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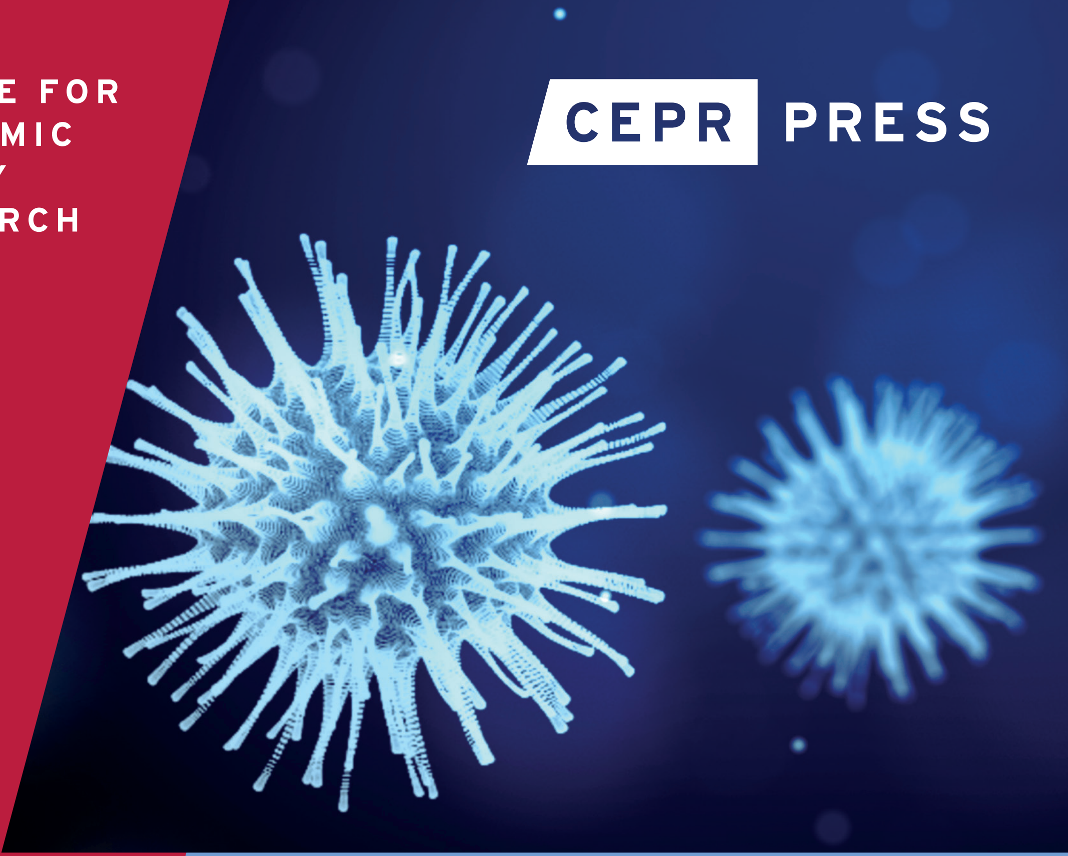

COVID ECONOMICS

VETTED AND REAL-TIME PAPERS

ISSUE 80
9 JUNE 2021

Covid Economics
Vetted and Real-Time Papers

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

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Ethics

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

Submission to professional journals

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

American Economic Journal, Applied Economics
American Economic Journal, Economic Policy
American Economic Journal, Macroeconomics
American Economic Journal, Microeconomics
American Economic Review
American Economic Review, Insights
American Journal of Health Economics
Canadian Journal of Economics
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Journal of Public Finance and Public Choice
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Journal of Population Economics
Quarterly Journal of Economics
*Review of Corporate Finance Studies**
Review of Economics and Statistics
*Review of Economic Studies**
Review of Financial Studies

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

Covid Economics
Vetted and Real-Time Papers

Issue 80, 9 June 2021

Contents

Covid Economics **Issue 80, 9 June 2021**

The impact of university reopenings on COVID-19 cases in Scotland[1]

Hector Rufrancos,[2] Mirko Moro[3] and Eva Moore[4]

Date submitted: 4 June 2021; Date accepted: 6 June 2021

This paper estimates the impact of University reopenings in Scotland in Autumn 2020 on COVID-19 cases in Scottish neighbourhoods. We geolocate all student halls in Scotland, and merge this data with neighbourhood-level case data. We employ a local differences-in-differences strategy and tackle two research questions. First, we ask what was the impact of the start of semester on cases in the student neighbourhoods? Next, we turn our attention to the spillover of cases in the nearby communities to student neighbourhoods. University semester start dates in Scotland are staggered over the month of September, and we deal with this by focusing on each start cluster, as well as implementing the Callaway and Sant'Anna (2020) estimator. We find a substantial and persistent increase in cases in areas containing halls and evidence of persistent spillovers. These effects are linked to the group of Universities that started on 14th September, which include large Universities located in the major urban areas. The cases began to rise on 21st September, with 100 extra cases per 100,000 per day, and peaked a week later with 400 additional cases per 100,000 per day, after which they started declining, but persist until the Autumn tightening of coronavirus restrictions bit in November, two months after the restrictions were enacted. Our results invite a re-think of how close contact activities may safely resume.

1 Many thanks to Antonia Schwarz, Till Stowasser, Willem Sas and an anonymous referee for their comments. Any and all errors are the authors'.

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Covid Economics 80, 9 June 2021: 1-44

Covid Economics 80, 9 June 2021: 1-44

1 Introduction

Understanding the effect of restrictions on the spread of pandemics is essential to inform decision-making models for the near future and future epidemic episodes. A controversial restriction which has been adopted world wide is the closing of Primary and Secondary education. There is a nascent literature which evaluates the impacts of school reopenings on COVID-19 transmission, and the evidence is mixed.(Goldhaber et al., 2021; Bravata et al., 2021; Harris et al., 2021; Courtemanche et al., 2021; Isphording et al., 2021; von Bismarck-Osten, Borusyak, and Schönberg, von Bismarck-Osten et al.) An overlooked group in the policy discussion has been the role of Higher Education in the spread of COVID-19. This paper investigates the impact of students returning to University campuses during the start of the new academic year in September/October 2020 on transmission of the virus in the local area. During the gradual reopening of the economy during the summer, Universities Scotland, the umbrella group of all Scottish Universities, worked with the Scottish Government towards safely returning students to University campuses through the adoption of a blended teaching approach, i.e., a mix between face-to-face and online teaching, for the next academic year. In many instances, students were encouraged to move back into University towns anticipating further relaxation of rules in the autumn. There are no official statistics on how many students moved back to campuses or student halls, but reports point to some residences being at near of full capacity at the beginning of the academic year.[1]

While a number of papers examined the effect of school reopenings on the spread of COVID-19 infections in the community, we are not aware of any study looking at the impact of university reopenings. Universities and colleges contribute significantly to the national and local economy, adding £4.6 billion to the Scottish economy, employing 36,850 people and supporting another 36,050 jobs across the country through their supply chains and the expenditure of staff and students[2]. However, during the pandemic, these benefits must be weighed against the costs associated with the potential exposure of the public to COVID-19, including hospital capacity, health risks, and long-term health issues.[3] University reopenings are likely to pose higher infection risks to the broader community than school reopening because of several factors associated with the nature and degree of interactions between students, staff, and the local community. First, a large number of national and international students are likely to move into University towns and campuses a few days before the start of the term, increasing the chance of imported cases into communities. Epidemic modelling stresses that higher education settings are likely to experience intra-organisational transmission, citing shared accommodation and spaces (Christensen et al., 2020). Students tend to live in multi-occupancy

[1] https://www.bbc.co.uk/news/uk-scotland-54575788

[2] https://www.ucu.org.uk/article/10924/Study-shows-Scotlands-universities-support-tens-of-thousands-of-jobs-and-generate-billions-for-the-country

[3] https://www.nhs.uk/conditions/coronavirus-covid-19/long-term-effects-of-coronavirus-long-covid/

households or university halls, which may also be located across urban areas, tend to commute and use public transport and have a high propensity to interact socially. This is why UK COVID-19 modelling by the Scientific Advisory Group for Emergencies (SAGE) concluded that 'Higher Education could amplify local and national transmission'(SAGE, 2020). The risk to the wider community may be accentuated by the higher proportion of asymptomatic cases among younger age groups, meaning that cases and outbreaks are likely to be harder to detect among student populations (SAGE, 2020).

We estimate the impact of Scottish University reopenings in autumn 2020 on COVID-19 spread to the wider community using hand-collected information on locations of students halls combined with daily neighbourhood data on confirmed COVID-19 cases from Public Health Scotland. [4]

We do so by addressing two related research questions. The first research questions asks whether we can observe increase in cases within locations containing student halls that are attributable to Universities reopenings. A rise in cases in these locations will be taken as evidence of direct infections among students, first and foremost, and staff members. Some of these cases, however, might be found among local-area residents (we call this baseline analysis). The second research question addresses this potential spillover of COVID-19 from student halls to the wider community and adjacent areas (i.e., spillover analysis).

We draw from the differences-in-differences literature and employ an event study approach that compares *daily* positive COVID-19 cases between neighbourhoods over the period 10th August 2020 to 21st January 2021. To study the spread of the virus within locations with student halls, we contrast daily cases in neighbourhoods characterised by the presence of student halls with cases found in immediately adjacent neighbourhoods, i.e., areas that are located k kilometres from the areas containing the student residences. To evaluate potential spillovers to more distant areas, we employ a 'donut' approach. We compare the cases occurring in areas adjacent to those neighbourhoods where halls are located (the inner ring of the donut) to the their contiguous areas (the outer ring donut areas). The locations of the student halls are excluded from this analysis (i.e., they are the donut hole). In other words, the inner ring of the donut consists of those areas within k kilometres from the nearest hall that previously acted as a comparison group and the outer ring donut areas include units that are within $2k$ kilometres of the student halls. Note that this donut analysis addresses concerns about the

[4] The concept of neighbourhood employed here are the Scottish Intermediate Zones. These are geographical units created by the Scottish Government for censal reasons. Their size varies. As of 2019 they contained between 900-10,500 residents and an urban neighbourhood measures 1.4km^2, on average. These geographical units are particularly suitable for our analysis. Public Health Scotland publishes the seven-day count of positive cases for each day instead of the actual number of cases per day. The reason behind this is twofold. First, it protects confidentiality as the number of cases can be very small within these areas. Second, it reduces the variability by smoothing the time series. Using this measure does not impact our analysis as we are estimating the difference in cases between statistically similar areas for each day in our sample. For simplicity, we refer to this variable as 'daily' cases. More details can be found in the Data section 2.2.

[4] We experiment with different values of k, from 1 to 5km.

Covid Economics 80, 9 June 2021: 1-44

potential violation of the Stable Unit Treatment Value Assumption (SUTVA) arising from the baseline analysis as the comparison group areas may not represent a valid counterfactual because of contamination and spillovers.

The local dynamic differences-in-differences approach allows the assessment of pre-treatment trends, i.e., to test whether there is a systematic difference in daily cases across neighbourhoods before University reopenings, while also accounting for the gradual spread of infections. This strategy has other advantages. It relaxes the single treatment effect of standard differences-in-differences and explicitly accounts for incubation periods, testing delays, and multiple rounds of transmissions. Every model includes day and neighbourhood fixed effects (two-way fixed effects estimator, TWFE) and additionally controls for differences in baseline health and socio-demographic characteristics at neighbourhood level (including the proportion of over 50s, overcrowded housing and disability rates from the Scottish Indices of Multiple Deprivation interacted with month time trend) and a weekly measure of public transport mobility that vary at the local authority level.

Estimates from TWFE specifications such as these may be biased because the start of the autumn term varies for each university and hence neighbourhood (see for example: Goodman-Bacon, 2018; Callaway and Sant'Anna, 2020; Sun and Abraham, 2020; Roth and Sant'Anna, 2021). To address this, we run a series of heterogeneous TWFE models, i.e., we estimate separate event studies according to the term start date. Four start-of-the-semester dates generate four different sets of estimates for the increase in cases within areas containing halls relative to the nearest neighbourhoods.[5]

We find a substantial increase in COVID-19 cases in areas where halls are located, compared to their neighbours, in the second half of September (peaking at around 28th September). The heterogeneity TWFE analysis confirms that the large rise in cases detected in September is linked to the group of Universities opening on 14th September, which are include large Universities in urban campuses of Glasgow and Edinburgh. The delay between the term start dates and the wave of cases is due to lags between exposure and symptoms that are highlighted by the epidemiological literature that finds about 3-5 days of incubation periods and another 5-14 days before symptoms appear (Lauer et al., 2020; Qin et al., 2020; Chun et al., 2021).[6] For the group starting on 14th September, our estimates show a sharp increase in cases from 21st September, a rise equivalent to an additional 100 cases per 100,000 relative to their 1km nearest neighbours. The wave peaks at an additional 400 cases per 100,000 per day on 28th September, from which it declines until 19th October. The decline seems to suggest that the measures adopted on 22nd-23rd September had begun to have an effect. These measures included the prohibition of household mixing, a 10 pm

[5]As further robustness, we implement the doubly robust estimator of Callaway and Sant'Anna (2020), which substantially confirms the results from the heterogeneous TWFE models.

[6]Note that students were allowed to arrive in their halls one or two weeks prior to the start date. See Section 2 for more information.

Covid Economics 80, 9 June 2021: 1-44

curfew for pubs and restaurants, and measures implemented by the Universities including the isolation of hundreds of identified and suspected cases and their contacts in halls and the requirement to avoid socialising in pubs and restaurants. This heterogeneity analysis adds another crucial insight as it reveals that, although the decline from the peak is apparently rapid, the number of cases remains stubbornly higher relative to their contiguous areas until the end of November, with these areas reporting an extra 100 cases per 100,000 per day.

We find evidence of spillovers with the transmission of the virus extending beyond areas containing halls. Those locations nearest to student halls see an increase in cases (relative to their surrounding areas) approximately one week after the spike that was detected in the students' neighbourhoods. The increase in cases represents roughly an additional 20-80 daily cases per 100,000 and persist until the end of November. This represents a 20-50% increase in cases between the inner and the outer donut.

These results are important because the estimated increase in cases occurred in a context of relatively low prevalence and with several Government measures still in place to contain the virus. Scotland was cautious in reopening the economy after the national lockdown that lasted for three months, from the end of March to the end of June. The reopening was occurring while at the same time maintaining several public health measures in place, including mandatory mask-wearing in shops and public transport, physical distancing rules in indoor spaces, restrictions about indoor gathering and household mixing. Our analysis suggests that, even against this backdrop, Student in migration into student halls represented a risk for areas hosting the halls and for their surroundings. The outbreaks were ultimately dealt with, but the adverse direct and indirect health effects and the containment measures adopted were socially costly.

These findings provide valuable insights for the academic year 2021/22. While the mass vaccination program is underway in Scotland, our research invites a re-think of how close contact activities at Universities can safely resume.

This paper is structured as follows. The following section 2 describes the evolution of the pandemic in Scotland from March 2020 to January 2021 and the data used to analyse the spread of the virus after University reopenings in the autumn 2020. Section 3 detailed the empirical strategy adopted and Section 4 presents the results. Finally, section 5 highlights contributions, potential policy implications and current ongoing work aimed at expanding the analysis to cover mortality.

2 Context and Data

This section will provide a background of the COVID-19 situation in Scotland since March 2020, with particular reference to local and national restrictions, as well as guidelines and actions adopted by the Scottish Universities. This is followed by a description of the key data

Covid Economics 80, 9 June 2021: 1-44

Covid Economics 80, 9 June 2021: 1-44

exploited in the analysis.

2.1 COVID-19 in Scotland

The first positive case of COVID-19 in Scotland was confirmed and announced on 1st March 2020.[7] Likely, the virus was already in circulation in the country well before then as Scotland's testing strategy was in its infancy, and the actual number of infections would have been vastly higher. A few days later, several Scottish Universities were handling the first cases of students testing positive for the virus, and quickly announced measures for containment which included special arrangements for online assessments and plans to transfer teaching to online delivery by or during the week commencing 20th March 2020.[8] On 23rd March, the UK Prime Minister announced a set of very stringent national restrictions, the "lockdown", that applied to all the nations within the UK, including Scotland. This nationwide lockdown included the closure of all non-essential shops, outdoor gyms and places of worship, as well as the prohibition of social events and the gathering in public spaces.

The restrictions also included the closure of schools, Colleges and Universities. By that date, however, most University students already left or were about to leave their University's accommodations as the Scottish Universities were operating online and continued to do so for the remainder of the academic year. Those students still living in University halls were subject to the same restrictions as the rest of the population[9], which meant studying remotely from home and going outside to exercise once a day or to buy food. Universities provided essential services for students on campus throughout this period.[10].

Quickly thereafter, the first wave was underway. Daily cases rapidly increased from 70-80 just before the lockdown to over 350 for most of April. The number of cases reported arguably underestimated the severity of the transmission, given the lack of testing capacity at the onset of the pandemic. However the exponential increase in the observable figures was indicative of a troublesome situation. At the same time, hospital admissions and deaths rose at a fast pace, the rates of both going from zero to their first wave peak in a matter of a four weeks. The death rate lagged behind the hospital admissions rate, but only by a fortnight - an indication of the fast progression of this disease in the sickest patients. The national lockdown brought the spread under control, but it took about two months for the number of hospitalisations to decline to March levels. By the end of May, the number of patients in intensive care had fallen by 80% since the peak. Deaths where COVID-19 was mentioned on the death certificate declined for four consecutive weeks. Excess deaths rose from mid-March, peaking to 750-880 weekly excess deaths, equivalent to 40% extra deaths for the first three

[7] https://www.gov.scot/publications/novel-coronavirus-covid-19-update/

[8] https://www.bbc.co.uk/news/uk-scotland-51880666

[9] The UK Government's message was "Stay at home. Protect the NHS. Save lives".

[10] https://www.gov.scot/news/colleges-and-universities-preparing-for-phased-return/

weeks in April.[11] Deaths from all causes declined steadily from mid-April and were back to the previous five-year average by mid-June, which can be taken as the end of the first wave.[12].

These positive trends led to the implementation of a route map out of lockdown. The route map, published on 21st May, under the title *Framework for Decision Making: Scotland's Route Map through and out of the crisis* set out the criteria to gradually lift lockdown restrictions by following four phases.[13] The route map also marked a slightly divergent path out of lockdown from the rest of the UK. A path characterised by a more cautionary approach than England.

Under the Scottish Government's route map, universities and colleges were to return to campus for the 2020/21 academic year and provide flexible blended digital and face-to-face teaching while adhering to public health measures, including physical distancing and enhanced hygiene measures. On 27th May, the Government announced preparations for the phased return to campus activities. The document left the details of arrangements for learning and teaching to individual institutions.[14]

Further restrictions were relaxed gradually between the end of June and mid-July. This phase was marked by, first, the reopening of high street shops followed by, the reopening of restaurants, pubs, cinemas and holiday accommodation under physical distance and strict hygiene measures on 15th July. Concerning Universities, teaching, learning and assessment was to continue online while lab research was allowed to be back on campus at the end of June, subject to physical distancing.[15]

Over the summer, Scottish Universities' plans were characterised by efforts to welcome students back to halls with a combination of measures ranging from physical distancing rules (of one or two meters depending on the level of risk and according to Scottish Government guidelines), the creation of 'students bubbles', and staggered arrival in the campus. The communication to students varied across the Institutions but broadly included references to blended teaching. Controversies arose later about how plans were communicated to students by some Universities that recommended coming back to campus more strongly than others.[16]

Primary Schools and high schools reopened on 11th August, with most face-to-face activities across Scotland starting in the week beginning 18th August.

The gradual move out of lockdown initiated at the end of May was interrupted in Aberdeen and Glasgow following local outbreaks. On 5th August, restrictions including the closure of restaurants, limits to indoor household visits, and travel restrictions to and from the city

[11] Given the limited ability to test, excess deaths are the best measure to assess the impact of COVID-19 during the first wave. Excess deaths are computed as the difference between total deaths in a week and the average deaths from the previous five years in the same week.

[12] https://data.gov.scot/coronavirus-covid-19/detail.html#deaths

[13] https://www.gov.scot/publications/coronavirus-covid-19-framework-decision-making-scotlands-route-map-through-out-crisis/

[14] https://www.gov.scot/news/colleges-and-universities-preparing-for-phased-return/

[15] https://www.gov.scot/publications/coronavirus-covid-19-framework-decision-making-scotlands-route-map-through-out-crisis-phase-2-update/pages/5/

[16] https://www.bbc.co.uk/news/uk-scotland-54575788

Covid Economics 80, 9 June 2021: 1-44

Covid Economics 80, 9 June 2021: 1-44

were implemented in Aberdeen. Measures limiting household gatherings were introduced in Glasgow and nearby local authority areas (East Renfrewshire and West Dunbartonshire) on 1st September, which were then extended to other nearby areas (Renfrewshire and East Dunbartonshire) by mid-September. It is important to note that these local restrictions did not include school closures nor slowed down the reopening of the University campuses.

Following Scottish guidelines, Universities introduced face covering mandates within their own buildings and maximum class size rules. They also took a range of extra precautions that included specific COVID19 inductions for staff and students, developing apps to track staff and student movement on and around campus and within buildings supporting Scotland's contact tracing scheme. To allow a safe quarantine, most of the Universities made halls and private accommodations available to students arriving from outside the UK, or from hotspots across the UK, two weeks ahead of the start of term.[17]

Unfortunately, the infection rate began to rise steeply in September with Public Health Scotland figures showing a rise in cases among younger people, in concomitance with students returning to halls.[18] In mid-September, more than 200 students across Scotland were testing positive and entire student households asked to self-isolate and threatened with disciplinary actions in case of non-compliance.[19] The Scottish Government gradually began to re-introduce a number of tighter restrictions nationally and locally. First, the Government limited and then prohibited indoor households gathering. On 23rd September a 10pm curfew was imposed on pubs and restaurants. The next day, and in response to outbreaks stemming from campuses, Universities introduced more stringent measures for their students, requesting students to avoid outdoor activities and pubs, and threatening disciplinary actions. [20] These actions were met with resentment by student unions and trade unions and were perceived as discriminatory.[21]. More controversies arose in the following days with some commentators denouncing Universities as having encouraged students to return to halls and then blaming students for the inevitable increase in infections. In response, Universities Scotland outlined further support measures including regular check-ins on those self-isolating and assistance with food and laundry provision. The guidelines explicitly allowed for the return home on a permanent basis for students who wished to do so.

Hospital admissions increased by almost 80% in a matter of a few days at the beginning of October, with a particularly rapid increase among the elderly. In response to this second wave, the Scottish Government introduced new restrictions planned to last for two weeks, from 9th October to 25th October, but that resulted being in place until the beginning of

[17] https://www.universities-scotland.ac.uk/sharedayrestartmsg/

[18] Outbreaks were reported in shared students accommodation in all the major urban areas.https://www.bbc.co.uk/news/uk-scotland-56399043

[19] https://www.bbc.co.uk/news/uk-scotland-54268780

[20] https://www.universities-scotland.ac.uk/preventing-spread-of-coronavirus-in-universities/

[21] https://www.ucu.org.uk/article/11021/UCU-response-to-rules-banning-Scottish-students-from-socialising?list=1676

November, when a new five-tier COVID-19 system came into effect. For most of the Scottish people living in the 'Central Belt' – the area encompassing Edinburgh, Stirling and Glasgow – this amounted to a mini-lockdown, with non-essential shops and pubs being forced to closed and schools closed because of the planned two-week October school holiday.[22]

The new five-tier system came into force from 2nd November, when temporary curbs on the hospitality trade were due to expire. The five levels indicated different levels of protection that might be needed based on different levels of transmission for the virus. Most of Scotland was placed under tier two or three. On 20th November, 2.3 million Scots living in the Central Belt area were placed under tier four, with the closure of non essential shops and restaurants. The restrictions were eased on 11th December when non-essential shops reopened. A few days later, nine cases with a more infectious variant of COVID-19 were reported in Scotland. On 19th December, the Scottish Government tightened the Christmas rules significantly and announced that level four restrictions would be applied to all of Scotland from 26th December. The number of cases continued to climb and the Scottish Government announced a full lockdown for mainland Scotland from 5th January with a new legal requirement forbidding anyone from leaving their home except for essential purposes. On 8th January Scottish Government announce that University students will be taught online throughout January and February.

The second national lockdown, together with the mass vaccination programme that started on 8th December, was successful in reducing the infection rate. Schools reopened in February, March and April, following a staggered approach. Universities around Scotland continued offering online teaching for the remainder of the term. Table A1 in the Appendix summarises the key measures adopted over the summer and autumn 2020 in Scotland.

2.2 Data

This paper studies data at the Scottish Intermediate Zone level. These statistical units aggregate Scottish Data Zones, which are the lowest level unit of aggregation for Scottish official statistics. According to the most recent estimates by the National Records of Scotland[23], there are 5.4 million people living in Scotland spread across 1,279 Intermediate Zones. Intermediate Zones can be considered to represent neighbourhoods and the current boundaries were created for the 2011 census, and were designed to range in size between 2,500 and 6,000 household residents. However, a decade out, the boundaries have not been redrawn, and as of the latest population estimates for 2019, they are now found to include between 900-10,500 individuals.[24] The largest neighbourhood currently includes Finnieston and Kelvinhaugh in

[22] For the rest of Scotland these restrictions amounted to banning alcohol sales in pubs and restaurants and a 6 pm curfew.

[23] https://www.nrscotland.gov.uk/statistics-and-data/statistics/scotlands-facts/population-of-scotland

[24] The term "neighbourhoods", "locations", "areas", and "Intermediate Zones" are used interchangeably throughout the paper. Note that Intermediate Zones are "intermediate" because they sit between the 6,500

Covid Economics 80, 9 June 2021: 1-44

Glasgow, which hosts three University of Glasgow student halls and eight private-provider student halls of residence.

Student halls of residence Student halls of residences are typically provided by universities for their first year students, or international students.[25] Student halls are usually purpose-built university owned buildings of multiple occupancy with shared amenities (such as kitchens and bathrooms). In Scotland there are more than half a million students in Further and Higher Education, of which approximately 8% live in student halls of accommodation. 45% of all students in accommodation are comprised of first year undergraduate degree students (Scottish Parliament, 2020). We hand collected the addresses of all halls of residence for each Scottish university. In general, students in the later years of study tend to move out from university managed accommodation and into privately rented accommodation. In recent years, with the expansion of university uptake this has caused significant housing pressures in cities, as students corner the private rental market and crowd out locals from the rental market. In Scotland there have been two complementary policy responses to this phenomenon. First, since 2000 any privately rented property that for the purposes of law can be construed as being a House of Multiple Occupancy has to be licensed.[26] Secondly, due to the increased pressure between licensing private properties and the increases in student numbers there has been an explosion in privately provided student halls. These are often built by housing groups or chains that run these nationally. These private halls tend to fall outside university regulations, though some may restrict themselves solely to taking students from a single university. We geolocated all private halls in Scotland by exploiting search engines for student accommodation. Figure 1a shows the geographical spread of these two types of halls. Note that dots shown in red are the exact locations of halls, whilst the areas shaded in dark blue represent the student hall neighbourhoods themselves, and the light blue denote those neighbourhoods (whose centroid is) within 5km of a student hall.

Though pictured we exclude from our analysis two universities. These are the University of the Highlands and Islands (UHI) and Scotland's Rural College (SRUC) which is an agricultural college that does further and postgraduate training. Both of these are excluded as their campuses are primarily in rural settings where COVID-19 was not as much of a concern, as outlined earlier. Our primary unit of analysis is the Intermediate Zone, or neighbourhood.

'Data Zones' (the smallest Scottish statistical unit areas consisting of 350 households) and the 32 local authorities or councils. For more information see: https://data.gov.uk/dataset/133d4983-c57d-4ded-bc59-390c962ea280/intermediate-zone-boundaries-2011.

[25] Students may choose to live in halls, or not. There is no formal requirement but usually most undergraduates from outside the region where the university is located will move into halls to develop peer networks.

[26] This is a household of at least 3 (unrelated) people who share toilet and kitchen facilities. Legislation Regulating the granting of Houses of Multiple Occupancy (HMO) in Scotland fall under the purview of The Civic Government (Scotland) Act 1982 (Licensing of Houses in Multiple Occupation) Order 2000 and were more recently tightened up with the introduction of the Housing (Scotland) Act of 2006.

[26] In practice with geographic segregation of universities, it is unlikely that a student would live at halls nearby a university they are not attending.

For the most part this characterisation is appropriate. However, when looking at the size of Intermediate Zones in rural areas this quickly ceases to hold. Using the Scottish Government 2016 Threefold Rural Classification, the discrepancy in size becomes clear. The average size of an urban neighbourhood is 1.4 km^2, for areas deemed to be accessible rural this is 8.4km^2, and for remote rural areas this balloons to 1,400 km^2. The average size of the excluded Intermediate Zones for SRUC and UHI is 409 km^2 as such we felt that this could no longer be characterised as the same scale of neighbourhood. For this reason, in Figure 1a we do not shade in the neighbourhoods of the halls belonging to these universities. Figures 1b-1f plot student halls in their neighbourhoods for the cities in Scotland. Note that the comparison units are drawn from nearby neighbourhoods whose centroid falls within the 5km radius from a student hall. This is done deliberately to ensure that neighbourhoods that are comparable are used in the analysis. In locations such as Stirling, and Paisley (bottom left hand cluster of Glasgow image) some of the neighbourhood boundaries are of a rather large size, and the centroid of these areas is outwith the radius of comparison units selected. It is also worthwhile noting that in Figure 1e the seemingly large gap between Dundee and Fife is due to the River Tay, and whilst there is a large gap between these locations and the comparison neighbourhoods drawn on the Fife coast, these represent Newport-on-Tay, Wormit and Woodhaven, which are commuter neighbourhoods of Dundee connected by the (unseen) Tay Bridge.

In our analysis we focus on the direct effect of student in migration into student halls. However, we are also interested in estimating the onward effects of COVID-19 transmission on neighbouring areas. To this aim Figures 2b-2f plot these areas for what we term the spillover/donut analysis. We exclude the donut centre, that is the neighbourhood that houses student halls, and we compare the inner donut (shaded light) and compare it to the next nearest neighbourhoods (shaded dark). We make these comparisons for a series of moving rings, or different sizes of donuts. The Figures represent the maximum extent we consider, that is the neighbourhoods whose centroids are 0-5km of student halls, which are contrasted to those neighbourhoods 5-10km from student halls.[27]

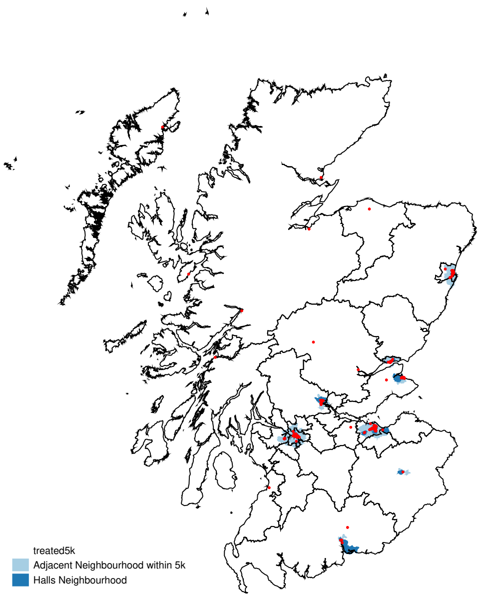
Universities begin their autumn Semesters in a staggered fashion throughout the months of September and October. Table 1 provides the start date for autumn 2020 of all Universities in Scotland. As can be seen, Universities are concentrated across a number of cities in the Central Belt (viz. Edinburgh, Glasgow, Stirling) Dundee and Aberdeen. In cities where there is more than one University, there is the possibility that student halls, either university provided, or private from two separate universities may co-exist in the same neighbourhood. This occurs in the Perth Road Neighbourhood of the West End of Dundee, for example. For the purposes of our analysis, these neighbourhoods are thought of as treated when the earliest university begins, so in this example, this would be when Abertay University begins their semester on 14th September.

[27]Please note in Figure 2c the seemingly floating comparison groups are the commuter towns of Burntisland and Dalgetty Bay which are connected by rail and road links to central Edinburgh.

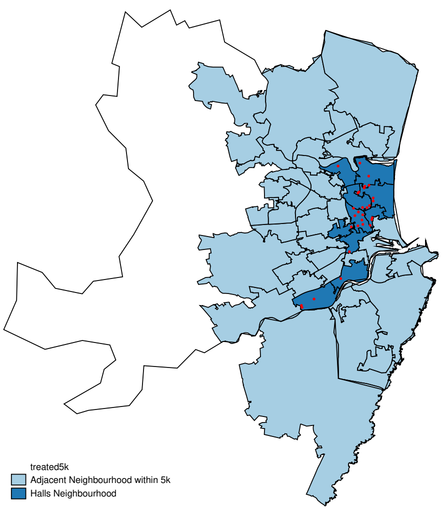
Covid Economics 80, 9 June 2021: 1-44

Figure 1: Intermediate Zones with student halls, and those within 5km of a student hall

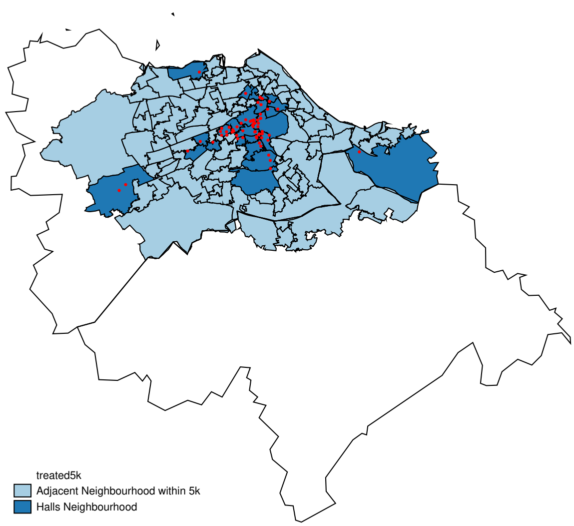
(a) Scotland



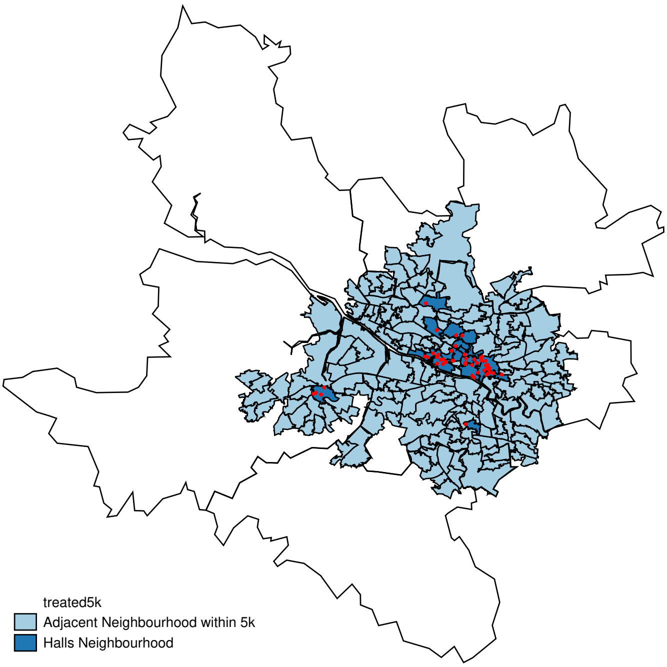
(b) Aberdeen



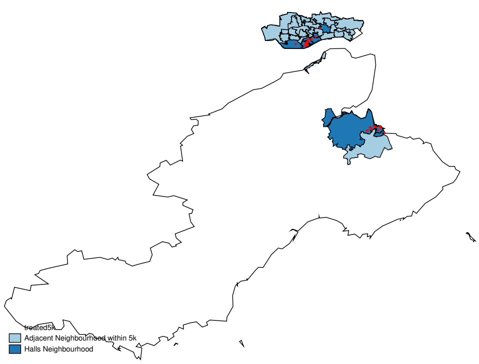
(c) Edinburgh



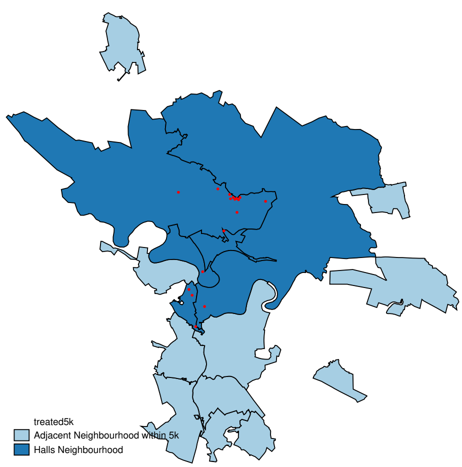
(d) Glasgow



(e) Dundee & Fife



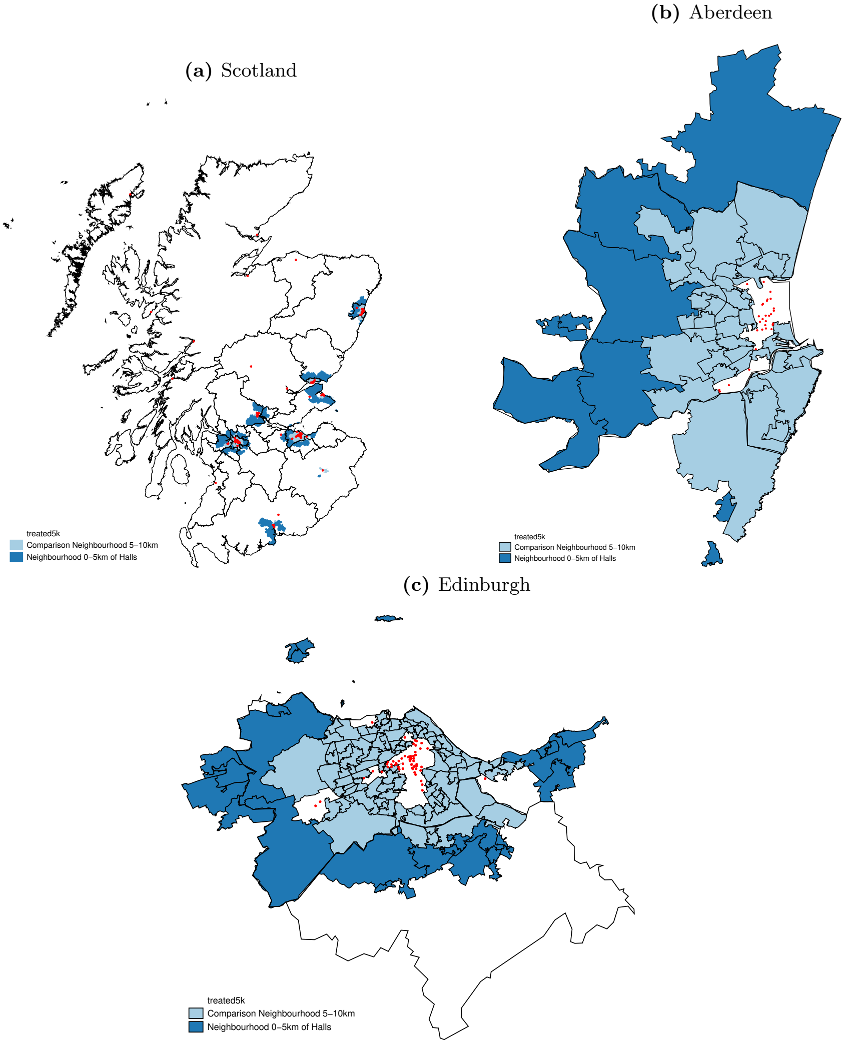
(f) Stirling



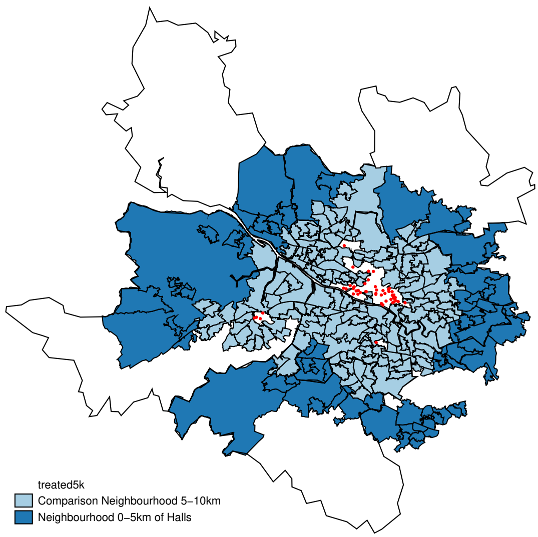
Note: Figures show neighbourhoods which have student halls, marked as red dots. Student neighbourhoods are shaded dark, whilst those who serve as comparison groups are shaded light. Subfigures b-f denote the neighbourhoods for each of the major Scottish cities. Note that the comparison areas are selected as having the centroid of the neighbourhood within 5km of student halls.

Covid Economics 80, 9 June 2021: 1-44

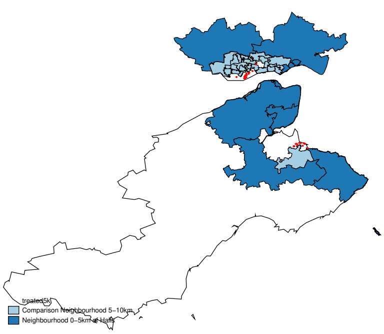
Figure 2: Intermediate Zones for spillover analysis; neighbourhoods within 0-5km of student halls vs neighbourhoods located 5-10km from student halls



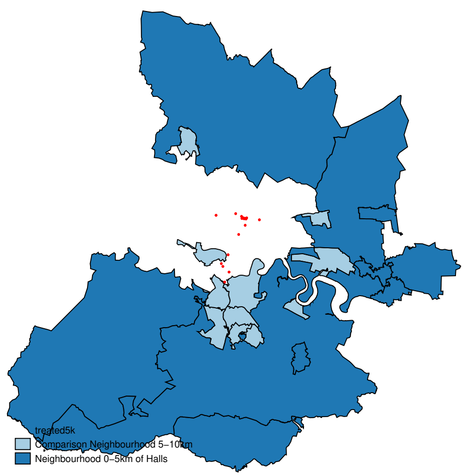
(d) Glasgow



(e) Dundee & Fife



(f) Stirling



NOTE: Figures show neighbourhoods which have student halls, marked as red dots. Subfigures b-f denote the neighbourhoods for each of the major Scottish cities. Blank areas within each figure denote areas where student halls are located (they represent the donut hole). Neighbourhoods within 0-5km of student halls are shaded light, whilst those locations that serve as comparison groups are shaded dark. Note that the the areas are selected as having the centroid of the neighbourhood within 0-5km of student halls. The comparison group areas are selected as being 5-10km from student halls, and similarly exclude some areas where the centroid of the neighbourhood falls outside the 10km radius.

CEPR PRESS

Covid Economics 80, 9 June 2021: 1-44

Table 1: Start dates for Universities in Scotland, Autumn 2020

University	Start date	Locality
Edinburgh Napier University	07 Sept 2020	Edinburgh
Heriot Watt University	07 Sept 2020	Edinburgh & Campus in Scottish Borders
Queen Margaret University	07 Sept 2020	Edinburgh
University of the Highlands and Islands	07 Sept 2020	Multiple Sites
Glasgow School of Art	14 Sept 2020	Glasgow
Abertay University	14 Sept 2020	Dundee
University of Edinburgh	14 Sept 2020	Edinburgh
University of Glasgow	14 Sept 2020	Glasgow
University of St Andrews	14 Sept 2020	St Andrews (Fife)
University of Stirling	14 Sept 2020	Stirling
University of Strathclyde	14 Sept 2020	Glasgow
Glasgow Caledonian University	21 Sept 2020	Glasgow
Robert Gordon University Aberdeen	21 Sept 2020	Aberdeen
Royal Conservatoire of Scotland	21 Sept 2020	Glasgow
SRUC	21 Sept 2020	Aberdeen
University of Aberdeen	21 Sept 2020	Aberdeen
University of Dundee	05 Oct 2020	Dundee
University of the West of Scotland	05 Oct 2020	Paisley (Greater Glasgow)

NOTE The table shows the start dates of each university across Scotland, and their nearest proximate location. For the University of the Highlands and Islands, the university is comprised of 13 different sites throughout the country. However, as outlined in the text this university is excluded from the present analysis as the locations tend to be in rural areas that had low prevalence of COVID-19 infections, and where the interpretation of Intermediate Zones as neighbourhoods breaks down due to the low population density.

Measuring COVID-19 spread The data we exploit for this analysis comes from Public Health Scotland (PHS) COVID-19 Open Data https://www.opendata.nhs.scot/dataset/covid-19-in-scotland. PHS provide daily cases of COVID-19 at Intermediate Zone level. The measure of cases is given as the number of cases for a given neighbourhood in the past seven days. For confidentiality purposes, where a neighbourhood has two or fewer reported cases in the previous seven day period, the information is suppressed. We treat these as zeros in our analysis. The reporting of data as a cumulative measure of cases is a means of smoothing out spikes in the data as well as protecting confidentiality of the data for a given neighbourhood. In our analysis this will not pose any issues, as we will be comparing the relative rates between neighbourhoods per day. Cases are allocated to neighbourhoods in accordance to the postcode given to the testing system. Due to the spatial aggregation of the data, there are large number of both legitimate zeros and suppressed information (which we treat as zeros). In order to make sense of the data, we convert this variable into population rates at neighbourhood level, we use the latest available population projections created for the 2019 Scottish Indices of Multiple Deprivation (SIMD).

To further account for the suppression of small numbers of positives in the data we focus on using the outcome as both levels, that is the number of cases in a neighbourhood in the past seven days per 100,000 of the local population, as well as the inverse hyperbolic sine of cases. This approach allows to interpret changes in relative percentage terms, similar to the interpretation as a logged dependent variable but gets around the well known lack of definition for ln(0), where the inverse hyperbolic sine is defined as $\tilde{y} = \text{arcsinh}(y) = \ln(x + \sqrt{y^2 + 1})$.

3 Empirical Strategy

3.1 Local Dynamic Differences-in-Differences Analysis

This paper is focused on estimating transmission of COVID-19 in neighbourhoods surrounding student halls. We draw from the differences-in-differences literature and employ flexible event-study approaches to first explore how *daily* cases vary between locations characterised by the presence of student halls and adjacent locations. We then apply the same methodology and estimate the spillover of cases in adjacent locations as we move k kilometers away from the resident hall. We discuss potential pitfalls of this approach and estimate alternative regressions in the next section.

The approach we adopt here is based on a 'local' differences-in-differences analysis. First, we geolocate all the residence halls for universities and aggregate the numbers up to the Intermediate Zone level. The group of neighbourhoods that contains student halls represent the treatment group. Following Tobler's First Law of Geography (i.e., 'near things are more related than distant things', (Tobler, 1970)), the comparison groups are constructed by selecting units within a k kilometre radius from each Intermediate Zone that contains one or more student halls. This approach restricts the sample relative to the whole of Scotland. As a result, the definition of a comparison group is somewhat arbitrary. Yet, it constitutes an arguably more valid counterfactual, as it is likely that nearby neighbourhoods share age and socio-demographic profiles and urban amenities, as opposed to more distant areas.

We start by running fully saturated regressions of confirmed COVID-19 cases at the daily level such as:

$$y_{ik,d} = \mu_{ik} + \tau_d + \sum_{d=224}^{D=387} \mu_{ik,d} \times \mathbb{1}(\text{D}ay = d) + \beta' M_{l,d} + \gamma' X_{ik,m} + \varepsilon_{i,m} \tag{1}$$

where $y_{ik,d}$ is the number of confirmed COVID-19 cases over the previous seven days per 100,000 population in date d, in the Intermediate Zone i within k kilometres from neighbourhoods containing student residences. The regression in (1) estimates dynamic treatment effects with two-way fixed effects: Intermediate Zone fixed effects μ_{ik} in the comparison bandwidth distance k, as well as daily fixed effects, τ_d. This dynamic differences-in-difference strategy include series of binary variables representing each date d, which represent the difference in COVID-19 cases in treatment locations vs control between a date d and the reference date 10th of August 2020. We select the 10th August 2020 as the reference date, a day where the number of cases was extremely low nationally. The data end on 22nd January 2021. As mentioned earlier, for confidentiality purposes, Public Health Scotland suppresses the reporting of cases where an Intermediate Zone has fewer than three cases. To mitigate this decision, we have re-coded all missing values as zero values. To attenuate potential measurement errors, every regression in (1) is also re-estimated using the inverse hyperbolic sine transformation of the number of cases.[28] It should be noted that there is a considerable difference if one focuses on the often-used natural logarithm, as the analysis would therefore only take into account locations that have seen a growth of cases in the last week.

One of the advantages of employing the dynamic local differences-in-differences approach described above is that it relaxes a single, homogeneous treatment effect assumption common for standard differences-in-difference estimators. It is implausible to believe that the treatment effect estimates would be constant over time given the rapidly changing situation, with restrictions to mitigate the spread of COVID-19 changing across regions and hardening over time as described in section 2.

The assumptions underlying a causal interpretation to our estimates are twofold. First, the Stable Unit Treatment Value Assumption (SUTVA), which states that there should no be no spillovers of infection between student neighbourhoods and their comparison groups. This is naturally likely to be violated, and we tackle this head on in section 3.2. However, if there

[28] A semi-elasticity can be obtained as normal by exponentiating the coefficient of interest (Bellemare and Wichman, 2020)

Covid Economics 80, 9 June 2021: 1-44

is some positive spillover, the estimates retrieved by our method will represent a lower-bound estimate. Second, it is the presence of counterfactual parallel trends, which states that in the absence of students moving into student halls the average rate of infection in student neighbourhoods would be the same as in those adjacent neighbourhoods. To explore the validity of this assumption we begin our analysis on 10th of August 2020, a full month before students were due back at University, and pre-dating the resumption of face to face primary teaching.

The empirical design adopted aims at estimating causal effects by comparing nearest neighbourhoods, hereby attenuating the potential bias originating from systematic differences across areas. For this reason, this approach can also be thought of as a linear differences-in-discontinuities with a fixed bandwidth of k kilometers (Grembi et al., 2016). One concern is that there may still be differences at baseline between neighbourhoods. Although these differences should be net out by the neighbourhood fixed effects, we cannot rule out that these differences may reflect different baseline propensities for infection due to the correlation between cases and underlying socio-demographic characteristics. To address these concerns a number of control variables are included in every model. X is a matrix containing a number of variables at neighbourhood level, such as the proportion of people aged 50+, and measures of overcrowded housing and disability rates from the Scottish Indices of Multiple Deprivation. It should be noted that these variables are not in of themselves time-varying but are allowed to affect the outcome at month level m by interacting them with a monthly time trend as in Feigenbaum and Muller (2016). Finally, we also include data on public transport mobility at local authority level l on a weekly basis w, which are compiled and controlled for in matrix M. A more detailed description of these control variables is offered below.

Two-way fixed effects specifications such as (1) may raise additional concerns. There is a novel and growing literature that highlights potential for bias stemming from differential treatment timing. As mentioned in the section 2, the start of the autumn term (i.e., treatment timing) varies for each university, and hence treatment neighborhoods (see for example: Goodman-Bacon, 2018; Callaway and Sant'Anna, 2020; Sun and Abraham, 2020; Roth and Sant'Anna, 2021). There is a panoply of competing estimators being proposed to address the bias, nevertheless, the underlying issue is that the two-ways fixed effects provides a weighted estimator that averages over all the estimates. At times this may average over periods that may not be appropriate comparison units, and this may lead to biased estimates of the true dynamic treatment effect. In order to avoid this pitfall, the approach we adopt here is to apply the same regression as (1) but do this over the g subsamples of term start dates. In other words, we will control for potential contamination of control groups by running separate event study according to the term start date. As before, the control units considered here are always neighbourhoods which do not have student halls which are drawn from a radius of k kilometres from the central point of a student neighbourhood. This approach

Covid Economics 80, 9 June 2021: 1-44

Covid Economics 80, 9 June 2021: 1-44

has the advantage to eliminate the biased warned by the growing differences-in-differences literature, while at the same time it is easy to interpret. As further robustness, we implement the doubly robust estimator of Callaway and Sant'Anna (2020). The results from this latter exercise are presented in section 4.1. Note that in the Callaway and Sant'Anna we opt for using later treated groups as the comparator group, losing out the final group of starters to the estimation procedure.

Control variables The present analysis controls for the matrices X and M. The contents of these matrices are as follows: The X matrix is derived from the 2019 Scottish Indices of Multiple Deprivation (SIMD) (Scottish Government, 2019). The SIMD are sub-neighbourhood statistics that are released at Data zone resolution. Recall that Intermediate Zones nest multiple data zones, which in turn are all nested in local authority districts.[29] As the data are more finely grained than the analysis presented here, it was necessary to aggregate these up to Intermediate Zones. The population weighted average of each SIMD measure was computed for each Intermediate Zone using the latest 2019 population estimates. The specific subcomponents of the SIMD that are used are the comparative illness factor (CIF) and the overcrowding rate. The CIF is a standardised ratio of the number of claimants of a number of government illness and disability benefits.[30] The overcrowding rate is the proportion of households in a given area that live in overcrowded housing based on an occupancy rating. The occupancy rating compares the actual number of rooms in the house to the number of rooms which are required by the household, based on the relationships between them and their ages.[31] Both of these factors were chosen as they account for some important underlying differences at baseline, namely the underlying health of the local population, and the suitability of their housing. We also have controls that account for differential socio-demographic makeup on neighbourhoods. We utilise data from the 2019 population estimates at neighbourhood level to estimate the proportion of population in a neighbourhood that is over 50.

The M matrix utilises data from the community mobility reports from (Google LLC, 2021). These data are derived from mobile phone user data using Google Maps. The data are released at daily level, and for Scotland these are released at Local Authority District level. For each district, the data represent the percentage deviation in visits to a certain amenity relative to the median response for the same day of the week in 03/01/2020-06/02/2020. The data are released for a number of amenities including transport, supermarkets, parks, retail, workplaces and residential. It should be noted that for some district-amenity combinations

[29] That is there is no Intermediate Zones or data zones that cross administrative borders.

[30] This is comprised of Disability Living Allowance, Attendance Allowance, Incapacity Benefit (not receiving DLA), Employment and Support Allowance, Severe Disablement Allowance, Income Support with disability premium, Personal Independence Payment and Universal Credit claimants with an accepted restricted ability to work. (Scottish Government, 2020)

[31] An occupancy rating of -1 or less indicates overcrowding. This means that there is at least one room too few in the household. For more informationa see `https://www.gov.scot/publications/simd-2020-technical-notes/`

Covid Economics 80, 9 June 2021: 1-44

due to privacy restrictions wherein the data do not meet the either the data quality or privacy threshold, the data are suppressed by Google. This means that these should be treated as true unknown data, that is one cannot infer there is a lack of movement due to the lack of data in a given combination. Unfortunately for the present analysis, the residential category in various Scottish districts falls afoul of this restriction, which means that using this as a control would restrict the analysis. For this reason, we have opted to only focus on the public transport amenity, as this is often, not missing, and will indicate the variation of movement into, within and out of a given local authority area.

3.2 Spillover Analysis

This section expands the empirical strategy with the specific purpose of tackling the second research question: Can the spillover to the neighbouring areas be measured? What is the effect of living in an neighbourhood adjacent to a student one?

