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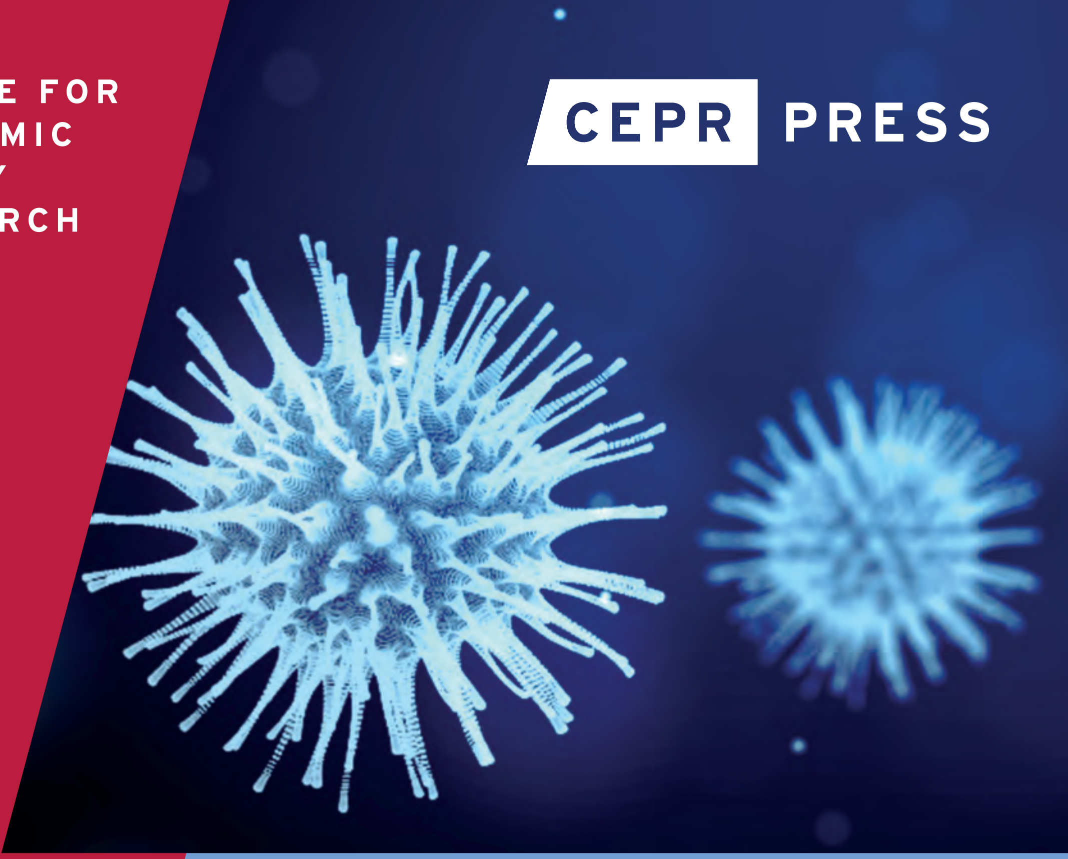

COVID ECONOMICS

VETTED AND REAL-TIME PAPERS

ISSUE 72
18 MARCH 2021

Covid Economics
Vetted and Real-Time Papers

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

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

Submission to professional journals

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

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

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

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The value of countercyclical capital requirements: Evidence from COVID-19[1]

Naz Koont[2] and Stefan Walz[3]

Date submitted: 16 March 2021; Date accepted: 16 March 2021

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We evaluate the implications of relaxing the Supplementary Leverage Ratio during the COVID-19 market disruption for bank balance sheet composition and credit provision. To the best of our knowledge, we are the first to causally identify the effect of the SLR regulation change on bank level outcomes. We find that the relaxation may have eased Treasury market liquidity by allowing banks to hold modestly greater inventories of Treasuries, and further allowed for a significant expansion of traditional bank credit. Our findings suggest that this risk-invariant leverage ratio was binding for banks during COVID-19, weakly affected bank liquidity provision in Treasury markets, and strongly affected banks' portfolio composition across asset classes, amounting to a shift of banks' loan supply schedules. Thus, we highlight that countercyclical relaxation of uniform leverage constraints can increase bank credit provision during economic downturns. Given the binding nature of the SLR, the relaxation of this constraint may be more effective than other countercyclical measures in allowing banks to extend credit.

1 We thank Olivier Darmouni, Yiming Ma, and Kairong Xiao for helpful comments.
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I. Introduction

Bank capitalization has been a key area of regulatory focus following the Global Financial Crisis of 2008, as policy makers aim to facilitate bank lending while limiting excessive risk taking. In this paper, we provide the first evidence that the countercyclical relaxation of bank capital requirements during the COVID-19 crisis allowed banks to extend new credit to the economy. We do so by examining the effects of the Federal Reserve's decision on April 1, 2020 to relax the Supplementary Leverage Ratio (SLR), a key pillar in the U.S. implementation of the Basel III regulation. By exploiting plausibly exogenous ex-ante variation in exposure to SLR relaxation across U.S. banks, we identify a causal effect of relaxing this uniform leverage constraint on bank balance sheet composition. Further, we strip out demand effects and isolate an increase in credit supply by comparing loan origination outcomes for banks which lend to a given borrower.

This setting is of particular interest for two reasons. First, the COVID-19 market disruption represents the first major stress test for the banking sector following the implementation of new regulations in the aftermath of the 2008 Global Financial Crisis. The banking sector absorbed an unprecedented inflow of reserves during the onset of the COVID-19 crisis, which contribute towards their leverage requirement under the Basel III regulations. This shock allows for an examination of the value of countercyclical leverage requirements within a new regulatory framework in which banks maintain greater capitalization, and we find that the relaxation is successful in expanding bank credit supply to the economy. While earlier work has focused on analyzing the effect of SLR implementation during a period of relative calm, we are the first to study the effect of this new regulation during a downturn. Further, we show that these policies are effective even when an adverse shock originates outside of the banking sector, as was the case during the COVID-19 health crisis.

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Secondly, we are able to examine the specific nature of the SLR on bank balance sheets. Implemented in 2018, the SLR is widely considered to be the binding constraint for many banks, and is distinct due to the risk-invariant contribution of bank assets towards the leverage constraint. As highlighted in earlier literature, this uniform leverage constraint may impede banks' intermediation in securities markets, including the market for U.S. Treasuries (Duffie (2020); He et al. (2020)). These papers document that the turmoil in Treasury markets during the onset of the COVID-19 shock can be attributed in part to the pressures on dealer balance sheets. Further, the risk-invariant nature of this constraint may affect banks' asset allocation across markets, inducing migration from low-return intermediation activities to those with higher yields (Choi et al. (2020); Allahrakha et al. (2018)). Through our empirical framework, we are able to evaluate these hypotheses within the setting of the COVID-19 market disruption. In line with existing work, we find some evidence that relaxing the SLR did allow for banks to hold more Treasuries on their balance sheets to improve liquidity in this market. Further, we show that banks do adjust their portfolio composition in response to a binding uniform leverage constraint. However, we demonstrate that in economic downturns, this effect on asset composition can serve to provide much needed credit to the real economy.

Consistent with the hypothesis that the SLR binds, we find that banks that were relatively more exposed to SLR relaxation increased their holding of Treasury securities as intended, and additionally substantially increased the size of their loan books net of credit line withdrawals. In order to control for borrower demand, we further analyze credit provision among banks lending to the same firm, and confirm that banks which benefited relatively more from SLR relaxation were able to subsequently extend greater credit to a given borrower. Thus, we highlight that countercyclical relaxation of uniform leverage constraints within the post-crisis regulatory framework can increase bank credit provision during economic downturns. Given the binding nature of the SLR, relaxation of this

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constraint may be more effective than other countercyclical measures in allowing banks to extend credit. As of 2021Q1, U.S. banks have been pressuring the Federal Reserve[1] to extend the relaxation of the SLR constraint past its initial end date of March 31st, 2021. Our analysis suggests that policy makers should consider the implications both for securities market intermediation and bank credit provision when determining the length of this countercyclical relaxation.

Our findings complement earlier work on bank liquidity provision during the COVID-19 crisis, which emphasized the importance of bank credit line drawdowns (Acharya and Steffen (2020); Greenwald et al. (2020); Li et al. (2020); Chodorow-Reich et al. (2020)). We highlight the interaction of bank credit supply with regulatory constraints by demonstrating that banks which enjoyed greater regulatory relief from a binding SLR constraint were able to lend new credit distinct from these drawdowns. Our findings are robust to controlling for pre-existing credit line exposure, and to netting out undrawn credit from our lending measures. Thus, we provide evidence that countercyclical capital buffers can mitigate the crowding out effect of credit line drawdowns on new lending, and increase credit provision during downturns. More generally, we contribute to the large literature on financial markets' reaction during the COVID-19 disruption[2]

Further, our paper borrows from the methodology of earlier papers that have analyzed the effects of various policies on the banking system. Firstly, our work provides a new empirical application of past literature that evaluates the effect of changing capital requirements to stimulate bank lending (Drehmann and Gambacorta (2012)). In line with both Rodnyansky and Darmouni (2017) who look at the effects of QE on bank lending following the Global Financial Crisis, and Sundaresan and Xiao (2018) who examine the

[1] Financial Times article, `https://on.ft.com/2Z69DCh`. Accessed February 28th, 2021.

[2] In particular, Haddad et al. (2020) show that the passage of the SLR relaxation did not have a significant effect during the COVID-19 bond market disruption, while in contrast we show that there was an effect on loan provision, which is in line with loan origination being the traditional business of banks.

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consequences of liquidity regulation in Basel III, we use ex-ante heterogeneity in exposure to the SLR relaxation in order to identify a causal effect on bank balance sheets. Additionally, following Khwaja and Mian (2008) and Rodnyansky and Darmouni (2017), we look at loan-level credit provision outcomes and control for borrower demand by exploiting variation in exposure to SLR relaxation among banks that lend to the same firm.

The paper is organized as follows. Section 2 reviews the Supplementary Leverage Ratio rule and places it in the context of the market turmoil in 2020. Section 3 reviews our data and empirical methodology. Section 4 presents formal empirical evidence regarding the effect of relaxing the SLR. Section 5 concludes.

II. SLR Implementation and Relaxation

A. SLR Implementation

The Supplementary Leverage Ratio (SLR) is a key pillar in the U.S. implementation of the Basel III regulation, and imposes a 3% risk-unadjusted minimum leverage ratio for U.S. banks with assets greater than $250 billion or with foreign exposures larger than $10 billion[3]. Following its implementation on January 1st, 2018, the SLR has been seen as a key constraint which restricts banks' intermediation activities, in contrast to other risk-weighted capital ratios which tend not to bind (Choi et al. (2020)). The SLR is distinct both in the comprehensive nature of assets which contribute towards banks' total exposures, and in the uniform contribution of these assets towards the leverage constraint. This uniform leverage constraint was meant to address potential misspecification of risk weights in the existing regulatory framework. Formally, the SLR is defined as:

[3]In addition to the SLR, the Federal Reserve enacted the enhanced Supplementary Coverage Ratio (eSLR) to impose stricter requirements on the largest and most systemically important banks and bring their minimum leverage ratio up to 5%, and must maintain a 6% ratio to be considered "well-capitalized".

$$SLR_{it} = \frac{\text{Tier 1 Capital}_{it}}{\text{Total Exposures}_{it}}$$

where total exposures include reported on- and off-balance sheet exposures. Importantly, quasi risk-free transactions involving secured borrowing and and secured lending backed by US Treasuries are included in the denominator. This has lead many academics (Duffie and Krishnamurthy (2016)) and market participants (Ruane (2015)) to suggest that simple leverage ratios introduce a cost of balance sheet space that limit banks' ability to provide liquidity. Relatedly, the uniform contribution of assets raises the potential for banks to engage in risk shifting in response to this binding constraint.

B. COVID-19 Turmoil and SLR Relaxation

The COVID-19 turmoil during the first quarter of 2020 was remarkable along multiple dimensions. There were massive disruptions in historically stable U.S. Treasury markets, enormous demand for bank liquidity via drawdowns of pre-existing credit lines by firms, large inflows of reserves into the banking system, and swift and unprecedented intervention by the Federal Reserve in order to provide liquidity and restore confidence. In particular, during February of 2020 yields on 10 year U.S. Treasuries fell from 1.5% to 1.16%, prime money market funds exhibited significant outflows, and liquidity in U.S. Treasury futures deteriorated substantially. This market turmoil continued throughout most of March despite the response of the Federal Reserve on March 14th, 2020 to lower interest rates by 100 basis points and announce large scale asset purchases and other stimulus measures. Concurrently, during March banks faced immense demands for liquidity due to the corporate sector's drawdown of preexisting credit commitments, as documented in Li et al. (2020).

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By the end of March, the liquidity demands from pre-existing credit lines had largely been absorbed onto bank balance sheets, and U.S. financial market conditions had also substantially improved in response to sustained asset purchases from the Federal Reserve. However, due to the large inflow of reserves, concerns remained regarding banks' abilities to adequately intermediate securities markets and continue to provide credit to the economy in light of their closeness to regulatory leverage constraints.

On April 1st, 2020 the Federal Reserve announced that bank holding companies could temporarily exempt U.S. Treasuries and reserves from the calculation of the SLR[4]. As justification for this relaxation, the Federal Reserve explicitly cites the potential for a binding SLR constraint to impede bank intermediation in securities markets as well as the ability of banks to extend credit: "the temporary increase in leverage exposure capacity should have countercyclical benefits as it supports financial market liquidity and increases these banking organizations' lending capacities in a time of unprecedented economic distress". Furthermore, the relaxation of the SLR during the COVID turmoil fits into the broader view that regulatory relief can be an effective tool in times of stress (Bank for International Settlements (2010)).

While the exclusion of U.S. Treasuries and reserves from banks' SLR calculation was intended to end after the second quarter of 2021, some U.S. banks are actively pushing to make this exemption permanent[5]. Jenn Piepszak, CFO at JPMorgan Chase, stated that if SLR relief is cut off at the end of March 2021, the bank might "simply shy away from taking new deposits, redirecting them elsewhere in the system... [or] retain additional capital and pass on some of their costs". In contrast, Bank of America CEO Brian Moynihan downplayed the significance of the potential re-imposition of the original SLR, claiming that the regulatory concession turned out to be "something we didn't need". The Federal

[4]Federal Reserve Press Release on April 1st, 2020, https://www.federalreserve.gov/newsevents/pressreleases/bcreg20200401a.htm

[5]Financial Times article, https://on.ft.com/2Z69DCh. Accessed February 28th, 2021.

Reserve's vice-chair for supervision in October 2020 reaffirmed that the SLR chans indeed temporary and that there is no current discussion around making it permanent. Throughout the rest of the paper, we contribute to the discussion over the potential extension of the SLR relaxation by providing an estimate of how bank lending might respond to the re-imposition of the original SLR calculation.

III. Data and Empirical Specification

A. Data and stylized facts

We use data on the balance sheet of US bank holdings companies (BHCs) as reported on the FR Y-9C regulatory forms. While we are primarily interested in the effect of loosening the Supplementary Leverage Ratio (SLR), our sample includes all BHCs, not just those that are subject to the leverage requirement. We aggregate reserves held at the Federal Reserve from the depository institution level up to the BHC level. We restrict our analysis to quarterly balance sheet data from 2016Q1 - 2020Q3. Table 1 lists the summary statistics for those banks with non-missing Treasury and reserves holdings holdings.

Table 1 Summary Statistics: Bank Balance Sheets

Statistic	N	Mean	Pctl(25)	Median	Pctl(75)
log(Assets)	5,873	15.810	14.888	15.501	16.432
Equity / Assets	5,873	0.117	0.097	0.110	0.129
Loans / Assets	5,873	0.667	0.625	0.705	0.768
Treasuries / Securities	5,873	0.050	0	0	0.03
(Treasuries + Reserves) / Assets	5,839	0.040	0.006	0.020	0.052
C & I Credit Line / Assets	5,873	0.063	0.031	0.052	0.086
Deposits / Assets	5,026	0.871	0.842	0.900	0.942

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Figure 1 shows the time series of the SLR for the 6 large US banks: Bank of America, Citigroup, Goldman Sachs, JPMorgan Chase, Morgan Stanley, and Wells Fargo. These large BHCs must maintain a minimum SLR of 6% in order to be "well-capitalized" according to the eSLR implementation by US regulators. The solid lines represent the actual reported SLR, which incorporates the exclusion of Treasuries and central bank reserves from the calculation. The dotted lines represent the counterfactual SLR without the exclusion. Note that for several banks the SLR approached 6 percent before the regulation change, but reported SLR figures substantially jumped starting in 2020Q2.

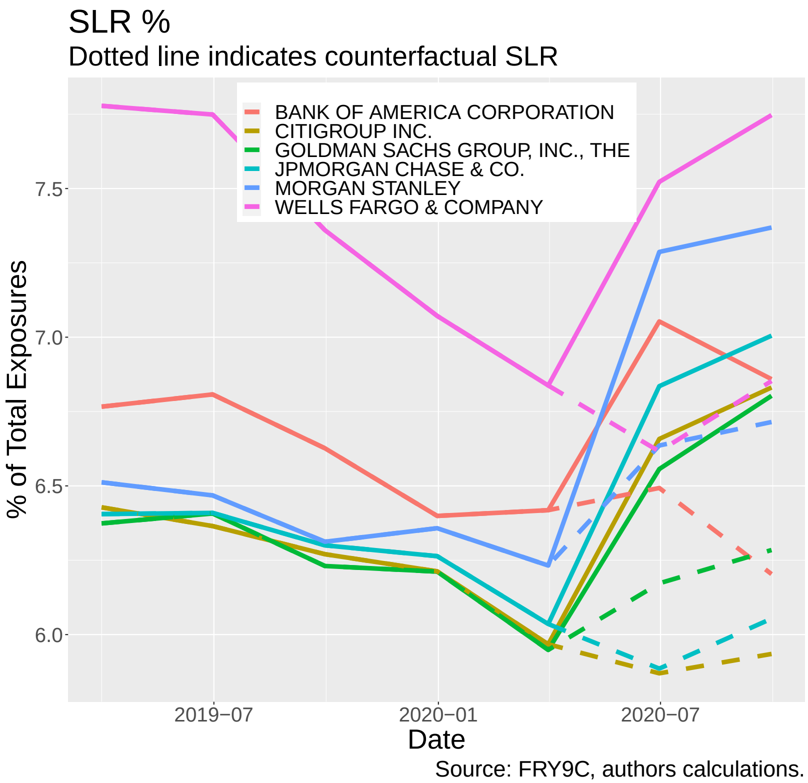

Figure 1. SLR Jumps in 2020Q2.

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B. Empirical Methodology

In this paper we are interested in the causal impact of relaxing the SLR regulation on bank asset composition. We exploit cross-sectional variation in exposure to the SLR relief granted by the Federal Reserve in a difference-in-difference framework. Banks that held relatively more Treasuries and reserves are more affected by the action of the Federal Reserve to exempt these assets from the SLR, and thus face greater regulatory relief on their capital ratios from the policy intervention. To be concrete, consider two banks A and B that both have large loan books and hold marketable securities to protect against liquidity risks. However, bank A generally holds Agency MBS and large non-reserve cash amounts, while bank B generally holds Treasuries and carries a large balance of excess reserves. Both securities portfolios are liquid and enjoy government guarantees to some degree, and while both cash portfolios gain earn potentially different in short term returns, they are otherwise quite similar. However, the SLR change provides differentially more relief to bank B.

We exploit this variation to identify the effect of a countercyclical relaxation of SLR on bank balance sheet composition and credit provision. Our hypothesis indicates that banks with a greater amount of relief will increase their loan provision by more than the amount provided by less treated banks. Additionally, we address the ongoing debate surrounding the effect of the Supplementary Leverage Ratio on broker intermediation in Treasury markets. More specifically, our setting allows us to test whether banks would otherwise conduct more Treasury intermediation without the cost of balance sheet space imposed by the SLR. In our baseline specification, we capture the degree of regulatory relief by measuring the balance sheet intensity of Treasuries and reserves as of 2019Q4, well before the onset of the COVID-19 market turmoil in the United States. We will show two different measures to capture ex-ante exposure to the regulatory relief. First, we

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consider a more narrow measure which captures variation in the relative of importance of Treasuries in a bank's securities portfolio.

$$Z_{1,i} = \frac{\text{Treasuries}_{i,t=2019Q4}}{\text{Total Securities}_{i,t=2019Q4}}$$

Second, we consider a broader measure that captures the total balance sheet intensity of the exempted items from the SLR calculation.

$$Z_{2,i} = \frac{\text{Reserves}_{i,t=2019Q4} + \text{Treasuries}_{i,t=2019Q4}}{\text{Total Assets}_{i,t=2019Q4}}$$

Figure 2 shows the time series of the two instruments for the largest banks. We believe there are advantages and disadvantages to both measures. The first instrument Z_1 has the advantage of capturing a clean variation in exposure: the ex-ante decision of a bank to hold Treasuries versus other similarly risk-free securities is likely made for idiosyncratic reasons related to historical market making specialization and unrelated to the composition of the bank's loan book. However, Z_1 has the disadvantage that it does not capture the relative prevalence of central bank reserves which have been a central part of the discussion regarding the SLR calculation. The second instrument Z_2 incorporates central bank reserves but may be correlated with other bank characteristics. We attempt to assuage these concerns by including time varying bank controls. Ultimately the results are qualitatively similar across the two measures.

