25 September 2020: 113-154

"hotness") used in the paper is the "demand score", measured as the online views per property, which is a credible and useful way to disentangle demand from the many other drivers of the housing price.

These data are merged with some other sources: first, the monthly mortgage rate data from the Primary Mortgage Market Survey, a frequently-used source provided by Freddie Mac— to be more representative, I use the rate for conventional, confirming 30-year fixed-rate mortgages, given that this is the most common product type in the US; second, the daily (national) data on effective federal funds rate (EFFR) from New York Fed, averaged to a monthly frequency; third, the latest American Community Survey (ACS)'s zip code-level median income (cross-sectional) data, which are the 2014-18 5-year estimates. According to the US Census Bureau, the 5-year estimates from the ACS are "period" estimates that represent data collected over a period of time, and "the primary advantage of using multiyear estimates is the increased statistical reliability of the data for less populated areas and small population subgroups."[8] These 5-year estimates are available for all geographical areas down to the block-group level. In total, over 578,000 geographical areas are covered. Chetty and others (2020) also use such data to track the economic activities during COVID-19 in their paper and real-time economic tracker.[9]

B. Summary Statistics

Table 1 summarizes the merged raw data for the key indicators. As expected, outliers exist for all the major housing indicators. For example, the (median) year-on-year growth rate of the (median) views per property (a measure of housing demand) can be as high as 4,248 percent, and even more extreme outliers exist for the growth rate of the median price per square foot.

The following steps are conducted to remove the outliers. First, I remove observations with a 500 percent or higher growth rate of property views. There are 102 such observations, but they are *disproportionally* distributed in June 2020 (10 percent), July (16 percent), and August (28 percent, left panel of Figure 1). Moreover, this is also the case for the 4,242 observations

---

and detailed descriptions for the data are available at https://www.realtor.com/research/data/. The residential listings database is also available at a weekly frequency, but the weekly data are at the national/metro levels only and have fewer indicators. Moreover, the hotness index data are only available at a monthly frequency. Hence, this study uses monthly data for both databases.

[8] See https://www.census.gov/data/developers/data-sets/acs-5year.html.

[9] See https://tracktherecovery.org/.

Covid Economics 50, 25 September 2020: 113-154

CEPR PRESS

with a 200-500 percent growth rate of property views (right panel of Figure 1): June 2020 accounts for 10 percent, July 2020 26 percent, and August 2020 46 percent. Since the scope of the paper is to analyze the *unusual* market behavior since March 2020, I keep all the observations whose growth rates of property views are below 500 percent.

Second, to be consistent with the treatment for demand, I remove observations with a 500 percent or higher growth rate of active listings, a measure of housing supply. 131 such observations are removed.

Third, for the key housing price variable (growth rate of median price per sqft), the 126 observations with a 500 or higher growth rate are distributed relatively evenly across months (left panel of Figure 2), so are the 412 observations with a 200-500 growth rate (right panel of Figure 2). Therefore, I drop these observations to obtain a dataset with more moderate price growth rates (without causing an obvious sample selection bias).

Table 1. Summary Statistics before Cleaning

Variable	Unit	N	Mean	Min	Median	Max	SD
zip	NA	525,606	NA	1,001	NA	99,925	NA
Month	NA	525,606	NA	201707	NA	202008	NA
PropertyViews_yy	percent	525,605	26	-96	18	4,248	47
Listing_yy	percent	467,583	-3	-87	-8	6,800	44
MedianP_sqft_yy	percent	490,215	77	-100	5	30e+6	42,783
MedianPrice_sqft	$	525,606	160	0	128	2,452	147
Median_sqft	NA	525,606	1,837	0	1,824	9,012	744
Median_sqft_yy	percent	490,215	2	-100	0	463,100	683
30Yr Fixed Mtg Rate	percent	525,606	4	3	4	5	1
MedianFamilyIncome	thousand $	523,200	79	10	71	249	32

Source: realtor.com; Freddie Mac; New York Fed; American Community Survey; Author's calculations.

Table 2. Summary Statistics after Cleaning

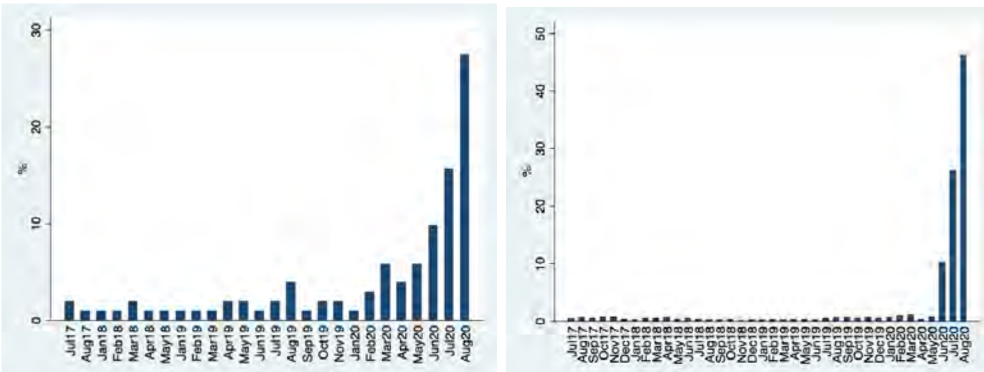
Variable	Unit	N	Mean	Min	Median	Max	SD
zip	NA	493,956	NA	1,001	NA	99,925	NA
Month	NA	493,956	NA	201707	NA	202008	NA
PropertyViews_yy	percent	493,956	26	-96	18	500	45
Listing_yy	percent	439,143	-4	-85	-8	500	30
MedianP_sqft_yy	percent	485,186	6	-98	5	200	15
MedianPrice_sqft	$	493,956	170	0	133	2,452	146
Median_sqft	NA	493,956	1,952	0	1,862	9,012	604
Median_sqft_yy	percent	485,186	2	-100	0	494.7	16
30Yr Fixed Mtg Rate	percent	493,956	4	3	4	5	1

Covid Economics 50, 25 September 2020: 113-154

MedianFamilyIncome	thousand $	491,766	78	10	71	249	31

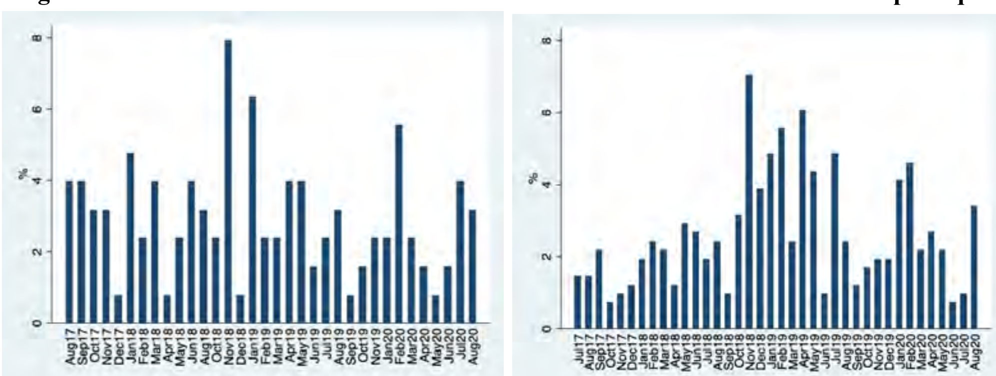
Source: realtor.com; Freddie Mac; New York Fed; American Community Survey; Author's calculations.

Figure 1. Distribution of Outliers across Months: Growth Rates of Property Views



Source: realtor.com; Author's calculations.

Figure 2. Distribution of Outliers across Months: Growth Rates of Median Price per Sqft



Source: realtor.com; Author's calculations.

Fourth, I drop the 12 observations with a 500 percent or higher grow rate of median square foot (evenly distributed across months), and some other obvious invalid observations, such as observations with 0 median price per sqft. The summary statistics after the cleaning are provided in Table 2. Note that I intentionally keep a relatively broad range for the median price per sqft and the median sqft variables; given that the analysis only involves the growth rates of these variables instead of their levels, doing so will unlikely bias the analysis and will make the analysis more comprehensive. The observations in the final sample are evenly distributed across

Covid Economics 50, 25 September 2020: 113-154

different months (Appendix Table 1), which eliminates the sample selection bias towards any particular month.

C. Methodologies

The paper uses a variety of approaches, including:

a. Descriptive analysis.

b. Zip code-level panel regressions. Importantly, the standard errors in all regressions are *clustered* at the month level. This is because the main right-hand-side variable (interest rate) only has time variation and no cross-sectional variation, which may cause correlation across zip codes and may artificially decrease standard errors. The clustering is done by implementing the widely-used estimator by Correia (2016), which augments the fixed-point iteration of Guimaraes and Portugal (2010).[10] For the same reason, I do not use the random-effect models, and only use the fixed-effect and pooled regressions.

c. Structural break models (with the Chow test) to see whether the pandemic and the unprecedented monetary easing in March 2020 triggered a structural break in housing demand.

d. The Nadaraya-Watson kernel nonparametric model to estimate the distributional effects. This class of models has the advantage of being model-free ("*let the data speak*") and is useful to discover the patterns in a flexible way.

e. With the zip code-level data, we can control for the composition change (hence quality change) of houses available in the market. This would allow us to separate the effects of supply disruptions and to improve the identification of the effects of interest rate on housing demand.

IV. RESULTS

This section presents the results, including five stylized facts on aggregate evidence, two facts on distributional evidence, and one fact after accounting for urban status. The results after

[10] The implementation is done by the reghdfe Stata package that runs linear and instrumental-variable regressions with many levels of fixed effects. More details can be found at http://scorreia.com/software/reghdfe/.

Covid Economics 50, 25 September 2020: 113-154

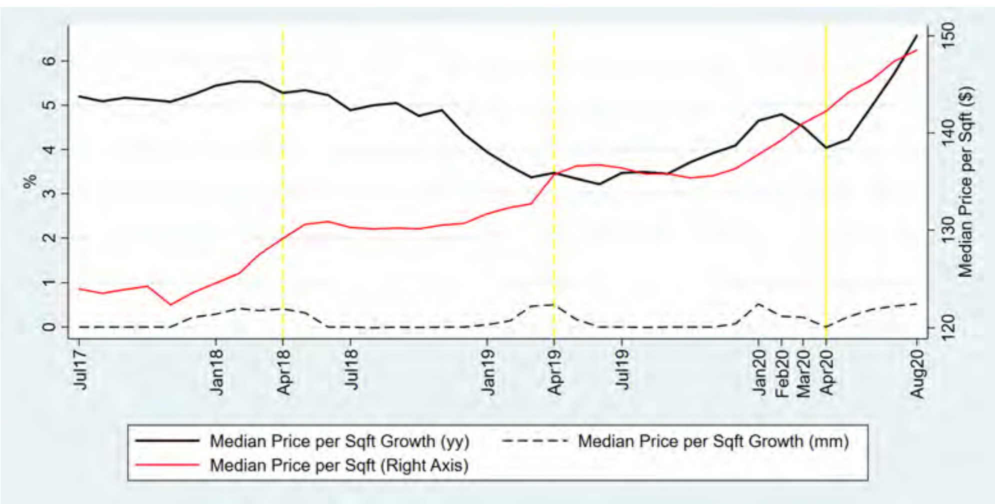
Covid Economics 50, 25 September 2020: 113-154

accounting for the potential changes in the composition of houses and buyers' behavioral changes are also presented.

A. Aggregate Evidence

Fact #1: After a temporary slow-down in March and April 2020, the median growth rate of the median housing price bounded back quickly and *exceeded* the growth rate before the COVID-19 crisis. As shown in Figure 3, the median growth rates (both year-on-year and month-on-month) of the median price per sqft temporarily slowed down in March 2020 (the yellow vertical solid line) and April 2020, but still remained positive. They then bounced back despite the worsening of the COVID crisis in the US: the year-on-year growth rates (the black solid line) accelerated to levels higher than that before the crisis (5.0 in June 2020, 5.7 in July, and 6.6 in August, all higher than the 4.8 in February 2020); and the month-on-month growth rates (the black dash line) after April 2020 have accelerated, even though they *decelerated* or remained flat after the same month in both 2018 and 2019 (the two yellow vertical dash lines). Some zip codes experienced a year-on-year growth of more than 30 percent in each month since March 2020.

Figure 3. Median Housing Price: July 2017 – August 2020



Source: realtor.com; Author's calculations.

Fact #2: The year-on-year growth rates of the median price per sqft in the recent four months (April-August 2020) have accelerated *faster* than any four-month period in the lead-up to the 2007-09 global financial crisis (GFC). As shown in the upper panel of Figure 4, the fastest four-month acceleration of the year-on-year growth rates of house prices in the lead-up to the GFC occurred from February 2004 to June 2004, when the year-on-year growth rates accelerated by 1.8 percentage points, from 8.1 percent year-on-year to 9.9 percent. However, the acceleration during the four-month period after the Fed's unprecedented COVID responses has been even faster (even though the growth rates themselves are lower during COVID than the pre-GFC): the year-on-year growth rates accelerated by 2.6 percentage points, from 4.0 percent in April 2020 to 6.6 percent in August 2020 (lower panel of Figure 4).

Another interesting observation is that after the significant interest rate cut in November 2002 (to 1.33 percent, from 1.76 percent in October 2002), it took about eight months for the house price growth rate to enter the steady acceleration phase, starting from July 2003. By contrast, after the interest rate cut in March 2020, it only took one month to enter the acceleration phase in April 2020. Given the rapidly deteriorating COVID outbreak at the same time, this phenomenon is worth exploring further.[11]

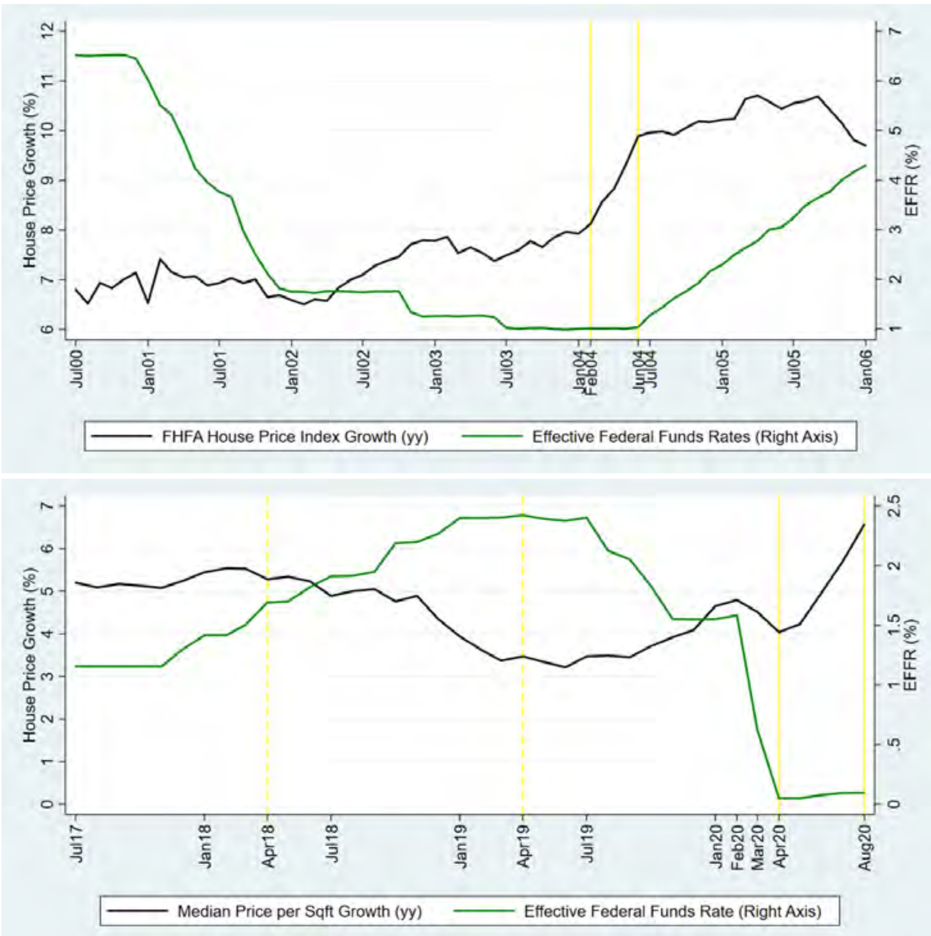
Fact #3: The decreasing trend of the housing supply since mid-2019 is amplified by COVID-19. As Figure 5 shows, the median year-on-year growth rate of the active listing (the solid purple line) has been negative since July 2019. Moreover, it has been dropping sharply since March 2020 due to the lockdown measures and possibly fears over the COVID-19 infections (sellers may be less willing to let buyers enter and view their houses). The month-on-month growth rates after April 2020 have become more and more negative, even though March-June in both 2018 and 2019 was a typical time when the month-on-month growth rates accelerated. This fact may have contributed to the rapid housing price growth since April 2020.

Fact #4: The hotness of housing demand, proxied by the online views per property, has been rising at an extraordinary rate since April 2020. As Figure 6 shows, the median views per property in a typical zip code rose by 99 percent in July 2020 and 121 percent in August 2020 (year-on-year), both of which are record highs since the data began in July 2017. The

[1111] Due to data constraints, the pre-GFC data are FHFA's house price index constructed from sales prices for the entire properties, as opposed to realtor.com's data on listing prices per sqft for the COVID period. Since the paper examines the year-on-year changes for both cases, this difference seems unlikely to bias the analysis.

month-on-month growth rates during March-June 2020 have accelerated, even though they *decelerated* during March-June in both 2018 and 2019 (the two yellow vertical dash lines).[12] [13] This fact may be another key contributor to the rapid housing price growth since April 2020.

Figure 4. Housing Price Growth: Comparison between GFC and COVID



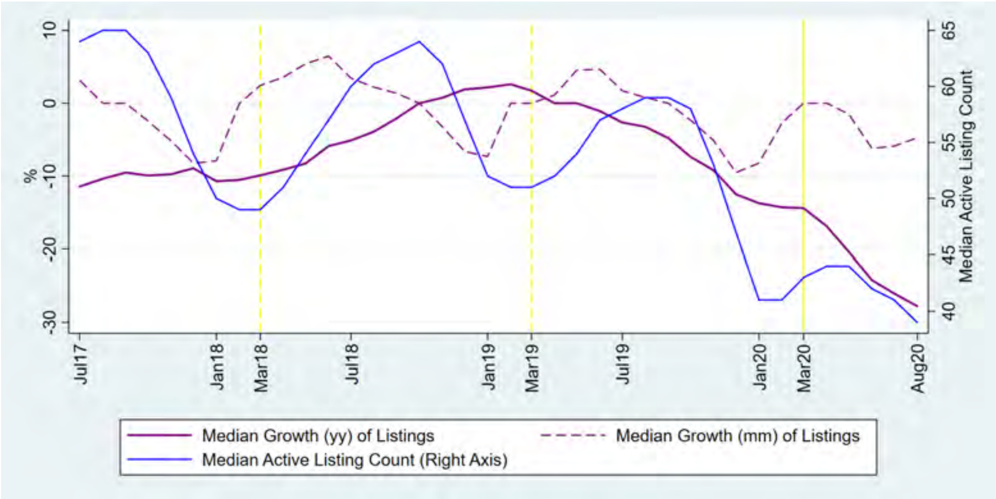
Source: FHFA; realtor.com; New York Fed; Author's calculations.

[12] The deceleration of the month-on-month growth rates during July-August 2020 is most likely due to seasonality because the same pattern was also observed in the same months of 2018 and 2019.

[13] Note that all the growth rates shown in the figures are the median growth across all zip codes, so they are not affected by the inclusion of observations with relatively high growth rates.

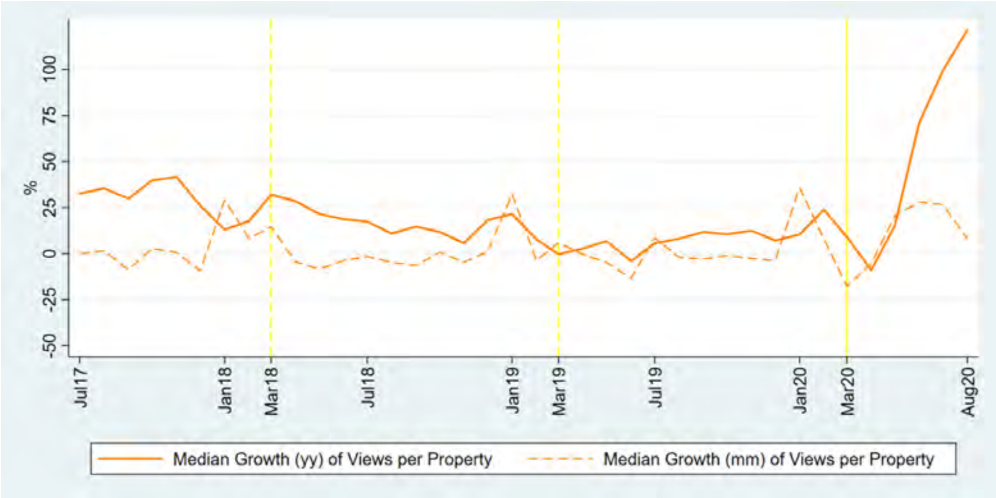
Covid Economics 50, 25 September 2020: 113-154

Figure 5. Housing Supply/Listings: July 2017 – August 2020



Source: realtor.com; Author's calculations.

Figure 6. Hotness of Housing Demand: July 2017 – August 2020



Source: realtor.com; Author's calculations.

Fact #5: The response of housing demand to mortgage rate displays a structural break, which is robust to falsification tests. Table 3 reports the results of zip code-level panel regressions, where the dependent variable is the (median) year-on-year growth rates of online

Covid Economics 50, 25 September 2020: 113-154

Covid Economics 50, 25 September 2020: 113-154

views per property (a measure of housing demand) and the independent variable is the mortgage rate. The mortgage rate rather than EFFR is used because the pass-through from EFFR to mortgage rate may be incomplete. In addition, mortgage rate can also reflect the Fed's *non*-interest rate easing, such as via MBS purchases and other quantitative easing measures, making it more suitable for studying the impact of the monetary easing comprehensively. As discussed earlier, since the interest rate variation comes from the time dimension, only fixed-effect and pooled regressions are reported; and all standard errors are clustered at the month level to account for the potential correlations among zip codes within the same month.

As shown in Table 3, across both the fixed-effect and pooled regressions, housing demand responds much more strongly to mortgage rate since March 2020, the month when the pandemic accelerated and when the Fed easing took place. From July 2017 to February 2020, the 30-year fixed mortgage rate variable is negatively associated with the year-on-year growth of online views per property (a measure of housing demand), but the effect is statistically insignificant, also suggesting that some other factors are omitted from this simple model. However, from March 2020 onwards, the coefficient of mortgage rate is both statistically and economically significant: as the 30-year fixed mortgage rate drops by 1 percentage point, the associated year-on-year growth of online views per property increases by about 267 percentage points. The Chow test is also strongly against the null hypothesis that there is no structural break.[14] As Table 4 shows, this pattern is not observed in the falsification tests that use March 2019 as the breakpoint. In particular, such tests rule out the possibility that the pattern is driven by seasonality.[15]

These results may appear extreme, but they are obtained after dropping some outliers of the housing demand measure and may be justifiable given that many other potential factors are not explicitly controlled for. Specifically, either or both of the following two scenarios are consistent with this Fact #5:

[14] Details of the Chow tests for the structural break models and the falsification tests, including the residual sums of squares and p-values, are available upon request.

[15] Note that it is complicated to compare the housing demand responses to interest rate estimated here with those in the literature because the latter uses housing value (or size) instead of online views to measure housing demand. One apparent challenge with that approach is that the observed housing value is an equilibrium object, which also reflects supply-side factors rather than "cleanly" measuring the underlying housing demand.

Scenario (a): Fear of Missing Out (FOMO). This suggests that potential homebuyers may indeed have responded more than usual to the historically low mortgage rate, possibly out of the fear that they may miss the golden opportunity of the ultra-low interest rate and the readily available financing on the market, as well as the fear that the housing price may go even higher if they wait any longer.

Scenario (b): Other COVID-induced fundamental changes in household behavior. These include more frequent online views due to the stay-at-home orders (to be discussed below)[16]; a preference shift that assigns a higher value to owning homes (as opposed to renting) or owning larger homes due to the expectation of, say, longer work-from-home arrangements; and a higher saving propensity due to depressed spending on non-housing consumption goods.

Relatedly, there are two sets of omitted variables from the simple regressions in Table 3. The first set includes local-level COVID case growth rates, unemployment rates, and measures of economic disruptions caused by COVID. Since these variables decrease housing demand and are high when interest rates are low (the Fed cut rates against the backdrop of high COVID cases and rising unemployment rates), controlling for them would require an even stronger response to interest rate and thus would reinforce the interest rate coefficients reported here. The second set of omitted variables includes subjective valuation of owning a house, saving rate, etc. Since these variables increase housing demand and also tend to be high in a low-interest rate environment, controlling for them would weaken the reported coefficients. Hence, the direction of the omitted variable bias is unclear.

