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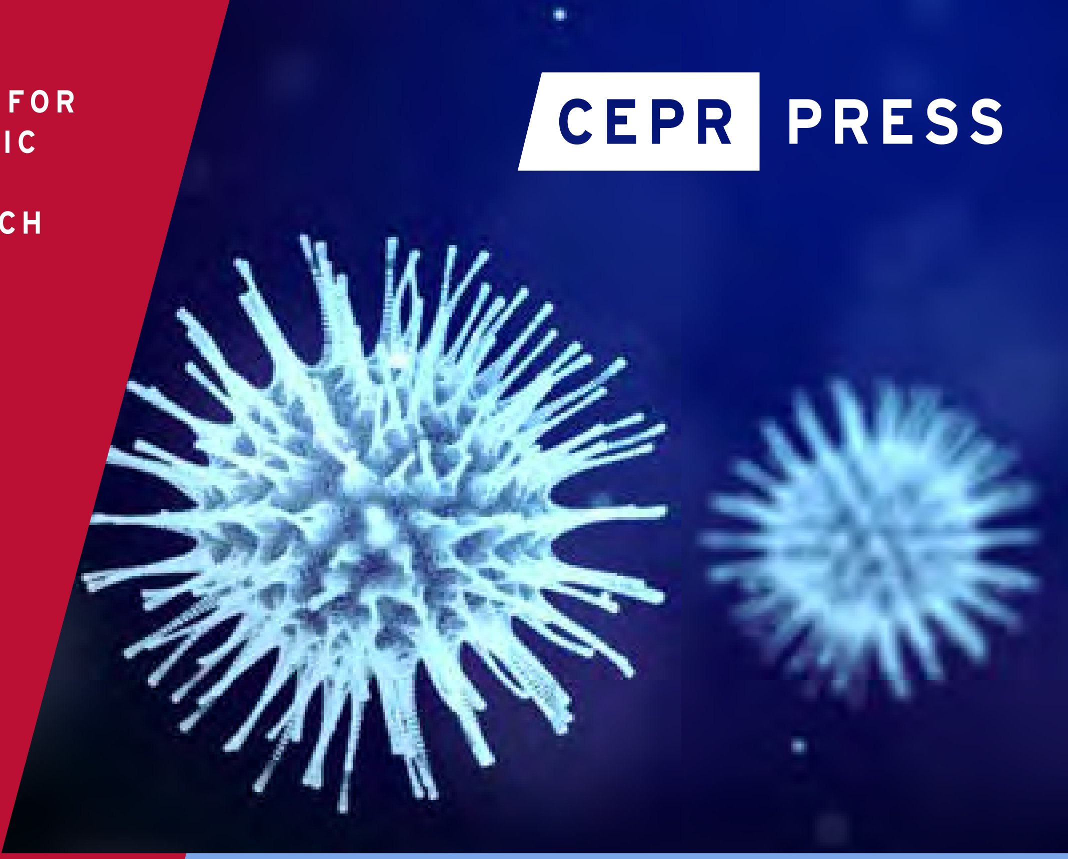

COVID ECONOMICS

VETTED AND REAL-TIME PAPERS

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Covid Economics
Vetted and Real-Time Papers

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

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

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

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Ethics

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

Submission to professional journals

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

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

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

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Contents

Changing Skill Structure and COVID-19

Elena Mattana[1], Valerie Smeets[2] and Frederic Warzynski[3]

Date submitted: 21 August 2020; Date accepted: 24 August 2020

In this paper, we provide evidence that the early labor market effects of COVID- 19 have been concentrated on subsets of the workforce already negatively hit by the recent wave of structural change in the occupation and skills of workers. We document that the occupation and education composition of furloughed workers in Denmark is concentrated among individuals with low education or vocational training, as well as specific occupational groups that were on the decline before the crisis hit. Our results strengthen the hypothesis that COVID-19 will accelerate the ongoing structural transitions in the economy.

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1 Aarhus University
2 Aarhus University
3 Aarhus University

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1 Introduction

A well documented fact in advanced economies has been the profound transformation of the typical job performed by individuals. As firms have adopted new ICT tools and modified their methods of production, they have also reorganized and upgraded their workforce (see e.g. the discussion in Bernard et al., 2020 for Denmark). This increased demand for skilled workers has led to the relative rise of skill-intensive occupation and education groups at the expense of lower-skill ones. There has recently been some discussion that COVID-19 might accelerate this structural transition and generate significant losses for individuals in relatively low-skill groups (see e.g. Barrero, Bloom and Davis, 2020; Erlanger, 2020; Lohr, 2020). Indeed, one hypothesis is that the economic disruptions generated by the virus have dramatically affected the ability of workers to perform their tasks, in particular in some specific subgroups of the workforce that were already in relative decline. Our paper shows that the early labor market effects of COVID-19 have been concentrated on subsets of the workforce already negatively hit by the recent wave of structural change.

Due to the unprecedented nature of the COVID-19 shock, the Danish government, like many other governments around the world, has designed an extensive help package including generous wage and salary subsidies provided to firms who commit not to fire workers during the covered period. Early in the process, Danmark Statistik (DST) has documented the relative importance in the government package of low-educated workers and workers in industries where close contact with the customers is necessary (such as hotels, bars, restaurants, hairdressers, fitness centers, cinemas).[1] We show that the whole economy has been affected by the crisis, including large parts of the manufacturing and service sectors. Moreover, we dig deeper by analyzing the role of individual, firm and sector characteristics on the likelihood to be included in the help package.

We use Danish administrative data covering all workers included in the government wage and salary compensation package and relate them to the recent labor market evolution of the occupational and education groups of the workers. Our results show that there is a lot of heterogeneity in the sectors, education and occupation groups covered by the package. The package covered not only those service activities that were almost entirely shut down from mid March and gradually reopened by the end of May, but also the manufacturing and service sectors as a whole. Moreover, many of the workers most exposed to the COVID-19 shock belonged to declining educations and occupations. In

[1] For updated experimental aggregate statistics on this topic we refer to the DST website: https://www.dst.dk/da/Statistik/nyt/relateret?pid=398

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particular, employment in lower skill level occupations such as associate professionals, clerks, and some manufacturing and service sector workers was declining in the decade prior to the COVID-19 shock, due to the increasing demand of skills and the overall structural transformation in the economy.

We document a clear negative relationship between the share of workers on the package and the past growth rate in employment by industry and occupation or education. This relationship is mainly driven by variation in occupations and education within the manufacturing, retail and service sectors and not only by those activities that were directly affected by the lockdown. This result gives credence to the hypothesis that the COVID-19 shock will accelerate the ongoing structural transformation of the economy. Moreover, the patterns uncovered here give us an indication of which occupations' decline will be accelerated by this crisis.

The rest of the paper is structured as follows. In Section 2, we discuss the most relevant literature for our study and how we differ from previous work. Section 3 gives a brief overview of the help package designed by the Danish government. In Section 4, we describe the datasets that we use and provide summary statistics about the characteristics of firms and individuals covered by the package. Section 5 introduces a more formal empirical exercise and shows the results of our analysis, and Section 6 concludes.

2 Literature Review

A large literature has been devoted to increased inequality, rising skill premium and polarization of the labor market (see e.g. Autor; Acemoglu and Autor, 2011; and more recently Autor, 2019). A well accepted explanation for this evolution relates to skill biased technical change, although offshoring and rising import competition from China in particular have also been blamed. (see also Acemoglu, 2003 for an early paper looking at the relationship between import competition and skilled biased technological change). Recently, increased use of industrial robots and more generally automation have considerably affected the nature of work. Preliminary anecdotal evidence suggest that COVID-19 will only accelerate this trend in the near future (see e.g. Cohen, 2020; Erlanger, 2020; Lohr, 2020).

Our analysis is complementary to three recent papers on the reallocation effect of COVID-19 and the role of government intervention. The first two papers analyze the US labor market. Barrero, Bloom and Davis (2020) use the Survey of Business Uncertainty (SBU) to build forward-looking measures of job reallocation triggered by the virus outbreak. In the survey, firms are asked (among many other topics) how many workers

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they are planning to hire or fire on a monthly basis. They report that the April figure of expected excess job reallocation was 2.4 times larger than the pre-crisis figure and that 10.8% of the jobs from March were terminated. Meanwhile, new hiring was equivalent to 3.9% of the workforce in the previous month. This suggests that some firms were able to generate additional sales during the pandemic (the image of Amazon and other online delivery companies naturally comes to mind) while others were hardly hit and had to let go a significant amount of their workforce. More dramatically, they project that between 32% and 42% of these jobs destroyed are never coming back. In line with these elements and with a large literature on creative destruction and job flows, the authors also argue and provide preliminary evidence that this reallocation is expected to take place within industries and within regions. They also predict and show evidence that firms expect the amount of full days performed from home to triple after the pandemic is over, suggesting a significant shift for some activities.

Petroulakis (2020) focuses on job losses by task content using the Current Population Survey (CPS) during the early stage of the crisis (up to April 2020). He shows that individuals with jobs with high non-routine content are more likely to be protected even if the job is not "teleworkable". Another finding is that individuals are more likely to lose their jobs in industries and occupations with historically higher (past) turnover. On the other hand, low-skill individuals are less likely to lose their jobs if the share of high-skill workers with high non routine content in the industry is higher. He also finds little evidence of a change in the share of workers switching occupations or industries, and of a change in the hiring rate, suggesting it might be too early to detect evidence of reallocation with the available CPS data at least. Compared to these papers, we dig deeper into the type of education and occupational groups most represented in the government help package, in the light of the recent relative employment growth of these groups in the economy. In this sense, our analysis identifies the type or workers that are less essential in the short run for companies during a crisis.

Our paper is also closely related to the paper by Bennedsen et al. (2020). They use firm-level survey data for a large sub-sample of firms in the Danish economy to analyze how the government package helped firms to maintain their economic activity and level of employment. The main result is that, while the majority of firms reported a significant decline in revenue (a quarter of firms experienced a decline in revenue of more that 35% relative to a previous year base period; the median firm expected a 20% decrease), they were able to maintain their workers thanks to government support. Their results suggest that government policy saved 81,000 jobs while increasing furloughs by 285,000. As opposed to Bennedsen et al. (2020), who use survey data, our analysis focuses on

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workers rather than firms, and also uses administrative data provided by DST covering the population of individuals receiving help from the government, linked to register data covering the entire Danish population. Moreover, our research question is very different, as we compare patterns of covered individuals to the characteristics of their occupation, education or industry groups.

Our work is also connected to a recent literature documenting which subgroups of the population were more likely to be hit by the COVID-19 shock. Starting with Denmark, Andersen, Schroeder and Svarer (2020)—appointed as experts to advice the Danish government on the reopening policy—provided an early overview of the economic sectors and groups covered by the Danish wage compensation. They show the over-representation of workers with basic education, and of some sectors like hospitality, culture and leisure, and retail. A Norwegian study (Alstadsæter et al., 2020) looks at individuals affected by temporary unemployment and finds that the crisis mostly affected the "financially vulnerable population", parents with younger children, and also that most layoffs took place in smaller, less productive, and financially weaker firms. Interestingly, they also relate occupations to the need of physical proximity (using O*net), and show that occupations needing more physical proximity were more likely to be hit, as expected. More recently, a similar study for Japan (Kikuchi et al., 2020) shows that women, singles, low-skilled, workers on contingent contracts and engaged in social and non-flexible jobs were more likely to be hit by the COVID-19 shock. These papers however do not explicitly relate their analysis to structural change (i.e. the past employment growth of the subgroups they identify).

3 The Danish COVID-19 Economic Response

On March 11, the Danish government announced a major lockdown of economic activities following the worsening of the COVID-19 crisis.[2] The government prioritized closing the public sector and leaving the private sector—especially production firms—more leeway, and created guidelines that were specific to different industries, e.g. school, stores, offices, and production firms.[3]

Together with the lockdown, the government introduced a series of relief packages to

[2]A timeline of the lockdown and gradual re-opening can be found in Appendix Figure 1.

[3]In particular, the guidelines involved three main areas: the planning of the work (provide PPE, shift to remote all activities not requiring physical attendance, reschedule work hours and shifts to respect safe distance between workers), the design of production facilities (close food halls and insure safe distances), and behavior and hygiene (provide hand sanitiser, multiple daily cleaning of surfaces, clear advertising of general safety guidelines).

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safeguard the economy. There were two main types of relief packages: liquidity packages directed to banks and enterprises and short-term relief packages for firms and workers. The first type included state guarantees to bank loans for struggling companies and lowering of liquidity requirements for banks. Short-term relief packages included wage and salary compensations for workers of struggling firms, coverage of fixed costs for companies with large drops in revenue VAT-tax extensions for small and medium-size companies, revenue compensation and tax extensions for self-employed workers, additional flexibility in work schedules and sick leave for all firms for COVID-19-related circumstances.[4]

Our data covers one of the larger packages, the wage and salary compensation package offering employers partial coverage of the salary of workers sent home during the lockdown. The package was known in the media as the "three-part deal" as it was negotiated with the employers and employee unions and it implied an effort from the government, the employers and the furloughed employees. The package was initially intended to cover the 3-months period from March 9 to June 9. On April 18, coverage was extended until July 9 and, on June 6, it was extended again to last until August 29.[5]

A company expecting to layoff either 30% of or more than 50 employees could apply for the compensation package for its workers. The company was then able to send home with full pay as many workers as needed for as long as needed until the end date of the package. These workers' pay was covered in part by the government and in part by the employer according to the following rules:

- The government pays up to 75% of the salary or up to DKK 30,000 for fixed full-time workers, and up to 90% of the wage or up to DKK 30,000 for hourly full-time workers.[6]
- The company covers the remaining pay.
- The workers have to give up 5 vacation days, either by taking them if available or by taking 5 days of non-paid leave.[7]

[4]See the discussion in Bennedsen et al., 2020 or Ministry of Finance, 2020a for more details about the relief package.

[5]If a company is fully closed due to ongoing lockdown restrictions (e.g. bars and nightclubs), the compensation package will run until these restrictions are lifted and the company can operate again.

[6]The inital caps were of DKK 23,000 and 26,000 for the two types of workers, respectively, and were raised retroactively on March 30. A full-time contract consists of 37+ hours a week. For part-time employees with a contractual weekly work time of x equal to less 37 hours, the cap is then equal to $x/37$ times the full-time cap.

[7]With the second extension, the government required that three of the weeks between July 9 and August 29 are paid in full by the company, ideally through accrued vacation days.

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Importantly, the company is not allowed to lay-off any of their employees during this time and workers are not allowed to work for the time the company receives compensation for them. Workers of the same company can be sent home on the package for different length of times, and they can be sent home partially, e.g. work 3 days a week and be sent home 2 days.[8]

Workers compensated under this policy are commonly called "sent" home or "on the package" in Denmark. The policy is similar to the German "kurzarbeit" in the sense that firms promise not to lay off workers while the government makes up for lost income. The common English term to identify a sent home worker is "furlough". For simplicity, we will use this term in the remainder of the paper, but we want to stress that, in the US context, a furlough is a mandatory temporary unpaid (or reduced pay) leave of absence from a job. This is different from the Danish context in two main aspects: the worker receives full-pay (up to some requirements on vacation days) and the firm can not lay off anyone while its workers are furloughed.

4 Descriptive Analysis

4.1 The Compensation Package

The main dataset for our paper (referred to as LONKOMP) covers 288,801 records of compensations requests granted under the conditions of the package until the end of week 25 (i.e. June 21). This includes a total of 249,897 individuals, as 35,245 workers received compensations from different companies or following two or more separate compensation requests from the same company.[9] The dataset provides a unique personal identifier, the birth date, the amount received as compensation, the beginning and end of the furlough period, a rough employment description distinguishing between apprentices, hourly paid and monthly paid workers (not *per se* sufficient for our analysis, as described later in this section), the firm identifier and sector of activity (DB07 6-digit code).

The dataset also contains information about the size of the firm where the individuals work (the number of full time equivalent workers, from here on referred to as FTEs) and the quarter of reference when the figure was obtained from the firm register (usually the

[8]In case of unforeseen need, firms can pause the package and recall sent-home employees to work for a period. The company must then repay the wage compensation for the days when the employees in question were recalled for work. The cap of the compensation is not affected by the number of days the employee works or receives compensation but rather their contractual relationship to the company.

[9]This was especially true for firms in the culture and leisure (like fitness centers) and hospitality industries (like bars and restaurants) where it is not uncommon for workers to be employed for a few hours at different establishments.

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third quarter of 2019). We observe 30,489 unique firm identifiers in the raw dataset. We drop a few sectors with few observations: agriculture, extraction, public administration and utilities. For our analysis, we need firm size; therefore, we only keep firms reporting this information, reducing the sample to 28,080 firms. We also only keep individuals aged between 18 and 65.

Appendix Figure 2 shows the timing of adoption of the package, with the starting date of each compensation spell in the left panel and the end date in the right panel. Firms moved quickly after the announcement and around 80% were covered already by mid-end March. In line with the reopening strategy of the government, many firms ended coverage on June 9th.

Table 1 provides basic summary statistics regarding the characteristics of the firms in our sample splitting the firms by size and by industry. Specifically, in the first part of Table 1, we show the size of the package in terms of firms and workers. We show the total number of FTEs employed in the third quarter of 2019 by the firms in the package, the average size of each firm, the size of the furloughed workforce in terms of headcount, FTEs, and the average firm-level share of furloughed workers relative to the firm size in the third quarter of 2019. The total number of FTEs employed by the 28,080 firms in our clean sample is slightly below 340,000, while the number of furloughed FTEs is around 188,000. The share of furloughed FTEs at the firm level is high, at 72.6%. The last three columns of Table 1 show the size of the compensation package. The total size of the package amounts to around 9.7 billion DKK, with an average of almost 350 thousand DKK per firm. On average, workers were furloughed for 85 days, i.e. almost for the full extent of the initial three-month duration of the package.

The upper panel of Table 1 shows the distribution and summary statistics of workers and firms in the package by firm size. Small firms (defined as firms with less than 11 employees) represent 82.4% of all firms that received a compensation from the package but only 32.2% of all furloughed workers. In fact, large firms (defined as firms with more than 100 employees) account for a disproportionately large proportion of the FTEs, as is commonly known: the 400 largest firms in the package employ more than 150,000 workers, i.e. around 40% of the total. Small firms were more likely to furlough a larger proportion of their workforce.[10] While the total size of the package was shared almost equally between firms in the three size classes, large firms received on average almost 53 times the amount of small firms. The average number of furlough days was larger for

[10]This is further emphasized in Appendix Figure 3 that shows the histogram of the share of furloughed workers per firm for all firms and by size class, graphically illustrating the difference in behavior between small and large firms.

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Table 1: Characteristics of firms covered by the compensation package.

		Workers					Wage compensation		
	Number of Firms	Total FTE (2019)	Average FTE (2019)	Furloughed FTE Total	Furloughed FTE Share	Furloughed Headcount Total	Number of days	Total (mo.DKK)	Average (DKK)
All	28,080	339,551	12.1	187,704	72.6%	246,273	85	9,738	346,782
By firm size									
0-10 workers	23,133	73,716	3.2	57,926	75.6%	79,206	86	3,254	140,644
11-100 workers	4,547	114,139	25.1	68,180	59.8%	85,414	80	3,523	774,711
100+ workers	400	151696	379.2	61,598	49.9%	81,653	73	2,962	7,403,806
By Industry									
Business Services	1,930	15,142	7.8	7,896	67.4%	9,194	88	414	214,720
Construction	1,023	7,170	7.0	3,594	71.3%	3,789	79	194	189,918
Culture/Leisure	1,232	13,633	11.1	10,079	75.4%	20,176	86	524	425,674
Finance	170	1,616	9.5	717	70.7%	833	89	43	253,634
Health/Education	2,773	16,446	5.9	12,140	76.9%	16,421	82	684	246,612
Hospitality	4,590	41,312	9.0	29,723	75.2%	46,560	88	1,595	347,515
ICT	1,013	11,039	10.9	5,085	67.6%	6,896	87	262	258,681
Manufacturing	2,188	52,398	23.9	27,512	68.8%	29,229	81	1,280	585,104
Real Estate	664	5,307	8.0	2,815	65.4%	4,123	80	143	214,638
Retail	7,703	97,519	12.7	52,601	70.5%	67,922	84	2,690	349,231
Services	3,600	38,425	10.7	18,575	77.5%	22,771	84	957	265,808
Transportation	1,145	34,620	30.2	16,326	74.3%	17,668	88	914	798,255

Source: LONKOMP. Notes: Firms and workers covered by the compensation package and size of the package, overall and by firm size and industry. Variables by column: (1) number of firms, (2) total number of full time equivalents (FTE) employed in the third quarter of 2019, (3) average number of full time equivalents (FTE) employed per firm in the third quarter of 2019, (4) total number of furloughed FTEs, (5) share of furloughed FTEs over total employed (capped at 1 if firm size in 2019 lower than number of workers furloughed in 2020), (6) total number of furloughed workers, (7) average number of days of compensation, (8) total amount of the compensation in millions DKK, (9) average compensation per firm in DKK. ICT stands for Information and Communication Technology.

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small firms (86 days) than large ones (73 days).

The bottom panel of Table 1 shows the distribution and summary statistics of workers and firms in the package by industry. Not surprisingly, we observe a concentration of furloughed FTEs in retail (especially in the clothing retail and wholesale), hospitality, and culture and leisure. However, Table 1 shows that few parts of the economy were not affected by the COVID-19 shock: In particular, 29,229 manufacturing workers and 31,965 service and business service workers were in the package, amounting to 12% (15%) and 13% (14%) of the total number of workers (FTEs) and attracting 13% and 14% of the total budget.

4.2 The Compensation Package in the Economy

The LONKOMP dataset only provides information about individuals covered by the package and about firm size for firms taking the package. In order to compare firms and individuals included in the package to those who are not, we need information about the population of firms and individuals. Unfortunately, other datasets are not updated in such a timely manner, but we can go back to December 2018 using Statistics Denmark's monthly income register (BFL). This linked employer-employee dataset is based on income information reported to the tax authority by the employers and provides monthly information about all contracts between firms and workers. Among the variables included, we use the number of hours worked, the monthly wage and the detailed occupational code (ISCO-08, 6-digit). In December 2018, we observe 159,788 firms, employing 2,173,936 FTEs.

Table 2 provides information about the number of firms and FTEs in the BFL dataset in December 2018 and compares these numbers with those in LONKOMP. Specifically, we calculate the share of firms (workers) in the package by firm size and industry by dividing the total number of firms (workers) in the package over the total number of firms (workers) in the economy in December 2018. Those covered by the package represent 17.6% of firms and 15.6% of FTEs in the economy in December 2018. Small and large firms are similarly represented: 16.8% of small firms and 19.4% of large firms request compensation. All sectors of the economy were hit in larger or smaller measure by the shock. The share of firms covered by the package over the total count in December 2018 goes from 45.4% in hospitality, 30.9% in culture and leisure, and 26.6% in retail, to 3.1% in finance, 5.7% in construction, and 7.5% in the real estate sector. In particular, 21.7% of all manufacturing firms and 20.2% (11.9%) of all (business) service firms request compensation. As can also be seen from Table 2, workers in health and education represent a large proportion of the

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Table 2: Fraction of all firms and workers covered by the compensation package.

	Firms		Workers (FTEs)		
	Total in the Economy (2018)	Share in the package	Total in the Economy(2018)	Share in covered firms	Share furloughed
All	159,788	17.6%	2,173,936	15.6%	8.6%
By firm size					
0-10 workers	137,380	16.8%	319,003	23.1%	18.2%
11-100 workers	20,348	22.3%	531,093	21.5%	12.8%
100+ workers	2,060	19.4%	1,323,841	11.5%	4.7%
By Industry					
Business Services	16,280	11.9%	131,817	11.5%	6.0%
Construction	17,980	5.7%	129,831	5.5%	2.8%
Culture/Leisure	3,982	30.9%	25,728	53.0%	39.2%
Finance	5,538	3.1%	75,648	2.1%	0.9%
Health/Education	13,151	21.1%	700,422	2.3%	1.7%
Hospitality	10,106	45.4%	56,597	73.0%	52.5%
ICT	8,894	11.4%	90,979	12.1%	5.6%
Manufacturing	10,067	21.7%	257,483	20.4%	10.7%
Real Estate	8,884	7.5%	31,334	16.9%	9.0%
Retail	29,010	26.6%	311,491	31.3%	16.9%
Services	17,821	20.2%	129,586	29.7%	14.3%
Transportation	6,822	16.8%	106,646	32.5%	15.3%

Source: LONKOMP, BFL. Notes: Fraction of firms and workers covered by the compensation package over the economy (snapshot Dec. 2018), overall and by firm size and industry. Variables by column: (1) total number of firms, (2) share of firms covered by the package over the total number in the economy, (3) total number of workers, (4) share of workers in the economy employed in the covered firms, (6) share of furloughed workers among those employed in covered firms (capped at 1 if reported firm size lower than number of workers furloughed in 2020). ICT stands for Information and Communication Technology.

workforce, but only 2.3% work in covered firms, and only 1.7% are furloughed. This is because, with a few exceptions (e.g. dentists or physiotherapists), these workers belong to the public sector and were either essential (health) or working from home (teachers). We therefore omit them in our formal analysis.

4.3 Occupation and Education

The next step is to understand which occupation and education groups were most exposed to COVID-19 by comparing the occupations of individuals covered in the package relative to the population. In order to do so, we use the unique individual identifier to merge BFL and LONKOMP and assume that the the primary occupation reported in December 2018 in BFL is still the main occupation in the Spring of 2020. We acknowledge that individuals might have switched occupations between the two periods, but this is an assumption we are forced to make because of data limitations.

To understand whether the specific occupation was growing or declining prior to the COVID-19 shock, we calculate the growth of employment by occupation between December 2010 and December 2018. Throughout the paper, we use different aggregation levels of the DISCO-08 occupational code. DISCO-08 is the official Danish version of the international professional classification, International Standard Classification of Occupations (ISCO-08), which is prepared by the International Labor Organization (ILO).[11]

In Table 3[12], we show the number of FTEs by occupation, employment growth in 2010-2018, the relative size of the occupation in the overall economy, the relative size covered by the package, and an index of representation in the package. For ease of reading, we use the rougher 2-digit occupation code. Whenever the share in the package is larger than the share in 2018, the occupation was disproportionately hit by the COVID-19 shock in the sense that it is over-represented in the LONKOMP dataset. We construct an index of representation in the package by simply dividing the share of FTEs in 2018 by the share of furloughed FTEs for each occupation to have a clear picture of the magnitude of over-representation. According to this interpretation, service workers are the most over-represented, as they are 3 times more likely to be on the package. Other occupations that are over-represented are life science, health and teaching professionals, law, social sciences and culture associate professionals, customer service clerks, and workers in sales, care and protective service, metal and machinery, precision and craft, assembly, cleaning and manual food handling, renovation and other manual work.

[11] For details, see https://www.dst.dk/en/Statistik/dokumentation/nomenklaturer/disco-08.

[12] This table does not include workers in the health and education sector. In an online appendix, we provide a similar table that includes this sector.

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Table 3: Occupation distribution (2-digit) in the compensation package.

Occupation title	Total FTE (2018)	Growth 2010 -2018	Share in 2018	Share in the package	Index of representation
Top managers	20,021	36.1%	1.8%	0.4%	0.22
Middle managers	54,081	-11.9%	5.0%	3.6%	0.72
Science and engineering professionals	54,497	-2.6%	5.0%	2.2%	0.44
Life science and health professionals	8,950	0.3%	**0.8%**	**1.0%**	**1.25**
Teaching professionals	10,671	17.7%	**1.0%**	**1.6%**	**1.60**
Finance, administration and sales professionals	62,561	1.7%	5.7%	2.1%	0.37
ICT professionals	44,336	21.6%	4.1%	1.3%	0.32
Law, social sciences and culture professionals	30,513	9.4%	2.8%	0.9%	0.32
Science and engineering associate professionals	47,356	-24.0%	4.4%	3.2%	0.73
Life science and health associated professionals	7,053	-34.0%	0.6%	0.4%	0.67
Business, finance, administration and sales associate professionals	93,693	8.1%	8.6%	8.1%	0.94
Law, social sciences and culture associate professionals	9,692	-63.9%	**0.9%**	**2.2%**	**2.44**
ICT associate professionals	11,733	-7.6%	1.1%	0.6%	0.54
Office clerks	51,141	7.7%	4.7%	4.4%	0.94
Customer service clerks	16,864	-34.9%	**1.5%**	**2.9%**	**1.93**
Accounting and registration clerks	37,901	39.4%	3.5%	3.0%	0.86
Other clerks	18,495	-31.2%	1.7%	1.4%	0.82
Service workers	39,238	4.5%	**3.6%**	**11.6%**	**3.07**
Sales workers	88,705	-12.3%	**8.1%**	**14.2%**	**1.75**
Care workers	9,937	19.2%	**0.9%**	**1.8%**	**2.00**
Protective services workers	6,254	31.3%	**0.6%**	**1.3%**	**2.17**
Skilled agricultural and fishery workers	4,245	-0.5%	0.4%	0.2%	0.50
Construction workers	52,151	34.2%	4.8%	1.8%	0.37
Metal and machinery workers	49,468	-18.5%	**4.5%**	**5.9%**	**1.31**
Precision and craft workers	4,097	-14.6%	**0.4%**	**0.8%**	**2.00**
Electric and electronic workers	23,366	22.5%	2.1%	1.2%	0.48
Other manufacturing craft workers	13,657	39.9%	1.3%	1.1%	0.85
Machine and stationary plant operators	43,060	19.6%	4.0%	3.4%	0.85
Assembly workers	11,377	-21.0%	**1.0%**	**1.3%**	**1.30**
Drivers	43,523	19.5%	4.0%	2.2%	0.55
Cleaning workers	30,936	3.0%	**2.8%**	**4.1%**	**1.46**
Manual agricultural and fishery workers	850	20.8%	0.1%	0.0%	0.00
Manual manufacturing, construction and transportation workers	67,904	-11.5%	6.2%	5.3%	0.85
Manual food workers	11,671	60.4%	**1.1%**	**2.7%**	**2.45**
Renovation and other manual workers	8,467	55.4%	**0.8%**	**1.8%**	**2.25**

Source: LONKOMP, BFL. Population of workers aged between 18 and 65 years old. Workers in the health and education sector are not included. Notes: Distribution of occupations (primary job) by 2-digit occupation code. Variables by column: (1) total number of FTEs in the economy (snapshot Dec. 2018), (2) employment growth by occupation between 2010 and 2018, (3) share of FTEs in each occupation (snapshot Dec. 2018), (4) share of furloughed FTEs in each occupation, (5) index of representation of the occupation in the package calculated as (4)/(3). ICT stands for Information and Communication Technology.

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One striking observation is that several of the occupation groups disproportionately exposed to the COVID-19 shock are also those that have been shrinking over the last decade. In particular, over-represented occupations unique to the manufacturing industry were already declining in the prior decade, with negative rates of employment growth equal to 18.5% for metal and machinery workers, 14.6% for precision and craft workers, and 21% for assembly workers. Similarly, also customer service clerks, sales workers, and law, social sciences and culture associate professionals showed high rates of decline prior to the COVID-19 shock. This provides suggestive evidence to the hypothesis that COVID-19 might accelerate the decline of these occupations and the overall structural transformation of the economy. This negative relationship between the share of workers in the package and past employment growth is stronger within some industries. Appendix Figure 4 shows the relationship between the share of furloughed workers and past employment growth at the 3-digit occupation group and industry level.[13] There is a clear negative relationship between the share of furloughed workers and past employment growth in the construction, manufacturing, retail, services and transportation sectors. On the other hand, occupations such as manual food workers, renovation and other manual workers, teaching professional, care and protective service workers—that were growing prior to COVID-19—were also the most directly exposed to the shock, as restaurants and schooling and care activities were heavily restricted during the lockdown.

Finally, we match individuals with information about their highest-completed education at the end of 2019 through the education register (UDDA) using the unique individual identifier. The coverage is close to full, with the exception of some foreigners and a few individuals with missing information. The UDDA dataset contains a detailed education code that can be divided in two components: the level of education (basic, vocational and short-term college; and university level) and the field of education (divided in 10 categories). The field is not available by definition for individuals with basic education.

Table 4 shows the number of FTEs by education level and firm size or industry, employment growth in 2010-2018, the relative size in the overall economy, the relative size covered by the package, and the index of representation in the package described above. As expected, workers with basic education are over-represented in the package, as they are 36.4% of the individuals covered by the package, but only 23.6% of workers in the economy. Workers with basic education are over-represented in the package across firm size and across sector, and in particular in the finance sector. Only university graduates in the construction sector are slightly over-represented in the package—being 1.1 times

[13] In both Appendix figures 4 and 5 we do not show results for the health and education industry, available upon request.

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more likely to be furloughed, although overall university graduates are under-represented. Workers with a vocational or a short-term degree tend not to be over-represented in the package, aside from the business service sector.

The relative employment share of individuals with basic education has been declining between 2010 and 2018 by almost 14%, as new generations entering the labor market have typically chosen higher levels of education. This evolution was even more dramatic in large firms, where the relative share of basic education workers has been declining by 19.2% over the period. On the other hand, the share of FTEs with a university degree has increased by 42.6% over the same period. Only the culture and leisure, hospitality, construction and overall services sectors increased the number of FTEs with basic education over the period 2010-2018. The growth rate was instead negative in all other industries. Conversely, all sectors increased the number of FTEs with university education over the period 2010-2018, notably the hospitality industry more than tripled it while also increasing employment of vocational and short-term college graduates and workers with basic education.

Table 5 focuses on workers with a vocational, short-term, or university degree. It shows the number of FTEs by education level and field of study, employment growth in 2010-2018, the relative size in the overall economy, the relative size covered by the package and the index of representation in the package. While workers with a post-high school degree tend not to be over-represented in the package when we slice the data by firm size and industry, Table 5 shows that workers with degrees in arts and humanities, services and—to a lesser extent—business, administration and law are between 1.3 and 2.2 times more likely to be on the package. The share of FTEs with a university degree has increased in all fields in the past decade, although some fields, like ICT and education, substantially more than others. For individuals with vocational or short-term college, a few fields are growing (natural sciences, ICT and services), while most are declining.

Appendix Figure 5 shows the relationship between the share of furloughed workers and past employment growth at the education group (level and field) and industry level. The negative relationship between the share of furloughed workers and past employment growth is clearer for some sectors, particularly manufacturing, services and business services, ICT, and retail.

The overall analysis in this section points to an increasing demand of skills in the economy during the past decade, and to a decline of employment of low-skilled workers in most industries and firm size. For all sectors, some occupations declined more than others, with particularly high declines for lower level occupations such associate professionals, clerks, and some manufacturing and service sector workers. While some growing occupations and sectors were over-represented on the package due to the direct impact of

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Table 4: Distribution of education levels in the compensation package, by firm size and industry.

	Basic					Vocational/Short-term college					University				
	Total FTE (2018)	Growth 2008 -2018	Share in 2018	Share in the package	Index of representation	Total FTE (2018)	Growth 2008 -2018	Share in 2018	Share in the package	Index of representation	Total FTE (2018)	Growth 2008 -2018	Share in 2018	Share in the package	Index of representation
All	480,268	-13.8%	23.6%	36.4%	1.54	1,212,575	-2.2%	60.0%	54.6%	0.91	339,738	42.6%	16.7%	9.0%	0.54
By firm size															
0-10 workers	90,370	-11.6%	31.2%	37.8%	1.21	165,628	-2.0%	57.2%	53.4%	0.93	33,344	49.9%	11.5%	8.7%	0.76
11-100 workers	131,742	-2.7%	26.6%	34.1%	1.28	292,119	7.9%	59.0%	56.1%	0.95	71,123	48.5%	14.4%	9.9%	0.69
100+ workers	258,156	-19.2%	20.7%	37.0%	1.79	754,828	-5.6%	60.5%	54.8%	0.91	235,270	39.9%	18.8%	8.2%	0.44
By Industry															
Business Services	16,835	-2.0%	13.5%	22.3%	1.65	58,279	12.9%	46.9%	50.9%	1.08	49,185	43.2%	39.6%	26.8%	0.68
Construction	34,064	10.0%	28.0%	32.2%	1.15	85,424	16.7%	69.7%	65.2%	0.93	2,821	121.8%	2.3%	2.5%	1.09
Culture/Leisure	7,908	6.4%	35.5%	44.7%	1.26	9,210	7.8%	41.4%	42.3%	1.02	5,153	55.4%	23.2%	13.0%	0.56
Finance	10,699	-23.2%	14.9%	31.4%	2.11	42,392	-17.1%	59.0%	50.4%	0.85	18,703	45.5%	26.1%	18.2%	0.70
Health/Education	100,257	-31.2%	15.1%	24.9%	1.65	442,950	-7.6%	66.7%	59.1%	0.89	120,840	31.3%	18.2%	16.1%	0.88
Hospitality	25,978	33.6%	52.9%	53.3%	1.01	19,952	42.6%	40.1%	40.4%	1.01	3,164	253.5%	6.4%	6.2%	0.97
ICT	17,898	-11.1%	20.7%	33.9%	1.64	39,844	-4.7%	46.0%	41.8%	0.91	28,887	53.5%	33.3%	24.3%	0.73
Manufacturing	61,733	-14.9%	25.1%	28.5%	1.13	152,948	-25.5%	62.3%	63.8%	1.02	30,805	57.8%	12.5%	7.7%	0.62
Real Estate	6,931	-11.7%	24.9%	29.8%	1.20	18,107	8.7%	65.1%	62.9%	0.97	2.783	43.0%	10.0%	7.3%	0.73
Retail	92,329	-9.3%	32.2%	34.0%	1.06	170,363	-2.1%	59.3%	60.5%	1.02	24.402	70.5%	8.5%	5.5%	0.65
Services	38,506	3.1%	33.2%	37.9%	1.14	58,455	21.3%	50.5%	52.3%	1.04	18,832	39.6%	13.3%	9.9%	0.74
Transportation	39,090	-18.9%	40.0%	41.8%	1.04	52,105	-6.9%	53.3%	52.1%	0.78	6,551	41.4%	6.7%	6.0%	0.89

Source: LONKOMP, BFL. Population of workers aged between 18 and 65 years old. Notes: Distribution of workers across type of firms by educations level. Education levels: Basic includes high school graduates and below, Vocational/short-term college includes all post-high school vocational education and short college programs, University includes everything above a bachelor. Variables by column, by education level: (1) total number of FTEs in the economy (snapshot Dec. 2018), (2) employment growth between 2010 and 2018, (3) share of FTEs (snapshot Dec. 2018), (4) share of furloughed FTEs, (5) index of representation of the occupation in the package calculated as (4)/(3). ICT stands for Information and Communication Technology.

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Table 5: Distribution of field of education in the compensation package, by education level.

	Vocational/Short-term college					University				
	Total FTE (2018)	Growth 2010 -2018	Share in 2018	Share in the package	Index of representation	Total FTE (2018)	Growth 2010 -2018	Share in 2018	Share in the package	Index of representation
All	1,182,146	-2.2%	-	-	-	320,942	37.0%	-	-	-
By Field of Education										
Agriculture, forestry and fishing	32,983	4.8%	2.8%	1.5%	0.54	7,156	7.4%	2.2%	1.0%	0.45
Arts and humanities	30,807	-1.8%	2.6%	5.6%	2.15	52,322	14.5%	16.3%	24.5%	1.50
Business, administration and law	319,304	-12.4%	27.0%	36.3%	1.34	79,922	40.6%	24.9%	25.2%	1.01
Education	66,545	-16.1%	5.6%	1.3%	0.23	10,438	105.8%	3.3%	2.3%	0.70
Engineering, manufacturing and construction	350,957	-6.4%	29.7%	29.0%	0.98	39,004	25.4%	12.2%	10.3%	0.84
Health and welfare	276,250	-1.6%	23.4%	10.4%	0.44	32,383	31.6%	10.1%	10.9%	1.08
Information and communication technology	16,428	9.1%	1.4%	0.9%	0.64	15.098	118.7%	4.7%	4.8%	1.02
Natural sciences	706	34.6%	0.1%	0.0%	0.00	29,306	13.9%	9.1%	4.8%	0.53
Services	79,287	7.2%	6.7%	14.5%	2.16	3,041	12.2%	0.9%	1.6%	1.78
Social sciences and journalism	8,879	-20.1%	0.8%	0.5%	0.62	52,273	42.7%	16.3%	14.6%	0.90

Education level

Source: LONKOMP, BFL. Population of workers aged between 18 and 65 years old. Notes: Distribution of workers across fields of education by educations level. Education levels: Vocational/short-term college includes all post-high school vocational education and short college programs, University includes everything above a bachelor. Variables by column, by education level: (1) total number of FTEs in the economy (snapshot Dec. 2018), (2) employment growth between 2010 and 2018, (3) share of FTEs (snapshot Dec. 2018), (4) share of furloughed FTEs, (5) index of representation of the occupation in the package calculated as (4)/(3).

the lockdown measures, for the declining education and occupation groups the COVID-19 shock hit the hardest and workers belonging to these groups were more likely to be furloughed during Spring 2020.

5 Empirical analysis

5.1 Empirical strategy

Section 4 documents that individuals who were most exposed to labor market risk because of the COVID-19 shock and subsequent lockdown were also those in education or occupation subgroups that were already in relative decline prior to the shock. In this section, we formally test the relationship between COVID-19 exposure and pre-COVID-19 employment trend by performing a simple regression analysis. We define COVID-19 exposure as the share of workers covered by the package, calculated as the ratio of all workers in the package over all workers in the population (total employment observed at the end of 2018, population of workers aged between 18 and 65 years old). We calculate this share at two levels: for a first exercise we calculate it at the 3-digit occupational code (o) and industry level (j), for a second exercise at the education (l), field (f) and industry (j) level. We then define pre-COVID-19 employment trend as the growth rate in FTEs within each of these cells over the period 2010-2018. We only keep cells with at least 100 individuals in BFL in 2010 and 2018, and with at least 10 individuals in LONKOMP. As discussed in Section 4.2, we remove the health and education industry from the regression analysis.[14]

In the first exercise, we test the relationship between COVID-19 exposure and pre-COVID-19 employment trends at the occupation-industry level. We use the following specification:

$$Sh_{oj}^{LK} = \alpha_0 + \alpha_1 \Delta n_{oj} + \alpha_2 X_o + \nu_j$$

where Sh_{oj}^{LK} is the share of workers covered by the package in occupation group o and industry j, Δn_{oj} is the growth of occupation group o in industry j over the period 2010-2018, X_o is a vector of control variables for the skill level of occupation o; and ν_j is a vector of industry dummies. We run three different specifications: one including only our variable of interest Δn_{oj} and industry fixed effects, a second and a third one adding broad occupation fixed effects using either ILO skill levels (column 2) or 1-digit occupa-

[14] All results including health and education are provided in the online appendix. They are qualitatively similar.

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tion codes (column 3), respectively.[15] In all specifications, we run weighted regressions using employment weights of each cell in 2018, to account for the unequal distribution of employment across cells.

In the second exercise, we test the relationship between COVID-19 exposure and pre-COVID-19 employment trends at the education-industry level. We use the following specification:

$$Sh_{lfj}^{LK} = \alpha_0 + \alpha_1 \Delta n_{lfj} + \alpha_2 X_{ej} + \nu_j$$

where Sh_{lfj}^{LK} is the share of workers covered by the package in education level group l, field f and industry j, Δn_{lfj} is the growth of education level group l and field f in industry j over the period 2010-2018, X_{ej} is a vector of dummy variables for the field or the level of education within industry j; and ν_j is a vector of industry dummies. We run five different specifications: one including only our variable of interest Δn_{oj} and industry fixed effects, a second and third one including controls for the field of education, and a fourth and fifth one including controls for the level of education. Because information on education fields are not available for workers with basic education, we drop the cells for basic education in the third and fifth specifications. Controlling for field and industry, we exploit the variation between levels within cells, while we explore variation within fields when controlling for level and industry. Again, in all specifications, we run weighted regressions using employment weights of each cell in 2018, to account for the unequal distribution of employment across cells.

5.2 Results

We show the results of the two empirical exercises in Table 6 for occupational groups and Table 7 for education groups.

Starting with the analysis at the occupation-industry level, the first column of Table 6 shows the coefficients with only our variable of interest—past growth within a cell—and industry dummies. The coefficient is negative and significant, meaning that occupational groups that were declining within industries were also more likely to be included in the package. The coefficients of the industry dummies reflect the fact that the nature of work

[15]The ILO skill level is defined as a function of the complexity and range of tasks and duties to be performed in an occupation. The four skill levels require the performance of: 1. simple and routine physical or manual tasks, 2. more complex manual, technical and practical tasks, 3. complex technical and practical tasks which require an extensive body of factual, technical and procedural knowledge in a specialised field, 4. tasks which require complex problem solving and decision making based on an extensive body of theoretical and factual knowledge in a specialised field. They map into 4 levels of education: 1. primary, 2. secondary, 3. short/medium-term tertiary education, 4. long term tertiary education. For more details see https://www.ilo.org/public/english/bureau/stat/isco/docs/annex1.pdf.

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in some industries make it more likely to be more exposed to the shock, as was already shown in Table 2 at a different level of aggregation.

Columns 2 and 3 of Table 6 confirm that our main result remains unchanged when adding controls for broad occupation groups. This suggests that there is still enough variation across 3-digit occupational groups within ILO skill level group or within 1-digit occupation group and within industry. Furthermore, columns 2 and 3 confirm what we found in section 4.3: The higher the level of skill the lower the risk of furlough (column 2). These workers are more likely to be essential to the firm and able to work from home. Looking at more detailed skill description (column 3), relative to the omitted category (managers), service and sales workers, workers in elementary occupations, craft and related trade workers, clerical support workers and technicians and associate professionals are more likely to be included in the package. Indeed, the tasks performed in these large occupational groups are more difficult to be performed remotely, as they require physical proximity to customers or co-workers.

Turning to analysis at the education-industry level, the coefficient of our variable of interest in the first column of Table 7, with only industry controls, is very similar to the one in Table 6, suggesting that the variations in education fields and levels contain similar information to the case of occupations. This is true despite there being much more variation at the occupational level, as we have many more cells.

Controlling for education field (column 2) does not affect the coefficients. Graduates in STEM fields are overall less likely to be included in the package. As already mentioned, fields are not available for workers with basic education, so we run a similar analysis in column 3 without the groups of basic education by industry. Results remain roughly similar.

However, when we control for education level, in columns 4 and 5 (where we also omit individuals with basic education as in column 3), the coefficient of the within-cell past growth becomes positive although not significant. This suggests that the level of education might be correlated with past growth within cell. The coefficients of our education level dummies also clearly show that, relative to individuals with college degree, individuals with relatively lower levels of education are more likely to be included in the package. As mentioned above, workers with lower levels of education are typically employed in jobs that require physical proximity, as opposed to college graduates who can operate remotely from their home.

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Table 6: COVID-19 exposure on past growth at the 3-digit occupation-by-industry level.

		(1)	(2)	(3)
	Δn_{oj}	-0.034***	-0.032***	-0.025***
		(0.007)	(0.007)	(0.007)
Industry:	Construction	-0.024	-0.053***	-0.049***
		(0.017)	(0.018)	(0.018)
	Culture & leisure	0.296***	0.283***	0.280***
		(0.034)	(0.033)	(0.032)
	Finance	-0.066***	-0.066***	-0.061***
		(0.023)	(0.022)	(0.021)
	Hospitality	0.404***	0.374***	0.345***
		(0.024)	(0.024)	(0.024)
	ICT	-0.020	-0.018	-0.016
		(0.019)	(0.019)	(0.018)
	Manufacturing	0.032**	0.012	0.028*
		(0.015)	(0.015)	(0.015)
	Real estate	0.026	0.001	-0.01
		(0.035)	(0.034)	(0.033)
	Retail	0.095***	0.074***	0.064***
		(0.015)	(0.015)	(0.015)
	Transportation	0.085***	0.062***	0.057***
		(0.018)	(0.018)	(0.017)
	Services	0.073***	0.046**	0.068***
		(0.018)	(0.018)	(0.018)
Skill level:	2 (medium)		0.003	
			(0.013)	
	3 (medium/high)		-0.015	
			(0.015)	
	4 (high)		-0.050***	
			(0.014)	
Occupation:	Professionals			0.015
				(0.017)
	Technicians and and associate professionals			0.047***
				(0.016)
	Clerical support workers			0.052***
				(0.017)
	Service and sales workers			0.117***
				(0.017)
	Skilled agricultural, forestry and fishery workers			-0.073
				(0.066)
	Craft and related trades workers			0.061***
				(0.017)
	Plant and machine operators, and assemblers			0.010
				(0.018)
	Elementary occupations			0.063***
				(0.017)
	constant	0.066***	0.097***	0.031*
		(0.013)	(0.018)	(0.018)
Obs.		480	480	480

Notes: Weighted OLS of the share of workers covered by the package in occupation group o and industry j on the growth of occupation group o in industry j over the period 2010-2018, industry fixed effects (cols. 1-3), ILO skill level fixed effects (omitted: skill level 1, low) (col. 2), and 1-digit level occupation fixed effects (omitted: managers) (col. 3). Health and education industries excluded from the analysis.

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Table 7: COVID-19 exposure on past growth at the education-by-industry level.