We hypothesize that variation in the balance sheet intensity of Treasuries and reserves captures the extent to which balance sheet costs were reduced as a result of the change in the SLR calculation. Our identifying assumption is that banks' holdings of Treasuries and reserves before the onset of the COVID-19 turbulence is unrelated with other unobservable characteristics that correlate with bank asset composition and lending following the SLR

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relaxation. In other words, we assume and test for parallel trends across banks with heterogeneous Treasury and reserve holdings.

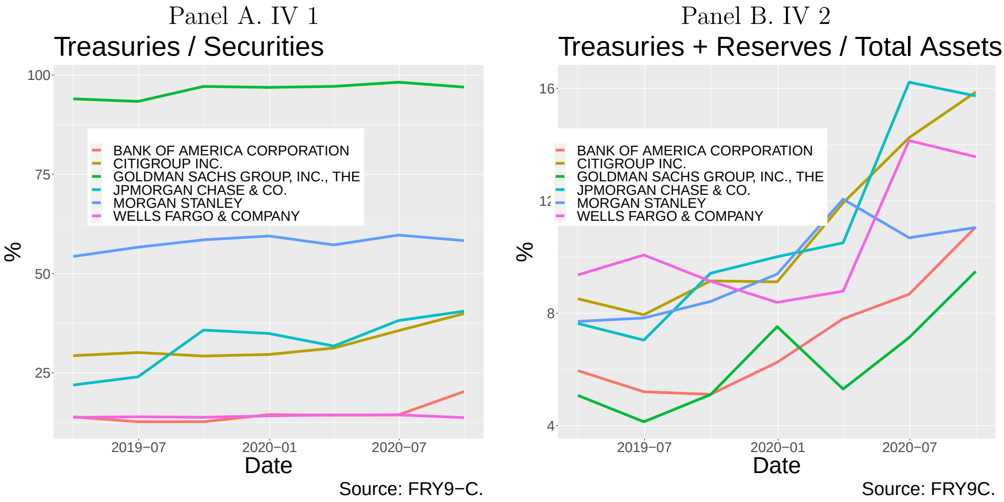

Figure 2. Time series of exposure measures.

In order to ensure our results are not driven by the large credit line drawdowns that have already been identified in the literature, we strip out their impact by calculating the change in outstanding unused Commercial and Industrial commitments, and add the difference back into the stock loans.

$$\text{Net Loans}_{it} = \text{Loans}_{it} + \Delta(\text{Unused C and I Commitments}_{it})$$

For instance, there were large drawdowns from 2020Q1 to 2020Q2, so our adjusted loan amount would be adjusted downward due to the fact that the amount of outstanding credit lines decreased dramatically. Figure 3 shows the distribution of the relationship between our exposure measures for regulatory relief and subsequent loan growth across the 13 large US banks. The x-axis captures the treatment variable, while the y-axis captures the loan growth from 2019Q4 to 2020Q3, stripping out the effect of C&I credit line drawdowns. This plot provides suggestive evidence that relaxing the SLR constraint indeed afforded banks the ability to extend greater loan financing.

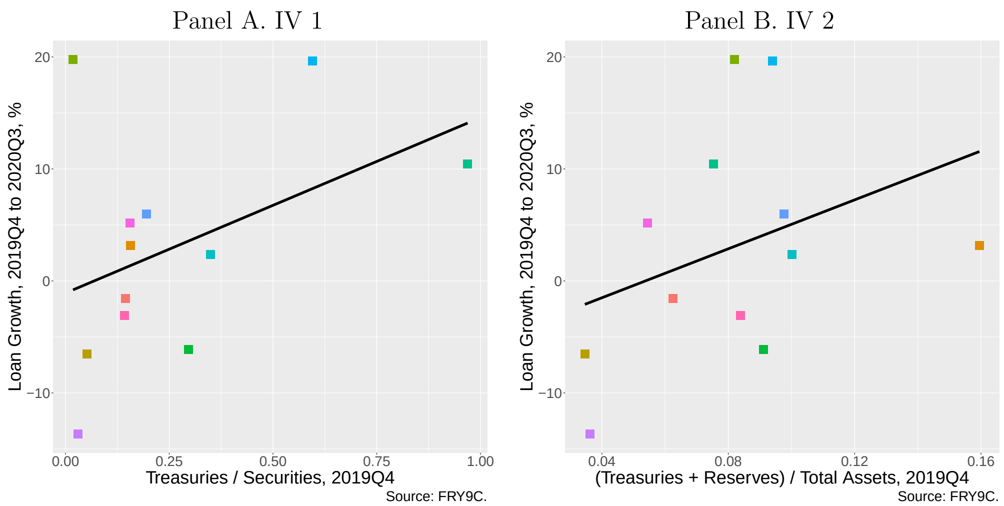

Figure 3. Exposure to SLR and Loan Growth

As described in section II, the Federal Reserve released the press announcement that Treasury and central bank reserves would be exempted from the SLR requirement on April 1st, 2020. Formally, we define $Post_t = I(t \geq 2020Q2)$. Further, we seek to identify the effect of regulatory relief only for those banks for which the SLR regulation was originally relevant. The regulation applied to bank holding companies with total assets in excess of $250 billion USD. Formally, we define $Reg_i = 1$ if the SLR regulation applies to bank i.

Table 2 shows the degree to which our treatment variables are correlated with other bank characteristics. More specifically, we regress the treatment variable on a vector of bank-specific covariates as of end-2019 to see if our measure is systematically related to other balance sheet characteristics. We will ultimately identify the treatment effect only

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for those banks for which the SLR regulation is relevant, so we focus only on the 12 banks for which the SLR applies. Column (1) presents regression results for the first instrument, and column (2) present the results for the second instrument. We note that banks with larger securities books tend to be less exposed to the regulation, and banks with larger loan books tends to be less exposed to the second instrument.

$$Treat_i = \alpha + \gamma' X_i + u_i \tag{1}$$

Table 2

	Dependent variable:	
	Treasures / Securities	(Treasuries + Reserves) / Assets
	(1)	(2)
log(Assets)	−0.131* (0.068)	−0.016 (0.018)
Equity / Assets	−7.727* (4.239)	−1.035 (1.139)
Loans / Assets	−0.079 (0.358)	−0.224** (0.096)
Securities / Assets	−1.491** (0.680)	−0.337* (0.183)
Deposits / Assets	−0.420 (0.381)	0.261** (0.102)
C& I Credit Lines / Assets	−0.398 (0.866)	−0.120 (0.233)
Constant	4.462** (1.782)	0.531 (0.479)
Observations	12	12
R^2	0.949	0.749

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

Our baseline empirical framework involves a triple difference specification where the differences are (1) before and after 2020Q2, (2) variation in $Treat_i$, and (3) application of the regulation Reg_i. The following regression tests our baseline specification:

$$y_{it} = \beta(Treat_i \times Post_t \times Reg_i) + \gamma_1(Reg_i \times Post_t) + \gamma_2(Treat_i \times Post_t) + \delta' X_{it} + \alpha_i + \alpha_t + \epsilon_{it} \tag{2}$$

The coefficient γ_1 controls for the average effect of simply being an SLR bank in the post-period. The coefficient γ_2 controls for the average effect of the treatment variable in the post period, irrespective of whether the SLR regulation applies. Thus our estimate for β captures the differential effect of experiencing larger regulatory relief in the post period only for those banks for which the regulation applies. X_{it} includes a vector of time varying bank level controls. Standard errors are clustered at the bank level.

Next, we turn to a loan level analysis in order to fully account for any potential borrower-level demand shocks that could differentially affect banks that are more exposed to SLR relaxation. In particular, the concern may be that banks with a higher exposure to SLR relaxation had lending relationships with firms that faced better investment opportunities during the post period, leading to a higher credit demand. By using the within-firm estimator of Khwaja and Mian (2008), we estimate the effect of SLR relaxation on bank lending while controlling for demand via borrower fixed effects. This methodology is widely used in the banking literature to identify supply-side effects while controlling for borrower demand.

Our loan-level analysis follows Rodnyansky and Darmouni (2017), who apply this within-firm estimation technique in order to quantify the effects of quantitative easing on bank lending following the 2008 crisis. We match the FR-Y9C regulatory data to DealScan data on commercial and industrial loans in order to construct a loan-level panel of lending banks and borrowing firms. We restrict our sample to banks with assets greater than \$250 billion. To identify the effect of SLR relaxation on bank credit provision, we compare the last loan received by the firm before the relaxation with the first loan received afterwards. We are interested in whether banks that were more exposed to SLR relaxation increased their lending relative to other banks, controlling for demand. DealScan provides information on the dollar amount of each loan facility. In order to impute the loan shares

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of individual syndicate participants, we use the imputation method outlined in Chodorow-Reich (2014). We alternatively consider a binary specification which examines whether firms borrow again from a given lender following the SLR relaxation, which circumvents the need to impute loan shares.

Thus, we regress the log change in lending for a firm-bank pair on bank exposure to SLR relaxation as measured by our narrow treatment variable $Z_{1,i}$, controlling for firm fixed effects. β_1 captures the effect of SLR relaxation on credit provision, controlling for firm demand.

$$\log(1 + L^{b,f}_{post\text{-}2020Q1}) - \log(L^{b,f}_{pre\text{-}2020Q1}) = \beta_0 + \beta_1 SLRExposure_b + \eta_f + \epsilon_{b,f} \quad (3)$$

IV. Empirical Results

A. Assets Decomposition

Our outcome variables of interest include Treasuries and loans net of credit line drawdowns. We present results in log terms and as a share of total assets. Table 3 presents the regression results for the first instrument, which captures the relative importance of Treasuries in a bank's securities portfolio. Table 4 shows the regression results for the second instrument, which captures the balance sheet intensity made up by Treasuries and central bank reserves. Standard errors are clustered at the bank level.

Starting with Table 3, column (1) indicates that treated banks increase their inventories of Treasuries in absolute terms. However, column (2) indicates that exposure to the regulation change did not lead treated banks to hold a significantly larger proportion of Treasury inventories relative to the size of their balance sheet. Column (3) shows that treated banks significantly increased their provision of loans to the economy in absolute terms. Column (4) shows a statistically positive and economically significant effect on the amount of loans scaled by total assets. A 50 percentage point increase in the relative share of securities made up by Treasuries corresponds to a 3.9 percentage point increase in the share of loans on the balance sheet. Comparing columns (2) and (4), we observe that treated banks expanded their loan-making businesses substantially more than their Treasury market making business, as a share of total assets. Recall that we have stripped out the influence of C & I credit line drawdowns, showing that the rule change amounted to a substantial increase in *new* credit provision.

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Table 3 Assets Decomposition IV 1: Treasuries / Securities

	(1) log(Treasuries)	(2) Treas/Assets	(3) log(Loans)	(4) Loans/Assets
Treat x Post x Reg	2.392***	0.012	0.453***	0.131***
	(0.842)	(0.025)	(0.104)	(0.031)
Post x Reg	-0.331	0.002	-0.220***	-0.035***
	(0.229)	(0.007)	(0.045)	(0.008)
Treat x Post	0.601	0.027	-0.211***	-0.054**
	(0.492)	(0.021)	(0.048)	(0.023)
Equity/Assets	1.351	-0.039	1.250	0.227*
	(3.666)	(0.049)	(0.872)	(0.131)
Deposits/Assets	0.345	0.016	-0.470*	0.077*
	(1.170)	(0.011)	(0.255)	(0.044)
Credit Lines/Assets	-1.547	0.008	1.295**	0.725***
	(2.959)	(0.041)	(0.520)	(0.154)
Bank FE	Yes	Yes	Yes	Yes
Date FE	Yes	Yes	Yes	Yes
Observations	2818	5596	5338	5338
R^2	0.924	0.814	0.990	0.972

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

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Moving to Table 4, the results are broadly similar when we use the second instrument, which captures the balance sheet intensity of Treasuries and reserves. Columns (1) to (3) indicates a limited effect on the level of Treasuries, the share of total assets made up by Treasuries, and the level of loans, respectively. Column (4) shows that treated banks substantially increased their provision of loans to the economy. More concretely, a treated bank with a 10 percentage point larger share of balance sheet space made up by reserves and Treasuries corresponds to an approximately 3.4 percentage point increase in loans, scaled by total assets. Comparing columns (2) and (4), we again observe that treated banks expanded their loan-making businesses substantially more than their Treasury market making business, as a share of total assets.

B. Pre-trends analysis

We next show pre-trends to show that our identified effect is associated with the SLR regulation change and not due to immediate negative effects of COVID disruption as of end-March 2020. Recall that our treatment period start in 2020Q2, after Treasury and other market had bottomed out by the end of March. Figure 4 shows pre trends for net loans and for Treasuries as a share of total assets.

C. Alternative specification: only the big 8 US banks

For robustness, we provide an alternative specification that considers a sample that includes only the 8 Globally and Systemically Important Banks (GSIBs). We use the first

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Table 4 Assets Decomposition IV 2: Treasuries + Reserves / Assets

	(1) log(Treasuries)	(2) Treas/Assets	(3) log(Loans)	(4) Loans/Assets
Treat x Post x Reg	-3.261	-0.122	-0.652	0.400**
	(4.810)	(0.138)	(0.745)	(0.194)
Post x Reg	0.619	0.013	-0.054	-0.039*
	(0.562)	(0.010)	(0.083)	(0.020)
Treat x Post	0.081	0.089	-0.653***	-0.140**
	(2.115)	(0.076)	(0.185)	(0.070)
Capital/Assets	1.434	-0.042	1.603	0.222
	(4.525)	(0.054)	(0.993)	(0.152)
Deposits/Assets	0.973	0.020	-0.626**	0.043
	(1.523)	(0.013)	(0.278)	(0.046)
Credit Lines/Assets	-1.407	0.020	1.418**	0.740***
	(3.374)	(0.049)	(0.563)	(0.172)
Bank FE	Yes	Yes	Yes	Yes
Date FE	Yes	Yes	Yes	Yes
Observations	2480	5084	4852	4852
R^2	0.916	0.789	0.989	0.969

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

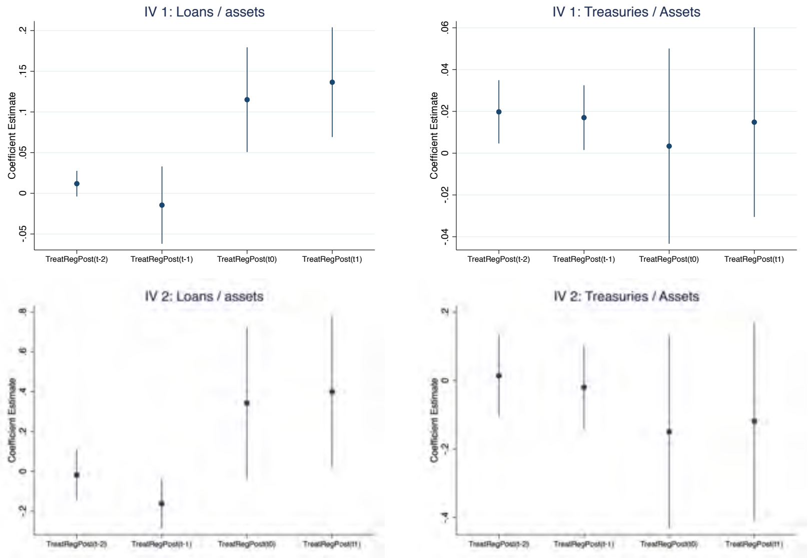

Figure 4. Pre Trends

instrument, namely Treasury share of the securities portfolio, and $Post_t$ as above, and run the following diff-in-diff regression, where we are comparing outcome variables pre- and post-regulation change within the treatment group only.

$$y_{it} = \beta Treat_i \times Post_t + \delta' X_{it} + \alpha_i + \alpha_t + \epsilon_{it} \tag{4}$$

Table 5 reports the results. Standard errors are clustered at the bank level. In line with our previous result, we see that for this subsample of banks there is a significant effect on Treasury holdings, reflecting that the SLR relaxation may have eased constraints for banks to increase their intermediation in Treasury markets. Columns (1) and (2) indicate that banks that had greater exposure to the leverage relief increased their Treasury intermediation activity by a modest amount. Columns (3) and (4) show cash and increased

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their loan exposures by a significantly positive amount. Our estimates suggest that a 50 percentage point increase in Treasuries' share of the securities portfolio corresponds to an approximately 3.6 percentage point increase in the amount of loans relative to total assets.

Table 5 Big 8 banks

	(1) log(Treasuries)	(2) Treas/Assets	(3) log(Loans)	(4) Loans/Assets
Treat x Post	1.994**	0.015	0.432***	0.072***
	(0.585)	(0.017)	(0.099)	(0.010)
Equity/Assets	-2.396	-0.067	1.649	0.962***
	(23.092)	(0.246)	(1.325)	(0.251)
Deposits/Assets	23.975	0.341***	0.611	0.087
	(15.238)	(0.092)	(1.093)	(0.098)
Credit Lines/Assets	32.795	0.100	7.719*	1.739***
	(40.889)	(0.371)	(3.570)	(0.324)
Bank FE	Yes	Yes	Yes	No
Date FE	Yes	Yes	Yes	No
Observations	152	152	144	144
R^2	0.712	0.859	0.998	0.996

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

D. Loan Level Analysis

To complement our bank level analysis, we examine whether banks that increased their overall loan holdings also increased their lending to a given firm, compared to other lenders. Thus, we regress the log change in lending for a firm-bank pair on bank exposure to SLR relaxation as measured by our narrow treatment variable, controlling for firm fixed effects. Table 6 reports the results and confirms that our loan level analysis is in line with our

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cross-sectional main specification. Columns (1) and (2) demonstrate that banks which are more exposed to SLR relaxation lend more credit to a given borrower, and columns (3) and (4) confirm that they are also more likely to renew a given loan relationship. We find that a 50 percentage point increase in a bank's relative share of securities made up by Treasuries corresponds to a 1 percentage point increase in the dollar amount of loans lent to a given borrower, and a 5.6% increase in the likelihood of renewing a given loan relationship.

Table 6 Loan Level Analysis

	(1) Lending Change	(2) Lending Change	(3) Renewal	(4) Renewal
Treatment Exposure	0.521***	2.026***	0.030***	0.112***
	(0.192)	(0.438)	(0.011)	(0.024)
Log Assets		-0.162**		-0.009**
		(0.064)		(0.004)
Securities/Assets		6.824***		0.376***
		(1.773)		(0.098)
Constant	-17.344***	-15.579***	0.046***	0.138
	(0.061)	(1.589)	(0.003)	(0.087)
Firm FE	Yes	Yes	Yes	Yes
Observations	6181	6181	6181	6181
R^2	0.649	0.652	0.642	0.645

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

V. Conclusion

We evaluate the effects of relaxing the Supplementary Leverage Ratio during the COVID-19 market disruptions on bank balance sheet composition and credit provision.

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We find that the relaxation may have allowed banks to hold greater inventories of Treasuries, but further allowed for an expansion of traditional bank credit. Our findings suggest that this leverage ratio was binding for banks during COVID-19, modestly increased bank liquidity provision in Treasury markets, and further affected banks' portfolio composition across asset classes, amounting to a shift of banks' loan supply schedules. Our findings are net of C&I credit line drawdowns, and hold after controlling for demand by looking among banks which lend to a given borrower. We thus highlight that countercyclical relaxation of uniform leverage constraints can increase bank credit provision during economic downturns. Given the binding nature of the SLR, relaxation of this constraint may be more effective than other countercyclical measures in allowing banks to extend credit.

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The impact of COVID-19 on global production networks: Evidence from Japanese multinational firms[1]

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Using aggregate-level data on Japanese multinational corporations (MNCs) in major host countries and regions, this paper investigates the impact of COVID-19 on global production and supply chains with a focus on East Asia. I use the numbers of COVID-19 cases and deaths as measures of the impact of the pandemic. I find that the pandemic had substantial impacts on the performance (sales, employment, and investment) of Japanese MNCs and global supply chains (exports to Japan and exports to third countries) in Q1–Q3 2020. China recovered quickly in Q2 and grew in Q3, whilst the countries of the Association of Southeast Asian Nations and the rest of the world had still not fully recovered in Q3 2020. Importantly, lockdown and containment policies in host countries had large negative impacts on the sales and employment of Japanese MNCs. In contrast, I did not find positive effects of economic support policies on firm performance. Interestingly, whilst the firm expectations and business plans of Japanese MNCs were negatively affected by the COVID-19 pandemic, their business confidence increased with strong overall government policy responses in host countries in Q1 2020.