Table 3. Panel Regressions for the Full Sample

	(1) Views-YY FE All	(2) Views-YY FE Until Feb	(3) Views-YY FE Since March	(4) Views-YY Pool All	(5) Views-YY Pool Until Feb	(6) Views-YY Pool Since March
FRM_30Yr	-22.927*	-1.007	-266.940***	-22.235*	-0.703	-264.443***
	(0.056)	(0.807)	(0.006)	(0.059)	(0.863)	(0.004)
Constant	117.286**	24.589	907.639***	114.530**	23.333	899.790***
	(0.023)	(0.177)	(0.004)	(0.024)	(0.194)	(0.003)
Observations	493,917	418,589	75,044	493,956	418,618	75,338

[16] This is also a form of a preference shift, which is a shift from in-person searching to online searching.

Covid Economics 50, 25 September 2020: 113-154

R-squared	0.201	0.220	0.701	0.062	0.000	0.387

Note: The dependent variable is the median year-on-year growth rates of hotness of housing demand; FRM_30Yr = 30-year fixed-rate mortgage; Standard errors are clustered at the month level, and *robust* p-values are in parentheses.
Source: Author's calculations.

Table 4. Falsification Tests: Panel Regressions for the Full Sample

	(1) Views-YY FE Until August 2019	(2) Views-YY FE Until Feb 2019	(3) Views-YY FE March-August 2019	(4) Views-YY Pool Until August 2019	(5) Views-YY Pool Until Feb 2019	(6) Views-YY Pool March-August 2019
FRM_30Yr	-5.199	-22.983***	-5.765	-5.052	-22.901***	-5.448
	(0.468)	(0.000)	(0.462)	(0.476)	(0.000)	(0.438)
Constant	43.482	125.605***	29.506	42.856	125.251***	28.299
	(0.181)	(0.000)	(0.365)	(0.182)	(0.000)	(0.336)
Observations	337,360	258,390	78,764	337,402	258,433	78,969
R-squared	0.267	0.372	0.621	0.003	0.045	0.002

Note: The dependent variable is the median year-on-year growth rates of hotness of housing demand; FRM_30Yr = 30-year fixed-rate mortgage; Standard errors are clustered at the month level, and *robust* p-values are in parentheses.
Source: Author's calculations.

B. Distributional Evidence

This subsection turns to distributional evidence and presents two stylized facts regarding the different responses across different income groups.

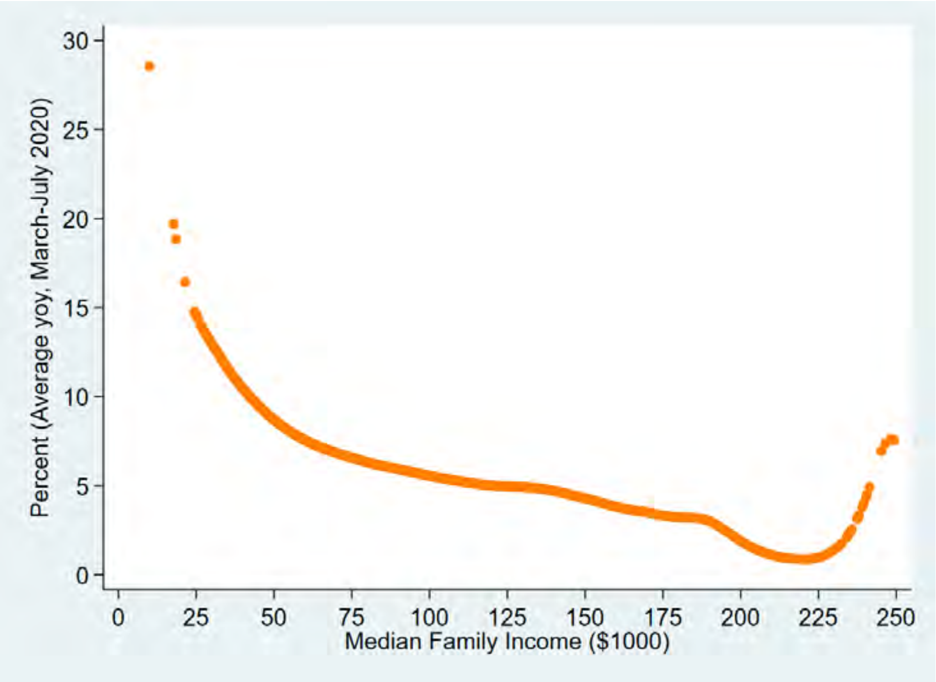
Fact #6: The increase in the housing price is particularly strong at the lower-to-middle end of the zip code-level income distribution. This pattern is shown in Figure 7, which plots the nonparametric estimates of the median growth rate (year-on-year) of the median price per sqft as a function of the median family income, during March-August 2020. Even if we disregard the segment for the extremely low income group (arguably driven by noise in the data), the year-on-year house price growth rate still decreases as we move up along the income distribution, from about 15 percent in zip codes with a median family income of $25,000 to close to 0 percent in zip codes with $225,000. It then increases again for the very rich zip codes, consistent with the subsequent fact. It is worth noting that this does not necessarily mean the overall income

Covid Economics 50, 25 September 2020: 113-154

Covid Economics 50, 25 September 2020: 113-154

inequality in the US is lower, because even within the low-income neighborhoods, it is the homeowners (who are relatively well-off and have stable incomes) who will likely benefit more from the low interest rates that also push up house prices.

Figure 7. Price per Sqft Growth vs Median Family Income: Nonparametric Estimation



Source: realtor.com; American Community Survey; Author's calculations.

Note that to mitigate the impact of time-specific factors and consistent with the paper's focus on the post-Fed action period, I first take the average of the "median growth rates of the median price per sqft" across March-August 2020 for each zip code, and then do nonparametric estimation.

Fact #7: The increase in the hotness of housing demand is particularly strong at the two ends of the zip code-level income distribution, displaying a U-shaped relationship. This pattern can be seen in Figure 8, which plots the nonparametric estimates of the median growth rate (year-on-year) of the median property views (a measure of housing demand) as a function of

Covid Economics 50, 25 September 2020: 113-154

the median family income, during March-August 2020.[17] If we again disregard the segment for the extremely low income group, the year-on-year growth rate of housing demand decreases as we move up along the income distribution, from around 58 percent in zip codes with a median family income of $45,000 to about 37 percent in zip codes with $200,000. It then quickly rises again to about 50 percent (disregarding the far-right segment).

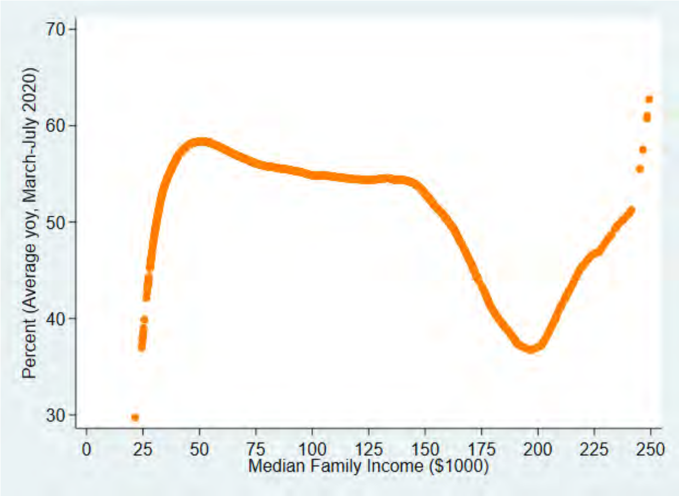
One possible explanation for this pattern is that it may reflect the liquidity constraints faced by low-income households, who were borrowing up to the debt-service limits before and are taking advantage of the ultra-easy monetary conditions to leverage up further. If that is indeed the case, then the financial stability concern would be stronger because the low-to-middle-income households are presumably less resilient to future shocks. Moreover, the high demand at the very high end of the income distribution could be explained by the particularly strong FOMO phenomenon, as these households have the financial means to afford the down payments for investment properties (as opposed to houses for residence purpose). Note that these two forces can reinforce each other: the higher housing prices caused by the speculative housing demand (from high-income households) push up the overall housing price index in the country, inducing low-income households to also expect higher future housing prices and thus rush to take mortgages and buy houses (in an attempt to avoid buying houses at higher prices in the future). Another possible explanation is analyzed below in the discussion of Fact #8.

C. Accounting for Urban Status

Several news articles have reported that urban residents in some areas are fleeing to the suburbs to seek more space and avoid the higher health risks in crowded urban neighborhoods.[18] If that is the case nationwide, then the above Fact #7 could also be explained by rich households buying second or larger homes in lower-income zip codes, which would have different economic and policy implications from those stated in the discussion of Fact #7.

[17] A similar figure for the housing supply is not very informative due to the large amount of missing values for the supply indicator (and if we plot the supply for the non-missing observations, the results would not be comparable to those for the demand and price because of the different sample coverages). However, with the figures for price and demand, we can infer the dynamics for supply.
[18] One example is this article: "New Yorkers Are Fleeing to the Suburbs," *New York Times*, August 30, 2020.

Figure 8. Views per Property vs Median Family Income: Nonparametric Estimation



Source: realtor.com; American Community Survey; Author's calculations.

To test this, I merge the zip code-level realtor.com housing data with the urban status classification data provided by the US Department of Agriculture. Such data classifies all zip codes into metropolitan (of 50,000 or more people), micropolitan (of 10,000-49,999 people), small-town (of 2,500-9,999 people), and rural commuting areas based on the size and direction of the primary (largest) commuting/traffic flows.[19] The *suburb* areas mentioned in the news articles broadly correspond to micropolitan areas and small towns. The results are summarized in the following stylized fact.

Fact #8: The changes in housing price, demand, and supply since April 2020 are broad-based, with *similar* magnitudes of changes across metropolitan, micropolitan, small-town, and rural areas.

In terms of the housing price (Figure 9), all four geographic categories have experienced rapid accelerations, although *less* so in *small towns*. In metropolitan and micropolitan areas, the median housing price growth rates (y-o-y) accelerated from 4-4.5 percent in April 2020 to about

[19] There are actually ten more granular geographic categories, but for the purpose of this paper and to mitigate small-sample bias, I consider the four higher-level categories (metropolitan, micropolitan, small town, and rural areas). More details can be found at https://www.ers.usda.gov/data-products/rural-urban-commuting-area-codes.aspx.

Covid Economics 50, 25 September 2020: 113-154

CEPR PRESS

7 percent in August 2020 (i.e., an acceleration of 3-3.5 percentage points). In small towns, they accelerated from 3.5 percent in April to 5.3 percent in August (a 1.8 percentage point acceleration). And in rural areas, they accelerated from 3.5 percent to 6.0 percent (a 3.5 percentage point acceleration).

In terms of the housing demand (Figure 10), all four geographic categories have experienced rapid accelerations, although more so in rural areas. In metropolitan, micropolitan, and small towns, the median growth rates (y-o-y) of median property views accelerated from about 0 percent in April 2020 to about 125 percent or so in August 2020. And in rural areas, they accelerated to 178 percent in August.

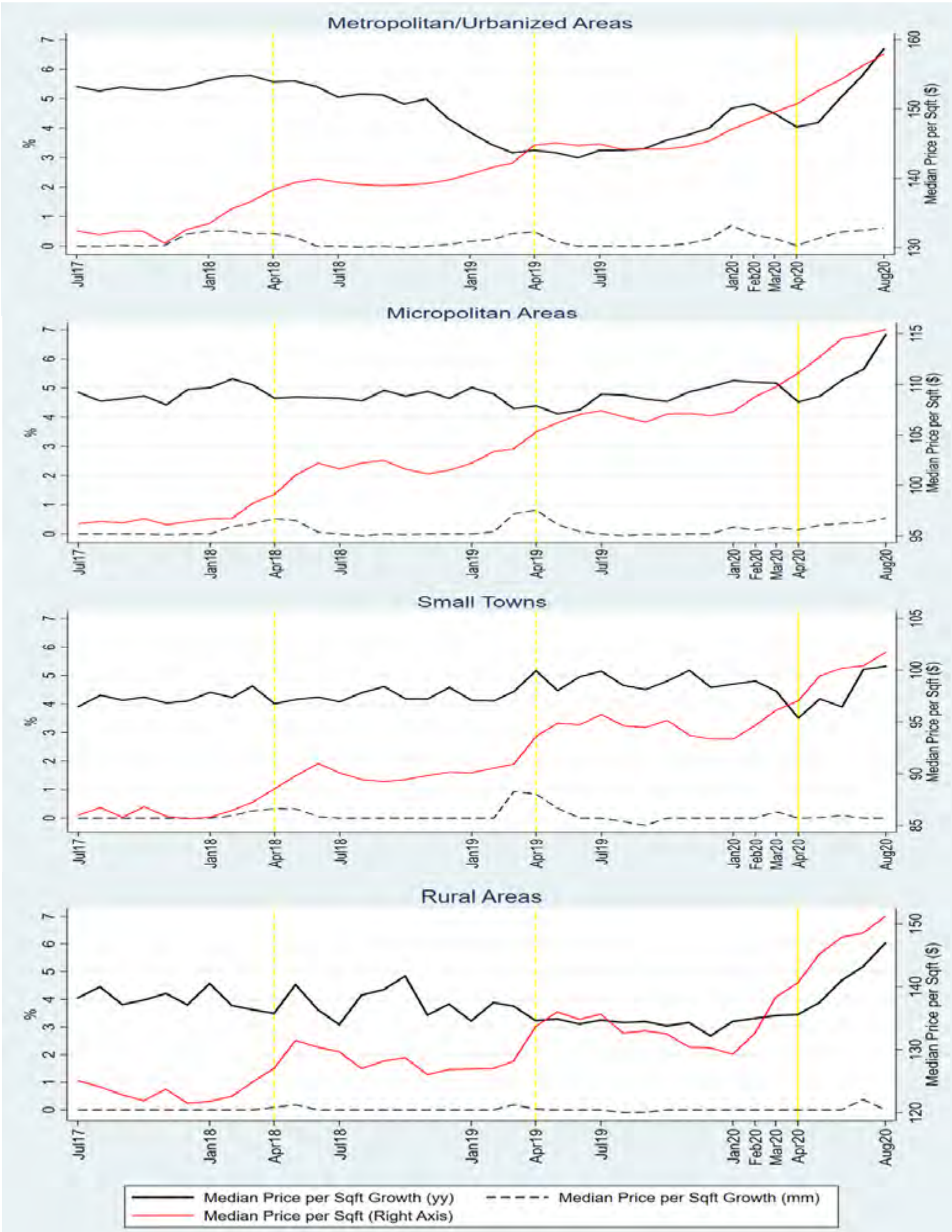
In terms of the housing supply (Figure 11), the decreasing trends of the housing supply were amplified by COVID-19 in all four geographic categories, although less so in metropolitan and small-town areas. In metropolitan areas, the median growth rates (y-o-y) of median active listings went from -18 percent in April 2020 to -28 percent in August 2020 (by 10 percentage points). In micropolitan and rural areas, they went from about -10 percent in April to about -30 percent in August (by 20 percentage points). And in small towns, they went from -11 percent in April to -24 percent in August (by 13 percentage points).

In summary, the results suggest that the phenomenon of urban residents fleeing to the suburbshas not had a nationwide impact yet.[20] This suggests that the (nationwide) U-shaped housing demand across income distribution is unlikely to be driven by rich households buying houses in low-income zip codes; instead, it may indeed reflect the relaxed liquidity constraints for low-income households and the fear-of-missing-out for high-income households, both of which are associated with Fed's unprecedented easing.

[20] Another possibility is that the increase in housing demand in urban areas (induced by, e.g., low mortgage rates and a preference shift from renting to owning houses) is so large that even after considering this fleeing effect, the urban housing demand and price have still increased substantially.

Covid Economics 50, 25 September 2020: 113-154

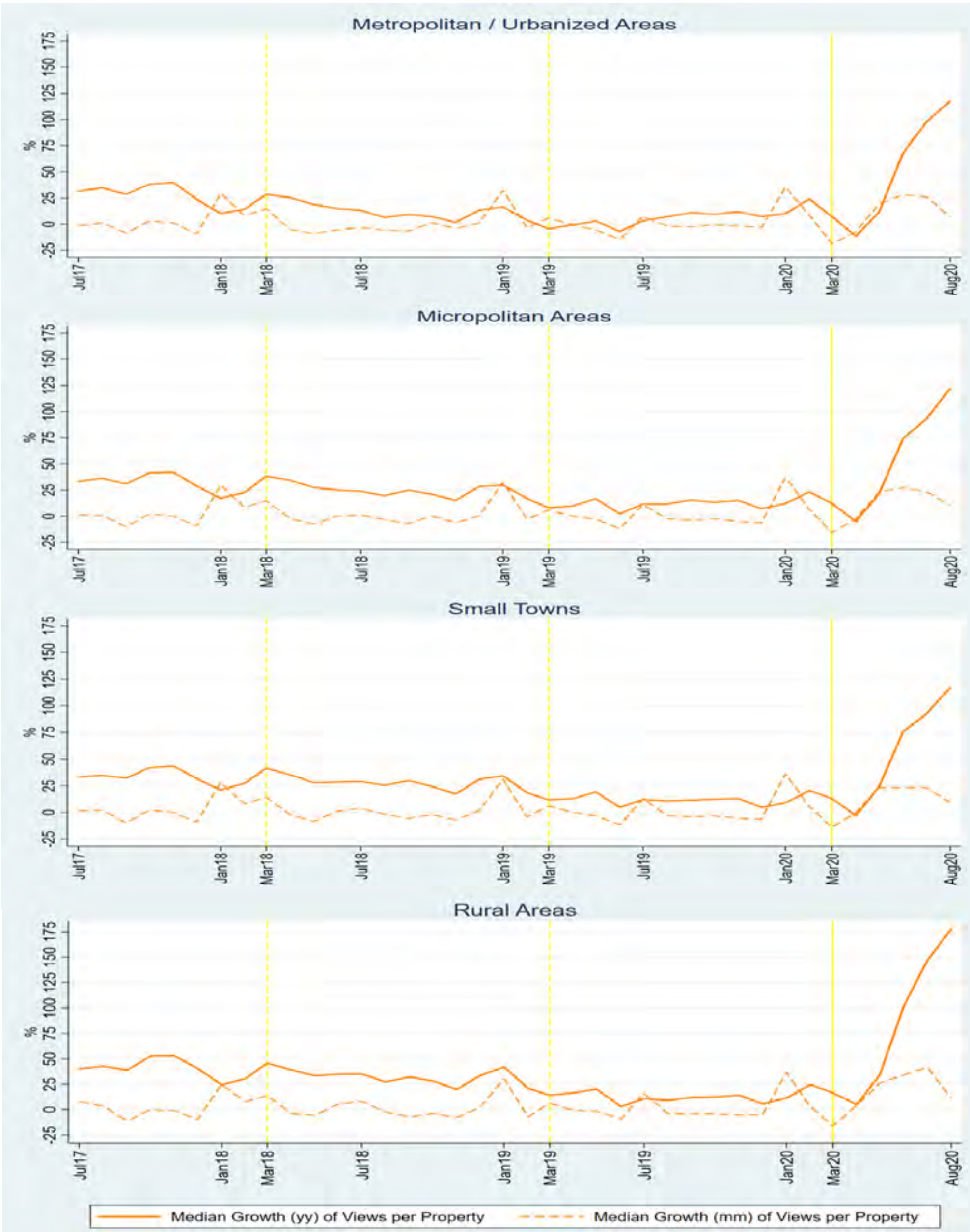
Figure 9. Median Housing Prices across Geographic Areas



Source: realtor.com; US Department of Agriculture; Author's calculations.

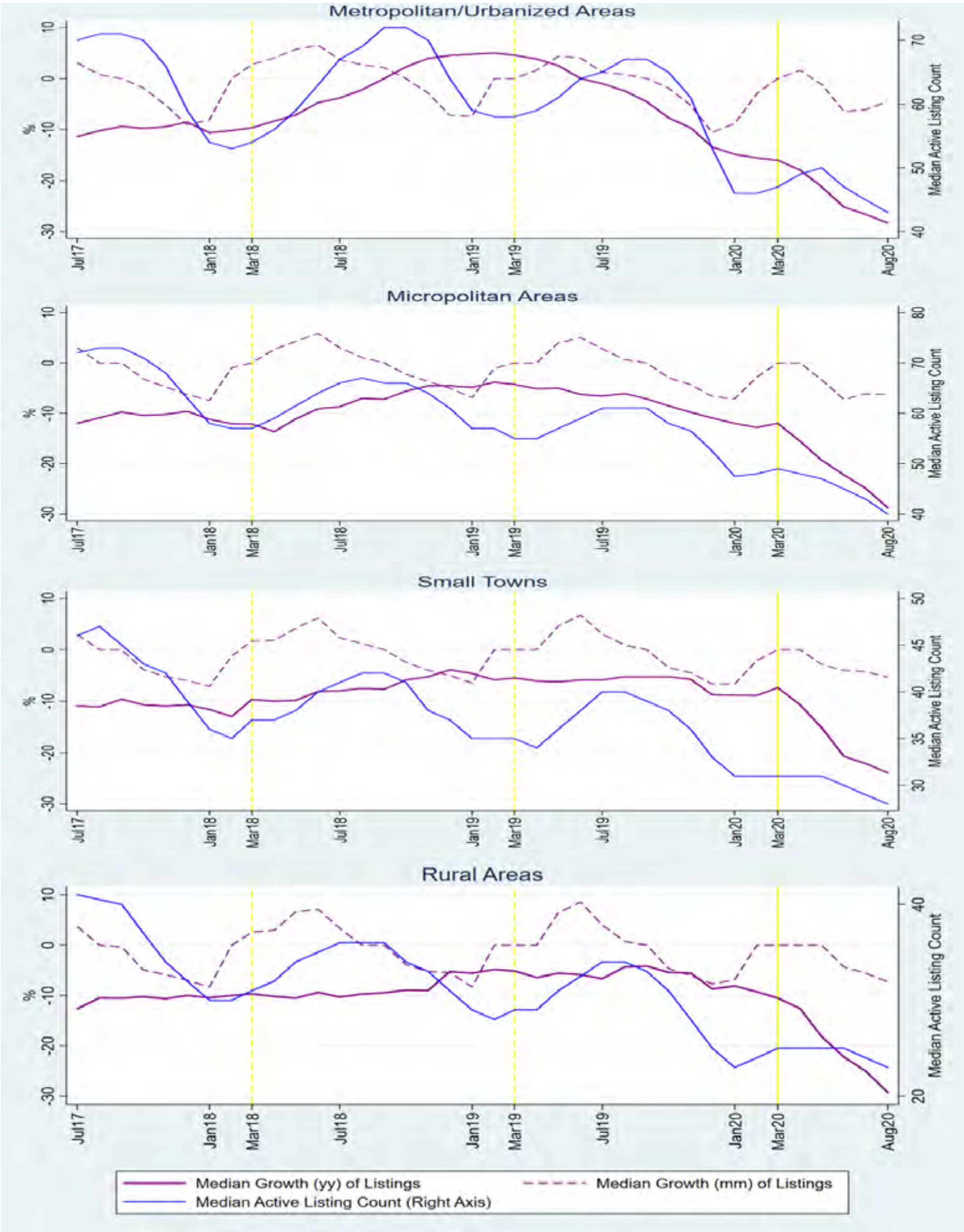
Covid Economics 50, 25 September 2020: 113-154

Figure 10. Median Housing Demands across Geographic Areas



Source: realtor.com; US Department of Agriculture; Author's calculations.

Figure 11. Median Housing Supplies across Geographic Areas



Source: realtor.com; US Department of Agriculture; Author's calculations.

Covid Economics 50, 25 September 2020: 113-154

CEPR PRESS

D. Accounting for Composition Effect

To mitigate the impact of the change in the housing supply composition, I restrict the sample to zip codes with little change in the median sqft of houses (no data on other property features are available to proxy for the quality of the house). In view of the small sample bias, I restrict to zip codes where the year-on-year median growth rates of the median sqft lie between -5 and 5 percent. Table 5 presents the summary statistics for the restricted sample, which are similar to those for the full sample.

Table 5. Summary Statistics after Cleaning for Zip Codes with Similar House Sizes

Variable	Unit	N	Mean	Min	Median	Max	SD
zip	NA	187,356	51,463	1,001	48,883	99,925	28,191
Month	NA	187,356	201,861	201,707	201,901	202,008	98
PropertyViews_yy	percent	187,356	26	-91	18	499	43
Listing_yy	percent	168,464	-4	-82	-8	470	27
MedianP_sqft_yy	percent	187,356	5	-96	4	199	11
MedianPrice_sqft	$	187,356	166	4	134	2,272	134
Median_sqft	NA	187,356	1,967	355	1,871	7,507	593
Median_sqft_yy	percent	187,356	0	-5	0	5	3
30Yr Fixed Mtg Rate	percent	187,356	4	3	4	5	1
MedianFamilyIncome	thousand $	186,671	78	10	71	249	30

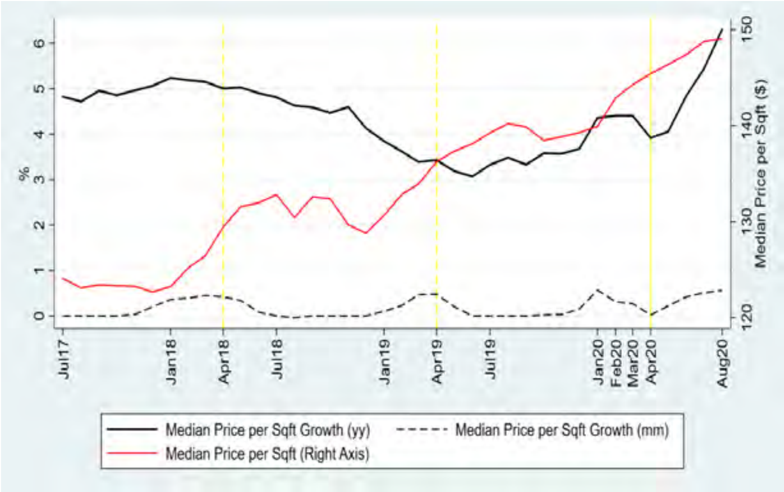
Source: realtor.com; Freddie Mac; New York Fed; American Community Survey; Author's calculations.

