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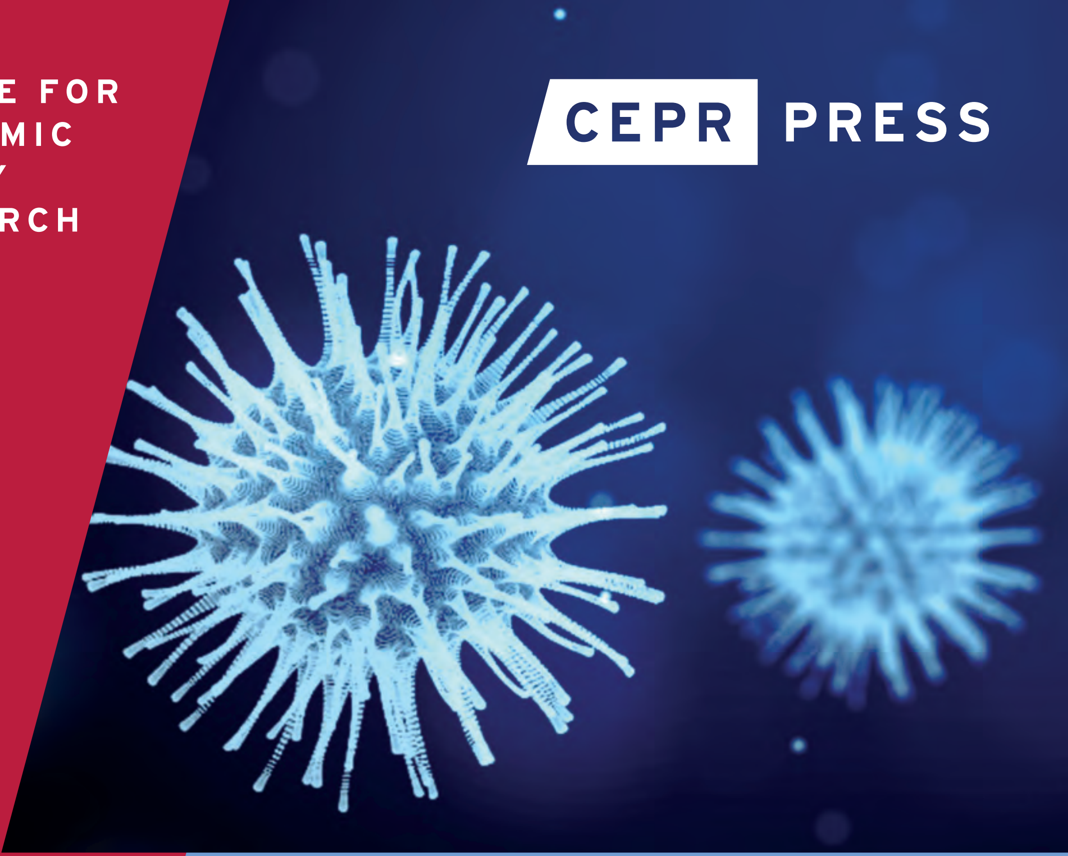

COVID ECONOMICS

VETTED AND REAL-TIME PAPERS

ISSUE 57
13 NOVEMBER 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.

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

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Ethics

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

Submission to professional journals

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

American Economic Review
American Economic Review, Applied Economics
American Economic Review, Insights
American Economic Review, Economic Policy
American Economic Review, Macroeconomics
American Economic Review, Microeconomics
American Journal of Health Economics
Canadian Journal of Economics
*Econometrica**
Economic Journal
Economics of Disasters and Climate Change
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*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*.

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Who should get it first? Public preferences for distributing a COVID-19 vaccine[1]

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Date submitted: 6 November 2020; Date accepted: 7 November 2020

Once a safe COVID-19 vaccine will become available, there will not be enough supply of it to vaccinate the entire population. Policy makers at national and international level are currently developing vaccine prioritization strategies. However, it is important that these strategies have sufficient levels of public support. We conducted a ranking exercise and a discrete choice experiment on a representative sample of 2,000 Belgians in order to elicit their preferences regarding how to distribute the COVID-19 vaccine across the population. We identified that three sub-groups had similarly high levels of support for access priority: the chronically ill, essential professions, and individuals likely to spread the virus the most. We identified two clusters of respondents. While both wanted to vaccinate essential professions, cluster one (N=1058) primarily wanted to target virus spreaders whereas cluster two (N=886) wanted to prioritize the chronically ill. Prioritizing those over 60 years of age was remarkably unpopular. Other strategies such as allocating the vaccine using a 'lottery', 'first-come, first-served' approach or willingness-to-pay received little support. Public opinion is a key variable for a successful engaged COVID-19 vaccination policy. A strategy simultaneously prioritizing medical risk groups, essential professions and spreaders seems to be most in line with societal preferences. When asked to choose, people agree to vaccinate essential professions but disagree whether to prioritise people with high-medical risk or virus spreaders.

1 This study did not fall under the Belgian law on experiments as anonymized data collected by a third party were analysed and the Social and Medical Ethics Committee (SMEC) of KU Leuven decided that no approval was needed.

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1. Introduction

After months of a global public health crisis, vaccines that are safe and effective in providing protection against the SARS-CoV-2 virus are expected to arrive (Mallapaty and Ledford 2020, Bloom, Nowak, and Orenstein 2020). Once available, a new challenge will emerge: their initial supply will be limited due to various production, logistic and regulatory constraints (Usher 2020, Khamsi 2020, Phelan et al. 2020). In the first stages, it will be inevitable to make tough choices regarding how to distribute the vaccine over the population and it is expected that not all who could benefit from it will be able to be vaccinated (Subbaraman 2020, Schmidt 2020, Schmidt et al. 2020, Emanuel, Persad, Upshur, et al. 2020, Roope et al. 2020). The decision who should get vaccinated first needs to be prepared well in advance, in order to make sure that rationing of a life-saving product goes as fairly and smoothly as possible. Experts organisations such as the World Health Organisation (WHO) Strategic Advisory Group of Experts (SAGE) on Immunisation, the National Academies of Sciences, Engineering, and Medicine (NASEM), the Centers for Disease Control and Prevention's (CDC) Advisory Committee on Immunization Practices (ACIP) and more recently the European Commission (EC), have already issued guidelines regarding COVID-19 vaccine allocation and prioritisation strategies (Gayle et al. 2020, European Commission 2020, World Health Organization 2020). They have all identified subgroups of the population that should be prioritised for vaccines while manufacturers scale up production. The priority list includes the front-line health care workers, the highest risk categories - those above 60 years old or with coexisting conditions, people with an essential work, people who live in crowded settings and in higher risk environments. While the WHO and EC guidelines explicitly have stated that the identified groups are not ranked in order of prioritisation, NASEM has suggested a phased vaccines allocation where priority is guided by risk-based criteria.

A key difficulty in finding a fair allocation of the COVID-19 vaccine will be to reconcile at least three objectives: to protect the medically worst-off, to protect public health, and to protect the economy and society functioning (Emanuel, Persad, Upshur, et al. 2020, Roope et al. 2020, Persad, Peek, and Emanuel 2020, Liu, Salwi, and Drolet 2020). Each of these objectives point at different target groups of the population to prioritise when distributing the vaccines. For example, one could prioritize those at most risk of developing severe forms of COVID-19: those with comorbidities and weak immune systems in which a COVID-19 infection is most likely to be fatal, and older people with higher mortality odds (Clark et al. 2020). Whereas this strategy will perform best in reducing the short-term disease burden, vaccinating medically vulnerable groups does not necessarily do best in containing virus transmission, especially if vulnerable groups are already self-protecting and avoiding contacts. From a public health perspective, vaccination of the individuals that are most important in the transmission of the virus within society would be most effective in controlling COVID-19 contagion and could indirectly translate into lower casualties amongst vulnerable groups (Wang et al. 2020, Adam et al. 2020). Similarly, essential professions could be vaccinated first in order to minimize the social impact of the virus. In the first place, this would apply to healthcare workers (The Lancet 2020), whose protection is essential to avoid implosion of the health system but, by extension, other professions essential to society's normal functioning could be targeted for vaccination. Finally, in order to mitigate further damages to already weakened economies, it might be a priority to vaccinate first the people who are most important to the economy, especially those who would cost more to society if they cannot keep working. Beyond specific population sub-groups, other strategies that have been suggested in the allocation of scarce medical resources, could be considered (Persad, Wertheimer, and Emanuel 2009, Emanuel, Persad, Kern, et al. 2020, Persad, Peek, and Emanuel 2020). One could give everyone an equal chance to get a vaccine using for instance a lottery. One could also distribute the vaccine on a 'first-come, first-served' basis as it is sometimes done in other policy domains such as allocation of social housing. Eventually, access to a vaccine could be granted using people's willingness-to-pay as it is done in a market system where the amount people are willing to pay would reflect the personal value they attribute to being vaccinated.

All these alternative strategies have their own rationale to allocate the limited supply of COVID-19 vaccines. It is far from obvious which specific mechanism is deemed most appropriate and most supported. Given the major collective dimension of the current crisis, the expected value of a vaccine and the turmoil that scarcity of it might instigate, it is important to understand which vaccines allocation mechanism seems the most acceptable to the public. Furthermore, as has already been evidenced with other measures (e.g physical distance, mouth masks, etc.) public support plays a crucial role in making pandemic countermeasures effective.

In this article, we present the results of a study carried out on a representative sample of the general population in Belgium. We asked members of the public first to rank different specific population groups by order of priority to access COVID-19 vaccines and then to state their preferences over multiple pairs of hypothetical individuals for priority allocation.

2. Methods

2.1. Sample and survey

We used a nationally representative panel of the market research agency SSI to complete a survey in between 6 and 16 October 2020. From a panel of 5,500 selected members that mirror the Belgian population as well as possible[1], a sample of N=2,060 was drawn randomly, fulfilling pre-determined Belgium quota for age, gender, level of education and province.

[1] The research company evaluates it continuously, eliminates low-quality responders systematically and participation is rewarded with bonus points that lead to vouchers to buy certain products or make donations.

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The survey first asked for a range of respondent sociodemographic characteristics along with their financial situation, general self-assessed health, attitude towards vaccination and toward the government's dealing with the corona crisis, whether they had had COVID-19, whether someone they knew had it, was hospitalized because of it and had died because of it. Respondents were also asked whether their profession was among the 'essential professions' (i.e. those that were obliged to keep working during the first 'lockdown' in March/April 2020) and whether they considered themselves to be part of a risk group for COVID-19 and if so, which group they belonged to (old age, chronic illness, obesity, or other). The questionnaire was then followed with an explanation of the background to the study where we explicitly asked the respondents to think about what they considered the fairest to society when allocating the limited supply of COVID-19 vaccines, and not to choose simply what would be the most to their own advantage. After the ranking exercise and the choice experiment, respondents were asked about whom should decide who gets the COVID-19 vaccine first (government, scientists or the population), whether they would choose to be vaccinated themselves once a vaccine becomes available, and how easy they found answering the survey.

2.2. Ranking exercise

We presented the respondents with eight alternative strategies to distribute the COVID-19 vaccines summarized in *Table 1*. Each strategy was presented one after the other using successive new screens that respondents were only able to progress from every 10 seconds. The eight strategies were then summarised as a list in their short version (with the possibility to go back to the full explanation if needed) and respondents were asked to rank all of them from the ''most appropriate' to 'least appropriate' according to their opinion. They were told that the vaccine was equally safe and effective in all people.

Table 1: Eight strategies to distribute a COVID-19 vaccine

Strategy (in short)	**Full explanation as presented in the experiment**
Prioritizing chronically ill	We could first give the vaccine to people who are medically most at risk of serious illness and death because they have another underlying condition: cancer patients, people with lung disease, heart disease, kidney disease, severe obesity, etc. By vaccinating them first, we would protect **the people most vulnerable to the virus**.
Prioritizing the elderly	We could first give the vaccine to people over 60 years old. We know that, on average, these people run a much higher risk of serious illness or death from a corona infection. By vaccinating them first, we would protect **the people most vulnerable to the virus**.
Prioritizing spreaders	We could first give the vaccine to the people who spread the virus the most because they have a lot of social contacts in their daily life (at work, at school, in their neighbourhood, in public transport, etc.). These people themselves are not at high risk of serious illness or death from COVID-19, but they can infect many others. By vaccinating them first, we would **slow down the spread of the virus as much as possible**.

Prioritizing workers	People who work will cause a greater economic cost when they become ill than those who do not work. By first vaccinating working people, we would ensure that the virus does **as little further damage as possible to the economy**.
Prioritizing essential professions	Some professions are more "essential" to society than others. During the pandemic, health workers, hospital staff, police and garbage services had to continue working as usual, while others had to work from home or were temporarily unemployed. By prioritizing workers from these vital sectors, we would protect the **normal functioning of society**.
Lottery	We could distribute the available vaccines randomly among the population, for example through a lottery. Therefore, each individual would have the **same chance to be vaccinated**, regardless of their health risk or the social impact of an infection.
First-come, first-served	We could distribute the available vaccines to the population according to the principle "**first-come, first-served**". People who present themselves the fastest for vaccination at the doctor, pharmacy or government would be given priority from the moment there is a vaccine.
Market	We could sell the available vaccines to the highest bidder. The people **who want to pay the most money** for a vaccine would be given priority.

2.3. Discrete choice experiment

We then subjected respondents to a discrete choice experiment (DCE). This is a widely used survey method to study individuals' preferences.(Ryan, Gerard, and Amaya-Amaya 2008, Louviere, Hensher, and Swait 2000) Participants are presented with a series of choice sets, usually between two or more products or services that are described by the same attributes but they differ in their attribute levels. By observing a large number of choices, researchers can infer how attributes and levels implicitly determine the value of the competing options. Here, we presented respondents with a choice between two hypothetical people candidates for COVID-19 vaccination. Both candidates were described with identical attributes, but they differed in terms of the levels of these attributes so that we could infer how important these attributes were to the respondents when prioritizing one or the other candidate for vaccination.

Attributes and levels. The DCE focused on five attributes of people: (1) their age, (2) whether they belonged to a medical risk group due to underlying illnesses, (3) their importance to the economy, (4) whether their profession was considered 'essential', and (5) whether they would spread the virus to many other people or not in case of infection (see *Table 2*). The remaining strategies from the ranking exercise (lottery, market, first-come first-served) were excluded in the DCE.

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Table 2: Attributes and levels used in the DCE

Attribute	Levels
Medical risk group	▪ Someone who has no underlying conditions ▪ Someone who has higher risk through chronic illness
Age	▪ Someone who is younger than 60 years ▪ Someone who is at least 60
Virus spreader	▪ In case of infection, someone who is expected to contaminate 1 other person ▪ In case of infection, someone who is expected to contaminate 10 other persons
Cost to society	▪ In case of infection, someone who is expected to cost society 0€ per day ▪ In case of infection, someone who is expected to cost society 100€ per day ▪ In case of infection, someone who is expected to cost society 1000€ per day
Essential profession	▪ Someone who has a profession that is considered 'essential' ▪ Someone who has a profession that is considered not 'essential'

Design. We designed the DCE using "partial profiles": we kept two levels constant between the two choice profiles whereas three levels varied.(Kessels, Jones, and Goos 2015, Kessels et al. 2011) This made the choice tasks easier to perform and therefore more reliable and valid for the analysis. The complete DCE survey consisted of 30 choice sets that we split into three different blocks of 10 choice sets. The three versions were then divided equally among respondents (one representative sample for each survey block). Within each survey, 10 choice sets were presented in a random order to respondents to counteract a possible "order effect". Before the DCE started, we presented the respondents with a mock choice set. This choice set was identical to their last 'real' choice set and allowed us to analyse the consistency in responses. *Figure 1* presents an example of a choice set.

The statistical design (the specific composition of the choice profiles) that we generated was "D-optimal" within a Bayesian framework.(Kessels et al. 2011) This design makes it possible to examine the importance of the attributes and their levels with maximum precision. The complete design of the DCE is presented in *Table A.2*.

We first tested various visualisations amongst a convenience sample (N=10) and then carried out a pilot study of the full survey in 174 respondents. After correcting for a few minor issues, we went ahead with the full launch of the study in 2,060 respondents.

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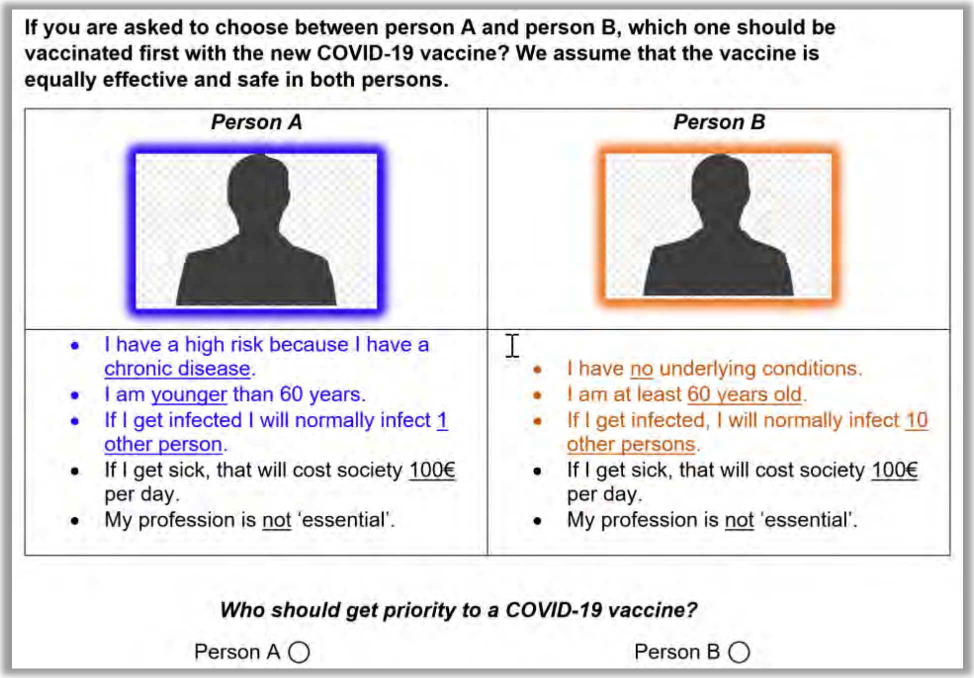
Figure 1: Example of a choice set



2.4. Statistical analysis

We analysed the choice data by estimating a panel mixed logit (PML) model using the hierarchical Bayes technique in the JMP Pro 15 Choice platform (based on 10,000 iterations, with the last 5,000 used for estimation). This model assumes normally distributed utility parameters over the respondents to accommodate unobserved heterogeneity in the respondents' preferences. The mean utility function is thereby the sum of the mean attribute effects. Using Ward's hierarchical cluster analysis on the individual utility estimates, we identified important respondent segments that we characterized through bivariate chi-square analyses on the respondents' covariates and multiple logistic regression with the cluster membership as response variable and the respondents' covariates as explanatory variables. In all our analyses we used a significance threshold of five percent.

3. Results

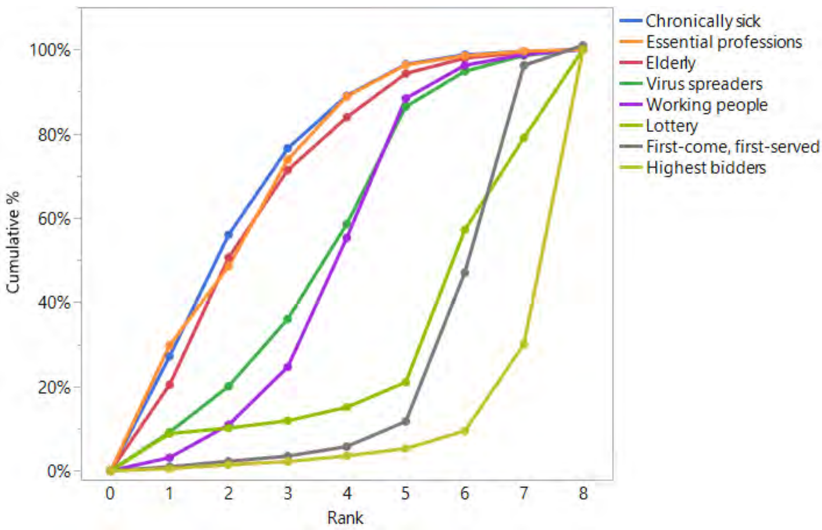
On average the survey took respondents 21 minutes to complete (median 15.3). When asked how difficult completion of the survey was, only 21 respondents (1%) indicated it was 'too difficult' whereas 1,154 (56%) said it was "easy" and 43% found it "difficult but doable". None of the response distributions nor answers to comment boxes raised concerns to the research team. A sample of 1,577 respondents (77%) gave the same answer twice to the repeated choice set, however differing answers do not point at invalid answers as the strength of preferences can be weak in this context. We did observe 116 respondents (6%) that gave the same answer throughout the DCE ('straightliners'). As this is unlikely, we decided to exclude these as a way of caution, leaving us with 1,944 respondents for the analysis.

Thirty-nine percent considered themselves part of a COVID-19 risk group. A small minority (<20%) of the sample had experience with a COVID-19 infection, either in themselves or their proximity. A small majority (59%) was dissatisfied with the government's approach to the crisis. A large majority of respondents (78%) thought that the vaccine allocation decision should ultimately be determined by scientists; 10% thought the government should decide and 12% thought that it should be the population only. When asked whether they would become vaccinated with a COVID-19 vaccine, 74% responded affirmatively (see *Table A.1*).

3.1. Ranking exercise results

The ranking exercise results are summarized in *Figure 2*. We use cumulative distribution functions to synthesize how each strategy was ordered by the respondents. The graphical representation shows that there was not one single strategy that dominated and was considered as absolute best by a large majority.

Figure 2: Cumulative distribution functions of alternative COVID-19 vaccine allocation strategies ranked from 'most appropriate' (rank of 1) to 'least appropriate' (rank of 8)



The eight strategies are clearly divided into three groups: three dominant strategies, two strategies ranked somewhere in the middle, and three strategies ranked in the three worst strategies. Prioritizing essential workers, chronically ill and elderly were found to be the three most supported strategies. On the other hand, market, lottery or "first-come, first-served" strategies were clearly the least preferred strategies with at least 80% of the respondents ranking them at the bottom of the ranking. Finally, targeting spreaders or protecting the economy were strategies ranked in the middle.

While the lottery strategy was very unpopular (79% ranked it in the top 3 of worst strategies), one in ten respondents thought that this was a very good strategy and ranked it as the most or second most appropriate strategy for allocating vaccines in the population. Analysing further this group of respondents, we found that the lottery strategy was more attractive to younger respondents (25-34), with a basic educational level, with regular financial problems, who think vaccination is useless and who doubt becoming vaccinated themselves with the COVID-19 vaccine, and who are dissatisfied with the government's policy towards the corona crisis (all chi-square test p-values <0.001). They were also more likely to think that vaccine allocation should be driven by the preferences of the population instead by those of policy makers or scientists.

3.2. DCE results

In total, we analysed 19,440 choices between hypothetical individuals competing for vaccination. We first estimated model A (see *Table 3* and *Figure 3*) that summarizes the choices made by the whole sample and that can reflect the preferences over the five attributes of the average respondent. This model showed that there was not one single attribute that dominated the other attributes and that gave a subgroup of the population lexical priority over others. Instead, we found that three attributes were of large importance: belonging to a medical risk group, having an 'essential profession' and being a relatively large spreader of the virus. Belonging to a medical risk group was found to be the most important one. While older people are also labelled as higher risk groups with COVID-19, being in an older age group was not found to be a strong predictor of priority to vaccine access by the public. Vaccinating first people who would be costly to the society if they have COVID-19 did not appear to matter either.

When adding all possible first-order interaction effects between the five attributes into model A, we identified a few interactions that were of practical relevance. The combinations of being older than 60 and having an essential profession, having a high cost to society and essential profession, or being part of a medical risk group and being a super-spreader, led to a higher priority to vaccine allocation.

Table 3: Model estimates for the entire sample and the two clusters

Term	Posterior	Posterior Std	Subject Std Dev	Lower	Upper 95%
		Model A (N=1944)			
Medical risk group					
Yes	0.676**	0.024	0.446	0.632	0.724
No	-0.676**	0.024	0.446	-0.724	-0.632
Older than 60					
Yes	0.093**	0.015	0.442	0.064	0.124
No	-0.093**	0.015	0.442	-0.124	-0.064
Virus spreader					
10 other persons	0.660**	0.024	0.468	0.614	0.708
1 other person	-0.660**	0.024	0.468	-0.708	-0.614
Cost to society					
0 €/day	-0.123*	0.026	0.251	-0.173	-0.078
100 €/day	-0.011*	0.022	0.146	-0.054	0.030
1000 €/day	0.134*	0.027	0.262	0.082	0.187
Essential profession					
Yes	0.567**	0.019	0.519	0.529	0.604
No	-0.567**	0.019	0.519	-0.604	-0.529
		Model B (N=1058)			
Medical risk group					
Yes	0.309**	0.023	0.072	0.265	0.352
No	-0.309**	0.023	0.072	-0.352	-0.265
Older than 60					
Yes	-0.202**	0.017	0.291	-0.236	-0.169
No	0.202**	0.017	0.291	0.169	0.236
Virus spreader					
10 other persons	0.911**	0.032	0.477	0.849	0.973
1 other person	-0.911**	0.032	0.477	-0.973	-0.849
Cost to society					
0 €/day	-0.334**	0.032	0.273	-0.400	-0.275
100 €/day	0.060**	0.029	0.224	0.002	0.114
1000 €/day	0.274**	0.030	0.298	0.213	0.334
Essential profession					
Yes	0.362**	0.020	0.381	0.323	0.402
No	-0.362**	0.020	0.381	-0.402	-0.323
		Model C (N=886)			
Medical risk group					
Yes	1.394**	0.060	0.547	1.276	1.521
No	-1.394**	0.060	0.547	-1.521	-1.276
Older than 60					
Yes	0.504**	0.029	0.438	0.449	0.564
No	-0.504**	0.029	0.438	-0.564	-0.449
Virus spreader					
10 other persons	0.480**	0.037	0.125	0.409	0.562
1 other person	-0.480**	0.037	0.125	-0.562	-0.409
Cost to society					
0 €/day	-0.050	0.033	0.130	-0.119	0.014
100 €/day	0.004	0.039	0.221	-0.071	0.072
1000 €/day	0.046	0.042	0.240	-0.039	0.129
Essential profession					
Yes	0.975**	0.046	0.737	0.886	1.071
No	-0.975**	0.046	0.737	-1.071	-0.886

** Significant at p<0.001, * Significant at p<0.05

Since an overall model based on the average respondent can be misleading in case a population is polarized, we investigated individual differences between respondents and identified two large clusters

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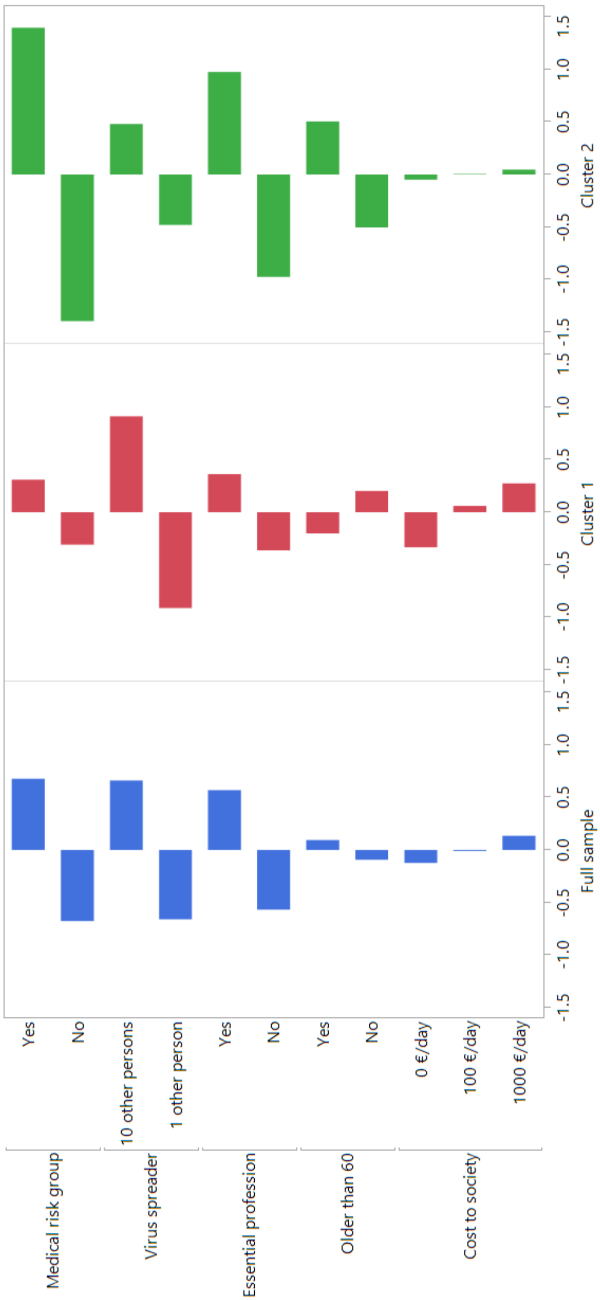
of respondents within the sample. The preferences of the first cluster (n=1,058, 54% of the sample) are summarized by model B. This cluster was in favour of prioritizing spreaders. The second cluster (n=886 respondents, 46%) is summarized in model C. These respondents prioritized vaccinating medical risk groups. Both clusters valued essential professions as the second most important attribute. Interestingly however, whereas people aged 60 or more were prioritized in cluster 2, they were not prioritized in cluster 1. Cluster 1 also valued people who were economically important whereas this attribute was statistically insignificant in cluster 2. *Figure 3* presents the main utility effects of all the models in predicting respondents' choices.

We analysed whether there were any of individual characteristics associated with membership to clusters 1 or 2. There were no strong profiles emerging. However, compared to those from cluster 1, bivariate analyses showed that respondents belonging to cluster 2 (prioritizing risk groups) were more likely part of a medical risk group for COVID-19, more likely to be working, more convinced of the value of vaccines in general, more likely to become vaccinated with the COVID-19 vaccine, less likely to think that the COVID-19 vaccine allocation strategy needed to be made only by the population, more likely to think that the government should make these decisions, and less likely to be French-speaking (all chi-square test p-values <0.001). When these six factors were analysed jointly in a multivariate regression, the effect of belonging to a risk group or general attitude to vaccination became insignificant whereas the other four characteristics remained. There was no relationship between being a member of cluster 1 or 2 and respondents' age, having an 'essential' profession, financial situation, level of education or other variables in our survey. We found no evidence that respondent choices were driven by self-interest.

Figure 3: Estimated utilities of the full sample (N=1944 respondents), cluster 1 (N=1058 respondents) and cluster 2 (N=886 respondents)



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4. Discussion

This study lays bare clear patterns in how the general public wants to allocate COVID-19 vaccines when available.

First, there is little support for approaches that are more libertarian-inspired such as highest willingness-to-pay or 'first-come, first served' strategies. A strict egalitarian approach like a lottery also receives little support. The most supported strategies are those where priority groups are explicitly defined at a policy level.

Second, when deciding which individual characteristics ought to matter to policy makers when ranking priority groups, respondents have a clear preference not to prioritize older aged individuals, even though they belong to higher risk groups for COVID-19. This was true also for respondents from older age groups. This would support the fair innings argument according to which priority should be given to the young over the old and age is an accepted criterion for scarce health care resources allocation under the assumptions that every individual is entitled to live for a reasonable length of life.(Williams and Evans 1997) The general public would not prioritise for vaccination those who are of particular economic importance such as those who work. Instead, they prioritized vulnerable people with medical conditions, people who are instrumentally important to public health by playing a role in wider virus transmission in the population or people who are more important to society functioning such as those with essential professions.

Third, when trying to compare and rank within the three main target groups, the population was divided in two clusters. A share adhered to a 'utilitarian' strategy of maximizing societal health outcomes by allocating vaccines strategically towards virus spreaders (cluster one).(Savulescu, Persson, and Wilkinson 2020) These people also thought that vaccinating those with high economic cost to society was to some extent relevant. The other cluster adhered to a 'prioritarian' strategy that put those people who are at medical highest risk first (cluster two). Being a virus spreader or someone who could cost a lot to the economy was of little or no importance in this cluster. However, both groups considered essential professions a priority group but of secondary importance. Age was of minor importance in both groups but whereas being older than 60 would receive priority in the 'prioritarian' group, in the 'utilitarian' group we observed the opposite. It was not the case that membership of these clusters coincided with the interests of the respondents. For instance, there was no relationship between priority choices and being young (respectively old) or with having an essential profession or not. Respondents who were not working (students, retired or unemployed people and homemakers) were more likely to be part of the 'utilitarian' cluster one. Those belonging to a risk group were more likely part of the 'prioritarian' cluster two, however that effect disappeared when multiple respondent characteristics were considered simultaneously.

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What is remarkable is that there were similarities but also discrepancies between the ranking exercise and the DCE. Whereas elderly vaccination was within the top three strategies in the ranking exercise (although the lowest ranked one), in the DCE this attribute was found of minor importance. In the same vein, vaccinating spreaders was only a second-rate strategy in the ranking exercise, however, when we assorted it with concrete figures so that an individual would spread infection to either one or ten other people, this attribute became very important in one of the two clusters.

How relevant are these observed preferences for the current debate? In the first place, we request some caution that, our results are based on experiment that can only be seen as an approximation of how the public thinks about COVID-19 vaccines allocation. Although we did our best to keep the exercise simple, and we did as many validity checks as possible, we cannot know how people would have responded if they had to consider these choices in a discussion format or if more details had been provided (e.g. on the actual sizes of the different priority sub-groups within the society). More fundamentally, while there is almost a consensus on the priority candidates to the COVID-19 vaccines, ranking within those key groups is not straightforward and there is not a consensus of whom should be vaccinated first, second, and so on. The difficulty of defining a clear ranking among the identified priority groups has also been observed in the COVID-19 vaccination strategies put forward by the EC and WHO SAGE expert groups (World Health Organization 2020, European Commission 2020).

Making social trade-offs between health, the economy and the health system is difficult and it is not clear what the exact value of the public opinion has in comparison to the more informed and deliberate judgment of experts and politicians. However, we believe that, in the light of the large collective dimension of the COVID-19 crisis, the preferences of the public opinion are an essential input value to the debate. It is the goal of this study to provide such an evidence base.

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6. Appendix

Table A.1: Sample characteristics

Characteristics	Responses item	N	%
Respondents' general background			
Gender	Female	951	49%
	Male	993	51%
Age	18-24	194	10%
	25-34	330	17%
	35-44	331	17%
	45-54	379	19%
	55-64	321	17%
	65-80	389	20%
Language	Dutch	1112	57%
	French	832	43%
Province	Vlaams-Brabant	191	10%
	Waals-Brabant	129	7%
	Brussels Capital	176	9%
	Antwerpen	288	15%
	Limburg	157	8%
	East Flanders	249	13%
	West Flanders	200	10%
	Hainaut	115	6%
	Liège	186	10%
	Luxembourg	102	5%
	Namur	151	8%
Education	None	7	0%
	Primary school	61	3%
	First degree secondary school	187	10%
	Second degree secondary school	247	13%
	Third degree secondary school	684	35%
	Higher education (non-university)	468	24%
	University or post-university	268	14%
	PhD	14	1%
	Other	8	0%
Have children	Yes	1213	62%
	No	731	38%
Profession	Working	915	47%
	Homemaker	80	4%
	Student	158	8%
	Unemployed	129	7%
	Disabled	127	7%
	Retired	472	24%
	Other	63	3%
Difficulties with monthly expenses	Never	802	41%
	Once a year	422	22%

	Once every three months	391	20%
	Every month	329	17%
Self-assessed health	Very good	248	14%
	Good	741	41%
	Rather good	602	34%
	Bad	167	9%
	Very bad	22	1%
	Don't know/don't want to say	14	1%
Respondents' Covid-19 related background			
Self-reported membership of a COVID-19 risk group	No	1183	61%
	Yes, elderly	366	19%
	Yes, chronically ill	400	21%
	Yes, severe obesity	124	6%
	Yes, other	68	3%
Self-reported profession is labelled as 'essential'	Yes	367	19%
	No	1577	81%
Has had a COVID-19 infection	Yes, confirmed with a test	57	3%
	Probably, but not confirmed with a	160	8%
	No	1727	89%
Know personally someone who has had COVID-19	Yes, confirmed with a test	293	15%
	Probably, but not confirmed with a	175	9%
	No	1476	76%
Know personally someone who was hospitalized for COVID-19	Yes	118	6%
	No	1826	94%
Know personally someone who died of COVID-19	Yes	83	4%
	No	1861	96%
Satisfaction with government's approach of COVID-19 pandemic	Very satisfied	58	3%
	Rather satisfied	729	38%
	Rather dissatisfied	787	40%
	Very dissatisfied	370	19%

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Table A.2: Complete design of the DCE

Survey	Choice	Medical	Older	Virus	Cost to	Essential
1	1	yes	yes	1 other person	0	yes
1	1	no	no	1 other person	1000	yes
1	2	no	yes	10 other persons	100	no
1	2	no	yes	1 other person	1000	yes
1	3	yes	yes	1 other person	0	no
1	3	no	yes	10 other persons	1000	no
1	4	no	yes	1 other person	100	yes
1	4	no	no	10 other persons	100	no
1	5	no	no	1 other person	100	yes
1	5	yes	no	1 other person	1000	no
1	6	no	yes	1 other person	1000	yes
1	6	yes	yes	10 other persons	1000	no
1	7	yes	no	10 other persons	1000	no
1	7	yes	yes	10 other persons	0	yes
1	8	yes	yes	1 other person	100	yes
1	8	yes	no	10 other persons	0	yes
1	9	no	yes	1 other person	0	yes
1	9	yes	no	1 other person	0	no
1	10	yes	no	1 other person	100	no
1	10	no	yes	10 other persons	100	no
2	11	yes	yes	1 other person	100	no
2	11	no	no	1 other person	0	no
2	12	yes	no	1 other person	100	yes
2	12	yes	no	10 other persons	0	no
2	13	yes	no	1 other person	0	yes
2	13	no	no	10 other persons	100	yes
2	14	no	yes	1 other person	0	yes
2	14	no	no	10 other persons	0	no
2	15	yes	no	10 other persons	100	no
2	15	no	no	10 other persons	1000	yes
2	16	yes	yes	1 other person	0	yes
2	16	no	yes	10 other persons	0	no
2	17	no	yes	1 other person	0	no
2	17	no	no	1 other person	100	yes
2	18	no	no	1 other person	1000	no
2	18	no	yes	10 other persons	0	no
2	19	no	no	10 other persons	0	yes
2	19	yes	yes	10 other persons	0	no
2	20	yes	yes	1 other person	1000	no
2	20	no	no	10 other persons	1000	no
3	21	no	no	10 other persons	1000	no
3	21	yes	yes	10 other persons	100	no
3	22	no	yes	1 other person	1000	no
3	22	no	yes	10 other persons	0	yes
3	23	no	no	10 other persons	0	yes
3	23	yes	no	1 other person	1000	yes
3	24	yes	no	1 other person	1000	no
3	24	yes	yes	10 other persons	1000	yes
3	25	yes	yes	10 other persons	100	yes
3	25	no	yes	10 other persons	1000	no
3	26	yes	yes	1 other person	100	no

3	26	no	yes	10 other persons	100	yes
3	27	no	yes	1 other person	100	no
3	27	no	no	1 other person	0	yes
3	28	yes	yes	1 other person	1000	yes
3	28	yes	no	10 other persons	100	yes
3	29	yes	yes	1 other person	100	yes
3	29	no	no	1 other person	100	no
3	30	yes	no	1 other person	1000	yes
3	30	no	yes	10 other persons	1000	yes

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Do masks reduce COVID-19 deaths? A county-level analysis using IV[1]

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Date submitted: 6 November 2020; Date accepted: 9 November 2020

I examine the relationship between mask usage and COVID-19 deaths at the county level. When examining this relationship, even the direction caused by the potential endogeneity bias is unclear. In one direction, characteristics that are known to correlate with a larger amount of potential COVID-19 deaths, such as an older population, may make people more likely to wear masks. This will cause a bias that makes mask usage look less effective than it truly is. In the other direction, areas with higher risk tolerances may have less mask usage, but may at the same time be engaging in other behavior that puts them at higher risk for contracting COVID-19. This will cause a bias that makes mask usage look more effective than it truly is. The identification approach exploits a large set of controls and employs percentage of vote for Donald Trump in the 2016 election as an instrumental variable for mask usage. The main finding is that a one percentage point increase in the amount of individuals who say they often or frequently wear a mask when within six feet of people will reduce COVID-19 deaths in a county by 10.5%, or six deaths in the average sized county.

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1 The author would like to thank Nikki Brendemuehl for research assistance. For helpful feedback and comments, the author thanks Jason Baron, Nicholas Lovett, and D vid Zimmer.

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1. Introduction

Many health officials have recommended the wearing of masks (face coverings) to mitigate the spread of the Corona Virus and resulting deaths due to COVID-19; in addition several areas have implemented orders requiring mask usage in public. However, many individuals are still reluctant to wear masks and are skeptical that masks can reduce COVID-19 related deaths. I utilize a unique county level dataset and employ several empirical techniques to examine whether a larger percentage of the population wearing masks can reduce deaths from COVID-19. This paper demonstrates that a correctly specified model, which accounts for omitted variable bias (endogeneity), produces a result showing that mask usage reduces deaths from COVID-19. The effect size from this appears to be large.

There are two main empirical complications when examining the structural relationship between mask usage and COVID-19 deaths at the county level. It is also unclear which direction these complications will bias the estimates. The first is that an area's prior experience with COVID-19 will influence both mask wearing and will likely be correlated with future COVID-19 deaths. Areas that have had more COVID-19 deaths in the past may be more likely to have more individuals willing to wear masks. If these places are also more "naturally prone" to experience COVID-19 deaths, and if this is not accounted for, it would make mask usage look less effective than it truly is. The second empirical complication is related to the area characteristics. Here there are two main concerns, each of which would bias the coefficient in opposite directions. First, some areas may be more "naturally" prone to a greater amount of COVID-19 deaths; in other words, certain areas may have characteristics that make them more likely to have COVID-19 deaths. One example would be counties that have an older population. These counties may be more likely to wear masks, but would likely have had more deaths than

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other similar areas that are less naturally prone to COVID-19 deaths. This again, if not accounted for, would make mask usage look less effective than it truly is. Second, some areas area may have individuals with a higher overall tolerance for risk (less area risk aversion). This would likely make these areas less prone to mask usage but also more likely to have more COVID-19 deaths. This would make mask usage look more effective than it truly is.