1 This study is conducted as a part of the research at the Research Institute of Economy, Trade and Industry (RIETI). I am grateful to Masayuki Morikawa for his guidance and encouragement. This paper was prepared for the project 'ERIA Research on Covid-19' at the Economic Research Institute for ASEAN and East Asia (ERIA). Financial support from ERIA is greatly appreciated. I thank Rashesh Shrestha for his valuable comments and suggestions. All remaining errors are mine.
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1. Introduction

The COVID-19 pandemic has had substantial impacts on production networks in East Asia and the rest of the world. Supply chains have been disrupted and both supply and demand shocks have been transmitted through supply chains and propagated across borders. As Baldwin and Tomiura (2020) point out, COVID-19 is contagious economically as it is medically. To slow down the spread of the coronavirus, many countries imposed some form of restrictions on people and businesses. Since Japanese multinational corporations (MNCs) are important drivers and players in global value chains (GVCs), their supply chains and overseas production were hit hard by the COVID-19 shock.

In this paper, I aim to examine the impact of the COVID-19 pandemic on global production networks as well as the effects of policy responses, using country-level aggregated data on Japanese MNCs. Specifically, I investigate the following questions: (1) How large was the impact of COVID-19 on production networks and supply chains in East Asia and other regions? (2) How did country-specific government policy responses affect firm performance? (3) How did the pandemic and policy responses affect firm expectations and business plans? Utilising the latest and unique dataset on Japanese foreign affiliates in manufacturing sectors, which contains information on overseas activities and firm expectations, I wish to explore these important issues and provide evidence-based policy implications.

Figure 1 shows the year-on-year (y-o-y) changes in total sales (sum of local sales and exports) of Japanese manufacturing MNCs in major regions from Q1 2019 to Q3 2020. Due to the outbreak of COVID-19 in Q1 2020, the total sales of Japanese affiliates in China declined substantially by 21.3% y-o-y. Japanese affiliates in non-China regions

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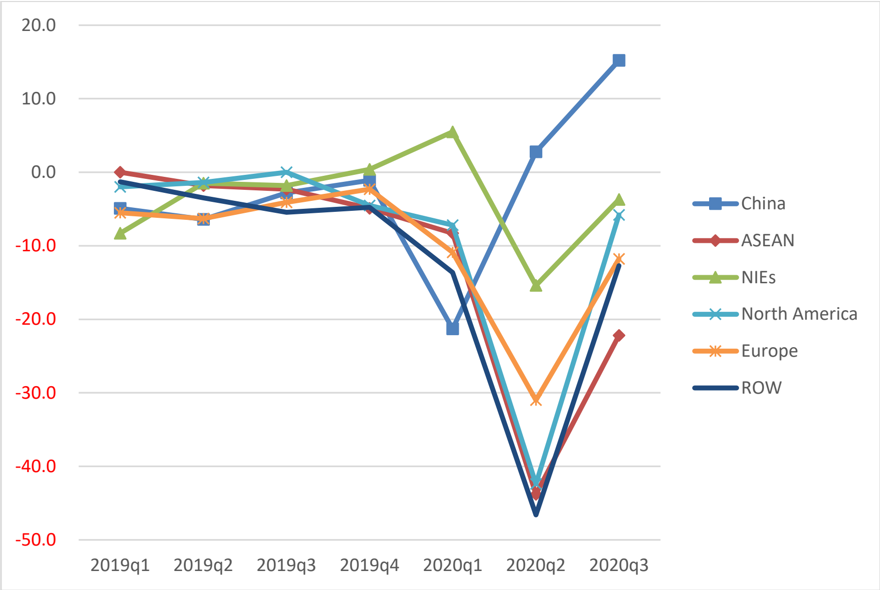
Figure 1. Industrial Disruption and Recovery: Total Sales by Region (%, y-o-y)



Note: Hong Kong is included in China. NIEs include the Republic of Korea, Singapore, and Taiwan. ROW include countries in Africa, Oceania, and South America. See Appendix A for the full country list.

Source: Author's own compilation based on the Quarterly Survey of Overseas Subsidiaries, Ministry of Economy, Trade and Industry.

except newly industrialised economies (NIEs) also saw significant declines in total sales. For example, the Association of Southeast Asian Nations (ASEAN), Europe, and North America had y-o-y decreases of 8.3%, 10.9%, and 7.2%, respectively. In Q2 2020, China had a 'V-shaped' recovery from COVID-19, and sales increased by 2.8% y-o-y. However, due to the fast spread of COVID-19 in non-China regions, on a y-o-y basis, total sales decreased sharply by 43.8% in ASEAN, by 42.4% in North America, and by 46.6% in the rest of the world (ROW), respectively. In Q3 2020, sales in China grew by 15.2% y-o-y,

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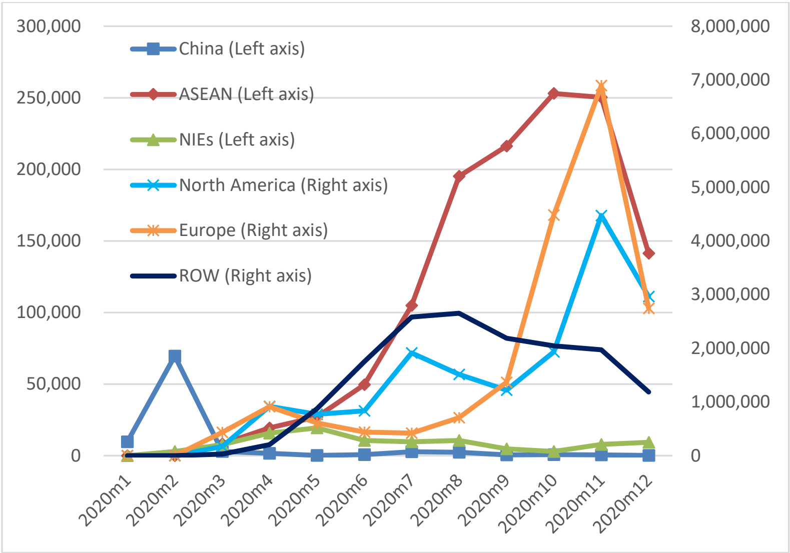
Figure 2. Monthly Number of COVID-19 Cases by Region



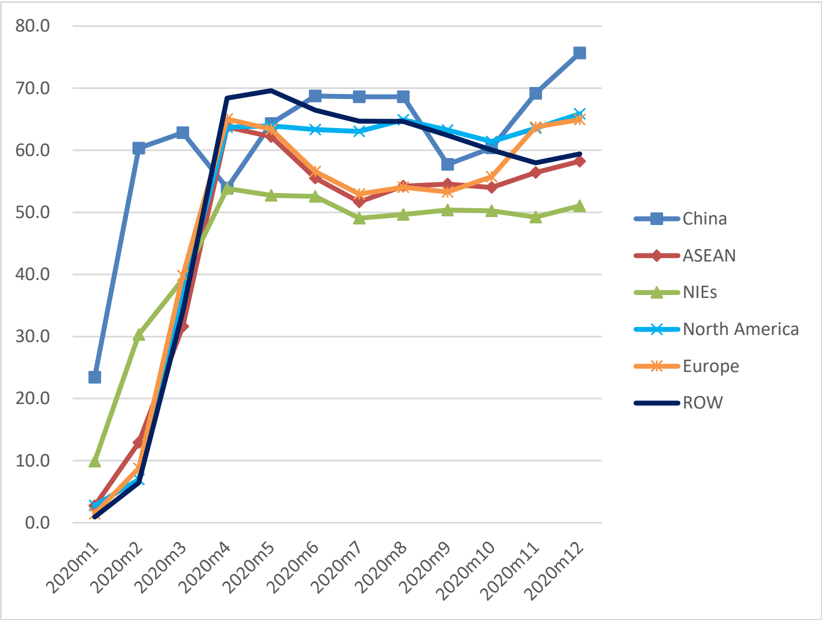
Source: Author's own compilation based on data from the Johns Hopkins Coronavirus Resource Center.

and sales in non-China partially recovered relative to Q2. However, growth rates were still very low in Europe, North America, and especially in ASEAN (–22.2% y-o-y). In fact, local sales, which account for about 70% of foreign affiliates' total sales, also show a similar pattern (see Appendix Figure B1).[1]

To understand the overseas business environment for Japanese MNCs, Figure 2 and Figure 3 provide an overview of COVID-19 and policy responses in the host countries and regions. Figure 2 depicts the monthly number of COVID-19 cases in the world. China was at the height of the COVID-19 pandemic in February, with more than 60,000 cases in 1 month. Afterwards, owing to China's extremely restrictive measures, there was a

[1] Furthermore, employment and investment by foreign affiliates decreased significantly in all regions between Q1 and Q3 2020 (see Appendix Figure B2 and Figure B3).

Figure 3. Monthly Government Response Index by Region



Source: Author's own compilation based on data from the Oxford COVID-19 Government Response Tracker.

significant decline in the number of new cases in March, and the spread of coronavirus was almost under control after Q1 2020. However, the virus spread fast around the world and cases increased exponentially in ASEAN, Europe, North America, and the ROW. The severity of the pandemic in ASEAN, Europe, and North America had substantial negative impacts on the total sales of Japanese firms in Q2 and Q3 2020 as shown in Figure 1.

Figure 3 shows the monthly government policy responses in major regions in 2020. The government response index measures the strength of policy responses, with larger numbers signifying stronger levels of government action (see Section 2 for details). The index for China jumped from January to February and stayed at a very high level during

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the period. The index for ASEAN, Europe, North America, and the ROW soared in February and March and remained at relatively high levels as well. The performance of Japanese MNCs is likely to have been heavily affected by the strong policy responses in the host countries.

This study is closely related to recent studies on the impact of COVID-19 and policy response in the context of production networks and supply chains. Using a unique Japanese firm-level survey conducted in January 2020 that contains information on sales forecasts, Chen, Senga, and Zhang (2020) find that the outbreak of COVID-19 in late January led to a substantial increase in firms' subjective uncertainty. This effect is especially large for Japanese firms that have direct exposure to China with supply chains and overseas production. Using monthly trade data, Hayakawa and Mukunoki (2020) investigate the impacts of COVID-19 on the GVCs of machinery products and find there were large negative supply chains effects, i.e. countries that export machinery parts to countries exporting finished machinery products were affected the most relative to countries mainly importing or exporting finished products. Based on quantitative estimations, Bonadio et al. (2020) show that the average real gross domestic product downturn due to the pandemic is expected to be −32.6%, with one-fifth of the total due to transmission through global supply chains. Importantly, they argue that the renationalisation of global supply chains does not make countries more resilient to pandemic-induced contractions in the labour supply.

Baldwin and Freeman (2020) argue the possibilities of manufacturing contagion and reinfection from the 'COVID concussion'. As manufacturers around the world rely on inputs from China, the industrial disruption in China hit the rest of the world via supply-chain contagion first. However, after that, the pandemic in other manufacturing giants,

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such as Germany and the United States (US), is likely to create a reverse effect, i.e. supply-chain reinfection. As policy responses, Baldwin and Freeman (2020) argue that international coordination on containment exceptions for essential goods may reduce the chances that multiple waves of supply-chain contagion hobble global production. Furthermore, Kimura et al. (2020) find that regional policy coordination is critical to mitigate and isolate COVID-19 shocks and note the importance of identifying pandemic events early to flatten the pandemic curve at the national and regional levels. They also emphasise that the stability of the GVC network is critical during the pandemic.

I find that the pandemic had substantial impacts on the performance of Japanese MNCs and global supply chains in Q1–Q3 2020. Importantly, the sales and employment of Japanese MNCs were also affected by the lockdown and containment policies in host countries, whilst economic support policies did not have positive effects on firm performance. This paper complements previous studies by examining the impact of the pandemic on global production networks and the effects of COVID-19 policy responses in host countries. Based on empirical results, this study provides policy implications for international production networks in the policy framework initiated by Kimura (2020) for overcoming COVID-19.

The rest of the paper is organised as follows. Section 2 introduces the data and variables; Section 3 presents the descriptive evidence; and Section 4 presents the empirical results. Section 5 concludes.

2. Data and Variables

2.1 Aggregate-level data on Japanese multinational firms

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To implement the analysis, I use the Quarterly Survey of Overseas Subsidiaries (QSOS) collected by the Ministry of Economy, Trade and Industry (METI), Japan. This survey covers Japanese foreign affiliates with 50 or more employees in manufacturing industries.[2] For simplicity, I refer to affiliates and subsidiaries as firms, and I use Japanese MNCs and Japanese foreign/overseas affiliates interchangeably. The QSOS data contain information on country and industry classifications, sales, acquisitions of tangible fixed assets (excluding land), and the number of employees. Importantly, a firm's total sales can be decomposed into local sales, sales (exports) to Japan, and sales (exports) to third countries (other than Japan). This allows me to investigate the impact of COVID-19 on the global production of Japanese MNCs as well as regional and global supply chains. I use country-level and industry-level aggregate data based on the quarterly surveys conducted in 2018–2019, and Q1–Q3 2020.[3]

The QSOS data also has unique information on qualitative forecasts of sales (local, to Japan, and to the third countries), capital investment, and the number of employees. I use the Diffusion Index (DI), which captures the business confidence of Japanese MNCs. In the survey, answers from the responding foreign affiliates ('Increase, Unchanged, and Decrease') are aggregated into the DI as follows: DI (percentage points) = percentage share of firms responding 'Increase' minus the percentage share of firms responding 'Decrease'. For instance, 'investment DI' indicates a respondent's judgement on the 'acquisition of tangible fixed assets'. Foreign affiliates are asked to choose one out of the

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[2] Specifically, this survey targets overseas subsidiaries of Japanese parent firms that meet all of the following criteria as of the end of the surveyed quarter: manufacturing subsidiaries; subsidiaries with 50 or more employees; and subsidiaries with 50% or more of their capital coming from parent firms, including both direct and indirect funds (such as funds provided via local subsidiaries).

[3] Firm-level data is not available for the time being, and I leave it as future work. In a previous study using the firm-level data of QSOS, Sun et al. (2019) find that relative to affiliates in other Asian countries, Chinese affiliates, especially those with high exposure to trade with North America, in general see a decline in sales since the US–China trade war began in March 2018.

three judgments, 'Increase', 'Unchanged', and 'Decrease'. The percentage share of the number of firms for each judgment is calculated, and the percentage share of those which replied 'Decrease' is subtracted from those that replied 'Increase'. I use this index to see how well Japanese foreign affiliates are performing in terms of their forecasts by comparing the 'forecast DI' of the previous survey with the 'actual DI' of the present survey. Unfortunately, since questions on firm expectations are excluded from the survey form after Q2 2020, I use information on forecasts for the period Q1 2019–Q1 2020 only in my analysis.

2.2 COVID-19 confirmed cases and deaths

The number of COVID-19 confirmed cases and the number of deaths are obtained from the Johns Hopkins Coronavirus Resource Center.[4] These data are recorded daily. I use the number of cases and the number of deaths in each country by the end of each quarter as measures of the impacts of the COVID-19 pandemic.

2.3 Government response tracker

The measures of country-specific government policy responses to COVID-19 shocks are constructed by the Oxford Blavatnik School of Government Coronavirus Government Response Tracker (Hale et al. 2020; henceforth, GRT).[5] Specifically, the GRT includes (1) an overall government response index, which measures how the response of governments has varied over all indicators in the database, becoming stronger or weaker over the course of the outbreak; (2) a stringency index, which measures the

[4] https://coronavirus.jhu.edu/map.html

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

strictness of lockdown policies that primarily restrict people's behaviour; (3) an economic support index, which records measures such as income support and debt relief; and (4) a containment and health index, which combines lockdown restrictions with measures such as testing policies and contact tracing, as well as investment in health care and vaccines. Each index ranges from 0 to 100, and a larger number reflects stronger levels of government action. These data are recorded daily. I take the average value for each index by host country and year-quarter. As the results of the stringency index and the containment and health index are quite similar, I focus on the first three indexes.

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3. Descriptive Evidence

Based on the dataset on Japanese MNCs, COVID-19 cases/deaths, and the GRT, I document three sets of descriptive evidence: (1) supply chain disruption, (2) COVID-19 shocks and global production, and (3) government policy responses and firm performance.

3.1 Supply chain disruption

COVID-19 disrupted supply chains regionally and globally. COVID-19 shocks hit China–Japan trade hard as the supply chains of Japanese firms rely heavily on China. According to the QSOS data, the total sales of Japanese overseas affiliates was US$1,129.6 billion in 2019, of which China accounted for 21%. Importantly, amongst total sales, sales (exports) to Japan were US$100.5 billion, and China accounted for 37.5%. This is the intra-firm trade only. If the sourcing from other Chinese firms is included in, the dependence on China is even higher.

Figure 4A shows the y-o-y changes of overseas affiliates' exports to Japan by region from Q1 2019 to Q3 2020. In Q1 2020, affiliates' exports from China to Japan had a substantial decrease by 17.8% y-o-y. On the contrary, in ASEAN, NIEs, North America, and Europe, exports to Japan increased by 0.7%, 44.5%, 7.4%, and 11.7% y-o-y, respectively. The sudden increase in NIEs, especially the Republic of Korea and Taiwan, may reflect the substitution effect as imports from China was disrupted in Q1. However, in Q2 2020, although Japanese affiliates' local sales in China almost recovered and increased by 6.7% y-o-y, their exports to Japan continued to see a y-o-y decrease of 12%. The situation in non-China regions such as ASEAN and Europe was even worse (about –20% y-o-y), implying that the supply shock was larger in non-China regions relative to China. Exports to Japan were recovering in all regions in Q3 2020, but partially due to the decrease in demand in Japan, exports to Japan still saw declines in almost all regions.

Similarly, Figure 4B shows that Japanese affiliates' exports to third countries fell sharply during Q1–Q2 2020. Importantly, the impact was much larger relative to exports to Japan. On average, the y-o-y changes of exports to third countries were –9.5% and –33.4% in Q1 and Q2, whilst the y-o-y changes of exports to Japan were –5.5% and –16.5% during the same period. As exports to third countries involve more complex production networks and coordination costs relative to exports to the home country (Japan), the disruptions in global supply chains were much more severe. Foreign affiliates' exports to third countries were recovering in Q3 2020 but did not return to pre-COVID-19 levels. It is also worth noting that ASEAN-based affiliates had the lowest recovery in Q3. Whilst their exports to Japan were –7.2% y-o-y in Q3, their exports to third countries were much lower, at –24.7% y-o-y. This suggests that the negative demand shock was much larger in the third countries, relative to Japan.

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Figure 4A. Exports to Japan (%, y-o-y)

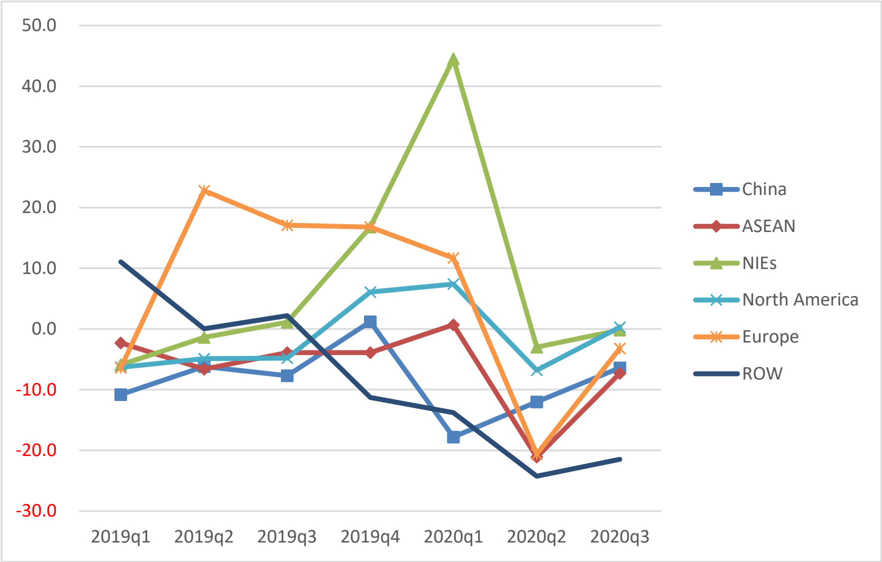
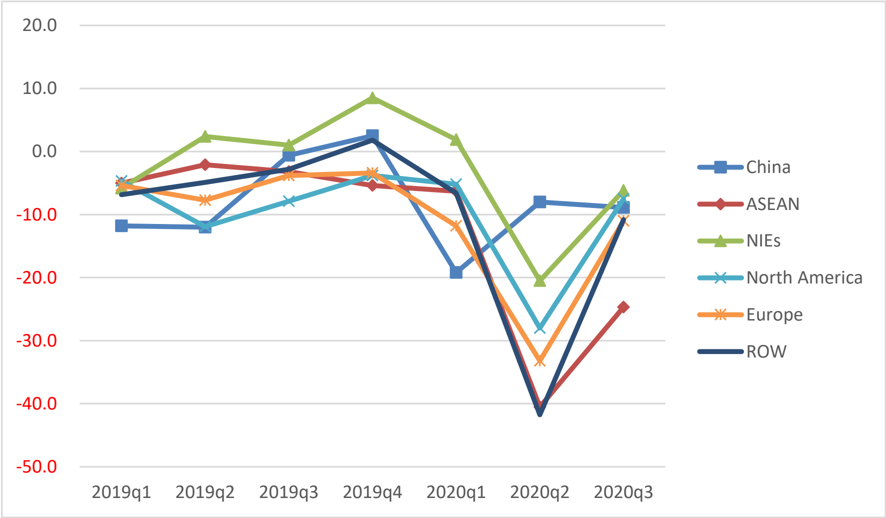


Figure 4B. Exports to Third Countries (%, y-o-y)



Source: Author's own compilation based on the Quarterly Survey of Overseas Subsidiaries, Ministry of Economy, Trade and Industry.