		(1)	(2)	(3)	(4)	(5)
	Δn_{oj}	-0.038***	-0.038***	-0.041***	0.018	0.016
		(0.010)	(0.010)	(0.010)	(0.013)	(0.013)
Industry:	Construction	-0.038***	-0.035***	-0.022*	-0.062***	-0.058***
		(0.012)	(0.011)	(0.012)	(0.012)	(0.014)
	Culture & Leisure	0.341***	0.325***	0.262***	0.328***	0.276***
		(0.022)	(0.019)	(0.023)	(0.021)	(0.026)
	Finance	-0.062***	-0.074***	-0.073***	-0.062***	-0.058***
		(0.015)	(0.013)	(0.014)	(0.014)	(0.015)
	Hospitality	0.466***	0.435***	0.407***	0.429***	0.433***
		(0.017)	(0.015)	(0.021)	(0.016)	(0.023)
	ICT	-0.008	-0.014	-0.017	-0.01	-0.011
		(0.013)	(0.012)	(0.013)	(0.012)	(0.014)
	Manufacturing	0.035***	0.034***	0.040***	0.027***	0.030***
		(0.011)	(0.009)	(0.010)	(0.010)	(0.011)
	Real estate	0.031	0.022	0.022	0.016	0.016
		(0.020)	(0.017)	(0.019)	(0.019)	(0.021)
	Retail	0.105***	0.095***	0.098***	0.092***	0.096***
		(0.010)	(0.009)	(0.010)	(0.010)	(0.011)
	Transportation	0.076***	0.060***	0.062***	0.066***	0.071***
		(0.013)	(0.012)	(0.013)	(0.012)	(0.015)
	Services	0.076***	0.062***	0.055***	0.061***	0.059***
		(0.012)	(0.011)	(0.012)	(0.012)	(0.014)
Education field:	Business		-0.011	-		
			(0.007)			
	Education		0.007	0.026		
			(0.035)	(0.034)		
	Arts		0.013	0.030**		
			(0.013)	(0.012)		
	Social sciences		-0.035*	-0.019		
			(0.018)	(0.017)		
	Sciences		-0.080***	-0.069***		
			(0.022)	(0.021)		
	ICT		-0.016	0		
			(0.021)	(0.020)		
	Engineering		-0.037***	-0.030***		
			(0.007)	(0.007)		
	Agriculture		-0.071***	-0.059***		
			(0.018)	(0.017)		
	Health		0.006	0.019		
			(0.015)	(0.015)		
	Services		0.062***	0.080***		
			(0.013)	(0.013)		
Education level:	Basic				0.072***	-
					(0.011)	
	Vocational				0.061***	0.058***
					(0.011)	(0.011)
	Short/Medium				0.037***	0.035***
					(0.010)	(0.010)
	constant	0.071***	0.091***	0.080***	0.025**	0.026**
		(0.009)	(0.009)	(0.009)	(0.011)	(0.011)
Obs.		278	278	267	278	267

Notes: Weighted OLS of the share of workers covered by the package in education level group l, field f and industry j on the growth of education level group l and field f in industry j over the period 2010-2018, industry fixed effects (cols. 1-5), education field fixed effects (omitted: basic education in col 2) (col. 2 and 3 without basic education), and education level fixed effects (basic, vocational, short/medium length, omitted: bachelor and above) (col. 4 and 5 without basic education). Health and education industries excluded from the analysis.

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6 Conclusions

In this paper, we use recently released data from Danmark Statistik covering the population of individuals and firms receiving emergency government support for the economic consequences of the lockdown provoked by COVID-19 between March and July 2020. We show evidence that a subset of relatively low-skill occupational and education groups that were on the decline before the crisis were dramatically hit by this shock. To the extent to which the patterns of furloughed workers during the first wave of COVID-19 anticipate the structural changes that will occur in the medium- and long-run, these patterns uncovered here give us an indication of which occupations' decline will be accelerated by this crisis.

Our analysis is preliminary in several dimensions. First, while we document that some declining occupational and education groups are more exposed to the COVID-19 shock, we do not look more in depth at the characteristics of these groups. In future work, we would like to elaborate on the relationship between jobs and tasks. Occupations can be linked to specific task dimensions such as the importance of physical proximity or cognitive ability. The COVID-19 shock promises to shift the importance of these dimensions in many occupations, as more employers and employees are forced to use remote work tools and, in general, learn new technologies related to the new reality of work.

Second, we also make some strong assumptions about the way current workers can be related to their past characteristics, especially in terms of their occupation. Future data releases—particularly of the matched employer-employee and the employment registers—will allow us to get an even clearer picture of the transformation of the Danish economy and the effect of COVID-19 on the re-allocation pattern that we document.

Third, we analyse the short-run reallocation process after the COVID-19 shock using data from a rescue package. This means that we miss adhesion to all alternative policy packages that would give us an even clearer idea of the firms and sectors hit. Furthermore, it is possible that such a government policy shielding workers and firms in the short run can also affect the reallocation process.

Finally, we argue that the COVID-19 shock might accelerate the reallocation process already undergoing in the economy. On the one hand, reallocation is associated with increased inequality and polarization of the labor market. On the other hand, it is also the manifestation of a natural process of skill upgrading and structural transformation of the economy. Providing more resources, e.g. through retraining, for a smooth transition therefore naturally appears as a welfare improving policy in the medium run. The Danish government has recognized this reality in its response to the crisis and included several

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retraining initiatives in the emergency measures taken since March.

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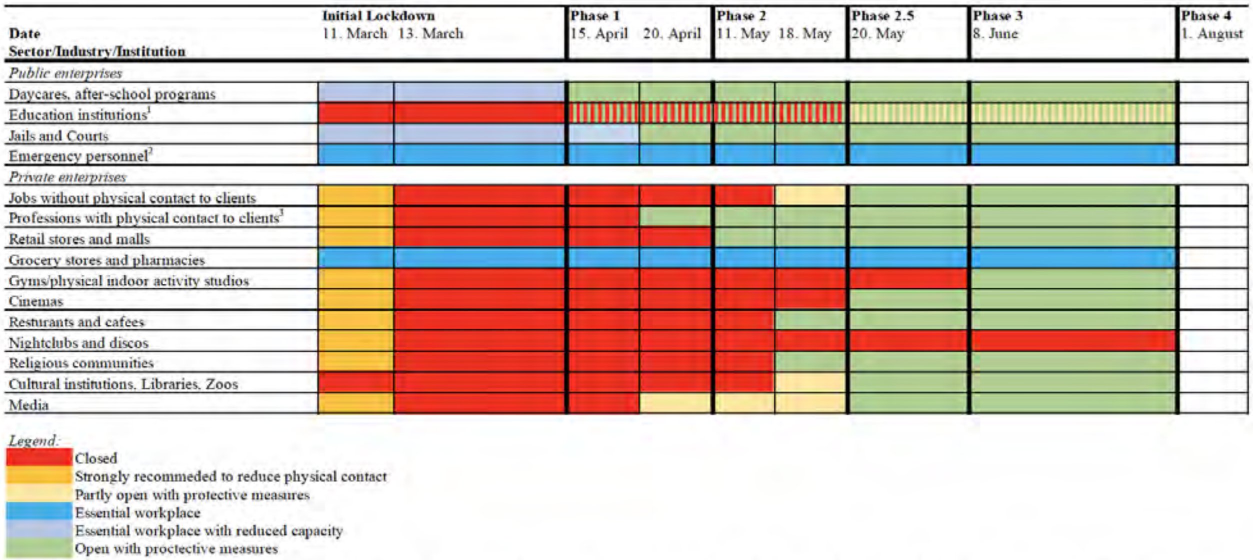
A Appendix Figures

Figure 1: Timeline of the lockdown and re-opening of economic activities in Denmark



Notes: [1]Primary school for grades 0-5 and the last year of high school re-opened with protective measures on April 15. Primary school for grades 6-10 re-opened on May 18 and the remaining years of high school re-opened on May 20. Labs and research centers (also within universities) partly re-opened on April 20 while Universities partly reopened on May 20.
[2]Includes health sector, social workers, elderly care, food control, police.
[3]Includes hair dressers, tattoo artists, beauticians.

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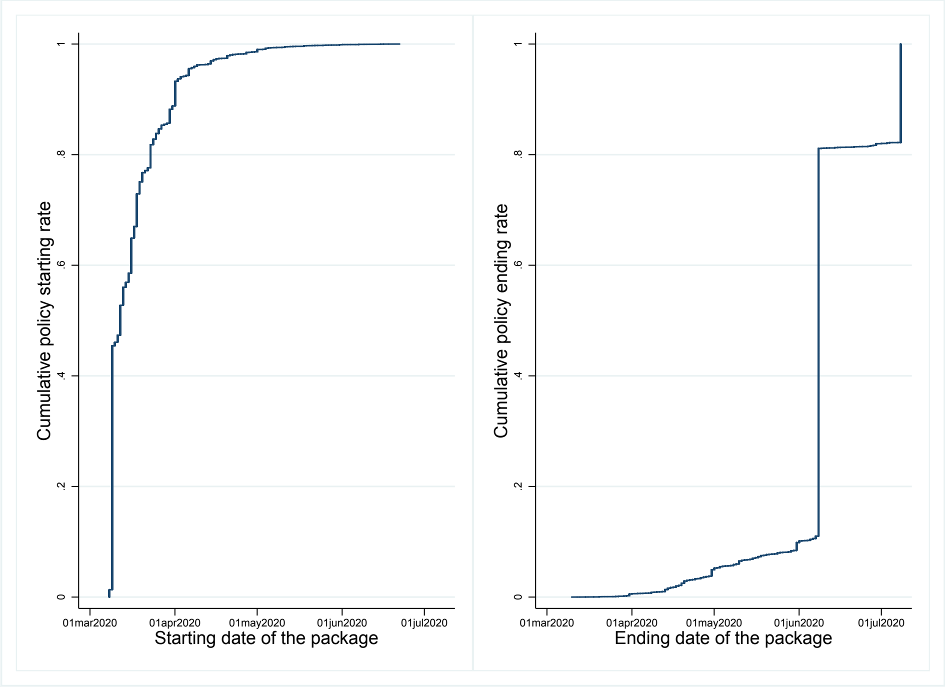
Figure 2: Starting date (left panel) and ending date (right panel) of the compensation package



Notes: The left hand side figure shows the cumulative share of all firms in the LONKOMP dataset that start being covered by the package. The right hand side figure shows the cumulative share of all firms in the LONKOMP dataset that stop being covered by the package.

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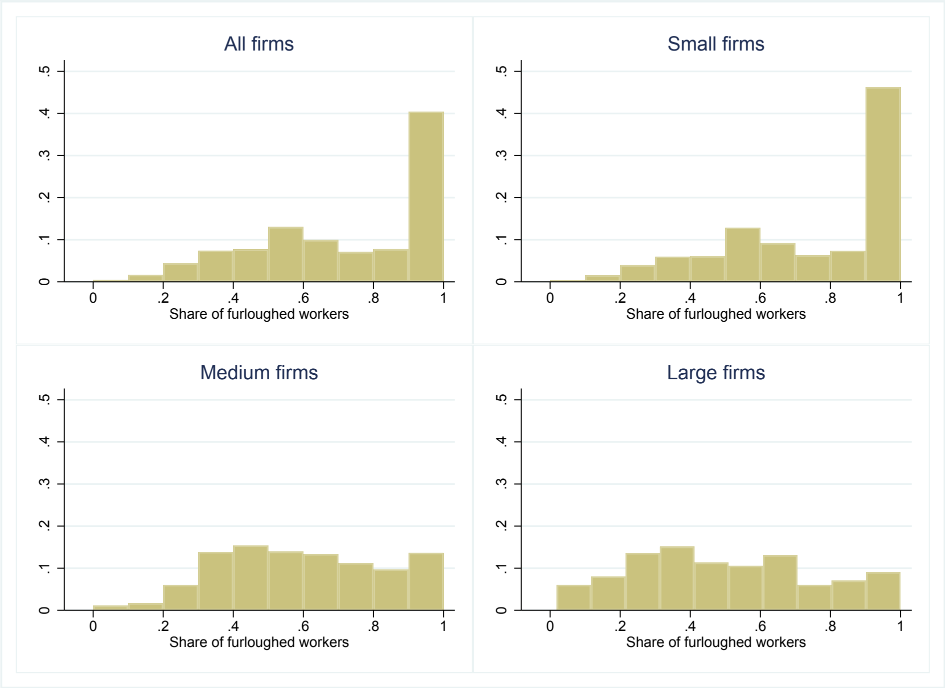
Figure 3: Distribution of the firm-level share of furloughed workers



Notes: The firm-level furloughed FTE share is as defined in Table 1 and is only reported for firms with no missing firm size when split by firm size (28,080 firms out of 30,489).

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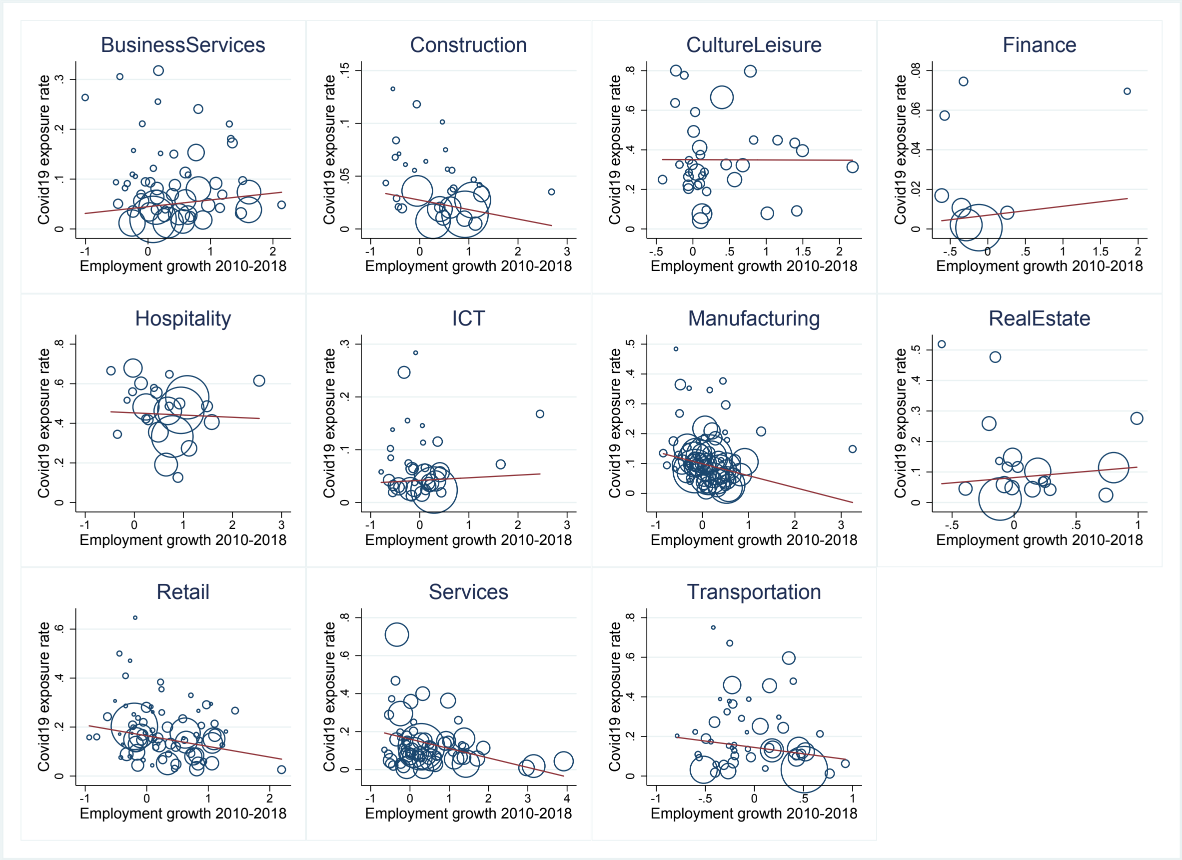
Figure 4: Relationship between COVID-19 exposure and past employment growth by 3-digit occupational group and industry



Notes: COVID-19 exposure defined as the within-cell share of workers covered by the package, calculated as the ratio of all workers in the package over all workers in the population (total employment observed at the end of 2018). Pre-COVID-19 employment trend defined as the within-cell growth rate in FTEs over the period 2010-2018. A cell is a 3-digit occupation within an industry. The size of the bubbles represents the size of each cell. Lfit performed with weights using FTEs in 2018 in BFL. We restrict cell size to min 100 FTEs for 2010 and 2018 in BFL and min 10 FTEs in LONKOMP. We use the same sample definition as described in the text. Health and education industries omitted in the figure.

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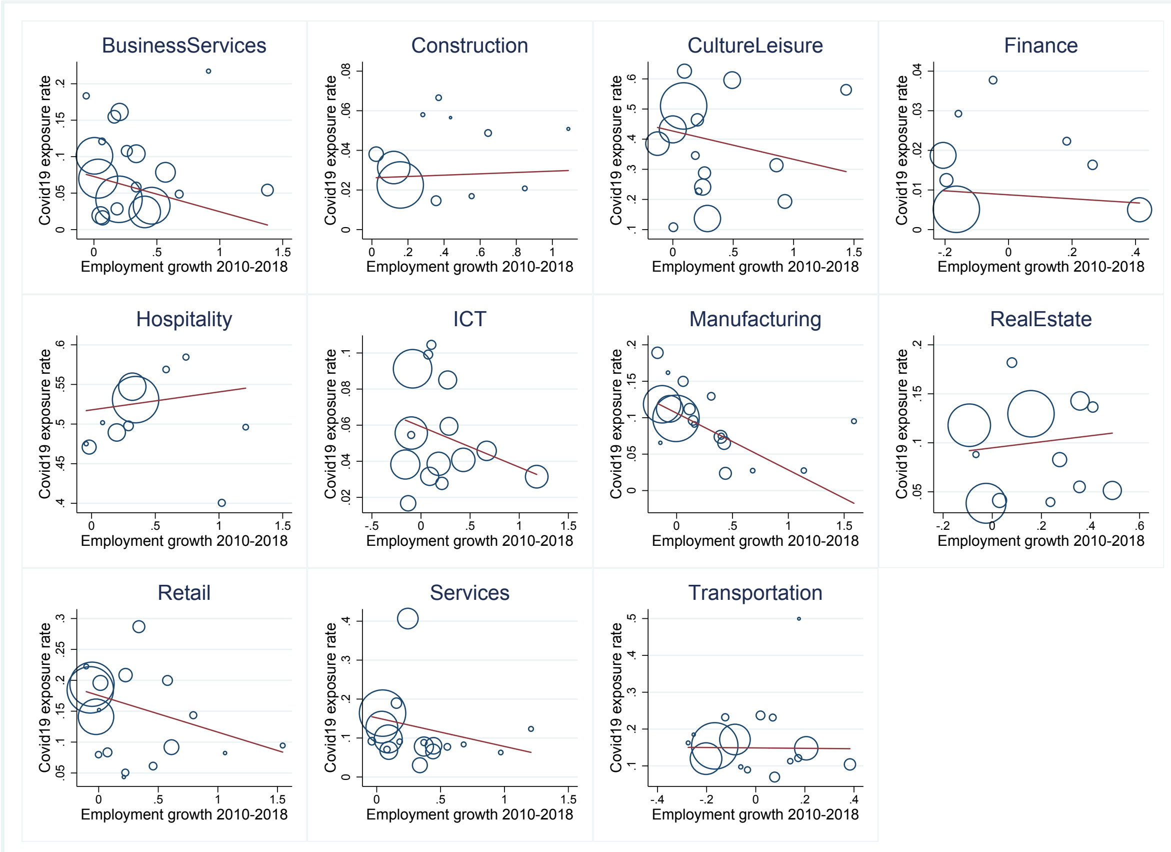
Figure 5: Relationship between COVID-19 exposure and past employment growth by education group and industry



Notes: COVID-19 exposure defined as the within-cell share of workers covered by the package, calculated as the ratio of all workers in the package over all workers in the population (total employment observed at the end of 2018). Pre-COVID-19 employment trend defined as the within-cell growth rate in FTEs over the period 2010-2018. A cell is an education level-field within an industry. Education levels and fields as defined in footnote ??. The size of the bubbles represents the size of each cell. Lfit performed with weights using FTEs in 2018 in BFL. We restrict cell size to min 100 FTEs for 2010 and 2018 in BFL and min 10 FTEs in LONKOMP. We use the same sample definition as described in the text. Health and education industries omitted in the figure.

Capital Markets, COVID-19 and Policy Measures[1]

Khalid ElFayoumi[2] and Martina Hengge[3]

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The COVID-19 pandemic and associated policy responses triggered a historically large wave of capital reallocation between markets, asset classes, and industries. Using high-frequency country-level data, we examine if and how the number of infections, the stringency of the lockdown, and the fiscal and monetary policy response determined the dynamics of portfolio flows, market-implied sovereign risk, and stock prices. We find that these factors played an important role, particularly for emerging markets. Our results indicate that domestic infections had an initial negative impact on flows. Cumulatively, however, the effect was positive and reflected increased demand for financing by affected economies. We also find that both lockdown and fiscal measures supported portfolio flows, driven by an increased supply of funds. Bonds, not equities, were the primary driver of portfolio flows, highlighting a pattern of reallocation to safety. Finally, we show that monetary policy loosening in developed markets led to a cumulative decline in flows, as investors searched for higher yield.

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1 The views expressed herein are those of the authors and should not be attributed to the IMF, its Executive Board, or its management. We thank Klakow Akepanidtaworn, Cian Allen, Katharina Bergant, Şebnem Kalemli-Özcan, Miguel Segoviano, Can Sever, and Cédric Tille for their comments and suggestions. All errors are our own.

2 Economist, International Monetary Fund.

3 Economist, International Monetary Fund.

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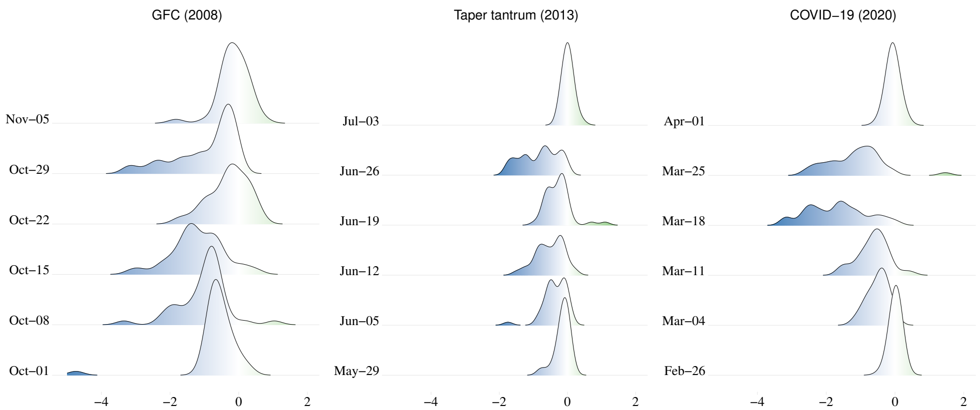
1 Introduction

Compounding the global recession triggered by the COVID-19 pandemic, the worldwide health emergency also spurred a dramatic response in international capital markets. As investors gauged the economic consequences of the pandemic and the subsequent policy responses, they initiated a wave of capital reallocation between markets and asset classes. Using high-frequency weekly data of country-level portfolio flows, figure 1 highlights two notable facts about this wave. First, the global shift in international portfolio flows was of historically large magnitude. Second, countries' experience at the height of the episode differed widely.

This heterogeneity suggests that domestic *pull* factors played an important role, standing in contrast to earlier risk-off events such as the global financial crisis and the taper tantrum. During these past events, the impact on portfolio flows was less dispersed across countries and largely determined by global *push* factors (among others, Avdjiev et al., 2020; Fratzscher, 2012; Rey, 2015). The question arises as to whether individual countries' handling of the crisis contributed to shaping the differing dynamics of portfolio flows.

Figure 1: Densities of portfolio flows (percent of allocation)



Notes: The horizontal axis shows total portfolio flows for all countries in our sample scaled by the allocation in the respective week.

In this paper, we address this question by evaluating international capital markets' response to countries' success, or lack thereof, in containing the pandemic and the key policy measures that governments enacted to limit the toll on public health and the economy. More specifically, we examine if and how the number of infections, the stringency of the lockdown, and the fiscal and monetary policy response determined the magnitude of portfolio flows, market-implied sovereign risk, and stock prices during the COVID-19 pandemic. To the best of our knowledge, this work is the first empirical contribution on the drivers of capital flows during the COVID-19 pandemic and their impact on sovereign financial stress and domestic asset prices.

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A priori, the effect of these policies on capital markets is ambiguous. On the one hand, stringent lockdown measures are detrimental to economic activity. Aggressive fiscal spending and monetary policy actions eliminate policy buffers and may raise concerns about the sustainability of sovereign debt and the stability of inflation and the exchange rate, especially in emerging markets. On the other hand, these policies also signal a less prolonged recession and therefore higher returns in the medium term.

Our empirical strategy relies on a *local projection* approach to estimate both the contemporaneous and the dynamic response of capital markets to the severity of the pandemic and key policy measures. We collect a high-frequency dataset of portfolio flows, spreads of sovereign credit default swaps (CDS), and stock prices for 38 countries over the first week of January 2014 to the second week of May 2020.[1]

Combined, information about the quantity (flows) and price (CDS spreads and stock prices) of assets provides an identification of the supply and demand channels of portfolio flows. To the extent that these two measures are tightly linked by market forces, shifts in supply are associated with changes in flows and spreads in opposite directions, whereas shifts in demand move both variables in the same direction.[2] In addition, we address four sources of heterogeneity based on market development: emerging markets (EMs) vs. developed markets (DMs), asset class (bond vs. equity flows), investors' domicile (foreign vs. domestic) and industries.

Overall, our findings demonstrate that capital market dynamics were not exclusively driven by undiscriminating global factors. Instead, the severity of the pandemic at the domestic level and governments' policy responses played an important role in explaining the heterogeneity of portfolio flows, sovereign risk, and stock prices across countries during the COVID episode, particularly for EMs. This finding is consistent with Kalemli-Özcan (2019) who shows that EM capital flows are particularly sensitive to country-specific risk sentiment, which is, in turn, determined by countries' policy credibility. With the exception of monetary policy, we do not find evidence that these domestic factors had a sizeable effect on the dynamics of portfolio flows in DMs. Three takeaways are particularly salient.

First, we find that the domestic spread of the virus led to a cumulative increase in total net flows[3] in EMs, despite an initial negative effect. This increase was associated with a reallocation towards safety as equity holdings declined and net bond flows rose. Sovereign CDS spreads increased, suggesting that the increase in net portfolio flows was driven by demand for liquidity, potentially reflecting widening financing needs to mitigate the fallout from the pandemic.

Second, governments' efforts to enact lockdown measures and provide fiscal stimulus were suc-

[1]As we explain in section 3, our identification strategy makes use of the high frequency of the data and the efficiency by which investors' portfolios adjust to new market information. To account for omitted variables concerns, we include a set of dynamic controls and fixed effects. The latter account for all weekly variations at the global level (e.g. global risk) and monthly variations at the country level (e.g. capital controls, U.S. credit swap lines, quality of institutions, etc.).

[2]This intuition is in line with Gilchrist and Zakrajšek (2012), who study the relative contribution of credit supply and demand factors in corporate bonds.

[3]Total net flows are defined as the net value of purchases and redemptions of bond and equity funds. Section 2 provides further details.

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cessful in supporting higher portfolio flows to the domestic economy. Unlike for COVID infection shocks, larger net portfolio flows in response to lockdown and fiscal stimulus measures, together with the negative response of sovereign CDS spreads, suggest that the impact of these measures was driven by an increased supply of financing, amidst stronger global demand for safer investment opportunities.

Third, we find that loosening policy rates in DMs, relative to the United States, triggered an increase upon impact in net portfolio flows and a decline in CDS spreads; that is, central bank actions provided reassurance to markets, thereby stimulating flows to the domestic economy. Over the horizon of a month, however, we find that monetary policy cuts led to a decline in net portfolio flows, as expected by the interest rate parity and search for yield channel. This finding concurs with that in Fratzscher et al. (2018) who report a similar pattern in response to U.S. quantitative easing policies in the aftermath of the financial crisis. In contrast, we do not find evidence that monetary policy actions in EMs played a role in the dynamics of portfolio flows. We interpret this finding in light of the *dilemma* hypothesis (Rey, 2015), whereby U.S. financial dominance renders EM monetary policy ineffective against market fluctuations.

This paper contributes to several strands of literature. We add to a large body of research on the drivers of cross-border capital flows. Seminal papers by Forbes and Warnock (2011), Milesi-Ferretti et al. (2011), Fratzscher (2012), Sarno et al. (2016), Davis et al. (2019), among others, established that push factors, reflecting global conditions, are important determinants of capital flows. Fratzscher (2012) finds that the effect of these push factors was highly heterogeneous across countries during the 2007-2009 global financial crisis and the subsequent recovery. Similarly, Davis et al. (2019) show that capital flows respond differently to global swings in risk perception or risk aversion across countries. The COVID-19 pandemic is widely perceived as a global risk-off episode that drove developments in international capital markets. In this paper, we explain the observed heterogeneity of capital flows across countries by demonstrating that exposure to the pandemic and policy actions mattered for cushioning the shock.

Our paper also relates to an emergent literature on the COVID-19 pandemic and its effect on financial markets. Alfaro et al. (2020), for example, show that the severity of the pandemic predicts equity market returns in the U.S. An increase in infections is associated with large and negative swings in equity prices. Likewise, a study by Esteves and Sussman (2020) demonstrates that greater exposure to the pandemic penalizes EMs through an increase in sovereign bond spreads whereas OECD economies are not affected. Our analysis complements these studies by investigating the effect of the pandemic and key policy measures on portfolio flows and financial stress, as measured by CDS spreads and stock returns. The remainder of this paper is organized as follows. In section 2, we describe the data and present a set of stylized facts about portfolio flows and policy measures during the COVID-19 pandemic. Section 3 lays out the empirical strategy while section 4 presents our results. Section 5 summarizes our conclusions.

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2 Portfolio flows, the pandemic, and policy measures

2.1 Portfolio flows

Our analysis uses data on weekly portfolio flows from EPFR Global. Flows are measured as the US$ net value of purchases and redemptions into investment funds, abstracting from valuation changes due to asset returns and exchange rate movements.[4] Most of the funds covered by EPFR are domiciled in DMs. Net injections into funds thus largely reflect international portfolio decisions of DM investors in EMs and other DMs.[5] The literature has shown that EPFR flows provide a fairly close match with BoP data and are thus suitable for a representative analysis of international portfolio flows (see Fratzscher et al., 2018; Jotikasthira et al., 2012; Miao and Pant, 2012). Likewise, Koepke and Paetzold (forthcoming) find that EPFR flows have significant predictive power for BoP portfolio flows.

We opt to use EPFR portfolio flows in our analysis due to their high frequency, allowing us to identify the effects of the pandemic and other policy measures on portfolio flows. Another advantage of the data is their granularity—with information on the asset class, domicile, and industry—and the broad geographic coverage: our estimation sample includes 21 DMs and 17 EMs over January 2014 to May 2020.[6,7] Table A.1 in appendix A provides an overview of the market classification for each country in our sample.[8]

We document three stylized facts on net portfolio flows during the pandemic. First, portfolio outflows were of a historically large magnitude. Figure 2 highlights the dynamics of cumulative net portfolio flows to both DMs and EMs during three episodes of capital outflows—the COVID-19 pandemic, the taper tantrum, and the global financial crisis. The pandemic has led to outflows that markedly exceeded those experienced during earlier episodes, with particularly large bond outflows. While net portfolio flows reversed sharply even in DMs, they started recovering less than two months into the pandemic. Net flows to EMs, in contrast, continued to decline for a longer period of time. Notably, however, the the magnitude of portfolio outflows was historically unprecedented when flows are measured in US$ but not necessarily when normalized by the portfolio allocation (figure B.1 in appendix B).

Second, as shown in figure 1 in section 1, the size of portfolio outflows during the COVID

[4]EPFR compiles flows for more than 100,000 investment funds. As of 31 December 2019, assets under management amounted to US$38,250 trillion. Equity funds accounted for 51.8%, bond funds for 32.7%, multi asset class and other funds for 12.7%, and money market funds for 2.8% of EPFR portfolio flows.

[5]See Koepke and Paetzold (forthcoming) for a detailed overview of EPFR data.

[6]While COVID cases, lockdown stringency and the fiscal spending response take the value zero for most of the sample period, we still choose to use the longer sample available. The larger number of observations allows for a better identification of the effects of the COVID-related variables through (i) lowering the estimation uncertainty around the effects of the set of control variables and (ii) enabling a dynamic specification while limiting the Nickell bias. See section 3.

[7]Our sample ends in the second week of May due to a change in the availability of the fiscal spending data which we obtained from the IMF. In the second half of May, the IMF switched from a weekly to biweekly information update on governments' policy measures for some of the countries in our sample.

[8]EPFR offers an even broader country coverage. We keep all DMs and EMs for which we observe both equity and bond flows as well as the relevant controls in the empirical analysis. In addition, we take out Iceland due to low coverage and Argentina due to its large outliers of CDS spreads. Our analysis does not include any frontier markets.

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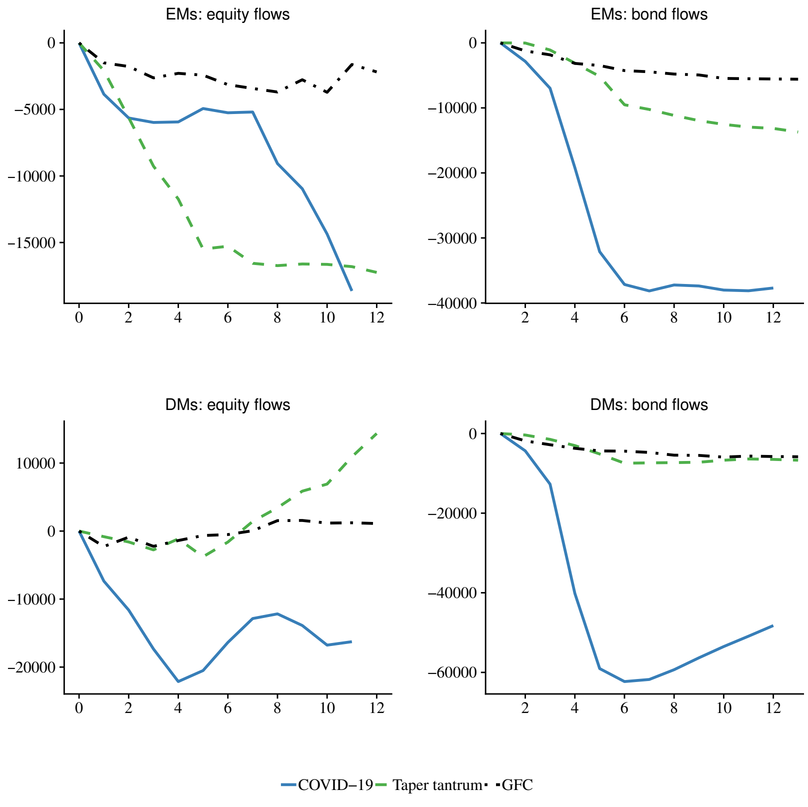
Figure 2: Cumulative equity and bond flows (US$ million)



Notes: x-axis shows weeks following the start date for the following episodes: COVID: 2020/02/26-2020/04/29; taper tantrum: 2013/05/22-2013/08/14; global financial crisis: 2008/10/08-2008/12/31

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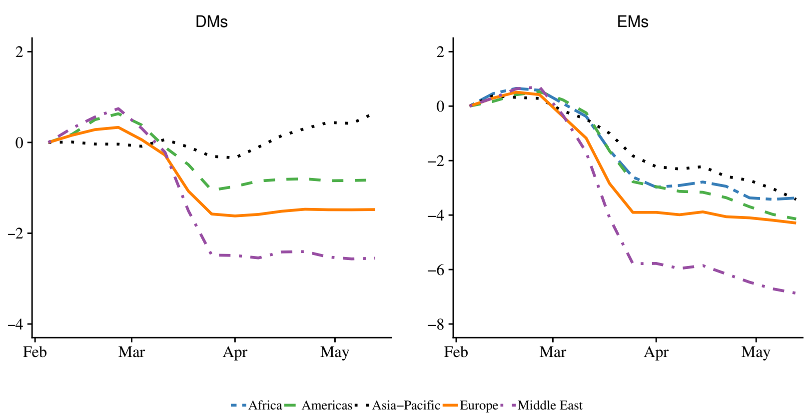
episode was strongly heterogeneous across countries, as reflected in the wide distribution of flows. At the height of the crisis, the standard deviation of net portfolio flows as a share of allocation was 1.1 percent. A comparison with previous capital outflow episodes illustrates that the range of net flows was larger than during the taper tantrum (standard deviation of 0.6 percent) and similar to the global financial crisis (standard deviation of 1.1 percent). *A priori*, this pattern suggests that COVID-related domestic factors might have played an important role in driving investors' portfolio reallocation during the crisis.

Last, zooming in on flows by geographical allocation in figure 3, illustrates that the observed heterogeneity in net portfolio flows across countries and time was aligned with the geographical progression of the COVID pandemic: both DMs and EMs in Asia-Pacific started to experience outflows in mid-February—earlier than other regions. Emerging Africa[9] and Europe followed with net flows declining from the last week of February 2020 onward. Finally, EMs in the Americas and the Middle East as well as other DMs saw their initial outflows by the first week of March—one week before the WHO characterized COVID as a pandemic. With the virus having widely spread across the globe, net flows started to decline more sharply across all regions. Comparing the magnitudes of outflows across regions also highlights that measures to control the pandemic or mitigate the economic fallout might have affected investors' allocation decisions; outflows from Asia-Pacific were lower than outflows from other regions, such as the Americas, where governments' response was perceived less assertive.[10]

Figure 3: Cumulative equity and bond flows (percent of allocation)



2.2 COVID-19 and policy measures

We study if the pandemic and policies implemented by governments around the world played a role in driving portfolio flows. The policy measures analyzed focus on containment measures—

[9]Emerging Africa represents South Africa, the only African country in our sample.
[10]Large outflows from the Middle East might also reflect the impact of the pandemic on the oil price collapse.

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reflected in the stringency of the lockdown—as well as on macroeconomic stabilization trough fiscal and monetary policy. We collected data on COVID cases from Haver Analytics. Information on the stringency of the lockdown was obtained from Hale et al. (2020). This index measures the strictness of lockdown policies based on publicly available information. Data on the fiscal stimulus related to the pandemic are from the IMF's Survey on Country Responses which contains information on discretionary fiscal spending in response to the pandemic. Finally, we collected data on monetary policy rates from Haver Analytics and calculated the policy rate differential to the U.S. Fed Funds rate. In addition, we explore if the pandemic and its effect on portfolio flows also reflected in sovereign CDS spreads and stock prices, using data from Bloomberg.

Figure 4 shows the distribution of COVID cases and policy measures over January to May 2020. There was considerable heterogeneity of COVID cases and lockdown measures across countries and time. The number of infections per capita was more heterogeneous for DMs than EMs, which is partially driven by the end of our sample period when the pandemic was not yet as widespread in EMs. The distribution of lockdown stringency shows that the observations are clustered at the lower and upper end, suggesting that policymakers did not see much middle ground for containing the pandemic. This pattern was driven by both differing approaches across countries—particularly at the beginning of the pandemic—and changes in lockdown stringency across time within countries. The majority of countries tightened lockdown measures sharply as infections increased, as reflected in the accumulation of observations at the upper end of the distribution.

Policymakers also responded with vastly different macroeconomic stabilization measures. While fiscal spending in our sample was largely concentrated in the range up to 5 percent of GDP, some countries spent even more sizeable amounts to shield the economy. In contrast, policy rate differentials were relatively homogeneous for DMs, whereas they were more scattered for EMs, possibly reflecting limited ability to loosen monetary policy due to capital outflow pressures in some countries. Table 1 reports the summary statistics for COVID infections and policy measures.

Table 1: Summary statistics of COVID cases and policy measures

Statistic	N	Mean	St. Dev.	Min	Median	Max
Developed markets						
COVID-19 cases (per 1000 pop.)	337	0.8	1.2	0.0	0.1	5.1
Lockdown stringency (0 to 100)	357	44.3	33.2	0.0	45.8	96.0
Fiscal spending (percent of GDP)	100	4.6	3.1	1.1	3.9	11.2
Policy rate differential (pp)	357	−0.6	0.8	−2.4	−0.1	1.6
Emerging markets						
COVID-19 cases (per 1000 pop.)	272	0.2	0.8	0.0	0.01	9.5
Lockdown stringency (0 to 100)	289	48.0	35.7	0	56.9	97.0
Fiscal spending (percent of GDP)	81	3.4	2.6	0.2	2.6	9.8
Policy rate differential (pp)	289	3.0	2.9	−1.6	2.4	11.4

Notes: Observations over 2020/01/22-2020/05/13.

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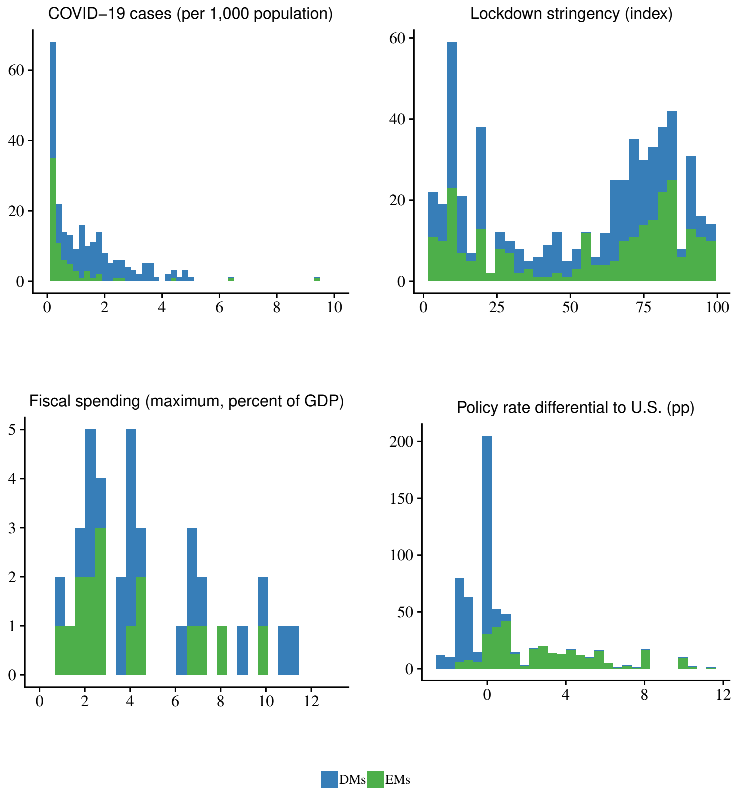
Figure 4: Distribution of COVID-related controls



Notes: Observations over 2020/01/22-2020/06/10. COVID cases excludes observations with fewer than 0.1 cases per 1,000 population. Zero observations are excluded from fiscal spending and lockdown stringency to show the distribution over enacted measures.

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3 Empirical strategy

The goal of the analysis is to empirically evaluate the impact of countries' exposure to the pandemic and the key policy measures that governments enacted on capital markets. We rely on the empirical specification in Fratzscher (2012) and Fratzscher et al. (2018) as a starting point for our analysis. We control for the expected component of portfolio flows and model the unexpected component as a function of both global and domestic factors. Our empirical specification allows for heterogeneity between countries (developed and emerging markets), investors' domicile (foreign and domestic), asset classes (bond and equity flows), and industries.

We stress two aspects of our empirical approach. First, we present a *structural* empirical specification. That is, we argue that the estimated coefficients capture the causal elasticities of our dependent capital market variables to the key identified (i.e. exogenous) COVID-related controls. Second, we explicitly model the endogeneity inherit in capital markets between portfolio flows, financial stress, and asset prices, which is essential to identify the mechanisms behind the estimated responses.

The linkage between these three main variables is developed in the theoretical framework of Caballero and Simsek (2020), which we rely on to guide our empirical analysis. Countries in the model are asymmetric with regards to both market liquidity and expected returns, and Caballero and Simsek (2020) explicitly distinguish between asset classes (risky and safe assets) and investors' domicile (foreign and domestic). The model therefore provides a structural foundation for some of the key patterns typically observed in the data (e.g. fickleness, retrenchment, reach for safety and reach for yield), which we could contrast our empirical findings against.

3.1 Empirical model

We estimate the following baseline regression at a weekly frequency using the local projections approach (Jordà, 2005):

$$\begin{aligned} Y_{c,t+h} = {} & E_{t-1}\{Y_{c,t+h}\}+ \\ & \beta_1^h \text{New COVID Cases}_{c,t} + \beta_2^h \text{Lockdown}_{c,t} + \beta_3^h \text{Fiscal Stimulus}_{c,t} + \beta_4^h \text{Policy Rate}_{c,t} + \\ & \eta_c + \eta_{DMs,t+h} + \eta_{EMs,t+h} + \eta_c \times \eta_m + e_{c,t+h} \end{aligned} \tag{1}$$

where $Y_{c,t+h}$ is alternatively: net portfolio flows, sovereign CDS spreads, or stock market returns in country c at time $t+h$. We study the model dynamics over the horizon $h=0$ and cumulatively over $h=0,...,3$, whereby we estimate the cumulative effect by summing the dependent variable over the month, $\sum_{h=0}^{h=3} Y_{c,t+h}$. We choose to focus on sovereign CDS spreads as a measure of risk in the economy, given the large and dominant role of sovereign borrowing in EMs where private sector access to foreign borrowing may be limited (Avdjiev et al., 2020).

We scale net portfolio flows by their pre-COVID allocation (stock) to capture the dynamics of the average country in our sample. This scaling guarantees that the estimated coefficients are not driven by the experience of a selected group of countries, given that the absolute value of portfolio

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flows depends on market size. Similar to the analysis in section 2, we choose the allocation value reported for the week of February 26^{th}, 2020. Next, we standardize the portfolio flows to allocation ratio by its standard deviation over the full sample.

$E_{t-1}\{Y_{c,t+h}\}$ is the expected value of $Y_{c,t+h}$ using the information available at time $t-1$. To capture this expectation term, we use one lag of each of the following variables: portfolio flows, sovereign CDS spreads, stock market returns, the interest rate differential with the U.S., the total number of domestic COVID-19 cases, and log portfolio allocation.[11]

New COVID Cases captures the change in domestic COVID infections. We scale the new COVID cases by population[12] to arrive at the number of new cases per capita, and standardize the variable by dividing it by its full sample standard deviation for the week of April 29^{th}, 2020. As the distribution of new COVID cases continued to evolve over time, we avoid using the standard deviation for the full sample across all weeks.

Lockdown is a lockdown stringency index ranging over 0 to 100, which we standardize to facilitate the interpretation of the estimates. Fiscal Stimulus captures COVID-related discretionary fiscal spending in percent of GDP, and Policy Rate is the difference between the policy rate of each country and the U.S. Given the absence of a comparable measure of unconventional monetary policy measure across countries, we focus on the short-term policy rate. η_c is a country fixed effect while $\eta_{DMs,t}$ and $\eta_{EMs,t}$ are week fixed effects for DMs and EMs, respectively, to allow for heterogeneity in the impact of global shocks across the two groups. $\eta_c \times \eta_m$ is a country $\times$ month fixed effect, which controls for all slow-moving domestic factors.

3.2 Identification strategy

Our identification strategy relies on three elements: first, the high (weekly) frequency of capital market data; second, the efficiency by which portfolio allocations adjust to new market information as soon as it becomes available; third, the comprehensive set of controls and fixed effects that we include in our specification. We discuss below how these three elements combined allow us to identify the structural elasticities of the four COVID-related domestic factors we study.

COVID infections are driven by epidemiological factors that are mostly independent (i.e exogenous) of capital market outcomes and form a predictable trajectory for the pandemic. However, to the extent that past government crisis management measures (including lockdown and social distancing) contribute to this trajectory, an omitted variable concern arises as the estimated effect of new COVID cases on portfolio flows could be driven in part by market responses to past policies.

While this concern is valid for slow-moving market outcomes, adjustment in financial portfolios tends to take place at a high pace, absorbing in the same period all available news about expected future developments, including the expected COVID trajectory.[13] The implication is that the

[11] The large T dimension of our sample ensures that the Nickell bias brought about by our dynamic specification is negligible (Nickell, 1981).

[12] We use population reported in the World Bank database for 2017 which is the last reported year for the whole world.

[13] We also implement and test our results against the approach followed in Alfaro et al. (2020) who distinguish between the expected and surprise components of new COVID cases using curve fitting methods. While our main

impact of past policy actions on current portfolio flows is expected to be of a limited magnitude. In addition, in the $E_{t-1}Y_t$ term on the right-hand side of our specification, we include lagged flows, their allocation, and more importantly domestic stock prices. These variables readily react to any new information that markets receive (shocks) about the expected trajectory of cases.

Finally, including *country* $\times$ *month* fixed effects controls for institutional variations that may influence the country's capacity to test and report up-to-date figures of COVID infections. Combined, these arguments allow us to maintain the assumption that the new COVID cases variable, conditional on the specification in equation 1, is exogenous to contemporaneous capital market events, and that its estimated effect captures the elasticity to COVID infection shocks.

For lockdown stringency as well as fiscal and monetary policy actions, identification relies on the fact that these measures are typically taken with a lag of more than one week in response to unexpected events and new information, because of the time governments and central banks need to reach these decisions. Hence, with the high frequency at which we observe portfolio flows, we can maintain the assumption that these measures are *contemporaneously* exogenous. Similar intuition motivates other work that uses EPFR data to study the impact of monetary policy actions for instance (Fratzscher et al., 2018).