In order to address these complications I employ three main strategies. To account for prior COVID-19 experience I include the number of COVID-19 deaths prior to the survey on mask usage. To account for area characteristics I employ two techniques. First, I am able to control for many observable characteristics, such as, but not limited to, the age profile and historic fragility of the population. Second, to account for any remaining unobserved characteristics that influence mask usage and COVID-19 deaths, I employ the percentage of individuals who voted for Donald Trump in the 2016 election as an instrumental variable (IV) for mask usage.

I find that a simple correlation shows a small but *positive* and statistically significant relationship between percentage of individuals in a county wearing masks and COVID-19 deaths. Once I control for county population and COVID-19 deaths prior to the survey this relationship shrinks but remains positive and statistically significant. Once a measure that captures riskiness and overall health in a county is included, this relationship becomes statistically insignificant. When my full set of county level controls are added, the relationship between mask usage and COVID-19 deaths becomes negative and statistically significant, but practically insignificant. In my preferred model that employs percentage of vote for Donald Trump in 2016 as an instrumental variable and a full set of controls variables, I find that more individuals wearing masks more often reduces COVID-19 deaths. This result is both statistically

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significant and practically significant. Specifically, a one-percentage point increase in the amount of individuals who say they often or frequently wear a mask when within six feet of other people will reduce COVID-19 deaths in a county by 10.5%, or about six people in an average sized county. In section 3, I present evidence that the instrument is not weak and present several arguments that the exclusion and monotonicity assumptions are satisfied. In Section 5, I show that the findings are robust across several different specifications, and perform an informal check examining whether the identification strategy satisfies the exclusion restriction.

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2. Literature Review and Data

2.1 Literature Review

Not surprising, the existing research on the effectiveness of mask usage is sparse. The few studies that do exist focus mainly on the ability of masks to filter aerosol particles or droplets. A limited number of econometric studies have examined the effect of mask usage on COVID-19 deaths or cases (Chernozhukov et al., 2020; Karaivanov et al., 2020; Mitze et al., 2020; Yilmazkuday, 2020; Zhang et al., 2020). These studies generally find that mask usage reduces COVID-19 cases or deaths. Yilmazkuday has a similar dataset to this paper, however there are many important differences between the two papers, including importantly his identification strategy; he relies on a difference-in-difference strategy in an attempt to identify the relationship between mask usage and COVID-19 deaths. There have also been some descriptive epidemiological analyses that attempt to forecast the results of different levels of mask usage. The most recent of these claims that 95% mask usage could mitigate the effects of a resurgence in many states (Reiner et al. 2020)

2.2 Data

The dataset in the preferred estimation consists of 3,079 counties and county equivalents. This covers nearly every county in the U.S., excluding the small number that had a missing variable used in the analysis. As there are currently 3,141 counties and county equivalents in the US, only 62 are omitted from the analysis.

The dependent variable of interest is deaths from COVID-19 at the county level. This was obtained from the CDC (Centers for Disease Control and Prevention).[1] I also use the CDC's population estimates to measure county population. For COVID-19 deaths I use log of deaths (+1) per capita. I use log of deaths per capita in my preferred specification because it is much more "bell shaped" than any other transformation of this variable. However, I examine the effect of other transformations in the robustness check section.

Information on mask usage is from a survey conducted by Dynata at the request of the New York Times. It was a survey of approximately 250,000 interviews conducted from July 2 to July 14 2020. Each individual was asked "*How often do you wear a mask in public when you expect to be within six feet of another person?"* Individuals were allowed to respond with: "never", "rarely", "sometimes", "frequently", or "always". I combine the percentage who responded with "frequently" or "always" into a single measure, which I use as the main variable of interest in the study. However, I also use other combinations of these in the robustness check section.

Information on the percentage of vote Donald Trump received in the 2016 election was obtained from the MIT Election Data and Science Lab. I use this percentage as an instrumental variable in the preferred specification.

[1] For information on how this variable was collected please see: https://usafacts.org/articles/detailed-methodology-COVID-19-data/ .

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Information on educational attainment, median income, minority demographics, and population density is from the US Census. Since information is not available for 2020, previous census information along with a combination of weighted averages and linear projections are used to predict the current value of many of these variables (more specific information on the construction of these variables is available from the author). The additional important control of overall mortality in 2016 is from CDC mortality file.

The mean and standard deviation for all variables can be found in Table 1. The three main variables, COVID-19 deaths on September 1, mask usage, and percentage of vote for Donald Trump, all exhibit a fair amount of variation.

3. Model

The main empirical complication is omitted variable bias (endogeneity). First, counties with a larger amount of COVID-19 deaths in the past may have higher mask usage but at the same time be prone to more current COVID-19. This higher death rate could be due to factors that make this county more prone to COVID-19 or because a large amount of COVID-19 exists in the county. Second, there are other factors that affect both wearing a mask and COVID-19 deaths in a county. In one direction, counties with a "naturally" higher risk for COVID-19 deaths may both have more individuals wearing masks and have more COVID-19 deaths. This would cause mask usage to look less effective than it truly is. On the other hand, counties with a higher average level of risk aversion will have higher mask usage, but would likely have had less COVID-19 deaths due to other steps individuals would have taken to avoid exposure to COVID-19. This would cause mask usage to look less effective than it truly is.

The empirical approach attempts to correct for the above concerns in three ways. First, I utilizing the timing of the survey on mask usage and when COVID-19 deaths are measured; I use

Table 1: Descriptive Statistics

	Mean	SD
Dependent Variables		
COVID deaths Sept. 1	58.11	310.77
Per capita COVID deaths Sept. 1 per 10,000	3.42	4.69
Log per cap. COVID deaths Sept. 1	-8.22	1.02
Main Variable of Interest		
Mask usage frequently/always	71.58	13.11
Instrumental Variable		
% vote Trump	63.04	15.82
Control Variables		
COVID deaths July 1	40.26	270.87
Per capita COVID deaths July 1 per 10,000	1.72	3.46
Log per cap. COVID deaths July 1	-8.22	1.02
Population in 10,000's	1.04	3.33
Log per cap. all deaths 2016	5.76	1.34
% college[a]	21.66	9.53
% High school graduates	33.99	7.55
Percentage minorities	13.66	15.82
Percentage Hispanic	9.47	13.73
Percentage female	49.90	2.24
Percentage age 20 to 29	12.31	3.17
Percentage age 30 to 39	11.70	1.66
Percentage age 40 to 49	11.62	1.41
Percentage age50 to 59	13.86	1.52
Percentage age 60 over	25.78	5.65
Average household income	51,060.51	13,486.54
Population density (square miles)	242.61	1,669.58

overall COVID-19 deaths up until approximately six weeks after the survey is completed as the outcome variable, while controlling for the number of deaths prior to the survey. Second, I am able to incorporate a rich set of county level controls. Third, I employ an external “instrument”

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that, once other factors are controlled for, correlates with mask usage but not with any remaining unobservables that are correlated with COVID-19 deaths.

The remainder of this section presents the empirical approach. The first subsection outlines the baseline equation, followed by discussion of my identification approach.

3.1 Baseline Model

The baseline model attempts to address endogeneity through two of the channels mentioned above, timing of the survey and timing of measured COVID-19 deaths, along with a large set of county controls. The formal version of the baseline model takes the following form:

$$COVIDdeaths_{Sep1} = \delta_0 + \delta_1 Mask_{July} + \delta_2 COVIDdeaths_{July1} + \delta_3 Mort_{2016} + \boldsymbol{x}'\boldsymbol{\beta} + \mu \quad (1)$$

where $COVIDdeaths_{Sep1}$ is the log of amount of COVID-19 deaths +1 in a county per capita up until and including September 1, $Mask_{July}$ is the percentage of individuals in a county that responded that they frequently or always wear a mask in public when they expect to be within six feet of another person, $COVIDdeaths_{July1}$ is the log of amount of COVID-19 deaths +1 in a county per capita up until and including July 1, and $Mort_{2016}$ is the log of overall total deaths in a county +1 per capita in 2016.[2] Additional controls are include in the vector $\boldsymbol{x}$; which includes population of the county, educational attainment in the county, percentage of minorities in a county, percentage Hispanic in a county, percentage female in a county, the age demographics of a county, average household income of a county, and population density; the specifics of these can be found in Table 1. The following are estimable coefficients: $\delta_0, \delta_1, \delta_2$, while μ is the error term.

[2] Any choice of date for the dependent variable would necessarily be arbitrary. I choose September 1st because it is the first of a month and not too far removed from the survey; this is an attempt to make sure that attitudes in the counties may not have changed much. In the robustness check section I demonstrate that the choice of date does not affect the results.

By including COVID-19 deaths prior to the survey on mask usage, the endogeneity concern above is mitigated to some extent. This allows me to control how much the county has been affected by COVID-19 in the past. It would be expected that a county with a higher amount of deaths prior to the survey would have more mask usage in the future and at the same time a larger amount of COVID-19 deaths up until and including September 1st.

The other controls also help to alleviate the endogeneity concern. While some of the controls, such as age categories and percentage minority, are included for obvious reason, some do bear discussing. Log of all deaths per capita in 2016 is included because it accounts for both the "historic" amount of risk aversion and the fragility of the population. Education variables and median income are included because more educated or higher income individuals may be more or less likely to wear masks and may at the same time be more or less likely/able to engage in other COVID-19 prevention measures. Robust standard errors are used in all estimations.

Even with the rich set of controls and the timing of the survey there may still be some endogeneity concerns. If there is any remaining unobservables that affect both mask usage and deaths due to COVID-19, or the controls do not fully account for the risk aversion of the population or the natural level of COVID-19 risk in a county estimates of δ_1 will still be biased. In the subsequent subsection I use an instrumental variable technique in an attempt to fully identify the model.

3.2 Instrumental Variable Approach

The identification approach relies upon instrumental variable techniques where equation (1) above becomes the second stage and the following is the first stage equation:

$$Mask_{July} = \alpha_0 + \alpha_1 CovidDeaths_{July1} + \alpha_2 Trump16 + \alpha_3 Mort_{2016} + \boldsymbol{x}'\boldsymbol{\gamma} + \varepsilon \qquad (2)$$

where $Trump16$ is the percentage of individuals in the county that voted for Donald Trump in the 2016 election, $\alpha_0, \alpha_1, \alpha_2, \boldsymbol{\gamma}$ are estimable coefficients, and ε is the error term. The other variables are defined above.

$Trump16$ is the excluded instrumental variable. Instrument validity rests on three assumptions. First $Trump16$ must significantly correlate with mask usage conditional on other control variables, and this must be powerful (instrument strength). There is strong documentation that some President Trump supporters may be influenced by his comments to not wear masks as is illustrated by the following quotes: “The C.D.C. is advising the use of nonmedical cloth face covering as an additional voluntary public health measure. So it’s voluntary. You don’t have to do it. They suggested for a period of time, but this is voluntary. I don’t think I’m going to be doing it.” (April 3, at the White House) and “I don’t agree with the statement that if everybody wears a mask, everything disappears.” (July 19, to the Fox News host Chris Wallace).[3]

Instrument strength is also testable. Staiger and Stock (1997) and Stock and Yogo (2005) set the often cited benchmark for an instrument’s strength. They argue that the first stage F-statistic of the excluded instrument must be, at a minimum, 10 or 16.38 respectively. To test this condition, I examine the estimation from equation 2. These results appear in the Appendix Table A1. The F-stat for the excluded instrument is approximately 435.07 and 49.73 in the models without and with the full set of controls respectively, thus exceeding the threshold conventional benchmark for power. In a recent paper, Lee et al. 2020 demonstrate that the "F threshold" either should be increased to 104.7 or if kept at 10, the critical value for 5% significance needs to be increased to 3.43 for the second stage endogenous variable's coefficient. In the preferred model

[3] These quotes can be found in the New York Times Article "*In His Own Words, Trump on the Coronavirus and Masks*" by Daniel Victor, Lew Serviss and Azi Paybarah on October 2, 2020

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the F-stat does not reach this 104.7 threshold, however the t-stat in the main result, found later, is 6.26, exceeding this new 3.34 critical value.[4] Also, note that the estimated coefficient on $Trump16$ has the expected sign.

The second assumption holds that the instrument must be conditionally uncorrelated with the error term in equation (1) (exclusion restriction), or more loosely with the amount of COVID-19 deaths on September 1. This assumption cannot be tested directly in my exactly-identified models so I rely on the instrument's intuitive appeal for validity. Recall the two potential omitted factors of concern are the risk tolerance of the population and the susceptibility of the county's population to contracting and dying from COVID-19. In other words, the percentage of individuals who voted for Donald Trump in 2016 should not be *directly* correlated with amount of risk aversion in a county or the "natural tendency" for individuals to acquire and die from COVID-19 *once* other factors are controlled for. With regard to both but more so the former I note that I control for a county's previous (overall) death rate. In addition, it seems unlikely, especially after controlling for things such as the age of the population of a county, that places with different percentages of votes for Donald Trump are more or less likely to have a natural propensity for COVID-19 deaths or a different level of risk aversion. In Section 5, I also perform some tests to investigate the relationship between percentage of vote for Donald Trump and historic levels of death rates due to injury.

The third assumption required is monotonicity. For monotonicity to hold the percentage of vote for Donald Trump is allowed not to impact mask usage in some counties, but in all counties where it has an impact this effect must be negative ($\alpha_2 \leq 0$); in other words, there are

[4] More technically, they present formulas on how to use the value of the F-statistic to adjust the critical values. Using my first stage F-statistic value of 49.73, the new 5% critical value is 2.16, which again is smaller than my t-stat.

no "defiers". It seems unlikely that, everything else equal, if a specific county spontaneously had more support for Donald Trump in 2016 that it would increase mask usage. It does seem plausible that in some counties that if support for Donald Trump were to be increased it may not reduce mask usage (having "never-takers"), such as counties with many COVID-19 cases, however this does not violate the monotonicity assumption. If there are some counties where the percentage of vote for Donald Trump does not impact mask usage, this would mean the result found is a LATE (local average treatment effect).[5] This implies that the results found later may not hold in counties with a large amount of mask usage and COVID-19 cases, where most people wear a mask and mask usage is not influenced by political affiliation.

Note that the instrument I utilize is unable to circumvent the issue that mask wearing may be correlated with other COVID-19 prevention methods, such as participating in less large social gatherings. *If* these other preventive measures are negatively correlated with the percentage of individuals who voted for Donald Trump, the effect of mask usage on COVID-19 deaths will be overstated. However, even if the exclusion restriction is partially violated in this way it would only result in some attenuation bias, it will not negate the results. In addition, I investigate this possibility in Section 5 by employing a crude measure of social distancing and find this would not qualitatively alter the results.

I also perform the Hausman-WU-Durbin test for endogeneity, which is robust to heteroskedasticity. It is important to note that this test relies on the fact that one has identified a valid instrument. The null hypothesis of this test is that the variable of interest is not endogenous.

[5] Having identified a LATE simply means the relationship revealed would not apply to counties where changing the amount of support for Donald Trump does not affect mask usage.

The p-value from this test can be found at the bottom of Table A2. It shows, given that the instrument is valid, that there is a strong indication of endogeneity.

4. Results

In addition to the main IV model I also estimate three OLS models with various amounts of controls and an IV model with fewer controls. A full set of results are included in the appendix Table A1 and a summary of the main results can be found in Table 2. This section begins with a brief discussion of estimates of the control variable's coefficients before moving to the main point of interest: the influence of mask usage on COVID-19 related deaths.

4.1 Control variables

Many control variables have the associations one would expect from basic correlations and antidotal evidence. The results for the five main estimations can be found in the Appendix Table A2. In OLS estimates, counties with a larger percentage of college graduates (relative to the percentage of individuals with no high school degree) have fewer deaths due to COVID-19. Counties with a larger percentage of minority individuals, Hispanic individuals, and women all have larger percentage of deaths due to COVID-19. A larger percentage of individuals over the age of 60, between 20-29, and 40-49 (all relative to the percentage of individuals that are less than 20 years old) are all associated with a larger percentage of deaths due to COVID-19. Interestingly, in the OLS estimates a larger percentage of individuals 50-59 and a larger population density is associated with fewer COVID-19 deaths.

Additionally, the IV estimations with all the controls emphasize the importance of more fully accounting for endogeneity. The counterintuitive sign on the estimated coefficient for percentage of individuals 50-59 found above now has the expected positive sign and is

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statistically significant under IV, and several other coefficients such as for percentage of individuals over the age of 60 are much larger in magnitude. This may demonstrate so called "smearing", the idea that when the endogeneity of one variable is not accounted for, all coefficients are inconsistent, not just the one associated with the endogenous variable.

4.2 Mask Usage and COVID-19 deaths

I turn now to examining the effect a larger percentage of county's population wearing masks has on deaths from COVID-19. I have two main objectives: first, to demonstrate that incorrect modeling leads to incorrect results and second, to examine both the direction but also the size that mask usage has on COVID-19 related deaths. Table 2 summarizes these main results.

Table 2: Main Results (Dependent Variable: Log Per Capita COVID Deaths on and before September 1)

	OLS (1)	OLS (2)	OLS (3)	OLS (4)	IV (5)	IV (6)
Mask usage frequently/always	0.007***	0.004***	0.001	-0.004***	-0.009***	-0.106***
	(0.002)	(0.001)	(0.001)	(0.001)	(0.003)	(0.017)
Log per cap. COVID deaths July 1		0.724***	0.742***	0.697***	0.754***	0.739***
		(0.010)	(0.011)	(0.010)	(0.012)	(0.022)
Population		✓	✓	✓	✓	✓
Log all deaths per capita 2016			✓	✓	✓	✓
Other Controls				✓		✓
F-test of exclud. instr. in 1st Stage					435.07	49.73
Hausman-WU-Durbin F-stat p-value					0.0005	0.0000

All estimations also include a constant
The estimated coefficients for other controls can be found in Table A2.
Heteroskedasticity-robust standard errors are in parenthesis.
* , ** , and *** represent significance at the 10%, 5%, and 1% levels, respectively.

The simple OLS estimation that includes no controls reveals a positive and significant relationship between mask usage and COVID-19 deaths. When COVID-19 deaths on July 1 and population are added as controls this relationship shrinks by nearly half but remains positive and statistically significant. Furthermore when total deaths in a county in 2016 is added, thus to some extent accounting for the overall risk aversion and fragility of the population, the coefficient on mask use loses both size and statistical significance, but remains positive. In the fourth OLS estimation that includes the full set of controls (estimation (4)), the coefficient on mask usage becomes negative and highly statistically significant, but is small in magnitude. In the final two estimations presented in this table I further account for endogeneity through IV. When IV is used with limit controls (estimation (5)) the coefficient on mask usage remains negative and statistically significant and nearly doubles in value.

Finally, in the preferred specification, when both a full set of controls are included and the IV strategy is implemented, the coefficient on mask usage is negative, statistically significant, and large in magnitude. In particular, in model (6) the coefficient is 21 times larger in magnitude than the coefficient in model 3, which is OLS with all of the control variables. This result indicates that implementing IV substantially alters the average effects of mask usage on deaths.

Specifically, the coefficient on mask usage in model (6) indicates that a one percentage point increase in the amount of individuals in a county who always or frequently wear masks is associated with a 10.5% decrease in deaths related to COVID-19 in a county. To get some indication of effect size I examine this at mean values of all variables. If the mean mask usage would increase from the mean value of 72% to 73%, expected deaths decrease by approximately six (from 58 to 72).[6]

[6] When taking the partial derivatives of COVID-19 deaths per capita and mask usage and solving for the coefficient, the population variable will cancel out.

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5. Robustness Checks and a Further Examination of Excludability

The robustness checks show that the results are not sensitive to different functional forms, the date when COVID-19 deaths are measured, and measures of mask use. The results for these robustness checks can be found in Table A3. The first three estimates found in this table change the way the dependent variable on COVID-19 deaths is measured; it is first measured as per capita without logging (now scaled at per 10,000 individuals) (result (1)), next as a level variable not measured per capita or logged (result (2)), and as log variable not in per capita terms (result (3)). Note in these three estimations the two other measures of deaths, COVID-19 deaths on July 1 and all deaths in 2016, are measured in the same manner as the dependent variable. Next, I return to measuring the dependent variable in log per capita, but alter other aspects of the model. First, the dependent variable of deaths is now the amount of *new* deaths between September 1 and July 1 (result (4)), then the amount of deaths as of the most current date available (result (5)), then the difference between the amount of deaths currently available and amount of deaths on July 1. [7] Finally, I return to original measure of the dependent variable and alter the mask usage variable; first I limit it to the percentage of individuals who say they "always wear a mask…" and then expand it to those who either say they "always, frequently, or sometimes wear a mask…".

None of the results are qualitatively or quantitatively much different from the main results. In fact if I choose the amount of new deaths between September 1st (or the current date) and July 1st it increases the size of the coefficient on mask use. The result from (2) may seem to slightly contradict the six death approximation found in Section 4, however this must be

[7] In this version of paper the most current COVID-19 death number is from November 3.

interpreted with caution since this specification of the dependent variable is not bell shaped and it may not capture the mean county's results.

As stated above, I am unable to formally test for instrument excludability: is the instrument correlated with remaining unobservables once control variables are considered? Of particular concern is the extent that a county's natural proclivity towards COVID-19 deaths and the risk tolerance are correlated with percentage of vote for President Trump. As stated above it is not possible to test this formally however, I am informally able to examine the relationship between the instrument and a county's historic fragility/risk aversion by examining the relationship between my instrument and historic death rate. There is a positive but very weak association between percentage of vote for Donald Trump in 2016 and overall deaths in 2016. A one percentage point increase in the amount of vote for Trump is associated with 0.0000676 more deaths. When county population and other factors are accounted for this amount decreases to 0.000021 deaths. In addition, the association between percentage of vote for Donald Trump in 2016 and *injury* related deaths in 2016 has a negative association, possibly indicating that counties with more overall support for Donald Trump actually have more average risk aversion not less.

An additional concern is that other COVID-19 preventive measures are exclude from the analysis, such as amount of social distancing in a county. This is problematic if these factors are strongly correlated with mask usage *and* the percentage of vote for Donald Trump in 2016. To investigate this possibility I use a measure of social distancing, the difference of average distance traveled by individuals in a county on November 1 2020 relative to average distance traveled by individuals in that same county on the same day of the week in a non-COVID-19 time period

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(the larger this number the less relative social distancing).[8] I standardize this variable since it has a bell-shaped distribution, and doing so will give some indication of size in the interpretations. With this measure I first examine the relationship between it and percentage vote for Donald Trump. While the relationship between these two variables is statistically significant it appears to be small in magnitude: a one percentage point increase in the amount of vote for Donald Trump is associated with an approximately 0.02 standard deviation increase in this relative distance measure (less relative social distancing). When other factors are controlled for this relationship is reduced to a 0.006 standard deviation increase.

Now I examine if including this measure of social distancing as an additional control in my main estimation changes the results. I do not use this variable as a control in my main results because this measure is missing for many counties and reduces the sample size by over 10%. These results can found in Table A4. The first important result is that, in the first stage the relationship between mask usage and social distancing is statistically insignificant. Importantly showing no statistical relationship between this potentially omitted variable and the endogenous variable. Also note from the first stage, the F-statistic for percentage of vote for Donald Trump actually increases with this additional control; however, this F-statistic comparison should be interpreted with caution because of the change in sample size. Most importantly, while the coefficient on mask usage does decrease by a small amount, the results are still qualitatively the same. This indicates that the results are robust to this specification change. Finally, while the coefficient on social distancing in the second stage is statistically significant it is only significant at the 10% level, indicating that this is potentially a weak predictor of COVID-19 deaths.

[8] This number is available from Unacast, and a description of this calculation can be found at: https://www.unacast.com/post/the-unacast-social-distancing-scoreboard.

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6. Conclusion

There is some skepticism about the efficacy of mask usage on COVID-19 outcomes. This study uses a unique county level dataset to examine the effect of mask usage on COVID-19 deaths. Examining this relationship is difficult because of the endogeneity or omitted variable bias that occurs. That is, there are factors that are correlated with both mask usage in a county and COVID-19 deaths. In one direction, the average risk aversion in a county will likely be associated with more COVID-19 deaths and may also be associated with fewer individuals wearing masks. In the other direction, counties with a higher "natural" risk of COVID-19 deaths may have more individuals on average willing to wear masks and also have more COVID-19 deaths. Thus, even the direction of this bias is ambiguous.

In an attempt to identify this relationship, I use a rich set of controls, including COVID-19 deaths prior to the survey on mask usage, and an instrumental variable technique that employs percentage of vote for Donald Trump as an instrument for mask usage. The main finding is that a one-percentage point increase in mask usage would decrease COVID-19 deaths by approximately 10.5% or approximately six people in the average county.

One potential caveat of this study should be noted. It is possible that mask usage could be correlated with general "COVID-19 cautiousness" or lack thereof. It could be that places that have more mask usage also have more social distancing, less social gatherings, more likely use of quarantine or isolation when ill, after a positive test, etc. If the instrument used is also strongly correlated with those activities as well, it would lead to my results over stating the effectiveness of masks on reducing deaths. However, if the exclusion restriction is violated in this way it will only lead to some attenuation bias, but will not negate the main result showing the efficacy of mask usage. In addition, I show that including a crude measure of social distancing will not

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qualitatively alter my results. This potential caveat notwithstanding future work should include a thorough cost-benefit analysis based on the results from this paper.

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Data Sources

CDC COVID-19 death data and county population:
https://usafacts.org/visualizations/coronavirus-COVID-19-spread-map/

CDC Overall Mortality Data:
https://wonder.cdc.gov/wonder/help/cmf.html#
https://wonder.cdc.gov/cmf-icd10.html

The New York Times and Dynata Mask-Wearing Survey Data:
https://github.com/nytimes/COVID-19-data/tree/master/mask-use

County level presidential election data:
https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/VOQCHQ

Social Distancing Data:
https://www.unacast.com/post/the-unacast-social-distancing-scoreboard

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Appendix: Additional Tables

Table A1: First Stage Results (Mask usage frequently/always)

	1st Stage	1st Stage
% Vote Trump	-0.301***	-0.130***
	(0.014)	(0.018)
Log per cap. COVID deaths July 1	0.784***	0.547***
	(0.178)	(0.172)
Population per 100,000	-0.072*	-0.264***
	(0.037)	(0.048)
Log per cap. All deaths 2016	2.938***	2.251***
	(0.195)	(0.202)
% college[a]		0.032
		(0.045)
% High school graduates[a]		-0.038
		(0.044)
Percentage minorities		0.138***
		(0.017)
Percentage Hispanic		0.392***
		(0.018)
Percentage female		0.590***
		(0.119)
Percentage age 20 to 29[b]		1.802***
		(0.125)
Percentage age 30 to 39[b]		0.282
		(0.215)
Percentage age 40 to 49[b]		2.722***
		(0.206)
Percentage age 50 to 59[b]		1.566***
		(0.203)
Percentage age 60 and over[b]		1.215***
		(0.085)
Average household income (1000s)		0.213***
		(0.025)
Population density (square miles)		$-3.34\text{x}10^{-4}$***
		($7.59\text{x}10^{-5}$)
F-stat of the excluded instrument	435.07	49.73

All estimations also include a constant.
Heteroskedasticity-robust standard errors are in parenthesis.
* , ** , and *** represent significance at the 10%, 5%, and 1% levels, respectively.
a: The reference group is % with no high school degree.
b: The reference group is % under the age of 20.

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Table A2: Full Set of Results

	OLS (1)	OLS (2)	OLS (3)	OLS (4)	IV (5)	IV (6)
Mask usage frequently/always	0.007***	0.004***	0.001	-0.004***	-0.009***	-0.106***
	(0.002)	(0.001)	(0.001)	(0.001)	(0.003)	(0.017)
Log per cap. COVID deaths July 1		0.724***	0.742***	0.697***	0.754***	0.739***
		(0.010)	(0.011)	(0.010)	(0.012)	(0.022)
Population per 100,000		0.009***	-0.006**	-0.009***	-0.005*	-0.034***
		(0.003)	(0.003)	(0.002)	(0.003)	(0.007)
Log per cap. All deaths 2016			0.078***	0.076***	0.119***	0.328***
			(0.009)	(0.010)	(0.017)	(0.048)
% college[a]				-0.012***		$7.74\text{x}10^{-6}$
				(0.002)		(0.005)
% High school graduates[a]				$6.95\text{x}10^{-5}$		-0.002
				(0.002)		(0.005)
Percentage minorities				0.012***		0.033***
				(0.001)		(0.004)
Percentage Hispanic				0.017***		0.061***
				(0.001)		(0.008)
Percentage female				0.040***		0.102***
				(0.007)		(0.018)
Percentage age 20 to 29[b]				0.021***		0.218***
				(0.007)		(0.036)
Percentage age 30 to 39[b]				0.004		0.043
				(0.011)		(0.026)
Percentage age 40 to 49[b]				0.093***		0.349***
				(0.011)		(0.048)
Percentage age 50 to 59[b]				-0.055***		0.140***
				(0.010)		(0.040)
Percentage age 60 and over[b]				0.026***		0.154***
				(0.004)		(0.023)
Average household income (1000s)				0.001		0.023***
				(0.001)		(0.005)
Population density (square miles)				$-1.10\text{x}10^{-5}$*		$-4.08\text{x}10^{-5}$***
				($6.47\text{x}10^{-6}$)		($1.25\text{x}10^{-5}$)
Number of Observations	3142	3142	3103	3096	3079	3079

All estimations also include a constant.
Heteroskedasticity-robust standard errors are in parenthesis.
* , ** , and *** represent significance at the 10%, 5%, and 1% levels, respectively.
a: The reference group is % with no high school degree.
b: The reference group is % under the age of 20.

Table A3: Robustness Checks

	Per cap Not logged	Not Per Cap Not Logged	Not Per Cap Logged	Log Per Cap Between 7-1 and 9-1	Log Per Cap Current	Log Per Cap Between 7-1 and Current	Log Per Cap Mask always only	Log Per Cap Mask Always, freq., and Sometimes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mask usage	-0.130***	-1.456*	-0.100***	-0.171***	-0.200***	-0.170***	-0.076***	-0.150***
	(0.037)	(0.744)	(0.016)	(0.026)	(0.047)	(0.025)	(0.010)	(0.026)
COVID deaths 7-1	1.081***	0.913***	0.750***	0.281***	0.610***	0.193***	0.733***	0.729***
	(0.023)	(0.022)	(0.022)	(0.034)	(0.059)	(0.032)	(0.018)	(0.022)
Pop in 100,000s	-0.017	29.138***	-0.033***	-0.009	-0.149***	-0.020**	-0.027***	-0.038***
	(0.011)	(6.255)	(0.007)	(0.010)	(0.038)	(0.009)	(0.005)	(0.009)
All deaths 2016	0.006	-0.003	0.564***	0.165**	2.265***	0.254***	0.312***	0.329***
	(0.004)	(0.008)	(0.043)	(0.074)	(0.139)	(0.071)	(0.039)	(0.051)
% college[a]	-0.002	-0.088	4.76×10^{-5}	0.005	-0.044***	-0.003	-0.003	-0.004
	(0.012)	(0.180)	(0.005)	(0.008)	(0.016)	(0.008)	(0.004)	(0.006)
% High sch, grads[a]	0.012	0.162	-0.003	-0.007	-0.013	-0.008	0.001	0.007
	(0.012)	(0.126)	(0.005)	(0.008)	(0.015)	(0.008)	(0.004)	(0.006)
% minorities	0.081***	0.420**	0.032***	0.053***	0.065***	0.050***	0.030***	0.035***
	(0.008)	(0.173)	(0.004)	(0.006)	(0.010)	(0.006)	(0.003)	(0.004)
% Hispanic	0.119***	1.143***	0.060***	0.091***	0.104***	0.082***	0.059***	0.061***
	(0.016)	(0.237)	(0.008)	(0.012)	(0.021)	(0.011)	(0.006)	(0.008)
% female	0.254***	0.968	0.096***	0.152***	0.213***	0.140***	0.101***	0.112***
	(0.054)	(0.761)	(0.017)	(0.027)	(0.051)	(0.026)	(0.015)	(0.021)
% age 20 to 29[b]	0.329***	1.817	0.211***	0.313***	0.431***	0.296***	0.189***	0.235***
	(0.092)	(1.822)	(0.035)	(0.056)	(0.098)	(0.053)	(0.026)	(0.042)
% age 30 to 39[b]	0.080	-3.622**	0.042*	0.083**	0.016	0.071*	0.040*	0.044
	(0.068)	(1.543)	(0.025)	(0.042)	(0.071)	(0.039)	(0.022)	(0.028)
% age 40 to 49[b]	0.682***	4.736**	0.340***	0.527***	0.553***	0.462***	0.294***	0.375***
	(0.129)	(2.014)	(0.047)	(0.074)	(0.134)	(0.071)	(0.034)	(0.057)
% age 50 to 59[b]	0.067	1.891	0.126***	0.195***	0.221**	0.180***	0.135***	0.126***
	(0.086)	(1.701)	(0.038)	(0.062)	(0.112)	(0.059)	(0.032)	(0.041)
% age60 and over[b]	0.220***	1.126	0.143***	0.247***	0.198***	0.224***	0.136***	0.162***
	(0.063)	(1.137)	(0.023)	(0.036)	(0.064)	(0.034)	(0.018)	(0.027)
Household income	0.018	-0.324	0.024***	0.029***	0.072***	0.031***	0.017***	0.026***
	(0.011)	(0.290)	(0.004)	(0.007)	(0.013)	(0.007)	(0.003)	(0.005)
Population density	-1.61×10^{-4}***	4.67×110^{-4}	-4.13×10^{-5}***	-8.61×10^{-5}***	-7.60×10^{-5}**	-7.88×10^{-5}***	-3.06×10^{-5}***	-4.41×10^{-5}***
	(5.56×10^{-5})	(1.6810^{-3})	(1.25×10^{-5})	(2.01×10^{-5})	(3.39×10^{-5})	(1.82×10^{-5})	(1.09×10^{-5})	(1.33×10^{-5})
1st stage F-stat of excl. IV	68.22	73.33	49.27	49.31	49.73	49.73	84.93	43.29

All estimations also include a constant
Heteroskedasticity-robust standard errors are in parenthesis.
* , ** , and *** represent significance at the 10%, 5%, and 1% levels, respectively.
a: The reference group is % with no high school degree.
b: The reference group is % under the age of 20.

Table A4: Including Social Distancing Measure

	Mask Usage as Left-hand side Variable (1st Stage)	COVID-19 Deaths as Left-hand side Variable (2nd Stage)
Mask usage		-0.086***
		(0.012)
% Vote Trump	-0.170***	
	(0.020)	
Social distancing	0.109	0.038*
	(0.219)	(0.022)
Log per cap. COVID deaths July 1	0.700***	0.751***
	(0.182)	(0.020)
Population per 100,000	-0.276***	-0.029***
	(0.051)	(0.006)
Log per cap. All deaths 2016	2.300***	0.296***
	(0.214)	(0.037)
% college[a]	-0.010	-0.003
	(0.046)	(0.005)
% High school graduates[a]	-0.020	-0.002
	(0.046)	(0.005)
Percentage minorities	0.097***	0.028***
	(0.018)	(0.003)
Percentage Hispanic	0.370***	0.050***
	(0.020)	(0.006)
Percentage female	0.575***	0.084***
	(0.125)	(0.015)
Percentage age 20 to 29[b]	1.799***	0.185***
	(0.130)	(0.028)
Percentage age 30 to 39[b]	0.271	0.032
	(0.225)	(0.023)
Percentage age 40 to 49[b]	2.617***	0.304***
	(0.218)	(0.035)
Percentage age 50 to 59[b]	1.524***	0.106***
	(0.219)	(0.032)
Percentage age 60 and over[b]	1.184***	0.127***
	(0.088)	(0.017)
Average household income (1000s)	0.221***	0.019***
	(0.025)	(0.004)
Population density (square miles)	-3.54×10^{-4}***	-3.41×10^{-5}***
	(8.05×10^{-5})	(1.06×10^{-5})
F-stat of the excluded instrument	72.08	
Number of Observations	2766	2766

All estimations also include a constant.
Heteroskedasticity-robust standard errors are in parenthesis.
* , ** , and *** represent significance at the 10%, 5%, and 1% levels, respectively.
a: The reference group is % with no high school degree.
b: The reference group is % under the age of 20.

Aggregate and intergenerational implications of school closures: A quantitative assessment[1]

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Date submitted: 7 November 2020; Date accepted: 11 November 2020

A majority of governments around the world unprecedentedly closed schools in response to the COVID-19 pandemic. This paper quantitatively investigates the macroeconomic and distributional consequences of school closures through intergenerational channels in the medium and long term. The model economy is a dynastic overlapping generations general equilibrium model in which schools, in the form of public education investments, complement parental investments in producing children's human capital. We calibrate the stationary equilibrium of the model to the U.S. economy and compute the equilibrium responses following unexpected school closure shocks. We find that school closures have moderate long-lasting adverse effects on macroeconomic aggregates such as output. In addition, we find that school closures reduce intergenerational mobility, especially among older children. Finally, we find that lower substitutability between public and parental investments induces larger damages in the aggregate economy and overall lifetime incomes of the affected children, while mitigating negative impacts on intergenerational mobility. In all findings, heterogeneous parental responses to school closures play a key role. Our results provide a quantitatively relevant dimension to consider for policymakers assessing potential costs of school closures.

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1 We are grateful to Hans-Martin von Gaudecker, Ingo Isphording, Krisztina Molnar, Kjetil Storesletten, Michele Tertilt and seminar participants at Norwegian School of Economics for helpful comments and suggestions. Financial supports from the German Science Foundation (DFG) through the CRC TR 224 Project A3 and Shanghai Municipal Education Commission through Gaofeng Chuangxin Project Grant are gratefully acknowledged.

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1 Introduction

In early 2020, a majority of governments around the world unprecedentedly decided to close daycares, pre-schools, and primary and secondary schools nationwide in response to the COVID-19 pandemic. Interestingly, the extent to which governments engage in or maintain school closures varies significantly over time across countries.[1] The key to such decisions is understanding the benefits and costs of school closures during the pandemic. In this regard, there has been relatively active research on the short-run consequences of school closures, such as the benefit of keeping parents to be involved in economic activities (e.g., Alon et al. 2020) and the epidemiological risk associated with the reopening of schools (e.g., Isphording et al. 2020). However, there have been few studies that quantify and enhance the understanding of various factors behind the longer-term consequences of school closures. This line of research is important for policymakers who assess the relative costs and benefits of school closures, not only today but also as related to potential pandemics in the future.

In this paper, we quantitatively investigate the medium- and long-term aggregate and distributional consequences of school closures through intergenerational channels.[2] Specifically, we use a dynastic overlapping generations general equilibrium model where parents are linked to children through multiple transmission channels to study how study how school closures affect aggregate dynamics, inequality, and intergenerational mobility over time and across cohorts. The model economy combines a standard heterogeneous-agent incomplete-markets framework with production (Aiyagari 1994) with the model of altruistic dynasties in the tradition of Becker and Tomes (1986), while endogenizing several additional key ingredients relevant to our research questions. These include multi-stage human capital production technology for children (Cunha and Heckman 2007), where inputs include not only parental financial and time investments but also schools in the form of public investments that complement parental investments. Children become young adults with human capital and assets shaped by their parents and make their own college decisions that affect their future life-cycle wage profiles. Aggregate production combines skilled and unskilled workers along with capital to produce final outputs.

We calibrate the stationary equilibrium of the model to the U.S. economy in normal times. The stationary equilibrium of our model is consistent with various empirical features such as the increasing importance of parental financial investments over children's age, the income quintile transition matrix, and the rising income inequality over the life cycle, all of which are important for the main analysis of school closures effects. For the main quantitative analysis, we model the

[1] The United Nations Educational, Scientific and Cultural Organization (UNESCO) provides a daily map showing the global status on school closures caused by COVID-19 at https://en.unesco.org/covid19/educationresponse.

[2] We note that we do not attempt to quantify the overall effects of school closures induced by COVID-19. Instead, our study focuses on the consequences of school closures on intergenerational human capital transmission through the human capital production function, while abstracting their potential effects on parents' income or parents' human capital accumulation, among others. This is not because we believe that the other effects are irrelevant. Rather, it is because COVID-19 induced various drastic measures in addition to school closures, which makes it very difficult to empirically disentangle the partial effects of school closures on parents.

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school closure shock as an unexpected temporary decline in public investments in the child human capital production (Fuchs-Schündeln et al. 2020). We then investigate the economy over the full transition equilibrium paths.[3] In particular, our rich framework naturally enables us to answer how the effects of school closures differ across child cohorts of different ages at the time of the school closure and what role the substitutability between public and private investments plays in determining the consequences of school closures.

Our first finding on aggregate consequences is that school closures have moderate yet long-lasting adverse effects on the aggregate economy. For instance, the year-long closure (including vacations) would lead to up to 0.3% decline in outputs over a number of decades to follow. In the short term, as parents' incentive to substitute for the reduced public inputs increases, aggregate capital accumulation is negatively affected, which in turn affects aggregate output negatively. More importantly, as the children directly affected by the school closure shocks enter the labor market, the decreased human capital accumulated during their childhood contributes negatively to outputs persistently in the following decades. On the other hand, we find that the adverse effects of school closures on college attainment and cross-sectional inequality are too negligible to be economically meaningful over time. We show that general equilibrium plays a very important quantitative role in mitigating the above aggregate effects. Specifically, when we fix the prices at the stationary equilibrium level, we find that output effects are overstated by 50%, and college-educated labor falls by twice as much.