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Table 1. Exports to Japan by Industry (%, y-o-y)

Year 2020	Q1		Q2		Q3	
Industry/Region	China	ASEAN	China	ASEAN	China	ASEAN
Food and tobacco	-21.9	-9.7	6.8	-12.7	-16.2	-11.3
Textiles	-25.2	-8.5	-29.4	-12.8	-26.8	-13.6
Lumber, pulp, paper and paper products	8.6	-6.4	16.8	-12.1	-8.0	-30.9
Chemicals	-5.3	-12.9	-1.2	-19.7	-3.2	-19.8
Ceramic, stone and clay products	-39.7	-14.0	-31.8	-35.0	-34.9	-19.6
Iron and steel	-44.0	-7.8	-16.2	-31.4	-22.9	-15.6
Non-ferrous metals	-22.3	-8.9	-12.1	-2.4	-0.7	1.5
Fabricated metal products	-22.4	-9.4	-19.5	-20.6	-9.2	-29.5
General-purpose, production and business oriented machinery	-27.5	-5.0	-23.9	-22.7	-9.6	-18.6
Electrical machinery	-9.9	18.8	-2.6	-11.5	-0.7	8.0
Transportation equipment	-27.6	-17.6	-25.5	-39.5	-16.7	-26.3
Miscellaneous manufacturing	-32.5	-10.8	-34.0	-39.9	-29.0	-11.0
Total	-17.9	0.8	-12.1	-21.2	-6.4	-7.3

Source: Author's own compilation based on the Quarterly Survey of Overseas Subsidiaries, Ministry of Economy, Trade and Industry.

COVID-19 had heterogeneous effects on supply chains by industry. Table 1 presents foreign affiliates' exports to Japan by industry in China and ASEAN in Q1–Q3 2020. First, in China, all industries (excluding lumber) had sharp declines in exports to Japan since Q1. Compared with the industry average of –17.9%, textiles and transportation equipment dropped by 24.8% and 28.6% y-o-y in Q1, respectively. Exports to Japan did not fully recover in Q2–Q3 largely due to the demand shock in Japan. Second, ASEAN, which is expected to be an important alternative sourcing origin, also experienced significant decreases in all industries (excluding electrical machinery). The supply chains between ASEAN and Japan were in crisis as well. Third, there were large variations across industries. One possible explanation is that compared with consumption goods, such as foods and textiles, durable goods, such as cars and other transportation equipment, are

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more likely to be hit hard by the pandemic. This is similar to the great trade collapse during the global financial crisis (GFC) in 2008–2009. The disruption of supply chains may lead to significant declines in trade in capital goods (general machinery) and intermediate inputs for production (iron and steel, parts and components), which rely heavily on international production networks. Furthermore, the relatively small impact on electronic machinery may reflect the increasing work-from-home demand for computers and other related electronic products.

3.2 COVID-19 shocks and global production

Next, I document the impact of COVID-19 on the performance of Japanese MNCs in major host countries. Figures 5A–5C show the relationship between the number of COVID-19 cases (in logarithms) and the y-on-y changes of total sales (the sum of local sales and exports), the number of employees, and capital investment, respectively. In each figure, the blue circles indicate Q1, the red triangles represent Q2, and the green squares represent Q3.

First, in Figure 5A, the plots of y-o-y changes in sales against COVID-19 cases across countries show that sales decline significantly with increases in COVID-19 cases. In other words, COVID-19 cases are significantly negatively associated with sales of Japanese MNCs in major countries. In Q1 2020, affiliates' sales in China, Brazil, and Malaysia sharply decreased by about 20% y-o-y, whilst sales in Taiwan and Singapore only increased. In Q2, whilst China had almost recovered from the COVID-19, the sales of Japanese MNCs dropped substantially in all other countries. India, Brazil, and Indonesia were the worst amongst them. Importantly, the fitted lines for Q1 and Q2 show that the negative correlations between COVID-19 cases and sales growth rates became significantly stronger from Q1 to Q2 2020. This suggests that the pandemic and the performance of Japanese MNCs were getting worse in major countries, except China. In Q3, affiliates' sales in China continued to increase. The situation in other countries was getting better relative to Q2, but most of them still had large decreases in sales.

Second, the shock of COVID-19 also had large negative impacts on the local employment of Japanese MNCs in host countries during Q1–Q3 2020. Figure 5B shows that declines in employment are large, especially in China (Q1), India (Q2), Indonesia and Malaysia (Q2 and Q3), and France (Q3). Compared with Q1, employment was getting worse in many countries in Q2 and Q3, but the correlation between COVID-19 cases and employment growth rate was not significant in Q3, suggesting that, on average, affiliates' employment was recovering along with their total sales.

Third, capital investment dropped significantly in many countries in Q1–Q3 2020, amongst which, investment in India, Indonesia, Malaysia, Singapore, and even Taiwan decreased by about 50% y-o-y in Q2. Surprisingly, Figure 5C shows that the correlations between the number of COVID-19 cases and capital investment were *not* significantly negative. Since COVID-19 was a global shock, it seems that Japanese MNCs reduced their global investment even in counties relatively less affected by the pandemic in Q2. There were substantial variations across countries in Q3, and it is worth noting that investment significantly increased in Mexico, the Netherlands, and Viet Nam. Investment in these countries may suggest the possibility of the supply chain reorganisation of Japanese MNCs in ASEAN, Europe, and North America.

Figure 5A. COVID-19 and Global Production: Sales

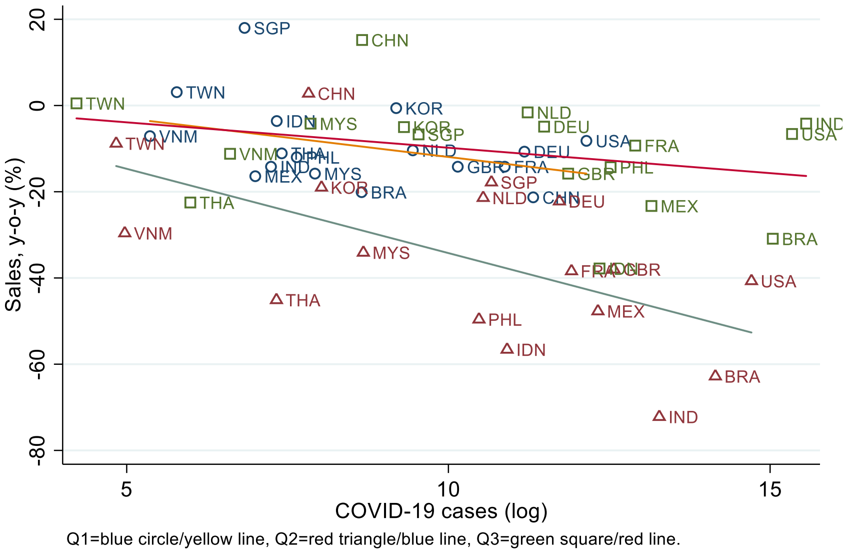


Figure 5B. COVID-19 and Global Production: Employment

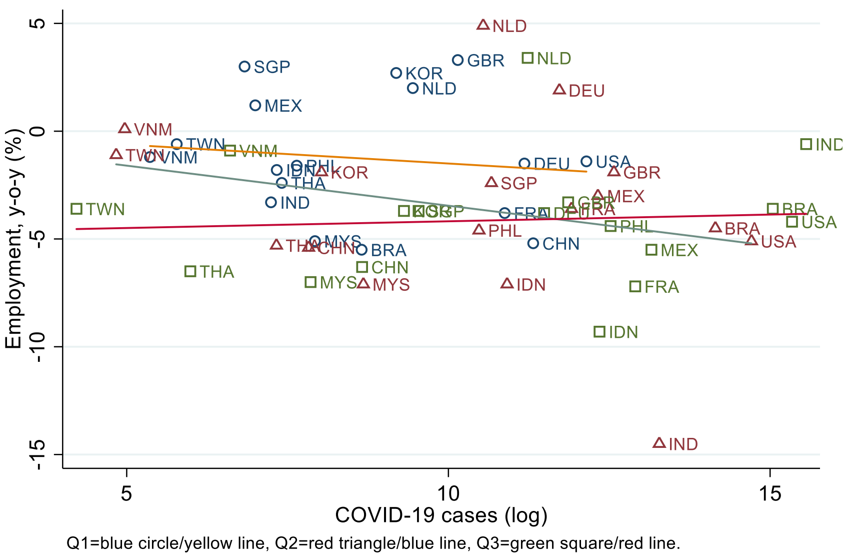
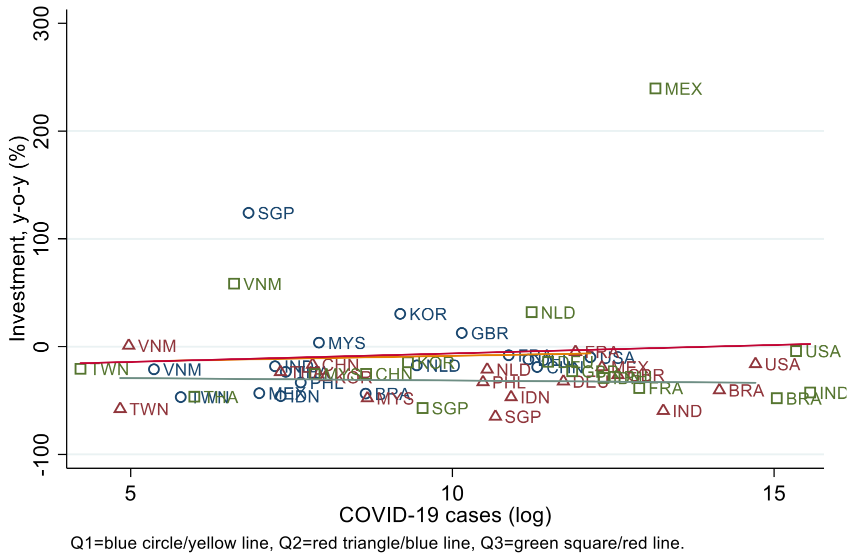


Figure 5C. COVID-19 and Global Production: Investment



Source: Author's own compilation based on data from the Quarterly Survey of Overseas Subsidiaries, Ministry of Economy, Trade and Industry, and Johns Hopkins Coronavirus Resource Center.

To better understand the impact of COVID-19 on firms, it is useful to take a look at firms' judgement on business conditions. Figure 6A shows that relative to Q1 2020, the current (Q2) DI of total sales, employment, and investment is significantly negatively correlated with the number of COVID-19 cases, respectively. Specifically, compared with Q1 (previous quarter), the business confidence of Japanese MNCs in Q2 (current quarter) was getting worse very quickly in countries such as Indonesia, India, Brazil, the US, and the United Kingdom, which were hit hard by COVID-19. This is especially true in terms of investment, which is costly and irreversible relative to employment. Interestingly, since the COVID-19 pandemic was almost under control in China in April, the DI of sales and

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Figure 6A. COVID-19 and Firm Expectations: Current DI

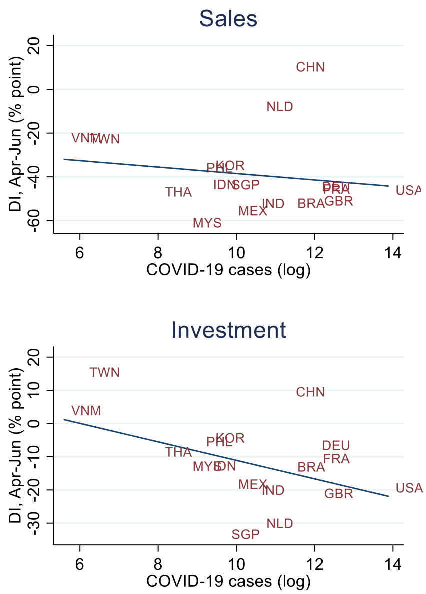
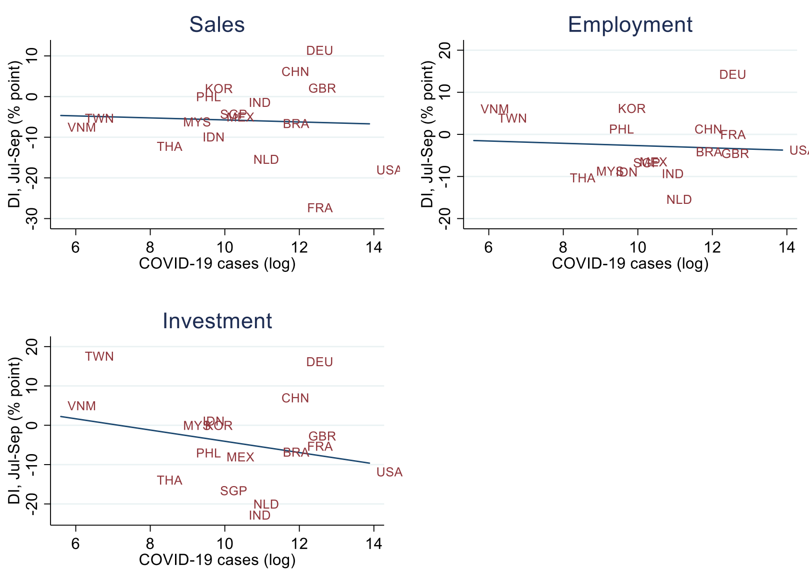


Figure 6B. COVID-19 and Firm Expectations: Next DI



Source: Author's own compilation based on data from the Quarterly Survey of Overseas Subsidiaries, Ministry of Economy, Trade and Industry, and Johns Hopkins Coronavirus Resource Center.

investment in China improved by approximately 10 percentage points. The investment DI in Taiwan was even higher as Taiwan has been one of the most successful regions in fighting against COVID-19. Figure 6B presents the next DI, i.e. business outlook on Q3 relative to Q2. It is clear that more Japanese MNCs answered that they will not increase their investment and hiring in Q3 relative to Q2. In sum, COVID-19 had substantial impacts not only on firm performance but also on firm expectations and business plans.[6]

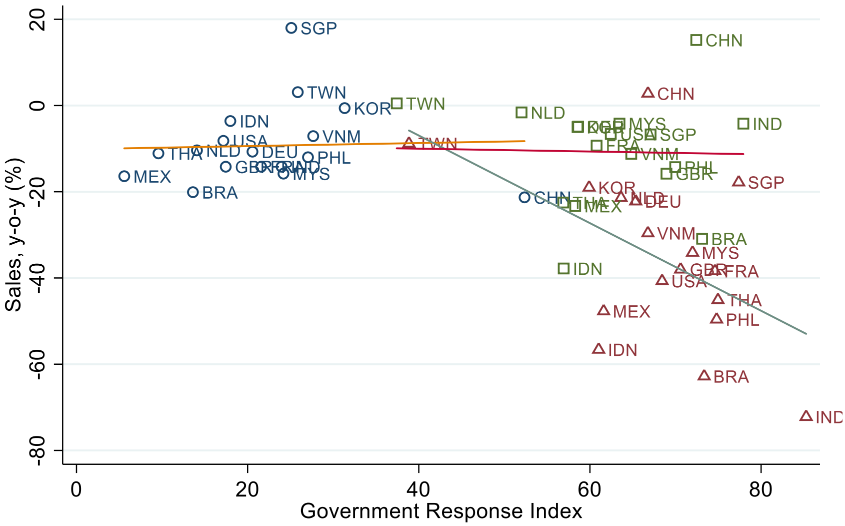
3.3 Policy responses, firm performance, and expectations

To mitigate and isolate the COVID-19 shock on firms and the economy, the governments in all countries and regions have enacted various COVID-19 policies. The Oxford COVID-19 GRT points out that 'government responses vary significantly from one country to another, and like any policy interventions, their effect is highly contingent on the local political and social context. COVID-19 Government Response Indices, like all aggregate indices which combine different indicators into a general index, should not be interpreted as measuring the appropriateness or effectiveness of a country's response.'[7] Thus, it is not easy to estimate the impact of such policies and evaluate which COVID-19 policy is effective or not. However, it would be interesting to investigate the relationship between COVID-19 policy and the performance and expectations of Japanese MNCs in host countries since the government responses can influence the

[6] Figures B4–B6 in the Appendix show that the sales and exports expectations in major regions changed dramatically after the outbreak of COVID-19.

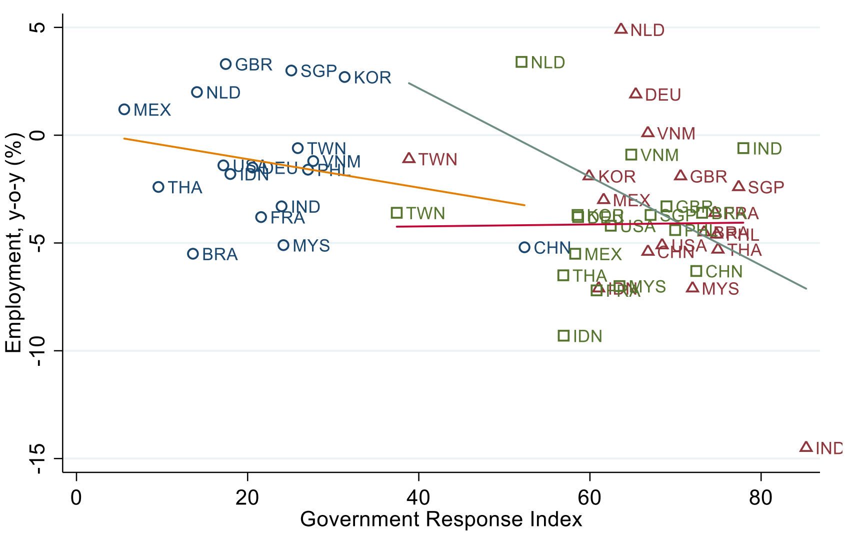
[7] https://www.bsg.ox.ac.uk/research/research-projects/coronavirus-government-response-tracker

Figure 7A. Policy Responses and Firm Performance: Sales



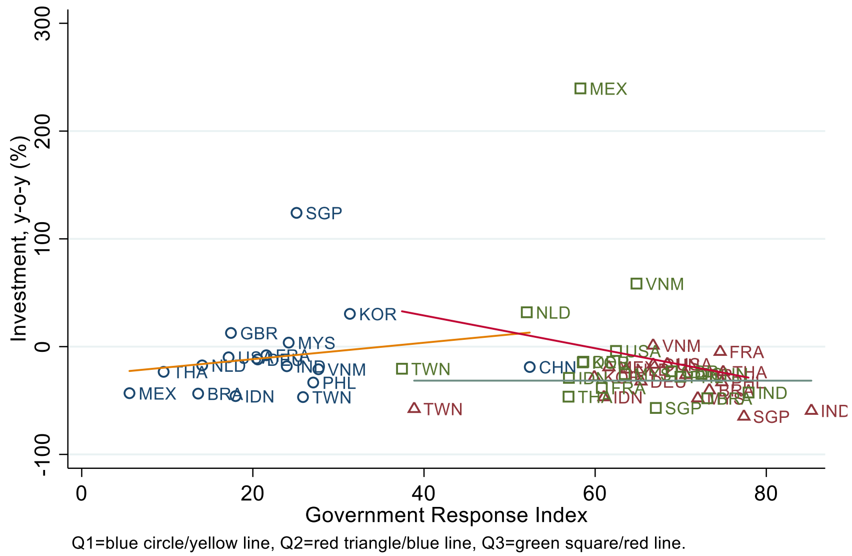
Q1=blue circle/yellow line, Q2=red triangle/blue line, Q3=green square/red line.

Figure 7B. Policy Responses and Firm Performance: Employment



Q1=blue circle/yellow line, Q2=red triangle/blue line, Q3=green square/red line.

Figure 7C. Policy Responses and Firm Performance: Investment



Source: Author's own compilation based on data from the Quarterly Survey of Overseas Subsidiaries, Ministry of Economy, Trade and Industry, and Oxford COVID-19 Government Response Tracker.

activities of both domestic firms and foreign firms through global supply chains.