It is plausible that if the infection reproduction rate is above 1 in areas containing student halls, people living in surrounding neighbourhoods are more likely to become infected as their chance of encountering the virus increases. This chance may contaminate the comparison group used in the previous analysis. This concern is similar to that encountered in the RDD literature in relation to heaping or discontinuous density to the border of the RDD. One often-used robustness check follows from Barreca et al. (2011) who propose to do a 'donut' RDD; where one drops the potentially problematic centre of the donut, and the RDD is run over the the remaining whole. Eggers et al. (2018) extend this approach and suggest that a donut-differences-in-discontinuities approach may mitigate the issue of sorting around the threshold of the discontinuity.

We have sought to motivate the regression analysis presented here as a 'local differences-in-differences' approach, that is the distance to the nearest treated hall is not a covariate in the control matrix of the regression. However, one can extend the present analysis to incorporate the donut approach to estimate the spillover effects of COVID-19 from the neighbourhoods of their halls to their neighbours, and their neighbours-neighbour's.

This approach can be extended to the present analysis. The donut hole, is therefore the neighbourhood that contains one or more student halls of residence. The inner donut, or the group to be investigated, are those whom are within k kilometres from the nearest hall, and who previously acted as a comparison group. Whilst the outer donut are those units which are within $2k$ kilometres of the student halls. So for example, in the 1km donut, the inner donut is those neighbourhoods within 0-1km of the student halls (excluding the student halls neighbourhood), and the outer donut are those between 1-2km of the student halls. The spillover analysis therefore follows on from the main analysis suggesting that the best comparison groups are those which are nearest to the potential spillover zones. In the present analysis we will investigate donuts that range from 1km to 5km, comparing their nearest and

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respective 1-5kms.

4 Results

4.1 Baseline Analysis

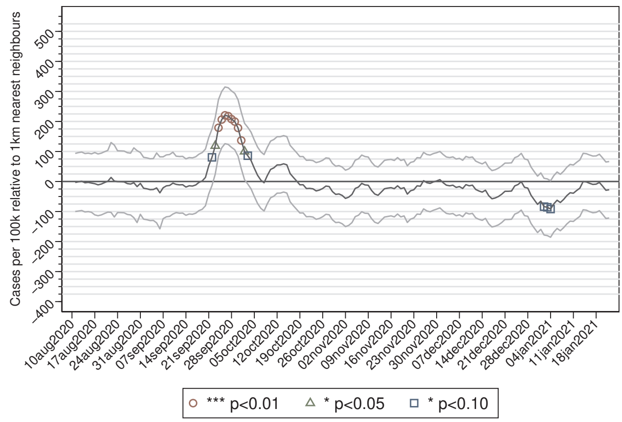
We start by presenting results from the analysis using the functional form outlined in 1, which includes all the control variables described in the previous section, and thus accounts for differential trends in transport mobility within local authority districts, and allows for underlying demographic and health differences to have differential effects on outcomes. Figures 3 and 4 present the estimates of the new daily confirmed COVID-19 cases relative to the surrounding (comparison group) areas. For robustness purposes, the definition of the comparison group area changes by varying the distance to the area containing student halls, from 1km to 5km. These estimates reveal a wave of COVID-19 within student's halls areas compared to the nearest areas, with infections rising approximately to 100 cases per day per 100,000 on 21st September and doubling one week later at above 200. The number of cases declines quickly and are found to be not statistically different from the neighbourhood areas approximately on 5th October. The evolution of infections seem to have responded to the new Government restrictions and bans imposed on students by Universities on 22nd and 23rd September (See 2). This first spike in cases is immediately followed by a minor second spike, which is detected in every model. Still, it is statistically significant only when compared to areas within 3 and 4 km from halls. This second wave has a peak of 50 cases per day per 100,000 and lasts for less than two weeks. The remaining of the period is characterised by a number of cases that is not different from the surrounding areas. If anything, some models reveal fewer infections than the adjacent areas in two instances: from the second part of October to mid-November, and at the end of December-beginning of January. The fewer cases may be driven by the fact that students were given the possibility to return home after October and others left halls for the Christmas holidays.

TWFE Heterogenous Treatment timing A major concern is that due to the staggered start to the autumn Semester across Scottish universities there could be substantial heterogeneity in effects in response to the shifting public health policy mitigations introduced by the Scottish Government. As such, the effect of being a neighbourhood that contains halls for university that started classes in the first week of September 2020 on cases is likely to be very different than that for those in the first week of October. This section speaks to this concern by running models using a TWFE estimator to obtain estimates of the increases in daily cases in locations with halls relative to contiguous areas by semester date. For ease of exposition we plot estimates from models using areas within 1 km as comparison groups. in Figures 5 and 6. These estimates reveal the presence of substantial heterogeneity. Note

Covid Economics 80, 9 June 2021: 1-44

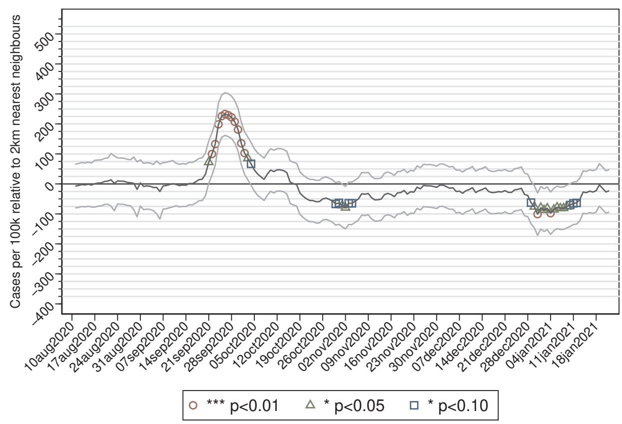
Figure 3: Daily cases in areas with halls relative to adjacent areas, COVID-19 cases per 100k

(a) Comparison group within 1km of halls



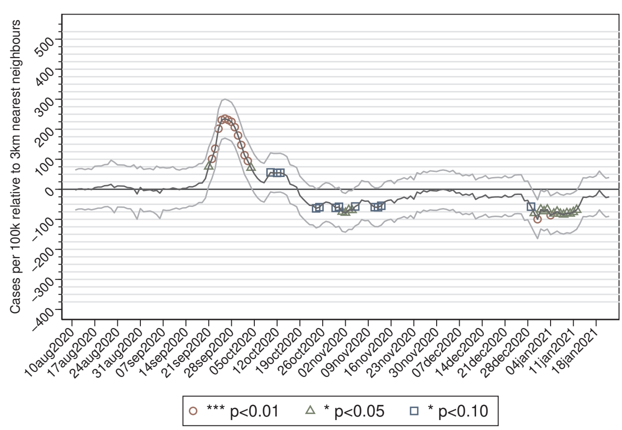
AIC: 166222.45

(b) Comparison group within 2km of halls



AIC: 288816

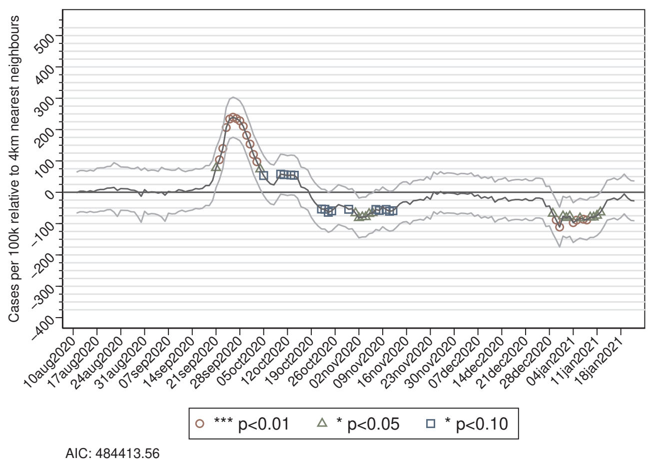
(c) Comparison group within 3km of halls



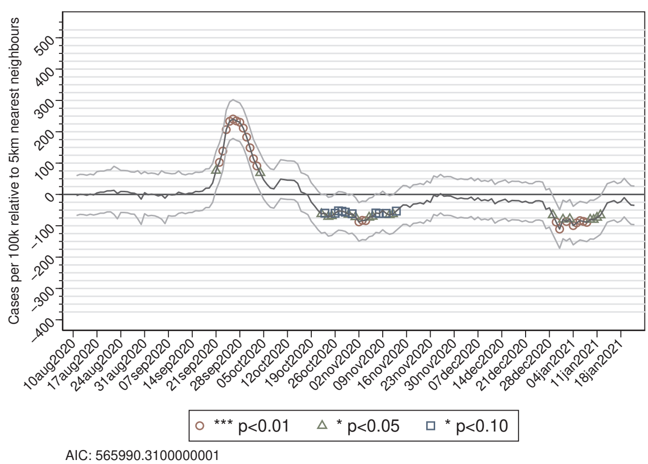
AIC: 397639.22

Covid Economics 80, 9 June 2021: 1-44

(d) Comparison group within 4km of halls



(e) Comparison group within 5km of halls

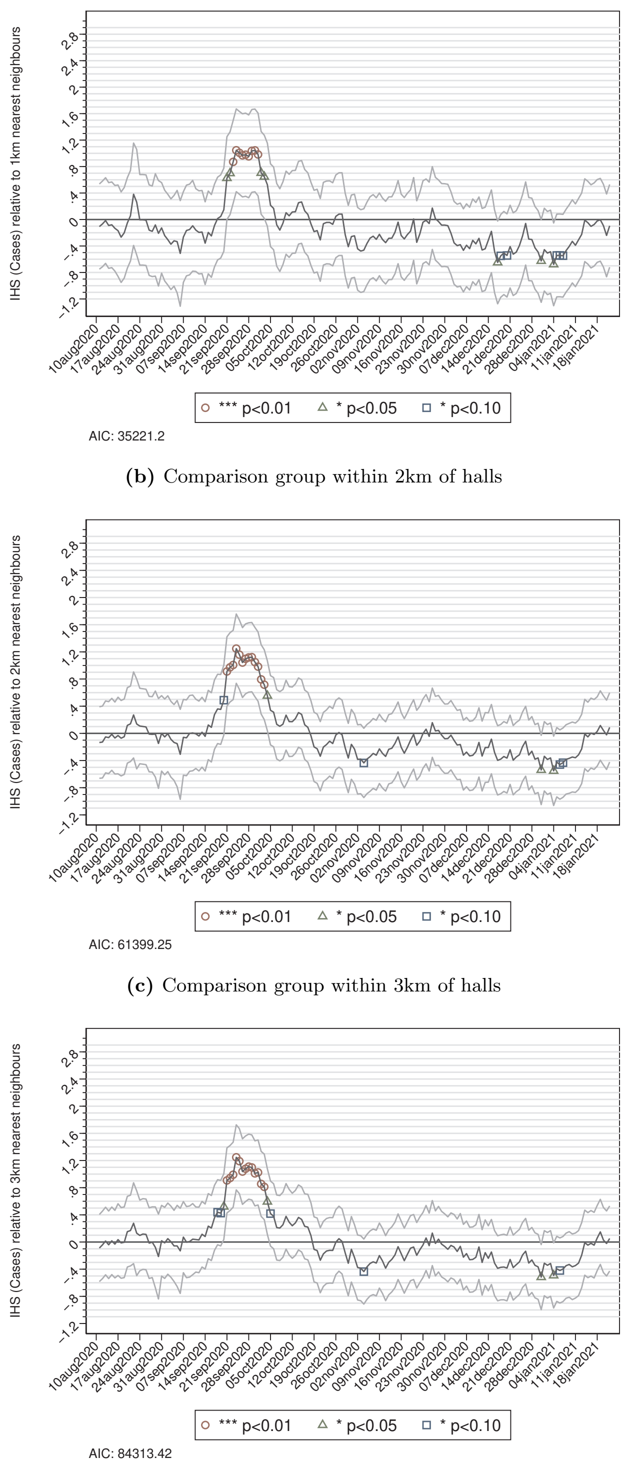


Note: Figures report the difference in daily COVID-19 cases between contiguous areas using (1). Each plot shows standard significance levels through different markers. Note each subfigure alters the comparison group for the treatment group where the comparison groups are drawn within $k$km of a student halls neighbourhood. Confidence intervals from neighbourhood halls and month clustered standard errors are plotted. All regressions estimated here use the full battery of controls. Mobility: Daily Transport usage relative to January 2020. Health: Allowed to vary at monthly level, Comparative illness factor and housing overcrowding. Age: Effect allowed to vary monthly, proportion of over 50s living in neighbourhood. Note that due to data restrictions the mobility data do not cover all time periods, thus any temporary losses should be considered missing at random.

Covid Economics 80, 9 June 2021: 1-44

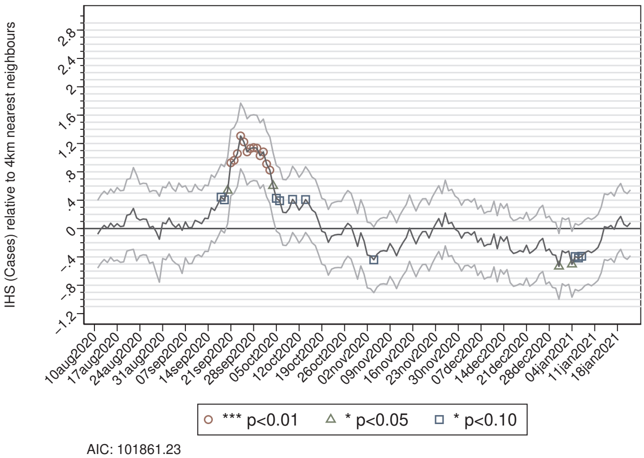
Figure 4: Daily cases in areas with halls relative to adjacent areas, inverse hyperbolic sine of COVID-19 cases

(a) Comparison group within 1km of halls

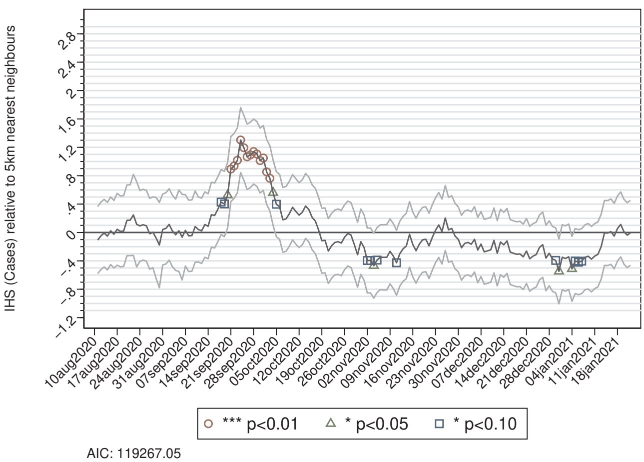


Covid Economics 80, 9 June 2021: 1-44

(d) Comparison group within 4km of halls



(e) Comparison group within 5km of halls



Note: Figures report the difference in daily COVID-19 cases between contiguous areas using (1). Each plot shows standard significance levels through different markers. Note each subfigure alters the comparison group for the treatment group where the comparison groups are drawn within $k$km of a student halls neighbourhood. Confidence intervals from Neighbourhood Halls Cluster and Month clustered standard errors are plotted. All regressions estimated here use the full battery of controls. Mobility: Daily Transport usage relative to January 2020. Health: Allowed to vary at monthly level, Comparative illness factor and housing overcrowding. Age: Effect allowed to vary monthly, proportion of over 50s living in neighbourhood. Note that due to data restrictions the mobility data do not cover all time periods, thus any temporary losses should be considered missing at random.

that the findings do not change as the presence of heterogeneity is evident regardless of the composition of the comparison group. The main spike in cases visible in the normal TWFE estimates is mainly driven by the cluster of student halls whose universities began on 14th September 2020. For those neighbourhoods containing halls whose universities began in the second week of September the rate of COVID-19 cases rises by the 21st of September initially by an additional 100 cases per 100,000 relative to their 1km nearest neighbours, and eventually peaks at an additional 400 cases per 100,000 per day on the week of 28th September from which it declines until 19th October, when the raft of new measures introduced on 25th September begin to have an effect. The number of cases settle at what appears to be a new equilibrium running at approximately 100 additional cases per 100,000 per day relative to their neighbours until the end of November. Conversely for universities beginning on 9th September the peak from their start of semester is never significantly higher than their neighbours, and quickly tails off until it sees a second wave in late November that peaks with an additional 350 cases per 100,000 relative to their neighbours that is contained by the start of December. For the universities that began their semester on 21st of September there is a significant increase in the number of cases one week after their start of semester, though again at its peak it does not exceed an excess of 250 cases per 100,000 per day relative to their neighbours, the effect dissipates, and then abruptly peaks a second time on the week of 12th October, only to dissipate again. Finally, for Universities beginning 5th October, these neighbourhoods were running significantly fewer cases in the lead up to the start of their semester, and these locations, though they do see a rise in cases, they trend with fewer cases than their neighbours, until January 2021. However, given that only two universities started on October 5th, and one is a campus based university, whilst the other is an urban based university, caution must be adopted in interpreting these results.

When looking at the inverse hyperbolic sine results presented in Figure 6, the increase in cases is notable in areas where universities began on 14th September we find this to be 230% of cases relative to the nearest 1km neighbours on the 14th of September, and rising to a peak of 847% on 6th October. It is worth noting that though cases seem to be following an upward trend ahead of 14th September, students were allowed to move into student halls two weeks ahead of the start of semester, this may in part explain the upwards trend evidenced ahead of the start of semester. This alternative specification picks up an an earlier spike of case load in August, which starts to decline come the week of September 7th, before the start of semester on 14th September. We conjecture that this spike can be attributed to two things. First, schools in Scotland went back for in person teaching between 10th-17th August. It is not implausible to conjecture that there was some contagion from nearby clusters emerging from schools for the urban universities. Second, urban universities are located in areas with lots of amenities including restaurants. As documented by Fetzer (2020) the Eat Out to Help Out (EOHO) scheme, which subsidised restaurant meals to help the sector, led to an increased

Covid Economics 80, 9 June 2021: 1-44

number of cases. The programme run until the end of August. The fact that a decline in new cases can be seen one week after the conclusion of the EOHO scheme, indicates that may indeed have played a part in the higher than average case load, though it should be noted that in terms of spread of COVID-19 this EOHO induced spike is less than half of the increased cases caused by student migration at its peak.[32]

Doubly Robust Estimation TWFE is not the most efficient estimator in this instance. The Estimator developed by Callaway and Sant'Anna (2020) may yield better estimates. As an additional robustness check Figures 7 and 8 shows the heterogeneity by start dates as estimated by the Callaway and Sant'Anna estimator. The estimator reveals qualitative similar findings to the TWFE heterogenous treatment timing estimates presented earlier. But to summarise the previous discussion, there is considerable heterogeneity in the estimates of the student effect conditional on university start. The heterogeneity is due to the changes in COVID-19 restrictions which differed slightly across locations and became increasingly tighter across the whole of period between September and December. The changes in public policy obviously leave differential effects due to these policy changes.

[32]Note that though we find that EOHO accoutns for less than half the number of cases as the student peak, it is not our intention to estimate the effects of this policy, but this is the incidental estimate insofar as the neighbourhoods we study are affected by the EOHO policy.

Covid Economics 80, 9 June 2021: 1-44

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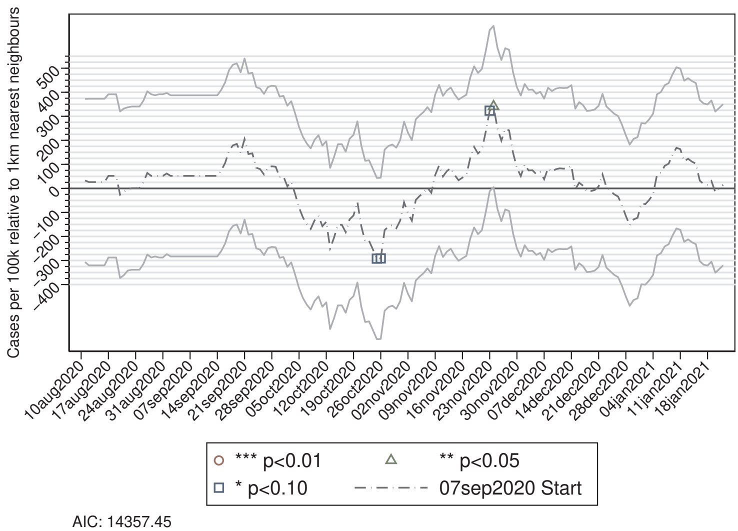
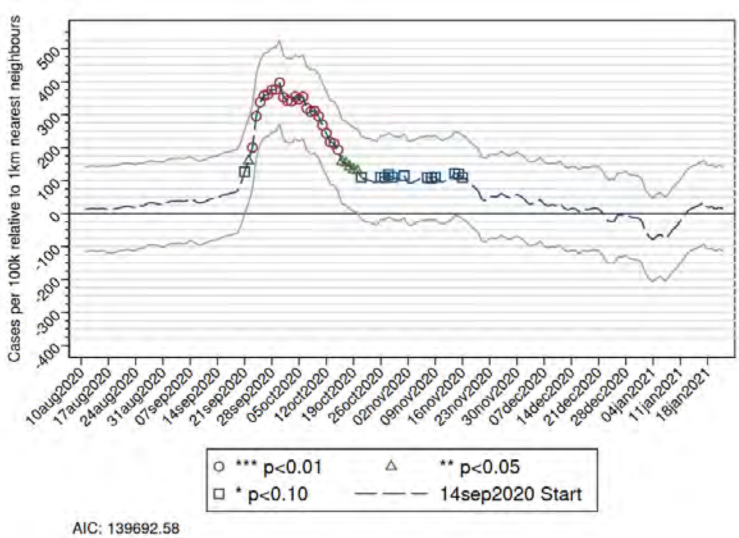
Figure 5: Daily cases in areas with halls relative to 1km nearest neighbours by semester start date, COVID-19 cases per 100k

(a) 07 September 2020 Semester Start

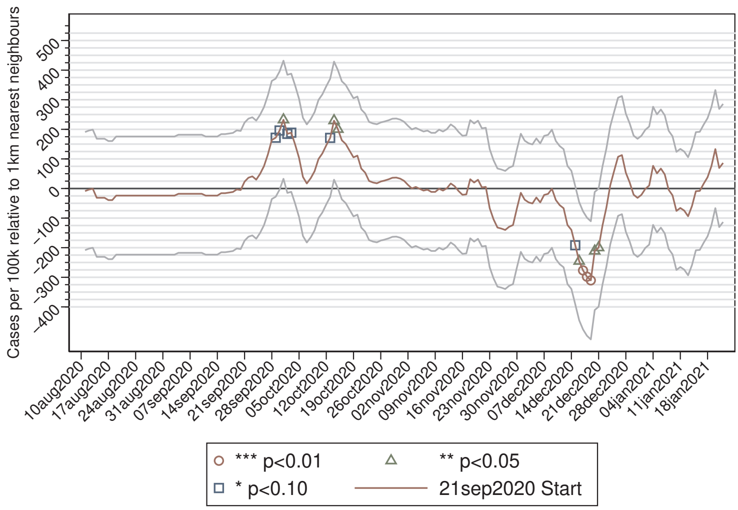


(b) 14 September 2020 Semester Start



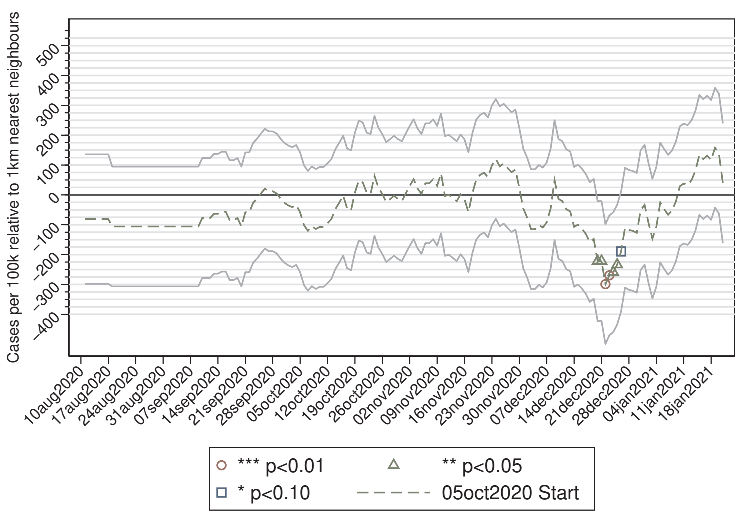
Covid Economics 80, 9 June 2021: 1-44

(c) 21 September 2020 Semester Start



AIC: 20409.19

(d) 05 October 2020 Semester Start

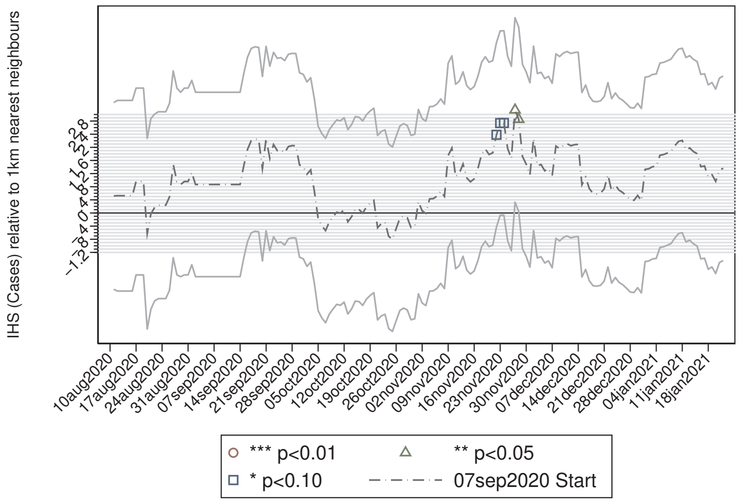


AIC: 5464.23

Note: Figures report the difference in daily COVID-19 cases between contiguous areas using (1). Each plot shows standard significance levels through different markers. Note each subfigure alters the comparison group for the treatment group where the comparison groups are drawn within $k$km of a student halls neighbourhood.

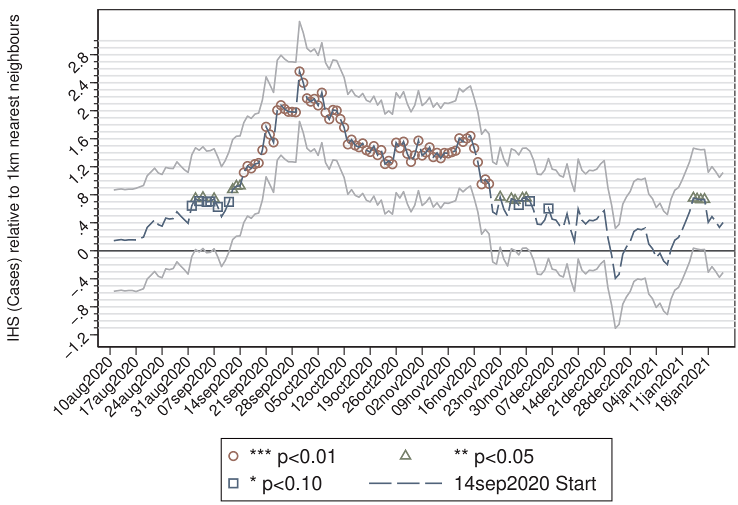
Figure 6: Daily cases in areas with halls relative to 1km nearest neighbours by semester start date, inverse hyperbolic sine of COVID-19 cases

(a) 07 September 2020 Semester Start



AIC: 3520.55

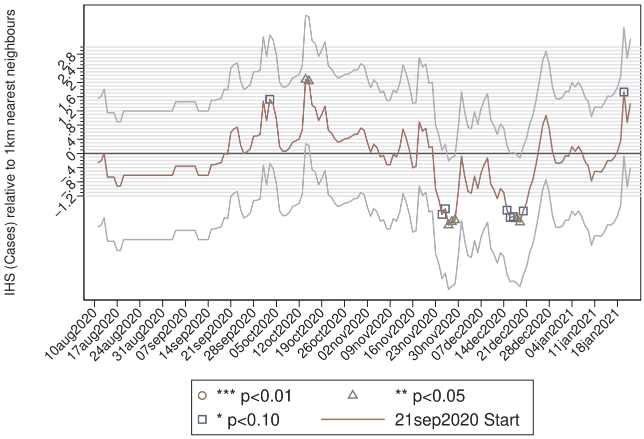
(b) 14 September 2020 Semester Start



AIC: 29465.44

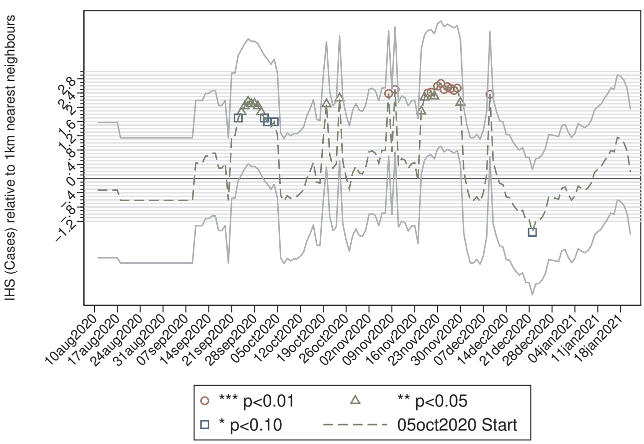
Covid Economics 80, 9 June 2021: 1-44

(c) 21 September 2020 Semester Start



AIC: 4801.04

(d) 05 October 2020 Semester Start



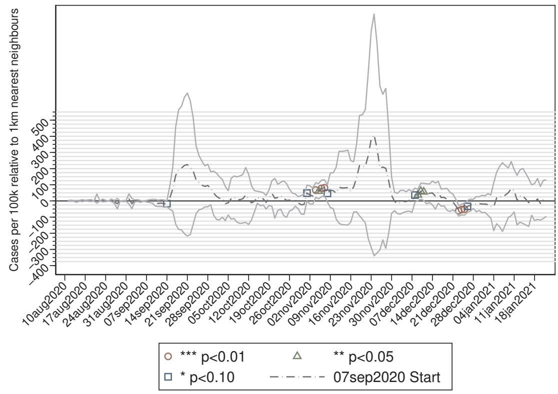
AIC: 829.9300000000001

Note: Figures report the difference in daily COVID-19 cases between contiguous areas using (1). Each plot shows standard significance levels through different markers. Note each subfigure alters the comparison group for the treatment group where the comparison groups are drawn within $k$km of a student halls neighbourhood.

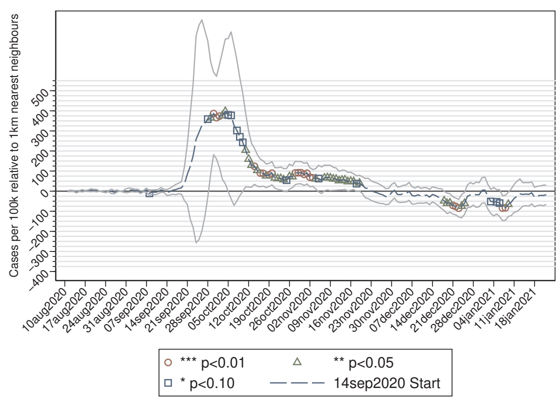
Covid Economics 80, 9 June 2021: 1-44

Figure 7: Doubly robust estimation of daily cases in areas with halls relative to areas within 1km using Callaway and Sant'Anna (2020), COVID-19 cases per 100k

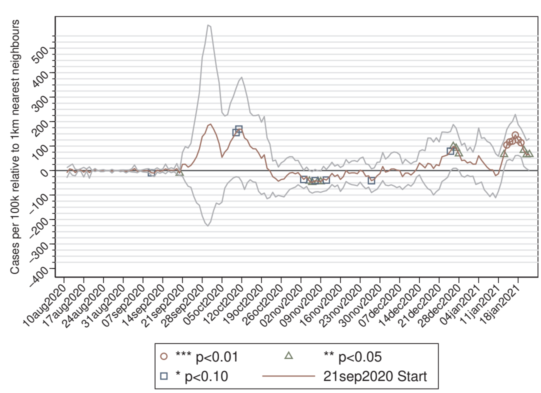
(a) 07 September 2020 Semester Start



(b) 14 September 2020 Semester Start



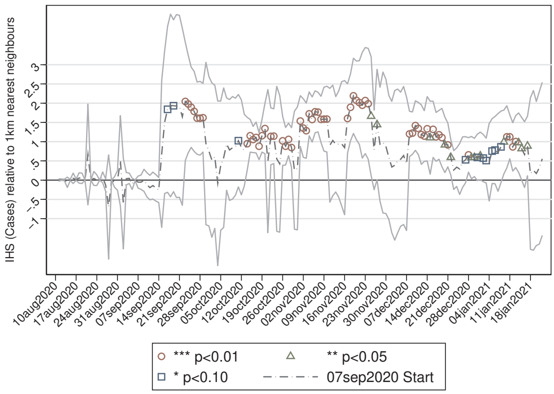
(c) 21 September 2020 Semester Start



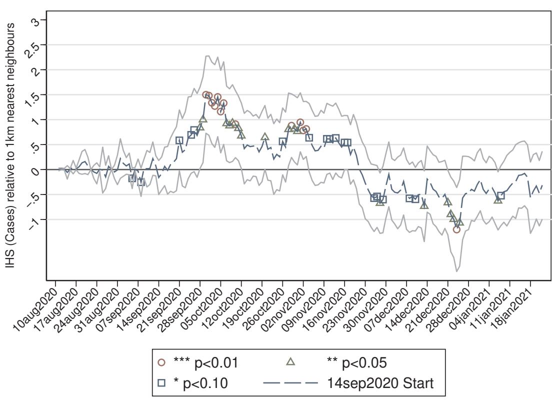
NOTE: Figures show estimates of daily cases in areas containing student halls compared to areas within 1km using the doubly robust estimator of Callaway and Sant'Anna (2020). The standard errors are bootstrapped. The estimates here are obtained using their R package `-did-` and Nick Huntington-Klein's `-att_gt-` wrapper for Stata (v.0.2.0).

Figure 8: Doubly robust estimation of daily cases in areas with halls relative to areas within 1km using Callaway and Sant'Anna (2020), inverse hyperbolic sine of COVID-19 cases

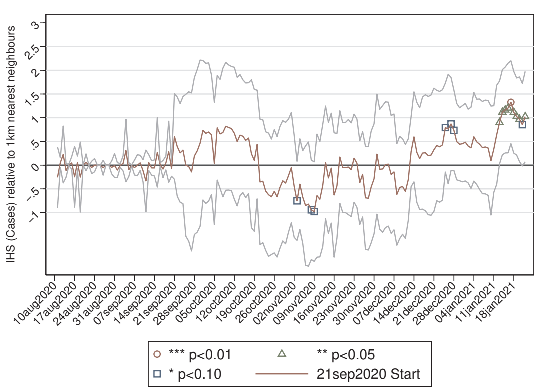
(a) 07 September 2020 Semester Start



(b) 14 September 2020 Semester Start



(c) 21 September 2020 Semester Start



NOTE: Figures show estimates of cases in areas containing student halls compared to areas within 1km using the doubly robust estimator of Callaway and Sant'Anna (2020). The standard errors are bootstrapped. The estimates here are obtained using their R package `-did-` and Nick Huntington-Klein's `-att_gt-` wrapper for Stata v0.2.0.

Covid Economics 80, 9 June 2021: 1-44

Covid Economics 80, 9 June 2021: 1-44

4.2 Spillover Analysis

This section aims to answer the second research question about the potential spread of COVID-19 in areas more distant to halls. To answer this question, we will now turn to what we term the 'donut' analysis. This analysis excludes all neighbourhoods containing student halls. Instead, the estimation will focus on the cases in those areas that were utilised as a comparison group in the previous analysis (i.e., the nearest locations to those containing halls) relative to their surrounding areas, i.e., those that are further away from the student residences. In other words, their comparison group is drawn from the same k kilometre distance from the neighbourhoods adjacent to student halls. So, for example, the 1km donut compares those neighbourhoods within 0-1km of student halls and contrasts the impact with that of the neighbourhoods which are 2-3km from student halls. Similarly, the 5km donut compares neighbourhoods within 0-5km of student halls and contrasts them with those neighbourhoods 5-10kms from student halls.

The results from this analysis are plotted in Figures 9 and 10. The estimates suggest that regardless of which sized donut one picks there is a substantial increase in the number of cases in those neighbourhoods near student halls, relative to their adjacent neighbourhoods. The specification which minimises the AIC is that of the 3km donut where one compares the neighbourhoods 0-3kms to 4-6kms. The figures have been drawn in the same scale as the student halls' analysis, to further contextualise the onwards transmission. Those locations nearest to student halls see an increase in cases approximately one week after the main impact was shown on 14th September. This lag is consistent with the literature showing that cases that finds 3-5 days for incubation and for symptoms to present as between 5-14 days after being exposed (Lauer et al., 2020; Qin et al., 2020; Chun et al., 2021). The increase in cases is approximately an additional 20-80 daily cases per 100,000, and persist until the end of November. The inverse hyperbolic sine results show a consistent story, where the increase in cases represents a 20-50% increase in cases between the inner and outer donut.

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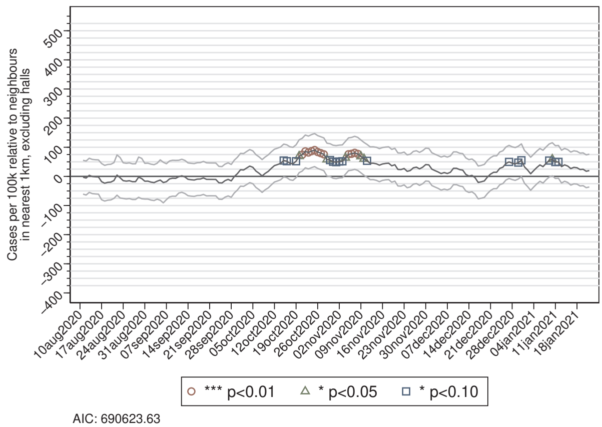
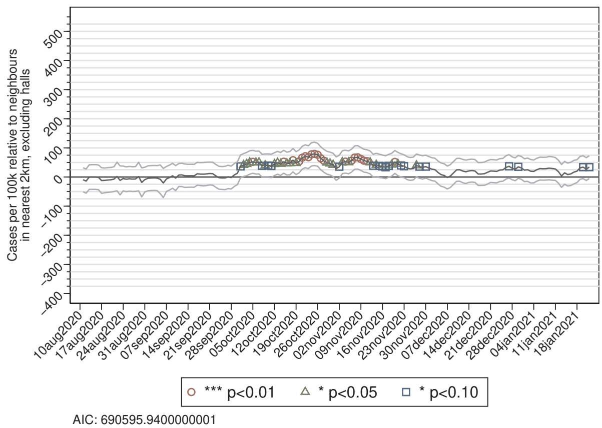
Figure 9: Estimates of impact of student halls on onward transmission, daily COVID-19 cases per 100,000

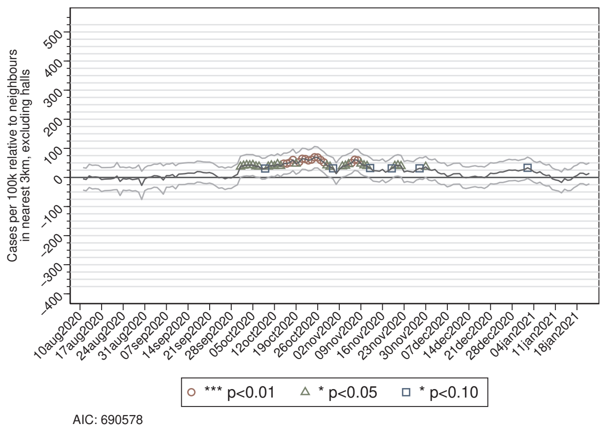
(a) 0-1km vs 2-3km of halls donut



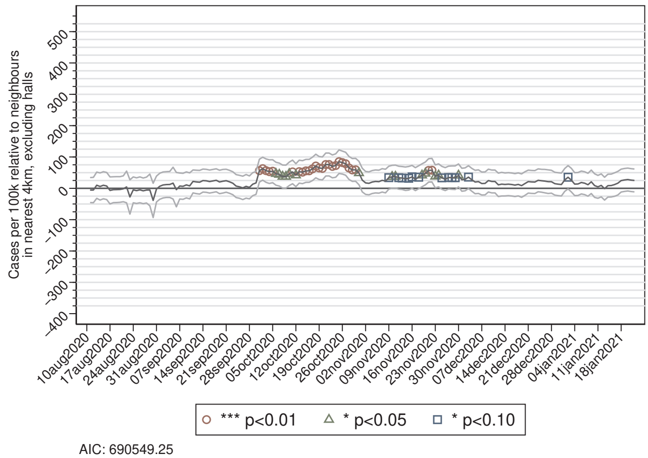
(b) 0-2km vs 3-5km of halls donut



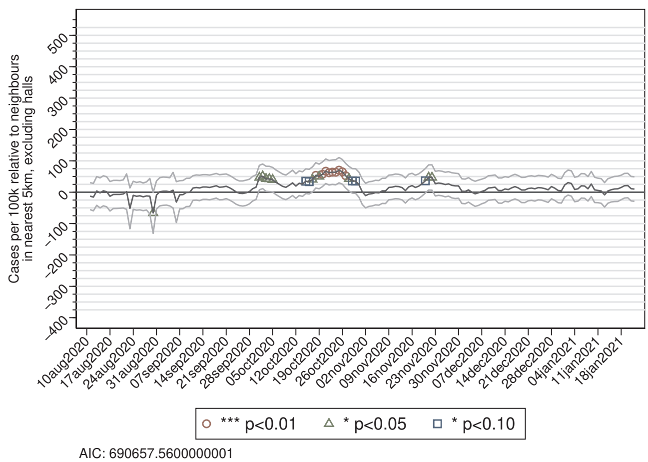
(c) 0-3km vs 4-6km of halls donut



(d) 0-4km donut vs 5-8km of halls donut



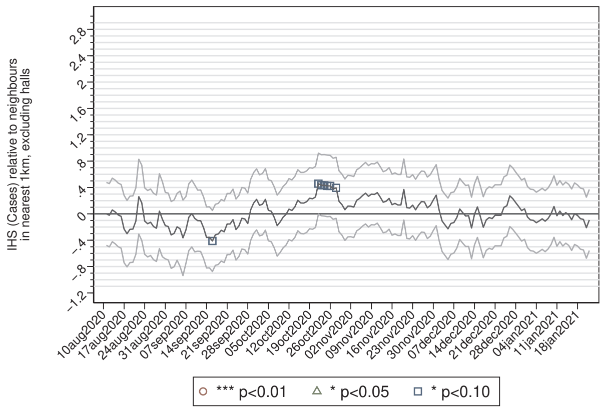
(e) 0-5km vs 6-10km of halls donut



Note: Figures report the difference in daily COVID-19 cases between contiguous areas using (1). Each plot shows standard significance levels through different markers. Note each subfigure displays a different donut, such that one is excluding all student hall neighbourhoods, and impact on onward transmission is obtained by comparing those neighbourhoods with in k km to student halls are compared to the next k km. For example, the 1km donut compares 0-1km nearest neighbourhoods with those 2-3kms. Confidence intervals from Neighbourhood Donut Halls Cluster and Month clustered standard errors are plotted. All regressions estimated here use the full battery of controls. Mobility: Daily Transport usage relative to January 2020. Health: Allowed to vary at monthly level, Comparative illness factor and housing overcrowding. Age: Effect allowed to vary monthly, proportion of over 50s living in neighbourhood. Note that due to data restrictions the mobility data do not cover all time periods, thus any temporary losses should be considered missing at random.

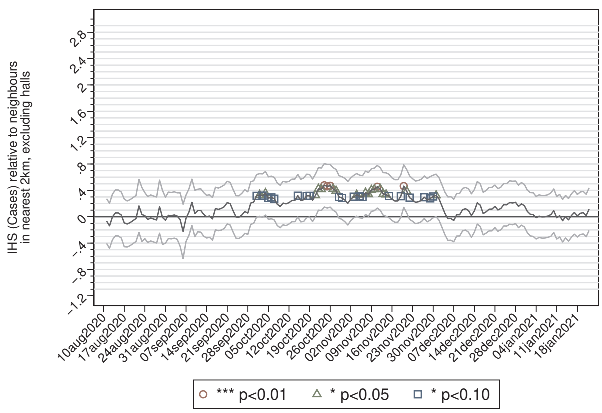
Figure 10: Estimates of impact of student halls on onward transmission, inverse hyperbolic sine of daily COVID-19 cases per 100,000

(a) 0-1km vs 2-3km of halls donut



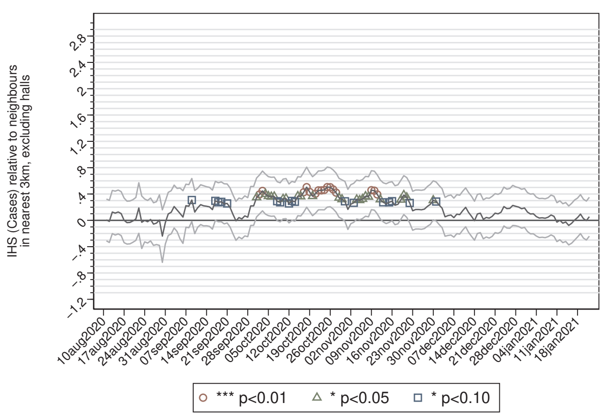
AIC: 147101.88

(b) 0-2km vs 3-5km of halls donut



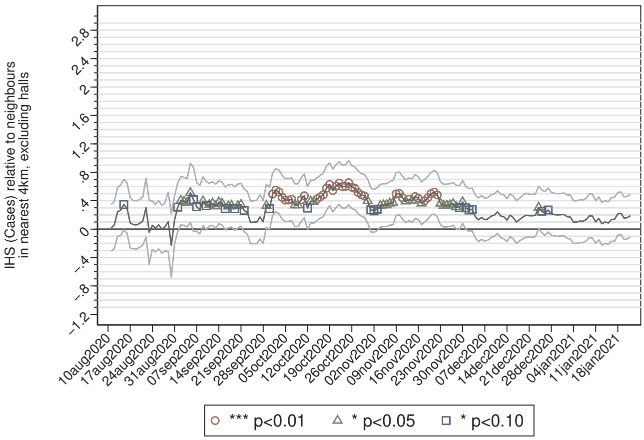
AIC: 147056.89

(c) 0-3km vs 4-6km of halls donut



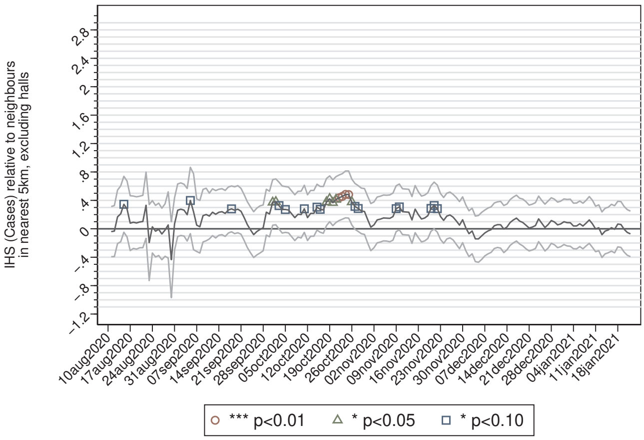
AIC: 146994.66

(d) 0-4km donut vs 5-8km of halls donut



AIC: 146966.28

(e) 0-5km vs 6-10km of halls donut



AIC: 147052.67

NOTE: Figures report the difference in daily COVID-19 cases between contiguous areas using (1). Each plot shows standard significance levels through different markers. Note each subfigure displays a different donut, such that one is excluding all student hall neighbourhoods, and impact on onward transmission is obtained by comparing those neighbourhoods with in k km to student halls are compared to the next k km. For example, the 1km donut compares 0-1km nearest neighbourhoods with those 2-3kms. Confidence intervals from Neighbourhood Donut Halls Cluster and Month clustered standard errors are plotted. All regressions estimated here use the full battery of controls. Mobility: Daily Transport usage relative to January 2020. Health: Allowed to vary at monthly level, Comparative illness factor and housing overcrowding. Age: Effect allowed to vary monthly, proportion of over 50s living in neighbourhood. Note that due to data restrictions the mobility data do not cover all time periods, thus any temporary losses should be considered missing at random.

5 Conclusions

This paper investigated the effect of Scottish University reopenings in September/October 2020 on COVID-19 transmission in the areas surrounding student halls of residence.

We created a dataset that merges information on the geolocation of student halls with data on daily COVID-19 cases at neighbourhood (Intermediate Zone) level from August 2020 to January 2021.

The empirical strategy followed a local dynamic difference-in-difference or event study design. The analysis estimated the difference in daily cases between adjacent areas for each day from 10th August 2020 to 22nd January 2021. Contiguous areas are more likely to be statistically similar, and our models included day and neighbourhood fixed effects. Nevertheless, we also control for baseline differences in socio-demographic and health characteristics at the neighbourhood level and public transport mobility at the local authority level.

First, we estimated the increase in daily cases in those Intermediate Zones where halls are located relative to their neighbouring areas, i.e., areas within k kilometres from student residences. Second, we focused our attention on estimating the spillover effects with a 'donut' analysis. The 'donut' analysis had the objective of detecting the potential transmission from halls to neighbouring areas. This analysis excluded areas where residences are located (the hole of the donut), and instead compare the daily cases in areas immediately adjacent to halls (inner ring of the donut) with their neighbouring areas (outer ring). This second strategy has the advantage to address also a potential concern of violation of the SUTVA arising from the first analysis.

We plan to expand the current study to capture hospital admissions, ambulance calls, and mortality. However, the extent and timing of the local transmission represents a crucial step to understand how to respond to outbreaks.

We found a substantial and persistent increase in cases in areas containing halls and evidence of persistent spillovers, i.e., transmissions to the wider community in areas contiguous to halls. These effects are linked to the return of students at the start of the academic year, especially for a group of Universities that started on 14th September. These groups include large Universities located in the major urban areas. The cases began to rise on 21st September, with 100 extra cases per 100,000 of the population, and peaked a week later with 400 per 100,000, after which they started declining. However, the number of cases remained stubbornly higher relative to their contiguous areas until the end of November, recording an additional 100 cases per 100,000 per day. These estimates represent a lower bound as they are compared to contiguous areas that also experienced a persistent increase in cases relative to their neighbours following University reopenings. For these neighbourhoods, daily cases went up by an additional 20-80 cases per 100,000 until the end of November.

Our analysis suggests that student in migration into halls represented a risk for areas hosting the halls their surroundings, despite the pre-existing national restrictions on household

Covid Economics 80, 9 June 2021: 1-44

mixing and indoor gathering, and Universities mitigation with class-size limits, physical distancing rules in common areas, and enhanced hygiene measures. Noting that in the vast majority of cases there was no in person teaching over Autumn 2020. The findings broadly invite a re-think of how close proximity activity in Universities can be resumed, as the pre-existing mitigations barring severe curtailment of trading activity were not successful in keeping the spread in check. Looking further ahead it behooves policymakers to consider if the existing guidelines in combination with the vaccine roll out is sufficient to prevent future outbreaks.

Our findings confirm that more stringent containment measures introduced by the Government and Universities in the second half of September served to flatten the curve. However, these restrictions have been extremely costly from society's point of view.

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Covid Economics 80, 9 June 2021: 1-44

Covid Economics 80, 9 June 2021: 1-44

A Key Dates and Coronavirus containment Measures

Table A1: Key measures in summer/autumn 2020 in Scotland

Date	Restriction
10 Aug 2020	The UK Government Coronavirus Bill and Second Coronavirus (Scotland) Bill were in place and Scotland were in phase 3 of the route map out of lockdown thus many restrictions were relaxed. Pubs, cinemas and restaurants were open and some non-contact sports and leisure activities were allowed. Social distancing rules of a 2m physical distance were in place indoors, outside and on public transport (unless seated at a restaurant). Mixing in private residences was banned and face coverings were compulsory in most public indoor settings; there were exceptions including mixing to care for vulnerable persons and those with physical or mental impairments or disabilities were not required to wear a mask. Two households totalling 8 individuals could meet outside at a 2m distance. Work from home for non-essential work was mandatory. Schools and nurseries were closed. Universities were closed for teaching and it had been announced that colleges and universities should make plans for a phased return to campuses in the upcoming academic year. Those suspected to have or have come in contact with someone with the virus had to self-isolate. Life events such as weddings and funerals were permitted with limited attendees. Aberdeen was under local lockdown. The Eat Out to Help Out programme to encourage people to go to restaurants had been implemented.
11 Aug 2020	Schools reopened with most face-to-face teaching back by 18th August.
14 Aug 2020	New legislation introduced for the hospitality industry, including Track and Trace.
28 Aug 2020	Increased restrictions surrounding social gatherings, large house parties indoors became a criminal offence.
31 Aug 2020	The last day of Eat Out To Help Out.
1 Sept 2020	Local restrictions in East Renfrewshire, Glasgow and West Dunbartonshire.
11 Sept 2020	Local restrictions extended to North and South Lanarkshire. The Scottish Government limited indoor and outdoor gatherings to six people from two households.
23 Sept 2020	Introduction of a national curfew for pubs, bars and restaurants and new restrictions on household visits.
23 Sept 2020	Universities ask students to avoid socialising outdoor during the weekend.
7 Oct 2020	Hospitality opening times restricted to 6am-6pm indoors, the sale of alcohol was prohibited. Licensed premises in the Central Belt required to close, with only takeaway services allowed. Outdoor live events, adult contact sports, indoor exercise classes were banned and snooker/pool halls, indoor bowling, casinos and bingo halls closed.
2 Nov 2020	Local authority protection measures were enforced through the Five-level Strategic Framework, each level indicating the level of protection needed based on the transmission of the virus in each local authority.
17 Nov 2020	Protection levels were updated, restricting travel outside of local authorities in Level 3 or Level 4 areas except for essential purposes.
8 Dec 2020	The first vaccines were distributed to those administering vaccines.
11 Dec 2020	Self-isolation period for positive contacts and overseas travel reduced from 14 days to 10 days.
25 Dec 2020	Restrictions eased for Christmas Day, allowing a maximum of three households to meet without observing social distancing and travel between the four nations.
26 Dec 2020	All of Scotland placed under level 4 restrictions; non-essential retail and hospitality closed and travel between Scotland and the rest of the UK prohibited, specific exceptions apply.
5 Jan 2021	Mainland Scotland entered lockdown, individuals had to stay at home except for essential purposes.
8 Jan 2021	The Scottish Government announced that University students must be taught online throughout January and February, unless 'attendance is critical and time-sensitive to the course or assessment', or due to essential placement or student well-being.
16 Jan 2021	Lockdown restrictions tightened, including prohibiting non-essential click and collect in level 4 areas and restrictions on the consumption of alcohol in public places.
15 Jan 2021	Travel corridors suspended. (i.e. foreign travel suspended for non-essential purposes)

Covid Economics Issue 80, 9 June 2021

Lives versus livelihoods during the COVID-19 pandemic: How testing softens the trade-off[1]

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Date submitted: 3 June 2021; Date accepted: 4 June 2021

The early COVID-19 pandemic literature focused on the conflict between lives and livelihoods. But the cross-country evidence reveals that across countries high mortality rates were often associated with large GDP contractions. We show that the presumed trade-off was associated with lockdowns as the primary instrument of containment. Early transition from lockdowns to testing-tracing-isolation-based containment softened the trade-off within countries and explains the absence of a trade-off across countries. We find that testing had positive indirect effects on growth and perhaps even positive direct effects. By allowing countries to relax shutdowns without compromising on containment, testing could have indirectly contributed to about a 0.6 percentage point boost in growth. By infusing greater confidence in people to step out and engage in economic activity, testing could have added another 0.6 percentage point to growth. As the world struggles to scale up vaccination in the face of new waves and variants, continued emphasis on testing could help limit infection without recourse to costly lockdowns.