Another concern arises in identifying the effects of the policy measures if the variations we observe in these measures are a result of endogenous responses to other past shocks. An example would be a past increase in the number of COVID cases that triggers a more stringent lockdown today, and a stronger fiscal stimulus in turn. Similar to the previous discussion, the $E_{t-1}Y_t$ term and the country $\times$ month fixed effects on the right-hand side of our specification both play an important role in mitigating this dynamic endogeneity, specifically the omitted variable, concern. The lagged number of COVID cases per capita controls for the degree of the spread of the virus. More importantly, the lagged capital market variables absorb past shocks that could drive an endogenous policy response. In addition to these sets of controls, the tight country $\times$ month fixed effects specification captures shocks to aggregate economic conditions and market activities that enter the objective function of government authorities and central banks. Similar intuition is shared by the growing literature on high-frequency identification methods with respect to monetary policy actions (Nakamura and Steinsson, 2018).

Following up on our example, if the reason a country has a more stringent lockdown policy at time t is a surprise spike in the number of COVID cases at time $t-1$, then the endogenous increase in the lockdown stringency would be associated with a decline in the lagged values of both stock prices and net flows in addition to a higher number of COVID cases per capita. All three are controlled for in our specification, allowing us to identify the causal effect of variations in the lockdown measures that are independent of other economic and pandemic-related developments.

results do not change, we believe that our identification is robust to this decomposition since the expected component of flows (as well as new COVID cases) is controlled for by our $E_{t-1}\{Y_{c,t+h}\}$ term. Given the degrees of freedom embedded in this curve fitting approach (that is, assumptions have to be made about multiple parameters to limit the number of possible trajectories of a country's COVID cases series), we opt for the transparent approach of taking the first difference of the number of COVID cases instead.

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The same intuition holds for fiscal and monetary policy actions.

Finally, we include *country* fixed effects to control for time-invariant country-specific characteristics and *week* fixed effects to control for all global push factors. We also opt for a conservative specification and, as mentioned above, include country × month fixed effects to control for country-specific institutional variations and slow-moving shocks related to changes in policies, such as capital controls, and aggregate economic developments.

4 Results

This section shows that heterogeneity in countries' exposure to the COVID-19 pandemic and related measures taken by governments played a significant and large role in determining the dynamics of portfolio flows, default risk and asset prices during the COVID episode. We present the results for the contemporaneous and cumulative dynamic effects using the local projections approach discussed in section 3 and explore how the effects varied across DMs and EMs, bond and equity flows, across foreign and domestic investors, and across industries. We also quantify the contribution of the COVID-related variables to the dynamics of flows, using a historical decomposition. Finally, we extend the analysis beyond the average effects of the domestic factors and investigate the nonlinearities brought about by the interaction of global and domestic factors.

4.1 Domestic factors during the COVID episode

We start our analysis by focusing on foreign-domiciled flows, that is flows stemming from funds that are domiciled outside the recipient country. Table 2 demonstrates how the empirical estimates of the average effects on total net flows evolve as we tighten our model specification and add more controls, using all 38 countries in our sample. The first specification in column 1 controls for domestic factors, expected flows and country fixed effects. To account for global push factors, we complement this basic specification in column 2 with a set of the key push variables identified in the literature (VIX, Dow&Jones, and the U.S. interest rate and yield curve).

In column 3, we add week fixed effects to control for global factors in a more comprehensive and agnostic way, whereby we allow for two independent time trends for DMs and EMs. Finally, we include country×month fixed effects in column 4, which capture country-level slow-moving variations (e.g. market liquidity, balance sheet vulnerabilities, and capital controls). Column 5 presents the cumulative impact of column 4—our preferred specification—over a one-month horizon.

Comparing the results across these specifications allows us to make two observations. First, the estimated coefficients on the domestic factors change considerably as more global controls are added. These changes point at a common (i.e. global) component in these domestic variables that masks their domestic effect, if not controlled for. The second observation relates to the relatively large increase in the adjusted R^2 associated with adding country-month fixed effects. It hints at the importance of pull (i.e. domestic) factors in explaining contemporaneous variations in portfolio flows within countries in our sample.

In the remainder of the analysis, we focus on the most conservative specification in column 4.

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As discussed in section 3, this combination of fixed effects controls for unobserved heterogeneity at the country level, global weekly shocks, and country-specific monthly shocks. Hence, our analysis relies on country-month within variation. In all specifications, we control for expected portfolio flows, sovereign risk and domestic stock prices, measured as a function of the lagged values of these variables and other lagged determinants.

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Table 2: Total flows (foreign domicile)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	h=0	h=0	h=0	h=0	cum(h=0:3)	h=0	cum(h=0:3)
New Covid cases$_{c,t}$	0.15**	0.41***	−0.03	−0.07***	1.19***	−0.07	3.87***
	(0.06)	(0.06)	(0.02)	(0.02)	(0.08)	(0.05)	(0.47)
Lockdown stringency$_{c,t}$	−0.53***	−0.34**	−0.11***	−0.29***	0.32**	−0.36***	0.89***
	(0.10)	(0.17)	(0.03)	(0.05)	(0.15)	(0.06)	(0.19)
Fiscal stimulus$_{c,t}$	−0.16***	−0.18***	0.01	0.00	−0.01	0.02	1.17***
	(0.01)	(0.02)	(0.01)	(0.01)	(0.08)	(0.01)	(0.30)
Policy rate$_{c,t}$	−0.18	−0.32	−0.04**	−0.03	0.03	−0.01	0.00
	(0.14)	(0.25)	(0.01)	(0.06)	(0.08)	(0.06)	(0.12)
Policy rate$_{c,t}$ × isCOVID	−0.13***	−0.13***	−0.02*	−0.00	0.05	0.01	0.04
	(0.02)	(0.02)	(0.01)	(0.01)	(0.08)	(0.01)	(0.05)
log(VIX)$_t$		−0.58***					
		(0.17)					
log(Dow&Jones)$_t$		−0.13					
		(0.65)					
US 3M rate$_t$		−0.00					
		(0.09)					
US 5Y-3M spread$_t$		−0.06***					
		(0.02)					
New Covid cases$_{c,t}$ × isDeveloped						0.02	−3.04***
						(0.06)	(0.49)
Lockdown stringency$_{c,t}$ × isDeveloped						0.11	−0.98***
						(0.08)	(0.20)
Fiscal stimulus$_{c,t}$ × isDeveloped						−0.02	−1.19***
						(0.01)	(0.33)
Policy rate$_{c,t}$ × isDeveloped						−0.54***	0.84***
						(0.20)	(0.14)
Policy rate$_{c,t}$ × isCOVID$_t$ × isDeveloped						−0.04	0.16
						(0.03)	(0.12)
Num. obs.	10597	10432	10725	10725	10629	10725	10629
R^2 (full model)	0.45	0.48	0.70	0.83	0.94	0.83	0.94
Adj. R^2 (full model)	0.45	0.48	0.67	0.75	0.91	0.75	0.91
E_{t-1}{LHS}	✓	✓	✓	✓	✓	✓	✓
country FE	✓	✓	✓	✓	✓	✓	✓
week FE	✗	✗	✓	✓	✓	✓	✓
week × isDeveloped FE	✗	✗	✓	✓	✓	✓	✓
month × country FE	✗	✗	✗	✓	✓	✓	✓

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

Notes: All specifications use robust standard errors, clustered at country and year groups. LHS is total net flows, defined as the sum of equity and bond flows per country. Columns one to four present the contemporaneous effects, where column four is our preferred specification. Column five reports the cumulative effect over a one-month horizon. Columns six and seven condition on the country group (DMs and EMs).

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Table 3: Bond and equity flows (foreign domicile)

	Bond flows				Equity flows			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	h=0	cum(h=0:3)	h=0	cum(h=0:3)	h=0	cum(h=0:3)	h=0	cum(h=0:3)
New COVID cases$_{c,t}$	-0.08^{***}	1.38^{***}	-0.12^{***}	4.61^{***}	0.01	-0.19^{***}	0.00	-0.52^{***}
	(0.01)	(0.07)	(0.04)	(0.39)	(0.02)	(0.03)	(0.04)	(0.09)
Lockdown stringency$_{c,t}$	-0.18^{***}	0.44^{***}	-0.28^{***}	1.24^{***}	-0.08^{***}	-0.12^{***}	-0.02	-0.35^{***}
	(0.03)	(0.14)	(0.04)	(0.16)	(0.03)	(0.04)	(0.04)	(0.06)
Fiscal stimulus$_{c,t}$	0.00	-0.04	0.03^{***}	0.53^{***}	0.02	0.03^{*}	-0.00	0.61^{***}
	(0.01)	(0.05)	(0.01)	(0.14)	(0.01)	(0.02)	(0.01)	(0.19)
Policy rate$_{c,t}$	-0.02	0.01	-0.01	-0.00	-0.01	0.01	-0.00	-0.00
	(0.03)	(0.08)	(0.03)	(0.10)	(0.04)	(0.07)	(0.05)	(0.08)
Policy rate$_{c,t}$ × isCOVID	0.02	0.10^{**}	0.02^{**}	0.11^{**}	-0.01	-0.05	-0.01	-0.06
	(0.01)	(0.04)	(0.01)	(0.04)	(0.02)	(0.05)	(0.02)	(0.04)
New COVID cases$_{c,t}$ × isDeveloped			0.11	-3.59^{***}			0.01	0.31^{***}
			(0.07)	(0.43)			(0.04)	(0.11)
Lockdown stringency$_{c,t}$ × isDeveloped			0.17^{***}	-1.41^{***}			-0.13^{***}	0.43^{***}
			(0.06)	(0.21)			(0.05)	(0.07)
Fiscal stimulus$_{c,t}$ × isDeveloped			-0.04^{***}	-0.53^{***}			0.03^{**}	-0.61^{***}
			(0.01)	(0.14)			(0.01)	(0.21)
Policy rate$_{c,t}$ × isDeveloped			-0.27	0.48^{**}			-0.19^{**}	0.35^{**}
			(0.19)	(0.24)			(0.09)	(0.17)
Policy rate$_{c,t}$ × isCOVID × isDeveloped			-0.05^{*}	0.09			0.02	0.06
			(0.03)	(0.08)			(0.02)	(0.06)
Num. obs.	10725	10629	10725	10629	10725	10629	10725	10629
R^2 (full model)	0.90	0.94	0.90	0.94	0.71	0.89	0.71	0.89
Adj. R^2 (full model)	0.86	0.92	0.86	0.92	0.58	0.85	0.58	0.85
E_{t-1}{LHS}	✓	✓	✓	✓	✓	✓	✓	✓
country FE	✓	✓	✓	✓	✓	✓	✓	✓
week FE	✓	✓	✓	✓	✓	✓	✓	✓
week × isDeveloped FE	✓	✓	✓	✓	✓	✓	✓	✓
month × country FE	✓	✓	✓	✓	✓	✓	✓	✓

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Notes: All specifications use robust standard errors, clustered at country and year groups. LHS is bond and equity net flows. Columns one and two present the contemporaneous and cumulative effects on bond flows, respectively. Columns three and four condition on the country group (DMs and EMs). Similarily, columns five to eight present the effect on equity flows.

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Table 4: Sovereign CDS spreads and domestic stock returns

	Sovereign CDS spreads				Domestic stock returns			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
	h=0	cum(h=0:3)	h=0	cum(h=0:3)	h=0	cum(h=0:3)	h=0	cum(h=0:3)
New COVID cases$_{c,t}$	0.06	0.07***	1.61**	0.22***	0.32***	−0.00	−0.80***	−0.02*
	(0.38)	(0.01)	(0.67)	(0.04)	(0.12)	(0.00)	(0.24)	(0.01)
Lockdown stringency$_{c,t}$	0.37	−0.10***	−2.82**	−0.07***	−1.85***	0.02***	−1.47***	0.02***
	(0.89)	(0.01)	(1.33)	(0.01)	(0.19)	(0.00)	(0.52)	(0.00)
Fiscal stimulus$_{c,t}$	−1.31***	−0.01	−1.84***	−0.05	0.12**	0.01**	0.01	−0.04***
	(0.29)	(0.01)	(0.57)	(0.04)	(0.05)	(0.00)	(0.12)	(0.01)
Policy rate$_{c,t}$	−1.00	−0.00	−0.92	−0.00	0.00	0.01*	−0.03	0.01
	(0.96)	(0.01)	(1.07)	(0.01)	(0.25)	(0.00)	(0.24)	(0.01)
Policy rate$_{c,t}$ × isCOVID	−1.38***	−0.02***	−1.57***	−0.02***	0.23*	−0.00	0.19	−0.00
	(0.38)	(0.00)	(0.39)	(0.00)	(0.13)	(0.00)	(0.15)	(0.00)
New COVID cases$_{c,t}$ × isDeveloped			−3.32***	−0.18***			1.94***	0.02
			(0.82)	(0.05)			(0.44)	(0.01)
Lockdown stringency$_{c,t}$ × isDeveloped			6.74***	−0.05**			−0.63	0.00
			(1.93)	(0.02)			(0.70)	(0.00)
Fiscal stimulus$_{c,t}$ × isDeveloped			0.87	0.04			0.23	0.05***
			(0.66)	(0.06)			(0.15)	(0.01)
Policy rate$_{c,t}$ × isDeveloped			−4.95	0.03			0.96	0.03***
			(4.86)	(0.06)			(2.03)	(0.01)
Policy rate$_{c,t}$ × isCOVID × isDeveloped			2.78***	0.04***			0.17	0.00
			(0.95)	(0.01)			(0.37)	(0.00)
Num. obs.	10701	10605	10701	10605	10439	10040	10439	10040
R^2 (full model)	0.64	0.76	0.64	0.76	0.72	0.86	0.72	0.86
Adj. R^2 (full model)	0.47	0.66	0.47	0.66	0.59	0.79	0.59	0.79
E_{t-1}{LHS}	✓	✓	✓	✓	✓	✓	✓	✓
country FE	✓	✓	✓	✓	✓	✓	✓	✓
week FE	✓	✓	✓	✓	✓	✓	✓	✓
week × isDeveloped FE	✓	✓	✓	✓	✓	✓	✓	✓
month × country FE	✓	✓	✓	✓	✓	✓	✓	✓

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Notes: All specifications use robust standard errors, clustered at country and year groups. LHS is the growth rate of sovereign CDS spreads and domestic stock prices. Columns one and two present the average contemporaneous and cumulative effects (i.e. effect on monthly growth rate), respectively, across all countries. Columns three and four condition the results on country groups (DMs and EMs). Columns five to eight repeat the same specifications for stock prices.

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Exposure to the COVID-19 pandemic

We now turn to the response of portfolio flows to countries' exposure to the pandemic. According to the results of column 4 in table 2, a one standard deviation increase in the number of new *domestic* COVID cases per capita is associated with a decline in foreign net portfolio flows (as a ratio of total fund allocation) by 0.07 standard deviations. Interestingly, however, the cumulative effect estimated over the one-month horizon adds up to a positive 1.19 standard deviations. As we detail the result below, disaggregating our sample by market (emerging vs. developed) and asset class (bond vs. equity flows), the overall findings indicate that the average effect was mainly driven by emerging markets. In addition, they support the interpretation that the increase in the number of COVID cases was associated with widening financing needs, which were met by foreign investors in the form of bond flows.

Looking at the response of equity and bond flows (table 3), we find that the average impact of new COVID cases on total net flows was mostly driven by the response of bond flows, especially in emerging markets, in line with the stylized facts in section 2. Bond flows explain both the negative contemporaneous response and the cumulative positive response. Equity flows, in contrast, experienced a cumulative decline in both EMs and DMs, despite an insignificant initial effect.

The results for sovereign CDS spreads, a wider proxy for default risk in the economy in a cross-country context, in table 4 imply that, cumulatively, demand for financing was the dominant force behind the increase in bond flows both in developed and emerging markets. We see this in the positive cumulative effect the domestic COVID shock has on CDS spreads for both country groups; a one standard deviation increase in the number of new COVID cases per capita is associated with a strongly significant increase in the growth of CDS spreads by 0.22 and 0.04 percent in EMs and DMs, respectively.

The results for stock returns are consistent with the financing demand angle, particularly in EMs. As columns 5 and 6 in table 4 indicate, the increase in net flows in response to higher numbers of COVID cases was associated with a decline in EM stock prices of 0.81 percent on impact and 0.02 percent cumulatively. This result suggests that the supply side of the capital market remained tight. For DMs, the effect of an increase in COVID infections on stock returns was initially positive but close to zero over a month (columns 7 and 8).

Lockdown measures

The first policy measure governments took to address the pandemic was locking down the economy to contain the spread of the virus. *A priori*, it is ambiguous how markets weighed potential losses in economic activity against the possibility that stricter containment measures could imply a faster recovery. The results for the aggregate sample in table 2 (column 5) show that changes in the stringency of the lockdown led to a substantial increase in net portfolio flows over a month in the average country, after an initial decline. This result is consistent with the theoretical framework in Çakmakli et al. (forthcoming), which predicts that the economic cost due to the COVID-19 pandemic is lower under an effective full lockdown compared to a partial but longer one.

An increase in the lockdown intensity by one standard deviation led to a rise in foreign net

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portfolio flows by 0.32 standard deviations. Similar to the domestic COVID shock discussed above, zooming in on the results below suggests that lockdown measures led to an increase in net portfolio flows in EMs, and that this response was dominated by bonds. However, unlike the effect of domestic infection shocks, this policy measure triggered increased demand from the investors' side, resulting in increased supply of liquidity, especially for safer assets.

Columns 6 and 7 in table 2 allow for a comparison between DMs and EMs. We find that the observed rise in total net flows in EMs in response to more stringent lockdown measures was driven by investment. This finding indicates that investors did eventually welcome the introduction of lockdown measures to contain the pandemic, although they may have initially reacted negatively. The response in DMs was not economically significant.

Our estimates for the response of bond flows in table 3 show that bond flows to EMs drove the aggregate cumulative positive effect noted above. Limited only to EMs, we observe an initial negative effect that eventually cumulated to a positive value. Foreign equity flows, in contrast, reacted negatively both on impact and cumulatively. The decline took place in both EMs and DMs alike.

Supporting our hypothesis of an increased demand for safe assets, we find that the increase in bond flows was more likely to be driven by an increase in the supply of funds, as seen in the cumulative significant negative effect, albeit small in magnitude, on the growth in sovereign CDS spreads in table 4. The cumulative positive effect on stock prices, although small, also adds weight to this hypothesis.

Discretionary fiscal stimulus

Another key policy component of governments' response packages was large fiscal stimulus, implemented across the majority of countries in our sample. As we discussed in section 3, the expected impact of fiscal spending on markets is ambiguous due to a potential trade-off between positive demand effects and negative fiscal sustainability implications. Our results suggest that increased government spending was perceived as a positive policy measure, attracting portfolio flows to the domestic economy.

The findings in table 2 (column 7) show that the net effect on foreign portfolio flows was cumulatively positive, particularly in EMs. For every one percent of GDP increase in fiscal spending, net flows increased by 1.17 standard deviations. In DMs, fiscal measures appear to have been less relevant for foreign investors' appetite. The aggregate pattern holds for both bond and equity flows and remains more pronounced for EMs.

Moreover, similar to the lockdown shock, we find that sovereign CDS returns declined by 1.84 percent in EMs in response to an increase of one percent of GDP in fiscal spending (table 4). Together, the two findings for flows and spreads indicate that the effect of fiscal stimulus was more likely to be dominated by an increased supply of liquidity in response to the stimulus.[14]

[14]Many governments financed their stimulus spending through borrowing in international capital markets, suggesting that the estimated increase in flows following fiscal measures might partially reflect a mechanical effect. While we cannot directly quantify the relative share of the mechanical response (since the data do not allow us to observe the counter-party of the transaction), our findings indicate that the non-mechanical response, driven by an improved

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Discretionary monetary policy

In addition to lockdown measures and fiscal spending, the majority of countries deployed monetary policy measures to stabilize the domestic economy. As discussed in section 3, our high-frequency weekly data allow us to capture the effect of central banks' policy rate movements on net portfolio flows while alleviating concerns about potential reverse causality. Given the absence of a comparable measure of unconventional monetary policy measure across countries, we focus on the short-term policy rate.

We find that, on average, changes in the policy rate relative to the U.S. do not seem to have played a statistically significant role in the dynamics of portfolio flows (columns 4 and 5 in table 2). This result applies over the full length of the sample as well as throughout the COVID episode. Comparing DMs and EMs (columns 6 and 7), shows that the statistical insignificance of monetary actions in portfolio flows dynamics is a characteristic of EMs alone. We interpret this finding in light of the *dilemma* hypothesis (Rey, 2015), which suggests that U.S. financial dominance renders EM monetary policy ineffective against market fluctuations.

For DMs, the results indicate that a hundred basis points cut in the interest rate led to an increase in net flows by 0.54 standard deviations. However, the cumulative effect over the one-month period has a significant opposite sign, implying a decline in net flows of 0.84 standard deviations in response to the rate cut. Both the response upon impact and the cumulative elasticity do not appear to have been significantly different during the COVID episode. These findings suggest that central bank actions had an initial reassuring effect on markets. Eventually, however, the response was consistent with the interest rate parity and search for yield channels. These results are in line with Fratzscher et al. (2018) who find a similar pattern in response to U.S. quantitative easing policies in the aftermath of the global financial crisis.

Table 4 indicates that expansionary monetary policy was associated with an increase in CDS returns and a marginal decline in stock prices. Although CDS returns increased only upon impact, these findings are not consistent with the classical credit channel effects of monetary policy on risk spreads. In this respect, the information channel of monetary policy Nakamura and Steinsson (2018) could offer an alternative explanation, whereby aggressive rate cuts during the pandemic could have temporarily fueled markets' concerns about the expected fallout from the pandemic. Cumulatively, however, the impact on spreads in DMs was positive, in line with the expected effects of a monetary expansion.

4.2 Domestically domiciled portfolio flows

Unlike foreign investors who tend to be more diversified, domestic investors' balance sheet is more vulnerable to countries' idiosyncratic shocks. This exposure adds an additional dimension to their response to domestic shocks, when compared to that of foreign investors (Caballero and Simsek, 2020). Moreover, as (Maggiori et al., 2020) point out, domestic investors are more likely to be holding assets in local currency, which is likely to amplify the effects of domestic fluctuations.

economic outlook, did play an important role. This observation shows in the finding that net equity flows, not only net bond flows, increased in response to the stimulus and stock prices appreciated.

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In contrast, foreign investors allocate their funds in foreign-denominated assets, which offer them better insulation from local shocks and exchange rate fluctuations. Table 5 reports how the response of investors varied depending on their domicile in both EMs and DMs. Overall, we find evidence that domestic and foreign investors responded differently to the spread of the pandemic, resulting in lower domestic flows relative to foreign flows.

Columns 3 and 4 show that while foreign investors eventually increased their fund holdings in response to an increase in the number of new COVID cases, potentially responding to domestic financing needs as discussed above, domestic investors withdrew their investments. This pattern was particularly strong in EMs compared to DMs, where the cumulative impact on domestic funds was more muted.

The impact of lockdown measures on foreign and domestic funds was also heterogeneous. Cumulatively, both foreign- and domestically-domiciled investors increased their allocations in EMs. However, as columns 3 and 4 show, only domestic funds experienced a negative initial response, and their cumulative positive response was smaller in magnitude compared to foreign funds. This finding suggests that the negative effects of the economic shutdown may have played a larger role for domestic investors, in line with their greater exposure to domestic shocks.

We find a similar pattern in response to fiscal measures, providing further support to the general idea that domestically-domiciled funds are more sensitive to domestic fluctuations. Fiscal measures in EMs had a positive impact on both foreign and domestic funds, with a stronger magnitude in the response of the latter. As already discussed above, the impact of fiscal policy in DMs was limited in magnitude.

Finally, in DMs, where monetary policy actions played a significant role in the dynamics of portfolio flows, we find that rate cuts supported domestically-domiciled flows, both upon impact and cumulatively. As documented earlier, foreign investors, in contrast, initially responded positively to the rate cut but withdrew their capital in the following weeks. This stands in contrast to EMs where domestic investors appear to have reacted significantly to monetary policy actions, in contrast to foreigners whose response was insignificant; more precisely, domestic funds lowered their allocations in response to policy rate cuts. The negative impact of these policy actions on the domestic currency could be one explanation why domestic funds experienced negative pressure as policy eased.

4.3 Impact across industries

The COVID-necessitated lockdown measures had a varying impact on economic activity across different industries, given their heterogeneous degree of exposure to the related demand and supply factors, such as whether a product or service was considered essential and the extent to which the industry is reliant on global supply chains, among other factors. Consequently, investors might have differentiated not only by markets and asset class but also by industry. To shed light on this aspect, we use EPFR data that record equity flows at the industry × country level.[15] The data cover 27 different industries, which we further group into five sectors: Retail, Financial, Manufacturing,

[15] EPFR does not record bond flows at the industry × country level.

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Table 5: Total flows by domicile between EMs and DMs

	(1)	(2)	(3)	(4)
	h=0	cum(h=0:3)	h=0	cum(h=0:3)
New COVID cases$_{c,t}$	-0.10^{***}	1.01^{***}	-0.24^{***}	1.49^{***}
	(0.02)	(0.11)	(0.06)	(0.46)
Lockdown stringency$_{c,t}$	-0.09	1.39^{***}	-0.01	3.28^{***}
	(0.06)	(0.10)	(0.09)	(0.23)
Fiscal stimulus$_{c,t}$	0.11^{***}	0.07^{*}	0.17^{***}	0.61^{***}
	(0.03)	(0.03)	(0.07)	(0.21)
Policy rate$_{c,t}$	-0.07	-0.03	-0.01	-0.04
	(0.05)	(0.09)	(0.08)	(0.17)
Policy rate$_{c,t}$ × isCOVID	-0.02	0.13^{**}	-0.00	0.07
	(0.01)	(0.05)	(0.02)	(0.07)
New COVID cases$_{c,t}$ × isDomestic	-0.17^{***}	-1.03^{***}	-2.18^{***}	-5.26^{***}
	(0.05)	(0.15)	(0.32)	(0.47)
Lockdown stringency$_{c,t}$ × isDomestic	-0.00	-0.51^{***}	-0.40^{***}	-1.49^{***}
	(0.05)	(0.17)	(0.08)	(0.30)
Fiscal stimulus$_{c,t}$ × isDomestic	-0.08^{***}	-0.20^{***}	0.05^{*}	0.29^{***}
	(0.02)	(0.05)	(0.03)	(0.11)
Policy rate$_{c,t}$ × isDomestic	-0.01	-0.08	-0.01	-0.07
	(0.02)	(0.08)	(0.03)	(0.08)
Policy rate$_{c,t}$ × isCOVID × isDomestic	0.20^{***}	0.70^{***}	0.40^{***}	1.33^{***}
	(0.04)	(0.09)	(0.07)	(0.10)
New COVID cases$_{c,t}$ × isDeveloped			0.50^{***}	0.21
			(0.09)	(0.51)
Lockdown stringency$_{c,t}$ × isDeveloped			-0.25^{***}	-3.54^{***}
			(0.08)	(0.26)
Fiscal stimulus$_{c,t}$ × isDeveloped			-0.10^{*}	-0.50^{**}
			(0.06)	(0.22)
Policy rate$_{c,t}$ × isDeveloped			-0.67	1.06^{***}
			(0.46)	(0.18)
Policy rate$_{c,t}$ × isCOVID × isDeveloped			-0.35^{**}	-0.20
			(0.15)	(0.15)
New COVID cases$_{c,t}$ × isDeveloped × isDomestic			1.67^{***}	2.92^{***}
			(0.33)	(0.47)
Lockdown stringency$_{c,t}$ × isDeveloped × isDomestic			0.68^{***}	2.41^{***}
			(0.11)	(0.34)
Fiscal stimulus$_{c,t}$ × isDeveloped × isDomestic			-0.18^{***}	-0.65^{***}
			(0.04)	(0.12)
Policy rate$_{c,t}$ × isCOVID × isDeveloped × isDomestic			-0.15^{**}	-1.60^{***}
			(0.06)	(0.23)
Num. obs.	17270	16894	17270	16894
R^2 (full model)	0.65	0.77	0.65	0.77
Adj. R^2 (full model)	0.56	0.71	0.57	0.72
$E_{t-1}\{\text{LHS}\}$	✓	✓	✓	✓
country FE	✓	✓	✓	✓
week FE	✓	✓	✓	✓
week × isDeveloped FE	✓	✓	✓	✓
month × country FE	✓	✓	✓	✓

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Notes: All specifications use robust standard errors, clustered at country and year groups. LHS is total net flows, defined as the sum of equity and bond flows per country. Columns one and two present the contemporaneous and cumulative effects, respectively, conditioning on the domicile group (Foreign and Domestic). Columns three and four condition on both domicile and country groups (EMs and DMs).

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Services and Others.[16]

Table 6 and figure 5 report the results across the five sectors and the full set of available industries, respectively, using our baseline specification augmented with the relevant interaction terms. We scale industry-equity flows by their respective industry allocation prior to the COVID episode and normalize this ratio using its standard deviation over the full sample.

Against our prior expectation, we find that the effect of the lockdown on the retail sector was not particularly different from other sectors, such as the financial and services sectors. For a one standard deviation change in the lockdown stringency, these three sectors witnessed lower net flows by a cumulative 0.07-0.08 standard deviations compared to industries classified as "others". Net flows in the manufacturing sector, in contrast, increased strongly with a positive estimate that is 0.58 standard deviations higher than that for the benchmark group, as the lockdown became stricter. Overall, in addition to reallocation between markets and asset classes, our analysis provides evidence that investors also adjusted their portfolios across industries, by reallocating funds from the retail, financial, and services sectors towards manufacturing, in response to the lockdown measures.

4.4 Historical decomposition of portfolio flows

Thus far, the analysis has focused on quantifying the elasticity of portfolio flows to changes in COVID cases and policy measures. While these elasticity estimates are informative about the expected effect a particular shock on portfolio flows, they do not provide any insights about its actual contribution to the observed historical dynamics of portfolio flows. In this section, we use the estimated elasticities to compute their implied contributions to the historical path of the distribution of portfolio flows across countries. Our focus is on total net flows based on the aggregate sample, using the estimates in table 2, column 4.

We compute the contribution of a domestic factor F over two steps. The first step extracts the part of the factor's variations actually used in the estimation of the elasticities. We recover this independent component by regressing the factor on all right-hand side variables in the specification of table 2, column 4, including the fixed effects. The residual of this regression is independent of confounding variations driven by the other controls. In the second step, at each point in time, we account for contemporaneous and lagged contributions by multiplying the residual component by the contemporaneous and lagged coefficients estimated using the previously discussed local

[16] We group industries to the respective sectors as follows: Retail {Retailing, Food & Drug Retailing}; Financial {Banks, Diversified Financials, Insurance, Investment Funds}; Manufacturing {Capital Goods, Food Beverage & Tobacco, Materials, Consumer Durables & Apparel, Pharmaceut & Biotechnology, Technology Hardware & Equipment, Semiconductors & Semiconductor Equipment, Automobiles & Components, Household & Personal Products}; Services {Media, Commercial Services & Supplies, Software & Services, Telecom Services, Transportation, Consumer Services}; and Others {Cash, Energy, Real Estate, Utilities, Health Care, Equip & Services, Other}

Table 6: Equity flows at the industry and country level

	(1)	(2)	(3)	(4)
	h=0	cum(h=0:3)	h=0	cum(h=0:3)
New COVID cases$_{c,t}$	-0.06^{***}	0.08^{***}	-0.06^{***}	0.08^{***}
	(0.02)	(0.01)	(0.02)	(0.02)
Lockdown stringency$_{c,t}$	0.10^{***}	0.66^{***}	0.07^{***}	0.50^{***}
	(0.02)	(0.16)	(0.02)	(0.12)
Fiscal stimulus$_{c,t}$	0.00	0.01^{***}	0.00	0.02^{***}
	(0.00)	(0.00)	(0.00)	(0.00)
Policy rate$_{c,t}$	-0.02	0.02	-0.02	0.02
	(0.03)	(0.08)	(0.03)	(0.08)
Policy rate$_{c,t}$ $\times$ isCOVID	-0.01^{***}	0.06^{**}	-0.01^{***}	0.06^{*}
	(0.00)	(0.03)	(0.00)	(0.03)
Lockdown stringency$_{c,t}$ $\times$ isRetail			-0.00^{***}	-0.08^{**}
			(0.00)	(0.03)
Lockdown stringency$_{c,t}$ $\times$ isFinancial			-0.01^{**}	-0.07^{***}
			(0.00)	(0.02)
Lockdown stringency$_{c,t}$ $\times$ isManufacturing			0.10^{***}	0.58^{***}
			(0.03)	(0.14)
Lockdown stringency$_{c,t}$ $\times$ isServices			-0.00	-0.08^{***}
			(0.00)	(0.01)
Num. obs.	187706	185097	187706	185097
R^2 (full model)	0.02	0.08	0.02	0.08
Adj. R^2 (full model)	-0.01	0.05	-0.01	0.05
$E_{t-1}\{\text{LHS}\}$	✓	✓	✓	✓
country FE	✓	✓	✓	✓
week FE	✓	✓	✓	✓
week $\times$ isDeveloped FE	✓	✓	✓	✓
month $\times$ country FE	✓	✓	✓	✓

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Notes: All specifications use robust standard errors, clustered at country and year groups. LHS is equity flows at the industry×country level. Columns one and two present the contemporaneous and cumulative average effects, where column three and four condition on four key sectors.

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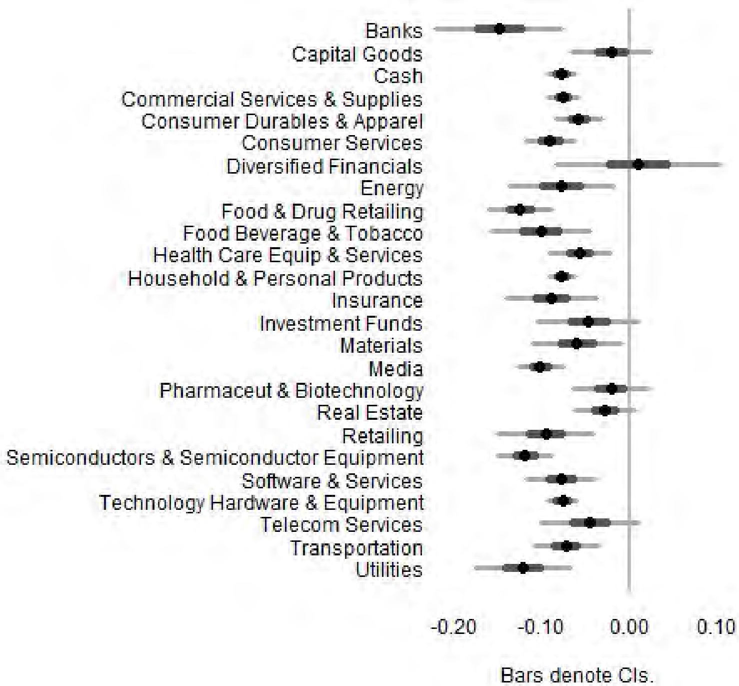
Figure 5: The relative impact of lockdown measures on equity flows by industry



Notes: Estimates for the one month cumulative effect of the interaction between stringency of lockdown and industry on equity flows at the industry level. Estimation uses the same specification in table 6, but at a more disaggregate industry classification level. The benchmark industry is Automobiles & Components. Estimation uses robust standard errors, clustered at country and year groups

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projection[17],

$$\text{shocks}^{F}_{c,t} = F_{c,t} - E\{F_{c,t}|\text{other COVID related factors} + \text{control variables} + \text{fixed effects}\} \quad (2)$$

$$contribution^{shocks^F}_{c,t} = \sum_{h=0}^{3} \beta_h \times \text{shocks}^{F}_{c,t-h} \quad (3)$$

Figure 6 reports the results for the computed $contribution^{shocks^F}_{c,t}$, where $^{shocks^F}$ refers to shocks (i.e. independent innovations) to domestic infections, the lockdown, fiscal spending, and monetary policy.[18] As discussed in section 2, the development of portfolio flows can be seen as part of two phases: a global shock phase, which took place in March as the distribution of flows flattened and shifted considerably to negative territories, and a normalization phase when markets seemed to converge back to their initial pattern. Overall, the estimates indicate that domestic COVID infections and policies had a considerable historical contribution to the dynamics of capital markets across individual countries during both phases of the COVID episode.

In March, the median of portfolio flow was -0.8 percent of allocation with a standard deviation of 0.9 percent. The median contribution of infection shocks in March was 0.02 percent, yet with a standard deviation stretching to 10.1 percent. A similar order of magnitude is observed with respect to the lockdown shocks, whose median contribution stood at -0.5 percent with a standard deviation of 6.2 percent. Finally, consistent with the previous elasticity results, the dispersion was much smaller for the contribution of monetary policy actions. They had a median of 0.07 percent and a standard deviation of 1.9 percent. These results indicate is that the four domestic factors were responsible for a considerably large share of the heterogeneity of portfolio flows in March.

A similar finding holds during the normalization phase in April and May. Domestic COVID factors drove an increase in net flows in the median country and contributed to narrower distributions. Specifically, the median portfolio flow was -0.02 percent of allocation with a standard deviation of 0.3 percent. The median contribution of infection shocks and the three policy measures (lockdown, spending and monetary policy) were, respectively, 0.08, 0.19, 1.1, and 0.11 percent. Their standard deviations were, respectively, 4.1, 5.8, 4.2 and 0.6 percent.

4.5 Policy measures and the *global* shock

Thus far, the analysis has focused on the average effects of the domestic infection and policy shocks, and abstracted from the global component of the COVID episode. However, one of the key channels through which these policy measures could have supported the domestic economy is through mitigating the impact of the global shock. In this section, we examine this interaction between global market fluctuations and the enacted policy measures.

We estimate the effects of the interaction between the VIX, as a continuous measure of global

[17] Ideally, a historical decomposition estimation calls for a long history of lags to exhaust the contribution of all past shocks. However, given the limited horizon of COVID infections and policy measures, we use only a one-month horizon.

[18] This contribution refers to the domestic shocks, which comes on top of, and is therefore independent of, the global impact of COVID and synchronized policy measures on global markets' sentiments and financial position.

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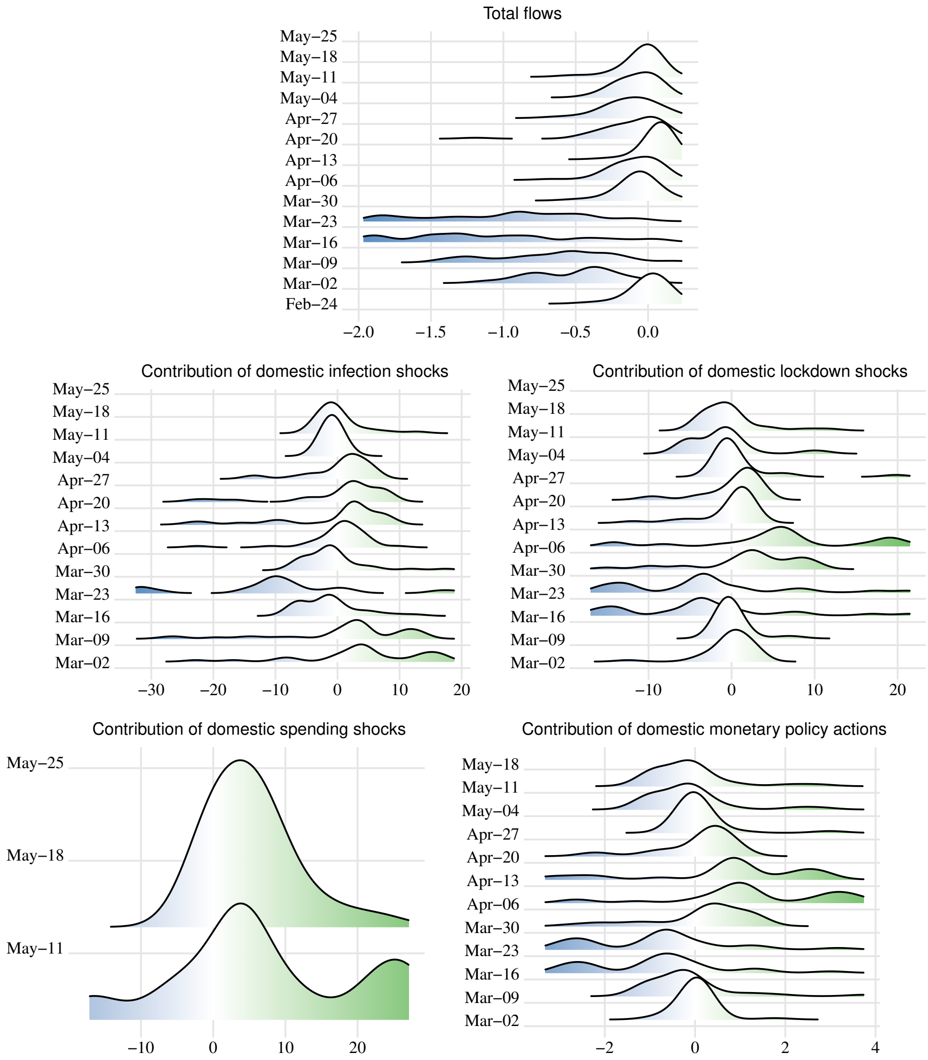
Figure 6: Historical contributions to total flows of all countries (percent of allocation)



Notes: Estimates for the historical contribution uses both contemporaneous and lagged contributions (3 lags) by multiplying the independent domestic variations in each factor by the corresponding contemporaneous and lagged elasticities. The number of available estimates for the historical contribution of fiscal spending is limited because fiscal spending started only in April, and we require four observations to compute a single historical contribution.

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investment sentiment, and the lagged values of the policy measures. Given the fluid and dynamic nature of the COVID crisis, we believe that a market-based measure is more successful in reflecting the evolving intensity of the global shock in comparison to an agnostic time dummy. Observed fluctuations in the VIX align with those of global outflows, capturing the changes in markets as the virus spread. In this respect, our analysis in table 2 discussed above shows that an increase in the VIX was associated with negative and significant response in net portfolio flows.

Table 7 reports the results for the interactions. Our findings, albeit limited in significance, suggest that lockdown measures were more likely to have exacerbated the adverse impact of the global shock on portfolio flows. Between any two emerging (developed) economies that differed in the intensity of their lockdown measures by one standard deviation, a hundred percent increase in the global VIX index was associated with a decline in net flows by 0.10 (-0.12) standard deviations in the country with the more stringent lockdown.

Our results also show that governments' discretionary spending played a significant role in mitigating the impact of the VIX. In line with the earlier discussion, the contribution of fiscal measures to the mitigation of the global shock was stronger in EMs. Between any two countries that differed in the amount of their fiscal spending by one percent of GDP, a hundred percent increase in the VIX index was associated with a 0.06 standard deviation increase in net flows in the emerging economy with the higher spending level. The estimated effect in DMs was 0.02 standard deviations smaller than that for EMs.

Similarly, we find that expansionary monetary policy actions were also successful in attenuating the negative impact of the global shock, and that their effectiveness was more pronounced in DMs, similar to the discussion above on their average effects. Between any two countries that differed with respect to their interest rate differential to the U.S. by a hundred basis points during the global COVID episode (referring to isCOVID dummy), a hundred percent increase in the VIX index was associated with a higher level of net flows by a 0.07 standard deviations in the EM with the lower rate. The estimated effect in DMs was 0.03 standard deviations higher than that for EMs.

5 Conclusion

The COVID-19 pandemic triggered a drastic reaction of international capital markets. Despite the global nature of the pandemic, there was a large degree of heterogeneity in portfolio flows across countries and asset classes, as well as in governments' policy responses around the world. Some emphasized a trade-off between lives and livelihood while others argued that a well functioning economy can only be sustained when public health is prioritized.

This paper, to the best of our knowledge, provides the first empirical assessment of how the number of infections, the stringency of the lockdown, and fiscal and monetary policy stimulus affected international capital markets during the global health emergency. Our findings indicate that capital market dynamics were not exclusively driven by global push factors. Instead, the severity of the pandemic at the domestic level and governments' policy responses played an important role in explaining heterogeneity in portfolio flows, market-implied sovereign risk, and stock prices across

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Table 7: Policy measures and the global shock for total flows (foreign domicile)

	(1)	(2)	(3)
New COVID cases$_{c,t-1}$	0.08***	0.08**	0.07**
	(0.03)	(0.03)	(0.03)
Lockdown stringency$_{c,t-1}$	0.36	−0.11***	−0.12***
	(0.36)	(0.03)	(0.03)
Fiscal stimulus$_{c,t-1}$	0.01	−0.17	0.01
	(0.02)	(0.11)	(0.02)
Policy rate$_{c,t-1}$	0.03	0.03	0.03
	(0.05)	(0.04)	(0.04)
Policy rate$_{c,t-1}$ × isCOVID	0.04**	0.04**	0.30***
	(0.02)	(0.02)	(0.09)
Lockdown stringency$_{c,t-1}$ × $log(\text{VIX})_t$	−0.10		
	(0.09)		
Lockdown stringency$_{c,t-1}$ × $log(\text{VIX})_t$ × isDeveloped	−0.02*		
	(0.01)		
Fiscal stimulus$_{c,t-1}$ × $log(\text{VIX})_t$		0.06**	
		(0.03)	
Fiscal stimulus$_{c,t-1}$ × $log(\text{VIX})_t$ × isDeveloped		−0.02***	
		(0.00)	
Policy rate$_{c,t-1}$ × isCOVID × $log(\text{VIX})_t$			−0.07***
			(0.02)
Policy rate$_{c,t-1}$ × isCOVID × $log(\text{VIX})_t$ × isDeveloped			−0.03**
			(0.01)
Num. obs.	10555	10555	10555
R^2 (full model)	0.86	0.86	0.86
Adj. R^2 (full model)	0.80	0.80	0.80
E_{t-1}\{LHS\}	✓	✓	✓
country FE	✓	✓	✓
week FE	✓	✓	✓
week × isDeveloped FE	✓	✓	✓
month × country FE	✓	✓	✓

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Notes: All specifications use robust standard errors, clustered at country and year groups. LHS is total net flows, defined as the sum of equity and bond flows per country.

countries, particularly in EMs.

One of our key messages is that the pandemic and governments' policy response impacted both the supply and demand curves of capital markets. We find that higher domestic infection rates led to a cumulative increase in EM flows—after an initial decrease—in affected economies, responding to widening financing needs. While this flow of funds may have helped countries smooth the effect of the shock at the height of the pandemic, it could also imply that countries that were less successful in maintaining the pandemic would be left to struggle with a larger debt burden in the aftermath of the crisis.

Another element we highlight in the analysis is that lockdown and fiscal policy measures were successful in stimulating portfolio investment in EMs. Despite potentially adverse effects of these measures on economic activity and deficit levels, the supply of funds increased in response to stricter containment and fiscal stimulus. In addition, fiscal stimulus had indirect effects through mitigating the transmission of the global shock on portfolio flows. We do not find evidence that monetary policy actions impacted the experience of EMs during the pandemic. For DMs, monetary policy loosening eventually led to lower flows, in line with the pattern we would expect when investors search for yield.

The findings in this paper are particularly relevant for EMs, which tend to be subject to larger policy constraints and fickle capital flows. In the context of the lives vs. livelihood debate, our results imply that markets viewed this potential trade-off as a false dilemma, rewarding countries for enacting a more stringent lockdown. Hence, going forward, public health should remain a priority. In addition, the positive and sizeable effect of fiscal stimulus on portfolio flows points at the importance of countercyclical fiscal policy, particularly as monetary policy appears to have been less effective in EMs. This finding suggests that building fiscal policy buffers in good times and maintaining access to financing during bad times can help EMs in shielding themselves from undesired volatility in capital flows.

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Appendix to Capital Markets, COVID-19, and Policy Measures

A Data

Table A.1: Country classification

Developed markets			
Australia	Austria	Belgium	Canada
Denmark	Finland	France	Germany
Hong Kong SAR	Ireland	Israel	Italy
Japan	Netherlands	New Zealand	Norway
Portugal	Spain	Sweden	Switzerland
United Kingdom			
Emerging markets			
Brazil	China	Czech Republic	Greece
Indonesia	Malaysia	Mexico	Pakistan
Peru	Philippines	Poland	Qatar
Russia	Saudi Arabia	South Africa	Thailand
Turkey			

Notes: Countries are categorized according to the 2020 MSCI developed and emerging market classification.

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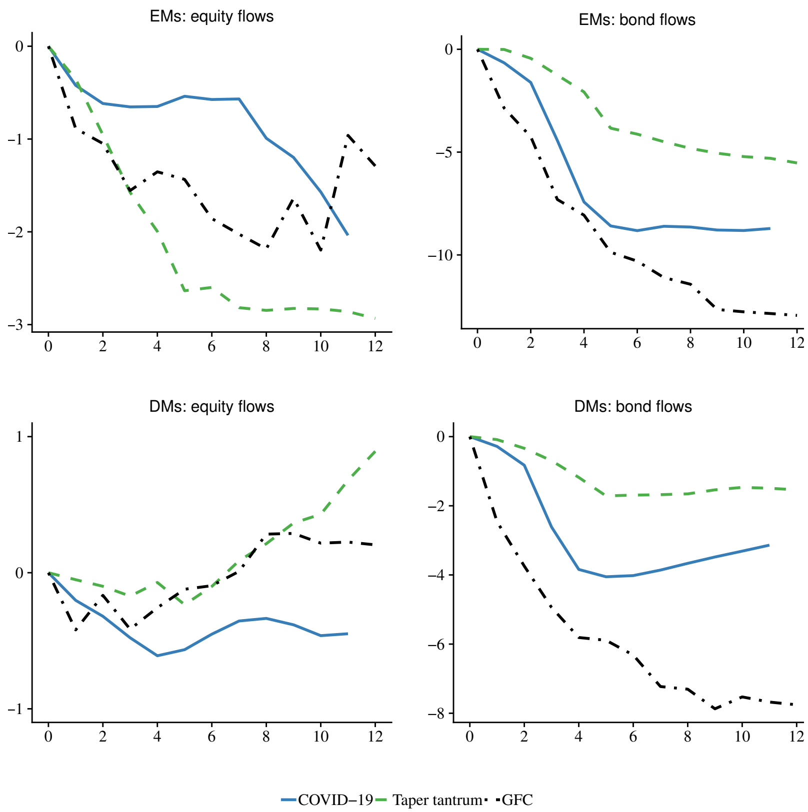
B Additional figures

Figure B.1: Cumulative equity and bond flows (percent of allocation)



Notes: x-axis shows weeks after the start date for the following episodes: COVID: 2020/02/26-2020/04/29; taper tantrum: 2013/05/22-2013/08/14; global financial crisis: 2008/10/08-2008/12/31. Flows are normalized by the allocation at the start of each episode.