I find that the above eight stylized facts still hold after controlling for the composition/quality change of houses, five regarding aggregate evidence, two regarding distributional evidence, and one regarding the evidence across different geographic regions. Specifically:

Fact #1 with similar house sizes: After a temporary slow-down in March and April 2020, growth rates of the median housing price bounded back quickly and exceeded the growth rate before the COVID-19 crisis (Figure 12).

Covid Economics 50, 25 September 2020: 113-154

Figure 12. Median Housing Price: July 2017 – August 2020



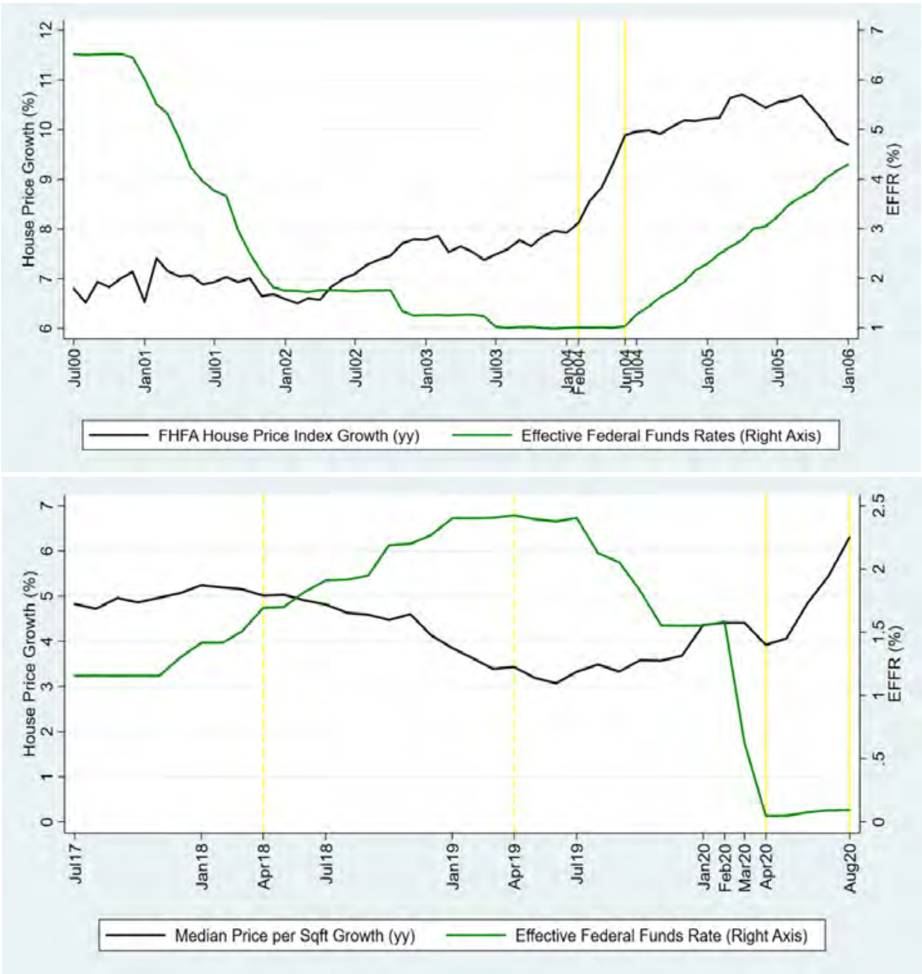
Source: realtor.com; Author's calculations.

Fact #2 with similar house sizes: The year-on-year growth rates of the median price per sqft in the recent four months (April-August 2020) have accelerated *faster* than any four-month period in the lead-up to the 2007-09 GFC (Figure 13). During the four-month period after the Fed's ultra-easing, the year-on-year grow rates have accelerated by 2.4 percentage points (from 3.9 percent in April 2020 to 6.3 percent in August 2020), faster than the 1.8 percentage points observed from February 2004 to June 2004.

Covid Economics 50, 25 September 2020: 113-154

Covid Economics 50, 25 September 2020: 113-154

Figure 13. Housing Price Growth: Comparison between GFC and COVID



Source: FHFA; realtor.com; New York Fed; Author's calculations.

Fact #3 with similar house sizes: The decreasing trend of the housing supply since mid-2019 is amplified by COVID-19 (Figure 14).

Figure 14. Housing Supply/Listings: July 2017 – August 2020

Source: realtor.com; Author's calculations.

Fact #4 with similar house sizes: The hotness of housing demand, proxied by the online views per property, has been rising at an extraordinary rate since April 2020 (Figure 15).

Figure 15. Hotness of Housing Demand: July 2017 – August 2020

Source: realtor.com; Author's calculations.

Covid Economics 50, 25 September 2020: 113-154

Covid Economics 50, 25 September 2020: 113-154

Fact #5 with similar house sizes: The response of housing demand to mortgage rate displays a structural break, which is robust to falsification tests (Tables 6-7).

Table 6. Panel Regressions with Similar House Sizes

	(1) Views-YY FE All	(2) Views-YY FE Until Feb	(3) Views-YY FE Since March	(4) Views-YY Pool All	(5) Views-YY Pool Until Feb	(6) Views-YY Pool Since March
FRM_30Yr	-23.799**	-2.232	-268.867***	-22.271*	-1.292	-269.286***
	(0.042)	(0.561)	(0.004)	(0.058)	(0.754)	(0.004)
Constant	120.490**	29.481*	912.329***	114.395**	25.604	914.352***
	(0.016)	(0.084)	(0.003)	(0.023)	(0.161)	(0.002)
Observations	187,128	159,482	24,702	187,356	159,784	27,572
R-squared	0.264	0.297	0.773	0.067	0.000	0.408

Note: The dependent variable is the median year-on-year growth rates of hotness of housing demand; FRM_30Yr = 30-year fixed-rate mortgage; Standard errors are clustered at the month level, and *robust* p-values are in parentheses.
Source: Author's calculations.

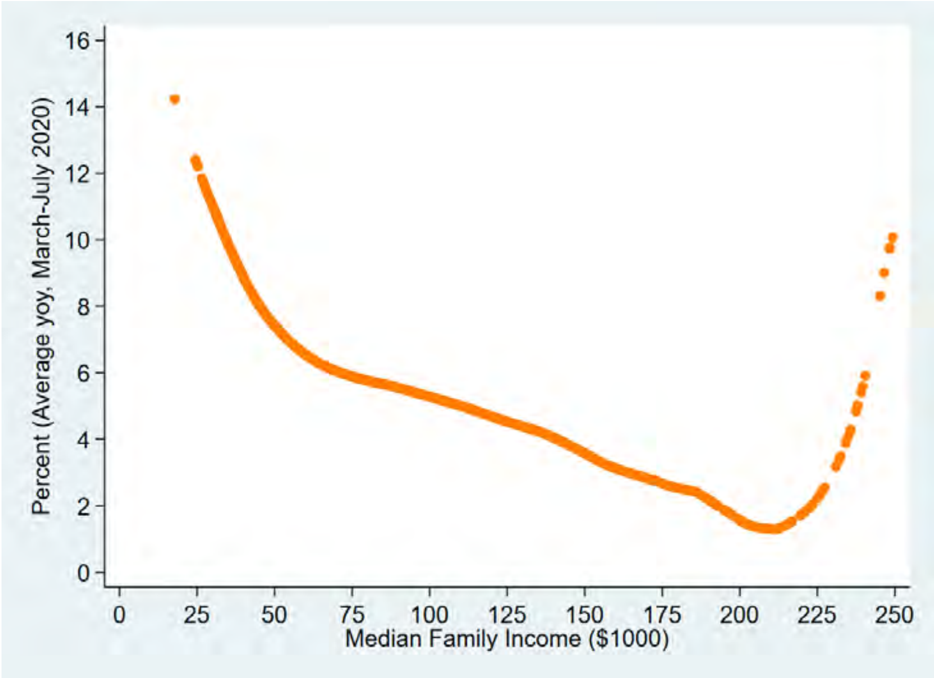
Table 7. Falsification Tests: Panel Regressions with Similar House Sizes

	(1) Views-YY FE Until August 2019	(2) Views-YY FE Until Feb 2019	(3) Views-YY FE March-August 2019	(4) Views-YY Pool Until August 2019	(5) Views-YY Pool Until Feb 2019	(6) Views-YY Pool March-August 2019
FRM_30Yr	-5.631	-22.222***	-8.197	-5.719	-23.272***	-5.836
	(0.380)	(0.000)	(0.291)	(0.420)	(0.000)	(0.377)
Constant	45.078	121.858***	38.697	45.454	126.418***	29.504
	(0.122)	(0.000)	(0.232)	(0.157)	(0.000)	(0.289)
Observations	129,250	99,077	27,439	129,699	99,743	29,956
R-squared	0.355	0.466	0.699	0.004	0.052	0.002

Note: The dependent variable is the median year-on-year growth rates of hotness of housing demand; FRM_30Yr = 30-year fixed-rate mortgage; *Robust* p-values are in parentheses.
Source: Author's calculations.

Fact #6 with similar house sizes: The increase in the housing price is particularly strong at the lower-to-middle end of the zip code-level income distribution (Figure 16).

Figure 16. Price per Sqft Growth vs Median Family Income: Nonparametric Estimation



Source: realtor.com; American Community Survey; Author's calculations.

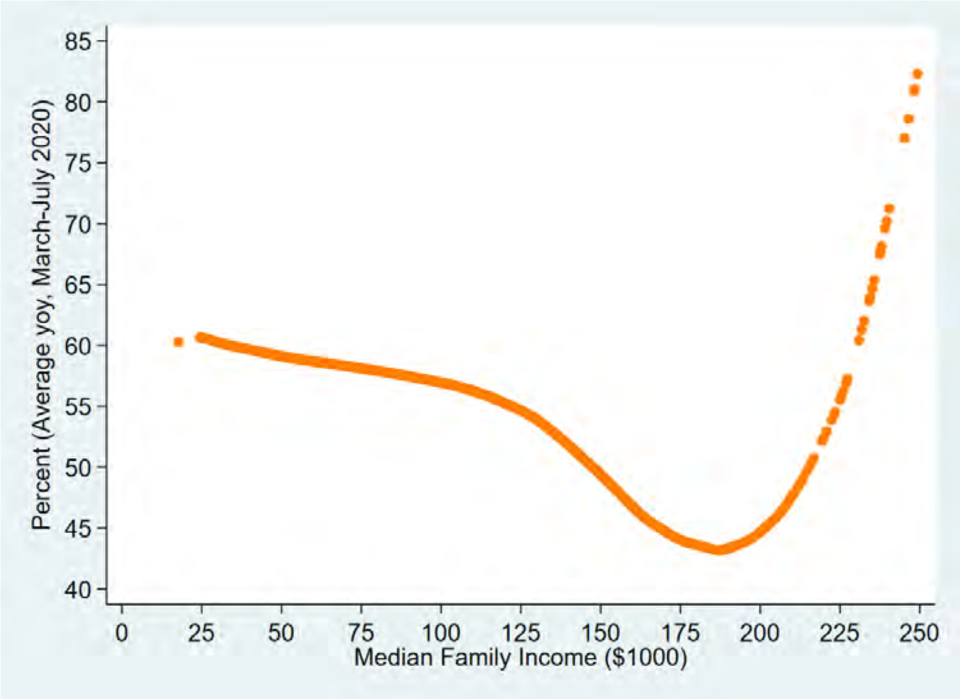
Fact #7 with similar house sizes: The increase in the hotness of housing demand is particularly strong at the two ends of the zip code-level income distribution, displaying a U-shaped relationship (Figure 17). With a narrower and potentially more comparable sample, the U-shaped relationship is actually more visible than the one with the full sample. The year-on-year growth rate of the housing demand measure decreases from around 60 percent in zip codes with a median family income of $25,000 to about 43 percent in zip codes with $190,000. It then quickly rises again to about 70 percent (disregarding the far-right segment).

Covid Economics 50, 25 September 2020: 113-154

CEPR PRESS

Covid Economics 50, 25 September 2020: 113-154

Figure 17. Views per Property vs Median Family Income: Nonparametric Estimation



Source: realtor.com; American Community Survey; Author's calculations.

Fact #8 with similar house sizes: The changes in housing price, demand, and supply since April 2020 are broad-based, with *similar* magnitudes of changes across metropolitan, micropolitan, small-town, and rural areas. The figures are very similar to Figures 9-11, so they are omitted for brevity.[21]

E. Accounting for Mobility

Besides the aforementioned composition effect, another concern is: the observed extraordinarily high growth rates of housing demand in the COVID era may be simply caused by the much lower outdoor mobility (associated with the stay-at-home orders) and much more frequent

[21] They are available upon request. After restricting the sample to zip codes with similar house sizes, the only major difference is for the house price in rural areas, which show that the median house price growth rate (y-o-y) came back to 3 percent in August 2020. However, this is most likely due to the small-sample bias (with only 246 observations in August 2020), so the full-sample version of the result for rural area's house price growth may be more reliable.

internet viewing in general, rather than an indication of the higher "genuine" housing demand associated with the record-low interest rates.

However, there is a second force at play: the stay-at-home orders (and the worsening of the pandemic itself) also imply more disruptions to the housing search and bidding processes, which *decrease* households' incentives to spend time in viewing houses online right now when they believe it is unsafe to view the houses in person and proceed with the purchasing process. This is the force emphasized by D'Lima, Lopez, and Pradhan (2020), who argue that "shutdown orders affect the process of buyers and sellers matching up for prospecting properties and subsequently completing the sale. Such frictions can be characterized as causing a demand side shock wherein buyers are unable to conduct an optimal search and bidding process."

Therefore, it is unclear which of the two forces would dominate and whether the omission of the mobility variable would significantly lower the usefulness of the online views in proxying housing demand. To empirically address this, I merge the zip code-level monthly housing data with the county-level daily Google mobility data (averaged to monthly) and rerun the housing demand regressions. To ensure the robustness of the results, I use several mobility measures, including the workplace mobility, residential mobility, and overall mobility that also accounts for grocery, recreation activities, etc. These daily indicators measure how visits and length of stay at different places change compared to the baseline (which is the 5-week period during January 3–February 6, 2020).[22] To mitigate the impact of the composition change, I still restrict the sample to zip codes where the year-on-year median growth rates of the median sqft lie between -5 and 5 percent. The results for zip codes with -2 and 2 percent growth rates are very similar and are available upon request.

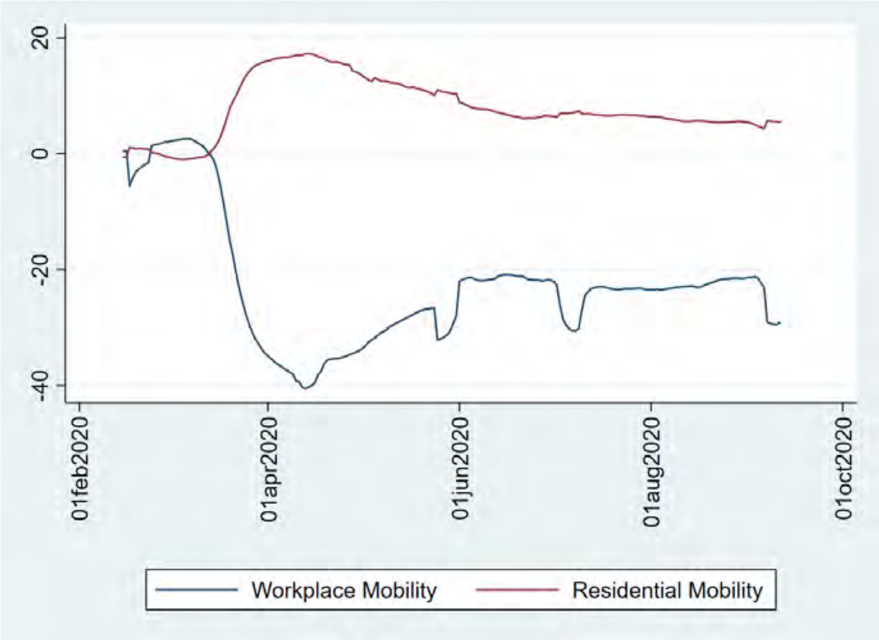
As shown in Table 8, across both specifications (fixed-effect and pooled regression models), workplace mobility is positively correlated with online views per property (so is overall mobility), and residential mobility is negatively correlated. Therefore, during the stay-at-home period when workplace mobility is lower and residential mobility is higher (as indicated in the daily time series in Figure 18), the online views per property tend to be *lower*, all else being equal. These results provide suggestive evidence that the aforementioned second force dominates

[22] For the data and detailed documentation, see https://www.google.com/covid19/mobility/.

the first one, and that the observed unusually high growth in online views per property since March 2020 may indeed be driven by the historically low interest rates.

Figure 18. Google Mobility in the US: February 15 – September 11, 2020

(7-day moving average)



Source: Google; Author's calculations.

To address a related concern that the signs of these mobility indicators may just reflect the impacts of the outbreak dynamics or the negative shocks on the macroeconomy, I further control for the (composite) Purchasing Managers' Index (PMI), which is available at monthly frequency until August 2020. Since the original PMI data are reported relative to the previous month, with above 50 being expansion and below 50 being contraction, I normalize them to absolute levels (with the PMI in July 2016 equal to 100) and then calculate the year-on-year growth for each month during July 2017-August 2020. The results are reported in Table 9: For four out of the six columns (4-6), both the mobility indicator and the PMI are significant; Moreover, workplace mobility is still positively correlated with the housing demand measure, and residential mobility is still negatively correlated, even after controlling for PMI, a measure of

Covid Economics 50, 25 September 2020: 113-154

CEPR PRESS

the overall macroeconomy. As a side note, consistent with previous discussions on omitted variable bias, the impact of the mortgage rate on housing demand becomes much stronger after controlling for the macroeconomic performance (262-341 in Table 9, as opposed to 202-265 in Table 8).

Note that because the Google mobility data starts from February 2020, it is *not* possible to analyze the existence of a structural break (in March 2020) in the housing demand response to lower interest rates. However, the estimated mortgage rate elasticities (in Table 9) after controlling for the mobility indicators are comparable with, and in some cases larger than, those without these controls (the "Since March" columns in Table 6).

Table 8. Panel Regressions with Similar House Sizes and Mobility Indicators
(February 2020 – August 2020)

	(1) Workplace FE	(2) Residential FE	(3) Overall FE	(4) Workplace Pool	(5) Residential Pool	(6) Overall Pool
FRM_30Yr	-264.645***	-223.346***	-201.779***	-251.646***	-221.336***	-204.501***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Workplace	2.531***			1.851***		
	(0.000)			(0.000)		
Residential		-5.236***			-3.808***	
		(0.000)			(0.000)	
Overall			1.990***			1.013***
			(0.000)			(0.008)
Constant	981.441***	825.775***	714.664***	919.132***	804.548***	716.835***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	20,327	19,090	16,072	22,044	20,664	17,380
R-squared	0.806	0.810	0.789	0.439	0.442	0.411

Note: The dependent variable is the median year-on-year growth rates of hotness of housing demand; FRM_30Yr = 30-year fixed-rate mortgage; Workplace, Residential, Overall are workplace mobility, residential mobility, and overall mobility from Google; *Robust* p-values are in parentheses.
Source: Author's calculations.

Covid Economics 50, 25 September 2020: 113-154

Table 9. Panel Regressions with Similar House Sizes, Mobility Indicators, and PMI

(February 2020 – August 2020)

	(1) Workplace FE	(2) Residential FE	(3) Overall FE	(4) Workplace Pool	(5) Residential Pool	(6) Overall Pool
FRM_30Yr	-313.395***	-261.913***	-312.828***	-334.613***	-311.265***	-340.559***
	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
PMI_yy	0.315	0.208	0.554**	0.500*	0.482*	0.693***
	(0.227)	(0.439)	(0.024)	(0.056)	(0.074)	(0.009)
Workplace	1.900***			1.175***		
	(0.008)			(0.005)		
Residential		-4.313***			-2.314***	
		(0.006)			(0.008)	
Overall			1.252***			0.608***
			(0.006)			(0.002)
Constant	1,129.149***	946.864***	1,084.556***	1,181.306***	1,093.530***	1,173.619***
	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	20,327	19,090	16,072	22,044	20,664	17,380
R-squared	0.811	0.811	0.811	0.459	0.458	0.464

Note: The dependent variable is the median year-on-year growth rates of hotness of housing demand; PMI_yy is the year-on-year growth of the normalized PMI index; FRM_30Yr = 30-year fixed-rate mortgage; Workplace, Residential, Overall are workplace mobility, residential mobility, and overall mobility from Google; *Robust* p-values are in parentheses.
Source: Author's calculations.

V. Conclusion and Policy Implications

Motivated by the unprecedented monetary easing, the plunging mortgage rates, and anecdotal evidence about the rapidly-rising residential housing prices, I document eight stylized facts on the COVID-era US housing market based on results from structural break models and nonparametric estimation, including both aggregate and distributional facts. One surprising result on the *aggregate* front is that the growth rate of the median housing price during the four months (April-August 2020) since Fed's unprecedented monetary easing has accelerated faster than the fastest four-month acceleration in the lead-up to the GFC. On the *distributional* front, I find that both the year-on-year growth rate of housing price and that of housing demand are higher at the lower-income zip codes, possibly reflecting relaxed liquidity constraints for low-income households (who were borrowing up to the debt-service limits before the monetary easing). In particular, I find that this (nationwide) result is unlikely driven by urban residents fleeing to the

Covid Economics 50, 25 September 2020: 113-154

suburbs, as the changes in housing price, demand, and supply since April 2020 have similar magnitudes across metropolitan, micropolitan, small-town, and rural areas. These results are robust after accounting for the composition change in housing supply and the mobility change of households during the COVID era.

Most of the results are stylized facts that do not rely on stringent modeling assumptions, although they can be interpreted in various ways. For example, in terms of the aggregate results, as discussed in Section IV(A), they are consistent with both Scenario (a) *(fear of missing out scenario)*, where potential homebuyers may be rushing to the housing market to take advantage of the historically low mortgage rate, and Scenario (b) *(COVID-induced behavioral change scenario)*, where households now value homeownership higher and are also more able to afford the down payments due to higher forced savings.

Hence, further studies are needed to more closely examine these areas. For example, given that the aforementioned two scenarios involve different nature of behavior (Scenario (a) involves more speculative, unsustainable housing demand, and Scenario (b) involves more fundamental housing demand), it is important to empirically tease out which scenario plays a more dominant role in the current conjuncture, and to theoretically simulate their implications on household leverage, financial stability, resource allocation across sectors, and the medium-to-long-run economic growth. Controlling for the omitted variables discussed in Section IV(A) may be one step forward towards this goal. And to shed more light on the distributional effect, one possible approach is to merge with data on homebuyers' incomes (at least at the zip code level) to quantitatively assess the drivers for the high growth in housing demand in low-income areas.

Despite the need for further work, hereby I still lay out some preliminary policy implications to stimulate more discussions. In view of the *aggregate* evidence, the results highlight the importance of complementing monetary easing (needed for combating the COVID crisis) by implementing carefully-designed *macroprudential* policies, such as enforcing loan documentation requirements and stringent limits on loan-to-value ratio, debt-to-income ratio, and debt-service-to-income ratio. Of course, as noted earlier, there are many complex forces driving the US housing market, and the rapid acceleration of housing price growth may slow down, e.g., after the housing supply returns to normal. Besides, the US financial system appears to be more resilient now than the pre-GFC period, as evidenced by the strong stress test results released on

Covid Economics 50, 25 September 2020: 113-154

June 25, 2020.[23] Moreover, the role of the Fed's low-interest-rate policy in driving the GFC appears to be debatable, as discussed in the literature review section.

However, the consensus among the vast majority of literature seems to be thatthe loosening in mortgage lending standards, the implicit government guarantee in the housing finance system, as well as the feedback loop between rapid credit provision and rising housing prices had played a major role in fueling the housing boom in the lead-up to the GFC. And all these forces coexisted with an extended period of low federal funds rates back then. Therefore, in the current environment where the rates are even lower, the implicit government mortgage guarantee is still present, and the housing price growth so far has accelerated at a faster pace than the pre-GFC era, macroprudential regulations and close scrutiny of bank/nonbank mortgage lending would be crucial to prevent the GFC history from repeating itself.