Covid Economics 80, 9 June 2021: 45-77

1 The authors thank Young Kim, Norman Loayza, Ravindra Rannan-Eliya, and Aparnaa Somanathan for useful comments and suggestions. The findings, interpretations, and conclusions are entirely those of the authors. They do not necessarily represent the views of the World Bank Group, its Executive Directors, or the governments they represent.

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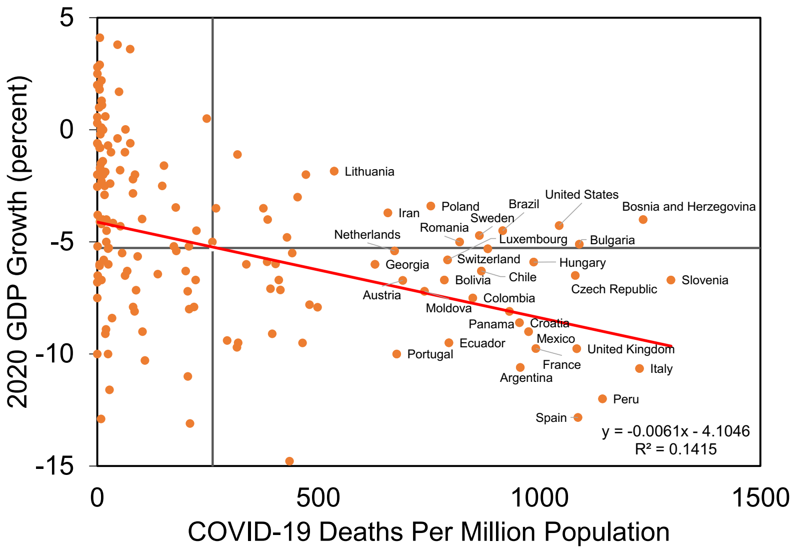
Covid Economics 80, 9 June 2021: 45-77

I. Introduction

The COVID-19 shock has destroyed lives and livelihoods across the world. In the early policy debate, countries were seen as facing a trade-off between "flattening the pandemic and recession curves" (Gourinchas, 2020). By this logic, heterogeneous health and economic outcomes across countries were in part the result of governments making different choices in the face of this trade-off. But the cross-country evidence reveals that many of the countries that suffered high mortality rates were often also the countries that saw the largest GDP contractions (Figure 1). Thus, saving lives was associated with saving rather than sacrificing livelihoods. Why the supposed trade-off did not materialize has been subject of much casual observation but little empirical analysis.

Figure 1. Lives vs. Livelihoods: correlation of growth and mortality in 2020



Note*:* Data obtained from World Bank Economic Monitoring, Global Economic Prospect-January 2021, and Oxford Covid-19 Government Response Tracker 2020. GDP growth (y-axis) are annual 2020 forecast obtained from Global Economic Prospect and World Economic Outlook. Mortality rate (x-axis) is calculated as the number of Covid-19 related deaths reported per one million population in 2020. Quadrants are classified by the mean thresholds of global GDP growth (y-reference line) and mortality rate (x-reference line). Labelled dots represent countries suffering significant mortality (above 500 per million). Red curve (downward sloping) represents the linear fitted line of the distribution.

Covid Economics 80, 9 June 2021: 45-77

Anecdotal evidence suggests that the observed heterogeneity in outcomes across countries is related to whether a country was able to transition away early and safely from debilitating economy-wide shutdowns. The difference across countries in the speed of transition can in turn be related, first, to whether a country imposed early and effective domestic and international shutdowns to suppress infections. Countries such as China and Vietnam did so and emerged relatively unscathed from the pandemic in health and economic terms; Brazil and the United Kingdom did not and suffered significantly on both counts. The second reason for the difference in duration of shutdowns across countries relates to the capacity for "smart containment." Countries like South Korea and New Zealand were able to move way quickly from economy-wide shutdowns because they were able to use testing, tracing and isolation, along with social distancing and masks, as instruments of containment; Indonesia and India were not.

Several studies have emphasized the role of smart containment, including targeted lockdowns, testing, tracing and other non-pharmaceutical interventions (NPIs), in fighting infections while lowering the economic costs of the pandemic (Acemoglu et al. 2021; Eichenbaum et al 2021; Glover et al. 2020; Brotherhood et al. 2020).[1] But all these studies rely on ex-ante simulations of the impact of alternative strategies. We have not so far seen an ex-post integrated assessment of the health and economic implications of smart containment.

Our argument is constructed in four steps. We begin by examining the determinants of COVID-19 infections in a panel for a sample of 174 countries. The focus is on policy actions that successfully lowered infections and deaths across countries. We find that both mobility restrictions and open COVID testing policy helped to contain the spread of infections. We also find that restrictions were more effective when implemented early in the pandemic (as reflected in the level of infections at the time). These results are confirmed in an array of econometric specifications and using multiple indices of government's policy responses.

[1] Most papers rely on extensions and simulations of the Kermack and Mc Kendrick (1927) SIR-based macroeconomic model to study the impact of smart containment on health and economic outcomes.

Second, we examine the factors that influenced reliance on shutdowns as a measure of containment. We find that reliance was significantly and negatively associated inter alia with levels of testing. This finding is consistent with the argument that greater capacity for containment through testing, tracing and isolation, allowed an earlier relaxation of stringent mobility restrictions.

Third, we assess how policy actions, as well as other country-specific factors, influenced economic growth during the COVID-19 shock in 2020. We identify four key correlates of growth performance: the severity of the COVID-19 impact; the measures taken to contain its spread; the exposure to the global economy; and the capacity of the government to provide fiscal support. As far as containment measures are concerned, growth was strongly negatively associated with shutdowns. In contrast, testing is positively (though less robustly) correlated with output growth across countries, even after controlling for the level of mortality rate and the stringency of lockdowns.

Finally, drawing the above elements together, we argue that the supposed trade-off between lives and livelihoods was associated with lockdowns as the primary instrument of containment. Testing softened the trade-off within countries and explains the absence of a trade-off across countries. Testing had positive indirect effects on growth and perhaps even positive direct effects. By allowing countries to relax shutdowns without compromising on containment, testing could have indirectly contributed to about a 0.6 percentage point boost in growth. It is possible that testing also infused greater confidence in people to step out and engage in economic activity, and thus could have added another 0.6 percentage point to growth.

This paper contributes to an emerging literature that studies the effectiveness of non-pharmaceutical interventions (NPI) that have been imposed by governments, which generally include lockdowns and mobility reduction, testing, and contact tracing. On lockdown measures, the existing literature suggests heterogeneous effects on disease containment.[2] Some evidence

[2] For instance, Askitas et al. (2020), Bonardi et al. (2020), and Weber (2020) argue that the closure of borders or travel restrictions had little effect. In contrast, studies on international air travel (Chinazzi et al. 2020, Keita 2020) find sizeable effects. Eckardt et al. (2020) find limited effectiveness of border controls during the first wave of

Covid Economics 80, 9 June 2021: 45-77

based on high frequency electricity data for a limited number of countries suggests that countries that implemented non-pharmaceutical interventions in the early stages of the pandemic appear to have lower cumulative mortality, compared with countries that imposed non-pharmaceutical interventions during the later stages of the pandemic (Demirguc-Kunt et al. 2020).[3] We add to this literature by providing an ex-post evaluation of the impact of early action along with other government interventions in reducing infections and deaths for a large set of countries.

We also contribute to the evidence that testing helped to lower infections. The importance of testing accompanied by rigorous contact tracing has been emphasized since the early stages of the pandemic (Rae and Friedman 2020, WEF 2020). Indeed, among various interventions including mask usage, school closures, restrictions on gatherings, intensive testing has been found to have the greatest impact on controlling the spread of COVID-19 and is the common characteristic among countries that successfully controlled the disease (Ranan-Eliya et al. 2021; Chinazzi et al. 2020; and Andrabi et al. 2020). We confirm the role of testing in controlling infections, and also show that testing contributed to growth by allowing the earlier relaxation of restrictions.

This paper also contributes to the rapidly expanding literature on the economic effects of the COVID-19 shock. Most existing papers have pointed to the damage of lockdowns in terms of employment losses, decline in spending, and deterioration in local economic conditions in the United States (Baek et al., 2020; Baker et al., 2020; Béland et al., 2020; Chernozhukov et al., 2020; Coibion et al., 2020; Gupta et al., 2020) and across different countries (Carvalho et al., 2020, Chronopoulos et al., 2020; Deb et al., 2020; Demirgüç-Kunt et al., 2020; Kaplan et al.

COVID-19 in 18 Western European countries. Effective implementation of lockdown measures, especially in capacity-constrained economies, is likely to be challenged by unfavorable socio-economic factors, such as the existence of extended informal sector, lacks comprehensive social protection, or government's limited fiscal space to provide financial support or to sustain stay-at-home restrictions (Loayza et al. 2020).

[3] In part, this is because the interventions have been less stringent. Moreover, there is evidence that COVID-19 mortality at the peak of the local outbreak has been lower in countries that acted earlier. In this sense, the results suggest that the sooner non-pharmaceutical interventions are implemented, the better are the health outcomes. Deb et al. (2020) and Fotiou and Lagerborg (2021) further show the importance of early implementation on the success of containment policies.

Covid Economics 80, 9 June 2021: 45-77

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2020; Fotiou and Lagerborg, 2021).[4] Several papers also study the impact of lifting lockdowns, showing that it led to only a limited rebound in mobility (Dave et al., 2020) and economic activity, and arguing that voluntary social distancing might have played a greater role than lockdowns (Allcott et al., 2020; Bartik et al., 2020; Kahn et al., 2020; Maloney and Taskin, 2020).[5] Our results suggest that the testing-induced shortening of lockdowns had a significant direct effect on economic activity and testing may also have boosted economic activity directly by helping relax private precautionary behavior.

The rest of the paper is organized as follows. Section II outlines a simple framework to analyze how containment policies--lockdowns and testing--influence health and economic outcomes. Section III discusses the data. Sections IV and V present the empirical strategies and results on the determinants of containment and economic growth, respectively. Section VI discusses the implications of our findings for the lives-versus-livelihoods debate. Finally, Section VII concludes.

II. Framework to analyze the effects of lockdowns and testing policies

We assume infections and economic output are functions of containment policies and other exogenous variables. Specifically,

$$I_i = I(L_i, T_i, Z_i^I) \qquad (1)$$

[4] Some papers use rich structural models of production to predict the damage of lockdowns, mostly finding very large effects on economic activities (Barrot et al., 2020; Baqaee and Farhi 2020; Bonadio et al., 2020; Cakmaklı et al., 2021a; Fadinger and Schymik, 2020; Inoue and Todo, 2020) and on firms' liquidity and solvency (Carletti et al., 2020; Gourinchas et al., 2020; Schivardi and Romano, 2020). Furthermore, some papers study how supply shocks may cause demand shortage (Guerrieri et al., 2020) and interact with nominal rigidities (Baqaee and Farhi, 2020).

[5] The literature also documents that the early phases of the pandemic have had a harsher effect on more economically vulnerable individuals, both in the United States and other countries (Alstadsæter et al., 2020; Béland et al., 2020). These individuals include those with lower income and educational attainment (Cajner et al., 2020; Chetty et al., 2020), minorities (Fairlie et al., 2020), immigrants (Borjas and Cassidy, 2020), and women (Alon et al., 2020; Del Boca et al., 2020; Papanikolaou and Schmidt, 2020). One reason is that lower-paid workers are often unable to perform their jobs from home (Barrero et al., 2020; Dingel and Neiman, 2020; Gottlieb et al., 2020). This warns of a potential widening of inequality (Mongey et al., 2020; Palomino et al., 2020) and speak to the importance of government's economic support policies.

Covid Economics 80, 9 June 2021: 45-77

where I_i represents infection for any country i. Containment policies include lockdowns, L_i, and testing, T_i. Z_i^I represents other exogenous variables that may affect infections.

We are interested in change of infections, so taking the total differential yields:[6]

$$dI = I_L dL + I_T dT + I_{Z^I} dZ^I \qquad (2)$$

Where dI refers to a change in infections/deaths in country i, and dL and dT refer to a change in government-imposed mobility restrictions and testing, respectively, and dZ^I refers to changes in other exogenous variables that affect infections. The terms I_L and I_T represent the marginal impact on infections of restrictions and testing, respectively, and are expected to have a negative sign. Equation (2) can be used to evaluate $\left.\frac{dL}{dT}\right|_{dI=0}$, which is the rate of substitution of testing for lockdowns for which infections remain unchanged. Then,[7]

$$\left.\frac{dL}{dT}\right|_{dI=0} = -\frac{I_T}{I_L} \qquad (3)$$

Similarly, economic output is a function of lockdowns and other government measures, such as testing:

$$Y_i = Y(L_i, T_i, Z_i^Y) \qquad (4)$$

Where Y_i, L_i, T_i, and Z_i^Y represent GDP, lockdowns, testing and other exogenous variables that may affect output for any country i.

The change in output can be presented as:[8]

$$dY = Y_L dL + Y_T dT + Y_{Z^Y} dZ^Y \qquad (5)$$

[6] One can also consider the time dimension. In that case, one can take the derivative with respect to time t and equation (2) can be written as $\frac{d}{dt} I_{it} = \frac{\partial I_{it}}{\partial L}\frac{\partial L_{it}}{\partial t} + \frac{\partial I_{it}}{\partial T}\frac{\partial T_{it}}{\partial t} + \frac{\partial I_{it}}{\partial Z^I}\frac{\partial Z_{i,t}^I}{\partial t}$.

[7] To arrive at equation (3), we assume that $\frac{dZ^I}{dT} = 0$, which says that other exogenous policy variables are targeting infections and are independent of testing.

[8] The derivative with respect to time t can be written as $\frac{d}{dt} Y_{it} = \frac{\partial Y_{it}}{\partial L}\frac{\partial L_{it}}{\partial t} + \frac{\partial Y_{it}}{\partial T}\frac{\partial T_{it}}{\partial t} + \frac{\partial Y_{it}}{\partial Z^Y}\frac{\partial Z_{i,t}^Y}{\partial t}$.

Covid Economics 80, 9 June 2021: 45-77

Covid Economics 80, 9 June 2021: 45-77

The change in output with respect to testing is the sum of a direct effect and an indirect effect due to the induced relaxation in lockdowns:[9]

$$\frac{dY}{dT} = Y_L \frac{dL}{dT} + Y_T \qquad (6)$$

For an unchanged level of infections ($\left.\frac{dL}{dT}\right|_{dI=0} = -\frac{I_T}{I_L}$), we obtain

$$\frac{dY}{dT} = -\frac{I_T}{I_L} Y_L + Y_T \qquad (7)$$

where $\frac{I_T}{I_L}>0$.

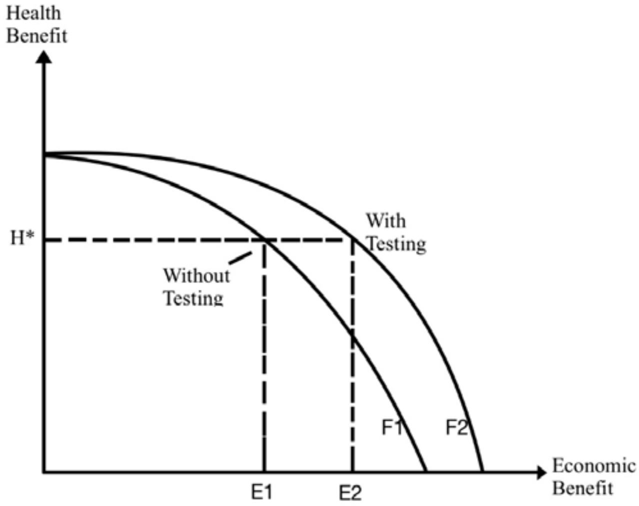
Equation (7) shows the indirect and direct channels through which testing could affect growth. The first term captures the indirect channel and shows that higher testing can make it possible to ease lockdowns while maintaining the same level of infections. The second term captures the direct effects of testing on growth, as testing may help alleviate supply constraints by allowing more workers to go to work and boost demand by increasing consumer confidence.

Visually, this discussion can be illustrated in a simple diagram. Figure 2 demonstrates the trade-off between the health benefit (e.g., lower infections and/or mortality) and the economic benefit (e.g., lowering economic output) associated with a more stringent government lockdown measure. In the absence of testing, the socially optimal level of the lockdown lies somewhere on the health-economic frontier curve 1 (the "inner frontier" F_1). The potential benefit of testing in helping to soften the trade-off associated with lockdowns is represented in Figure 2 as the rightward shift of the health-economic frontier curve, from F_1 to F_2. Testing can help attain any desired containment level, H^*, at a lower economic cost– measured as the horizontal distance from E_1 to E_2. In subsequent sections, we discuss both the direct effects of lockdowns and

[9] To arrive at equation (5), we assume that $\frac{dz_i^Y}{dT} = 0$, which presumes that other exogenous policy variables are independent of testing.

testing, as well as the potential indirect economic of testing through enabling lockdown relaxation.

Figure 2. How testing can soften the health-economic trade-off associated with lockdowns



Source: Authors' illustration.

III. Data

We rely on two primary sources of high-frequency data. To examine the effect of policy actions in containing the spread of the virus, we use daily data on confirmed COVID-19 infections and deaths reported by Oxford Covid-19 Government Response Tracker 2020 for the period of January 1–December 31, 2020. We also rely on several indices of government's policy responses from the same source.

Covid Economics 80, 9 June 2021: 45-77

The data suggest that the virus was rampant across most countries in the world during 2020 and that most countries witnessed waves of infections. We distinguish between three groups of countries: (i) the *mildly affected*, i.e., those suffered relatively few infections, defined as those countries for which the 15-day average of daily infection rate was always below the 10th percentile of highest infection rate for all countries in 2020;[10] (ii) the *under control*, i.e., those that experienced significant infection (above the 10 percentile threshold) but were able to successfully contain the spread; and (iii) the *ongoing*, i.e., those suffered significant infection without successful containment in 2020.

Out of 174 countries for which data is available, as of the end of 2020, only 17 were mildly affected, whereas 157 countries experienced significant infections (Table 1). The least affected group was predominantly in East Asia and Pacific (EAP) and Sub-Saharan Africa (SSA).[11] Less than a quarter of countries (36) managed to contain the disease after suffering significant infections. Over two-third of all countries were still facing high infections at the end of 2020. This group consisted of almost all countries in Europe and Central Asia (ECA) (48 of 49—with the exception being Greenland) and most countries in the other regions except for EAP and SSA.

Table 1. Summary statistics - widespread infection observed across most regions in 2020

Region	Number of Country	Not or mildly affected		Under control		Ongoing	
		Countries	Percent	Countries	Percent	Countries	Percent
East Asia and Pacific	20	7	35.00	5	25.00	8	40.00
Europe and Central Asia	49	1	2.04	0	0.00	48	97.96
Latin America and Caribbean	31	0	0.00	3	9.68	28	90.32
Middle East and North Africa	20	1	5.00	2	10.00	17	85.00
North America	3	0	0.00	0	0.00	3	100.00
South Asia	7	0	0.00	1	14.29	6	85.71
Sub-Saharan Africa	44	8	18.18	25	56.82	11	25.00
Total	174	17	0.00	36	0.00	121	0.00

Note: Authors' calculation based on daily data in 2020 from European CDC. Containment groups categorized based on infection data in 2020.

[10] The 10th percentile of highest infection rate in 2020 was 2.7 cases per million.
[11] Mattoo and Rannan-Eliya (2021) argue that COVID-19 infections in Sub-Saharan Africa are significantly higher than shown from available official data.

To assess the determinants of economic growth during the COVID-19 shock, we utilize quarterly GDP data for a large sample of 88 advanced and emerging & developing economies (EMDEs).[12] The shock to economic growth was severe and widespread (Table 2). On average, the year-on-year GDP growth decreased by over 11 percent in the second quarter of 2020 relative to the 2019Q2. The decline was approximately twice as large for advanced economies compared with EMDEs. During the third and fourth quarters, most countries witnessed less severe contractions; the growth contraction dropped to below 4 percent in advanced economies, and to about 1 percent in emerging markets and developing economies.

Table 2. Summary statistics – economic activity dropped sharply during 2020

		Quarterly GDP growth (y-o-y) (*percent*)		
Year	Quarter	All countries	Advanced Economies	Emerging-Market and Developing Economies
2018	1	3.75	2.61	5.41
2018	2	3.55	2.55	5.03
2018	3	3.05	2.04	4.55
2018	4	2.74	1.69	4.16
2019	1	2.62	1.72	4.05
2019	2	2.65	1.54	3.97
2019	3	2.58	1.69	3.77
2019	4	2.17	1.39	3.73
2020	1	-0.19	-1.20	-1.89
2020	2	-11.11	-11.27	-6.76
2020	3	-4.03	-3.86	-1.03
2020	4	-2.68	-3.13	1.49

Note: Data from World Bank's Global Economic Monitoring. Statistics for 2018Q1-2020Q3 is computed based on quarterly GDP data from 87 countries. Statistics for 2020Q4 is computed based on available quarterly GDP data from 83 countries.

The largest economic contractions were observed in South Asia (SAR) (-24.27 percent; population-weighted average) and Latin America and the Caribbean (LAC) (-15.22percent) during the second quarter (Table A1). Output growth recovered after the second quarter, with

[12] The sample if determined by availability of quarterly GDP data.

Covid Economics 80, 9 June 2021: 45-77

EAP being the first region to report (barely) positive growth in the third quarter (1.51 percent), led by China and Vietnam.

IV. Correlates of COVID-19 infections

We assess the association between the COVID-19 fatality rate and the policy responses using panel data regressions. Fatality due to COVID-19 could be explained by measurable policy indicators, such as government policy to restrict human mobility, the availability of testing, and economic support provided during the pandemic. We test the following specification with daily data for 174 countries for the period January 01–December 31, 2020:

$$\Delta I_{it,t-L} = \alpha_0 + \alpha_1 I_{it-L} + \alpha_2 \{policies\}_{it-L} + \lambda_i + \sigma_t + \varepsilon_{it} \qquad (8)$$

where $\Delta I_{it,L}$ denotes the change in COVID-related fatality rate (deaths per thousand) for country *i* between dates *t-L* and *t,* with *L* denoting the lag in days. I_{it-L} refers to the number of deaths per thousand population at time *t-L,* when the policy was first implemented. $\{policies\}_{it-L}$ consists of the three policy indicators: (i) lockdown stringency (index score from Oxford Covid-19 Government Response Tracker 2020), (ii) the availability of open testing (binary indicator, constructed from the testing index from Oxford Covid-19 Government Response Tracker 2020), and (iii) economic support (index score from Oxford Covid-19 Government Response Tracker 2020). λ_i and σ_t represent country and day fixed effects, respectively. ε_{it} denotes robust standard errors.

To compare coefficients of policy indicators across different time lags, the growth outcome is standardized as a unit of standard deviation from the global mean growth rate. To control for possible endogeneity, we focus on changes in fatality rate at least 10 days after a policy is implemented, but also estimate specification (8) for various time lags. In essence, the association of policy responses and the containment of COVID-19, represented by α_2, is captured from within-country variations of the standardized infection growth rates across time.

Covid Economics 80, 9 June 2021: 45-77

Table 3. Correlates of COVID-19 fatality across policy lags

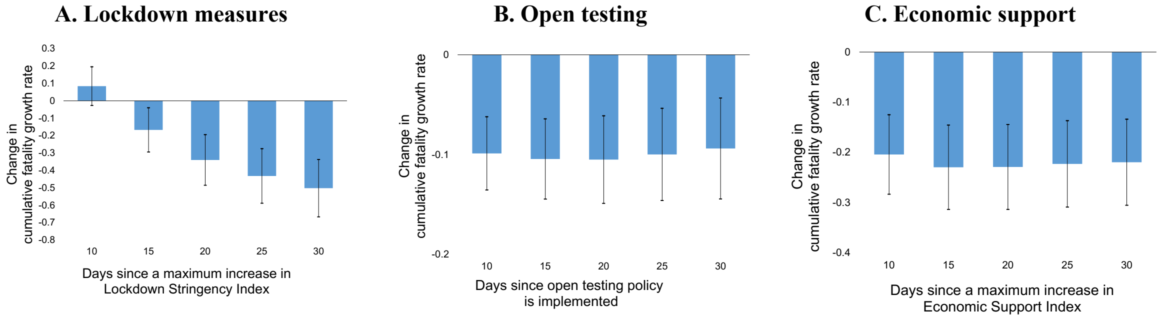
	Dependent variable: fatality growth rate (percent; standardized)				
	(1)	(2)	(3)	(4)	(5)
Policy Lag Period (days)	10 days	15 days	20 days	25 days	30 days
Open testing policy (0/1)	-0.0988***	-0.104***	-0.105***	-0.0998***	-0.0938***
	(0.0223)	(0.0244)	(0.0266)	(0.0281)	(0.0307)
Stringency Index [0-1]	0.0842	-0.167**	-0.340***	-0.432***	-0.502***
	(0.0675)	(0.0773)	(0.0884)	(0.0953)	(0.100)
Economic Support Index [0-1]	-0.204***	-0.230***	-0.229***	-0.223***	-0.220***
	(0.0481)	(0.0511)	(0.0514)	(0.0523)	(0.0522)
Cases per thousand (cases/1000)	-0.00560***	-0.00522***	-0.00436***	-0.00416***	-0.00462***
	(0.000699)	(0.000725)	(0.000765)	(0.000843)	(0.000985)
Constant	0.160***	0.322***	0.420***	0.468***	0.508***
	(0.0584)	(0.0742)	(0.0896)	(0.0976)	(0.101)
Observations	42,724	41,941	41,151	40,357	39,567
R-squared	0.364	0.305	0.249	0.226	0.228
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes
Date Fixed Effects	Yes	Yes	Yes	Yes	Yes

Note: The table presents result from daily regressions of the growth rate in new COVID-related deaths, standardized as a unit of deviation from the global mean, on representative lags of open testing policy (constructed as a binary indicator which equals 1 if testing is open and available to all, and 0 otherwise), lockdown stringency index (rescaled from [0-100] to [0-1] for chart representation), and economic-support index (rescaled from [0-100] to [0-1] for chart representation). Data collected from Oxford Covid-19 Government Response Tracker 2020 between January 01 and December 31, 2020. All regressions control for country and day fixed effects.

The measures taken to contain COVID-19 across the world, specifically restricting travel and mobility, testing (combined with tracing and isolating in some countries), and providing economic support, such as sick pay to encourage sick people to stay at home, are statistically significantly associated with a slower spread of COVID-19 infections (Figure 3; Table 3). The statistically significant negative correlations persist over time.

Covid Economics 80, 9 June 2021: 45-77

Figure 3. Correlates of COVID-19 fatality



Note: Authors' estimations, based on data from Oxford Covid-19 Government Response Tracker 2020 and Europe CDC, for January 1–December 31, 2020. This figure presents the point estimates from regressing cumulative growth rate in new deaths on lockdown index (Panel A), open testing policy (proxied indicator for public-health measure; Panel B) and availability of economic support (Panel C). We control for country-specific and day-specific fixed effects. Whiskers represent 95-percent confidence intervals of the estimates. The dependent variable (fatality growth rate) is standardized as a unit of deviation from the global mean. The testing policy index is constructed as a binary indicator which equals 1 if testing is open and available to all, and 0 otherwise based on data on testing policy from Oxford Covid-19 Government Response Tracker 2020. The economic-support and lockdown-stringency indices are rescaled from [0-100] to [0-1].

A related question is whether there exists a benefit to an early introduction of containment policies. We interact each policy variable in the regression framework with the infection rate, defined as number of total infected cases per thousand, when the policy was introduced:

$$\Delta I_{it,t-L} = \alpha'_0 + \alpha'_1 ifr_{it-L} + \alpha'_2\{policies\}_{it-L} + \alpha'_3\{policies * ifr\}_{it-L} + \lambda'_i + \sigma'_t + \varepsilon'_{it} \qquad (9)$$

where all else remains the same as in Eq. (8), and $\{policies * ifr\}_{it-L}$ represents the interaction between the policy indicators and infection rate at the time of the introduction of the policy. The coefficient α'_3 reflects the additional benefit of a timely policy response, measuring how the effectiveness of policy measures depends on the infection rate prevailing when they are introduced.

Table 4 shows that for several specifications, especially the ones pertaining to longer lags, the coefficient α'_3 is positive and statistically significantly different from zero, suggesting that the

Covid Economics 80, 9 June 2021: 45-77

overall effect of lockdowns and testing on the fatality rate are stronger when they are introduced early, i.e., when the infection level is still low (Table 4).[13]

Table 4. Correlates of COVID-19 fatality: interaction effects

	Dependent variable: fatality growth rate (standardized)				
	(1)	(2)	(3)	(4)	(5)
	10 days	15 days	20 days	25 days	30 days
Testing Policy (binary; 0/1)	0.125	-0.135***	-0.365***	-0.475***	-0.557***
	(0.0758)	(0.0211)	(0.0986)	(0.107)	(0.113)
Lockdown Stringency Index [0-1]	-0.121***	-0.125***	-0.125***	-0.119***	-0.111***
	(0.0251)	(0.0280)	(0.0310)	(0.0328)	(0.0355)
Economic Support Index [0-1]	-0.217***	-0.238***	-0.235***	-0.232***	-0.236***
	(0.0489)	(0.0507)	(0.0499)	(0.0503)	(0.0505)
Total cases per thousand (cases/1000)	-0.00947***	-0.0140***	-0.0174***	-0.0209***	-0.0248***
	(0.00290)	(0.00318)	(0.00373)	(0.00432)	(0.00504)
[Lockdown Index] X [cases/1000]	-0.0105***	-0.163*	0.00762*	0.0132***	0.0171***
	(0.00333)	(0.0863)	(0.00415)	(0.00465)	(0.00510)
[Testing Policy] X [cases/1000]	0.00969***	0.00916***	0.00885***	0.00838***	0.00770***
	(0.00184)	(0.00199)	(0.00224)	(0.00244)	(0.00253)
[Econ Support Index] X [cases/1000]	0.00141	0.000257	-0.000544	0.000287	0.00248
	(0.00189)	(0.00197)	(0.00208)	(0.00223)	(0.00236)
Constant	0.161**	0.341***	0.455***	0.515***	0.566***
	(0.0638)	(0.0803)	(0.0969)	(0.106)	(0.111)
Observations	42,724	41,941	41,151	40,357	39,567
R-squared	0.364	0.305	0.249	0.226	0.229
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes
Date Fixed Effects	Yes	Yes	Yes	Yes	Yes

Note: The table presents result from daily regressions of the growth in COVID-related deaths, standardized as a unit of deviation from the global mean, on representative lags of open testing policy (constructed as a binary indicator which equals 1 if testing is open and available to all, and 0 otherwise), lockdown stringency index (rescaled from [0-100] to [0-1] for chart representation), and economic-support index (rescaled from [0-100] to [0-1] for chart representation), and the interactions of these policy determinants with the number of cases per thousand (same lag). Data collected from Oxford Covid-19 Government Response Tracker 2020 between January 01 and December 31, 2020. All regressions control for country and day fixed effects.

[13] We also test the correlation of government policies with the growth of infection rate (infected cases per thousand), and the results are similar: lockdowns, testing, and economic support are strongly correlated with a lower growth of infection rate (Tables A2 and A3).

Covid Economics 80, 9 June 2021: 45-77

Covid Economics 80, 9 June 2021: 45-77

V. Correlates of economic growth during the COVID-19 shock

We utilize a quarterly panel data estimation approach to assess the relevance of four country-specific factors that could have impacted economic growth during 2020, corresponding to the COVID-19 shock period: (i) the severity of COVID-19 virus in a particular country, measured by the number of COVID-19 deaths per million population (fatality rate); (ii) the policy response to contain the disease, which includes mobility restrictions and testing; (iii) exposure to the global recession, proxied by the country's dependence on tourism; and (iv) capacity to provide fiscal support, proxied by the country's gross debt position.

As far as the containment policies are concerned, for testing, we use indicators for both the extensive and intensive margins of testing, including the number of tests (in thousands) performed per confirmed infection case (intensive margin) and a dummy variable indicating the introduction of open public testing (extensive margin). For the lockdown, the intensity of the restrictions is captured by the quarterly average of the stringency index published by the University of Oxford.

Economic growth is affected by the extent of the disease, by measures to control the disease at home, and also by shocks emanating from abroad. We control for the exposure to the global recession by including the country's dependence on tourism (tourism revenues as a share of GDP). The tourism sector has been hit hard by cancellations of trips by non-residents (Gössling et al. 2020). Additionally, we control for the capacity of the governments to provide fiscal support by including the country's gross debt position (government gross debt as a share of GDP). Likewise, the ability to respond to the crisis is captured by the country's fiscal space reflected in the level of government indebtedness. Tourism revenues to GDP and government gross debt to GDP are obtained from the World Development Indicators and refer to pre-COVID-19 data.

Our panel data framework for the growth regression is as follows:

$$\Delta y_{i,t} = \alpha + \beta_1 \times COVID_{i,t} + \beta_2 \times R_{i,t} + \beta_3 \times E_{i,t} + \beta_4 \times G_{i,t} + \varepsilon_{i,t} \qquad (10)$$

where i refers to country and t refers to quarters. $y_{i,t}$ is the GDP growth rate of country i at time t. $COVID_{i,t}$ refers to the fatality rate (total deaths per million), $R_{i,t}$ refers to the government response (lockdown stringency index, tests per case, and open testing indicator), $E_{i,t}$ refers to exposure to the rest of the world (proxied by tourism as a share of GDP), and $G_{i,t}$ refers to the capacity of the government to support the economy (proxied by gross government debt as a share of GDP). Finally, we follow Lane and Milesi-Ferretti (2011) and König and Winkler (2020) and control for GDP per capita as well as the average GDP growth rate between 2014 and 2019.

Table 5. Quarterly panel regression (Ordinary Least Squares)

	Dependent variable: quarterly GDP growth (y-o-y)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total deaths per thousand	-0.0614***						-0.0362***	-0.0194**
	(0.00126)						(0.00114)	(0.000924)
Stringency index (daily mean)		-0.00109***					-0.000910***	-0.000624***
		(0.000182)					(0.000183)	(0.000230)
Tests per confirmed case (thousands)			0.00786***				0.00626***	0.00117
			(0.00267)				(0.00167)	(0.000914)
Open testing (0/1)				0.00336			0.00573	0.00936
				(0.00942)			(0.00861)	(0.00810)
Tourism (%GDP)					-0.00150**		-0.00165**	.
					(0.000649)		(0.000722)	.
Gross Debt Position (%GDP)						-0.000200**	-0.000156***	.
						(8.56e-05)	(5.54e-05)	.
Constant	-0.0630***	-0.0147	-0.0725***	-0.0739***	-0.0665***	-0.0570***	-0.00170	-0.0187
	(0.00687)	(0.0112)	(0.00709)	(0.00908)	(0.00738)	(0.0103)	(0.0135)	(0.0142)
Observations	311	311	294	311	303	296	280	279
R-squared	0.571	0.599	0.583	0.542	0.559	0.581	0.669	0.844
Number of quarters	4	4	4	4	4	4	4	4
quarter FE	Y	Y	Y	Y	Y	Y	Y	Y
country FE	N	N	N	N	N	N	N	Y

Note: The sample is a quarterly panel consisting of countries with available quarterly GDP data for the four quarters of 2020 as of April 20, 2021. Quarterly GDP obtained from Global Economic Monitoring (GEM). The dependent variable is year-on-year quarterly GDP growth. COVID-19 indicators (deaths per thousand, stringency index, and tests per case, and open testing indicator) collected from Oxford Covid-19 Government Response Tracker 2020. Tourism and gross debt position are pre-covid annual measures and obtained from World Development Indicators. All regressions control additionally for country's baseline (2019) annual GDP level and average quarterly GDP growth between Q1-2014 and Q4-2019. Standard errors clustered at the country level. *** p<0.01, ** p<0.05, * p<0.1.

The results show that countries that experienced more severe contractions in 2020 had higher fatality rates, imposed more stringent mobility restrictions, were more dependent on earnings from tourism, and had more highly indebted governments (Table 5). Specifically, every 100 less deaths per million population is associated with three-fifth of a percentage point increase in annual output growth on average. While we observe no significant direct effect of open testing policy on growth, the quantitative measure of testing—i.e., number of tests per case—is positively correlated with economic growth, even after controlling for the fatality rate and the stringency of lockdowns. An increase in tests per case by 1000 is associated with an increase in growth by 0.6 percentage point. A higher intensity of testing may have contributed directly to growth by alleviating private precautionary behavior and hence infused greater confidence in people to step out and engage in productive activity.

We run several robustness exercises. First, the intensity of government's restrictions is likely to have been influenced by the progression of the disease (both infections and mortality), as well as the economic hardship imposed on the population. This could lead to an endogeneity between economic growth and the restrictions imposed. To check if this empirical concern would affect our main result, we utilize a two-stage least squares (2SLS) estimation framework. In the first stage, the monthly restrictions on mobility indicator is regressed on the one-month lags of monthly industrial production growth (a proxy for economic growth in the absence of data on monthly gross output), monthly mortality and infection rates, and country and month fixed effects. In the second-stage regression, we use a quarterly lockdown stringency measure constructed by aggregating the monthly predicted values obtained from the first stage.

The results from the first-stage estimation shows that the severity of lockdowns is statistically significantly and negatively associated with the previous month's industrial production, positively associated with the previous month's cases, and negatively correlated with higher tests per case (Table 6). The previous month's fatality rate is positively correlated with lockdowns but the correlation is not statistically significant when controlling for both infection rate and fatality rate. These results are in line with the framework introduced in Section II, which argued that higher testing was likely to be associated with less stringent lockdowns.

Covid Economics 80, 9 June 2021: 45-77

Table 6. Two-stage Least Square: first-stage regression

Dep. var: Stringency Index (monthly aggregate)	
Industrial Production (monthly growth, YoY, L1)	-4.406**
	(1.937)
Monthly deaths per million (L1)	0.241
	(0.300)
Monthly cases per million (L1)	0.0359***
	(0.00699)
Tests per case (L1)	-0.00135***
	(0.000259)
Constant	1,622***
	(16.87)
Observations	765
R-squared	0.805
spec	panel
months	3-12
month FE	Y
country FE	Y

Note: first-stage result from the 2SLS estimation framework discussed in the text. Monthly stringency index is regressed on the one-month lags of monthly industrial production growth (a proxy for economic growth in the absence of data on monthly gross output), monthly mortality and infection rates, and country and month fixed effects.

Table 7 shows the results of the second-stage estimation, where the constructed IV for government's lockdown response, namely "Lockdown IV", is the instrument for the original indicator of stringency index. The findings are broadly consistent with the results in Table 5 as far as the policy variables are concerned.

Table 7. Quarterly panel regression (two-stage Least Square)

	Dependent variable: quarterly GDP growth (y-o-y)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total deaths per thousand	-0.0348***						8.82e-04	0.00458
	(0.000863)						(0.00244)	(0.00177)
Lockdown IV (daily mean)		-0.00366***					-0.00496***	-0.00346**
		(0.000855)					(0.00171)	(0.00137)
Tests per confirmed case (thousands)			0.00165				0.00548***	0.00198**
			(0.00182)				(0.00167)	(0.000837)
Open testing (0/1)				0.00799			0.000294	0.00153
				(0.00726)			(0.00978)	(0.00810)
Tourism (%GDP)					-0.00150**		-0.000655	.
					(0.000649)		(0.000879)	.
Gross Debt Position (%GDP)						-0.000200**	-0.000230***	.
						(8.56e-05)	(8.16e-05)	.
Constant	-0.0423***	0.143***	-0.0470***	-0.0509***	-0.0665***	-0.0570***	0.213**	0.131*
	(0.000965)	(0.0444)	(0.000258)	(0.00435)	(0.00738)	(0.0103)	(0.0859)	(0.0697)
Observations	311	243	292	311	303	296	219	217
R-squared	0.804	0.817	0.831	0.799	0.559	0.581	0.695	0.859
Number of quarters	4	4	4	4	4	4	4	4
quarter FE	Y	Y	Y	Y	Y	Y	Y	Y
country FE	Y	Y	Y	Y	N	N	N	Y

Note: The sample is a quarterly panel consisting of countries with available quarterly GDP data for the four quarters of 2020 as of April 20, 2021. Quarterly GDP obtained from Global Economic Monitoring (GEM). The dependent variable is year-on-year quarterly GDP growth. COVID-19 indicators (deaths per thousand, stringency index, and tests per case) collected from Oxford Covid-19 Government Response Tracker 2020. Lockdown stringency measure ("Lockdown IV") is constructed as a daily-mean of the quarterly aggregation of monthly predicted values obtained from a 2SLS first-stage regression of monthly stringency index on the one-month lags of monthly industrial production growth, monthly mortality and infection rates, and country-specific and month-specific fixed effects. Tourism and gross debt position are pre-covid annual measures and obtained from World Development Indicators. All regressions control additionally for country's baseline (2019) annual GDP level and average quarterly GDP growth between Q1-2014 and Q4-2019. Standard errors clustered at the country level. *** p<0.01, ** p<0.05, * p<0.1.

We also consider the possibility that economic agents responded to the rising health risk posed by COVID-19 by voluntary social distancing, which in turn could affect economic activity. To capture this possibility, we use a de-facto mobility reduction in place of a de-jure lockdown stringency imposed by the government. The results are similar when we replace the stringency index score with a quarterly-average measure of daily mobility reduction obtained from Google Mobility Data (Table 8).[14]

We also utilize monthly Industrial Production (IP) data between March and December 2020 to capture higher frequency variations in output. The results are largely consistent with the previous findings (Table A4).

[14] We use the mobility indicator associated with "Retails and Recreations" destinations as categorized by Google. Our result is robust to other indicators that refer to mobility to other destination categories, such as "Workplaces", or "Public Transits".

Table 8. Robustness: Quarterly panel regression (de-facto mobility reduction)

	Dependent variable: quarterly GDP growth (y-o-y)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total deaths per thousand	-0.0614***						-0.0152	5.24e-04
	(0.00126)						(0.00115)	(0.00105)
Mobility to retails (% change from baseline)		-0.00137***					-0.00131***	-0.00119***
		(0.000204)					(0.000216)	(0.000250)
Tests per confirmed case (thousands)			0.00786***				0.00662	0.00238
			(0.00267)				(0.00752)	(0.00484)
Open testing (0/1)				0.00336			0.00756	0.00789
				(0.00942)			(0.00873)	(0.00764)
Tourism (%GDP)					-0.00150**		-0.00127*	.
					(0.000649)		(0.000707)	.
Gross Debt Position (%GDP)						-0.000200**	-0.000169***	.
						(8.56e-05)	(5.32e-05)	.
Constant	-0.0630***	-0.0354***	-0.0725***	-0.0739***	-0.0665***	-0.0570***	-0.0221*	-0.0243***
	(0.00687)	(0.00901)	(0.00709)	(0.00908)	(0.00738)	(0.0103)	(0.0114)	(0.00761)
Observations	311	292	294	311	303	296	263	262
R-squared	0.571	0.663	0.583	0.542	0.559	0.581	0.712	0.860
Number of quarters	4	4	4	4	4	4	4	4
quarter FE	Y	Y	Y	Y	Y	Y	Y	Y
country FE	N	N	N	N	N	N	N	Y

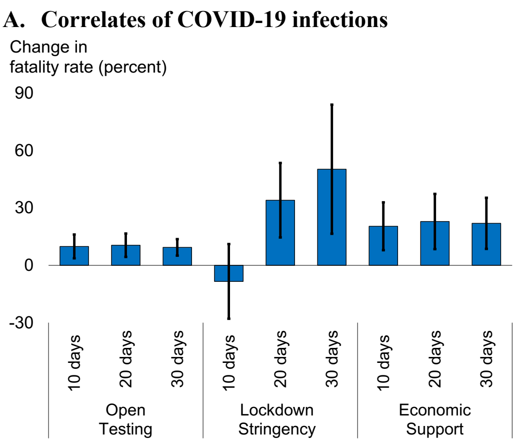
Note: The sample is a quarterly panel consisting of countries with available quarterly GDP data for the four quarters of 2020 as of April 20, 2021. Quarterly GDP obtained from Global Economic Monitoring (GEM). The dependent variable is year-on-year quarterly GDP growth. COVID-19 indicators (deaths per thousand and tests per case) obtained from Oxford Covid-19 Government Response Tracker 2020. Mobility reduction, measured as a percentage change from baseline, is obtained from Google Mobility Index. Tourism and gross debt position are pre-covid annual measures and obtained from World Development Indicators. All regressions control additionally for country's baseline (2019) annual GDP level and average quarterly GDP growth between Q1-2014 and Q4-2019. Standard errors clustered at the country level. *** p<0.01, ** p<0.05, * p<0.1.

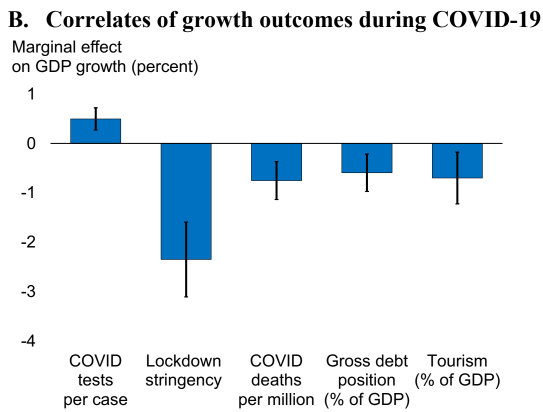
Covid Economics 80, 9 June 2021: 45-77

VI. Softening policy trade-offs

In order to better visualize the policy tradeoffs between saving lives (i.e., containing the disease at the cost of lowering economic activity) and saving livelihoods (i.e., resuming economic activity at the cost facing greater infection spread), we draw together the results presented in Sections IV and V.

Figure 4. Determinants of disease containment and growth





Note: Authors' calculation, based on data from the World Development Indicators, Global Economic Monitoring, Europe CDC, and Oxford Covid-19 Government Response Tracker 2020. Panel A presents point estimates from a daily regression from January 01–December 31 2020, of change in fatality rates, standardized as a unit of deviation from the global mean, on open testing policy (constructed as a binary indicator which equals 1 if testing is open and available to all, and 0 otherwise), lockdown stringency index (rescaled from [0-100] to [0-1]), and economic-support index (rescaled from [0-100] to [0-1]). The model controls for country and day fixed effects. Three separate point estimates presented for specifications with one-day, one-month, and two-month lags of policy responses. Whiskers represent 95-percent confidence intervals of the estimates. Panel B presents estimates from the sample of quarterly panel consisting of countries with available quarterly GDP data in 2020 (as of April 20, 2021). The dependent variable is year-on-year quarterly GDP growth. All quarterly explanatory indicators—tests per case, lockdown stringency, and deaths per million—are standardized and expressed in unit of standard deviation from global mean for each quarter. Gross debt position and tourism are annual measures at baseline (2019), also standardized and expressed in unit of standard deviation from global mean. Bar heights represent the sizes of the estimated coefficients. Whiskers represent 95-percent confidence intervals.

Figure 4 suggests that lockdowns are effective policy measures to contain the spread of the disease (Panel A) but entail a substantial cost to economic growth (Panel B). On average, reducing the average daily lockdown stringency by ten index points—a fifth of the world's median daily stringency score in 2020 (52)—would boost GDP growth by approximately one percentage point. In contrast, open and comprehensive testing policies are positively associated with both containment and growth outcomes, even after controlling for the level of mortality rate and the stringency of lockdowns. On average, every one thousand additional tests per positive case is associated with a one percentage point increase in output growth. As noted above, more testing may have infused greater confidence in people to step out and engage in economic

Covid Economics 80, 9 June 2021: 45-77

activity. Thus, the lives-livelihoods trade-off is associated with lockdowns; testing saves both lives and livelihoods.

For magnitude interpretations, the estimated coefficients of the 30-day lag regression reported in Table 3 (column 5) suggests that the government's introduction of an open testing policy can help the country to relax the degree of lockdown stringency by about 19 percent while keeping its one-month fatality rate unchanged.[15]

Relying on the estimates obtained from the growth regression that controls for country and quarter fixed effects (Table 5; column 7) and the global average increase in daily lockdown stringency index between Q1 and Q2 2020 (+55.8 index points), the introduction of open testing policy could have *indirectly* reduced global output contraction in 2020 by 0.63 percentage point.[16] That is, introducing open testing could allow governments to relax the degree of lockdown, thereby lowering the economic damage while maintaining socially desired levels of COVID-19 containment.

VII. Conclusion

This paper examined determinants of COVID-19 infections and economic growth outcomes. We find that countries that succeeded in containing the spread of the disease implemented early mobility restrictions, strong public health measures such as open COVID testing, and economic support packages. Countries that experienced greater growth contraction in 2020 had higher infection rates, imposed more stringent mobility restrictions, had more highly indebted

[15] This trade-off is obtained by computing the $\left(-\frac{\alpha_3}{\alpha_2}\right)$ ratio in Equation 8 using estimates from the 30-day fatality regression reported in Table 3, column 5: $\left(-\frac{\alpha_3}{\alpha_2}\right) = \frac{0.0938}{0.502} = 18.7\%$. Equivalently, a regression with infection rate serving as the dependent variable (Appendix Table A2) yields the trade-off ratio of 12.7%.

[16] The indirect economic benefit of testing (through relaxing lockdown) is computed as $\left(-\frac{\alpha_3}{\alpha_2}\right)$ x β_2 x σ, where $\left(-\frac{\alpha_3}{\alpha_2}\right)$ is the trade-off ratio obtained from Equation 8 (Table 3; column 5), β_2 is the coefficient associated with lockdown policy in Equation 10 (Table 5; column 8), and σ denotes the global average increase in daily lockdown stringency index between Q1-2020 and Q2-2020. The global average lockdown stringency index scores in Q1-2020 and Q2-2020 are 17.25 and 73.07 (out of 100), respectively.

Covid Economics 80, 9 June 2021: 45-77

governments, and were more externally exposed to global recession, as measured by the dependence on earnings from tourism.

Drawing these results together, it is evident that the presumed trade-off between lives and livelihoods was associated with lockdowns as the primary instrument of containment. Early transition from lockdowns to testing-tracing-isolation-based containment softened the trade-off within countries and explains the absence of a trade-off across countries. We find that testing had positive indirect effects on growth and perhaps even positive direct effects. By allowing countries to relax shutdowns without compromising on containment, testing could have indirectly contributed to about a 0.6 percentage point boost in growth. By infusing greater confidence in people to step out and engage in economic activity, testing could have added another 0.6 percentage point to growth.

As the world struggles to scale up vaccination in the face of new waves and variants, continued emphasis on testing could limit the spread of the disease and the need for costly lockdowns. Although vaccines can slow viral transmission, countries with substantial local incidence will still experience substantial transmission and deaths until very late in any vaccination deployment, which in many cases will take longer than a year. The extent of the human cost depends not only on vaccine efficacy, but also on how much interventions like testing reduce the effective reproductive number of the virus at the time a vaccine is deployed (Paltiel et al., 2021). The extent of the economic cost depends on how far outbreaks can be controlled without resorting to economically costly lockdowns.[17] The key implication is that countries will need to combine vaccines with continue emphasis on testing to bring the virus under control and save both lives and livelihoods.

[17] Delays in vaccinations in emerging markets and developing economies are estimates to lead to a significant global GDP loss through disruptions in trade (Çakmaklı et al. 2021b).

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Covid Economics 80, 9 June 2021: 45-77

Appendix

Table A1. GDP growth by region

Year	Quarter	Quarterly GDP growth (y-o-y)						
		EAP	**ECA**	**LAC**	**MENA**	**NAR**	**SAR**	**SSA**
2018	1	6.32	3.29	2.61	3.98	3.03	8.77	3.19
2018	2	6.22	3.04	1.89	3.84	3.22	7.59	2.65
2018	3	5.61	2.34	1.87	4.02	3.06	6.41	2.78
2018	4	5.60	1.73	0.96	4.25	2.46	6.32	2.68
2019	1	5.80	1.65	0.76	3.88	2.21	5.74	2.71
2019	2	5.39	1.73	1.31	3.63	1.98	5.33	3.09
2019	3	5.15	2.08	1.13	3.42	2.06	4.62	2.88
2019	4	4.93	2.24	0.86	3.32	2.27	3.30	2.54
2020	1	-3.77	-0.64	-1.54	2.20	0.25	2.87	2.56
2020	2	-0.74	-12.24	-15.22	-6.67	-9.41	-24.27	-7.05
2020	3	1.51	-3.47	-6.57	-2.44	-3.10	-7.18	-2.97
2020	4	3.30	-3.09	-2.68	-1.29	-2.47	0.46	-0.99

Note: Authors' computed population-weighted averages, using WDI 2019's population. Data from World Bank's Global Economic Monitoring. Statistics for 2018Q1-2020Q3 is computed based on available quarterly GDP data from 87 countries. Statistics for 2020Q4 is computed based on available quarterly GDP data from 83 countries.

Covid Economics 80, 9 June 2021: 45-77

Table A2. Correlates of COVID-19 infection across policy lags

	Dependent variable: change in infection rate (%; standardized)					
	(1)	(2)	(3)	(4)	(5)	(6)
Policy Lag Period (days)	10 days	20 days	30 days	40 days	50 days	60 days
Open testing policy (0/1)	0.0102	0.00837	-0.0357*	-0.0908***	-0.133***	-0.162***
	(0.0226)	(0.0247)	(0.0237)	(0.0211)	(0.0192)	(0.0179)
Stringency Index [0-1]	-0.243***	-0.228***	-0.281***	-0.357***	-0.449***	-0.548***
	(0.0310)	(0.0260)	(0.0280)	(0.0322)	(0.0355)	(0.0392)
Economic Support Index [0-1]	-0.149***	-0.166***	-0.194***	-0.218***	-0.234***	-0.217***
	(0.0359)	(0.0395)	(0.0383)	(0.0364)	(0.0353)	(0.0355)
Constant	0.217***	0.214***	0.289***	0.381***	0.473***	0.548***
	(0.0288)	(0.0268)	(0.0327)	(0.0378)	(0.0401)	(0.0416)
Observations	48,957	47,304	45,640	43,971	42,301	40,631
R-squared	0.142	0.088	0.116	0.165	0.214	0.244
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Date Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table presents result from daily regressions of the growth rate in new cases, standardized as a unit of deviation from the global mean, on representative lags of open testing policy (constructed as a binary indicator which equals 1 if testing is open and available to all, and 0 otherwise), lockdown stringency index (rescaled from [0-100] to [0-1] for chart representation), and economic-support index (rescaled from [0-100] to [0-1] for chart representation). Data collected from Oxford Covid-19 Government Response Tracker 2020 between January 01 and December 31, 2020. All regressions control for country and day fixed effects.