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The impact of COVID-19 international travel restrictions on services-trade costs: some illustrative scenarios[1]

Sebastian Benz[2], Frederic Gonzales[3] and Annabelle Mourougane[4]

Date submitted: 21 August 2020; Date accepted: 22 August 2020

This paper casts some light on the impact of regulatory restrictions on the movement of people across international borders, implemented on health and safety grounds following the COVID-19 outbreak, on services trade costs using some illustrative scenarios where all the countries are assumed to close their borders to passengers, but leave freight trade open. Services trade costs are estimated to increase by an average of 12% of export values across sectors and countries in the medium term in such a hypothetical scenario. The analysis identifies a large variability in the increase in services-trade costs across sectors and across countries, reflecting the stringency of initial regulations and the relative importance of business travel and labour mobility to international services trade.

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2 Economist, OECD
3 Junior economist, OECD
4 Lead Economist, OECD

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1. Introduction

Almost all OECD countries and emerging market economies have announced temporary restrictions in movements of people to contain the COVID-19 pandemic (see OECD COVID-19 policy tracker). These range from border closures, either complete or restricted to some regions or countries, to specific restrictions on visas, quarantine and flight suspensions. Those restrictions appear to have helped to delay the pandemic by 3-4 weeks when 90% of air travel is restricted in affected countries, or by two months if more restrictive measures are introduced (OECD, 2020). Countries have started to ease some restrictions but most are still in place. According to UNWTO (2020), as of 18 May, 100% of all destinations worldwide continue to have some form of COVID-19-related travel restrictions in place. Furthermore, 75% continued to have their borders completely closed for international tourism. In 37% of all cases, travel restrictions have been in place for 10 weeks, while 24% of global destinations have had restrictions in place for 14 weeks or more.

There is already evidence that the time to cross borders has increased in Europe (Baldwin, 2020). While these measures focus on reducing passenger flows, governments have sought to preserve trade and the transport of freight. As such, the various transport modes and sectors are likely to be affected differently.

Regulatory restrictions on the movement of people across international borders, implemented on health and safety grounds following the COVID-19 outbreak, have implications for services exporters. The paper seeks to quantify the services trade costs associated with an illustrative scenario where all the countries are assumed to close their borders to passengers, but leave freight trade open. Drawing on the OECD COVID-19 policy tracker and the OECD Services Trade Restrictiveness Index (STRI) database, a set of recent regulatory changes affecting business travel and labour mobility was identified. The outcomes were subsequently translated into trade costs, expressed as a percentage of export values, following Benz and Jaax (2020a).

Services trade costs are estimated to increase by an average of 12% across sectors and countries in the medium term in a hypothetical scenario where all the countries are assumed to close their borders to passengers, but leave freight trade open. Countries where sectoral regulations related to travels and labour market access were initially more liberal would be by design more affected than those where regulations were already stringent.

It is therefore important that emergency measures designed to tackle COVID-19 be targeted, proportionate, transparent and temporary.

A large variability in the increase in services-trade costs is identified across sectors in the scenario, reflecting the stringency of initial regulations and the relative importance of business travel and labour mobility to international services trade:

- Trade costs for professional services are found to increase by around 9%-13%, and vary across professions depending on the pre-existing degree of openness.
- Trade costs in logistics services could jump by slightly more than 10%.
- Trade costs could rise by 6% to 9% across transport modes, reflecting the fact that transport is more capital intensive than other services sectors.

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- The regulatory environment for commercial banking and insurance is particularly sensitive to disruption, but uncertainties around trade costs estimates in these sectors are large.
- Remote connection and teleworking could help to mitigate somewhat increases in trade costs, especially for professional services and insurance, but the extent of this is difficult at present to assess.

Across countries, the highest increases in trade costs are generally found in Brazil, China, India, France and Korea in this illustrative scenario. Restricted movement of business travellers is found to contribute significantly to the overall rise in the stringency of regulation in services trade, and the effects would be broadly similar across countries. The impact of limiting temporary employment of international services providers, via quotas or limitations in the duration of stays, is expected to vary widely across countries.

Countries are necessarily focussed on ensuring the health and economic security of their people today. Looking beyond the immediate, steps to reduce services trade costs will promote a recovery that is robust, widespread and sustainable. Easing of COVID-19-related international travel restrictions, when health and safety considerations permit, will ensure that trade in services, which is highly intertwined with manufacturing in global value chains, can support the recovery. Increased investment in digital infrastructure will also help the adjustment to new working arrangements.

The rest of the paper is organised as follow. The next section describes the empirical strategy and the following sections present the main findings.

2. Empirical strategy

The impact of travel restrictions on services trade costs is estimated using a two-step procedure. In a first step, the impact of a ban on international passenger traffic on the stringency of services-trade regulations is quantified using the OECD Services Trade Restrictiveness Index (STRI) framework. The STRI provides information on regulations that affect trade in services in 22 sectors across OECD countries and several emerging-market economies. The STRI covers limitations on market access and national treatment, as well as national regulatory and competition policies which apply to both national/resident and foreign/non-resident companies, and investment policies. The policy measures accounted for in the STRI database are organised under five policy areas: restrictions on foreign entry; restrictions on movement of people; other discriminatory measures; barriers to competition; and regulatory transparency. Further information can be found in Trade Policy Paper N°177 and Policy trends up to 2020.

Several types of measures are considered in this paper.

- In the first step, conditions on business travel are assumed to become more restrictive. These include the time and costs to deliver visas, the time taken for customs clearance, and other restrictions on business travels. Only measures regarding visas of passengers have been made more restrictive. Visa procedures for transport crews remain unchanged. This is consistent with the fact that policies have aimed to restrict passenger traffic, while leaving freight traffic unchanged.
- In a second step, quotas on intra-corporate transferees have been set to zero, in addition to the measures considered in the first step.
- In a third step, sector-specific measures have been put in place. These are related to measures on mutual recognition of qualifications, temporary licensing, or

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residency requirements, which have been rendered more difficult or impossible by the restrictions on passenger travel.

- In a final step, measures taken for air transport but which are not related to the movement of people (e.g. restrictions on foreign entry, closures of airports, loan guarantees, and tax deferrals) have been introduced.

Measures are assumed to be applied to all countries and, with the exception of sector-specific measures, to all sectors. Although not all countries have put in place all the measures considered in this exercise, it was judged preferable to focus on an illustrative scenario, rather than a country-specific assessment which would have been rapidly outdated. The idea is to gain insight on average effects and to identify sectors or countries that would be the most affected by these restrictions. As the exercise was not calibrated on measures actually implemented, results should not be interpreted as predictions.

Several caveats should be kept in mind. First, estimates capture only part of the impact of restrictions on passenger travel and do not account for the effects of policy changes since the COVID-19 outbreak on cross-border trade, for example, nor on labour supply. Policies that have considerably softened monetary and fiscal policy stances since the COVID-19 outbreak have also been omitted, as is the effect of the considerable fall in oil prices. Second, neither international nor inter-sectoral spillovers are incorporated in estimates. Finally, some approximations needed to be made for the purpose of this exercise. For instance, measures on quotas, which normally concern both foreign and domestic workers, have been assumed to apply to foreign workers. By contrast, measures on temporary licences which in practise apply to new services, have been assumed here to apply to all services in the sector. Other measures, such as changes to *de minimis* regimes (specific thresholds, below which goods are exempted from import duties and/or full declaration procedures) which might impact firms and consumer cross-border transactions, have been omitted.

In a second step, the increase in the stringency of the STRI is translated into trade costs following the approach Benz and Jaax (2020) have developed and is based on a gravity model. Traditionally used to analyse patterns of trade in goods, gravity equations have also been widely applied to cross-border trade in services (Eaton and Kortum, 2018; Nordas and Rouzet, 2017; Van der Marel and Shepherd, 2013; Anderson et al., 2015).

Formally, the gravity model can be expressed as follows:

$$Exports_{ij} = \frac{GDP_i\, GDP_j}{GDP_{world}} \left(\frac{tradecost_{ij}}{\Pi_i P_j}\right)^{(1-\sigma)}$$

where the left-hand side variable represents the trade flow from exporter *i* to importer *j*. The second term ensures that the model takes into account GDP proportionality, whereas the third term captures the role of trade costs which encompass two main components. First, pair-specific costs of economic transactions between two countries *i* and *j*. Second, the above-mentioned country-specific costs of engaging in trade with the rest of the world, here represented by Π_i and P_j. The parameter σ is the elasticity of substitution between foreign and domestic goods and services.

This model remains valid when *i* and *j* reference the same country. In this case, the variable $tradecost_{ij}$ indicates internal trade costs within a country, while Π_i and P_j are defined as above and now indicate inward multilateral resistance and outward multilateral resistance of the same country. Calculated as the share of gross production that is not exported, the addition of a country's trade with itself aligns the gravity estimations with the modelling of choices between domestic and foreign goods (Yotov et al., 2016; Dai, Yotov and Zylkin,

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2014; Yotov, 2012). Moreover, the inclusion of within-country trade permits to analyse the effect of multilateral policy variables, i.e. variables that do not vary bilaterally, without omitting multilateral resistance terms (Heid, Larch and Yotov, 2015).

The gravity estimations presented in this paper are run separately for each sector and estimated using the Poisson Pseudo Maximum Likelihood (PPML) technique introduced by Santos Silva and Tenreyro (2006). This approach is now commonly used for the estimation of the gravity model. It is superior to the traditional log-linearized estimation with ordinary least squares due to its robustness to different patterns of heteroscedasticity. Moreover, it allows retaining zeros in bilateral trade data, which would otherwise get lost in the logarithmic transformation of the model.

The regressions rely on variations of the following specification:

$$exports_{ij,k} = \exp(\beta_1 STRI_{j,k} border_{ij} + \beta_2 border_{ij} + \gamma\, Z_{ij} + \eta_{i,k} + \mu_{j,k} + \varepsilon_{ij,k})$$

where the dependent variable are services exports from country i to country j in sector k measured in million USD. Exporter and importer fixed effects $\eta_{i,k}$ and $\mu_{j,k}$ control for multilateral resistance terms and all other country-specific variables. A set of standard gravity variables (represented by Z) control for other determinants of bilateral trade costs. β_1 is the main coefficient of interest; it represents the effect of changes in the STRI score of the importer j on the estimated flow of services exports from country i to country j relative to domestic services consumption in country j. Standard errors are clustered by exporter and importer.

Trade costs (expressed in percentage) are computed using the following equations

$$trade\_cost \quad = 100 * \exp(\Delta STRI * \frac{\beta_1}{1-\sigma})$$

Table 1. Coefficients used for the computation of trade costs

Sectors	Trade elasticities (β_1)	Import demand elasticities (1- σ)
Communication	-4.403	-2.67
Business	-3.959	-2.21
Finance	-7.355	-1.54
Insurance	-5.042	-1.77
Transports and logistics	-3.606	-2.39

Source: Benz and Jaax (2020a).

3. Findings

3.1. Increase in the stringency of regulations

The imposition of new restrictions on passenger travel in this hypothetical scenario implies a rise in the stringency of services-trade regulations. The level of restriction is estimated to increase by around one-quarter on average of their initial level across sector and country, but with large variations (Figure 1.).

Across sectors, regulations tighten especially in professional services and, to a lesser extent, in logistics. Sectoral differences reflect initial services-trade regulations stringency and the relative importance of business travel and labour mobility to international services trade.

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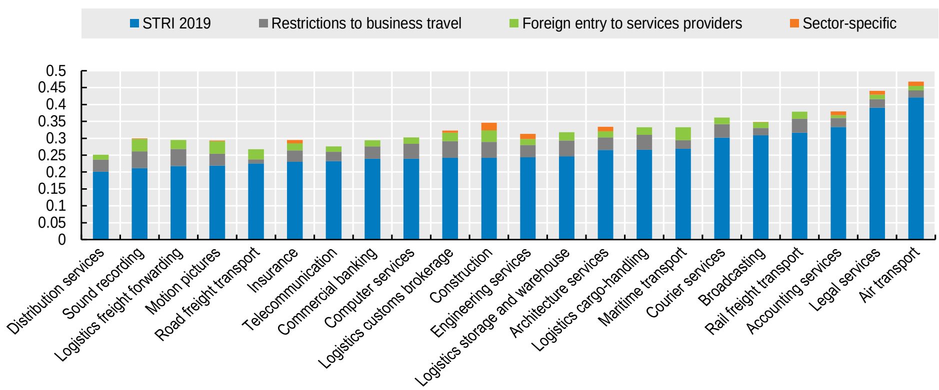
In most sectors, measures that restrict business travel have a higher impact on the overall increase in restrictions than those related to foreign entry into the labour market.

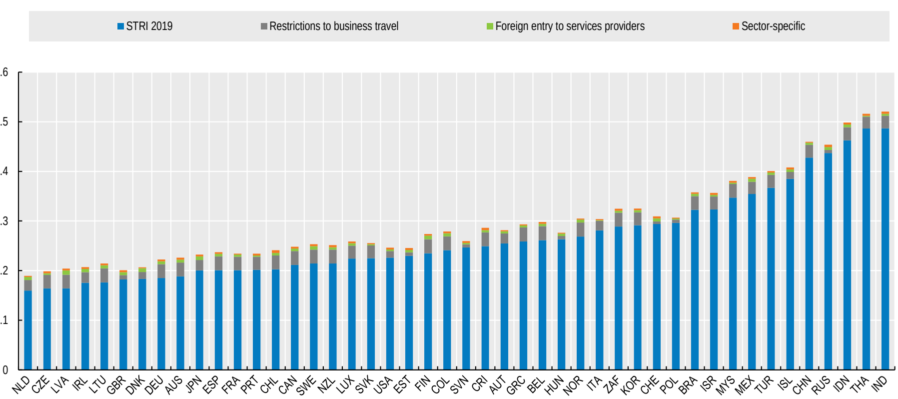
Across countries, differences reflect pre-existing restrictions in services sectors. Countries where regulations related to travels and labour market access were initially more liberal would be by design more affected than those where regulations were already stringent. At the limit, no impact will be visible in a country where regulations were already restrictive in all sectors. In so far as the sectors are concerned, a larger variability of impacts is observed for measures related to foreign entry into labour markets.

Figure 1. Increase in the stringency of services-trade regulations

A - Services Trade Restrictiveness Indicator by sector



B- Services Trade Restrictiveness Indicator by country



Note: The STRI varies from 0 (less restrictive) to 1 (most restrictive).
Source: Calculations using the OECD STRI database.

3.2. Increase in trade costs

Trade costs on services are estimated to increase by an average of 12% across sectors and countries in the medium term, in a hypothetical scenario where all countries close their borders to passenger travel, but leave freight trade open.

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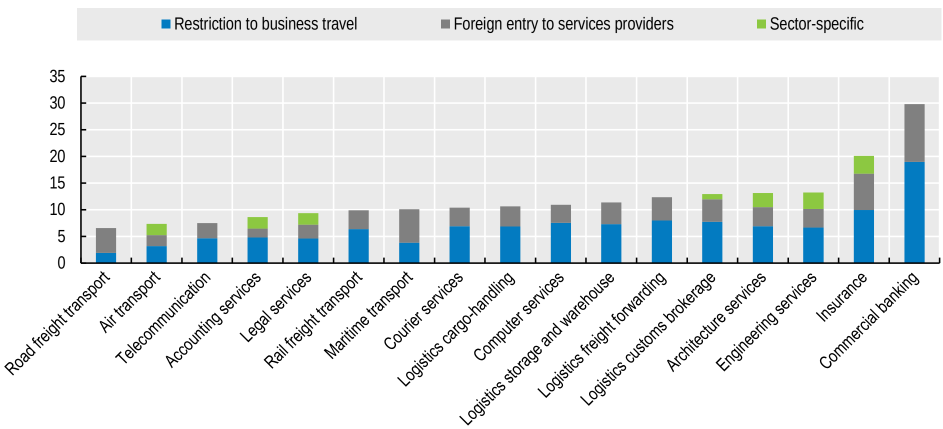
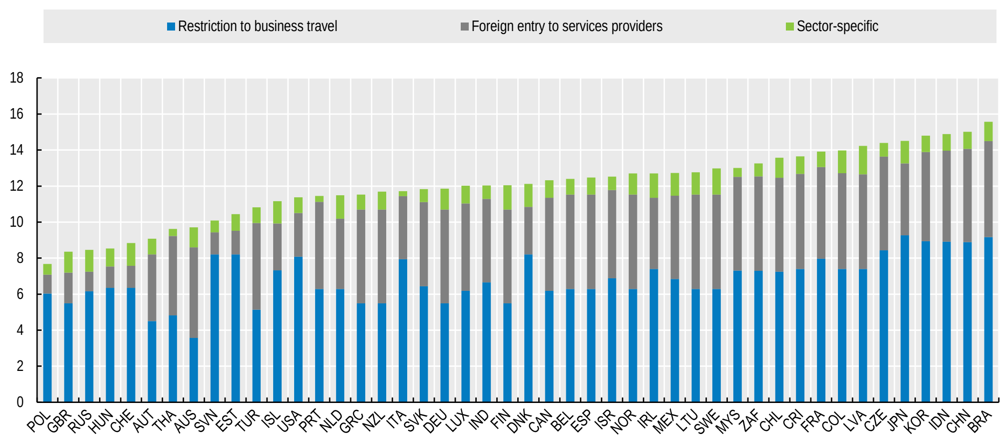
To a large extent, differences across sectors and countries reflect difference in STRI changes (Figure). However, the ranking in terms of trade costs across sectors can vary as the various elasticities used in the calculations are sector-specific. As a result, there is no strict proportionality between an increase in the STRI and the related changes in trade costs (Benz and Jaax, 2020a).

Figure 2. Rise in trade costs

A - By sector, percentage of export values



B - By country, percentage of export values



Source: OECD calculations using Benz and Jaax (2020a).

Overall, the large variability in the increase in services-trade costs across sectors in the scenario reflects their initial regulations stringency and the relative importance of business travel and labour mobility in international services trade.

Impact on trade costs varies across professional services. While engineering and architecture services could experience a rise in trade costs of 13%, accounting and legal services would experience a less pronounced increase of about 9%. The difference between the two groups reflects the pre-existing degree of restrictions.

Logistic services (cargo handling, freight forwarding, storage and warehousing, and customs brokerage) are under severe stress from the current lockdown. Trade costs in logistics services could jump by slightly more than 10%, reflecting the steep increase in the stringency of regulations. Time taken for customs clearance appear to be the main contributor to the rise in trade costs in all the logistics sub-sectors.

Trade costs could rise by 6% to 9% across all transport modes. The transport sector would be less affected than other sectors as it is more capital intensive and only freight is considered in road, rail and maritime transport. Although this is not captured in the estimates, it is important to bear in mind that the reduction in the number of passenger flights has implications for freight transport, as a substantial share of air cargo is transported on passenger flights.

Commercial banking could experience a large rise in trade costs. Insurance would also be hit, but to a lesser extent. In these sectors, estimates suggest a strong impact of small regulatory changes on trade costs (Benz and Jaax, 2020a; Nordas and Rouzet, 2017; Benz, 2017; Fontagné et al., 2019). A tentative interpretation of this large effect could be that restrictions on travels have contributed to a rise in uncertainty which has added to trade costs. Those estimates are surrounded by large uncertainties (see below) and should be interpreted with caution.

Across countries, the highest rises in trade costs are generally found in emerging-market economies (Brazil, China, India), France and Korea in the scenario, while those costs would increase less in Eastern European countries, the United Kingdom and Switzerland. Differences reflect essentially pre-existing restrictions in foreign entry to services providers.

3.3. Uncertainties around trade costs

The computation of trade costs is subject to a number of uncertainties. To cast some light on these considerations, 90% confidence bands have been computed using the standard errors around the sector-specific trade elasticity estimates from the gravity model estimated in Benz and Jaax (2020a). These confidence bands capture only part of the uncertainties, but provide some useful insights into the relative precision of estimates by sector or country. Confidence bands around trade costs in commercial banking and insurance are sizeable, for example (Figure 3). By contrast, trade costs increases in telecommunications, courier services or road freight are estimated with greater precision. Across countries, confidence bands are found to be, on average around +/- 5 percentage points and would be particularly large in some emerging-market economies. Differences would reflect the country’s regulatory environment, relative to other economies, and the relative precision of the sectoral estimates.

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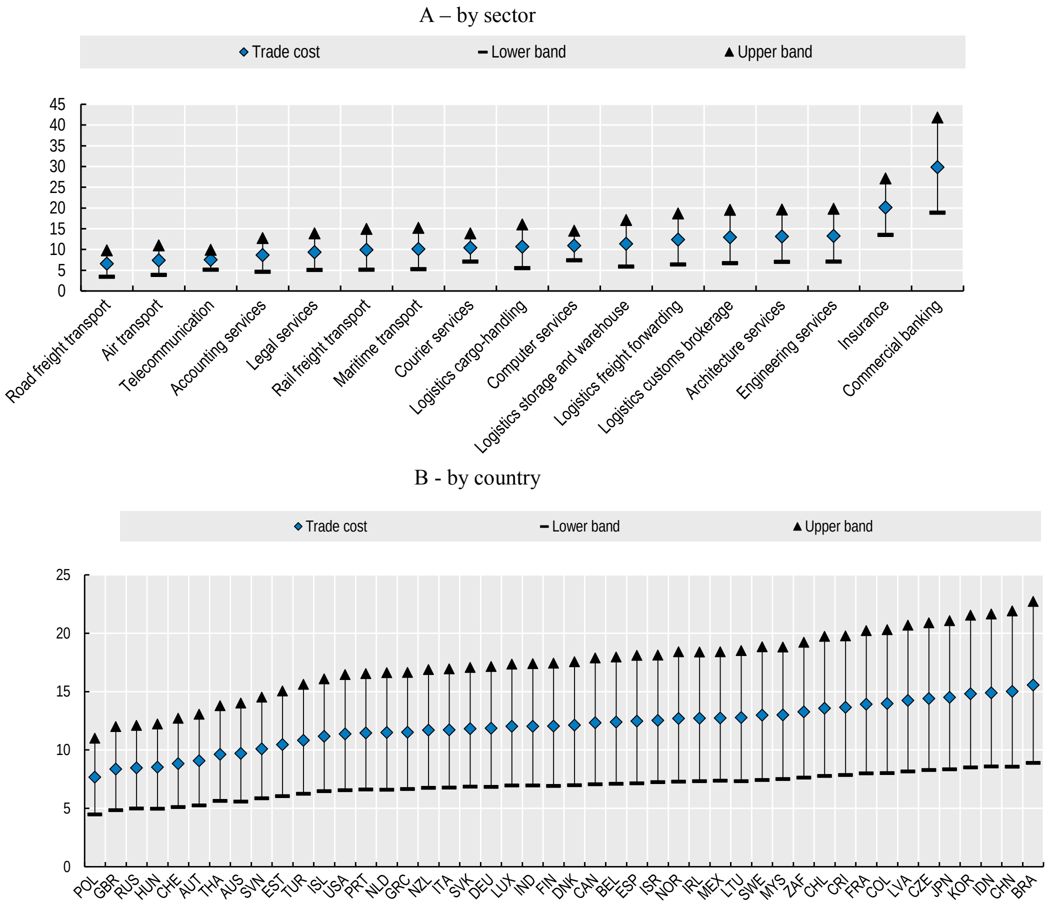
Figure 3. Uncertainties around trade costs

Per cent



Note: 90% confidence bands are computed using standard errors around estimates of trade elasticities.
Source: OECD calculations using Benz and Jaax (2020a).

3.4. Impact of remote connection

New working arrangements (e.g. increase in teleworking and remote connections) are likely to mitigate the increase in trade costs due to restrictions on passenger travel. The extent that travel restrictions on trade costs to be mitigated by digitalisation varies by industry. Most services are digital-intensive industries, including sectors such as telecommunications, IT, finance, legal and accounting services, scientific and research services, advertising and other business services. By contrast, transportation and storage or accommodation and food services are industries with low digital intensity (Calvino et al., 2018). Dingel and Neiman (2020) found that teleworking can be more easily performed at home in professional services and technical services, management, finances and insurance and education services than in other sectors. This is consistent with Adam-Passl et al.

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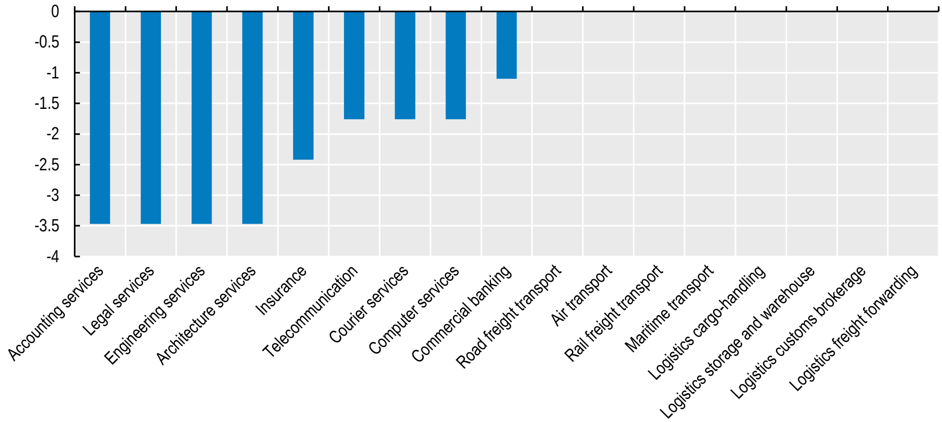
(2020) and Del Rio-Chanona et al. (2020). In line with existing evidence, estimates using a gravity model suggest that an increase in the recent take up of remote connection has lowered trade costs to various extents across sectors, ranging from -3.5% in professional services to no significant effect in transport or logistics (Figure 4.).

Figure 4. Impact of remote connection on trade costs

Percentage, average per year



Source: Benz and Jaax (2020b, forthcoming).

Recent evidence from teleconference service providers suggest that these services have experienced strong growth after only a few weeks since the COVID-19 outbreak, of a magnitude similar to what was observed on average per year since 2014. At this stage, it is difficult to assert whether these high rates will persist or whether a correction will occur once the pandemic is over. Assuming recent observations are early indications of structural behavioural changes towards more teleworking would imply lower trade costs than those reported above. If, on the contrary, workers revert back to their previous working arrangements, the estimates presented above would remain valid.

3.5. Impact of other restrictions in air transport

Most countries have introduced measures in air transportation that are not directly related to the travel bans. For instance, they have closed airports, prohibited the lease of foreign aircraft with crew, or have introduced discriminatory measures towards foreign suppliers. These measures are found to have a significant impact, raising trade costs in air transport on average across countries by a further 5.4 percentage points to a total of 12.8% (Figure).

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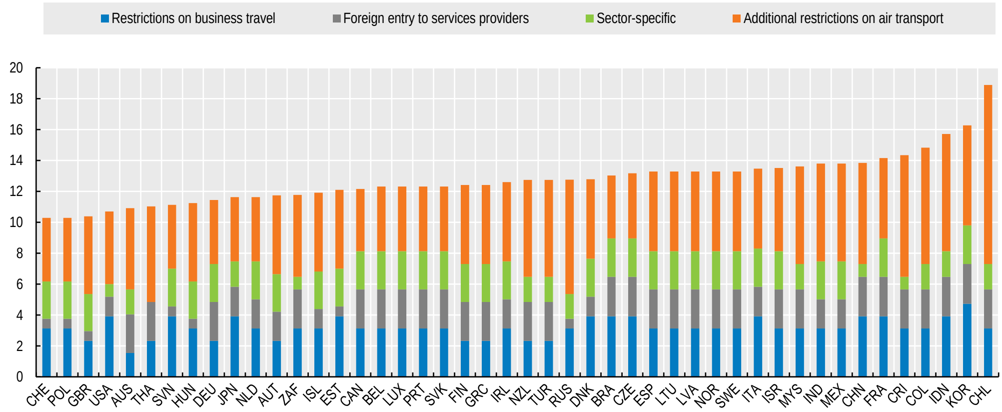
Figure 5 Rise in trade costs in air transport by country

Percentage of export value



Source: OECD calculations using Benz and Jaax (2020a).

4. Conclusions

Increasing restrictions on passenger travel are found to increase trade costs by 12% on average across countries and sectors in the medium term. A large variability can be observed across both dimensions, reflecting essentially the production structure and pre-existing levels of regulations. Repealing the restrictive measures introduced to address the current sanitary crisis, as conditions permit, will therefore be an important consideration in promoting sustainable economic recovery.

The present analysis is subject to several caveats. The most important is that the shock considered here is applied to all countries, while only some of the costs and features of the current environment have been captured. As such, the outcomes illustrate that emergency measures designed to tackle COVID-19 should be targeted, proportional, transparent and temporary - - but results should not be interpreted as a prediction of the likely effect of the travel bans in individual countries. In addition, the computation of trade costs makes use of elasticities estimated over time and therefore may not perfectly reflect the current environment and the unprecedented nature of the COVID-19 crisis.

Several extensions could enrich the analysis. First, a simulation using a general equilibrium model could help to quantify the international and intersectoral spillover effects from the travel restrictions. Second, it could be useful to identify regulatory or trade facilitation measures that would help to limit the expected increase in trade costs.

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COVID Social Distancing and the Poor: An Analysis of the Evidence for England

Parantap Basu[1], Clive Bell[2] and T.Huw Edwards[3]

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Social distancing is a matter of individuals' choices as well as of regulation, and regulation arguably responds to those choices. We analyse weekly panel data on such behaviour for English Upper Tier Local Authorities (UTLAs) from March to July 2020, paying attention to the influence of poverty, as measured by free school meals provision. Panel regressions suggest that, although more stringent regulation and slightly lagged local cases of infection increase social distancing, both effects are weaker in UTLAs with higher levels of poverty. Thus motivated, we develop a two-class (rich/poor) model, in which a Nash non-cooperative equilibrium arises from individual choices in a regulatory regime with penalties for non-compliance. The model yields results in keeping with the empirical findings, indicating the desirability of generous measures to furlough workers in low-paid jobs as a complement to the stringency of general regulation.

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1 University of Durham, UK
2 Universität Heidelberg, Germany
3 Loughborough University, UK

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1 Introduction

It is now well known that Covid-19 infections and fatalities can be lowered by social distancing. Different countries have adopted different measures. In a majority, there is mandatory social distancing (including lockdowns), while in a few, such as Sweden, Japan and in some US states, social distancing is left to people's choices. A number of studies have already indicated that social distancing is partly endogenous, in the sense that it responds, not just to policy – whether advisory or by diktat – but also voluntarily to the perceived threat (e.g. Toxvaerd (2020), Chudik et al. (2020)).

We address three questions, paying particular attention to inequality: (i) What are the private incentives for voluntary social distancing when people worry about infection and the loss of income? If people gauge their own private costs and benefits, what dictates their privately optimal choice of social distance? (ii) If the government comes up with distancing rules, with credible means of enforcement, what effect will they have on people's social distancing behaviour? Who are likely to respond more to these measures, poor or rich? (iii) If their responses differ, how do the aggregate benefits of other measures to reduce the ensuing costs of infection depend on the share of the poor in the whole population and their costs of voluntary distancing?

There is a rapidly growing literature on modelling Covid economies. Many of these models integrate epidemiological SIR models with DSGE, focusing on the dynamics of infection (see Section 2 for a brief survey). Our study is motivated by some recent evidence on infections and social distancing in England. We have daily COVID test statistics disaggregated by Upper Tier Local Authority (UTLA), which we are able to combine with data on social distancing from Google and regional poverty, as measured by free school meals. Our panel regression results indicate that greater stringency is associated with stronger social distancing; but the poor still distance less than the rich.

Motivated by these empirical results, we develop a simple static microeconomic model of social distancing in a two-class society of rich and poor, who differ in their respective costs of social distancing. A Nash equilibrium is analyzed, wherein agents may, in effect,

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free ride on others' choices when deciding on their own social distancing behaviour. Agents choose social distancing by minimizing the voluntary cost of distancing plus the expected costs of infection. Two scenarios are considered: (i) no legal enforcement of minimum social distancing, and (ii) a minimum is enforced, with a penalty for non-compliance. Our main results are, first, that the poor distance less than the rich, and second, that aggregate distancing falls not only with the voluntary costs of distancing faced by the poor, but also with the share of the poor in the population, despite the endogenous choices of the rich. The overall effect of this distancing behaviour is a higher incidence of infections. Thirdly, if a mandated minimum social distancing is imposed with a credible penalty, all agents respond by distancing more, but the poor relatively less. These results are consistent with the chief empirical findings for England.

Other measures to combat the pandemic include the development of a fairly efficacious vaccine and improved treatment of those infected. If social distancing is endogenous, the benefits yielded by any package of such measures may well depend on the cost of social distancing faced by the poor and their share in the population. The model yields the expected result that the benefits yielded by a given improvement in the package of measures employed rise with the said distancing cost. The same result holds for the population share, provided the value of a parameter reflecting the costs of infection for the rich is sufficiently close to that of the poor.

The paper is organized as follows. In the next section, Section 2, we summarise some of the developing literature on the Covid crisis and the relevant implications. In Section 3 we report our key empirical findings. Section 4 lays out the theoretical model, followed by the derivation of the main results. These are then simulated numerically in Section 5. Finally, we conclude the paper with a brief discussion.

2 Literature on social distancing, poverty and the pandemic

To place our paper in context: while a number of economists have always had an interest in the economics of communicable disease in poor communities (Malaney et al., 2004), the

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COVID-19 pandemic has spurred a good deal of research, and undoubtedly led to the development of much wider and deeper understanding of the issues (albeit much not yet published at journal level). We identify certain important strands.

First, a number of studies have started from the S-I-R (Susceptible-Infected-Resistant) model, central to the epidemiological approach to disease spread, adapting it to take account of behavioural changes in response to the pandemic - we can essentially term this 'endogenous social distancing'. Critical theoretical studies on this include Toxvaerd (2020). Eichenbaum et al. (2020) , Farboodi et al. (2020). Getachew (2020) uses a model of endogenous social distancing to integrate a SIR model into a DSGE framework. Crucially, empirical evidence has also been emerging of the relevance of treating social distancing as endogenous: A central study being Chudik et al. (2020)'s study of social distancing in China in the early stages of the epidemic there, which indicates that people do indeed respond to disease prevalence, albeit only once the disease has become widespread.

In contrast to this first strand of dynamic modelling, we need to consider a second strand of papers, which deals with the link between the vulnerability to disease and poverty. This is not novel to COVID: it has been well-known in the case of malaria (Malaney et al., 2004; Russel, 2004). In the early stages of COVID-19, studies suggested that wealthier areas were harder hit (Mukherji and Mukherji, 2020), although even at this stage greater inequality (Gini coefficient) implied more disease incidence and deaths. Preliminary U.S. county level analysis suggested existing rates of poverty, disease and the presence of ethnic minorities were all associated with higher infection rates (Abedi et al., 2020). Against this, enforced social distancing also has very strong differential effects upon the poor compared to the rich (Palomino et al., 2020; Bonaccorsi et al., 2020). Glover et al. (2020) also stress the important interaction between virus containment policy and differential impact on poorer social groups. Chan (2020) examines the the geography of social distancing in Canada using Facebook data and finds that people living closely together in apartment buildings find it difficult to stay home and maintain a safe social distance. This finding also tallies with Papageorge et al. (2006), who find that people living in these conditions are unlikely to engage in safe social distancing.

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Our paper is of interest because it aims at combining the two strands mentioned above: endogenous social distancing (albeit with less emphasis in this case on the dynamics) and the interaction of the disease with, and implications for poverty and inequality. Our results provide policy implications about the stringency of social distancing measures, and as such can be seen as complementing studies such as Dergiades et al. (2020), who use cross-country data to investigate the effectiveness of government policy stringency. In this regard, we are suggesting that, since poor and rich may respond differently, policy needs also to take this into account.

As such, our work can be seen as of interest to the United Nations' concerns on COVID-19, as summarised by the Secretary General: *"We must come to the aid of the ultra-vulnerable millions upon millions of people who are least able to protect themselves. This is a matter of basic human solidarity. It is also crucial for combating the virus. This is the moment to step up for the vulnerable."* - U.N. Secretary General Antonio Guterres, 23 March 2020.[1]

Since the thrust of the paper is to understand the differential effects of social distancing policies on the rich and the poor, an important qualification is needed concerning how the costs of infection are treated in the analysis. When life-and-death matters are involved, difficult ethical problems arise when formulating policy. Covid-19 fatality rates are under 1 per cent. This suggests that it is defensible to appeal to the statistical value of a life, as, for example, in the closing discussion in Goldstein and Lee (2020) and more centrally in Holden (2020). If we place the same monetary value on pain and suffering by rich or poor during a bout of infection, fatal or otherwise, then the remaining difference between the ensuing costs facing rich and poor will arise from differences in the cost of treatment and the loss of income resulting from a non-fatal bout. The sum of these components of the so-called cost-of-illness (COI) measure[2] is arguably larger for the rich than for the poor, even in a health system such

[1]From the UN DESA's page on the social impact of COVID. https://www.un.org /development/desa/dspd/everyone-included-covid-19.html. See also UN (2020)

[2]Commonly used in calculating the economic burden of a disease, the measure is defined as follows (see, for example, Malaney *et al.* [2004]):

$$\text{COI} \equiv \text{Private Medical Costs} + \text{Non-Private Medical Costs} + \text{Foregone Income} + \text{Pain and Suffering}.$$

Medical costs include expenditures on prevention, diagnosis, treatment, transport to medical facilities and, for the public sector alone, health facilities, medical education and research. These are termed the 'direct costs', the

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as the UK's; for the state's income replacement programme is proportional up to a cut-off.[3] Yet the difference is likely to be rather small.

3 COVID in England: stylized facts and empirical analysis.

A number of studies cited in Section 2 have already indicated that social distancing is partly endogenous, in the sense that it responds, not just to policy - whether advisory or by diktat - but that people also socially distance voluntarily in response to virus threats. We investigate certain questions empirically: namely, how do the poor differ in terms of measurable social distancing, and in terms of their responses both to virus threats, and to central policy stringency. It is already known that death rates are higher among poorer people, but this may reflect the greater prevalence of other medical conditions among the poor. Even evidence of greater disease spread among poor communities may reflect the effects of crowding rather than necessarily of a failure to change behaviour.[4] When it comes to measuring behavioural changes, however, extensive daily data are available from Google, which are differentiated regionally for some countries. It is these which we use.

We focus on the case of England, partly because it represents a single regulatory regime at any one time[5], but that regime in fact changed dramatically in mid-March, as policymakers revised their assessment of the situation. For England (rather than the whole UK) we have daily COVID test statistics disaggregated by Upper Tier Local Authority (UTLA), which we

remaining two items, the 'indirect costs'. 'Foregone Income' includes the losses caused by premature mortality. To give examples for communicable diseases, Malaney et al. (2004), citing studies of Burkino Faso, Chad, the Republic of the Congo and Rwanda in the late 1980s, put the COI of a case of malaria at \$9.84, of which only \$1.83 took the form of direct costs. (That elusive item 'pain and suffering' was apparently not estimated.) For seasonal influenza in Germany in the years 2012-2014, Scholz et al. (2019) estimate direct costs at 82.90 Euros with 576.54 Euros for 'sick leave', the latter varying strongly over age groups. Neither premature mortality nor pain and suffering is mentioned.

[3] The UK government's Coronavirus Job Retention Scheme is summarised on https://www.gov.uk/guidance/claim-for-wages-through-the-coronavirus-job-retention-scheme. Workers may be furloughed, with the government paying 80% of salary, up to a maximum of £2500 per month. Employers may or may not make up the difference. Workers on furlough retain normal rights (statutory sick pay, pensions etc). There is a similar scheme for the self-employed; the government pays up to 80% of the average monthly profits for the last 3 years, again up to a maximum of £2500 per month. These schemes are currently expected to end in October.

[4] The US study by Papageorge et al. (2006) is a useful contribution, however.

[5] Or rather, did until the government imposed local lockdowns in July, starting in Leicester

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are able to combine, in particular, with social distancing data from Google.

3.1 Data sources

Daily and cumulative data on diagnosed cases are presented by English UTLA on the UK governent's coronavirus dashboard, [6] along with daily total and cumulative deaths, although only for England as a whole. We concentrate on data for the period 5 March-25 July, and in fact, since we aggregate local cases weekly (to reduce zeroes) we effectively start on 12 March.

Figure 1 shows a clear pattern in the initial wave of the infection: taking off in the middle of March and peaking in early April (the lockdown from 23 March halting and reversing the growth of the disease with roughly a 3 week delay), and then declining gradually. Importantly, it also shows considerable heterogeneity across the 83 local authorities at any one time. We drop Leicester from our sample as an outlier, also partly because it had a local lockdown in July. Figure 1 also shows an outlier in the earlier stages of the disease - this was Devon, and is also dropped.[7]

The Blavatnik School at Oxford[8] has compiled daily indices of policy stringency for many countries: we show the UK in Figure 2, alongside a series of French stringency data from the same source, for comparison.

Google publish daily data on several measures of social distancing, based upon logins with mobile phones. These have been available online since April 2020 as the Google Community Mobility Reports.[9] In the UK, these are distinguished by Upper Tier Local Authority (UTLA), although data are aggregate for metropolitan counties. We concentrate on four of their six series: RESIDENTIAL logins (which increase with social distancing), WORK logins and TRANSIT logins (which decline with social distancing) and retail and recreational (RETREC) logins. Although there are clear trends in social distancing over time across Eng-

[6]on https://coronavirus.data.gov.uk/category=utlasmap=rate

[7]Incidentally, despite its salubrious image, Devon has the highest rate of poverty in England, measured by free school meals. Leicester is 13th out of 83 UTLAs.

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

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

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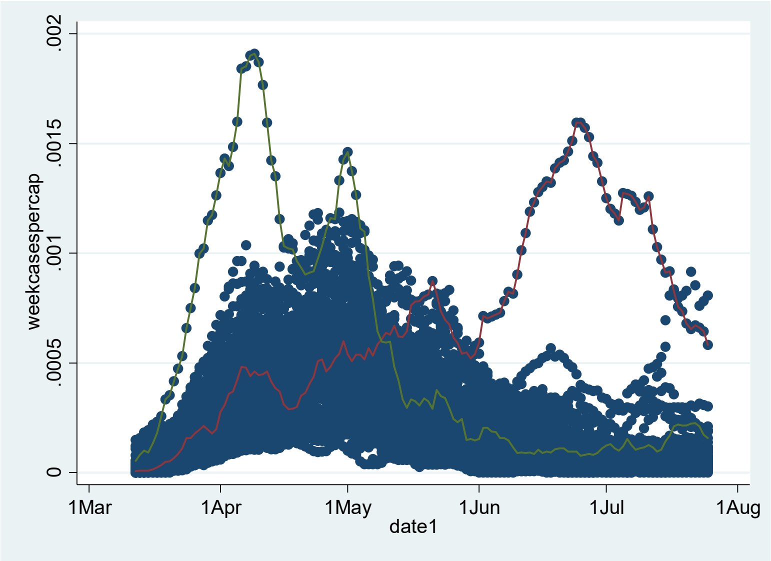

Figure 1: England:New diagnosed cases per week by local authority. Leicester in red. Devon in green.

land, there are also significant differences by local authority: something which cannot be attributed to differences in national policy stringency. Hence, as with the UTLA COVID data, we have cross-sectional variation, as well as variation over time (see Figures 7 and 8 in the Appendix).

3.2 Panel Regression analysis of social distancing

In this section, we estimate behavioural equations for social distancing, focusing on its response to policy stringency and to disease rates, and the role of local poverty variations in accounting for differences in the behavioural responses.

We define $X_{i,j,t}$ as an index of social distancing variable i across UTLA, j, and time, t. There are four social distancing variables, which we index i = [RESIDENTIAL, WORK,

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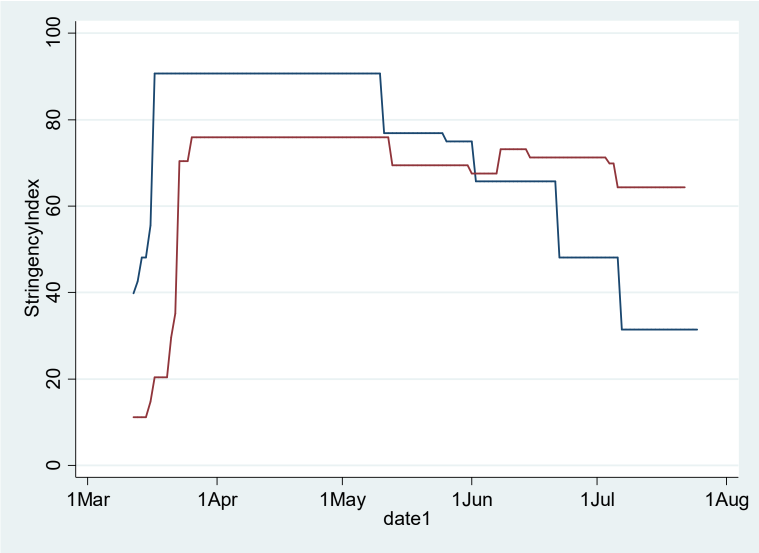

Figure 2: Oxford Index of Policy Stringency for the UK in red. France is in blue for comparison.

TRANSIT and RETREC (Retail/Recreational) logins]. We should note that for RESIDENTIAL logins, an increase (+) indicates increasing social distancing, while for the other three variables a decrease (-) in the variable indicates greater social distancing.

We wish to estimate these four social distancing variables as functions of national policy stringency, S_t, local weekly cases of covid $I_{j,t}$, and indices, $P_{k,j}$, of local time-invariant features, k, such as poverty rates (which we proxy by the proportion of children receiving free school meals) and population density. In addition, we need to incorporate day of the week and bank holiday dummies, which we denote as $D_{d,t}$, where d is the type of dummy (e.g. bank holiday). Hence we formulate

$$X_{i,j,t} = X\left\{S_t, I_{j,t}, P_{k,j}, D_{d,t}\right\}. \tag{1}$$

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Since the social distancing variables, $X_{i,j,t}$ can take positive or negative values, it is not appropriate to use a logarithmic formulation - for simplicity we apply a linear formulation, such as the following, which can be estimated either by ordinary least squares or as a *random effects* (RE) model:

$$X_{i,j,t} = \alpha_i + \beta_{i,1} S_t + \beta_{i,2} I_{j,t} + \sum_k \gamma_{i,k} P_{k,j} + \sum_k \{\delta_{i,1,k} P_k S_t + \delta_{i,2,k} P_{k,j} I_{j,t}\} + \sum_d \theta_{i,d} D_{d,t} + u_{i,j,t}. \tag{2}$$

The day type variables, $D_{d,t}$ are essentially included as controls. We also interact these with the other variables (S_t, $I_{j,t}$ etc), but again just as controls.

It is also possible to estimate the corresponding *fixed effects* (FE) model. In the first instance, this incorporates UTLA-specific errors, $\epsilon_{i,j}$, which imply that time-invariant UTLA-specific data, such as population density and free school meal uptake have to be dropped due to multicollinearity. In addition, one can incorporate time-specific fixed effects or dummies, $\eta_{i,t}$. Note that, if we include these, then we can no longer take advantage of the variation in time in national policy stringency (since changes in this were common, over time, to all UTLAs, until the local lockdown in Leicester in July), or indeed in common (national) time trends in infection rates, although local variations in infection rates and the interaction terms with UTLA-level socioeconomic variables remain. Hence the equivalent fixed effects model to Equation 2 is:

$$X_{i,j,t} = \alpha_i + \beta_{i,2} I_{j,t} + \sum_k \{\delta_{i,1,k} P_{k,j} S_t + \delta_{i,2,k} P_{k,j} I_{j,t}\} + \epsilon_{i,j} + \eta_{i,t} + u_{i,j,t}. \tag{3}$$

In general, while statistical tests (Hausman or Sargan-Hansen) often favour the use of fixed effects, dependent upon the data, these may end up proxying most of the variation in variables such as disease rates, in which case an OLS or random effects model may be more informative.

Our tentative hypothesis is not only that greater stringency should raise social distancing, but also that the effect of this policy would depend on various local fixed effects. Among these fixed effects, we focus on poverty, which is proxied by free school meals (fsm). This motivates us to add interaction terms with fsm. We also include interaction terms with population density as a control. We find that in both RE and FE regressions, greater stringency has a direct positive effect on social distancing, but this effect is weakened where poverty, as measured by fsm, is more widespread: that is to say, β_{i1} and δ_{i1} in Equation 2 have opposite signs, at least for RESIDENTIAL and WORK variables in Table 1 below. These findings are in keeping with the theoretical results derived in Section 4.