In view of the *distributional* evidence, the optimal policy design faces a difficult trade-off. On the one hand, from the perspective of lowering systemic risks, it is advisable not to relax the underwriting standards for new homebuyers, particularly when households with low or volatile incomes appear to be more actively seeking home purchase and mortgage borrowing during the monetary easing. On the other hand, from the perspective of reducing inequality, it is also essential to ensure that those who have experienced COVID-related employment disruptions are able to benefit from the historically low rates. On balance, the *"streamlined" refinance programs* proposed by Gerardi and others (2020), which allow existing homeowners to refinance and cut monthly payment without the need to document employment/income, seem to be an appealing option. Assuming that the existing low-income homeowners are less risky than the low-income renters, such programs could help a subgroup of low-income households (i.e., low-income homeowners) benefit from the low rates and contribute to reducing the *between-group* inequality.

Moreover, given my finding that the low-income zip codes are experiencing higher growth in housing price and if future studies confirm that this is mainly driven by local homeowners (rather than by well-off households buying houses in low-income areas), then it means low-income homeowners may be benefiting from the easy financing conditions more than

[23] Vice Chair Randal Quarles concluded that "The banking system has been a source of strength during this crisis." See Federal Reserve Board (2020) and the summary here: https://www.federalreserve.gov/newsevents/pressreleases/bcreg20200625c.htm.

CEPR PRESS

low-income renters. In that case, *targeted* financial assistance to renters could be helpful for reducing the *within-group* inequality between these two low-income subgroups. One example of such assistance is the renter direct payment program as discussed in Goodman and Magder (2020), where payments are made in a voucher form or directly to property owners (to ensure the rent payments are used as intended).

APPENDIX

Appendix Table 1. Number of Observations across All Months (after Cleaning)

Month	Frequency	Percent	Cumulative
201707	12,927	2.6	2.6
201708	12,961	2.6	5.2
201709	12,979	2.6	7.9
201710	12,988	2.6	10.5
201711	12,483	2.5	13.0
201712	12,655	2.6	15.6
201801	12,479	2.5	18.1
201802	12,533	2.5	20.7
201803	12,664	2.6	23.2
201804	12,755	2.6	25.8
201805	12,886	2.6	28.4
201806	13,105	2.7	31.1
201807	13,175	2.7	33.7
201808	13,205	2.7	36.4
201809	13,265	2.7	39.1
201810	13,233	2.7	41.8
201811	13,183	2.7	44.4
201812	13,113	2.7	47.1
201901	12,954	2.6	49.7
201902	12,890	2.6	52.3
201903	13,089	2.7	55.0
201904	12,959	2.6	57.6
201905	13,063	2.6	60.2
201906	13,219	2.7	62.9
201907	13,265	2.7	65.6
201908	13,374	2.7	68.3
201909	13,418	2.7	71.0
201910	13,565	2.8	73.8
201911	13,610	2.8	76.5
201912	13,623	2.8	79.3
202001	13,738	2.8	82.1
202002	13,262	2.7	84.8
202003	13,022	2.6	87.4

Covid Economics 50, 25 September 2020: 113-154

202004	12,918	2.6	90.0
202005	12,790	2.6	92.6
202006	12,477	2.5	95.1
202007	12,195	2.5	97.6
202008	11,936	2.4	100.0
Total	493,956	100.0	

Source: Author's calculations.

Reference

Acharya, Viral V. and Sascha Steffen. 2020. "The Risk of Being A Fallen Angel and the Corporate Dash for Cash in the Midst of COVID." *COVID Economics*, 10: 44–61.

Acharya, Viral V., Matthew Richardson, Stijn Van Nieuwerburgh, and Lawrence J. White. 2011. "*Guaranteed To Fail: Freddie, Fannie, and the Debacle of U.S. Mortgage Finance*," Princeton University Press.

Ambrus, Attila, Erica Field, and Robert Gonzalez. 2020. "Loss in the Time of Cholera: Long-run Impact of A Disease Epidemic on the Urban Landscape," *American Economic Review,* 110, 475–525.

Bayoumi, Tamim, and Yunhui Zhao. 2020. "Incomplete Financial Markets and the Over-Development of the Chinese Housing Sector," IMF Working Paper.

Barro, Robert J., José F. Ursúa, Joanna Weng. 2020. "The Coronavirus and the Great Influenza Pandemic: Lessons from the 'Spanish Flu' for the Coronavirus's Potential Effects on Mortality and Economic Activity," *NBER Working Paper No. 26866.*

Bolhuis, Marijn, and Judd N. L. Cramer. 2020. "Baby Boomers and the Housing Market on the Cusp of COVID-19," arXiv preprint arXiv: 3102262.

Bronka, Patryk, Diego Collado, and Matteo Richiardi. 2020. "The Covid-19 Crisis Response Helps the Poor: The Distributional and Budgetary Consequences of the UK Lockdown," *COVID Economics*, 26: 79–106.

Chen, Sophia, Deniz Igan, Nicola Pierri, and Andrea F. Presbitero. 2020. "Tracking the Economic Impact of COVID-19 and Mitigation Policies in Europe and the United States," *COVID Economics*, 36: 1–24.

Chetty, Raj, John N. Friedman, Nathaniel Hendren, Michael Stepner, and the Opportunity Insights Team. 2020. "How Did COVID-19 and Stabilization Policies Affect Spending

Covid Economics 50, 25 September 2020: 113-154

and Employment? A New Real-Time Economic Tracker Based on Private Sector Data," Working Paper.

Correia, Sergio. 2016. "A Feasible Estimator for Linear Models with Multi-Way Fixed Effects," Duke University Working Paper.

Correia, Sergio, Stephan Luck, and Emil Verner. 2020. "Pandemics Depress the Economy, Public Health Interventions Do Not: Evidence from the 1918 Flu," Working Paper.

Deb, Pragyan, Davide Furceri, Jonathan D. Ostry, and Nour Tawk. 2020. "The Economic Effects of Covid-19 Containment Measures," *Covid Economics*, 24: 32–75.

D'Lima, Walter, Luis A. Lopez, and Archana Pradhan. 2020. "COVID-19 and Housing Market Effects: Evidence from U.S. Shutdown Orders," *SSRN Working Paper No. 3647252.*

Dokko, Jane, Brian M. Doyle, Michael T. Kiley, Jinill Kim, Shane Sherlund, Jae Sim, Skander Van Den Heuvel. 2011. "Monetary policy and the global housing bubble," *Economic Policy*, Volume 26, Issue 66(1): 237–287.

Domanski, Dietrich, Scatigna, Michela, and Anna Zabai. 2016. "Wealth Inequality and Monetary Policy," *BIS Quarterly Review*.Elenev, Vadim, Tim Landvoigt, and Stijn Van Nieuwerburgh, 2020, "Can The Covid Bailouts Save The Economy?" *NBER Working Paper No. 27207.*

Francke, Marc, and Matthijs Korevaar. 2020. "Housing Markets in a Pandemic: Evidence from Historical Outbreaks," Working Paper.

Davide Furceri, Prakash Loungani, Jonathan D. Ostry and Pietro Pizzuto. 2020. "Will Covid-19 Affect Inequality? Evidence from Past Pandemics," *COVID Economics*, 12: 138–157.

Furceri, Davide, Prakash Loungani, and Aleksandra Zdzienicka. 2018. "The Effects of Monetary Policy Shocks on Inequality," *Journal of International Money and Finance*, 85: 168–186.

Galletta, Sergio, and Tommaso Giommoni. 2020. "The Effect of the 1918 Influenza Pandemic on Income Inequality: Evidence from Italy," *COVID Economics*, 33: 73–109.

Gerardi, Kristopher, Lara Loewenstein, and Paul Willen. 2020. "Evaluating the Benefits of a Streamlined Refinance Program," *Federal Reserve Bank of Atlanta Policy Hub, No. 08-2020.*

Glover, Andrew, Jonathan Heathcote, Dirk Krueger, and José-Víctor Ríos-Rull. 2020. "Health versus Wealth: On the Distributional Effects of Controlling A Pandemic," *COVID Economics*, 6: 22–64.

Covid Economics 50, 25 September 2020: 113-154

Goodman, Laurie, and Dan Magder. 2020. "Avoiding a COVID-19 Disaster for Renters and the Housing Market," Washington, DC: Urban Institute.

Greenwald, Daniel L., John Krainer, and Pascal Paul. 2020. "The Credit Line Channel," *Federal Reserve Bank of San Francisco Working Paper Series.*

Guimarães, Paulo, and Pedro Portugal. 2010. "A Simple Feasible Procedure to Fit Models with High-Dimensional Fixed Effects," *Stata Journal* 10 (4). College Station, TX: Stata Press:628–64922.

Hohberger, Stefan, Romanos Priftis, and Lukas Vogel. 2020. "The Distributional Effects of Conventional Monetary Policy and Quantitative Easing: Evidence from An Estimated DSGE Model," *Journal of Banking and Finance*, 113, 105483.

Keys, Benjamin J., Tanmoy Mukherjee, Amit Seru, and Vikrant Vig. 2010. "Did Securitization Lead to Lax Screening? Evidence from Subprime Loans." *Quarterly Journal of Economics*, 125 (1): 307-362.

Keys, Benjamin J., Amit Seru, and Vikrant Vig. 2012. "Lender Screening and the Role of Securitization: Evidence from Prime and Subprime Mortgage Markets." *Review of Financial Studies*, 25 (7): 2071-2108.

Ling, David C., Chongyu Wang and Tingyu Zhou. 2020. "A First Look at the Impact of COVID-19 on Commercial Real Estate Prices: Asset-Level Evidence," *COVID Economics*, 22: 205-260.

McDonald, John F., and Houston H. Stokes. 2013. "Monetary Policy and the Housing Bubble," *Journal of Real Estate Finance and Economics*, 46: 437–451.

Merrouche, Ouarda, and Erlend W. Nier. 2010. "What Caused the Global Financial Crisis - Evidence on the Drivers of Financial Imbalances 1999-2007," *IMF Working Paper No. 10/265.*

Palomino, Juan C., Juan G. Rodríguez, and Raquel Sebastian. 2020. "Wage Inequality and Poverty Effects of Lockdown and Social Distancing in Europe," *COVID Economics*, 25: 186–229.

Romem, Issi. 2019. "The Silver Tsunami: Which Areas will be Flooded with Homes once Boomers Start Leaving Them?" *Zillow Research*.

Schmitt-Grohé, Stephanie, Ken Teoh, and Martín Uribe. 2020. "Covid-19: Testing Inequality in New York City," *COVID Economics*, 8: 27–43.

Covid Economics 50, 25 September 2020: 113-154

Wong, Grace. 2008. “Has SARS Infected the Property Market? Evidence from Hong Kong,” *Journal of Urban Economics*, 63, 74–95.

Zhao, Yunhui. 2019. “Evidence of Government Subsidy on Mortgage Rate and Default: Revisited,” *Journal of Housing Research*, 28(1): 23–49.

Zhao, Yunhui. 2020. “Got Hurt for What You Paid? Revisiting Government Subsidy in the U.S. Mortgage Market,” *SSRN Working Paper No. 2895822.*

Covid Economics 50, 25 September 2020: 113-154

Covid Economics Issue 50, 25 September 2020

Too afraid to vote? The effects of COVID-19 on voting behaviour[1]

Tania Fernández-Navia,[2] Eduardo Polo-Muro[3] and David Tercero-Lucas[4]

Date submitted: 15 September 2020; Date accepted: 17 September 2020

This paper studies the causal effect of local exposure to the COVID-19 on voting behavior and electoral outcomes using evidence from the regional elections held in Spain on July 12, 2020. Exploiting the variation in exposure to the COVID-19 and using a difference-in-differences identification strategy, we show that turnover was between and 2.2 and 3.3 percentage points lower in municipalities that experienced positive cases of COVID-19. However, we do not find evidence of changes in the vote shares to the incumbent parties at the regional or national levels. We further discuss fear as the potential mechanism driving our results.

Covid Economics 50, 25 September 2020: 155-180

1 Eduardo Polo-Muro acknowledges financial support from the Basque Government (PRE-2016-1-0429). David Tercero-Lucas wants to acknowledge the support of a fellowship from "la Caixa" Foundation (ID 100010434). The fellowship code is "LCF/BQ/ES18/11670005". The ideas and opinions in this paper are exclusively those of the authors and do not necessarily represent the views of Compass Lexecon.
2 Compass Lexecon.
3 University of the Basque Country.
4 Autonomous University of Barcelona.

Covid Economics 50, 25 September 2020: 155-180

1 Introduction

The worldwide spreading of the COVID-19 infection, caused by the virus SARS-CoV-2, has created unique political and economic challenges. Governments have to face with a global health problem that not only jeopardizes the lives of millions of citizens but also has severe effects on, among others, economic growth (Guerrieri et al., 2020), inequality (Blundell et al., 2020; Hacioglu et al., 2020), mental health (Brodeur et al., 2020; Fetzer et al., 2020), or domestic violence (Bullinger et al., 2020). In addition, the management of the situation is prone to create important trade-offs between rigid measures to contain the transmission of the virus and civil liberties (Amat et al., 2020).

Indeed, the COVID-19 has the potential to endanger democracy (Amat et al., 2020). As time goes by, more countries have to decide between celebrating elections under uncertain circumstances or postponing them. Which one is the best choice is unclear. As Prof. Toby James states *"Intuitively, we think postponing an election sounds anti-democratic, [...] but actually democracy in some ways could be undermined by holding an election in these times".*[1] For example, elections may weaken democracy if the outcome under-represents the voice of voters that would have liked to participate but decided against it for being affected or for the fear of being exposed to the infection.[2]

This paper is, to the best of our knowledge, the first to examine the causal effects of local exposure to the COVID-19 on voting behavior and electoral outcomes. We use evidence from the regional elections held in the Basque Country, Spain on July 12, 2020. We combine data at municipal level on (i) the number of confirmed positive cases per 100,000 inhabitants since the outbreak of the pandemic to the week before the elections, and (ii) on participation rates and vote shares for the main parties.[3] Figure 1a looks at the relation between turnout and confirmed COVID-19 cases at the municipal level. The descriptive evidence shows a negative association between participation rates and exposure to the pandemic.

To estimate the causal effect of exposure to the COVID-19 on voting behaviour and electoral outcomes, the primary empirical strategy compares voting decisions in municipalities with *(treated group)* and without *(control group)* positive cases of COVID-19 in elections both before and after the outbreak of the pandemic. We include municipality, province, and election-year fixed-effects in all specifications to control for overall trends in electoral outcomes and all time-invariant differences across municipalities.

[1]See https://www.minsait.com/ideasfordemocracy/en/elections-during-pandemic-covid-19

[2]A special concern might be the reduction in turnout of minorities and low-income individuals. According to Patel et al. (2020), infection rates are larger in poorer areas. Moreover, it is well established in the literature the gradient between income and political participation (Akee et al., 2020). Therefore, those who already are less prone to vote may be also more affected by the pandemic, which could lead to an even larger reduction in the political participation for this group.

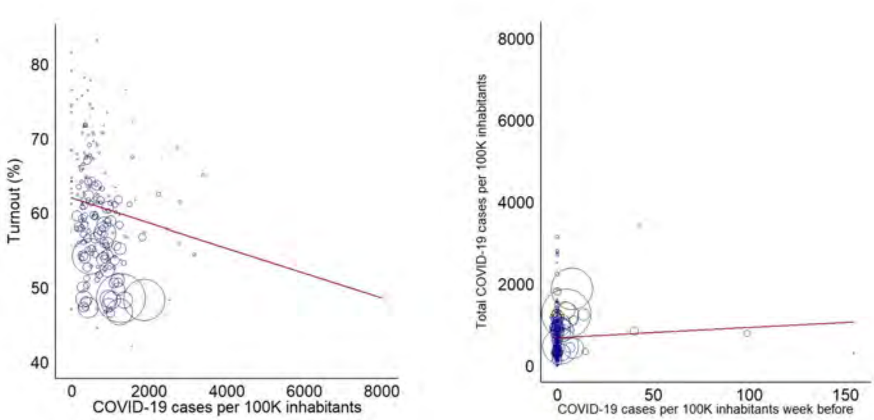
[3]The Basque Country is a region in the north of Spain composed by three provinces (Álava, Bizcaia, and Gipuzkoa). 4% of the Spanish population lives in the Basque Country, and it represents 6% of the national GDP. In addition, Basque Country's GDP per capita −around €33,000− is the highest in Spain.

Covid Economics 50, 25 September 2020: 155-180

Figure 1: COVID-19 situation in the Basque Country

(a) COVID-19 and Turnout

(b) Total & Recent COVID-19



Notes: Figure 1a shows the correlation between the confirmed cases of COVID-19 and the turnout at the municipal level. We can observe that participation rates were lower in those municipalities exposed to more cases of COVID-19. Figure 1b presents the correlation between the accumulated number of cases from the outbreak of the pandemic to the week before the elections (y-axis) and the number of COVID-19 cases the week before the elections (x-axis). The correlation between these two variables is 0.04. In both graphs, the size of the markers reflects the population size of the municipality. Figures A1a and A1b replicate these two graphs excluding the municipality of Samaniego (with 8% of infections) to show that the observed associations (or lack of them) are not driven by this outlier.

The main results show that the turnout was between 2.2 and 3.3 percentage points lower in municipalities with positive cases of COVID-19. This result is consistent with the findings in Campante et al. (2020), which shows lower participation rates in areas more exposed to Ebola in the congressional and governmental U.S. elections.[4] Contrary to previous studies, however, we do not find evidence of changes in the vote shares to the incumbent parties at the regional or national levels. These results are robust the inclusion of controls and to a battery of alternative specifications.

This paper contributes to several strands of the literature. First, this research is closely related to very recent and scarce literature analyzing the effects of epidemics on electoral behavior. Beall et al. (2016) show correlational evidence that voter intentions to the Republican Party increased in places with more intense concerns about Ebola.[5] Similarly, Campante et al. (2020) find that Ebola concerns (measured by online activity such as Google searches or Tweets) caused a decrease in the Democratic vote share in the 2014 midterm elections and lower turnout. Mansour et al. (2020) study the effect of exposure to HIV/AIDS on vote shares for the U.S House of Representatives. They find a strong, positive association between HIV/AIDS mortality and the vote share received by Democratic candidates. In this sense, our results may have important implications for forthcoming U.S. and other

[4]Campante et al. (2020) consider fear as the potential driver of their results. In our context, albeit the COVID-19 presents a more realistic threat, in the moment of the elections there were almost no active cases. Indeed, as figure 1b shows, the correlation between the accumulated number of cases and the cases just before the elections is almost zero. This might be suggestive evidence that fear is also the mechanism in our study.

[5]This study was challenged in Tiokhin and Hruschka (2017).

elections. Previous research has found that supporters to the U.S. Democratic party are significantly more worried about the COVID-19 than Republican voters.[6] Thus, if our results apply to the U.S., we might expect to see lower levels of turnout in those population groups that present larger levels of fear to the pandemic.

This paper also relates to the recent research that studies the political effects of the lockdown caused by the COVID-19. Adam-Troian et al. (2020) study the first round of 2020 French municipal elections and find that areas where the COVID-19 perceived threat was higher showed more support for conservative parties. Interestingly, they do not find any effect of the real COVID-19 threat (measured by prevalence rates) on vote shares. Bol et al. (2020) rely on a representative online survey covering several Western European countries and compare the individual responses before and after the implementation of the lockdowns. They find that lockdowns increased the intention to vote for the incumbent, trust in the government, and satisfaction with democracy. De Vries et al. (2020) show that the support to incumbents in several European countries increased after the introduction of restrictions in Italy. Giommoni1 and Loumeau (2020), using evidence from the French municipal elections and exploiting the differential lockdown measures implemented across departments, find that in localities with stricter lockdowns, the support for the incumbent and for Green parties was higher. Besides, voter turnout was also larger in the departments where harder lockdowns were imposed. Using experimental data from New Zealand, Sibley et al. (2020) show that in comparison with the pre-lockdown scenario, people exposed to more restrictive measures increased their trust in science, politicians, and the police. They also presented higher levels of patriotism, and more mental distress. For Spain, Amat et al. (2020) find that the pandemic increased the support for strong leadership and for technocratic governments, and the willingness to give up individual freedom.

Last but not least, this paper is also related to the literature that studies the political impact of threats such as terrorism/conflicts or natural disasters in electoral outcomes. Bellows and Miguel (2009), focusing on 1991–2002 Sierra Leone civil war, conclude that those households more exposed to war were more likely to vote. Related to conflicts, Gardeazabal (2010) looks at the effect of terrorism in voting behaviour in Spain. He finds that terrorist activities increase turnout and that it has an important impact on vote shares depending on the party and type of violence. Montalvo (2011) analyses the 2004 terrorist attacks in Madrid that happened three days before the general elections. He finds that the attacks had an important electoral impact, determining the winner. Kibris (2011) finds that exposure to PKK terrorism in Turkey increases turnout and the vote share for the right-wing parties (tougher against the PKK terrorist organization cause). Getmansky and Zeitzoff (2014), analysing the exposure to rocket attacks from the Gaza Strip in Israel, find that right-wing vote shares are higher in localities that are within the radio of attack. Gallego (2018) studies the impact of different types od attacks on participation in Colombia. He finds that guerrilla violence lowers turnout. However, paramilitary violence has no effect on participation, but benefits non-traditional

[6] See Calvillo et al. (2020); Simonov et al. (2020) and https://www.pewresearch.org/politics/2020/06/25/republicans-democrats-move-even-further-apart-in-coronavirus-concerns/.

Covid Economics 50, 25 September 2020: 155-180

Covid Economics 50, 25 September 2020: 155-180

third parties. Using evidence from several countries in Europe, Peri et al. (2020) finds that international terror attacks increase vote shares for nationalistic parties.

Related to natural disasters, Healy and Malhotra (2009) show that voters reward the incumbent presidential party for delivering disaster relief spending, but not for investing in disaster preparedness spending, creating relevant distortions on the incentives of public servants. Gasper and Reeves (2011) find that voters punish governments and officials in charge when there is severe weather damage, although voters are also able to some extent to distribute responsibilities. Heersink et al. (2017) evaluate the effects of one of the 1927 Great Mississippi Flood. They find that this natural disaster negatively affected incumbent's vote share in the next elections. Ramos and Sanz (2020), using evidence from Spain, find that unintended wildfires up to nine months ahead of the elections increase support incumbent's party vote share in the following elections.

The remainder of the paper is organized as follows. Section 2 presents the context of the COVID-19 crisis in Spain and the 2020 regional basque elections. Section 3 describes the data employed in the analysis. Section 4 describes the empirical strategy. Section 5 presents the main results. Section 6 concludes.

2 Institutional Setting

2.1 COVID-19 Pandemic in Spain

Spain has been one of the earliest and hardest-hit countries by the COVID-19.[7] Due to the rapid spread of the infection (see figure 2), and after localized actions in hot-spot areas, the Spanish Prime Minister, Pedro Sánchez, announced the implementation of the *State of Alarm* in March 14^{th}, enforcing one of the strictest lockdowns worldwide.[8]

Indeed, effective on March 15^{th}, individuals could only leave their households for indispensable reasons (e.g., grocery shopping, going to the hospital or buying medicines, taking care of dependents, or work-related obligations).[9] The compliance with the restrictions was controlled by the police and the army, and violators could face important fines.

On April 28^{th}, the government announced a plan to move from the confinement to the so-called "new-normal". This plan included four different stages, being the fourth one the least restrictive. To move from one stage to another, several factors such as the capacities of the health system, the epidemiological situation, or the compliance with collective protection measures were considered. Finally, on June 21^{th}, after more than three months of very strict measures, the *State of Alarm* was lifted.

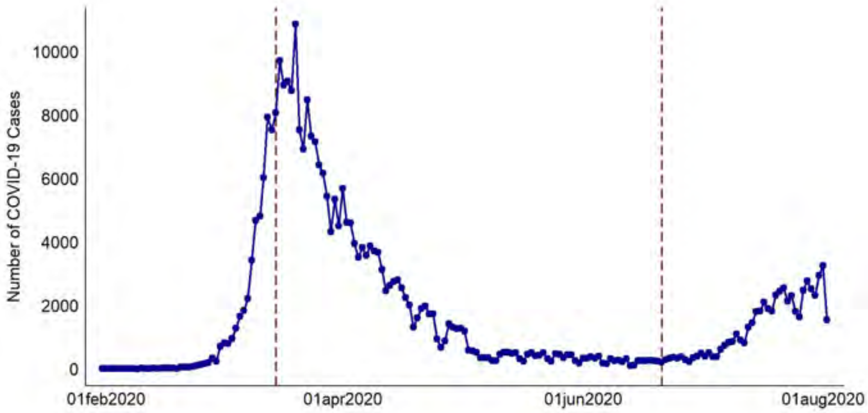
[7] See Felgueroso et al. (2020) for a detailed description of the effects of the COVID-19 on the Spanish economy.

[8] See https://www.ft.com/content/428babc4-66c9-11ea-800d-da70cff6e4d3.

[9] Albeit two weeks after the announcement of the *State of Alarm*, the central government prohibited all non-essential economic activity.

CEPR PRESS

Figure 2: COVID-19 Daily Cases in Spain



Notes: This figure shows the total daily number of COVID-19 cases in Spain between January, 31 (when the first case of COVID-19 in Spain was registered) and August, 2. The brown dashed lines indicate the beginning (March, 15) and the end (June, 21) of the lockdown measures. The source is from the Centro Nacional de Epidemiología (https://cnecovid.isciii.es/).