Covid Economics 80, 9 June 2021: 45-77

Table A3. Correlates of COVID-19 infection: interaction effects

	Dependent variable: change in infection rate (%; standardized)					
	(1)	(2)	(3)	(4)	(5)	(6)
Policy Lag Period (days)	10 days	20 days	30 days	40 days	50 days	60 days
Testing Policy (binary; 0/1)	0.0144***	0.0137***	-0.284***	-0.344***	0.0254***	0.0366***
	(0.00338)	(0.00224)	(0.0315)	(0.0328)	(0.00338)	(0.00423)
Lockdown Stringency Index [0-1]	0.0104	0.0104	-0.0363	-0.0941***	-0.135***	-0.158***
	(0.0252)	(0.0276)	(0.0263)	(0.0231)	(0.0207)	(0.0189)
Economic Support Index [0-1]	-0.140***	-0.162***	-0.191***	-0.214***	-0.235***	-0.230***
	(0.0354)	(0.0388)	(0.0368)	(0.0339)	(0.0318)	(0.0309)
Total cases per thousand (cases/1000)	-0.00962***	-0.00914***	-0.0147***	-0.0253***	-0.0405***	-0.0605***
	(0.00235)	(0.00219)	(0.00243)	(0.00293)	(0.00374)	(0.00484)
[Lockdown Index] X [cases/1000]	0.0144***	0.0136***	0.0135***	0.0169***	0.0254***	0.0366***
	(0.00215)	(0.00249)	(0.00243)	(0.00276)	(0.00312)	(0.00323)
[Testing Policy] X [cases/1000]	0.000764	-0.000211	0.00274	0.00671***	0.00867***	0.00851***
	(0.00173)	(0.00196)	(0.00197)	(0.00198)	(0.00216)	(0.00255)
[Econ Support Index] X [cases/1000]	-0.00284	-0.00153	-0.00147	-0.00128	0.00226	0.0120***
	(0.00200)	(0.00123)	(0.00173)	(0.00233)	(0.00327)	(0.00451)
Constant	0.234***	0.231***	0.305***	0.403***	0.512***	0.610***
	(0.0300)	(0.0277)	(0.0329)	(0.0383)	(0.0410)	(0.0429)
Observations	48,957	47,304	45,640	43,971	42,301	40,631
R-squared	0.143	0.088	0.116	0.166	0.216	0.248
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Date Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table presents result from daily regressions of the growth rate in new cases, standardized as a unit of deviation from the global mean, on representative lags of open testing policy (constructed as a binary indicator which equals 1 if testing is open and available to all, and 0 otherwise), lockdown stringency index (rescaled from [0-100] to [0-1] for chart representation), and economic-support index (rescaled from [0-100] to [0-1] for chart representation). Data collected from Oxford Covid-19 Government Response Tracker 2020 between January 01 and December 31, 2020. All regressions control for country and day fixed effects.

Covid Economics 80, 9 June 2021: 45-77

Table A4. Monthly panel regression with Industrial Production

	Dependent variable: monthly gorwth in Industrial Production (y-o-y)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total deaths per thousand	-1.260						-1.932	2.911
	(0.435)						(0.759)	(0.465)
Stringency index (daily mean)		-0.0983**					-0.132**	-0.111***
		(0.0380)					(0.0619)	(0.0417)
Tests per confirmed case (thousands)			-0.0172***				0.0427***	-0.0218***
			(0.00391)				(0.0145)	(0.00460)
Open testing (0/1)				-1.208			-1.148	-1.230
				(1.540)			(2.287)	(1.611)
Tourism (%GDP)					-0.156		-0.215	.
					(0.198)		(0.176)	.
Gross Debt Position (%GDP)						-0.0311*	-0.0251	.
						(0.0159)	(0.0184)	.
Constant	-6.767***	-0.729	-6.746***	-5.932***	-7.577***	-5.495**	4.702	1.277
	(0.214)	(2.359)	(0.00172)	(1.144)	(2.459)	(2.231)	(3.482)	(2.806)
Observations	676	676	668	676	646	666	636	636
R-squared	0.693	0.698	0.695	0.694	0.303	0.319	0.349	0.686
months	3-12	3-12	3-12	3-12	3-12	3-12	3-12	3-12
month FE	Y	Y	Y	Y	Y	Y	Y	Y
country FE	Y	Y	Y	Y	N	N	N	Y

Note: The sample is a quarterly panel consisting of countries with available monthly Industrial Production (IP) data for the months between March and December in 2020 (as of April 20, 2021). Monthly IP obtained from Global Economic Monitoring (GEM). COVID-19 indicators (deaths per thousand, stringency index, and tests per case) collected from Oxford Covid-19 Government Response Tracker 2020. Tourism and gross debt position are pre-covid annual measures and obtained from World Development Indicators. Standard errors clustered at the country level. *** p<0.01, ** p<0.05, * p<0.1.

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Covid Economics **Issue 80, 9 June 2021**

Liquidity constraints, cash transfers and the demand for health care in the Covid-19 pandemic[1]

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Date submitted: 24 May 2021; Date accepted: 31 May 2021

In this article, we propose the hypothesis that liquidity con-straints may delay or even prevent individuals infected with COVID-19 from seeking medical help. If this is the case, a cash transfer can directly increase the demand for medical care. We evaluated it empirically in the context of the Emergency Aid (EA) implementation, a large-scale cash transfer program in Brazil. We used the program's implementation calendar along with a Regres-sion Discontinuity in Time (RDiT) to assess the causal effects of EA on the search for the health system. Consistent with our hypothesis, we estimate that the transfer decreased the time to search for the health system by 13% and in- creased COVID-19 hospitalizations by 0.4%.We also estimate that the impact of the cash transfer on hospitalizations decreases with time, which is consistent with the liquidity constrain hypothesis.

Covid Economics 80, 9 June 2021: 78-93

1 We acknowledge valuable comments from Breno Sampaio, Eduardo Zilbermann, Guil-herme Lichand, Gustavo Gonzaga, Marcelo Fernandes, Marcelo Medeiros and Paulo Vaz. Any remaining errors are ours.

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Covid Economics 80, 9 June 2021: 78-93

1 Introduction

Several countries have established emergency cash transfer programs to deal with the direct and indirect negative effects of COVID-19. They sought to mitigate the economic crisis's impacts on the well-being and allow individuals to adhere to non-pharmacological measures. However, emergency transfers can generate another less considered desirable effect.

It is possible that poor individuals will be liquidity constrained and not be able to seek medical care if becoming ill. This might be the case even if there is no cost for healthcare. Let's consider an individual who works in the informal market without savings or access to credit markets. Even if the individual is infected and healthcare is free, the opportunity costs of his daily work might be too high, making him postpone or even prevent the search for medical care. A cash transfer can provide liquidity, allowing individuals to anticipate searching for the health system, reducing the risk of deteriorating health and successive infections.

Thus, the *first hypothesis* we test in this paper is that a cash transfer to individuals reduces the lag between individuals feeling sick and searching for medical care. The cash transfer might not only make sick individuals anticipate medical care but also allow those who would skip it entirely to search for a hospital. Henceforth, we also expect that the cash transfer increases the number of Covid-19 hospitalizations. However, the effect on hospitalizations is less straightforward than the effect on the delay in searching for medical care. Other than alleviating liquid constraints, the cash transfer also allows individuals to protect themselves further, which might decrease the probability of getting sick and being hospitalized.

These two countervailing effects (e.g., alleviating liquidity constraints and allowing further protection from infections) have different timings. The liquidity alleviating effect is felt immediately after the cash transfer is received, while protection from infections shows up on hospitalization only 7 to 10 days after the transfer. Thus, the *second hypothesis* of this paper is that the cash transfer increases hospitalizations up to ten days after the cash transfer is received. After this period, the treatment effect is uncertain and could reverse sign.

To evaluate this hypothesis, we took advantage of a natural experiment generated by the Emergency Aid (EA) granting in Brazil. The government granted this large cash transfer on specific days, according to the individuals' birth cohort. We use a Regression Discontinuity in Time (RDiT) to assess the causal effect of the EA on demand for medical care. We find that the treatment reduced, by 13%, the time between the onset of symptoms and the search for the health system in all analyzed time-horizons. It also increased hospitalizations by 0.4% up to 7 days after the cash transfer is received. Once we analyze larger time windows, the estimates for hospitalizations decrease and become statistically insignificant. Both results are consistent with the liquidity constraint hypothesis. We also implemented placebo estimates that reinforce reliance on the causal interpretation of results.

We contribute to three strands of literature. First, to the literature that analyzes if individuals are liquidity constrained in their demand for health care (Gross and Tabacman, 2014; Gross, Layton and Prinz, 2020). So far, these papers present mixed evidence on the importance of liquidity constraints. We contribute to this literature in two ways. First, we have a direct proxy for the necessity of seeking health care (the COVID-19 test and the day of the symptoms' onset). We show that alleviating individuals' liquidity constraints increases healthcare consumption (hospitalizations) but also that it anticipates necessary care. Our study context also brings new insights to this literature. The analysis of the Covid-19 pandemic illustrates that in preventing the seek for medical care, these liquidity barriers might have enormous costs to individuals. Also, healthcare has no direct financial costs in Brazil, which suggests that even daily individual earnings might be too high to be foregone.

The second literature strand we contribute to is the one that examines the direct effect of cash transfers or exogenous income increases on health (Lindahl, 2005; Dobkin and Puller, 2007; Cawley, Moran, and Simon, 2010; Evans and Moore, 2012; Van Kippersluis and Galama, 2013; Apouey and Clark, 2015). Most of these studies point to adverse health effects of income, mainly driven by changes in consumption. We show that, under certain circumstances, cash transfers might benefit not only the direct recipients but also important externalities for others' health.

Finally, the third strand this paper contribute to is the one that evaluates the impacts of cash transfers during the pandemic (Casado et al., 2020; Coibion, Gorodnichenko, and Weber, 2020, Ree-Jones et al., 2020 and Han, Meyer and Sullivan, 2020). We contribute to them by highlighting a neglected positive effect of cash transfers, a direct increase in the health system's demand.

2 Institutional background

COVID-19 hit Brazil particularly hard and, in order to fight the pandemic evolution, Brazilian states started a quarantine period in the second week of March. Economic measures were adopted to enable the implementation and maintenance of non-pharmacological measures. The most important one was the EA, a direct and unconditional transfer to unemployed individuals or informal market employees subject to an upper-income limit.

The program provided a monthly transfer of R$600,00 for all individuals and R$1.200,00 for single-parent households with children, representing around US$123.50 and US$ 265.00, respectively. The standard benefit represents approximately 40% of the household income per capita, 60% of the established minimum wage, and about twice the maximum benefit granted by the largest income transfer program prior to the pandemic, *Bolsa Família*.

The program had a significant penetration. Until July, around 66 million individuals have benefited from transfers. Initial research suggests that 50% of Brazilians lived in a household that received a transfer, leading to the lowest levels of poverty and inequality in the decade (Duque, 2020).

Transfers started being made at the end of April. In order to minimize

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agglomerations and avoid excessive burden on bank branches, installments were not transfered to all individuals simultaneously. Here is the calendar for the first aid payment:

Table 1: Aid availability dates

Cohort	Month of birth	Availabily dates
1	Jan/Feb	04/27
2	Mar/Apr	04/28
3	May/Jun	04/29
4	Jul/Aug	04/30
5	Sep/Oct	05/04
6	Nov/Dec	05/05

Note: Availability dates of the first emergency aid installment for different birth cohorts.

This calendar will be used in our empirical strategy. We focused on the first emergency aid payment, disregarding the other ones. After the first one, several parallel calendars were generated with overlapping payment dates, which prevents the expansion of the empirical analysis.

3 Data and sample

3.1 Data

We used two data sources, covering the period from March 1 to August 15, 2020.

Hospitalizations data: Microdata of hospitalizations for respiratory syndromes in the public health system (SUS). Detailed information on patients and medical procedures is available for the universe of hospitalizations. We are interested in the number of COVID-19 hospitalizations registered daily in each municipality.

Tests data: National microdata for COVID-19 tests. These data are generated by local health units and gathered by state entities. For each test, we observe its type, results, dates, and some individuals' characteristics. Individuals also report the date when they started experiencing symptoms. We are interested in the average time elapsed between symptoms and testing for each municipality.

3.2 Sample and descriptive statistics

For our empirical analysis, we are interested in two variables: the number

Covid Economics 80, 9 June 2021: 78-93

CEPR PRESS

of Covid-19 hospitalizations[1] [2] and the average time elapsed between the first symptoms reported until the search for the health system. We use the day individuals' made their first Covid-19 test as a proxy for the first time individuals reached the healthcare system[3].

We include in the sample all registered positive Covid-19 tests and Covid-19 related hospitalizations for working-age adults[4]. We aggregated these two variables of interest in three dimensions: municipalities, cohorts, and days. An observation indicates, for example, the number of Covid-19 related hospitalizations and the average of days it took for individuals to seek the healthcare system recorded among individuals in a cohort, in a given municipality, and on a given day[5].

In Table 2, we present descriptive statistics for our aggregate sample at the municipality/day level to facilitate interpretation:

Table 2: Descriptive statistics

	Average	Standard-deviation	Minimum	Maximum
Municipalities/day				
Hospitalizations	0.32	5.54	0	556
Deaths	0.11	1.83	0	198
Days to test	8.81	8.77	0	90
Observations				
Municipalities (A)		5,570		
Cohorts (B)		6		
Days (C)		165		
Observations (A*B*C)		5,514,300		

Note: Descriptive statistics for aggregate variables of interest at the municipality and day level. Total number of observations is given by multiplying municipalities, cohorts and days.

The table indicates that we have an average of one hospitalization every three days and one death every ten days for each municipality. In addition,

[1] We have information for the universe of hospitalizations in the public system, but not from the private system. This restriction decreases the generality of the sample, but we do not consider it a significant constraint. The public health system attends three-quarters of the Brazilian population (BAHIA e SCHEFFER, 2018). Also, since we are interested in individuals in situations of economic vulnerability, the potential variation in hospitalizations should be almost universally concentrated in the public health system.

[2] In the main analysis, we use the log of hospitalizations plus one as the dependent variable. Our main results are robust to different variable specifications.

[3] National guidelines required physicians to test for Covid-19 as soon as individuals with suspected symptoms looked for a hospital.

[4] We restrict the sample to working-age adults because only they can require the EA. The alleviation of liquidity constraints can have spillovers for individuals who do not receive the cash transfer but benefit from it (children in families that receive the EA, for instance). We discuss this possibility in section 5.1.

[5] Availability of testing time depends on the existence of at least one COVID-19 case for that observation.

Covid Economics 80, 9 June 2021: 78-93

individuals take an average of about nine days from the first symptoms to seek health care.

4 Empirical analysis

4.1 Regression Discontinuity in Time

To evaluate the impact of the EA on the demand for medical care, we propose a Regression Discontinuity in time (RDiT):

$$y_{mct} = \alpha + \beta * \mathbf{1}[d_t - EA_c > 0] + \lambda f(d_t) + \delta \boldsymbol{X}_{mct} + \psi_m + \epsilon_{mct} \qquad (1)$$

where y_{mct} is the variable of interest for municipality m, cohort c and day t,$\mathbf{1}[.]$ is an indicator function, d_t is the calendar day t; EA_c is the day on which cohort c receives the transfer and $f(.)$ is a flexible function. We also included weekday dummies ($\boldsymbol{X}_{mct}$) and fixed effects of municipalities (ψ_m) as control variables.

In our favorite specification, we included a five-day bandwidth and a linear local function on each side of the cutoff (IMBENS and LEMIEUX, 2008) and a uniform kernel. The bandwidth choice is similar to those suggested in the procedure of Calonico, Cattaneo, and Farrell (2020).

4.2 Identification and inference

The fundamental identification hypothesis is that the error term distribution is continuous around the cutoff. In this case, β, recovers the causal effect of EA on the variable of interest. Intuitively, we need to assume that non-observable confounders do not change discontinuously at the same time the EA is given.

As pointed out by Hausman and Hapson (2018), the RDiT framework generates specific difficulties for identification and inference: 1) impossibility of sorting tests (McCrary, 2008; Bugni e Canay, 2019) due to the uniform running variable; 2) the frequent need to use a very large window of observations and 3) non-independence of the terms of error per time dependence.

The first concern is not particularly important for our application. The aid implementation was drastic in preventing the money available to individuals before the official release day, seeking to avoid agglomerations. We can also test the presence of sorting around the cutoff behavior using the different timing cohorts that received treatment. We show these results that reinforce the confidence in the absence of sorting in section five.

Second, since we do not depend exclusively on time variation, we can use a very restrictive bandwidth of only five days in our favorite specification, which decreases the likelihood that time-variant confounders affect our estimates. Finally, in order to deal with the time-dependent error terms, we present standard errors clustered in two dimensions[6], at day and municipality level, similarly to Anderson (2014).

[6] We use the two-way clustering procedure suggested by Cameron, Gelbach, and Miller (2011).

Covid Economics 80, 9 June 2021: 78-93

CEPR PRESS

Covid Economics 80, 9 June 2021: 78-93

4.3 Time-varying treatment effects and bandwidth choice

Another specificity of the RDiT framework is that the implementation requires the specification of how the treatment effect varies according to time (Hausman and Hapson, 2018). Most recent studies that implemented this methodology assumed time-invariant treatment effects or only broadly discussed short and long-run differences in treatment effects[7].

In our context, we can make stricter predictions about the time-varying treatment effects of the cash transfer on the variables of interest:

- **Very short-run effects (up to one week after treatment)**: We expect the cash transfer to alleviate liquidity constraints and allow individuals to search for the health care system. Thus, we expect to observe an increase in Covid-19 related hospitalizations and a decrease in the lag between symptoms and the search for the healthcare system.

- **Short-run effects (after one week of treatment)**: The cash transfer not only alleviate liquidity constraints but also allows individuals to protect themselves against the disease and decrease the total number of contagions. This "protection" effect influences the number of hospitalizations with a lag of about one week, the Sars-CoV-2 incubation time (BACKER et al., 2020). Therefore, after seven days, we expect that the estimated treatment effect of the cash transfer on hospitalizations to be smaller than the estimated in the first week after treatment. On the other hand, we expect the effect of the treatment on the lag for individuals to seek healthcare not to vary considerably relative to the window of analysis.

4.4 Main results

The results of the estimates of the equation (1) for the variables of interest can be seen in Table 3[8]. We also present the results for several possible bandwidths: five days (our favorite specification); optimal choice of Calonico, Cattaneo, and Farrell (2020); twelve; fifteen; and twenty days.

[7] Several studies that implement the regression discontinuity in time regression methodology assumed constant treatment effects in their window of study (Busse et al., 2006; Auffhammer and Kellogg, 2011; De Paola et al., 2013; Bento et al., 2014, Gallego et al., 2014). Others presented a discussion of the differences between short-term and long-term treatment effects (Davies, 2008; Anderson, 2014).

[8] We implement Correia's (2017) procedure.

Covid Economics 80, 9 June 2021: 78-93

Table 3: Main results

Panel A: Delay to medical care					
Treatment	-0.791**	-1.019**	-0.742**	-0.766***	-0.958***
	(0.24)	(0.41)	(0.27)	(0.29)	(0.31)
Control average	8.06	8.06	8.06	8.06	8.06
Bandwidth	CCF	5	12	15	20
Effective N	5,859	14,879	48,727	68,078	115,716
Panel B: log(Hospitalizations)					
Treatment	0.003**	0.003***	0.001	0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Control average	0.02	0.02	0.02	0.02	0.02
Bandwidth	5	CCF	12	15	20
Effective N	321,840	386,208	965,520	1,287,360	1,931,040

Note: Estimates of equation (1). In panel A, the dependent variable is the number of hospitalizations and, in panel B, it is the average time from the first symptoms until testing. Each column shows a distinct bandwidth. We used a uniform kernel. clustered standard errors at the municipality and day level are in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

The results in Table 3 are consistent with the liquidity constraint hypothesis. We estimate that the aid causes a substantial decrease in the lag for medical care of up to 13% independent of the considered bandwidth. All estimates are statistically significant at the 5% level. We also observe a moderate size increase in hospitalizations of up to 0.4% in the days following the handing of the cash transfer. The small point estimate for hospitalization is driven by the rarity of these events. Still, these estimates imply that the cash transfer generated an increase of almost 400 hospitalizations in the days immediately following transfer.[9]

The effect of the cash transfer on hospitalizations decreases quickly as we increase the bandwidth size. This is consistent with our prediction, discussed in the last subsection, that the "liquidity effect" of the cash transfer is balanced by the the additional "protection effect" after the first week of treatment.

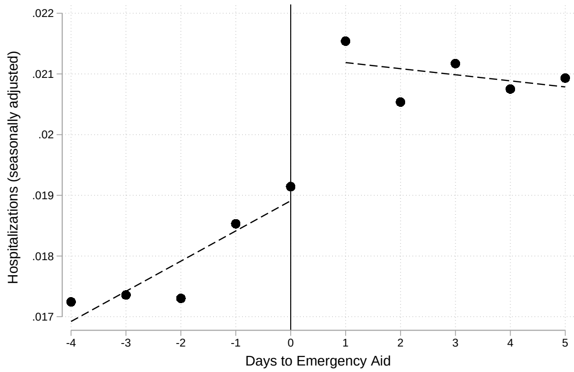
Figure 1 presents visual evidence of discontinuity in hospitalizations (panel A) and of the lag for medical care (panel B).

[9]Results are robust to changes in kernel and polynomials.

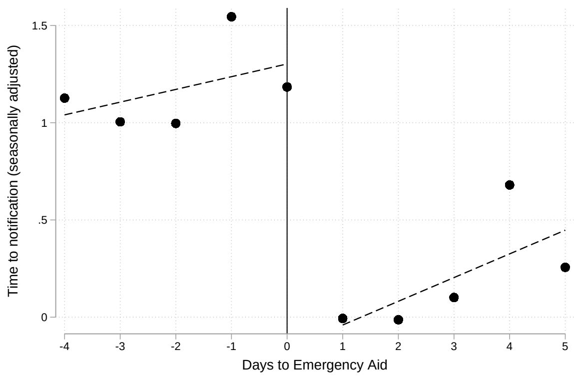
Covid Economics 80, 9 June 2021: 78-93

Figure 1: Visual discontinuity evidence

Panel A: Hospitalizations



Panel B: Lag to medical care



Note: Visual evidence for the estimate of the equation (1). The dependent variable is the number of hospitalizations, in Panel A, and the average time from the first symptoms until testing, in Panel B. We use our favorite specification.

5 Robustness checks

5.1 Threats to identification

As discussed in section 4.2, our empirical strategy is grounded on the idea that cash transfers discontinuously increase liquidity in a certain cutoff. The identification hypothesis is that non-observed covariates should be continuous around the cutoff.

In this section, we discuss threats to this identification hypothesis and some robustness checks that increase the confidence in the main results. The first concern is that there might be sorting around the cutoff. It is possible, for instance, that individuals anticipate their transfer and change their behavior before the specified cutoff. We argue that this concern is unlikely to explain our main results since poor individuals eligible to receive the program usually do not have any access to credit markets

A second concern is that there might be within-family spillovers. The day of the cash transfers is handed out to families is determined by the birth cohort of the person of reference in the family. Since the transfer increases liquidity for all family members, it might affect individuals of different birth cohorts. In section 5.2, we test the importance of these two first concerns by taking advantage of the fact that not all cohorts receive the transfer simultaneously.

A third concern is that our empirical framework is confounding spurious trends related to the dynamics of the pandemic as discontinuities. In section 5.3, we test the plausibility of this concern by randomly assigning cutoff dates and estimating placebo treatment effects using our main specification.

5.2 Sorting and spillovers

Cohorts five and six were paid seven and eight days after the first one, respectively. Thus, we can evaluate whether these cohorts already showed any discontinuous jump in variables of interest before payment was made. We can envisage that cohorts five and six received the transfer on April 27, and we estimate equation (1) using the five-day bandwidth favorite specification in this placebo, not letting the period overlap the one in which cohorts five and six starts receiving the payment. We expect not finding any effects in the absence of sorting and discontinuous confounders. Results are in Table 4:

Covid Economics 80, 9 June 2021: 78-93

CEPR PRESS

Covid Economics 80, 9 June 2021: 78-93

Table 4: Effects on the cohorts that have not yet been granted

	Hospitalization	Days to test
Panel A: Cohort 5		
Placebo	0.000	7.306
	(0.002)	(4.220)
Bandwidth	5	5
Panel B: Cohort 6		
Placebo	0.002	4.582
	(0.003)	(7.500)
Bandwidth	5	5

Note: Estimates of equation (1). We adjusted the granting days for cohorts five and six, for April 27. All estimates use our favorite specification. clustered standard errors at the municipality and day level are in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

We obtained lower and close to zero estimates for hospitalizations and opposite sign effects, positive, for the delay in seeking the health system. The pattern is the same for both cohorts and all four estimates are not statistically significant, reinforcing the reliance in the absence of relevant patterns sorting and spillovers.

5.3 Spurious trends

Another possible concern is our methodology confusing non-linear trends related to the pandemic with discontinuities generated by EA. To evaluate this possibility, we drafted a vector of six fictitious treatment dates $\overline{\boldsymbol{EA}}_c$ and assigned them to the six cohorts in the sample. So, we used this vector of dates and estimated the effects of placebo treatment on our variables of interest.

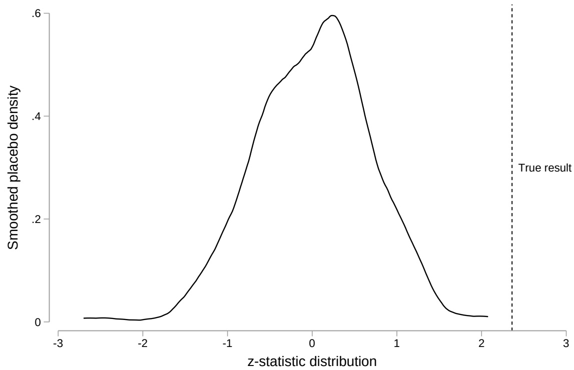
We repeated this procedure 500 times and, for each one, we recorded the associated z statistics. We expect not being able to reproduce z statistics as extreme as the true ones with these placebos.

In Figure 2, we present the estimated distribution of placebos:

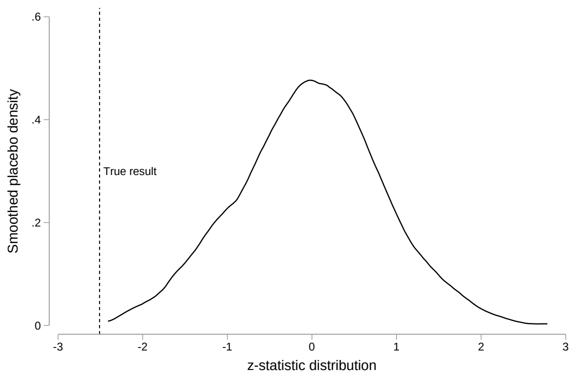
Covid Economics 80, 9 June 2021: 78-93

Figure 2: Distribution of z statistics for placebos

Panel A: Placebos for hospitalizations



Panel B: Placebo for time for reaching medical care



Note: Smoothed distribution of the 500 z statistics obtained with for $\overline{\boldsymbol{EA}}_c$ drafts. In Panel A, the dependent variable is the number of hospitalizations and, in Panel B, the time to seek medical care.

The estimated z statistics are centered around zero. In none of the 500 drafts that were carried out, we found z statistics as extreme as the true results obtained in Table 3, indicated by the vertical lines in the Figure above.

6 Conclusion

In this article, we evaluated the impact of a direct transfer to individuals on demand for medical care. We conjectured that liquidity constrained individuals might not being able to seek medical care when they are ill. We used a RDiT to identify the effect of the Emergency Aid in Brazil on demand for medical care. We estimated that the transfer increased COVID-19 hospitalizations by 0.4% and decreased the time spent in the search for medical care by about 13%, relative to control group averages. Our results highlight another dimension of direct transfers as a social benefit during the pandemic.

Covid Economics 80, 9 June 2021: 78-93

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Covid Economics 80, 9 June 2021: 78-93

Banks' liquidity management during the COVID-19 pandemic[1]

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Date submitted: 1 June 2021; Date accepted: 2 June 2021

How banks managed the COVID-19 pandemic shock? The eruption of the financial crisis in 2007 evolved to a crisis of banks as liquidity providers (Acharya and Mora, 2015). The COVID-19 pandemic shock was associated with a surge in households' deposits and a subsequent liquidity injection by the Federal Reserve. We show how the pandemic affected banks' liquidity management and therefore by extension, the creation of new loans. We empirically evaluate the creation and management of banks' liquidity through three well established mechanisms: market discipline (supply-side), internal capital markets (demand-side), and the balance-sheet mechanism which captures banks' exposure to liquidity demand risk. We provide novel empirical evidence showing that households increased savings as a precaution against future declines in their income. Also, depositors did not discipline riskier banks, and the internal capital market mechanism was not in work during the pandemic. Hence, weakly-capitalized banks were not forced to offer higher deposit rates to stem deposit outflows. Furthermore, weakly-capitalized banks increased lending in the first phase of the pandemic, while in the midst of the pandemic, they cut back new lending origination and increased their exposure to Fed's liquidity facilities. Well-capitalized banks on the other hand, increased lending in line with the increase in their deposits. Banks with higher exposure to liquidity risk were vulnerable to deposit outflows and increased their exposure in Fed's liquidity facilities significantly more than low-commitments' exposed banks.

1 For useful comments and discussions, we thank Manthos Delis, Nikolaos Kokonas, Ivan Lim, David Newton, Michael Nower, Steven Ongena, Emmanouil Platanakis, Dennis Philip, Rebecca Stratling and seminar participants at Durham University.

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Covid Economics 80, 9 June 2021: 94-154

1. Introduction

In December 2019, a novel coronavirus was detected in Wuhan, China and spread worldwide within less than three months causing a global health crisis. To contain the COVID-19 virus, governments launched unprecedented measures, including partial (work-from-home policies) or even full shutdown of businesses and economic activities. In the United States the COVID-19 pandemic triggered a severe economic downturn of uncertain duration. At the first phase of the pandemic-induced crisis firms drew heavily on credit lines. As a result, banks faced unprecedented credit line drawdowns which caused a dramatic spike in loan growth and stressed their liquidity. As a response, the Federal Reserve introduced liquidity schemes to facilitate financial institutions in distress condition. In the 2007 Global Financial Crisis, increased takedown demand for unused credit lines displaced lending capacity, and consequently banks exposed to liquidity risk adjusted their credit exposures and did not honor their credit commitments to firms (Ivashina 2009; Cornett et al. 2011; Acharya and Mora, 2015).

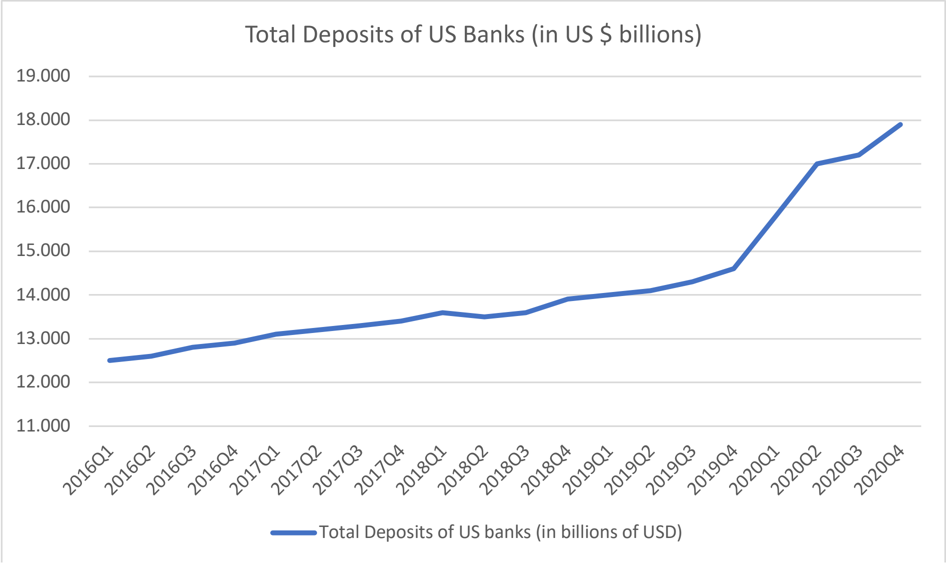
In this paper, we investigate how the COVID-19 pandemic shock affected banks' liquidity management and consequently, the supply of new loans? More specifically, we investigate how banks adjusted their lending, their deposit rates, and their exposure to Federal Reserve's liquidity facilities in response to the pandemic. We investigate these questions by studying both the supply and the demand side of banks' liquidity creation through three mechanism: i. the market-discipline theory (supply-side); ii. the internal capital markets theory (demand-side); and iii. the balance sheet channel, which captures banks' exposure to liquidity demand risk.

The supply determinant of banks' liquidity management investigates how banks' depositors reacted during the pandemic. Deposits are a critical source of funding for banks since an increase (decrease) in deposit rates reflects a rise (drop) in banks' cost-of-credit. As

Covid Economics 80, 9 June 2021: 94-154

figure 1 shows, households increased bank deposits for the whole period of 2020, with most notable the dramatic increase that took place in the first phase of the pandemic (quarter 1, 2020). Theory suggests that households increase bank savings either as a precaution against uncertainty in future income ("precautionary savings" theory, Browning and Lusardi, 1996), or as a shift to safer investments ("flight to safety" theory, Bernanke, Gertler, and Gilchrist, 1996). We show that the increase in deposits is a result of precautionary savings since households increased the inflow of deposits for both risky and safe banks. Furthermore, we find that in states with higher COVID-19 cases, banks experienced a significantly higher increase in deposit amount, and they offered significantly lower deposits rates than in states with lower COVID-19 cases. These results support the suggestion that households increased savings as a precaution against uncertainty in future income.

Figure 1. Aggregate deposits. This figure shows the quarterly aggregate deposits from 2016 till 2020.



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Financial intermediation theory suggests that depositors exercise market discipline by demanding higher deposit rates from weakly capitalized (hence, riskier) banks (e.g., see Diamond and Rajan, 2000). We use branch-level data of deposit rates in Certificate of Deposits (CDs) of U.S. banks from 2016 (pre-pandemic era) till the end of 2020 (pandemic era) to identify whether riskier banks offered higher rates to depositors. To effectively investigate the hypothesis, we test for both insured ($10,000 and $100,000) and uninsured CDs with amounts higher than $250.000, and we measure banks' riskiness based on their equity-to-assets ratio in line with the literature (e.g., see Ben-David, Palvia, and Spatt, 2017; and Brown and Dinc, 2017). If flight to safety and market discipline are exercised by depositors, then deposit rates should be higher for weakly capitalized (i.e., riskier) banks. We find that the coefficient on the equity-to-assets ratio is not negative implying that deposit rates are actually lower on average for weakly capitalized banks during the pandemic. This result indicates the absence of market discipline and provides further support to our suggestion that households increase savings as a precaution against uncertainty in future income. Our results remain similar across different deposit sizes.

Next, we investigate the demand determinant for deposits which suggests that banks adjust deposit rates based on their internal funding needs, that is the triple interaction between deposit growth, deposit rates and lending growth. Concretely, this mechanism suggests that banks use deposits as an internal capital market, that is they increase deposit rates based on their internal funding needs, such as to attract new deposits in order to originate new loans. Theory suggests that during distress conditions, banks have a sui generis advantage of using deposit inflows to fund their liabilities or to grant new loans (see for example Diamond and Dybvig 1983, and Diamond 1984). To tackle endogeneity concerns, we test the relation between bank-level deposit rates and loan growth based on the state-level loan growth. This approach has also been implemented by Ben-David, Palvia, and Spatt (2017), and one of its

Covid Economics 80, 9 June 2021: 94-154

advantages is that it is based on market-level lending activity rather than on the bank's endogenous lending decisions. We measure state-level loan growth as the median loan growth of single-state banks for states in which the bank operates.

Our expectation is to find a positive relationship between bank-level deposit rates and loan growth during stressful financial conditions, if the hypothesis holds and banks determine deposit rates based on their funding needs. We find that before the pandemic, an increase in the flow of deposits is positively related with both loan growth and deposit rates. Although this result is consistent with the internal capital market theory which suggests that banks offer higher deposit rates to attract more deposits, during the COVID-19 pandemic this relationship changes. Precisely, deposit flows are positively correlated with loan growth, but *negatively* correlated with deposit rates, which indicates that the internal capital market was not in work during the pandemic. The results remain similar across different categories of loans, such as for Net Loans and Leases, and for Commercial and Industrial (C&I) loans.

The third mechanism through which banks create and manage their liquidity is the off-balance-sheet unused credit commitments that can be converted to loans and therefore, represent liquidity risk for banks. In addition, we employ banks' exposure in wholesale funding that represents short-term liquidity commitments. In line with Acharya and Mora (2015), and Acharya, Engle and Steffen (2021), we define liquidity demand risk as banks' undrawn lending commitments and wholesale funding that exceed their deposits. We find that commitments-exposed banks with a liquidity shortage during the pandemic offered higher deposit rates to axe deposit outflows. Further, we document that banks with high liquidity risk increased their exposure in Fed's liquidity facilities significantly more than banks exposed to low liquidity risk. We also test whether banks responded to the liquidity shock by cutting back new credit to meet increased loan commitment. The results reveal that banks with high exposure in credit

Covid Economics 80, 9 June 2021: 94-154

commitments reduced the provision of new loans and leases. In contrast, banks with low credit commitments increased the supply of new loans and leases to firms.

Our paper contributes in four ways in the empirical literature that investigates how banks manage their liquidity and adjust their lending during crisis episodes. First, our work shows that during the COVID-19 crisis a precautionary savings effect is in work: in states with high COVID-19 cases deposit amounts increase significantly more and deposit rates decrease significantly more than in banks exposed to states with low COVID-19 cases. In a recent work, Hasan, Politsidis, and Sharma (forthcoming) find that syndicated loan spreads rise for lenders with exposure to COVID-19, a result which supports our suggestion that households increased deposits as a precaution to future declines in income caused by the COVID-19 shock. Second, we show that banks with high liquidity risk honored their credit commitments by adjusting their lending, in contrast to the 2007 Global Financial Crisis (Ivashina 2009; Cornett et al. 2011; Acharya and Mora, 2015). We also find that banks with low liquidity risk expanded the supply of commercial and industrial loans during the COVID-19 crisis. In a similar vein, Li, Strahan and Zhang (2020), show that banks were able to accommodate liquidity demands during the first quarter of the COVID-19 pandemic shock.

Third, we also contribute on the literature that investigates whether depositors exercise market discipline. Calomiris and Kahn (1991) and Calomiris (1999) advocate the market discipline theory as a regulatory tool where depositors are concerned about the safety of their deposits, and hence they demand a higher rate from riskier banks. Park and Peristiani (1998), Peria and Schmukler (2001), Hett and Schmidt (2017) provide empirical evidence that riskier institutions provide higher deposit rates. In contrast, Ben-David, Palvia, and Spatt (2017) find that deposit rates in the US during the financial crisis of 2008 were determined by internal capital markets rather than by market discipline. In a similar vein, we document the absence of market discipline during the COVID-19 pandemic.

Covid Economics 80, 9 June 2021: 94-154

Furthermore, we contribute on the literature that investigates banks' liquidity creation through their internal capital markets, which argues that banks create liquidity on the balance sheet by financing illiquid assets with liquid liabilities (Diamond and Rajan, 2000). Therefore, banks' internal demand for funding prompts a rise in their deposit rates in order to increase the flow of deposits. As a result, loan growth and deposit growth are strongly correlated. Ben-David, Palvia, and Spatt (2017) provide empirical support that this mechanism was in work during the 2008 financial crisis for banks. We document that during the COVID-19 pandemic the internal capital market was not in work. However, we also find that banks generated liquidity even though they lowered their deposit rates, a result which indicates that the injection of liquidity from the Federal Reserve alleviated the effect of the pandemic in banks' liquidity risk.

The rest of the paper is organized as follows. Section 2 presents a summary of the related literature. Section 3 describes the dataset and the econometric procedure used in the study. Section 4 presents and discusses the main empirical results. Section 5 provides additional tests for the robustness of our findings. Finally, section 6 concludes.

2. Literature Review

According to the financial intermediation theory, a fundamental feature of banks is the creation of liquidity and the subsequent transformation of risk, jointly referred to as banks' qualitative asset transformation (QAT) function (Bhattacharya and Thakor, 1993). Banks' risk transformation refers to the issuance of riskless deposits to finance risky loans. This theory argue that banks create liquidity when they issue riskless deposits to fund illiquid loans (Diamond 1984; Ramakrishnan and Thakor 1984; Boyd and Prescott 1986). Banks have two ways to create and manage liquidity through deposits: i. by rising rates to stem deposit outflows or to increase inflows, that is the supply of deposits by households (the market discipline channel); and ii. by rising rates to attract new deposits with the intention to increase their lending, that is the demand for deposits by banks (the internal capital markets channel). Banks' exposure to risky loans is associated with a third channel of liquidity pressure, the so-called liquidity demand risk (Acharya and Mora 2015). Unused credit lines are commitments that banks have to honor and especially during stressful conditions, corporations drawdown these loans to mitigate liquidity problems. Below we provide more analysis and develop our hypotheses.

2.1.1 Market discipline

More concretely, the financial intermediation theory suggests that during crises episodes, depositors are concerned about the safety of their deposits. Therefore, they punish riskier banks by requiring a higher rate, giving rise to the market discipline theory. In the empirical literature, Gorton (1988), Saunders and Wilson (1996), and Calomiris and Mason (1997), document that banks with worse fundamentals experience greater deposit outflows in a crisis. Calomiris and Kahn (1991), and Calomiris (1999) argue that, the market discipline theory operates as a regulatory tool where depositors demand a higher rate or withdraw their deposits as a form of discipline on risky banks. Park and Peristiani (1998), Peria and Schmukler (2001), and Hett and Schmidt (2017) provide empirical evidence that riskier institutions provide higher deposit rates. Also, the empirical literature in market discipline finds that larger banks experience higher deposit growth in a crisis (Martinez-Peria and Schmukler (2001), and that larger banks suffer fewer withdrawals than smaller ones (Billett, Garfinkel, and O'Neal, 1998; Goldberg and Hudgins, 2002).

In a similar vein, we test the market discipline hypothesis, that depositors require from riskier banks a higher premium for their deposits during a crisis, and in return riskier banks offer to pay higher deposit rates to stem deposit outflows during the COVID-19 pandemic.

Theory suggests that higher capital improves banks' ability to absorb risk and hence their ability to create liquidity. Therefore, for riskier banks (i.e., weakly-capitalized banks) the relation between deposit rates and the equity-to-assets ratio should be negative. Building on this hypothesis, we suggest that the injection of liquidity from the Federal Reserve during the COVID-19 pandemic, indirectly recapitalized riskier banks through its positive impact on banks' liquidity and consequently it helped to avoid the exercise of market discipline.

2.1.2 The internal capital markets mechanism

Another way through which banks create and manage liquidity is the internal capital market. This mechanism is in work when banks increase deposit rates with the aim of boosting their deposits to fund new loans. Houston, James, and Markus (1997), Campello (2002) and Aschraft and Campello (2007) show that loans increase and decrease with the level of deposits providing empirical support for the importance role of the internal capital market in the creation and management of liquidity. Banks can respond to a funding shock by reallocating funds across locations through their internal capital markets. For the internal funding reallocation dynamics, Cetorelli and Goldberg (2012a), (2012b) provide direct evidence for the existence of a cross-border capital market where international banks transfer deposits across different regions to support new lending opportunities or to overcome liquidity shocks. A notable contribution in the literature is the work of Ben-David, Palvia, and Spatt (2017) which shows that deposit rates are mainly driven by banks' incentives for new loans rather than to stem deposits outflow.

Similarly, we test the internal capital markets theory hypothesis which suggests that banks determine deposit rates based on their funding needs. Precisely, banks' incentives to provide new loans drives deposit rates higher and by extension deposits grow. Then, the increase in deposits is used to fund new loans. Therefore, our second hypothesis suggests that

Covid Economics 80, 9 June 2021: 94-154

the relationship between deposit growth, lending growth and deposit rates should be positive. Since, the results show that this mechanism was not in work during the pandemic, we test an additional hypothesis which suggests that the introduction of ample liquidity by the Federal Reserve during the COVID-19 pandemic helped banks to improve their liquidity levels. Therefore, banks achieved to increase their deposits, to provide new loans, while lowering their cost of capital -i.e., their deposit rates-.

2.1.3 Balance-sheet liquidity mechanism

Another important source through which banks create and manage their liquidity stems from exposure to undrawn loan commitments and exposure in wholesale funding. Unused loan commitments are the parts of credit lines that have not been drawn down, but banks are supposed to honor their obligation to fund these loans when requested by firms. Therefore, theory suggests that banks also create and manage their liquidity off the balance sheet through loan commitments (Diamond and Dybvig, 1983; Holstrom and Tirole, 1998; Kashyap, Rajan, and Stein, 2002). During periods of crisis, government interventions can help to prevent deposit outflows from banks, as showed in Gatev and Strahan (2006). However, another source of concern is that the higher the exposure to unused loan commitments the higher the liquidity risk and the liquidity needs for banks.

In addition, banks' exposure in wholesale funding represents short-term liquidity commitments. In line with Acharya and Mora (2015), and Acharya, Engle and Steffen (2021), we define liquidity demand risk as the ratio of banks' undrawn lending commitments and wholesale funding that exceed their deposits divided by banks' assets. The higher the exposure to unused credit commitments, the higher the liquidity risk for banks. In the empirical literature, Acharya and Mora (2015) report that during the 2007 financial crisis, deposit inflows into banks weakened and banks' loan-to-deposit shortfalls widened. As a result, banks with high

Covid Economics 80, 9 June 2021: 94-154

exposure to unused credit commitments failed to meet their obligations. Also, the rest of the banks honored credit lines to firms because of the liquidity programs introduced by the government-sponsored agencies. Ivashina and Scharfstein (2010) study the 2007 crisis and show that more vulnerable banks with the higher credit line drawdowns adjusted their credit by cutting new lending. In a similar vein, Cornett, McNutt, Strahan, and Tehranian (2011) document that liquidity shocks led to a decline in credit supply during the financial crisis of 2007. Banks with strong capitalization continued to lend compared to relative poorly capitalized banks. However, banks with high off-balance-sheet liquidity risk significantly constrained new lending origination.

Holstrom and Tirole (1998) and Kashyap, Rajan, and Stein (2002) suggest that banks can also face liquidity risk through off-balance-sheet unused loan commitments. Acharya and Mora (2015) provide empirical evidence that during the 2007 crisis, banks' with high exposure in undrawn lending commitments failed to meet their credit commitments.

We also test whether commitments-exposed banks experience high liquidity needs to honor their credit commitments, and therefore they offer to pay higher deposit rates to stem deposit outflows. Based on that hypothesis, banks with high liquidity risk adjust their credit lines by cutting new lending. Our fourth hypothesis suggests that banks with high liquidity risk increased their exposure to liquidity facilities offered from the Federal Reserve significantly more than banks with low commitments. This in turn, indirectly recapitalized banks, and helped banks to improve their liquidity, to honor their credit commitments, and to avoid an increase in their deposit rates.

3. Data and methodology

3.1 Data

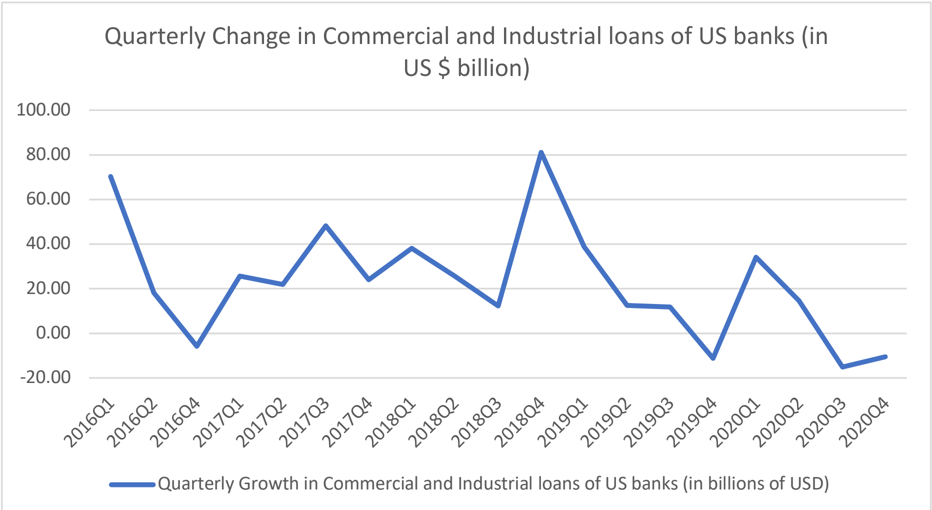
Covid Economics 80, 9 June 2021: 94-154

In this section we provide a brief description of the dataset used in this study. We use several sources of data from January 2016 till December 2020 in a quarterly frequency:

- **Deposit rates**: We use deposit rates provided by RateWatch in branch level of US banks (money market deposits as well as Certificate of Deposits data). We use 3 kinds of deposit products across all U.S. branches: 12-month Certificates of Deposit with an account size up to $10,000; 12-month Certificates of Deposit (CDs) with an account size up to $100,000; and 12-month Certificates of Deposit (CDs) with an account size up to $500,000. The data is available in a monthly frequency.
- **Syndicated loans:** We collect detailed information on syndicated bank loans from Thomson Reuter's Dealscan. This source reports each loan as a deal, and contains information on the amount, the rate, the maturity, the lenders' and the borrowers' names.
- **Banks' loans and financial condition:** We use the Reports of Condition and Income (also known as Call Reports) to collect information to measure banks' capitalization, liquidity, commercial and industrial loans growth (see also Figure 2 below), net loans and leases growth, deposits, asset size, unused credit commitments, and wholesale funding.
- **COVID-19 Cases:** We collect detailed information for the number of cases per capita per state in the U.S. by using the rich dataset of CDC COVID Data Tracker.

Figure 2. Unused credit commitments to total assets. This figure shows the quarterly change in the origination of Commercial and Industrial loans of US banks from 2016 till 2020.

Covid Economics 80, 9 June 2021: 94-154



Covid Economics 80, 9 June 2021: 94-154

3.2 Methodology

In this section we present the methodology used to test our hypotheses. We begin with the investigation of the market discipline mechanism and the precautionary savings hypothesis, followed by next we present the empirical model for the internal capital market hypothesis and we conclude with the test for the effect of liquidity risk in the deposit rates, the flow of deposits, the supply of credit and on the use of Fed's liquidity facilities.

3.2.1 Market Discipline Theory and Precautionary Savings

If market discipline is exercised, then during distress conditions, depositors are concerned about their deposits and therefore they discipline riskier banks by demanding higher rates. In line with the literature, we employ the equity-to-assets ratio as a proxy to identify which banks are riskier (e.g., see Diamond and Rajan, 2000; Brown and Dinc, 2011; Ben-David, Palvia, and Spatt 2017 *inter alia*).

$$Deposit\_Rate_{i,q} = \alpha + \beta_1 EA_{i,q-n} \times I(q < 2020) + \beta_2 EA_{i,q-n} \times I(q \geq 2020) + \gamma B_{i,q-1} + \delta T_q + \varepsilon_{i,q} \quad (1)$$

where $Deposit\_Rate_{i,q}$ is defined as the deposit rate of bank i at quarter q. $EA_{i,q-n}$ represents the lagged equity-to-assets ratio, with the number of lags being $n = \{1, 2\}$. $I(q < 2020)$ and $I(q \geq 2020)$ denote whether quarter q precedes 2020. $B_{i,q-1}$ is a set of bank-quarter control variables. T_q represents a set of quarter fixed effects. All specifications are estimated with robust standard errors clustered by bank and quarter. We regress deposit rates of the same duration (12 months) but across different amounts of Certificates of Deposits ($10,000 and $500,000, and for $100,000 as a robustness test). If market discipline is exercised, the sign of the coefficient on the equity-to-assets ratio should be negative for weakly-capitalized banks.

Furthermore, to test for the precautionary savings theory we suggest that in counties with the higher COVID-19 cases banks' deposits grow faster and deposit rates decrease significantly more than in counties with low COVID-19 cases.

$$Deposit\_Rate_{i,q} = \alpha + \beta_1 COVID - 19\ Cases_{c,q} + \gamma B_{i,q-1} + \delta T_q + \varepsilon_{i,q} \quad (2)$$

where, $COVID - 19\ Cases$ represents COVID-19 cases in county i at quarter q.

And accordingly, for deposits:

$$Deposit\_Growth_{i,q} = \alpha + \beta_1 COVID - 19\ Cases_{c,q} + \gamma B_{i,q-1} + \delta T_q + \varepsilon_{i,q} \quad (3)$$

3.2.2 Internal Capital Markets

If the internal capital market mechanism is at work, banks' incentive to provide new loans lead to an increase in deposit rates, which in turn results to a rise in deposits. The increase in liquidity is used to fund the provision of new loans. Therefore, in Equation (4) below, if banks use their internal capital markets to create liquidity, there should be a positive coefficient on loan growth (i.e. β_2) and on the deposit rate (i.e. β_4).

Covid Economics 80, 9 June 2021: 94-154

$$Deposit\_Growth_{i,q} = \alpha + \beta_1 Loan\_Growth_{i,q-n} + \beta_2 Loan\_Growth_{i,q-n} \times I(q \geq 2020) + \beta_3 Deposit\_Rate_{i,q-n} + \beta_4 Deposit\_Rate_{i,q-n} \times I(q \geq 2020) + \gamma B_{i,q-1} + \delta T_q + \varepsilon_{i,q} \quad (4)$$

where $Deposit\_Growth_{i,q}$ represents the quarter-on-quarter net growth in total deposits for bank i. $Loan\_Growth_{i,q-n}$ is lagged quarter-on-quarter growth in Net Loans and Leases for bank i. The number of lags is $n = \{1, 2\}$. $Deposit\_Rate_{i,q-n}$ represents the lagged quarter-on-quarter deposit rate of bank i at quarter $q - n$. All specifications are estimated with robust standard errors clustered by bank and quarter. Similar to Equation (1), we regress deposit rates of the same duration (12 months) but across different amounts of Certificates of Deposits ($10,000 and $500,000).

Since the internal capital markets mechanism suggests that loan growth determines deposit rates, we also test the relation between the two instruments before and during the pandemic. If our hypothesis holds, then the coefficient of loan growth (i.e., β_1 and β_2) in Equation (5) should be positive.

$$Deposit\_Rate_{i,q} = \alpha + \beta_1 State\_Level\_Loan\_Growth_{i,q-n} + \beta_2 State\_Level\_Loan\_Growth_{i,q-n} \times I(q \geq 2020) \gamma B_{i,q-1} + \delta T_q + \varepsilon_{i,q} \quad (5)$$

where $Deposit\_Rate_{i,q}$ represents the quarter-on-quarter deposit rate of 12-month maturity and $500,000 for bank i. Similar to Ben-David, Palvia, and Spatt (2017), to easy endogeneity concerns, we measure state-level loan growth to test its relation with bank-level deposit rates $State\_Level\_Loan\_Growth_{i,q-n}$ is lagged quarter-on-quarter growth across different categories of loans: Commercial & Industrial (C&I) and Net Loans and Leases for bank i. The number of lags is $n = \{1, 2\}$. All specifications are estimated with robust standard errors clustered by bank and quarter.

Furthermore, we assess the relation between deposit growth and loan growth, since our internal capital markets hypothesis suggests that banks' incentives to provide new loans leads to a haunt to increase their core deposits. To test the first part of the hypothesis we ask: Is

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deposit growth positively correlated with bank lending growth? Empirically, we follow the approach of Jayaratne and Morgan (2000).

$$Deposit\_Growth_{i,q} = a + \beta_1 Loan\_Growth_{i,q-n} + \beta_2 Loan\_Growth_{i,q-n} \times I(q \geq 2020) + \gamma B_{i,q-1} + \delta T_q + \varepsilon_{i,q} \quad (6)$$

where $Deposit\_Growth_{i,q}$ represents the quarter-on-quarter change in deposits to total assets ratio for bank i. $Loan\_Growth_{i,q-n}$ is lagged quarter-on-quarter growth across two categories of loans: Syndicated loans and Net Loans and Leases for bank i. The number of lags is $n = \{1, 2\}$. All specifications are estimated with robust standard errors clustered by bank and quarter.