To review this evidence in some more detail, we examine the data series for social distancing, focusing on the RESIDENTIAL variable. Figure 7 in the Appendix shows the scatterplot of UTLA-level data across time: series for the 10 poorest UTLAs (in terms of free school meals) are shown as lines within the scatterplot. These congregate towards the lower end of the cross-sectional range at most points during the wave of the virus: as these values are changes compared to 'normal', the indication is that the poorer districts responded with relatively less change in behaviour. The difference between the poorest 11 UTLAs (residentialmean7), the 10 least poor (residentialmean1) and a middle group of 10 can be seen in figure 3a below. This shows the overall pattern of social distancing, increasing and then falling back, but also that the poorest UTLAs increased social distancing less than the others. Figure 3b shows the differences between these lines, indicating that the differences between the richest and middle groupings was relatively small, but that the poorest grouping socially distanced much less than the others in April, with this gap narrowing in May and June, as disease rates fell and there was a slight easing of the lockdown. The differential behaviour of the two differences in 3b should be taken as an indication that relationships between poverty rates and social distancing may not be linear, an issue which may perhaps merit further investigation.

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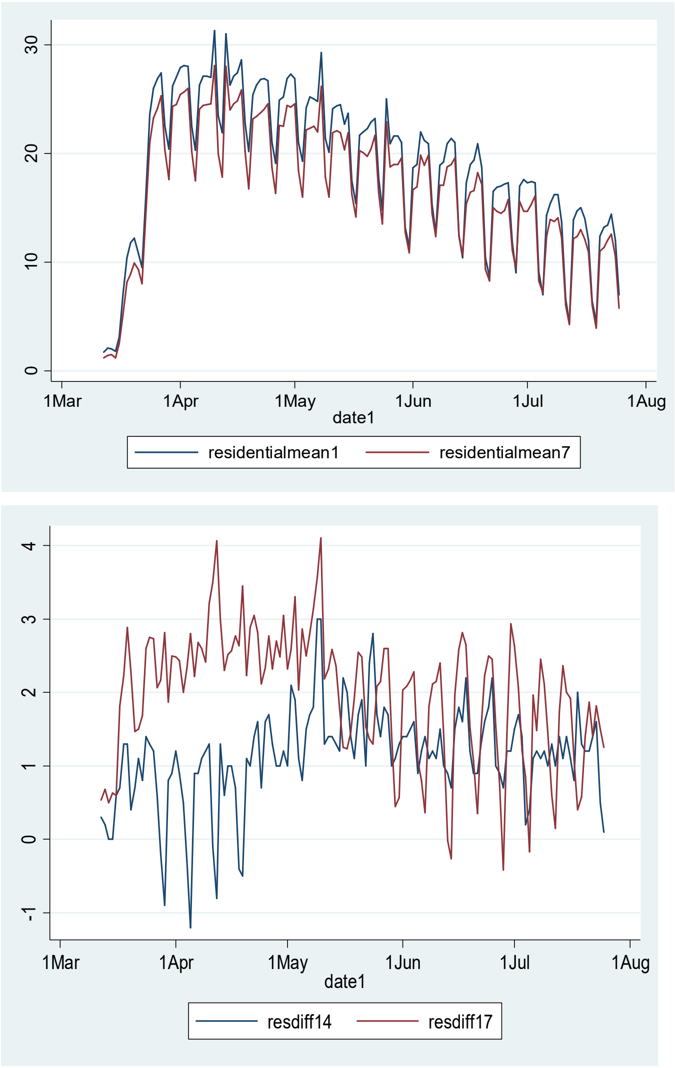

Figure 3: a) Mean RESIDENTIAL logins by UTLAs ranked 1-10 (Group1), 40-50 (group4) and 70-81 (group7) in terms of lowest free school meals uptake. Group 1 has the least poverty, and 7 has the most. b) Differences between Groups 1 and 4 and Groups 1 an 7 over time.

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Notwithstanding the last point, we start with a linear (rather than loglinear) formulation for tractibility: hence, it makes sense to normalise all RHS variables, by dividing by their mean values. This means that estimated coefficients are marginal effects at the mean values of the RHS variables.

Table 1: Panel random and fixed effects regression analysis of the RESIDENTIAL and WORK variables.

	(1)	(2)	(3)	(4)	(5)	(6)
DEPENDENT VARIABLE	RESIDENTIAL	RESIDENTIAL	RESIDENTIAL	WORK	WORK	WORK
MODEL	RE	UTLA FEs	UTLA and time FE	RE	UTLA FEs	UTLA and time FE
stringency	17.51***	17.48***	15.45***	-46.02***	-45.95***	-54.09***
	(0.616)	(0.614)	(0.589)	(1.188)	(1.187)	(0.950)
Weekly cases per capita	4.304***	4.327***	0.197	-9.295***	-9.359***	-0.134
	(0.313)	(0.316)	(0.171)	(0.793)	(0.804)	(0.184)
pop density	-0.0695			-0.275		
	(0.123)			(0.385)		
free school meals share	-0.837***			2.718***		
	(0.324)			(0.888)		
Pop density $*$ stringency	0.508***	0.508***	0.131	-0.997***	-0.999***	-0.158
	(0.167)	(0.169)	(0.0857)	(0.283)	(0.288)	(0.209)
Pop density $*$ weekly cases pc	0.330***	0.334***	0.0961	-0.412*	-0.420*	0.112
	(0.115)	(0.116)	(0.0632)	(0.215)	(0.219)	(0.0840)
Free school meals $*$ stringency	-1.715***	-1.707***	-0.983**	1.895*	1.894*	0.274
	(0.619)	(0.615)	(0.448)	(1.126)	(1.116)	(0.529)
Free school meals $*$ weekly cases pc	-1.425***	-1.438***	-0.255*	2.539***	2.565***	-0.0751
	(0.279)	(0.283)	(0.151)	(0.667)	(0.681)	(0.142)
Saturday, Sunday and Bank Holiday variables omitted						
Date fixed effects omitted						
Constant	1.273***	0.394*	-0.926***	-2.347**	0.0522	8.042***
	(0.359)	(0.206)	(0.268)	(0.997)	(0.430)	(0.556)
Observations	10,773	10,773	10,773	10,773	10,773	10,773
Number of lacode1	81	81	81	81	81	81
R-sqd within	0.7244	0.7244	0.9001	0.8188	0.8178	0.9814
R-sqd between	0.0671	0.0387	0.1702	0.0199	0.0004	0.0912
R-sqd overall	0.6380	0.6345	0.8125	0.7558	0.7521	0.9249

Note: Robust Standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Leicester and Devon are dropped as outliers. Sargan-Hansen χ^2 for random effects=1624.716 (5) for RESIDENTIAL . Sargan-Hansen χ^2 for random effects 4392.154 (5) for WORK.

Table 1 summarises the initial results of this analysis. Starting with the first three columns, which refer to the social distancing variable RESIDENTIAL: as discussed above, an increase in this variable can be considered an increase in social distancing. Column (1) is a panel random effects regression across all 81 UTLAs (excluding Devon and Leicester), without any instrumentation or time or UTLA dummies. In column (2) we include fixed effects for UTLAs only, while in column (3), time dummies (fixed effects) are also included. It is worth noting that estimated coefficients are almost identical across the first two models. Tentatively, we therefore discuss the equation in panel (1) as our preferred choice.

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We can note that the estimated coefficient on stringency for RESIDENTIAL logins is +17.51, indicating that increased policy stringency (lockdown) increases social distancing. This variable is modestly but significantly increased by population density (marginal effect of the interaction +0.508), and reduced (-1.715 mean marginal effect) by the free school meals variable (the poor respond less to policy, on this measure).

Our model also provides evidence of endogenous social distancing: local weekly cases per capita[10] have a positive effect (+4.304 at the mean) on RESIDENTIAL logins. This is only about 1/4 of the effect of stringency (although at the peak weekly cases went well above their mean level, so implying a stronger effect). Greater population density significantly increases this endogenous social distancing (by +0.330), but the endogenous social distancing effect is reduced (by -1.425 or about 1/3 at the mean) by poverty, as measured by free school meals.[11]

For comparison, the Fixed Effects model with time dummies in column (3) shows much smaller and less significant coefficients (except on stringency), although the interaction between weekly cases and free school meals remains negative and significant at the 10 % level. In general, the fixed effects model with time dummies/fixed effects provides a stronger statistical fit (as indicated by the Sargan-Hansen test)[12], but is less behaviourally informative. This indicates that the most useful information on social distancing comes from the longitutinal (time-varying) dimension of the model, which is lost when time fixed effects are included.

Moving to WORK logins in columns (4)-(6), many of the same comments apply as with RESIDENTIAL logins, except that the signs on estimated coefficients are reversed (since lower work logins imply more social distancing). Stringency is clearly strongly significant: -46.02 in column (4), with marginal effects also of -0.997 from the interaction with population density, but +1.895 from the interaction with free school meals. It seems poverty weakens the reaction to policy stringency, Regarding endogenous social distancing, there is a strong negative effect (-9.295) from weekly cases, but reduced significantly (+2.539) by the interaction

[10]Cases diagnosed in the previous 7 days

[11]Weekly cases respond to social distancing measures only with 2-3 week lags, so we judge endogeneity not to be a major issue with this variable.

[12]We use Sargan-Hansen rather than the Hausman test due to the use of robust standard errors.

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with free school meals. Hence, poverty reduces endogenous social distancing. Again, free school meals have a significant intercept, so there may be some nonlinearity of functional form not being picked up in this model. The model with UTLA fixed effects in column (5) is virtually identical to the random effects model while once time fixed effects are included (column (6)), significance is lost on almost all variables except policy stringency.

Columns (4)-(6) are equivalent regressions with WORK logins as the dependent variable. Note that, in this case, lower WORK indicates greater social distancing. Again, the three specifications produce very similar results, and we focus on the instrumental variables RE model in (5). The intercept terms on population density and free school meals are insignificant here. Greater poverty (measured by free school meals) reduces the effect of weekly cases per capita, and also weakens the effects of policy stringency (it seems that the poor were more likely to keep working during the lockdown).

In Table 5 in the Appendix, we carry out equivalent panel regressions for the TRANSIT and RETREC (retail/recreational) login data. There is again a strong overlap with the results for the RECREATIONAL and WORK variables.

3.3 Robustness tests of the modelling

In this section, we focus mainly on the robustness of modelling RESIDENTIAL logins, focusing on the issues of possible endogeneity and of making the relationship with free school meals less linear.

In Table 2 we carry out some checks on alternative specifications. We start by considering the potential endogeneity of the right hand side variables in equation 2. Since there are lags of 2-3 weeks in the response of recorded disease cases to social distancing[13], we are less concerned with endogeneity of weekly cases, and more concerned about policy stringency, which could be seen as responding to cases.

Column (1) in Table 2 modifies the regression (1) in Table 1 by replacing the Oxford stringency variable with two dummies, for the lockdown (starting 23 March) and pre-lockdown

[13]This is confirmed empirically, results available from authors, but also makes sense given a 1-2 week incubation period and a further lag until diagnosis

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Table 2: Robustness checks for RESIDENTIAL variable.

VARIABLES	(1) LOCKDOWN RES	(2) IV RES	(3) ENGLANDCASES RES	(4) wkcas*stringency RES
prelockdown	7.602***		7.341***	
	(0.385)		(0.664)	
lockdown	9.523***		9.118***	
	(0.430)		(0.428)	
weekly cases per capita	5.528***	4.309***	4.222***	-0.467
	(0.377)	(0.118)	(0.134)	(2.553)
pop density	-0.182*	-0.0679	-0.0907	0.254
	(0.110)	(0.259)	(0.289)	(0.217)
pop density ∗ prelockdown	0.180*		0.197	
	(0.0945)		(0.229)	
pop density ∗ lockdown	0.488***		0.335**	
	(0.179)		(0.144)	
pop density ∗ weekly cases pc	0.391***	0.331***	0.313***	0.358***
	(0.136)	(0.0469)	(0.0538)	(0.115)
fsm	-0.320*	-0.873	-0.432	-1.103***
	(0.189)	(0.780)	(0.848)	(0.417)
free school meals ∗ prelockdown	-0.0943***		-0.0939**	
	(0.0231)		(0.0461)	
free school meals ∗ lockdown	-0.0670**		-0.0501*	
	(0.0335)		(0.0293)	
free school meals ∗ weekly cases pc	-1.799***	-1.430***	-1.376***	-1.450***
	(0.334)	(0.116)	(0.133)	(0.277)
stringency		17.46***		16.83***
		(0.483)		(0.693)
pop density ∗ stringency		0.506***		0.178
		(0.139)		(0.255)
free school meals ∗ stringency		-1.675***		-1.398**
		(0.464)		(0.633)
		(0.00672)		(0.00979)
england weekly cases			1.547***	
			(0.110)	
pop density ∗ england weekly cases			0.0340	
			(0.0358)	
free school meals ∗ england weekly cases			-0.111	
			(0.113)	
weekly cases pc ∗ stringency				4.686*
				(2.446)
Constant	1.596***	1.317*	1.611*	2.055***
	(0.182)	(0.781)	(0.844)	(0.502)
Saturday, Sunday and Bank Holiday variables omitted				
Observations	11,016	10,773	11,016	10,773
Number of lacode1	81	81	81	81
R-sqd within	0.6751	0.7244	0.7256	0.7033
R-sqd between	0.0468	0.0670	0.0685	0.0721
R-sqd overall	0.5824	0.6380	0.6208	0.6414

Note: Robust Standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Leicester and Devon are dropped as outliers. Sargan-Hansen χ^2 for instrumentation in 4961.423 (2).

(starting one week earlier). This is supported by the argument that the UK, which responded later than most of its neighbours, underwent a dramatic rethink in the middle of March, shifting from a mild policy of recommended social distancing to a fairly strong lockdown in the

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space of a few days. Comparison with Table 1 column (1) indicates that this rather simpler representation of stringency does not make a great deal of difference to the estimated equation, but somewhat increases the proportion of the variance of RESIDENTIAL logins attributed to weekly cases. In column (2) we extend this line by instrumenting stringency with a combination of lockdown and prelockdown dummies, and the series for French policy stringency shown in 2. This instrumentation produces a regression almost identical to Table 1 Column (1), indicating that the instruments are strong: however, a Sargan-Hansen test questions the statistical validity (they are correlated with English weekly cases, even if not directly caused by them). In Column (3), we modify column (1) by adding a series for national weekly cases for England: the regression suggests that endogenous social distancing is responding partly to national, and partly to local cases, although the combined effect is not greatly changed. In Column (4), we modify Column (1) of 1 by introducing an interaction term for weekly cases and stringency. This is strongly significant, while the term for weekly cases per capita becomes insignificant (since the two are highly correlated). One possibility is that this implies that endogenous and mandated social distancing are complementary (or that official support made endogenous social distancing decisions more viable): however, alternatively, it may simply be that, as in Chudik et al. (2020), there was a delay in endogenous social distancing, until the public became more aware of the problem.

Table 3 examines the response of the RESIDENTIAL social distancing variable, by splitting UTLAs into 7 groups according to free school meal uptake, from 1 = lowest (least poverty) to 7=highest. This confirms the hints in the previous section that the relationship between free school meals uptake and social distancing is not strictly linear: in particular, there is an indication that the size of both the stringency and weekly cases variables begins a significant decline with respect to free school meals only in the last 3 groups. This is worthy of further investigation.

Table 3: Regressions for RESIDENTIAL variable run separately on 7 groups ranked according to free school meals.

VARIABLES	(1) Fsm group 1	(2) Fsm group 2	(3) Fsm group 3	(4) Fsm group 4	(5) Fsm group 5	(6) Fsm group 6	(7) Fsm group 7
stringency	15.29***	15.81***	12.30***	14.99***	14.33***	14.65***	13.50***
	(0.929)	(0.707)	(0.467)	(0.697)	(0.972)	(0.603)	(0.511)
weekly cases per cap	3.133***	3.731***	3.838***	3.535***	2.874***	2.824***	2.410***
	(0.294)	(0.402)	(0.371)	(1.021)	(0.498)	(0.491)	(0.257)
sat	-5.130***	-4.968***	-6.951***	-4.951***	-3.856***	-6.537***	-4.506***
	(0.992)	(0.873)	(0.602)	(1.071)	(1.143)	(1.007)	(0.986)
sun	-7.149***	-6.288***	-9.957***	-6.830***	-5.167**	-9.077***	-6.551***
	(1.413)	(1.258)	(0.593)	(1.442)	(1.583)	(1.232)	(1.395)
bank holiday	5.154***	3.890***	5.370***	5.004***	5.255***	5.258***	4.651***
	(0.587)	(0.842)	(0.374)	(0.781)	(0.651)	(0.612)	(0.593)
Constant	2.280*	2.246*	3.718***	1.460*	0.531	2.468***	1.655**
	(1.171)	(1.030)	(0.315)	(0.753)	(0.917)	(0.605)	(0.588)
Observations	1,330	1,330	1,330	1,330	1,197	1,330	1,596
R-squared	0.708	0.693	0.839	0.654	0.664	0.752	0.727
Number of lacode1	10	10	10	10	9	10	12

Note: Robust Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All models are estimated with UTLA fixed effects (but no time fixed effects).

4 Social Distancing: Individual and Aggregate

The population's members form a continuum, with individuals $i, j \in [0, 1]$. Individual i has an endowment ω_i and chooses – or is subject to – a social distance x_i. His or her net pay off is

$$y_i = \omega_i - c_i(x_i) - \ell_i - t_i, \tag{4}$$

where the function $c_i(x_i)$ is increasing in x_i and strictly convex, the loss arising from the chance of infection is

$$\ell_i = \max[0, (B - x_i)I_i(X)], \tag{5}$$

$$I_i(X) = I_{i0} - \lambda \int_0^1 x_j dj \equiv I_{i0} - \lambda X, \ \lambda > 0, \tag{6}$$

$I_{i0} > 0$ is a constant, and t_i is a lump sum tax. The loss index I_i has a fixed component specific to individual i and a common component that is decreasing in the aggregate social distance X, thus reflecting the nature of a communicable disease as a common property 'bad'. The term $B - x_i$ reflects i's ability to reduce I_i, even to zero, by choosing a personal level of social distancing closer to the common environmental parameter B. The term $B - x_i$ depends

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directly only on i's choice, not his or her characteristics. Individuals i and j who choose the same x $(< B)$ will suffer different losses if $I_{i0} \neq I_{j0}$.

We consider a two-class society, the poor, P, and the rich, R, whose members are indexed such that $P = \{i \in [0, n]\}$ and $R = \{i \in (n, 1]\}$, with individual endowments ω_p and ω_r, respectively. The distancing cost functions $c_i(x_i)$ and the infection cost parameters I_{p0} and I_{r0} may differ by class. It is arguable that the functions c_i depend directly on the respective endowments. The poor live in more crowded conditions than the rich, which make social distancing inherently more difficult, and having very limited means, they are under strong pressure to work, whereby their jobs are not usually the sort whose tasks can be performed at home.

The cost function is assumed to be quadratic:

Assumption 1. $c_i(x_i) = a_i x_i^2/2,\ i \in P, R.$

Any difference between a_p and a_r arises from, and reflects, inequality in the distribution of the aggregate endowment, whereby it is assumed that a_i is non-increasing in ω_i. A central element of the analysis that follows involves increasing a_p holding a_r constant. Given any population share n, this stems from reducing ω_p, with ω_r fixed.

Given any choice x_i and the aggregate X, the size of the loss ℓ_i depends on the parameters I_{i0} and λ, the latter being a common, environmental parameter. The discussion of the cost-of-illness measure in Section 1 indicates that the difference between I_{p0} and I_{r0} is likely to be small, but with I_{r0} at least as large as I_{p0}. Where differences in the two forms of costs are concerned, we make the following assumption:

Assumption 2. The cost factors are such that $a_p \geq a_r$ and $I_{r0} \geq I_{p0}$, with at least one holding as a strict inequality.

4.1 The policy problem

The public measures available to improve on outcomes in the above setting are broadly of two kinds. First, and directly, the government can impose binding restrictions on individuals' choices, with penalties for violations. Secondly, it can attempt to reduce the level of B, and

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thus the losses $\{\ell_i\}$ for any given pattern of distancing $\{x_i\}$, by making suitable expenditures.

In order to analyse choices among policies, a social welfare function is needed. Let

$$W = \int_0^1 y_j dj = \int_0^1 [\omega_j - c_j(x_j) - \ell_j] dj - C(B), \tag{7}$$

where $C(B)$ is the cost of the measures taken, if any, to reduce B. The function W exhibits no aversion to inequality, but the tax profile $\{t_j\}$ can be chosen so that taxes do not change the ranking of net endowments, $\omega_j - t_j$. The total revenues $\int_0^1 t_j dj$ $(\leq \int_0^1 \omega_j dj \equiv \Omega)$ just finance $C(B)$ at some chosen, feasible B.

The policy problem is to maximise W, employing whatever instruments are actually available. Motivated by the findings in Section 3, we first analyse measures of enforced distancing, treating B as exogenous. The benefits of reductions in B are analysed in Section 4.3.

4.2 A minimum social distance rule

Bearing Figure 2 in mind, let individual choices $\{x_i\}$, at some point in time, be suddenly subject to a uniform restriction. The government announces a minimum social distance x^s, with a fixed penalty θ for a violation. In practice, enforcement may not be complete, and the probability of getting caught will depend on, *inter alia*, how gross the violation is, as measured by $x^s - x_i$. To be specific, let it take the following form:

Assumption 3. The probability of paying the penalty is $p(x_i; x^s) = \max(0, 1 - x_i/x^s)$. Faced with this risk when choosing x_i, individual i's expected net pay-off is

$$y_i = \omega_i - c_i(x_i) - \ell_i - t_i - \theta \cdot [\max(0, 1 - x_i/x^s)]. \tag{8}$$

Individuals are assumed to make Nash conjectures concerning their fellow citizens' choices of x when deciding on their own. Suppose, to start with, that θ is sufficiently modest that x^s is always violated to some degree. This state will be called the limited-compliance regime.

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Noting that y_i is strictly concave in x_i, individual i's f.o.c. is, provided $\ell_i > 0$,

$$-a_i x_i + (I_{i0} + \theta/x^s) - \lambda X \leq 0,\ x_i \geq 0,\ i \in P, R, \tag{9}$$

Observe that individuals' choices are independent of the environmental parameter B if, and only if, $B > \max_i(x_i)$.

It is assumed that there is symmetric behaviour within each group. Let (x_p^*, x_r^*) solve (9). Then, assuming $(x_p^*, x_r^*) >> \mathbf{0}$,

$$x_p^*(x^s) = \frac{(I_{p0} + \theta/x^s)a_r - \lambda(1-n)(I_{r0} - I_{p0})}{a_p a_r + (a_r n + a_p(1-n))\lambda}, \tag{10}$$

$$x_r^*(x^s) = \frac{(I_{r0} + \theta/x^s)a_p + \lambda n(I_{r0} - I_{p0})}{a_p a_r + (a_r n + a_p(1-n))\lambda}, \tag{11}$$

$$X^*(x^s) = \frac{n(I_{p0} + \theta/x^s)a_r + (1-n)(I_{r0} + \theta/x^s)a_p}{a_p a_r + (a_r n + a_p(1-n))\lambda}. \tag{12}$$

If individuals are unrestrained by any public measures to enforce social distancing, their choices in equilibrium are obtained from the above by setting $\theta/x^s = 0$. The absence of any minimum distancing rule provides a benchmark.

The said closed forms yield the following proposition under assumptions 1, 2 and 3.

Proposition 1. *If $(x^s, \theta, B, \lambda)$ are such that $\ell_i > 0$ $(i \in P, R)$, then:*
(i) $x_r^* > x_p^*$;
(ii) $x_p^* > 0$ if $(a_r + (1-n)\lambda)(I_{p0} + \theta/x^s) > (1-n)\lambda I_{r0}$;
(iii) x_p^* is decreasing in the own cost parameter a_p and x_r^*/x_p^* is increasing in a_p; and
(iv) the aggregate distance X^* is decreasing in a_p and n.

Proof. Parts (i) - (iii): obvious. Part (iv): see appendix.

Remark. As n increases, individuals who distance less replace those who distance more; but in equilibrium, individuals of both types also respond to n in their distancing behaviour. Inspection of (10) and (11) in the light of assumption 2 reveals that both $x_p^*(x^s)$ and $x_r^*(x^s)$ are increasing in n. Part (iv) establishes, however, that the replacement effect dominates the

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individual adjustment effect. This result is consistent with the findings in section 3 that social distancing is less in UTLAs with high takeup of free school meals.

Part (iv) is a key result. By inducing reductions in the aggregate distance X^*, increases in a_p and n result in heavier losses by increasing the (infection) index I_{i0} for rich and poor alike. This endogenous response lends importance to B when that parameter can be influenced by policy, as in Section 4.3.

It remains to check that $B > \max_i(x_i)$. In view of part (i), the required condition is

$$B > \frac{(I_{r0} + \theta/x^s)a_p + \lambda n(I_{r0} - I_{p0})}{a_p a_r + (a_r n + a_p(1-n))\lambda}. \tag{13}$$

In what follows, it is assumed that this condition is satisfied.

There is also the possibility that θ is so draconian that all comply with x^s. Since $x_p^* < x_r^*$, the limiting value of θ which induces full compliance with x^s is such that $x_p^*(x^s) = x^s$. In order that (11) hold in equilibrium, however, no rich individual's best response to $x_p = x^s$ may be an $x_i \in [x^s, x_r^*(x^s))$, thus still avoiding any penalty. It is seen from the analogue of (9), when x_i must be at least as large as x^s that a necessary and sufficient condition to rule out such a deviation is $I_{r0} < (a_r + \lambda n)x^s + (1-n)x_r^*(x^s)$. It follows from (10) that the limiting value of θ is

$$\theta^* = (x^s/a_r)\left\{[a_p a_r + (a_r n + a_p(1-n))\lambda]x^s + \lambda(1-n)(I_{r0} - I_{p0}) - a_r I_{p0}\right\}.$$

No fines are collected, and it is clear that given x^s, it is never optimal to impose a penalty exceeding θ^*, though it may be optimal to impose a smaller one.

Part (iv) of proposition 1 concerning n also holds in the full-compliance regime, again in keeping with the empirical findings.

Proposition 2. *If $\ell_i > 0$ $(i \in P, R)$, then X^* is decreasing in n in both regimes.*

Proof. See appendix.

A second, salient empirical finding is that the effect of stringency on $X^*(x^s)$ is weaker when n is large. This result is supported by the signs of the interaction term for free school

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meals $*$ stringency for RESIDENTIAL and WORK in Table 1 (although these are insignificant for the TRANSIT and RETREC variables in Appendix Table 5). In the light of Figure 2 we therefore compare $X^*(x^s)$ with X^*, noting that (12) holds for all $\theta \leq \theta^*$. We have

$$X^*(x^s) - X^* = \frac{(na_r + (1-n)a_p)(\theta/x^s)}{a_p a_r + \lambda(na_r + (1-n)a_p)} = \frac{\theta/x^s}{\lambda + a_p a_r/(na_r + (1-n)a_p)}.$$

If, plausibly, $a_p > a_r$, it follows that $X^*(x^s) - X^*$ is decreasing in n. Although it is increasing in the ratio θ/x^s, that effect also weakens as n increases. To summarise:

Proposition 3. *If $\ell_i > 0$ $(i \in P, R)$, then the effect of the introduction of the policy (x^s, θ) on the aggregate $X^*(x^s)$ weakens with n, as does the effect of an increase in θ/x^s.*

4.3 The benefits of reductions in B

Distancing rules may be accompanied by public expenditures or regulation aimed at reducing the parameter B. Development of a vaccine followed up by a mass vaccination campaign is one measure. Improvements in the whole health system's capacity and specific treatments of infection also commend themselves. In view of the fact that developing at least one efficacious vaccine will be an international undertaking in the broad sense, we examine the benefit generated by a reduction in B, without going into how much it costs and how it is financed.

Under the distancing rules of Section 4.2 and given the hypothesis that B remains such that $\ell_i > 0$ $(i \in P, R)$, the benefit generated by a small reduction in B is

$$\Delta_B = (nI_{p0} + (1-n)I_{r0} + \theta/x^s) - \lambda X^*(x^s). \tag{14}$$

The cost parameter a_p and the population share n influence this marginal benefit as follows:

Proposition 4. *If $B > x_r^*(x^s)$, then Δ_B is increasing in a_p, and also in n if $I_{r0} - I_{p0}$ is sufficiently small.*

Proof. Differentiate partially and apply part (iv) of proposition 1. ■

Remark. In virtue of part (iv) of proposition 1, increases in a_p and n each induce reductions

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in the aggregate distance $X^*(x^s)$ and hence, *cet. par.*, larger losses ℓ_i. Any given reduction in B will temper these losses, and will do so the more strongly, the larger they are; for the term $(B - x_i)$ multiplies the index $(I_{i0} - \lambda X)$. It should be noted from (14) that the result holds in the limiting case $I_{p0} = I_{r0}$.

5 Illustrative simulations

We report here some simulation results for our model. We focus on the scenario of partial compliance. The baseline parameter values are: $a_p = 1, a_r = 0.5, I_{p0} = 0.5, I_{r0} = 0.6, n = 0.5, \theta = 0.1, \lambda = 0.1$. The value of B is fixed at 5.0 to ensure interior solutions for social distancing for both poor and rich in all environments. The minimum distancing x^s is set at 1.35 although compliance is imperfect. Figure 4 summarizes the results. In response to a higher cost of distancing (a_p), the poor sharply distance less and the rich more, although the latter response is quantitatively insignificant. The ratio of their social distancing, x_p/x_r, falls. The aggregate distance also falls, which lowers the aggregate social cost of distancing, but contributes to higher aggregate infection. Poor flagrantly violate minimum social distancing as their cost of social distancing rises. Rich increase their distancing although they are still below the mandated minimum distancing for the entire range of a_p. These results exemplify propositions 1 and 2.

Figure 5 shows the effect of a higher proportion of the poor (n) on the relevant social distancing variables. Here both poor and rich minutely increase their social distancing. The ratio x_p/x_r rises. Since the poor outnumber the rich, the aggregate social distancing falls, which lowers aggregate infection as per our proposition 2. The aggregate social distancing cost, however, rises with the population share of the poor.

Figure 6 illustrates the effect of an increase in the size of the penalty (θ). Quite intuitively, both poor and rich increase their social distancing. Although the response of the poor is greater, they are still far below the minimum social distancing even for the highest level of penalty. The aggregate distance rises, which contributes to lower infections. The aggregate social distancing cost rises.

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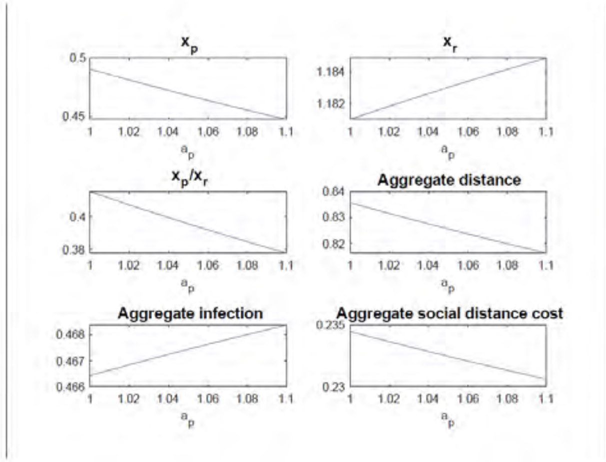

Figure 4: Effect of an increase in the poor's cost of social distancing

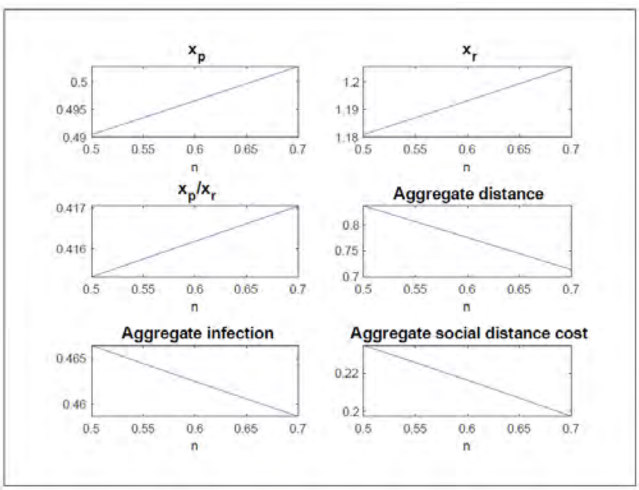

Figure 5: Effect of an increase in the proportion of poor in the population

6 Policy Implications

Rich and poor alike will benefit from the provision of an efficacious vaccine. In the meantime, while awaiting its arrival, policy-makers are confronted by problems of containing the

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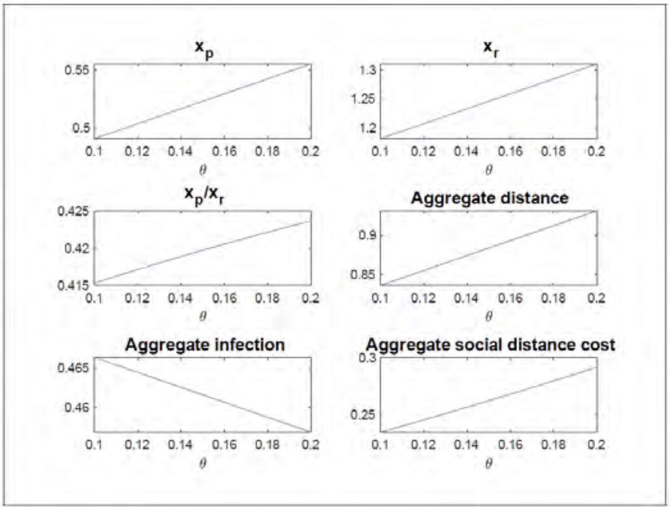

Figure 6: Effect of an increase in the size of the penalty

epidemic that arise from poverty. Although the poor live in cramped conditions, home still poses lower chances of infection than the workplace and the associated roundtrip there, usually by public transport. If, as is plausible, the poor have limited options of working remotely, then a subsidy to these types of workers can take the form of a furlough payment, with a job retention scheme in case of job losses. The UK government has already made some progress on this front by changing the Coronavirus job retention scheme from 1 July[14]. This relatively selective subsidy goes some way towards addressing our salient finding that higher levels of poverty weaken the effect of stringency on the level of social distancing in the aggregate. It could be strengthened, and made more selective, through provisions that treat more generously those subsectors of the economy and categories of jobs that pay low wages.

[14]See https://www.cbi.org.uk/articles/cbi-coronavirus-job-retention-scheme-factsheet/?utm_campaign=20200806_Coronavirus%20Bulletin_Members_Unopens&utm_medium=email&utm_source=Eloqua

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A Descriptive statistics of data in the empirical section.

Table 4: Descriptive Statistics of key variables

Variable	Obs	Mean	Std. Dev.	Min	Max
All 83 UTLAs					
weekcasespercap	11,288	.0002412	.0002398	0	.0019095
popdensity	11,288	1853.575	2718.248	64	16237
fsm	11,288	14.60226	6.746104	4.288068	33.60141
stringency	11,039	67.32564	15.2776	11.11	75.93
TRANSIT	11,288	-47.44835	20.85276	-92	65
WORK	11,288	-46.552	19.02245	-90	5
RESIDENTIAL	11,288	17.93568	7.570169	0	38
RETREC	11,288	-56.30351	21.08143	-95	27
LOW POVERTY UTLAs					
weekcasespercap	1,360	.0001745	.0001659	0	.0007041
popdensity	1,360	473.9	243.7461	169	956
fsm	1,360	6.310654	.8651854	4.288068	7.359261
stringency	1,330	67.32564	15.28265	11.11	75.93
TRANSIT	1,360	-46.225	19.46876	-85	31
WORK	1,360	-48.02574	20.02923	-90	5
RESIDENTIAL	1,360	19.40882	7.923043	0	35
RETREC	1,360	-56.97206	21.23319	-91	10
HIGH POVERTY UTLAs					
weekcasespercap	1,360	.0003229	.0002875	0	.0011856
popdensity	1,360	2624.864	2731.434	74	9696
fsm	1,360	27.49061	2.36961	24.96368	33.60141
stringency	1,330	67.32564	15.28265	11.11	75.93
TRANSIT	1,360	-46.23824	20.73862	-85	55
WORK	1,360	-44.40882	18.24921	-85	4
RESIDENTIAL	1,360	16.65515	6.756528	0	33
RETREC	1,360	-56.25588	21.09952	-94	15

B Estimated models for the TRANSIT and RETAIL/RECREATION variables

We start by discussing columns (1)-(3) of Table 5 below, which cover TRANSIT (public transport logins). As in Table 1, the first two formulations make very little difference. Again, we focus on column (1), the random effects regression. Policy stringency has a strong negative (i.e. social distance increasing) effect on this measure, with a basic coefficient of -35.16, plus a significant marginal effect of -1.485 from the interaction with population density (and an insignificant interaction with free school meals). Weekly cases show a strong negative effect (-11.55), which is increased somewhat by the interaction with population density (marginal effect -0.713), but strongly and significantly reduced by the interaction with free school meals (+4.241). Hence, again, endogenous social distancing exists, but is weakened where there is high inequality.

The intercept terms on population density and free school meals are insignificant. The model in column (2) is almost identical to column (1), but the introduction of time fixed effects in column (3) renders all coefficients except stringency insignificant. For retail and recreational (RETREC) logins, again we have a very similar message. Focusing on the random effects model in column (4), there is a strongly significant negative (i.e. social distance increasing) coefficient on stringency, and likewise (though again between 1/3 and 1/4 of the marginal effect) for weekly cases, indicating endogenous social distancing. The significant positive interaction of weekly cases and free school meals indicates that the poor have less endogenous social distancing, while other coefficients are insignificant. UTLA fixed effects in column (5) make virtually no difference, while time fixed effects in column (6) again sadly wipe out most significant effects.

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Table 5: Panel random and fixed effects regression analysis of the TRANSIT and RETREC variables.

DEPENDENT VARIABLE MODEL	(1) TRANSIT RE	(2) TRANSIT UTLA FEs	(3) TRANSIT UTLA and time FE	(4) RETREC RE	(5) RETREC UTLA FEs	(6) RETREC UTLA and time FE
stringency	-35.16***	-35.18***	-35.91***	-54.73***	-54.51***	-29.33***
	(2.520)	(2.527)	(2.804)	(1.578)	(1.586)	(1.682)
weekly cases per capita	-11.55***	-11.54***	-0.0919	-12.28***	-12.47***	-0.509
	(1.325)	(1.330)	(1.069)	(1.054)	(1.080)	(0.470)
pop density	-1.155			-0.540		
	(1.220)			(0.745)		
free school meals share	2.516			0.462		
	(2.827)			(1.530)		
pop density ∗ stringency	-1.485***	-1.486***	-0.579	-0.716	-0.714	0.426
	(0.464)	(0.467)	(0.516)	(0.677)	(0.698)	(0.353)
pop density ∗ weekly cases pc	-0.713*	-0.715	0.0972	-0.475	-0.504	0.138
	(0.428)	(0.432)	(0.385)	(0.332)	(0.348)	(0.207)
free school meals ∗ stringency	0.303	0.333	-1.339	1.065	1.026	-1.252
	(2.123)	(2.123)	(2.080)	(1.520)	(1.534)	(0.890)
free school meals ∗ weekly cases pc	4.241***	4.227***	0.730	3.648***	3.742***	0.417
	(1.108)	(1.113)	(0.841)	(0.910)	(0.940)	(0.466)
Saturday, Sunday and Bank Holiday variables omitted						
Constant	-5.805**	-4.382***	1.667	9.517***	9.412***	5.256***
	(2.720)	(0.802)	(1.248)	(1.442)	(0.591)	(0.569)
Observations	10,773	10,773	10,773	10,773	10,773	10,773
Number of lacode1	81	81	81	81	81	81
R-sqd within	0.5704	0.5704	0.8701	0.7337	0.7337	0.9695
R-sqd between	0.0865	0.0667	0.0506	0.0280		0.0893
R-sqd overall	0.4147	0.4096	0.6016	0.6685	0.7521	0.9224

Note: Robust Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Leicester and Devon are dropped as outliers. Sargan-Hansen χ^2 for random effects=620.377 (5) for TRANSIT . Sargan-Hansen χ^2 for random effects 2142.540 (5) for RETREC.

C Proofs

Proof of part (iv) of Proposition 1. We have

$$\frac{\partial X^*(x^s)}{\partial a_p} = \frac{(1-n)[(I_{r0}+\theta/x^s) - \lambda X^*(x^s) - a_r x_r^*] - a_r n x_p^*}{a_p a_r + \lambda(a_r n + a_p(1-n))}.$$

From (9), the numerator reduces to $-a_r n x_p^*$, so that $X^*(x^s)$ is decreasing in a_p.

Likewise,

$$\frac{\partial X^*(x^s)}{\partial n} = \frac{(I_{p0}+\theta/x^s)a_r - (I_{r0}+\theta/x^s)a_p + \lambda(a_p - a_r)X^*}{a_p a_r + \lambda(a_r n + a_p(1-n))}. \tag{15}$$

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Since $x_i^* > 0$, we obtain from (9),

$$-a_p a_r x_p^* + (I_{p0} + \theta/x^s)a_r - \lambda a_r X^*(x^s) = -a_p a_r x_r^* + (I_{r0} a_p + \theta/x^s)) - \lambda a_p X^*(x^s) = 0.$$

Rearranging, it is seen that the numerator in (15),

$$(I_{p0} + \theta/x^s)a_r - (I_{r0} + \theta/x^s)a_p + \lambda(a_p - a_r)X^*(x^s) = -a_p a_r(x_r^* - x_p^*) < 0,$$

where the inequality follows from part (i). ■

Proof of Proposition 2: full-compliance regime. Given $x_p = x^s$, (11) yields

$$x_r^* = \frac{I_{r0} + \theta/x^s - \lambda n x^s}{a_r + (1-n)\lambda},$$

and

$$X^* = \frac{a_r n x^s + (1-n)(I_{r0} + \theta/x^s)}{a_r + (1-n)\lambda}.$$

Hence,

$$\frac{\partial X^*}{\partial n} = \frac{a_r x^s - (I_{r0} + \theta/x^s) + \lambda X^*}{a_r + (1-n)\lambda}.$$

It follows that X^* is decreasing in n if, and only if, $a_r x^s + \lambda X^* < I_{r0} + \theta/x^s$, which indeed holds in virtue of the f.o.c. (9) for $i \in R$ and $x^s < x_r^*$. ■

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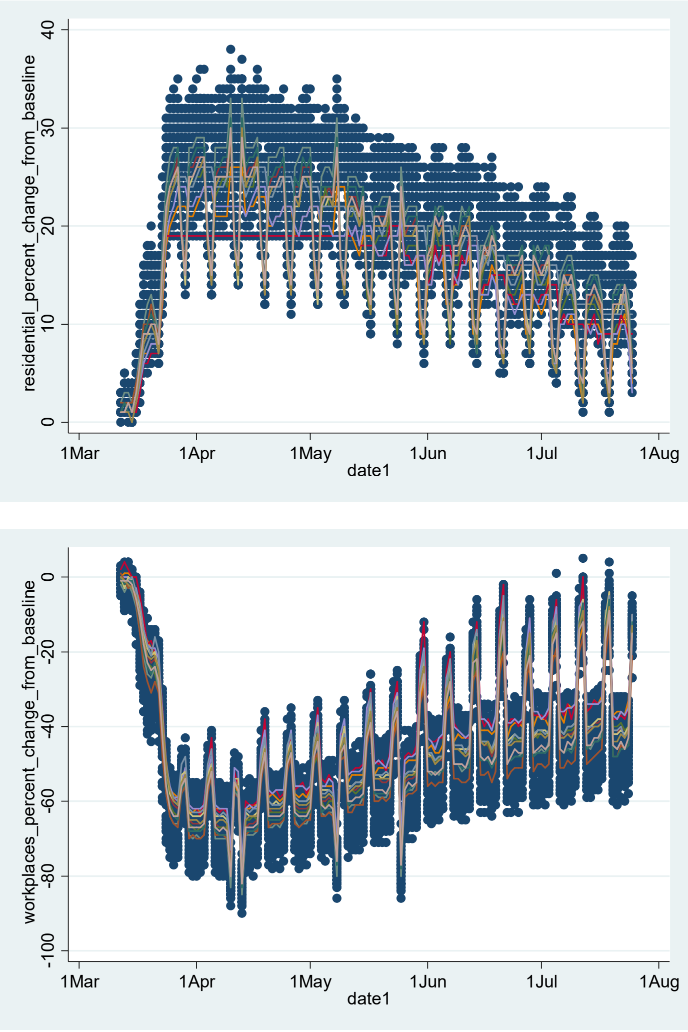

Figure 7: Scatterplots of Google social distancing measures for English UTLAs, based on RESIDENTIAL and WORK logins. The 11 UTLAs with highest poverty rates (including Devon) are shown.

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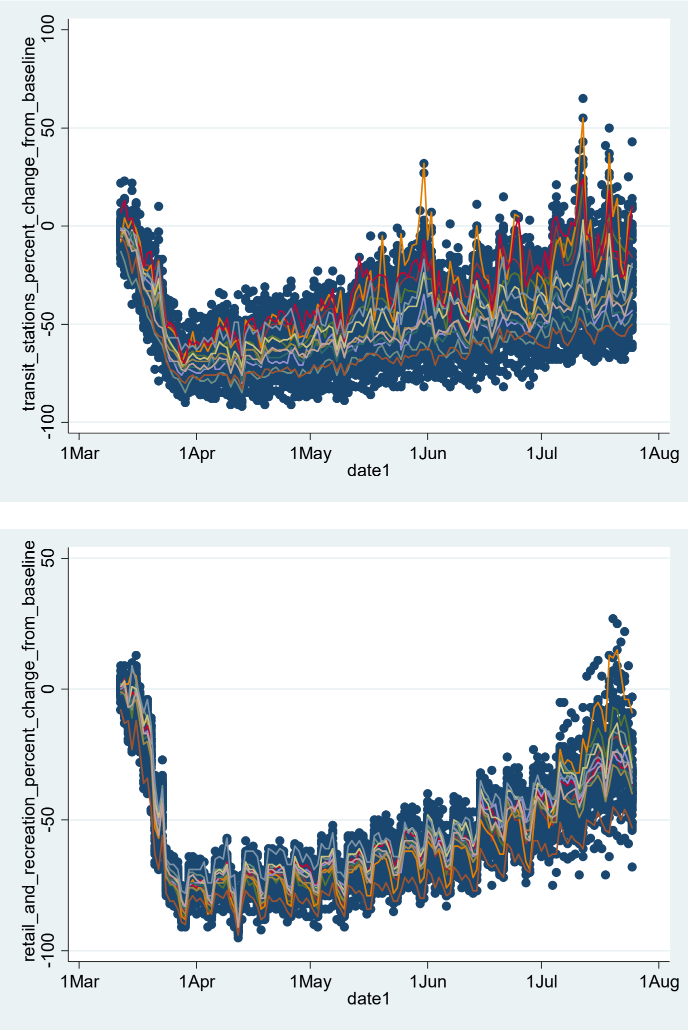

Figure 8: Scatterplots of Google social distancing measures for English UTLAs, based on TRANSIT and Retail/Recreational logins. The 11 UTLAs with highest poverty rates (including Devon) are shown.

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Emergency loans and consumption – evidence from COVID-19 in Iran

Mohammad Hoseini[1] and Thorsten Beck[2]

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We use monthly and daily transaction data from Iran, disaggregated by provinces, good and service categories, and retail store segments to gauge the impact of government emergency loans on consumption patterns. We find that emergency loans are positively related with higher consumption of non-durable and semi-durable goods, suggesting that the emergency loans were predominantly used for their intended purpose. The effects were strongest in the first few days and then dissipated over time. We find effects only for in-store but not online transactions and in poorer rather than richer provinces, suggesting that it is the poorer who reacted more strongly with higher consumption to the emergency loans.

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1 Institute for Management and Planning Studies, Tehran
2 The Business School (formerly Cass), University of London, and CEPR

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1. Introduction

The COVID-19 crisis has hit advanced and emerging markets alike. The lock-down, imposed for public health reasons, has forced many firms and sectors to temporarily shut down, resulting in income losses for owners and employees alike. Many governments have implemented fiscal support measures to mitigate this economic and social shock. This paper assesses the effect of emergency loans in Iran on consumption of different goods and services using granular data on the province-day level across different categories of goods and services. We show that emergency loans helped increase consumption of non-durable and semi-durable goods and services, including food and beverages, with the effect stronger for the poor, thus suggesting that the programme overall met its goal.

Across the globe, the COVID-19 crisis has hit poorer population segments more heavily, especially in developing markets (Furceri et al., 2020). Working in the informal economy, primarily in services, most low-income workers are not able to work from home or benefit from the employment benefit protection of large formal enterprises. The high degree of informality also makes public-health oriented containment and their enforcement less effective, while limited fiscal space and limited access to international financial markets make economic support policies more difficult to implement (Djankov and Panizza, 2020). Nevertheless, many developing country governments implemented support programmes for households and firms and an evaluation whether these programmes were successful in reaching the most affected in the economy and what support payments were used for is thus important. This paper offers such an assessment for emergency household loans in Iran.

Iran was the first country in the region to be hit by COVID-19, with the first confirmed case reported on 19 February 2020. By the end of February, 593 confirmed cases and 43 deaths had been reported. In response to the pandemic, the government on 22 February announced the cancellation of all cultural and religious events as well as closure of schools, and universities in the affected provinces, extended to all provinces on 4 March. However, it was not until 21 March (right before the start of the Persian holiday Nowruz) that the government announced a ban on travel between cities as well as closure of shopping centres and bazaars across the country with exceptions for pharmacies and grocery stores.