2.2 Regional Elections in the Basque Country

The elections for the Basque Country are mainly regulated by the 1978 Spanish Constitution and the 1979 Statute of Autonomy of the Basque Country. The electoral constituencies correspond to the three Basque provinces: Álava, Bizkaia, and Guipuzkoa. Regardless of the population of the three territories, each constituency elects 25 parliamentarians. If a party gets at least 38 out of the 75 seats, it obtains absolute majority.[10]

On February 10, 2020, Iñigo Urkullu, the incumbent of the regional government of the Basque Country, announced that the next regional elections were to be hold on April 5, 2020. However, on March 16^{th}, two days after the national government announced the *State of Alarm*, and the day after the implementation of the national lockdown, it was announced that the vote would be postponed due to the COVID-19 pandemic situation. Indeed, the Basque Country was and still is among the most affected regions by the COVID-19. As of September, 6.4% of the total infections at the national level had happened in this region.

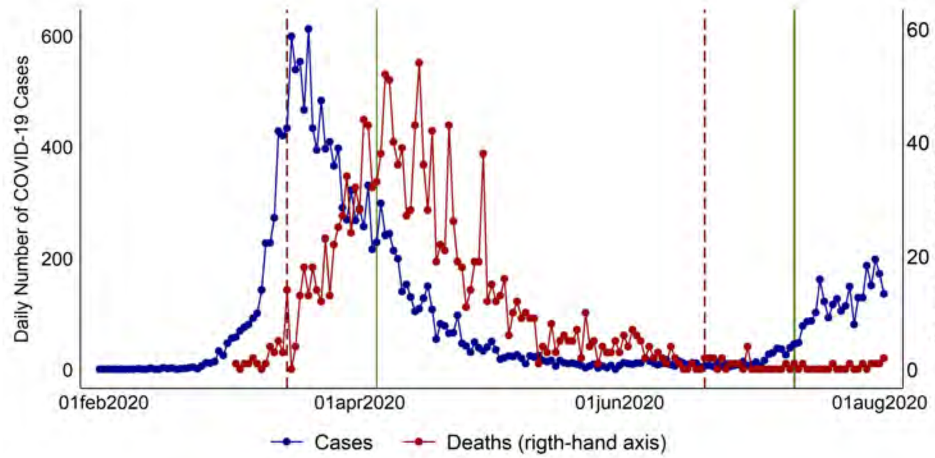
At the end, the Basque Country hold the regional elections on July, 12^{th} (three weeks after the *State of Alarm* was lifted) under strict hygiene measures. As figure 3 shows, while the number of new cases was low at the moment of the elections, there was a growing trend. This is mostly due to an outbreak in Ordizia, a municipality with around 10,000 individuals situated in Gipuzkoa.[11]

[10] The allocation of seats to the electoral lists is done through the D'Hondt system. The electoral threshold for the distribution of seats in each constituency is 3% of the valid votes.

[11] In this paper, we show the results including and excluding the municipality of Ordizia. In addition, individuals infected with the COVID-19 or pending the test results could not vote. This affected around 1% of the census.

Covid Economics 50, 25 September 2020: 155-180

Figure 3: Basque Country COVID-19 Daily Cases/Deaths



Notes: This figure shows the total daily number of COVID-19 cases and deaths (right-hand side axis) in the Basque Country between January, 31, and August, 2. The brown dashed lines indicate the beginning (March, 15) and the end (June, 21) of the lock-down measures. The thin green line indicates the original date of the regional elections (April, 5). The thicker green line indicates the actual date of the elections (July, 12). The source is from the Centro Nacional de Epidemiología (https://cnecovid.isciii.es/).

In this 2020 elections, electoral turnout was 50.78%, the lowest participation rate since 1984, and 9.24 points lower than in the previous regional elections in 2016 (see figure 4a). The incumbent from the Basque Nationalist Party (PNV), Iñigo Urkullu, was re-elected with a 39.06% of the vote and 31 of the 75 seats, obtaining the highest result for the PNV since 1984 (see figure 4b).

3 Data

We compile data from several sources. First, we use information on COVID-19 exposure at the municipal level in the Basque Country. Second, we use electoral outcome information (turnout and vote shares) from the three last Basque Country's regional elections. Third, we include information on several socio-economic variables at the municipal level. Our sample includes data on all Basque municipalities for the years 2012, 2016, and 2020.

3.1 COVID-19 exposure

To measure exposure to COVID-19, we use official data from the Basque Country government.[12] This source has daily information at the municipal level on (i) the number of infected people with COVID-19, and (ii) the number of deaths caused by COVID-19. To account for population size, we compute the ratios per 100,000 inhabitants.

[12]See https://www.eitb.eus/es/noticias/sociedad/detalle/7110777/casos-coronavirus-euskadi-pueblo-pueblo-edades/.

Covid Economics 50, 25 September 2020: 155-180

Covid Economics 50, 25 September 2020: 155-180

In our main specification, we distinguish between those municipalities with and without exposure to COVID-19 cases since the outbreak of the pandemic up to the week before the elections.[13] We can see, in table A1, that there was a lot of variation in exposure to COVID-19. In particular, 12 percent of the municipalities did not present any COVID-19 contagion during the studied period. Conditional on having at least one positive COVID-19 case, municipalities were exposed, on average, to 689 cases per 100,000 inhabitants (SD = 742.40).[14]

3.2 Electoral results

To analyse the effect of exposure to COVID-19 on electoral outcomes, we use municipal data on turnout and incumbent parties' vote shares (both at the national and regional levels) for the regional elections held in the Basque Country on the year 2020. To control for pre-trends in voting behaviour, we include data from the 2016 and 2012 elections. All this information is publicly provided by the Basque Country government.[15]

We can see in table A1 (in the Appendix) that, on average, turnout was 66.13% (SD = 8.37). The incumbent party at the regional level – PNV – received 41% of votes on average (SD = 9.94%). On the other hand, the incumbent party at the national level – PSOE – only received about 7.43% of the votes (SD = 6.9%).[16]

Figure 4a shows the evolution of participation rates over time at the aggregate level. We can see that turnout was lower in 2020 than in previous years. In fact, the participation rate was the lowest since 1984. Regarding the vote to the incumbent party in the Basque Country, PNV's vote share increased slightly in the 2020 elections. The PSOE's vote share, on the other hand, remained stable (see figure 4b).

3.3 Other data

In addition, we use information from the Basque Institute of Statistics to control for socio-economic variables available at the municipal level. Namely, we include age distribution, education level, and unemployment rates. As we can see in table A1, the average municipality had 15 percent of population younger than 16 years, 50 percent was population aged between 16 and 54 years old, 14 percent population between 55 and 64, and the other 20 percent of the population was older than 65 years old. In addition, 14.4 percent of the inhabitants had tertiary education, 46.7 percent had some degree of secondary education, and the other 38.88 percent were non-educated or had completed up to primary education. Finally, the average municipality's' unemployment rate was 7.55 percent, and the average municipality had 8687 inhabitants.

[13] Notice that July 6^{th} was the last official public update until the elections day.

[14] Notice that the descriptive statistics in table A1 include municipalities with zero COVID-19 cases.

[15] See https://www.euskadi.eus/elecciones/.

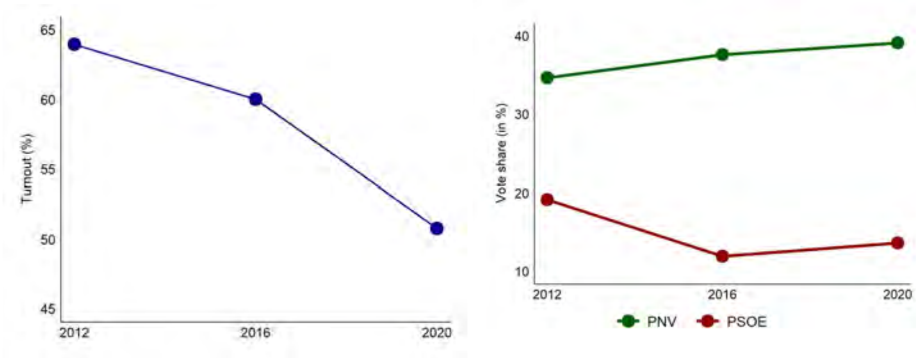
[16] These figures does not match the official ones because they have not been weighted by population.

Covid Economics 50, 25 September 2020: 155-180

Figure 4: Electoral Results

(a) Turnout

(b) Incumbents Vote Shares



Notes: These figures show turnout, PNV vote share, and PSOE vote share in the last three regional elections hold in the Basque Country. Usually, the elections take place each four years, albeit incumbents can move up the dates. Notice that here we are using information from the final outcomes (weighted by population), while in table A1 we do not weight differently large and small municipalities. The source is the Basque government (https://www.euskadi.eus/elecciones/).

4 Empirical strategy

The main purpose of this paper is to understand the effect of exposure to COVID-19 on voting behaviour and electoral outcomes. For that, we start with the following *naive* econometric model:

$$\begin{aligned} Y_{m,p,t} = \alpha + \beta_1 COVID19_{m,p,t} + \beta_2 COVID19^2_{m,p,t} \\ + \gamma X_{m,p,t} + \delta Post_t + \kappa_m + \zeta_p + \psi_t + \epsilon_{m,p,t} \end{aligned} \tag{1}$$

where $Y_{m,p,t}$ measures (i) turnout in municipality m in province p during the elections held in year t, and (ii) vote shares in these elections to the incumbent parties at the regional and national levels. $COVID-19_{m,p,t}$ measures the number of confirmed cases in municipality m in province p in year t from the outbreak of the pandemic to the week before the elections.[17] $COVID-19^2_{m,p,t}$ also control for the potential non-linear effects of the exposure to COVID-19 on the outcomes. We include socio-economic characteristics at the municipal level ($X_{m,p,t}$), and a categorical variable that takes the value 1 for the elections hold in 2020, and zero otherwise ($Post_t$). In addition, we include municipality(κ_m), province (ζ_p), and year(ψ_t) fixed-effects. We cluster standard errors at the municipal level to address serial correlation concerns and the possibility of heterogeneity in response to the exposure to COVID-19 at the municipal level.

In this scenario, our parameter of interest is β_1, which measures the relation between being exposed to COVID-19 cases and electoral outcomes. The problem with the OLS specification is that the estimated coefficient for the parameter β_1 may

[17]This observation only takes positive values on year 2020.

be biased. For example, there might be unobserved factors affecting simultaneously to the probability of being exposed to COVID-19 as well as voting behaviour.

In order to isolate the causal effect of exposure to the infection in electoral outcomes, we estimate a difference-in-differences econometric model. We define as treated units those municipalities who reported any positive cases of COVID-19 before the elections day. Thus, municipalities with zero cases of COVID-19 are used as control units. Our estimation equation is given below:

$$\begin{aligned} Y_{m,p,t} &= \alpha + \beta Post_t * COVID19_{m,p,t} \\ +\delta Post_t + \omega COVID19_{m,p,t} + \gamma X_{m,p,t} &+ \kappa_m + \zeta_p + \psi_t + \epsilon_{m,p,t} \end{aligned} \quad (2)$$

where $Y_{m,p,t}$ denotes the turnout and vote share for the incumbent parties in municipality m, province p and time t. $Post_t$ is an indicator variable that equals one for 2020, the year when there were positive cases of exposure to COVID-19, and 0 otherwise; $COVID19_{m,p,t}$ is an indicator that equals one for municipalities with positive cases of COVID-19, and zero for those municipalities with no exposure to COVID-19 from the outbreak of the pandemic to the week before the elections. $X_{m,p,t}$ is a vector of socio-economic controls at the municipal level, κ_m, ζ_p, and ψ_t includes municipality, province, and year fixed-effects, and $\epsilon_{m,p,t}$ is the error term, clustered at the municipal level. Given that we expect bigger municipalities to be more exposed to COVID-19 cases, in some specifications we weight these models by population. In this context, the parameter of interest, β, measures the change in the turnout and vote share between areas exposed to COVID-19 and those that were not exposed, controlling for the previous trends.

To credibly identify the causal effects of COVID-19 on electoral outcomes, it is necessary for both the treated and the controlled municipalities to have exhibited similar trends in turnout and voting behaviour in elections prior the outbreak of the pandemic. To analyse whether treatment and control group trends were, indeed, similar, we show in figures 5a - 5c the electoral outcomes for the elections held in the years 2012 and 2016, − our pre-treatment periods −, and 2020 − our post-treatment period −. We can observe that, albeit different in levels, the evolution of the outcomes was similar for both groups.

To formally test for equality of trends, we conduct a falsification test using the years 2012 and 2016. Our results, in table A2 in the Appendix, show that there are no statistically significant differences in turnout or voting behaviour to the regional incumbent party between the municipalities in the control and treatment groups in the years prior to the COVID-19 pandemic. Albeit we observe some differences in voting behaviour to the national incumbent party, those are not stable across specifications. Overall, the graphical and formal analyses confirm that the difference-in-differences approach is adequate to estimate the causal effect of exposure to COVID-19 on electoral outcomes.[18]

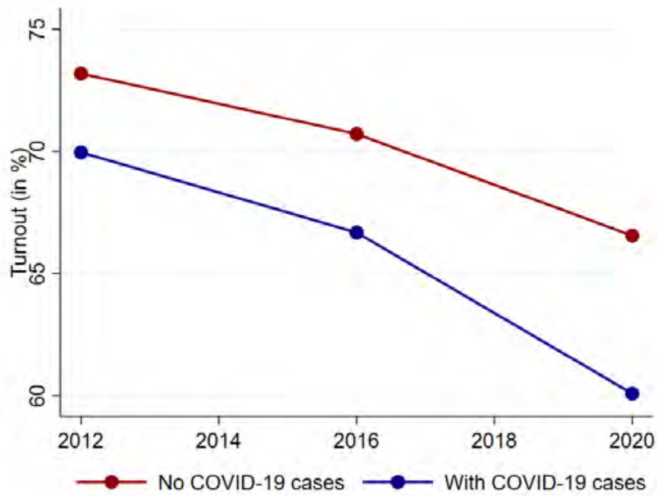
[18]See Appendix D for a detailed pre-trends analysis.

Covid Economics 50, 25 September 2020: 155-180

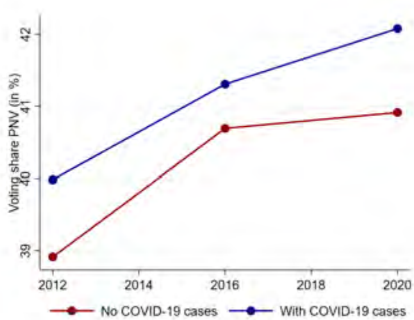
Covid Economics 50, 25 September 2020: 155-180

Figure 5: Turnout and Incumbents Votes in the 2020 Elections

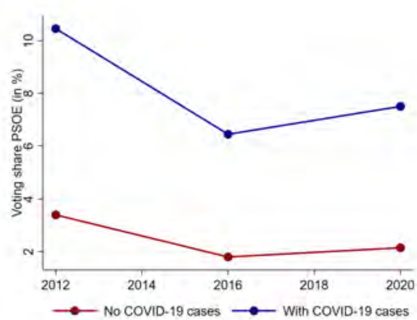
(a) Elections Turnout



(b) PNV Vote Share



(c) PSOE Vote Share



5 Results

5.1 OLS Results

Table 1 presents the results of estimating equation 1. Column 1 estimates the coefficient for the parameter β_1 without covariates, column 2 controls for socio-economic characteristics at the municipal level, and column 3 adds population weights. All specifications include municipality, province, and year fixed-effects.

The results in columns 1 and 2 show that an increase by one in the number of cases per 100,000 inhabitants is associated, on average, with a reduction in the participation of 0.002 percentage points (s.e. = 0.001). To better understand this

CEPR PRESS

magnitude, we can also interpret the estimates as follows: one extra case of COVID-19 over 100 people reduces participation around 2 percentage points. Looking at column 3, the result for the weighted model indicates that being exposed to one additional COVID-19 case over 100 inhabitants is associated with an average decrease in turnout of 5 percentage points (s.e. = 0.001).

Panel B of Table 1 reports the vote share to the regional incumbent party, the PNV. The estimated coefficients for specifications 1 and 2 suggest that being exposed to 1 additional case over 100 inhabitants is associated to an increase in the support for the regional government of around of 2 percentage points (s.e. = 0.001). After adding population weights in column 3, the results are no longer statistically significant. Finally, results in panel C show that there is no relation between the exposure to COVID-19 and the vote share for the PSOE, the incumbent party at the national level.

Table 1: OLS Results

	(1)	(2)	(3)
Panel A: Turnout			
Treatment intensity	-0.002***	-0.002***	-0.005***
	(0.001)	(0.001)	(0.001)
Treatment nonlinearity	0.000***	0.000***	0.000**
	(0.000)	(0.000)	(0.000)
Panel B: Support to PNV			
Treatment intensity	0.002**	0.002**	0.001
	(0.001)	(0.001)	(0.001)
Treatment nonlinearity	-0.000**	-0.000***	0.000
	(0.000)	(0.000)	(0.000)
Panel C: Support to PSOE			
Treatment intensity	-0.000	-0.000	0.001
	(0.000)	(0.000)	(0.001)
Treatment nonlinearity	-0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)
Covariates	No	Yes	Yes
Weights	No	No	Yes
Municipalities	251	251	251
Observations	753	753	753

Notes: ***, ** and * indicate 1%, 5% and 10% significance levels respectively. In parentheses are presented robust standard errors clustered by municipality. Municipality, region and year fixed effects are included in all specifications. In table A5 in the Appendix, we show that these results are not driven by the increase in COVID-19 cases in the municipality of Ordizia during the week of the elections.

The main concern about the OLS results in table 1 is they may be biased. In the next section we present the results of estimating the causal effect of COVID-19 on voting behaviour.

Covid Economics 50, 25 September 2020: 155-180

Covid Economics 50, 25 September 2020: 155-180

5.2 DiD Results

Table 2 presents the main findings from estimating equation 2. We start by presenting the results of the DiD analysis for turnout (panel A) and for incumbent's party vote share (panels B and C). The table reports three different models: column 1 presents the simplest regression without covariates, column 2 shows the estimation including socio-economic characteristics at the municipal level, and column 3 adds population weights. All regressions include municipality, province, and year-fixed effects.

The results in Panel A show that the turnover was, on average, between 2.3 and 3.8 percentage points lower in municipalities with positive cases of COVID-19. In terms of magnitude, treated municipalities presented a drop in the participation of 6.6 points between 2016 and 2020. Therefore, between 34 and 49 percent of the overall drop is explained by the exposure to the pandemic. This result is in line with the findings in Campante et al. (2020). They use evidence from the congressional and governmental 2014 U.S. elections and find lower participation rates in areas where concerns towards Ebola were higher.

Looking at the results in Panels B and C, we can see that the exposure to the pandemic did not have a statistical significant effect on PNV or PSOE vote shares, suggesting that citizens did not punish nor reward incumbents' parties management.

Table 2: DiD Results

	(1)	(2)	(3)
Panel A: Turnout			
Treatment effect	-2.838***	-2.273**	-3.328**
	(1.084)	(1.051)	(1.295)
Panel B: Support to PNV			
Treatment effect	0.324	0.193	0.531
	(1.137)	(1.240)	(1.715)
Panel C: Support to PSOE			
Treatment effect	-0.499	0.300	1.175
	(0.368)	(0.474)	(0.944)
Covariates	No	Yes	Yes
Weights	No	No	Yes
Municipalities	251	251	251
Observations	753	753	753

Notes: ***, ** and * indicate 1%, 5% and 10% significance levels respectively. In parentheses are presented robust standard errors clustered at the municipal level. Municipality, region and year fixed effects are included. In table A6, we show that these results are not driven by the increase in COVID-19 cases in the municipality of Ordizia during the week of the elections

5.3 Robustness checks

In this section, we look at whether our results are robust to different specifications of treatment and control groups. Namely, we estimate equation 2 defining treated municipalities as those exposed to more than the (i) average number of COVID-19 cases, and the (ii) median number of COVID-19 cases. Tables 3 and 4 present the results. We can see that the findings presented in table 2 are robust to the use of alternative definitions of treatment and control groups. Namely, turnout was lower in municipalities with more cases of COVID-19 per 100,000 inhabitants. In addition, we do not find any effect of exposure to the infection on vote shares to the incumbent's parties.

Table 3: DiD results using the average number of COVID-19 cases

	(1)	(2)	(3)
Panel A: Turnout			
Treatment effect	-1.865***	-1.639***	-2.756***
	(0.515)	(0.487)	(0.664)
Panel B: Support to PNV			
Treatment effect	0.860	0.693	-0.439
	(0.602)	(0.662)	(0.806)
Panel C: Support to PSOE			
Treatment effect	-0.499	0.300	1.175
	(0.368)	(0.474)	(0.944)
Covariates	No	Yes	Yes
Weights	No	No	Yes
Municipalities	251	251	251
Observations	753	753	753

Notes: ***, ** and * indicate 1%, 5% and 10% significance levels respectively. Robust standard errors clustered at the municipal level in parenthesis. Municipality, region and year fixed effects are included in all specifications.

6 Conclusions

The COVID-19 disease has been the biggest unexpected health shock in a century. Some countries registered large drops in GDP and huge increases in unemployment rates. Fatalities experienced a large rise as well. However, managing an epidemic not only requires tackling the health and economic consequences of the outbreak, but also its social and political dimensions. While public health and economics were the main affected fields at the beginning of the pandemic, the consequences may be extended to some other aspects, such as electoral outcomes.

In this paper, we study the extent to which exposure to the COVID-19 affects voting behaviour and electoral outcomes. To do so, we use evidence from the regional

Covid Economics 50, 25 September 2020: 155-180

Table 4: DiD results using median number of COVID-19 cases

	(1)	(2)	(3)
Panel A: Turnout			
Treatment effect	-1.887***	-1.606***	-2.584***
	(0.510)	(0.520)	(0.677)
Panel B: Support to PNV			
Treatment effect	0.860	0.693	-0.439
	(0.602)	(0.662)	(0.806)
Panel C: Support to PSOE			
Treatment effect	-0.143	-0.052	0.613**
	(0.250)	(0.269)	(0.302)
Covariates	No	Yes	Yes
Weights	No	No	Yes
Municipalities	251	251	251
Observations	753	753	753

Notes: ***, ** and * indicate 1%, 5% and 10% significance levels respectively. Robust standard errors clustered at the municipal level in parenthesis. Municipality, region and year fixed effects are included in all specifications.

elections that were hold in the Basque Country on July 12, 2020, and exploit the variation in exposure to COVID-19 cases across municipalities.

Our main results indicate that turnout was between 2.2 and 3.3 percentage points lower in those municipalities where there was a positive number of COVID-19 cases. This accounts for 27-41 percent of the registered drop in turnout. However, according to our estimations, this reduction in electoral participation did not result in changes in the support to incumbent parties. These results are robust to different specifications of the model and the treatment status.

As a potential mechanism for our results, we discuss fear. In the moment of the elections, there were almost no active cases of COVID-19 in the Basque Country. In addition, as figure 1b shows, the effects we observe cannot be driven by municipalities in the treated group being recently more exposed to the virus. This interpretation is consistent with the findings in Campante et al. (2020).

These results may be considered by policymakers when they design the forthcoming electoral campaigns. For example, Simonov et al. (2020) or Calvillo et al. (2020) find, for the U.S., that republican voters perceive less personal vulnerability to the virus and the virus's severity. This sentiment, together with the variation in the exposure to COVID-19 across states, may end up having an important role in the forthcoming U.S. general elections.

Covid Economics 50, 25 September 2020: 155-180

References

Adam-Troian, J., Bonetto, E., Varet, F., Arciszewski, T., and Guiller, T. (2020). Pathogen Threat Increases Electoral Success for Conservative Parties: Results from a Natural Experiment with COVID-19 in France. *PsyArXiv*.

Akee, R., Copeland, W., Holbein, J. B., and Simeonova, E. (2020). Human Capital and Voting Behavior Across Generations: Evidence from an Income Intervention. *American Political Science Review*, 114(2):609–616.

Amat, F., Arenas, A., Falcó-Gimeno, A., and Muñoz, J. (2020). Pandemics meet Democracy. Experimental Evidence from the COVID-19 Crisis in Spain. *SocArXiv*.

Arenas, A. (2020). Party Bans: Deterrence or Backlash? Evidence from the Basque Country. *Forthcoming in the The Quarterly Journal of Political Science*.

Beall, A. T., Hofer, M. K., and Schaller, M. (2016). Infections and Elections: Did an Ebola Outbreak Influence the 2014 US Federal Elections (and if so, how)? *Psychological Science*, 27(5):595–605.

Bellows, J. and Miguel, E. (2009). War and Local Collective Action in Sierra Leone. *Journal of Public Economics*, 93(11-12):1144–1157.

Blundell, R., Costa Dias, M., Joyce, R., and Xu, X. (2020). COVID-19 and Inequalities. *Fiscal Studies*, 41(2):291–319.

Bol, D., Giani, M., Blais, A., and Loewen, P. J. (2020). The Effect of COVID-19 Lockdowns on Political Support: Some Good News for Democracy? *European Journal of Political Research*.

Brodeur, A., Clark, A., Fleche, S., and Powdthavee, N. (2020). Covid-19, Lockdowns and Well-being: Evidence from Google Trends. *IZA Discussion Paper*, (13204).