3.2.3 Liquidity Risk from Loan Commitments

Banks can also create liquidity off the balance sheet through unused credit commitments. These commitments are the parts of credit lines that have not been drawn down, but banks are supposed to honor their obligation to fund these loans when requested by firms. Therefore, the higher the exposure to unused credit commitments, the higher the liquidity needs and hence, the higher the liquidity risk. We follow Acharya, Engle and Steffen (2021) to measure liquidity risk, by adding wholesale funding exposure to unused credit commitments, and subtracting this with available cash in banks' balance sheets. The empirical literature suggests that during crisis episodes banks with high liquidity risk offer to pay higher deposit rates to attract more deposits. In turn these deposits will be used to honor their credit commitments. To test this hypothesis, we evaluate the relation between unused credit commitments with deposit rates, and deposit growth. If our hypothesis holds, then the sign of the coefficient on liquidity risk should be positive, and negative on deposit growth.

$$Liquidity\ Risk = \frac{Unused\ Commitmetns + Wholesale\ Funding - Cash}{Total\ Assets} \quad (7)$$

Covid Economics 80, 9 June 2021: 94-154

$$Deposit\_Rates_{i,q} = a + \beta_1 Liquidity\_Risk_{i,q-n} + \beta_2 Liquidity\_Risk_{i,q-n} \times I(q \geq 2020) + \gamma B_{i,q-1} + \delta T_q + \varepsilon_{i,q} \quad (8)$$

and for the relation with deposits:

$$Deposit\_Growth_{i,q} = a + \beta_1 Liquidity\_Risk_{i,q-n} + \beta_2 Liquidity\_Risk_{i,q-n} \times I(q \geq 2020) + \gamma B_{i,q-1} + \delta T_q + \varepsilon_{i,q} \quad (9)$$

Furthermore, we assess whether banks with high liquidity risk adjust their credit by cutting new lending during the pandemic. To test this hypothesis, we use three different categories of lending: Syndicated loans, Commercial and Industrial Loans, and Net Loans and Leases:

$$Loan\_Growth_{i,q} = a + \beta_1 Liquidity\_Risk_{i,q-n} + \beta_2 Liquidity\_Risk_{i,q-n} \times I(q \geq 2020) + \gamma B_{i,q-1} + \delta T_q + \varepsilon_{i,q} \quad (11)$$

Finally, we assess whether banks exposed to higher liquidity risk increased their exposure to liquidity facilities introduced by the Federal Reserve during the COVID-19 pandemic. Our hypothesis is that vulnerable banks with exposure to high liquidity risk increased their exposure to facilities offered by the Federal Reserve to improve their liquidity levels:

$$Liquidity\_Risk_{i,q} = a + \beta_1 FED\_Liquidity_{i,q-n} + \beta_2 FED\_Liquidity_{i,q-n} \times I(q \geq 2020) + \gamma B_{i,q-1} + \delta T_q + \varepsilon_{i,q} \quad (12)$$

where $FED\_Liquidity_{i,q-n}$ is the lagged quarter-on-quarter change on the sum of Total Federal Funds sold to Commercial Banks, Reverse Repurchases, Vault Cash and Cash Items, and Balances due from Federal Reserve Banks for each bank i.[5]

4. Empirical results

[5] Total Federal Funds sold to Commercial Banks and Reverse Repurchases and Vault Cash and Cash Items and Balances due from Federal Reserve Banks are available from the Board of Governors of the Federal Reserve System, account H8 "Assets and Liabilities of Commercial Banks in the United States", and can be found here.

Covid Economics 80, 9 June 2021: 94-154

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Covid Economics 80, 9 June 2021: 94-154

In this section we present the results of the empirical tests for the effect of the pandemic and the effectiveness of the liquidity programs implemented by the Federal Reserve in banks' management of liquidity and credit commitments. Table 1 presents summary statistics for the main variables.

Table 1. Summary Statistics

This table presents summary statistics for the variables used in the analysis. The sample period is from 2016Q1-2020Q4. Panel A and Panel B show summary statistics for the analysis for the main sample. Panel C shows summary statistics for the sample with deposit rates for a variety of account sizes per bank per branch.

Panel A. Summary Statistics of Main Sample (bank-quarter observations) amounts in US$ millions.

	N	Mean	Std.Dev	P5	P25	P50	P75	P95
Total Assets	105,632	3,252.2	51,600.0	36.7	102.5	218.7	525.5	3740.4
Total Deposits	105,642	2,512.6	39,000.0	29.6	85.5	183.6	437.8	2963.1
Total Equity	105,471	359.7	5,354.0	4.4	11.9	24.8	59.2	411.0
Commercial and Industrial loans	105,642	387.8	5,988.6	0.2	1.3	14.1	43.2	393.8
Net Loans and Leases	105,632	1,768.0	24,100.0	16.5	59.4	140.7	359.0	2563.7
Total Unused Commitments	105,642	1,388.8	24,500.0	0.7	6.3	19.6	62.5	602.5
Total Federal Reserve Repurchase	105,632	91.7	3,605.6	0.0	0.0	0.0	1.6	14.6
Cash	105,632	358.5	7,527.5	2.2	6.9	15.1	36.3	209.0
Syndicated loans	5,206	9,280.0	22,300.0	0.0	81.3	652.0	6,490.0	54100.0

Panel B. Summary Statistics of ratios in Main Sample (bank-quarter observations)

	N	Mean	Std.Dev	P5	P25	P50	P75	P95
Total Equity to Assets ratio	105,471	0.123	0.069	0.081	0.096	0.110	0.130	0.188
Total Deposits to Assets ratio	105,642	0.829	0.092	0.705	0.804	0.848	0.879	0.907
Total Liabilities to Total Assets ratio	105,642	0.878	0.070	0.812	0.870	0.890	0.904	0.920
Unused Commitments ratio	104,995	0.137	0.090	0.025	0.084	0.128	0.174	0.259

Panel C. Summary Statistics of Deposit Rates (bank-quarter observations)

	N	Mean	Std.Dev	P5	P25	P50	P75	P95
12-month CD rate, $10k accounts (12MCD10K rate)	83,565	0.658	0.486	0.150	0.300	0.500	0.900	1.730
12-month CD rate, $100k accounts (12MCD100K rate)	80,787	0.686	0.496	0.150	0.341	0.500	0.950	1.750
12-month CD rate, $500k accounts (12MCD500K rate)	71,679	0.708	0.507	0.150	0.350	0.520	1.000	1.760

4.1 Precautionary Savings and Market Discipline

We start by investigating how the pandemic affected depositors' behavior. Table 2 shows that higher COVID-19 cases are associated with a significant decrease in deposit rates decrease and significant growth in deposit amounts. These results support the suggestion that households increase savings as a precaution against declines in future income. Next, we test whether depositors exercise market discipline during the pandemic-induced crisis by demanding a higher premium from risky banks. If weakly capitalized banks offer higher deposit rates to stem deposit outflow, the coefficient on the equity to assets ratio will be negative, and market discipline is exercised. On the other hand, if the injection of liquidity from the Federal Reserve is effective, then depositors will not request higher rates from weakly-capitalized banks, and hence, the coefficient on the equity to assets ratio will be positive. In Table 3 we present the results obtained from regressing deposit rates for different amounts ($10k and $500k Certificate of Deposits) on the equity to assets ratio. Deposits in the United States above $250k are not insured by the FDIC and therefore these deposits should, at least in theory, be more sensitive during crisis episodes. Panel A of Table 3 presents results for the period before and during the pandemic for the overall sample of banks. The results show that all coefficients are positive and statistically significant at 1% before and during the pandemic, indicating that market discipline was not exercised by depositors.

Next, we construct a subsample of weakly-capitalized banks to further investigate the market discipline hypothesis. Weakly capitalized banks are defined as the bottom 10% of capitalization within each quarter. If market discipline is exercised, the sign of the coefficient on capitalization should be negative for risky banks. Panel B shows that this coefficient is positive and statistically significant for deposits of $10k, while the coefficient for deposits of $500k is not significant. Therefore, we argue that risky banks do not offer higher deposit rates to stem deposit outflows. If market discipline was a salient factor, we should observe a

Covid Economics 80, 9 June 2021: 94-154

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significant negative relation between deposit rates and weakly-capitalized banks. Furthermore, these results support the findings presented in Panel A which show that market discipline was not exercised for the overall sample of banks. In contrast, the results indicate that the injection of liquidity from the Federal Reserve alleviated the concerns of liquidity stress and strengthened the liquidity condition of weakly capitalized banks. Since deposits are a critical source of funding for banks, this result also implies that banks lowered their cost of credit. In the robustness tests section, we provide additional robustness analyses which show the weak relationship between deposit rates and the capital ratio for deposits of different account size.

4.2 The Internal Capital Market Channel

In this section we test whether the pandemic affected the demand side for deposits, that is the internal capital market through which banks create liquidity. The market discipline hypothesis suggests that deposit rates mirror bank risk, since weakly-capitalized banks should offer higher deposit rates. However, depositors might favor banks that offer lower deposit rates, due to differences in risk. In contrast, the internal capital market hypothesis suggests that banks' intention to provide new loans leads to offer higher deposit rates to increase the flow of deposits and thus by extension to fund these loans. For this hypothesis to hold, deposit growth must be positively correlated with loan growth and with deposit rates.

Covid Economics 80, 9 June 2021: 94-154

Covid Economics 80, 9 June 2021: 94-154

Table 2. Deposit Rates, Deposit growth and Covid-19 cases

This table presents regressions of deposit rates, deposit growth and Covid-19 cases per capita. Panel A presents the regression results that estimate the effect of COVID-19 on deposit rates. Panel B presents the regression results that estimate the effect of COVID-19 in the growth of deposits. In columns (1) and (3) the independent variables are lagged by 1 quarter (i=1) and therefore for the pandemic period it reflects results for Q3 2020.In columns (2) and (4) the independent variables are lagged by 2 quarters (i=2) and therefore for the pandemic period it reflects results for Q2 2020. The sample is from March 2020 till December 2020 for the pre-pandemic period, and from January 2020 till December 2020 for the pandemic period. All regressions are ordinary least square regressions and have bank and quarter (time) fixed effects. Standard errors are clustered by bank and quarter. Inside the parentheses are the t-statistics. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Deposit rates and Covid-19 cases	12-Month CD $10k account rate		12-Month CD $500k account rate	
	(1)	(2)	(1)	(2)
	i=1	i=2	i=1	i=2
Covid-19 cases per capita (t-i)	-2.000***	-2.000***	-0.976	-2.812
	(37.17)	(37.16)	(0.27)	(0.65)
State Fixed effects	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.0913	0.0913	0.0741	0.0741
No. of obs.	41,491	41,484	4,097	4,097

Panel B. Deposit growth and Covid-19 cases	Deposit growth (in thousands of $)		Deposit to total assets growth (in %)	
	(1)	(2)	(1)	(2)
	i=1	i=2	i=1	i=2
Covid-19 cases per capita (t-i)	2,388.1*	2,423.7*	0.009	0.011
	(1.73)	(1.65)	(0.87)	(0.97)
State Fixed effects	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.0133	0.0133	0.0155	0.0155
No. of obs.	15,222	15,222	15,222	15,222

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Covid Economics 80, 9 June 2021: 94-154

Table 3. Deposit Rates and Bank Tier 1 Ratio

This table presents regressions of 12-month Certificate of Deposits (CD) rates on lagged Tier 1 Capital to Risk-Weighted-Assets Ratio. In Panel A the whole sample of banks is used. Panel B splits the sample by bank capitalization using the bottom 10% decile. In columns (2) and (4) the independent variables are lagged by 3 quarters (i=3) and therefore for the pandemic period it reflects results for Q1 2020 only. The sample is from January 2016 till December 2019 for the pre-pandemic period, and from January 2020 till December 2020 for the pandemic period. All regressions are ordinary least square regressions and have bank and quarter (time) fixed effects. Standard errors are double-clustered by bank and quarter. Inside the parentheses are the t-statistics. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Whole sample — Dependent Variables:	12-Month CD $10k account rate		12-Month CD $500k account rate	
	(1)	(2)	(1)	(2)
	i=1	i=3	i=1	i=3
Equity to assets (t-i) × I *(q<2020Q1)*	0.005***	0.006***	0.005***	0.006***
	(7.40)	(8.61)	(6.25)	(7.48)
Equity to assets *(t-i)* × I *(q≥2020Q1)*	0.002***	0.003***	0.004***	0.004***
	(2.95)	(3.84)	(3.19)	(3.72)
Bank Fixed effects	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.703	0.703	0.724	0.724
No. of obs.	83,565	83,565	71,679	71,679

Panel B. Deposit Rates and Bank Tier 1 Capital to Risk-Weighted-Assets Ratio (10% lower-capitalized banks) — Dependent Variables:	12-Month CD $10k account rate		12-Month CD $500k account rate	
	(1)	(2)	(1)	(2)
	i=1	i=3	i=1	i=3
Equity to assets (t-i) × I (q<2020Q1)	0.006	0.013**	0.016**	0.019***
	(1.02)	(2.01)	(2.35)	(2.71)
Equity to assets *(t-i)* × I *(q≥2020Q1)*	0.020**	0.029***	0.028*	0.023
	(2.03)	(2.78)	(1.84)	(1.48)
Bank Fixed effects	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes
R-square	0.740	0.739	0.770	0.765
No. of obs.	8,062	8,072	6,861	6,875

4.2.1 The Relationship between Deposit Growth, Lending Growth, and Deposit Rates

In Table 4 we present the results from the regression estimated with standard errors clustered by bank and quarter (time). Loan growth is lagged, and it is measured by the quarterly change in net loans and leases, while deposit rates are measured through the Certificate of Deposits for accounts of $10k (Table 4). In Panel A of Table 4 we include all banks, and in columns (1) and (2) we first test the relation between loan growth and deposit growth. The results show that before the pandemic deposit growth has a significantly opposite direction of loan growth. However, this relationship changes during the pandemic, when deposit growth is very strongly correlated with loan growth. Further, the correlation increases during the COVID-19 pandemic crisis and it is statistically significant at 1%. Precisely, column (1) in Table 4 reveals that a 1-standard-deviation change in net leases and loan growth is associated with a change of 0.5% in the same direction in deposit growth. Next, in columns (3) and (4), we include deposit rates on the right-hand side to examine the relation between lagged loan growth, lagged deposit rates and deposit growth. We find that deposit flows are positively correlated with growth in net loans and leases but *negatively* correlated with deposit rates during the pandemic. As a result, during the pandemic banks increase their deposit growth even though they offer lower deposit rates.

In Table A1 in the Appendix, we test the same regression for accounts of £500k. When we use the alternative deposit account of $500k in Panel A of Table A1, the pattern continues to hold and the relation between deposit growth, lending growth and deposit rates is the same with the one observed in Table 4. Moreover, we breakdown the sample by bank capitalization to examine the same relation in Panels B (weakly capitalized) and C (well-capitalized) of Table 4. Weakly capitalized banks are defined as the bottom 10% of capitalization within each quarter, and well-capitalized banks as the top 10% of capitalization within each quarter. The results

Covid Economics 80, 9 June 2021: 94-154

show that deposit growth is *negatively* correlated with deposit rates and with loan growth for weakly capitalized banks. When we test the same relationship with accounts of $500k, the results hold, indicating the robustness of our findings (Table A1). Notably, at the onset of the pandemic (quarter 1), the relation between deposit growth and lending growth was *positively* correlated. These results reflect the drewdown in credit lines from firms which resulted to an increase in bank lending during the first phase of the pandemic. However, weakly-capitalized banks adjusted their lending in the next quarters, by decreasing the provision of new loans. Finally, in Panel C of Table 4 we find that deposit growth is positively correlated with loan growth but *negatively* correlated with deposit rates for well-capitalized banks. The positive relation between deposit growth and lending growth implies that well-capitalized banks increased lending during the pandemic. Similar results are reported when we test for deposit accounts of $500k in Table A1 providing strong robustness for our findings.

Furthermore, we use Commercial and Industrial loans as an alternative measure of bank lending to examine the internal capital markets hypothesis. In Table 5 we present the results from the regression estimated with standard errors clustered by bank and quarter (time). Deposit rates are measured through the Certificate of Deposits for accounts of $10k. In Panel A of Table 5 we include all banks and in columns (1) and (2) we first test the relation between loan growth and deposit growth. For commercial and industrial loans, our results show a *positive* relationship in the first phase (Quarter 1) of the pandemic, and then it changes to negative afterwards. This result is consistent with the dramatic increase in involuntary lending that was caused when firms drewdown credit lines at the onset of the pandemic. In contrast, loan growth and deposit growth are always negatively correlated in the pre-pandemic period. A 1-standard-deviation change in loan growth is associated with a change of 13% in the same direction of deposit growth. Similar with the results in net loans and leases, we find that deposit growth is negatively correlated with deposit rate.

Covid Economics 80, 9 June 2021: 94-154

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Covid Economics 80, 9 June 2021: 94-154

Table 4. Deposit Growth, Net Loans and Leases Growth, and Deposit Rates of Small Accounts

This table presents regressions of quarter-on-quarter deposits growth on lagged quarter-on-quarter loan growth and 12-month Certificate of Deposit (CD) rate, and lagged quarter-on-quarter loan growth. The CD rates are for accounts of $10k. Loan growth is defined as quarterly change in net loans and leases. In Panel A the whole sample of banks is used. Panel B splits the sample by bank capitalization using the bottom 10% decile. Panel C splits the sample by bank capitalization using the top 10% decile. In columns (1) and (3) independent variables are lagged by 1 quarter (i=1) and therefore for the pandemic period it reflects results after Q1 2020. In columns (2) and (4) the independent variables are lagged by 3 quarters (i=3) and therefore for the pandemic period it reflects results for Q1 2020 only. The sample is from January 2016 till December 2019 for the pre-pandemic period, and from January 2020 till December 2020 for the pandemic period. All regressions are ordinary least square regressions and have bank and quarter (time) fixed effects. Standard errors are double-clustered by bank and quarter. Inside the parentheses are the t-statistics. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Deposit Growth, Loan Growth and Deposit Rates

	Dependent Variable: Deposit Growth			
	(1)	(2)	(3)	(4)
	i=1	i=3	i=1	i=3
12-Month CD $10k account rate *(t-i)* × I (q<2020Q1)			17.866	-7.624
			(0.85)	(0.33)
12-Month CD $10k account rate *(t-i)* × I *(q≥2020Q1)*			-309.194***	-319.647***
			(8.55)	(10.17)
Net loans and Leases Growth *(t-i)* × I (q<2020Q1)	-0.384***	-0.119***	-0.385***	-0.132***
	(39.73)	(11.73)	(37.16)	(12.04)
Net loans and Leases Growth *(t-i)* × I (q≥2020Q1)	0.587***	0.121***	0.601***	0.1076***
	(85.77)	(14.65)	(83.74)	(12.40)
Bank Fixed effects	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.289	0.227	0.318	0.248
No. of obs.	105,581	105,552	83,520	83,451

Panel B. Deposit Rates and Bank Tier 1 Capital to Risk-Weighted-Assets Ratio (10% low-capitalized banks)

	Dependent Variable: Deposit Growth			
	(1)	(2)	(3)	(4)
	i=1	i=3	i=1	i=3
12-Month CD $10k account rate *(t-i)* × I (q<2020Q1)			12.479	2.003
			(0.67)	(0.10)
12-Month CD $10k account rate *(t-i)* × I *(q≥2020Q1)*			-63.529**	-46.218*
			(2.02)	(1.73)
Net loans and Leases Growth *(t-i)* × I (q<2020Q1)	-1.551***	-0.571***	-0.818***	0.254***
	(11.84)	(3.92)	(15.48)	(4.20)
Net loans and Leases Growth *(t-i)* × I (q≥2020Q1)	-0.802***	1.138***	-0.354***	0.109**
	(10.39)	(10.97)	(10.78)	(2.30)

	(1)	(2)	(3)	(4)
Bank Fixed effects	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.289	0.135	0.261	0.182
No. of obs.	10,536	10,533	8,054	8,044

Panel C. Deposit Rates and Bank Tier 1 Capital to Risk-Weighted-Assets Ratio (10% highly-capitalized banks)

	Dependent Variable: Deposit Growth			
	(1)	(2)	(3)	(4)
	i=1	i=3	i=1	i=3
12-Month CD $10k account rate *(t-i)* × I (q<2020Q1)			-33.341	-25.561
			(1.55)	(1.07)
12-Month CD $10k account rate *(t-i)* × I *(q≥2020Q1)*			-50.141	-67.307**
			(1.33)	(1.99)
Net loans and Leases Growth *(t-i)* × I (q<2020Q1)	-0.248***	0.441***	-0.089***	-0.010
	(11.35)	(19.68)	(3.27)	(0.35)
Net loans and Leases Growth *(t-i)* × I (q≥2020Q1)	0.216***	0.374***	0.129***	0.337***
	(4.31)	(6.62)	(3.11)	(6.97)
Bank Fixed effects	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.114	0.184	0.178	0.252
No. of obs.	10,680	10,677	6,905	6,912

To ensure that our results are not driven by the regulatory protection that depositors enjoy for accounts less than $250k, we test the relation between deposit growth, lagged loan growth, and lagged deposit rates for Certificates of Deposits of $500k. In Table A2 in the Appendix, we report the results with the alternative deposit account of $500k., and we find that the same relations hold for both the pandemic and the pre-pandemic period. Since we do not find a positive relationship between deposit growth, deposit rates and loan growth for both loan categories, we conclude that banks improve their liquidity without the use of their internal capital market – the supply side of liquidity creation.

Further, we breakdown the sample by bank capitalization to examine the same relation with weakly capitalized (Panel B) and well-capitalized (Panel C) banks in Table 5. For weakly capitalized banks, our results show that deposit growth is negatively correlated with loan growth, while we find weak significance for deposit rates during quarter 1 only. For well-capitalized banks the results show that there is positive relation between deposit growth and loan growth, while we do not find significance for deposit rates. These results indicate that in contrast with well-capitalized banks, less capitalized banks were forced to cut back in new credit origination as a response to the liquidity shock, while deposit rates do not play a significant role in the creation of liquidity to fund new loans. In Table A2, we test the same relationship with accounts of $500k hold, and we obtain similar results, indicating strong robustness for our findings.

Covid Economics 80, 9 June 2021: 94-154

Covid Economics 80, 9 June 2021: 94-154

Table 5. Deposit growth, Commercial and Industrial Loans Growth, and Deposit Rates of Small Accounts

This table presents regressions of quarter-on-quarter deposits growth on lagged quarter-on-quarter loan growth and 12-month Certificate of Deposit (CD) rate, and lagged quarter-on-quarter loan growth. The CD rates are for accounts of $10k. Loan growth is defined as quarterly change in net loans and leases. In Panel A the whole sample of banks is used. Panel B splits the sample by bank capitalization using the bottom 10% decile. Panel C splits the sample by bank capitalization using the top 10% decile. In columns (1) and (3) independent variables are lagged by 1 quarter (i=1) and therefore for the pandemic period it reflects results after Q1 2020. In columns (2) and (4) the independent variables are lagged by 3 quarters (i=3) and therefore for the pandemic period it reflects results for Q1 2020 only. The sample is from January 2016 till December 2019 for the pre-pandemic period, and from January 2020 till December 2020 for the pandemic period. All regressions are ordinary least square regressions and have bank and quarter (time) fixed effects. Standard errors are double-clustered by bank and quarter. Inside the parentheses are the t-statistics. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Deposit Growth, Loan Growth and Deposit Rates

	Dependent Variable: Deposit Growth			
	(1)	(2)	(3)	(4)
	i=1	i=3	i=1	i=3
12-Month CD $10k account rate *(t-i)* * I (q<2020Q1)			13.774	-3.761
			(0.67)	(0.16)
12-Month CD $10k account rate *(t-i)* * I *(q≥2020Q1)*			-298.991***	-341.083***
			(8.41)	(10.87)
Commercial and Industrial Loans *(t-i)* * I (q<2020Q1)	-1.033***	-0.441***	-1.050***	-0.430***
	(39.67)	(16.25)	(38.52)	(14.95)
Commercial and Industrial Loans *(t-i)* * I (q≥2020Q1)	-0.299***	1.299***	-0.307***	1.312***
	(19.61)	(101.88)	(19.17)	(98.70)
Bank Fixed effects	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes
R-Squared	0.229	0.310	0.250	0.341
No. of obs.	105,581	105,552	83,520	83,451

Panel B. Deposit Rates and Bank Tier 1 Capital to Risk-Weighted-Assets Ratio (10% low-capitalized banks)

	Dependent Variable: Deposit Growth			
	(1)	(2)	(3)	(4)
	i=1	i=3	i=1	i=3
12-Month CD $10k account rate *(t-i)* * I (q<2020Q1)			11.539	3.158
			(0.61)	(0.16)
12-Month CD $10k account rate *(t-i)* * I *(q≥2020Q1)*			-46.136	-51.278*
			(1.45)	(1.92)
Commercial and Industrial Loans *(t-i)* * I (q<2020Q1)	-3.398***	-2.628***	-0.738***	0.404**
	(9.12)	(6.89)	(4.84)	(2.53)
Commercial and Industrial Loans *(t-i)* * I (q≥2020Q1)	-0.476***	-0.260	-0.344***	-0.510***

	(3.19)	(0.84)	(6.38)	(3.98)
Bank Fixed effects	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes
R-Squared	0.281	0.123	0.238	0.182
No. of obs.	10,536	10,533	8,054	8,044

Panel C. Deposit Rates and Bank Tier 1 Capital to Risk-Weighted-Assets Ratio (10% low-capitalized banks)

	Dependent Variable: Deposit Growth			
	(1)	(2)	(3)	(4)
	i=1	i=3	i=1	i=3
12-Month CD $10k account rate *(t-i)* * I (q<2020Q1)			-30.407	-25.385
			(1.49)	(1.07)
12-Month CD $10k account rate *(t-i)* * I *(q≥2020Q1)*			-46.110	-63.929*
			(1.29)	(1.90)
Quarter change in commercial and industrial loans *(t-i)* * I (q<2020Q1)	1.564***	-0.416***	1.921***	-0.672***
	(17.48)	(4.53)	(25.13)	(8.11)
Quarter change in commercial and industrial loans *(t-i)* * I (q≥2020Q1)	0.736***	0.798***	0.681***	0.813***
	(6.42)	(5.59)	(7.51)	(6.92)
Bank Fixed effects	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes
R-Squared	0.131	0.152	0.257	0.259
No. of obs.	10,680	10,677	6,905	6,912

Covid Economics 80, 9 June 2021: 94-154

4.2.2 The Relationship between Deposit Rates and Lending Growth

Next, we investigate the relationship between deposit rates and loan growth. In Table 6 we present the results from the regression estimated with standard errors clustered by bank and quarter (time). Loan growth is lagged, and it is measured by the quarterly change in net loans and leases. According to the internal capital market hypothesis, deposit rates should be positively correlated with loan growth. In Panel A of Table 6 we include all banks, while deposit rates are measured as Certificate of Deposits of $10k in columns (1) and (2), $100k in columns (3) and (4), and $500k in columns (5) and (6). We find that there is no strong relationship between deposit rates and loan growth, indicating that internal capital markets are not in work. We also breakdown the sample by bank capitalization in Panels B and C. For weakly-capitalized banks (Panel B) we find a negation relation for accounts of $10k., and 100k., however the significance is weak. Similarly, for well-capitalized banks (Panel C) the regression provides a meaningful positive coefficient between deposit rates and lending growth in quarter 3 for all account sizes in the pandemic crisis period only. This result indicates that well-capitalized banks have the ability to increase their lending by offering higher deposit rates. In contrast, for weakly-capitalized banks, deposit rates are not the dominant factor to grow their lending.

We examine further the relationship between deposit rates and lending growth by replacing net loans and leases with commercial and industrial loans in Table 7. To avoid any endogeneity concerns, we use banks' loan growth rate with loan growth per bank per state to examine the internal capital market across states. The results show that there is no strong significant coefficient in the relationship between deposit rates and loan growth. In addition, we breakdown banks by their capitalization in Panels B and C. For weakly-capitalized banks (Panel B) we find a strong negative relation which is significant at the 1% for accounts of $10k., and $100k. and only for quarter 3 of the pandemic. In contrast, for well-capitalized banks

Covid Economics 80, 9 June 2021: 94-154

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Covid Economics 80, 9 June 2021: 94-154

Table 6. Deposit rates and Net Loans and Leases Growth

This table presents regressions of 12-month Certificate of Deposit (CD) rate, and lagged quarter-on-quarter loan growth. The CD rates are for accounts of $10k., 100k., and $500k. Loan growth is defined as quarterly change in net loans and leases. In Panel A the whole sample of banks is used. Panel B splits the sample by bank capitalization using the bottom 10% decile. Panel C splits the sample by bank capitalization using the top 10% decile. Independent variables are lagged by 1 quarter (i=1) and therefore for the pandemic period it reflects results after Q1 2020. Also, independent variables are lagged by 3 quarters (i=3) and therefore for the pandemic period it reflects results for Q1 2020 only. The sample is from January 2016 till December 2019 for the pre-pandemic period, and from January 2020 till December 2020 for the pandemic period. All regressions are ordinary least square regressions and have bank and quarter (time) fixed effects. Standard errors are double-clustered by bank and quarter. Inside the parentheses are the t-statistics. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Whole sample	Dependent Variables:					
	12-Month CD $10k rate		12-Month CD $100k rate		12-Month CD $500k rate	
	i=1	i=3	i=1	i=3	i=1	i=3
Loan Growth (t-i) * I (q<2020Q1)	-0.0015	0.0003	-0.0009	0.0033*	-0.0020	0.0003
	(0.91)	(0.16)	(0.49)	(1.73)	(1.03)	(0.16)
Loan Growth (t-i) * I (q≥2020Q1)	0.0001	0.0025*	0.0002	0.0022	0.0001	0.0006
	(0.08)	(1.66)	(0.15)	(1.47)	(0.05)	(0.36)
Bank Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.7031	0.7031	0.7064	0.7064	0.7239	0.7239
No. of obs.	83,554	83,545	80,813	80,804	71,674	71,666

Panel B. Deposit Rates and Bank Tier 1 Capital to Risk-Weighted-Assets Ratio (10% low-capitalized banks)	Dependent Variables:					
	12-Month CD $10k rate		12-Month CD $100k rate		12-Month CD $500k rate	
	i=1	i=3	i=1	i=3	i=1	i=3
Loan Growth (t-i) * I (q<2020Q1)	0.0201	0.0808**	-0.0184	-0.0185	-0.1190**	-0.0388
	(0.56)	(2.00)	(0.51)	(0.45)	(2.35)	(0.76)
Loan Growth (t-i) * I (q≥2020Q1)	-0.0445**	0.0565*	-0.0490**	-0.0254	-0.1230	-0.0155
	(2.01)	(1.81)	(2.19)	(0.80)	(1.60)	(0.23)
Bank Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.7404	0.7390	0.7460	0.7459	0.7700	0.7651
No. of obs.	8,062	8,072	7,798	7,810	6,861	6,875

Panel C. Deposit Rates and Bank Tier 1 Capital to Risk-Weighted-Assets Ratio (10% low-capitalized banks)	Dependent Variables:					
	12-Month CD $10k rate		12-Month CD $100k rate		12-Month CD $500k rate	
	i=1	i=3	i=1	i=3	i=1	i=3
Loan Growth (t-i) * I (q<2020Q1)	0.0010	0.0208*	0.0187	0.0215*	0.0126	0.0141

	(0.85)	(1.81)	(1.59)	(1.84)	(1.08)	(1.18)
Loan Growth (t-i) * I (q>=2020Q1)	0.0600**	0.0136	0.0538**	0.0203	0.0691**	-0.0243
	(2.56)	(0.51)	(2.26)	(0.75)	(2.27)	(0.79)
Bank Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.7429	0.7460	0.7471	0.7506	0.7706	0.7747
No. of obs.	10,982	11,019	10,537	10,578	9,364	9,397

(Panel C) we do not find any significant results. These results indicate that deposit rates are not a dominant factor for banks' lending growth in commercial and industrial loans during the pandemic. Also, we use an alternative lending category: the quarter change in syndicated loans. The results reported in Table A3 in the Appendix, indicate that the internal capital market tool was not used during the pandemic, similar to our findings with new loans and leases and with commercial and industrial loans.[6]

4.3 The Relationship between Lending and Deposit Growth

Hitherto, we do not find a strong positive relation between deposit growth and deposit rates, and between deposit rates and loan growth during the COVID-19 pandemic. Since deposits and lending increased during the first phase of the pandemic, the decrease in deposit rates reflect that banks were not stressed on the deposit funding. This also implies that market discipline was not exercised and that the internal capital markets mechanism was not in work during the pandemic. Therefore, next we want to assess the relation between deposit growth and loan growth and whether this relation changed during the pandemic. The data on aggregate deposits suggest that the banking system was successful in strengthening their deposits in the first phase of the crisis. With the unprecedented credit line drawdowns and the subsequent increase in lending, we expect a positive relation between deposit growth and loan growth. The results in Panel A of Table 8 show that bank lending growth increased in line with deposits. Before the pandemic, the results show a negative coefficient, which implies that lending growth moved in the opposite direction of deposit growth. From Panel B and C we find that well-capitalized and large banks have greater ability to fund their lending with their deposits relative to weakly capitalized and small banks.

[6] In Table A4 in the Appendix, we also test the relationship based on bank size, measured as total assets. The results provide small statistical significance, and provide evidence that capitalization represents a bank characteristic which contains more information for banks' liquidity condition.

Covid Economics 80, 9 June 2021: 94-154

Table 7. Deposit rates and Commercial and Industrial Loan Growth

This table presents regressions of 12-month Certificate of Deposit (CD) rate, and lagged quarter-on-quarter loan growth. The CD rates are for accounts of $10k., 100k., and $500k. Loan growth is defined as quarterly change in Commercial and Industrial Loans. In Panel A the whole sample of banks is used. Panel B splits the sample by bank capitalization using the bottom 10% decile. Panel C splits the sample by bank capitalization using the top 10% decile. Independent variables are lagged by 1 quarter (i=1) and therefore for the pandemic period it reflects results after Q1 2020. Also, independent variables are lagged by 3 quarters (i=3) and therefore for the pandemic period it reflects results for Q1 2020 only. The sample is from January 2016 till December 2019 for the pre-pandemic period, and from January 2020 till December 2020 for the pandemic period. All regressions are ordinary least square regressions and have bank and quarter (time) fixed effects. Standard errors are double-clustered by bank and quarter. Inside the parentheses are the t-statistics. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Whole sample	Dependent Variables					
	12-Month CD $10k rate		12-Month CD $100k rate		12-Month CD $500k rate	
	i=1	i=3	i=1	i=3	i=1	i=3
Loan Growth (t-i) * I (q<2020Q1)	-0.0050	-0.0059	0.0004	-0.0022	0.0012	-0.0025
	(1.00)	(1.20)	(0.09)	(0.45)	(0.23)	(0.48)
Loan Growth (t-i) * I (q≥2020Q1)	-0.0016	0.0041	0.0014	0.0036	0.0033	0.0022
	(0.67)	(1.50)	(0.57)	(1.30)	(0.89)	(0.33)
Bank Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.7031	0.7031	0.7064	0.7064	0.7239	0.7239
No. of obs.	83,554	83,545	80,813	80,804	71,674	71,666

Panel B. Deposit Rates and Bank Tier 1 Capital to Risk-Weighted-Assets Ratio (10% low-capitalized banks)	Dependent Variables:					
	12-Month CD $10k rate		12-Month CD $100k rate		12-Month CD $500k rate	
	i=1	i=3	i=1	i=3	i=1	i=3
Loan Growth (t-i) × I (q<2020Q1)	0.0998	0.2070**	0.0777	-0.1680	-0.0542	0.2230**
	(0.98)	(2.07)	(0.76)	(1.57)	(0.53)	(2.01)
Loan Growth (t-i) × I (q≥2020Q1)	-0.1130***	0.0299	-0.1190***	-0.1640*	-0.9670	0.2080
	(3.16)	(0.35)	(3.29)	(1.92)	(1.29)	(0.64)
Bank Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.7406	0.7390	0.7462	0.7461	0.7698	0.7653

No. of obs.	8,062	8,072	7,798	7,810	6,861	6,875

Panel D. Deposit Rates and Bank Tier 1 Capital to Risk-Weighted-Assets Ratio (10% low-capitalized banks)	Dependent Variables:					
	12-Month CD $10k rate		12-Month CD $100k rate		12-Month CD $500k rate	
	i=1	i=3	i=1	i=3	i=1	i=3
Loan Growth (t-i) * I (q<2020Q1)	0.0022	0.0047	0.0025	0.0233	0.0082	-0.0025
	(0.04)	(0.10)	(0.05)	(0.46)	(0.16)	(0.05)
Loan Growth (t-i) * I (q≥2020Q1)	0.0200	0.0167	0.01320	0.0456	0.2190	-0.1990
	(0.34)	(0.24)	(0.22)	(0.64)	(1.47)	(1.26)
Bank Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.7493	0.7533	0.7539	0.7565	0.7740	0.7754
No. of obs.	6,925	6,952	6,642	6,669	5,890	5,917

Covid Economics 80, 9 June 2021: 94-154

CEPR PRESS

Covid Economics 80, 9 June 2021: 94-154

Table 8. Loan Growth and Deposit Growth

This table presents regressions of quarter-on-quarter loan growth on lagged quarter-on-quarter deposits growth. In columns (1) and (2) the dependent variable is net loans and leases growth and the independent is deposits growth. In columns (3) and (4) the dependent variable is net loans and leases to total assets growth and the independent is deposits to total assets growth. In Panel A the whole sample of banks is used. Panel B separates the sample by bank capitalization using the bottom 10% decile. Panel C separates the sample by bank capitalization using the top 10% decile. In columns (1) and (3) independent variables are lagged by 1 quarter (i=1) and therefore for the pandemic period it reflects results after Q1 2020. In columns (2) and (4) the independent variables are lagged by 3 quarters (i=3) and therefore for the pandemic period it reflects results for Q1 2020 only. The sample is from January 2016 till December 2019 for the pre-pandemic period, and from January 2020 till December 2020 for the pandemic period. All regressions are ordinary least square regressions and have bank and quarter (time) fixed effects. Standard errors are double-clustered by bank and quarter. Inside the parentheses are the t-statistics. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Dependent Variables:			
Panel A. Whole sample	Quarter-on-quarter Loans Growth		Quarter-on-quarter Loans to Assets Growth	
	(1)	(2)	(3)	(4)
	(i=1)	(i=3)	(i=1)	(i=3)
Deposits growth (t-i) * I (q<2020Q1)	-0.3842***	-0.1193***	-0.0301*	0.0335**
	(39.73)	(11.73)	(1.95)	(2.17)
Deposits growth (t-i) * I (q≥2020Q1)	0.5871***	0.1205***	0.0259**	0.0389***
	(85.77)	(14.65)	(2.37)	(3.11)
Bank Fixed effects	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.2892	0.2275	0.0634	0.0562
No. of obs.	105,581	105,552	105,581	105,552

	Dependent Variables:			
Panel B. Deposit Rates and Bank Tier 1 Capital to Risk-Weighted-Assets Ratio (10% low-capitalized banks)	Quarter-on-quarter Loans Growth		Quarter-on-quarter Loans to Assets Growth	
	(1)	(2)	(3)	(4)
	(i=1)	(i=3)	(i=1)	(i=3)
Deposits growth (t-i) * I (q<2020Q1)	-1.5514***	-0.5714***	0.3110	-0.0266
	(11.84)	(3.92)	(1.13)	(0.88)
Deposits growth (t-i) * I (q≥2020Q1)	-0.8024***	1.1375***	-0.0434	0.0392
	(10.39)	(10.97)	(0.27)	(0.17)
Bank Fixed effects	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.2895	0.1356	0.1711	0.1378
No. of obs.	10,536	10,533	10,536	10,533

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Panel C. Deposit Rates and Bank Tier 1 Capital to Risk-Weighted-Assets Ratio (10% low-capitalized banks)	Dependent Variables: Quarter-on-quarter Loans Growth		Quarter-on-quarter Loans to Assets Growth	
	(1)	(2)	(3)	(4)
	(i=1)	(i=2)	(i=3)	(i=4)
Deposits growth (t-i) * I (q<2020Q1)	-0.2478***	0.4409***	-0.2910**	0.0843
	(11.35)	(19.68)	(2.19)	(0.62)
Deposits growth (t-i) * I (q≥2020Q1)	0.2161***	0.3742***	0.5260*	0.9020***
	(4.31)	(6.62)	(1.72)	(2.64)
Bank Fixed effects	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.1142	0.1840	0.2468	0.1367
No. of obs.	10,680	10,677	10,680	10,677

Covid Economics 80, 9 June 2021: 94-154

Next, we use quarter-on-quarter change of commercial and industrial loans to assess the relation between growth in lending and deposit growth. The results in Table 9 show that an increase in deposit growth is associated with a strong increase in lending growth during the second phase of the pandemic. Notably, the relationship between these two, was negative before the pandemic, indicating that growth in lending was not funded by deposits. The results from Table 9 also reveal that banks with the higher exposure in Fed's liquidity facilities expand their lending in line with the growth in their deposits. In contrast, banks with low exposure in Fed's liquidity facilities expand their lending only after the first phase of the pandemic.

Covid Economics 80, 9 June 2021: 94-154

4.3 The Balance-Sheet Mechanism

4.3.1 The Relation between Deposit Rates and Liquidity Risk

In this section we investigate how liquidity-exposed banks adjust their lending, deposit rates and their exposure to the Federal Reserve liquidity facilities in reaction to the liquidity shock during the pandemic. *Banks' liquidity risk is measured through their unused credit commitments, and their wholesale funding minus available cash.* We start by assessing the relation between liquidity risk and deposit rates. To improve their liquidity condition, banks with high unused credit commitments are expected to offer higher deposit rates to increase their deposit flows. We use Certificates of Deposits (CDs) of $10k. accounts of deposit rates in columns (1) and (2) of Table 10 and CDs of $500k. in columns (3) and (4). The results show a strong negative coefficient between liquidity risk and deposit rates, which is significant at 1%. These findings imply that liquidity risk and deposit rates move in the opposite direction. This pattern continues when we test the relation for banks with the higher liquidity risk in Panel B of Table 10, indicating that banks exposed to high liquidity risk did not increase deposit rates to stem deposits outflow. One explanation for this is the surge in precautionary savings during

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Covid Economics 80, 9 June 2021: 94-154

Table 9. Loan Growth, Federal Reserve Liquidity and Deposit Growth

This table presents regressions of quarter-on-quarter loan to total assets growth on lagged quarter-on-quarter deposits to total assets growth. In Panel A columns (1) and (2) the dependent variable is commercial and industrial loans growth and the independent is deposits to total assets growth before the pandemic. In columns (3) and (4) the dependent variable is commercial and industrial loans to total assets growth and the independent is deposits to total assets growth during the pandemic. In Panel A the whole sample of banks is used. Panel B separates the sample by banks' exposure to Federal Reserve Liquidity Facilities (denoted as FL) using the bottom and top 10% deciles. In columns (1) and (3) independent variables are lagged by 1 quarter (i=1) and therefore for the pandemic period it reflects results after Q1 2020. In columns (2) and (4) the independent variables are lagged by 3 quarters (i=3) and therefore for the pandemic period it reflects results for Q1 2020 only. The sample is from January 2016 till December 2019 for the pre-pandemic period, and from January 2020 till December 2020 for the pandemic period. All regressions are ordinary least square regressions and have bank and quarter (time) fixed effects. Standard errors are double-clustered by bank and quarter. Inside the parentheses are the t-statistics. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Whole Sample	Dependent Variable: Δ Commercial and Industrial Loans to Total Assets			
	(1)	(2)	(3)	(4)
	(i=1)	(i=3)	(i=1)	(i=3)
Deposits to total assets growth (t-i) × I (<2020)	-0.006***	-0.001		
	(3.67)	(0.85)		
Deposits to total assets growth (t-i) × I (≥2020)			0.129***	0.0142
			(12.34)	(1.03)
Bank Fixed effects	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.0683	0.0667	0.5084	0.5009
No. of obs.	90,351	90,331	15,230	15,221

Panel B. Pandemic Period	Dependent Variable: Δ Commercial and Industrial Loans to Total Assets			
	Lower 10% with exposure to Fed's Liquidity		Higher 10% with exposure to Fed's Liquidity	
	(1)	(2)	(3)	(4)
	(i=1)	(i=3)	(i=1)	(i=3)
Deposits to total assets growth (t-i) × I (≥2020) × ***FL***	0.146***	-0.071*	0.061**	0.086**
	(3.79)	(1.73)	(2.11)	(2.16)
Bank Fixed effects	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.6447	0.5490	0.4914	0.4916
No. of obs.	1,522	1,521	1,523	1,523

Covid Economics 80, 9 June 2021: 94-154

Table 10. Deposit rates and Liquidity Risk

This table presents regressions of 12-month Certificate of Deposit (CD) rate, and lagged quarter-on-quarter liquidity risk. The CD rates are for accounts of $10k., and $500k. Liquidity risk is defined as quarterly change in Unused Credit Commitments and Wholesale Funding. In Panel A the whole sample of banks is used. Panel B presents results for banks with the higher (top 10%) exposure to liquidity risk. Independent variables are lagged by 1 quarter (i=1) and therefore for the pandemic period it reflects results after Q1 2020. Also, independent variables are lagged by 3 quarters (i=3) and therefore for the pandemic period it reflects results for Q1 2020 only. The sample is from January 2016 till December 2019 for the pre-pandemic period, and from January 2020 till December 2020 for the pandemic period. All regressions are ordinary least square regressions and have bank and quarter (time) fixed effects. Standard errors are double-clustered by bank and quarter. Inside the parentheses are the t-statistics. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Dependent Variables:			
Panel A. Whole sample	12-Month CD $10k rate		12-Month CD $500k rate	
	(1)	(2)	(3)	(4)
	(i=1)	(i=3)	(i=1)	(i=3)
Liquidity Risk (t-i) * I (q<2020Q1)	-0.1641***	-0.1523***	-0.1906***	-0.1943***
	(4.03)	(3.72)	(4.13)	(4.21)
Liquidity Risk (t-i) * I (q≥2020Q1)	-0.3021***	-0.3053***	-0.3345***	-0.3670***
	(6.11)	(6.25)	(5.04)	(5.50)
Bank Fixed effects	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.7033	0.7033	0.7240	0.7240
No. of obs.	83,565	83,565	71,679	71,679

	Dependent Variables:			
Panel B. Banks with higher Liquidity Risk (top 10%)	12-Month CD $10k rate		12-Month CD $500k rate	
	(1)	(2)	(3)	(4)
	(i=1)	(i=3)	(i=1)	(i=3)
Liquidity Risk (t-i) * I (q<2020Q1)	-0.4710***	-0.4278***	-0.4273***	-0.3170**
	(4.36)	(3.74)	(3.17)	(2.27)
Liquidity Risk (t-i) * I (q≥2020Q1)	-0.7390***	-0.7191***	-0.5374***	-0.4195**
	(5.83)	(5.63)	(2.93)	(2.28)
Bank Fixed effects	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.7468	0.7438	0.7658	0.7646
No. of obs.	7,969	7,995	6,628	6,644

the first phase of the pandemic and a second explanation is that these banks used the liquidity facilities offered by the Fed in order to increase their liquidity levels. We test for the latter in the following sections.

4.3.2 The Relation between Deposit Growth and Liquidity Risk

In Table 11 we present results for the relation between deposit growth and liquidity risk. The dataset on aggregate deposits suggest that the banking system was successful in attracting deposits during the pandemic. The results show a strong negative relationship between deposit growth and liquidity risk which is significant at 1%. In columns (5) and (6) the results also show that banks with the higher exposure to liquidity risk experience the higher decline in their deposits. These results indicate that banks with high liquidity risk are more vulnerable to deposit outflows, since the higher the exposure to liquidity risk, the higher the decline in deposits.

4.3.3 The Relation between Lending and Liquidity Risk

We also test how liquidity risk-exposed banks adjust lending in reaction to the liquidity shock. Liquidity risk is measured through: i) off-balance unused credit commitments which are converted into loans and add pressure to banks' liquidity condition; and ii) wholesale loan commitments which are loan commitments the interbank market. We use three categories of loans to measure growth in lending: Commercial and Industrial Loans, Net Loans and Leases, and Syndicated Loans. The results in Table 14 show a positive relationship between Liquidity Risk and growth in Commercial and Industrial Loans. This result is consistent with the dramatic increase in involuntary lending that was caused when firms drewdown credit lines and therefore, off-balance-sheet commitments were converted to loans. In contrast, when we use net loans and leases to test the relationship with liquidity risk in Panel B, we find a strong negative relationship between banks with high exposure to liquidity risk and growth in lending. These results imply that banks with high exposure to liquidity risk respond to liquidity shock

Covid Economics 80, 9 June 2021: 94-154

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by cutting back new lending. Similarly, the findings in Panel C reveal that increased exposure to liquidity risk is associated with a drop in the provision of syndicated loans.

Table 11. Deposit Growth and Liquidity Risk

This table presents regressions of quarter-on-quarter deposits to total assets growth on lagged quarter-on-quarter liquidity risk. In columns (1) and (2) the whole sample of banks is included. In columns (3) and (4) the sample is separated to banks with lower (bottom 10%) exposure to liquidity risk. In columns (5) and (6) the sample includes banks with higher (top 10%) exposure to liquidity risk. In columns (1), (3) and (5) independent variables are lagged by 1 quarter (i=1) and therefore for the pandemic period it reflects results after Q1 2020. In columns (2), (4), and (6) independent variables are lagged by 3 quarters (i=3) and therefore for the pandemic period it reflects results for Q1 2020 only. The sample is from January 2016 till December 2019 for the pre-pandemic period, and from January 2020 till December 2020 for the pandemic period. All regressions are ordinary least square regressions and have bank and quarter (time) fixed effects. Standard errors are double-clustered by bank and quarter. Inside the parentheses are the t-statistics. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Dependent Variable: Deposits to total assets growth					
	Whole sample of Banks		Lower 10% of Liquidity Risk Banks		Higher 10% of Liquidity Risk Banks	
	(1)	(2)	(3)	(4)	(5)	(6)
	(i=1)	(i=3)	(i=1)	(i=3)	(i=1)	(i=3)
Liquidity Risk (t-i) * I (q<2020Q1)	-4.2251***	-2.2994***	-5.2655	-1.3958	-6.2469***	-6.8107***
	(14.84)	(8.13)	(1.33)	(0.88)	(6.79)	(7.46)
Liquidity Risk (t-i) * I (q≥2020Q1)	-3.6686***	-1.9597***	-9.5066	-5.1037	-5.8180***	-6.2368***
	(10.75)	(5.83)	(1.63)	(1.21)	(5.75)	(6.21)
Bank Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.0768	0.0754	0.2278	0.2279	0.1886	0.1799
No. of obs.	105,611	105,611	10,487	10,436	11,129	11,154

Covid Economics 80, 9 June 2021: 94-154

Table 12. Loans Growth and Liquidity Risk

This table presents regressions of quarter-on-quarter lending growth on lagged quarter-on-quarter liquidity risk. Panel A reports results for *Commercial and Industrial (C&I) Loans*, Panel B reports results for *Net Loans and Leases*, and Panel C for *Syndicated Loans*. In columns (1) and (2) the sample includes banks with higher (top 10%) exposure to liquidity risk. In columns (3) and (4) the sample includes banks with lower (bottom 10%) exposure to liquidity risk. In columns (1), and (3) independent variables are lagged by 1 quarter (i=1) and therefore for the pandemic period it reflects results after Q1 2020. In columns (2), and (4) independent variables are lagged by 3 quarters (i=3) and therefore for the pandemic period it reflects results for Q1 2020 only. The sample is from January 2016 till December 2019 for the pre-pandemic period, and from January 2020 till December 2020 for the pandemic period. All regressions are ordinary least square regressions and have bank and quarter (time) fixed effects. Standard errors are double-clustered by bank and quarter. Inside the parentheses are the t-statistics. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: C&I Loans Growth and Liquidity Risk	Dependent Variable: Δ Commercial and Industrial Loans			
	Higher 10% of Liquidity Risk Banks		Lower 10% of Liquidity Risk Banks	
	(1)	(2)	(3)	(4)
	(i=1)	(i=3)	(i=1)	(i=3)
Liquidity Risk (t-i) * I (q<2020Q1)	89.066*	103.071**	-26.175	-1.236
	(1.73)	(2.02)	(0.81)	(0.10)
Liquidity Risk (t-i) * I (q≥2020Q1)	202.036***	223.717***	4.344	145.474***
	(3.27)	(3.69)	(0.09)	(4.25)
Bank Fixed effects	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.0443	0.0444	0.1193	0.1162
No. of obs.	11,129	11,154	10,487	10,436

Panel B: Net Loans and Leases Growth and Liquidity Risk	Dependent Variable: Δ Net Loans and Leases			
	Higher 10% of Liquidity Risk Banks		Lower 10% of Liquidity Risk Banks	
	(1)	(2)	(3)	(4)
	(i=1)	(i=3)	(i=1)	(i=3)
Liquidity Risk (t-i) * I (q<2020Q1)	359.191***	195.909*	25.230	-575.118***
	(3.52)	(1.93)	(0.09)	(5.21)
Liquidity Risk (t-i) * I (q≥2020Q1)	-243.094**	-418.632**	665.850	176.158
	(1.99)	(3.48)	(1.63)	(0.60)
Bank Fixed effects	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.0978	0.0977	0.0682	0.0713
No. of obs.	11,129	11,154	10,487	10,436

Panel C: Syndicated Loans Growth and Liquidity Risk	Dependent Variables: Syndicated Loans

Covid Economics 80, 9 June 2021: 94-154

	Whole sample of Banks		Lower 10% of Liquidity Risk Banks		Higher 10% of Liquidity Risk Banks	
	(1)	(2)	(3)	(4)	(5)	(6)
	(i=1)	(i=3)	(i=1)	(i=3)	(i=1)	(i=3)
Liquidity Risk (t-i) * I (q<2020Q1)	-19.664*	-11.464	-297.833	0.586	-27.065	-24.566
	(1.64)	(1.23)	(1.34)	(0.04)	(1.57)	(1.43)
Liquidity Risk (t-i) * I (q≥2020Q1)	-55.565***	-47.065***	126.860	175.900	-108.039***	-102.291***
	(4.38)	(4.53)	(0.74)	(1.47)	(5.55)	(5.31)
Bank Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.8222	0.8218	0.6577	0.6487	0.8367	0.8363
No. of obs.	1,706	1,706	183	182	971	972

4.4 The Relation between Fed's Liquidity Injection and Liquidity Risk

Finally, we assess the relationship between liquidity risk and banks' exposure to the Fed's liquidity facilities. We measure banks' exposure in these liquidity facilities through the quarter-on-quarter change on the sum of Total Federal Funds sold to Commercial Banks and Reverse Repurchases and Vault Cash and Cash Items and Balances due from Federal Reserve Banks. The results in Table 13 show a strong positive relationship between liquidity risk and banks' exposure to Fed's facilities. Precisely, we find that banks with high exposure to liquidity risk increase their exposure to the liquidity facilities offered by the Fed significantly more than low- liquidity risk exposed bank.