We then investigate the implications of school closures for intergenerational mobility. Unlike the negligible effects on inequality, we find that the school closure shocks strengthen the extent to which income distribution is associated between parents and children. Specifically, a 1-year school closure would lower the probability of children born into the bottom income quintile moving up to the top quintile by 2-3%. We also find a significant loss (around 1%) in average lifetime income for the affected cohorts. In particular, these adverse effects on relative mobility (measured by intergenerational elasticities and the upward mobility rate) and absolute mobility (measured by average lifetime income changes) are found to be larger among older children. This is due to the temporary nature of the school closure shock. We show that although young children are more negatively affected on impact, they recover due to the equalizing effect of public education (Fernandez and Rogerson 1998) over time without school closures.[4] We further show that both the direct impact of the school closures on the child human capital production function as well as the endogenous parental responses, featuring positive income gradients especially in financial investments for older children, underlie the above findings.[5]

[3] We also confirm that our model-generated data following the school shocks are reasonably in line with the causal evidence of school closures on test scores in the Netherlands (Engzell et al. 2020) as well as time-use evidence in Germany (Grewenig et al. 2020).

[4] In fact, this is consistent with the empirical evidence by Kuhfeld et al. (2020) showing that students who lose more ground during summer break experience steeper growth during the following school year.

[5] We also explore how these school-closure effects would change in the presence of virtual schooling that disproportionately benefits children from college-educated parents, capturing better-educated parents' advantages with better skills and network. We find that these would mitigate average lifetime income losses of the affected children at the

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Finally, we also systematically analyze the role of substitutability between public and parental investments in producing children's human capital. Motivated by the possibility that this elasticity of substitution could vary across countries, depending on the relative importance of private versus public education system, we consider an alternative model economy with a lower elasticity (1.5 versus 3.0 in the baseline economy).[6] We find that although the alternative economy is able to match the important target statistics equally well, it results in school closure effects that differ substantially as compared to the baseline economy. Specifically, it generates substantially larger declines in aggregate output and lifetime income of the affected children, whereas it reduces intergenerational mobility much less. As public investments are harder to substitute, children experience greater losses in human capital during childhood, which is amplified by the weaker parental motive to compensate for the fall in human capital. This muted incentive to respond also implies a smaller parental background role, thereby generating much weaker impacts on intergenerational mobility.

Following a seminal study by Restuccia and Urrutia (2004), the literature increasingly investigates intergenerational economic persistence in quantitative macroeconomic models with heterogeneous households where the distribution of income across generations is endogenously determined. The steady-state version of our general equilibrium model herein builds on the model in Yum (2020) by allowing flexible substitutability between public and private investments – a departure from most existing papers in the literature that assume that public and parental investments are perfectly substitutable (e.g., Holter 2015, Lee and Seshadri 2019, Daruich 2020).[7]

A recent paper by Fuchs-Schündeln et al. (2020) also studies the implications of school closures in a rich two-generations lifecycle model. Although both studies share similar emphasis on the importance of parental income and children's age, the focus is quite different. Specifically, while they focus on implications of school closures for affected children's welfare and inequality, we focus on the implications for macroeconomic aggregates and intergenerational mobility and on the role of substitutability between public and parental investments. Moreover, unlike theirs, our key interest of aggregate implications requires an overlapping-generations general equilibrium framework as a natural laboratory.[8]

The empirical education and economics literature has shown that school interruptions can have negative consequences for children's learning and skills (e.g., Cooper et al. 1996, Meyers and Thomasson 2017). A number of papers explore learning losses in terms of test scores during summer breaks, but the evidence is somewhat mixed in terms of magnitudes (see Atteberry and McEachin (2020) and references therein). Other papers exploit teacher strikes, weather-related school closures,

expense of lower intergenerational mobility.

[6] For example, East Asian countries generally have large private education markets, which are believed to be very good substitutes for public education. By contrast, in Scandinavian countries, where public educations play a huge role, parental education investments are less likely to be an adequate substitute.

[7] Unlike most existing studies that focus on steady-state comparisons, our quantitative exercise provides one of the few numerical implementations of the equilibrium paths over the perfect foresight transition in general equilibrium models with endogenous intergenerational human capital transmission (e.g., Lee and Seshadri 2019, Daruich 2020).

[8] In Section 4, we indeed confirm that general equilibrium effects are quantitatively important for our research question.

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and the German short school years in the 1960s, as summarized in Hanushek and Woessmann (2020) and Kuhfeld et al. (2020). There is a growing body of empirical literature that estimates how the COVID-19 pandemic has affected parental responses using real-time data (e.g., Adams-Prassl et al. 2020, Bacher-Hicks et al. forthcoming, Chetty et al. 2020). For example, Chetty et al. (2020) find that during the school closures, children, especially those who live in low-income areas, experienced reductions in math learning, measured by online Zearn Math participation. There are also empirical studies, such as Engzell et al. (2020) and Grewenig et al. (2020), which estimate these effects on learning losses and parental responses in European countries, which we discuss more extensively in Section 5. These empirical findings are broadly in line with the key mechanisms in our quantitative theory; that is, that school closures induce human capital losses, especially among children from low income families, and that parents try to compensate for these losses. Our quantitative theoretical results could help better understand the underlying sources of these empirical observations.

To the best of our knowledge, our paper is the first to conduct analysis on aggregate effects of school closures in a dynamic general equilibrium model with endogenous parental decisions. Hanushek and Woessmann (2020) document the empirical literature on learning losses and suggest that such short-term evidence could potentially point to the sizeable long-term consequences of school closures. Building on their insight, we bring various relevant factors, such as endogenous parental investment responses, dynamic effects on human capital, and general equilibrium considerations, into a structural framework. Our consequential estimates of the negative effects on the aggregate economy, based on the model that is broadly in line with the existing short-run empirical evidence, are conservative but are still highly relevant given that these output declines last for many decades to follow.[9]

This paper is organized as follows. Section 2 presents the core of the model economy. Section 3 describes the calibration strategy and the properties of the stationary equilibrium of the calibrated model economy. Section 4 presents the main quantitative analysis of school closures along the full equilibrium transitional paths. Section 5 concludes the paper. The Appendix includes the details about the model and additional quantitative results.

2 Model Economy

We begin by describing the model economy used for the quantitative analysis. It is based on the model in Yum (2020), which builds on a standard incomplete-markets general equilibrium framework in a production economy (Aiyagari 1994) while following the tradition of Becker and Tomes (1986) for intergenerational transmissions. Parents face the identical multi-period human capital production technology but are heterogeneous in assets and productivity. To enrich the analysis of

[9]For instance, Hanushek and Woessmann (2020) computes that the year-long closure would lead to 4.3% lower GDP on average for the remainder of the century, based on the back-of-the-envelope calculations relying on estimates of short-term learning losses and returns to education. In our model, the persistent reductions in GDP following the same closure peaks at approximately -0.3%.

school closures, our model allows the elasticity of substitution between parental and public investments to be less than perfect. In our equilibrium model with altruistic parents, parental choices such as parental investments and inter-vivos transfers take into account parents' expectations of the future paths of the economy following unexpected school closures today.

Time (t) is discrete, and a model period corresponds to five years. Our analysis not only considers steady states but also transitional dynamics across steady states. We now describe the key part of the model: households' decision problems when they are young. The remaining model environments, including the firm's problem, the government budget and the definition of equilibrium, are standard and are provided in the Appendix.

There is a continuum (measure one) of overlapping generations in the economy. A household always includes an adult but it can also include a child. As summarized in Table 1, an adult lives for eleven model periods (age 20-74) as an active decision maker. Specifically, in the first model age $j = 1$, an agent chooses whether or not to obtain a college education. Once this higher education choice is made, the adult agent supplies labor from $j = 1$ until retirement at the beginning of $j = 10$ (age 65). The agent then lives for two more periods as a retiree and dies at the end of period $j = 11$ (age 75). In all periods, the agent makes a standard consumption-savings decision.

An important building block of our model is the intergenerational transmission. This initially happens at the beginning of $j = 3$ (age 30) when the adult is endowed with a child. In addition to the stochastic ability draw for the child, the parent invests time and money in their children in multiple periods $j = 3, 4, 5$ while taking into account the presence of public education. Before the child becomes independent, the parent decides the amount of inter-vivos transfers to give in $j = 6$. Then, the child, now an adult, forms a new household when the parent enters $j = 7$, and faces the same lifetime structure, described above.

All households share identical preferences over consumption c and hours worked n, represented by a standard separable utility function:

$$\frac{c^{1-\sigma}}{1-\sigma} - b\frac{n^{1+\chi}}{1+\chi}, \tag{1}$$

where $\sigma > 0$ and $\chi > 0$ capture the curvatures and $b > 0$ is the disutility constant.

In all working-age periods ($j = 1, 2, ..., 9$), labor earnings y are subject to progressive taxation. Specifically, after-tax earnings with respect to pre-tax earnings y are given by:

$$\lambda_j \left(y/\bar{y}\right)^{-\tau_j} y, \tag{2}$$

following a simple, yet widely used, parametric form (Benabou 2002; Heathcote, Storesletten and Violante 2014). Note that τ_j shapes the degree of progressivity, λ_j captures the scale of taxation and $\bar{y}$ denotes average earnings. We allow τ_j and λ_j to depend on age in order to capture differences in labor taxation across the family structure (Guner, Kaygusuz and Ventura 2014; Holter, Krueger and Stepanchuk 2019).

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Table 1: Timeline of life-cycle events for a parent-child pair

	Parent											
Age	*20-24*	*25-29*	*30-34*	*35-39*	*40-44*	*45-49*	*50-54*	*55-59*	*60-64*	*65-69*	*70-74*	
Model age j	1	2	3	4	5	6	7	8	9	10	11	
	$\leftarrow$ – – – – – – – – – – – – – – – – Consumption-savings – – – – – – – – – – – – – – – $\rightarrow$											
	$\leftarrow$ – – – – – – – – – – – – – – Labor supply – – – – – – – – – – – – – $\rightarrow$									Retirement		
	College		$\leftarrow$ – Parental – $\rightarrow$ investments			Inter-vivos						
	Child											
Age			*0-4*	*5-9*	*10-14*	*15-19*	*20-24*	*25-29*	*30-34*	*35-39*	*40-44*	...
Model age j			$\leftarrow$ –– Childhood –– $\rightarrow$				1	2	3	4	5	...
							$\leftarrow$ – – – – – – Consumption-savings – – – – $\rightarrow$					
							$\leftarrow$ – – – – – – – – –– Labor supply – – – – $\rightarrow$					
							College		$\leftarrow$ – Parental – $\rightarrow$ investments			

In all periods, capital income is subject to a tax rate of τ_k if the capital income is positive. Households receive lump-sum transfers T and are allowed to borrow up to the borrowing limit (Aiyagari 1994).

We now present the household's decision problems sequentially starting with the first adult age $j = 1$.

Model Age 1 In period t, a child who forms a new household in the model age $j = 1$ (20 years old) begins their adult life with individual state variables such as age j, a human capital stock of h_t, a level of asset holdings a_t, the childhood learning ability ϕ, and the aggregate state variable of the distribution of households in the economy $\boldsymbol{\pi}_t$. The two individual state variables, h_t and a_t, are endogenously shaped by the parent of the agent during childhood. Although childhood ability does not enter adults' economic decisions directly, it is still a state variable because it determines the learning ability of their own child later in $j = 3$. The distribution of households in period t, $\boldsymbol{\pi}_t$, is an aggregate state variable because equilibrium prices depend on the equilibrium distribution.

Given the state variables, the agent first decides whether or not to obtain a college education. The value of not going to college ($\kappa = 1$) is given by:

$$N(h_t, a_t, \phi; \boldsymbol{\pi}_t) = \max_{\substack{c_t \geq 0;\ a_{t+1} \geq \underline{a} \\ n_t \in [0,1]}} \left\{ \frac{c_t^{1-\sigma}}{1-\sigma} - b\frac{n_t^{1+\chi}}{1+\chi} + \beta \mathbb{E}_{z_{t+1}} V_2(h_{t+1}, a_{t+1}, \kappa, \phi, \boldsymbol{\pi}_{t+1}) \right\} \tag{3}$$

$$
\begin{aligned}
\text{subject to} \quad c_t + a_{t+1} &\leq \lambda_1 \left(w_{\kappa,t}(\boldsymbol{\pi}_t) h_t n_t / \bar{y}\right)^{-\tau_1} w_{\kappa,t}(\boldsymbol{\pi}_t) h_t n_t + P_t + T_t \\
P_t &= \left(1 + r_t(\boldsymbol{\pi}_t)\right) a_t - \tau_k r_t(\boldsymbol{\pi}_t) \max\{a_t, 0\} \\
h_{t+1} &= \exp(z_{t+1}) \gamma_{1,\kappa} h_t \\
\kappa &= 1 \\
\boldsymbol{\pi}_{t+1} &= \Gamma(\boldsymbol{\pi}_t),
\end{aligned}
$$

where $w_{\kappa,t}(\boldsymbol{\pi}_t)$ is the rental price of human capital for skill type κ per unit hours of work, $r_t(\boldsymbol{\pi}_t)$ is the real interest rate, and P_t is the initial assets given by the parents (i.e., inter-vivos transfers). Human capital increases at the gross growth rate of $\gamma_{j,\kappa}$, which is allowed to depend on age j and education κ to capture the empirical age-profile of wage for each education type. Human capital is subject to the idiosyncratic shock z, which follows an independent and identically distributed (i.i.d.) normal distribution with mean zero and the standard deviation of σ_z. We assume a standard incomplete-markets structure by assuming that the idiosyncratic shock z is not fully insurable as a is not a state-contingent asset. $\Gamma(\boldsymbol{\pi}_t)$ captures the law of motion for the distribution of households as perceived by households, which should be consistent with the actual evolution of the distribution in equilibrium. Because h_{t+1} is uncertain in period t, households form expectation regarding the next period's value.

An alternative choice is to go to college and become a skilled worker. College education is costly and requires the agent to pay a stochastic fixed cost where ξ follows an i.i.d. log normal distribution with a mean of μ_ξ and a standard deviation of σ_ξ. The value of going to college after the realization of ξ is given by:

$$
C(h_t, a_t, \phi, \xi; \boldsymbol{\pi}_t) = \max_{\substack{c_t \geq 0;\ a_{t+1} \geq \underline{a} \\ n_t \in [0,1]}} \left\{ \frac{c_t^{1-\sigma}}{1-\sigma} - b\frac{n_t^{1+\chi}}{1+\chi} + \beta \mathbb{E}_{z_{t+1}} V_2(h_{t+1}, a_{t+1}, \kappa, \phi; \boldsymbol{\pi}_{t+1}) \right\}
$$

$$
\begin{aligned}
\text{subject to} \quad c_t + a_{t+1} + \xi &\leq \lambda_1 \left(w_{\kappa,t}(\boldsymbol{\pi}_t) h_t n_t / \bar{y}\right)^{-\tau_1} w_{\kappa,t}(\boldsymbol{\pi}_t) h_t n_t + P_t + T_t \\
P_t &= \left(1 + r_t(\boldsymbol{\pi}_t)\right) a_t - \tau_k r_t(\boldsymbol{\pi}_t) \max\{a_t, 0\} \\
h_{t+1} &= \exp(z_{t+1}) \gamma_{1,\kappa} h_t \\
\kappa &= 2 \\
\boldsymbol{\pi}_{t+1} &= \Gamma(\boldsymbol{\pi}_t).
\end{aligned} \tag{4}
$$

The above conditional decision problem illustrates how college education could benefit households in the model. First, college educated workers are in the skilled labor market ($\kappa = 2$), which gives $w_{\kappa,t}(\boldsymbol{\pi}_t)$. Second, college-educated workers experience a life cycle profile of wages that differs from that of their counterparts without a college degree through $\gamma_{j,\kappa}$.

Given the above two conditional value functions, households make a discrete college choice after observing a draw of ξ. The expected value at the beginning of $j = 1$ is:

$$V_1(h_t, a_t, \phi; \boldsymbol{\pi}_t) = \mathbb{E}_\xi \max \left\{ N(h_t, a_t, \phi; \boldsymbol{\pi}_t), C(h_t, a_t, \phi, \xi; \boldsymbol{\pi}_t) \right\}. \tag{5}$$

Model Age 2 In $j = 2$, households face a standard life cycle problem with consumption-savings and labor supply decisions, represented by the following:

$$V_2(h_t, a_t, \kappa, \phi; \boldsymbol{\pi}_t) = \max_{\substack{c_t \geq 0;\ a_{t+1} \geq \underline{a} \\ n_t \in [0,1]}} \left\{ \frac{c_t^{1-\sigma}}{1-\sigma} - b \frac{n_t^{1+\chi}}{1+\chi} + \beta \mathbb{E}_{z_{t+1}, \phi' | \phi} V_3(h_{t+1}, a_{t+1}, \kappa, \phi'; \boldsymbol{\pi}_{t+1}) \right\}$$

$$\begin{aligned} \text{subject to} \quad c_t + a_{t+1} &\leq \lambda_2 \left(w_{\kappa,t}(\boldsymbol{\pi}_t) h_t n_t / \bar{y} \right)^{-\tau_2} w_{\kappa,t}(\boldsymbol{\pi}_t) h_t n_t + (1 + r_t(\boldsymbol{\pi}_t))\, a_t - \tau_k r_t(\boldsymbol{\pi}_t) \max\{a_t, 0\} + T_t \\ h_{t+1} &= \exp(z_{t+1}) \gamma_{2,\kappa} h_t \\ \boldsymbol{\pi}_{t+1} &= \Gamma(\boldsymbol{\pi}_t). \end{aligned}$$

The higher education decision made in $j = 1$ shows up as a state variable κ. Because a child is going to be born in the next period, households take expectation over the ability of the new child to be born (ϕ'). We assume that it is correlated across generations, following an AR(1) process in logs

$$\log \phi' = \rho_\phi \log \phi + \epsilon_\phi \tag{6}$$

where $\epsilon_\phi \sim \mathcal{N}(0, \sigma_\phi^2)$. This form of the exogenous source of a positive correlation of human capital across generations is standard in the literature (e.g., Restuccia and Urrutia 2004; Lee and Seshadri 2019; Yum 2020), capturing any intergenerational persistence, such as genetic transmission, not endogenously explained by the model.

Model Ages 3-5 At the beginning of $j = 3$, a child is born with learning ability ϕ. Building on the childhood skill formation literature (Cunha and Heckman 2007, Caucutt and Lochner 2020), human capital formation is modeled as a multi-stage process that takes place in $j = 3, 4, 5$, featuring parental inputs in different periods that are complementary and parental investments that are more effective for those who have higher current human capital stock. In addition, we also introduce public investments in different stages, which are complementary inputs to parental investments, to capture the effects of schools (Fuchs-Schündeln et al. 2020).

The structure is similar to those in Lee and Seshadri (2019) and Yum (2020). Specifically, let I_j denote the total investment inputs in period j, aggregated following the two nested constant elasticity of substitution (CES) technology:

$$I_j = \left\{ \left(\theta_j^x \left(\frac{x_j}{\bar{x}} \right)^{\zeta_j} + (1 - \theta_j^x) \left(\frac{e_j}{\bar{e}} \right)^{\zeta_j} \right)^{\frac{\psi}{\zeta_j}} + \left(\frac{g_j}{\bar{g}} \right)^{\psi} \right\}^{\frac{1}{\psi}}, \tag{7}$$

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where x_j denotes parental time investments, e_j is parental monetary investments, g_j denotes public education investment, and $\theta_j^x \in (0,1)$ captures the relative share of time investments in period j. Each input is entered after being normalized by its unconditional mean. The first CES aggregation is about parental time and money inputs. The elasticity of substitution between parental time and money investments depends on the stage j and is given by $1/(1-\zeta_j)$, where $\zeta_j \leq 1$. The second CES aggregation is about the aggregated parental inputs and public investments. There, we allow the elasticity of substitution to be less than perfect, which is given by $1/(1-\psi)$, where $\psi \leq 1$. Although this departure from perfect substitutability is relatively unexplored, we are going to show that this elasticity is highly relevant to the implications of school closures in various dimensions, as analyzed systematically in Section 5.

The aggregated inputs in different periods $j = 3, 4, 5$ shape the child's human capital at the end of $j = 5$. In other words, $h_{c,6}$, is given by the technology f :

$$h_{c,6} = \phi f(I_3, I_4, I_5). \tag{8}$$

As is standard in the literature, we assume unit elasticity of substitution across periods and constant returns to scale (e.g., Lee and Seshadri 2019, Fuchs-Schündeln et al. 2020, Yum 2020). This is captured by the following recursive formulation:

$$\begin{aligned} h_{c,j+1} &= \phi I_j^{\theta_j^I} h_{c,j}^{1-\theta_j^I}, \quad \text{if } j = 5; \\ &= I_j^{\theta_j^I} h_{c,j}^{1-\theta_j^I}, \quad \text{if } j = 3, 4, \end{aligned} \tag{9}$$

where $\theta_j^I \in (0,1)$. Note that this technology features two properties highlighted by Cunha and Heckman (2007) and Caucutt and Lochner (2020): (i) dynamic complementarity, meaning that a higher $h_{c,j}$ increases the productivity of investments in period j ($\frac{\partial^2 f}{\partial I_i \partial h_{c,j}} > 0$) and (ii) self-productivity, meaning that a higher $h_{c,j}$ increases human capital in the next period $h_{c,j+1}$. The initial human capital h_c in $j = 3$ when a child is just born is set to 1 as we allow for heterogeneity in learning ability ϕ (Lee and Seshadri 2019).

We now incorporate the above technology into the decision problem of parents. The following functional equation summarizes a parent's problem in $j = 3$:

$$V_3(h_t, a_t, \kappa, \phi; \boldsymbol{\pi}_t) = \max_{\substack{c_t, e_t \geq 0;\ a_{t+1} \geq \underline{a} \\ x_t, n_t \in [0,1]}} \left\{ \frac{(c_t/q)^{1-\sigma}}{1-\sigma} - b\frac{n_t^{1+\chi}}{1+\chi} - \varphi x_t + \beta \mathbb{E}_{z_{t+1}} V_4(h_{t+1}, a_{t+1}, \kappa, h_{c,t+1}, \phi; \boldsymbol{\pi}_{t+1}) \right\}$$

$$\text{subject to} \quad c_t + a_{t+1} + e_t \leq \lambda_j \left(w_{\kappa,t}(\boldsymbol{\pi}_t) h_t n_t/\bar{y}\right)^{-\tau_j} w_{\kappa,t}(\boldsymbol{\pi}_t) h_t n_t + (1 + r_t(\boldsymbol{\pi}_t))\, a_t - \tau_k r_t(\boldsymbol{\pi}_t) \max\{a_t, 0\} + T_t$$

$$x_t + n_t \leq 1$$

$$h_{t+1} = \exp(z_{t+1}) \gamma_{3,\kappa} h_t$$

$$h_{c,t+1} = \left\{ \left(\theta_3^x \left(x_t/\bar{x}\right)^{\zeta_3} + (1-\theta_3^x) \left(e_t/\bar{e}\right)^{\zeta_3} \right)^{\frac{\psi}{\zeta_3}} + (g_3/\bar{g})^{\psi} \right\}^{\frac{\theta_3^I}{\psi}} h_{c,t}^{1-\theta_3^I} \tag{10}$$

$$\boldsymbol{\pi}_{t+1} = \Gamma(\boldsymbol{\pi}_t). \tag{11}$$

We assume that the child shares the household consumption c, captured by the household equivalence scale q. (10) is obtained by combining (7) and (9). Parents decide how much time and money to invest, while taking into account the returns to such investments, according to the production technology (8), the associated costs in terms of utility φ, and the reduced income available for consumption and savings.

The parent's decision problems in $j = 4, 5$ are similarly given by:

$$V_j(h_t, a_t, \kappa, h_{c,t}, \phi; \boldsymbol{\pi}_t) = \max_{\substack{c_t, e_t \geq 0;\ a_{t+1} \geq \underline{a} \\ x_t, n_t \in [0,1]}} \left\{ \frac{(c_t/q)^{1-\sigma}}{1-\sigma} - b \frac{n_t^{1+\chi}}{1+\chi} - \varphi x_t + \beta \mathbb{E}_{z_{t+1}} V_{j+1}(h_{t+1}, a_{t+1}, \kappa, h_{c,t+1}, \phi; \boldsymbol{\pi}_{t+1}) \right\}$$

$$\text{subject to} \quad c_t + a_{t+1} + e_t \leq \lambda_j \left(w_{\kappa,t}(\boldsymbol{\pi}_t) h_t n_t/\bar{y}\right)^{-\tau_j} w_{\kappa,t}(\boldsymbol{\pi}_t) h_t n_t + (1 + r_t(\boldsymbol{\pi}_t))\, a_t - \tau_k r_t(\boldsymbol{\pi}_t) \max\{a_t, 0\} + T_t$$

$$x_t + n_t \leq 1$$

$$h_{t+1} = \exp(z_{t+1}) \gamma_{j,\kappa} h_t$$

$$h_{c,t+1} = \left\{ \left(\theta_4^x \left(x_t/\bar{x}\right)^{\zeta_4} + (1-\theta_4^x) \left(e_t/\bar{e}\right)^{\zeta_4} \right)^{\frac{\psi}{\zeta_4}} + (g_4/\bar{g})^{\psi} \right\}^{\frac{\theta_4^I}{\psi}} h_{c,t}^{1-\theta_4^I} \quad \text{if } j = 4 \tag{12}$$

$$= \phi \left\{ \left(\theta_5^x \left(x_t/\bar{x}\right)^{\zeta_5} + (1-\theta_5^x) \left(e_t/\bar{e}\right)^{\zeta_5} \right)^{\frac{\psi}{\zeta_5}} + (g_5/\bar{g})^{\psi} \right\}^{\frac{\theta_5^I}{\psi}} h_{c,t}^{1-\theta_5^I} \quad \text{if } j = 5 \tag{13}$$

$$\boldsymbol{\pi}_{t+1} = \Gamma(\boldsymbol{\pi}_t).$$

where state variables further include the child's human capital level at the beginning of the period h_c.

Model Age 6 At the end of $j = 6$, the child leaves the original household and forms a new household. The asset level of the newly formed household is shaped by the parents' decision on inter-vivos transfers a_c. Holding other things constant, this would facilitate the child's college decision indirectly by alleviating the financial burden of college. The decision problem in $j = 6$ is summarized by:

$$V_6(h_t, a_t, \kappa, h_{c,t}, \phi; \boldsymbol{\pi}_t) = \max_{\substack{c_t \geq 0;\ a_{t+1} \geq \underline{a} \\ n_t \in [0,1] \\ a'_c \geq [0, \bar{a}_c]}} \left\{ \frac{(c_t/q)^{1-\sigma}}{1-\sigma} - b\frac{n_t^{1+\chi}}{1+\chi} + \beta \mathbb{E}_{z_{t+1}} \left[V_7(h_{t+1}, a_{t+1}, \kappa; \boldsymbol{\pi}_{t+1}) + \eta V_1(h'_c, a'_c, \phi; \boldsymbol{\pi}_{t+1}) \right] \right\} \tag{14}$$

$$\begin{aligned} \text{subject to} \quad c_t + a_{t+1} + a'_c &\leq \lambda_j \left(w_{\kappa,t}(\boldsymbol{\pi}_t) h_t n_t / \bar{y} \right)^{-\tau_j} w_{\kappa,t}(\boldsymbol{\pi}_t) h_t n_t + (1 + r_t(\boldsymbol{\pi}_t)) a_t - \tau_k r_t(\boldsymbol{\pi}_t) \max\{a_t, 0\} + T_t \\ h_{t+1} &= \exp(z_{t+1}) \gamma_{6,\kappa} h_t \\ h'_c &= \gamma_c h_{c,t} \\ \boldsymbol{\pi}_{t+1} &= \Gamma(\boldsymbol{\pi}_t). \end{aligned}$$

Note that the continuation value now includes the initial value function of the child V_1, defined above in (5), discounted by the degree of altruism $\eta > 0$. This continuation value clearly shows our dynastic set-up, where parents care about their child's utility, which in turn depends on the following generations' utilities in the spirit of Becker and Tomes (1986). Note also that parents cannot borrow from their child's future income since a'_c cannot be negative.

3 Calibrating the Model Economy in Stationary Equilibrium

Before we evaluate the aggregate and intergenerational implications of school closures using numerical experiments in the next section, we discuss how we calibrate the model economy. Our approach is to calibrate the model in stationary equilibrium to U.S. data.

We consider two model economies in which the elasticity of substitution between public and parental investments differs. There is limited evidence of this in the literature. A number of papers assume perfect substitutability whereas a few papers estimate that this elasticity of substitution is less than perfect.[10] Given that there is no clear consensus on this parameter that could be useful for understanding the theoretical mechanisms we study here, we consider two different values. Specifically, the baseline economy uses $\psi = 2/3$, implying that the elasticity of substitution is 3. This implies that public and parental investments are highly substitutable, close to a common assumption of perfect substitutability in the literature, yet are less than perfect substitutes.[11] In addition, we also consider an alternative model economy with $\psi = 1/3$, implying a lower value of the elasticity of substitution (1.5). This alternative model would enable us to investigate the role of the elasticity of substitution between public and private investments, which could differ across countries.

[10] In the literature, it is common to assume that parental and public investments are perfect substitutes. For example, see Restuccia and Urrutia (2004), Holter (2015), Lee and Seshadri (2019), Daruich (2020), Yum (2020) among others. On the other hand, there are lower estimates of this elasticity of substitution, such as 1.92 by Blankenau and Youderian (2015) and 2.43 by Kotera and Seshadri (2017).

[11] This value is similar to the one in Fuchs-Schündeln et al. (2020).

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We first discuss the parameter values that are commonly set across the two model economies. Then, we explain the remaining parameters that are internally calibrated to match the relevant target statistics in the U.S. We then present the properties of the baseline model economy in stationary equilibrium before we conduct numerical experiments on school closures in the next section.

3.1 Common Parameters

We adopt a standard approach to match relevant U.S. statistics externally and internally. We first discuss the first set of parameters that are calibrated externally. These are also commonly set across the two model economies that vary in terms of the elasticity of substitution between public and parental investments.

First, for preference parameters, we set the value of σ equal to 1.5 such that the intertemporal elasticity of substitution for consumption is 2/3 and set the value of χ equal to 4/3 such that the Frisch elasticity is 0.75 (Chetty et al. 2013). Because our model frequency is five years, the relevant margin of labor supply adjustments includes both intensive and extensive margins. The value of q, which determines how consumption enters into utility in the presence of a child in the household is set to 1.59, based on the OECD equivalence scale. Next, we set $\bar{a}_c$ to be 25% of average income, which is close to the exemption limit of gift tax in the U.S.

The life cycle wage profiles for high- and low-skilled workers are governed by the gross growth rates of human capital during adulthood $\{\gamma_{j,\kappa}\}_{j=1}^{8}$. These values are computed based on Rupert and Zanella's (2015) estimates from the Panel Study of Income Dynamics (PSID). As reported in Table $A2$, these estimates show two notable patterns: (i) for each education group, the growth rates are higher in the early adult periods and then decline with age, and (ii) college-educated workers experience much higher growth rates.

We now discuss parameters related to government. Recall that the degree of progressivity in labor taxation differs based on household structure in the model. As reported in Table $A3$, progressivity tends to be higher for households with a child. The capital income tax rate τ_k is set to 0.36. These taxation-related parameters are based on the estimates by Holter et al. (2019). The next parameters are related to the public education expenditures. Here, we follow the approach used by Restuccia and Urrutia (2004) and Holter (2015): education expenditures by state and federal governments are defined as public investments, while those by local government are considered to be private investments. This is because education in the U.S. is financed locally depending on where people live. Using the information in 2016 from the Education at Glance published by the OECD, we compute the g_j in $j = 3, 4, 5$ relative to steady-state output per capita to be 0.060, 0.098 and 0.111, respectively.[12] A key feature of g_j is that it increases as achild progresses through education stages. Next, following Lee and Seshadri (2019), the value for government lump-sum transfers T is

[12]These values are in line with estimates by Holter (2015) and Lee and Seshadri (2019), the latter of who use micro data (PSID-CDS).

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Table 2: Internally calibrated parameters and target statistics for the baseline model economy

Parameter		Target statistics	Data	Model
β	.972	Equilibrium real interest rate (annualized)	0.04	0.04
b	23.3	Mean hours of work in $j = 3, ..., 9$	.287	.287
η	.323	Mean inter-vivos transfers/GDP per-capita	.056	.057
θ_3^x	.753	Mean parental time investments in $j = 3$	.061	.060
θ_4^x	.397	Mean parental time investments in $j = 4$	.036	.036
θ_5^x	.259	Mean parental time investments in $j = 5$	.020	.020
θ_3^I	.571	Mean parental monetary investments in $j = 3$	.098	.098
θ_4^I	.349	Mean parental monetary investments in $j = 4$	.113	.111
θ_5^I	.229	Mean parental monetary investments in $j = 5$	.128	.125
ζ_3	-1.78	Educational gradients in parental time in $j = 3$ (%)	20.9	20.5
ζ_4	-0.19	Educational gradients in parental time in $j = 4$ (%)	14.8	15.1
ζ_5	-0.12	Educational gradients in parental time in $j = 5$ (%)	20.2	20.5
ν	.529	Fraction with a college degree (%)	34.2	34.2
μ_ξ	.244	Average college expenses/GDP per-capita	.140	.140
δ_ξ	.732	Observed college wage gap (%)	75.0	85.8
ρ_ϕ	.103	Intergenerational corr of percentile-rank income	.341	.342
σ_ϕ	.590	Gini wage	.370	.369
σ_z	.140	Slope of variance of log wage from $j = 2$ to $j = 8$	.180	.184

set to 2% of steady-state output per capita to capture welfare programs, and the borrowing limit is set as $\underline{a} = -T/(1 + r)$, where r is the real interest rate in the steady state. The value of Ω is set to imply that the social security replacement rate is 40%.

Finally, we discuss parameters related to the production sector. We set $\alpha_K = 0.36$ to be consistent with the capital share in the aggregate US data. The five-year capital depreciation rate δ is based on 2.5% of the quarterly depreciation rate. These values are standard in the literature. We set $\rho = 1/3$, implying that the elasticity of substitution between skilled and unskilled workers is 1.5 (Ciccone and Peri 2005).

3.2 Parameters Calibrated Internally

We now discuss the parameters that are calibrated internally by matching the relevant target statistics in U.S. data, given the value of ψ. The discussion herein focuses on the baseline economy with $\psi = 2/3$, as summarized in Table 2, and the Appendix provides the calibrated parameters for the model economy with $\psi = 1/3$. These parameter values are determined as minimizers of the squared sum of the distance between the relevant statistics from the data and those from the model-generated data. Although there is a relatively large number of parameters and targets, each parameter is connected to its corresponding target quite well. We now explain these relationships. All target statistics reported in Table 2 are constructed and discussed in details by Yum (2020).

The first parameter in Table 2 is β, which captures the household's discount factor. Its relevant target is chosen to be the annual interest rate of 4%. The next parameter b is the disutility constant

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for labor supply. Its relevant target is chosen to be the mean hours worked by those aged between 30 and 65 (or $j = 3, ..., 9$). Assuming that the weekly feasible time endowment is $105(= 15 \times 7)$ hours, excluding sleeping time and basic personal care, this statistic in the data yields $30.16/105 = 0.287$ as a target. There is a disutility parameter φ for parental time investments. This parameter is linked to B such that the marginal disutility of parental time investment is given by the marginal disutility of work evaluated at the mean hours worked.[13] Next, η governs the degree of altruism and is calibrated to match the mean inter-vivos transfers. Because inter-vivos transfers in the model are meant to capture financial help for college, we choose the total parental transfers made for children during the college years.[14] As a result, we obtain a target statistic of 0.056 – the ratio of the mean parental financial transfers to the five-year GDP per-capita.

We now discuss parameters related to the child human capital production functions. Recall that in each j, there are three parameters—θ_j^x, θ_j^I and ζ_j—in (10), (12) and (13). We calibrate these parameters by exploiting the clear linkages between each of these parameters and its corresponding target moment in the model economy. Specifically, θ_j^x captures the relative importance of parental time investments (vs. parental financial investments), and it clearly increases the mean parental time investments in period j, which are used as target statistics. Statistics on parental time investments are obtained from the 2003-2017 American Time Use Survey (ATUS) only with educational, interactive activities that require the presence of both a parent and a child in a common space.[15] A key feature of these moments is that the mean time investment is highest in the earliest period $j = 3$ (0.061 in the model or 6.4 hours per week) and it decreases with children's age. The next parameter θ_j^I increases overall parental investments in period j. Hence, we use the mean private education spending in each period as a target moment for θ_j^I. As discussed above, the mean private education expenditure in the data is constructed as the sum of private spending and local government spending because public schools are largely funded locally in the U.S. Consequently, we obtain the target statistics of $0.098, 0.113$ and 0.128 for $j = 3, 4$ and 5, respectively. Note that, unlike the parental time inputs, parental financial inputs increase with children's education stage, in line with the existing evidence. Finally, ζ_j shapes the elasticity of substitution between time and money in period j. These are calibrated to match the salient facts in the U.S. that more educated parents spend more time with children (Guryan et al. 2008; Ramey and Ramey 2010). Specifically, we allow our model to replicate the fact that parents who are college-educated spend around 20 percent more time with their children than those without a college degree.[16] In particular, we allow the elasticity of substitution to be j-dependent since the same elasticity of substitution would lead

[13] Specifically, φ is given by $b\bar{n}^{\chi}$. We calibrate θ_j^x to match the mean parental time investments in j, as described below.

[14] Specifically, we sum the money from parents and college transfers from age 18 to 26, reported in Table 4 of Johnson (2013), while accounting for the fraction of recipients.

[15] Such activities include reading to/with children, playing with children, doing arts and crafts with children, playing sports with children, talking with/listening to children, looking after children as a primary activity, caring for and helping children, doing homework, doing home schooling, and other related educational activities.

[16] To be precise, the education gradient is defined as the percentage difference in mean parental time investments between education groups while controlling for parental observables. See the Appendix for details.

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to a lower educational gradient in early periods (Yum 2020). As a result, our calibration leads to a lower elasticity of substitution in $j = 3$ (0.36) than in later periods (0.84 and 0.89 in $j = 4$ and 5, respectively), implying that parental time and monetary investments are especially complementary to each other when children are very young. Although not reported in Table 2, the parameter γ_c that maps childhood human capital to adulthood human capital is also internally calibrated to be 3.03 such that the steady-state output per capita is normalized to 1.

The next parameters are related to college education. In the aggregate production function (19), ν is calibrated to match the fraction of people with a college degree (34.2%). The mean of college costs is determined by μ_ξ, which naturally gives a target statistic: the equilibrium ratio of the mean (tuition and non-tuition) expenses after financial aid to per capita GDP. According to detailed procedures explained by Yum (2020), this statistic (relative to the five-year GDP) is 0.140. The next parameter is is related to the variance of the college costs. Note that as σ_ξ increases, the observed wage premium would decline since college decisions are more strongly shaped by costs relative to pre-college human capital. Therefore, its relevant target is set to be the observed college wage premium of 75% (Heathcote et al. 2010).

Next, ρ_ϕ determines the persistence of exogenous ability across generations. We set its relevant target as the rank correlation of family income of 0.341 (Chetty et al. 2014). Note that Chetty et al. (2014) estimate intergenerational persistence using a proxy income variable instead of lifetime income due to the data limitation, as is common in the literature. Therefore, our target statistic from the model also uses proxy income.[17] The last two parameters in Table 2 govern the variability of wages in different ways. Although either would increase the overall wage inequality in the model, the variability of the idiosyncratic shocks to adult human capital σ_z also shapes the rising lifecycle inequality. Therefore, the two target statistics are the Gini coefficient of wage and the difference between the variance of log wage at age 55-59 ($j = 2$) and that of log wage at age 25-29 ($j = 8$), as reported in Table 2 (Heathcote et al. 2010).

The alternative model with a lower elasticity of substitution between public and parental investments ($\psi = 1/3$) is calibrated using the same calibration strategy. The calibration results are reported in the Appendix.[18]

3.3 Properties of the Baseline Model in Stationary Equilibrium

In this subsection, we present the properties of the baseline model in stationary equilibrium before we conduct the main quantitative analysis on school closures.

We first evaluate the intergenerational mobility implied by the model. Specifically, we measure the model-implied intergenerational mobility in three ways and compared them to the data coun-

[17] Specifically, Chetty et al. (2014) measure a child's income at around 30 years old, averaged over two years. The parent's income is averaged over five years when parents' ages are around 45 years. Equivalently, our model-based proxy income is measured for parents in $j = 6$, and for children in $j = 3$.

[18] The most notable difference, compared to the baseline model with $\pi = 2/3$, is that the elasticity of substitution between parental time and money investments is lower (0.22, 0.58 and 0.57 for $j = 3, 4$, and 5, respectively).

Table 3: Intergenerational persistence estimates

	U.S. data	Model	
	Chetty et al. (2014)	Proxy income	Lifetime income
IGE: log-log slope	0.344	.309	.375
Rank corr: rank-rank slope	0.341	.342	.361

terparts. The data counterparts are from Chetty et al. (2014) who use administrative data.[19] As mentioned above, income in the model is the five-year per parent sum of labor earnings, interest income, and social security benefits.

The first measure is the intergenerational elasticity (IGE), obtained from the following log-log equation:

$$\mathcal{Y}_{child} = \rho_0 + \rho_1 \mathcal{Y}_{parent} + \varepsilon, \tag{15}$$

where $\mathcal{Y}$ is log permanent income. This is a conventional way to measure the degree of intergenerational persistence in the empirical literature. Its interpretation is straightforward: a 1% increase in parental permanent income is associated with a ρ_1% increase in their children's permanent income. The second measure is to use a rank-rank specification instead of a log-log specification (Chetty et al. 2014). This can be estimated when $\mathcal{Y}$ is the percentile rank of income. This slope coefficient (or the rank correlation) tells us that a one percentage point increase in parent's percentile rank is associated with a ρ_1 percentage point increase in their children's percentile rank. In the model, we estimate these slopes using both proxy income, which is defined equivalently as its empirical counterpart, and the lifetime income, which is constructed as present-value lifetime income discounted according to the interest rate (Haider and Solon 2006) in stationary equilibrium.

Table 3 reports the two slope estimates from the data and the model. Recall that we directly targeted to match the rank correlation using proxy income. Although data limitation prevents researchers from investigating the lifetime income, it is possible to estimate the mobility measures using the lifetime income in the model. As is well known in the literature, we can see that the estimate of the IGE using lifetime income (0.376) is substantially larger than the counterpart using proxy income (0.309) because the short-term income may not represent the long-term lifetime income (Haider and Solon 2006). Interestingly, this attenuation bias is smaller in the rank correlation (0.342 versus 0.361).