Bullinger, L. R., Carr, J. B., and Packham, A. (2020). COVID-19 and Crime: Effects of Stay-at-Home Orders on Domestic Violence. *NBER WP*, (27667).

Calvillo, D. P., Ross, B. J., Garcia, R. J., Smelter, T. J., and Rutchick, A. M. (2020). Political Ideology Predicts Perceptions of the Threat of Covid-19 (and Susceptibility to Fake News about it). *Social Psychological and Personality Science*, pages 1–10.

Campante, F. R., Depetris-Chauvin, E., and Durante, R. (2020). The Virus of Fear: The Political Impact of Ebola in the US. *NBER WP*, (26897).

De la Calle, L. and Sánchez-Cuenca, I. (2013). Killing and Voting in the Basque Country: An Exploration of the Electoral Link between ETA and its Political Branch. *Terrorism and Political Violence*, 25(1):94–112.

De Vries, C. E., Bakker, B. N., Hobolt, S., and Arceneaux, K. (2020). Crisis Signaling: How Italy's Coronavirus Lockdown Affected Incumbent Support in Other European Countries. *Available at SSRN*.

Felgueroso, F., de la Fuente, A., Boscá, J., Doménech, R., Ferri, J., and Rodríguez, D. (2020). Aspectos Económicos de la Crisis del Covid-19. *Estudios sobre la Economía Española. Boletín de Seguimiento*, (1).

Fetzer, T., Hensel, L., Hermle, J., and Roth, C. (2020). Coronavirus Perceptions and Economic Anxiety. *Review of Economics and Statistics*, pages 1–36.

Gallego, J. (2018). Civil Conflict and Voting Behavior: Evidence from Colombia. *Conflict Management and Peace Science*, 35(6):601–621.

Gardeazabal, J. (2010). Vote shares in Spanish General Elections as a Fractional Response to the Economy and Conflict. *Economics of Security Working Paper*, (33).

Gasper, J. T. and Reeves, A. (2011). Make it rain? Retrospection and the Attentive Electorate in the Context of Natural Disasters. *American Journal of Political Science*, 55(2):340–355.

Getmansky, A. and Zeitzoff, T. (2014). Terrorism and Voting: The Effect of Rocket Threat on Voting in Israeli Elections. *American Political Science Review*, 108(3):588–604.

Giommoni1, T. and Loumeau, G. (2020). Lockdown and Voting Behaviour: A Natural Experiment on Postponed Elections during the COVID-19 Pandemic. *Covid Economics, Vetted and Real-Time Papers*, (41):69–99.

Guerrieri, V., Lorenzoni, G., Straub, L., and Werning, I. (2020). Macroeconomic Implications of COVID-19: Can Negative Supply Shocks Cause Demand Shortages? *NBER WP*, (26918).

Hacioglu, S., Känzig, D., and Surico, P. (2020). The Distributional Impact of the Pandemic. *CEPR Discussion Paper*, (15101).

Healy, A. and Malhotra, N. (2009). Myopic Voters and Natural Disaster Policy. *American Political Science Review*, 103(3):387–406.

Heersink, B., Peterson, B. D., and Jenkins, J. A. (2017). Disasters and Elections: Estimating the Net Effect of Damage and Relief in Historical Perspective. *Political Analysis*, 25(2):260–268.

Kibris, A. (2011). Funerals and Elections: The effects of Terrorism on Voting Behavior in Turkey. *Journal of Conflict Resolution*, 55(2):220–247.

Mansour, H., Rees, D. I., and Reeves, J. M. (2020). Voting and Political Participation in the Aftermath of the HIV/AIDS Epidemic. *NBER WP*, (27504).

Montalvo, J. G. (2011). Voting After the Bombings: A Natural Experiment on the Effect of Terrorist Attacks on Democratic Elections. *Review of Economics and Statistics*, 93(4):1146–1154.

Patel, J., Nielsen, F., Badiani, A., Assi, S., Unadkat, V., Patel, B., Ravindrane, R., and Wardle, H. (2020). Poverty, Inequality and COVID-19: the Forgotten Vulnerable. *Public Health*, 183:110–111.

Covid Economics 50, 25 September 2020: 155-180

Peri, G., Rees, D. I., and Smith, B. (2020). Terrorism, Political Opinions, and Election Outcomes: Evidence from Europe. *IZA Discussion Paper*, (13090).

Ramos, R. and Sanz, C. (2020). Backing the Incumbent in Difficult Times: The Electoral Impact of Wildfires. *Comparative Political Studies*, 53(3-4):469–499.

Sibley, C. G., Greaves, L. M., Satherley, N., Wilson, M. S., Overall, N. C., Lee, C. H., Milojev, P., Bulbulia, J., Osborne, D., Milfont, T. L., et al. (2020). Effects of the COVID-19 Pandemic and Nationwide Lockdown on Trust, Attitudes toward Government, and Well-being. *American Psychologist*, 75(5):618–630.

Simonov, A., Sacher, S. K., Dubé, J.-P. H., and Biswas, S. (2020). The Persuasive Effect of Fox News: Non-compliance with Social Distancing during the Covid-19 Pandemic. *National Bureau of Economic Research WP*, (27237).

Tiokhin, L. and Hruschka, D. (2017). No Evidence that an Ebola Outbreak Influenced Voting Preferences in the 2014 Elections after Controlling for Time-series Autocorrelation: A Commentary on Beall, Hofer, and Schaller (2016). *Psychological Science*, 28(9):1358–1360.

Covid Economics 50, 25 September 2020: 155-180

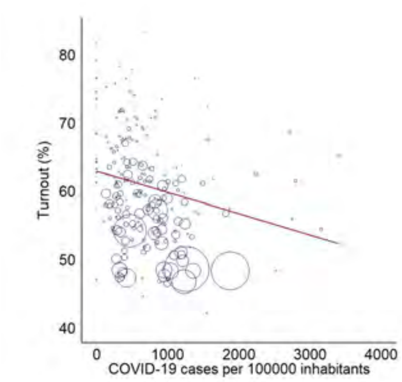
Appendix

This Appendix provides additional tables and figures, which are also discussed in the paper.

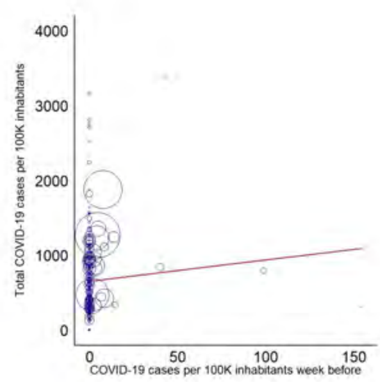
Appendix A: COVID-19, Turnout, and Fear of Exposure

Figure A1: COVID-19 Situation in the Basque Country

(a) COVID-19 and Turnout



(b) Total & Recent COVID-19



Notes: Figure A1a shows the correlation between the confirmed cases of COVID-19 and the turnout at the municipal level. We can observe that participation rates were lower in those municipalities exposed to more cases of COVID-19. Figure A1b presents the correlation between the accumulated number of cases from the outbreak of the pandemic to the week before the elections (y-axis) and the number of COVID-19 cases the week before the elections (x-axis). The correlation between these two variables is 0.05. In both graphs, the size of the markers reflects the population size of the municipality, and the municipality of Samaniego (with 8% of infections) is excluded.

Covid Economics 50, 25 September 2020: 155-180

CEPR PRESS

Appendix B: Descriptive Statistics

Table A1 presents the descriptive statistics.

Table A1: Descriptive statistics

	Mean	St. Dev.	Min.	Max.	Obs.
Panel A: Electoral Data					
Turnout	66.13	8.37	42.12	89.89	753
Vote share PNV	41.00	9.94	2.60	68.39	753
Vote share PSOE	7.43	6.90	0	35.89	753
Panel B: COVID-19					
Cases per 100k inhabitants	689.20	740.64	0	7983.19	251
Panel C: Socio-economic Characteristics					
Age distribution					
< 16	0.15	0.03	0.04	0.28	753
16-24	0.08	0.02	0.03	0.15	753
25-34	0.10	0.02	0.03	0.19	753
35-44	0.16	0.03	0.05	0.30	753
45-54	0.16	0.02	0.09	0.27	753
55-64	0.14	0.03	0.04	0.23	753
> 65	0.20	0.04	0.08	0.42	753
Education					
Tertiary educ.	14.39	5.03	4.69	36.22	753
Post-secondary educ.	7.29	1.89	1.56	13.70	753
Upper secondary educ.	19.94	2.96	9.15	29.98	753
Secondary educ.	19.51	3.26	8.22	29.09	753
Primary educ.	36.37	7.57	14.24	65.62	753
Non-educated	2.17	2.48	0	24.55	753
Illiterate	0.34	0.35	0	3.92	753
Unemployment	7.55	2.90	0.69	22.44	753
Population	8687.24	30455.31	110	350559	753

Notes: This table presents the summary statistics for the 251 municipalities in the Basque Country observed in years 2012, 2016, and 2020. Unemployment rates are measured in the month of February of each year. Due to lack of availability, we proxy educational data for 2102 and 2020 with data from 2013 and 2018, respectively. We proxy the data for age distribution in 2020 with data from 2018.

Covid Economics 50, 25 September 2020: 155-180

Appendix C: Placebo years 2012 and 2016

Table A2: Placebo Tests

	(1)	(2)	(3)
Panel A: Turnout			
Placebo effect	-0.812	-0.444	0.128
	(1.060)	(1.094)	(1.278)
Panel B: Support to PNV			
Placebo effect	-0.454	-0.687	-0.408
	(0.962)	(1.103)	(1.419)
Panel C: Support to PSOE			
Placebo effect	-2.418***	-1.514**	-2.178
	(0.588)	(0.735)	(1.719)
Covariates	No	Yes	Yes
Weights	No	No	Yes
Municipalities	251	251	251
Observations	502	502	502

Notes: ***, ** and * indicate 1%, 5% and 10% significance levels respectively. In parentheses are presented robust standard errors clustered at the municipal level. Municipality, province, and year fixed effects are included in all specifications.

Covid Economics 50, 25 September 2020: 155-180

Appendix D: Pre-trends before 2012

The Basque country is a region with a strong nationalist sentiment and a complicated political context. Since the first democratic elections after Franco's dictatorship, the PNV has always won, and nationalist parties usually receive more than a 50% of the vote share.

Besides, from 1958 until 2011, a paramilitary and terrorist group, named ETA, engaged in a campaign of assassinations and kidnappings with the main aim of achieving the independence of the Basque Country.[19] They also created a political branch named Batasuna, which was outlawed in Spain in 2003.[20] After the prohibition, Batasuna's members tried to enter in existing parties in order to be in the next elections. Some of them moved into the Communist Party of the Basque Homelands and the Basque Nationalist Action (ANV). The first one was banned in 2007. Nevertheless, the Court did not have time to prove that ANV had a strong connection with Batasuna (and therefore ETA) before the 2007 local elections. Hence, the Court only could ban ANV in a subset of municipalities (Arenas, 2020). In 2008, the party was completely banned. This created a lot of tension in the Basque Country, with numerous demonstrations in Bilbao to protest against the Court decision.

This fact was reflected in the 2009 regional elections, were null votes exceeded 100000. This represents 2410% more null votes than in previous elections. Finally, in 2011 Batasuna grouped different parties and organizations into one single party called "Bildu" (unity) in order to participate in the 2011 local elections. After some legal disputes, the Spanish Constitutional Court allowed Bildu to contest the 2011 local elections, being now a fully legalized party. Since the legalization of Bildu and after the disappearance of ETA, from 2012 on, the Basque country had shown more political stability. This is the underlying reason why we just employed 2012 and 2016 regional elections in our analysis.

The unstable situation described above, specially present in 2009, can be clearly observed in figures A2a - A2c.

Despite this instability in the Basque Country political system before 2012, we show the results of analysing electoral data covering the period 2001-2020. In this second approach, we run a set of pre-trends test, exploiting different timings when we define the pre-treatment and post-treatment periods. We estimate if control and treated municipalities exhibit different trends in the period 2001-2016 following the next approach:

$$Y_{m,p,t} = \alpha + \eta Period_t * COVID19_{m,p,t} + \delta Period_t + \omega COVID19_{m,p,t} + \kappa_m + \zeta_p + \psi_t + \epsilon_{m,p,t} \tag{3}$$

where η tests for common trends between treated and control municipalities.

[19]The consequences of violence in the Basque Country provoked by the terrorist organization ETA on voting was analyzed by De la Calle and Sánchez-Cuenca (2013). They estimate that killings by ETA had an electoral cost assumed by its political branch, Batasuna.

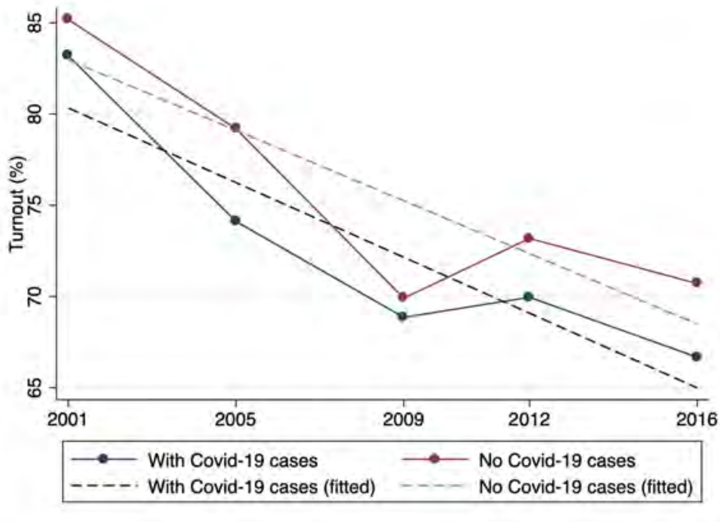
[20]Organic Law 6/2002 of Political Parties.

Covid Economics 50, 25 September 2020: 155-180

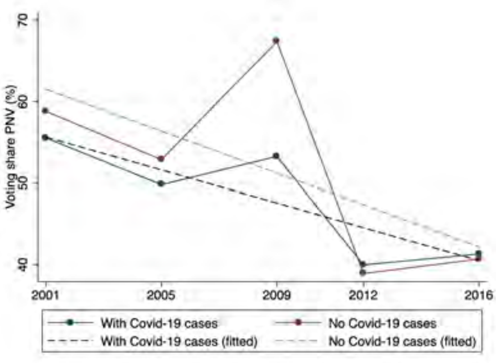
Covid Economics 50, 25 September 2020: 155-180

Figure A2: Commond Trends

(a) Elections Turnout



(b) PNV Vote Share



(c) PSOE Vote Share

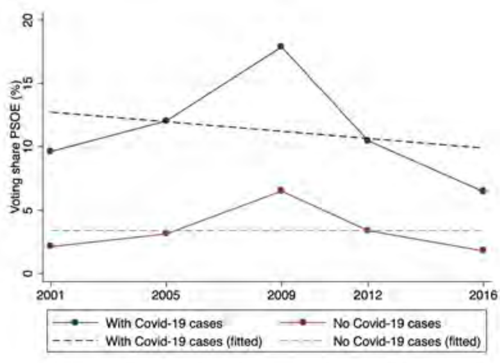


Table A3 presents the results for our second pre-trends approach. First, when we focus on electoral turnout, we find no evidence against the common trends assumptions. When looking at vote share to PNV, we find inconsistent results, with some specifications reporting small and imprecise negative effects, while other presenting large and statistically significant effects. We obtain similar findings looking at PSOE's vote shares.

Taking this into consideration, we re-estimate 2 using the electoral outcomes for the period 2001-2020. The results are presented in table ??. We can observe that turnover was, on average, 3.4 percentage points lower in municipalities with

Covid Economics 50, 25 September 2020: 155-180

Table A3: Pre-trends

	(1) Turnout	(2) PNV	(3) PSOE
Placebo 1	-1.381	-0.661	0.540
	(0.997)	(1.861)	(0.400)
Placebo 2	0.738	-1.046	-0.451
	(0.863)	(1.938)	(0.384)
Placebo 3	-0.943	7.626***	-3.361***
	(0.967)	(2.267)	(0.384)
Placebo 4	-1.214	5.435***	-4.031***
	(0.783)	(1.810)	(0.522)
Common trends	-0.230	1.169*	-0.741***
	(0.329)	(0.688)	(0.100)
Municipalities	251	251	251
Observations	1253	1253	1253

Notes: ***, ** and * indicate 1%, 5% and 10% significance levels respectively. Robust standard errors clustered by municipality in parenthesis. Municipality, region and year fixed effects are included in all specifications. Socio-demographic characteristics are not in the model. Two observations are missing for years 2001 and 2005 because Bolibar (a village in Bizkaia) was part of Markina-Xemein.

Placebo 1, Pre-treatment: 2001, post-treatment: 2005-2016. Placebo 2, Pre-treatment: 2001-2005, post-treatment: 2009-2016. Placebo 3, Pre-treatment: 2001-2009, post-treatment: 2012-2016. Placebo 4, Pre-treatment: 2001-2012, post-treatment: 2016.

positive cases of COVID-19. This result is in line with the main results in the paper (see table 2). On the other hand, we find a that being exposed to COVID-19 has a positive and statistically significant effect for the regional incumbent (PNV) vote share and a negative and statistically significant effect for the national incumbent (PSOE) vote share. Notwithstanding, these last results may be biased because they might be capturing differences in the pre-trends (see table A3).

Table A4: Treatment Effect Since 2001

	(1) Turnout	(2) PNV	(3) PSOE
Treatment effect	-3.404***	4.904***	-2.517***
	(1.080)	(1.718)	(0.472)
Municipalities	251	251	251
Observations	1504	1504	1504

Notes: ***, ** and * indicate 1%, 5% and 10% significance levels respectively. In parentheses are presented robust standard errors clustered by municipality. Two observations are missing for years 2001 and 2005 because Bolibar (a village in Bizkaia) was part of Markina-Xemein.

Covid Economics 50, 25 September 2020: 155-180

Appendix E: Robustness checks

Table A5, shows that results presented in table 1 are not driven by the increase in COVID-19 cases in the municipality of Ordizia during the week of the elections.

Table A5: OLS Results

	(1)	(2)	(3)
Panel A: Turnout			
Treatment intensity	-0.002***	-0.002***	-0.005***
	(0.001)	(0.001)	(0.001)
Treatment nonlinearity	0.000***	0.000***	0.000**
	(0.000)	(0.000)	(0.000)
Panel B: Support to PNV			
Treatment intensity	0.002**	0.002**	0.001
	(0.001)	(0.001)	(0.001)
Treatment nonlinearity	-0.000**	-0.000***	0.000
	(0.000)	(0.000)	(0.000)
Panel C: Support to PSOE			
Treatment intensity	-0.000	0.000	0.001*
	(0.000)	(0.000)	(0.000)
Treatment nonlinearity	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)
Covariates	No	Yes	Yes
Weights	No	No	Yes
Municipalities	250	250	250
Observations	750	750	750

Notes: ***, ** and * indicate 1%, 5% and 10% significance levels respectively. In parentheses are presented robust standard errors clustered by municipality. Municipality, region and year fixed effects are included in all specifications.

CEPR PRESS

Table A6, shows that results presented in table 2 are not driven by the increase in COVID-19 cases in the municipality of Ordizia during the week of the elections.

Table A6: DiD Results

	(1)	(2)	(3)
Panel A: Turnout			
Treatment effect	-2.829***	-2.256**	-3.326**
	(1.085)	(1.050)	(1.303)
Panel B: Support to PNV			
Treatment effect	0.341	0.163	0.611
	(1.137)	(1.241)	(1.721)
Panel C: Support to PSOE			
Treatment effect	-0.487	0.319	1.216
	(0.368)	(0.474)	(0.949)
Covariates	No	Yes	Yes
Weights	No	No	Yes
Municipalities	250	250	250
Observations	750	750	750

Notes: ***, ** and * indicate 1%, 5% and 10% significance levels respectively. In parentheses are presented robust standard errors clustered at the municipal level. Municipality, region and year fixed effects are included.

Covid Economics 50, 25 September 2020: 155-180

Covid Economics Issue 50, 25 September 2020

Empirical evidence of the effects of COVID-19 on voter turnout[1]

Miguel Vázquez-Carrero,[2] Joaquín Artés,[3] Carmen García[4] and Juan Luis Jiménez[5]

Date submitted: 22 September 2020; Date accepted: 23 September 2020

This paper studies the effects of COVID-19 on voting turnout using as a case study an election that took place right after the peak of the first wave of the pandemic, the Basque Country regional elections. With the spread of COVID-19 there is a fear that in-person voting will spread the virus, which adds an additional burden to voters that is expected to decrease turnout. We confirm this hypothesis using a difference-in-difference model. We find that COVID-19 caused turnout to decrease by approximately 4.7% in municipalities affected by the virus compared to those that at the time of the election had not been affected by it. This effect on turnout is higher for municipalities affected also by deaths from coronavirus than when affected only by infected cases.

Covid Economics 50, 25 September 2020: 181-208

1 The authors want to thank Raúl Sánchez from eldiario.es for retrieving the data on the incidence of the pandemic from official sources and kindly sharing it. The data, code, and other materials used in this paper will be made available at Harvard Dataverse.
2 Universidad Carlos III de Madrid.
3 ICEI, Universidad Complutense de Madrid.
4 Universidad de Las Palmas de Gran Canaria.
5 Universidad de Las Palmas de Gran Canaria.

Covid Economics 50, 25 September 2020: 181-208

1 Introduction

No other event in this century has affected the foundations of what most in western democracies consider 'normality' as the COVID-19 pandemic. The daily lives of millions have changed due to the unprecedented scale of this pandemic and the severe and unexpected measures that most governments have taken in their efforts to save lives and protect the economy. The economic and health consequences of the pandemic are being studied extensively, but little is still known about its political consequences and how citizens have responded to it when called to the polls in the midst of the pandemic.

Studying the effect of the COVID-19 pandemic into an election turnout is relevant to understand how the citizens are reacting to an uncertain scenario of anxiety, fear and extraordinary measures by the governments, and how this reaction can affect modern democracies [1]. Although voting is not the only way of participating in a democracy, it allows to aggregate the preferences of the citizens and keep political elites accountable to some extent [2]. Voting turnout is also an indicator of the public support for democracy and, thus, it is important to ensure its permanence [3].

In this paper, we offer the first observational study of the effect of the incidence of the COVID-19 pandemic on voter turnout by studying an election held after the first wave of the pandemic. We investigate the effect of the incidence of the pandemic in this outcome by exploiting the data on the regional election of 12 July 2020 in the Basque Country, Spain. These elections took place right after the Spanish national Government lifted the strict confinement measures that were in place since mid-March. The timing of the elections and the fact that its municipalities have been affected unequally by the pandemic provides an ideal setting to study this effect.

The Basque Country region of Spain is comparable in many aspects such as unemployment rates or quality of life to many other European countries and regions [4, 5, 6]. Also, in terms of territorial decentralization, its political system is comparable to many other regional parliaments in western democracies [7]. In this sense, this regional election provides an interesting case to study the incidence of the COVID-19 pandemic in the context of an election and how its political consequences could be extrapolated at greater scale [1]

To study this question, we use a difference-in-difference design in which we exploit the quasi-experimental variation in the incidence of the pandemic to address its effect on the election turnout using data at municipality level. We use three definitions of the treatment in relation with the distribution of infected cases and deaths from COVID-

[1]Citizens from several countries are still going to be called to the polls in the remainder of 2020: in Chile, on October 25, with the occasion of a national plebiscite, and in the United States, on November 3, with the occasion of the presidential elections.

19 and we compare municipalities affected by the pandemic (treated group) with those municipalities not affected by it (control group). Since the spread of the pandemic is also affected by other factors that could influence turnout too, our estimations include those variables as controls. By providing different measures and definitions of the treatment, we also study if there are different channels by which the pandemic could have influenced the voters' decision of going to the polls.

We find that the incidence of the pandemic has significantly reduced turnout on the municipalities of the treatment group with respect to the control group by an average -3.18 percentage points when all municipalities affected by COVID-19 cases are included in the treatment group, and by an average of -4.54 percentage points if only municipalities affected by COVID-19 cases and deceased are considered as treatment group. Compared to the mean turnout rate in the Basque Country of municipalities in the 2016 election (67.15 percent) the magnitude of the effect reaches -4.7% and -6.6%, respectively, which is a substantial decrease.

2 The effect of pandemics in voter turnout

A precedent of this research is the literature on the political consequences of pandemics and their effect on voter turnout. In the years before the COVID-19 pandemic, other viral diseases have spread globally and impacted western democracies generating research about these effects. Although not getting to spread massively within western democracies, the fear-driven effects of the pandemic of Ebola in the United States elections have been studied showing a negative effect on turnout [8, 9].

There is also evidence on the association between the prevalence of seasonal Influenza and lower turnout rates in Finland and the United States [10], and this same negative effect in turnout has been found for the case of the H1N1 Influenza Virus in Mexico [11]. Finally, and more importantly in terms of both cultural and political impact, Mansour et al. [12] studied the AIDS/HIV Pandemic on the United States Elections and its differential effect on Democrat and Republican turnout.