As the number of new cases started to fall, restrictions were gradually relaxed starting in April. On 5 April, the government allowed low-risk activities to open all over the country except Tehran province starting on 11 April. On 18 April, Iran allowed more businesses to re-open

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but schools, restaurants, malls, and grand bazaar remained closed. The partial lockdown continued to be relaxed in May when restrictions on restaurants and shopping malls eased. Also, in April, the government announced that eligible households can apply for an emergency loan (≈ 54% of the minimum wage). This loan of 10 million IRR (240 USD) is based on eligibility for a monthly cash transfer that the government has been paying to every Iranian above 18 supported by oil income (now around 10 USD per month), with the exception of top income earners. The loan is to be repaid out of future cash transfers, starting in July-August 2020. Out of 25.6 million Iranian households, 24.2 million are eligible for this monthly cash transfer and among them 21 million applied for the loan. The loans were paid out in four waves, with 17.1 million households being paid on 23 April, 2.3 million on 30 April, 775 thousand on 7 May and 867 thousand on 11 June.[2] Hence, over 80 percent of 83.5 million Iranian individuals are covered by the emergency loan program.

We use monthly transaction data between November-December 2018 and June-July 2020 to assess the effect of the COVID-19 crisis on consumption of different goods and services and daily data for April-May 2019 and 2020 to assess the effect of the emergency loans.[3] Specifically, we have data on POS and online purchases across provinces for different types of goods and services, which allow us to document patterns of consumption across different goods and services and the effect that emergency loans had on these patterns.

We find that emergency loans are positively related with higher consumption of non-durable and semi-durable goods, while there is no significant effect on the consumption of durables or asset purchases, suggesting that the emergency loans were predominantly used for their intended purpose. The effects were strongest in the first few days and then dissipated over time. We find effects only for in-store but not online transactions and in poorer rather than richer provinces, suggesting that it is the poorer who reacted more strongly with higher consumption to the emergency loans. In summary, the programme seemed to have been effective in reaching the poor and increasing consumption of essential goods.

[2] As the loans are to be paid back with 12% nominal interest (which is far less than the current inflation of 40% in Iran), not all eligible households applied, and some were initially reluctant but applied later on. To apply for the loan, the household head had to send an SMS to the government with a mobile number registered to his/her name and ID number. As some household heads might not have a simcard with their own name, free simcards were disbursed to household heads without one.

[3] Data are organised according to the Iranian calendar year, which has 365 days and starts with Farvardin (21 Mar – 20 Apr) followed by Ordibehesht (21 Apr – 21 May), Khordad (22 May – 21 June), Tir (22 June – 22 July), Mordad (23 July – 22 Aug), Shahrivar (23 Aug – 22 Sep), Mehr (23 Sep – 22 Oct), Aban (23 Oct – 21 Nov), Azar (22 Nov – 21 Dec), Dey (22 Dec – 19 Jan), Bahman (20 Jan – 18 Feb), and Esfand (19 Feb – 20 Mar).

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Our paper contributes to a small literature assessing the impact of the COVID-19 crisis on consumption that uses transaction data for impact assessment of COVID-19, most of which are on advanced countries, including on France (Bounie et al., 2020), Portugal (Carvalho et al., 2020), Denmark (Andersen et al, 2020), Spain (Carvalho et al., 2020b), China (Chen et al., 2020), Japan (Watanabe and Omori, 2020), UK (Hacioglu et al. 2020), the U.S. (Baker et al, 2020; Cox et al. 2020; Dunn, Hood and Driessen, 2020) and Mexico (Campos-Vazquez and Esquivel, 2020). Chetty et al. (2020) use an array of different granular high-frequency data sources to document the development of macroeconomic aggregates including consumption during the COVID-19 crisis in the US as well as fiscal support policies.[4] Unlike these papers, our paper focuses on a developing economy and the MENA region. In addition, ours is (one of) the first paper to assess the impact of fiscal support measures on consumption. Our paper is also related to Hoseini and Valizadeh (2020) who use the same monthly data to document the decline of consumption in Iran during the lock-down and subsequent recovery as well as a spike in online transactions.

Our study also contributes to the more general literature on consumer spending and its reaction to unanticipated income changes (see Jappelli and Pistaferri, 2010, for a literature survey). This literature suggests that consumers respond to negative shocks by reducing spending, especially in the presence of liquidity and credit constraints. Baker and Yannelis (2017) use the US government shutdown as temporary and unanticipated shock and show – inconsistent with the permanent income hypothesis – an excess sensitivity of consumption patterns, but widely varying elasticities across different goods and services (see also Gelman et al., 2020), while Baker (2018) uses firm-specific shocks during the Great Recession and shows that the consumption of highly-indebted households is more sensitive to income fluctuations. The setting in our paper is that of an economy with a high degree of financial inclusion (94% account ownership and 79% of adults with a debit card in 2017, according to Global Findex), but with large parts of the population facing liquidity and credit constraints (only 38% had emergency funds available in 2017). While in 2017 (2014), 24% (32%) borrowed from a financial institution, 40% did so in 2014 from stores and 49% from friends and families. We thus expect unanticipated and symmetric negative income shocks such as the COVID-19 shock to result in substantial consumption declines even if seen only as transitory and support

[4] See also Granja et al. (2020) who assess the impact of the Payroll Protection Program.

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payments by the government resulting in consumption increases, even if this support is in the form of loans and has to be repaid.

The remainder of the paper is structured as follows. The next section presents the data and simple graphs. Section 3 introduces the methodology and section 4 discusses the results. Section 5 concludes and looks forward to future research questions.

2. **Data and simple graphs**

We use payment transaction data to proxy for high-frequency changes in consumption patterns across provinces and across different goods and services. This follows the approach by Aladangady et al. (2019) who shows that aggregating anonymized transactions data from a large electronic payments technology company to the national level provides similar patterns of monthly consumption growth rates as the Census Bureau's Monthly Retail Trade Survey.

Our monthly and daily transaction data are from Shaparak, a company belonging to Iran's Central Bank that acts as the clearinghouse for all transactions done via point of sale (POS) and online terminals using Iranian Rial. While we do not capture cash purchases, this includes only a small bias as according to CBI (2018), 97% of Iranian households use electronic cards as the main payment method for their purchases. We have monthly data for POS and online transactions for each of the 31 provinces from November-December 2018 to May-June 2020 and daily data for April-May 2019 and April-May 2020. In addition to data on the province level, we can also distinguish between 280 different goods and services, mainly based on ISIC 5-digit coding system with some adjustments for Iran-specific goods and services, which we aggregate into the following 12 groups: food & beverages, tobacco, clothing & footwear, housing repair & expenses, appliance & furniture, health, transport, communication, recreation, education, restaurants & hotels, and miscellaneous goods and services. We also distinguish between durable, semi-durable and non-durable goods, defined in detail in Appendix A. We also differentiate between 18 different retail sectors: non-chain grocery, chain grocery, specialised food & beverages, tobacco, fuel, computer & telecommunication, audio & video equipment, hardware & paint, carpets & covering, appliances & furniture, book & newspapers, music & video, sporting equipment, toys, clothing & footwear, pharmaceutical, other retails and second-hand shops. Appendix Tables A1 to A3 list the Shaparak activities belonging to each of the classifications.

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We have information both on the number and value of transactions and can distinguish between POS (i.e., in-store) and online transactions. All values are in real terms, i.e. we adjust data for inflation using monthly price index of about 600 items per province (obtained from the Central Bank of Iran with base year 2016) by matching each of the 280 activity codes with the proper item code in the price index data.

Table 1 presents descriptive statistics of the data we use in the regression analysis. In total, we have 1,922 observations (31 provinces times 31 days times two). On average, there are 2 million transactions on a given day in a province, but with a large variation both across provinces as across different days of the two months. The value of transactions is, on average, 2.8 trillion IRR (at 2016 price) per day and province, but ranging from 123 billion IRR to 55 trillion IRR. The value of transaction per household is 2.71 million IRR on average and it is changing between 500 thousand IRR and 12 million IRR per day. POS transactions comprise 87 percent of total value and range up to 26 trillion IRR. We also show the average value of daily transactions and daily transactions per households across non-, semi- and durable goods and services and assets and the 12 COICOP groups of goods and services. We find that expenditures on non-durable goods and services are by far the largest group; among the 12 COICOP groups, food and beverages is the largest group, followed by miscellaneous goods and services and transportation.

Figure 1 shows the change in number and value of transactions between November-December 2018 (2019) and June-July 2019 (2020). In the three Persian months between November and February, while the number of transactions is about 20% higher in 2019/20 than 2018/19, the value of transaction is very close in the two years and moves in parallel. Because the Persian new year holiday of Nowruz starts in 21 March, every year in Iran there is a rise in shopping before Nowruz in Esfand (Feburary-March) followed by a drop in Farvardin (March-April). We observe this pattern in both number and value of transactions in 2018/2019, but with the start of pandemic in early March 2020 there is no rise in number and value of transactions in Esfand compared to previous months and they decrease further in Farvardin.

Figure 2 shows the value of transactions for the same periods differentiating between durable, semi-durable and non-durable goods. We note that the value of non-durable good transactions follows a very similar path in 2018/19 and 2019/20 until April-May when the aggregate transaction value increases in 2020 to a higher level than in 2019. Although emergency loans are one reason for this increase, another reason is the reopening of the economy and the discharge of accumulated demand in lockdown time. In the case of semi-durable and durable

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goods, we see that the total value of transactions started dropping in February-March 2020 below the 2019 level, in line with the onset of COVID-19 and the subsequent lockdown. The value goes back to the 2019 level in April-May (when emergency loans were paid out) and moved to a higher level than 2020 in the following month, most likely a catch-up effect.

Figure 1- Number and total value of transactions via POS and online shopping terminals

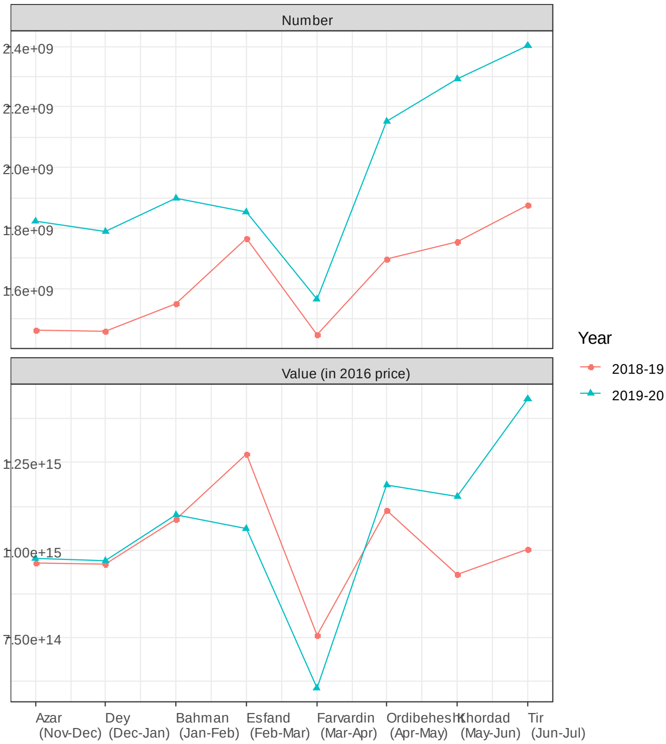

Figure 3 shows the value of transactions for the same periods differentiating between the 12 goods and services groups. We find that food & beverages and tobacco consumption in 2019/20 follow the trend in 2018/19, while all other good and service categories see consumption in 2019/20 fall below their 2018/19 level starting in February-March, though most catch up in May-June.

Second, and as already discussed we have information on the number and value of emergency loans across provinces and for the first three of four rounds. We have data on the value of these loans for each round and province and will use both total loans relative to total transactions and loans per household (in million IRR) in our regression analysis. The descriptive statistics in Table 1 show that emergency loans relative to monthly transactions are, on average, 2.7 percent, but ranging from 0.1 to 10.3 percent. Across the three rounds, the first one has the

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highest coverage and on average loans are 7 percent of total monthly transactions. Loans per household varies between 0.21 to 8.7 (million IRR), with an average of 2.75. If the emergency loan had full coverage, we would expect the sum of mean loan per household over the three rounds to be 10 (million IRR). As that the sum is 8.24, it shows that 82.4 percent of Iranian household received emergency loans in these first three rounds.

Figure 2 – Total value of transactions via POS and online shopping terminals based on durability of goods

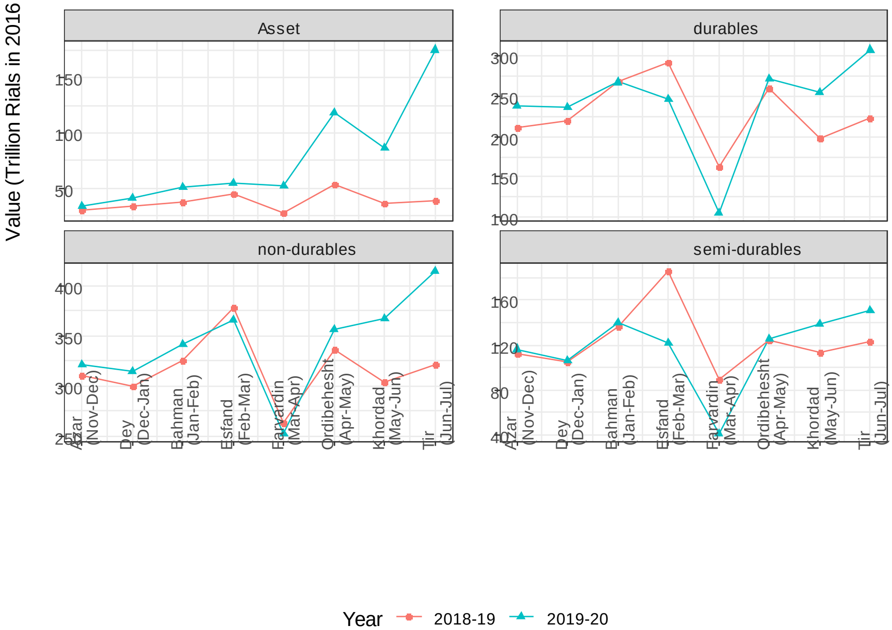

Figure 4 shows the number and value of transactions during April-May, the month of the first three rounds of emergency loans, using daily data, both for 2019 and 2020. We first note a clear weekday pattern, with Fridays seeing a much lower number and value of transactions. Second, there is an increase in the number of transactions in 2020 on the day of all three loan waves, which we do not observe in 2019. On the other hand, we observe a marked increase in the value of transactions on Saturday (first day after weekend) after each loan wave.

Figure 5 shows the value of transactions during April-May for non-durable, semi-durable and durable goods. We see that the increase in transactions and thus consumption is primarily on the day of the first loan wave and in non-durable goods, where consumption in 2020 reaches the level of 2019 only on Thursday, Friday and Saturday of the first loan wave, while it stays below the 2019 level (for the same weekday) for the rest of the month. Semi-durable and durable consumption, on the other hand, stays below the level of the same weekday in 2019 across the whole month.

Figure 3 - Total value of transactions via POS and online shopping terminals based on COICOP groups

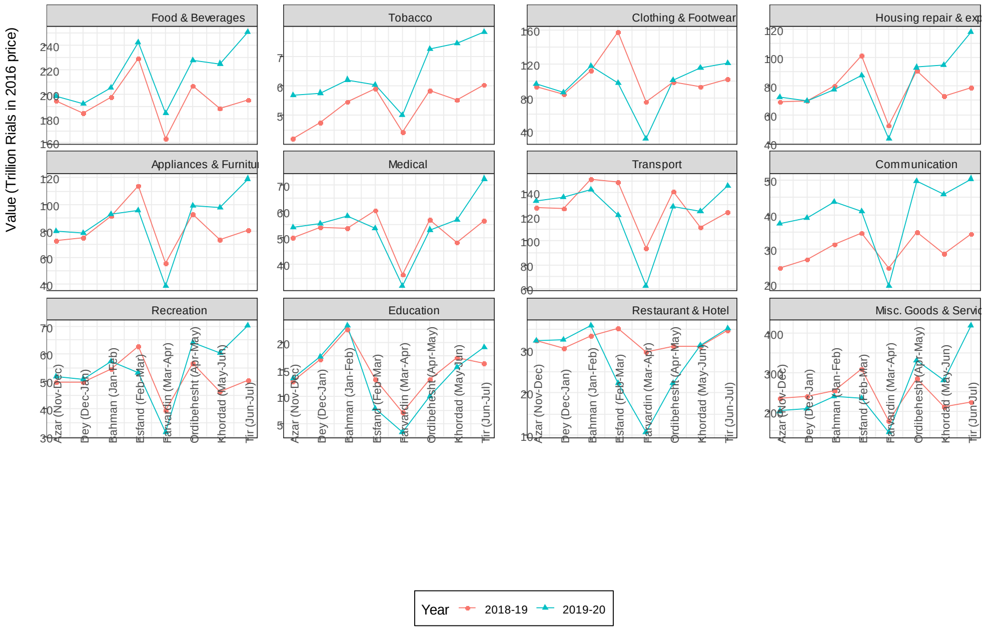

Figure 4- Daily trend of value and number of transactions in Ordibehesht (April-May) in 2019 and 2020. The drops correspond to Fridays which is the weekend in Iran. Sunday 1 Ordibehesht 2019 was a religious holiday. The blue lines show the different rounds of the emergency loan dispersion

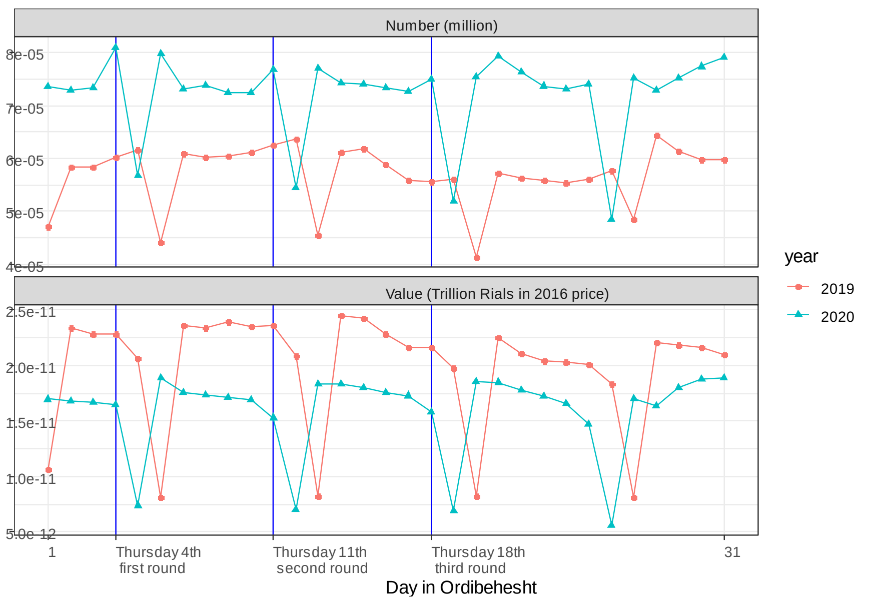

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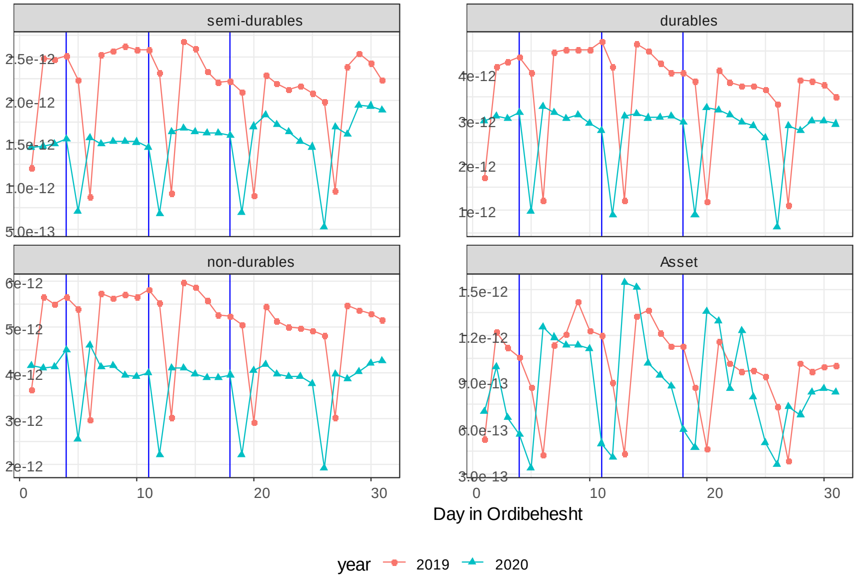
Figure 5- Daily trend of value of transaction in Ordibehesht (April-May) in 2019 and 2020 by durability of the goods. The drops correspond to Fridays which is the weekend in Iran. Sunday 1 Ordibehesht 2019 was a religious holiday. The blue lines show the different rounds of the loan dispersion



.**Figure 6- Daily trend of value of transactions in Ordibehesht (April-May) in 2019 and 2020.** For illustration Fridays and holidays are removed from plots. The blue lines show the different rounds of the loan dispersion.

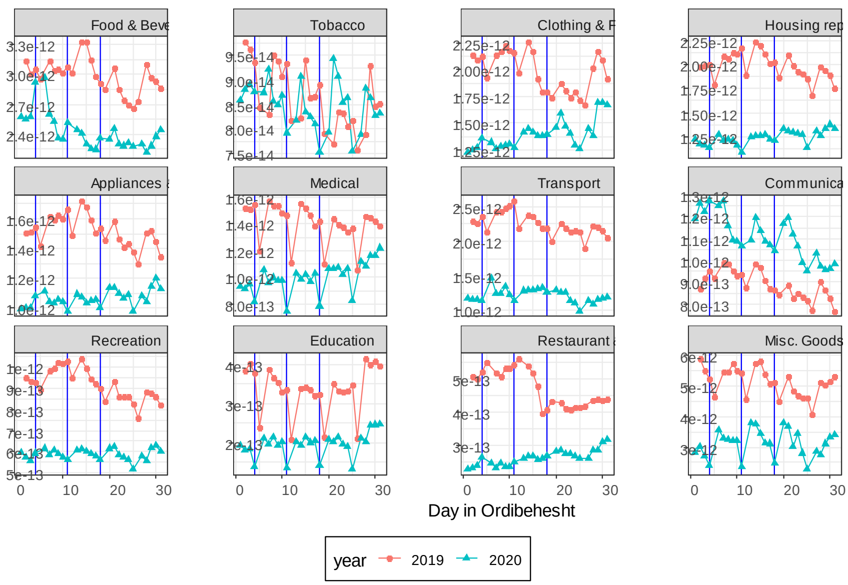


Figure 6 differentiates across the 12 different categories of goods and services. We see for all but four categories, 2020 consumption is below 2019 consumption. Tobacco consumption in 2020 is above 2019 levels, as is communication, while miscellaneous goods and services do not show a clear trend in comparison between 2019 and 2020. Most importantly, food and beverage consumption sees a spike in the first three days of the first loan wave and smaller spikes the days of the second and third loan wave. In the following, we will use formal regression analysis to confirm whether these initial ocular econometrics reflects indeed consumption patterns following from the emergency loan programme.

3. Methodology

In order to estimate the effect of the emergency loans on consumption across different provinces and categories, we use a difference-in-differences set-up, which stacks daily province-level transaction data for April-May 2019 and 2020. We assume that the treatment days are from 23 April to 13 May, between the day of the first loan payment and six days after the third loan payment, while 20 to 22 April and May 14 to 20 are the control dates. We also use April-May 2019 as control period. In our regression analysis we focus on the first loan wave, for several reasons. First, as we cannot distinguish between transactions of households who received loans in the first, second and third week and since the effect of loans on consumption could go beyond one week, the relationship between consumption and the second and third loan waves might pick up the effect of the first loan wave on consumption. Second, the first loan wave is by far the largest. We will assess the effect of the first loan wave on consumption both during the first week and the first three weeks. Specifically, we run the following regression:

$$Y_{pt} = \alpha_1 Period_t + \alpha_2\, Loan_p \times Period_t + \ Day_t + Wday_t + Year_t + Holiday_t \\ + Province_p + \epsilon_{pt}$$

where Y_{pt} is either the log of the total value of transactions or the log of value of transactions per household, on day t in province p, $Period_t$ is a dummy that takes on the value one for the treatment dates, $Loan_p$ is either the loan volume in the first round divided by total monthly transactions or loans per households in the first week, in province p.[5] We saturate the model with day, weekday, year, holiday[6] and province dummies. The coefficient of interest α_2 thus captures the relationship between emergency loans and transactions relative to transactions on the same day in 2019, a given weekday and the average in the same province. The standard

[5] To exclude the effect of the emergency loans from monthly transactions, we compute it by multiplying total expenditure in the first day of Ordibehesht 2020 by 30.
[6] There is one holiday not on the weekend in 2019.

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errors are clustered at province level. We run the regression for total transaction, transactions on non-, semi- and durable goods and assets, and transactions for different goods and services and transactions across different retail segments.

Given the saturation of the model with fixed effects and as we exploit variation in emergency loans and transactions across provinces, we are confident that α_2 captures the reaction of consumption (as captured by transactions) to the emergency loans during April-May. However, there are several factors that might bias our coefficient estimates. First, as the emergency loans go primarily to poorer provinces, where households spend a larger part of their income on food and beverage consumption, there might be a spurious correlation; we test for this possibility by splitting the sample between richer and poorer provinces. Second, during the lockdown period (and thus shutdown of income sources for many households), some households might borrow from others and then use the receipts from the emergency loans to repay these loans. However, this should bias against finding significant coefficient estimates. Third, given data constraints, we can only test for short-term reactions of households but not long-term implications; we might be able to do so in future work.

4. Results

The results in Panel A of Table 2 show that while during the first week emergency loans have no significant relationship with aggregate transactions, they have a positive and significant relationship with non-durable and semi-durable goods and services transactions. Here, we focus on the effect of the first wave of emergency loans on consumption during the following seven days, which is by far the cleanest specification. The coefficient estimates suggest that a one standard deviation in emergency loans (relative to monthly transactions) increases the value of non-durable (semi-durable) consumption by 3.4 (5.5) percent per day[7] while there is no significant relationship with durable consumption or asset purchases. By multiplying the estimated coefficients with the mean value of each item, we can compare the impact in terms of absolute values. In this regard, one standard deviation increase in loan volume increases non-durable consumption per day by 28.7 billion IRR (= 0.0193 * 1.8 * 827 billion) and daily consumption of semi-durables by 14.9 billion IRR, suggesting that two thirds of the emergency loans went into non-durable consumption. Across the four regressions, we also see that the first

[7] As the dependent variable is in logs, we can interpret the relationship in percent terms.

week after the issue of the first wave of emergency loans is characterised by lower consumption of semi- and durable goods, but not non-durable goods, compared to 2019 and the 2020 days outside the first week of the emergency loan window. These findings suggest that the emergency loans were used not only for non-durable but also semi-durable expenditures, while they were not used for purchases of durable goods or assets.

The results in Panel B of Table 2 show that a higher first wave emergency loan volume increased consumption of almost all goods and services, with the exception of transportation (likely related to limited demand due to the lockdown), restaurant and hotel services (for the same reasons) and miscellaneous goods and services. The positive effect is strongest for communication and clothing and footwear, where a one standard deviation increase in loan volume increases daily consumption by 8.3 and 6.6 percent, respectively. The largest increase in absolute value of daily transactions by a one standard deviation increase in loan volume happened in food and beverages (15.7 billion IRR), followed by clothing and footwear (13.0 billion IRR) and appliance and furniture (10.1 billion IRR).

The results in Panel C of Table confirms these findings, presenting results across 18 different retail segments. We find a positive and significant relationship of emergency loans and transactions across a number of retail segments, with the strongest percentage increase (for a one standard deviation increase in loan value relative to monthly transactions) in media equipment (27.3 percent) and clothing and footwear (6.5 percent), in line with the findings in Panel B. While we do not find a significant relationship between the first-wave emergency loans and transactions in chain stores, we find a daily increase of 4.7 (2.7) percent in speciality food and beverages stores (non-chain stores) for a one percent increase in emergency loans.

The results in Table 3 confirm the results from Table 2 with emergency loans per household and transactions per household. We find that higher emergency loans per household increases non-durable and semi-durable consumption, though the effect is significant only at the 10 percent level (Panel A). Specifically, an increase of loans per household by one standard deviation increases non-durable (semi-durable) transactions per household by 2.9 (4.9) percent. Regarding the absolute values of transactions, one standard deviation of loan per household raises non-durable and semi-durable transactions per household per day by 233 and 129 thousand IRR, respectively. Similar as in Table 2, the Panel B results suggest an increase across almost all good and services category, with the exception of health, transportation, recreation, restaurant and hotel services and miscellaneous goods and services. The Panel C results show a significant effect of the first-wave emergency loans on transactions in tobacco, computer and

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communication, media equipment, construction material, floor and carpeting, appliances and furniture, sporting equipment, and clothing and footwear stores but not on chain, non-chain or specialised food and beverages stores.

In robustness tests, reported in Appendix Table A3, we extend the analysis to the first three waves of emergency loans, testing separately for the effect of each wave on the transactions in the following week. The results show that our findings are indeed driven by the emergency loans given in the first week rather than loans given in the second or third wave. In Panel A, we find that the coefficient on emergency loans enters positively and significantly only for the first loan wave, while it enters positively and significantly for semi-durable consumption in both the first and second wave. The results in Panel B show a positive and significant impact of emergency loans on food and beverage consumption only for the first wave of loans. On the other hand, we find positive and significant effects of emergency loans in the first and second wave for a number of other good and service categories. Finally, the results in Panel C show that there is a positive and significant effect of emergency loans on transactions in the non-chain and specialised food & beverage stores only for the first loan wave, while we find a positive and significant (at the five percent level) effect during the first wave on transactions in tobacco stores, media equipment, construction material, appliances and furniture and clothing and footwear, suggesting some leakage of the emergency loans to non-essential goods and services.

So far, we have explored the immediate impact of the first wave of emergency loans. In the following, we will assess the impact of the first wave of emergency loans during the three weeks that emergency loans were paid out in April-May. On the one hand, extending the treatment period to three weeks allows to gauge the medium-term effects of the first wave of emergency loans (again compared to the control period in 2019); on the other hand, the effect of the first loan wave is now confounded with the second and third waves of emergency loans, although these were much smaller.

The results in Table 4 confirm our previous results, though with smaller coefficients. While there is no significant relationship between total transactions and the first wave loan volume, non-durable (semi-durable) transactions increase 3.1 (4.5) percent for a one standard deviation increase in total loan volume. Over the three-week period after the first emergency loan wave, the largest effect is now in clothing and footwear and recreation, though we also find that food and beverage consumption increases by 2.7 percent per day for a one standard deviation increase in emergency loan volume. The results in Panel C show similar results as in Table 2

Panel C, with positive and significant relationships between transactions across several retail segments and emergency loan volume, with the strongest effects in media and sporting equipment. It is important to note, however, that these are retail segments that make up a relatively small part of overall transactions.

The results in Table 5 confirm these findings using emergency loans and transactions per household. Specifically, for a one standard deviation in loans per household we find a statistically significant 3.4 (4.2) percent increase in non-durable (semi-durable) consumption. As before, this relationship is significant across many COICOP categories, with the largest effect in education and transportation (Panel B) and across different retail segments, with the largest effects in media equipment and books and newspaper (Panel C); we note, however, that in Panel C, many of the coefficients only enter at the 10% level – an extension of the treatment period to three weeks thus turns the estimates less accurate.

To shed further light on the time variation of the reactions of durable and non-durable consumption to the emergency loans, we re-run the regressions of Panel A of Table 2 but interacting the emergency loan variable with the dummies for the 21 days following the pay-out. We plot the coefficients with the significance bands in Figure 7. We find significantly positive coefficients for non-durable consumption on the first Thursday, Saturday and Sunday (with Fridays showing positive but insignificant coefficients throughout). During the rest of the period, we find positive, but increasingly smaller coefficients, suggesting that the impact of the first wave of emergency loans is dissipating over time. In the case of semi-durable consumption, we find positive and significant coefficients for almost all days, except for Fridays, although the impact also dissipates over time. While we find a positive and significant effect of the first wave of emergency loans for consumption of durable goods, the rest of the coefficient estimates is mostly insignificant, as it is for the estimates of asset purchases.

The results in Table 6 show that the relationship between emergency loans and consumption is driven by POS (in-store) rather than online transactions. Here we consider the log value of POS transactions and online transactions separately, in the same set-up as Table 2 Panel A. In the case of POS transactions, we find positive and statistically significant results for non-durable and semi-durable transaction, with coefficient estimates close to the ones in Table 2, in addition to a positive and statistically significant result for total POS transactions. On the other hand, emergency loans do not show any significant relationship with online transactions for any of the different categories and coefficients are even negative. In unreported regressions, we find similar results when considering transactions across the 12 different categories of goods and

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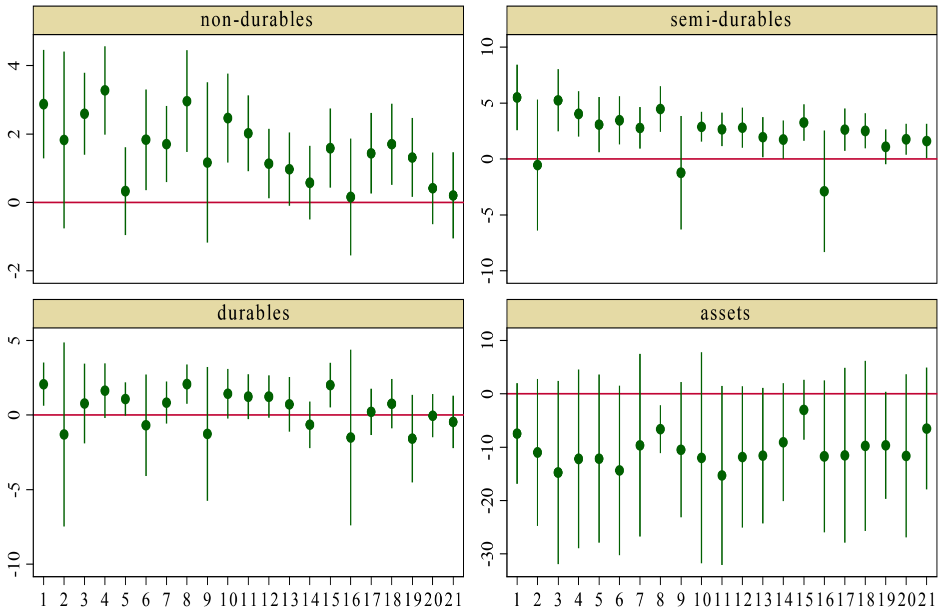
services and when considering the effect over three weeks following the first wave of emergency loans rather than just the first week. Overall, this suggests that the increase in consumption in non-durable goods, especially food and beverages came through in-store POS transactions, more likely to be used by lower-income segments of the population.

Figure 7- Day effects of the first round of loans. The graphs show the estimated coefficients δ_{2i} of the regression $log\ (Y_{pt}) = \sum_i \delta_{1i}\, D_i + \sum_i \delta_{2i}\, D_i \times Loan_1 + \ Day_t + Wday_t + Year_t + Holiday_t + Province_p + \epsilon_{pt}$, which give the effect of loan in D_i days after the first round (4th Ordibehesht) of emergency loans. 2, 9, 16th days are Friday.



The results in Table 7 show a positive effect of the first wave of emergency loans primarily in poorer provinces. Here we focus on the top and bottom quartile of provinces based on the level of transactions per household in the first day of Ordibehesht as a measure of income. We find a positive and significant relationship between emergency loans and overall consumption and non-durable consumption for in the bottom quarter of provinces, but no significant relationship for semi-durable, durable and asset purchases. On the other hand, there is no significant relationship between the first wave of emergency loans and any transaction variable for the top quartile of provinces, even though the coefficients on semi-durable and durable consumption enter positively. In panel B of Table 7, we repeat estimations for a dummy of three-weeks period after the first round as independent variable and we find similar but less significant results compared to one-week period.

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5. Conclusions

This paper documents the effect of COVID-19 on consumption patterns in Iran and assesses the effectiveness of emergency loans provided by the Iranian government to all but the 5% top income households. We find that the emergency loans were primarily used for non-durable and semi-durable consumption, suggesting that the emergency loans were predominantly used for their intended purpose though with some leakage effects. The effects were strongest in the first few days and then dissipated over time. We find effects only for in-store but not online transactions and in poorer rather than richer provinces, suggesting that it is the poorer who reacted more strongly with higher consumption to the emergency loans. This is line with theory and previous studies on the impact of temporary income shocks in the presence of credit and liquidity constraints.

While this paper provides a snapshot of the COVID-19 crisis and government support measures in a developing country, there are further important questions that will arise in the near future. First, as these support payments are in the form of loans, to be repaid starting in June/July 2020 there are concerns of repayment burdens on the lower income segments, which calls for assessing the effect of repayments (out of income subsidies) on consumption patterns. Second, will there be a permanent shift towards online transactions away from POS transactions in store? As data become available over time, we will be able to answer these questions.

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Table 1- Summary statistics. All values of transaction are at fixed price of 2016-17. Loan depth is measured as total value of loans in a province divided by total value of monthly transactions. Loan per *household* is computed as total value of loans in million IRR divided by total number of households in a province.

Variable	Obs	Mean	Std. Dev.	Min	Max
Number of transactions	1,922	2016421	2596725	249527	1.81E+07
Value of transactions (IRR)	1,922	2.83E+12	5.84E+12	1.23E+11	5.50E+13
Value of transaction per household (Million IRR)	1,922	2.714402	1.213053	0.507609	12.12135
Value of POS transactions (IRR)	1,922	2.47E+12	3.78E+12	1.22E+11	2.66E+13
Value of Online transactions (IRR)	1,922	3.64E+11	2.51E+12	6.94E+07	3.34E+13
Province level variables:					
Loan depth		0.027897	0.0076	0.000729	0.103459
Round 1		0.070852	0.019308	0.019212	0.103459
Round 2		0.009595	0.002521	0.001948	0.014182
Round 3		0.003244	0.000971	0.000729	0.005296
Loan per household		2.750873	3.056153	0.214745	8.729568
Round 1		6.985508	0.945453	4.637726	8.729568
Round 2		0.948153	0.142664	0.5733621	1.180539
Round 3		0.318958	0.055779	0.2147454	0.4927443

sector	Value of transactions (IRR)		Value per household (Million IRR)		sector	Value of transactions (IRR)		Value per household (Million IRR)	
	Mean	**Std. Dev.**	**Mean**	**Std. Dev.**		**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**
Non-Durables	8.27E+11	1.06E+12	0.930379	0.267356	Appliance & Furniture	2.46E+11	4.25E+11	0.255217	0.14544
Semi-Durables	2.66E+11	5.22E+11	0.257925	0.129752	Health	9.01E+10	1.77E+11	0.084231	0.041775
Durables	6.58E+11	1.19E+12	0.666292	0.299597	Transport	3.59E+11	5.89E+11	0.381342	0.138912
Asset	2.40E+11	1.60E+12	0.095877	0.349745	Communication	1.06E+11	3.31E+11	0.08202	0.073121
Food & Beverages	5.75E+11	6.93E+11	0.661468	0.191667	Recreation	1.20E+11	2.33E+11	0.114545	0.053377
Tobacco	1.72E+10	2.82E+10	0.018476	0.010313	Education	1.77E+10	4.09E+10	0.015951	0.009879
Clothing & Footwear	1.94E+11	3.94E+11	0.185667	0.10191	Restaurant & Hotel	6.29E+10	1.08E+11	0.061599	0.026935
Housing Expenses	1.64E+11	2.23E+11	0.180944	0.06435	Miscellaneous	7.87E+11	2.75E+12	0.581388	0.583308

Table 2- The impact of emergency loan depth in round 1 on total consumption expenditure. The estimated regression is $log\ (Y_{pt}) = \alpha_1 Week_1 + \alpha_2 Week_1 \times Loan_1\ +\ Day_t + Wday_t + Year_t + Holiday_t + Province_p + \epsilon_{pt}$, where Y_{pt} is total consumption in province p and day t, $Loan_1$ is the total value of loans in round 1 divided by total monthly transactions in province, $Week_1$ is a dummy of the week following Thursdays of first round ($Week_1$ is one between 4th and 10th of 2020). Day, weekday, year, and holiday fixed effects are included in all regression. Standard errors are clustered at province level. Standard errors are presented in the parenthesis. α_2 shows the percent growth in daily expenditure compared to the previous year in the week following the emergency loan if $Loan_1$ increases by 0.01 unit: $\frac{\Delta Y}{Y} = \alpha_2 \times \Delta Loan$.

Panel A: Decomposition by durability

Subsample	All	Non-durable	Semi-durable	Durable	Asset
$Week_1$ * $Loan_1$	0.387	1.800**	2.908**	0.770	-8.613
	(0.927)	(0.567)	(1.044)	(0.550)	(5.042)
$Week_1$	-0.0193	-0.0265	-0.237**	-0.0943*	0.442
	(0.0671)	(0.0441)	(0.0802)	(0.0430)	(0.370)
Constant	27.51***	26.39***	24.91***	25.85***	23.33***
	(0.0228)	(0.0179)	(0.0299)	(0.0395)	(0.0757)
Observations	1922	1922	1922	1922	1922

Panel B: Decomposition by COICOP group

Subsample	Food & Beverage	Tobacco	Clothing & Footwear	Housing	Appliance & Furniture	Health	Transport	Communication	Recreation	Education	Restaurant & Hotel	Miscellaneous
$Week_1$ * $Loan_1$	1.417**	2.837**	3.490**	2.986***	2.136**	1.549*	0.721	4.413***	1.992*	3.159*	1.542	-3.588
	(0.461)	(0.912)	(1.168)	(0.788)	(0.682)	(0.685)	(0.921)	(1.061)	(0.841)	(1.485)	(1.055)	(2.521)
$Week_1$	0.0750*	-0.222**	-0.293**	-0.234***	-0.100	-0.199**	-0.119	-0.201*	-0.134*	-0.271**	-0.281**	0.104
	(0.0351)	(0.0688)	(0.0904)	(0.0614)	(0.0514)	(0.0554)	(0.0724)	(0.0757)	(0.0630)	(0.0980)	(0.0777)	(0.183)
Constant	26.12***	22.16***	24.58***	24.56***	24.55***	23.50***	25.64***	23.17***	24.10***	21.63***	23.87***	25.43***
	(0.0204)	(0.0548)	(0.0305)	(0.0347)	(0.0377)	(0.0291)	(0.0308)	(0.0412)	(0.0252)	(0.0500)	(0.0222)	(0.0523)
Observations	1922	1922	1922	1922	1922	1922	1922	1922	1922	1922	1922	1922

Panel C: Decomposition by retail sector

Subsample	Non-chain stores	Chain stores	Spec. Food & Beverages	Tobacco	Fuel	Computer & Communication	Media equipment	Construction material	Carpet & Flooring
$Week_1$ * $Loan_1$	1.410*	0.549	2.458***	2.837**	1.062	1.858	14.39**	2.563***	3.183*
	(0.677)	(1.525)	(0.594)	(0.912)	(1.196)	(1.177)	(4.569)	(0.678)	(1.262)
$Week_1$	0.0680	0.200	0.0186	-0.222**	-0.157	-0.0392	-0.932*	-0.196***	-0.225*
	(0.0527)	(0.117)	(0.0432)	(0.0688)	(0.0926)	(0.0905)	(0.376)	(0.0499)	(0.0866)
Constant	25.64***	22.32***	24.74***	22.16***	23.36***	21.65***	18.11***	23.57***	22.68***
	(0.0246)	(0.0515)	(0.0232)	(0.0548)	(0.0274)	(0.0519)	(0.190)	(0.0393)	(0.0489)
Observations	1922	1922	1922	1922	1922	1922	1922	1922	1922

Subsample	Appliances & Furniture	Book & Newspapers	Video & Music	Sporting equipment	Toys	Clothing & Footwear	Pharmaceutical & Cosmetic	Other retail stores	Second-hand shops
$Week_1$ * $Loan_1$	2.511***	1.657	13.22	2.478*	3.364	3.416*	1.440	-0.0999	1.322
	(0.647)	(1.301)	(9.792)	(0.994)	(2.303)	(1.270)	(0.715)	(1.133)	(2.659)
$Week_1$	-0.103*	-0.168	-0.927	-0.160*	-0.306	-0.289**	-0.156**	-0.233*	-0.0145
	(0.0490)	(0.111)	(0.686)	(0.0703)	(0.161)	(0.0986)	(0.0534)	(0.0885)	(0.176)
Constant	24.13***	22.12***	17.72***	21.26***	21.32***	24.21***	23.31***	23.94***	20.40***
	(0.0410)	(0.0296)	(0.247)	(0.0486)	(0.0643)	(0.0350)	(0.0295)	(0.0606)	(0.0887)
Observations	1922	1922	1898	1922	1922	1922	1922	1922	1922

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Table 3- The impact of emergency loan depth in round 1 on consumption expenditure per household. The estimated regression is $log\ (y_{pt}) = \beta_1 Week_1 + \beta_2 Week_1 \times LoanPH_1\ + Day_t + Wday_t + Year_t + Holiday_t + Province_p + \epsilon_{pt}$, where y_{pt} is consumption per household in province p and day t, $LoanPH_1$ is the total value of loans in round 1 divided by total monthly transactions in province, $Week_1$ is a dummy of the week following Thursdays of first round ($Week_1$ is one between 4th and 10th of 2020). Day, weekday, year, and holiday fixed effects are included in all regression. Standard errors are clustered at province level and are presented in the parenthesis. β_2 shows the increase in daily expenditure compared to the previous year in the week following the emergency loan if $LoanPH_{pt}$ increases by 1 unit: $\Delta y/y = \beta_2 \times \Delta LoanPH$

Panel A: Decomposition by durability

Subsample	All	Non-durable	Semi-durable	Durable	Asset
$Week_1$ * $LoanPH_1$	0.0161	0.0303*	0.0516*	0.0192	-0.122
	(0.0137)	(0.0124)	(0.0247)	(0.0134)	(0.0823)
$Week_1$	-0.104	-0.110	-0.392*	-0.174	0.685
	(0.0980)	(0.0919)	(0.178)	(0.0960)	(0.597)
Constant	0.808***	-0.308***	-1.789***	-0.854***	-3.375***
	(0.0231)	(0.0182)	(0.0303)	(0.0395)	(0.0763)
Observations	1922	1922	1922	1922	1922

Panel B: Decomposition by COICOP group

Subsample	Food & Beverage	Tobacco	Clothing & Footwear	Housing	Appliance Furniture &	Health	Transport	Communication	Recreation	Education	Restaurant Hotel &	Miscellaneous
$Week_1$ * $LoanPH_1$	0.0212*	0.0559**	0.0625*	0.0523*	0.0434**	0.0184	0.0341	0.0765***	0.0391	0.0863*	0.0423	-0.0365
	(0.00993)	(0.0177)	(0.0278)	(0.0194)	(0.0154)	(0.0168)	(0.0184)	(0.0203)	(0.0194)	(0.0333)	(0.0256)	(0.0369)
$Week_1$	0.0276	-0.411**	-0.482*	-0.388**	-0.253*	-0.217	-0.307*	-0.423**	-0.266	-0.650**	-0.467*	0.105
	(0.0733)	(0.124)	(0.201)	(0.141)	(0.112)	(0.124)	(0.134)	(0.142)	(0.139)	(0.232)	(0.183)	(0.265)
Constant	-0.584***	-4.542***	-2.127***	-2.140***	-2.152***	-3.207***	-1.067***	-3.536***	-2.605***	-5.073***	-2.831***	-1.274***
	(0.0207)	(0.0550)	(0.0307)	(0.0349)	(0.0379)	(0.0294)	(0.0307)	(0.0413)	(0.0252)	(0.0497)	(0.0219)	(0.0521)
Observations	1922	1922	1922	1922	1922	1922	1922	1922	1922	1922	1922	1922

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Panel C: Decomposition by retail sector

Subsample	Non-chain stores	Chain stores	Spec. Food & Beverages	Tobacco	Fuel	Computer & Communication	Media equipment	Construction material	Carpet & Flooring
$Week_1 * LoanPH_1$	0.0209	0.0359	0.0334	0.0559**	0.0395	0.0702**	0.244*	0.0336*	0.0721*
	(0.0136)	(0.0277)	(0.0176)	(0.0177)	(0.0236)	(0.0205)	(0.102)	(0.0149)	(0.0313)
$Week_1$	0.0219	-0.0116	-0.0408	-0.411**	-0.358*	-0.398**	-1.616*	-0.249*	-0.502*
	(0.101)	(0.205)	(0.125)	(0.124)	(0.172)	(0.139)	(0.744)	(0.107)	(0.217)
Constant	-1.058***	-4.386***	-1.967***	-4.542***	-3.342***	-5.056***	-8.603***	-3.136***	-4.029***
	(0.0249)	(0.0518)	(0.0234)	(0.0550)	(0.0273)	(0.0516)	(0.190)	(0.0394)	(0.0493)
Observations	1922	1922	1922	1922	1922	1922	1922	1922	1922

Subsample	Appliances & Furniture	Book & Newspapers	Video & Music	Sporting equipment	Toys	Clothing & Footwear	Pharmaceutical & Cosmetic	Other retail stores	Second-hand shops
$Week_1 * LoanPH_1$	0.0537**	0.0384	0.202	0.0470*	0.0231	0.0654*	0.0205	-0.0113	0.0421
	(0.0163)	(0.0259)	(0.193)	(0.0220)	(0.0496)	(0.0307)	(0.0168)	(0.0245)	(0.0517)
$Week_1$	-0.300*	-0.319	-1.399	-0.313*	-0.228	-0.503*	-0.197	-0.161	-0.215
	(0.118)	(0.198)	(1.317)	(0.150)	(0.345)	(0.222)	(0.119)	(0.173)	(0.343)
Constant	-2.574***	-4.588***	-8.991***	-5.442***	-5.383***	-2.493***	-3.394***	-2.759***	-6.300***
	(0.0412)	(0.0294)	(0.247)	(0.0486)	(0.0646)	(0.0352)	(0.0296)	(0.0606)	(0.0890)
Observations	1922	1922	1898	1922	1922	1922	1922	1922	1922

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Table 4- The impact of emergency loan depth in round 1 on total consumption expenditure. The estimated regression is $log\,(Y_{pt}) = \alpha_1 Period_t + \alpha_2\, Loan_1 \times Period_t + \, Day_t + Wday_t + Year_t + Holiday_t + Province_p + \epsilon_{pt}$, where Y_{pt} is total consumption in province p and day t, Loan_1 is the total value of loans in **round one** divided by total monthly transactions in province, Period is a dummy of three weeks following Thursdays that the first round of loans is paid (three weeks in Ordibehest between 4th and 25th). Day, weekday, year, and holiday fixed effects are included in all regression. Standard errors are clustered at province level and are presented in the parenthesis. α_2 shows the percent growth in daily expenditure compared to the previous year in the week following the emergency loan if $Loan_{pt}$ increases by 0.01 unit: $\frac{\Delta Y}{Y} = \alpha_2 \times \Delta Loan_1$.