The above slope estimates are easy to interpret and convenient, but they do not fully describe how income distribution persists across generations. The income quintile transition matrix provides

[19] Specifically, parental income is defined as the average five-year pre-tax income per parent, which is either the sum of Adjusted Gross Income, tax-exempt interest income and the non-taxable portion of Social Security and Disability benefits (if a tax return is filed) or the sum of wage earnings, unemployment benefits, and gross social security and disability benefits. For children's income, they use a short horizon (2-year average) due to data availability.

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Table 4: Income quintile transition matrices: data vs. model

Unit: %	U.S. data					Model									
	Chetty et al. (2014)					Proxy income					Lifetime income				
Parent	Child quintile					Child quintile					Child quintile				
quintile	1st	2nd	3rd	4th	5th	1st	2nd	3rd	4th	5th	1st	2nd	3rd	4th	5th
1st	33.7	28.0	18.4	12.3	7.5	35.2	25.8	17.7	14.1	7.2	35.9	27.0	16.1	14.3	6.8
2nd	24.2	24.2	21.7	17.6	12.3	24.5	22.2	21.7	18.1	13.5	25.1	22.1	21.7	18.2	12.9
3rd	17.8	19.8	22.1	22.0	18.3	18.9	19.2	22.6	20.6	18.7	19.4	18.5	23.6	20.0	18.5
4th	13.4	16.0	20.9	24.4	25.4	14.0	18.1	20.2	22.4	25.3	13.4	17.6	21.6	21.9	25.6
5th	10.9	11.9	17.0	23.6	36.5	7.3	14.8	17.7	24.9	35.3	6.4	14.9	16.9	25.5	36.3

a richer description of how economic status is transmitted across generations.[20] We now compare the quintile transition matrix from the model-generated data to the empirical quintile transition matrix (Chetty et al. 2014). Because calibration does not directly target any elements in the income quintile transition matrix, this is a natural way of evaluating how successful a model is as a quantitative theory of intergenerational mobility (Yum 2020).[21]

Table 4 reports the transition matrices, obtained from U.S. (Chetty et al. 2014) and model-generated data. The data shows that the probability of children remaining in the bottom quintile when their parents' income is also in the bottom quintile is 33.7%. Similarly, the probability of staying in the top income quintile is quite high at 36.5%. A particularly interesting one is the probability of moving up from the bottom quintile to the top quintile, namely upward mobility. In the data, the upward mobility rate is 7.5%. The middle panel of Table 4 displays the quintile transition matrix from the model when the equivalent measure of proxy income is used. The model successfully replicates the empirical patterns noted above. In particular, the upward mobility rate in the model is 7.2%, which is very close to the data counterpart.

Table 4 also reports the quintile transition matrix using lifetime income. Compared to the one with proxy income, we can see that the diagonal elements are generally higher, which is consistent with lower intergenerational mobility measured by the slope coefficients in Table 3. The upward mobility rate in terms of lifetime income is slightly lower at 6.8%. In the following numerical experiments, we use the intergenerational mobility measures based on lifetime income because the mobility measures based on proxy income are subject to attenuation biases (Haider and Solon 2006) as also confirmed by the model-generated data in stationary equilibrium.

As is well known, cross-sectional inequality in labor market variables tends to increase over the lifecycle in the data (e.g., Heathcote et al. 2010). As Figure 1 shows, the model replicates

[20] An income quintile transition matrix is a 5 by 5 matrix where the (a, b) element provides the conditional probability that a child's lifetime income is in the b-th quintile, conditional on the parent's income belonging to the a-th quintile. Quintiles are based on their own generation.

[21] Note that the same correlation of income across generations can be consistent with different quintile transition matrices. This is similar to the fact that the same Gini coefficient can be consistent with different shapes of income distributions.

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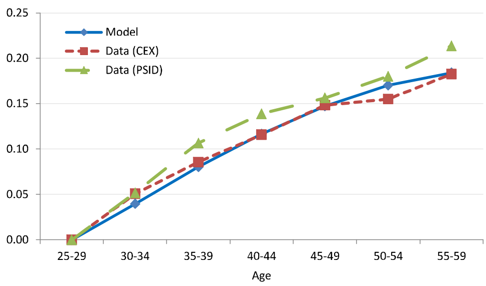
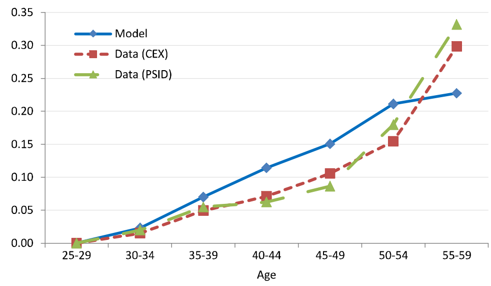
Figure 1: Inequality over the life cycle





Note: The left figure shows the variance of log wage by age relative to age 25-29. The right figure shows the variance of log wage by age relative to age 25-29. US data is from Heathcote et al. (2010).

the increasing dispersion in wages (left) and earnings (right) quite well.[22] We note that these features are important because a higher dispersion in income among relatively older parents would be transmitted into the extent to which parents with different permanent incomes afford additional parental investments in response to school closures.

4 Quantitative Analysis of School Closures

We now move on to the main analysis of this paper on the implications of school closures. This requires us to compute the equilibrium away from the steady state. We first explain how we conduct the numerical experiments and then briefly discuss empirical consistency with the best existing evidence on the short-run effects of school closures. Afterwards, our main analyses on the medium- and long-run effects follow.

4.1 Computational Experiment Design

In this section, we analyze transitional dynamics following unexpected school closure shocks. In the simulation, in each period, the economy consists of 11 cohorts, and each cohort is composed of 500,000 household units. Thus, the total number of households is 5,500,000 in each period t. We first simulate the model economy for sufficiently long periods until it reaches the stationary equilibrium.[23] The economy is in stationary equilibrium at $t = ..., -2, -1, 0$, and school closures unexpectedly take place at the beginning of $t = 1$. Our baseline exercise considers universal,

[22] Note that this is disciplined mainly by the calibrated dispersion in idiosyncratic shocks to adult human capital.
[23] Specifically, we simulate 55 periods to reach the steady state from a given initial distribution and drop the first 50 periods. We keep the five periods of the steady state economy to keep information about parents whose children directly experience school closures.

nationwide school closures where all schools are closed for the same period of time.[24] As in Fuchs-Schündeln et al. (2020), we represent these school closures by reducing the size of public investments in the child human capital production according to the closure length. For example, if a school closure lasts for one year, we reduce one-fifth the public investments in $t = 1$. We consider three different lengths of school closures: 0.5, 1 and 1.5 years. We note that our notion of school closure length should be interpreted in terms of academic years (AY), and should be mapped to the actual days of school closures with caution due to the presence of breaks, even in normal times.[25] In $t = 2, 3, ...$, there are no further shocks and the economy returns to the original stationary equilibrium.[26] We compute the transitional equilibrium paths under perfect foresight.

In addition to the consequences of school closures on macro aggregates such as output, our analysis also focuses on heterogeneous impacts on children of different ages in which the school closure shock hits the economy. Therefore, we will also present the results for three child cohorts that directly experience the school closure in different ages: the cohort aged between 0 and 4 (*Cohort 1* or C1) at the school closure; that aged between 5 and 9 (*Cohort 2* or C2); and that aged between 10 and 14 (*Cohort 3* or C3). We also keep track of parents matched to these children to examine intergenerational implications.

4.2 Quantitative Results

Consistency with short-run evidence on school closures Since most governments (including the U.S. government) closed schools in early 2020 in response to the COVID-19 pandemic, there has been limited empirical evidence on the direct effects of such closures on the general child performance even in the short run.[27] Although there has been suggestive evidence to indicate significant drops in the amount of learning (Chetty et al. 2020), the lack of data prevents researchers from investigating the negative consequences of learning loss in a broader setting with causal interpretations. Ideally, we would need to have observations on a large number of representative students whose academic progress (e.g., in terms of test scores) in multiple points within a year is observed, not only in the regular year but also during the pandemic period when schools were

[24] In the Appendix, we also examine the effects of *partial* school closures where there is a stochastic difference in closure lenghs across households. This exercise reflects the fact that there could be regional variations in the effective length of school closures, caused by the uncertain local pandemic progress and political factors not modeled herein.

[25] For example, as 4-5 months of vacation already exist in normal years, the school closure of 1-year length would correspond to the actual days of closure for 7-8 months (including weekends). One might think that 1.5 AY is not realistic, but given that the current forecast projects that the vaccine is going to be widely available only in 2022, we think that it is still worth considering as an extreme case. Further, it helps us to investigate potential nonlinearity in the effects. Finally, we note that, although school closures reduce g_j for children's human capital production, they are not changed in the government budget because these shocks are meant to capture non-permanent school closures.

[26] Although shocks are temporary and relatively small, it is important to run the model economy long enough for several reasons. First, as our key variable is lifetime income, we need to generate the whole life-cycle for the youngest cohort that directly experienced the school closures. In addition, as we show below, school closure shocks have long-lasting effects. In our exercises, we use $t = 30$.

[27] The empirical literature on the learning loss during summer break (Cooper et al. 1996, Atteberry and McEachin 2020) could be useful, although it might be nontrivial to apply the summer break effects to the effects of closing during regular school periods, especially at longer horizons.

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almost universally closed.

An exception is Engzell et al. (2020) who use a rich nationally representative data set from the Netherlands. Their data set satisfies all of the ideal settings mentioned above, thereby allowing them to conduct a different-in-difference estimation. According to their estimates based on composite scores aggregating math, reading and spelling scores for the students aged 7-11, they estimate a learning loss of about 3.1 percentile points or 0.08 standard deviations during the lockdown which induced school closures of 2 to 2.5 months. Although child human capital in the model does not exactly correspond to the observed test scores, it is useful to compare how school closures affect human capital loss in the model. In our model, we find that a 0.5-year closure leads to a human capital loss of 2.4 percentile points or 0.07 standard deviations.[28] In addition, we also find a larger fall in children's human capital with lower parental permanent income (Figure 6), in line with their findings that parental education is the only significant factor shaping the negative impacts. This comparison shows that our model generates reasonable magnitudes of negative impacts on the children's outcomes.[29]

As discussed below, parental responses to school closures are an important channel that not only mitigates the aggregate effects but also impacts intergenerational effects. A recent paper by Grewenig et al. (2020) provides interesting results related to our findings. They use a survey in Germany with detailed time use information and find that children reduced their daily learning time significantly during school closures. More interestingly, they also find that the reduction in learning time was not statistically different by parental education or income. This is in fact consistent with our finding below that the positive income gradients in parental responses materialize in terms of money, not in terms of time (Figure 5).[30]

Aggregate Implications We now present the main systematic results from the quantitative exercises. Figure 2 plots the dynamics of output, capital, efficiency units of labor for non-college and college graduates following unexpected school closures of different lengths in $t = 1$. Overall, the changes of these aggregate variables are small yet persistent. The top-left panel shows that the aggregate output declines gradually over time, and this decline continues until period 11. The top-right panel implies that the initial drop in output is due to dissaving to increase parental investments. This reduction in capital is amplified over time by lower human capital formations of those who experienced the school closures during their childhood. The bottom panels suggest that parents increase their labor supply to earn more income, thus raising parental investments to counter school closures. The aggregate efficiency unit of labor for each skill type starts to decrease

[28] Note that the 0.5-year-closure in the model should approximately correspond to 3.5 months closure net of summer breaks and holidays.

[29] Recall that human capital in our model is supposed to be a broader concept than test scores and that our model allows any compensatory parental investments within a model period of five years, which might dampen the very short-run loss right after school closure shocks.

[30] For example, parents could spend more on better tablets or online resources of higher quality (Bacher-Hicks et al. forthcoming), which would increase the efficiency of learning, but not necessarily the time spent on these activities.

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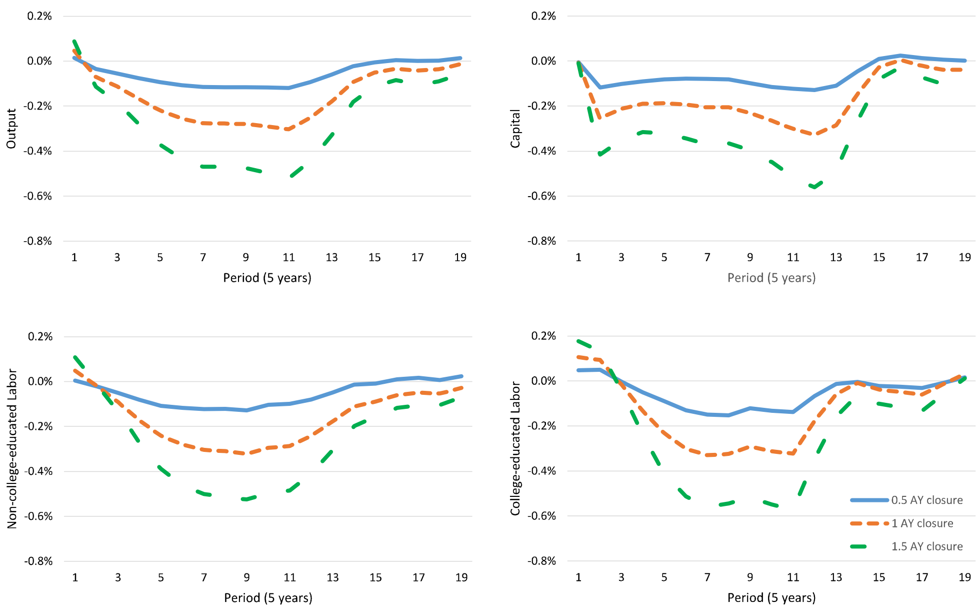
Figure 2: Evolution of macroeconomic aggregates



when the cohorts, experiencing these school closures during childhood, enter the labor market with lower levels of human capital. This reduction in the aggregate labor continues to decline until $t = 11$ and gradually recovers afterward.

Another noticeable feature is that the responses of the aggregate variables are non-linear to the length of school closures. The top-left panel of Figure 2 demonstrates that in period 11, while the 0.5-year-closure reduces output by less than 0.1%, the 1.5-year-closure decreases output by more than 0.4%. The top-right panel shows that the 1.5-year-closure reduces capital three times more than does the 0.5-year-closure. These non-linear responses also appear in the aggregate labor responses, as shown in the bottom panels. These findings imply that, since school closures would have small, non-linear, and long-lasting impacts on the aggregate economy, these impacts might be difficult to forecast empirically in a reduced-form way.

Another noticeable feature we highlight is the general equilibrium effects that play a role in adjusting the magnitude of the responses of these aggregate variables to the school closures. In particular, as revealed by a comparison between Figures 2 and 3, these general equilibrium effects tend to balance the responses of the efficiency units of labor between college and non-college graduates. Specifically, Figure 3 shows that when prices are fixed at their stationary equilibrium

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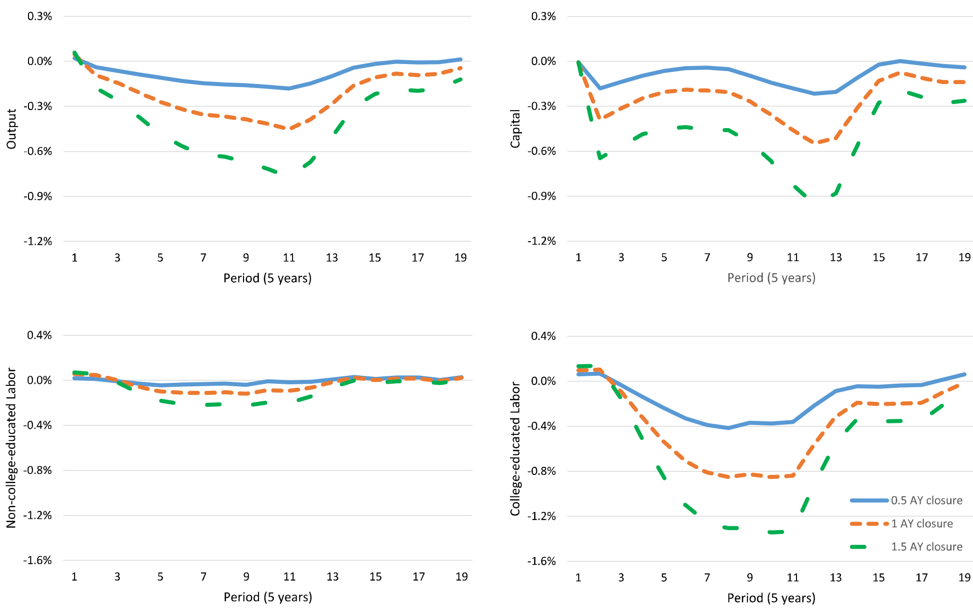
Figure 3: Evolution of macroeconomic aggregates without GE feedback



Note: Factor prices are fixed at stationary equilibrium levels

levels, aggregate labor for college graduates is more significantly reduced in response to these school closures. A change in efficiency units of labor in each education group can be driven by (i) the fraction of the skill group relative to population (extensive margin), (ii) hours worked conditional on working (intensive margin), and (iii) the quality of the work force (human capital). The large reduction in the efficiency units of labor for the college-educated individuals that materializes gradually over time is due to the direct loss of human capital and a relatively noticeable decrease in college attainment indirectly driven by lower child human capital, both of which were caused by school closures. By contrast, we see that labor efficiency for non-college graduates does not decline as much because the reduction in human capital is offset by an increased number of people who do not go to college in the case where prices are exogenously fixed. However, in general equilibrium, the decrease in college attainment tends to increase the relative premium of college graduates, thereby dampening the reductions in the efficiency units of labor for college graduates and amplifying those for non-college graduates. Similarly, general equilibrium effects mitigate the reductions in aggregate capital by increasing the risk-free interest rate. Consequently, general equilibrium effects moderate the overall responses of output to these school closure shocks.

We now move on to the distributional changes over time. Table 5 reports the effects of school

Table 5: Distributional changes over time

	Steady state	Time (1 period: 5 years) 1	2	3	4	5
		% change rel. to no school closure				
Closure length: 0.5 AY						
Gini income	.341	0.0	0.0	0.0	0.1	0.1
Bottom 20% inc (%)	7.73	-0.0	-0.0	-0.1	-0.1	-0.0
Share of college (%)	34.2	0.0	0.0	0.0	-0.0	-0.0
Closure length: 1 AY						
Gini income	.341	0.0	-0.0	0.1	0.1	0.1
Bottom 20% inc (%)	7.73	-0.0	0.0	-0.1	-0.1	-0.0
Share of college (%)	34.2	0.0	0.1	0.0	-0.1	-0.1
Closure length: 1.5 AY						
Gini income	.341	0.0	-0.1	0.1	0.2	0.2
Bottom 20% inc (%)	7.73	-0.1	0.0	-0.1	-0.2	-0.1
Share of college (%)	34.2	0.0	0.1	-0.0	-0.1	-0.2

closures on three cross-sectional inequality measures, demonstrating that school closure shocks bring about negligible changes in cross-sectional inequalities. In the 0.5-year-closure scenario, there is almost no change in the Gini coefficient of current income for the first three periods and there is an increase of at most 0.1% in the last two periods. Just as the income share held by the lowest 20 percent shows no significant change for five periods, so does the share of college graduates. Although longer school closures result in stronger impacts on cross-sectional inequalities, the magnitude is still insignificant. Compared to the steady state, the economy with the 1.5-year-closure generates differences in the absolute value of the Gini income coefficient near 0.1% until $t = 3$ and at most 0.2% in the last two periods. This little difference also appears in both the income share held by the lowest 20 percent and the share of college graduates. However, this finding does not necessarily imply that school closures have limited impacts on changes in inequality across generations, which we investigate next.

Intergenerational Implications Table 6 reports that the school closure shocks reduce intergenerational mobility quite substantially. Compared to the steady state, the 0.5-year-closure increases the IGE by 0.5 to 1.0% and the rank correlation by 0.4 to 0.9%, while decreasing the upward mobility by 0.9 to 1.6% across cohorts. These changes are amplified by the length of school closures. Across cohorts, the 1.5-year-closure generates increases in the IGE and rank correlation three times as large as the 0.5-year-closure. Likewise, the 1.5-year-closure reduces the upward mobility two and

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Table 6: Effects on intergenerational mobility of lifetime income

	IGE			Rank cor.			Upward Mobility		
Steady state	.375			.361			6.8%		
	% **change** rel. to no school closure, by cohort								
Closure length	C1	C2	C3	C1	C2	C3	C1	C2	C3
0.5 AY	0.5	0.8	1.0	0.4	0.7	0.9	-0.9	-1.3	-1.6
1.0 AY	1.0	1.6	2.0	0.9	1.5	1.8	-1.6	-2.6	-2.7
1.5 AY	1.5	2.5	3.0	1.3	2.2	2.8	-2.4	-4.0	-4.3

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a half to three times more than does the 0.5-year-closure.

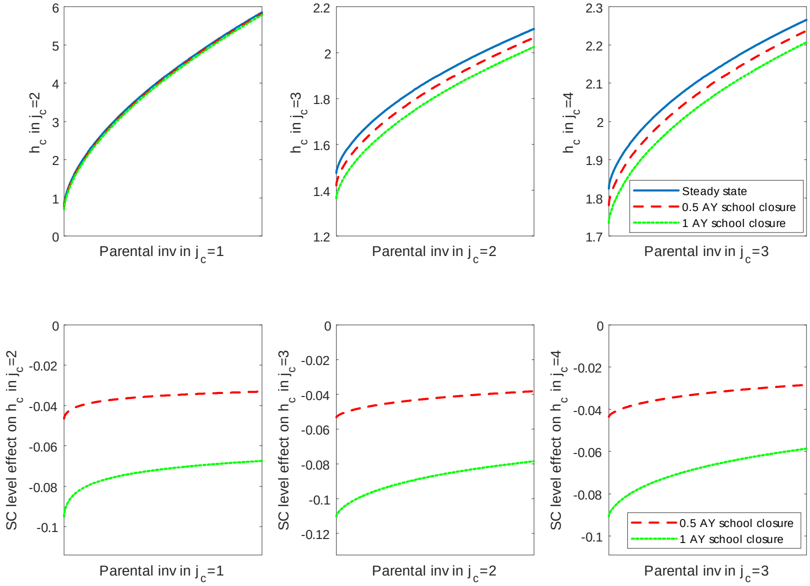
Note that the school closure effects on intergenerational mobility are quantitatively heterogeneous across cohorts: the older cohorts are, the more reduced intergenerational mobility is. While the 1-year-closure increases the IGE by 1% for C1, it does so by 2% for C3. The rank correlation also has similar differences across cohorts. In response to the 1-year school closure, the rank correlation increase for C3 is three times that for C1. Similarly, given a school closure, older cohorts suffer from a greater reduction in upward mobility. The 1-year-closure decreases C1's upward mobility rate by 1.6% but C3's by 2.7%. These patterns are preserved regardless of the length of school closures. Both the 0.5-year-closure and the 1.5-year-closure lead older cohorts to experience greater reductions in upward mobility and larger increases in IGE and rank correlation.

To understand these intergenerational implications, it is useful to first examine the *direct* effects of school closures on the human capital production function. For this purpose, Figure 4 plots the effects of changes in g_j on the level of human capital produced for an average child as a function of parental investments aggregated from time and money with the calibrated parameters. Note that because parental investments are largely shaped by income, the horizontal axis can be interpreted as the parental socioeconomic status (SES).

There are several noticeable features. First, longer school closures bring about greater reductions in child human capital. Second, within a cohort, parents with low SES experience greater reductions in child human capital. Since the portion of public investment g_j is greater for lower SES parents, they are more adversely affected by school closures. In addition, this parental SES gap tends to increase with the child's age. These features suggest that overall damages to child human capital increase with the length of school closures; children from parents with low SES experience larger damages than those with high SES parents and this difference is greater for older cohorts.

In addition to the direct effects of school closures, the other important mechanism is related to endogenous parental responses: parents have incentives to respond to this reduced child human capital following school closures by increasing their parental investments. Note that parental

Figure 4: Direct effects of school closures on the child human capital production function



Note: The figures plot the j_c-period human capital output for children with average human capital in j_c in stationary equilibrium and $\phi = 1$. The range of parental investments in the horizontal axis are a composite of parental investments when parental time and money vary from 0 to twice the mean values in stationary equilibrium. The upper panels show production levels with different school closure lengths. The bottom panels plot changes in level relative to the case without school closures.

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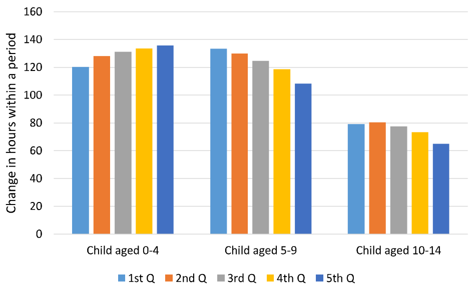
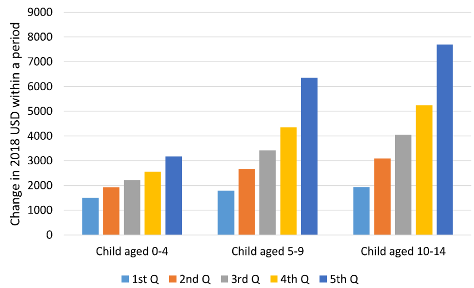
Figure 5: Parental responses by parental permanent income





Note: A set of five bars plots changes in parental investments by the quintile of parent's permanent income for each cohort, ordered by the child's age during the 1-year school closure. The left shows time investment responses and the right shows monetary investment responses.

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investment behaviors are different according to their children's age. As shown in Table 2, the importance of financial relative to time investments increases with children's age, in line with estimates by Del Boca et al. (2014). These calibration results imply that parental investments in time are more crucial in forming human capital in the very early childhood period (C1), but parental financial investments become more important in later periods (C2 and C3). In addition, the degree of complementarity between time and monetary investments is much stronger in C1 than in C2 and C3.

This age-dependent human capital production technology brings about differences in the composition of parental investments according to the child's age. Figure 5 presents the parental responses to the 1-year-closure by parental lifetime income (or permanent income). Although the average time investment response is smaller, the monetary one is larger for older children (C2 and C3). Note that when children are aged between 0 and 4, richer parents invest in time more than poorer parents do, but this gap is small because time constraints are more equally distributed across parents than budget ones. The richer parents cannot easily compensate financially for the lack of time investments, as monetary investments are not as effective as or easily substitutable for time investments for children in the early period. In the later periods, as financial investments become more important, richer parents substitute time between the two more than poor parents do. Further, note that financial investments can better substitute time investments for older children (due to lower elasticities of substitution) and that parents' income dispersion increases with age, which would show up as greater dispersions in financial investments for older children, as demonstrated in Figure 1. These jointly result in substantial positive income gradients in monetary investment responses for the older children cohorts (C2 and C3).

These heterogeneous parental investments play an important role in generating disparities in

Table 7: Effects on inequality and loss of lifetime income

	Lifetime income						Fraction of		
	Gini index			Average			College-educated		
Steady state	.293			4.15			.342		
	% **change** rel. to no school closure, by cohort								
Closure length	C1	C2	C3	C1	C2	C3	C1	C2	C3
0.5 AY	0.0	0.0	0.1	-0.4	-0.7	-0.6	-0.2	-0.4	-0.4
1.0 AY	0.1	0.1	0.2	-0.9	-1.3	-1.3	-0.3	-0.7	-0.8
1.5 AY	0.1	0.2	0.2	-1.3	-2.0	-2.0	-0.4	-1.0	-1.3

child human capital formations. Recall that, as demonstrated by Figure 4, all three cohorts experience a reduction in human capital due to the direct effects of school closures, but children with low-income parents are disproportionately affected. In addition, the heterogeneous parental investments discussed above amplify these differences. For the older cohorts (C2 and C3), larger differences in parental monetary investments lead to greater disparities in the changes of human capital across parental income groups, which in turn shapes intergenerational mobility. As a result, intergenerational mobility decreases more in the older cohorts (C2 and C3).

Next, we investigate how school closures influence the overall economic status (or absolute mobility) by cohort and the dispersion of lifetime income within cohorts. Table 7 reports the effects of school closures on the average and inequality of lifetime income. While these school closures have small impacts on lifetime income inequality, the average reveals substantial losses. Specifically, the 0.5-year-closure increases the lifetime income Gini coefficient by less than 0.1% over cohorts. The effect on lifetime inequality is still negligible for the longer school closures. The two longer school closures increase the Gini coefficient by 0.1 to 0.2%, while resulting in sizeable lifetime income losses. The 0.5-year-closure reduces average lifetime income by 0.4 to 0.7%; the magnitude increases with the length of school closures.

Note that school closures bring about more significant losses in lifetime income for older children (C2 and C3) than for the youngest cohort (C1). This is indeed not necessarily inconsistent with the literature highlighting the importance of early childhood in human capital formation (Heckman 2008). On impact, school closures have the strongest substantial impacts on child human capital for the youngest pre-school-aged cohort (C1), as shown in Figure 6. Although this might seem inconsistent with the above results on lifetime income loss, it becomes clearer when we recall that the school shocks are temporary. As can be seen in Figure 7, differences in losses in human capital for C1 become narrower over time, as the public investments play an equalizing role (Fernandez and Rogerson 1998). This is in line with the empirical evidence by Kuhfeld et al. (2020) who find

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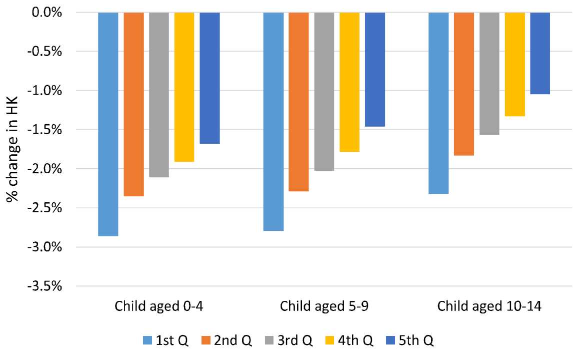
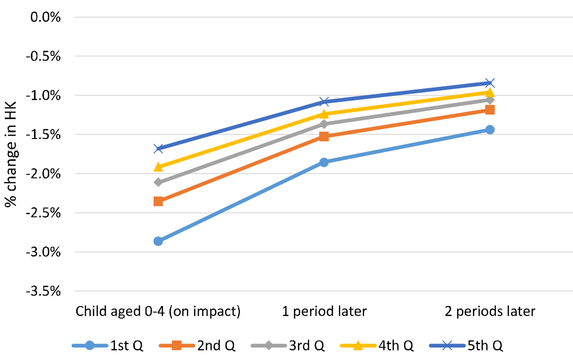
Figure 6: Child human capital in the next period by parental permanent income



Note: A set of five bars plots percent changes in the next period human capital on impact by the quintile of parent's permanent income for each cohort, ordered by the child's age during the 1-year school closure.

Figure 7: Effects of school closures on child human capital (initially aged 0-4) over time, by parental permanent income



Note: A model period corresponds to five years. This figure plots percent changes in the next period human capital (relative to the case without school closure shocks) of children who are affected by school closures when $j_c = 1$ (age 0-4).

that students who lose more ground during the summer of 2018 tend to experience steeper growth during the next school year. In fact, this narrowing gap in school closures' negative consequences is closely related to the direct effect of school closures on the human capital production function that is heterogeneous across parental SESs. Specifically, Figure 4 shows that, although child human capital with a lower parental SES is more adversely affected, corresponding marginal productivity of aggregate investments, measured by the slope of the below graph becomes greater. While these young children experience gains in the next coming years, children from lower parental SES benefits the most, thereby narrowing the gap over time. This results in differences in the changes of college attainment across cohorts. The fraction of college-educated is reduced more in C2 and C3 than in C1.

The role of parental skills and knowledge in virtual schooling Although schools have been struggling in the beginning, they gradually adapt to online teaching during the school closures induced by the COVID-19 pandemic (Kuhfeld et al. 2020). In principle, virtual schooling could mitigate the negative consequences of school closures on child learning. The empirical evidence tends to suggest potential positive income gradients in online learning (Bacher-Hicks et al. 2020). Although it can be explained by financial investment responses for the quality of home learning environment such as laptops and tablets (Andrew et al. 2020), it could also capture the direct effects of parental skills and knowledge, which could enhance their children's virtual teaching experience given the same financial investments.

To quantitatively explore how much this effect can be relevant to the school-closure effects we have studied, we consider an alternative scenario where college-educated parents are able to fully mitigate the school closures through virtual schooling. More precisely, college-educated parents do not experience the fall in public investments in their children's human capital production function when the school closure shock hits the economy. This exercise is designed to provide an upper bound of the effects of such skill-gradients in virtual schooling.

As expected, we find that average income losses are mitigated substantially. For example, the year-long closure reduces the average lifetime income by -0.6% for C1 and by -1.0% for C2 and C3. These are much weaker than the baseline results reported in Table 7 (-0.9% for C1 and -1.3% for C2 and C3). More importantly, we find that the model generates stronger impacts on intergenerational mobility, as compared to the baseline experiment (e.g., IGE increases by 1.4%, 2.2% and 2.4% for C1, C2, and C3, respectively, as compared to 1.0%, 1.6% and 2.0%, respectively). The effects of school closures on lifetime income inequality almost doubles but are still very small, raising a Gini coefficient by 0.1-0.3%. These results suggest that virtual schooling that disproportionately benefits children from more-educated parents could mitigate average income losses at the expense of lower intergenerational mobility.

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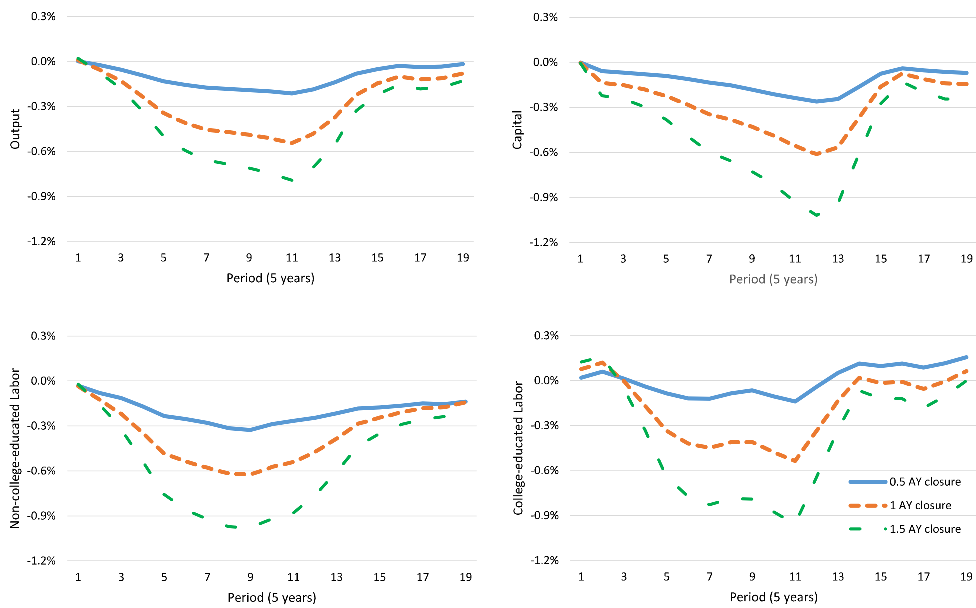
Figure 8: Evolution of macroeconomic aggregates with a lower elasticity of substitution between public and parental investments



4.3 The role of the elasticity of substitution between public and private investments

To examine the role of the elasticity of substitution between public and private investments, we consider an alternative model economy with a lower elasticity of substitution (1.5 or $\psi = 1/3$) than the baseline economy (3.0 or $\psi = 2/3$) and recalibrate the model to match the same set of target statistics presented in Table 2. We note that this elasticity of substitution, $1/(1-\psi)$, could reflect education systems that vary across countries. For example, it is likely that Scandinavian countries where public services play a major role in education would have a lower elasticity of substitution than East Asian countries, such as South Korea, where private education is prevalent and large in market size (Kim et al. 2020). Therefore, our analysis herein intends to provide useful considerations for different countries with different approaches to public and private education.

Figure 8 shows the aggregate level evolution of output, capital, efficiency units of labor for non-college and college graduates in the case with a lower elasticity of substitution. As shown in a comparison of Figure 2, although all these aggregate variables fall as in the case with a higher elasticity of substitution, the magnitudes are greater in the case with a lower elasticity of

Table 8: Effects on intergenerational mobility of lifetime income with a lower elasticity of substitution between public and parental investments

	IGE			Rank cor.			Upward Mobility		
Steady state		.372			.359			6.7%	
	% change rel. to no school closure, by cohort								
Closure length	C1	C2	C3	C1	C2	C3	C1	C2	C3
0.5 AY	0.3	0.4	0.4	0.3	0.4	0.4	-0.6	-0.5	-0.8
1.0 AY	0.7	0.9	0.9	0.6	0.7	0.9	-0.9	-0.9	-1.2
1.5 AY	1.2	1.4	1.4	0.9	1.2	1.3	-1.4	-1.7	-1.8

substitution. While the 1-year-closure decreases the aggregate output by up to 0.3% in the case with a higher elasticity of substitution (Figure 2), it does so by around 0.5% in the case with a lower elasticity of substitution. A decrease in aggregate capital in $t = 12$ in the case with a lower elasticity of substitution is double that of the baseline economy. Effective labor units for both non-college and college graduates also show two times greater reductions. These results suggest that school closures bring greater declines in aggregate variables for countries wherein public educational investment is difficult to substitute with private educational investment, such as Scandinavian countries.

Table 8 shows intergenerational mobility of lifetime income in the case with a lower elasticity of substitution between public and parental investments. As revealed by a comparison with Table 6, as the degree of complementarity increases (lower ψ), the effects of school closures become weaker on intergenerational mobility. In all cases with three different closure lengths, increases in the IGEs in the case with a lower elasticity of substitution is half as large as in the baseline model. Likewise, increases in the rank correlation in the case with a lower elasticity of substitution is less than those in the case with a higher elasticity of substitution. The upward mobility also displays similar patterns: the declines in the upward mobility rate in the case with a lower elasticity of substitution are smaller than those in the baseline model.

As demonstrated previously with Figures 5 and 6, for C2 and C3, the substitution of time into financial investments plays a role in increasing differences in the responses of child human capital to school closures across parental permanent income groups, thereby reducing intergenerational mobility. These differences in the case with a lower elasticity of substitution are smaller than those in the case with a higher elasticity of substitution because this lower elasticity weakens parents' incentive to compensate for school closures. As a result, Figure 9 shows that, on average, parental responses are substantially weaker in the model with a lower elasticity of substitution as compared to the baseline model. These more muted responses in parental investments result in smaller gaps in child human capital changes, as shown in a comparison between Figures 6 and 10. These findings

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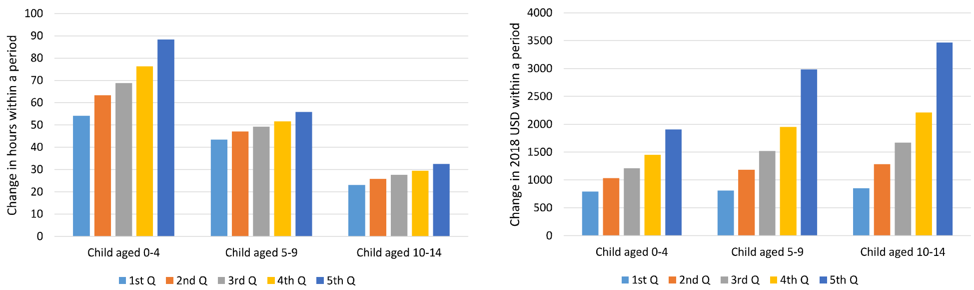
Figure 9: Parental responses by parental permanent income with a lower elasticity of substitution between public and parental investments



Note: A set of five bars plots changes in parental investments by the quintile of parent's permanent income for each cohort, ordered by the child's age during the 1-year school closure. The left shows time investment responses and the right shows monetary investment responses.

imply that in countries where public investments are crucial and irreplaceable, school closures could have smaller impacts on intergenerational mobility.

Finally, Table 9 shows the responses of the average and inequality of lifetime income to school closures in the case with a lower elasticity of substitution. As in the baseline model (Table 7), these school closures have little impact on lifetime inequality. However, school closures generally induce larger losses in lifetime income in this model. As mentioned previously, under a lower elasticity of substitution between public and private investments, it is difficult to compensate for the lack of public investments with parental financial investments, thus increasing overall loss of child human capital, as shown in the comparison between Figures 6 and 10. Therefore, this greater reduction in child human capital leads to a larger decrease in overall college attainment and a larger drop in average lifetime income. Another interesting difference from the baseline model is that the average lifetime income loss for C1 becomes disproportionately larger, mainly caused by the much more significant initial impact on human capital, as shown in Figure 10.

To summarize, a decrease in the elasticity of substitution between public and private investments leads to a larger reduction in the aggregate variables and average lifetime income but a lesser reduction in intergenerational mobility. These results are driven by reduced substitutions by parental financial investments, generating overall greater but less heterogeneous changes in child human capital across parental permanent income groups.

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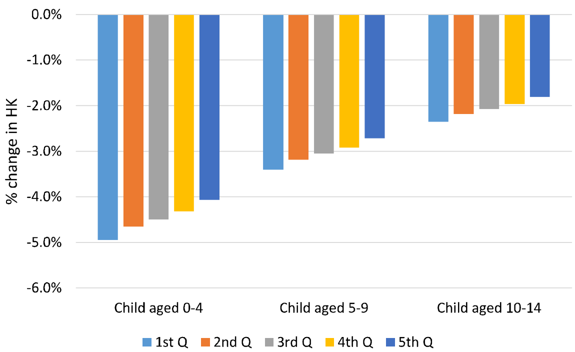
Figure 10: Child human capital by parental permanent income with a lower elasticity of substitution between public and parental investments



Note: A set of five bars plots percent changes in the next period human capital on impact by the quintile of parent's permanent income for each cohort, ordered by the child's age during the 1-year school closure.