Figures 7A–7C show the correlations between the overall government response index and the y-o-y changes of Japanese foreign affiliates' total sales, employment, and investment in major countries, respectively. In each figure, the blue circles indicate Q1, the red triangles represent Q2, and the green squares represent Q3. The overall government response index records how the response of governments become stronger or weaker over the course of the outbreak of COVID-19. It is obvious that the index shifts significantly to the right, suggesting that COVID-19 policy became very strong in all countries in Q2–Q3 relative to Q1 2020. The stronger policy responses are significantly negatively associated with declines in total sales and employment but not capital investment in Q2. This is quite similar to the effects of COVID-19 cases on firm performance shown in Figures 3A–3C. As the overall government response index consists of various indicators, from lockdown restrictions to income support, it seems that the overall policy responses did not have positive effects on Japanese MNCs.

To take a look at the effects of COVID-19 policy on firm expectations, in Figures 8A and 8B, I plot the DI of sales/employment/investment against COVID-19 cases across countries in Q1 2020. It is clear that the government policy response indices are strongly positively associated with the current (Q2) DI of sales, employment, and investment relative to Q1. This implies that stronger overall government responses likely gave more business confidence to Japanese MNCs in host countries such as China and Viet Nam. This is also true for next (Q3) DI relative to current (Q2) DI. Unfortunately, limited data availability makes it hard to examine the relationship between the changes in policy responses and updating of firm expectations.

Figure 8A. Policy Responses and Firm Expectations: Current DI

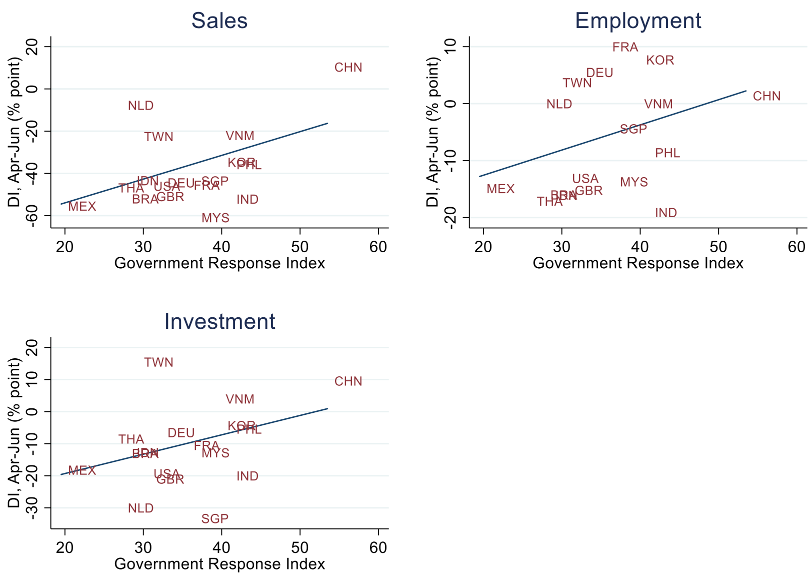
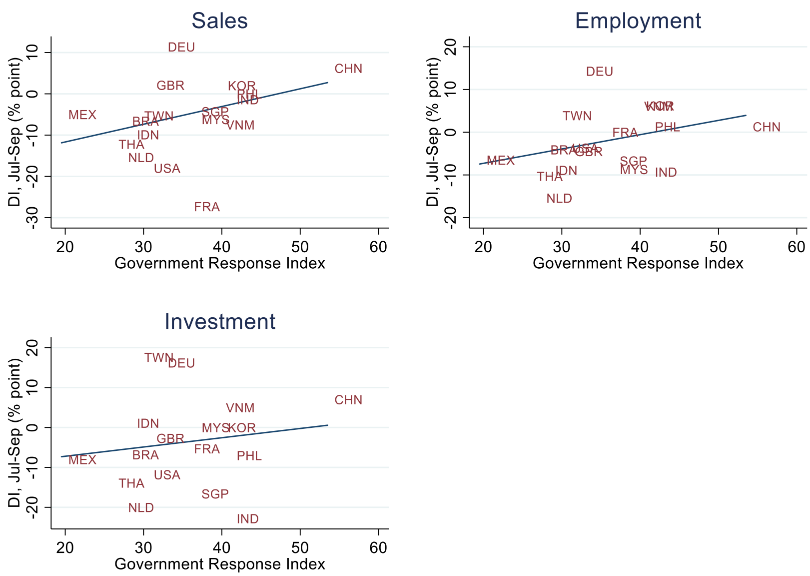


Figure 8B. Policy Responses and Firm Expectations: Next DI



Source: Author's own compilation based on data from the Quarterly Survey of Overseas Subsidiaries, Ministry of Economy, Trade and Industry, and Oxford COVID-19 Government Response Tracker.

4. Empirical Analysis

4.1 Specifications

The empirical specification explores the across-country variations in COVID-19 cases and deaths and governments' policy responses. First, I estimate the impact of the COVID-19 shock on firm performance as follows:

$$Y_{ct} = \alpha_0 + \alpha_1 COVID_{ct} + FE_c + FE_t + \varepsilon_{ct} \quad (1)$$

where Y_{ct} is the y-o-y change (%) or the logarithm of total sales, number of employees, and capital investment of Japanese foreign affiliates in country c and year-quarter t. $COVID_{ct}$ is the logarithm of the number of COVID-19 cases or the number of deaths, which measures the impacts of COVID-19 pandemic in country c and year-quarter t. I also include country fixed effects and year-quarter fixed effects to eliminate the time-invariant differences across countries and control for various other macroeconomic shocks.[8]

Second, to estimate the effects of COVID-19 policy on the firm performance of Japanese affiliates, I run the regressions as follows:

[8] The aggregate-level data of QSOS publicly available at the METI's website only contains information on exports to Japan and exports to third countries by major regions (China, ASEAN, NIEs, North America, etc.), not by country, so it is not possible to estimate the impacts of the demand shock in destination countries.

$$Y_{ct} = \beta_0 + \beta_1 GRT_{ct} + FE_c + FE_t + \varepsilon_{ct} \quad (2)$$

where GRT_{ct} measures the country-specific government policy response to the COVID-19 pandemic, including measures such as the overall government response index, stringency index, and economic support index.

The coefficients of interest are α_1 and β_1. I expect α_1 is negative, but the sign of the coefficient β_1 is not clear. It could be negative if the business activities of Japanese affiliates were affected by lockdowns and severe mobility restrictions in the host countries. On the other hand, it could be positive if COVID-19 policy is effective and the impacts of COVID-19 on Japanese MNCs were mitigated in the host countries. Using the combined datasets, I estimate α_1 and β_1 in equations (1) and (2). The summary statistics and correlation matrix of the variables used in the estimation are reported in Tables B1 and B2 in the Appendix, respectively.

4.2 Empirical results

Table 2 reports the estimation results of equation (1). Panel A presents the results using the number of COVID-19 cases and panel B presents the results using the number of deaths. The dependent variables, i.e. sales/employment/investment, are in y-o-y changes in columns (1)–(3) and in logarithms in columns (4)–(6) in both panels. In panel A, it is evident that COVID-19 has statistically significant negative impacts on the total sales and employment of Japanese MNCs in host countries. The magnitude of the impact is big. A 1% increase in the number of COVID-19 cases lead to a 2.2% decrease in sales y-o-y (column 1). Similarly, a 1% increase in the number of COVID-19 cases decreases

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Table 2. Impact of COVID-19 on Firm Performance

	(1) sales_yoy	(2) emp_yoy	(3) invest_yoy	(4) logsales	(5) logemp	(6) loginv
Panel A:						
COVID-19 cases (log)	-2.231***	-0.269	1.982	-0.033***	-0.003*	0.006
	[0.747]	[0.174]	[2.453]	[0.010]	[0.001]	[0.017]
R-sq	0.717	0.615	0.374	0.988	1.000	0.944
Panel B:						
COVID-19 deaths (log)	-1.569***	-0.097	3.691	-0.024***	-0.002*	0.017
	[0.569]	[0.143]	[2.789]	[0.007]	[0.001]	[0.018]
R-sq	0.702	0.606	0.392	0.987	1.000	0.945

Note: Country fixed effects and year-quarter fixed effects are included in all columns. The number of countries is 17 and observations is 119. The sample period is Q1 2019–Q3 2020. Robust standard errors are in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

sales by 3.3% (column 4) and employment by 0.3% (column 5). However, the impact on investment is not significant by the end of Q3 2020. This probably implies that it takes time for firms to adjust capital investment. Panel B shows that the number of deaths has similar impacts on firm performance. It is reasonable that compared with the number of COVID-19 cases, the magnitude is a little small. Given that the pandemic was accelerating over time in the world, the effects of COVID-19 on firm performance differ by quarter. To examine this difference, I interact the number of cases (deaths) with quarter dummy variables and set Q1 as the base quarter. As reported in Table B3 in the Appendix, the coefficients for COVID-19 cases and the interaction terms with Q2 dummy are significantly negative. Importantly, the magnitude of the interaction term with the Q2 dummy for sales is bigger, implying a significant increase in the negative effects on Japanese MNCs in Q2 2020.

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Table 3. Effect of Policy Responses on Firm Performance

	(1)	(2)	(3)	(4)	(5)	(6)
	sales_yoy	emp_yoy	invest_yoy	logsales	logemp	loginv
Panel A:						
Stringency index	-0.407***	-0.015	0.668	-0.006***	-0.001**	0.005
	[0.132]	[0.032]	[0.565]	[0.002]	[0.000]	[0.004]
R-sq	0.708	0.605	0.382	0.987	1.000	0.945
Panel B:						
Economic support index	0.002	-0.031*	-0.998*	0.000	0.000	-0.005*
	[0.089]	[0.017]	[0.512]	[0.001]	[0.000]	[0.003]
R-sq	0.713	0.611	0.572	0.985	1.000	0.955
Panel C:						
Government response index	-0.426**	-0.047	-0.136	-0.008***	-0.001**	0.001
	[0.197]	[0.049]	[0.425]	[0.003]	[0.000]	[0.005]
R-sq	0.693	0.609	0.369	0.987	1.000	0.944

Note: Country fixed effects and year-quarter fixed effects are included in all columns. The number of countries is 17 and observations is 119. The sample period is Q1 2019–Q3 2020. Robust standard errors are in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 3 shows the estimation results for equation (2). Panels A–C show the results for the stringency index in panel A, the economic policy support index in panel B, and the overall government response index in panel C. Panel A shows that the strict lockdown policies that primarily restrict people's movement have significant negative impacts on the total sales and employment of Japanese MNCs, but the impact on investment is not significant. Panel B shows that the economic support policies, such as income support and debt relief, do not have positive effects on firm performance. In fact, the effect on employment and investment are even negative in columns (2)–(3) and (6). The coefficients are close to zero in columns (1) and (4)–(5). This suggests that on average,

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Table 4. COVID-19, Policy responses, and Firm Performance

	(1)	(2)	(3)	(4)	(5)	(6)
	sales_yoy	emp_yoy	invest_yoy	logsales	logemp	loginv
Panel A:						
COVID-19 cases (log)	-1.899**	-0.239	2.817	-0.024***	-0.001	0.005
	[0.726]	[0.166]	[3.117]	[0.009]	[0.001]	[0.020]
Government response index	-0.192	-0.018	-0.484	-0.005*	-0.001*	0.001
	[0.192]	[0.048]	[0.601]	[0.003]	[0.000]	[0.006]
R-sq	0.721	0.616	0.377	0.988	1.000	0.944
Panel B:						
COVID-19 deaths (log)	-1.242**	-0.049	4.435	-0.017**	-0.001	0.018
	[0.550]	[0.143]	[3.186]	[0.007]	[0.001]	[0.020]
Government response index	-0.284	-0.041	-0.646	-0.006**	-0.001*	-0.001
	[0.198]	[0.050]	[0.563]	[0.003]	[0.000]	[0.006]
R-sq	0.711	0.609	0.398	0.988	1.000	0.945

Note: Country fixed effects and year-quarter fixed effects are included in all columns. The number of countries is 17 and observations is 119. The sample period is Q1 2019–Q3 2020. Robust standard errors are in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

the economic support polices in host countries are not likely to help Japanese MNCs in recovering from the COVID-19 crisis. Panel C shows that the effects of overall government policy responses are quite similar with those of lockdown-style policies in panel A. Since relative to economic support, the lockdown restrictions and closures have much larger impacts on firms, the overall effects of COVID-19 policies are negative.

To examine the relative effects of the pandemic and policy responses on firm performance during the same period, I include both COVID-19 measures and policy responses in the specification and reran the regression. Table 4 shows that the coefficients for COVID-19 cases and deaths are significantly negative in columns 1 and 4, whilst the coefficients for the overall government policy responses index are relatively small and

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not significant. This suggests that the effect of COVID-19 dominates the effect of policy responses during the sample period.

5. Concluding Remarks

Using aggregate-level data on Japanese MNCs collected by the Japanese government, I examined the impact of COVID-19 pandemic and policy responses on global production networks. Not surprisingly, COVID-19 had large negative impacts on global supply chains, firm performance, expectations, and the business plans of Japanese MNCs in major host countries. Relative to Q1 2020, Japanese affiliates in China recovered in Q2–Q3 but the effects of COVID-19 became worse in other major countries during the same period. Importantly, I did not find that overall COVID-19 policy responses in host countries have positive effects on firm performance. As the effects of the containment and lockdown policies dominated the effects of economic support polices, the overall effect was actually negative. However, stronger government policy responses are likely to improve firm expectations of their sales, employment, and investment, at least in the short term.

The ongoing COVID-19 pandemic will likely transform global production. UNCTAD (2020) shows that reshoring, diversification, and regionalisation will drive the restructuring of GVCs in the coming years. My findings provide some evidence-based policy implications for global production and the re-evaluation of supply chain strategy in the post-COVID era. To reduce the reliance on supply chains in China, in April 2020, the Japanese government approved a fiscal stimulus package including ¥220 billion (US$2 billion) for manufacturing firms to move production home and ¥23.5 billion ($0.2

billion) to move it to ASEAN countries. However, China has brought the spread of COVID-19 under control, and the supply chains and economic activities have recovered since Q2 2020. On the other hand, Japan, ASEAN, and the ROW were hit hard by the pandemic at the same time. Therefore, it is geographical diversification in sourcing and sales, not the reallocation of production and supply chains, that makes firms and the economy more resilient to supply chain disruptions and disasters. For example, it is estimated that the 2003 SARS epidemic reduced Chinese firm imports by 8% on average, but it was as much as 56% for firms without any diversification (Huang, 2017). Resilience in global supply chains can be increased through building buffer stocks and making standardised inputs easier to be replaced, identifying places and suppliers less subjective to risk, and assessing the time to recover for each type of supplier (Miroudot, 2020). Policies in the future should support business efforts to build more robust and resilient supply chains. Furthermore, mega free trade agreements, such as the newly signed Regional Comprehensive Economic Partnership in December 2020, are expected to promote trade, investment, and supply chain diversification in the post-COVID-19 world.

Finally, as this study uses aggregate-level data, there are many limitations to the analysis. When micro-level data is available, it will be interesting to investigate how did Japanese MNCs adjust their global production and whether they reorganise their global supply chains. To separate and estimate the supply shock and demand shock on global productions is also challenging. I leave these research questions as future work.

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Appendix A. Regions and Countries

The classification of countries and regions is based on the Quarterly Survey of Overseas Subsidiaries, Ministry of Economy, Trade and Industry.

China: CHN, HKG

ASEAN: BRN, IDN, KHM, LAO, MMR, MYS, PHL, SGP, THA, VNM

NIEs: KOR, SGP, TWN

North America: CAN, USA

Europe: AUT, BEL, BGR, CHE, CZE, DEU, DNK, ESP, FIN, FRA, GBR, GRC, HUN, IRL, ITA, LUX, MNE, ROM, NLD, POL, PRT, RUS, SVK, SWE, TUR, UKR

ROW: ARG, AUS, BRA, CHL, COL, CRI, EGY, GTM, ISR, KEN, MAR, MEX, NGA, NZL, PER, SAU, SLV, SWZ, TUN, URY, VEN, ZAF

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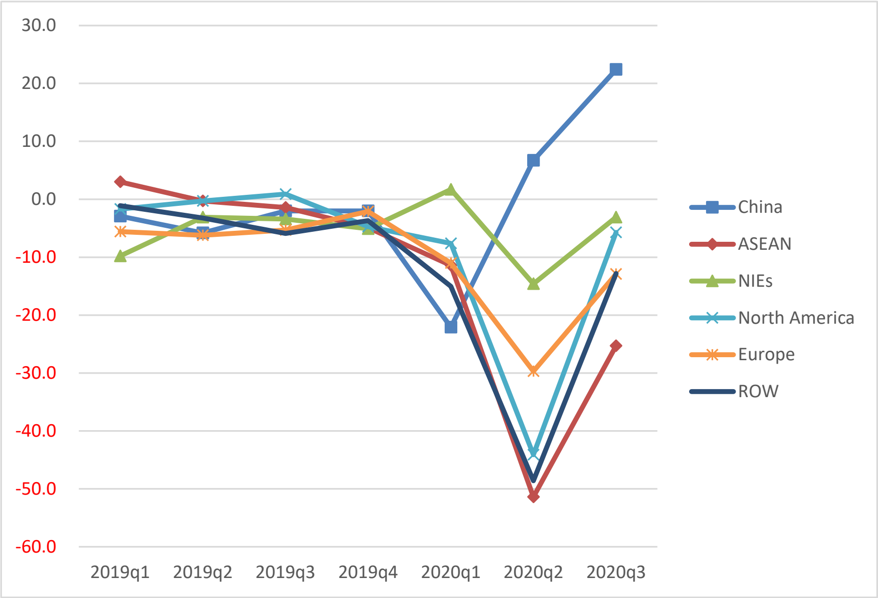
Appendix B. Figures and Tables

Figure B1. Local Sales by Region (%, y-o-y)



Source: Author's own compilation based on the Quarterly Survey of Overseas Subsidiaries, Ministry of Economy, Trade and Industry.

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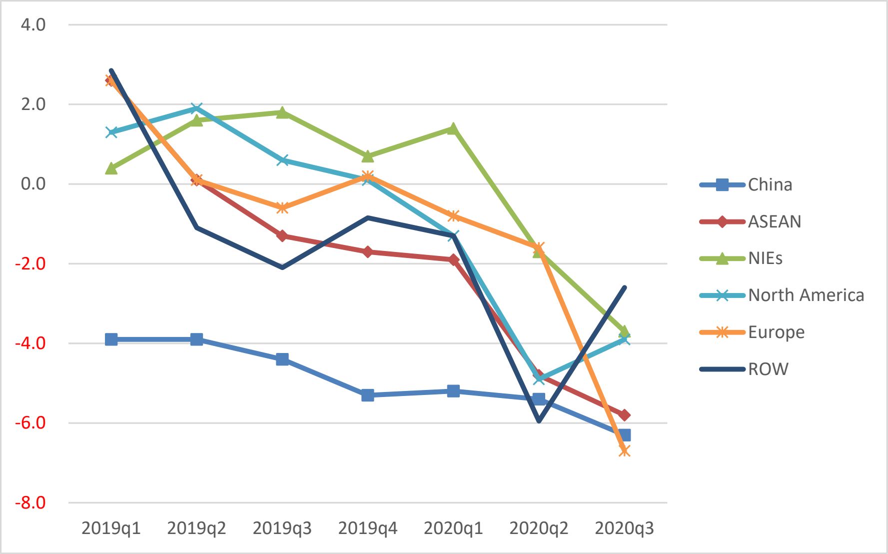
Figure B2. Employment by Region (%, y-o-y)



Source: Author's own compilation based on the Quarterly Survey of Overseas Subsidiaries, Ministry of Economy, Trade and Industry.