Panel A: Decomposition by durability

Subsample	All	Non-durable	Semi-durable	Durable	Asset
Period * Loan_1	-0.262	1.657***	2.357**	0.635	-10.17
	(1.298)	(0.371)	(0.797)	(0.548)	(5.866)
Period	-0.0421	-0.142***	-0.218**	-0.0921*	0.590
	(0.0939)	(0.0283)	(0.0612)	(0.0436)	(0.416)
Constant	27.50***	26.36***	24.90***	25.84***	23.34***
	(0.0199)	(0.0171)	(0.0312)	(0.0421)	(0.0673)
Observations	1922	1922	1922	1922	1922

Panel B: Decomposition by COICOP group

Subsample	Food & Beverage	Tobacco	Clothing & Footwear	Housing	Appliance & Furniture	Health	Transport	Communication	Recreation	Education	Restaurant & Hotel	Miscellaneous
Period * Loan_1	1.431***	1.516	2.948**	2.639**	1.705*	1.804**	1.815*	2.074	2.093**	1.694	1.247	-4.787
	(0.287)	(1.036)	(0.832)	(0.781)	(0.660)	(0.556)	(0.814)	(1.385)	(0.723)	(1.098)	(1.088)	(3.071)
Period	-0.102***	-0.167*	-0.239***	-0.288***	-0.178**	-0.261***	-0.0837	-0.165	-0.193**	-0.0672	-0.230**	0.219
	(0.0225)	(0.0705)	(0.0650)	(0.0593)	(0.0528)	(0.0424)	(0.0647)	(0.0986)	(0.0533)	(0.0835)	(0.0826)	(0.219)
Constant	26.08***	22.14***	24.57***	24.52***	24.52***	23.46***	25.66***	23.14***	24.08***	21.66***	23.85***	25.43***
	(0.0195)	(0.0566)	(0.0320)	(0.0357)	(0.0399)	(0.0301)	(0.0316)	(0.0425)	(0.0238)	(0.0515)	(0.0215)	(0.0490)
Observations	1922	1922	1922	1922	1922	1922	1922	1922	1922	1922	1922	1922

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Panel C: Decomposition by retail sector

Subsample	Non-chain stores	Chain stores	Spec. Food & Beverages	Tobacco	Fuel	Computer & Communication	Media equipment	Construction material	Carpet & Flooring
Period * Loan$_1$	1.283***	1.491	2.214**	1.516	0.964	0.542	15.73*	2.398***	1.365
	(0.305)	(1.648)	(0.612)	(1.036)	(0.931)	(1.253)	(6.134)	(0.625)	(1.102)
Period	-0.0870***	-0.133	-0.179***	-0.167*	-0.132	-0.0657	-1.141*	-0.259***	-0.122
	(0.0216)	(0.113)	(0.0471)	(0.0705)	(0.0663)	(0.0918)	(0.467)	(0.0497)	(0.0836)
Constant	25.61***	22.26***	24.69***	22.14***	23.35***	21.62***	18.03***	23.53***	22.66***
	(0.0232)	(0.0501)	(0.0245)	(0.0566)	(0.0264)	(0.0523)	(0.192)	(0.0415)	(0.0511)
Observations	1922	1922	1922	1922	1922	1922	1922	1922	1922

Subsample	Appliances & Furniture	Book & Newspapers	Video & Music	Sporting equipment	Toys	Clothing & Footwear	Pharmaceutic al & Cosmetic	Other retail stores	Second-hand shops
Period * Loan$_1$	1.850**	2.672*	12.42	3.285**	1.609	2.660**	1.177	-0.901	3.023
	(0.649)	(1.044)	(10.83)	(1.054)	(1.711)	(0.862)	(0.802)	(0.822)	(2.989)
Period	-0.179**	-0.266**	-0.792	-0.205**	-0.118	-0.215**	-0.169*	-0.228**	-0.258
	(0.0522)	(0.0869)	(0.773)	(0.0721)	(0.135)	(0.0664)	(0.0617)	(0.0648)	(0.210)
Constant	24.10***	22.09***	17.71***	21.26***	21.33***	24.20***	23.28***	23.89***	20.36***
	(0.0429)	(0.0325)	(0.280)	(0.0506)	(0.0610)	(0.0357)	(0.0301)	(0.0634)	(0.0830)
Observations	1922	1922	1898	1922	1922	1922	1922	1922	1922

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Table 5- The impact of emergency loan depth in round 1 on consumption expenditure per household. The estimated regression is $log\ (y_{pt}) = \beta_1 Period_t + \beta_2\ LoanPH_1 \times Period_t +\ Day_t + Wday_t + Year_t + Holiday_t + Province_p + \epsilon_{pt}$, where y_{pt} is consumption per household in province p and day t, LoanPH is the total value of loans in **round one** divided by total number of households in each province, Period is a dummy of three weeks following Thursdays that the first round of loans is paid (three weeks in Ordibehest between 4th and 25th).Day, weekday, year, and holiday fixed effects are included in all regression. Standard errors are clustered at province level and are presented in the parenthesis. β_2 shows the increase in daily expenditure compared to the previous year in the week following the emergency loan if $LoanPH_{pt}$ increases by 1 unit:$\Delta y/y = \beta_2 \times \Delta LoanPH$

Panel A: Decomposition by durability

Subsample	All	Non-durable	Semi-durable	Durable	Asset
Period * LoanPH1	0.0146	0.0362***	0.0447*	0.0180	-0.114
	(0.0181)	(0.00920)	(0.0196)	(0.0139)	(0.0905)
Period	-0.163	-0.277***	-0.364*	-0.173	0.668
	(0.130)	(0.0672)	(0.141)	(0.0993)	(0.649)
Constant	0.790***	-0.344***	-1.811***	-0.867***	-3.364***
	(0.0207)	(0.0175)	(0.0323)	(0.0420)	(0.0692)
Observations	1922	1922	1922	1922	1922

Panel B: Decomposition by COICOP group

Subsample	Food & Beverage	Tobacco	Clothing & Footwear	Housing	Appliance & Furniture	Health	Transport	Communication	Recreation	Education	Restaurant & Hotel	Miscellaneous
Period * LoanPH1	0.0324***	0.0416*	0.0520*	0.0504*	0.0298	0.0261*	0.0590**	0.0373	0.0446*	0.0599*	0.0304	-0.0422
	(0.00692)	(0.0172)	(0.0207)	(0.0188)	(0.0149)	(0.0126)	(0.0200)	(0.0207)	(0.0171)	(0.0231)	(0.0260)	(0.0462)
Period	-0.227***	-0.350**	-0.393*	-0.453**	-0.266*	-0.315**	-0.367*	-0.279	-0.356**	-0.365*	-0.354	0.175
	(0.0514)	(0.122)	(0.148)	(0.134)	(0.107)	(0.0906)	(0.143)	(0.144)	(0.121)	(0.158)	(0.187)	(0.330)
Constant	-0.624***	-4.567***	-2.140***	-2.183***	-2.187***	-3.244***	-1.055***	-3.568***	-2.632***	-5.058***	-2.856***	-1.279***
	(0.0199)	(0.0576)	(0.0329)	(0.0364)	(0.0400)	(0.0308)	(0.0327)	(0.0424)	(0.0235)	(0.0500)	(0.0223)	(0.0482)
Observations	1922	1922	1922	1922	1922	1922	1922	1922	1922	1922	1922	1922

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Panel C: Decomposition by retail sector

Subsample	Non-chain stores	Chain stores	Spec. Food & Beverages	Tobacco	Fuel	Computer & Communication	Media equipment	Construction material	Carpet & Flooring
Period * $LoanPH_1$	0.0304***	0.0508	0.0435*	0.0416*	0.0403	0.0434	0.289*	0.0283	0.0363
	(0.00774)	(0.0319)	(0.0160)	(0.0172)	(0.0215)	(0.0230)	(0.123)	(0.0148)	(0.0253)
Period	-0.208***	-0.382	-0.325**	-0.350**	-0.345*	-0.330*	-2.047*	-0.287*	-0.279
	(0.0560)	(0.220)	(0.115)	(0.122)	(0.151)	(0.154)	(0.861)	(0.106)	(0.180)
Constant	-1.094***	-4.451***	-2.020***	-4.567***	-3.359***	-5.090***	-8.699***	-3.170***	-4.044***
	(0.0235)	(0.0508)	(0.0258)	(0.0576)	(0.0263)	(0.0504)	(0.194)	(0.0414)	(0.0511)
Observations	1922	1922	1922	1922	1922	1922	1922	1922	1922

Subsample	Appliances & Furniture	Book & Newspapers	Video & Music	Sporting equipment	Toys	Clothing & Footwear	Pharmaceutical & Cosmetic	Other retail stores	Second-hand shops
Period * $LoanPH_1$	0.0344*	0.0600*	0.235	0.0594*	-0.00848	0.0517*	0.0224	-0.0194	0.0644
	(0.0152)	(0.0229)	(0.206)	(0.0251)	(0.0370)	(0.0224)	(0.0180)	(0.0178)	(0.0539)
Period	-0.288*	-0.496**	-1.557	-0.387*	0.0550	-0.387*	-0.242	-0.156	-0.493
	(0.109)	(0.170)	(1.436)	(0.169)	(0.266)	(0.159)	(0.130)	(0.123)	(0.360)
Constant	-2.610***	-4.621***	-9.020***	-5.450***	-5.368***	-2.505***	-3.420***	-2.813***	-6.347***
	(0.0428)	(0.0320)	(0.286)	(0.0507)	(0.0621)	(0.0367)	(0.0302)	(0.0627)	(0.0889)
Observations	1922	1922	1898	1922	1922	1922	1922	1922	1922

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Table 6 - The impact of emergency loan depth on total purchases via POS and online terminals. The estimated regression is $log\ (Y_{pt}) = \alpha_1 Week_1 + \alpha_2 Week_1 \times Loan_1 + Day_t + Wday_t + Year_t + Holiday_t + Province_p + \epsilon_{pt}$, where Y_{pt} is total consumption in province p and day t, Loan is the total value of loans in each round divided by total monthly transactions in province, period is a dummy of the weeks following Thursdays that the loans are paid (three weeks in Ordibehest between 4th and 25th). Day, weekday, year, and holiday fixed effects are included in all regression. Standard errors are clustered at province level. t-statistic are presented in the parenthesis.

Terminal	POS					Online				
Subsample	All	Non-durables	Semi-durables	Durables	Assets	All	Non-durables	Semi-durables	Durables	Assets
$Week_1 * Loan_1$	1.994**	1.773**	2.914**	1.406	0.662	-5.801	-4.699	-3.713	-5.717	-44.63
	(0.700)	(0.549)	(1.050)	(0.706)	(1.483)	(3.901)	(6.377)	(16.03)	(7.029)	(36.78)
$Week_1$	-0.137*	-0.0245	-0.238**	-0.144*	-0.235	0.379	0.419	0.195	0.472	2.065
	(0.0550)	(0.0428)	(0.0806)	(0.0558)	(0.119)	(0.289)	(0.447)	(1.131)	(0.480)	(2.125)
Constant	27.52***	26.39***	24.91***	25.85***	23.39***	21.10***	20.02***	15.57***	17.31***	7.008***
	(0.0180)	(0.0180)	(0.0300)	(0.0402)	(0.0543)	(0.0931)	(0.162)	(0.308)	(0.149)	(0.504)
Observations	1922	1922	1922	1922	1922	1922	1920	1830	1922	542

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Table 7 - The impact of emergency loan depth on total purchases between richer and poorer provinces. The provinces are divided based on the level of transactions per household as a measure of income. The estimated regression is $log\,(Y_{pt}) = \alpha_1 Week_1 + \alpha_2 Week_1 \times Loan_1 + Day_t + Wday_t + Year_t + Holiday_t + Province_p + \epsilon_{pt}$, where Y_{pt} is total consumption in province p and day t, Loan is the total value of loans in each round divided by total monthly transactions in province, period is a dummy of the weeks following Thursdays that the loans are paid (three weeks in Ordibehest between 4th and 25th). Day, weekday, year, and holiday fixed effects are included in all regression. Standard errors are clustered at province level. t-statistic are presented in the parenthesis.

Panel A: effect in first week Province's transactions per household	Bottom quartile					Top quartile				
Subsample	All	Non-durables	Semi-durables	Durables	Assets	All	Non-durables	Semi-durables	Durables	Assets
$Week_1$ * $Loan_1$	2.806*	4.239**	2.670	1.775	-4.664	-3.246	1.551	1.756	-0.579	-26.61
	(1.140)	(0.901)	(1.806)	(2.360)	(5.477)	(1.409)	(1.729)	(3.021)	(1.201)	(12.55)
$Week_1$	-0.233	-0.257*	-0.251	-0.213	0.213	0.150*	-0.0132	-0.139	-0.0306	1.091
	(0.106)	(0.0805)	(0.154)	(0.244)	(0.517)	(0.0433)	(0.101)	(0.161)	(0.0573)	(0.674)
Constant	26.66***	25.82***	24.10***	25.17***	21.41***	28.66***	27.69***	26.35***	27.10***	25.12***
	(0.0404)	(0.0478)	(0.0618)	(0.0494)	(0.121)	(0.0836)	(0.0500)	(0.0683)	(0.0628)	(0.196)
Observations	434	434	434	434	434	434	434	434	434	434

Panel B: effect in three weeks Province's transactions per household	Bottom quartile					Top quartile				
Subsample	All	Non-durables	Semi-durables	Durables	Assets	All	Non-durables	Semi-durables	Durables	Assets
Period * $Loan_1$	2.817*	3.395*	2.522	2.989	1.067	-4.921	1.470	1.168	-0.467	-27.82
	(1.032)	(1.034)	(1.252)	(1.462)	(3.985)	(2.322)	(0.855)	(2.072)	(1.232)	(13.98)
Period	-0.310*	-0.328*	-0.306*	-0.350	-0.254	0.187	-0.119*	-0.128	-0.0739	1.263
	(0.0988)	(0.0947)	(0.0954)	(0.160)	(0.358)	(0.101)	(0.0482)	(0.111)	(0.0756)	(0.716)
Constant	26.6***	25.79***	24.08***	25.15***	21.40***	28.66***	27.67***	26.34***	27.09***	25.16***
	(0.0427)	(0.0456)	(0.0624)	(0.0564)	(0.141)	(0.0757)	(0.0491)	(0.0725)	(0.0683)	(0.171)
Observations	434	434	434	434	434	434	434	434	434	434

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Appendix

Table A1- List of Shaparak activity codes based on durability.

Non-durables	Supermarkets and grocery stores; Miscellaneous Grocery Store - Mini Market (Grocery) - Specialty Store; Vendors of packaged and frozen meat; Confectioneries and nut shops; Food stores and restaurants; drug stores; Cosmetics Store; Electronics Stores; Chemicals (Other); Dairy Stores; Timber and building materials stores; Fast food restaurants; Beauty salons and hairdressers; Grocery store; Cigarette shops and stalls; Bakery; Food preparation; Crude oil and petroleum products; Stationery and office equipment store; Paints, polishes and painting consumables; Automatic fuel stations; Florists; Stationery, office supplies, printer paper and writing software; Handicraft and handicraft equipment stores; Building materials [Other]; Leather, wool and leather; carwash; Herds, florists and greenhouses; Dry wash; Industrial Consumables [Other]; Fuel sellers; Newspapers and newsstands; Fabrics, wardrobes, yarn and other dry goods; Carpet washing and upholstery; Drugs, prescription drugs and miscellaneous drugs; Car painting; Other non-durable goods; Laundry of houses;
Semi-durables	Men' and boys' clothing and accessories store; Sewing supplies, embroidery and fabric; Adult clothing; Pre-sewn women's clothing store; Shoe stores; Electrical parts and equipment; Tire stores; Building facilities; Baby & Baby Clothing Store; Curtains, blinds and upholstery; Miscellaneous clothing and accessories; Glass, paint and wallpaper stores; Sportswear and Equestrian Clothing Store; Special and miscellaneous home accessories and decorations; Family clothing store; Sporting goods store; Special clothing store (accessory); Office supplies, printing and photography; Women's, men's and children's uniforms and work clothes; Commercial shoes; Movie and DVD rental stores; Rental of clothes - uniforms, uniforms and special clothes; Religious goods stores; Artificial hair and wig shop;
Durables	Car and truck (new and second hand) - sales, service, repair, parts and mortgage; Jewellery stores, watches, watches, silver; Home Appliances Store; Offices and sales centres for metal products; Telecommunication and telephone equipment and tools; Car spare parts; Tool shops; Furniture, decorations, home appliances; Flooring stores; Computers and accessories; Precious stones and metals Watches and jewellery; Interior decoration stores; Carpentry and woodworking; Sellers and stores of motorcycle accessories; Miscellaneous machinery, aircraft and agricultural machinery; Auto parts and accessories; Crystal and crystal store; Plumbing and heating equipment; Horticultural & Greenhouse Supplies Stores; Medical, dental and hospital equipment and supplies; Cars and trucks (second hand only) - Sales, service, repair, parts and mortgages; Bookstores; Toy, toy and entertainment stores; Pet stores and related goods; Suitcases and leather goods; Gifts, cards and rhinestones; Tools and equipment; Office and commercial furniture; Media Products Store; Books, newspapers and magazines; Second category stores for sale (brokerage and lending); Commercial Machines [Other]; Bicycle shops - sales and services; Sellers of works of art and owners of galleries; Cameras and photographic and video equipment; Coin and stamp shops; Tent and mosquito net; Music Supplies Stores; Antiques for sale, repair and reconstruction; Sellers of video games and entertainment devices; Snow car dealers; Medical hearing aids (hearing aids); Digital goods of books, movies, music and multimedia; Sellers of recreational, installation and residential trailers; Sellers of mobile homes; Distribution and production of multimedia products; Book Fair; Medical orthopedic supplies and artificial limbs; Fireplaces, fireplaces and their accessories; Boat sellers; Sellers of mobile homes; Application software digital goods (other than games); Digital Goods - Digital Games; Durable Goods [Other]; Digital Goods Sell large-scale digital goods; Printing machine - sales and services;
Assets	Foreign exchange non-financial institutions, non-electric remittances, traveller's checks, savings and deposit funds; Real estate management and rental sales agencies; Equity trading agencies;

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Table A2- list of activity codes for each COICOP group.

COICOP group	Activity codes in Shapark
Food & Beverages	Supermarkets and grocery stores; Miscellaneous Grocery Store - Mini Market (Grocery) - Specialty Store; Vendors of packaged and frozen meat; Confectioneries and nut shops; Chain and department stores; Dairy Stores; Grocery store; Bakery;
Tobacco	Cigarette shops and stalls
Clothing & Footwear	Men's and boys' clothing and accessories store; Sewing supplies, embroidery and fabric; Adult clothing; Pre-sewn women's clothing store; Shoe stores; Baby & Baby Clothing Store; Sewing, sewing and embroidering clothes; Miscellaneous clothing and accessories; Sportswear and Equestrian Clothing Store; Family clothing store; Special clothing store (accessory); Leather, wool and leather; Women's, men's and children's uniforms and work clothes; Dry wash; Commercial shoes; Repair, cleaning and waxing of shoes and hats; Rental of clothes - uniforms, uniforms and special clothes; Laundry of houses; Laundry shops;
Housing Expenses	Other construction services; Building, architecture, stone cutting, carpet and parquet and building paint; Flooring stores; Timber and building materials stores; Building facilities; Plumbing and heating equipment; Building electrical services; Glass, paint and wallpaper stores; Crude oil and petroleum products; Paints, polishes and painting consumables; Automatic fuel stations; Welding services; Building materials [Other]; Roofing and roofing services; Service companies (water, electricity, gas); Concrete services; Fuel sellers; Cleaning, polishing and flooring services; Sellers of mobile homes;
Appliance & Furniture	Home Appliances Store; Electrical parts and equipment; Tool shops; Furniture, decorations, home appliances; Interior decoration stores; Carpentry and woodworking; Curtains, blinds and upholstery; Crystal and crystal store; Horticultural & Greenhouse Supplies Stores; Special and miscellaneous home accessories and decorations; Tools and equipment; Office and commercial furniture; Cleaning services; Second category stores for sale (brokerage and lending); Carpet washing and upholstery; Mortgage, rental of equipment, tools, furniture and various devices; Antiques for sale, repair and reconstruction; Repair, reconstruction and replacement of furniture upholstery; Spraying and pest control services; Electronics repair shop; Refrigeration and freezer repair shop; Fireplaces, fireplaces and their accessories;
Health	drug stores; Hospitals; Doctors [Other]; Dental and orthopaedic services; Medical Services and Health Specialists (Other); Medical, dental and hospital equipment and supplies; Medical and dental laboratories; Glasses and lens services; Drugs, prescription drugs and miscellaneous drugs; Ambulance service; Ophthalmologists and optometrists; Nursing and personal care centres; Chiropractors; Medical hearing aids (hearing aids); Health and beauty centres; Medical orthopaedic supplies and artificial limbs; Bone disease specialists; Massage salons; Paediatricians;
Transport	Car and truck (new and second hand) - sales, service, repair, parts and mortgage; Fuel service station; Car spare parts; Car service [not dealers]; Travel agency, tourism services and tours; Tire stores; Car tire repair and coating services; Sellers and stores of motorcycle accessories; Miscellaneous machinery, aircraft and agricultural machinery; Auto parts and accessories; Urban and non-urban freight services; Cars and trucks (second hand only) - Sales, service, repair, parts and mortgages; Car smoothing; Shipping and shipping lines; Taxi and car rental; In-town and suburban transportation; carwash; Bus lines; Bicycle shops - sales and services; Airlines and air transport; Rail lines; Parking, parking meter and garage; Transportation Services [Other]; Maritime services; Car painting; Truck and trailer rental companies; Car rental companies; Car Warehouses; Airports and air terminals; Towing and towing a car; Railway passenger; Road tolls; Boat sellers;
Communication	Telecommunication and telephone equipment and tools; Office of Local and Road Telecommunication Services; Postal and courier services; Postal services - especially government; Repair shop of small electronic devices and devices; Telegraph service; Computer programmers, information analysts, and designers of specific computer systems; Agricultural and horticultural services; Computer network and Internet services;

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Recreation	Computers and accessories; Electronics Stores; Agricultural and horticultural services; Stationery and office equipment store; Photography studios; Florists; Bookstores; Stationery, office supplies, printer paper and writing software; Sporting goods store; Handicraft and handicraft equipment stores; Toy, toy and entertainment stores; Commercial sports, professional sports associations, sports fields, tournament organizers; Miscellaneous printing and publishing services; Pet stores and related goods; Reproduction, copy and reconstruction services; Veterinary services; Office supplies, printing and photography; Media Products Store; Books, newspapers and magazines; Computer maintenance services; Herds, florists and greenhouses; Sellers of works of art and owners of galleries; Private clubs, private golf and equestrian clubs; Cameras and photographic and video equipment; Tent and mosquito net; Newspapers and newsstands; Bands, orchestras and miscellaneous entertainment; Exhibitions and tourist attractions; Computer software stores; Music Supplies Stores; Swimming pools - sales and services; Billiard halls; Amusement forest parks; Sellers of video games and entertainment devices; Snow car dealers; Boat, jet ski and water recreation services; Movie and DVD rental stores; Public aquariums, zoos and dolphin display pools; Amusement, circus and carnival; Digital goods of books, movies, music and multimedia; Recreational sports camps; Sellers of recreational, installation and residential trailers; Recreational Services [Other]; cinema; Sellers of mobile homes; Distribution and production of multimedia products; Religious goods stores; Book Fair; Bowling alleys; Typing and lithography services; Video game and entertainment halls; Application software digital goods (other than games); Rental facilities for recreational vehicles and mobile homes; Digital Goods - Digital Games; Photo emergence and retouching labs; Photography, art and commercial graphics; Cable and pay TV services; Printing machine - sales and services; Theatre Producers and Theatre Ticket Agencies; Auctions;
Education	Other educational service providers; Primary, middle and high schools; Colleges, universities and higher education centres; Technical and vocational schools; Macathic and virtual training centres; Organizers of public golf classes; Business and secretarial training centres;
Restaurant & Hotel	Food stores and restaurants; Fast food restaurants; Accommodation services of hotels, inns, resorts and accommodation service centres; Food preparation;
Miscellaneous	Financial institutions - manual payment operations; Other professional services; Jewellery stores, watches, watches, silver; Foreign exchange non-financial institutions, non-electric remittances, traveller's checks, savings and deposit funds; Other government services; Insurance and financial contracts; Cosmetics Store; Real estate management and rental sales agencies; Precious stones and metals Watches and jewellery; Accounting, auditing and bookkeeping services; Other membership organizations; Electronic funds transfer centre; Beauty salons and hairdressers; Management, consulting and public services; Charities and social services; Equity trading; Legal and judicial services; Direct Marketing - Insurance Services; Miscellaneous personal services [Other]; Suitcases and leather goods; Financial institutions - goods and services; Religious organizations; Financial institutions - automatic payment operations; Advertising services; Retail sale of miscellaneous and special goods; Miscellaneous general goods; Tax payment services; Childcare services; Court and Judicial Services; Business Services [Other]; Civic, social and fraternal associations and unions; Counselling services - loans, marriage and personal; Suppliers of security systems, privates, bulletproof vests and guard dogs; Businesses for sale and purchase; Coin and stamp shops; Wristwatch, wall clock and jewellery repair shop; Shaver sales and services; Burial services; Recruitment agencies and temporary labour supply; Car unions; Judicial power; Miscellaneous repair shop and related services; Government funds; Political organizations; Artificial hair and wig shop; Test laboratories (non-medical); Replication of antiques; Complaint;

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Table A3- list of activity codes for each retail sector

Retail sector	Activity codes in Shapark
Non-chain grocery	Supermarkets and grocery stores; Miscellaneous Grocery Store - Mini Market (Grocery) - Specialty Store; Agricultural and Livestock Cooperative;
Chain grocery	Chain and department stores; Retail sale of miscellaneous and special goods; Miscellaneous general goods
Specialized Food & Beverages	Vendors of packaged and frozen meat; Dairy Stores; other grocery store; Bakery
Tobacco	Cigarette shops and stalls
Fuel	Automatic fuel stations; Fuel sellers; Fuel service stations
Computer & telecommunication	Telecommunication and telephone equipment and tools; Computers and accessories; Computer software stores; Sellers of video games and entertainment devices; Application software digital goods (other than games); Digital Goods & Games;
Audio & video equipment	Distribution and production of multimedia products
Hardware & paint	Tool shops; Timber and building materials stores; Plumbing and heating equipment; Glass, paint and wallpaper stores; Paints, polishes and painting consumables; Building materials [Other]; Tools and equipment;
Carpets & covering	Flooring stores; Curtains, blinds and upholstery
Appliances & furniture	Home Appliances Store; Electrical parts and equipment; Furniture, decorations, home appliances; Electronics Stores; Interior decoration stores; Crystal store; Special and miscellaneous home accessories and decorations; Office and commercial furniture; Shaver sales and services; Fabrics, wardrobes, yarn and other dry goods; Music Supplies Stores;
Book & newspapers	Stationery and office equipment store; Bookstores; Stationery, office supplies, printer paper and writing software; Handicraft and handicraft equipment stores; Books, newspapers and magazines; Newspapers and newsstands; Book Fair;
Music & video	Digital goods of books, movies, music and multimedia
Sporting equipment	Sportswear and Equestrian Clothing Store; Sporting goods store; Bicycle shops - sales and services; Boat sellers
Toys	Toy, toy and entertainment stores
Clothing & footwear	Men' and boys' clothing and accessories store; Adult clothing; Pre-sewn women's clothing store; Shoe stores; Baby & Baby Clothing Store; Miscellaneous clothing and accessories; Family clothing store; Suitcases and leather goods; Special clothing store (accessory); Women's, men's and children's uniforms and work clothes; Commercial shoes;
Pharmaceutical	drug stores; Cosmetics Store; Medical, dental and hospital equipment and supplies; Drugs, prescription drugs and miscellaneous drugs; Medical orthopaedic supplies and artificial limbs;
Other retails	Jewellery stores, watches, watches, silver; Precious stones and metals Watches and jewellery; Chemicals (Other); Horticultural & Greenhouse Supplies Stores; Florists; Pet stores and related goods; Gifts, cards and rhinestones; Herds, florists and greenhouses; Commercial Machines [Other]; Industrial Consumables [Other]; Coin and stamp shops; Tent and mosquito net; Other non-durable goods; Sellers of recreational, installation and residential trailers; Religious goods stores; Artificial hair and wig shop; Durable Goods [Other]; Stores of tax-free products;
Second hand shops	Second category stores for sale (brokerage and lending); Antiques for sale, repair and reconstruction; Replication of antiques

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Table A2- The impact of emergency loan depth on total consumption expenditure. The estimated regression is $log\ (Y_{pt}) = \gamma_1 Week_1 \times Loan_1 + \gamma_2 Week_2 \times Loan_2 + \gamma_3 Week_3 \times Loan_3 + Week_t + Day_t + Wday_t + Year_t + Holiday_t + Province_p + \epsilon_{pt}$, where Y_{pt} is total consumption in province p and day t, $Loan_i$ is the total value of loans in round i divided by total monthly transactions in province, $Week_t$ is a dummy of the week following Thursdays of each round ($Week_1$ is one between 4th and 10th of 2020, and so on). Day, weekday, year, and holiday fixed effects are included in all regression. Standard errors are clustered at province level. t-statistic are presented in the parenthesis. γ_i shows the percent growth in daily expenditure compared to the previous year in the week following the emergency loan if $Loan_i$ increases by 0.01 unit: $\frac{\Delta Y}{Y} = \alpha_2 \times \Delta Loan$.

Panel A: Decomposition by durability

Subsample	All	Non-durable	Semi-durables	Durables
$Week_1$ * $Loan_1$	0.271	1.880**	3.101**	0.836
	(0.26)	(3.15)	(2.80)	(1.41)
$Week_2$ * $Loan_2$	-5.770	7.654	12.93**	6.724
	(-0.63)	(2.01)	(2.76)	(1.63)
$Week_3$ * $Loan_3$	-20.84	2.119	23.57	0.233
	(-1.16)	(0.18)	(1.61)	(0.01)
Observations	1922	1922	1922	1922

Panel B: Decomposition by COICOP group

Subsample	Food & Beverage	Tobacco	Clothing & Footwear	Housing	Appliance & Furniture	Health	Transport	Communication	Recreation	Education	Restaurant & Hotel	Miscellaneous
$Week_1$ * $Loan_1$	1.475**	2.810**	3.758**	3.242***	2.266**	1.753*	0.876	4.371**	2.176*	3.318*	1.721	-4.252
	(3.12)	(2.91)	(3.07)	(3.86)	(3.05)	(2.36)	(1.01)	(3.64)	(2.46)	(2.11)	(1.52)	(-1.46)
$Week_2$ * $Loan_2$	6.351	-3.642	16.99**	18.88**	10.39**	14.34***	10.84	2.305	12.79**	21.95***	9.140	-40.55
	(1.59)	(-0.40)	(3.45)	(3.33)	(3.27)	(3.67)	(1.71)	(0.20)	(3.19)	(3.86)	(1.38)	(-1.85)
$Week_3$ * $Loan_3$	-0.984	2.632	36.04**	25.39	10.38	22.64	17.46	-22.39	21.06	-18.64	31.24*	-93.85
	(-0.09)	(0.07)	(2.82)	(1.22)	(0.45)	(1.86)	(0.74)	(-0.87)	(1.32)	(-0.59)	(2.54)	(-1.95)
Observations	1922	1922	1922	1922	1922	1922	1922	1922	1922	1922	1922	1922

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Panel C: Decomposition by retail sector

Subsample	Non-chain stores	Chain stores	Spec. Food & Beverages	Tobacco	Fuel	Computer & Communication	Media equipment	Construction material	Carpet & Flooring
$Week_1 * Loan_1$	1.420*	0.680	2.594***	2.810**	0.969	1.711	16.23**	2.867***	3.203*
	(2.09)	(0.41)	(4.17)	(2.91)	(0.78)	(1.37)	(3.20)	(3.85)	(2.36)
$Week_2 * Loan_2$	4.766	7.530	9.374*	-3.642	1.241	-6.553	93.06*	20.89***	6.503
	(0.98)	(0.78)	(2.66)	(-0.40)	(0.12)	(-0.83)	(2.22)	(5.24)	(0.93)
$Week_3 * Loan_3$	-12.37	20.27	15.70	2.632	-36.33	-28.90	325.6*	35.29	-14.71
	(-1.04)	(0.46)	(1.08)	(0.07)	(-1.48)	(-1.11)	(2.06)	(1.46)	(-0.57)
Observations	1922	1922	1922	1922	1922	1922	1922	1922	1922

Subsample	Appliances & Furniture	Book & Newspapers	Video & Music	Sporting equipment	Toys	Clothing & Footwear	Pharmaceutical & Cosmetic	Other retail stores	Second-hand shops
$Week_1 * Loan_1$	2.630***	1.984	13.49	2.680*	3.350	3.605**	1.490	-0.146	1.638
	(3.79)	(1.42)	(1.23)	(2.58)	(1.40)	(2.75)	(1.87)	(-0.12)	(0.56)
$Week_2 * Loan_2$	10.18**	22.79**	53.60	8.884	8.316	13.35**	5.886	-0.744	7.481
	(3.17)	(3.07)	(0.83)	(1.09)	(0.86)	(3.05)	(0.97)	(-0.12)	(0.43)
$Week_3 * Loan_3$	6.769	36.81	-82.55	40.30	-32.51	20.85	-2.354	-13.48	84.11
	(0.27)	(1.41)	(-0.29)	(1.95)	(-0.88)	(1.69)	(-0.13)	(-0.89)	(1.33)
Observations	1922	1922	1898	1922	1922	1922	1922	1922	1922

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Sectoral Employment Effects of State Fiscal Relief: Evidence from the Great Recession, Lessons for the Covid-19 Crisis

Christian Bredemeier[1], Falko Juessen[2] and Roland Winkler[3]

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This paper documents that the employment effects of financial aid to U.S. states during the Great Recession were strongly unevenly distributed across sectors. We show that state fiscal relief had a double dividend: not only did it preserve a substantial number of jobs, but it also fostered employment most strongly in the sectors hit hardest by the recession. We exploit differences in the distribution of recessionary job losses across states to draw conclusions for the Covid-19 recession. Our results suggest that the double dividend of state fiscal relief cannot be taken for granted.

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1 University of Wuppertal and IZA
2 University of Wuppertal and IZA
3 University of Antwerp

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1 Introduction

For state governments in the U.S., the Covid-19 crisis has led to precipitous declines in revenues and soaring expenses. Since states are effectively required by law to run balanced budgets, many of them will have to slush costs or raise taxes if not supported financially by the federal government. Spending contractions or tax hikes on the part of state governments are likely to deepen the economic downturn further. Accordingly, there are prominent calls for Congress to provide financial help to state governments beyond the $150 billion Coronavirus Relief Fund established in the Coronavirus Aid, Relief, and Economic Security (CARES) Act. The National Governors Association requested an additional $500 billion in federal aid. The Coronavirus supplemental spending bill proposed by the Democratic majority in the House of Representatives in May 2020 includes $1 trillion in funding for state and local governments. While Fed Chairman Jerome H. Powell warned that leaving state governments fight for themselves would make the economic crisis worse, extending the federal aid to state governments faces powerful opposition from, e.g., Senate majority leader Mitch McConnel and President Donald Trump.

In this paper, we seek to learn from the Great Recession about the labor-market consequences of state fiscal relief and to draw conclusions for the Covid-19 crisis. During the Great Recession, state fiscal relief was one of the major components of the American Recovery and Reinvestment Act (ARRA), the around $800 billion fiscal stimulus package signed by President Obama in February 2009. The spending component of the ARRA stimulus (which also included about $350 billion in transfers and tax cuts) was largely channeled through state and local governments who received close to $250 billion from the federal government. A considerable fraction of this money was explicitly intended to relax the strain on states' budgets, and almost all transfers were fungible, i.e., states could effectively use the money as they wished (Chodorow-Reich et al., 2012, Conley and Dupor, 2013). Most transfers to states took the form of relieving state governments from payment obligations, either through increasing federal spending shares in, e.g., Medicaid, or through waiving states' cost shares in (e.g., infrastructure) projects financed by the federal government. In both cases, the respective funds effectively increased states' budgetary leeway. State fiscal relief has also been implemented in the course of previous recessions, but the context of the Great Recession is

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particularly suited to learn about its effects due to the detailed documentation of the outlays.[1]

Chodorow-Reich et al. (2012), Wilson (2012), Conley and Dupor (2013), and Chodorow-Reich (2019), among others, have shown that the financial transfers to state governments implemented in the ARRA had positive employment effects, including substantial effects in the private economy. In this paper, we look beyond this aggregate effect and study its distribution across sectors. Specifically, we investigate to what extent state fiscal relief fostered employment in those sectors that had been hit hardest by the economic downturn. This is important because it is difficult for workers to switch industries (Weinberg, 2001; Artuç and McLaren, 2015). As a consequence, promoting employment is particularly valuable in sectors that have been affected severely by the crisis because this will improve the labor-market prospects of the hardest-hit groups of workers. From an aggregate perspective, the costs of a recession can be reduced when displaced workers are enabled to find new jobs in their old industries such that the loss of industry-specific human capital (Neal, 1995; Sullivan, 2010) are avoided. This is reflected in the statement of purpose of the ARRA which includes the goals to preserve and create jobs and to assist those most impacted by the recession.

We build on the approach by Chodorow-Reich (2019) to estimate how the employment effects of financial aid to states were distributed across sectors. This approach uses *pre-recession* information on the size of states' obligations to avoid endogeneity due to states in worse shape receiving more federal assistance. We find substantial heterogeneity in the employment effects of state fiscal relief across sectors. Most strikingly, about 40% of the employment effects (roughly 0.8 out of a total of 2 job-years per additional $100,000 in aid) materialized in the construction sector, which made up only about 5.5% of pre-crisis employment. Importantly, we find that the positive employment effects of transfers to states occurred disproportionately in sectors that were hit harder by the recession. For example, the construction sector was the sector in which employment had declined strongest in the early phase of the recession, and our results show that this sector benefitted most strongly from additional intergovernmental transfers. Hence, state fiscal relief during the Great

[1] In smaller volume than in the Great Recession, state fiscal relief measures were also implemented in the 1972 State and Local Fiscal Assistance Act and the 2003 Jobs and Growth Tax Relief Reconciliation Act. The ARRA included an unusually strict provision on documentation – section 1512 of the bill requires federal agencies to report outlays in each state and all prime recipients to report the funds received – as part of President Obama's transparency and open government promises.

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Recession had a double dividend. Not only did it preserve a substantial number of jobs, but it also protected employment most strongly in the hardest-hit parts of the economy.

We then investigate in how far the double dividend of state fiscal relief can also be expected in the Covid-19 crisis. This time, most job losses accrued in industries that are characterized by a high intensity of face-to-face contact between workers and clients such as the leisure and hospitality sector and retail trade (Adams-Prassl et al., 2020). To shed light on whether extending state fiscal relief measures would again help most strongly the hardest-hit parts of the economy, we exploit differences across states in the extent to which the Great Recession hit different sectors disproportionately. We find that, in states where a specific sector had been hit harder, federal transfers did not have a significantly more pronounced effect on employment in this sector. This result hints at the strong employment effects in these sectors, e.g., the construction sector, being mostly systematic and unrelated to the specifics of the Great Recession. We therefore conclude that to support the sectors which are hit hardest by the Covid-19 recession, state lawmakers would have to use intergovernmental transfers in a sharply different way than they did during the Great Recession.

The remainder of this paper is organized as follows. Section 2 summarizes the econometric approach, and Section 3 presents the results and discusses the implications of our findings for the Covid-19 crisis. Section 4 concludes.

2 Methodology

To determine the relationship between the effects of state fiscal relief in a sector and the degree to which the Great Recession hit the sector, we proceed in two steps. First, we estimate sector-specific job-year coefficients, i.e., we estimate, sector by sector, the number of additional job-years in this sector per additional $100,000 of ARRA spending. As discussed in the Introduction, transfers received through the different programs of the ARRA were essentially alike from the perspective of a state's government as they increased budgetary leeway. We therefore analyze the effects of total ARRA payouts to states. In the second step, we translate the estimated sector-specific job-year coefficients from the first step into percentage employment effects and regress those on the percentage job losses before ARRA by sector.

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To estimate sector-specific job-year coefficients (step 1), we use the Chodorow-Reich (2019) approach, which exploits variation in ARRA outlays across U.S. states. To address that outlays were endogenous to a state's economic condition in the crisis, they are instrumented by states' pre-crisis payments in domains where the federal government took over parts of the states' obligations. Chodorow-Reich (2019) has harmonized the instrumental-variable approaches developed in the literature, and his updated analysis provides a template for studies on the effects of ARRA intergovernmental transfers. We follow Chodorow-Reich (2019)'s preferred specification and combine three instruments: states' pre-recession Medicaid spending (as proposed by Chodorow-Reich et al., 2012), the formulaic component of states' highway spending (Wilson, 2012; Conley and Dupor, 2013), and the formulaic component of all ARRA spending by federal agencies allocated independently of state-specific developments in the recession (Dupor and Mehkari, 2016; Dupor and McCrory, 2018). For our purpose, it is important that the instruments do not directly affect the industry mix of employment. The studies cited above carefully demonstrate that funds received by states through ARRA were fungible, i.e., could be used by state governments as they wished. This means that, e.g., Medicaid relief did not constitute a stimulus directed to the health sector.

We run separate regressions for each NAICS supersector.[2] For supersector i, the baseline cross-sectional 2SLS regression is given by

$$\sum_{h=0}^{H} (Y_{s,i,t+h} - Y_{s,i,t}) = \alpha_i + \beta_i F_s + \gamma_i' X_s + \varepsilon_{s,i}, \tag{1}$$

with

$$F_s = \Pi_0 + \Pi_1' Z_s + \Pi_2' X_s + \nu_{s,} \tag{2}$$

where s denotes federal states, i denotes sectors, and t is the start of the treatment period (in our case, this is December 2008, when important components of the ARRA became known publicly). The dependent variable is the cumulated monthly employment level from December 2008 through December 2010 (by state and sector), net of the level in December 2008, normalized by the adult

[2]We separate both retail trade and wholesale trade from the trade, transportation, and utilities supersector. We label the remaining group of industries in this supersector the transportation, warehousing, and utilities sector. We further separate the manufacturing supersector into durable goods and non-durable goods manufacturing.

population, and translated into job-years, i.e.,

$$Y_{s,i,t+h} - Y_{s,i,t} = \frac{1}{12}\left(\frac{\text{Employment}_{s,i,t+h} - \text{Employment}_{s,i,t}}{\text{Working age population}_{s,t}}\right). \tag{3}$$

Accordingly, the time span is $H = 24$ months. The endogenous variable F_s is total ARRA outlays to state s from December 2008 to December 2010, measured in \$100,000 increments and per person of working age in December 2008. It is instrumented by the vector Z_s, as described above, where instruments are normalized by the adult population in December 2008. Following Chodorow-Reich (2019), we include as control variables (captured in vector X_s) states' pre-ARRA employment-to-population ratio as well as pre-ARRA trends in employment and production to account for the potential threat to identification that states' differential pre-crisis trends were correlated with the pre-crisis spending levels measured by the instruments. Specifically, the regressions account for the December 2008 employment-to-population ratio, the change in employment from December 2007 to December 2008, and the change in gross state product (GSP) from the fourth quarter of 2007 to the fourth quarter of 2008. As in Chodorow-Reich (2019), the control variables are normalized to have unit variance. In robustness checks, we also control for sector-specific employment trends within states. The coefficient on ARRA outlays, β_i, measures the number of additional job-years in sector i due to an additional \$100,000 spent across all sectors. It compares the actual employment development in a sector to the counterfactual with fewer ARRA transfers. The approach does not allow us to disentangle between prevented job destruction and induced job creation. As discussed by Chodorow-Reich et al. (2012), relief payments were used in two ways: to avoid or alleviate spending cuts and to prevent or lower tax and fee increases. Accordingly, we phrase our results in terms of job-years preserved through state fiscal relief.

To determine the relationship between the sectoral employment effects of ARRA transfers to states and sectoral job losses during the recession (step 2 of our analysis), we calculate, for each sector i, relative employment gains from an additional \$1 billion in yearly ARRA payments,

$$\text{Gains}_i \equiv \frac{\widehat{\beta}_i \cdot \kappa}{\text{Employment}_{i,t-13}}, \tag{4}$$

where $\widehat{\beta}_i \cdot \kappa$ with $\kappa \equiv \$1\text{ billion/year/}\$100,000$ is the absolute employment gain from an additional \$1 billion, which we divide by the sector's pre-crisis employment level in November 2007,

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$\text{Employment}_{i,t-13}$. We then regress these relative employment gains on sector-specific relative employment changes during the first year of the recession (i.e., the part of the downturn before ARRA):

$$\text{Gains}_i = \delta + \zeta \cdot \frac{\text{Employment}_{i,t-1} - \text{Employment}_{i,t-13}}{\text{Employment}_{i,t-13}} + \epsilon_i. \tag{5}$$

To take into account estimation uncertainty from step 1, we weigh observations by the statistical significance (one minus p-value) of the estimated job-year coefficients $\widehat{\beta}_i$. This regression does not aim at identifying a causal relation between recessionary job losses and the sectoral effects of the ARRA, but it is merely an accounting tool that helps to summarize the descriptive relationship between the two.[3]

Data. Monthly employment data by state and industry come from the Current Employment Statistics (CES) of the Bureau of Labor Statistics (BLS).[4] For a few sector-state combinations, the required monthly employment information is missing (see Table A.1 in the Appendix for sample sizes). We use the data on ARRA outlays and the instruments from Chodorow-Reich (2019). Population data are from the BLS Local Aea Unemployment Statistics and GSP data are from the Bureau of Economic Analysis (BEA) Regional Data, GDP by state.[5]

3 Results

Sector-specific employment effects. The results from the first step of our analysis are summarized in Figure 1, which displays the estimated sector-specific job-year coefficients (the full regression results are shown in Table A.1 in the Appendix).[6] As Chodorow-Reich (2019), we estimate that an additional \$100,000 in ARRA payouts increased total employment by the equivalent of about two jobs, each of which lasts for one year.[7] Figure 1 illustrates that there was substantial heterogeneity in the effects across sectors. Close to 0.8 job-years, or nearly 40% of the total impact, accrued in the construction sector. Professional and business services, wholesale trade, and the residual "other services" sector also experienced significant employment effects. Furthermore,

[3] As emphasized in Chodorow-Reich et al. (2012), it is unlikely that employment developments until November 2008 already reflected anticipated effects of the ARRA stimulus. Important components of the ARRA became apparent no sooner than in December 2008.

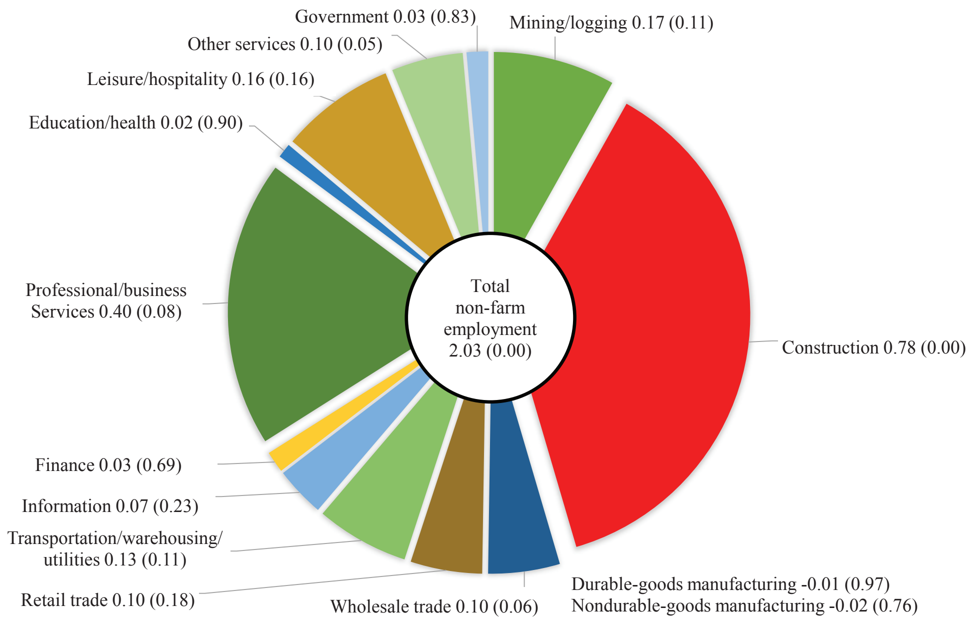
[4] See `https://www.bls.gov/sae/data/home.htm`

[5] See `https://www.bls.gov/lau/` and `https://www.bea.gov/data/gdp/gdp-state`, respectively.