Table 9: Effects on inequality and loss of lifetime income with a lower elasticity of substitution between public and parental investments

	Lifetime income Gini			Lifetime income Average			Fraction of College-educated		
Steady state		.291			4.15			.342	
				% change rel. to no school closure, by cohort					
Closure length	C1	C2	C3	C1	C2	C3	C1	C2	C3
0.5 AY	0.1	0.1	0.1	-0.9	-0.9	-0.8	-0.3	-0.3	-0.2
1.0 AY	0.1	0.1	0.1	-1.8	-1.9	-1.7	-0.9	-0.9	-0.7
1.5 AY	0.2	0.2	0.2	-2.8	-3.0	-2.7	-1.4	-1.5	-1.3

5 Conclusion

In this paper, we have investigated how school closures affect the aggregate economy, inequality, and intergenerational mobility through intergenerational human capital transmissions in the medium and long term. Using a dynastic overlapping generations general equilibrium model wherein altruistic parents invest in the children's human capital, which complements public schooling, we have found three main results. First, school closures bring about moderate yet long-lasting adverse effects on the aggregate economy. General equilibrium effects play a substantial role in reshaping aggregate variables' dynamics. Second, school closures reduce the average lifetime income and intergenerational mobility of directly affected children, and these reductions are more severe for older children cohorts. These results are driven mainly by parental investment responses that differ by a child's age and parental income. Finally, we have shown that substitutability between public and private investment shapes school closure costs in a non-trivial way. While a lower elasticity of substitution induces more significant damages in the aggregate economy and overall lifetime incomes of the affected children, it mitigates a reduction in intergenerational mobility. The key underlying mechanism for both changes is the dampened parental motives to compensate for the lack of public investments in the presence of lower substitution possibility.

Given these clear, interesting differences driven by substitutability between public and parental investments, we believe that school closure shocks could provide ideal opportunities to estimate the elasticity of substitution between public and private investments, which could vary across countries. The availability of more data in the near future would make it possible to perform such analysis and contribute to the literature in which there is limited empirical evidence for this substitution elasticity that we have shown to matter quantitatively not only for child human capital formations but also for aggregate dynamics. Likewise, our model framework would be useful for studying unexplored interesting research topics as data become more available and more accessible. For example, an interesting normative question is how to optimally make up for losses from school closures dynamically by adjusting the length of school operations in the near future. This more short-run oriented question would require the model to have a high frequency such as 6 months, but probably not additional forces such as general equilibrium, which could balance the computational burden. We leave these interesting related questions for future work.

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A Appendix

A.1 Additional model environments

The main text includes the decision problems of young households – the core of the model economy. We now explain the remaining model environments.

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A.1.1 Households' Problem in Model Ages 7-11

In periods $j = 7$ and onwards, the state variables do not include h_c and ϕ because there is no need to keep track of these after the child leaves the original household. Until they retire in $j = 10$, households make consumption-savings and labor supply decisions. Hence, the household's problems in $j = 7, 8, 9$ are standard:

$$V_j(h_t, a_t, \kappa; \boldsymbol{\pi}_t) = \max_{\substack{c_t \geq 0;\ a_{t+1} \geq \underline{a} \\ n_t \in [0,1]}} \left\{ \frac{c_t^{1-\sigma}}{1-\sigma} - b\frac{n_t^{1+\chi}}{1+\chi} + \beta \mathbb{E}_{z_{t+1}} V_{j+1}(h_{t+1}, a_{t+1}, \kappa; \boldsymbol{\pi}_{t+1}) \right\}, \quad \text{if } j = 7, 8, 9 \tag{16}$$

$$\text{subject to} \quad c_t + a_{t+1} \leq \lambda_j \left(w_{\kappa,t}(\boldsymbol{\pi}_t) h_t n_t / \bar{y} \right)^{-\tau_j} w_{\kappa,t}(\boldsymbol{\pi}_t) h_t n_t + (1 + r_t(\boldsymbol{\pi}_t))\, a_t - \tau_k r_t(\boldsymbol{\pi}_t) \max\{a_t, 0\} + T$$
$$h_{t+1} = \exp(z_{t+1}) \gamma_{j,\kappa} h_t$$
$$\boldsymbol{\pi}_{t+1} = \Gamma(\boldsymbol{\pi}_t).$$

After retirement, households receive social security pension benefits Ω_t. The value functions in the retirement periods ($j = 10, 11$) are given by:

$$V_j(h_t, a_t, \kappa; \boldsymbol{\pi}_t) = \max_{c_t \geq 0;\ a_{t+1} \geq \underline{a}} \left\{ \frac{c_t^{1-\sigma}}{1-\sigma} + \beta V_{j+1}(h_t, a_{t+1}, \kappa; \boldsymbol{\pi}_{t+1}) \right\} \tag{17}$$

$$\text{subject to} \quad c_t + a_{t+1} \leq (1 + r_t(\boldsymbol{\pi}_t))\, a_t - \tau_k r_t(\boldsymbol{\pi}_t) \max\{a_t, 0\} + T_t + \Omega_t$$
$$\boldsymbol{\pi}_{t+1} = \Gamma(\boldsymbol{\pi}_t),$$

and $V_{j=12}(\cdot) = 0$.

A.1.2 Firm's Problem and Government

There is a representative firm that produces output with technology featuring constant returns to scale and nested CES specifications. Specifically, we assume that output is given by the Cobb-Douglas function:

$$Y_t = K_t^{\alpha} H_t^{1-\alpha}, \tag{18}$$

where K_t is the aggregate capital stock, H_t is the aggregate labor input, and $\alpha \in (0, 1)$. The aggregate labor input H is then aggregated under the CES technology following:

$$H_t = \left[\nu H_{1,t}^{\rho} + (1 - \nu) H_{2,t}^{\rho} \right]^{\frac{1}{\rho}}, \tag{19}$$

where $\rho < 1$ shapes the elasticity of substitution $(1/(1-\rho))$ between skilled workers H_2 and unskilled workers H_1.

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Given the above production technology, the representative firm in competitive markets maximizes profits. One can easily show that the optimality conditions are given by:

$$\alpha K_t^{\alpha-1} H_t^{1-\alpha} = r + \delta \tag{20}$$

$$(1-\alpha)K_t^{\alpha} H_t^{-\alpha}\frac{1}{\rho}\left[\nu H_{1,t}^{\rho} + (1-\nu)H_{2,t}^{\rho}\right]^{\frac{1}{\rho}-1} \nu\rho H_{1,t}^{\rho-1} = w_{1,t} \tag{21}$$

$$(1-\alpha)K_t^{\alpha} H_t^{-\alpha}\frac{1}{\rho}\left[\nu H_{1,t}^{\rho} + (1-\nu)H_{2,t}^{\rho}\right]^{\frac{1}{\rho}-1} (1-\nu)\rho H_{2,t}^{\rho-1} = w_{2,t}, \tag{22}$$

where δ is the capital depreciation rate.

The government collects taxes from households through (progressive) labor income taxation and capital income taxation. These tax revenues are spent on four categories: (i) social security pension Ω to retirees; (ii) lump-sum transfers T to all households, (iii) public education expenditures $\{g_j\}_{j=3}^{5}$; and (iv) government spending $G \geq 0$ that is not valued by households. We assume that the government balances its budget each period j.

A.1.3 Equilibrium

Let us denote by $x_{j,t} \in X_j$ a vector of individual state variables at age j in period t in the household's recursive problems described in the previous subsection. Given an initial distribution $\boldsymbol{\pi}_{-T} \equiv (\pi_{j,-T})_{j=1}^{11}$, a competitive general equilibrium is a sequence of factor prices $\{w_{1,t}(\boldsymbol{\pi}_t), w_{2,t}(\boldsymbol{\pi}_t), r_t(\boldsymbol{\pi}_t)\}_{t=-T}^{\infty}$, the household's decision rules, value functions $\left\{\{V_j(x_{j,t}, \boldsymbol{\pi}_t)\}_{j=1}^{11}\right\}_{t=-T}^{\infty}$, government policies including $\{(g_{j,t})_{j=3}^{5}\}_{t=-T}^{\infty}$, and distributions $\{(\pi_{j,t}(\cdot))_{j=1}^{11}\}_{t=-T}^{\infty}$ over $x_{j,t}$ such that:

1. given the government policies and factor prices, household decision rules solve the associated household's life cycle problems in the previous subsection, and $V_j(x_{j,t}, \boldsymbol{\pi}_t)$ are the associated value functions;

2. factor prices are competitively determined according to (20), (21), and (22);

3. market clears;

$$K_t = \sum_{j=1}^{11} \int a_{j,t} d\pi_{j,t}(x_{j,t})$$

$$H_{s,t} = \sum_{j=1}^{11} \int h_{j,t} n_{j,t}(x_{j,t}, \boldsymbol{\pi}_t) d\pi_{j,t}(x_{j,t}|\kappa = s), s = 1, 2;$$

4. the government budget is balanced for each period: the sum of transfers payments, social security pension payments, public education spending, and government spending is equal to the sum of labor income tax revenues and capital income tax revenues for each period;

Table A1: Education gradients in parental time investments

	$j = 3$	$j = 4$	$j = 5$
College-educated	1.342	.561	.416
	(.133)	(.109)	(.091)
Sex	-2.62	-1.51	-1.20
	(.123)	(.101)	(.083)
Age	-.041	.016	.023
	(.009)	(.007)	(.006)
Married	-.911	-.318	-.102
	(.085)	(.064)	(.053)
R^2	.023	.014	.017
Average x	6.43	3.78	2.06

Notes: Numbers in parentheses are standard errors. The dependent variable is parental time investments (weekly hours). These estimates are from Yum (2020).

5. the evolution of the distribution $\boldsymbol{\pi}_t$ is given by $\boldsymbol{\pi}_{t+1} = \Gamma(\boldsymbol{\pi}_t)$, which is consistent with the household optimal choices and the exogenous probability distributions.

Note that this competitive equilibrium nests its stationary version of equilibrium where market-clearing prices and aggregate quantities are constant over time.

A.2 Calibration details

Most calibration targets are based on samples from the 2003-2017 waves of the ATUS, combined with the Current Population Survey (Yum 2020). Table $A1$ reports the estimation results that are used to compute the educational gradients in parental time investments. The sample is restricted to households who have any number of children and aged between 21 and 55 (inclusive), as in Guryan et al. (2008). The three periods in the model ($j = 3, 4, 5$) correspond to the youngest children's age bands: ages 0-4, ages 5-9, and ages 10-14, respectively. The coefficient on the dummy college variable, divided by the corresponding average, captures the educational gradient while controlling for parents' sex, age, and marital status. We note that the college coefficients are quite stable regardless of control variables, in line with the evidence in Guryan et al. (2008).

Table $A2$ reports the gross growth rates of human capital by age and education. These are computed based on the estimates from the PSID samples in Rupert and Zanella (2015).

Table $A3$ reports the estimates of τ_j and λ_j in labor taxation by age, obtained from Holter et al. (2019). We use the estimates for single households for $j = 1, 2$, and the estimates for married households for the later periods (either with a child for $j = 3, ..., 6$ or without children for $j = 7, 8, 9$). Table $A3$ also reports the estimates of g_j. The public and private education investments are based on the 2016 information in the 2019 Education at a Glance by the OECD. We consider

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Table A2: Gross growth rates of human capital by age and education

$j =$	1	2	3	4	5	6	7	8
$\gamma_{j,1}$	1.231	1.052	1.017	1.004	0.998	0.995	0.994	0.994
$\gamma_{j,2}$	1.317	1.152	1.101	1.063	1.032	1.004	0.975	0.942

Notes: The reported values are based on the estimates from the PSID samples in Rupert and Zanella (2015).

Table A3: Parameter values for progressive taxation and public education investments

	τ_j	λ_j		g_j
$j = 1, 2$	.1106	.8177	$j = 3$	0.060
$j = 3, ..., 6$	.1585	.9408	$j = 4$	0.098
$j = 7, 8, 9$	.1080	.8740	$j = 5$	0.111

Notes: τ_j and λ_j are based on the estimates in Holter et al. (2019). Public education investments g_j are based on 2019 Education at a Glance (OECD).

pre-primary as $j = 3$, primary as $j = 4$, and secondary as $j = 5$ in the model. We treat state and federal government spending as public investments while local government spending is included in the private investments (Restuccia and Urrutia 2004; Holter 2015). See Yum (2020) for more details. Although this method does not exploit micro-level data directly, it is reassuring that these estimates are generally in line with those by Lee and Seshadri (2019) who use a micro data set (PSID-CDS).

A.3 Partial (stochastic) closures

We also consider additional experiments based on partial school closures. Specifically, we assume that school closures are still unexpected but there is another dimension of uncertainty: half of the agents still experience full closures, but the other half experience a school closure of limited intensity. This within-period variation could capture additional closures due to local outbreaks of COVID-19 cases even after re-opening nationwide. This could also capture the variability of effectiveness of online substitute teaching by schools. The results reported below are based on a partial intensity of 50%. Our findings suggest that the main findings are generalizable in terms of the relationship between average school closure length and the corresponding aggregate effects. But they also suggest that partial closures induce additional variations that happen within each cohort.

A.4 Additional figures and tables

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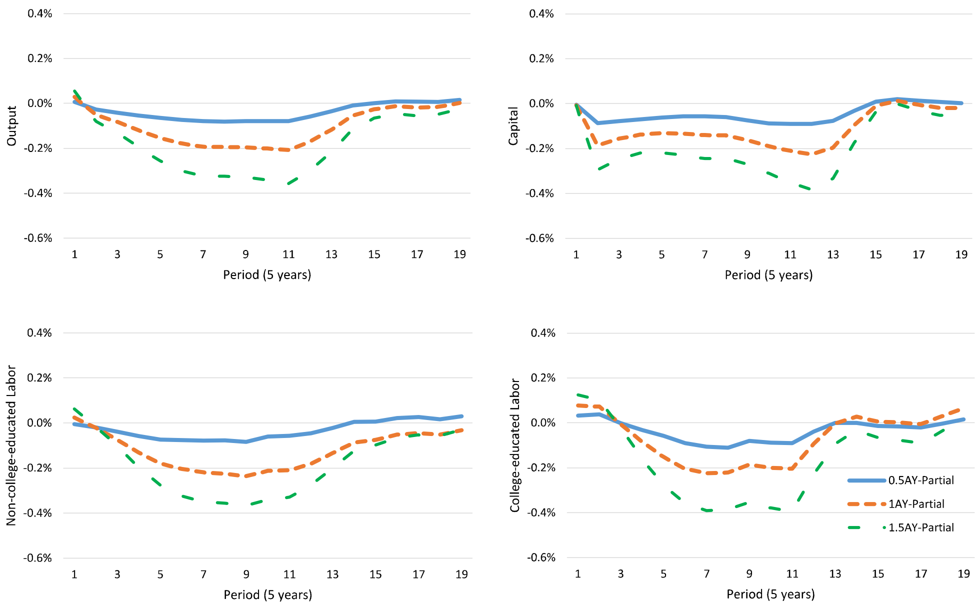
Figure A1: Evolution of macroeconomic aggregates: Partial closures



Note: A half of agents experience full closures whereas the other agents experience partial closures, the intensity of which is given by 50%.

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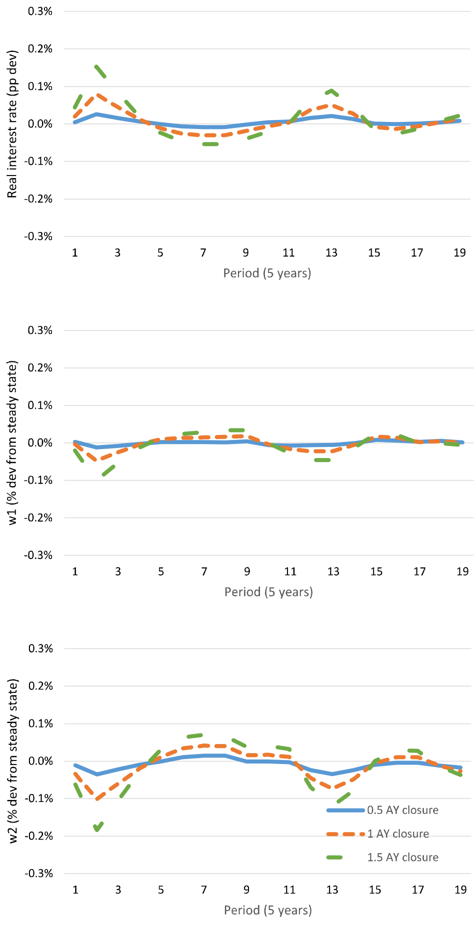
Figure A2: Evolution of equilibrium prices in the baseline model



Note: The top panel shows the equilibrium interests over the transition. The middle panel shows the equilibrium wages for non-college workers, and the bottom panel shows the equilibrium wages for college-educated workers over the transition

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Table A4: Distributional changes over time: Partial closures

		Time (1 period: 5 years)				
		1	2	3	4	5
	Steady state	**% change** rel. to no school closure				
Closure length: 0.5 AY						
Gini income	.341	0.0	0.0	0.0	0.1	0.0
Bottom 20% inc (%)	7.73	-0.0	-0.0	-0.0	-0.0	-0.0
Share of college (%)	34.2	0.0	0.0	0.0	-0.0	-0.0
Closure length: 1 AY						
Gini income	.341	0.0	0.0	0.1	0.1	0.1
Bottom 20% inc (%)	7.73	-0.0	0.0	-0.1	-0.1	-0.0
Share of college (%)	34.2	0.0	0.1	0.0	-0.0	-0.1
Closure length: 1.5 AY						
Gini income	.341	0.0	-0.0	0.1	0.2	0.1
Bottom 20% inc (%)	7.73	-0.0	0.0	-0.1	-0.1	-0.0
Share of college (%)	34.2	0.0	0.1	-0.0	-0.1	-0.1

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Table A5: Effects on intergenerational mobility of lifetime income: Partial closures

	IGE			Rank cor.			Upward Mobility		
Steady state	.375			.361			6.8%		
Closure length	**% change** rel. to no school closure, by cohort								
	C1	C2	C3	C1	C2	C3	C1	C2	C3
	All children								
0.5 AY	0.4	0.6	0.7	0.3	0.6	0.7	-0.7	-1.2	-1.0
1.0 AY	0.7	1.2	1.5	0.6	1.1	1.4	-1.2	-2.2	-2.2
1.5 AY	1.1	1.8	2.2	1.0	1.7	2.1	-1.8	-3.1	-3.3
	Children who experienced full closure								
0.5 AY	0.5	0.8	1.0	0.4	0.7	0.9	-0.7	-1.5	-1.0
1.0 AY	1.0	1.6	2.0	0.9	1.5	1.8	-1.5	-2.8	-2.5
1.5 AY	1.5	2.4	3.0	1.3	2.2	2.8	-2.4	-4.3	-4.1
	Children who experienced 50% closure								
0.5 AY	0.2	0.4	0.5	0.2	0.4	0.6	-0.4	-0.9	-0.9
1.0 AY	0.5	0.8	1.0	0.4	0.7	0.9	-0.9	-1.5	-1.9
1.5 AY	0.7	1.2	1.5	0.6	1.1	1.3	-1.2	-2.0	-2.3

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Table A6: Effects on inequality and loss of lifetime income: Partial closures

	Lifetime income						Fraction of		
	Gini			Average			College-educated		
Steady state	.293			4.15			.342		
Closure length	**% change** rel. to no school closure, by cohort								
	C1	C2	C3	C1	C2	C3	C1	C2	C3
				All children					
0.5 AY	0.0	0.0	0.1	-0.3	-0.5	-0.5	-0.2	-0.3	-0.3
1.0 AY	0.1	0.1	0.1	-0.7	-1.0	-1.0	-0.2	-0.5	-0.6
1.5 AY	0.1	0.1	0.2	-1.0	-1.5	-1.5	-0.3	-0.8	-0.9
				Children who experienced full closure					
0.5 AY	0.1	0.2	0.2	-0.4	-0.7	-0.6	-0.3	-0.5	-0.5
1.0 AY	0.1	0.1	0.1	-0.9	-1.3	-1.3	-0.5	-0.8	-1.0
1.5 AY	0.1	0.2	0.2	-1.3	-2.0	-2.0	-0.7	-1.3	-1.5
				Children who experienced 50% closure					
0.5 AY	0.1	0.1	0.1	-0.2	-0.3	-0.3	0.0	-0.1	-0.1
1.0 AY	0.1	0.1	0.1	-0.4	-0.6	-0.6	0.1	-0.1	-0.2
1.5 AY	0.1	0.1	0.1	-0.6	-1.0	-1.0	0.1	-0.3	-0.1

Table A7: Internally calibrated parameters and target statistics for the alternative model economy with a lower elasticity of substitution between public and parental investments

Parameter		Target statistics	Data	Model
β	.972	Equilibrium real interest rate (annualized)	0.04	0.04
b	23.4	Mean hours of work in $j = 3, ..., 9$	.287	.287
η	.322	Mean inter-vivos transfers/GDP per-capita	.056	.056
θ_3^x	.895	Mean parental time investments in $j = 3$	.061	.061
θ_4^x	.374	Mean parental time investments in $j = 4$	.036	.036
θ_5^x	.168	Mean parental time investments in $j = 5$	.020	.020
θ_3^I	.623	Mean parental monetary investments in $j = 3$	.098	.098
θ_4^I	.348	Mean parental monetary investments in $j = 4$	.113	.112
θ_5^I	.224	Mean parental monetary investments in $j = 5$	.128	.127
ζ_3	-3.55	Educational gradients in parental time in $j = 3$ (%)	20.9	20.3
ζ_4	-0.73	Educational gradients in parental time in $j = 4$ (%)	14.8	14.8
ζ_5	-0.74	Educational gradients in parental time in $j = 5$ (%)	20.2	20.1
ν	.530	Fraction with a college degree (%)	34.2	34.2
μ_ξ	.245	Average college expenses/GDP per-capita	.140	.140
δ_ξ	.732	Observed college wage gap (%)	75.0	84.9
ρ_ϕ	.141	Intergenerational corr of percentile-rank income	.341	.338
σ_ϕ	.590	Gini wage	.370	.370
σ_z	.141	Slope of variance of log wage from $j = 2$ to $j = 8$	.180	.181

Table A8: Distributional changes over time with a lower elasticity of substitution between public and parental investments

$\xi = 1/3$		Time (1 period: 5 years)				
		1	2	3	4	5
	Steady state	% **change** rel. to no school closure				
Closure length: 0.5 AY						
Gini income	.340	0.0	0.0	0.1	0.1	0.1
Bottom 20% inc (%)	7.73	-0.0	-0.0	-0.1	-0.1	-0.0
Share of college (%)	34.2	0.1	0.1	0.1	0.1	0.1
Closure length: 1 AY						
Gini income	.340	0.0	-0.0	0.1	0.2	0.2
Bottom 20% inc (%)	7.73	-0.0	0.0	-0.1	-0.1	-0.1
Share of college (%)	34.2	0.1	0.2	0.1	0.1	0.0
Closure length: 1.5 AY						
Gini income	.340	0.0	-0.1	0.2	0.3	0.3
Bottom 20% inc (%)	7.73	-0.0	0.0	-0.2	-0.2	-0.1
Share of college (%)	34.2	0.1	0.2	0.1	-0.0	-0.1

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CEPR PRESS

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Polarized politics of pandemic response and the Covid-19 connectedness across the US states

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Date submitted: 8 November 2020; Date accepted: 8 November 2020

This paper investigates the state-level differences in government and community responses to the Covid-19 pandemic, leading to different growth trajectories of Covid-19 cases and their connectedness across the U.S. states. Our regression analysis shows that higher growth trajectories are observed in the states that implemented the lax government and community response to the pandemic. Moving to the analysis of spillovers/connectedness of Covid-19 cases across the states, we apply the Diebold-Yilmaz connectedness methodology to the growth rates of Covid-19 cases. Using the total directional connectedness measures, we find that the states with lax government and community response generated connectedness of Covid-19 cases to others. These findings are also supported by the secondary regression analysis of pairwise connectedness measures over time. Finally, the travel intensity between the pairs of states, indirectly measured by the data on smartphone location exposure, contributes significantly to the pairwise directional connectedness of Covid-19 across the states.

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1 Introduction

Covid-19 pandemic has shaken the roots of the modern welfare state and society in several weeks. Governments worldwide were caught unprepared to contain the spread of a new virus that quickly turned into a pandemic; it infected more than 47 million and killed more than 1.2 million people around the world as of November 3.

The government response to Covid-19 has been the most critical element that has shaped how the pandemic evolved in various parts of the world. Once the virus struck, the policies to contain it ranged from recommending strict social distancing to closures of daycares, schools, and public gatherings and to effectively locking down entire regions and sometimes the whole nations for a specified period.

The trade-off between strict public health measures to contain the virus and the continuation of economic activity has forced policymakers to strike a balance between the two, which has proven to be rather tricky. Countries with efficient health systems such as Germany could spare their citizens from a complete lockdown. Others, such as Italy and Spain, whose health systems were not as efficient, implemented regional and national lockdown measures.

The situation in the United States, on the other hand, was quite different. The existence of an effective healthcare system and warnings received in advance provided ample time to respond to the pandemic at an early stage. However, the U.S. federal government was slow, disorganized, and even reluctant to stage an effective response to the pandemic. Consequently, it received widespread criticisms from domestic and international organizations and experts for the lack of federal leadership and assistance to states, delays in testing, and misleading public statements.

The U.S. federal government's response to criticisms politicized the whole process. While many coastal states that got severely hit in March responded immediately to the pandemic, some others have just followed the federal government, downplaying the seriousness of the public health threat posed by the virus. Sharp differences of opinion and policy response competed and survived in the last six months. They led to the formation of demarcation lines across the party lines in a year of presidential elections.

In this paper, we focus on both the government and community response to the pandemic at the state-level and how critical they have been for the wide variation in the new coronavirus case trajectories.[1] The availability of daily big data sets made it possible to link the differences in Covid-19 case trajectories and the transmission of

[1] In general, the literature has focused on government policy response (school closures, cancellation of public meetings, stay-at-home orders, etc.) as the most critical element of containment efforts (see, for example, Alexander and Karger (2020)). However, recently, Goolsbee and Syverson (2020) shows that individuals' voluntary decisions to disengage from commerce played a significant role in the decline in economic activity.

infections across states to government and community response at the state level.

In the literature, many studies interpret the transmission dynamics of the Covid-19 outbreak maintaining certain assumptions of different structural models. In many cases, these models do not elicit convincing empirical evidence on the transmission of the virus; they usually concentrate on the partial impact of different responses to the course of the pandemic. On the other hand, connectedness analysis, which has been widely used in empirical financial and economic network studies, offers a rather global approach to approximate the linkages among the variables using multivariate time series techniques. It is comparatively useful when the structural models are limitedly available to constitute a complete approach to the underlying linkages. Therefore, we choose to study the regional connectedness in the United States because there has been substantial variation in the public health policy measures across the states from the beginning onwards. The absence of leadership from the federal government in spearheading a joint policy drive and coordination resulted in relatively high rates of increases in many states, which in time took an irregularly-shaped distribution of total and new cases across the states.

The connectedness analysis of new cases' growth provides further empirical evidence on the dissociation between the pandemic's course in states along the party lines. At the next step, secondary regressions are used to investigate the potential determinants of Covid-19 connectedness across states. Secondary regressions reveal the pairwise impact of the community mobility, i.e., the use of public or private spaces, state government policy stringency, and the travel intensity between states on the connectedness of Covid-19 across states. Even more importantly, the secondary regression analysis shows that the governor's party affiliation can be a contributing factor to the Covid-19 connectedness to other states.

In the next section, we briefly review the literature and pose the contributions of this paper to the literature on Covid-19. In section 3, we introduce the data and briefly summarize how the pandemic progress at the national level. We also show that governors' party affiliation also contributes to the state-level government response and the resulting differences in new cases' growth rates. Section 4 analyzes the dynamic system-wide and directional connectedness measures that will help us understand the transmission of new infections across states over time. Section 5 analyzes factors that contributed to the pairwise connectedness of Covid-19 across states over time. Section 6 concludes the paper.

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2 Literature Review

Since March, economists around the world turned their attention to the study of Covid-19 and its economic impact.[2] The broad consensus on the strong link between the course of the epidemic and its potential economic impact has prompted economists to visit epidemiological models, the Susceptible-Infected-Recovered (SIR) model of Kermack and McKendrick (1927), in particular.

Eichenbaum et al. (2020) extend the SIR model by incorporating the aggregate demand and supply and show how containment policies can save many lives. Acemoglu et al. (2020) incorporate the SIR model with an age structure and show that the economic impact of the pandemic can be taken under control by restricting the 65+ years old from participating in social and economic activities. Among many other studies using the SIR model, Berger et al. (2020) and Wangping et al. (2020) focus on the transmission dynamics and derive policy recommendations. Gapen et al. (2020) focuses on the estimates of epidemiological model parameters to assess the effectiveness of different measures, including increased testing, masking requirements, and others, to slow down the transmission of Covid-19 in the United States. Finally, Favero (2020) applies the SIR model to the Italian case and shows that the strict application of the nationwide lockdown led the reproduction number of the virus (R0) to drop from 2.2 to 0.95.

A majority of studies in the recent pandemic literature focus on the univariate intensity of the Covid-19 epidemic (such as cases and/or deaths) at the national/regional/municipal level. In this study, we introduce a multivariate framework that measures nationwide comovement of coronavirus spread. This approach is essential as the government's objective is to flatten the "total infected curve"; however, a proper policy action should also curb the regional spillovers. Regional spillovers have become more evident in the increase of Covid-19 cases in many U.S. states since mid-June in response to the relaxation of the containment measures.

We also build on the literature that investigates the effectiveness of policy responses to Covid-19. Several studies link how flattening the pandemic curve is related to different measures taken. Deb et al. (2020) relied on the "stringency index" to explain the number of confirmed cases and illustrate the significance of the timing of the containment measures. Furthermore, Alvarez et al. (2020) inspects the effectiveness of policy actions and conclude that they are likely to be successful if they were executed earlier.

Recent studies provide evidence on the critical importance of regional characteristics

[2] See Anyfantaki et al. (2020) and Brodeur et al. (2020) for well-structured literature reviews.

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for the transmission of the virus. Bluhm and Pinkovskiy (2020) studies the regional differences in contagion and show that there is a sizeable discontinuity in Covid-19 cases at the the Inner German border between the East and West Germany. Furthermore, both the transmission of the virus and the community mobility vary substantially across different pairs of regions. Lattanzio and Palumbo (2020) compares the London and Lombardy regions to present whether different scenarios covering various mobility levels and dates of re-opening influence the observed and unobserved numbers of the infected. The distance among the two regions can also be a factor, as Valsecchi (2020) argues that diffusion of the Covid-19 is exploded by the internal migrants who traveled to their home towns across different regions in Italy.

Kuchler et al. (2020) focuses on the Lodi region and demonstrates how social networks correlates with the geographic spread in the region. In another contribution, Eckardt et al. (2020) test for treatment effects of border control in the Schengen Area and find that the border controls had a significant impact of limiting the pandemic. To the best of our knowledge, there was no such control in the U.S. across the borders of individual states.

There is also a growing literature on how political party affiliation and partisanship affects the individuals' responses to the pandemic. As those have been recently witnessed in many countries, the bitter race among the political actors in a highly-polarized domain put up a further challenge to applying sound governance principles to fight against the Covid-19 pandemic. Evidently, the ideological positioning of political parties, leaders, and their followers has a remarkable impact on the public attitude and belief on the course of the pandemic and the perception of the Covid-19 risk. Gadarian et al. (2020) find that the political difference based on partisanship is the only consistent factor that differentiates American people's political attitude towards the Covid-19 pandemic. Baker et al. (2020) review many poll outcomes from different sources and claim that Republican voters tend to be less concerned about the impact of the growing pandemic in the United States. Arguing that the demographic reasons behind the difference in the people's beliefs about the COVID pandemic are not evident, they also analyze the impact of heterogeneity of political party affiliations and other demographic factors on the consumer behavior during the Covid-19 pandemic. Their results indicate a marginal drop in community mobility in Republican states; people who live in Republican states have spent more time in restaurants and retail markets, while people in Democratic states cut their public transportation spending drastically.

Painter and Qiu (2020) argues that the differences in political beliefs limit the effectiveness of social distancing policies. Using geolocation data, they show that residents in Democratic states are more likely to follow the social distancing orders. Similarly, using the Governor's Twitter communications, publicly available Google

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search and location data of individuals, Grossman et al. (2020) finds that political partisanship affects the voluntary engagement to the social distancing recommendations. Ajzenman et al. (2020) explores the effect of far-right President Jair Bolsonaro's speeches and comments on the pandemic to the social distancing behavior of individuals in Brazil. They find that those communications played a crucial role in decreasing the social distancing behavior in pro-Bolsonaro municipalities. On the other hand, Calvillo et al. (2020) argues the partisan media coverage on Covid-19 mediates the ideology and the perception of Covid-19 risk.

The closest paper to ours in the literature is Rothert et al. (2020), which analyzes the effect of the state governments' fragmented mitigation policies on the diffusion of Covid-19 cases across counties and states of the U.S. They use both spatial and time series econometric methods to show that the implementation of scattered lockdown policies contributed to the diffusion of the virus across counties and states. This paper differs from Rothert et al. (2020) in that we are using time series and panel estimations to analyze the differences in terms of the rate of increase in new infections across states. The use of time series and panel estimation framework allows us to analyze how the dynamics of Covid-19 cases and their connectedness across states have changed over time.

Despite the earlier work using similar data on government response and the social distancing behavior of individuals, our paper stands out from others. It shows that the Covid-19 cases spilled over from states that implemented lax policies, and the community's attitude towards the pandemic was less accommodative. Furthermore, this paper is the first to identify the role of the governors' political party affiliation in the implementation of public health policy measures against the pandemic.

3 State-level Government and Community Response

This section introduces the dataset and presents a further univariate analysis of how the growth of the new confirmed cases of Covid-19 progressed at the state-level and whether this progress had anything to do with the different responses to the pandemic.

3.1 Data

Our primary data of interest is the daily number of confirmed new cases of Covid-19, which is accessible at the Github page of *Johns Hopkins University Center for Systems Science and Engineering*. We are aware of the limitations of the data on daily confirmed new cases; due to reasons such as limited testing opportunities, the rate of asymptomatic people, political stance on reporting, the number of daily confirmed new cases is likely to be different from the number of daily actual new cases or infections. The difference was perhaps even higher earlier in the pandemic when the daily number of tests conducted in the U.S. was very low (68,682 on March 21 and 127,428 on March 31) compared to the average daily number of tests conducted since the end of July (which is approximately 800,000).

The further irregularities in reporting such as the weekend effect, the political preference on the reporting day, different conventions on revisions and corrections, make daily new cases fluctuate significantly from one day to the other. In the literature, reporting, and analyzing the moving average of daily new cases to deal with irregular fluctuations, has been a standard treatment since the Covid-19 outbreak. The daily growth rates also contain the impairments in the reported data, Figure 1 illustrates the time-series of observed average growth rates of new cases across 51 states of the U.S. and the 7-day moving averages, starting from April 1 when the total cases hit a hundred in all states. The difference between the two series is apparent. While observed average new cases growth rate across states demonstrates huge swings due to irregularities in the reported data of new cases, 7-day moving average induces a more smooth and stable pattern.

Moving average is plausibly useful to surmount the short-term distortions in the data removing the irregularities generated by the reporting process; on the flip side, it averages out valuable short-term information in the dataset. However, it is still convenient to analyze the course of the Covid-19 pandemic capturing the emerging trends in the new cases. To summarize, we are working with the average daily growth rate of the 7-day average of the new confirmed cases in the multivariate analysis.

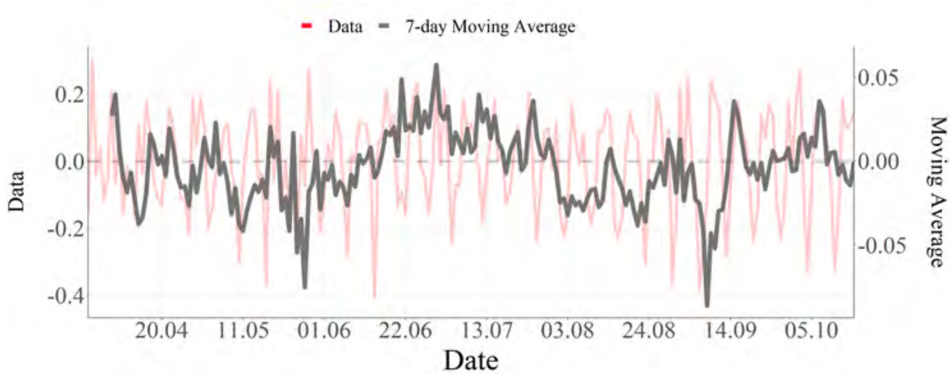

Figure 1: New Cases Growth – Observed Data vs. Moving Average (MA)

Along with the 7-day average growth rate of the daily new cases of Covid-19, we use the government policy stringency index (GSI) as a measure of government response to

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the Covid-19 pandemic, which is developed by a group of researchers at the *Blavatnik School of Government, University of Oxford*. For a detailed account of the measurement framework see Hale et al. (2020).

It is essential to gauge how the government and the community response to the pandemic affected the nationwide and regional economic and social activity. In this regard, Google generated the *Google Community Mobility Indicators* dataset using the anonymous users' location histories aggregated for six distinct spaces: retail & recreation, groceries & pharmacies, parks, transit stations, workplaces, and residential areas for each day, each county and each state. While the first five variables measure the uses of alternative public spaces, an increase in the residential space use indicates a decline in public space use. All six measures are benchmarked to a control period (January through the first week of February) and cover the daily changes in community mobility in public and private places relative to the control period.

Finally, we use mobile phone exposure indices derived from *PlaceIQ movement data* to measure the mobility of phones and hence individuals across states irrespective of the distance. While the geodesic distance is a good measure of the movements across states through highway and/or railroads, the exposure indices would help measure highway, railroad, and air-based movements across states.

3.2 Covid-19 Pandemic At the National Level

We start our empirical analysis with a brief overview of how the pandemic progressed in the U.S. and how the state governments and communities reacted to the pandemic. Figure 2(a) presents the number of daily confirmed new Covid-19 cases since the beginning of March. In just three weeks, the number of new cases increased rapidly from zero to reach several thousand by mid-March. As the number of Covid-19 cases started to grow in the Eastern and Western coastal states in mid-March, the federal and state governments responded by closing schools, canceling public events, asking federal employees to work from home while recommending other employees to do the same.

As a result of the tightening public health policy measures, the average stringency index across the U.S. states (see Figure 2(b)) climbed from less than 10 in the first week of March to 20 by the end of the second week and all the way to 75 by the end of the third week of March. This was the highest level achieved in the average stringency index.

Figure 2(b) presents the average community mobility measures for the U.S, along with the average Covid-19 policy stringency index (GSI) for the state governments. Consistent with the rapid increase in the number of cases and more stringent policy response, communities also responded by reducing their mobility in public spaces while incasing their presence in their private residential spaces.

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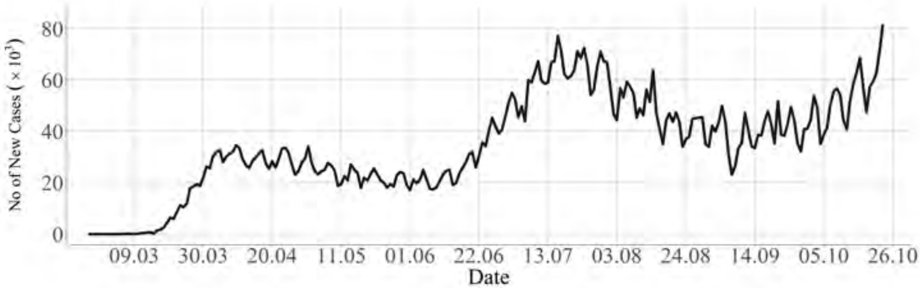
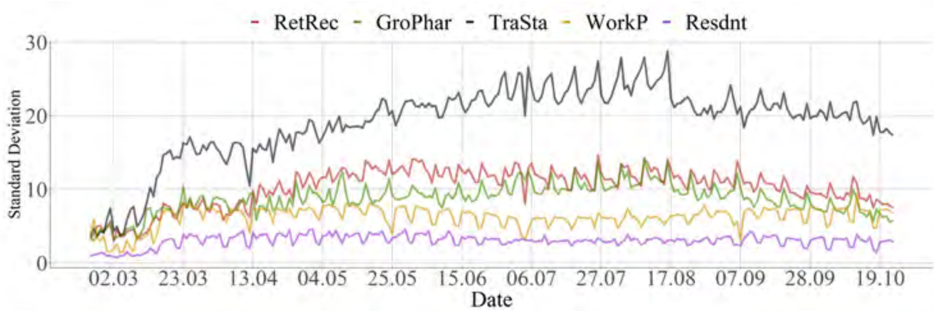


(a) Daily Number of New Covid-19 Infections

(b) Average Community Mobility Indices and Government Stringency Index



(c) Standard Deviation of Community Mobility Indices across Regions

Figure 2: United States – Daily New Cases of Covid-19 Virus, Federal Government Policy Response and Google Community Mobility Index

Despite the increased stringency of government policies, new daily Covid-19 cases continued to grow to reach 20 thousand by the end of March. Interestingly, even the number of new daily Covid-19 infections reached 30 thousand in early April; the U.S. federal government introduced no further measures. The government avoided imposing a nationwide lockdown, leaving the decision to state governments, many of which decided not to impose lockdowns. In the meantime, many European countries took strict measures to control the number of infections in April, the peak month, which pushed the government stringency index to as high as 90. It was bizarre not to observe a similar reaction by the U.S. government.[3] Consequently, the stringency index for the U.S. stayed flat at 41.7 for a long time, before declining to 38 on July 30.

As Figure 2(b) illustrates, the drop in community mobility started towards the end of the second week of March but gained momentum only in the third week of March for the whole country. The average of four CMI measures (Avg.CMI4) indicates close to a 25-percent reduction in the community's use of public spaces by this time. It declined further to 60% of the average mobility level during the reference period, picking up pace gradually to reach around 75% in May, about 80% in June through August.

Community mobility in transit stations (TraSta) and workplaces (WorkP) declined close to 50% by the end of March. It fluctuated around that level (50-55%) in April, picking up pace gradually to reach around 70% of the mobility observed in January and the first week of February. In the meantime, the community activities in parks (Park), and groceries & pharmacies (GroPhar) started to decline only approximately 20%.