The political consequences of the COVID-19 pandemic in turnout have only been investigated by a few observational studies and none of them has studied the effects of its incidence on an election posterior to the first wave of the pandemic. For example, while studying vote choice in the Democratic Party Presidential Primaries, a study by Bisbee et al. [13] found a decline in voter turnout for the calls for primary elections after March 10 due to the pandemic. A similar result was found by Morris et al. [14], whose research showed a decrease in the turnout rate of the Wisconsin Primary in April 7.

In the context of multi-party elections, a working paper by Leininger and Schaub [15]

Covid Economics 50, 25 September 2020: 181-208

studied the municipal Bavarian election in Germany on March 15, when the pandemic still had not dominated the political agenda. They found that the turnout was not affected by its incidence. More recent results by Giommoni and Loumeau [16] studied the effects of the lockdown in the municipal elections in France. In this case, the municipal election had a first round in March 15 before a differential lockdown by department was ruled and a second electoral round in June 28 after this differential lockdown was lifted. They found an increased turnout in the locations where the stricter containment measures took place. Lastly, the inverse causal route, from election turnout to excess deaths, has also been investigated finding a positive relation between both [17, 18].

In summary, the previous literature that has studied the effect of the pandemic on voter turnout is based on evidence from the beginning of the pandemic in March. Consequently, there is no observational evidence about the effects of its incidence on voter turnout for an election in the midst of the pandemic because many calls to vote were postponed in order to prevent the spread of the virus by agglomerations.

We want to contribute to this literature by offering novel causal evidence on the effect of the incidence of the pandemic in voter turnout by studying a multi-party election that is comparable to other elections in European countries in terms of both the type of election and the characteristics of its voters.

To our knowledge, this is the first observational study that does so in the context of an election held after the first wave has transformed both the economy and the lives of the people called to vote.

Furthermore, the characteristics of the spread of the COVID-19 pandemic in the Basque Country (Spain) provide an ideal setting to study this effect due to the unequal incidence of the pandemic in municipalities, thus being a source of quasi-experimental variation that can be exploited to identify the causal effect of the pandemic (see Figures 1 and 2). Since there are municipalities *with no infected cases nor deaths*, this group of municipalities can be used as a control group in our comparisons with other municipalities that *had infected cases* (whether they have deaths or not), municipalities *with COVID-19 cases and deaths*, and municipalities *with COVID-19 but no deaths*.

Covid Economics 50, 25 September 2020: 181-208

CEPR PRESS

Covid Economics 50, 25 September 2020: 181-208

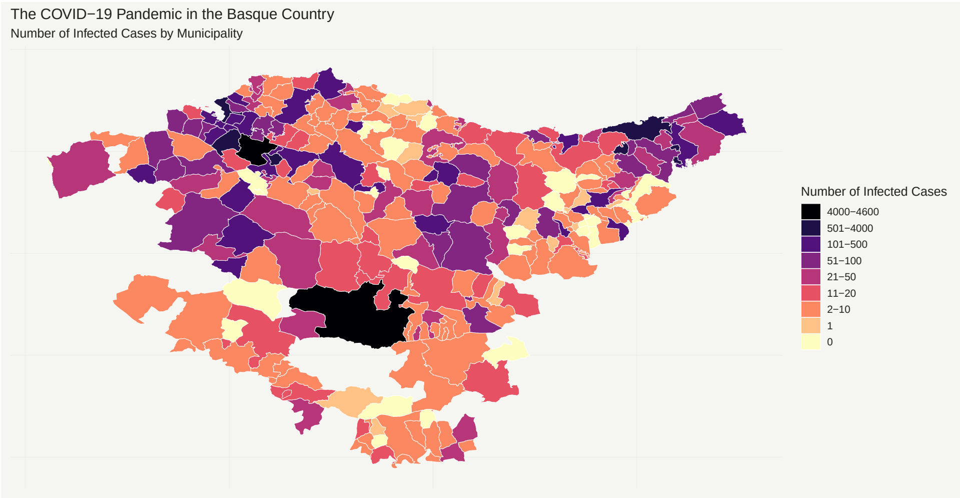


Figure 1: Incidence of the COVID-19 Pandemic by Municipalities. Infected Cases.

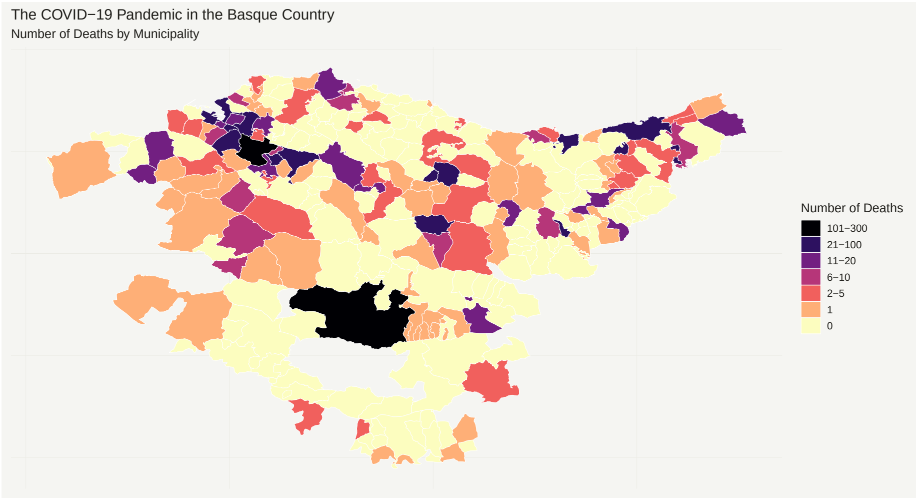


Figure 2: Incidence of the COVID-19 Pandemic by Municipalities. Deaths.

CEPR PRESS

3 The COVID-19 Pandemic in Spain and the Basque Country

Spain has been one of the most affected countries by the COVID-19 pandemic in the European Union [19]. In March 14, the Spanish Government declared a State of Alarm by which power was seized from regional authorities and mandated under a unified rule.

The Basque Parliamentary system includes a mix of national parties and regional parties that only participate in the Basque regional elections. This election was going to be held on April 4, but the uncontrolled outbreak of the pandemic and the declaration of the State of Alarm led to the postponement of this elections until July 12.

This State of Alarm allowed the government to impose restrictions on civil and economic liberties and, specially, to mandate a strict confinement at home that lasted until late June[2]. These measures had an impact into both the economy [20] and on the political attitudes and perceptions of the Spaniards [21]. Interestingly, Foremny et al. [22] found a substantial increase in the support for more public health expenditures, especially for those with affected relatives or living in the regions with more deaths from COVID-19.

Specifically, the incidence in the Basque Country was severe, with a total of 13,955 infected cases and 1,562 deaths from COVID-19 according the data from Spain Health Authorities on the week of the regional election. Evidence on the mental health effects of the COVID-19 pandemic in the Basque Country [23] and the rest of Spain [24, 25] show that the pandemic had created a situation of uncertainty and economic anxiety for the citizens called to vote.

4 Research Design

4.1 Data

We use datasets from three different sources in order to form an balanced panel data with observations for the municipalities and their information about political participation, incidence of COVID-19 and the sociodemographic factors that are used in our estimation. All the datasets are linked using a municipality identifier. Our sample includes data for all the municipalities in the Basque Country across five elections. The descriptive statistics for these variables are included in the Appendix, Table S1. The three sources of data are, respectively:

[2]The confinement measures were lifted at (from mid-May onwards) for the regions of Spain according to the evolution of the pandemic in their territories.

Covid Economics 50, 25 September 2020: 181-208

Covid Economics 50, 25 September 2020: 181-208

Basque Regional Elections Data. We use data from the Basque Government on the results of the five regional elections that have been held from 2005 to 2020 disaggregated at the level of municipality. These data include variables on turnout and eligible voters, which are used to compute the voter turnout. Since we are using regional election data these variables are the similar for all municipalities.

COVID-19 Data. We use data from the Basque Health Authorities on the evolution of the COVID-19 pandemic in the Basque Country disaggregated at the level of municipality. These data include variables on the number of cases of COVID-19 detected, the number of deaths from coronavirus and the population size of the municipalities, which are used to compute two rates of incidence for the pandemic: cases and deaths per each 100,000 people (*infrate* and *mrate* in our database, respectively).

Sociodemographic Variables Data. We use data from the Basque Institute of Statistics on several sociodemographic controls disaggregated at the level of municipality from 2005 to 2020. These data include data on municipality density, share of male/female population, population over the age of 65 and unemployment.

4.2 Estimation

To estimate the effect of the COVID-19 pandemic on voter turnout, we use a difference-in-difference approach where we exploit the variation in COVID-19 cases and deaths due to the pandemic occurring at different rates within municipalities. Prior to 2020, there were no cases nor deaths from COVID-19 and, consequently, no difference in terms of treatment between municipalities.

Four months after the outbreak in March, these municipalities have had an unequal incidence of the pandemic, with municipalities with no cases of COVID-19, municipalities with only infected cases of COVID-19 and municipalities with both infected cases and deaths from COVID-19. The distribution of municipalities within each category is shown in Appendix, Table S2.

Our empirical model can be summarized with the following equation:

$$turnout_{i,t} = \alpha + \beta DD_{i,t} + \gamma X_{i,t} + \delta_i + \eta_t + \epsilon_{i,t}$$

In the above equation, *turnout* refers to the turnout in municipality i on a election year t; *DD* is the treatment variable interacted with the treatment period; and X is a set of sociodemographic controls. In all the the specifications we account for time fixed effects, η_t, and municipality fixed effects, δ_i. In a standard DD design, the variables for treatment (treated) and time (after) control the common trends among treated and control groups and the interaction between both captures the DD estimate. Our approach is more demanding than a standard DD design since controlling for both municipality fixed effects and time fixed effects takes into account not just average pre-treatment differences between treatment and control municipalities, but also for other time-invariant factors idiosyncratic to each municipality that could be also correlated with the spread of the COVID-19.

Our coefficient of interest is β, which is the difference-in-difference estimate for the effect of the treatment in voter turnout. We use three definitions of the treatment in our approach to study the incidence of the COVID-19 pandemic with a variety of specifications. In the three definitions we assume a similar level of information among the treated and control groups since all the voters were involved in an uncertain scenario where the principal trusted source of information were the Official Authorities.

The first definition of the treatment employs the comparison between municipalities with COVID-19 cases, whether they have resulted in deaths or not (treatment group) and municipalities without COVID-19 cases (control group), not infected nor deaths. This is our benchmark specification.

A second definition selects the municipalities in which the infected cases of COVID-19 have resulted in deaths (treatment group) and compares those with municipalities with no cases of COVID-19 and, thus, not infected nor death (control group).

Our third definition selects those municipalities in which the infected cases of COVID-19 have not resulted in any deaths from coronavirus (treatment group) and compares them with those municipalities with no cases of COVID-19 and no deaths (control group). This allows us to study the differential effect of having infected people in a municipality.

A comparison between the estimates of the first and the second definition for the treatment allows us to study the differential effect of having deaths from coronavirus in a municipality.

Although the three definitions for the treatment imply a different composition for the treatment group, they are compared to the same control group, municipalities without infected cases or deaths due to the COVID-19 pandemic. An advantage of this approach is that using different definitions for our treatment allows us to study whether not only the treatment but also its intensity had an effect in voter turnout. For each definition of the treatment, we have a dichotomic measure (which takes value 1 if the municipality experienced any COVID-19 cases or deaths) and, when applicable, a continuous measure (rates of infected or deaths per each 100,000 people) that allows for a more precise estimate of the incidence of the pandemic in each municipality.

4.3 Identification

The identifying assumption for our design is that our outcome variable would have evolved similarly for both treatment and control groups in absence of the pandemic, namely, the

Covid Economics 50, 25 September 2020: 181-208

Covid Economics 50, 25 September 2020: 181-208

parallel trends assumption. For this assumption to hold we must compare the evolution of voter turnout prior to the pandemic for the treatment and control groups for the three definitions of the treatment.

As a first piece of evidence for the validity of the comparisons between the treatment and control groups in our specifications, we plot the evolution of voter turnout over time, which shows that the behavior of both groups before the pandemic is similar in the pre-treatment period (Appendix, Figures S3-S5). Particularly, the trends in 2012 and 2016 are closely parallel, thus providing a reliable source for the counterfactual in our estimation.

A more formal test of the fulfillment of the parallel trends assumption is provided by a regression framework in which we use only pre-treatment data and regress turnout on the interactions between time and treatment and time and controls. This test estimates a separate pre-treatment trend for treated and controls observations and allows us to test whether the slope is statistically similar on both trends. Table 1 shows the results of these regressions and the computed F-Tests for equality of coefficients for treatment and control trends are shown in Appendix, Tables S4-S6. The results of these tests confirm that overall trends are parallel in the treatment and control groups during the pre-treatment period in each of the three definitions of the treatment.

	(1)	(2)	(3)
	Turnout	Turnout	Turnout
Treatment Trend (TT)	-0.606***	-0.597***	-0.615***
	(0.0221)	(0.0233)	(0.0385)
Control Trend (CT)	-0.642***	-0.642***	-0.642***
	(0.0771)	(0.0773)	(0.0773)
Treatment	1,287***	1,268***	1,309***
	(44.33)	(47.04)	(77.34)
Control	1,364***	1,364***	1,364***
	(154.6)	(155.0)	(155.1)
Observations	1,000	580	544
R-squared	0.990	0.992	0.989
H0: (TT) = (CT)	Accept H0	Accept H0	Accept H0

Clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1: Parallel Trends Regression

A potential threat for our difference-in-difference design is that assignment into the treatment group is unlikely to be random, as there is evidence of higher rates of prevalence and mortality from COVID-19 in denser, male and older populations [26, 27].

Furthermore, turnout has also been found to be different in cities vs. rural areas and to depend on other demographics such as age or income [28]. These factors could affect both our treatment variable and outcome variable, making our coefficients biased. A standard solution to this potential problem of omitted variables is to include these alternative explanatory factors as controls in our estimation models [29]. We show the tests for comparison of means of our outcome and control variables for the three treatment definitions in Appendix, Table S3.

5 Results

	(1) Turnout	(2) Turnout	(3) Turnout	(4) Turnout	(5) Turnout	(6) Turnout	(7) Turnout
COVID-19 Cases Dummy	-3.184***						
	(0.958)						
COVID-19 Infection Rate		-0.00157***			-0.00179***		-0.00147**
		(0.000395)			(0.000559)		(0.000623)
COVID-19 Cases and Deaths Dummy			-4.541***				
			(0.964)				
COVID-19 Mortality Rate				-0.00644***			
				(0.00222)			
COVID-19 Cases but No Deaths Dummy						-1.983*	
						(1.012)	
Constant	73.25***	72.19***	75.52***	76.52***	74.86***	75.73***	75.33***
	(7.141)	(7.050)	(10.45)	(10.61)	(10.41)	(8.967)	(8.931)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed-Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Fixed-Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,250	1,250	725	725	725	680	680
R-squared	0.736	0.736	0.801	0.786	0.794	0.653	0.653
Number of municipalities	250	250	145	145	145	136	136

*Clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1*
In Columns 1 and 2 the treatment group is defined as municipalities with COVID-19 cases, whether they have deaths or not.
In Columns 3, 4 and 5 the treatment group is defined as municipalities with COVID-19 deaths.
In Columns 6 and 7 the treatment group is defined as municipalities with COVID-19 cases but no deaths.
The list of controls includes Population Density, Unemployment Rate, Share of Female Population and Share of Population over 65 years old.

Table 2: Difference in Difference Estimates. Dependent Variable: Voter Turnout

5.1 Decrease in Turnout as a consequence of the Pandemic

Table 2 shows the Difference-in-Difference regression estimates of the effect of the pandemic in voter turnout (the complete table can be found in Appendix, Table S7). Each column describes the estimates for a measure of the incidence of the pandemic. A common result of our specifications is that the incidence of the pandemic has caused a substantial decrease in voter turnout and that the significance of this effect does not

Covid Economics 50, 25 September 2020: 181-208

Covid Economics 50, 25 September 2020: 181-208

disappear after controlling for sociodemographic factors and municipality and time fixed effects. However, these estimates vary according to the definition of treatment and the measure used in the specification.

Columns 1 and 2 estimate the average effect of having infected cases in a municipality on its voter turnout, whether these infected cases turn or not into causalties. Column 1 shows that municipalities with infected cases have decreased its voter turnout on the election by -3.184 percent points with respect to those municipalities without infected cases (this would amount to a decrease of -4.7% with respect to the mean turnout in 2016). Column 2 provides a more precise estimate of this effect by using the infected rate per each 100,000 people as the measure of the treatment. The estimated effect of an increase of this rate by 1 person is a decrease in turnout of -0.00157 percentage points.

Columns 3 and 4 estimate the effect of the pandemic by using as treated group municipalities with deaths from COVID-19. Column 3 shows that the average effect in voter turnout of having victims of coronavirus is a decrease of -4.541 percentage points with respect to the control group (this would amount to a decrease of -6.7% with respect to the mean turnout in 2016). Column 4 provides a more detailed measure of this effect by using the mortality rate per each 100,000 people. Increasing the mortality rate in one person would cause a decrease in the voter turnout of -0.00644 percentage points with respect to the control group.

We provide a further specification within the subsample formed by the treatment and control group of Columns 3 and 4. Column 5 shows that the estimated effect on voter turnout of increasing the infection rate by 1 person is a decrease of -0.00179 percentage points. This result is very similar to the same estimate on the whole sample shown in column 2.

Finally, Columns 6 and 7 study the incidence of the pandemic by using the municipalities with infected cases but no deaths as the treatment group. Column 6 shows that the average effect of having infected cases of COVID-19 in a municipality is a decrease in voter turnout of -1.983 percentage points with respect to the control group. Column 7 includes a detailed measure of this effect with the infected rate showing that increasing the infected rate by 1 person decreased voter turnout by -0.00147. This effect is also very similar to the other estimations with the infected rates shown in Columns 2 and 5, which is a good indication that our results are robust.

5.2 Differential Effects and Heterogeneity of the Results

We could interpret the estimates of columns 1, 3 and 6 as estimates of the impact of the first infected case and the first deaths from coronavirus in a municipality. This would imply that having a first deceased case of coronavirus has deterred more voters than having

CEPR PRESS

the first infected in a municipality. These observations are consistent with our intuitive expectations about how the fear of dying from COVID-19 has bigger consequences than only getting the disease. The comparisons between the estimates in columns 2, 5 and 7 using mortality rates and infected rates also reflect this idea, being the effect found with the mortality rate estimate more than three times the size of the effects found with the infection rates estimates.

One obvious question is whether the reduction in turnout affects the composition of the electorate. We tested whether there are differences on the effects of COVID-19 on turnout among relatively older municipalities and denser municipalities, the hypothesis being that in older and denser populations the opportunity cost of in-person voting would be higher. We did not find, however any clear composition effect.

5.3 Robustness of results

In order to assess the robustness of our results, we perform two placebo tests in which we alter the sample to estimate our DD design in settings that, if the identification correctly captures a genuine treatment effect in turnout, it should not find a significant treatment effect.

In the first placebo model, we randomize the dichotomic for the measures of the treatment definitions across municipalities in 2020. Since the assignment of the treatment is random, there should not be an effect of the pandemic in voting turnout and the placebo difference-in-difference estimates should not be significant.

In the second placebo model, we replicate the main analysis but altering the sample in two ways. First, we use only the observation in the pre-treatment period. Second, we create new treatment variables reconsidering 2005 and 2009 as the artificial pre-treatment period and 2012 and 2016 as the artificial post-treatment period. This way, there should not be an effect of our placebo treatment regardless of the measure if the evidence is consistent with the parallel trends assumption in our pre-treatment period.

The result of the first placebo test is displayed in Appendix, Table S8. The DD estimate of the placebo treatment in each specification is not significant, which is what we should expect since the treatment has been randomized. Like in our main estimates, we find that the fixed effects of municipality and year capture a high share of variation for our outcome but that in this case the placebo estimates are not significant.

Our second placebo test is displayed in Appendix, Table S9. The placebo DD estimate of the treatment after 2009 is not significant at any level for out benchmark specification and our specification for municipalities with infected cases but no deaths. The Placebo DD Estimate on the specification for municipalities with deaths from COVID-19 is significant at a 10 percent level of confidence. This could raise some caution about the results of these

Covid Economics 50, 25 September 2020: 181-208

CEPR PRESS

Covid Economics 50, 25 September 2020: 181-208

estimates, but is nonetheless a nonsignificant coefficient at standard levels of confidence. In addition, working with more observations for the pre-treatment period that follow parallel trends (as it is the case in our analysis and is shown in Appendix, Tables S4-S6) would soften this issue. Furthermore, our testing of the identification assumption is robust and the randomized placebo test reinforces this validy.

Taken together, these tests results support the validity of our Difference-in-Difference design and show that the estimates of our various specifications are robust.

6 Discussion

A standard rational theory approach to the mechanisms behind voter turnout indicates that individuals may weigh the costs and benefits (steaming from individual expected gains or to other factors such as a sense of civic duty) of voting before deciding whether to vote or not (see Mueller [30] for a review). These costs may include external factors that make voting convenient, such as the distance to the voting site [31], or the weather on voting day [32]. More relevant to our research, there is recent evidence on the importance of health factors into turnout [33].

Consequently, we can discuss two mechanisms that may have been at work in the decision of voting in an election held in the midst of the COVID-19 pandemic. A first mechanism links the perception of the risk of getting infected (or infecting others) with an increased cost of voting, thus inducing to a decrease in the in-person voting turnout and an increase in the alternatives to in-person voting, such as vote by mail. A second mechanism links the renewed importance of which government will manage the pandemic with an increased value of the act of voting, thus inducing to an increase in voting turnout. Our data sources combine data from both presential and by-mail voting and, thus, we cannot test the effect of each mechanism separately, only observe which of both dominates[3].

Regardless of the definition of the incidence of the pandemic and the measure we use, all of our estimations show that the pandemic has caused a substantial decrease on voter turnout and that this effect does not disappear after controlling for both fixed effects and sociodemographic factors. The size of these effects is quite large, considering that the mean turnout for the 2016 election for municipalities was 67.15 percent and that this estimations control for sociodemographic factors and both time and municipality fixed

[3] In Spain, to vote by mail citizens have to apply several weeks prior to election day. Aggregate data about the number of applications to vote by mail shows a more-than double increase for this election. In the 2016 elections, there were 51,981 applications, a 3.03 percent of the census. In the 2020 election, there were 124,473 applications, a 7.24 percent of the census. This is consistent the first mechanism being the one that dominates.

effects. The magnitude of the effect reaches -4.7% with our benchmark specification for municipalities with infected, which is a substantial decrease. We have also shown that this effect on turnout is higher for municipalities affected by deaths, reaching -6.6% with the dummy estimate. When considering the analogous measures for the intensity of the treatment, we also find a three times higher measure of the effect when mortality rate than when using the infection rate. These results show further light on the consequences that the impact of the pandemic is having into the perception of fear, risk and economic anxiety among the people called to the polls, thus reducing voter turnout.

This decrease in turnout has important implications for modern democracies. If the results obtained in these estimates can be extrapolated at greater scale, the political consequences in terms of loss of political inclusion, representation and legitimacy could be substantial. Consequently, these results show the potential value of public policies with the aim of protecting political participation from the consequences of the COVID-19 Pandemic. In this sense, governments should consider measures that improve the safety of the polling stations, other forms non-presential voting (when secure) as well as measures to overcome the pandemic or prevent its spread. The benefits of this policies should be considered not only for its economic benefits, but also to avoid its negative political consequences.

References

[1] T. Landman and L. D. G. Splendore, "Pandemic democracy: elections and COVID-19," *Journal of Risk Research*, pp. 1–7, 2020.

[2] R. A. Dahl, *Democracy and its critics*. New Haven: Yale Univ. Press, 1991. OCLC: 249206498.

[3] C. Claassen, "Does Public Support Help Democracy Survive?," *American Journal of Political Science*, vol. 64, no. 1, pp. 118–134, 2020. Number: 1.

[4] OECD, "Building subjective well-being indicators at the subnational level: A preliminary assessment in OECD regions," Tech. Rep. 2016/03, Mar. 2016. Issue: 2016/03 Series: OECD Regional Development Working Papers Volume: 2016/03.

[5] OECD, *OECD Regions and Cities at a Glance 2018*. OECD Regions and Cities at a Glance, OECD, Oct. 2018.

[6] Eurostat, "European Regional Statistics – Reference Guide," 2003.

Covid Economics 50, 25 September 2020: 181-208

[7] C. Colino, "Decentralization in Spain: federal evolution and performance of the estado autonómico," in *The Oxford Handbook of Spanish Politics* (D. Muro and I. Lago, eds.), pp. 61–81, Oxford University Press, 2020.

[8] A. T. Beall, M. K. Hofer, and M. Schaller, "Infections and Elections: Did an Ebola Outbreak Influence the 2014 U.S. Federal Elections (and if so, How)?," *Psychological Science*, vol. 27, no. 5, pp. 595–605, 2016. Number: 5.

[9] F. Campante, E. Depetris-Chauvin, and R. Durante, "The Virus of Fear: The Political Impact of Ebola in the U.S.," Tech. Rep. w26897, National Bureau of Economic Research, Cambridge, MA, 2020. Issue: w26897.