[6] First-stage F-statistics range from 39.8 to 46.1.

[7] The difference between our estimate (2.03) and Chodorow-Reich (2019)'s estimate (2.01) is due to data revisions.

Figure 1: Sector-specific employment effects of ARRA outlays (job-years per marginal \$100,000; p-values in parentheses)



Notes: Coefficients on ARRA outlays from sector-specific 2SLS regressions. Dependent variable: sector-specific cumulated monthly employment from December 2008 through December 2010 net of December 2008 employment. Regressor of interest: Total ARRA outlays between December 2008 and December 2010, instrumented as described in the text. Control variables: December 2008 total employment, change in total employment from December through December 2009, 2007Q4-2008Q4 change in gross state product.

there are noticeable, yet statistically insignificant, employment effects in retail trade, the leisure and hospitality sector, mining and logging, as well as the trade, warehousing, and utilities sector. Employment effects in other sectors tend to be small.

To put the small estimate for government employment into perspective, recall that our cross-sectional analysis determines the effects of the ARRA payments that some states received more than others. Our results do not rule out that inframarginal ARRA dollars were used to preserve government jobs across states, they rather indicate that the marginal ARRA dollar was used otherwise and affected employment most strongly in the private sector. The estimate for mining and logging should be considered with caution due to the fracking boom that is potentially confounded with the effects of ARRA transfers because of similar timing.[8]

[8]The fracking boom cannot easily be accounted for by, e.g., including pre-crisis trends as control variables because fracking hit off almost simultaneously with the ARRA stimulus, especially in small states where this development may be particularly influential. E.g., in North Dakota, production of shale gas rose more than eightfold from 2008 to 2009 after being virtually constant at low levels before (according to data from the U.S. Energy Information Agency).

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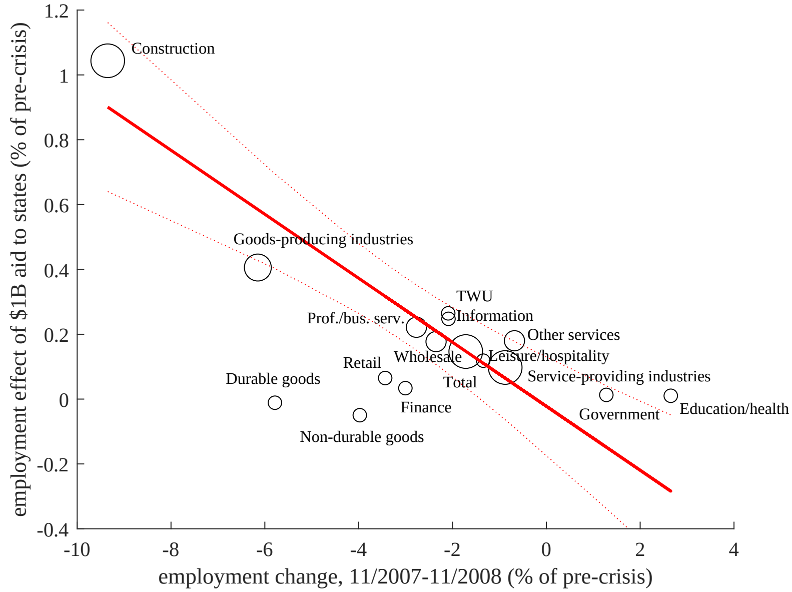
Figure 2: Employment effects of ARRA outlays by sector's exposure to downturn.



Notes: Vertical axis shows relative employment gains of $1 billion of additional ARRA outlays, as defined in equation (4). The size of circles indicates the statistical significance of the underlying job-years coefficient. Large circles: p-value$\leq$0.01; medium circles: p-value$\leq$0.05; small circles: p-value$\leq$0.10, tiny circles: p-value>0.10. The regression uses one minus p-value as weights. Estimated employment gains for total employment and supersector groups (goods-producing and services-providing industries) are shown in the scatter plot for comparison but omitted from the regression. TWU = Transportation, warehousing, and utilities. Prof./bus. serv. = Professional and business services

Overall, our estimates imply that, had yearly ARRA payments to states (which averaged $131.5 billion in 2009 and 2010) been lower by $1 billion, total employment would have been lower by 0.15% of pre-crisis employment (equivalent to 20,000 jobs) and employment in the construction sector would have been lower by as much as 1.04% of its pre-crisis level.

Figure 2 plots estimated relative employment gains due to ARRA payments, as defined in equation (4), against sector-specific relative employment changes during the first year of the recession (i.e., the part of the downturn before ARRA), see equation (5). Larger circles indicate more precise estimates of the underlying job-year coefficients, and the red lines show the fitted linear relation from regression (5), along with a 95% confidence interval. The figure shows that, in general, additional ARRA transfers to states had more substantial employment effects in sectors that had been hit harder by the crisis. Hence, ARRA payments to states stimulated the labor market especially in the sectors that had already suffered the most. On average, a one percentage

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point stronger decline in employment during the first year of the recession is associated with a roughly 0.1 percentage points stronger estimated employment effect of an additional \$1 billion in ARRA outlays (i.e., $\widehat{\zeta} = 0.099$, p-value<0.001).[9] This reveals the double dividend of state fiscal relief in the Great Recession: many jobs were preserved, and disproportionately so in sectors that were affected disproportionately by the downturn. This way, state fiscal relief prevented further deterioration of the labor-market prospects of the hardest-hit groups of workers.

We corroborated our results in several robustness checks. In particular, we applied the Medicaid instrument suggested by Chodorow-Reich et al. (2012) rather than the baseline combination of instruments controlled for sector-specific pre-ARRA employment trends, and accounted for the full set of control variables considered in Chodorow-Reich (2019)'s sensitivity analysis. See Appendix for details.

Lessons for the Covid-19 crisis. While the Great Recession was a typical recession regarding the distribution of job losses across sectors with construction and manufacturing being the hardest-hit sectors (Hoynes et al., 2012), the Covid-19 crisis is different. This time, other sectors are most strongly affected by the downturn, such as retail trade, leisure, and hospitality. We now discuss whether extending financial aid to states in the Covid-19 crisis would also yield a double dividend in the sense that the additional funds would save or create jobs disproportionately in the sectors that are hit hardest by this recession. One the one hand, we have documented that the overall employment gains due to state fiscal relief during the Great Recession were moderate in those sectors that are now hit hard by the Covid-19 crisis, see Figure 1. On the other hand, we have shown that, during the Great Recession, there was a positive relationship between the employment gains due to state fiscal relief and how strongly a sector was affected by the downturn, see Figure 2. If the latter finding applies to recessions in general, we can expect that extending state fiscal relief in the Covid-19 crisis would support employment predominantly in the hardest-hit parts of the economy, in particular retail trade, leisure, and hospitality. To shed light on this, we exploit that, in the Great Recession, the distribution of job losses across sectors differed between *states*. For example, in the first twelve months of the Great Recession, the construction sector in the U.S.

[9] In line with our previous results, the construction sector is an important driver of this result, being the sector most affected by the crisis and the strongest beneficiary of the relief money. Leaving out this sector weakens the relation between crisis exposure and employment effects of ARRA outlays, but the relationship continues to be negative.

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was about six percentage points more affected by job losses than the U.S. economy as a whole. In New York State and Texas, however, job losses in the construction sector were less than two percentage points higher than the drop in total employment in these states, while in California and Florida, they were over 11 percentage points higher than the state-specific average. Exploiting this variation between state-specific recessions allows us to investigate how the distribution of job losses in a downturn affects the distribution of the effects of state fiscal relief. Our empirical approach is to examine whether a particular sector tended to benefit more strongly from ARRA payments in those states where it had previously suffered more severely from the crisis. If this is the case, then it can be concluded that systematic forces ensured that specific sectors benefited strongly from the ARRA payments *because* they had been strongly affected by the crisis. Then, we may expect the double dividend of state fiscal relief to occur also in the Covid-19 crisis.

Technically, we consider an additional set of regressions where we interact ARRA payouts with the pre-ARRA drop in sector-specific employment relative to total employment. We define, for each sector i in each state s, a measure of the excess exposure to the downturn in 2007/08 as the percent employment change for sector i in state s between December 2007 and December 2008 minus the percent change in total employment in state s, and normalize this variable to have mean zero and variance one. Our baseline empirical model (1) is then augmented by the interaction between ARRA outlays and the excess-exposure measure, and the excess-exposure measure further enters the second stage as an additional control variable. Here, we use the three instruments from the baseline regressions as well as their respective interactions with the excess exposure measure as instruments (giving a total of six instruments) to instrument ARRA outlays and the interaction term.

Figure 3 illustrates the estimated coefficients on the interaction terms in the sector-specific regressions (the full regression results are documented in Table A.2 in the Appendix). In most sectors, the estimated coefficient on the interaction term is negative. This result implies that, in states where the respective sector was hit harder by the Great Recession, ARRA transfers to this state tended to have a more substantial effect on employment in this sector. The strong employment effects in the construction sector may thus partly result from this sector's significant exposure to the crisis, for example, because state governments deliberately decided to use the

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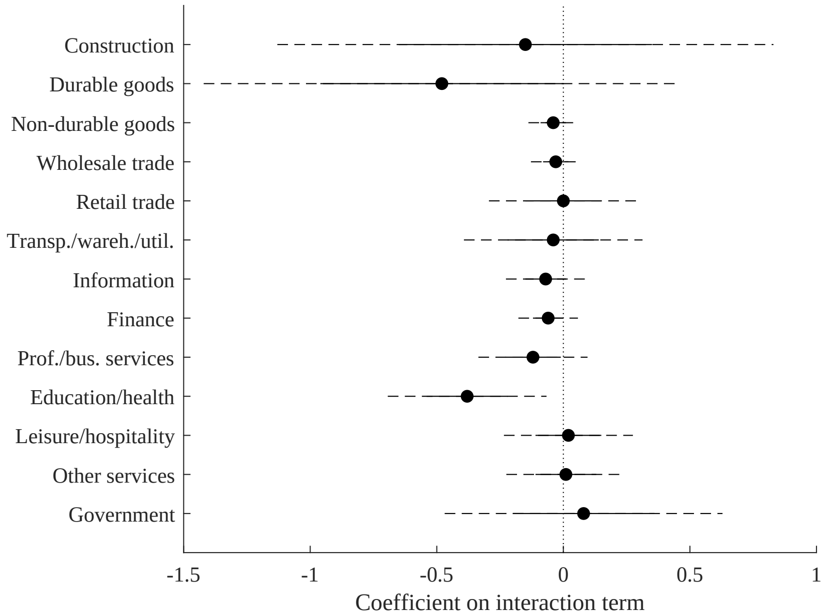
Figure 3: Estimated coefficients on the interaction between ARRA payouts and sector-specific excess exp



Notes: Estimated coefficients $\widehat{\xi}_i$ from sector-specific 2SLS regressions $\sum_{h=0}^{H}(Y_{s,i,t+h} - Y_{s,i,t}) = \alpha_i + \beta_i F_s + \xi_i F_s \widetilde{E}_{i,s} + \omega_i \widetilde{E}_{i,s} + \gamma_i' X_s + \varepsilon_{s,i}$. $\widetilde{E}_{i,s}$ is $(E_{i,s} - \text{mean}(E_{i,s}|i))/(\text{var}(E_{i,s}|i)^{1/2})$, where $E_{i,s} = (\text{Employment}_{s,i,t-1} - \text{Employment}_{s,i,t-13})/\text{Employment}_{s,i,t-13} - (\text{Employment}_{s,t-1} - \text{Employment}_{s,t-13})/\text{Employment}_{s,t-13}$. F_s and $F_s \widetilde{E}_{i,s}$ are instrumented as described in the main text. Dots: point estimates. Solid lines: point estimate plus/minus one standard deviation. Dashed lines: 95% confidence intervals. Transp./wareh./util. = transportation, warehousing, and utilities. Prof./bus. services = professional and business services.

ARRA payments to support their hardest-hit sectors. However, the estimates for the interaction terms are insignificant for most sectors. When calculating the estimated marginal effect of ARRA payouts from these regressions, we find that they are similar across the range of our excess-exposure measure. Using again the construction sector as an example, our estimates imply that, in states where the construction sector was hit only slightly more strongly than the economy as a whole, employment gains in construction due to ARRA payments were sizeable, too. This hints at the strong employment effects in the construction sector being mostly systematic and unrelated to the specifics of the Great Recession. For the sectors particularly relevant in the context of the Covid-19 crisis (leisure, hospitality, retail trade), we find that the interaction is insignificant and point estimates are close to zero. This indicates that these sectors would not profit more strongly from state fiscal relief in the Covid-19 recession, which has hit them substantially harder than did the Great Recession.

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An explanation for our findings is that state governments used additional ARRA payments to a large degree to extend construction-related spending or to alleviate cuts in this type of spending, largely irrespective of how job losses were distributed across sectors in their states. Leduc and Wilson (2017) document, for one component of the ARRA stimulus, that intergovernmental transfers were used mostly for infrastructure spending. This can be a consequence of, e.g., the relative easiness of cutting back on construction expenditures and these cuts being avoided due to the relief payments or intensive lobbying of firms in the construction sector (as documented by Leduc and Wilson, 2017). An alternative explanation for the distribution of the employment effects of ARRA could be that structural characteristics of the construction sector make employment in this sector distinctly responsive to demand changes. This explanation seems unlikely as the literature has identified several sources of strong sector-specific employment reactions to changes in government demand – an upstream position in the production network (Bouakez et al. 2020), low unionization (Nekarda and Ramey 2011), and large shares of pink-collar workers (Bredemeier et al., 2020a, 2020b) – none of which apply to the construction sector.

The finding that ARRA outlays did not have more substantial employment effects in a sector in states where this sector was hit harder by the recession implies that the double dividend of state fiscal relief cannot be taken for granted in other recessions. To help the industries that have been struck this time, such as retail trade, leisure, and hospitality, the funds would have to be used in a distinctly different way than during the Great Recession. For example, these sectors could be exempted from tax or fee increases, or the relief payments could be used for direct subsidies to the severely affected industries.

4 Conclusion

We have provided evidence of pronounced heterogeneity in the employment effects of the ARRA's state fiscal relief program during the Great Recession, with the construction sector being the main beneficiary. Our findings imply that intergovernmental transfers to states did not only protect jobs, but they also protected jobs in the industries hit hardest, thereby preventing further accelerations in the distributional costs of the crisis. We have argued that such a double dividend could be generated in the Covid-19 recession only if states use relief payments in distinctly different ways than they did during the Great Recession.

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Appendix

Table A.1 shows the full results of our baseline 2SLS regressions, which we use to estimate sector-specific job-year coefficients as displayed in the pie chart in Figure 1. Each column corresponds to a sector-specific regression.

Figures A.1 through A.3 show the results of robustness checks. In Figure A.1, we used the Medicaid relief instrument suggested by Chodorow-Reich et al. (2012) rather than the baseline combination of instruments. In Figure A.2, we controlled for sector-specific pre-ARRA employment trends. In Figure A.3, we accounted for the full set of control variables considered in Chodorow-Reich (2019)'s sensitivity analysis. In all specifications, we find that sectors hit harder by the recession were more strongly affected by state fiscal relief, as in our baseline specification.

Table A.2 shows the full results of the augmented sector-specific 2SLS regressions with interaction terms, given by

$$\sum_{h=0}^{H}(Y_{s,i,t+h}-Y_{s,i,t})=\alpha_i+\beta_i F_s+\xi_i F_s\widetilde{E}_{i,s}+\omega_i\widetilde{E}_{i,s}+\gamma_i' X_s+\varepsilon_{s,i},$$

where

$$\widetilde{E}_{i,s}=\frac{E_{i,s}-\text{mean}(E_{i,s}|i)}{\text{var}(E_{i,s}|i)^{1/2}}$$

with

$$E_{i,s}=\frac{\text{Employment}_{s,i,t-1}-\text{Employment}_{s,i,t-13}}{\text{Employment}_{s,i,t-13}}-\frac{\text{Employment}_{s,t-1}-\text{Employment}_{s,t-13}}{\text{Employment}_{s,t-13}}.$$

Each column of Table A.2 corresponds to a sector-specific regression.

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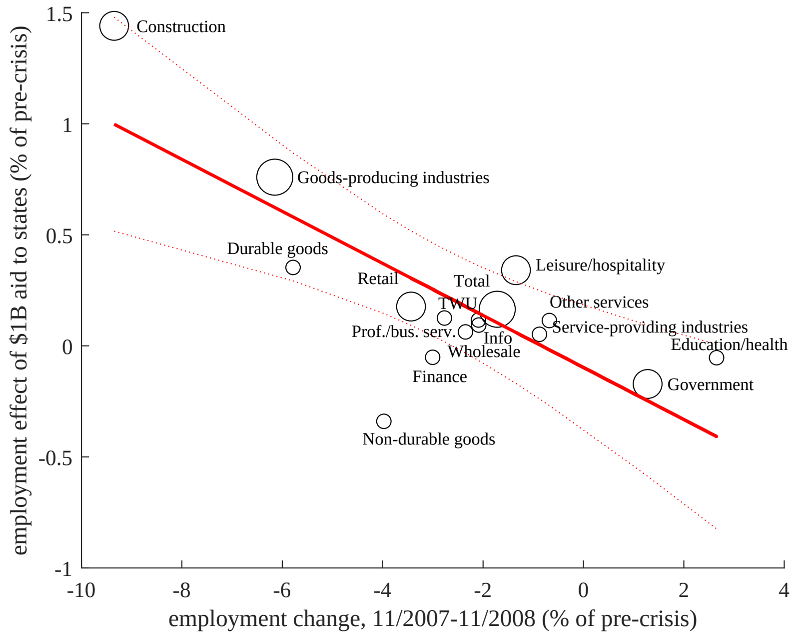
Table A.1: Regression results for sectoral employment changes, 12/2008 through 12/2010 (p-values in parentheses)

	Total non-farm	Mining/ logging	Construction	Dur.-goods manufctng.	Non-d.-goods manufctng.	Wholesale trade	Retail trade	Transport., wareh., util.
Total ARRA payouts	2.03	0.17	0.78	-0.01	-0.02	0.10	0.10	0.13
	(0.00)	(0.11)	(0.00)	(0.97)	(0.76)	(0.06)	(0.18)	(0.11)
Dec-08 state employment /population 16+	-3.74	-0.15	-1.57	-0.20	0.14	-0.07	-0.39	-0.11
	(0.12)	(0.86)	(0.13)	(0.72)	(0.44)	(0.71)	(0.25)	(0.63)
State employment change, Dec-07 to Dec-08	11.95	0.55	5.10	-1.02	-0.05	0.43	1.28	0.10
	(0.00)	(0.50)	(0.00)	(0.28)	(0.88)	(0.03)	(0.00)	(0.52)
GSP change, 2007Q4-2008Q4	2.27	2.37	2.33	-1.73	-0.44	-0.17	0.59	-0.06
	(0.47)	(0.07)	(0.01)	(0.04)	(0.03)	(0.45)	(0.24)	(0.80)
Constant	-6.59	-2.24	4.13	-5.95	-3.49	-2.36	0.67	-2.25
	(0.82)	(0.81)	(0.76)	(0.42)	(0.18)	(0.34)	(0.86)	(0.38)
First-stage F-statistic	46.09	44.16	45.75	44.67	45.32	46.09	46.09	46.09
Number of observations	50	47	48	48	48	50	50	50

	Infor-mation	Financial services	Prof./bus. services	Education/ health	Leisure/ hospitality	Other services	Govern-ment
Total ARRA payouts	0.07	0.03	0.40	0.02	0.16	0.10	0.03
	(0.23)	(0.69)	(0.08)	(0.90)	(0.16)	(0.05)	(0.83)
Dec-08 state employment /population 16+	0.01	-0.24	-0.60	-0.19	-0.67	0.19	0.02
	(0.95)	(0.18)	(0.11)	(0.40)	(0.07)	(0.10)	(0.95)
State employment change, Dec-07 to Dec-08	-0.37	0.49	1.10	0.74	2.03	0.40	1.69
	(0.02)	(0.06)	(0.03)	(0.02)	(0.00)	(0.01)	(0.00)
GSP change, 2007Q4-2008Q4	-0.33	-0.24	0.39	-0.33	1.05	0.11	-0.99
	(0.03)	(0.37)	(0.42)	(0.25)	(0.01)	(0.56)	(0.01)
Constant	-2.61	0.82	-1.55	5.95	5.13	-3.93	2.37
	(0.03)	(0.68)	(0.78)	(0.07)	(0.29)	(0.00)	(0.61)
First-stage F-statistic	39.80	46.09	46.09	46.09	46.09	46.09	46.09
Number of observations	48	50	50	50	50	50	50

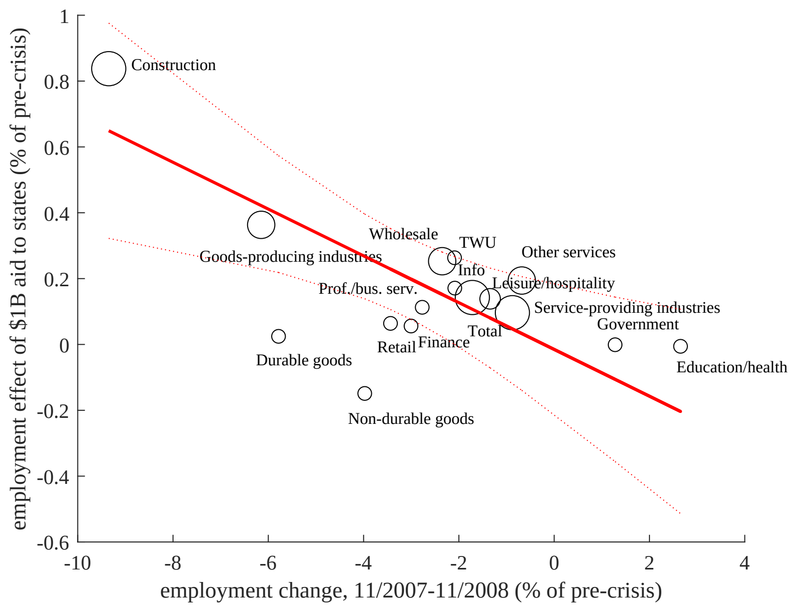
Notes: Dur.-goods manufctng. = Durable-goods manufacturing, Non-d.-goods manufctng = Non-durable-goods manufacturing. Transport., wareh., util. = Transportation, warehousing, and utilities. Prof./bus. services = Professional and business services. P-values are given in parentheses. First-stage F-statistic and number of observations differ across columns because, for a few sector-state combinations, the required monthly employment information is missing.

Figure A.1: Robustness check: Medicaid relief instrument



Notes: see Figure 2.

Figure A.2: Robustness check: controlling for sector-specific employment trends



Notes: see Figure 2.

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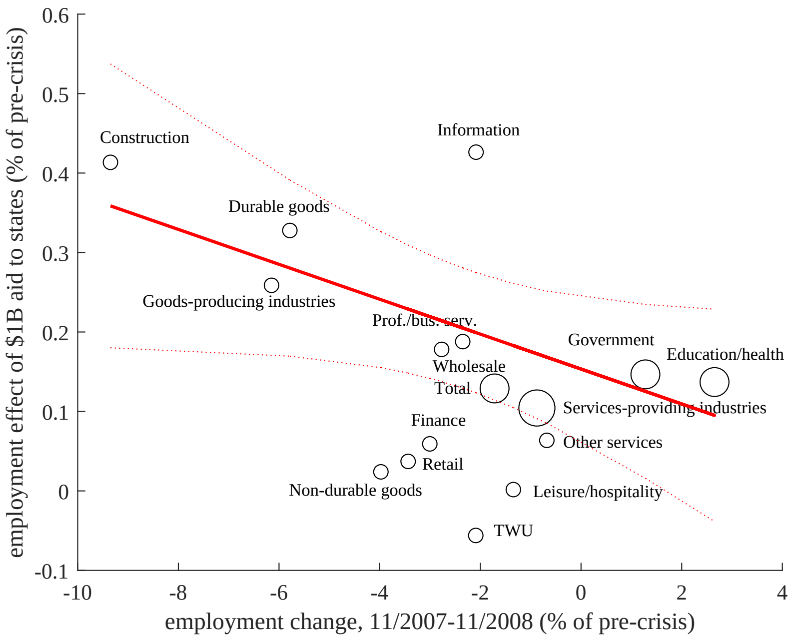
Figure A.3: Robustness check: including all control variables considered in Chodorow-Reich (2019)'s sensitivity analysis



Notes: see Figure 2.

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Table A.2: Regression results for sectoral employment changes, 12/2008 through 12/2010, interaction-term specification (p-values in parentheses)

	Construction	Dur.-goods manufctng.	Non-d.-goods manufctng.	Wholesale trade	Retail trade	Transport., wareh., util.	Infor-mation
Total ARRA payouts	0.88 (0.00)	0.03 (0.90)	-0.03 (0.76)	0.10 (0.09)	0.11 (0.18)	0.15 (0.08)	0.04 (0.35)
Total ARRA payouts × sector's excess exposure	-0.15 (0.77)	-0.48 (0.32)	-0.04 (0.45)	-0.03 (0.48)	0.00 (0.99)	-0.04 (0.81)	-0.07 (0.38)
Sector's excess exposure	2.83 (0.64)	7.09 (0.26)	0.72 (0.33)	0.52 (0.39)	-0.06 (0.97)	0.55 (0.81)	1.05 (0.33)
Dec-08 state employment+ /population 16+	-1.21 (0.39)	-0.53 (0.38)	0.07 (0.71)	-0.12 (0.54)	-0.38 (0.24)	-0.09 (0.69)	0.05 (0.62)
State employment change, Dec-07 to Dec-08	4.11 (0.17)	-1.01 (0.28)	0.01 (0.99)	0.46 (0.02)	1.27 (0.00)	0.09 (0.55)	-0.28 (0.02)
GSP change, 2007Q4-2008Q4	2.20 (0.03)	-1.92 (0.03)	-0.43 (0.04)	-0.18 (0.48)	0.61 (0.22)	-0.06 (0.80)	-0.25 (0.02)
Constant	-2.36 (0.90)	-2.58 (0.75)	-2.63 (0.37)	-1.84 (0.47)	0.48 (0.90)	-2.66 (0.28)	-2.62 (0.04)
Number of observations	48	48	48	50	50	50	48
	Financial services	Prof./bus. services	Education/ health	Leisure/ hospitality	Other services	Govern-ment	
Total ARRA payouts	0.01 (0.94)	0.32 (0.20)	-0.03 (0.81)	0.16 (0.09)	0.08 (0.19)	0.07 (0.79)	
Total ARRA payouts × sector's excess exposure	-0.06 (0.33)	-0.12 (0.26)	-0.38 (0.02)	0.02 (0.85)	0.01 (0.93)	0.08 (0.79)	
Sector's excess exposure	1.14 (0.13)	2.80 (0.09)	6.27 (0.00)	0.18 (0.91)	0.01 (1.00)	-1.04 (0.76)	
Dec-08 state employment /population 16+	-0.28 (0.07)	-0.48 (0.21)	-0.51 (0.02)	-0.54 (0.13)	0.17 (0.16)	0.04 (0.91)	
State employment change, Dec-07 to Dec-08	0.55 (0.02)	0.50 (0.29)	1.87 (0.00)	1.98 (0.00)	0.47 (0.00)	1.64 (0.02)	
GSP change, 2007Q4-2008Q4	-0.25 (0.37)	0.43 (0.35)	-0.21 (0.48)	0.91 (0.02)	0.14 (0.42)	-1.02 (0.01)	
Constant	1.64 (0.38)	-2.49 (0.65)	11.36 (0.00)	3.66 (0.43)	-3.47 (0.05)	1.65 (0.75)	
Number of observations	50	50	50	50	50	50	

Notes: Dur.-goods manufctng. = Durable-goods manufacturing, Non-d.-goods manufctng = Non-durable-goods manufacturing. Transport., wareh., util. = Transportation, warehousing, and utilities. Prof./bus. services = Professional and business services. P-values are given in parentheses. Number of observations differ across columns because, for a few sector-state combinations, the required monthly employment information is missing.

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Defaulting on Covid Debt: Foreign or Domestic?

Wojtek Paczos[1] and Kirill Shakhnov[2]

Date submitted: 25 July 2020; Date accepted: 6 August 2020

The COVID-19 pandemic causes sharp reductions in economic output and sharp increases in government expenditures. This increases the riskiness of sovereign borrowing both domestically and internationally. We propose a framework to study debt sustainability by introducing domestic debt into a sovereign default model, in which the government sets distortionary labour taxes and decides whether to repay its past domestic and foreign obligations. The results show, that foreign default is more likely after a negative productivity shock, while domestic default is more likely after a negative expenditure shock. Even in the case of a recently proposed broad restructuring of foreign debt, governments may still selectively default on their domestic debt obligations.

1 Lecturer in Economics at Cardiff University, and Assistant Professor in the Institute of Economics, Polish Academy of Sciences
2 Lecturer in Economics at University of Surrey

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1 Introduction

In response to the COVID-19 pandemic, governments around the globe imposed containment measures. These lockdowns reduce economic activities and increase budget deficits. Due to commitment problem emerging economies find it hard to issue debt to smooth the impact of the shock. In this paper we study the impact of output and government expenditure shocks on the sustainability of domestic and foreign debt.

The International Monetary Fund has projected that the world output will contract by 3.0% in 2020. At the same time, to mitigate economic costs of those measures governments provide economic rescue packages at an unprecedented scale. Figure 1 provides the expected scale of the real GDP growth and the primary deficit changes from 2019 (green stars) to 2020 (red circles). Relative to 2019, in 2020 emerging markets on aggregate are expected to lose 5.7 percentage points of GDP growth and to have a primary deficit larger by 4.3% of GDP. The new fiscal measures, which include additional spending and forgone revenue and loans, equity, and guarantees, account for 5.1% of GDP in emerging economies (*World Economic Outlook Update*, 2020). The arrows, which show the average changes for country groups, are all pointing towards southeast.

How to finance such unprecedented deficits and stimulate economies? Governments stockpiled debt in order to finance rescue and recovery packages. Elevated debt levels coupled with falling output and rising government expenditure bring back the question of debt sustainability. Economists and policymakers call for urgent measures to be taken. The G-20 countries have suspended interest rate payments on bilateral debts. Bolton, Buchheit, Gourinchas, Gulati, Hsieh, Panizza and Weder di Mauro (2020) argue for a broad "debt standstill" that will including private creditors and will be available to a large set of countries. The total stock of debt under consideration is $3 trillions.

The discussion focuses on external debt. Yet, the external debt accounts only for a third of the total public debt of emerging economies, while the remaining debt is owed to domestic investors. Defaults on domestic debt are not uncommon and are associated with economic disruptions comparable to those after foreign default - the fact considered as "forgotten history" in macroeconomics (Reinhart and Rogoff, 2011). Domestic and foreign debts are hardly similar. Foreign debt involves transferring resources into and out of an economy, which can help to achieve consumption smoothing over the business cycle. Domestic debt cannot achieve this, as its issuance and repayment occur within an economy: domestic

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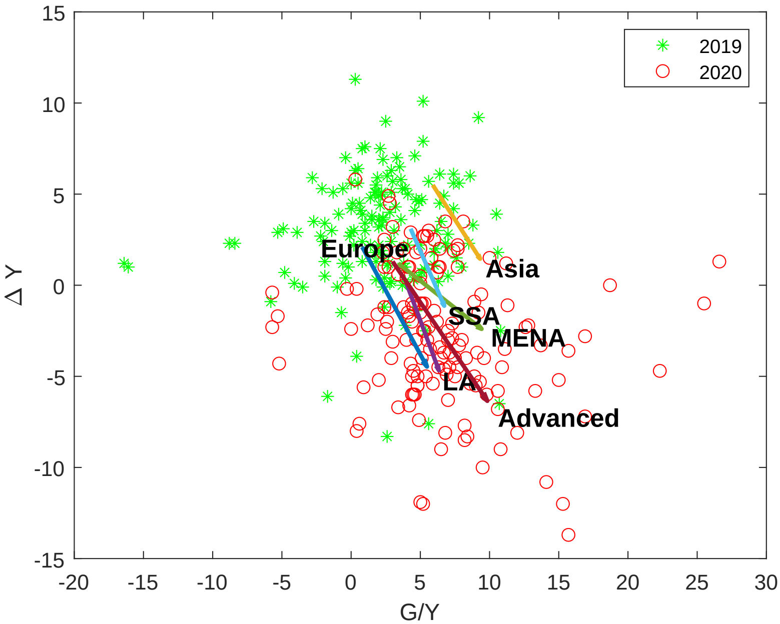

Figure 1: GDP growth and primary deficit-to-GDP in 2019 and 2020

Notes: This figure plots the real GDP growth against the primary deficit, which is defined as the difference between government expenditures and tax revenues in percentage of GDP, for 152 economies. Green stars represents the actual data for 2019 and red circles are IMF projections for 2020 as of June 2020. The arrows represents the changes in the averages for advanced economies and five emerging market regions: Emerging and Developing Asia; Emerging and Developing Europe; Latin America and the Caribbean; Middle East and Central Asia; Sub-Saharan Africa. The data are from *World Economic Outlook Update* (2020)

borrowing does not bring in additional resources.

We propose a model to analyze debt sustainability in a optimal taxation framework, where the economy is subject to two simultaneous shocks and the government has limited commitment. After observing shock realizations the government decides on distortionary labour tax and whether to default on either its foreign or domestic obligations (or both). If it defaults, the economy is subject to an output penalty in the form of a reduced productivity. Otherwise, it has to increase taxes to finance repayment. Thus, repayment imposes endogenous distortion on the economy. However, in the case of domestic debt, this distortion is partially mitigated by the fact, that resource flow back to domestic households. As a result, foreign default is more likely after a negative output shock and domestic default is more likely after a negative expenditure shock.

This paper argues that after a Covid shock; a simultaneous output drop and a government expenditure hike, even in case of a broad restructuring of foreign debt, governments might still choose to selectively default on their domestic debt obligations.

The model in this paper is in the tradition of the strategic sovereign default framework of Eaton and Gersovitz (1981). We contribute to the recent studies of the selective nature of sovereign defaults (Erce and Mallucci, 2018, Niepelt, 2016, Paczos and Shakhnov, 2016, Sunder-Plassmann, 2017). Although the dynamic debt accumulation is important in general, it is less relevant for the Covid shock, which is a rare disaster shock due to its size, global scale, and variety. We also contribute to the new Covid-macro literature; e.g. Arellano, Bai and Mihalache (2020) embed the epidemiological SIR model into the standard sovereign default model to analyze foreign default risk. We provide insights about the riskiness of domestic debt as well. This paper is also related to the literature on distortionary taxation with default (Karantounias, 2017, Pouzo and Presno, 2014), where a government defaults to mitigate endogenous tax distortions, albeit in a closed economy setting. Finally, since we solve static optimal labour taxation problem, our model can be viewed as an extension of the Basic Model in Piketty and Saez (2013), that allows for defaultable debt.

2 The Model

The model is a static open economy populated by a domestic representative household and a benevolent government. At the beginning of the period the government has two outstanding obligations: domestic debt b^d towards the domestic representative household and foreign debt b^f towards foreign investors. It takes two discrete decisions whether to default on each debt $d^d \in \{0,1\}, d^f \in \{0,1\}$. The government sets labour income tax at the marginal rate τ. We use the primary approach to find the Ramsey allocation, that is, we set up the government's problem in terms of allocations and, after having solved for the optimal allocations, the optimal tax rate is derived.

Households choose labour supply and consumption to maximize utility subject to bud-

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get constraint:

$$\max_{c,n} \quad u(c,n) \tag{1}$$

$$\text{s.t.: } c = wn(1-\tau) + (1-d^d)b^d + \pi, \tag{2}$$

where w is wage and π is a lump sum profit transfered from the representative firm owned by the household. Household's optimality condition reads:

$$u_n(c,n) = -u_c(c,n)w(1-\tau) \tag{3}$$

The optimality condition together with the budget constraint form the solution of the household's problem.

$$c = -\frac{u_n(c,n)}{u_c(c,n)}n + (1-d^d)b^d \tag{4}$$

The government will take the household's solution as a constraint to its maximization problem, hence we call equation (4) the Implementability Constraint (IC).

Firms produce using Cobb-Douglas production function with labour as the sole input and chose labour demand to maximize static profit:

$$\max_{n} \{\pi = f(n) - wn\} \tag{5}$$

$$\text{s.t.: } f(n) = \gamma A n^{\alpha}, \tag{6}$$

where A is the total factor productivity (TFP) shock and γ is the output cost in the case of a default:

$$\gamma = \begin{cases} \gamma^r & \text{if } d^d = 0 \text{ and } d^f = 0 \\ \gamma^d & \text{if } d^d = 1 \text{ and } d^f = 0 \\ \gamma^f & \text{if } d^d = 0 \text{ and } d^f = 1 \\ \gamma^d\gamma^f & \text{if } d^d = 1 \text{ and } d^f = 1, \end{cases} \tag{7}$$

where $\gamma^r = 1$ and $\gamma^d, \gamma^f < 1$. The solution to the firms' problem reads:

$$w = \alpha\gamma A n^{\alpha-1} \tag{8}$$

$$\pi = (1-\alpha)\gamma A n^{\alpha}. \tag{9}$$

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Optimal policy: the government acts as a Ramsey planner. The planner decides whether to default on foreign and domestic debts (d^f, d^d) to maximize households utility. The planner must respect implementability constraint (IC) (4) and the resource constraint (RC). The economy is subject to two shocks: the aggregate TFP shock A and the government spending shock g, which reduces resources available for consumption and via the RC constraint:

$$\max_{d^d, d^f} \quad u(c,n) \tag{10}$$

$$\text{s.t.: } \gamma A n^{\alpha} = g + c + (1-d^f)b^f \quad \text{(RC)} \tag{11}$$

$$c = -\frac{u_n(c,n)}{u_c(c,n)} n + (1-d^d)b^d \quad \text{(IC)} \tag{12}$$

In the primal approach, the households and firm first-order conditions are used to eliminate prices and tax rates, and the problem of determining optimal policy reduces to a simple programming problem in which the choice variables are the allocations.

2.1 Functional forms and calibration

We assume the non-separable preferences, which is the standard form in the optimal taxation literature Conesa, Kitao and Krueger (2009):

$$u(c,n) = \frac{\left[(c)^{\eta}(1-n)^{(1-\eta)}\right]^{1-\sigma}}{1-\sigma} \tag{13}$$

The IC constraints reads:

$$c\left[1 - \frac{1-\eta}{\eta}\frac{n}{1-n}\right] = (1-\delta^d)b^d + (1-\alpha)\gamma A n^{\alpha} \quad \text{(IC)} \tag{14}$$

For each $d^f = \{0,1\}$ and $d^d = \{0,1\}$ equations (11) and (14) completely characterize the solution. Foreign default directly relaxes the Resource Constraint, and the domestic default directly tightens Implementability Constraint. A negative TFP shock tightens both constraints: Resource Constraint via reduced resources and Implementability Constraint through wages via labour-leisure margin (3). A negative government spending shock g directly affects only Resource Constraint through reduction in the available resources for consumption.

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Table 1: Parameter values

	Parameter	Value
σ	Risk aversion	4
η	Consumption weight in utility	0.377
α	Labour share in production	0.64
b^f	Foreign debt	0.0205
b^d	Domestic debt	0.0205
γ^f	Foreign default penalty	0.94
γ^d	Domestic default penalty	0.99

The problem does not admit an analytical solution, so we solve numerically imposing plausible parameter values. Table 1 summarizes parametrization choices. We assume standard values in the optimal taxation literature for risk aversion $\sigma = 4$, consumption weight in utility $\eta = 0.377$ and labour share $\alpha = 0.64$. The model is static and all past debt is due in the single period. We calibrate foreign debt level to reflect the amount of the foreign debt maturing in 2020: Bolton et al. (2020) estimate that a debt standstill would free up 4.7% of emerging economies annual income. We set the target for b^f at 5% of y^*, where y^* is the reference output produced in repayment with $A = 1$ and $g = 0$, which gives the value of $b^f = 0.0205$. For the clarity of exposition of the mechanism in the model we assume that domestic debt is equal to its foreign counterpart. With this set of parameters the reference output is equal to $y^* = 0.418$. We plot the solution in the state space (A, g) setting the range for A between 0.85 and 1.15 and for g between 0 and 10% of y^*. The two remaining parameters are the productivity losses upon domestic and foreign default (γ^d and γ^f). These parameters govern how much debt is sustainable in the repayment equilibrium. Or equivalently, how big shocks are necessary to trigger defaults. As discussed previously, in the model economy it is easier to sustain domestic debt. We calibrate the parameters such that domestic and foreign defaults occur within the chosen state-space implying $\gamma^d = 0.99$ and $\gamma^f = 0.94$.

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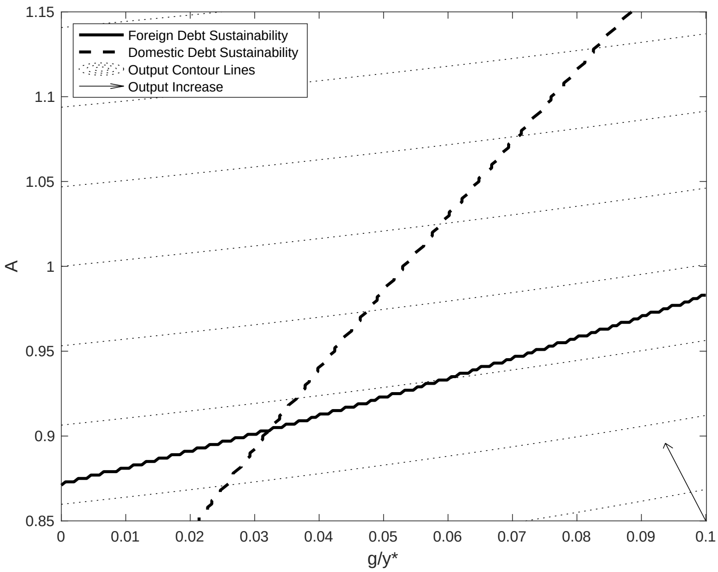

Figure 2: Domestic and foreign debt sustainability

Source: Authors' calculations.

2.2 Results

Figure 2 plots the solution of the model. The graph is the model equivalent of Figure 1 which motivated this paper. The axes represent the two exogenous states, with the TFP level (A) on the vertical axis and the government spending (g) on the vertical axis, scaled by the reference output (y^*). The dotted lines represent output contours: equal output levels (in repayment equilibrium) in the (A, g) state space. Contours are plotted at intervals representing 5% of the reference output. Output moves more than proportionally with the TFP level: a 5% increase in A increases output by more than 5%, because as A increases the labour supply increases as well. Output contours are upward sloping in the government expenditures g, which means that output is falling with g. This is because financing higher g requires higher taxes, which cause increased distortions and a decreasing labour supply.

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The solution is characterized by the two lines: domestic and foreign debt sustainability lines, which can also be referred to as default schedules. Foreign debt sustainability line (solid) is almost parallel to the output contours, while the domestic debt sustainability line (dashed) steeply crosses output contours. Let us study the mechanics of the model starting from the reference situation in which $A = 1$ and $g = 0$. A sudden and large drop in A would cause the government to declare a default on its foreign debt. A sudden and large increase in g on the other hand would cause the government to declare a default on its domestic debt. Both shocks reduce economy's output.

The sustainability lines are not perfectly parallel / perpendicular to output contours: they are upward sloping in g, but the domestic debt sustainability line is markedly steeper. The consequence is the following. With a high TFP level (e.g. $A = 1.05$) a relatively smaller shock in g is necessary to trigger a domestic default, while a relatively larger shock in g is necessary to trigger a foreign default. Whith a low TFP (e.g. $A = 0.95$) the situation is reverse: a relatively smaller shock in g is required to trigger domestic default, while a relatively larger shock in g is necessary to trigger a foreign default. With a Covid shock, when both A falls and g increases the economy risks a total default - moving to the bottom-right part of the graph.

The positions of the sustainability lines are governed by the parameters of the model. A higher default penalty (lower γ^i) shifts its respective debt sustainability line downwards: more debt can be sustained in repayment equilibrium (or, equivalently, larger shocks are necessary to trigger a default). A higher foreign debt level has two effects that reinforce each other. It reduces consumption because more resources are being transferred out of the economy. It also reduces consumption because higher taxes are necessary to finance repayment and higher taxes increase distortions and reduce output. Reduced consumption makes repayment option less attractive - on both markets. Both sustainability lines shift up. A higher domestic debt level brings about this second effect only and also shifts both lines up. The remaining parameters (σ, η, α) do not affect the mechanics of the model.

Although the exact positions of the lines are dependent on parameters, the economic mechanism is governed by their shapes (their different slopes). This is endogenous in the model and is robust to parameter changes. The intuition is the following. A default has two effects: it reduces tax burden - a positive *tax effect*, and it reduces productivity via the default penalty - a negative *productivity effect*. For a given A the productivity effect is the

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same across g realizations. On the other hand, for a given A the tax effect increases with g - more tax is needed to finance increasing government expenditure. Thus, when g increases, labour supply (and so output and consumption) decrease.

Most importantly, for any given A, the slope of the labour supply and consumption in g is different across regimes. This is depicted in the left panel of Figure 3. The chosen level of TFP in this figure is $A = 0.95$. At this TFP level all: repayment, foreign default and domestic default occur in equilibrium at different levels of g. Labour supply decreases at the fastest pace in repayment and at the slowest pace in domestic default. This is driven by the shape of the IC constraint (14). Along the IC constraint consumption c is convex in labour supply n in the neighbourhood of the solution. Domestic default reduces the right-hand-side of the IC constraint: representative household loses her savings, and needs to work more to sustain the same level of consumption. The equilibrium is achieved on the steeper part of the IC constraint. Thus, when a government expenditure shock g reduces resources (via the RC constraint) a drop in labour supply is relatively small in domestic default (because IC is the steepest), larger in foreign default and the largest in repayment (where IC is the flattest).

Labour supply decreases with g, which increases utility directly, but also reduces resources and disposable consumption, which decreases utility indirectly. The second effect dominates: utility decreases in g. As is the case for consumption, utility decreases at the fastest pace in repayment and at the slowest pace in domestic default. For $A = 0.95$ and $g = 0$ utility is the highest in repayment. The gap between the utility in repayment and the utility in domestic default narrows down quickly as visible in the right panel of Figure 3. As a result, for high levels of g domestic default is the preferred option and the domestic debt sustainability line is almost vertical in Figure 1. On the other hand, the gap between the utility in repayment and the utility in foreign default narrows down slowly in g. As a result, the preferred option depends more on the relative position of the lines in the origin: for high levels of A repayment is the preferred option, and for low levels of A foreign default is the preferred option throughout the range of g. As a result, the foreign debt sustainability line is almost horizontal in Figure 1.

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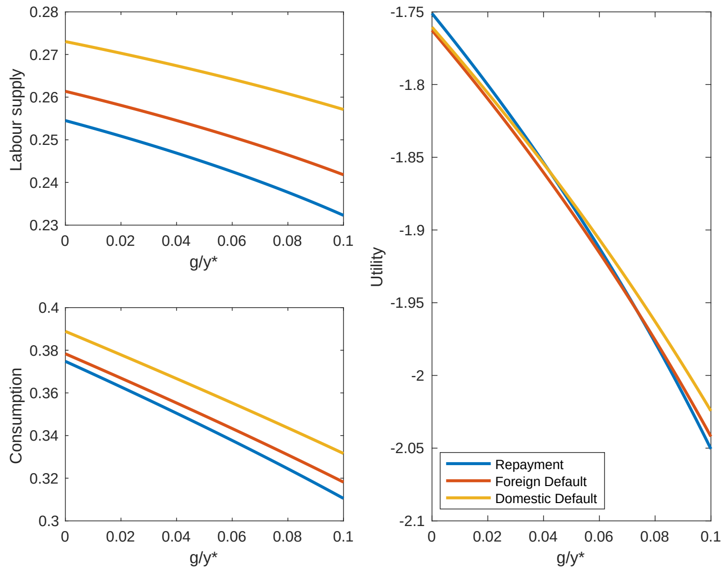

Figure 3: Labour supply, consumption and utility for $A = 0.95$.

Source: Authors' calculations.

3 Conclusion

This paper illustrates the mechanism that drives strategic foreign and domestic default in the presence of two shocks. Foreign default is more likely after a negative productivity shock and domestic default is more likely after a negative government expenditure shock. A Covid shock, which reduces output and increases government expenditure brings the economy closer to a total default. Even in the case of well designed foreign debt restructuring and "standstill" programmes, we can still expect a wave of domestic defaults.

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