The other important measure of community mobility is the use of residential places (Resdnt). As individuals ended spending more time at home, the residential space indicator reached 20% higher in late March (compared to the reference period) and, on average, stayed around that level throughout April, declining gradually to 15% in May and 10% from June onwards.

As we have noted in the introduction, our objective in this paper is to go one step further and show that those states with less restricted community mobility experienced a faster growth rate of Covid-19 infections. Those states would experience a faster rate of increase of Covid-19 cases and more likely to spread the virus to other states. To gauge the variation in community mobility across regions, we present the standard deviation of CMIs across states in Figure 2(c).

Figure 2(c) is quite revealing. While the standard deviation of CMIs declined substantially in countries that instituted nationwide lockdowns to stem the spread of the Covid-19 virus in March and early April, in the U.S., it is the other way around:

[3] Another major point of criticism of the Trump administration was the application of a rather low number of Covid-19 tests in the first two months of the pandemic, which perhaps kept the number of observed infections lower than what would have otherwise been the case.

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The standard deviation of the CMIs increased, almost doubled in March. The standard deviation of transit stations, on the other hand, tripled during the same period. It increased from around 5 to 15 in mid-March and fluctuated around that level in April. The rise in standard deviations of CMIs in the second half of March results from the absence of a nationwide lockdown in the U.S. and the wide variation in policy reaction and hence community mobility across the 51 states.

3.3 Politicization of the Response and Pandemic Momentum

In the previous section, we discussed the development of the Covid-19 pandemic at the national level along with measures of policy response and community mobility. We also noted the substantial variation in the number of Covid-19 cases and the government and community response to the pandemic across the U.S. states. In this section, we use state-level data on government stringency and community mobility indices to understand how much they contributed to the containment of the pandemic at the state-level. First, we use the panel fixed-effect regressions to estimate common quasi-elasticity of the growth rate of new cases of Covid-19 w.r.t. changes in the government stringency index and community mobility indicators response across states and over time (see subsection 3.3.1). Second, we use OLS regressions to estimate quasi-elasticity estimates for each state separately (subsection 3.3.2). Here, we show that not only had the states with Republican governors followed less stringent policies in response to Covid-19 but also, as a result of these policies, they ended suffering from faster growth of new Covid-19 cases. Finally, in this section, we show that the politicization of the whole pandemic response process is observed when one compares the states in terms of the community mobility indicators and travel intensity measures (mobile phone location) as well.

3.3.1 The Government & Community Response Matters

Both the government stringency index and the CMIs are likely to be subject to the endogeneity problem. As the number of cases starts to follow an upward trend, the state governments would be under pressure to take more serious public health policy measures. Similarly, local communities will be restricting their public space use in reaction to the increased growth momentum of the virus. Taking this fact into account, we correct for the possible simultaneity of all the right-hand side variables, using residuals from a regression on lags of the 7-day moving average of the growth rate of new cases. Then, we regress the 7-day average new case growth rate on the residuals and its own lags.

Fixed-effect regression results in Table 1 show that policy stringency is effective in lowering the 7-day average growth rate of new confirmed cases. As the number of lags

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of the policy variable is increased from one to 4 and 7 days, the elasticity of the growth rate w.r.t. to the policy stringency increases in absolute value from -0.005 to -0.026 and -0.051, respectively. The lagged policy effect on the new cases' growth rate increases further to -0.105 at 14 days before declining to -0.083 with a 21-day lag. The 14-day coefficient estimate implies that a ten % increase in the policy stringency index generates as high as a 1.0 percentage point drop in the growth rate of new Covid-19 cases (7-day average) within two weeks. This points to a significant impact on public health policy measures. The lagged growth rate coefficients are positive and statistically significant; as the number of lags is increased from one-day to 21-days, the coefficient estimate of the lagged growth rate declines monotonically from 0.911 to 0.021, and eventually becomes statistically insignificant.

Variables \ Lags	**1 day**	**4 days**	**7 days**	**14 days**	**21 days**
Stringency Index	-0.005**	-0.026**	-0.051**	-0.105**	-0.083**
	(0.001)	(0.003)	(0.004)	(0.007)	(0.005)
Lagged Growth Rate	0.911**	0.670**	0.433**	0.194**	0.0207
	(0.006)	(0.017)	(0.026)	(0.019)	(0.018)
Adjusted R^2	0.832	0.475	0.279	0.308	0.442
Observations	11,952	11,809	11,668	11,519	11,319
Avg. CMI4	0.044**	0.168**	0.220**	0.222**	0.137**
	(0.006)	(0.021)	(0.026)	(0.022)	(0.014)
Lagged Growth Rate	0.912**	0.677**	0.453**	0.240**	0.060**
	(0.005)	(0.017)	(0.026)	(0.020)	(0.017)
Adjusted R^2	0.831	0.484	0.286	0.226	0.090
Observations	11,399	11,348	11,195	10,838	10,481
Residential Places	-0.110**	-0.457**	-0.603**	-0.614**	-0.403**
	(0.011)	(0.041)	(0.053)	(0.045)	(0.030)
Lagged Growth Rate	0.914**	0.686**	0.464**	0.254**	0.068**
	(0.006)	(0.018)	(0.027)	(0.019)	(0.017)
Adjusted R^2	0.831	0.491	0.300	0.257	0.121
Observations	11,475	11,424	11,271	10,914	10,557
Robust standard errors in parentheses; ** p<0.01, * p<0.05, + p<0.1					

Table 1: Panel Regressions – Growth Rate of New Confirmed Covid-19 Cases on Policy Stringency and Community Mobility Measures (We corrected right-hand side variables for potential endogeneity by regressing them on the 7-day lagged new confirmed cases).

As the regression results suggested, strict public policy implementation is crucial for containing the virus. When used in a separate regression, the changes in community mobility patterns are also critical in the containment of the Covid-19 pandemic. Even though community mobility can be influenced by strict public policy measures, such as nationwide or regional lockdowns, it also depends on the public awareness of the risks involved and the desire to act to save lives.

As more stringent policies are likely to restrict community mobility as well, it is not feasible to put both stringency and community mobility measures together in a regression.

When we use the average of the four alternative community mobility indices[4] (Avg.CMI4) published by Google, its coefficient estimate increases from 0.044 with a one-day lag to 0.222 with a 14-day lag before decreasing to 0.137 with a 21-day lag. A 10% increase in Avg.CMI4 variable lowers the Covid-19 growth rate by up to 2.22 percentage points.

An alternative to the Avg.CMI4 variable that can be used in the regressions is the residential space variable; it measures the intensity with which the residential space is used relative to its use in the reference period (namely the whole month of January and the first week of February). As the residential space use increases, each of the four mobility measures (summarized by the Avg.CMI4) decreases. The increased residential use turns out to be more effective in lowering the growth momentum of the daily new Covid-19 cases: Its coefficient estimates vary from -0.11 with a one-day lag to -0.614 with a lag of 14 days before declining (in absolute value) to -0.403 with a 21-day lag.

According to the estimated coefficients, the increase in residential space use appears to be more effective compared to Avg.CMI4 (as an average of four other CMI measures) in lowering the growth momentum of the Covid-19 pandemic. The difference in coefficient estimates is in part due to the fact that the residential space use fluctuated by much less compared to other CMIs. While the residential space use increased to 19.5% above the reference period, the Avg.CMI4 declined by as much 41.4% compared to the reference period.

3.3.2 Response and Performance Varies Along the Party Lines

The findings of the panel regressions on the growth of new cases (see Table 1) show that strict public health policies and reduction in public space utilization by the communities would lower the growth momentum of the Covid-19. In this section, we shift our attention to investigate the government and community response, and the resulting Covid-19 cases vary along the party lines.

We first show that the growth momentum of the Covid-19 pandemic is much stronger among the states with Republican governors. Figure 3 presents the daily average number of new cases per capita (7-day MA) when states are grouped on the basis of the governor's party affiliation. While the number of new cases per capita in March through the end of May was much higher in the states with Democratic governors, states with Republican governors outstripped their Democratic counterparts as of the first week of June. Since

[4] In fact, there is the fifth measure of community mobility; the use of parks, which we do not include in the calculation of the Avg.CMI4 variable because even during the lockdowns in some states, people were allowed to use parks observing social distance rules. Consequently, while all other community mobility measures may decline, reflecting the effect of containment measures, the data of the use of parks may increase. Indeed, the coefficient estimate of the social space use in parks (which we do not report here to save space) is negative and statistically significant.

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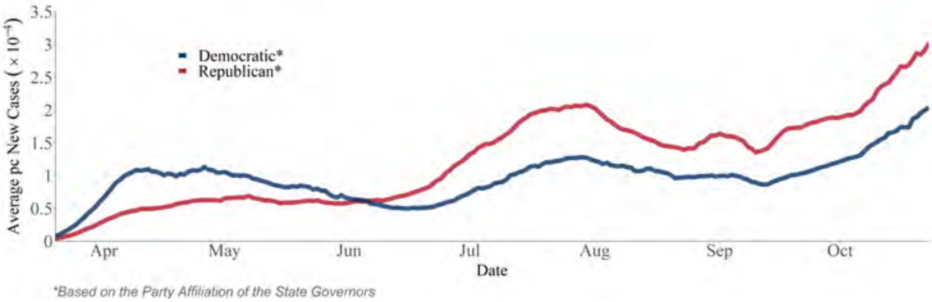

Figure 3: Average Number of New Cases Per Capita (7-day MA) – States Grouped on the basis of the Party Affiliation of the Governor

then, the number of new cases per capita in states with Republican governors has been 30-40 percent higher than the corresponding number for the states with Democratic governors.

We can also have a closer look at the performance of each state in terms of the policy stringency throughout the sample period. Figure 4 plots the stringency of the state government policies to contain the coronavirus pandemic. States are ordered on the basis of the difference of the state's sample-period average stringency index from the cross-section average over all 51 states in the March-October period (from the highest to the lowest). It is clear from the plot that the states with Democratic governors (blue-colored bars) overwhelmingly followed more stringent policies to contain the virus, whereas the states with Republican governors (red-colored bars) overwhelmingly followed less stringent policies.

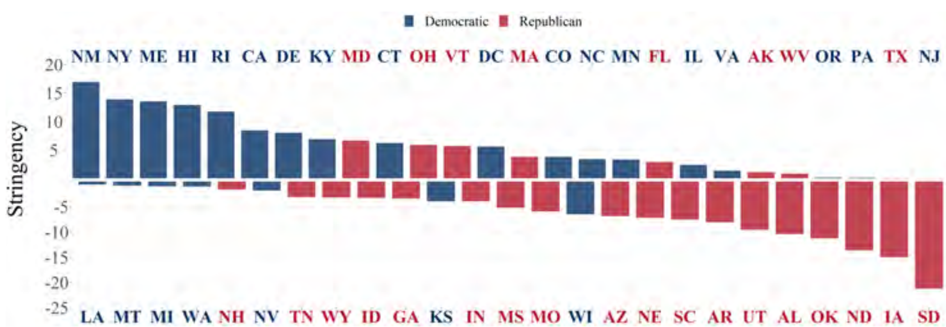

Figure 4: State Government Response to Coronavirus Pandemic - Difference from the 51 State Average, of the Average Stringency Index in the March 12-October 23 period.

Differences in state government policy stringency are likely to lead to differences in the growth rate of Covid-19 cases across the states. In order to understand this relationship better, we repeat the regressions of the new case growth rates (7-day average) on its lags

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and the lagged government stringency index for each state separately. This provides us with a different estimate of the quasi-elasticity for each state. Then we multiply the estimated quasi-elasticity of each state with the difference between the state's average government stringency index and the average government stringency index for all 51 states over the sample period.

The resulting growth rate is the implied growth rate of new Covid-19 cases due to state governments following public policy measures that have been less/more stringent than the average of all 51 states. We plot the implied average daily growth rates for each state in Figure 5.

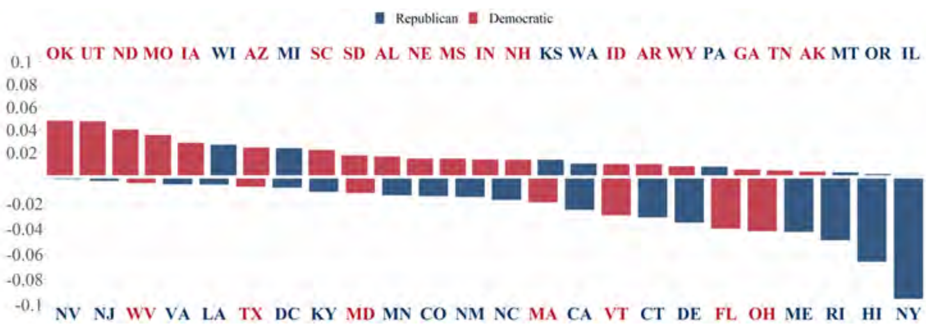

Figure 5: State Government Policy and the Implied New Case Growth Impact Estimates (based on regressions with 14-day lags) The state's actual average daily new Covid-19 case growth is X percentage points lower/higher than it would have achieved had it followed a policy response equivalent to the 51-state average stringency index over the full sample.

Each state's implied average daily new cases growth over the March 12-October 23 period is higher/lower than what would have otherwise been had the state implemented policies equivalent to the 51-state average stringency level. To make this more concrete, let us take Oklahoma as an example. Oklahoma's estimated quasi elasticity vis-a-vis the stringency index is -0.108. Its average stringency index for the sample period is 35.78, whereas the average value of average stringency indices of all states is 55.8. Oklahoma's policy restrictiveness was 20 points lower than the average for all states over the sample period. As a result, Oklahoma's average new cases' growth rate ends up being 4.8 percentage points higher than it would have been the case if it followed stricter policies to attain the median stringency index level.

In the other extreme, New York was able to lower its Covid-19 new case growth rate by almost 9.5 percentage points thanks to following very strict public health policy (with a period average stringency index of 76, 21.5 points higher than the median value for all 51 states) in the six months from March 12 to October 23.[5]

[5] We are aware of the relatively high value of the implied growth impact estimates of the state-level

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When we rank the states in terms of the success in controlling the growth rate of the new infections of the Covid-19 virus, 7 of the top 10 performers have a Democratic governor, whereas only three have Republican governors. Two of these states are Florida and Ohio that are considered as the battleground states during the 2020 Presidential elections, and the other one is Vermont. In contrast, eight of the bottom-ten performers have a Republican governor, and only two have a Democratic governor. The picture does not change when we look into the top 20 and bottom 20 performers in terms of the reigning in the growth rate of the cases of Covid-19 virus: 14 of the top 20 performing states have Democratic governors, where 16 of the bottom 20 performing states have Republican governors.

As a consequence of the substantial differences in policy response, states with Democratic governors were able to contain the spread of the virus better than the median, as measured by the difference of each state's (period average) growth rate of the new Covid-19 cases from the median growth rate over 51 states. In contrast, mostly the states with Republican governors that followed rather lax public health policies ended up suffering from faster growth of new cases of the virus in their jurisdictions (see Figure 5).

Having shown the differences between states with Democratic and Republican governors in terms of the public health policy implementation (or community mobility patterns) and the growth momentum of new Covid-19 cases over the full sample, we now focus on the behavior of daily average policy stringency index and community mobility measures for the states with Democratic and Republican governors.

Figure 6 shows the behavior of the average policy stringency index in the states with Democratic and Republican governors. From the beginning on, states with Democratic governors responded faster than the states with Republican governors to the Covid-19 virus by undertaking stricter public policy measures to contain the virus. The average stringency index for the states with Democratic governors reached as high 75 in mid-April, while that of the states with Republican governors reached 68. After the end of April, both groups of states relaxed the public health measures, gradually dropping their respective indices to respective values of 60 and 45 in mid-June and to 50 and 36 as of October 23, 2020.

We also need to pay closer attention to the difference between the two groups. The difference between the two groups increased to 12 points (out of a maximum of 100) by the end of March. As the states with Republican governors started to suffer from increased numbers of Covid-19 cases in April, they started to undertake stricter measures in the

stringency indices. We nevertheless present these results to show how much the policy differences play into the estimated growth effects across states.

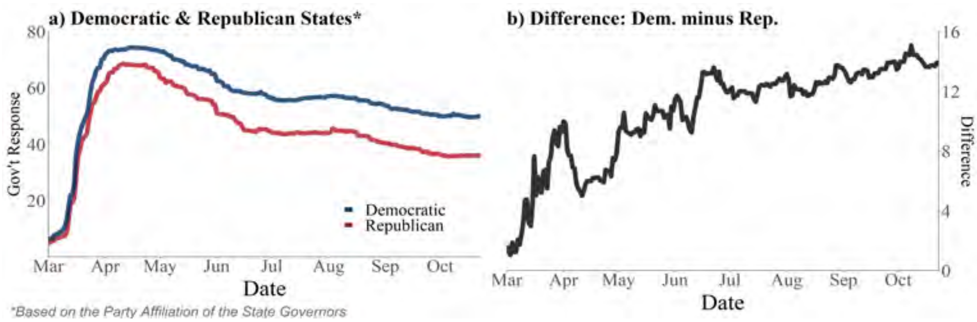

Figure 6: Government Response Over Time – States with Democratic vs. Republican Governors

rest of the month, dropping the difference to as low as 6 points. Starting in May, however, the difference increased further to reach 14 points by June 20 and fluctuated between 12 and 14 points since then.

The comparison of the average mobility index (Avg.CMI4) for the two groups of states in Figure 7 depicts a similar picture as we obtained from the comparison of policy stringency index in Figure 6.[6] Communities in states with Democratic governors, on average, spent as much as 40% less time in public spaces compared to the level they had in the comparison period. The communities in the states with Republican governors, on the other hand, reduced their public space use by as much as 33% in mid-April relative to the level in the comparison period. From the beginning of July to October 23, the states with Democratic governors reduced their public space use by 7.5-9 percentage points more relative to the states with Republican governors.

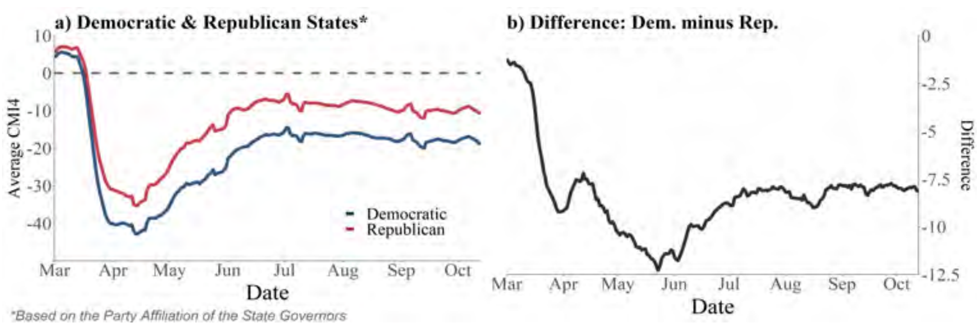

Figure 7: Avg.CMI4 Over Time – States with Democratic vs. Republican Governors

Both the government stringency index and the community mobility indicators provide us with the government and community response within each state. Neither of the two

[6] Avg.CMI4 is the average of the four public space use measures excluding the use of parks.

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measures provides any information about the links between states that would help the virus spread across states. The channels through which the virus is spread across the states is very closely related to the intensity of travel between states from the beginning of the pandemic and whether there are any differences of travel intensity between and within groups of the states with Republican and Democratic governors. Towards that end, we aggregate the mobile phone location exposure (LEX) data between states to obtain the directional travel intensities within and between the states with Republican and Democratic governors (based on the party affiliation of the governor), presented in Figure 8.

The traveling among the states with Democratic governors declined by as much as 60% as of April 19 (relative to the comparison period of January and the first week of February), while traveling among the states with Republican governors declined by 50% at the most around the same day. In both groups of states, traveling intensity among the states with Republican governors increased gradually over time to reach levels higher than the reference period in July and August. A similar tendency is observed in the intensity of travelling among states with Democratic governors, but it reached only to 90% of the level in the reference period.

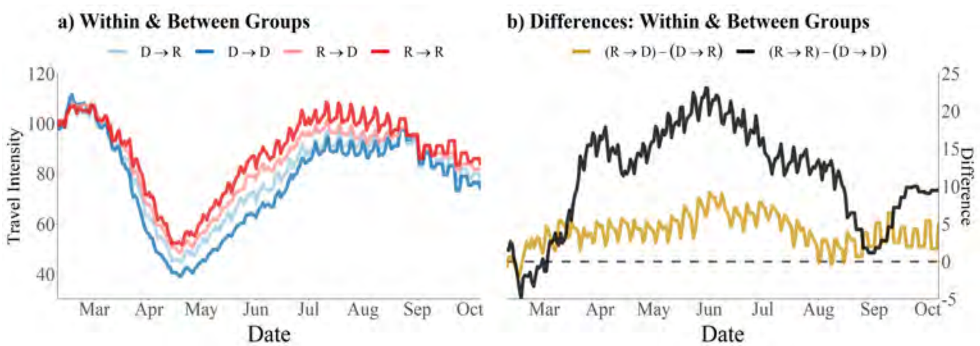

Figure 8: Location Exposure Index Over Time – States with Democratic vs. Republican Governors

Interestingly, from early March onwards, the difference between the travel intensity within the groups of the states with Republican and Democratic governors increased and reached to 15% by the last week of March. After a brief hiatus in the first half of April, the difference between the two groups started to increase again to reach as high as 22% by the end of May. The difference declined in the course of the summer vacations and fall to below 5% at the end of August, before increasing again to 10% as of the end of September. To summarize, the within-group LEX measure indicates that the states with Republican governors restricted travel by much less than the states with Democratic governors.

When we look at the intensity of travel between the states with Republican and Democratic governors, the between-group travel intensities follow a time-series pattern similar to that of the within-group travel intensities (See Figure 8(a)). Yet, the intensity of travel from the states with Republican governors to the ones with Democratic governors appears to be higher than the intensity of travel from the the states with Democratic governors to the ones with Republican governors during the pandemic (yellow line in Figure 8(b)).

4 Covid-19 Connectedness Across States

In the previous section, we showed that states with Democratic governors implemented more stringent public health policies at the state level, compared to the Republican governors. Based on the state-level average policy stringency index multiplied by the estimated average policy impact coefficient, we showed that the growth rate of new Covid-19 cases could be lower in the states with Republican governors if they had followed stricter policies. We repeated the same exercise with the community mobility indicators, and a similar result applies in that case as well. In this section, we shift our focus to the analysis of the regional connectedness dynamics of Covid-19.

4.1 Connectedness Framework

Our empirical approach builds on the Diebold-Yilmaz connectedness methodology, which was developed in a series of papers (Diebold and Yilmaz (2009, 2012, 2014)).

The connectedness approach starts with a covariance stationary N-variable vector autoregression (VAR(p)), $x_t = \sum_{i=1}^{p} \Phi_i x_{t-i} + \varepsilon_t$, where $\varepsilon \sim (0, \Sigma)$.

The moving average representation of VAR(p) is given by $x_t = \sum_{i=0}^{\infty} A_i \varepsilon_{t-i}$, where the NxN, coefficient matrices A_i obey the recursion $A_i = \Phi_1 A_{i-1} + \Phi_2 A_{i-2} + \ldots + \Phi_p A_{i-p}$, with A_0 an NxN identity matrix and $A_i = 0$ for $i < 0$.

The coefficients of the MA representation (or its transformations such as impulse response functions or variance decompositions) are critical for understanding the dynamics of the VAR system. Diebold-Yilmaz connectedness approach uses the variance decompositions, which splits the forecast error variances of each variable into parts attributable to the various system shocks. Variance decompositions also allow one to assess the fraction of the H-step-ahead error variance in forecasting x_i that is due to shocks to $x_j, \forall i \neq j$, for each i.

A study of the connectedness of Covid-19 across the states of the U.S. requires one

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to obtain the pairwise directional connectedness measures. Calculation of variance decompositions requires orthogonal innovations, whereas the VAR innovations are generally correlated. Identification schemes such as that based on Cholesky factorization achieve orthogonality. However, the resulting variance decompositions then depend on the ordering of variables, making it impossible to use them to study directional connectedness. With this understanding, Diebold and Yilmaz (2012) proposed to circumvent this problem by exploiting the generalized VAR framework of Koop et al. (1996) and Pesaran and Shin (1998), which produces variance decompositions invariant to ordering.

Instead of attempting to orthogonalize shocks, the generalized approach allows correlated shocks but accounts for them appropriately using the historically observed distribution of the errors. As the shocks to each variable are not orthogonal, the sum of contributions to the forecast error variance (that is, the row sum of the elements of the variance decomposition table) is not necessarily equal to one.

The generalized impulse response and variance decomposition analyses also rely on the MA representation of the N-variable VAR(p) equation above. Pesaran and Shin (1998) show that when the error term ε_t has a multivariate normal distribution, the h-step generalized impulse response function scaled by the variance of the variable is given by:

$$\gamma_j^g(h) = \frac{1}{\sqrt{\sigma_{jj}}} A_h \Sigma \mathbf{e_j}, \qquad h = 0, 1, 2, ... \tag{1}$$

where Σ is the variance matrix for the error vector ε, σ_{jj} is the standard deviation of the error term for the j^{th} equation and $\mathbf{e}_i$ is the selection vector with one as the i^{th} element and zeros otherwise.

Variable j's contribution to variable i's H-step-ahead generalized forecast error variance, $\theta_{ij}^g(H)$, for $H = 1, 2, ...$, is defined as:

$$\theta_{ij}^g(H) = \frac{\sigma_{jj}^{-1} \sum_{h=0}^{H-1} (e_i' A_h \Sigma e_j)^2}{\sum_{h=0}^{H-1} (e_i' A_h \Sigma A_h' e_i)} \tag{2}$$

As explained above, the sum of the elements of each row of the variance decomposition table is not necessarily equal to 1: $\sum_{j=1}^{N} \theta_{ij}^g(H) \neq 1$.

To use the information available in the variance decomposition matrix to calculate the connectedness index, Diebold and Yilmaz (2012) normalizes each entry of the variance decomposition matrix by the row sum. While normalized off-diagonal entries of the generalized variance decomposition matrix measure the pairwise directional connectedness between pairs of variables in the system, their column–sum and row-sum give the 'to' and 'from' directional connectedness measures between them. Finally, the

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sum of all off-diagonal entries of the generalized variance decomposition matrix is a measure of the system-wide or total connectedness of the variables.

Once we obtain the connectedness measures, we plot the system-wide connectedness index for the US, analyze its behavior over time, and relate it to the government and community response to the pandemic at the state level. Then we move to the regression analysis of the pairwise regional connectedness measures.

After showing the close association between government and community response to the pandemic and the growth momentum of infections at the state-level, we now shift our focus to the possibility of spillovers of infections across states. In particular, it is of great interest to understand whether the states with higher and rapidly growing infections spread the virus to neighboring and distant states through road and air travel, which have not been restricted in the United States since the beginning of the Covid-19 pandemic. Similar to the logic at the individual level, some states implementing strict public health policy measures, communities restricting their daily movements would not be as fully effective in controlling the disease because it is possible that other states and their communities do not follow similarly responsible policies and behavior can transmit the virus to the others.

4.2 Dynamic System-wide and Directional Connectedness

In Figure 9, we present the connectedness index across 51 states obtained from 56-day rolling window estimations of a VAR(3) of the weekly growth rates of (7-day average) daily new Covid-19 cases. Along with the index, we plot the three-days moving average of it.

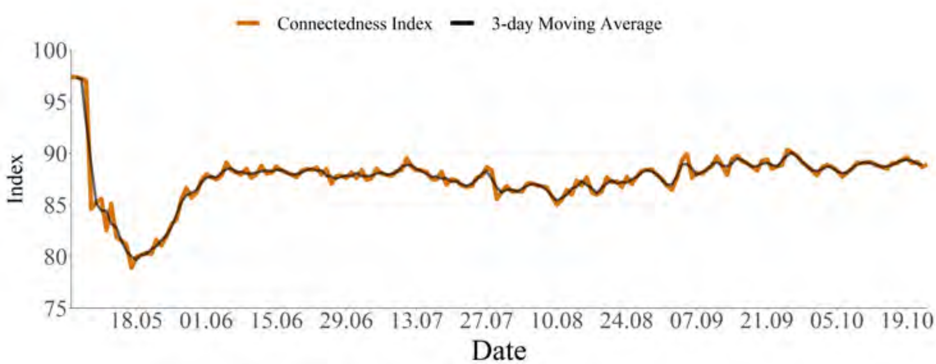

Figure 9: Dynamic System-wide Covid-19 Connectedness

As our sample starts on March 8, the connectedness measures for the first rolling window is obtained as of May 5. The fact that early on, new Covid-19 cases in especially Western and Eastern coastal states increased and spread rather fast, the connectedness index starts at 96.1. As the window is rolled over, the index drops to 80 within ten days and fluctuates between 80 and 85 percent until the Memorial Day holiday weekend at the end of May. With the easing of restrictions at the state and federal level in late May and increased intensity of travel and community mobility during the Memorial Day holiday, the connectedness index quickly moves to 88 percent and fluctuates around that level in June and July. Once the observations for late May and early June are dropped from the sample window as the window is rolled further, the index drops to 84 and gradually increased afterward. It reaches a local peak of 92 in mid-September and fluctuates 88 and 90 until the end of the sample.

In the previous section, we showed that states with Republican governors had implemented less stringent policies than the states with Democratic governors. As a result, they experienced higher daily growth rates of new Covid-19 infections than states with Democratic governors, and new Covid-19 cases per capita in the states with Republican governors were higher than the states with Democratic governors since the beginning of June. Having calculated the directional connectedness measures, now we have a closer look at whether a clear pattern emerges in the net directional connectedness between the states with Republican and Democratic governors.

The net directional connectedness measures in Figure 10 fluctuate substantially over time. Yet, starting in mid-May, we can detect a clear pattern of net connectedness of Covid-19 cases from the states with Republican governors to the to the ones with Democratic governors.

In the first two weeks of rolling sample windows (05/05–05/10), we observe that the connectedness from the states with Democratic governors to the ones with Republican governors was rather high, 2.5 points, compared to the connectedness from the states with Republican governors to the ones with Democratic governors, 1.51 points (see Figure 10a), implying a net connectedness of around -1.0 points between the two (see Figure 10(b)).

When considered together with the system-wide connectedness plot in Figure 9, it is clear that from early March to the first week of May, the states with Democratic governors contributed substantially to the system-wide connectedness. Within two weeks, the net connectedness of the states with Democratic governors to the states with Republican governors declined and eventually fell below zero (Figure 10).

This result is fully consistent with what we know: The Western Pacific and North-Eastern Atlantic states, many of which are governed by Democratic governors, received the virus from abroad (both Europe and Asia) in early March before the landlocked states that are mostly Republican. These states implemented very strict measures to contain

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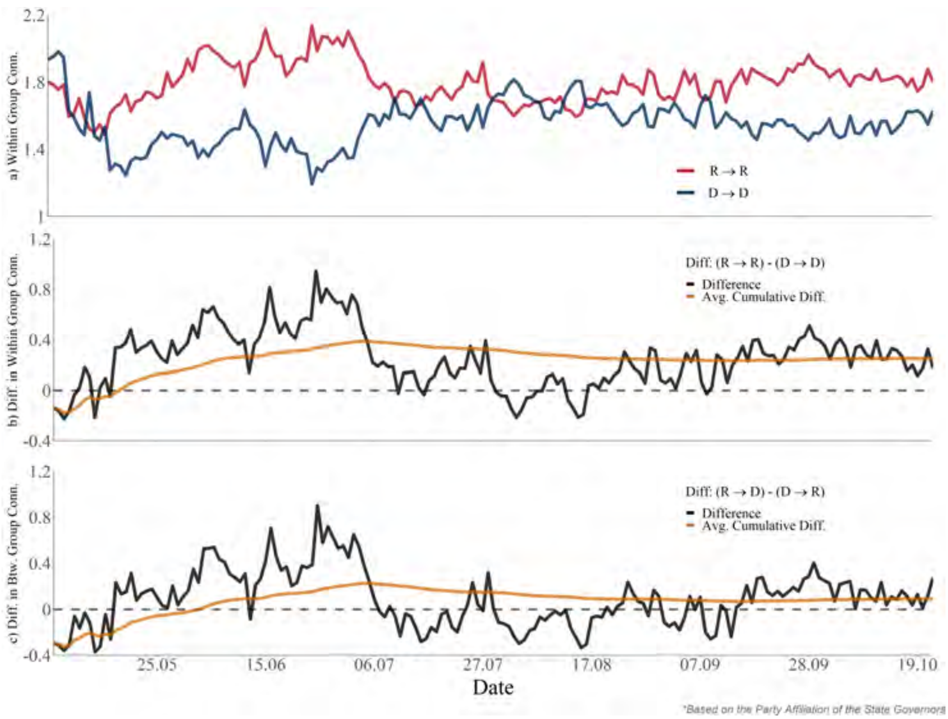

Figure 10: Directional Connectedness Within and Between Groups – States with Democratic vs. Republican Governors

the virus and succeeded in flattening the curve (that is, lowering the growth rate of new Covid-19 cases, while at the same lowering the connectedness to other states.

Faced with a rapid increase in the number of cases in a very short period of time, the Democratic state governments realized the gravity of the situation and implemented stringent public policy measures to contain the virus. Republican state governments, on the other hand, did not display a strong policy response to the pandemic threat. In the absence of active policy response from state governments, the majority of the states with Republican governors started experiencing an increasing number of infections. From mid-May onwards, the states with Republican governors started generating positive net directional connectedness to the the states with Democratic governors. The size of net connectedness that ranged between 0 and 0.4 points in the first week of June moved up to the 0.4-0.8 points range in the second half of June and the first week of July.

As a result of the pandemic and the strict policy measures, the United States, like many other countries around the world, suffered substantial human losses and economic costs. The governments were under severe pressure to start opening up businesses, especially the service sectors. Millions of families who were effectively locked in their own homes for several months looked for ways to get out and go back to normal. Even states with Democratic governors relaxed their public health policies as the new infections lost momentum in the second half of May, along with the increasing daily temperatures.

We have already seen in Figures 6 and 7 that as the government policies were relaxed in late May, the travel restrictions were lifted in both groups of states. However, the relaxed state government policy, as well as the increased community mobility, prepared the ground for further infections in the summer (see Figure 3) and increased connectedness of the virus across the states.

The connectedness from the states with Republican governors to the ones with Democratic governors declined in July and August compared to the months of May and June (see Figure 10(a)). Despite the fluctuations, net connectedness from the states with Republican governors to the ones with Democratic governors stayed on the positive side for most of the period since August. Figure 10(b) shows that the cumulative daily average net connectedness from the states with Republican governors to the ones with Democratic governors (represented by the yellow line) stayed since early June.

The difference between the within-group net connectedness, which we plot in Figure 10(c), follows a time-series pattern similar to the between-group net connectedness. But it is always higher than the between-group net connectedness. While the average cumulative between-group net connectedness stabilizes around 0.09, where the average cumulative within-group connectedness stabilizes around 0.25 (as represented by the solid yellow lines in Figure 10(b) and 10(c), respectively). The within-group connectedness patterns also support the results we obtained from the between-group net connectedness patterns over time: Lax government and community response in the states with Republican governors not only led to higher growth rates of Covid-19 cases in each of these states but also generated connectedness of the Covid-19 cases to other states, more so to other states with Republican governors.

5 Covid-19 Connectedness & Secondary Regressions

So far, we have analyzed the dynamic behavior of Covid-19 cases and its connectedness across the U.S. states and provided some evidence that the states with Republican governors tend to generate Covid-19 connectedness to each other as well as the states with Democratic governors. This analysis, however, could not help us identify the factors that contribute to the pairwise connectedness of cases across states. In this section, we undertake secondary times series and panel regressions to understand the relationship between the system-wide connectedness and the number of new cases at the

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national level and identify the factors that possibly drive pairwise connectedness across states, including the party affiliation of the governor.

5.1 Impulse Response Analysis of Cases and Connectedness

Having discussed the dynamic behavior of the connectedness measures, we think the index by itself carries important information to gauge the momentum of newly reported infections. To understand how a shock to system-wide connectedness affects the new infections in the country, *ceteris paribus*, we apply impulse-response analysis on the 7-day growth rates of the (7-day average) new confirmed cases and the connectedness index. Figure 11 illustrates the responses of new infections to the connectedness shocks along the forecast horizon. The figure also includes the results from VAR models with different lag structures up to 3-days lagging while the results of VAR(1) models are emphasized. Moreover, we prefer to stay silent about the orderings of the variables to achieve identification of the VAR system, as models with alternative orderings do not affect the validity of our argument: the positive shocks to the system-wide connectedness lead to an increase in the number of new infections at the national level.

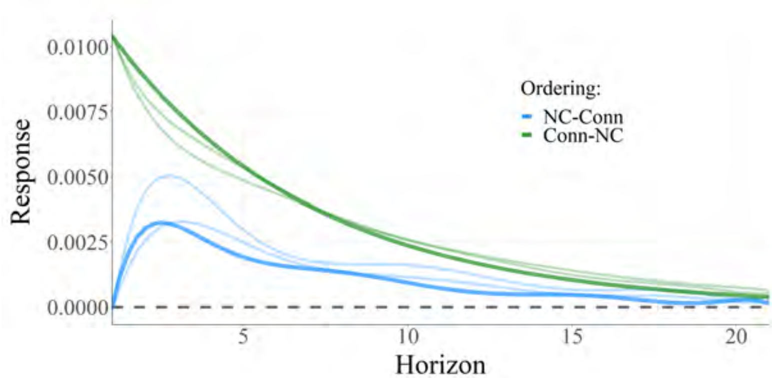

Figure 11: The Response of New Cases (7-day average) to Connectedness Shocks

The intuition behind that result is not trivial. As seen in the following sections, a change in Covid-19 growth connectedness among regions is significantly associated with the conditions that facilitate the spread across states. Not surprisingly, restrictions on community mobility and widespread adoption of protective measures keep both the number of new cases and its connectedness across states under control. When the strict public health policy measures are relaxed, we are likely to observe the connectedness outcome surge along with an increasing number of infections. The analysis also points out the presence of a circular relationship where the positive feedback from the regional

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connectedness allows the number of new cases to grow further. This result is essentially important from a policy-making perspective. Since the beginning of the Covid-19 outbreak, many researchers and public health officials repeatedly put particular emphasis on "flattening the curve" (FTC) policies against the "herd-immunity" approach to keep the daily new cases at a manageable level regarding the limited medical capacity of health institutions. Our result yields a further implication that "flattening the connectedness curve" (FTCC) at the national level also matters to support the conventional FTC approach. Along with other measures, FTCC implies that the fight against the growing pandemic may require a more active policy stance at the federal government level that would directly reduce the connectedness across states.

Armed with the result that information embedded in the system-wide connectedness can be a useful indicator for the future course of Covid-19 cases, our next task is to measure the impact of government stringency and community mobility on the pairwise Covid-19 growth connectedness outcomes. Instead of estimating secondary regressions on aggregate measures such as system-wide index, to- and from-connectedness, and other aggregate measures, we prefer to utilize a more granular approach and focus on the pairwise measures connectedness. In pairwise secondary regressions, we use variables that gauge the potential for connectedness among pairs of states as well as variables that capture state characteristics that can be of importance for the spread of the virus.

5.2 Panel Regressions Analysis of Pairwise Connectedness

In order to identify factors that contribute to pairwise connectedness across the U.S. states, we estimate panel data regressions of the following form over the full sample (from May 5 through October 23, 2020):

$$\tilde{\theta}^g_{ij,t} = \beta_0 + \beta X_{ij,t} + \alpha_j + \gamma_i + \tau_t + u_{ij,t} \tag{3}$$

where

- $\tilde{\theta}^g_{ij,t}$: daily directional pairwise connectedness from state j to state i
- $X_{ij,t}$ denotes a set of regressors that include
  - Government policy stringency index (GSI) in state j relative to state i
  - Community mobility measures (CMIs) in state j relative to state i
  - State-level location exposure index (LEX): Among the smartphones that pinged in state i on a given day, the share that pinged in state j at least once in the previous 14 days.

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- γ_j, α_i and τ_t are the source, target and time fixed-effects, respectively.

Let us start the discussion of the variables with the ratios of community mobility indicators (CMIs) for the source and target states. CMIs are crucial to understanding the difference in the community preference on the public space use across regions. Except for the use of residential places (*Resdnt*) indicator, we expect that an increase in the source to target state ratios of all CMIs to have a positive impact on pairwise regional Covid-19 connectedness. The higher the mobility in the source state relative to the target state, the more likely would it be for the source state to have a higher number of Covid-19 cases. Then we would expect it to be more likely to have the virus transmission take place from the source to the target state, rather than the other way around. Wilson (2020) shows the dynamic response of Covid-19 cases to different mobility measures, including CMI and device exposures, and argues that mobility increases the number of new cases in 3-10 weeks and deaths in 8-10 weeks significantly. Wang and Yamamoto (2020) attempt to forecast the new Covid-19 cases in regions of Arizona and verify the positive short-term effect of precautionary measures on the spread of Covid-19 using the Google mobility indices.

Next, we include the ratios of public policy stringency index in distinct pairs of states to see whether the differences in public policy have anything to do with the spread of the virus across states. As we have already highlighted above, the stringency index was one of the factors that slowed down the rate of growth of the number of new Covid-19 cases. Therefore, it is legitimate to incorporate it in the pairwise connectedness regressions as well.

It is also crucial to note that community mobility and stringency measures are not necessarily exogenous to the increase in the number of Covid-19 cases. As the Covid-19 cases in a region increases, the local government or the members of the local community themselves may introduce restrictions to diminish mobility outside of residences. As a result, the community mobility measures we intend to use as RHS variables in the secondary regressions can be endogenous to changes in the number of new Covid-19 cases.

Taking this possibility into account, we first apply state-level regressions for each of the community mobility indicators on the 7-day MA new cases. Then, we use the resulting residual term as the community mobility and stringency measures, which are assumed to be orthogonal to the number of new cases and can be used as a right-hand-side variable in the secondary regressions described above.