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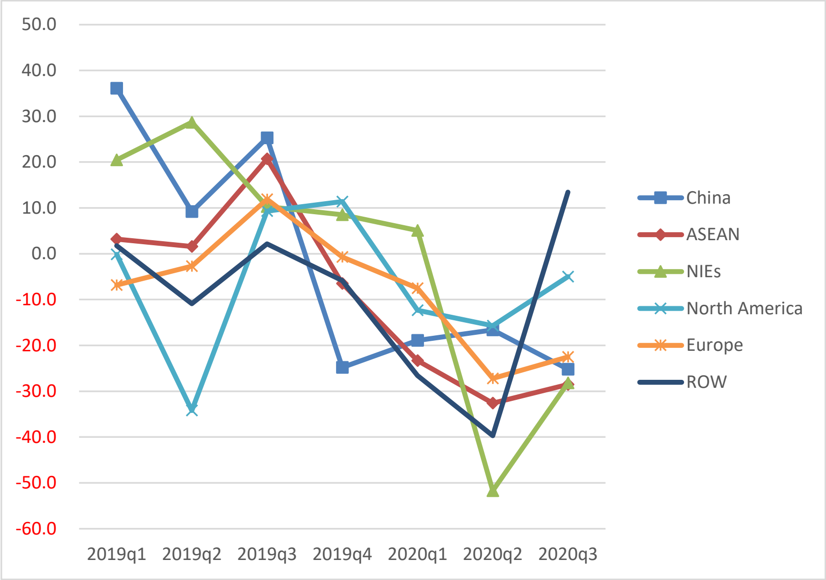
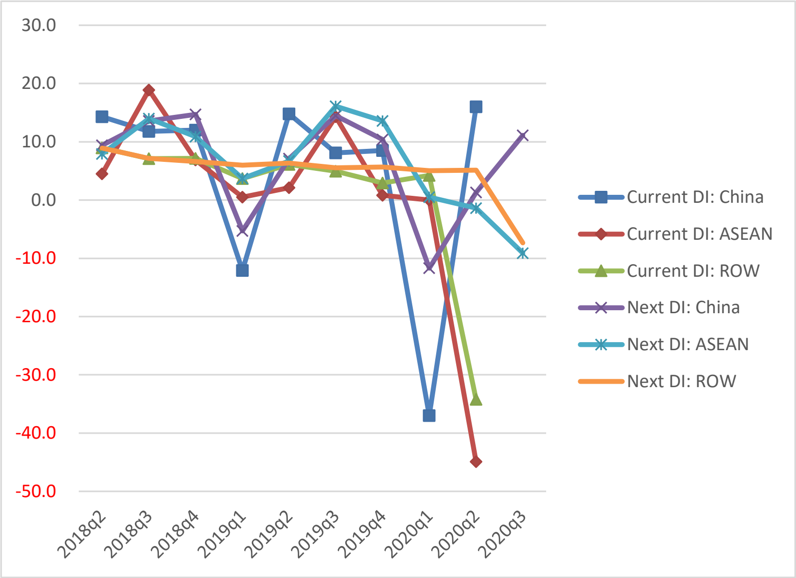
Figure B3. Capital Investment by Region (%, y-o-y)



Source: Author's own compilation based on the Quarterly Survey of Overseas Subsidiaries, Ministry of Economy, Trade and Industry.

Figure B4. Diffusion Index: Local Sales (percentage points)



Source: Author's own compilation based on the Quarterly Survey of Overseas Subsidiaries, Ministry of Economy, Trade and Industry.

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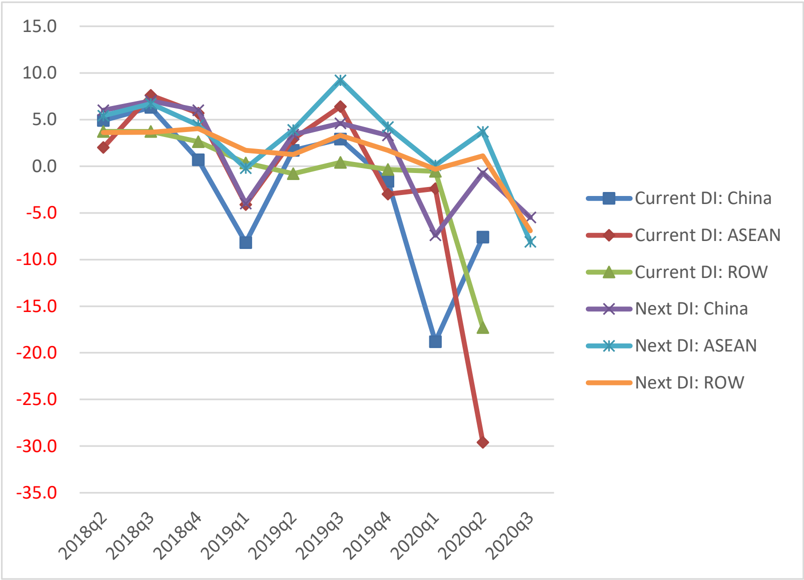
Figure B5. Diffusion Index: Exports to Japan (percentage points)



Source: Author's own compilation based on the Quarterly Survey of Overseas Subsidiaries, Ministry of Economy, Trade and Industry.

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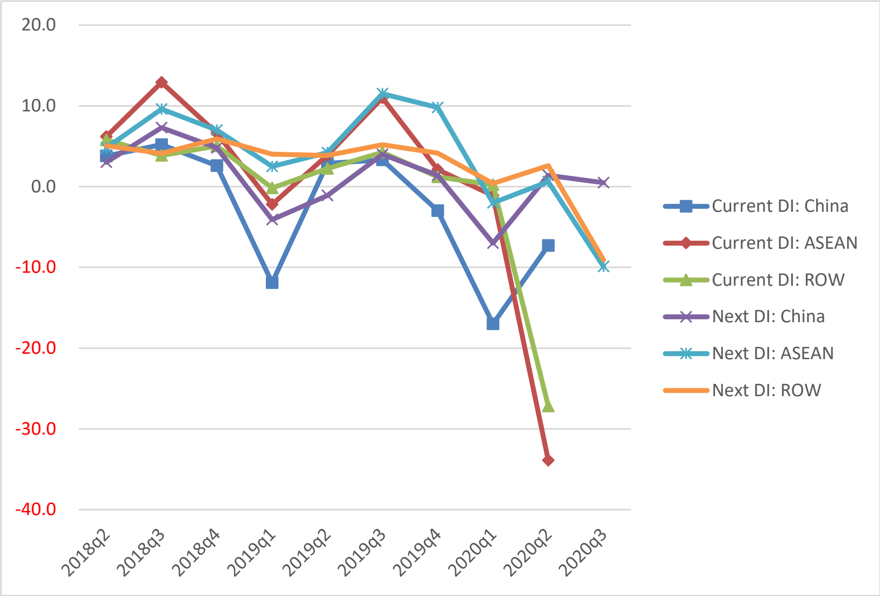
Figure B6. Diffusion index: Exports to Third Countries (percentage points)



Source: Author's own compilation based on the Quarterly Survey of Overseas Subsidiaries, Ministry of Economy, Trade and Industry.

Table B1. Summary Statistics

Variables	Obs	Mean	Std. Dev.	Min	Max
COVID-19 cases	119	179,565	774,840	0	5,727,091
COVID-19 deaths	119	5,372	18,733	0	123,547
sales_yoy (%)	119	-10.0	14.8	-72.2	18.0
invest_yoy (%)	119	-0.3	43.1	-64.8	239.6
emp_yoy (%)	119	-1.1	3.7	-14.5	10.3
logsales	119	13.5	1.0	12.1	15.9
loginv	119	10.1	1.0	8.1	12.5
logemp	119	11.8	1.1	10.1	13.9
Government response index	119	21.8	29.3	0	85.3
Economic support index	119	16.5	28.7	0	100
Stringency index	119	22.0	29.9	0	91.7

Source: Author's own compilation based on the data from the Quarterly Survey of Overseas Subsidiaries, Ministry of Economy, Trade and Industry, Johns Hopkins Coronavirus Resource Center, and Oxford COVID-19 Government Response Tracker.

Table B2. Correlation matrix

		1	2	3	4	5	6	7	8	9	10	11
1	COVID-19 cases	1.00										
2	COVID-19 deaths	0.85	1.00									
3	sales_yoy (%)	-0.19	-0.40	1.00								
4	invest_yoy (%)	-0.12	-0.04	0.27	1.00							
5	emp_yoy (%)	-0.15	-0.21	0.47	0.21	1.00						
6	logsales	0.10	0.08	0.19	-0.04	-0.14	1.00					
7	loginv	0.15	0.15	0.07	0.06	-0.17	0.92	1.00				
8	logemp	0.08	0.08	-0.03	-0.11	-0.31	0.84	0.87	1.00			
9	Government response index	0.38	0.46	-0.62	-0.31	-0.50	-0.12	-0.12	0.01	1.00		
10	Economic support index	0.33	0.39	-0.50	-0.36	-0.40	-0.14	-0.18	-0.10	0.85	1.00	
11	Stringency index	0.38	0.48	-0.66	-0.28	-0.49	-0.11	-0.09	0.05	0.99	0.79	1.00

Source: Author's own compilation based on the data from the Quarterly Survey of Overseas Subsidiaries, Ministry of Economy, Trade and Industry, and Johns Hopkins Coronavirus Resource Center, and Oxford COVID-19 Government Response Tracker.

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Table B3. Impact of COVID-19 by Quarter

	(1) sales_yoy	(2) emp_yoy	(3) invest_yoy	(4) logsales	(5) logemp	(6) loginv
Panel A:						
COVID-19 cases (log)	-0.793***	-0.365***	-1.978*	-0.012***	-0.001	-0.019*
	[0.291]	[0.108]	[1.092]	[0.003]	[0.001]	[0.010]
*Dummy (Q2=1)	-2.358***	-0.040	-1.754	-0.034***	-0.003**	-0.018
	[0.433]	[0.140]	[1.369]	[0.007]	[0.001]	[0.012]
*Dummy (Q3=1)	0.139	0.075	0.392	0.001	-0.003**	0.000
	[0.399]	[0.136]	[2.098]	[0.005]	[0.001]	[0.013]
R-sq	0.733	0.618	0.349	0.989	1.000	0.941
Panel B:						
COVID-19 deaths (log)	-1.527***	-0.540***	-3.545**	-0.021***	-0.001	-0.033*
	[0.437]	[0.161]	[1.597]	[0.006]	[0.001]	[0.017]
*Dummy (Q2=1)	-2.317***	0.046	-0.428	-0.038***	-0.003	-0.008
	[0.670]	[0.195]	[1.859]	[0.010]	[0.002]	[0.019]
*Dummy (Q3=1)	0.494	0.194	3.253	0.003	-0.003*	0.018
	[0.612]	[0.203]	[3.563]	[0.007]	[0.002]	[0.023]
R-sq	0.687	0.574	0.318	0.987	1.000	0.936

Note: Country fixed effects and quarter fixed effects are included in all columns. The number of countries is 17 and observations is 119. The sample period is Q1 2019–Q3 2020. Robust standard errors are in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

The COVID-19 pandemic's evolving impacts on the labour market: Who's been hurt and what we should do[1]

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In this paper, we shed light on the impacts of the COVID-19 pandemic on the labor market, and how they have evolved over most of the year 2020. Relying primarily on microdata from the CPS and state-level data on virus caseloads, mortality, and policy restrictions, we consider a range of employment outcomes—including permanent layoffs, which generate large and lasting costs—and how these outcomes vary across demographic groups, occupations, and industries over time. We also examine how these employment patterns vary across different states, according to the timing and severity of virus caseloads, deaths, and closure measures. We find that the labor market recovery of the summer and early fall stagnated in late fall and early winter. As noted by others, we find low-wage and minority workers are hardest hit initially, but that recoveries have varied, and not always consistently, between Blacks and Hispanics. Statewide business closures and other restrictions on economic activity reduce employment rates concurrently, but do not seem to have lingering effects once relaxed. In contrast, virus deaths—but not caseloads—not only depress current employment, but produce accumulating harm. We conclude with policy options for states to repair their labor markets.

1 This paper has been prepared for the conference on Uneven Outcomes in the Labor Market, organized by the Board of Governors of the Federal Reserve Bank, February 1, 2021. The authors thank Shane Reed and Steve Yesiltepe for capable research assistance; all errors are the authors' own.
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I. Introduction

The broad outlines of the effects of the COVID-19 pandemic on the US labor market have been known for months, and are apparent from the Employment Situation Reports published each month by the Bureau of Labor Statistics (BLS).

For instance, we know that the labor market experienced a very steep decline, beginning in March and sharply accelerating in April, with over 20 million jobs lost. The recovery began in May and picked up steam in June; employment growth remained strong in the summer, but monthly increases began diminishing in magnitude by the fall and flatlined after October.

Unemployment increased broadly in March and April, but the jump was especially steep for African Americans, Hispanics, and workers in retailing, leisure, and hospitality. Labor force participation also dropped and involuntary part-time employment rose. All of these measures began to show improvement in May, but at increasingly modest rates over the summer; and as of late fall, long-term unemployment rates have risen, as has the share—and number—of layoffs that are permanent.

Though these broad patterns are well known, many questions remain. For instance, to what extent are the worse employment outcomes that workers of color have experienced caused by their lower average educational attainment, their concentration in low-wage service jobs, or something else (perhaps discrimination)? As many indicators improve, but permanent layoffs and long-term unemployment rise, who is still showing progress, and on which dimensions—and who is suffering longer-term dislocations?

Most importantly, we know that the path of the COVID-19 virus has been quite nonlinear and uneven across states and regions, as have its labor market impacts. On the one hand, the shutdown in economic activity in March and April was truly national (Forsythe et al. 2020a), even though some states were hit harder than others (especially on the coasts and those with very large metropolitan areas like Chicago and Detroit). But the virus surged in some states (especially in the South and Southwest) over the summer, and then in the Midwest and Plains in the fall, while mostly staying under control in the states hit hardest earlier. Beginning in late October, cases began to rise nearly everywhere, and by the end of the year remained at record-high levels.

It is likely that this uneven virus path has affected labor markets differently across states and regions, as well as across occupations, industries, and demographic groups. Yet the published national data tell us little to date about these patterns or how they have changed over the past several months. Of course, COVID-19 papers have become something of a cottage industry among economists; a search of the term "COVID-19" on the NBER working papers website yielded 487 papers released between March 1 and December 15, 2020, at least 60 of which relate to labor markets, with most of these coming before the fall and focusing on the initial period of job losses rather than more recent trends.[1]

In this paper, we seek to shed light on how the impacts of the COVID-19 pandemic on the labor market have evolved over time. We pay particular attention to patterns of decline and recovery, with rapid and then slowing improvements, in different states. We investigate differing impacts on multiple employment outcomes across demographic and education groups as well as occupations and industries, and how these have varied from the spring to the fall as COVID case and mortality rates—and state restrictions on economic activity—have changed.

[1] Two exceptions are Gallant et al. (2020) and Forsythe et al. (2020b), both of which stress the unusually high share of temporary layoffs in the current recession as complicating standard job search models, but differing in interpretation of existing labor market slack and the likely rate of recovery. Neither focuses on subgroups or regional variation.

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We employ monthly microdata from the Current Population Survey (CPS) through December 2020, supplemented with other sources. After describing our data and our methods at greater length in the next section, we provide graphical (and tabular) time trends in key employment outcomes: in the aggregate, for different demographic and wage groups, and then separately by groups of states defined by the timing of peak virus caseloads. We then more systematically investigate the role of COVID-19 severity and economic restrictions on employment, allowing for contemporaneous and lagged effects. Finally, we summarize lessons learned and implications for employment policy in the months and years ahead.

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II. Data and Methods

We begin our analysis by compiling summary monthly data from the CPS through December 2020. Although several papers (e.g., Bartik et al. 2020, Cajner et al. 2020) have used alternative private-sector employment data from sources such as Homebase and ADP, the advantages of these data in timeliness and geographic detail come at the expense of representativeness and demographic detail, for which the CPS is still the gold standard. We limit our analysis to individuals aged 18–64 and focus on select, summary measures of employment—including an adjusted employment rate described below, the share of individuals reporting permanent job loss, and total weekly hours worked—although we also briefly report more conventional measures, such as labor force participation and unemployment rates.[2]

Our adjusted employment rate measure modifies the more typical employment rate (or employment-population ratio) to exclude individuals away from work for "other" non-

[2] We have calculated numerous additional measures, available on request, but we believe the ones described in the paper adequately summarize employment trends and their evolution during the pandemic.

specified reasons (e.g., besides vacation, own illness, personal leave, etc.). The share of workers absent from work for "other" reasons skyrocketed in April and has only gradually come down, and the Bureau of Labor Statistics believes most of these individuals should have been classified as unemployed (U.S. Bureau of Labor Statistics 2020). We further modify the employment rate to exclude individuals who report working part-time involuntarily due to economic conditions, either on a "usual" basis or specifically during the reference week of the survey. The adjusted employment measure thus captures changes in work at both the extensive and intensive margins.

As a related summary measure in aggregate analyses, we also analyze the total weekly hours worked for a group, which can capture more subtle hours changes than the adjusted employment rate. Finally, we regard the share of people (and not just of the unemployed) with permanent job loss as particularly important, since it is the best measure we have to date of long-term employment disruption associated with the pandemic, and research has shown the enormous social costs it imposes on workers (Davis and Von Wachter 2011).

We prefer these measures also because they are invariant to endogenous changes in labor force status, such as the (U3) unemployment rate (which is conditioned on labor force participation) or the duration of unemployment (which is conditioned on unemployment). However, we present some of these latter measures for comparison and completeness.

For all the graphs we present below, we first collapse the data to a month-group level and seasonally adjust by residualizing each series separately on calendar month dummies over the period 2015 through 2019. We then present the seasonally adjusted series running from January through December 2020. In some cases, especially when making comparisons across groups, we present trends that have been normalized (at 0) to

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respective January baselines. We present trends over 2020 in the aggregate and then separately for select demographic groups (race/ethnicity and gender) and occupation-based wage quartiles.[3,4]

We then turn to the geographic breakdown of employment changes, as defined by the time patterns of the COVID-19 caseload through December 2020. We group states into 3 categories: 1) those whose virus caseload peaked in the spring; 2) those whose caseload peaked during the summer months of June-August; and 3) those where the peak occurred after August.

This breakdown correlates only loosely with region. Viruses peaked in the spring in many coastal states, but also some Midwestern states, such as Illinois, Michigan, and Minnesota, with very large metro areas and airline hubs through which many travelers pass. Caseloads surged in many Southern or Southwestern states in the summer, but also peaked in Idaho, Nevada, and Ohio. In the fall, cases rose sharply in the upper Plains states, but also in Alaska, Indiana, Vermont, and West Virginia.[5] We consider the first category of states the most informative for measuring long-term unemployment or permanent job loss.

After presenting results graphically, we turn to regressions across first individuals and then states to examine how employment measures have evolved over time and subject to different sets of covariates.

[3] We merge occupational wage data from the Occupational Employment Statistics program (https://www.bls.gov/oes/home.htm) at the detailed occupation level and construct population-weighted quartiles.
[4] We have also examined trends in many additional demographic and job-characteristic groups, such as age, education, five categories of occupation, 12 categories of industry, and the Dingel and Neiman (2020) categorization of teleworkable jobs. Graphical trends for these groups are available upon request, but we omit them here for brevity.
[5] See Appendix Table 1 for the full list. States where cases peaked in the summer tended to be those that lifted restrictions in economic activity somewhat earlier than others, especially before the Memorial Day holiday weekend. Those peaking later also lifted restrictions earlier (or failed to ever implement them fully), and relaxed enforcement efforts in the late summer and around Labor Day weekend. For an analysis of employment trends by region see Crump et al. (2020).

More specifically, for individual-level regressions, we estimate coefficients on monthly time dummies, interacting these dummies with group identifiers in order to illustrate time trends separately by group. To understand the extent to which group-level differences in the education and occupational structure influence the patterns, we also estimate versions that control for education and occupational wage quartile categories, each interacted with the monthly dummies. In this latter case, the time interactions on the group indicators identify the differential time path of the group's outcome since January relative to the omitted group, net of the dynamics by education and occupation structure.

Formally, we run OLS regressions of the form:

$$y_{igt} = \boldsymbol{\alpha_t} + Black_{igt}\boldsymbol{\theta_t} + Hispanic_{igt}\boldsymbol{\delta_t} + \sum_{i=2}^{4} \beta_{it} \cdot wageq_{it} + \sum_{i=2}^{5} \gamma_{it} \cdot edu_{it} + \varepsilon_{igt} \quad (1),$$

where y_{igt} is the outcome—binary indicators for adjusted employment or permanent job loss, as well as weekly hours worked last week—for individual *i*, of group *j*, in month *t*, and the sample consists of the population age 18–64 from January through December 2020.[6] The vector $\boldsymbol{\alpha_t}$ is a sequence of monthly time dummies, ranging from February through December, with the omitted January serving as baseline. $Black_{igt}$ and $Hispanic_{igt}$ are indicator variables for membership in the respective groups, and $\boldsymbol{\theta_t}$ and $\boldsymbol{\delta_t}$ are the coefficients of interest, vectors of time dummies that capture the *differential* from $\boldsymbol{\alpha_t}$ (which represent the time path, relative to January, for non-Black, non-Hispanic individuals). In some specifications, we include the terms $\sum_{i=2}^{4} \beta_{it} \cdot wageq_{it}$ and $\sum_{i=2}^{5} \gamma_{it} \cdot edu_{it}$, which respectively capture the time dynamics (notice the t subscripts on β and γ) for occupational

[6] More accurately, y_{igt} is a seasonally adjusted measure of the outcome, in which we first run an ancillary regression of the outcome on only calendar month dummies (11, omitting April) in a sample that ranges from January 2015 through December 2020 but otherwise with the same sample restrictions as mentioned previously. We use residuals from these regressions as y_{igt}.

wage quartile and education categories.[7] In these cases, we are interested in how estimates for $\boldsymbol{\theta}_t$ and $\boldsymbol{\delta}_t$ change with the additional controls, which helps address the question of whether differences in employment trends for Blacks and Hispanics can be accounted for by salient human capital characteristics. The term ε_{igt} is an idiosyncratic error, which we allow to be heteroskedastic.