Table 13. Liquidity Risk and Federal Reserve Liquidity Facilities

This table presents regressions of quarter-on-quarter change in liquidity risk on lagged change in Fed's liquidity facilities. In columns (1) and (2) the sample includes banks with higher (top 10%) exposure to liquidity risk. In columns (3) and (4) the sample includes banks with lower (bottom 10%) exposure to liquidity risk. In columns (1), and (3) independent variables are lagged by 1 quarter (i=1) and therefore for the pandemic period it reflects results after Q1 2020. In columns (2), and (4) independent variables are lagged by 3 quarters (i=3) and therefore for the pandemic period it reflects results for Q1 2020 only. The sample is from January 2016 till December 2019 for the pre-pandemic period, and from January 2020 till December 2020 for the pandemic period. All regressions are ordinary least square regressions and have bank and quarter (time) fixed effects. Standard errors are double-clustered by bank and quarter. Inside the parentheses are the t-statistics. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Dependent Variable: Δ Liquidity Risk			
	Higher 10% Liquidity Risk Banks		Lower 10% Liquidity Risk Banks	
	(1)	(2)	(3)	(4)
	(i=1)	(i=3)	(i=1)	(i=3)
Δ Fed Liquidity (t-i) × I (q<2020Q1)	0.0026	0.0289***	0.0371***	-0.0060
	(0.29)	(3.07)	(8.31)	(1.29)
Δ Fed Liquidity (t-i) × I (q≥2020Q1)	0.0601***	0.0408*	-0.0039	-0.0146
	(3.13)	(1.85)	(0.44)	(1.28)
Bank Fixed effects	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.9542	0.9502	0.6113	0.6107
No. of obs.	10,488	10,488	10,472	10,418

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Covid Economics 80, 9 June 2021: 94-154

5. Robustness check

We present sensitivity tests in Tables 14, and 15. In addition, many sensitivity tests are also presented in the Appendix. In Table 14 we provide robustness test for the supply-side of banks' liquidity creation: the market discipline channel. Precisely, we use a different account of deposit rates, the Certificate of Deposits for $100k. The results show that the relationship between deposit rates and weakly-capitalized banks is positive again. This result supports our initial findings that market discipline was not the dominant factor for deposit rates during the pandemic. Next, we provide sensitivity tests for the demand channel of banks' liquidity creation. We assess the relation between deposit growth, loan growth and deposit rates on the county level for each bank. Deposit rates are measured based on the rate provided in Certificate of Deposits for accounts of $10k. (Panel A) and of $500k. (Panel B), while lending growth is measured through the quarter-on-quarter change of Commercial and Industrial Loans for each bank. From Panel A in Table 15 we find that, the relationship between deposit growth and lending growth is strongly positive, while the relationship with deposit rates is negative. The same pattern continues in Panel B: the relationship between deposit growth and lending growth is strongly positive, while the relationship with deposit rates is negative. All coefficients are significant at 1%. These results indicate that banks' deposit rates move in the opposite direction of deposit and lending growth, and hence the internal capital market mechanism is not in work during the pandemic.

Covid Economics 80, 9 June 2021: 94-154

Table 14. Deposit Rate and Bank Tier 1 Ratio

This table presents regressions of 12-month Certificate of Deposits (CD) rates on lagged Tier 1 Capital to Risk-Weighted-Assets Ratio. In columns (1) and (3) independent variables are lagged by 1 quarter (i=1) and therefore for the pandemic period it reflects results after Q1 2020. In columns (2) and (4) the independent variables are lagged by 3 quarters (i=3) and therefore for the pandemic period it reflects results for Q1 2020 only. The sample is from January 2016 till December 2019 for the pre-pandemic period, and from January 2020 till December 2020 for the pandemic period. All regressions are ordinary least square regressions and have bank and quarter (time) fixed effects. Standard errors are double-clustered by bank and quarter. Inside the parentheses are the t-statistics. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Dependent variable: 12-Month CD $100k rate					
	Whole Sample		Lower 10% by Capitalization		Higher 10% by Capitalization	
	(1)	(2)	(3)	(4)	(5)	(6)
	(i=1)	(i=3)	(i=1)	(i=3)	(i=1)	(i=3)
Equity to Assets ratio *(t-i)* × *I (<2020)*	0.0024***	0.0025***	0.0642***	0.0681***	0.0013*	0.0012
	(6.00)	(6.14)	(9.00)	(9.55)	(1.80)	(1.57)
Equity to Assets ratio *(t-i)* × I *(≥2020)*	0.0004	0.0007	0.0153**	0.0298***	-0.0019	-0.0021
	(0.65)	(1.03)	(2.02)	(3.82)	(1.46)	(1.56)
Bank Fixed Effects	Y	Y	Y	Y	Y	Y
Quarter Fixed Effects	Y	Y	Y	Y	Y	Y
R-squared	0.7045	0.7064	0.7592	0.7582	0.7545	0.756
Observations	122,538	117,877	12,241	11,774	12,241	11,774

Table 15. Deposit Growth, Loan Growth and Deposit Rates

This table presents regressions of quarter-on-quarter deposits growth on lagged quarter-on-quarter loan growth and 12-month Certificate of Deposit (CD) rate, and lagged quarter-on-quarter loan growth. In Panel A, the CD rates are for accounts of $10k and in Panel B for accounts of $500k. Loan growth is defined as quarterly change in net loans and leases. In columns (1) and (3) independent variables are lagged by 1 quarter (i=1) and therefore for the pandemic period it reflects results after Q1 2020. In columns (2) and (4) the independent variables are lagged by 3 quarters (i=3) and therefore for the pandemic period it reflects results for Q1 2020 only. The sample is from January 2016 till December 2019 for the pre-pandemic period, and from January 2020 till December 2020 for the pandemic period. All regressions are ordinary least square regressions and have bank and quarter (time) fixed effects. Standard errors are double-clustered by bank and quarter. Inside the parentheses are the t-statistics. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Deposit Growth, Loan Growth and 12-Month CD $10k rates

	Dependent Variable: Deposit Growth			
	(1)	(2)	(3)	(4)
	(i=1)	(i=3)	(i=1)	(i=3)
12MCD10K rate *(t-i)* × I (q<2020Q1)			137.182	-28.770
			(0.82)	(0.16)
12MCD10K rate *(t-i)* × I *(q≥2020Q1)*			-1757.555***	-1864.103***
			(5.59)	(6.96)
Net Loans and Leases Growth *(t-i)* × I (q<2020Q1)	-0.377***	-0.108***	-0.377***	-0.119***
	(12.26)	(3.35)	(11.65)	(3.47)
Net Loans and Leases Growth *(t-i)* × I (q≥2020Q1)	0.570***	0.107***	0.578***	0.084
	(26.22)	(4.08)	(25.78)	(3.11)
Bank Fixed effects	Y	Y	Y	Y
Quarter Fixed Effects	Y	Y	Y	Y
R-squared	0.2907	0.2321	0.3220	0.2561
No. of obs.	10,552	10,548	8,621	8,616

Panel B. Deposit Growth, Loan Growth and 12-Month CD $500k rates

	Dependent Variable: Deposit Growth			
	(1)	(2)	(3)	(4)
	(i=1)	(i=3)	(i=1)	(i=3)
12MCD500K rate *(t-i)* × I (q<2020Q1)			-20.795	30.749
			(0.12)	(0.17)
12MCD500K rate *(t-i)* × I *(q≥2020Q1)*			-1705.219***	-1474.226***
			(5.03)	(5.50)
Net Loans and Leases Growth (t-i) × I (q<2020Q1)	-0.377***	-0.108***	-0.342***	-0.096***
	(12.26)	(3.35)	(10.28)	(2.88)

Net Loans and Leases Growth *(t-i)* × I (q≥2020Q1)	0.570***	0.107***	0.636***	0.066**
	(26.22)	(4.08)	(26.46)	(2.50)
Bank Fixed effects	Y	Y	Y	Y
Quarter Fixed Effects	Y	Y	Y	Y
R-squared	0.2907	0.2321	0.3250	0.2502
No. of obs.	10,552	10,548	7,703	8,519

Covid Economics 80, 9 June 2021: 94-154

6. Conclusion

The COVID-19 pandemic-induced recession brought aggregate shocks to the United States banking system, with liquidity pressure and significant financial disruptions especially across the funding markets. From the onset of the pandemic, households increased their bank savings and the Federal Reserve injected liquidity to stabilize the financial system. This study shows how this expansion of liquidity was distributed across the banking system. In our first set of tests, we find that households increase savings as a precautionary action against future declines in income, while also we find no evidence for the exercise of market discipline by depositors. More precisely, depositors should discipline risky (weakly-capitalized) banks, however deposit rates are not negatively correlated with banks' capitalization, contradicting the market discipline theory for the COVID-19 pandemic period. This implies that during the pandemic weakly-capitalized banks were not forced to offer higher deposit rates to stem deposits outflow, and hence achieved to keep their cost-of-capital in sustainable levels.

Furthermore, we provide strong evidence that deposit growth is positively correlated with lending growth, but negatively correlated with lagged deposit rates. This result indicates that banks' internal capital market was not in work during the pandemic. Next, we assess how liquidity risk in the form of undrawn and wholesale loan commitments affected bank lending activities. In the beginning of the COVID-19 pandemic, undrawn loan commitments materialized as borrowers drew on preexisting commitments to improve their liquidity. Weakly-capitalized banks with high liquidity risk, increased their exposure to the liquidity facilities introduced by the Federal Reserve, more than low-commitments banks. As a result, banks honored their credit commitments, however these takedowns displaced lending capacity since weakly-capitalized banks adjusted their lending by reducing the origination of new loans. In contrast, well-capitalized banks increased lending in line with the increase in their deposits. Finally, we find that banks with high exposure to Federal Reserve's liquidity facilities

Covid Economics 80, 9 June 2021: 94-154

originated significantly more loans than banks with lower exposure. In aggregate, our results provide strong evidence that most of the decline in bank lending creation during the height of the pandemic can be explained by the degree of their exposure in liquidity risk and in the Federal Reserve's liquidity facilities.

Our findings are especially relevant for macroprudential and monetary policy makers. Since banks with greater risk to credit line drawdowns reduced their lending more than other banks, monetary policy makers can respond by designing liquidity tools that target the characteristics of these banks in future crises. Accordingly, macroprudential policy makers might wish to use banks' exposure in liquidity risk as a signal for tightening (or loosening) the time-varying loan-to-value ratios, and the counter-cyclical capital buffers. Furthermore, the absence of market discipline from depositors implies that the new deposit insurance framework introduced by the Dodd-Frank act in 2010 enhances financial stability, while also that deposit rates are not reflecting bank riskiness, but rather reveal frictions in access to new funding.

Covid Economics 80, 9 June 2021: 94-154

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Covid Economics 80, 9 June 2021: 94-154

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Appendix with Supplementary Materials

Banks' Liquidity Management During the COVID-19 Pandemic

Covid Economics 80, 9 June 2021: 94-154

Covid Economics 80, 9 June 2021: 94-154

Appendix A. Robustness Analysis

Table A1. Deposit growth, Net Loans and Leases Growth, and Deposit Rates of Large Accounts

This table presents regressions of quarter-on-quarter deposits growth on lagged quarter-on-quarter loan growth and 12-month Certificate of Deposit (CD) rate, and lagged quarter-on-quarter loan growth. The CD rates are for accounts of $500k. Loan growth is defined as quarterly change in net loans and leases. In Panel A the whole sample of banks is used. Panel B splits the sample by bank capitalization using the bottom 10% decile. Panel C splits the sample by bank capitalization using the top 10% decile. In columns (1) and (3) independent variables are lagged by 1 quarter (i=1) and therefore for the pandemic period it reflects results after Q1 2020. In columns (2) and (4) the independent variables are lagged by 3 quarters (i=3) and therefore for the pandemic period it reflects results for Q1 2020 only. The sample is from January 2016 till December 2019 for the pre-pandemic period, and from January 2020 till December 2020 for the pandemic period. All regressions are ordinary least square regressions and have bank and quarter (time) fixed effects. Standard errors are double-clustered by bank and quarter. Inside the parentheses are the t-statistics. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Deposit Growth, Loan Growth and Deposit Rates

	Dependent Variable: Deposit Growth			
	(1)	(2)	(3)	(4)
	i=1	i=3	i=1	i=3
12-Month CD $500k account rate *(t-i)* × I (q<2020Q1)			-6.099	29.469
			(0.28)	(1.27)
12-Month CD $500k account rate *(t-i)* × I *(q≥2020Q1)*			-275.610***	-222.678***
			(7.26)	(7.05)
Net Loans and Leases *(t-i)* × I (q<2020Q1)	-0.384***	-0.119***	-0.347***	-0.106***
	(39.73)	(11.73)	(32.84)	(9.56)
Net Loans and Leases *(t-i)* × I (q≥2020Q1)	0.587***	0.121***	0.658***	0.0769***
	(85.77)	(14.65)	(86.09)	(8.89)
Bank Fixed effects	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.289	0.227	0.322	0.248
No. of obs.	105,581	105,552	75,691	78,532

Panel B. Deposit Rates and Bank Tier 1 Capital to Risk-Weighted-Assets Ratio (10% low-capitalized banks)

	Dependent Variable: Deposit Growth			
	(1)	(2)	(3)	(4)
	i=1	i=3	i=1	i=3
12-Month CD $500k account rate *(t-i)* × I (q<2020Q1)			12.479	2.003

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Covid Economics 80, 9 June 2021: 94-154

	(1)	(2)	(3)	(4)
			(0.67)	(0.10)
12-Month CD $500k account rate *(t-i)* × I *(q≥2020Q1)*			-63.529**	-46.218*
			(2.02)	(1.73)
Net Loans and Leases *(t-i)* × I (q<2020Q1)	-1.551***	-0.571***	-0.818***	0.254***
	(11.84)	(3.92)	(15.48)	(4.20)
Net Loans and Leases *(t-i)* × I (q≥2020Q1)	-0.802***	1.138***	-0.354***	0.109**
	(10.39)	(10.97)	(10.78)	(2.30)
Bank Fixed effects	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.289	0.135	0.261	0.182
No. of obs.	10,536	10,533	8,054	8,044

Panel C. Deposit Rates and Bank Tier 1 Capital to Risk-Weighted-Assets Ratio (10% highly-capitalized banks)

	Dependent Variable: Deposit Growth			
	(1)	(2)	(3)	(4)
	i=1	i=3	i=1	i=3
12-Month CD $500k account rate *(t-i)* × I (q<2020Q1)			-33.341	-25.561
			(1.55)	(1.07)
12-Month CD $500k account rate *(t-i)* × I *(q≥2020Q1)*			-50.141	-67.307**
			(1.33)	(1.99)
Net Loans and Leases *(t-i)* × I (q<2020Q1)	-0.248***	0.441***	-0.089***	-0.010
	(11.35)	(19.68)	(3.27)	(0.35)
Net Loans and Leases *(t-i)* × I (q≥2020Q1)	0.216***	0.374***	0.129***	0.337***
	(4.31)	(6.62)	(3.11)	(6.97)
Bank Fixed effects	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.114	0.184	0.178	0.252
No. of obs.	10,680	10,677	6,905	6,912

Covid Economics 80, 9 June 2021: 94-154

Table A2. Deposit Growth, Commercial and Industrial Loans Growth, and Deposit Rates of Large Accounts

This table presents regressions of quarter-on-quarter deposits growth on lagged quarter-on-quarter loan growth and 12-month Certificate of Deposit (CD) rate, and lagged quarter-on-quarter loan growth. The CD rates are for accounts of $500k. Loan growth is defined as quarterly change in net loans and leases. In Panel A the whole sample of banks is used. Panel B splits the sample by bank capitalization using the bottom 10% decile. Panel C splits the sample by bank capitalization using the top 10% decile. In columns (1) and (3) independent variables are lagged by 1 quarter (i=1) and therefore for the pandemic period it reflects results after Q1 2020. In columns (2) and (4) the independent variables are lagged by 3 quarters (i=3) and therefore for the pandemic period it reflects results for Q1 2020 only. The sample is from January 2016 till December 2019 for the pre-pandemic period, and from January 2020 till December 2020 for the pandemic period. All regressions are ordinary least square regressions and have bank and quarter (time) fixed effects. Standard errors are double-clustered by bank and quarter. Inside the parentheses are the t-statistics. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Deposit Growth, Loan Growth and Deposit Rates

	Dependent Variable: Deposit Growth			
	(1)	(2)	(3)	(4)
	i=1	i=3	i=1	i=3
12-Month CD $500k account rate *(t-i)* * I (q<2020Q1)			-7.972	31.865
			(0.38)	(1.38)
12-Month CD $500k account rate *(t-i)* * I *(q≥2020Q1)*			-263.759***	-242.399***
			(7.15)	(7.71)
Commercial and Industrial Loans *(t-i)* * I (q<2020Q1)	-1.033***	-0.441***	-0.930***	-0.444***
	(39.67)	(16.25)	(33.69)	(15.34)
Commercial and Industrial Loans *(t-i)* * I (q>=2020Q1)	1.299***	-0.299***	1.550***	-0.383***
	(101.88)	(19.61)	(108.68)	(22.73)
Bank Fixed effects	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes
R-Squared	0.310	0.229	0.359	0.253
No. of obs.	105,581	105,552	75,691	78,532

Panel B. Deposit Rates and Bank Tier 1 Capital to Risk-Weighted-Assets Ratio (10% low-capitalized banks)

	Dependent Variable: Deposit Growth			
	(1)	(2)	(3)	(4)
	i=1	i=3	i=1	i=3
12-Month CD $500k account rate *(t-i)* * I (q<2020Q1)			37.043**	15.877
			(2.10)	(0.85)
12-Month CD $500k account rate *(t-i)* * I *(q≥2020Q1)*			41.619	-4.487
			(1.37)	(0.18)
Commercial and Industrial Loans *(t-i)* * I (q<2020Q1)	-3.398***	-2.628***	-0.443***	0.308**
	(9.12)	(6.89)	(3.25)	(2.12)
Commercial and Industrial Loans *(t-i)* * I (q≥2020Q1)	-0.476***	-0.260	-2.082***	-0.603***
	(3.19)	(0.84)	(18.20)	(5.19)

Bank Fixed effects	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes
R-Squared	0.281	0.123	0.368	0.226
No. of obs.	10,536	10,533	7,254	7,538

Panel C. Deposit Rates and Bank Tier 1 Capital to Risk-Weighted-Assets Ratio (10% low-capitalized banks)

	Dependent Variable: Deposit Growth			
	(1)	(2)	(3)	(4)
	i=1	i=3	i=1	i=3
12-Month CD $500k account rate *(t-i)* * I (q<2020Q1)			-40.998*	-15.793
			(1.88)	(0.64)
12-Month CD $500k account rate *(t-i)* * I *(q≥2020Q1)*			-54.863	-48.174
			(1.40)	(1.36)
Commercial and Industrial Loans *(t-i)* * I (q<2020Q1)	1.564***	-0.416***	1.929***	-0.644***
	(17.48)	(4.53)	(24.28)	(7.48)
Commercial and Industrial Loans *(t-i)* * I (q≥2020Q1)	0.736***	0.798***	1.120***	0.778***
	(6.42)	(5.59)	(10.57)	(6.40)
Bank Fixed effects	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes
R-Squared	0.131	0.152	0.278	0.257
No. of obs.	10,680	10,677	6,175	6,413

Covid Economics 80, 9 June 2021: 94-154

Covid Economics 80, 9 June 2021: 94-154

Table A3. Deposit Rates and Syndicated Loans

This table presents regressions of 12-month Certificate of Deposit (CD) rate, and lagged quarter-on-quarter syndicated loans. The CD rates are for accounts of $10k., $100k., and $500k. Independent variables are lagged by 3 quarters (i=3) and therefore for the pandemic period it reflects results for Q1 2020 only. The sample is from January 2016 till December 2019 for the pre-pandemic period, and from January 2020 till December 2020 for the pandemic period. All regressions are ordinary least square regressions and have bank and quarter (time) fixed effects. Standard errors are double-clustered by bank and quarter. Inside the parentheses are the t-statistics. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Dependent Variables:					
	12-Month CD $10k rate		12-Month CD $100k rate		12-Month CD $500k rate	
	i=1	i=3	i=1	i=3	i=1	i=3
Syndicated Loans (t-i) × I *(q<2020Q1)*	0.0036***	0.0022*	0.0000	0.0026*	0.0000	0.0040**
	(2.96)	(1.68)	(0.07)	(1.85)	(1.12)	(2.49)
Syndicated Loans (*t-i*) × I (*q≥2020Q1)*	0.0042*	0.0022	0.0006	0.0034	0.0000	0.0041
	(1.76)	(1.02)	(0.24)	(1.41)	(0.46)	(1.33)
Bank Fixed Effects	Y	Y	Y	Y	Y	Y
Quarter Fixed Effects	Y	Y	Y	Y	Y	Y
R-squared	0.6888	0.6865	0.6579	0.659	0.66	0.6619
Observations	907	907	879	879	739	739

Table A4. Deposit Rates and Commercial and Industrial loans

This table presents regressions of 12-month Certificate of Deposit (CD) rate, and lagged quarter-on-quarter loan growth. The CD rates are for accounts of $10k., 100k., and $500k. Loan growth is defined as quarterly change in Commercial and Industrial (C&I) Loans. Panel B presents the results for banks with the lower size as measured by bank assets, using the bottom 10% decile. Panel B presents the results for banks with the higher size as measured by bank assets, using the top 10% decile. Independent variables are lagged by 1 quarter (i=1) and therefore for the pandemic period it reflects results after Q1 2020. Also, independent variables are lagged by 3 quarters (i=3) and therefore for the pandemic period it reflects results for Q1 2020 only. The sample is from January 2016 till December 2019 for the pre-pandemic period, and from January 2020 till December 2020 for the pandemic period. All regressions are ordinary least square regressions and have bank and quarter (time) fixed effects. Standard errors are double-clustered by bank and quarter. Inside the parentheses are the t-statistics. ***, ** and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Bottom 10% banks by size (assets)

	Dependent Variables					
	12-Month CD $10K rate		12-Month CD $100K rate		12-Month CD $500K rate	
	i=1	i=3	i=1	i=3	i=1	i=3
C&I loans growth (t-i) × I (q<2020Q1)	-9.740	-9.790	-10.500	-5.950	-11.20*	-12.10*
	(1.49)	(1.47)	(1.57)	(0.88)	(1.66)	(1.77)
C&I loans growth (t-i) × I (q>=2020Q1)	3.090	-5.700	-5.480	-28.50*	-45.60**	-5.90**
	(0.42)	(0.34)	(0.07)	(1.73)	(2.10)	(2.32)
Bank Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.7170	0.7168	0.7179	0.7189	0.7346	0.7339
No. of obs.	6,446	6,441	6,117	6,115	5,501	5,505

Panel B. Top 10% banks by size (assets)

	Dependent Variables					
	12MCD10K rate		12MCD100K rate		12MCD500K rate	
	i=1	i=3	i=1	i=3	i=1	i=3
C&I loans growth (t-i) * I (q<2020Q1)	-0.0062	-0.0061	-0.0012	-0.0026	-0.0000	-0.0028
	(1.05)	(1.05)	(0.20)	(0.44)	(0.00)	(0.45)
C&I loans growth (t-i) * I (q>=2020Q1)	0.0035	0.0058*	-0.0033	0.0055*	0.0053	0.0036
	(1.20)	(1.79)	(1.13)	(1.65)	(1.19)	(0.45)
Bank Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.6781	0.6784	0.6733	0.6729	0.6999	0.6997
No. of obs.	8,616	8,609	8,531	8,526	7,284	7,280

Covid Economics Issue 80, 9 June 2021

The impact of mask mandates on the trade-off between GDP and COVID-19 mortality[1]

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Date submitted: 22 May 2021; Date accepted: 1 June 2021

We develop a utilitarian model to find the maximum reduction in GDP that a representative agent would be willing to accept in the current period to avoid all deaths associated with COVID-19. We discover that the answer changes with the adoption and compliance of universal mask mandates in the United States (US). In the absence of a mask mandate, we determine that the representative agent would be willing to reduce GDP by 30% (approximately \$5.7 trillion USD using 2019 levels). With mask mandates and strong compliance, the answer changes to 21% (approximately \$4.0 trillion USD). This represents a \$1.7 (USD) trillion improvement in the agent's willingness to trade-off GDP and COVID-19 deaths, attributed to how mask mandates reduce the death rate faced by the representative agent.

Covid Economics 80, 9 June 2021: 155-182

1 We thank Audra Bowlus, James B. Davies, Juan Carlos Hatchondo, and Gregory Pavlov for helpful comments. This research was completed as part of the senior undergraduate research seminar in economics at the University of Western Ontario. The views expressed in this paper are solely those of the authors and may differ from official Bank of Canada views. No responsibility for them should be attributed to the Bank of Canada. Responsibility for any errors or omissions is our own.

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Covid Economics 80, 9 June 2021: 155-182

1 Introduction

Just over a year ago, the first cases of COVID-19 began to spread in the United States (US). Since this time, COVID-19 has spread to every state, leading to the deaths of over 586,000 people and approximately 33 million cases (Johns Hopkins Coronavirus Resource Center May 18, 2021). During the initial wave of COVID-19, most state governments swiftly imposed lockdowns to curb the spread of the virus, reducing GDP to save lives. Undoubtedly, the economic costs of these lockdowns were enormous, but the virus was brought under control. However, as states grappled with subsequent waves of COVID-19, the debate between locking down and staying open was framed as a trade-off between lives and livelihood. States faced a dilemma: how should we trade-off[5] COVID-19 deaths with reductions in GDP?

To answer the previous question, Hall, Jones, and Klenow (HJK) (2020) developed a framework to find the maximum reduction in consumption that a representative agent would be willing to accept in the current period to eliminate all deaths associated with COVID-19. With an average mortality rate of 0.81% (or 0.44%), they estimated that the US should be willing to reduce consumption by 41% (or 28%).

We believe that the HJK (2020) model should be extended in several ways. First, a growing amount of literature suggests that mask mandates can decrease COVID-19 deaths while allowing the economy to remain open (Chernozhukov, Kasahara, and Schrimpf 2021; Mitze et al. 2020; Karaivanov et al. 2020). Thus, we incorporate variables for mask mandates and mask compliance in our model. Second, HJK assumes that investment is unchanged, meaning that the reduction in

[5] Please note that, unless specified otherwise, all references to a trade-off in this paper pertain to the trade-off between COVID-19 deaths and GDP.

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consumption is equal to the reduction in GDP. To address this limitation, we develop a new theoretical model based on an infinite horizon consumption-investment maximization problem. The model estimates the maximum reduction in GDP that a representative agent would be willing to accept in the current period to eliminate all deaths associated with COVID-19, with and without a mask mandate. Ultimately, our theoretical extensions of the HJK model allow us to achieve our primary objective: finding the impact of mask mandates on the trade-off.

Overall, we find that the maximum reduction in GDP that a representative agent would be willing to accept in the current period to avoid all deaths associated with COVID-19 changes with the adoption of a universal mask mandate. In the absence of a mask mandate, we discover that the representative agent would be willing to reduce GDP by 30% (approximately $5.7 trillion USD using 2019 levels). With a mask mandate and strong compliance, the answer changes to 21% (approximately $4.0 trillion USD), representing a $1.7 (USD) trillion improvement in the GDP trade-off value.

Our paper is organized as follows. Section 2 provides a brief literature review. Sections 3, 4, and 5 describe our theoretical model, numerical method, and calibration. Section 6 discusses our results and Section 7 concludes.

2 Relationship to Past Research

Similar to many other disciplines, economists have been busy since the start of COVID-19, producing extensive research on policies to reduce both losses to output (or consumption) and COVID-19 mortality. We consider related papers that present (1) economic models of the mortality versus GDP trade-off and (2) evidence on the importance of mask mandates on COVID-19 mortality.

Covid Economics 80, 9 June 2021: 155-182

Covid Economics 80, 9 June 2021: 155-182

2.1 Economic Models of The Trade-off

Our study adds to the growing body of literature that examines the trade-off between losses to output or consumption and COVID-19 deaths. One such strand of this literature combines the susceptible-infectious-recovered (SIR) model with economic theory to derive optimal policy approaches to the pandemic[6]. These models analyze the economy and the virus in a dynamic fashion, varying lockdown measures as the pandemic progresses.

Our study differs from the SIR-based models in that we do not aim to derive dynamic policy responses. Rather, we analyze how much a representative agent would be willing to reduce consumption and investment to avoid all COVID-19 deaths, which builds on the framework of standard intertemporal consumption and utilitarian welfare theory presented in HJK. Of the SIR-based models, our study aligns most closely with Jones, Philippon, and Venkateswaran (2020) who considers optimal mitigation policies in response to an epidemic. They assume a social planner must consider a health congestion externality and an infection externality[7]. They highlight how the full social cost of the pandemic is not internalized by private citizens. The social planner then implements a policy to suppress the spread of the virus, concluding that a drop in consumption of 25% is needed to decrease the death rate to 0.15%.

[6] A non-exhaustive list includes Alvarez, Argente, & Lippi (2020), Piguillem & Shi (2020), Berger, Kerkenhoff, & Mongey (2020), Acemoglu et al. (2020), Eichenbaum, Rebelo, & Trabandt (2020), and Bethune & Korinek (2020).

[7] A health congestion externality is when agents do not consider that spreading the virus increases the mortality rate from an overwhelmed healthcare system. On the other hand, an infection externality is when agents do not consider the risks of spreading the virus to others.

2.2 Mask Mandates and The Spread of COVID-19

Our analysis is also related to the literature on the adoption of universal mask mandates in reducing the spread of the virus. Van Dyke et al. (2020) examine the impact of the Kansan Governor's executive order requiring the use of face masks in public spaces, which was conditional on counties opting out. In total, 81 counties opted out of the mandate and saw a 100% increase in the incidence of COVID-19 between July 3-9 and August 17-23. On the other hand, the 24 counties with mask mandates saw a 6% decrease in COVID-19 incidence between July 3-9 and August 17-23. More broadly, in the US, evidence from Chernozhukov, Kasahara, and Schrimpf (2021) indicates that mask mandates can decrease COVID-19 deaths without the need for stricter lockdowns. Finally, Mitze et al. (2020) and Karaivanov et al. (2020) also provide evidence showing that mask mandates reduced the spread of COVID-19 in Germany and Canada. These studies suggest that the trade-off between lives and livelihoods may not be a simple binary choice of whether the government should impose non-pharmaceutical interventions (e.g., closure of nonessential businesses, stay-at-home orders) or save jobs. Indeed, universal adoption of face masks reduces the spread of the virus and thus deaths with negligible economic costs. To date, the argument in favour of mask mandates has focused more on their role in limiting the spread of the virus. Our analysis extends this argument by considering how mask mandates not only impact COVID-19 mortality, but also GDP levels.

3 Model

In this section, we introduce a new model that characterizes a consumption-investment problem with an infinite horizon. This diverges from HJK as they implicitly assume that the change in consumption is equal to the change in GDP, and therefore, investment is unchanged. However, this may be sub-optimal, as one could cut investment and smooth the consumption loss across

Covid Economics 80, 9 June 2021: 155-182

time. Note that in our model α represents the amount of GDP (as opposed to consumption) the representative consumer would be willing to forego to avoid the risk of COVID-19 deaths. Let $\alpha = 1 - \lambda$.

Consumer Problem

We introduce a closed economy[8] with investment and an infinite time horizon for a representative consumer[9]. For a given time period t, let c_t be the consumption flow, let i_t be the consumer's investment, let w_t be the hourly wage for the representative consumer, let l_t be the labour supplied by the consumer, let r_t be the rental rate for capital, and let k_t be the capital stock owned by the consumer. β is the discount factor and $u(c)$ is the flow utility from consumption c. φ is the depreciation rate of the capital stock, which is assumed to be constant throughout time. The consumer solves:

$$\max_{c_t} \sum_{t=0}^{\infty} \beta^t u(c_t)$$

$$subject\ to\ c_t + i_t = w_t l_t + r_t k_t$$

$$where\ k_{t+1} = (1 - \varphi)k_t + i_t\ and\ l_t \leq \bar{l}$$

[8] We considered introducing either: (1) a closed economy with investment or (2) an open economy without investment. Our research revealed that, during the pandemic, investment has significantly higher declines relative to the current account. Thus, we have chosen to model our economy as closed with investment.

[9] We justify a representative agent as we calculate δ based on age-specific mortality rates summed across population shares, thus accounting for the variation in mortality risk from age. HJK shows that their linearized approach is the same for a representative agent and a fully specified model for each age. Moreover, we assume no taxes in this model as there is no government, aligning with HJK and allowing for a more direct comparison of estimates. However, future extensions could consider including a government and thus, lump-sum or distortionary taxes.

Covid Economics 80, 9 June 2021: 155-182

Note, we set $\beta = \hat{\beta}\,\bar{S}$, where $\hat{\beta}$ is the pure discount time factor and $\bar{S}$ is the average survival probability across all ages from t to $t+1$. The assumption of a constant average survival probability across time is appropriate in a developed country, such as the US. As in the HJK, we assume that contracting COVID-19 is associated with a decline in the average survival rate $\bar{S}$ by $\bar{\delta}$, which is the increased mortality rate. However, we define the mortality risk as:

$$\bar{\delta} = \delta * (1 - \rho * MM * C)$$

where δ is the increased death rate among the population from COVID-19, MM is an indicator variable of whether or not the society has a mask mandate (taking values 0 or 1), C represents the percentage of mask compliance in society, and ρ is the percentage reduction in the death rate if all citizens wore masks in public spaces.

Note that this model assumes the pandemic lasts for one year and thus the decreased mortality, δ, only affects the survival probability for the second period value. Further, the only form of savings for the consumer is through capital accumulation (i.e., investment decisions).

The maximum number of hours the consumer can work is $\bar{l}$, which is normalized to 1. Note that leisure is not valued in our simplified model, thus a rational consumer would optimally supply the full number of hours working, as income is strictly increasing as a function of labour supply. This means we can remove the inequality constraint and assume that $l_t = \bar{l} = 1$.

<u>Firm Problem</u>

We assume that a firm only lives for one period, where they aim to maximize profits. However, new firms are created in subsequent periods. We assume that the firm's production function, which is the level of GDP, depends on the capital and labour amounts. Moreover, we will assume that the firm has a Cobb-Douglas production function, where z_t is the productivity parameter for t and ψ is the capital share of income. Thus, the production function is:

$$f(k_t, l_t) = z_t k_t^{\psi} l_t^{1-\psi}$$

Therefore, we define the firm's problem for a given period as the following:

$$\max_{l_t, k_t} \pi_t = z_t k_t^{\psi} l_t^{1-\psi} - r_t k_t - w_t l_t$$

We derive the equilibrium conditions by setting the first order conditions for the partial derivatives with respect to k_t and l_t. These are:

$$r_t = \frac{\partial f(k_t, l_t)}{\partial k_t} = \psi * z_t k_t^{\psi-1} l_t^{1-\psi}$$

$$w_t = \frac{\partial f(k_t, l_t)}{\partial l_t} = (1-\psi) * z_t k_t^{\psi} l_t^{-\psi}$$

The first order conditions imply that:

$$y_t = z_t k_t^{\psi} l_t^{1-\psi} = w_t l_t + r_t k_t$$

Revisiting the Consumer Problem:

By substituting the optimality conditions of the firm, we simplify the consumer's problem:

$$\max_{c_t} \sum_{t=0}^{\infty} \beta^t u(c_t)$$

$$subject\ to\ c_t + k_{t+1} = z_t k_t^{\psi} + (1-\varphi) k_t$$

Solving the Consumer Problem:

Covid Economics 80, 9 June 2021: 155-182

We can solve the consumer's problem through solving the Bellman equation for our model. We define the welfare function V, which is a value function, as the present discounted utility flows for a consumer beginning with capital amount k. We will make a further assumption that the productivity parameter is constant over time. The firm chooses k', the capital stock in the next period, to maximize the following:

$$V(k) = \max_{k'}[u(c) + \beta V(k')] \qquad (1)$$

$$s.t. \quad c + k' = zk^{\psi} + (1 - \varphi)k \quad (2)$$

Equivalent Variation:

We aim to determine $\lambda = 1 - \alpha$ such that the consumer is indifferent between the reduction in GDP and COVID-19 deaths. We re-define the consumer's problem to solve this[10]:

$$V(k, \lambda, \delta) = \max_{k'}\left[u(c) + \hat{\beta}(\bar{S} - \delta)V(k')\right]$$

$$s.t. \quad c + k' = z\lambda k^{\psi} + (1 - \varphi)k$$

Thus, we solve the following indifference relationship: $V(k, \lambda, 0) = V(k, 1, \delta)$, where:

$$V(k, \lambda, 0) = \max_{k'}\left[u(c) + \hat{\beta}\bar{S}V(k')\right]$$

$$s.t. \quad c + k' = z\lambda k^{\psi} + (1 - \varphi)k$$

$$V(k, 1, \delta) = \max_{k'}\left[u(c) + \hat{\beta}(\bar{S} - \delta)V(k')\right]$$

[10] In this first relationship, we assume there are no mask mandates in place, thus $\bar{\delta} = \delta$.

Covid Economics 80, 9 June 2021: 155-182

$$s.t.\ c + k' = zk^{\psi} + (1 - \varphi)k$$

Next, to determine the influence of mask mandates on this trade-off, we use the following indifference condition: $V(k, \lambda, 0, 0, 0, 0) = V(k, 1, \delta, \rho, MM, C)$, where:

$$V(k, \lambda, \delta, \rho, MM, C) = \max_{k'} \left[u(c) + \hat{\beta}(\bar{S} - \delta * (1 - \rho * MM * C))V(k')\right]$$

$$s.t. c + k' = z\lambda k^{\psi} + (1 - \varphi)k$$

4 Numerical Method

We solve the model through a dynamic programming method for optimization problems over time. Equation 1 is the Bellman equation for our model. A full description of how we numerically solve the model can be found in Appendix A.

4.1 The Steady State of Capital

In order to solve the indifference condition $V(k, \lambda, 0) = V(k, 1, \delta)$, we will assume this condition holds for the steady state of capital, k^*. Thus, we solve $V(k^*, \lambda, 0) = V(k^*, 1, \delta)$.

We calculate k^* through the Euler's condition for our model, which implies that:

$$\beta\left(1 - \varphi + \psi z(k^*)^{\psi - 1}\right) = 1$$

4.2 The Steady State of Consumption

We solve for the steady state of consumption, based on the steady state of capital. We use the consumer budget constraint to solve for this:

$$c^* + k^* = zk^{*\psi} + (1 - \varphi)k^*$$

$$c^* = zk^{*\psi} - \varphi k^*$$

Covid Economics 80, 9 June 2021: 155-182

4.3 Initial Level of Utility

Next, we solve for the initial level of utility for the consumer in terms of v for the steady state level of consumption. First, note that v, the value of a year of life in terms of years of per capita consumption, is equal to the following, as informed from HJK:

$$v = \frac{u(c^*)}{u'(c^*) * c^*}$$

Further, as per HJK, we assume diminishing marginal returns of consumption based on the CRRA utility function of the following form, where γ is the constant risk aversion parameter:

$$u(c^*) = \bar{u} + \frac{c^{*1-\gamma}}{1-\gamma}$$

$$u'(c) = c^{*-\gamma}$$

Therefore, we find that:

$$\bar{u} = v * c^{*1-\gamma} - \frac{c^{*1-\gamma}}{1-\gamma}$$

Note that this value of $\bar{u}$ is used for when solving the model with the grid search method.[11]

5 Calibration for Representative Consumer

We begin by calibrating for the steady state of capital. For the depreciation rate, we use estimates from the Groningen Growth and Development Centre (February 18, 2021). From this,

[11] An alternate form of the CRRA utility function is $u(c^*) = \bar{u} + \frac{c^{*1-\gamma}-1}{1-\gamma}$. Note this can be used for our model, but one must adjust $\bar{u} = v * c^{*1-\gamma} - \frac{c^{*1-\gamma}}{1-\gamma} + \frac{1}{1-\gamma}$.

we use the depreciation rate for 2019 in the US as 4.56965%. We assume $\hat{\beta} = 0.95$ and $\psi = 0.3$ for our analysis, which are common assumptions based on economic literature and estimates for the capital share of income (Gourinchas and Parker 2002; Borjas, n.d.). Next, we also found that $\bar{S} = 0.99120414$ [12], which is the average survival probability in the US. This was calculated using the most recent Actuarial Life Table from the Social Security Administration and the US's population age structure (Office of the Chief Actuary, n.d.; US Census Bureau June 17, 2020). As well, we normalize the productivity parameter and mask compliance to be 1. Finally, we use $\gamma = 2$ and $v = 6$, based on HJK. Using our steady state equation, these values imply $k^* = 4.322726$, $c^* = 1.353879$, and $\bar{u} = 5.170329$.

Next, we searched for an accurate estimate of the death rate of COVID-19 among the population. We estimate the infection fatality ratio as 0.740689%, which is obtained using Brazeau et al. (2020) and the US's population age structure (US Census Bureau 2019; Werner 2011). Further, note this is slightly lower than the higher calibrated values from HJK, which reflects an improved understanding of COVID-19. Using the same approach at HJK and the fact that herd immunity is achieved at approximately 75% of the population becoming infected (HJK 2020), we can represent the overall increase in death among that population as $\delta = 0.740689\% * 0.75 = 0.55551675\%$. Based on the findings in Chernozhukov, Kasahara, and Schrimpf (2021), we set $\rho = 0.40$. This means that mask mandates have a policy effect of reducing deaths by approximately 40%. In our model, this means the representative consumer faces a death rate of 40% lower than in the base case scenario without mask mandates.

[12] Note that this implies $\beta = \hat{\beta}\,\bar{S} = 0.95 * 0.99120414 = 0.941643933$

Covid Economics 80, 9 June 2021: 155-182

Figure 1: Summary of Calibration

Parameter	Symbol	Calibrated Value	Source
Depreciation rate	φ	0.0456965	Groningen Growth and Development Centre (February 18, 2021)
Pure discount time	$\hat{\beta}$	0.95	Gourinchas and Parker (2002)
Capital share of income	ψ	0.3	Borjas (n.d.)
Average survival probability	$\bar{S}$	0.99120414	Office of the Chief Actuary (n.d.); US Census Bureau (June 17, 2020).
Productivity	z	1	Normalized
Mask compliance	C	1	Normalized
Constant risk aversion	γ	2	HJK (2020)
Value of a year of life in terms of years of per capita consumption	v	6	HJK (2020)
Steady state of capital	k^*	4.322726	Follows from γ and v
Steady state of consumption	c^*	1.353879	Follows from γ and v
Initial level of utility	$\bar{u}$	5.170329	Follows from γ and v
Increase in mortality rate	δ	$0.740689\% * 0.75 = 0.55551675\%$	Brazeau et al. (2020); US Census Bureau (2019); Werner (2011); HJK (2020).
Percentage reduction in the mortality risk with a mask mandate	ρ	0.40	Chernozhukov, Kasahara, and Schrimpf (2021)

6 Presentation and Discussion of Findings

6.1 Value Function

We solve for the value function in the Bellman Equation (1) with constraint Equation (2). Appendix B.1 shows the value function, which depends on the initial capital level. The function is concave, displaying decreasing marginal returns to initial capital.

6.2 Trading-off GDP and COVID-19 Deaths

We aim to solve the following indifference relationship: $V(k^*, \lambda, 0) = V(k^*, 1, \delta)$. Based on our calibration of δ, we solve the following indifference relationship by choosing λ such that $V(4.322726, \lambda, 0) = V(4.322726,1,0.55551675)$. We find that $V(4.322726,1,0.55551675) \approx 69.63378$. Next, we solve for λ and uncover that $V(4.322726, \lambda, 0) \approx V(4.322726,1,0.55551675)$ when $\lambda = 0.70$. This implies $\alpha = 0.30$ or in other words, the representative consumer would be willing to reduce GDP by approximately 30% for one year to remove all of the risk of mortality from COVID-19. Using 2019 Real GDP levels, this represents an approximate \$5.7 (USD) trillion decline in GDP (Bureau of Economic Analysis March 25, 2021a). See Appendix B.2 for the value function under the full mortality risk (i.e., no lockdown, completely open). As well, see Appendix B.3 for the graphs for the value function under the reduction in GDP (i.e., complete lockdown).

6.3 Mask Mandates

We aim to solve the following indifference relationship: $V(k^*, \lambda, 0, 0, 0, 0) = V(k^*, 1, \delta, \rho, MM, C)$. Based on our calibration, we must solve the following indifference relationship for λ: $V(4.322726, \lambda, 0, 0, 0, 0) = V(4.322726, 1, 0.55551675, 0.40, 1, 1)$. First, we determine that $V(4.322726, 1, 0.55551675, 0.40, 1, 1) \approx 72.01957$. Next, we solved for λ and found that $V(4.322726, \lambda, 0, 0, 0, 0) \approx V(4.322726,1,0.55551675,0.40,1,1)$ when $\lambda = 0.79$. This implies $\alpha = 0.21$ or in other words, the representative consumer would be willing to reduce GDP by approximately 21% for one year to remove the risk of mortality from COVID-19. Using 2020 Real GDP levels, this represents an approximate \$4.0 (USD) trillion decline in GDP (Bureau of Economic Analysis March 25, 2021a). Our theoretical results suggest that mask mandates improve the trade-off between GDP and COVID-19 deaths by 9% of GDP, which represents a $1.7 (USD) trillion improvement in GDP (Bureau of Economic Analysis March 25, 2021a).

6.4 US Data

We obtain quarterly economic data for the US from the Bureau of Economic Analysis (March 25, 2021b) shown in Figure 2. In general, the data from Q2 2020 aligns closely with the findings in our theoretical table, as seen in Figure 2. Our interpretation of this is as follows: during the initial wave of the pandemic, with uncertainties about the future course of the virus, nearly all state governments imposed strict lockdown measures, optimistic that COVID-19 infections and deaths could be reduced to as low as possible. This reduction in GDP would be equivalent to the maximum reduction in GDP they would be willing to accept to avoid all deaths. However, as states faced subsequent waves of the pandemic, there was growing reluctance to impose additional restrictions due to other economic costs of lockdown not considered in this model (e.g., mental health). Hence, the economic data from Q2 2020 offer the most accurate representation of our theoretical model. Furthermore, our model can be used to justify the strict lockdown approach taken at the start of the pandemic when governments were trying to halt the spread quickly.

Figure 2: Real GDP for the US (Bureau of Economic Analysis March 25, 2021b)

Seasonally Adjusted at Annual Rates, Percent Change from the Previous Quarter

	Q1 2020	Q2 2020	Q3 2020	Q4 2020
Real Gross Domestic Product (GDP)	-5.0	-31.4	33.4	4.3
Personal Consumption Expenditures (PCE)	-6.9	-33.2	41.0	2.3
Gross Private Domestic	-9.0	-46.6	86.3	27.8

Investment (GPDI)	

6.5 Comparisons to Other Results

We compare our result of $5.7 (USD) trillion as the maximum reduction in GDP in the lockdown scenario without mask mandates to other findings. Our estimate is comparable to the HJK estimate of $6.1 (USD) trillion reduction in consumption (environment without investment). Part of the differences in the estimates stem from different mortality rates being used. Interestingly, our new model with investment and a Cobb-Douglas production function results in similar estimates to HJK, who approach the problem from a social welfare standpoint.

6.6 Variation Across Regions

We estimate how the willingness to reduce GDP to avoid all the deaths from COVID-19 varies across US regions. We use large regions of the US as they are more likely to exhibit closed economy characteristics. Therefore, we follow the approach of the US Census Bureau and define four main regions: the Northeast, the Midwest, the South, and the West. We use the same estimation procedure as above, but we vary the death rate between the four regions. We use the death rate found in Brazeau et al. (2020). We then calculate the death rate for each region based on their age distribution using data from the US Census Bureau (2019) and Werner (2011). Figure 3 displays the death rate for each US region and the corresponding maximum willingness to reduce GDP based on our theoretical model. Figure 4 shows the decrease in real GDP in each US region.

Covid Economics 80, 9 June 2021: 155-182

Figure 3: Death Rate by Region based on the Imperial College study (Brazeau et al. 2020) and Corresponding Maximum Willingness to Reduce GDP

Region	Death Rate	Willingness to reduce GDP
Northeast	0.008117517	32%
Midwest	0.007654169	31%
South	0.007245094	30%
West	0.0069428	29%
US	0.00740689	30%

Figure 4: Real GDP by US Region (Bureau of Economic Analysis March 26, 2021)

Seasonally Adjusted at Annual Rates, Percent Change from the Previous Quarter

	Q1 2020	Q2 2020	Q3 2020	Q4 2020
Northeast	-0.0531667	-0.3472328	0.33324258	0.04208033
Midwest	-0.0593815	-0.321371	0.37501723	0.04220805
South	-0.052582	-0.2985217	0.32386159	0.05076179
West	-0.0393652	-0.3045457	0.3251295	0.03699876

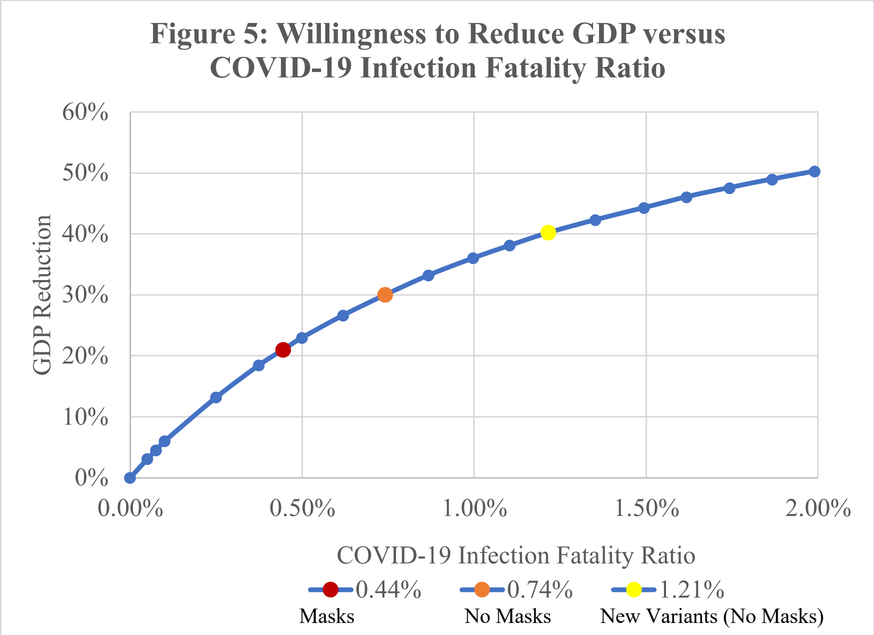
Our analysis shows there is cross-sectional variety on the willingness to reduce GDP across US regions. This variation stems from differences in the increased death rate faced, which is influenced by the population age structure. Comparing predictions in Figure 3 to Figure 4, we again see that Q2 2020 GDP most closely aligns to our model, which can be explained in a similar fashion to the US data. Furthermore, the cross-sectional variety in the US Q2 2020 GDP largely aligns with the theoretical predictions: the Northeast has the largest GDP reduction and the Midwest has the second largest decrease. However, the West and the South have similar decreases. Nevertheless, our model provides support for the Northeast's decision of a relatively larger GDP reduction in Q2 2020 compared to the rest of the US.

Covid Economics 80, 9 June 2021: 155-182

Covid Economics 80, 9 June 2021: 155-182

6.7 Comparative Statics

We conduct comparative statics analysis on the value of the increased death rate in the population ($\delta = 0.75 * infection\ fatality\ ratio$), constant risk aversion (γ) and the value of a year of life in terms of years of per capita consumption (υ). We present the results in the figures below. In our base case, we set the values to our calibrated choices. All values are held constant while only varying one of the parameters in the following figures.



We find that as the death rate increases, the consumer's willingness to decrease GDP to avoid deaths increases. This confirms the intuition that higher death rates are "bad" for society. Moreover, we observe a concave shape, suggesting decreasing returns to the willingness to reduce GDP. Moreover, this graph suggests that countries should be willing to forego more GDP in response to the new variants with higher death rates to avoid mortality risk (Challen et al. 2021).

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Figure 6: Willingness to Reduce GDP versus Level of Risk Aversion

Level of Risk Aversion (γ)	% Willingness to Reduce GDP to Avoid COVID-19 Deaths (Without Mask Mandates)
1 (i.e., $\boldsymbol{ln(c)}$)	36%
1.5	33%
2	30%
3	26%
4	23%

We show that as gamma increases, the representative consumer is willing to reduce GDP by a lower amount to avoid COVID-19 deaths. This is interpreted as higher levels of risk aversion in terms of consumption, resulting in a lower willingness to reduce GDP in the current period. We see a concave shape to this relationship as gamma increases.

Figure 7: Willingness to Reduce GDP versus Value of a Year of Life

Value of a Year of Life in Years of Per Capita Consumption (v)	% Willingness to Reduce GDP to Avoid COVID-19 Deaths (Without Mask Mandates)
4	22%
5	27%
6	30%
7	33%
8	37%

We determine that as the value of a year of life in years of per capita consumption increases, which is directly related to $\bar{u}$, the willingness to reduce GDP to avoid COVID-19 deaths increases. This follows a natural interpretation: as society values lives more, they are willing to forego more GDP in order to avoid deaths. The relationship appears to be approximately linear for the parameter values considered[13].

[13] Note these values were tested as informed by HJK.

Covid Economics 80, 9 June 2021: 155-182

7 Conclusion

The objective of our analysis was to assess the impact of mask mandates on the trade-off between GDP and COVID-19 mortality, which was achieved through the following:

1. We created a new model with an infinite horizon consumption-investment maximization problem to estimate the maximum reduction in GDP that a representative agent would be willing to accept in the current period to eliminate all deaths associated with COVID-19.
2. We set the death rate to be a function of mask mandates and mask compliance.

Our model was solved using a dynamic optimization approach. More specifically, we iteratively solved the Bellman equation numerically using the grid search method. We obtained the following results:

1. Without a mask mandate, the representative agent would be willing to accept a reduction in GDP of 30% (approximately $5.7 trillion USD using 2019 Real GDP levels).
2. With a mask mandate, (1) changes to 21% (approximately $4.0 trillion USD).

Our model results suggest that mask mandates improve the trade-off between GDP and COVID-19 deaths by 9% of GDP, which represents a $1.7 (USD) trillion improvement in GDP (Bureau of Economic Analysis March 25, 2021a). With negligible economic costs and positive effects on reducing the spread of the virus, mask mandates allow a larger portion of the economy to remain open for a given mortality rate.

Comparing the results in our model without a mask mandate to observed choices in the US, we can conclude that during the initial wave of the pandemic (Q2 2020), most state governments imposed strict lockdowns with the goal of driving infection and death rates close to zero. However, as the pandemic progressed, the combination of the additional economic costs of imposing restrictions (e.g., mental health) and the near epidemiological impossibility of driving infection rates to zero, led many states to keep the economy open. However, through our counterfactual policy experiment, we can see that a mask mandate would have positively improved the trade-off by decreasing the maximum willingness to reduce GDP by 9 percentage points. Further, we find our model can help explain the cross-sectional variation in Q2 2020 GDP in the four main US regions. Comparative statics reveal that the maximum willingness to reduce GDP increases with the death rate, decreases with the risk aversion of consumption, and increases with the value of a year of life in years of per capita consumption.