In addition, we would like to have a variable that provides us with information on the intensity of travel from the source to the target state. After all, individual mobility is the only way for the virus to spread across states. We don't have data that directly measures

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travel intensity between states. Instead, we use the publically available mobile phone location exposure (LEX) data for the U.S. states, which is collected by PlaceIQ. Couture et al. (2020) similarly emphasize that the index can be used as a proxy for individual mobility.[7]

To be more specific, the variable LEX measures the share of mobile phones that are pinged in the target state j on a specific day were also pinged on the source state i within the previous 14 days. This is a variable that is more valuable for the secondary regressions because it varies from one day to another. Furthermore, unlike the ratios of variables in pairs of distinct states, it is truly pairwise and directional. It measures the tendency of mobile phones and hence persons to travel from the source to the target state.

All three RHS variables, LEX, CMIs, and GSI, vary both across pairs of states and over time, allowing us to capture their possible effects on pairwise connectedness. We present the secondary regression results in Table 2. In secondary regressions, we use 438,600 observations over time, and across 51 states.[8] Adjusted R^2 varies between 0.024 and 0.028 for all eight sets of regressions.[9] It is not unusual to obtain very low values of the goodness of fit. The dependent variable, as well as the RHS variables, are obtained for pairs of states, and statistically significant coefficient estimates are of key importance to establish the relationship between the RHS variables and the measures of pairwise connectedness.

All independent variables are in logs. The dependent variable, that is, the pairwise connectedness measure, is in levels and theoretically can vary between 0 and 100.[10]

Column (1) presents the coefficient estimates for the location exposure index (LEX) between the source and target states, as well as the ratios of the state government policy stringency indices in the source and target states, all lagged for one, 4, 7, 14 and 21 days.

Column (2) through (8) of Table 2 presents the coefficient estimates for the variables of Column (1) when they are included in the regressions along with the six community mobility indicators and *AvgCMI*, included in the regressions one at a time.

In Column (1), the LEX coefficient estimates (in the middle panel) show that the pairwise Covid-19 connectedness between the source and target states is directly related

[7] Huang (2020) uses smartphone data and reports that social distancing measures based on mobility indicators reduce the average daily infection cases by %12. Brinkman et al. (2020) utilize LEX data to document spatial mobility changes during the early phase of the Covid-19 outbreak and show a significant drop in county-level mobility. Weill et al. (2020) use the alternative indices using the mobile phone location data, which is also distributed by *PlaceIQ*, the "device exposure index" to analyze the mobility within specific venues.

[8] As we use daily data for 51 states, there are 2550 observations of state pairs for each day.

[9] We report the adjusted R^2's for regressions with one-day lagged RHS variables with. The adjusted R^2's for 4- to 21-day lagged regressions are very close to the ones reported in Table2.

[10] The maximum value of the pairwise connectedness measures out of 438,600 observations is 30.417. Ninety-nine percent of all pairwise connectedness measures fall below 5.46.

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	Dependent Variable: Pairwise Connectedness							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Community Mobility Measures included as RHS Variables – One at a Time								
		RetRec	*GroPhar*	*Parks*	*TraSta*	*WorkP*	*Resdnt*	*AvgCMI*
Lagged: 1-day	–	-3.858	-2.363	-1.594**	-0.620	-16.966**	54.865**	-4.588
		(2.412)	(3.913)	(0.430)	(2.130)	(5.062)	(7.672)	(3.302)
4-day	–	6.398**	7.896*	-1.816**	9.716**	15.355**	9.914	13.538**
		(2.469)	(3.943)	(0.442)	(2.192)	(5.230)	(8.022)	(3.400)
7-day	–	15.998**	17.511**	-1.221**	16.458**	36.339**	-24.832**	25.480**
		(2.514)	(3.991)	(0.436)	(2.219)	(5.373)	(8.299)	(3.475)
14-day	–	28.877**	14.977**	-3.391**	16.967**	49.797**	-63.066**	30.825**
		(2.516)	(3.962)	(0.412)	(2.217)	(5.302)	(8.471)	(3.479)
21-day	–	29.121**	18.803**	-4.808**	12.706**	32.068**	-58.726**	27.568**
		(2.414)	(3.774)	(0.395)	(2.117)	(5.033)	(8.310)	(3.328)
Mobile Phone Location Exposure Index (LEX)								
Lagged: 1-day	0.652**	0.639**	0.647**	0.664**	0.659**	0.637**	0.600**	0.649**
	(0.111)	(0.111)	(0.111)	(0.115)	(0.112)	(0.111)	(0.111)	(0.112)
4-day	0.699**	0.718**	0.713**	0.658**	0.734**	0.711**	0.691**	0.727**
	(0.112)	(0.112)	(0.112)	(0.116)	(0.114)	(0.112)	(0.112)	(0.114)
7-day	0.661**	0.700**	0.686**	0.600**	0.715**	0.683**	0.678**	0.711**
	(0.113)	(0.113)	(0.113)	(0.116)	(0.114)	(0.113)	(0.113)	(0.114)
14-day	0.494**	0.543**	0.510**	0.371**	0.527**	0.511**	0.526**	0.534**
	(0.113)	(0.113)	(0.113)	(0.117)	(0.115)	(0.113)	(0.113)	(0.114)
21-day	0.345**	0.381**	0.357**	0.070	0.368**	0.354**	0.372**	0.378**
	(0.112)	(0.112)	(0.112)	(0.116)	(0.113)	(0.112)	(0.112)	(0.113)
State Policy Stringency Index								
Lagged: 1-day	-1.619**	-1.671**	-1.640**	-1.089*	-1.298**	-1.643**	-1.772**	-1.339**
	(0.444)	(0.444)	(0.445)	(0.463)	(0.453)	(0.444)	(0.444)	(0.452)
4-day	-1.636**	-1.531**	-1.560**	-1.253**	-1.063*	-1.603**	-1.673**	-1.095*
	(0.444)	(0.445)	(0.445)	(0.463)	(0.454)	(0.444)	(0.444)	(0.453)
7-day	-1.755**	-1.477**	-1.570**	-1.537**	-1.172*	-1.660**	-1.653**	-1.186**
	(0.450)	(0.451)	(0.451)	(0.470)	(0.460)	(0.450)	(0.451)	(0.460)
14-day	-2.146**	-1.509**	-1.954**	-2.070**	-1.398**	-1.955**	-1.816**	-1.334**
	(0.462)	(0.464)	(0.464)	(0.485)	(0.474)	(0.462)	(0.463)	(0.474)
21-day	-1.825**	-1.054*	-1.534**	-2.261**	-1.289**	-1.658**	-1.447**	-1.132*
	(0.476)	(0.479)	(0.479)	(0.505)	(0.491)	(0.477)	(0.478)	(0.491)
$\bar{R}^2$ *(21-day lag)*	0.024	0.024	0.024	0.028	0.025	0.024	0.024	0.025
Number of Obs.	438,600	438,600	438,600	408,426	431,228	438,600	438,600	431,228

Note: Robust standard errors in parentheses; $^{+}$p<0.1; *p<0.05; **p<0.01

Table 2: Secondary Panel Regressions – Government & Community Response, Travel intensity & Pairwise Connectedness (May 5-Oct. 23, 2020)

to the mobility of the smartphones between the two states. All else equal, a one percent decrease in travel intensity (as measured by LEX) between the source and the target states leads to a decline of 0.345-0.699 percentage points in the pairwise Covid-19 connectedness (see Column (1)). As we include the CMIs one at a time, the coefficient estimate for the LEX variable does not change much; it fluctuates between 0.354 and 0.734.

The implication of the LEX coefficient is quite straightforward: The higher the travel intensity between two states, the likelier it is to have higher Covid-19 connectedness between the two states. Furthermore, the coefficient estimate for LEX declines as we increase the number of lags from 7 to 14 and 21 days. This is consistent with our expectations. We consider up to 21 lags in our regressions, since Cartenì et al. (2020) points out that 21 days effectively covers the *positivity detection time*, the number of Covid-19 infections that can be transmitted from state i to state j due to travel between two states is likely to drop as the number of lags is increased above 7 days to 14 and 21 days.

This result supports the policy perspective that in the Fall-Winter 2020 wave of the Covid-19 pandemic, implementation of travel restrictions would be effective in curbing the spread of the virus across the U.S. states.

We have already shown in section 3.3 that the states that implemented strict public health measures against the pandemic were able to curb the rate of growth of new cases of Covid-19 (see Table 1). Interestingly, when we incorporate the state-level government stringency index in secondary regressions, the resulting estimates are mostly negative and statistically significant, which implies that strict policy measures do help lower the connectedness of Covid-19 cases across the U.S. states as expected. The coefficient estimate for the GSI (in regressions without the CMIs) increases in absolute value from -1.62 to -2.15 as we increase the number of lags to 4, 17, and 14 days, before declining to -1.83 with 21-day lags. The effectiveness of the state government public health policy measures become more effective as days pass by up to 14 days, but eventually, its effectiveness is likely to experience a downturn.

This result establishes that the stringent state government policy not only reduces the number of cases in the state, but it also reduces the connectedness of new cases from that state to others. On the contrary, lax state government policy not only leads to an increase in the state-level cases but also generates spillovers/connectedness to other states.

Finally, we present the source state to target state ratios of CMIs in the first panel of Table 2. Except for the coefficient estimates of the one-day lagged CMIs and the 4-day lagged *Resdnt*, coefficient estimates for all CMI variables are statistically significant and have the expected sign. Despite the fact that all CMI variables are corrected for potential endogeneity problems, their one-day lagged coefficient estimates are mostly of wrong sign

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and statistically insignificant. This result is perhaps a reflection of the fact that it is not meaningful to expect a reaction in Covid-19 cases one day after a decline in CMI variables.

As we increase the number of lags, the coefficient estimates are all of the expected sign. While all other community mobility measures provided by Google track the sharing of non-overlapping segments of social space, the residential space variable is the only one that measures the increased use of private space and hence the lower use of all segments of the social space. The coefficient estimate for the residential space at 7-, 14- and 21-day lags is negative as expected, and it increases in absolute value as the number of lags considered increases from 7 days (-24.8) to 14 days (-63.1). However, as the number of lags is increased further, the dampening effect of the increased residential space use on Covid-19 connectedness declines significantly to -32.1 (in absolute value) at 21-day lag. By itself, this variable shows that the community's response to isolate itself is effective in reducing the connectedness of the virus to other states.

Even though parks provide communities with space that can be used by the members of the community, the use of parks actually did actually have a small but statistically significant contribution to lowering the growth rate of the new Covid-19 cases. As a consequence, we would expect social space use in parks to lower the Covid-19 connectedness across the U.S. states. Consistent with these expectations, the coefficient estimates for the social space use in parks are all negative and statistically significant, varying between -1.2 and -4.8 as the number of lags increases.

As parks offer large open spaces and its use by the members of the local communities is not necessarily in conflict with the social distancing rules. As a consequence, members of the communities that cut the social space use in retail and recreation, grocery and pharmacy, transit stations, and workplaces can continue enjoying the wide parks without running the risk of catching the virus. Hence, the increased use of parks is likely to lower the growth rates of the new Covid-19 cases and hence their connectedness across states.

Among the variables that aim to measure the use of social space, workplace use $WorkP$ has higher coefficient estimates in lags 4 through 21 days (varying between 15.4 and 49.8) than the other three measures. Even though the workplace use declined more than the other three measures, it nevertheless appears to contribute to the containment efforts more than the other three measures of community mobility.

According to the coefficient estimates, the decline in the social space use in retail and recreation ($RetRec$) contributed to the decline in connectedness across states more than the social space use in transit stations and groceries and pharmacies. Furthermore, its contribution continues to increase when the number of lags is increased to 14 and 21 days.

The coefficient estimates for the grocery and pharmacy and the transit stations reach

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their highest levels at 7-day lags (17.5 and 16.5, respectively) and stabilize around those levels at higher lags of 14 and 21 days considered.

In general, the coefficient estimates of the source-to-target ratios of relative CMIs tend to increase as the number of lags considered is increased from one-day to 7- or 10-day, indicating stronger effectiveness of government or self-imposed restrictions on public space use. As the number of lags is increased further to 14 days, the coefficient estimates decline, which indicates the existence of a peak in the effectiveness of the restrictions on Covid-19 connectedness across states at around 10-days. As expected, the use of parks has very little contribution to Covid-19 connectedness across states because it is possible to comply with social distancing rules in large open areas offered in parks.

Finally, the coefficient estimate for the average of the four CMI variables (*AvgCMI*) turns out to have relatively higher coefficient estimates compared to other CMI variables. This is quite expected because it reflects the behavior of four different measures of social space use, and a decrease in AvgCMI would generate a more significant response from the connectedness measures.

To summarize, the coefficient estimates for all CMIs basically show that irrespective of which measure of community variable we use, those states that tend to have high community mobility in public spaces not only end up having a higher case of Covid-19 infections but they are likely to generate connectedness of Covid-19 cases to other states.

So far, in this section, we have shown that proactive government and community response can play a critical role in limiting the connectedness of Covid-19 cases across the U.S. states. In addition, we have shown the critical role played by the between-state traveling in generating connectedness of Covid-19 cases across the U.S. states. Finally, in this section, we will search for econometric evidence about the role politics played in the spread of the virus across states.

Table 3 presents the panel regression of pairwise connectedness on the pairs of state governors' political party affiliation dummies in addition to the variables that were already included in Table 2. In order to save space, in Table 3 we present the results for 7-, 14- and 21-day lagged variables and use only two CMI variables, namely, *Resdnt* and *Avg.CMI4*.

As for the dummy variable, $R \rightarrow R$ denotes the dummy variable where the source is a state with a Republican governor while the target is a state with a Democraqtic governor. Similarly, $R \rightarrow D$ is the dummy variable for the source state with a Republican governor and the target state with a Democratic governor, and $D \rightarrow R$ is the dummy variable for the source state with a Democratic governor and the target state with a Republican governor. The term $D \rightarrow D$ is not included and treated as the reference variable. Statistically significant coefficient estimates for any of the three variables would indicate the difference from the coefficient estimate of the missing dummy variable, $D \rightarrow$

	7-day lag			14-day lag			21-day lag		
LEX	0.466**	0.485**	0.511**	0.291*	0.323**	0.330**	0.139	0.167	0.172
	(0.114)	(0.114)	(0.115)	(0.114)	(0.114)	(0.116)	(0.113)	(0.113)	(0.114)
Stringency	0.007	0.133	0.651	-0.432	-0.037	0.414	-0.019	0.44	0.686
	(0.50)	(0.50)	(0.51)	(0.51)	(0.51)	(0.52)	(0.53)	(0.53)	(0.54)
$R \to R$	12.35**	12.34**	12.55**	12.47**	12.45**	12.38**	12.57**	12.56**	12.43**
	(0.72)	(0.72)	(0.73)	(0.73)	(0.73)	(0.73)	(0.73)	(0.73)	(0.73)
$R \to D$	12.73**	12.75**	12.95**	12.69**	12.76**	12.60**	12.86**	12.95**	12.70**
	(0.73)	(0.73)	(0.74)	(0.73)	(0.73)	(0.74)	(0.73)	(0.73)	(0.74)
$D \to R$	1.315**	1.281**	1.280**	1.470**	1.383**	1.410**	1.404**	1.300**	1.385**
	(0.49)	(0.49)	(0.50)	(0.49)	(0.49)	(0.50)	(0.49)	(0.49)	(0.50)
Residential	–	-25.88**	–	–	-64.6**	–	–	-60.85**	–
		(8.30)			(8.47)			(8.31)	
Avg.CMI4	–	–	25.27**	–	–	30.84**	–	–	27.84**
			(3.47)			(3.48)			(3.33)
No. of Obs.	438,600	438,600	431,228	438,600	438,600	431,228	438,600	438,600	431,228
R^2	0.024	0.024	0.026	0.024	0.025	0.026	0.024	0.025	0.026

Note: Robust standard errors in parentheses; $^+$p<0.1; *p<0.05; **p<0.01

Table 3: Secondary (Panel) Regressions – Accounting for Within and Between Group Connectedness of the States with Democratic and Republican Governors (May 5-Oct. 23, 2020)

D.

The coefficient estimate for the LEX variable is positive and statistically significant for the 7- and 14-day lags, but not significant for the 21-day lag. As we have noted in the discussion of Table 2, the effect of travelling on Covid-19 cases and hence their connectedness across states is likely to diminish after 14 days. Therefore, it is no surprise to obtain statistically insignificant coefficient estimates for the 21-day lag.

The second variable in the regression is the government stringency index, whose coefficient estimates become statistically insignificant at all lags. Once we include the dummies for pairs of source and target states on the basis of the governor's political party affiliation, the stringency of the government policy ends up having no effect on the connectedness across states. This result clearly shows that the governor's party affiliation directly affectss the stringency of the public health policy against the pandemic.

Third, we focus on the coefficient estimates of the community mobility indicators. Both residential space use and Avg.CMI4 variables are statistically significant and have the expected signs. Their respective coefficient estimates are not much different from what we presented in Table 2.

The coefficient estimates for $R \to R$ and $R \to D$ dummies are statistically significant at all lags, and their values vary between 12.3 and 12.95, depending on the lags used and the inclusion of CMI variables in the regression.[11] Both of these estimates are much greater than the coefficient estimate for $D \to R$, which ranges between 1.28 and 1.47.

The coefficient estimates from the panel regressions of pairwise connectedness measures enforce our findings from the graphical analysis of total directional connectedness measures between the states with Democratic vs Republican governors (see Section 4.2). First, the states with Republican governors tend to generate higher pairwise connectedness to each other and to the ones with Democratic governors compared to the respective measures of connectedness generated by the states with Democratic governors. Second, the within-group connectedness of the states with Democratic governors is statistically higher than the connectedness from the states with Democratic governors to the ones with Republican governors.

6 Conclusion

In this paper, we studied how the regional variation in the implementation of public health policy measures and local community attitudes towards the pandemic, in part shaped by the polarized politics of the presidential elections, determined the local trajectories of Covid-19 cases and their connectedness across the U.S. states.

The public health policy response to the Covid-19 pandemic varied substantially around the world. In the United States case, the federal government failed to put together a public health policy strategy that outlined policy measures against the pandemic at the federal level while guiding and coordinating the states' policies. As a consequence, since the beginning, there has been a wide variation in containment measures implemented by the U.S. states. The state-level public health policy response to the pandemic was excessively politicized, perhaps due to 2020 being a presidential election year.

In addition to documenting the differences in measures such as the government stringency index and the community mobility indicators across the states, we can identify a pattern in these measures across political party lines. State governors' political party affiliations also influenced the trajectories of the Covid-19 cases over time and their connectedness across the states. Unlike the states with Democratic governors, the states with Republican governors, in general, have not implemented strict policy measures against the pandemic. Nor are the communities in these states strictly followed social distancing recommendations of experts.

[11] When the panel regressions in Table 3 are repeated with $R \to R$, instead of $D \to D$, is designated as the reference variable, the coefficient estimate for $R \to D$ is not statistically different from zero, indicating that the pairwise connectedness among the states with Republican governors is as high as the pairwise connectedness from states with Republican governors to the ones with Democratic governors.

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Our paper is the first in the literature showing that the states with lax government and community response to the pandemic experienced higher new cases of Covid-19 and generated connectedness of Covid-19 cases to each other and to those states that followed stricter policy and community response. Finally, we also found that Covid-19 connectedness across states was quite strongly related to the travel intensity across states, as measured by the mobile phone location exposure index, LEX.

Based on these findings, we conclude that strong government and community responses to the pandemic are needed to bring down the growth rates of Covid-19 infections in each state and lower the connectedness of Covid-19 cases across the states. Finally, we conjecture that once the federal government show leadership in implementing stringent public health policies at the federal level and coordinate the state-level policies, both the Covid-19 cases and their connectedness across the states can be brought under control.

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Monitoring in real time: Cross-country evidence on the COVID-19 impact on GDP growth in the first half of 2020[1]

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Date submitted: 6 November 2020; Date accepted: 11 November 2020

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The COVID-19 pandemic has a severely negative impact on economic activity. We analyze whether and to what extent mandatory social distancing imposed by lockdown policies and voluntary social distancing triggered by COVID-19 fatality rates have driven growth developments in the first and second quarter of 2020. Based on a sample of 46 countries and making use of OLS, IV and panel fixed effects regressions we find that the stringency of lockdown policies drives growth developments over time, while fatality rates carry an additional weight in explaining cross-country growth differences for each quarter. Finally, vulnerabilities to mandatory and social distancing performed abroad captured by tourism exposure and trade openness, play a non-negligible role in explaining growth differences across countries in the first half of 2020.

1 We thank an anonymous reviewer, Roland Beck, Benjamin Born, Afschin Gandjour and Karol Kempa for helpful comments and suggestions.

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1. Introduction

The global COVID-19 health crises has caused massive economic disruptions and precipitated a dramatic slowdown of the global economy (IMF 2020). However, as different countries were differently affected by the virus and have responded differently in terms of policies (Hale et al. 2020, EIU 2020, Cheng et al., 2020) the impact on economic activity varies across countries. This paper addresses the question whether cross-country differences in GDP development over the first and second quarter of 2020 are largely driven by differences in the intensity with which governments enacted restrictions, such as lockdowns, i.e. by the economic effects of *mandatory* social distancing, or largely by differences in the fatality rate associated with the virus, i.e. by the economic effects of *voluntary* social distancing.

The paper is motivated by the debate on the economic implications of government-imposed restrictions and lockdowns responding to rising infection rates. As these measures hamper activity, some countries introduced lockdowns rather hesitantly (the UK and the US) or opted for a liberal approach in addressing the pandemic, such as Sweden (Krueger, Uhlig and Xie 2020, Born, Dietrich and Müller 2020). The policy response reflected concerns that the social and economic costs of a stringent government response would be too high even if it reduced health risks. By contrast, other countries enacted strict lockdowns, either because infection rates quickly transformed into high fatality rates, such as Italy or Spain, or because governments aimed at preventing high fatality rates, such as in Germany or Denmark, because they deemed the degree of voluntary social distancing as insufficient to keep the pandemic under control (Farboodi, Jarosch and Shimer 2020).

In addition, stringent measures were justified on economic grounds (Gros 2020, Dorn et al. 2020, IMF 2020). While governments of the respective countries acknowledged that tight restrictions would negatively affect economic activity, strongly rising and high infection rates were associated with even greater economic damage either due to direct costs such as a loss of working time and the rise in medical costs (Gros 2020) or by costs associated with the overall response of agents to rapidly rising health risks in the form of voluntary social distancing causing an even larger drop in consumption and work than under a temporary government-imposed lockdown (Eichenbaum, Rebelo and Trabandt 2020, IMF 2020).[3]

[3] Historical evidence supports the view that the negative economic effects of pandemics are large even when governments do not intervene with severe lockdowns as in the current COVID-19 case (Jordá, Singh and Taylor 2020, Barro, Ursúa and Weng 2020, Carillo and Jappelli 2020).

Disentangling the economic effects of mandatory and voluntary social distancing via the stringency index and the fatality rate is inherently difficult as endogeneity challenges loom large. On the one hand, the government is likely to impose stricter measures for mandatory social distancing when the fatality rate rises, i.e. when voluntary social distancing is insufficient to keep the pandemic under control. On the other hand, government-imposed measures influence the fatality rate. Moreover, the degree of mandatory social distancing and the level of fatality rates are likely driven by country characteristics such as social cohesion and life expectancy. We address these concerns by accounting for factors influencing the stringency of government measures and the fatality rate by running instrumental variable and panel fixed effects regressions. By doing so, we separate the economic impact of mandatory and voluntary social distancing triggered by the pandemic. Based on this, we address the question whether the decline in economic activity observed during the first and second quarter of 2020 was basically triggered by the "policy decision to lock down the economy" (Blanchard 2020) or whether health risks triggering voluntary social distancing have also been responsible for the recession. This demand is likely to increase with the new rise in infections triggering a new debate on the question governments answered in different ways in the first half of 2020.

Several studies address this question as very early in the pandemic evidence emerged that risk aversion rises when people are confronted with COVID-19 cases in the region they live in (Dryhurst et al. 2020, Huynh 2020, Maloney and Taskin 2020). This suggests that the economic impact of government imposed measures is likely to be smaller than commonly assumed as the counterfactual is not the smooth pre-pandemic environment, but an economy operating under substantial voluntary social distancing. Against the background, Goolsbee and Syverson (2020) compare activity along the borders of US states with different lockdown intensities and find that "legal shutdown orders account for only a modest share of the decline of economic activity". In a similar vein, Bartik et al. (2020) report that shelter-in-place orders account for a small share of job losses in the US labor market related to COVID-19 only. Overall, studies – including the latest IMF World Economic Outlook (IMF 2020) – seem to suggest "that voluntary behavioral changes induced by the outbreak have been at least as important as, if not more important than, policy measures in terms of explaining reductions in mobility and activity." (Gapen et al. 2020).

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This paper analyses the impact of mandatory and voluntary social distancing related to COVID-19 on GDP developments in 46 countries for the first two quarters of 2020.[4] Concretely, we test whether the fatality rate, i.e. the number of reported deaths related to COVID-19 (per 100,000 inhabitants) (hereafter: Fatality), serving as a proxy for the severity of health risks triggering *voluntary* social distancing, has a significantly negative effect on GDP developments when already accounting for lockdown severity, i.e. the degree of *mandatory* social distancing imposed by the authorities, captured by the Stringency Index (hereafter: Stringency) compiled by Oxford University (Hale et al. 2020).[5]

Results indicate that mandatory social distancing represented by the stringency of government-imposed measures (lockdowns) dominates within-country GDP developments over time in panel regressions while voluntary distancing related to health risks captured by the fatality rate plays an important role in explaining cross-country growth differences for each quarter in OLS and IV regressions. Social distancing abroad has a significantly negative effect of growth as well: countries with a larger exposure to tourism and – to a lesser extent – with a higher degree of trade openness record a deeper fall in growth. Thus, our analysis provides support for both views: lockdown intensity is the driver of economic decline in the first half of 2020, but in addition voluntary behavioral changes induced by rising health risks account for a non-negligible part of cross-country differences in GDP growth in the first and second quarter of 2020. On top of this, a higher degree of integration into the global economy via tourism and trade makes countries more vulnerable to mandatory and voluntary social distancing abroad.

We conclude from this that the economic implications of lockdowns are double-edged: on the one hand they have a direct negative impact on economic activity, on the other hand they might support economic activity if they contain rising health risks leading to higher fatality rates which would negatively impact economic activity. The first conclusion, supported by the panel fixed effects estimations, suggests that policymakers are right when stressing that lockdowns should be avoided because they are harmful to economic activity. At the same time, our OLS and IV estimation results indicate that the cross-country growth evidence in the first half of

[4] IMF (2020) conducts a similar analysis but employs non-conventional economic indicators, such as mobility data and job postings, as measures of economic activity.

[5] The countries are Australia, Argentina, Austria, Belgium, Bulgaria, Brazil, Canada, Switzerland, Chile, China, Columbia, Czech Republic, Germany, Denmark, Spain, Estonia, Finland, France, the United Kingdom, Greece, Hungary, Indonesia, India, Ireland, Iceland, Israel, Italy, Japan, Luxembourg, South Korea, Lithuania, Latvia, Mexico, the Netherlands, New Zealand, Norway, Poland, Portugal, Romania, Russia, the Slovak Republic, Slovenia, Sweden, Turkey, the United States and South Africa.

2020 also reflects the negative economic effects of voluntary distancing triggered by high fatality rates. Thus, from a policy making perspective these results serve as a reminder that the detrimental effects of stricter government measures on economic activity do not imply that lockdowns should be avoided “at all costs”. Of course, it has to be emphasized that these conclusions are based on evidence from two quarters only. Accordingly, more evidence is needed to reach firm conclusions.

Our paper is structured as follows: In section 2 we present the data and the methodologies we employ. Section 3 presents our main results, followed by robustness checks in section 4. Section 5 concludes.

2. Data and methodology

Our analysis is based on a sample of 46 countries, including all OECD countries. Our main dependent variable is the quarterly growth rate of real GDP, i.e. the change in real GDP over the same quarter in the previous year. Descriptive statistics (Table 1) illustrate the depth of the downturn. On average, countries in the sample record GDP growth of minus one in the first and minus eleven in the second quarter of 2020.

The unprecedented decline in GDP growth is widely explained by mandatory social distancing following government-imposed mobility restrictions. We capture the degree of these restrictions by the quarterly average of the stringency index published by the University of Oxford. For the countries in the sample, the stringency index rose on average from zero in the fourth quarter of 2019 to 20.04 in the first and 67.73 in the second quarter of 2020. In addition, agents responded to the rising health risk posed by COVID-19 by voluntary social distancing. We capture these risks by the number of confirmed deaths related to COVID-19 per 100,000 inhabitants.[6] On average, the COVID-19 fatality rate rose from zero in the fourth quarter of 2019 to 1.73 in the first and 14.54 in the second quarter (Table A1 in the Appendix). Descriptive statistics also show the rise in stringency index and fatality rates over time. In the first quarter no country recorded a fatality rate above 20, and governments of most countries enacted rather mild measures to contain the pandemic. Notable exceptions are China (average stringency index of 59) as well as Italy, South Korea and France (Figure 1).

[6] We opt for the fatality rate rather than the rate of infections as the latter is allegedly subject to larger cross-country differences unrelated to health risks triggered by COVID-19, such as different testing and reporting policies, than the former.

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Table 1: Descriptive Statistics

	Mean		Median		Standard Deviation		Minimum		Maximum		Countries	Source
	Q1	Q2	Q1	Q2	Q1	Q2	Q1	Q2	Q1	Q2		
Economic indicator												
Growth rate (%)	-0.01	-0.11	0.00	-0.11	0.03	0.06	-0.07	-0.23	0,04	0,03	46	OECD[7]
COVID-19												
Stringency	20.04	67.73	18.90	70.00	8.18	11.63	6.17	37.9	58,98	92,59	46	
Stringency (ln)	2.99	4.21	2.99	4.26	0.34	0.18	1.97	3.66	4,09	4,54	46	
Fatality	1.73	14.54	0.23	5.93	3.88	17.92	0.00	0.09	19,12	73,67	46	
Fatality (ln)	0.58	2.09	0.21	1.94	0.77	1.19	0.00	0.09	3,00	4,31	46	
Other stringency indicator												Oxford Stringency Index database[8] (Hale et al. 2020)
Hard Lockdown	0.06	0.24	0.01	0.10	0.07	0.29	0.01	0.00	0,25	1,00	46	
Cancel public events	0.48	1.79	0.45	2.00	0.24	0.30	0.00	1.00	1,54	2,00	46	
Gather restrictions	0.65	3.11	0.62	3.34	0.49	1.01	0.00	0.00	3,08	4,00	46	
Close public transport	0.14	0.71	0.11	0.83	0.22	0.56	0.00	0.00	1,43	2,00	46	
Restr. internal move	0.28	1.31	0.25	1.27	0.25	0.61	0.00	0.00	1,52	2,00	46	
Stay at home reqs.	0.30	1.30	0.25	1.27	0.34	0.66	0.00	0.00	2,08	2,67	46	
Intl. travel controls	1.12	3.20	0.91	3.31	0.66	0.82	0.00	0.00	2,25	4,00	46	
Mobility												
Retail Change	-19.88	-40.67	-20.57	-40.63	8.55	16.53	-42.30	-73.23	0,37	-8,40	44	
Grocery Change	-5.11	-13.55	-3.41	-12.41	5.64	12.02	-20.67	-43.05	7,30	8,62	44	Google Mobility Data[9]
Workplace Change	-15.18	-34.50	-16.30	-32.25	7.36	9.33	-32.83	-54.51	0,87	-5,92	44	
Controls												
Trade	95.48		78.43		64.01		27.56		387.10		46	
Tourism	7.84		5.37		6.06		1.52		26.38		46	
GDP p.c (ln)	10.36		10.42		0.52		8.85		11.46		46	World Bank[10]
Trend growth	2.78		2.48		1.91		-1.25		9.88		46	
Instruments												
Speed	66.87		70		11.59		22		84		46	Own calculation
Life Expectancy	79.13		81.16		4.25		63.86		84.21		46	World Bank
Population (ln)	9.94		9.79		1.81		5.82		14.17		46	

Note: All logarithmic values are scaled by ln(x+1). *Growth variables* are drawn from quarterly national account data provided by the OECD. *COVID-19 variables* are taken from Oxford Stringency Index database and calculated as quarter means. *Google Mobility Data* retrieved from Google. *Controls* (Trade, Tourism, GDP p.c., Life expectancy, Population) are drawn from the World Bank Database representing 2018 values. Speed refers to the number of days it took from beginning 1 January 2020 for governments to enact mandatory measures representing a stringency index level of 20 and above (i.e.. Speed takes the value 74 if the stringency index stood at a level of 20 for the first time on 14 March 2020).

By contrast, in the second quarter, only four countries imposed measures with a stringency level below 50, as several countries experienced high fatality rates passing 50 (Belgium, the UK and Sweden, see also Table A1 in the Annex). Correlation analysis reveals that the stringency index and the fatality rate are positively correlated in the first, but not in the second quarter (Table A2 in the Annex).

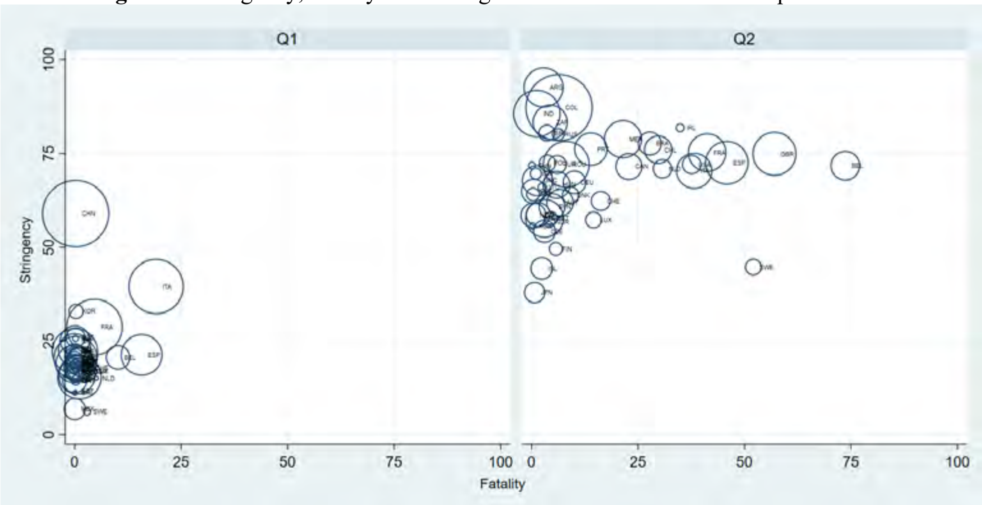
[7] Data: https://stats.oecd.org/index.aspx?queryid=350

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

[9] Data: https://www.google.com/covid19/mobility/

[10] Data: https://tcdata360.worldbank.org/indicators/ST.INT.RCPT.XP.ZS

Figure 1: Stringency, fatality and GDP growth in the first and second quarter 2020



Sources: Hale et al. (2020), OECD and authors' calculations. Note: *Stringency* represents non-weighted mean average in the respective quarter. *Fatality* is the number of COVID-19 deaths per 100,000 inhabitants in the respective quarter. Size of bubbles represent GDP growth rates in Q1 and Q2 2020, respectively, with a more negative rate represented by larger size.

Moreover, the stringency of government-imposed measures is strongly negatively correlated with GDP growth in the second, but not in the first quarter of 2020, while fatality rates show a negative correlation with GDP growth in both quarters, albeit at a somewhat lower level of significance than the one recorded for stringency and growth in the second quarter.

Domestic economic activity is not only affected by mandatory and voluntary distancing at home but also by government-imposed and voluntary social distancing abroad. For example, the domestic tourism sector is hit hard by travel bans imposed by foreign governments and voluntary cancellations of trips by non-residents (Gössling et al. 2020, IMF 2020). Following König and Winkler (2020), we capture the vulnerability of countries to mandatory and voluntary social distancing applied abroad by the share of tourism receipts in total exports in 2018 and by trade openness, measured by the sum of exports and imports divided by GDP in 2018.[11] Country studies suggest that the combined effect of tourism and openness can be substantial. For Switzerland, estimates suggest that the combined effects of mandatory and voluntary social distancing abroad might account for up to 70% of the total decline in economic activity related to COVID-19 in the spring of 2020 (Rathke, Sarferaz and Streicher 2020).

[11] There is also evidence that openness impacts the spread of the virus, i.e. cross-country differences in the number of infections and deaths (Luong and Nguyen 2020, Zimmermann et al. 2020).

Our analysis employs two approaches. First, we run robust OLS regressions as well as robust IV regressions for the first and second quarter of 2020, respectively, taking the form:

$$\Delta y_{i,2020} = \alpha + \beta_1 * \mathrm{COVID}_i + \beta_2 * \mathrm{SP}_i + \beta_3 * \mathrm{Z}_i + \varepsilon_i. \quad (1)$$

where y_i is the quarterly GDP growth rate of country i in either the first or second quarter of 2020. COVID_i are our main variables of interest, i.e. the stringency index and fatality rate, while SP_i represent tourism and openness, accounting for the vulnerabilities of countries to COVID-19 spillovers from abroad. Finally, we follow Lane and Milesi-Ferretti (2011) and employ some general controls, Z_i. Given the limited number of countries in the sample, we only account for GDP per capita as well as the average GDP growth rate between 2014 and 2019. We expect that richer countries are likely to respond more efficiently to the health and economic crisis than low-income countries given access to larger resources and that the pre-crisis growth rate average picks up convergence effects, i.e. countries with higher trend growth likely record a higher growth rate in the pandemic period. Given its skewed nature GDP per capita is employed in logarithmic form.[12]

Cross-country growth regressions are subject to omitted variable bias and endogeneity concerns which feature prominently in any analysis of the impact of mandatory and voluntary distancing related to COVID-19 on economic activity (IMF 2020). We address these concerns in two ways. First, we instrument both COVID-19 variables, i.e. the stringency index and the fatality rate, by:

a) The number of days starting from 1 January 2020 it took governments to respond to the pandemic in the form of mandatory restrictions defined as a stringency index level of 20.[13] For example, in Germany the stringency level reached 20 for the first time on 29 February, representing the sixtieth day of the year, while in the UK the threshold was hit for the first time on 17 March, i.e. the variable takes the value “76”. Our choice is motivated by the hypothesis that lockdown severity and fatality rates likely reflect the speed with which governments responded to the global outbreak of the virus (Bretschger et al. 2020).

[12] In robustness checks we also employ natural logarithms for the COVID-19 variables.

[13] We chose the level of 20 as the benchmark as it is close to the mean of the stringency index observed in the first quarter of 2020 for the country sample our analysis is based upon.

b) The life expectancy at birth as reported in 2018. The instrument is motivated by the fact that COVID-19 mortality rates increase substantially with age, making countries with higher life expectancy more vulnerable to the pandemic. Thus, countries with a higher life expectancy are likely to experience higher fatality rates and more stringent government measures.

c) Country size, measured by the natural logarithm of population size, as larger countries are likely to be more heterogeneous in terms of attitude and hence exhibit less of social cohesion (Anckar 2002, Gerring and Veenendaal 2020). This might make it more challenging to keep the pandemic under control creating the need for tighter measures and leading to higher fatality rates.

Second, we run equation (1) by employing robust fixed effects panel regressions where all time invariant country characteristics are accounted for.[14] The observation period runs from 2014 Q1 to 2020 Q2, with stringency and fatality set to zero for all quarters until 2020 Q1.[15] Thus, we test for the influence of the COVID-19 variables on GDP growth within countries for the first and second quarter 2020.

3. Results

Cross-country OLS regressions

Our baseline (Table 2) has three specifications for explaining cross-country differences in GDP growth in the first and second quarter 2020, respectively. Concretely, we account for lockdown severity (columns 1 and 2) and the fatality rate (columns 3 and 4) only, before running a specification that includes both variables as well as all controls (columns 5 and 6). Results show that as stand-alone variables, stringency index and fatality rate drive cross-country differences in GDP growth in the first and second quarter in the expected direction, i.e. a stricter government response and a higher fatality rate are associated with a lower level of economic activity.

When estimating a model including both COVID-19 variables and all controls, we find that the fatality rate loses significance in the first quarter, while the stringency index is significant in

[14] In doing so, we follow Barro, Ursúa and Weng (2020) when assessing the economic damage associated with fatality rates of the Spanish influenza at the end of the 1910s for 48 countries.
[15] The observation period begins 2014 in order to exclude the effects of the global financial and euro crisis on GDP developments. Again, our choice is influenced by Barro, Ursúa and Weng (2020) who end their observation period in 1929 for similar reasons, namely to exclude effects related to the Great Depression.

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both quarters. In terms of economic significance, the results for the second quarter suggest that the stringency index accounts for a GDP growth decline between five and ten percent, while fatality rates account for growth declines between basically zero, like in China, South Korea and Australia, and six to eight percent in countries with high fatality rates, such as Belgium, Spain, and the United Kingdom.

In addition, tourism exposure and – to lesser extent – trade openness account significantly for cross-country differences in GDP growth in the second quarter of Q2. The economic significance can be illustrated for Greece, the country with the highest tourism exposure in 2018 within our sample. Second quarter GDP growth was minus fifteen percent in Greece of which about nine percentage points are explained by the negative impact of mandatory and voluntary social distancing abroad via tourism exposure. As trade openness is very high in small economies, the negative impact via trade becomes economically significant in countries such as the Czech Republic, Estonia, Luxembourg and Ireland. The regression result implies that in these countries COVID-19 spillovers via trade account for up to 40% of the estimated negative growth effect related to variables capturing mandatory and social distancing at home and abroad. Finally, richer countries (second quarter) and countries with higher pre-crisis trend growth (first and second quarter) do relatively better than countries with a lower per capita income and lower pre-crisis growth.