Turning to state-level regressions, we are interested in how outcomes at the state-month level evolve as a function of caseloads, death rates, and economic restrictions. We pay special attention to the possibility that these covariates can have enduring effects by allowing for their lags to enter the model.[8] Using aggregate rates of the same dependent variables as before, our regression focuses is of the form[9]:

$$y_{st} = \boldsymbol{\eta}_t + \tau \cdot caserate_{st} + \pi \cdot deathrate_{st} + \phi \cdot restriction_{st} + \epsilon_{st} \qquad (2),$$

where $\boldsymbol{\eta}_t$ is a vector monthly indicator variables (omitting January 2020) to capture national time trends in 2020, $caserate_{st}$ is the 14-day moving average of the number of newly diagnosed COVID-19 cases per 100,000 population in state s for month t, $deathrate_{st}$ is the 14-day moving average of the number of COVID-19 fatalities per 100,000 population, and $restriction_{st}$ is an index of state economic restrictions in effect in month t. Rather than include state fixed effects, we normalize y_{st} to be the difference from each state's January 2020 value. We take case rate and mortality data from the Economic Tracker of Opportunity Insights (Chetty et al. 2020; https://github.com/OpportunityInsights/EconomicTracker),

[7] The education categories are less than high school, high school graduate/some college, associate degree, bachelor's degree, advanced degree. In practice, we omit wage quartile 2 and high school graduate/some college; this choice does not affect $\boldsymbol{\theta}_t$ and $\boldsymbol{\delta}_t$, but does affect $\boldsymbol{\alpha}_t$.

[8] We have also estimated specifications with leads to allow for anticipation effects. These specifications yield qualitatively similar patterns and are available on request.

[9] We emphasize we use the population as the denominator for adjusted employment rates and permanent unemployment shares, and the natural log of the total weekly hours worked across individuals, not just across the employed.

which in turn takes in data from *The New York Times* and the COVID Tracking Project. We further smooth the 7-day moving averages reported there by additionally averaging over the 7 days of the week preceding the reference week of the CPS survey (the week containing the 12th of the month); this effectively creates a 14-day moving average.

Our policy restrictions come from Fullman et al. 2020 (available at (https://github.com/COVID19StatePolicy/SocialDistancing/tree/master/data). They provide the dates in which numerous state-level restrictions on economic and social activity are in effect. We focus on eight restrictions likely to affect economic activity—bar limitations, gathering restrictions, non-essential business closures, other business closures, mandatory quarantines, restaurant limitations, school closures, and stay-at-home orders—and code each as 0 or 1 based on whether the restriction is in effect as of the end of the reference week for each month's CPS survey. (If a restriction was eased but not removed, we code it as 0.5 for the month.) For simplicity, we then create an index by summing the restrictions in effect in each state for a given month, and then rescaling so that the index ranges from 0 to 1 across state-months.[10] Thus ϕ captures the effect of moving from no restrictions to the most restrictive state-month.

In related specifications, we modify (2) to also include both one-month and two-month lags of each covariate, as well as cumulative measures of each covariate. These specifications allow the influence of COVID conditions and policies to accumulate over time. All these state-level regressions rely on cross-sectional state variation in these covariates to capture evolution in different labor market measures. Because we use state-month averages, we weight each cell by the number of observations contributing to it (down-

[10] We have also created an index using a polychoric factor matrix, essentially a generalization of principal components to include categorical as well as Gaussian latent variables. This approach effectively adds the orthogonal components of each of the eight restrictions. Our results are similar using this measure.

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weighting small cells with imprecise averages), and we cluster ϵ_{st} at the state level to allow for arbitrary autocorrelation. We run these regressions on March through December 2020 to ensure variation in the covariates, as well as allow lags to reach to earlier months.

III. Employment Trends in 2020

A. Aggregate and by Demographic or Job Categories

Figure 1 presents aggregate employment trends through December 2020, with employment measured in a variety of ways. In part A, we present traditional measures such as labor force participation, the employment-to-population ratio, and the unemployment "share" (measured relative to the overall population rather than the labor force). In part B, we refine our measures of employment to exclude those missing from work (for "other" reasons) or those working part-time involuntarily, and also present total hours worked. In part C, we present the trend in permanent job loss, either relative to the unemployed or to the total population; while in part D we present the median duration of unemployment among those who report being unemployed.

The results in part A for the three most traditional employment measures illustrate an aggregate pattern that is, by now, well known: the rise in unemployment (and declines in employment and labor force participation) reached their extremes in April, recovered fairly rapidly in May and June, improved more slowly from July through October, and were essentially stagnant by the end of the year. For instance, the employment-population ratio declined from 76 percent in February to 64 percent in April, before recovering to about 68 percent in June and to 72 percent by October through December.

Part B illustrates that the temporal patterns of employment decline and recovery are similar when we exclude those missing from work for "other" reasons (solid blue line) and

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involuntary part-time workers (dashed red line), though the magnitude of observed employment loss rises (and that of the recovery shrinks) when we implement these exclusions. For instance, excluding the "other" absent and involuntary part-time workers reduces the employment-population ratio from 64 to 56 percent in April and from 72 to 68 percent in October through December. (The latter gap is more than twice as large as in February.) The pattern we observe in total hours worked, our single most comprehensive measure of employment, is also similar as well, and this measure as of October remains roughly 7 percent below its February level.[11]

Part C of Figure 1 illustrates the temporal pattern of permanent job loss during 2020. We present two measures: one where workers with permanent job loss are measured as a fraction of the unemployed (as often done in the BLS Employment Situation Reports), and another where they are measured relative to the population. Both show large increases in such employment loss since April, though the patterns differ in the early months of the year: as a share of the unemployed, the rate *falls* between February and April, since so many temporary layoffs occurred then, and then rises afterwards.[12] As a share of the population, however, they grow nearly monotonically over time. Permanent job loss increased to over 1.5 percent of the population (and nearly one-third of the unemployed) by October and November, before dipping slightly in December.[13] These patterns illustrate the large and lasting economic and social costs that the pandemic has already and likely will continue to impose on U.S workers.

[11] We see a modest and temporary dip in hours during the month of September, perhaps associated with the school year beginning with unanticipated ongoing closures. This dip also appears in Donovan and Labonte (2020).

[12] Forsythe at el. (2020b) and Gallant et al. (2020) find little evidence that individuals with temporary layoff transition to permanent unemployment. Rather, the increase seems to come directly from the employed.

[13] The permanent job loser share was last at this level in early 2014 but peaked at almost twice this level in early 2010, the trough of the Great Recession.

Part D of Figure 1 then confirms this pattern by presenting the median duration of unemployment in weeks (measured only for those unemployed). The pattern is similar to the one we observe for permanent job loss among the unemployed: median weeks initially fell in April, as many workers lost their jobs, but then rose consistently over subsequent months until declining slightly near the end of the year.[14]

In Figure 2 we present trends in employment over 2020, broken down by key worker wage or demographic categories: occupational wage quartile (part A), race/ethnicity (part B), or gender (part C). We use our most restrictive dichotomous measure of the employment rate (or most inclusive measure of nonemployment), that which excludes those not at work for "other" reasons and those working part-time involuntarily.[15] Part A of Figure 2 shows dramatic and consistent differences in employment patterns by wage quartile, with both the greatest employment losses and the slowest recoveries occurring among the lowest-wage workers. Specifically, we find relatively modest employment losses in the highest wage quartile by April (96 to 86 percent), with most of the lost employment recovered by December (back to 94 percent). In contrast, we observe dramatically larger employment loss by April for the lowest quartile, which declines from 85 percent in February to 51 percent two months later, before recovering to 75 percent in October and November, and then dipping slightly for the first time in December—marking an 11-percentage point gap from pre-pandemic levels. Such differences in both initial and

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[14] The effective exhaustion of additional unemployment benefits in the later months of the year—both the automatic Extended Benefits programs and the Pandemic Emergency Unemployment Assistance program authorized by the CARES Act, which provided an additional 13 weeks of benefits—complicate the interpretation of this decline, as people may have shifted from reporting unemployment to reporting not being in the labor force.

[15] We present analogous graphs of total hours worked in Appendix Figure 1.

lasting employment loss between the highest- and lowest-wage workers are almost certainly unprecedented among U.S. recessions over the past 100+ years.

Part B of Figure 2 also illustrates dramatic differences in employment patterns by race and ethnicity, with workers of color showing both the largest initial and lasting employment losses. Among white workers, the adjusted employment rate drops from about 76 to 59 percent by April, and then recovers by October to 71 percent, where it stayed for the next two months. In contrast, employment rates among Blacks and Hispanics drop from about 70 and 73 percent to 52 and 50 percent respectively by April, and recover to only 63 and 65 percent by October, with the rates for Blacks staying stagnant over the last two months of 2020, and the rate for Hispanics slipping a percentage point in December. The relatively larger employment losses among both minority groups in part results from their greater concentration in the lower-wage service jobs that have been hit so hard by the pandemic-induced recession. The job loss among Hispanics remains large, even though their employment rate has almost reverted to its usual higher *level* relative to Blacks.

Finally, part C of Figure 2 presents employment patterns by gender. Though employment is consistently lower among females than males, the magnitudes of loss and recovery are quite similar between the two. This is consistent with what we have learned from published BLS numbers over time—the rise in unemployment has been slightly smaller among women, while their drops in labor force participation have been slightly larger. Although women are more concentrated in lower-wage quartiles than men (a consequence of occupational and industry gender segregation), their employment losses within these groups are slightly smaller.

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In Figure 3, we present the share of the population reporting permanent job loss by wage quartile (part A), race/ethnicity (part B), and gender (part C).[16] As expected, permanent job loss is substantially higher in the lowest compared to the highest wage quartile; indeed, at the October peak, such losses as shares of the population reached nearly three times as high among the bottom quartile as among the top quartile (0.032 vs. 0.012). This measure has come down slightly, especially in December, for all groups—likely a result of exhaustion of unemployment benefits and respondents changing their reporting from unemployed to out of the labor force, especially since part A of Figure 2 did not show appreciable gains in the employment rate. Nonetheless, the relative magnitudes of permanent job loss have changed little across the wage quartiles.

Permanent job loss as of December is also substantially higher among Black workers (0.022) than white workers (0.012), although the gap is smaller for Hispanic workers (0.017) and has exhibited less increase since the summer. In accordance with the labor force participation gender differential discussed above, the permanent job loser share is also larger for men (0.016) than women (0.013), and this gap had been steadily widening until December.

Finally, Table 1 presents a more complete breakdown of employment losses and recovery across a more complete range of demographic and job categories. We show the adjusted employment rate in February, April, June, October, and December for demographic groups (Part A) and job categories (Part B).[17]

[16] From here onward, we do not present the graphs of unemployment duration across groups, since these follow relatively similar patterns to what we observed in Figure 1d, and they are harder to interpret as the share of the unemployed changes.

[17] Appendix Tables 2A and 2B present analogous estimates for the share of the population with permanent job loss.

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Beyond the differentials across wage quartiles, race and ethnicity, and gender illustrated in the figures, the results in part A of Table 1 show relatively larger losses among younger workers and less educated workers (but also somewhat faster recovery). Indeed, for 18–24-year-olds, the employment rate in April had fallen to just about three-fifths of its level in February, and even by December remained 10 percent (6 percentage points) below its February level; employment among older workers fell by less than one-quarter at trough and was down 7–8 percent down by December. Similarly, employment rates among those with high school education or less fell by more than 30 percent by April and remain depressed by 10 percent in December, while the relative losses of those with at least a bachelor's degree are much smaller. Somewhat ominously, and congruent with the K-shaped recovery noted by many others, the employment rates continued to improve slightly between October and December for those with at least a bachelor's degree, while falling slightly for those with less education.

The results in part B of the table clearly show which job categories have borne the greatest brunt of job loss. By occupation, the losses (both by April and later) are greatest in the low-wage services and least among professional and managerial workers. By industry, losses are greatest in the "arts, accommodation, and food services" and "other services" categories (which mostly includes lower-wage personal services jobs rather than professional, business, health, or education services). However, they are also high, especially initially, in trade and construction, likely reflecting differing degrees of customer or coworker contacts. While these latter sectors have recovered about in line with average, the former two remain substantially depressed in December, with employment rates roughly 20 percent below February levels. Furthermore, these two industries also showed among the largest drops in employment rates between October and December.

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Finally, changes in employment rates are dramatically different by the extent to which work can be done remotely: those who cannot easily do so lose about one-third of employment in April and are still down by one-tenth by December, whereas among remote workers the losses are closer to one-sixth and one-twentieth, respectively.

The patterns of greatest long-term employment loss among the most vulnerable workers—those with the least education, disproportionately people of color, and in the lowest-wage job categories—remain clear no matter how we slice the data.

B. Employment Patterns by States: Categorized by COVID-19 Caseload Patterns

Since the timing of COVID-19 caseloads varies greatly across states, it stands to reason that employment patterns could also vary across states. We therefore consider employment rates and permanent job loss shares (out of the population) across our three (population-weighted) categories of states: 1) those where caseloads peaked in the spring, mostly in April and May; 2) those where cases peaked in the summer months, between June and August; and 3) those peaking (or still climbing) in the fall.

Figure 4 shows the trend throughout 2020 in our broadest employment variable (the employment rate excluding workers absent for other reasons and those involuntarily employed part-time) for each of the three state groups. To facilitate comparison, we have normalized each state group to its own January 2020 level.

All groups share a basic pattern of dramatic declines in employment in March and especially April, followed by rapid recoveries in May and June that flattened somewhat in subsequent months. However, while employment rates dropped dramatically everywhere, they did so somewhat more in states with spring and summer peaks than those with fall peaks. Additionally, while employment rates rebounded quite sharply everywhere beginning

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in May, the recovery was slightly slower over the summer in states with spring caseload peaks. We find some convergence of employment rates across groups in late summer and fall, as employment growth flattened during the latter seasons more in states with later caseload peaks. Nonetheless, the states with the latest case peaks have on average the smallest reduction in employment rates by December (although this could still change over the winter).

Of course, it is not possible to determine exactly what caused the greater decline in employment in the states with earlier peaks or the convergence later, though in both cases it is likely linked to trends in COVID-19 cases. For instance, to what extent was the steeper decline in employment for the first two categories of states driven by the worse caseloads *per se* during those times, by stricter shutdowns (and later relaxations), or by customers themselves choosing to venture less frequently to shops or leisure and hospitality venues? Goolsbee and Syverson (2020), using cell phone mobility data, find evidence suggesting the latter channel was more important during the pandemic's initial months, but it is still an open question to what extent public messaging and actual shutdowns were more serious and longer-lasting in states with spring peaks (which tend to be Democratic leaning) than those with summer or fall peaks (which tend to lean Republican).

Since employment rebounded fairly rapidly in all three areas beginning in May, but from different troughs and with some convergence over time, we need to consider the variance in long-term employment damage, as represented by permanent job loss, across the three state categories. Figure 5 presents the trends over time in permanent job loss as a share of the total population, in each of the three categories of states and normalized (at 0) to each state group's January level.

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The results show substantially more reported permanent job loss in states with spring caseload peaks than in those with summer or fall peaks. Interestingly, although these shares fell slightly at the end of the year for the summer and fall peak states, that for spring peak states has barely budged. Nonetheless, the rapid rise all three state groups experienced in late summer have largely persisted.

Finally, we measure trends in permanent job loss across a few key occupational and demographic breakdowns for the states with the earliest caseload peaks. In part A of Figure 6 we present these trends for the highest and lowest wage quartiles, while in part B we do so by race and ethnicity. In both cases, but especially for the lowest wage quartile and for Hispanics, permanent job loss rises substantially in the states where caseloads peaked earliest. And disparities in such job loss across wage quartiles and racial groups remain dramatic, even within the group of states with the earliest peaks.

C. Regression Analysis

In the second part of the paper, we adopt a more systematic approach and investigate labor market trends for different groups as a function of secular time trends, state policies to restrict (or relax) economic activity, and cumulative measures of COVID diagnoses and mortality.

We begin with estimation of equation (1), designed to measure what accounts for differences across racial groups in their employment responsiveness to the pandemic, before moving to a fuller consideration of how COVID-19 caseloads and state actions affect employment trajectories (equation 2).

In Table 2, we present results from linear probability model estimation of equation (1) on individuals. We present coefficients on monthly dummies from March through

December 2020, with January as the reference group. In these regressions, we include interactions of month dummies with indicators for being Black or Hispanic. We first run the equations without and then with interactions between time dummies and indicators for education and wage quartiles (with high school/some college degree and the second quartile as reference groups, respectively). Comparisons between the first and second specifications then indicate the extent to which education and wage quartile account for the relatively more negative employment trends we observe for Blacks and Hispanics in 2020.

The first six columns of Table 2 present estimates for the adjusted employment rate (excluding those absent from work for other reasons and those working part-time involuntarily, in consecutive pairs for the overall time trends, Blacks, and Hispanics. The overall time trends reflect the progression for non-Blacks and non-Hispanics, while the columns for Blacks and Hispanics represent the deviation from the overall trend. The first column of each pair omits the education and wage quartile time interaction controls, while the second column includes them. The next six columns are similar but have as the dependent variable an indicator for permanent job loss. (Appendix Table 3 presents results for total hours worked, including 0s.)

The results of Table 2 mostly recreate what we observed in the figures above, except that we can now see the extent to which education and occupational wage quartile account for the differential time patterns by race. The overall coefficient estimates, both without and with controls (columns 1 and 2), show dramatic employment declines in April and then initially strong but slowing recovery afterwards. The estimates in columns 3 and 5 show that Blacks and especially Hispanics suffered relatively greater employment declines in April and May. Although Hispanics were recovering more quickly than Blacks over the summer and into the fall, this pattern appears to have reversed by the end of the year. By December,

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Blacks were not statistically behind in employment rates—relative to their own January baseline—than the overall trend, but Hispanics had slipped further behind. Controlling for education and wage quartile dynamics (columns 4 and 6) reduces by roughly half the initially larger employment declines for Blacks and Hispanics, but these controls play a smaller role in later months. These patterns are remarkably similar (albeit reversed in sign) for the permanent job loss share in columns 7 through 12, down to the differentials in recovery between Blacks and Hispanics.

To summarize, most racial groups demonstrate at least partial recovery from initially large declines in employment, but as of December, while Blacks have converged with the overall population, Hispanics have not. The ongoing disadvantage for Hispanics (and the earlier disadvantages for both Blacks and Hispanics) is *not* mostly driven by differences in education or concentration in low-wage jobs. Commensurately, permanent job loss rises for all groups, but especially for Hispanics and Blacks.

Shifting to state-level regressions and the role of COVID cases, mortality, and state policies, we present summary statistics of these covariates (as well as for the dependent variables of the adjusted employment rate and permanent job loss share) in Table 3. There is substantial cross-state and within-state variation in these covariates—indeed, although it is not shown in the table, many states have non-monotonic trends in both case and mortality rates, as well as state restrictions (and in outcomes, as we have already seen).

In Table 4, we present estimates of equation (2), where the data are a panel of states over the months in 2020; we are interested how case rates, mortality rates, and an index of state restrictions affect the adjusted employment rate (columns 1 through 4) and the share of the population reporting permanent job loss (columns 5 through 8). For each of these outcomes, we present estimates for four versions of equation (2). In the first, we use the

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contemporaneous rates of new caseloads, deaths, and the restrictions index; in the second, we add one-month lags of all three covariates; in the third, we use both one-month and two-month lags to capture additional accumulation; and in the fourth, we replace the lags with total cumulative versions of the same variables.

The first column of Table 4 shows that the current mortality rate and economic restrictions index are negatively associated with the adjusted employment rate, although the current case rate has a *positive* association. The latter relationship may stem from the high correlation of case rates and mortality rates (r = 0.63) as well as short- term tradeoffs: heightened economic activity correlated with greater employment but also greater virus transmission.[18] To interpret magnitudes, we can consider changes of one standard deviation in each covariate (Table 3). For the new case rate, such an increase implies a rise in the adjusted employment rate of about 1.8 percentage points; for the mortality rate, it implies a decrease of about 1.1 percentage points; and for the restrictions index, it implies a decrease of approximately 1.6 percentage points. If all three were to increase by one standard deviation, the adjusted employment rate would be expected to drop by about 0.9 percentage points, or about 16 percent of the gap between February and October (Table 1A).

Column 2 adds one-month lags of each covariate. While the overall picture changes little, the combined coefficients on the mortality rate and its lag are larger than the contemporaneous coefficient in column 1, suggesting that mortality rates have an accumulating effect in depressing employment rates. In contrast, the lagged economic restrictions index is much smaller in magnitude than its contemporaneous coefficient and

[18] The estimate on contemporaneous case rates is weaker when entered as a single regressor, and, unlike contemporaneous mortality rates and restriction indices, statistically insignificant if the surge periods in November and December are excluded.