[10] R. Urbatsch, "Influenza and Voter Turnout: Influenza and Voter Turnout," *Scandinavian Political Studies*, vol. 40, pp. 107–119, Mar. 2017. Number: 1.

[11] E. Gutiérrez, J. Meriläinen, and A. Rubli, "Electoral Repercussions of a Pandemic: Evidence from the 2009 H1N1 Outbreak," *SSRN Electronic Journal*, 2020.

[12] H. Mansour, D. Rees, and J. Reeves, "Voting and Political Participation in the Aftermath of the HIV/AIDS Epidemic," Tech. Rep. w27504, National Bureau of Economic Research, Cambridge, MA, July 2020. Issue: w27504.

[13] J. Bisbee and D. Honig, "Flight to safety: 2020 Democratic primary election results and Covid-19," *Journal of COVID Economics*, no. 3, p. 31, 2020.

[14] K. Morris and P. Miller, "Voting in a Pandemic: COVID-19 and Primary Turnout in Milwaukee, Wisconsin," *SSRN Electronic Journal*, 2020.

[15] A. Leininger and M. Schaub, "Voting at the dawn of a global pandemic," preprint, SocArXiv, Apr. 2020.

[16] T. Giommoni and G. Loumeau, "Lockdown and Voting Behaviour: A Natural Experiment on Postponed Elections during the COVID-19 Pandemic," *SSRN Electronic Journal*, 2020.

[17] S. Bertoli, L. Guichard, and F. Marchetta, "Turnout in the Municipal Elections of March 2020 and Excess Mortality during the COVID-19 Epidemic in France," *IZA Discussion Papers 13335, Institute of Labor Economics (IZA)*, p. 22, 2020.

[18] W. D. Flanders, W. D. Flanders, and M. Goodman, "The association of voter turnout with county-level coronavirus disease 2019 occurrence early in the pandemic," *Annals of Epidemiology*, vol. 49, pp. 42–49, 2020.

Covid Economics 50, 25 September 2020: 181-208

[19] M. Roser, H. Ritchie, E. Ortiz-Ospina, and Hasell, Joe, "Coronavirus Pandemic (COVID-19)," 2020.

[20] E. Gutiérrez and Moral-Benito, Enrique, "Containment measures, employment and the spread of COVID-19 in Spanish municipalities. Documentos Ocasionales N.º 2022," p. 22, 2020.

[21] F. Amat, A. Arenas, A. Falcó-Gimeno, and J. Muñoz, "Pandemics meet democracy. Experimental evidence from the COVID-19 crisis in Spain," preprint, SocArXiv, 2020.

[22] D. Foremny, P. Sorribas-Navarro, and J. Vall Castelló, "Living at the Peak: Health and Public Finance During the Covid-19 Pandemic," *SSRN Electronic Journal*, 2020.

[23] N. Ozamiz-Etxebarria, N. Idoiaga Mondragon, M. Dosil Santamaría, and M. Picaza Gorrotxategi, "Psychological Symptoms During the Two Stages of Lockdown in Response to the COVID-19 Outbreak: An Investigation in a Sample of Citizens in Northern Spain," *Frontiers in Psychology*, vol. 11, p. 1491, June 2020.

[24] R. Rodríguez-Rey, H. Garrido-Hernansaiz, and S. Collado, "Psychological Impact and Associated Factors During the Initial Stage of the Coronavirus (COVID-19) Pandemic Among the General Population in Spain," *Frontiers in Psychology*, vol. 11, p. 1540, June 2020.

[25] C. González-Sanguino, B. Ausín, M. n. Castellanos, J. Saiz, A. López-Gómez, C. Ugidos, and M. Muñoz, "Mental health consequences during the initial stage of the 2020 Coronavirus pandemic (COVID-19) in Spain," *Brain, Behavior, and Immunity*, vol. 87, pp. 172–176, 2020.

[26] S. Hamidi, S. Sabouri, and R. Ewing, "Does Density Aggravate the COVID-19 Pandemic?: Early Findings and Lessons for Planners," *Journal of the American Planning Association*, pp. 1–15, 2020.

[27] J. B. Dowd, L. Andriano, D. M. Brazel, V. Rotondi, P. Block, X. Ding, Y. Liu, and M. C. Mills, "Demographic science aids in understanding the spread and fatality rates of COVID-19," *Proceedings of the National Academy of Sciences*, vol. 117, no. 18, pp. 9696–9698, 2020. Number: 18.

[28] J. E. Leighley and J. Nagler, *Who votes now? demographics, issues, inequality and turnout in the United States*. Princeton: Princeton University Press, 2014.

Covid Economics 50, 25 September 2020: 181-208

[29] O. Y. Nizalova and I. Murtazashvili, “Exogenous Treatment and Endogenous Factors: Vanishing of Omitted Variable Bias on the Interaction Term,” *Journal of Econometric Methods*, vol. 5, Jan. 2016. Number: 1.

[30] D. C. Mueller, “Public Choice: An Introduction,” in *Readings in public choice and constitutional political economy*, p. 16, Springer, 2008. Mueller, Dennis C. ”Public choice: an introduction.” Readings in public choice and constitutional political economy. Springer, Boston, MA, 2008. 31-46.

[31] J. J. Dyck and J. G. Gimpel, “Distance, Turnout, and the Convenience of Voting*,” *Social Science Quarterly*, vol. 86, no. 3, pp. 531–548, 2005. Number: 3.

[32] J. Artés, “The rain in Spain: Turnout and partisan voting in Spanish elections,” *European Journal of Political Economy*, vol. 34, pp. 126–141, 2014.

[33] J. Pacheco and J. Fletcher, “Incorporating Health into Studies of Political Behavior: Evidence for Turnout and Partisanship,” *Political Research Quarterly*, vol. 68, pp. 104–116, Mar. 2015. Number: 1.

Covid Economics 50, 25 September 2020: 181-208

CEPR PRESS

Appendix

Database

	N	mean	sd	min	max
Municipalities	1,250	125.5	72.20	1	250
Infected Cases	1,250	17.24	191.9	0	4,668
Number of Deaths	1,250	1.296	14.58	0	364
COVID-19 Cases Dummy	1,250	0.876	0.330	0	1
COVID-19 Cases and Deaths Dummy	725	0.786	0.410	0	1
COVID-19 Cases but No Deaths Dummy	680	0.772	0.420	0	1
COVID-19 Infection Rate	1,250	142.2	416.8	0	6,013
COVID-19 Mortality Rate	1,250	8.864	48.63	0	949.4
Control Dummy	1,250	0.124	0.330	0	1
Turnout	1,250	68.41	8.563	38.85	98.63
Unemployment Rate	1,250	4.469	1.997	0.291	30.73
% Female Population	1,250	45.16	2.863	36.45	58.86
% Population Over 65	1,250	19.88	4.473	8.170	41.60
Population Density	1,250	512.4	1,458	3.700	15,276

Table S1: Summary Statistics

Covid Economics 50, 25 September 2020: 181-208

CEPR PRESS

The incidence of COVID-19 Pandemic in the Basque Country (Spain)

	Not Deceased	Deceased	Total
Not Infected	31	0	31
Infected	105	114	219
Total	136	114	250

Table S2: Distribution of Municipalities by the Incidence of the COVID-19 Pandemic.

Covid Economics 50, 25 September 2020: 181-208

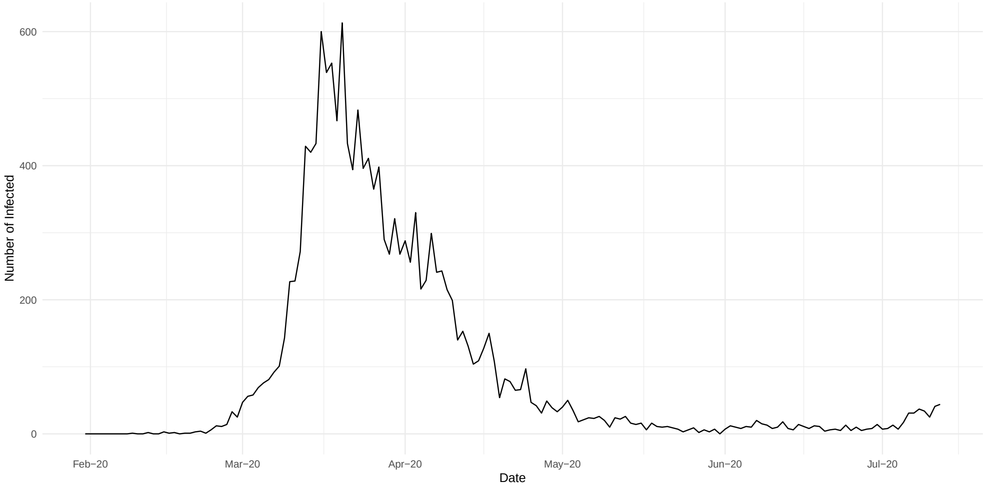


Figure S1: Evolution of the Pandemic. Daily New Infected Cases

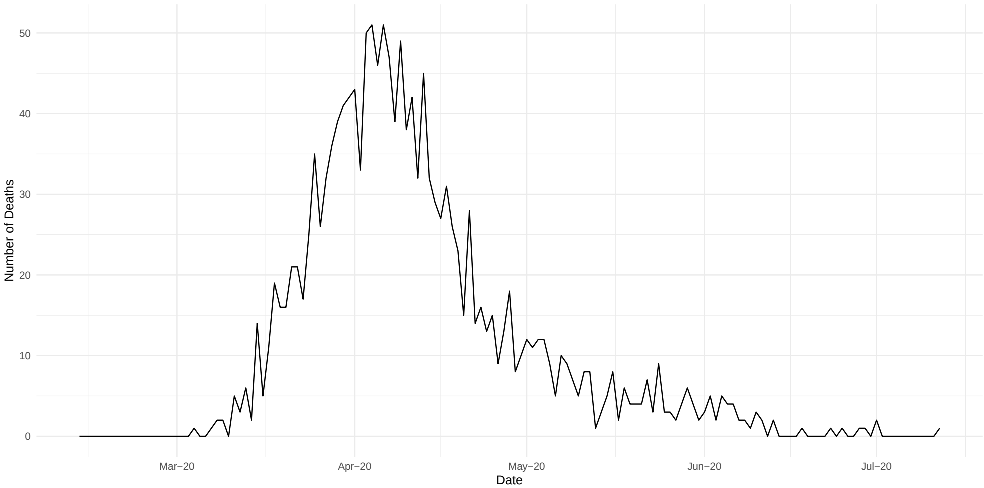


Figure S2: Evolution of the Pandemic. Daily Deaths

Covid Economics 50, 25 September 2020: 181-208

CEPR PRESS

Distribution of Variables within Treatment and Control

	Control Mean	Treated Mean	Mean Difference	P-Value
	Treatment: (All) Municipalities with COVID-19 Cases			
Turnout	73.24	69.88	3.36	0
Density	46.57	577.99	-531.42	0
Unemployment Rate	3.09	4.46	-1.36	0
% Female Population	44.33	45.80	-1.46	0
% Poputation Over 65	21.87	19.29	2.58	0
	Treatment: (Only) Municipalities with COVID-19 Cases and Deaths			
Turnout	73.24	68.36	4.87	0
Density	46.57	1,028.92	-982.34	0
Unemployment Rate	3.09	5.08	-1.98	0
% Female Population	44.33	46.02	-1.69	0
% Poputation Over 65	21.87	18.70	3.16	0
	Treatment: (Only) Municipalities with COVID-19 Cases but No Deaths			
Turnout	73.24	71.521	1.726	.037
Density	46.57	88.424	-41.84	.001
Unemployment Rate	3.09	3.787	-.694	0
% Female Population	44.33	45.558	-1.226	0
% Poputation Over 65	21.87	19.925	1.945	0

Table S3: T-Test. Treated and Control Groups for Outcome and Sociodemographic Variables in the Pre-treatment period

Covid Economics 50, 25 September 2020: 181-208

CEPR PRESS

Parallel Trends

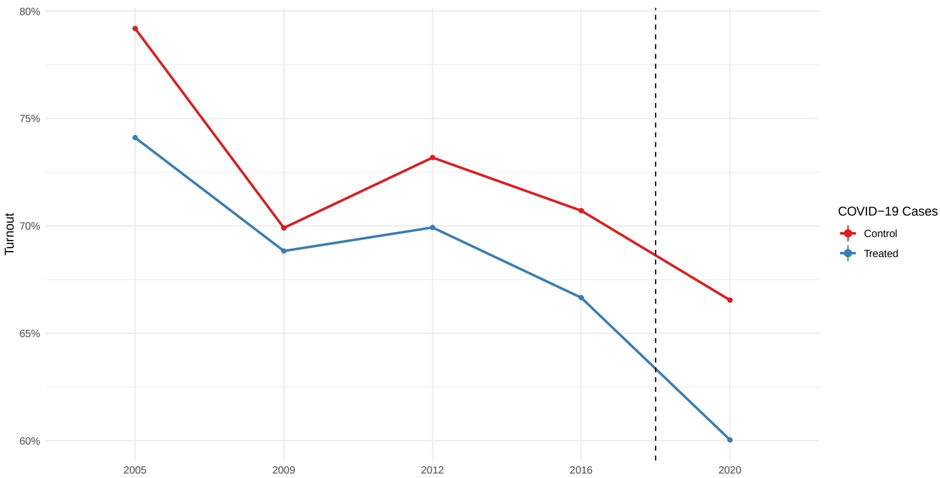


Figure S3: Treatment: (All) Municipalities with COVID-19 Cases

Covid Economics 50, 25 September 2020: 181-208

CEPR PRESS

Covid Economics 50, 25 September 2020: 181-208

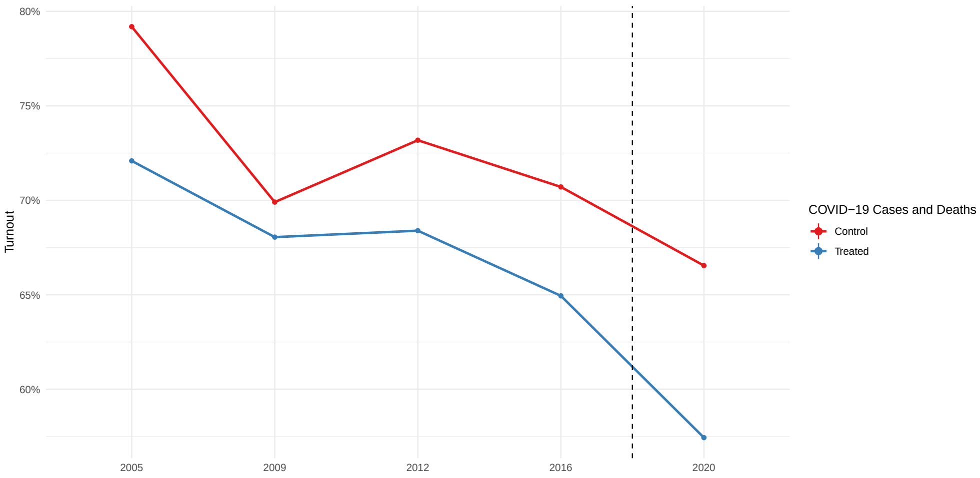


Figure S4: Treatment: (Only) Municipalities with COVID-19 Cases and Deaths

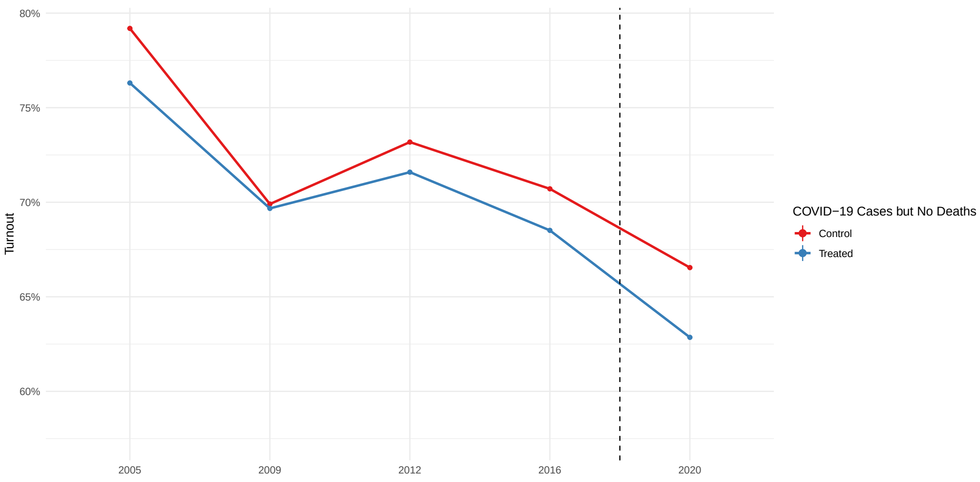


Figure S5: Treatment: (Only) Municipalities with COVID-19 Cases but No Deaths

CEPR PRESS

Covid Economics 50, 25 September 2020: 181-208

	(1)
	Turnout
Trend Infected	-0.606***
	(0.0221)
Trend Control	-0.642***
	(0.0771)
Dummy Infected	1,287***
	(44.33)
Dummy Control	1,364***
	(154.6)
Observations	1,000
R-squared	0.990

Clustered standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$
The pre-treatment trends for the control and treatment group are not significantly different, F(1, 249) = 0.21, p = 0.6480

Table S4: Parallel Trends Test. Treatment: (All) Municipalities with COVID-19 Cases

	(1)
	Turnout
Trend COVID-19 Cases and Deaths	-0.597***
	(0.0233)
Trend Control	-0.642***
	(0.0773)
Dummy COVID-19 Cases and Deaths	1,268***
	(47.04)
Dummy Control	1,364***
	(155.0)
Observations	580
R-squared	0.992

Clustered standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$
The pre-treatment trends for the control and treatment group are not significantly different, F(1, 144) = 0.32, p = 0.5728

Table S5: Parallel Trends Test. Treatment: (Only) Municipalities with COVID-19 Cases and Deaths

CEPR PRESS

Covid Economics 50, 25 September 2020: 181-208

	(1) Turnout
Trend COVID-19 Cases but No Deaths	-0.615***
	(0.0385)
Trend Control	-0.642***
	(0.0773)
Dummy COVID-19 Cases but No Deaths	1,309***
	(77.34)
Dummy Control	1,364***
	(155.1)
Observations	544
R-squared	0.989

Clustered standard errors in parentheses
**** p<0.01, ** p<0.05, * p<0.1*
The pre-treatment trends for the control and treatment group are not significantly different, F(1, 135) = 0.10, p = 0.7561

Table S6: Parallel Trends Test. Treatment: (Only) Municipalities with COVID-19 Cases but No Deaths

CEPR PRESS

Difference-in-Difference Estimates

	(1) Turnout	(2) Turnout	(3) Turnout	(4) Turnout	(5) Turnout	(6) Turnout	(7) Turnout
COVID-19 Cases Dummy	-3.184***						
	(0.958)						
COVID-19 Infection Rate		-0.00157***			-0.00179***		-0.00147**
		(0.000395)			(0.000559)		(0.000623)
COVID-19 Cases and Deaths Dummy			-4.541***				
			(0.964)				
COVID-19 Mortality Rate				-0.00644***			
				(0.00222)			
COVID-19 Cases but No Deaths Dummy						-1.983*	
						(1.012)	
Population Density	0.000561	0.000475	0.000516	0.000433	0.000438	-0.00474	-0.00656
	(0.000889)	(0.000861)	(0.000847)	(0.000895)	(0.000835)	(0.0153)	(0.0158)
Unemployment Rate	-0.0942	-0.0613	-0.0221	0.0301	0.0342	-0.0661	-0.0570
	(0.121)	(0.131)	(0.110)	(0.136)	(0.118)	(0.134)	(0.143)
% Female	0.0289	0.0524	-0.0758	-0.0593	-0.0439	0.0352	0.0505
	(0.112)	(0.111)	(0.157)	(0.162)	(0.159)	(0.136)	(0.136)
% Over 65 years old	0.00506	0.00146	0.0667	-0.0297	0.0181	0.00554	-0.00346
	(0.108)	(0.104)	(0.165)	(0.162)	(0.160)	(0.142)	(0.140)
2009	-5.629***	-5.677***	-5.122***	-5.249***	-5.223***	-7.121***	-7.144***
	(0.390)	(0.398)	(0.405)	(0.426)	(0.407)	(0.573)	(0.576)
2012	-4.114***	-4.178***	-4.241***	-4.381***	-4.363***	-4.785***	-4.804***
	(0.474)	(0.493)	(0.454)	(0.496)	(0.457)	(0.617)	(0.627)
2016	-7.294***	-7.303***	-7.679***	-7.656***	-7.666***	-7.708***	-7.696***
	(0.488)	(0.500)	(0.478)	(0.513)	(0.484)	(0.648)	(0.654)
2020	-10.78***	-12.45***	-11.00***	-13.98***	-13.20***	-11.45***	-12.18***
	(1.082)	(0.654)	(1.121)	(0.653)	(0.735)	(1.176)	(0.864)
Constant	73.25***	72.19***	75.52***	76.52***	74.86***	75.73***	75.33***
	(7.141)	(7.050)	(10.45)	(10.61)	(10.41)	(8.967)	(8.931)
Time Fixed-Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Fixed-Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,250	1,250	725	725	725	680	680
R-squared	0.736	0.736	0.801	0.786	0.794	0.653	0.653
Number of municipalities	250	250	145	145	145	136	136

*Clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1*

In Columns 1 and 2 the treatment group is defined as municipalities with COVID-19 cases, whether they have deaths or not.

In Columns 3, 4 and 5 the treatment group is defined as municipalities with COVID-19 deaths.

In Columns 6 and 7 the treatment group is defined as municipalities with COVID-19 cases but no deaths.

Table S7: Difference in Difference Estimates. Dependent Variable: Voter Turnout

Covid Economics 50, 25 September 2020: 181-208

CEPR PRESS

Robustness Checks

	(1) Turnout	(2) Turnout	(3) Turnout
COVID-19 Cases Dummy	-1.217		
	(0.757)		
COVID-19 Cases and Deaths Dummy		-0.682	
		(0.749)	
COVID-19 Cases but No Deaths Dummy			-0.812
			(0.847)
Population Density	0.000741	0.00112	4.72e-05
	(0.000893)	(0.00106)	(0.000959)
Unemployment Rate	-0.0579	-0.0642	-0.00553
	(0.148)	(0.156)	(0.142)
% Female	0.0273	-0.000585	0.0983
	(0.111)	(0.113)	(0.123)
% Over 65 years old	-0.0357	-0.0307	0.0304
	(0.104)	(0.102)	(0.114)
2009	-5.724***	-5.719***	-5.721***
	(0.413)	(0.422)	(0.417)
2012	-4.251***	-4.269***	-4.251***
	(0.528)	(0.544)	(0.522)
2016	-7.371***	-7.436***	-7.309***
	(0.524)	(0.534)	(0.534)
2020	-12.56***	-12.95***	-13.39***
	(0.933)	(0.858)	(0.948)
Constant	73.96***	74.94***	69.51***
	(7.029)	(7.118)	(7.775)
Time Fixed-Effects	Yes	Yes	Yes
Municipality Fixed-Effects	Yes	Yes	Yes
Observations	1,250	1,145	1,136
R-squared	0.731	0.681	0.698
Number of municipalities	250	250	250

*Clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1*
In Column 1 the treatment group is defined as municipalities with COVID-19 cases, whether they have deaths or not.
In Column 2 the treatment group is defined as municipalities with COVID-19 deaths.
In Column 3 the treatment group is defined as municipalities with COVID-19 cases but no deaths.

Table S8: Placebo Regressions 1. Randomize control and Treated.

Covid Economics 50, 25 September 2020: 181-208

Covid Economics 50, 25 September 2020: 181-208

	(1)	(2)	(3)
	Turnout	Turnout	Turnout
COVID-19 Cases Dummy	-0.586		
	(0.806)		
COVID-19 Cases and Deaths Dummy		-1.589*	
		(0.910)	
COVID-19 Cases but No Deaths Dummy			-0.190
			(0.856)
Population Density	0.000434	0.000449	0.00753
	(0.00105)	(0.00104)	(0.00949)
Unemployment Rate	-0.261**	-0.106	-0.347*
	(0.130)	(0.156)	(0.193)
% Female	0.121	0.0492	0.204
	(0.128)	(0.175)	(0.157)
% Over 65 years old	0.142	0.294*	0.110
	(0.105)	(0.158)	(0.139)
2009	-5.228***	-4.816***	-6.634***
	(0.407)	(0.447)	(0.631)
2012	-2.912***	-2.518***	-3.725***
	(0.828)	(0.872)	(0.960)
2016	-6.186***	-6.071***	-6.623***
	(0.764)	(0.755)	(0.881)
Constant	66.74***	65.58***	65.41***
	(7.753)	(11.08)	(9.519)
Time Fixed-Effects	Yes	Yes	Yes
Municipality Fixed-Effects	Yes	Yes	Yes
Observations	1,000	580	544
R-squared	0.552	0.617	0.528
Number of municipalities	250	145	136

*Clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1*

In Column 1 the treatment group is defined as municipalities with COVID-19 cases, whether they have deaths or not.

In Column 2 the treatment group is defined as municipalities with COVID-19 deaths.

In Column 3 the treatment group is defined as municipalities with COVID-19 cases but no deaths.

Table S9: Placebo Regressions 2. Treatment After 2012.