Our theoretical model could be extended in several ways. Firstly, one could imagine adding government to the consumption-investment maximization problem. It is apparent that governments, especially in developed countries, provided enormous stimulus packages to reduce some of the economic burden facing consumers. Furthermore, as mentioned in HJK, the model could be extended to account for morbidities, inequality of consumption, as well as additional costs (e.g., declining mental health) and benefits (e.g., reductions in pollution) from the pandemic (see HJK for a more in-depth discussion of these extensions).

Summing up, our paper presents an extended framework to analyze a representative consumer's decision on the trade-off between GDP and COVID-19 deaths. We believe this analysis is useful in analyzing optimal policy in response to a pandemic or other threats to mortality and can inform the decisions of agencies going forward.

Covid Economics 80, 9 June 2021: 155-182

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9 Appendices

Appendix A Grid Search Method

In order to solve the Bellman equation, one could do any of the following approaches: (1) guess and check the form of the value function, (2) iteratively solve the Bellman equation and value function analytically or (3) iteratively solve the Bellman equation numerically. We adopt the third method for our analysis since a closed form equation cannot easily be found.

To solve our model numerically, we employ the grid search method. Note, we use the grid search method for discrete levels of capital, which means that the consumer can choose a level of capital k' from the set $K = \{k_1, k_2, \ldots, k_N\}$ where $N \epsilon \mathbb{N}$. The method comprises of the following steps:

1) Assume the form $V^0(k) = 0$ for all k in the first iteration.
2) Solve the Bellman equation assuming the value function $V^0(k)$ for the following period. This means we solve:

$$V^1(k) = \max_{k' \epsilon K}[u(c) + \beta V^0(k')]$$

$$s.t. \quad c + k' = zk^{\psi} + (1 - \varphi)k$$

3) Find

$$L = \max_{i \epsilon [1,N]}[V^1(k_i) - V^0(k_i)]$$

4) If L is below 10^{-6}, which is our ϵ criterion for convergence, then we assume this is the solution to the problem and that $V(k) = V^1(k)$. If L is above this threshold, then we repeat steps 1-3 using $V^1(k)$ instead of $V^0(k)$. Finally, we repeat this iterative process until a $V^i(k)$ converges. This $V^i(k)$ is then assumed to be the form of the value function.

We implement the grid search method using R.

Covid Economics 80, 9 June 2021: 155-182

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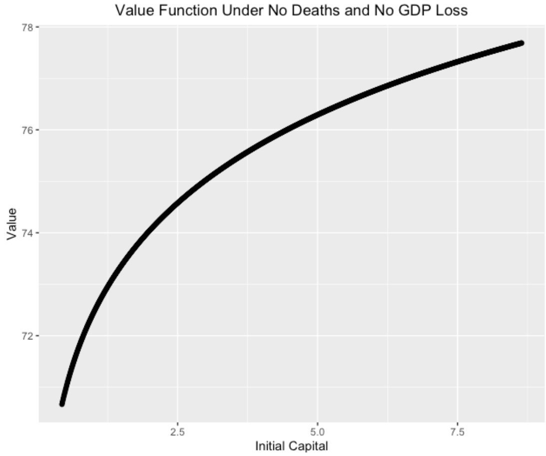
Appendix B Value Function Graphs

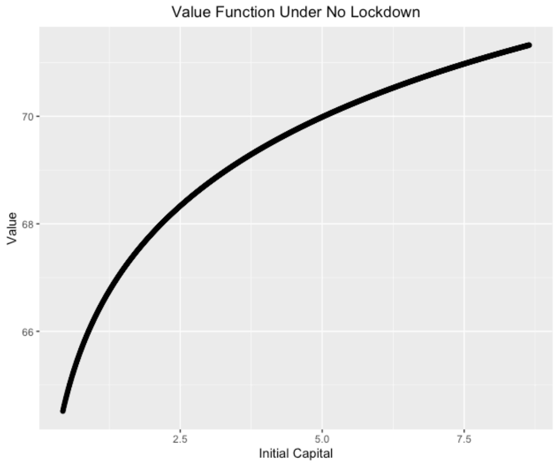
Figure B.1 below displays how the current capital levels affect the consumption in the steady state.

Figure B.1: Value Function for (8) with Constraint (9)



Figures B.2 below is the graphs for the value function under the full mortality risk.

Figure B.2: Value Function – No Lockdown



Covid Economics 80, 9 June 2021: 155-182

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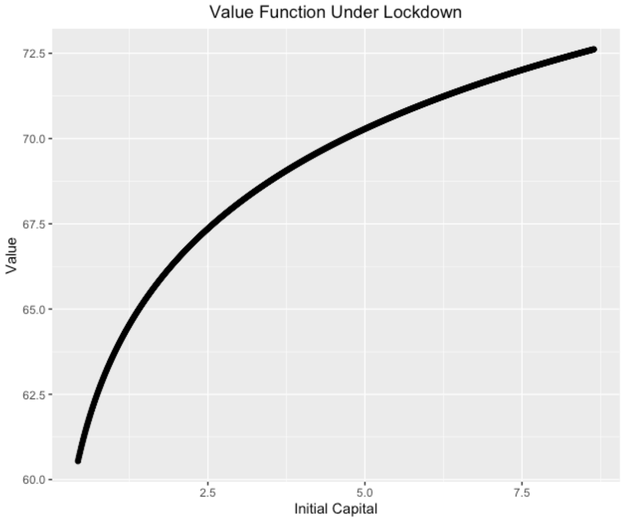
Figures B.3 below is the graph for the value function under the reduction in GDP.

Figure B.3: Value Function – Lockdown



Covid Economics 80, 9 June 2021: 155-182

CEPR PRESS

Covid Economics Issue 80, 9 June 2021

COVID-19 and the environment: An investigation of the COVID-19 pandemic effects of the air quality in Europe

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Date submitted: 22 May 2021; Date accepted: 27 May 2021

This study investigates empirically the effects the new COVID-19 virus has had on European air quality. We focus on 31 European countries and three air pollutants, namely NO_2, PM_{10}, and $PM_{2.5}$ and we start with a comparison of the air pollution levels before and after the pandemic, focusing on the years 2018-2019 on one side and 2020-2021 on the other. We find that the level of each pollutant dropped significantly after the beginning of the virus. In addition, we present an individual country analysis on how new monthly COVID-19 cases affect each pollutant for the years 2020 to 2021 and finally we present a fixed effects panel data analysis for the same years for the whole of Europe. We also consider how air pollution in earlier periods may have affected the spread of COVID-19 through its negative health effects. We conclude that the measures taken to control the spread of the COVID-19 virus did not only have negative impacts (as in the economy, tourism and other aspects) but they also had some positive effects mainly on the environment by reducing air pollution emissions due to restrictive government policies pursued to contain the spread of the virus. However, it is also clear that previous air pollution levels have acted as an important factor in explaining the COVID-19 spread due to their negative effects on health.

Covid Economics 80, 9 June 2021: 183-210

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Covid Economics 80, 9 June 2021: 183-210

1 Introduction

The highly infectious nature of the COVID-19 virus made it spread to the rest of the world within months since the first cases appeared in late 2019 in Wuhan, China. All European countries had at least one case by March 2020, with France having the first confirmed case there. Italy, Spain, and the United Kingdom were some of the countries that suffered most from the virus. Their high number of COVID-19 cases by the end of March 2020, combined with the cases of the rest of countries in Europe, made the continent to be the active epicenter of the pandemic. Because of the nature of the virus, and the high daily increase in its cases, countries decided to impose some measures to eliminate or at least reduce the spread of COVID-19. Mandatory quarantines, lockdowns, social distancing, restrictions on travel, entry bans, curfews, stay at home orders, work from home measures or even pause of production or work, were some of the measures that countries took. However, that had a strong negative impact on many aspects of activity in each country, with the more obvious to be economic activity. Many companies had to pause or stop their operation, leaving many people unemployed. Global stock markets fell due to the rise of COVID-19 cases on February of 2020, while the global insurance industry is estimated to have high losses equal to US$204 billion. Tourism is another sector that faced a negative impact, affecting the airlines, train stations, cruise lines, hotels and restaurants. Museums, theaters, concerts, sport events and other art related events were closed or canceled, in addition to the cancellation of many religious celebrations and events. Finally, COVID-19 impacted negatively the educational system as well, as most of the countries switched from in person to online education.

However, the spread of the virus had also some positive effects. Because of the lockdowns and the social distancing rules, there was a high reduction of hospital visits for other reasons except COVID-19, as well as a reduction of the spread of sexually transmitted infections. Moreover, the measures taken to minimize the new COVID-19 cases, contributed to a cleaner environment. Air quality improvement, water pollution mitigation and reduction in carbon emissions due to travel restrictions, are some of the positive impacts that the virus had on the environment, especially in China. However, that was accompanied by illegal activities such as poaching and deforestation, which have a negative impact on the environment.

Another issue naturally arising from the new pandemic, is the effect that previous pollution has on the spread of the virus. It is common knowledge that long term exposure to high air pollution levels leads to many health problems especially related to lung diseases. In that case, people might be more vulnerable to the new virus leading to easier and faster spread of COVID-19. In addition, under those circumstances, the virus might have more severe outcomes to the health of the infected people, leading some of them to even death.

The present study investigates the effects that the current COVID-19 pandemic had on the environment and specifically on the air quality in Europe, using data from before and after the beginning of the virus for NO_2, $PM_{2.5}$

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and PM_{10}. Our purpose with this study is to examine whether the measures taken to reduce the spread of the virus in Europe were enough to have a positive impact on the environment as they had in China. In addition, we aim to offer some additional evidence to an already fast developing literature that mostly relies on simple descriptive tools by adding a more formal empirical study on the topic. Finally, we aim to investigate how previous pollution levels affect the spread of the COVID-19 virus and offer robustness to the results of another fast developing part of the literature studying the effects of previous pollution levels on COVID-19.

The structure of that paper is as follows: in Section 2 we present the already existing literature on that topic, in Section 3 we present the data we are using in our analysis as well as the econometric method we base our results on, and in Section 4 we show our results. Finally, in Section 5 we include our concluding remarks followed up by the Appendix were we present some more plots for specific countries and the results of one of our econometric method.

2 Literature Review

How the spread of the COVID-19 affects the environment is an important research topic, since environmental degradation and pollution is something that affects the quality of everyone's life negatively, from young healthy kids to old sick people. In the past years there were many measures taken to improve environmental quality as recycling, composting, better public transportation schedules and many more. However, during the pandemic, many recycling firms closed down to avoid the spread of the virus which had a negative impact on the environment. On the other hand, travel restrictions and limited economic activity had a positive effect on the environment, as there were fewer toxic emissions. Our purpose is to examine the overall effect of COVID-19 on air quality. However, since this topic is fairly new, the existing literature consists mainly of papers presenting descriptive results.

2.1 How COVID-19 affect air quality

Most of the papers studying this topic agree that the COVID-19 had a positive impact on the air quality. Wang and Su (2020) focused on the COVID-19 outbreak in China and its results on air pollution. They have evidence that the air quality was significantly improved for China and for the rest of the world after the beginning of COVID-19, something that might only have a short-term nature though. Specifically, COVID-19 led to lower economic activity, significant fall in urban transportation as well as less economic growth, all of which helped on the reduction of coal and crude oil use, reduction of CO_2 emissions by more than 25% compared to previous years, and reduction of Nitrogen dioxide (NO_2), which are all very harmful to the human health. However, this environmental improvement might not hold in the long run and after the lock-

Covid Economics 80, 9 June 2021: 183-210

Covid Economics 80, 9 June 2021: 183-210

down is over, as the industrial production will go back to normal or even higher in order to boost the economy of the country. Another paper that focuses on the China case, is the one of Chen et al. (2020) where they support that the COVID-19 outbreak not only had a positive impact on the environmental quality and specifically on China's air quality, but also had a positive effect on mortality. Particularly, the traffic restrictions, the self-quarantine measures as well as the quarantine, reduced the concentration of traffic related air pollutants as Nitrogen dioxide (NO_2) and $PM_{2.5}$ in 367 Chinese cities. Those results are supported by data and satellite images from the Tropospheric Monitoring Instrument. Additionally, that reduction on air pollution contributed to a reduction of non-COVID-19 deaths by 8911 because of NO_2 and 3214 because of $PM_{2.5}$. Hence, the quarantine program had a positive effect on the air quality as well as the mortality level. Berman and Ebisu (2020) using data for NO_2 and $PM_{2.5}$ as well as satellite data taken from NASA for the United States, compared the air pollution levels before and during the COVID-19 pandemic. Their results agree with previous studies, showing that the air pollution was reduced during the COVID-19 pandemic and specifically, NO_2 declined by 25.5% while $PM_{2.5}$ declined slightly but significantly due to lockdowns and reduced economic activity. For the case of Korea, Ju et al. (2021) found that the social distancing and all the other measures imposed by the government to avoid the spread of the COVID-19 virus, led to a reduction on industrial activities as well as in traffic, both of which resulted in a better air quality level. Specifically, the levels of CO_2, NO_2, $PM_{2.5}$ and PM_{10} were all reduced for Korea but also for the entire world by 17.33%, 20.41%, 45.45% and 35.56% respectively, by the March of 2020.

Gautam (2020) found that the lockdown activities taken to reduce the effects of the virus, caused transportation and production limitations, which led to less energy consumption and less environmental pollution. Specifically, they used data for NO_2, an indicator of air pollution, for some European and South Asian countries, and they found that the lockdowns had a greater impact on the Asian countries compared to the European ones, as the NO_2 was reduced by 70% in India and around 30% in China, while only 20-30% in Spain, Italy and France. Chakraborty and Maity (2020) is another study that offers support for the reduction of air pollution due to COVID-19. Since many countries in Asia, America, Australia and Europe took measures to avoid the spread of the virus, like lockdowns, transportation, travel, and production restrictions as well as ban of gatherings and events, green house emissions and industrial waste have significantly been reduced, leading to a cleaner environment. Muhammad et al. (2020) show that the COVID-19 pandemic has a significant positive effect on environmental quality, as the lockdowns that took place to restrict the spread of the virus led to a reduction on mobility, production, and travel, which contributed to a high reduction of NO_2 across the entire world. More specifically, NO_2 emissions is reduced by up to 30% in Wuhan and USA and by 20 to 30% in China, Spain, Italy, and France, improving by a great degree the air quality of those countries.

Finally, there are papers supporting that the spread of the virus had both positive but also negative effects on the environment and the air quality. Zambrano-Monserrate et al. (2020) agree with the previous statement specifically for countries like China, Italy, Spain, and USA which were affected by the virus to a substantial degree. On one side, the lockdowns at those countries and the restrictions imposed to control the spread of the virus caused a reduction on NO_2 and $PM_{2.5}$, lower environmental noise level, and cleaner beaches. On the other side, both organic and inorganic waste were increased mainly due to online shopping and online food purchases, and also countries like USA stopped some of their recycling programs to avoid further spread of COVID-19, both of which have a negative impact on the environment. On the same note, Cheval et al. (2020) show that the COVID-19 pandemic has mainly positive effects on the environment but there are also some negative ones. They found that after the beginning of the virus, NO_2, $PM_{2.5}$ and PM_{10} were reduced leading to cleaner air, in addition to cleaner water for some countries like Italy due to less boat traffic and less tourism. From a comparison between countries of Asia, Europe, and America, they found that the effects of the mobility limitations are more visible at Western and Southern Europe as well as South America. However, the virus led to a negative effect on the environment as well, that of shoreline pollution due to the disposal of sanitary consumables.

Briz-Redón et al. (2021) focused on the lockdown period between March 15th and April 12th, of 2020 in Spain, showing that the lockdown due to the rapid COVID-19 spread, helped to the reduction of air pollution in 11 big cities. Particularly, CO SO_2, PM_{10} and NO_2 air pollutants faced a decline during that period, with NO_2 having the highest reduction. On the other hand, this lockdown led to an increase on the O_3 level for most of the cities which cannot however offset the positive impact of the reduction on air pollution from the other pollutants. The reduction of air pollution and specifically the reduction of NO_2, $PM_{2.5}$, CN, and PM_{10} by a high degree during the COVID-19 pandemic, is also supported by SanJuan-Reyes et al. (2020). The decline in industrial activity and the restrictions on transportation to avoid the spread of the virus, led to a reduction of greenhouse gas emissions but also to clearer and cleaner water of seas, rivers, and coasts due to the decline of tourism. However, the O_3 was increased something that is also confirmed by Venter et al. (2020) who found that the O_3 increased by 77%. On the other side, they also agreed with the previously mentioned literature that the air quality was improved during COVID-19 in 34 countries of Asia, Europe, and the United States, as the NO_2 and $PM_{2.5}$ levels declined by 26% and 50% respectively.

Finally, Wang et al. (2020) showed that even though the anthropogenic activities taken during the pandemic to reduce the spread of the virus resulted in a high degree reduction of $PM_{2.5}$ in China, this effect is smaller than expected due to the weather conditions especially in North China. Favorable meteorology helped even more to the reduction of $PM_{2.5}$, but unfavorable meteorology had a counter effect on $PM_{2.5}$, leading to an increase to its levels, sometimes even higher than the reduction of it due to lockdowns and reduced economic activity, as in Xi'an. Those results are also confirmed by Le et al. (2020) who showed that the unfavorable meteorological conditions, heterogeneous chem-

Covid Economics 80, 9 June 2021: 183-210

istry, and secondary aerosol formation, led to an increase of $PM_{2.5}$ in eastern China. However, they agree with the rest of the literature that the social distancing, the lower manufacturing, and the ban on travelling due to COVID-19 contributed to a reduction of NO_2 and SO_2.

2.2 How air quality affects COVID-19

There are also some studies investigating the opposite causal effect: if, and to what extent and how air quality affects the spread and mortality of COVID-19. Wu et al. (2020) showed that long term exposure to air pollution like $PM_{2.5}$ can lead to health problems associated with the lungs, which can then cause more severe COVID-19 outcomes or even death for those affected by the virus. Xu et al. (2020) focused on 3,739 global locations and 1,072 global cities and studied whether and if the weather conditions and the air pollution in those cities can affect the spread of the COVID-19 virus. They found that there is a weak positive correlation between SO_2, O_3, wind speed, air pressure and the spread of the virus. On the other hand, the relationship between the COVID-19 transmission and temperature is negative while it is U-shaped with outdoor ultraviolet exposure. The way that rainfall affects the COVID-19 outbreaks was studied by Kapoor et al. (2020), who showed that earlier social distancing due to the weather condition leads to a reduction on both new cases and deaths, meaning that other random effects that might take place early in a COVID-19 outbreak, can have significant effects on it's course and it's duration. Paraskevis et al. (2020) offered a review paper on that topic mentioning that the relationship between COVID-19 and air quality is complicated and cannot be explored in detail for now due to the lack of plethora data. However, they mention that COVID-19 and temperature as well as humidity are connected by a negative relationship.

Wilson (2020) studied the effects that weather has on COVID-19 cases and deaths for US countries. The correlation between the virus' cases and deaths, and the temperature is negative meaning that during winter, the effects of the virus on people's health are more dramatic than during the summer. In addition, Wu et al. (2020) found a strong connection between air pollution and COVID-19 deaths in US, as a small increase of 1 $\mu g/m3$ in $PM_{2.5}$ relates to a high increase in the virus-related deaths by 8%. Another study connecting the spread of COVID-19 with the air quality is the one of Travaglio et al. (2021), where they investigate the case of England. Their results show that NO_2 and $PM_{2.5}$ are very important pollutants affecting the spread of the virus, as NO_2 is positively related to COVID-19 mortality and an increase of $PM_{2.5}$ leads to an increase of COVID-19 cases. Moving on to the case of Northern Italy, one of the most polluted area in Europe, Conticini et al. (2020) found that air pollution can be a great co-factor of higher level of deaths due to COVID-19. According to them, higher air pollution leads to chronic health conditions even to healthy and young people, allowing the new virus to infect them easier and even lead to a faster death. Gupta et al. (2020) focused on nine different Asian countries and

Covid Economics 80, 9 June 2021: 183-210

showed that past exposure to high level of air pollution over a long period and specifically on $PM_{2.5}$, affects the mortality of COVID-19 positively. In other words, air pollution and COVID-19 mortality are positively correlated. On the other hand, this correlation does not hold for the case of PM_{10}. For the case of China, Martelletti and Martelletti (2020) found that higher air pollution level affects positively the fatality of COVID-19, with the relationship between those two variables to be linear. In addition, they support that this is the case for Northern Italy too, since the particulate matter and specifically the PM_{10} can be a carrier of the virus.

3 Model

3.1 Data

To measure the air pollution level, we are using data on NO_2, $PM_2.5$, and PM_10 which are the most common local air pollutants. NO_2 is mainly produced from vehicles, industrial emissions, construction and any other operation that burns fossil fuels. PM_{10} are tiny inhalable particles in the air that are 10 micrometers or smaller in width, while $PM_{2.5}$ are less than or equal to 2.5 micrometers. The main sources of their existence are burning wood, oil, or coal (forest fires, fireplaces, fuel-burning heaters), cooking, driving vehicles (car, snowmobile, bus), smoking and volcanic eruptions. Their tiny size make them extremely dangerous as they can easily be inhaled, affecting the lungs and even the bloodstream, leading to serious health problems. Those data were taken from the European Environmental Agency (EEA) and they are all measured in micrograms per cubic meter of air ($\mu g/m3$). The database includes data for most of the European countries and they collect their data using around 3,000 monitoring stations of all types in many cities across Europe, for the years between 2018 and 2021. The data are measured on an hourly basis which is then used to calculate a daily average of each station. Those daily averages are finally used to calculate a weekly and monthly average too. For our analysis we will use the monthly data coming from all available cities, but we will gather them for each country separately since we want to focus on the effect that COVID-19 had on the entire country, not in specific cities. Finally, we are testing each pollutant separately.

The second variable we are using in our analysis is about COVID-19 data and specifically data on new cases for all European countries from the beginning of COVID-19 till March of 2021. Those data are daily data from Our World in Data and they are measured per million of people. In their dataset they also include data for population, vaccinations, tests and many more categories which we will not use for our specific analysis since the main question we are trying to answer is how the new cases of COVID-19 affect air pollution for a number of European countries. To avoid noise coming from daily data, we are creating the corresponding monthly data for each country separately, to be in line with

Covid Economics 80, 9 June 2021: 183-210

Covid Economics 80, 9 June 2021: 183-210

the air pollution data.

We will first focus on an analysis for each country separately and then we will move on to a panel analysis for the European countries we are using. The final panel dataset we are creating consists of 31 countries[1], for all months of 2020 and the three first months of 2021, for the three pollutants and for the COVID-19 new monthly cases. We are focusing on data of 2020 and 2021 because we want to see the results of the pandemic on air quality. However, we will use data for the before and after COVID-19 country specific analysis where we check how air pollution changed after the pandemic. The following table (Table 1) shows the descriptive statistics of our panel dataset which includes the three pollutants (NO_2, PM_{10}, $PM_{2.5}$), and the COVID-19 new monthly cases for each country for the period of December 2020 to March 2021, and includes the mean, variable, median, and range[2].

Table 1: Descriptive Statistics of the Panel Dataset

	Mean	Variance	Median	Range
$\boldsymbol{NO_2}$	57.582	6676.772	21.741	1.504 - 351.539
$\boldsymbol{PM_{2.5}}$	23.263	1239.832	8.681	1.335 - 178.308
$\boldsymbol{PM_{10}}$	46.835	4535.375	18.272	2.054 - 314.098
Monthly new Cases	27.360	200.649	28.610	0.000 - 61.280

3.2 Empirical Method

The main purpose of this study is to investigate the effect of COVID-19 on air pollution in Europe, to compare the time trend of the air pollution level measured by NO_2, $PM_{2.5}$ and PM_{10} before and after the pandemic, as well as to check if previous pollution levels can affect the growth of the COVID-19 virus. For that reason, we are using different datasets for the three different goals, one panel dataset including 31 European countries for the years 2020 and 2021 for the first one, 31 different smaller time series datasets for the years 2018 to 2021 for the second one, and one final panel dataset including 31 European countries, having data for 2020 till 2021 for the COVID-19 cases and data for 2018 and 2019 for the air pollution. In addition, we are using different methodologies to approach each research question targetly which are described in the following paragraphs. All our data are expressed in logarithmic form to interpret the marginal responses in percentages. We will first discuss the simpler case of each

[1]The European countries we are using in our analysis are the following: Austria, Belgium, Bulgaria, Croatia, Cyprus, Czechia, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Iceland, Ireland, Italy, Latvia, Lithuania, Luxembourg, Malta, Netherlands, Norway, Poland, Portugal, Romania, Slovakia, Slovenia, Spain, Sweden, Switzerland and United Kingdom.

[2]Please note that the descriptive statistics are calculated for the logarithmic form of the data, which we use in our regressions to interpret the marginal responses in percentages.

Covid Economics 80, 9 June 2021: 183-210

country separately and then we will move on to the analysis of the panel dataset.

First, we aim to compare the pollution level before and after the COVID-19 pandemic. For the months before the COVID-19 pandemic, we include data for 2018 and 2019, while for after COVID-19, we include data for 2020 and 2021[3]. For that reason we create a dummy variable which represents months pre COVID-19 when it takes the value 0, and for the months after COVID-19 when it is equal to 1. A similar analysis but for each year separately is also available in the Appendix-Section A, considering each year as a different entry. The regression model used for that part is given below in a compact form :

$$log(pollutant_k)^i_t = \beta_0 + \beta_1 * year\ case^i_t + u^i_t \tag{1}$$

where pollutant is the dependent variable and represents one of the three air pollutants we have given by k, which can take one of the following values: $\{NO_2,\ PM_{2.5},\ PM_{10}\ \}$, subscript t refers to the months of each category, and superscript i indicates the Country we are using every time in the regression. β_0 is the constant, β_1 is the slope of the independent dummy variable, and u is the error term. The results for that part of our study can be seen in the next section (Section 4) and specifically in Figure 1 to 3.

Moving on to the second part of the individual country analysis we want to explore how the sudden COVID-19 pandemic affected air quality in Europe. We use a simple linear model having as dependent variable the air pollutant and as independent variable the new COVID-19 cases, both measured on a monthly basis[4]. For each country we have three different regressions for the three different air pollutants we are using, that is one for NO_2, one for $PM_{2.5}$ and a last one for PM_{10}, described below as:

$$log(pollutant_k)^i_t = \beta_0 + \beta_1 * log(cases)^i_t + \beta_2 * t^i_t + u^i_t \tag{2}$$

where pollutant refers to one of the three different air pollutants we are using with k taking being one of the following: $\{NO_2,\ PM_{2.5},\ PM_{10}\}$, cases refers to the new monthly COVID-19 cases in each country, t captures the time trend, i refers to the country, and subscript t refers to the specific year and month[5]. β_0 is the constant, u is the error term, while β_1 and β_2 are the slopes of the independent variables.

Next, we will present the analysis for the panel dataset and the effect of COVID-19 cases on Europe, using again three different regressions for the three different air pollutants we have. In that case, we care only for the after COVID-19 period, thus we do not include an additional dummy for the different periods

[3]Please note that for our analysis we are using the more recent available data for air pollution level in Europe, which are till the March of 2021. The data for the COVID-19 cases are updated daily.

[4]We are using monthly data and we focus on the period after the COVID-19 virus started, so each country's dataset includes at most 15 observations for each pollutant. Our coefficients for both the monthly cases and the time trend are most of the times not statistical significant due to the small sample size of each country's data. Table 8 of the Appendix-Section B gives the complete sets of results.

[5]Subscript t can take the values: January of 2020 till March of 2021 while the independent variable t can take the values 1 to 15 for the fifteen months that we have data for.

as we did previously. However, we are using time invariant individual country fixed effects to control for some unobserved factors that differ among countries and capture country specific factors. Thus, our regressions have the air pollutants as dependent variable, the COVID-19 cases, the time trend, and the fixed effects as independent variables. The regressions for this analysis are described as:

$$log(pollutant_k)_{it} = \beta_0 + \alpha_i + \beta_1 * log(cases)_{it} + \beta_2 * t_{it} + u_{it} \qquad (3)$$

where pollutant refers to one of the three different air pollutants we are using, α stands for the country fixed effects , cases refers to the COVID-19 new monthly cases, u is the error term, variable t captures the time trend of the data, while i refers to the country and subscript t to the year and month which takes the values between January of 2020 to March of 2021 as previously. Once again, all of our data are expressed in logarithmic form to interpret the marginal responses in percentages and all of our data are measured on a monthly basis as before.

Another important aspect that should be clarified, is the effect that previous pollution has on the COVID-19 cases. There are some papers supporting that long term exposure to air pollution can increase the chance of having health problems leading to higher COVID-19 cases and more severe COVID-19 outcomes, as Wu et al. (2020) and Wu et al. (2020), among others in the literature. Hence, it is of crucial importance to research how previous air pollution levels affect the new cases of the COVID-19 virus. For that reason, we include one more regression to our study investigating the effect that previous air pollution up to two years has on the COVID-19 cases. However, we are excluding the first six months after the pandemic started, as the virus was not spread in a high degree till then and the cases during those months were few. In other words, for that part of our study, we include the months of July of 2020 till March of 2021 for the COVID-19 cases and the months July of 2018 to March 2019 for the air pollution data. Once again, we are using country fixed effects to avoid bias on our results as well as a time trend variable. Our data are measured on a monthly basis as in the previous cases and we will have three different regressions for each air pollutant as before, described by the following general regression:

$$log(cases)_{it} = \beta_0 + \alpha_i + \beta_1 * log(pollutant_k)_{i(t-24)} + \beta_2 * t_{it} + u_{it} \qquad (4)$$

were cases are the new monthly COVID-19 cases, α refers to the country fixed effects, pollutant refers to one of the three different air pollutants we are using separating them by the letter k, t stands for the time trend while u is the error term. As we can see here, the right hand side of our regression and specifically the air pollution data have a lag of 24 months. It is important to let a significant amount of time to pass before we start seeing the results of air pollution on humans' health, as it is not something that affects people's health immediately. This is the reason why we have the lag of the two years period, so as to be sure that the effects of air pollution will be visible.

The results of each part of our analysis is described in detail in the following

Covid Economics 80, 9 June 2021: 183-210

section where we present our results, starting with the individual country analysis and then moving on to the two panel analyses. Particularly, we first present some plots for the air pollution in the case where we compare the period before and after the COVID-19 pandemic for each country separately, then we show some representative results of our individual regressions, finally we present the results of the two panel regressions with some plots and tables.

4 Results

Following the discussion in Section 3.2, we are able to find a negative correlation between the spread of COVID-19 and air pollution for 31 European Countries. We are now presenting the results from both the individual country analysis for the years 2018 to 2021, as well as the panel analysis which includes all the countries for the years 2020 and 2021, that is after the beginning of the pandemic. We will first present the individual country results and then we will move on to the panel analysis, both of which show that there is a negative relationship between air pollution and the new COVID-19 cases. In addition, we were able to find a positive correlation between previous air pollution levels and the COVID-19 cases, coming in agreement with the existing literature on that topic. The results of this analysis are presented in the end of this section.

Starting with the individual country analysis, we see that as time passes, air pollution and specifically the NO_2, $PM_{2.5}$, and PM_{10} emissions tend to fall. However, even though air pollution is falling in the past few months without the intervention of a pandemic, we are able to detect a huge reduction on those air pollutants after the beginning of COVID-19, confirming our conjecture that all the measures taken to reduce the spread of the virus have had a positive impact on air quality. In the following plots, we see that for all countries the emissions of the three pollutants we are using in our analysis fall after the COVID-19 pandemic. This is because, each country in Europe took measures to mitigate the rapid spread of the virus, as lockdowns, social distancing, ban in social gathering and unnecessary travels, and restrictions on manufacturing and production. All those measures led to a high reduction of economic activity, tourism, and traffic which are the main reasons of air pollution based on NO_2, PM_{10}, and $PM_{2.5}$. We will present some representative plots for each air pollutant for different countries, but our results can be generalized for all countries since we are not including all the cases to reserve space only[6]. At the Appendix-Section A where we present the individual country analysis, we include plots of different countries than here, which shows that our results hold for all the countries studied.

[6]The plots presented in this section only refer to a few countries in order to conserve space, but are indicative of the overall results. We are studying 31 European countries and 3 different pollutants for each one of them, leading us having 93 plots in total. The results are almost the same for each country, so we decided to include only 3 plots of each pollutant for different countries, to show that our results hold not only for one country but in general.

Covid Economics 80, 9 June 2021: 183-210

Covid Economics 80, 9 June 2021: 183-210

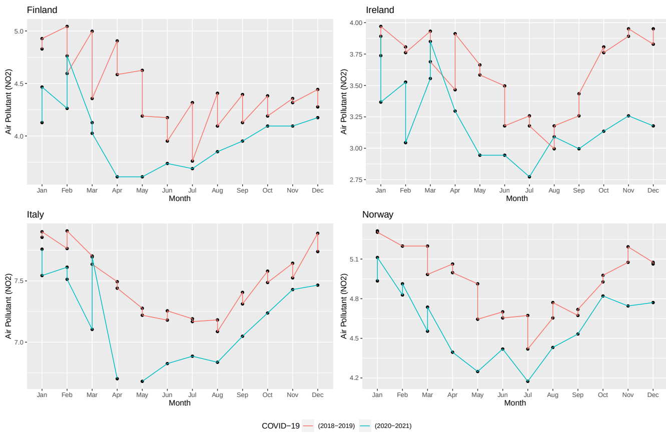

Figure 1: NO_2 Pollution Before and After the COVID-19 Pandemic

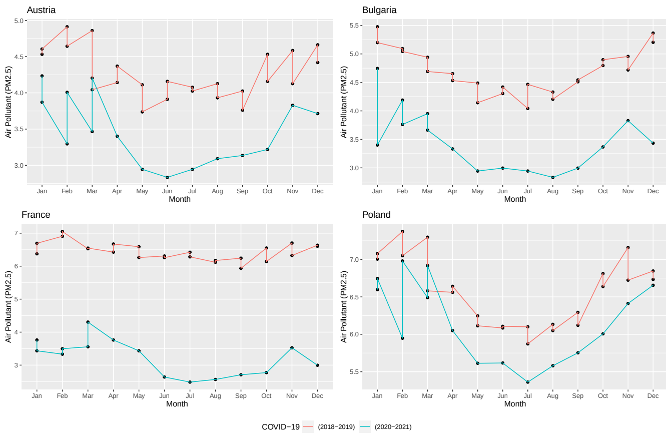

Figure 2: $PM_{2.5}$ Pollution Before and After the COVID-19 Pandemic

Covid Economics 80, 9 June 2021: 183-210

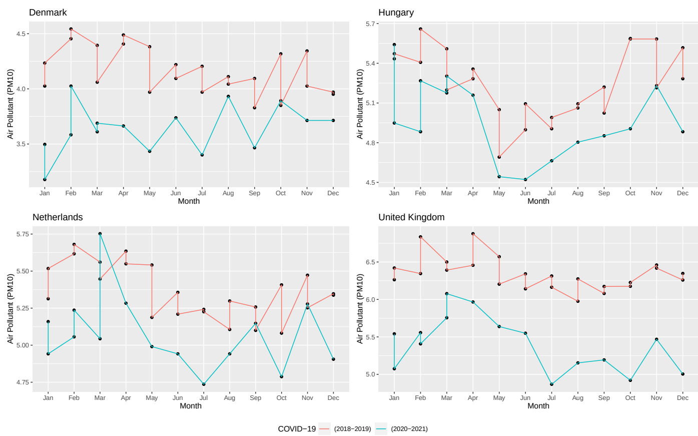

Figure 3: PM_{10} Pollution Before and After the COVID-19 Pandemic

The results from the individual country separate regressions can be found in Table 8 of the Appendix-Section B where we are showing the regression coefficients for all air pollutants for 31 European countries after the beginning of the pandemic. We can clearly see that the pandemic of COVID-19 had a positive impact on the environment since the increase of the virus cases led to a reduction of each air pollutant. The reason why that happens can be simply found on the measures taken to prevent the spread of COVID-19. All the lockdowns and the strict measures of social distancing allowed people to have less activities (gathering, tourism, driving, working) allowing the air quality to rejuvenate. Those results also support the course that all pollutants followed the past few years represented in the previous plots (Figure 1 to 3).

Some of the countries in Europe that were mostly negatively affected by COVID-19 were Italy, Spain, France, and the United Kingdom. Some of those took many measures by the beginning of the virus to ensure the safety of their citizens like Italy, France and Spain, while others chose a more relaxed policy without any kind of ban or many lockdowns like the United Kingdom. However, even with less measures taken, we see that the result is the same for all those countries: air pollution levels dropped during the pandemic. We see, that even with a small change on the way of living can have a huge impact on the environment but also on our health as air pollution is responsible for many deaths every year. In the above plots and tables, we showed the results of those countries but also from other European counties, all coming to the same conclusion that the pandemic of COVID-19 affected the air pollution negatively.

Moving on to the panel analysis, our results are not any different. Here, we include all 31 European countries we used for our study for the years after the COVID-19 pandemic, that is for the years 2020 to 2021. The fixed effects regression we did for that part of our analysis helped us avoid bias since we were keeping constant some unobserved factors which are the same between the countries but different between years. What we see is that, for those two years, the COVID-19 pandemic had a positive impact on air quality. While the COVID-19 new cases were rising on a monthly basis, pollution emissions emanating from NO_2, $PM_{2.5}$, and PM_{10} were falling. Our results agree with the previous analysis and we see that the measures taken to slow down the spread of the virus helped a lot on the reduction of air pollution in Europe. The following three plots (Figure 4 to 6) as well as the three next tables (Table 2 to 4), show the results of the fixed effects regressions described on Section 3.2 for each pollutant separately.

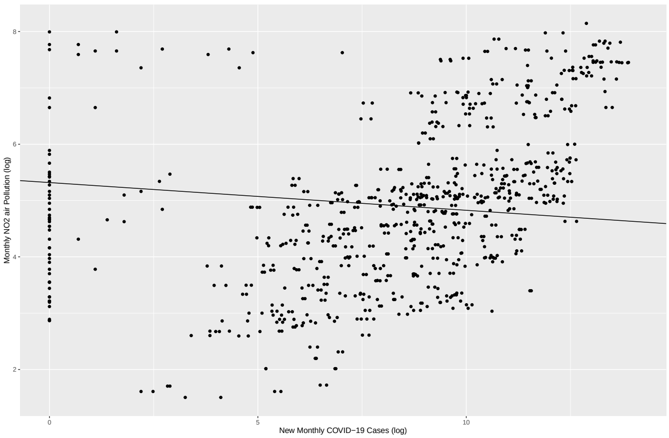

Figure 4: Correlation Between NO_2 and new Monthly COVID-19 Cases in 31 European Countries

Covid Economics 80, 9 June 2021: 183-210

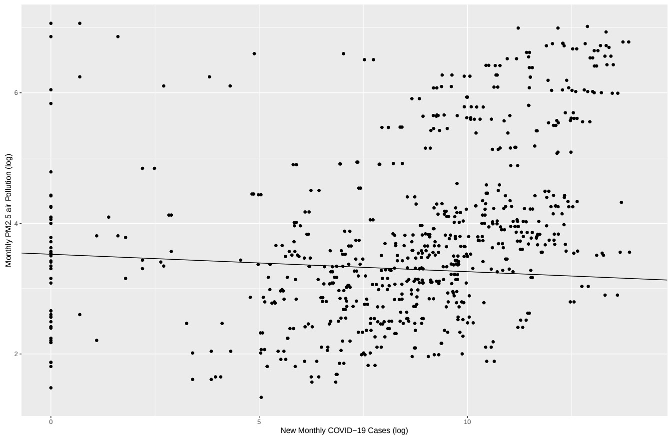

Figure 5: Correlation Between $PM_{2.5}$ and new Monthly COVID-19 Cases in 31 European Countries

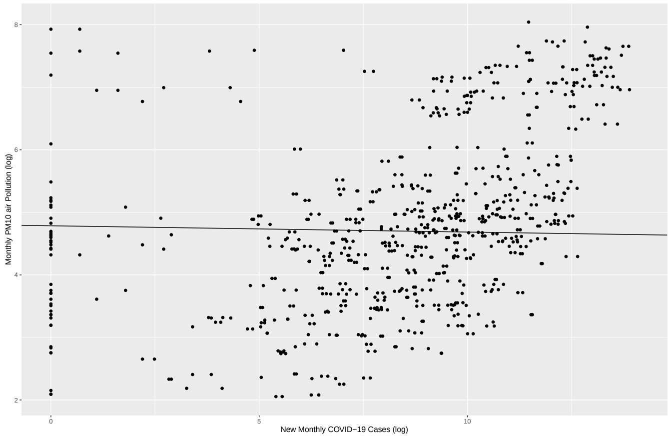

Figure 6: Correlation Between PM_{10} and new Monthly COVID-19 Cases in 31 European Countries

Finally, we present the results of the second panel analysis where we investigate how previous pollution levels affect the spread of the COVID-19. Specifi-

CEPR PRESS

Covid Economics 80, 9 June 2021: 183-210

Table 2: Estimated Results from the Panel Data Model with Fixed Effects for NO_2

Variable	Estimate	Std. Error	t value	Pr(> |t|)
Intercept	5.318928	0.054473	97.644	< 2e-16 ***
New Monthly COVID-19 cases	-0.049222	0.004668	-10.544	< 2e-16 ***
Time trend	0.055452	0.003454	16.055	< 2e-16 ***
N	783			
R^2	0.9705			

$^{***}p = 0,\ ^{**}p < 0.001,\ ^{*}p < 0.01,\ .\ p < 0.05,\ \ p < 0.1$

Table 3: Estimated Results from the Panel Data Model with Fixed Effects for $PM_{2.5}$

Variable	Estimate	Std. Error	t value	Pr(> |t|)
Intercept	3.526907	0.094109	37.477	< 2e-16 ***
New Monthly COVID-19 cases	-0.026669	0.008314	-3.208	0.00140 **
Time trend	0.048725	0.006092	7.998	5.39e-15 ****
N	720			
R^2	0.8999			

$^{***}p = 0,\ ^{**}p < 0.001,\ ^{*}p < 0.01,\ .\ p < 0.05,\ \ p < 0.1$

Table 4: Estimated Results from the Panel Data Model with Fixed Effects for PM_{10}

Variable	Estimate	Std. Error	t value	Pr(> |t|)
Intercept	4.785042	0.060060	79.672	< 2e-16 ***
New Monthly COVID-19 cases	-0.010022	0.005270	-1.902	0.057612 .
Time trend	0.024596	0.003861	6.370	3.39e-10 ***
N	745			
R^2	0.9619			

$^{***}p = 0,\ ^{**}p < 0.001,\ ^{*}p < 0.01,\ .\ p < 0.05,\ \ p < 0.1$

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cally, we are using data for 2020 and 2021 for the COVID-19 cases and data for 2018 and 2019 for the air pollution for 31 European countries. As the virus was not spread significantly in the beginning of the pandemic, we exclude the six first month after the beginning of COVID-19, that is we do not use data for the months January of 2020 till June of 2020. Since the data for air pollution we are using in that regression have a lag of two years, we use data from July 2018 till March of 2019 to match the data for the COVID-19 cases. Finally, the fixed effects regression allowed us to eliminate bias in our results which suggest that high previous pollution levels affect the spread of the virus positively. That is because air pollution leads to many health problems leaving people more vulnerable to new diseases, especially the one of COVID-19 which has a very infectious nature. The results can be seen in the following three plots (Figure 7 to 9 and three tables (Table 5 to 7 and they are based on the regression 4 described on Section 3.2.

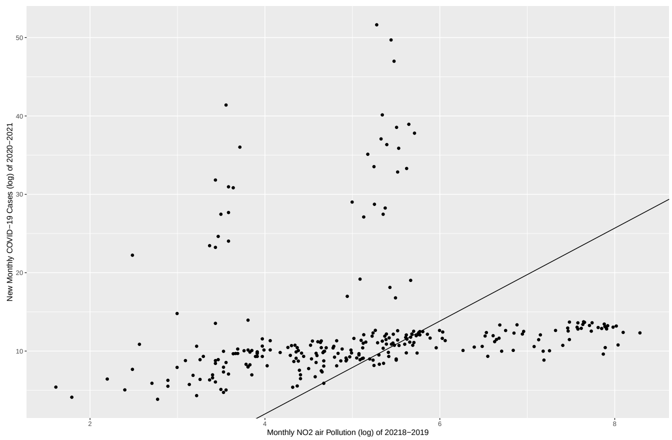

Figure 7: Correlation Between new Monthly COVID-19 Cases and Previous NO_2 Pollution in 31 European Countries

Covid Economics 80, 9 June 2021: 183-210

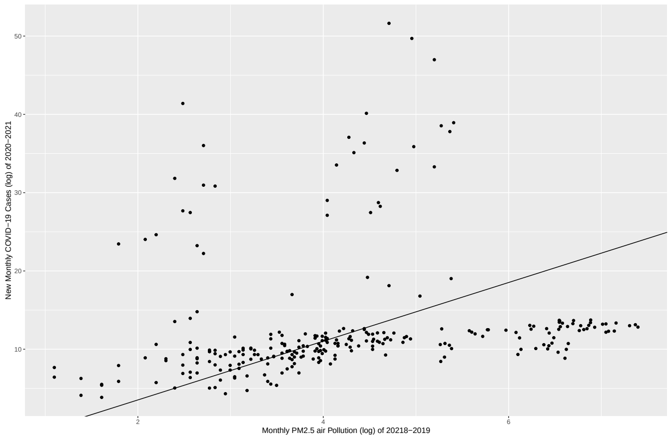

Figure 8: Correlation Between new Monthly COVID-19 Cases and Previous $PM_{2.5}$ Pollution in 31 European Countries

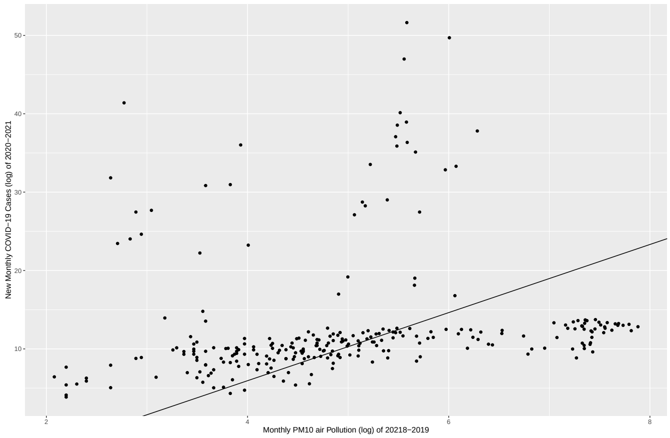

Figure 9: Correlation Between new Monthly COVID-19 Cases and Previous PM_{10} Pollution in 31 European Countries

Table 5: Estimated Results from the Lagged Panel Data Model with Fixed Effects for the lagged effect of NO_2 on COVID-19 cases

Variable	**Estimate**	**Std. Error**	**t value**	**Pr(> $\|t\|$)**
Intercept	-21.73730	7.99056	-2.720	0.00700 **
Lagged NO_2 effect	5.92792	1.48462	3.993	8.70e-05 ***
Time trend	-0.13085	0.13321	-0.982	0.32694
N	272			
R^2	0.7218			

$^{***}p = 0$, $^{**}p < 0.001$, $^{*}p < 0.01$, $.\ p < 0.05$, $\ p < 0.1$

Table 6: Estimated Results from the Lagged Panel Data Model with Fixed Effects for lagged effect of $PM_{2.5}$ on COVID-19 cases

Variable	**Estimate**	**Std. Error**	**t value**	**Pr(> $\|t\|$)**
Intercept	-3.976e+00	4.053e+00	-0.981	0.327523
Lagged $PM_{2.5}$ effect	3.750e+00	1.033e+00	3.631	0.000346 ***
Time trend	-9.849e-04	1.193e-01	-0.008	0.993422
N	272			
R^2	0.7187			

$^{***}p = 0$, $^{**}p < 0.001$, $^{*}p < 0.01$, $.\ p < 0.05$, $\ p < 0.1$

Table 7: Estimated Results from the Lagged Panel Data Model with Fixed Effects for lagged effect of PM_{10} on COVID-19 cases

Variable	**Estimate**	**Std. Error**	**t value**	**Pr(> $\|t\|$)**
Intercept	-11.44241	7.02291	-1.629	0.10457
Lagged PM_{10} effect	4.34545	1.41643	3.068	0.00241 **
Time trend	-9.849e-04	1.193e-01	-0.008	0.993422
N	272			
R^2	0.7144			

$^{***}p = 0$, $^{**}p < 0.001$, $^{*}p < 0.01$, $.\ p < 0.05$, $\ p < 0.1$

Covid Economics 80, 9 June 2021: 183-210

Covid Economics 80, 9 June 2021: 183-210

5 Conclusion

There are many studies showing that the COVID-19 pandemic had a positive impact on the air quality of many different countries and of the world in general like Wang and Su (2020), Berman and Ebisu (2020), and Chakraborty and Maity (2020), who show that the levels of PM_{10}, $PM_{2.5}$, and NO_2 dropped due to the measures taken to control the spread of COVID-19. However, there are other studies showing that the COVID-19 virus had also negative effects on the environment and the air quality of some countries as Briz-Redón et al. (2021), SanJuan-Reyes et al. (2020), and Wang et al. (2020)[7] who show that the level mainly of O_3 but also $PM_{2.5}$ is higher after the beginning of the pandemic.

Additionally, there are papers supporting that previous exposure to high levels of air pollution affect the spread and mortality of the virus in a high degree, as the ones of Wu et al. (2020), Conticini et al. (2020) and more. Air quality is a significant factor affecting people's health and it can lead to many health care problems when the pollution increases. In addition, it leaves people more vulnerable to new threats and viruses, especially those with high contaminating nature as is the COVID-19. Hence, higher exposure to previous air pollution can also affect the plot and the results of the COVID-19 pandemic and so it is important to investigate the relationship between those two factors.

Our study investigates the effects that COVID-19 has on the environment and specifically on the air quality of 31 European countries. We offer a new study on this topic with up to date data of three different air pollutants, NO_2, $PM_{2.5}$, and PM_{10} for the years 2018 till 2021. The individual country analysis shows a comparison between the air pollution level before the beginning of the COVID-19 pandemic (years 2018 and 20219) and the air pollution level after the pandemic (years 2020 and 2021). The results show that the air pollution level of each air pollutant dropped significantly for most countries after COVID-19. The results are presented on Table 8 of Section B of the Appendix and show that the relationship between the COVID-19 cases and the air pollution is negative. Using the entire data available as a panel with fixed effects, we see that the coefficients are negative for each air pollutant, indicating that the COVID-19 pandemic has reduced emissions in Europe.

The overall picture that emerges is that of a reduction of air pollution after the beginning of the COVID-19 pandemic. That is because all the countries took measures to avoid the spread of the virus and to protect their residents. Some countries decided to have stricter measures than others but the result was the same in each country: the air pollution level dropped. The measures taken include travel restrictions, production restriction, social distancing, curfews, quarantines, and many more. Those measures affected many aspects in a negative way like the economy, the insurance industry, the arts, the politics, and the mental health of the people. However, we see that they affected positively the environment as there was less pollution coming from production, traffic, and tourism. Even though we cannot be sure on whether this positive change on air

[7]More papers for both categories are mentioned in Section 2.1.

pollution will be permanent or not, we can say for sure that the pandemic gave a healthy boost to the environment. This pandemic is a lesson that even with small changes and more responsible actions (like less traffic), we can all help to reduce the air pollution and help the environment refresh.

Moreover, we study the effects that previous air pollution levels have on the COVID-19 pandemic. Under a panel analysis with two years lag on the data of air pollution, we see that higher previous pollution affect the cases of the virus positively. Air pollution can affect in a great degree the health and well being of the people, as it can lead to many health problems especially related to lung problems. In that case, it allows COVID-19 to infect those people as they are more vulnerable, leading to a more rapid spread of the virus which is also accompanied by more disastrous results of the pandemic, as people are unable to defeat the virus something that might also lead to a faster death.

Our paper presents some valuable insights, that the COVID-19 pandemic did not only had negative effects on the world but also positive ones, affecting air quality and furthermore the environment in a positive way. We also offer robustness to the literature supporting that COVID-19 had a positive impact on the environment and to the one suggesting that previous exposure to air pollution leads to higher COVID-19 cases. Our study is limited to 31 European countries and three air pollutants, but it can be extended to more countries, more air pollutants and even different kind of pollutants like water pollutants (pathogens, biochemical oxygen demand, and more) or noise pollutants.

Covid Economics 80, 9 June 2021: 183-210

CEPR PRESS

Covid Economics 80, 9 June 2021: 183-210

Appendices

A Individual country analysis

In that section we will discuss the methodology and the results of the individual country analysis. We want to see the time trend of each air pollutant for all 31 European countries separately, for the years 2018 to 2021. The main difference compared to the analysis of the Section 3.2 of the main part of this paper, is that now we do not separate the the years into two categories according to the beginning of COVID-19, but we allow each year to be a different entry, to see in more details the trend that each pollutant is following the past few years. For that reason, in that case we do not include a dummy variable to separate the before and after years of the pandemic, so the regressions for this part of our analysis are the following:

$$log(pollutant_k)_{it} = \beta_0 + \beta_1 * log(cases)_{it} + u_{it} \tag{5}$$

where k refers to the pollutant refers to the three different air pollutants we have, (NO_2, $PM_{2.5}$ and PM_{10}), cases are the new monthly COVID-19 cases in each country, i refers to the country, t refers to the year and it can take of of the following values: 2018, 2019, 2020, 2021. Finally, u is the error term, β_0 is the constant and β_1 is the slope. Our data are calculated in a monthly basis and are expressed in a logarithmic form as in the main part of this study.

Our results are described by the following plots for each country and each pollutant separately and they are all in agreement that while time passes, air pollution becomes less and less, having a big reduction to the years after COVID-19 pandemic started[8]. With that being said, we can clearly see that the COVID-19 pandemic had a positive impact on the environment and specifically on the air quality in Europe.

[8] In that section, we are presenting specific countries and pollutants to preserve space. The plots for all countries and for all pollutants are available after request from the authors. However, there are not big differences in the results, as all support that the COVID-19 had a positive effect on the air quality of those European countries.