Table 2: OLS Regressions

Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)
Growth rate	Q1	Q2	Q1	Q2	Q1	Q2
Stringency	-0.001***	-0.002***	-	-	-0.002**	-0.001*
	[0.000]	[0.001]	-	-	[0.001]	[0.001]
Fatality	-	-	-0.003***	-0.001**	-0.001	-0.001***
	-	-	[0.000]	[0.000]	[0.001]	[0.000]
Trade	-	-	-	-	-0.000	-0.000*
	-	-	-	-	[0.000]	[0.000]
Tourism	-	-	-	-	-0.000	-0.003***
	-	-	-	-	[0.000]	[0.001]
GDP p.c. (ln)	-	-	-	-	0.000	0.043**
	-	-	-	-	[0.005]	[0.021]
Trend growth	-	-	-	-	0.008***	0.009*
	-	-	-	-	[0.001]	[0.005]
Constant	0.019**	0.020	-0.002	-0.096***	0.012	-0.446*
	[0.008]	[0.038]	[0.004]	[0.010]	[0.059]	[0.234]
Adj. R^2	0.14	0.15	0.15	0.10	0.42	0.44
Countries	46	46	46	46	46	46
F-Statistic	11.85	10.21	45.30	5.14	10.96	7.96

Note: OLS estimations, robust standard errors. *, **, *** denote significance at 10, 5, and 1 percent levels, respectively. *Stringency* is the Oxford University Stringency Index mean value in the respective quarter. *Fatality* is the number of COVID-19 deaths per 100,000 inhabitants in the respective quarter. *Trade* is the sum of exports and imports divided by GDP in 2018. *Tourism* is the share of international tourism receipts in total exports in 2018. *GDP p.c. (ln)* is the ln(x+1) of GDP per capita in 2018. *Trend growth* is the mean average GDP growth rate over the period 2014-2019.

Instrumental variable regressions

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OLS regressions are subject to omitted variable bias and endogeneity concerns. Thus, we instrument stringency and fatality with the variables referred to in the previous section (Table 3). Results of the first stage regression show that the chosen instruments are valid, in particular for the second quarter. A slower response of the government and larger population size are associated with a significantly higher stringency index and fatality rate. Moreover, life expectancy, while insignificant in explaining the stringency index, is positively associated with the fatality rate.

For each quarter we run two IV-regressions: in the first one we exclude the COVID-19 variable we do not instrument, in the second regression we include the respective variable. Overall, this leads to eight regressions. Results show that the fatality rate is significant in five out of six specifications we account for the rate, while the stringency index is significant in three out of six specifications including the index.

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Table 3: Instrumental Variable Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	Q1		Q2		Q1		Q2	
Growth rate	**Second Stage**: Stringency (instrumented)				**Second Stage**: Fatality (instrumented)			
Stringency	-0.001**	-0.001	-0.005***	-0.004*	-	-0.001	-	0.000
	[0.001]	[0.001]	[0.002]	[0.002]	-	[0.001]	-	[0.001]
Fatality	-	-0.001**	-	-0.001	-0.005*	-0.004*	-0.003***	-0.003***
	-	[0.001]	-	[0.001]	[0.003]	[0.002]	[0.001]	[0.001]
Trade	-0.000	-0.000	-0.000	-0.000	0.000	-0.000	-0.000*	-0.000*
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Tourism	-0.000	0.000	-0.004***	-0.004***	0.000	0.000	-0.004***	-0.004***
	[0.000]	[0.000]	[0.001]	[0.001]	[0.001]	[0.000]	[0.001]	[0.001]
GDP p.c. (ln)	-0.002	0.000	-0.016	0.005	0.006	0.004	0.077***	0.079**
	[0.005]	[0.005]	[0.023]	[0.027]	[0.007]	[0.006]	[0.023]	[0.034]
Trend growth	0.008***	0.007***	0.012*	0.011*	0.004	0.006***	0.007	0.007
	[0.001]	[0.001]	[0.006]	[0.006]	[0.003]	[0.002]	[0.005]	[0.006]
Constant	0.025	-0.000	0.427	0.133	-0.072	-0.031	-0.831***	-0.854**
	[0.053]	[0.053]	[0.312]	[0.388]	[0.076]	[0.066]	[0.230]	[0.397]
Adj. R^2	0.397	0.407	-0.094	0.181	0.033	0.296	0.158	0.119
Countries	46	46	46	46	46	46	46	46
F-Statistic	7.032	6.666	7.373	5.289	1.809	1.723	6.652	3.369
Sargan (p-Value)	0.050	0.118	0.226	0.195	0.205	0.221	0.597	0.598
Wooldridge (p-Value)	0.216	0.204	0.022	0.114	0.204	0.211	0.015	0.038
	First Stage: Stringency (dependent variable)				**First Stage:** Fatality (dependent variable)			
Speed	-0.530***	-0.528***	0.397***	0.312***	-0.005	0.114	0.741***	0.626**
	[0.136]	[0.135]	[0.123]	[0.099]	[0.057]	[0.085]	[0.227]	[0.278]
Life expectancy	0.405	0.218	0.096	-0.123	0.437*	0.346*	1.901**	1.873**
	[0.289]	[0.266]	[0.507]	[0.605]	[0.246]	[0.198]	[0.886]	[0.869]
Population (ln)	0.335	-0.102	4.170***	3.332**	1.022**	0.946**	7.297***	6.096***
	[0.478]	[0.516]	[1.065]	[1.464]	[0.455]	[0.448]	[1.747]	[2.123]
Constant	42.485	58.064**	64.827	99.823**	-36.383**	-45.957**	-304.553***	-323.233***
	[25.795]	[27.178]	[41.424]	[47.206]	[15.359]	[18.541]	[70.429]	[68.691]
Adj. R^2	0.616	0.643	0.343	0.347	0.102	0.167	0.305	0.310
Countries	46	46	46	46	46	46	46	46

Note: *Speed* represents the number of days starting from 1 January until mandatory measures were imposed by the governments exceeds 20 (i.e.. 15 March 2020 = 74). *Life expectancy* is the mean value of total years at birth. *Population (ln)* is the ln(x+1) of the total population in 2018. For further explanations see Table 2.

For the second quarter results strongly diverge. The fatality rate is insignificant when instrumenting the stringency index (column 4), while the stringency index fails to make a significant contribution in explaining GDP growth when instrumenting the fatality rate (column 8). Indeed, the only robust effect identified by the IV-regressions for the second quarter refers to tourism exposure. As this is a variable that captures the vulnerability of countries to mandatory and social distancing abroad, i.e. to the global pandemic, IV-regressions provide inconclusive results with regard to the growth effects of mandatory and voluntary social distancing at home for the second quarter. By contrast, the fatality rate is the COVID-19 variable which drives cross-country differences in the first quarter.

Panel regressions

Finally, we run panel fixed effects regressions in order to focus on the time dimension of the pandemic's impact on growth. Results show that changes in the stringency index over time drive GDP growth as the fatality rate fails being significant when accounting for both COVID-19 variables (Table 4).

Table 4: Panel Fixed Effects Regressions

Dep. variable: *Growth rate*	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Stringency	-0.002***	-	-0.002***	-0.002***	-	-0.002***	-0.002***	-	-0.002***
	[0.000]	-	[0.000]	[0.000]	-	[0.000]	[0.001]	-	[0.001]
Fatality	-	-0.005***	-0.000	-	-0.004***	-0.000	-	-0.001	-0.000
	-	[0.001]	[0.000]	-	[0.001]	[0.000]	-	[0.000]	[0.000]
2020-Q1	-	-	-	-	-	-	0.018	-0.025***	0.018
	-	-	-	-	-	-	[0.012]	[0.004]	[0.012]
2020-Q2	-	-	-	-	-	-	0.010	-0.133***	0.010
	-	-	-	-	-	-	[0.035]	[0.013]	[0.036]
Constant	0.030***	0.025***	0.030***	0.032***	0.026***	0.032***	0.021***	0.021***	0.021***
	[0.000]	[0.000]	[0.000]	[0.001]	[0.001]	[0.001]	[0.002]	[0.002]	[0.002]
Fixed Effects		No			Seasonal			Quarter	
Countries	46	46	46	46	46	46	46	46	46
R^2 (within)	0.63	0.28	0.63	0.63	0.30	0.63	0.65	0.63	0.65
R^2 (overall)	0.48	0.23	0.48	0.48	0.24	0.49	0.50	0.49	0.50
R^2 (between)	0.01	0.05	0.00	0.01	0.05	0.00	0.01	0.05	0.00
Rho (inter. corr)	0.45	0.28	0.45	0.45	0.29	0.45	0.46	0.44	0.46
F-Statistic	254.32	49.94	168.81	82.44	39.16	81.38	43.46	45.84	48.99

Note: Fixed effects model. Robust standard errors. *, **, *** denote significance at 10, 5, and 1 percent levels, respectively. Observation period for *Growth rate* begins in 2014 Q1 (Barro et al., 2020), *Stringency* and *Fatality* is equal to zero until 2020-Q1. Specification 1-3 is a fixed effects model without fixed effects. Specification 4-6 includes seasonal (Q1-Q4) fixed effects with base season Q1. Specification 7-9 includes quarter fixed effects (2014-Q1 to 2020-Q2) with base quarter 2019 Q4.

This holds irrespective of whether we refrain from including time fixed effects (columns 1-3), accounting for seasonal fixed effects (columns 4-6) and including quarter fixed effects (columns 7-9). With regard to the latter, it is interesting to note that the quarter fixed effects for the first and second quarter 2020 are insignificant when accounting for the stringency index. Thus, the divergence in growth in the first half of 2020 from the long-term average of 2.1 percent is only explained by changes in the stringency index itself. Overall, panel regression results show that over time mandatory social distancing associated with government-imposed measures drive GDP developments while voluntary social distancing reflecting changes in the fatality rate has no significant impact on growth developments over time.

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4. Robustness checks

We run a battery of robustness checks. First, we employ the stringency index and the fatality rate in natural log form (ln (x+1)). Results (Tables A3-A5) are broadly in line with the baseline. However, in the second quarter IV regression the instrumented stringency index turns insignificant when accounting for the fatality rate (Table A4, column 4). As the fatality rate itself is insignificant as well, the specification implies that GDP growth differences in the second quarter on 2020 are driven by mandatory and voluntary social distancing abroad, only.

We continue by replacing the stringency index value by a variable named "Hard lockdown" which represents the percentage of days within a quarter the stringency index is above 80. The choice of this variable is motivated by the idea that hard lockdowns are likely to enforce social distancing behaviour that goes substantially beyond voluntary social distancing to be observed with very high fatality rates. Results (Tables A6-A8) are again largely in line with the baseline for the OLS and the IV estimations. In the panel regression, however, the fatality rate remains significant in specifications that do not account for time fixed effects proper (Table A8, columns 1-6). However, as in the baseline, the fatality rate turns insignificant when time fixed effects are included.

Our results are also robust when replacing in the OLS regression with all controls the overall stringency index as a proxy of mandatory social distancing with various sub-indices the stringency index is built upon (Table A9). Results show that in the second quarter all sub-indices are insignificant. We interpret this as indicating that it is the sum of the various measures taken which exerts the negative impact on GDP developments. Moreover, we follow the example of IMF (2020) and replace GDP as the outcome variable with mobility data capturing changes in visits to places, such as retail and grocery stores as well as workplace (Table A10). When including the fatality rate results indicate that in the second quarter the stringency index and the fatality rate show strongly significant and negative effects on mobility.

Finally, given the results of the Hausman test (Table A11) we run a random effects panel model. Results (Table A12) are broadly in line with those of the fixed effects panel specifications (Table 4). Most importantly, the fatality rate remains insignificant in all specifications including the stringency index. Moreover, tourism exposure rather than trade openness is the significant variable capturing vulnerabilities created by mandatory and voluntary distancing abroad.

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5. Conclusions

This paper presents early evidence on the impact of mandatory and voluntary distancing related to the COVID-19 pandemic on GDP developments in 46 countries. We do so based on GDP data for the first and second quarter 2020 and by capturing the degree of mandatory distancing by the Oxford University Stringency Index and voluntary distancing by the COVID-19 fatality rate. Making use of OLS, IV and panel fixed effects regressions we find that changes in mandatory distancing enforced by governments, i.e. different degrees of lockdown stringency, are the important driver of GDP developments over the first half of 2020. By contrast, OLS and IV regressions for the individual quarters suggest that cross-country differences in GDP developments are also influenced by voluntary distancing reflecting differences in health risks expressed by the fatality rate. Moreover, we find that country vulnerabilities to mandatory and voluntary social distancing conducted abroad, proxied by tourism exposure and trade openness, matter.

Our results are robust to changes in the variables used to measure lockdown severity and COVID-19 health risks as well as economic activity. Accordingly, given the current rise in infections our results provide support for voices arguing that all efforts should be undertaken to avoid hard lockdowns as lockdown intensity drove the downturn in the first half of 2020. At the same time, our cross-country results also suggest that high fatality rates trigger voluntary social distancing with strongly negative growth effects. Given that the economic implications of lockdowns are directly felt by the parties affected, i.e. by restaurant owners, artists etc., the results serve as a reminder for policymakers that lockdowns – despite these direct effects – might still serve as a useful *economic* policy instrument if lockdowns reduce the likelihood of health risks getting out of control and thereby contain the more evenly spread effects of voluntary social distancing. Testing for such effects will become a key issue as more quarterly data for the respective COVID-19 variables and growth become available.

We conclude by noting that our results on growth effects of mandatory and voluntary social distancing triggered by COVID-19 are based on evidence for two quarters only. Thus, like for many other studies on the impact of COVID-19 on economic activity, they will need to be reexamined when new data becomes available.

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Annex

Table A1: Country Data 2020

Country	Quarter 1			Quarter 2			Country	Quarter 1			Quarter 2		
	Fatality	Stringency	Growth	Fatality	Stringency	Growth		Fatality	Stringency	Growth	Fatality	Stringency	Growth
Argentina	0.05	22.25	-0.05	2.83	92.59	-0.20	Ireland	1.12	15.30	0.04	34.88	81.85	-0.04
Australia	0.08	20.06	0.02	0.34	64.08	-0.06	Iceland	0.59	18.60	-0.01	2.38	44.35	-0.11
Austria	1.21	19.41	-0.03	6.69	61.87	-0.13	Israel	0.20	25.68	0.01	3.60	80.45	-0.08
Belgium	10.17	20.61	-0.02	73.67	71.75	-0.14	Italy	19.12	39.52	-0.06	38.19	70.36	-0.18
Bulgaria	0.11	18.98	0.02	3.05	57.69	-0.08	Japan	0.04	21.73	-0.02	0.72	37.90	-0.10
Brazil	0.08	16.94	-0.01	27.76	77.65	-0.11	South Korea	0.32	32.88	0.01	0.23	55.64	-0.03
Canada	0.24	14.51	-0.01	22.86	71.53	-0.13	Lithuania	0.25	17.93	0.02	2.53	65.99	-0.04
Switzerland	3.46	17.95	-0.01	16.26	62.32	-0.09	Luxembourg	3.64	17.01	0.00	14.56	57.26	-0.08
Chile	0.04	11.14	0.00	29.72	75.99	-0.14	Latvia	0.00	14.41	-0.02	1.56	58.69	-0.09
China	0.23	58.98	-0.07	0.09	71.78	0.03	Mexico	0.02	6.85	-0.02	21.47	78.82	-0.19
Colombia	0.03	18.76	0.01	6.46	87.23	-0.15	Netherlands	5.06	15.08	0.00	30.73	70.92	-0.09
Czech Republic	0,23	25.89	-0.02	3.04	54.25	-0.11	Norway	0.49	19.57	0.00	4.18	56.74	-0.05
Germany	0.70	20.34	-0.02	10.09	67.30	-0.11	New Zealand	0.02	22.88	0.00	0.44	58.50	-0.12
Denmark	1.34	18.29	0.00	9.18	63.87	-0.08	Poland	0.08	16.63	0.02	3.73	72.42	-0.08
Spain	15.72	21.29	-0.04	45.86	72.51	-0.22	Portugal	1.37	20.43	-0.02	13.92	76.16	-0.16
Estonia	0.23	11.66	0.00	4.54	57.72	-0.06	Romania	0.23	20.62	0.03	8.15	72.09	-0.11
Finland	0.24	18.44	-0.01	5.70	49.56	-0.06	Russia	0.01	18.92	0.00	6.28	80.09	-0.06
France	4.65	28.73	-0.06	41.22	75.23	-0.19	Slovakia	0.00	18.88	-0.04	0.51	64.89	-0.12
United Kingdom	3,05	16.75	-0.02	57.03	74.90	-0.22	Slovenia	0.53	15.69	-0.04	4.81	60.84	-0.13
Greece	0.41	19.32	-0.01	1.41	67.76	-0.15	Sweden	2.90	6.17	0.01	52.08	44.69	-0.08
Hungary	0.16	16.77	0.02	5.86	66.65	-0.14	Turkey	0.20	20.42	0.04	6.01	72.04	-0.09
Indonesia	0.05	25.12	0.03	1.00	69.63	-0.05	United States	0.97	17.30	0.00	37.59	72.04	-0.09
India	0.00	22.62	0.03	1.25	85.63	-0.23	South Africa	0.01	14.56	0.00	4.37	83.32	-0.17

Note: *Stringency* represents non-weighted mean average. *Fatality* is the number of COVID-19 deaths per 100,000 inhabitants. *Growth* rate of real GDP represents the change in real GDP over the same quarter in the previous year.

Table A2: Pairwise Correlation Matrix

	Q1 Growth rate	Q2 Growth rate	Q1 Stringency	Q2 Stringency	Q1 Fatality	Q2 Fatality	Trade	Tourism	GDP p.c. (ln)
Q1 Growth rate	1								
Q2 Growth rate	0.304^{*}	1							
Q1 Stringency	-0.399^{**}	0.226	1						
Q2 Stringency	0.0107	-0.406^{**}	0.0604	1					
Q1 Fatality	-0.407^{**}	-0.357^{*}	0.239	0.0517	1				
Q2 Fatality	-0.210	-0.345^{*}	-0.163	0.172	0.630^{***}	1			
Trade	0.0988	0.191	-0.239	-0.257	0.0781	0.0534	1		
Tourism	0.0137	-0.277	-0.0535	-0.0817	0.0384	-0.161	-0.212	1	
GDP p.c. (ln)	-0.0841	0.277	-0.154	-0.497^{***}	0.241	0.305^{*}	0.473^{***}	-0.0306	1
Trend growth	0.352^{*}	0.293^{*}	0.282	-0.0865	-0.196	-0.304^{*}	0.186	0.0169	-0.216

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A3: Robustness check – Baseline OLS with logarithmic COVID-19 variables

Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)
Growth rate	Q1	Q2	Q1	Q2	Q1	Q2
Stringency (ln)	-0.023*	-0.115***	-	-	-0.025	-0.040
	[0.013]	[0.041]	-	-	[0.015]	[0.046]
Fatality (ln)	-	-	-0.013***	-0.017**	-0.010***	-0.021***
	-	-	[0.004]	[0.008]	[0.003]	[0.007]
Trade	-	-	-	-	-0.000	-0.000
	-	-	-	-	[0.000]	[0.000]
Tourism	-	-	-	-	0.000	-0.003***
	-	-	-	-	[0.001]	[0.001]
GDP p.c. (ln)	-	-	-	-	0.004	0.048**
	-	-	-	-	[0.006]	[0.022]
Trend growth	-	-	-	-	0.006***	0.008
	-	-	-	-	[0.002]	[0.005]
Constant	0.062	0.375**	0.001	-0.076***	0.017	-0.379
	[0.038]	[0.169]	[0.005]	[0.018]	[0.080]	[0.351]
Adj. R^2	0.07	0.12	0.13	0.11	0.32	0.42
Countries	46	46	46	46	46	46
F-Statistic	3.16	7.99	12.27	5.03	9.70	7.01

Note: *Stringency* and *Fatality* expressed as ln(x+1). For further explanations see Table 2.

Table A4: Robustness check – IV Baseline with logarithmic COVID-19 variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	Q1		Q2		Q1		Q2	
Growth rate	**Second Stage**: Stringency (instrumented)				**Second Stage**: Fatality (instrumented)			
Stringency (ln)	-0.033	-0.028	-0.338***	-0.220	-	-0.023	-	0.077
	[0.021]	[0.021]	[0.115]	[0.184]	-	[0.015]	-	[0.107]
Fatality (ln)	-	-0.009***	-	-0.008	-0.022**	-0.014	-0.046***	-0.055***
	-	[0.003]	-	[0.016]	[0.011]	[0.010]	[0.011]	[0.020]
Trade	-0.000	-0.000	-0.000	-0.000	0.000	-0.000	-0.000	-0.000
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Tourism	0.000	0.000	-0.004**	-0.003***	0.000	0.000	-0.004***	-0.004***
	[0.001]	[0.000]	[0.001]	[0.001]	[0.001]	[0.000]	[0.001]	[0.001]
GDP p.c. (ln)	-0.001	0.004	-0.014	0.010	0.011	0.007	0.072***	0.091**
	[0.006]	[0.006]	[0.023]	[0.039]	[0.010]	[0.008]	[0.024]	[0.037]
Trend growth	0.007***	0.006***	0.012*	0.010*	0.005	0.006***	0.005	0.003
	[0.002]	[0.002]	[0.006]	[0.005]	[0.003]	[0.002]	[0.006]	[0.007]
Constant	0.092	0.024	1.471**	0.741	-0.122	-0.015	-0.730***	-1.222
	[0.082]	[0.087]	[0.630]	[1.097]	[0.095]	[0.104]	[0.228]	[0.748]
Adj. R^2	0.265	0.315	.	0.193	0.152	0.300	0.216	0.043
Countries	46	46	46	46	46	46	46	46
F-Statistic	12.883	11.473	6.527	2.064	3.457	3.354	9.242	3.797
Sargan (p-Value)	0.046	0.118	0.172	0.123	0.101	0.186	0.716	0.874
Wooldridge (p-Value)	0.763	0.843	0.023	0.224	0.299	0.597	0.020	0.027
	First Stage: Stringency (dependent variable)				**First Stage:** Fatality (dependent variable)			
Speed	-0.022***	-0.022***	0.006***	0.003	0.002	0.011	0.059***	0.047***
	[0.005]	[0.005]	[0.002]	[0.002]	[0.010]	[0.014]	[0.015]	[0.017]
Life expectancy	0.010	0.006	0.000	-0.005	0.083*	0.079*	0.093*	0.092**
	[0.012]	[0.012]	[0.008]	[0.009]	[0.043]	[0.042]	[0.049]	[0.044]
Population (ln)	0.007	-0.007	0.064***	0.039	0.243***	0.240***	0.462***	0.325**
	[0.029]	[0.035]	[0.017]	[0.025]	[0.078]	[0.077]	[0.102]	[0.124]
Constant	3.940***	4.562***	4.145***	5.155***	-11.025***	-12.603***	-18.374***	-27.191***
	[1.073]	[1.218]	[0.610]	[0.822]	[2.833]	[3.656]	[4.722]	[5.781]
Adj. R^2	0.498	0.496	0.295	0.361	0.293	0.290	0.369	0.428
Countries	46	46	46	46	46	46	46	46

Note: *Stringency* and *Fatality* expressed as ln(x+1). For further explanations see Table 2.

Table A5: Robustness check – Panel Regressions with logarithmic COVID-19 variables

Dependent variable: *Growth rate*	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Stringency (ln)	-0.028***	-	-0.018***	-0.028***	-	-0.019***	-0.058***	-	-0.053***
	[0.002]	-	[0.002]	[0.002]	-	[0.002]	[0.016]	-	[0.017]
Fatality (ln)	-	-0.054***	-0.025***	-	-0.052***	-0.023***	-	-0.011*	-0.008
	-	[0.004]	[0.004]	-	[0.004]	[0.004]	-	[0.005]	[0.005]
2020-Q1				-	-	-	0.147***	-0.020***	0.137***
				-	-	-	[0.048]	[0.004]	[0.050]
2020-Q2				-	-	-	0.103	-0.120***	0.099
				-	-	-	[0.064]	[0.016]	[0.067]
Constant	0.030***	0.028***	0.030***	0.037***	0.030***	0.035***	0.021***	0.021***	0.021***
	[0.000]	[0.000]	[0.000]	[0.001]	[0.001]	[0.001]	[0.002]	[0.002]	[0.002]
Fixed Effects		No			Seasonal			Quarter	
Countries	46	46	46	46	46	46	46	46	46
R^2 (within)	0.53	0.48	0.57	0.54	0.49	0.57	0.64	0.63	0.64
R^2 (overall)	0.40	0.38	0.44	0.41	0.39	0.45	0.49	0.49	0.49
R^2 (between)	0.01	0.06	0.05	0.01	0.06	0.05	0.01	0.06	0.00
Rho (inter. cor.)	0.39	0.35	0.40	0.39	0.35	0.40	0.45	0.44	0.45
F-Statistic	258.26	141.72	123.49	79.69	43.58	65.35	45.06	46.45	53.88

Note: Robust fixed effects panel regression.. *Stringency* and *Fatality* expressed as ln(x+1). For further explanations see Table 4.

Table A6: Robustness check – OLS with Hard Lockdown

Dependent Variable: *Growth rate*	(1) Q1	(2) Q2	(3) Q1	(4) Q2
Hard lockdown	-0.131*	-0.071**	-0.168**	-0.039*
	[0.066]	[0.027]	[0.079]	[0.022]
Fatality	-0.002***	-0.001**	-0.001	-0.001***
	[0.001]	[0.000]	[0.001]	[0.000]
Trade	-	-	-0.000	-0.000*
	-	-	[0.000]	[0.000]
Tourism	-	-	0.000	-0.003***
	-	-	[0.001]	[0.001]
GDP p.c. (ln)	-	-	-0.005	0.047**
	-	-	[0.005]	[0.022]
Trend growth	-	-	0.006***	0.009*
	-	-	[0.002]	[0.005]
Constant	0.005	-0.079***	0.035	-0.547**
	[0.004]	[0.009]	[0.055]	[0.226]
Adj. R^2	0.24	0.22	0.38	0.44
Countries	46.00	46.00	46.00	46.00
F-Statistic	18.09	6.14	7.32	9.71

Note: *Hard lockdown* is value between 0 and 1. It is calculated by amount of days when stringency is above 80 divided by total days in respective quarter (i.e. 50 days out of 90 days the stringency is greater than 80 lead to 0.55 (50 divided by 90). For further explanations see Table 2.

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Table A7: Robustness check – IV with Hard Lockdown

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	Q1		Q2		Q1		Q2	
Growth rate	**Second Stage**: Stringency (instrumented)				**Second Stage**: Fatality (instrumented)			
Hard lockdown	-0.420***	-0.449***	-0.440*	-0.389	-	-0.121	-	-0.018
	[0.144]	[0.164]	[0.252]	[0.298]	-	[0.087]	-	[0.030]
Fatality	-	0.001	-	-0.000	-0.005*	-0.003	-0.003***	-0.003***
	-	[0.002]	-	[0.001]	[0.003]	[0.003]	[0.001]	[0.001]
Trade	-0.000	-0.000	-0.000	-0.000	0.000	0.000	-0.000*	-0.000*
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Tourism	0.001	0.001	-0.001	-0.001	0.000	0.000	-0.004***	-0.004***
	[0.001]	[0.001]	[0.002]	[0.002]	[0.001]	[0.001]	[0.001]	[0.001]
GDP p.c. (ln)	-0.012*	-0.015	-0.074	-0.056	0.006	-0.000	0.077***	0.071**
	[0.007]	[0.009]	[0.080]	[0.088]	[0.007]	[0.007]	[0.023]	[0.028]
Trend growth	0.007***	0.007***	0.014	0.013	0.004	0.005*	0.007	0.007
	[0.002]	[0.002]	[0.014]	[0.013]	[0.003]	[0.003]	[0.005]	[0.006]
Constant	0.125	0.147	0.742	0.558	-0.072	-0.008	-0.831***	-0.766***
	[0.081]	[0.096]	[0.883]	[0.953]	[0.076]	[0.075]	[0.230]	[0.282]
Adj. R^2	-0.006	-0.119	.	.	0.033	0.298	0.158	0.187
Countries	46	46	46	46	46	46	46	46
Adj. R^2	-0.006	-0.119	.	.	0.033	0.298	0.158	0.187
F-Statistic	1.402	1.259	1.141	0.560	1.809	1.298	6.652	6.002
Sargan (p-Value)	0.996	0.947	0.994	0.999	0.205	0.276	0.597	0.604
Wooldridge (p-Value)	0.102	0.139	0.008	0.028	0.204	0.398	0.015	0.026
	First Stage: Stringency (dependent variable)				**First Stage:** Fatality (dependent variable)			
Speed	-0.001	-0.001	0.007	0.006	-0.005	0.018	0.741***	0.716***
	[0.001]	[0.001]	[0.004]	[0.005]	[0.057]	[0.044]	[0.227]	[0.241]
Life expectancy	0.006*	0.004	0.014	0.012	0.437*	0.317	1.901**	1.850**
	[0.004]	[0.003]	[0.020]	[0.021]	[0.246]	[0.216]	[0.886]	[0.875]
Population (ln)	0.003	-0.003	0.043	0.035	1.022**	0.962*	7.297***	7.136***
	[0.008]	[0.008]	[0.030]	[0.036]	[0.455]	[0.491]	[1.747]	[1.776]
Constant	0.292	0.520	1.497	1.846	-36.383**	-41.774**	-304.553***	-310.127***
	[0.308]	[0.315]	[1.540]	[1.621]	[15.359]	[16.164]	[70.429]	[71.944]
Adj. R^2	0.020	0.110	0.156	0.137	0.102	0.185	0.305	0.290
Countries	46	46	46	46	46	46	46	46

Note: For further explanations see Table 2.

Table A8: Robustness check – Panel regression with Hard Lockdown

Dep. variable: *Growth rate*	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Hard lockdown	-0.329***	-	-0.259***	-0.318***	-	-0.254***	-0.115***	-	-0.114***
	[0.033]	-	[0.035]	[0.033]	-	[0.035]	[0.036]	-	[0.036]
Fatality	-	-0.005***	-0.003***	-	-0.004***	-0.003***	-	-0.001	-0.001
	-	[0.001]	[0.001]	-	[0.001]	[0.001]	-	[0.000]	[0.000]
2020-Q1	-	-	-	-	-	-	-0.020***	-0.025***	-0.019***
	-	-	-	-	-	-	[0.004]	[0.004]	[0.004]
2020-Q2	-	-	-	-	-	-	-0.114***	-0.133***	-0.105***
	-	-	-	-	-	-	[0.009]	[0.013]	[0.011]
Constant	0.026***	0.025***	0.027***	0.028***	0.026***	0.028***	0.021***	0.021***	0.021***
	[0.000]	[0.000]	[0.000]	[0.001]	[0.001]	[0.001]	[0.002]	[0.002]	[0.002]
Fixed Effects		No			Seasonal			Quarter	
Countries	46	46	46	46	46	46	46	46	46
R^2 (within)	0.40	0.28	0.49	0.42	0.30	0.50	0.66	0.63	0.66
R^2 (overall)	0.30	0.23	0.38	0.31	0.24	0.38	0.50	0.49	0.51
R^2 (between)	0.00	0.05	0.01	0.00	0.05	0.01	0.00	0.05	0.00
Rho (inter. corr)	0.34	0.28	0.37	0.34	0.29	0.37	0.46	0.44	0.46
F-Statistic	99.75	49.94	58.46	49.84	39.16	43.86	52.28	45.84	57.63

Note: Robust fixed effects panel regression. For further explanations see Table 4.

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Table A9: Robustness check – Stringency Indicator

Dep. variable: *Growth rate*	(1) Q1	(2) Q2	(3) Q1	(4) Q2	(5) Q1	(6) Q2	(7) Q1	(8) Q2	(9) Q1	(10) Q2	(11) Q1	(12) Q2
Fatality	-0.001	-0.002***	-0.002***	-0.002***	-0.001**	-0.001***	-0.001	-0.002***	-0.002***	-0.001***	-0.002***	-0.002***
	[0.001]	[0.000]	[0.000]	[0.000]	[0.001]	[0.000]	[0.001]	[0.000]	[0.001]	[0.000]	[0.000]	[0.000]
Gather restrictions	-0.028***	0.000	-	-	-	-	-	-	-	-	-	-
	[0.008]	[0.005]	-	-	-	-	-	-	-	-	-	-
Close public transport	-	-	-0.071***	-0.018	-	-	-	-	-	-	-	-
	-	-	[0.010]	[0.014]	-	-	-	-	-	-	-	-
Restr. internal move	-	-	-	-	-0.045**	-0.010	-	-	-	-	-	-
	-	-	-	-	[0.021]	[0.010]	-	-	-	-	-	-
Stay at home reqs.	-	-	-	-	-	-	-0.041***	0.001	-	-	-	-
	-	-	-	-	-	-	[0.013]	[0.013]	-	-	-	-
Cancel pub. events	-	-	-	-	-	-	-	-	-0.039	-0.019	-	-
	-	-	-	-	-	-	-	-	[0.026]	[0.021]	-	-
Intl. travel controls	-	-	-	-	-	-	-	-	-	-	0.004	0.005
	-	-	-	-	-	-	-	-	-	-	[0.006]	[0.011]
Trade	-0.000	-0.000*	-0.000	-0.000	-0.000	-0.000*	-0.000	-0.000*	-0.000	-0.000*	0.000	-0.000
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Tourism	-0.000	-0.003***	-0.000	-0.003***	-0.000	-0.003***	-0.000	-0.003***	-0.000	-0.003***	0.000	-0.003***
	[0.000]	[0.001]	[0.000]	[0.001]	[0.000]	[0.001]	[0.000]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
GDP p.c. (ln)	0.002	0.058***	-0.004	0.048***	0.000	0.055***	-0.005	0.059**	-0.000	0.054***	0.000	0.059***
	[0.006]	[0.019]	[0.005]	[0.016]	[0.006]	[0.019]	[0.006]	[0.022]	[0.006]	[0.020]	[0.006]	[0.019]
Trend growth	0.007***	0.009*	0.008***	0.009*	0.006***	0.009*	0.008***	0.009*	0.007***	0.009*	0.005	0.009*
	[0.001]	[0.005]	[0.001]	[0.005]	[0.002]	[0.005]	[0.001]	[0.005]	[0.002]	[0.005]	[0.003]	[0.005]
Constant	-0.023	-0.673***	0.028	-0.556***	-0.006	-0.626***	0.043	-0.683***	0.001	-0.600***	-0.030	-0.701***
	[0.056]	[0.191]	[0.055]	[0.164]	[0.058]	[0.194]	[0.058]	[0.236]	[0.061]	[0.213]	[0.062]	[0.213]
Adj. R^2	0.48	0.40	0.55	0.43	0.37	0.41	0.44	0.40	0.33	0.41	0.22	0.41
Countries	46.00	46.00	46.00	46.00	46.00	46.00	46.00	46.00	46.00	46.00	46.00	46.00
F-Statistic	14.24	8.00	22.35	7.49	10.14	7.35	13.04	8.20	12.47	8.31	11.56	7.78

Note: *Gather restrictions* records the cut-off size for bans on private gatherings (Ordinal scale between 0 (no restrictions) and 4 (less than 10 people). *Close public transport* refers to the closing of public transport (Ordinal scale between 0 (no measures) and 2 (required closing). *Restr. of internal move* measures restrictions on internal movement (Ordinal scale between 0 (no measures) and 2 (restrictions in place), *Stay at home reqs.* captures orders to "shelter-in- place" and otherwise confine to home (Ordinal scale between 0 (no measures) and 3 (require not leaving the house with minimal exceptions), and *Intl. travel controls* refer to restrictions on international travel (Ordinal scale between 0 (no measure) and 4 (ban on all regions or total border close). All measures represents mean values in the respective quarter. For further information see Oxford Working Paper BSG-WP-2020/032 Version 7 Variation in government responses to COVID-19, September 2020 (Hale et al., 2020). For further explanations see Table 2.

Table A10: Robustness check – Individual Mobility Change

	(1) Q1	(2) Q2	(3) Q1	(4) Q2	(5) Q1	(6) Q2	(7) Q1	(8) Q2	(9) Q1	(10) Q2	(11) Q1	(12) Q2
Dependent variable:	Retail change				Grocery change				Workplace change			
Stringency	-0.785***	-1.066***	-0.543***	-0.921***	-0.381**	-0.566***	-0.170*	-0.456***	-0.431*	-0.619***	-0.193	-0.483***
	[0.167]	[0.205]	[0.125]	[0.229]	[0.172]	[0.121]	[0.089]	[0.119]	[0.221]	[0.116]	[0.177]	[0.103]
Fatality	-	-	-0.847***	-0.226**	-	-	-0.736***	-0.172**	-	-	-0.833***	-0.211***
	-	-	[0.158]	[0.106]	-	-	[0.135]	[0.080]	-	-	[0.197]	[0.066]
Trade	-0.078***	-0.044	-0.070***	-0.050	-0.040***	-0.051	-0.033***	-0.056	-0.057***	-0.020	-0.049***	-0.025
	[0.018]	[0.040]	[0.016]	[0.043]	[0.012]	[0.032]	[0.010]	[0.034]	[0.018]	[0.023]	[0.015]	[0.025]
Tourism	-0.342*	-0.502	-0.303**	-0.607**	-0.100	-0.371	-0.066	-0.452	-0.126	-0.285	-0.087	-0.384**
	[0.177]	[0.309]	[0.143]	[0.291]	[0.149]	[0.466]	[0.108]	[0.452]	[0.231]	[0.204]	[0.222]	[0.183]
GDP p.c. (ln)	1.984	2.283	3.171	6.435	4.618***	5.248*	5.649***	8.408***	-2.141	-4.502**	-0.974	-0.616
	[2.133]	[4.087]	[2.082]	[4.024]	[1.431]	[2.612]	[1.361]	[2.859]	[2.100]	[2.136]	[2.124]	[2.246]
Trend growth	0.832	0.097	0.487	-0.160	0.311	0.622	0.011	0.427	0.572	-0.326	0.233	-0.567
	[0.563]	[0.919]	[0.538]	[0.937]	[0.343]	[0.763]	[0.313]	[0.826]	[0.504]	[0.507]	[0.422]	[0.485]
Constant	-17.414	16.128	-32.999	-31.278	-41.878***	-23.241	-55.420***	-59.316*	20.269	59.264**	4.944	14.900
	[22.301]	[49.154]	[21.685]	[49.402]	[14.837]	[30.397]	[14.085]	[33.217]	[21.753]	[24.827]	[22.368]	[24.656]
Adj. R^2	0.37	0.51	0.49	0.55	0.19	0.32	0.39	0.36	0.27	0.41	0.43	0.54
Countries	44.00	44.00	44.00	44.00	44.00	44.00	44.00	44.00	44.00	44.00	44.00	44.00
F-Statistic	8.28	8.59	24.85	6.97	4.13	7.66	12.52	6.77	5.50	6.78	22.05	6.89

Note: *Retail change* compared to baseline (the median value for the corresponding day of the week during the 5-week period 3 January and 6 February 2020) in percentage (For places like restaurants, cafes, shopping centers, theme parks, museums, libraries, and movie theatres). *Grocery change* compared to baseline in percentage (For places like grocery markets, food warehouses, farmers markets, specialty food shops, drug stores, and pharmacies). *Workplace change* compared to baseline in percentage (For places of work). Further explanations see Table 2.

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Table A11: Hausman test

	(b) Fixed	(B) Random	(b-B) Difference	sqrt(diag(V_b-V_B)) S.E.
Stringency	-0.0021151	-0.0021121	-0.0000029	0.0000050
Fatality	-0.0003918	-0.0004085	0.0000167	0.0000232

b = consistent under Ho and Ha; obtained from STATA command xtreg

B = inconsistent under Ha, efficient under Ho; obtained from STATA command xtreg

Test: Ho: difference in coefficients not systematic

chi2(2) = (b-B)'[(V_b-V_B)^(-1)](b-B)
chi2(2) = 0.55
Prob>chi2 = **0.7583**

Note: The Null Hypothesis assumes that there is no correlation. Results: Prob>chi2 is greater than 0.05. We fail to reject the Null Hypothesis and accept that there is no correlation between residuals and and regressors.

Table A12: Robustness check – Random Effects Model

Dependent variable: *Growth rate*	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Stringency	-0.002***	-	-0.002***	-0.002***	-	-0.002***	-0.002***	-	-0.002***
	[0.000]	-	[0.000]	[0.000]	-	[0.000]	[0.001]	-	[0.001]
Fatality	-	-0.004***	-0.000	-	-0.004***	-0.000	-	-0.001	-0.000
	-	[0.001]	[0.000]	-	[0.001]	[0.000]	-	[0.000]	[0.000]
Tourism	-0.000*	-0.000**	-0.000**	-0.000*	-0.000**	-0.000**	-0.000*	-0.000*	-0.000*
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Trade	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
GDP p.c. (ln)	-0.000	0.003	-0.000	-0.000	0.003	-0.000	-0.000	0.001	-0.000
	[0.002]	[0.002]	[0.002]	[0.002]	[0.002]	[0.002]	[0.002]	[0.002]	[0.002]
Trend growth	0.010***	0.010***	0.010***	0.010***	0.010***	0.010***	0.010***	0.010***	0.010***
	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
2020-Q1	-	-	-	-	-	-	0.019	-0.025***	0.018
	-	-	-	-	-	-	[0.012]	[0.004]	[0.012]
2020-Q2	-	-	-	-	-	-	0.011	-0.133***	0.011
	-	-	-	-	-	-	[0.034]	[0.013]	[0.035]
Constant	0.000	-0.030	-0.013	0.009	-0.027	0.006	-0.002	-0.014	-0.004
	[0.022]	[0.022]	[0.021]	[0.022]	[0.022]	[0.021]	[0.021]	[0.022]	[0.021]
Fixed Effects		No			Seasonal			Quarter	
Countries	46	46	46	46	46	46	46	46	46
R^2 (within)	0.63	0.28	0.63	0.63	0.30	0.63	0.65	0.63	0.65
R^2 (overall)	0.69	0.42	0.69	0.69	0.43	0.69	0.70	0.68	0.70
R^2 (between)	0.87	0.86	0.87	0.87	0.86	0.87	0.87	0.86	0.87
Rho (inter. corr)	0.08	0.02	0.08	0.08	0.02	0.08	0.08	0.07	0.08
Chi-2	607.83	388.18	619.83	728.13	472.73	739.91	4804.45	4462.27	5111.48

Note: Random effects model. Robust standard errors. *,**,*** denote significance at 10, 5, and 1 percent levels, respectively. Observation period for *Growth rate* begins in 2014 Q1 (Barro et al., 2020), *Stringency* and *Fatality* is equal to zero until 2020-Q1. Specification 1, 2, 3 is fixed effects model without fixed effects. Specification 1-3 is a fixed effects model without fixed effects. Specification 4-6 includes seasonal (Q1-Q4) fixed effects with base season Q1. Specification 7-9 includes quarter fixed effects (2014-Q1 to 2020-Q2) with base quarter 2019 Q4.