Covid Economics 80, 9 June 2021: 183-210

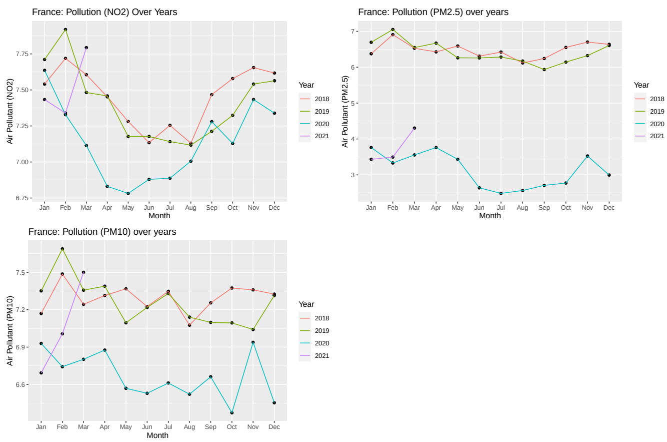

Figure 10: Air Pollution Over Time in France

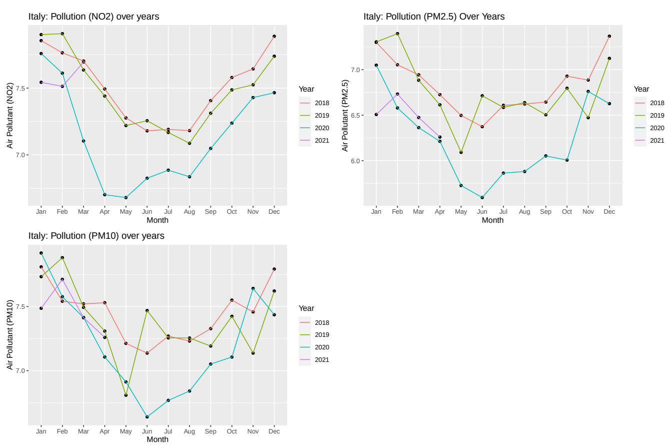

Figure 11: Air Pollution Over Time in Italy

Covid Economics 80, 9 June 2021: 183-210

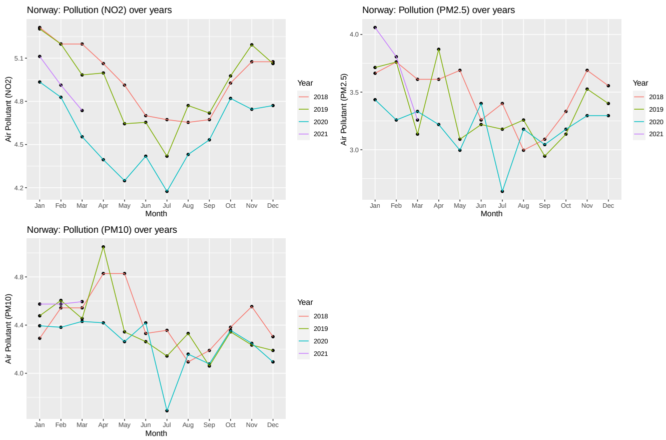

Figure 12: Air Pollution Over Time in Norway

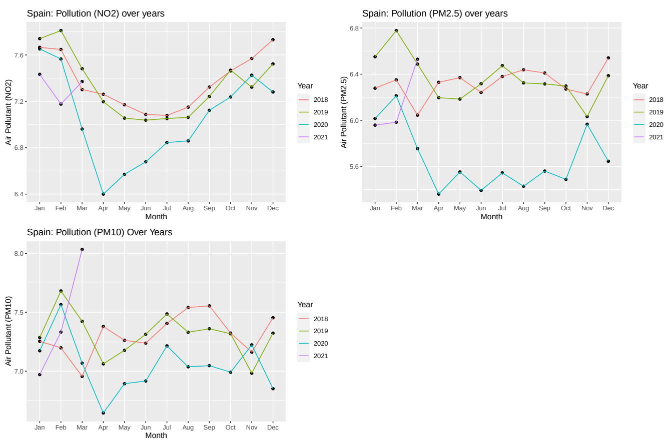

Figure 13: Air Pollution Over Time in Spain

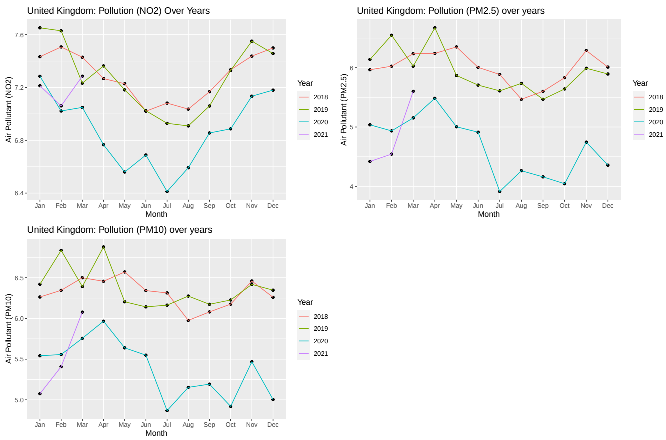

Figure 14: Air Pollution Over Time in the United Kingdom

B Results of individual country regressions

We will also present in detail all the coefficients for each air pollutant coming from the individual country analysis showed by equation 2. As we can see, most of the coefficients are negative, indicating that when COVID-19 new cases increase, the air pollution level falls. The few cases we have showing that the regression coefficients are positive, meaning that the spread of COVID-19 led to an increase on air pollution, cannot outweigh the negative relationship between air pollution and COVID-19 cases which are supported by most countries and most air pollutants. Even though the individual coefficient estimates of the countries are not statistical significant due to the dataset restrictions we face, we present them for completion purposes. The results are presented in Table 8.

Covid Economics 80, 9 June 2021: 183-210

Covid Economics 80, 9 June 2021: 183-210

Table 8: Regression Coefficients for the new COVID-19 Cases for the Three Pollutants (NO_2, $PM_{2.5}$,PM_{10}) for individual countries

Country	NO_2	$PM_{2.5}$	PM_{10}
Austria	-0.108730	-0.094630	-0.065880
Belgium	-0.043570	-0.014360	0.004592
Bulgaria	-0.001719	-0.007692	-0.001312
Croatia	-0.121770	-0.197130	-0.033070
Cyprus	0.013872	NA	NA
Czechia	-0.048260	-0.040360	-0.024550
Denmark	-0.023510	-0.000523	0.001858
Estonia	-0.091560	-0.018160	-0.009193
Finland	-0.066850	-0.002360	0.077720
France	-0.082240	-0.050050	-0.035600
Germany	-0.041610	-0.030250	-0.023360
Greece	-0.044610	-0.112970	-0.083910
Hungary	-0.005911	0.006547	-0.001784
Iceland	-0.065710	-0.049653	-0.009434
Ireland	-0.050470	-0.016280	-0.015640
Italy	-0.082440	-0.062770	-0.048870
Latvia	-0.047691	NA	NA
Lithuania	-0.049260	0.036695	0.032420
Luxembourg	-0.013155	-0.010200	-0.007044
Malta	NA	-0.001655	-0.020060
Netherlands	-0.040440	-0.035160	-0.022610
Norway	-0.060240	-0.026160	0.001648
Poland	-0.055670	0.033240	-0.002793
Portugal	-0.033000	-0.087930	-0.077500
Romania	-0.091800	NA	NA
Slovakia	-0.005706	-0.007044	-0.008978
Slovenia	-0.046380	NA	0.014710
Spain	-0.098820	-0.086160	-0.073620
Sweden	-0.052220	-0.014630	-0.006066
Switzerland	-0.065730	-0.045960	-0.032790
United Kingdom	-0.052080	-0.012030	-0.003573

Covid Economics 80, 9 June 2021: 183-210

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Covid Economics 80, 9 June 2021: 183-210

COVID-19, home advantage in professional soccer, and betting market efficiency[1]

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Date submitted: 28 May 2021; Date accepted: 29 May 2021

Fan support has often been considered to be a main driver of the home advantage in sports. Using the natural experiment of ghost games during the COVID-19 pandemic in German professional soccer, we test this claim. Indeed, we identify a reduction in the home performance - though with much heterogeneity across leagues. We moreover observe the home advantage to recover over time. In analysing whether betting markets anticipated this drop in the home advantage and its recovery, we test the efficient market hypothesis. We find that betting odds do not properly reflect the effect of ghost games regarding changes in the home advantage. Furthermore, we present evidence for a slow to non-existent adaptation process with respect to new match results, indicating a lack of semi-strong efficiency. Based on these findings, we show how simple betting strategies could have exploited the discussed phenomenon.

Covid Economics 80, 9 June 2021: 211-231

1 Kai Fischer acknowledges financial support funded by the Deutsche Forschungsgemeinschaft (DFG, German Research Foundation) – 235577387/GRK1974.
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1 Introduction

Betting markets have been analyzed rather frequently - often with the purpose to obtain a better understanding of prediction markets and the expectation formation on such. As every bet has a specific point of termination when all uncertainty is resolved (Thaler & Ziemba 1988), betting markets can be regarded as natural laboratories to study future markets. Many papers have been dealing with the efficiency of information processing and various forms of the efficient market hypothesis, which states that asset prices reflect all relevant information and that it is, by and large, impossible to outperform the market in a systematic fashion. In its weak form, the efficient market hypothesis suggests that asset prices reflect all information on past events. The semi-strong form follows the idea that all public information on past, current and future events is reflected in an asset's current price, while the strong form states that public and private information is contained in prices (Fama 1970, Malkiel 1973).

The analysis of sports betting markets and their efficiency has continuously found a number of biases such as the favorite-longshot bias or the sentiment bias (Thaler & Ziemba 1988, Sauer 1998, Vaughan Williams 1999). While such studies have often taken a static approach to market efficiency in focusing on weak-form inefficiencies that have been prevailing over some time, the present paper also contributes to the understanding of expectation adaptation in betting markets in the presence of an unforeseen exogenous shock. We study betting odds for German professional soccer matches before and after the introduction of ghost games during the COVID-19 pandemic. As ghost games have been a rare phenomenon before the pandemic and have never been conducted in succession, the COVID-19 induced ghost game series provides a unique natural experiment. In particular, after more than one year of ghost games in Germany[1], we investigate whether there is a reduction in the home advantage during ghost games as teams' better performance at home often is associated with fan support. Based on these results, we test whether betting markets anticipated these patterns and rationally updated expectations over time.

During the first ghost matches, we find a clear drop in the home advantage in the first German tier ("Bundesliga") whereas no change is observed in the second tier ("Bundesliga 2"). Over time, the home advantage recovers in the former division and especially teams which were used to high fan attendance suffer during ghost games as they need longer to recover. For a small number of COVID-19 matches, which have been played with a limited number of spectators, we find a higher home advantage than for ghost games.

Betting markets severely underestimated the loss in home advantage in Bundesliga soccer during the first ghost matches while markets overestimated it for the Bundesliga 2. Expectations on the overall loss of the home advantage have been similar for both divisions but were driven by different factors between divisions which market participants assumed to drive ghost games' match outcomes - such as an increasing relevance of the table ranking in the Bundesliga and rising importance of player ability in Bundesliga 2. In addition, we can only identify very weak, insufficient adaptation of expectations over time in the Bundesliga and none in the Bundesliga 2 although match outcomes deviated strongly from bettors' beliefs during the first ghost game matchdays. The lack of updating expectations may be seen as a violation

[1]The first ghost game took place on March 11, 2020, right before the lockdown break.

Covid Economics 80, 9 June 2021: 211-231

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of semi-strong market efficiency. Hence, unfamiliar 'first time ever' shocks can cause relevant inefficiencies which may apparently also persist for some time. Consequently, we identify a number of profitable betting strategies.

The remainder of this paper is organized as follows: In section 2, we provide an overview of related literature, before we present our empirical approach and results in sections 3 and 4. Section 5 discusses the robustness and section 6 the general relevance of our findings. Section 7 concludes.

2 Literature Review

We contribute to two different strands of literature as we add to the documentation of the home advantage in professional soccer and additionally discuss its relevance for betting markets. Works on the former issue mainly discuss reasons for and drivers of the home advantage. This literature primarily discusses travel fatigue and altitude changes for away teams (Oberhofer et al. 2010, van Damme & Baert 2019), psychological and mental circumstances in home matches (Bray et al. 2002, Neave & Wolfson 2003, Pollard & Pollard 2005, Terry et al. 1998), social pressure and crowd support (Dohmen 2008, Garicano et al. 2005, Goumas 2014, Nevill et al. 2002, Peeters & van Ours 2021, Sutter & Kocher 2004, Unkelbach & Memmert 2010), and location familiarity (Clarke & Norman 1995, Pollard 2002) as potential mediators of the home advantage. Regarding ghost games, recent studies have found that ghost games reduce the home advantage. Reade, Schreyer & Singleton (2020) find that the home advantage is affected by changing referee behavior without crowd pressure on the referee. Their results are consistent with for example Bryson et al. (2021), Dilger & Vischer (2020), Endrich & Gesche (2020), Ferraresi & Gucciardi (2020), Pettersson-Lidbom & Priks (2010), and Scoppa (2021) who also focus on the impact of empty seats on referee decisions. Nevertheless, these papers mostly focus on the few matchdays of the 2019/2020 season and hence only detect very short-term effects. To our knowledge, we are the firsts to extend the analysis to more than a year and a complete season of data which implies for example 43 instead of just nine analysed matchdays for the leagues we discuss. Hence, we can also overcome studying only short-term dynamics and are able to understand long-run adaptations of players and expectations on the betting market. Moreover, we focus on heterogeneity across divisions and potential underlying causes for this.

In general, there exists a broad literature on sports betting markets and soccer betting in particular. However, most studies take a static approach to betting markets and their inefficiencies, as they mainly focus on behavioral biases such as the favorite-longshot bias, sentiment bias or mispricing of the home advantage. Especially the favorite-longshot bias, namely that bets on clear favorites are more profitable than bets on underdogs, has attracted much attention (Angelini & De Angelis 2019, Cain et al. 2000, 2003, Deschamps & Gergand 2007, Oikonomidis et al. 2015, Reade, Singleton & Vaughan Williams 2020). However, there is also evidence of markets without any, with only a weak, or even with a reversed longshot pattern (Angelini & De Angelis 2019, Angelini et al. 2020, Elaad et al. 2020, Forrest & Simmons 2008, Franck et al. 2011, Goddard & Asimakopoulos 2004, Kuypers 2000, Oikonomidis et al. 2015). Potential reasons for the longshot bias can be risk-hedging pricing strategies of betting providers against insider trading (Cain et al. 2003, Shin 1991, 1992, 1993), bettors' overconfidence or image effects (Direr 2011, Golec & Tamarkin 1995, Sauer 1998, Vaughan Williams 1999)

Covid Economics 80, 9 June 2021: 211-231

and odd salience.

Secondly, the sentiment bias addresses the issue that odds do not always efficiently account for heterogeneity in teams' fan support or popularity (Feddersen et al. 2017, Forrest & Simmons 2008, Na & Kunkel 2019). As the sentiment mainly affects betting demand for specific match outcomes, betting providers react by using inefficient odds to ensure a balanced betting volume across the different options or to attract betting volume from highly supported teams by raising their odds.

Lastly and most relevant for our analysis, some studies offer evidence for a persistent mispricing of the home advantage. Elaad (2020) finds an overpredicted home advantage in some English soccer divisions. Vlastakis et al. (2009) provides supporting evidence for European matches. However, Elaad et al. (2020) and Franck et al. (2011) do not find any inefficiencies with regard to home performance. Forrest & Simmons (2008), on the contrary, argue that the home advantage is underestimated in data for Spanish and Scottish soccer.

In contrast to those static, backward-oriented perspectives on market efficiency, we can study the adaptation process of match-related expectations due to new experiences - implying an evaluation of the market's semi-strong efficiency from a dynamic, belief-updating point of view. To analyze this approach to efficiency, the literature has mainly focused on within-match news and their immediate or delayed effects in within-match betting. Angelini et al. (2020), Choi & Hui (2014), Croxson & Reade (2014), and Gil & Levitt (2007) identify that the surprisingness of events - such as unexpected goals - drive odd changes during matches. Less surprising goals, on the other hand, even tend to be underpriced at the beginning with an improving accuracy throughout the following minutes. From this literature, we conjecture that potentially very strong decreases of the home advantage may even lead to an overreaction in the odds, if the effect size is rather unexpected[2].

Finally, note that there are two recent papers with a similar research purpose to ours. Deutscher et al. (2021) and Meier et al. (2020) both analyze betting market efficiency in the presence of ghost games. While both papers reveal an overestimation of the probability of home wins, the latter finds an improving accuracy in the betting odds over time. We will find contradicting results later on and consider the better accuracy of betting odds at the end of the 2019/2020 season not to be related to an improving estimation of betting providers but purely to the return of the home advantage. We further shed more light on division heterogeneity while also analysing the determinants of the miscalculation of odds. Lastly, we extend the analysis to the 2020/2021 season which enables us to also study long-run reaction on betting markets.

3 Empirical Strategy

For our primary analysis of changes in the home advantage during COVID-19 ghost games, we build a dataset including all matches (N = 2,448) from the past four seasons (2017/18-2020/21). There were a total of 776 matches (Bundesliga: 389, Bundesliga 2: 387) played with zero or only few spectators after the first lockdown in spring 2020 which we consider

[2]Further, there is also evidence of time-sensitive inefficiencies at the beginning (Deutscher et al. 2018) and the end of soccer seasons (Goddard & Asimakopoulos 2004) as well as after coach dismissals (Bernardo et al. 2019).

Covid Economics 80, 9 June 2021: 211-231

Covid Economics 80, 9 June 2021: 211-231

to be matches during COVID-19. The dataset further includes match-specific and team-specific data such as the teams' table ranking or their players' average market value. Descriptive statistics of all data used are attached in Table A1 of the Appendix.

To estimate effects of potential shocks on the home advantage, we follow a regression approach like Ponzo & Scoppa (2018) or van Damme & Baert (2019). In particular, we treat every match as two observations - from each team's perspective once. We then determine COVID-19 effects by using ordered probit regressions

$$Points_{tm} = \beta_0 + \beta_1 Home_{tm} + \beta_2 Home_{tm} \times COVID19_m + X'_{tm}\gamma + \epsilon_{tm}$$

where $Points_{tm}$ gives the number of points (zero for loss, one for draw, three for win) which team t earned in match m. $Home_{tm}$ and $COVID19_m$ are dummies which characterise whether a team played at home and whether matches took place during the COVID-19 period. Hence, β_2 is our coefficient of interest. By including X_{tm}, we control for other factors which drive match outcomes like team ability. We later on extend this analysis to for example study the effects' development over time.

To better understand the timing of the analysis, we provide Figure 1 which gives precise information on important events. The last matches under usual conditions took place between March 6th and 9th, 2020. After that, the first lockdown was in place which also caused professional football to take a break. From May 16th onwards, football return in the form of ghost games with minor exceptions in late summer and autumn 2020 where some matches were played in front of visitors. These matches account for 8.8% percent of all COVID-19 matches in our sample, have experienced a maximum of 25% occupancy and are carefully taken into consideration in our later regression analysis.

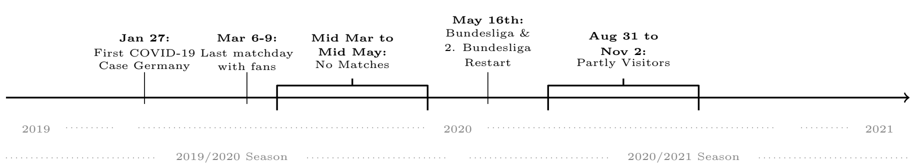

Figure 1: Time Setting of the Analysis

In a second step, we then study the reaction of the betting market. For that, we make use of a betting provider's closing odds for matches of the two German top soccer divisions - Bundesliga and 2. Bundesliga.

To scrutinize the betting market empirically, we rely on a standard regression design as used in previous literature (Deutscher et al. 2018, Forrest & Simmons 2008, Franck et al. 2011) though our regression analysis differs from those former works in the implementation of a test for semi-strong efficiency. As can be seen later on, we therefore introduce time-sensitive inefficiency terms into the regression model.

In general, it is assumed that efficient betting odds include all relevant information and hence on average perfectly predict all match outcomes. As we use closing odds, the betting provider had the opportunity to adapt odds throughout the days and hours before the match. Those adaptations should reflect demand for all three betting options - home win, draw, away win - and hence should account for public and private market information. The odds used are European decimal odds which give the revenue for the case of a successful bet. If the odd is 2.50 for a home win, this implies that one earns a revenue of 2.50 if one's bet was a home win and 0 otherwise. All bets refer to a wager of 1. We suggest that the odds o_{jm} for match m's outcomes $j \in \{H, D, A\}$ represent implicit probabilities p_{jm}:

$$p_{jm} = \frac{\frac{1}{o_{jm}}}{\sum\limits_{j \in \{H,D,A\}} \frac{1}{o_{jm}}}$$

Efficient odds include all relevant information, so that they should optimally forecast match outcomes. Thus, when regressing the implicit probabilities on the match outcomes, no other added variable should be significant in such a regression as information on the other variables should already be included in p_{jm}. Hence, we run the following probit regression for the two divisions:

$$Win_{tm} = \delta_0 + \delta_1 p_{tm} + \delta_2 Home_{tm} \times COVID19_m + \delta_3 Home_{tm} \times COVID19_m \times (\#Matchday_m) + C'_{tm}\eta + u_{tm}$$

where Win_{tm} is a dummy which turns one if team t won match m and is zero otherwise. p_{tm} gives the implicit probability - extracted from the odds - how likely a win of team t in match m is. $COVID19_m$ is a dummy variable which indicates matches played during COVID-19. We also interact this variable with a running time index $\#Matchday_m$ which should identify a potential adaptation process over time, as it indicates the number of ghost gamedays that have been played until match m. C_{tm} are further covariates which are match- and team-specific and which should control for other biases in the betting market such as the sentiment bias and a general mispricing of the home advantage. We consider the betting market to be efficient if δ_1 is not significantly different from 1 as this would be in line with a directly proportional relationship between odds and match outcome. Further, all other covariates have to be insignificant as this implies that the odds already include all relevant information. If we find that δ_2 or δ_3 are significantly different from 0, we interpret this as an indicator for mispricing and an inefficient expectations with regard to ghost games.

4 Results

4.1 Home Advantage

Recent papers (e.g., Bryson et al. (2021), Deutscher et al. (2021), Dilger & Vischer (2020), Fischer & Haucap (2021) and Santana et al. (2021)) find that the home advantage fell drastically for the first few Bundesliga ghost matchdays of the season 2019/2020. Less is known about Bundesliga 2 and the development of the home advantage in the 2020/2021 season. We now shed light on effects across the whole sample of 43 ghost matchdays under COVID-19. Table 1 presents descriptive t-test comparisons of the share of home wins and points of home teams before and during COVID-19. As

Covid Economics 80, 9 June 2021: 211-231

shown, there is hardly any difference in pre-COVID-19 home advantages between leagues, an observations consistent with Pollard (2006) and Leite & Pollard (2018). We clearly see that the home advantage shrank in the Bundesliga at the end of the 2019/2020 season after the restart as home performance was significantly higher than in pre-COVID-19 times. For example, the probability of a home win seems to have decreased by over 12 percentage points. This exceeds the three to four percentage point loss which Bryson et al. (2021) found in an extensive, international sample. With regard to the effect on the difference of points between home and away team (Δ Points) one can even say, that the home advantage completely vanished as away teams performed better. Interestingly, the reduction in the home advantage vanishes in the 2020/2021 season. Opposingly, there is no change (even an insignificant increase) at the first glance in Bundesliga 2.

Table 1: Two-Sided t-Test Analysis of Changed Home Advantage

	Home Win			Δ Points		
	Before	During	p-value	Before	During	p-value
End of 2019/2020 Season						
Overall	43.12%	37.80%	0.184	0.402	0.091	0.140
Bundesliga	44.67%	32.53%	0.028**	0.417	−0.361	0.011**
Bundesliga 2	41.58%	43.21%	0.779	0.387	0.556	0.553
Complete 2020/2021 Season						
Overall	43.12%	43.63%	0.829	0.402	0.368	0.777
Bundesliga	44.67%	42.16%	0.448	0.417	0.324	0.586
Bundesliga 2	41.58%	45.10%	0.289	0.387	0.412	0.886

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

We subsequently present our findings from multivariate regressions which can be found in Table 2. In particular, we control for the teams' difference in market value per player, table ranking, collected points throughout the last four matches and their rest pause from the last match on the match level[3].

Indeed, our descriptive results from above are confirmed in the regression output (s. baseline model (I)). Firstly, we observe a highly significant home advantage in both divisions before COVID-19. Home teams earn more than 0.35 points more than their guests. Moreover, we do not see a clear drop in home advantage during COVID-19 in the Bundesliga and in Bundesliga 2.

Still in models (I), we do not account for time-sensitive development such as the observed pattern of a recovered home advantage in the Bundesliga in 2020/2021. It is an open question whether Bundesliga home players adjust to the unknown situation as an explanation of a return of home wins. If the latter was the case, the ghost game effect should decrease over time. This hypothesis is tested in models (II) of Table 2 where we interact $COVID19_m$ with a running index for ghost matchdays. In particular, we find that the drop in the Bundesliga home advantage was extensive at the beginning of the ghost games (almost half a point). But the home advantage recovered over time with an increase of 0.016 points per

[3]As a robustness check, we also controlled for different other factors which have been named to be potential drivers of match outcomes and the home advantage. These are travelling distance (squared) (Oberhofer et al. 2010), altitude differences (van Damme & Baert 2019), a new coach (van Ours & van Tuijl 2016), kick-off time and weekday Goller & Krumer (2020), Krumer (2017), Krumer & Lechner (2018), derbies (Ponzo & Scoppa 2018, Seckin & Pollard 2008), stadium capacity, existence of stadium track (Dohmen 2008) and share of standing places. This did not change our results.

Covid Economics 80, 9 June 2021: 211-231

Covid Economics 80, 9 June 2021: 211-231

Table 2: Regressions on COVID-19 and Home Advantage

	# Points							
	Bundesliga				Bundesliga 2			
	(I)	(II)	(III)	(IV)	(I)	(II)	(III)	(IV)
Home	0.373***	0.373***	0.373***	0.374***	0.366***	0.366***	0.366***	0.366***
	(0.053)	(0.053)	(0.053)	(0.053)	(0.051)	(0.051)	(0.051)	(0.051)
× COVID-19	−0.117	−0.467***	−0.469***	−0.530***	0.028	0.102	0.101	0.013
	(0.072)	(0.128)	(0.128)	(0.134)	(0.070)	(0.124)	(0.124)	(0.134)
× COVID-19 × (# Matchday)		0.016***	0.167**	0.129**		−0.003	−0.015	−0.013
		(0.005)	(0.063)	(0.063)		(0.005)	(0.040)	(0.040)
× COVID-19 × (# Matchday) × ln(Attendance$_{2019/2020}$)			−0.011*	−0.011*			0.001	0.001
			(0.006)	(0.006)			(0.004)	(0.004)
× COVID-19 × ln(1+Attendance)				0.050*				0.047*
				(0.030)				(0.027)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,448	2,448	2,448	2,448	2,448	2,448	2,448	2,448
McFadden R^2	0.083	0.085	0.086	0.086	0.022	0.022	0.022	0.023

Note: *p<0.1; **p<0.05; ***p<0.01. Ordered probit regressions with heteroskedasticity-robust standard errors. Marginal effects at the variables' means. Controls are Δ Table, Δ Market Value, Δ Points Last Four Match, Δ Pause.

matchday which we interpret as a familiarization process with empty stadia. Considering that 43 COVID-19 matchdays have taken place by now, the Bundesliga's home advantage seems to have more than fully recovered. These findings fit Figure 2 which documents the recovery of the home advantage over time[4]. Again, there is no effect on Bundesliga 2 matches. Interestingly, Bryson et al. (2021) do not find a familiarization process over time in cross-country data. We argue this can be explained with the Bundesliga's very high average attendance and occupancy in contrast to other leagues, so that players elsewhere have not been affected as drastically. Similarly, this could explain the zero effect in the Bundesliga 2.

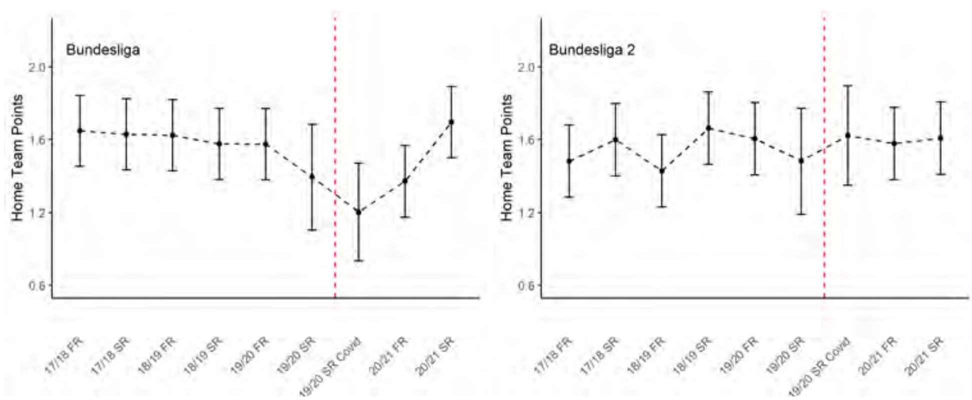

Figure 2: Development of Home Team Points over Time

Estimates are calculated from an OLS regression of collected points of each team on the above mentioned time group fixed effects interacted with a dummy for home teams. We control for performance and team strength covariates such as table position or market value. FR implies first round (first seventeen matchdays of the season), SR means second round. Error bars give the 90% confidence interval.

[4]The figure also documents a drop in home team points of about a fourth of the pre-COVID level during the end of the 2019/2020 season.

To test the latter claim and to determine what exactly drives the recovery of the home advantage, we check whether attendance differences can explain heterogeneity in the loss of the home advantage in the Bundesliga. We suggest that it could take longer to adapt for players which are used to play in front of more fans. Models (III) of Table 2 confirm our hypothesis. There we interact the COVID-19 matchday index with a measure of attendance. In particular, $\text{Attendance}_{2019/2020,m}$ is the mean attendance at home matches of every individual team in the pre-COVID-19 part of the season 2019/2020. Our results show that teams which are used to play in the presence of large crowds were more exposed to an aggregate loss in the home advantage as they recovered at a slower pace.

Lastly, we test the robustness of our results by checking whether findings changes if we account for the small share of matches (approx. 9%) which have been played with visitors. Models (IV) show that there is a significant impact of already a small number of visitors on home team performance in both leagues. This may indicate that there is a kinked, non-linear relation between spectator attendance and home team performance with already relevant effects for only few supporters. Importantly, we also see that our previous results do not change qualitatively.

4.2 Betting Market Efficiency

We now investigate betting market efficiency on basis of these results. Running the regressions explained in our empirical strategy, we arrive at Table 3.

Table 3: Accuracy of Implicit Probabilities

	Win					
	Bundesliga			Bundesliga 2		
	(I)	(II)	(III)	(I)	(II)	(III)
p_{tm}	1.104***	1.111***	1.111***	0.708***	0.708***	0.707***
	(0.072)	(0.082)	(0.082)	(0.141)	(0.134)	(0.134)
Home	0.002	0.001	0.001	0.045	0.045	0.046
	(0.031)	(0.029)	(0.029)	(0.029)	(0.030)	(0.030)
Home×COVID-19	−0.017	−0.165***	−0.182***	0.046	0.038	0.011
	(0.028)	(0.046)	(0.047)	(0.032)	(0.054)	(0.058)
Home×COVID-19×(#Matchday)		0.007***	0.008***		0.0003	0.001
		(0.002)	(0.002)		(0.002)	(0.002)
Home×COVID-19×ln(1+Attendance)			0.016			0.014
			(0.013)			(0.011)
(Δ Average Attendance)$\times 10^{-5}$	−0.007	−0.007	−0.007	0.001	0.001	0.001
	(0.006)	(0.006)	(0.006)	(0.009)	(0.009)	(0.009)
Observations	2,448	2,448	2,448	2,448	2,448	2,448
McFadden R^2	0.121	0.124	0.125	0.034	0.034	0.035

Note: *p<0.1; **p<0.05; ***p<0.01. Probit regressions with heteroskedasticity-robust standard errors clustered on match level. Marginal effects at the variables' means.

Intuitively, we find - for both divisions - that the implicit probability p_{tm} is highly significantly predicting match outcomes. Although this paper focuses on inefficiencies from shocks, also note that Bundesliga 2 odds imply a negative longshot bias,

Covid Economics 80, 9 June 2021: 211-231

Covid Economics 80, 9 June 2021: 211-231

as the coefficient of p_{tm} is significantly smaller than 1. This is consistent with previous results by Forrest & Simmons (2008) on Spanish and Scottish professional soccer, but contrasts earlier findings on the 2. Bundesliga by Oikonomidis et al. (2015). Our finding implies that bettors can outperform the market by betting on low probability wins, hence contradicting the efficient market hypothesis. A (negative) longshot bias cannot be found in the Bundesliga which contrasts Angelini & De Angelis (2019) who have found such a pattern for matches played between 2006 and 2017. The finding is consistent though with Oikonomidis et al. (2015).

In both divisions there is no systematic mispricing of the general home advantage.

With regard to the COVID-19 shock, at first glance the market reaction seems to be efficient, too (s. models (I)). However, Bundesliga bettors misprice the effect of ghost games over time which fits short-run findings by Deutscher et al. (2021). In general, model (II) reveals that the odds imply a probability of a home team win that is 16.5 percentage points higher than it actually was for the case of the first ghost gameday. Furthermore, the effect vanished with time over the ghost game period and disappeared after approximately 23 matchdays. Since it took several matchdays until the inefficiency disappeared, we conclude that expectations only changed rather slowly. Also, the findings imply that betting markets underestimate home performance after the 'break-even point' which implies that the return in the home advantage may have been unexpected. Interestingly enough, comparable patterns cannot be observed for the 2. Bundesliga.

As we cannot infer whether the fluctuation in the efficiency gap originated from an improving accuracy of the market (as assumed in Meier et al. (2020)) or from match outcomes better fitting the odds by coincidence, we cannot finally conclude whether market expectations adapted to ghost games.

Note that we also controlled for a mispricing of the small share of COVID-19 matches which have been played with visitors. No mispricing is evident here. Also, we checked for differences in the average attendance per season[5], as it is one typical proxy used to measure a potential sentiment bias. We do not find any significant effects.

To better understand whether the improved preciseness of betting odds over time originates from a suitable adaptation or just the recovery of the home advantage, we further investigate dispersion of betting odds in the market. For that, we also account for all relevant drivers of match outcomes to avoid an omitted variable bias. Hence, we include several covariates such as ability measures which, for example, capture market value or table ranking differences[6]. Results are listed in Table 4.

We find that the implicit probabilities p_{Hm} of home win odds do not sufficiently account for the decrease of the home advantage in the Bundesliga. Recall that the home advantage dropped by about twelve percentage points at the end of the 2019/2020 season. The implicit probability of an away team win is also only slightly corrected upwards (3.6 percentage points). Moreover, the lack of change in implicit probabilities dramatically ignores the actual development with regard to the interaction $COVID19_m \times Matchday_m$ in probabilities of a home team win. There is no indication for betting odds to consider the improving home team performance. Although we cannot directly conclude that weak-form inefficiency is violated due to the underestimation of ghost game effects on home advantage, as there was no past information, it

[5] For the 2020/2021 season, we also use the pre-COVID-19 attendance from the 2019/2020 season as data.

[6] Other match-specific covariates are e.g. measures for mid-week matches or the change of the head coach as there is evidence on the relevance of such factors (Krumer & Lechner 2018, van Ours & van Tuijl 2016, Flepp & Franck 2020).

Table 4: Determinants of Implicit Probabilities

	p_{Hm}		p_{Dm}		p_{Am}	
	(BL)	(BL2)	(BL)	(BL2)	(BL)	(BL2)
COVID-19	-0.020^{**}	-0.014^{*}	-0.003	-0.0003	0.036^{***}	0.016^{**}
	(0.009)	(0.007)	(0.003)	(0.002)	(0.009)	(0.007)
COVID-19 × (#Matchday)	-0.0003	-0.0002	0.0005^{***}	-0.0001	-0.0001	0.0003
	(0.0004)	(0.0003)	(0.0001)	(0.0001)	(0.0004)	(0.0003)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,224	1,224	1,224	1,224	1,224	1,224
Adjusted R^2	0.839	0.639	0.645	0.274	0.829	0.638

Note: *p<0.1; **p<0.05; ***p<0.01. OLS Regressions with heteroskedasticity-robust standard errors. Control variables are Δ Table, Δ Market Value, Δ Points Last Four Matches, Δ Pause, Travel Distance (Squared), Altitude Difference, a New Coach, Derby, Kick-off Time and Weekday Effects, Stadium Capacity, Existence of Stadium Track, Share of Standing Places.

Covid Economics 80, 9 June 2021: 211-231

should have been evident and plausible that ghost games are less advantageous for home teams when the stadium is empty. Hence, lack of statistical significance of the $COVID19_m$ dummy for p_{Hm} also questions the market's weak-form efficiency. This also holds for inserting an interaction of the quadratic matchday index with the $COVID19_m$ dummy. Note that we can derive from Figure 3 that the decrease in the expected home points is causal. Nevertheless, it can directly be observed that there is no adaptation over time as the treatment effect is constant over time which corresponds to the regression results.

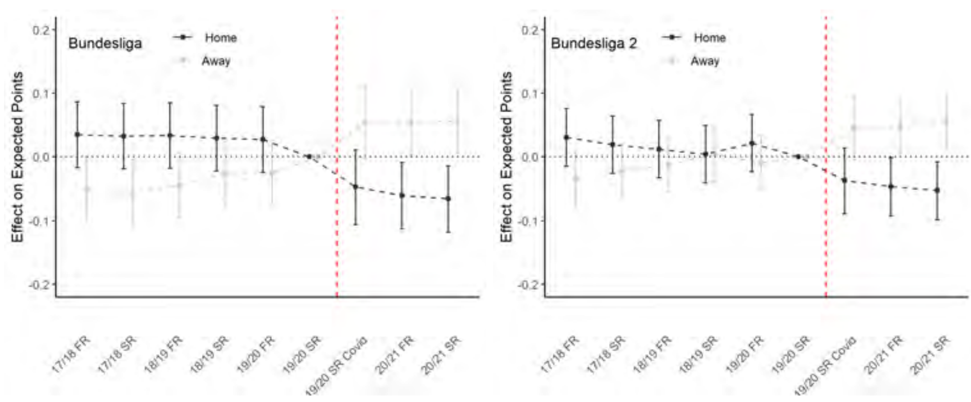

Figure 3: Causal Effects of COVID-19 on Betting Market Expectations
Estimates are calculated from an OLS regression of expected points (weighted sum of points by implicit outcome probabilities) of each team on the above mentioned time group fixed effects. We control for performance and team strength covariates such as table position or market value. FR implies first round (first seventeen matchdays of the season), SR means second round. Error bars give the 90% confidence interval.

Further, this gives us two interesting observations: First, we cannot observe an adaptation in the expected decline of the home advantage after the first ghost game results. That is, although the home advantage drastically decreased during the first ghost gamedays in the Bundesliga and afterwards also showed a positive trend, the market did not significantly update its expectations, as the interactions of the $COVID19_m$ dummy with the actual matchday index are not statistically significant or followed the trend as often typical for financial markets. Since ghost games have been rather rare before the COVID-19 pandemic (Reade, Schreyer & Singleton 2020), an adaptation of the expectations to observed outcomes would have been rational as not much past information has been available. Therefore, we conjecture that the inefficiency in the market also lies in the missing adaptation process over time. Second, we also observe an imperfect pattern in the market reaction to ghost games in the Bundesliga 2: Admittedly the market anticipates correctly the reduction of the home advantage to be smaller than in the Bundesliga, but the expected decrease in the home advantage is still positive and significant (s. p_{Am}) with about 1.6 percentage points more away wins which contradicts our findings of - if at all - more home wins (s. Table 1). When recapitulating that the development of the home advantage strongly differed between Bundesliga and Bundesliga 2, the Bundesliga 2 estimates have been more but still insufficiently accurate. The missing reaction of the market despite the salient results in the Bundesliga throughout the ghost game period is rather surprising and suggests semi-strong inefficiency. Moreover, the negative expected ghost game effect in Bundesliga 2 indicates that the betting market did not correctly assess the role of fan support and attendance for the reduction in the home advantage as a crucial driver of this effect.

After finding different violations of weak and semi-strong market efficiency, it remains an open question why expectations with regards to ghost games actually differed between Bundesliga and Bundesliga 2 as their competitive and administrative background in general are very similar. That is, why does the market ex-ante expect Bundesliga clubs to suffer slightly more from a reduced home advantage than Bundesliga 2 clubs and what drives the expectations? We investigate this by testing several hypotheses of potential explanations of the ghost game effect. For that, we interact the $COVID19_m$ dummy with different variables to analyse whether the betting market expects their importance to change during ghost games which then potentially could explain diverging expectations between divisions. We e.g. test whether betting markets expect player ability to matter more during ghost games which we control for by inserting the interaction term Corona$\times$(Δ Player Value) in the regression. We provide our results of such tests on the impact on the implicit probabilities p_{Hm} in Table A2 of the appendix.

We find that betting odds, and thereby implicit probabilities, include different expectations between Bundesliga and Bundesliga 2. In general, we see that the different consideration of ability measures are one key to explaining the expectation heterogeneity across leagues. Thus, betting markets expected the change in the home advantage to be a team-specific phenomenon which is another finding of our paper. In the Bundesliga, betting markets expected teams with higher player value, better table ranking and good shape throughout the latest matches to suffer more during COVID-19 matches. We find the exact opposing results for Bundesliga 2. Exemplarily, an additional one million Euro difference between the teams' average player value now e.g. implies an increase in the implicit Bundesliga 2 home win probability of 5.6 percentage points on top of the already existing effect size from pre-Corona matches. This implicates an increase of the importance of player ability of more than half in comparison to the market value's relevance before COVID-19. That player ability importance increases during ghost matches implies a reduced competitive balance during ghost games and

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Covid Economics 80, 9 June 2021: 211-231

hence a less present home advantage as the role of home matches is overcompensated by the relevance of player strength. This connotes that betting markets assume Bundesliga 2 supporters to effectively change match outcomes by increasing the surprisingness of match outcomes. That this is not observed in the Bundesliga alludes that Bundesliga player ability is expected to react differently to the appearence of an audience.

Lastly, we note that betting markets reduced Bundesliga home win probabilities for teams which were used to play in highly occupied stadia before COVID-19. This pattern is not appearent for the Bundesliga 2. For the latter league, especially teams with a high share of standing places (proxy for atmosphere) are expected to experience a lower home advantage. Hence, it seems that betting markets considered stadium and fan characteristics but underestimated their role in the Bundesliga and overstated the relevance of standing places in the Bundesliga 2.

Note that those findings across leagues show that betting markets indeed tried to account for ghost games and to some extent considered heterogenous factors - but this then happened partially inefficiently.

Let us now identify some simple but profitable betting strategies based on the findings above. For that purpose, we examine whether it would have been profitable to always bet on the away team in the Bundesliga, if one had been able to predict the reduction in the home advantage. Information on betting returns for various betting strategies are given in Table 5.

Table 5: Return of Simple Betting Strategies

	Bundesliga			**Bundesliga 2**		
	Before COVID-19	End of 2019/2020	2020/2021	Before COVID-19	End of 2019/2020	2020/2021
Home Win	−1.51%	−30.95%	−5.64%	−3.66%	11.74%	5.75%
Draw	2.90%	5.04%	7.04%	7.32%	17.72%	−18.01%
Away Win	−5.73%	16.20%	−15.66%	−4.38%	−17.80%	−12.17%

Indeed, we find it to be highly profitable to follow a simple 'always away win' strategy in the Bundesliga for ghost games - but only in the early matchdays as the home advantage recovered. On the contrary, as there was no decline in the home advantage in the 2. Bundesliga, an 'always home win' strategy would have been overall profitable for second league matches. This also holds for the 2020/2021 season. Whereas short-run strategies could have been hardly predictable in detail, we surprisingly also detect an easy strategy to just bet on draws. In fact, this attempt would have been profitable across all four seasons in the dataset and in both divisions which supports former findings on weak-form inefficiency, e.g., by Deschamps & Gergand (2007). An exception is the negative return to betting on draws in Bundesliga 2 in the 2020/2021. We take those results as further evidence for market inefficiency - during but also partly before ghost games. Also note if a bettor simply had bet symmetrically on all outcomes for all ghost games in both divisions for the matchdays in the 2019/2020 season, no loss would have been realized (revenue of 0.28 percent).

5 Robustness Checks

After having presented results on the violations of the efficient market hypothesis, we also provide an overview on conducted robustness checks. We for example further investigated the heterogeneity in the home advantage drop across divisions. To test whether our hypothesis of an adaptation process of Bundesliga players is correct, we check whether other factors influencing match outcomes changed over time. In particular, we shed light on the referee bias and with-in match performance. The former describes the phenomenon that referees typically are more likely to take decisions in favor of the home team and has been found to drive the reduction of the home advantage during COVID-19 (Reade, Schreyer & Singleton 2020). The latter implies the analysis of match data such as the number of corners or shots to better understand whether the home advantage changed due to adaptations in the playing style. In Table 6, we investigate whether both factors named changed over time. We do not observe an clear trend in the referee behavior and neither in within-match performance data such as corners and fouls. Moreover, the very similar COVID-19 effect on Bundesliga and Bundesliga 2 indicate the small relevance of the referee bias and performance changes on the home advantage in our sample. From that, we conclude that the underlying recovery of the home advantage really is associated with behavioral and psychological adaptations of the players.

Table 6: Potential Drivers of the Home Advantage and their Development over Time

Δ = Home - Away	Δ Yellow Cards		Δ Fouls		Δ Corners		Δ Shots	
	(BL)	(BL2)	(BL)	(BL2)	(BL)	(BL2)	(BL)	(BL2)
COVID-19	0.462***	0.471**	0.682	0.940*	−0.169	−0.023	−1.237	−0.858
	(0.175)	(0.206)	(0.526)	(0.538)	(0.430)	(0.415)	(0.764)	(0.740)
COVID-19 × (# Matchday)	−0.007	−0.003	−0.009	−0.009	−0.014	−0.017	−0.001	0.003
	(0.006)	(0.008)	(0.019)	(0.021)	(0.017)	(0.016)	(0.030)	(0.027)
Observations	1,224	1,224	1,224	1,224	1,224	1,224	1,224	1,224
Adjusted R^2	0.038	0.031	0.036	0.023	0.179	0.024	0.289	0.055

Note: *p<0.1; **p<0.05; ***p<0.01. OLS regressions with heteroskedasticity-robust standard errors. Controls like in Table 4.

In addition, we run identical regressions as in Table 4 for non-closing odds which are collected one or two days in advance of the matches. Here we can make use of data for five additional betting providers, so that we cross-check the findings between providers and hence offer a more disaggregated view. Results do not change. Importantly, no clear adaptation is evident for both divisions in all odds - so that experiences of match outcomes did not change expectations of new matches. Hence, also evidence for an active expectation updating process seems to be limited - also in the 2. Bundesliga.

As results are nearly identical across betting providers, we state that the market is efficient in the sense that ghost games did not create large-scale opportunities for arbitrage trading.

Additionally, we also tested for robustness by including lagged match outcomes instead of the interaction with the matchday running index to the regressions from Table 4. In detail, we calculated the difference between the number of realized and expected home wins, draws and away wins for every matchday. During the ghost game period, lagged values of those variables should be significant determinants of betting odds if odds account for new arriving information

Covid Economics 80, 9 June 2021: 211-231

on match outcomes and the development of the home advantage. Supporting our former results, we do not find evidence for the relevance of the lagged variables for odds of home and away wins, from which a violation of semi-strong market efficiency and non-optimal updating of expectations can be derived. We also ran the same regressions but interacted the differences of realized and expected match outcomes with the running matchday index, as the relevance of newly arriving information for subsequent matches' odds should decrease over time. We cannot find any evidence for that.

6 Discussions

Our analysis shows that the changes in the home advantage during ghost games have not been anticipated correctly. Only from this fact, we cannot directly conclude market inefficiency as no past data, public or private information has been available on ghost games before. Nevertheless, we detect a violation of efficiency in the run of the ghost games as the already played matches and their results have not been priced in correctly. It is important to notice this distinction.

Further, it is still an open question who exactly misses to adapt to the observed market outcomes - the betting provider or the majority of bettors. It would be very surprising if not a single provider out of the six mentioned in the robustness checks was aware of the drastic decrease in the home advantage in the Bundesliga - or at least understood that this effect could be heterogeneous across divisions. This hints at no reaction in the offered odds due to bettors not changing their behavior and expectations. Betting providers even seem to make use of bettors' inaccurate expectations as e.g. suggested by Levitt (2004). Although the majority of bettors usually are no rookies, most of them rely on their feelings and instinct. Year-long experience of home teams outperforming away teams could have caused a rigidity in bettors' beliefs. This is to some extent what Choi & Hui (2014) refer to as conservatism and overreliance on prior expectations. Still, literature on in-game betting (Angelini et al. 2020, Choi & Hui 2014, Croxson & Reade 2014, Gil & Levitt 2007) finds an adaptation process to in-game exogenous shocks such as goals over time which we hardly observe[7].

Moreover, it is important to transmit the results from betting markets to other markets where new and unknown shocks could occur, too. As it should be a goal to reduce inefficiencies as soon as possible after the shock - at least from a welfare perspective - we therefore make an important note: markets, and human agents on those respectively, do not always update beliefs rationally and at all. Especially the rigidity in expectations over time can be found in several other situations, too - e.g. just consider the disposition effect on financial markets when investors persist in their profit expectations. Still, for individual bettors or investors, this then allows outperforming the median investor. A delayed consideration of unknown shocks in e.g. stock or bond prices can also lead to an intermediate-term discrepancy between intrinsic and observable prices. This involves additional risk and hence uncertainty which is unfavorable in financial markets and could lead to delayed bad surprises when the assets' intrinsic values realize again. As another relevant observation for financial markets, we want to put on record that the weak adaptation over time is insensitive to the degree of the inefficiency. The larger pricing gap in the Bundesliga did not result in a quicker expectation update, so that one cannot always rely on invisible market forces to close at least major mispricing gaps.

[7] Gil & Levitt (2007) document inefficiencies in the sense of arbitrage opportunities for short intervals after goals.

Covid Economics 80, 9 June 2021: 211-231

Also importantly, we want to emphasize that there has been media coverage on the drastic decrease in the home advantage in the Bundesliga during the early ghost game period, so that especially experienced bettors should have been aware of this development. This indicates that even improving information transparency over time does not have to better market outcomes immediately. Nevertheless, there is also good news for efficient market pursuers. All betting providers analyzed show similar odds - even after the shock - which do not allow for much arbitrage in such extraordinary times. Moreover, Hegarty (2021) showed that odds' explanatory power in European leagues improved during the COVID-19 time as matches seem to be see less surprises.

Finally, let us mention that our analytical framework is not free of limitations. Ghost games have not been the only important change in the observation period. As a response to many matches being played in a short period of time, two additional substitutions for each team were allowed throughout the ghost game period. Though, there is no clear intuition why more substitutions should have reduced the home advantage.

Finally, we are aware that our research is only partly able to explain the behavioral mechanism behind our results, so that we recommend future research on the question what exactly causes expectation rigidity in the presence of such unfamiliar shocks.

Covid Economics 80, 9 June 2021: 211-231

7 Conclusion

Our analysis provides insights to a betting market's reaction to the present COVID-19 pandemic - making use of the introduction of ghost games in German professional soccer. We find that betting markets expected similar small reductions in the home advantage in the two main professional soccer divisions while detecting different drivers of the expectations across the two divisions. The very different match outcomes between the two leagues over the course of the ghost games did not result in a proper adaptation of expectations, pointing at inefficiencies in the market. For bettors, this provided an opportunity to exploit very simple and highly profitable betting strategies. In a broader sense, we believe that these findings are relevant for all types of prediction markets, especially financial markets, where a slow adaptation process of market participants' expectations can result in high losses and inefficient market outcomes. Interestingly, the high media coverage of the reduced home advantage in the Bundesliga did not affect bettors' behavior which suggests that inefficiencies may persist even in the presence of reasonably transparent markets.

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Covid Economics 80, 9 June 2021: 211-231

A Appendix

Table A1: Descriptive Statistics

	Bundesliga					2. Bundesliga				
	N	Mean	St. Dev.	Min	Max	N	Mean	St. Dev.	Min	Max
Corona	1,224	0.318	0.466	0	1	1,224	0.316	0.465	0	1
Home Win	1,224	0.432	0.496	0	1	1,224	0.426	0.495	0	1
Ability Covariates										
Δ Player Value	1,224	−0.001	9.021	−30.100	30.100	1,224	0.001	0.610	−2.650	2.650
Δ Table Ranking	1,224	0.173	7.123	−17	17	1,224	0.282	7.378	−17	17
Δ Points Last Four Matches	1,224	−0.105	4.085	−12	12	1,224	−0.128	3.607	−11	11
Δ Days Pause	1,143	−0.037	1.043	−7	7	1,224	0.007	1.560	−7	7
Geographical Factors										
Distance	1,224	299.808	146.681	14.507	640.690	1,224	304.540	143.485	5.840	628.920
$\lvert\Delta$ Altitude$\times 10^{-2}\rvert$	1,224	1.466	1.462	0.000	4.870	1,224	1.693	1.361	0.010	5.340
Specific Matches FE										
Derby	1,224	0.044	0.205	0	1	1,224	0.026	0.160	0	1
Within-Week Match	1,224	0.077	0.266	0	1	1,224	0.087	0.283	0	1
Match ≥ 6pm	1,224	0.382	0.486	0	1	1,224	0.342	0.474	0	1
New Coach Home Team	1,224	0.090	0.286	0	1	1,224	0.094	0.292	0	1
Stadium FE										
Capacity	1,224	46,953.8	17,940	15,000	81,365	1,224	25,845.0	14,081.0	10,700	60,559
Share Standing Places	1,224	0.253	0.158	0.000	0.836	1,224	0.470	0.191	0.164	0.836
Track	1,224	0.069	0.254	0	1	1,224	0.111	0.314	0	1
Within-Match Data										
Δ Yellow Cards	1,224	−0.252	1.600	−6	6	1,224	−0.263	1.786	−6	6
Δ Fouls	1,224	−0.484	5.011	−24	19	1,224	−0.573	5.297	−18	16
Δ Corners	1,224	0.689	4.347	−14	19	1,224	0.734	4.022	−12	14
Δ Shots	1,224	2.123	8.035	−29	25	1,224	1.821	7.039	−21	24
Betting Odds										
Home	1,224	2.963	2.352	1.050	22.230	1,224	2.469	0.773	1.280	7.510
Draw	1,224	4.411	1.560	3.000	15.370	1,224	3.608	0.392	2.850	5.940
Away	1,224	4.574	4.358	1.120	41.000	1,224	3.475	1.326	1.400	11.680

Table A2: Investigation of Division Heterogeneity in Expectations (p_{iH})

	p_{Hm}													
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
COVID-19	−0.020**	−0.014*	−0.021**	−0.013*	−0.022**	−0.013*	−0.020**	−0.014*	−0.020**	−0.014*	−0.031***	0.017	0.185	−0.306***
	(0.009)	(0.007)	(0.009)	(0.007)	(0.009)	(0.007)	(0.009)	(0.007)	(0.009)	(0.007)	(0.011)	(0.012)	(0.122)	(0.064)
×(Δ Player Value)			−0.004***	0.056***										
			(0.0005)	(0.008)										
×(Δ Table Ranking)					0.003***	−0.002***								
					(0.001)	(0.001)								
×(Δ Points Last 4 Matches)							−0.003**	0.004***						
							(0.001)	(0.001)						
×(Δ Days Pause)									−0.00003	0.004*				
									(0.005)	(0.002)				
×(Share Standing Places)											0.040	−0.068***		
											(0.027)	(0.021)		
×ln(Pre-COVID Attendance)													−0.019*	0.030***
													(0.012)	(0.007)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,224	1,224	1,224	1,224	1,224	1,224	1,224	1,224	1,224	1,224	1,224	1,224	1,224	1,224
Adjusted R^2	0.839	0.640	0.848	0.653	0.842	0.645	0.840	0.642	0.839	0.640	0.840	0.642	0.840	0.645

Note: *p<0.1; **p<0.05; ***p<0.01. OLS Regressions with heteroskedasticity-robust standard errors. For covariates, s. Table 4.