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not statistically significant, suggesting that the impact of past restrictions is relatively short-lived. The specification in column 3 adds an additional lag for each covariate. These two-month lags are statistically significant (marginally for the restrictions) and of larger magnitude than the one-month lags, with the same sign as the contemporaneous effects. These patterns could imply longer-term accumulation of the impact of the public health indicators on employment rates, but they could also capture possible nonlinearities.[19]

Thus, we turn to the estimates in column 4, which replace the lags with cumulative measures. We find that cumulative mortality rates reduce employment rates independent of current mortality rates (and in magnitudes, by a similar margin), while cumulative case rates and economic restrictions have little effect. This suggests that mortality rates inhibit employment well into the future but that case rates and economic restrictions, while possibly having nonlinear contemporaneous impacts (especially with the surge near the end of 2020), are less likely to cause labor market hysteresis.

The second four columns of Table 4 repeat the analysis but with the permanent job loss share as the outcome. Because this measure has been slowly but steadily increasing over time, it is perhaps not surprising that contemporaneous measures of case rates, mortality rates, and economic restrictions—which both rise and fall over the sample period—are only weakly associated with it. However, the one- and two-month lagged mortality rates (columns 6 and 7), as well as the cumulative mortality rate (column 8) both strongly predict increases in the share of the population with permanent job loss, as they did with employment rates.

[19] For example, the acceleration of mortality (a quadratic term) could influence mortality rates in the next period but also plausibly affects business and worker decisions contemporaneously. Unfortunately, with such a short panel, we lack the statistical power to test these hypotheses.

Magnitudes are relatively large, as well. A one-standard deviation increase in the (lagged) mortality rate induces a decline in employment rates of between 1.6 and 2.2 percentage points. The same shock leads to an increase of between 0.09 and 0.14 percentage points in the permanent job loss share—up 13–21 percent from the mean of 0.67 percent. A one-standard deviation increase in the cumulative mortality rate as of December (which is right-skewed) implies a decrease of about 1.2 percentage points in the adjusted employment rate and an increase of about 0.18 percentage points in the permanent job loss share, or more than 25 percent of the mean. Evidently, the static tradeoffs between lives and jobs postulated by some early commentators (The Economist, 2020) have considerably more complicated dynamics.

IV. Conclusion and Policy Implications

In this paper, we have used CPS microdata, supplemented with COVID case and mortality data and state economic restrictions data, to analyze how employment trends through October 2020 reflect the recession induced by the COVID-19 pandemic. We have presented these trends in graphical and tabular forms, using several (somewhat novel) measures of employment outcomes. We have analyzed these trends in the aggregate and separately by demographic as well as occupation groups. We have also estimated distributed lag regressions to shed greater light on these processes and what drives them. Our major findings can be summarized as follows:

- While employment fell dramatically in the spring of 2020 and recovered substantially thereafter, this recovery stalled after October and even deteriorated a bit (especially for Hispanics and in arts/accommodations/food and other services);

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- We observe rising unemployment durations and increasing shares of permanent job loss through the fall, indicating the pandemic's longer-term damage to workers;
- Workers in the lowest wage quartiles or education groups, those of color, and those working in lower-paying service occupations and industries have suffered the greatest longer-term losses in all measures of employment and, especially for Blacks, education and occupational differences mostly do not explain their relatively worse outcomes;
- While all states have endured substantial employment disruptions, states with earlier peak virus caseloads and deaths have had worse employment disruptions that have persisted; and
- While caseloads *per se* do not seem to have much impact on employment measures, contemporaneous economic restrictions and mortality rates do, and although the effects of the former fade once restrictions are eased, the effects of past mortality rates accumulate.

Of course, the reemergence of the virus in the fall and especially the winter will no doubt have lasting labor market implications as well. Employment levels will likely stagnate or deteriorate (as they did in November and December), and may decline further this winter; low-wage workers and those of color will likely bear the greatest brunt of any such developments, and permanent job loss (especially accounting for those who have left the labor force) will likely continue to grow. In addition, new patterns of employment gains and losses across states may emerge, depending on when and where mortality rates rise the most and the degree of new (or renewed) economic restrictions. On the other hand, the development and distribution of effective COVID-19 vaccines should lay the groundwork for more solid labor market recovery to begin in 2021, although if past experience from recessions is any guide, it may be a long haul.

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In the meantime, we consider the implications of our findings for policy, which has the potential to shorten that long haul. We believe labor market recovery efforts should include the following:

- Ongoing relief and stimulus efforts while unemployment remains high, including fiscal relief to state and local governments;
- Efforts to spur more rapid employment growth through public spending on infrastructure, subsidized jobs, and perhaps marginal employment tax credits;
- Upgrading workforce development services at community colleges and American Job Centers to help the long-term unemployed and permanent job losers (as well as essential low-wage workers who are employed) retrain and find well-paying jobs;
- Wage supplements or wage insurance for those who either remain in low-wage essential jobs or now have to take them after permanently losing better-paying jobs; and
- Targeting all such efforts on the demographic groups and states hardest hit by the pandemic.

Our nation's infrastructure needs are great, and investing in repairing our infrastructure enjoys bipartisan support (though large disagreements remain about exactly how to finance it, even with negative real interest rates that should encourage borrowing). The workers hardest hit by the pandemic should be given special access to any jobs created, and training them for the appropriate construction skills should be a high priority. Construction apprenticeships might be a particularly useful vehicle for skill training while workers are employed (National Skills Coalition 2017), so as not to slow the recovery process.

Tax credits for marginal employment growth—in other words, growth above some expected baseline level—have sometimes been used in previous recessions, and with some

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effectiveness (Neumark and Grijalvo 2016). Targeting such tax credits to the states hardest hit also makes sense economically (T. Bartik et al., 2020), though the politics of such targeting can be challenging. Subsidized public or private sector jobs for disadvantaged workers with permanent job loss should be part of the policy mix, as well (Roder and Elliott, 2013).

Our nation's workforce development efforts must also be strengthened to help workers retrain for new work and/or find new jobs. Support for workforce training and services can take a number of forms. For instance, a major one-time injection of dollars into programs funded by the Workforce Innovation and Opportunity Act (WIOA) is certainly warranted and has been proposed.[20] New funding for individual training accounts for low-wage and/or unemployed workers has also been proposed, as has block grant funding for community colleges and other providers of workforce training.[21]

More ambitious ideas, like a "GI Bill" for essential low-wage workers, have been discussed as well, and even implemented to some extent in Michigan (Jesse 2020). And there have been proposals for wage supplements for low-wage "essential workers" (Nunn et al. 2020) as well as more traditional calls for wage insurance for those displaced from better-paying jobs than the new ones with which they are replaced (Wandner 2016).

Whichever path is chosen, it is important that those hardest hit by the pandemic and recession—including those displaced from low-wage jobs—get both training and workforce services to help them regain employment, ideally at higher wages than before. Unlike previous recessions or other periods of structural change, when somewhat more skilled or

[20] For instance, Rep. Bobby Scott (D-VA), chair of the House Committee on Education and Labor, has proposed an injection of $15B into the WIOA system through the Relaunching America's Workforce Act (RAWA).
[21] See the Markle Foundation's proposal (2020) for Opportunity Grants for disadvantaged and unemployed workers, as well as the Aspen Institute's Economic Strategy Group report (2020) calling for block grant funding to public higher education institutions, including community colleges.

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higher-wage workers (in manufacturing and other industries) have been displaced, this time these workers are especially disadvantaged to begin with. Making the best training programs, as identified in rigorous evaluations, available to these groups at scale should be high on policymakers' agenda.[22]

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[22] The strongest impacts on earnings to date for low-wage workers have been observed in "sector-based" training programs, like Per Scholas and Project Quest. See Roder and Elliott (2019) and Schaberg (2017). For a discussion of how to scale up such programs see Holzer (2021).

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Figure 1A: Aggregate Employment Trends in 2020: LFP, Epop, and Unemployment Share

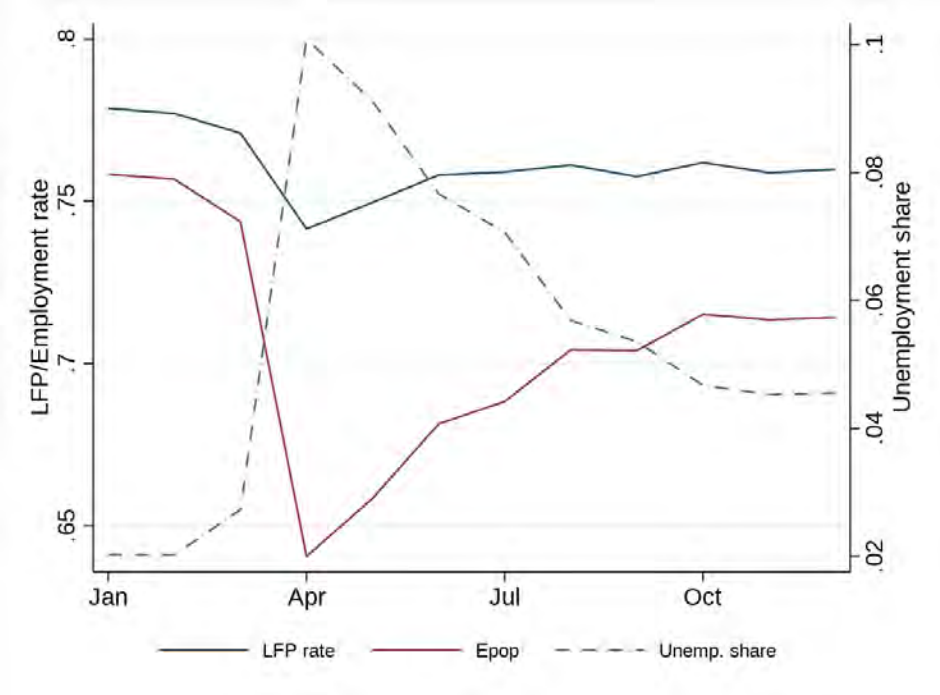

Figure 1B: Aggregate Employment Trends in 2020: Adjusted Epops and Total Weekly Hours

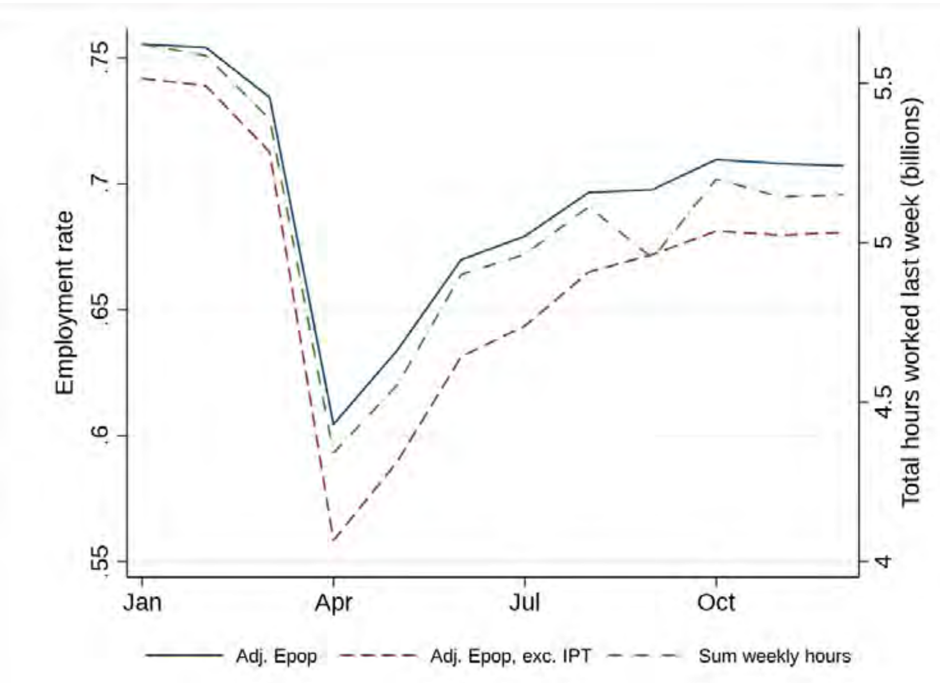

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Figure 1C: Aggregate Employment Trends in 2020: Permanent job loser share/rate

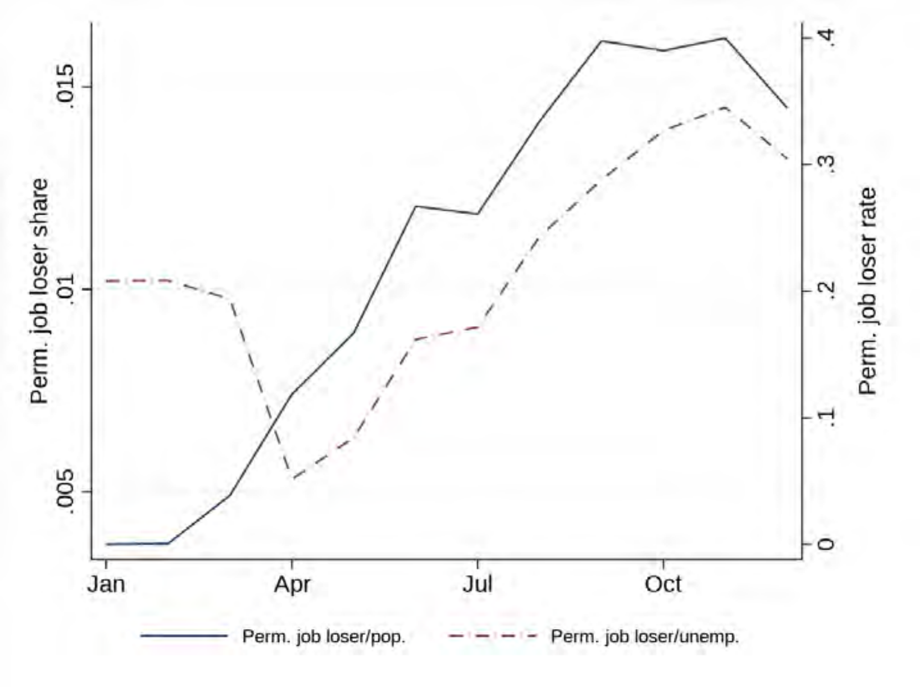
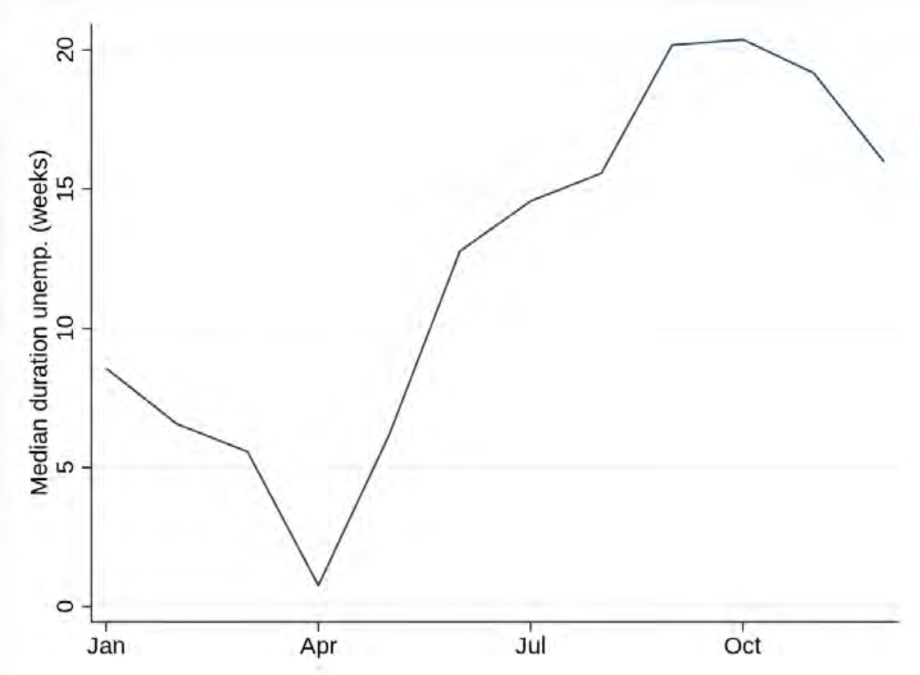


Figure 1D: Aggregate Employment Trends in 2020: Unemployment Duration



Note: See text for definitions. All series have been seasonally adjusted as described in the text.

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Figure 2A: Adjusted Employment Rates by Occupational Wage Quartile

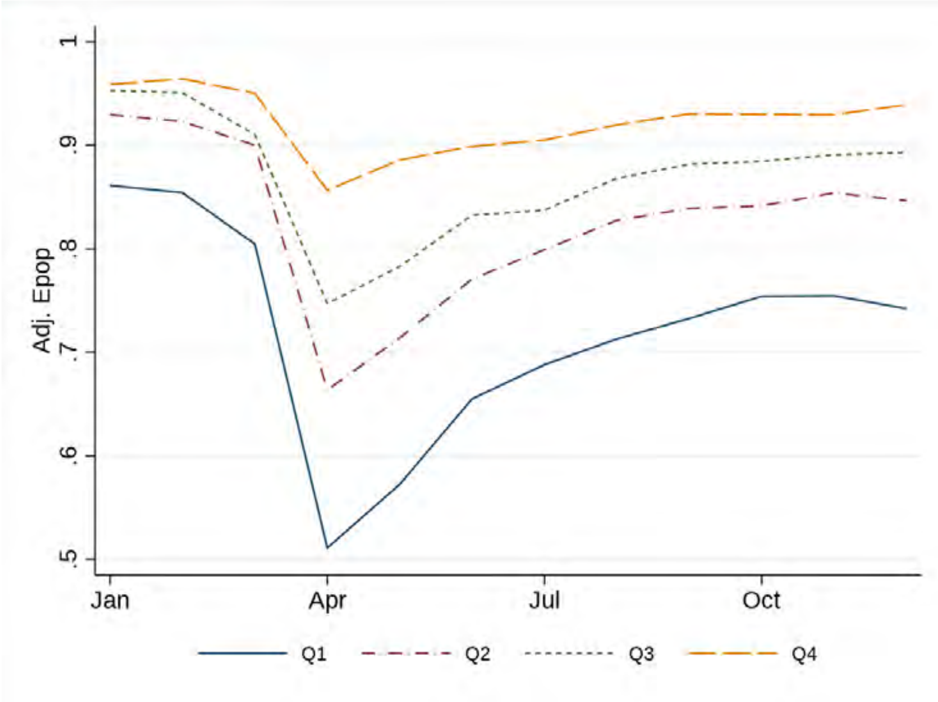

Figure 2B: Adjusted Employment Rates by Race/Ethnicity

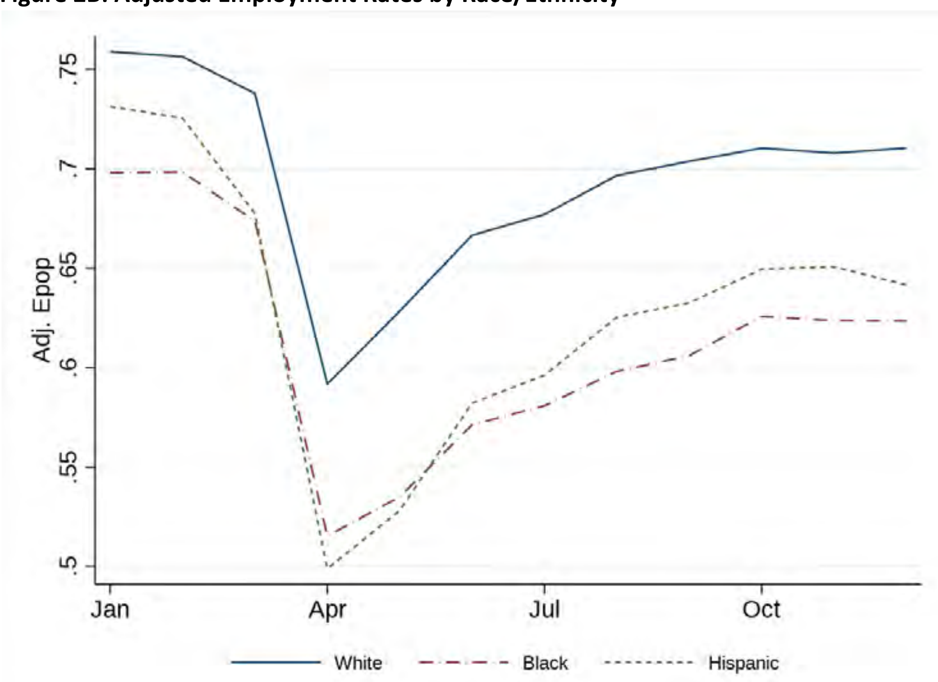

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Figure 2C: Adjusted Employment Rates by Gender

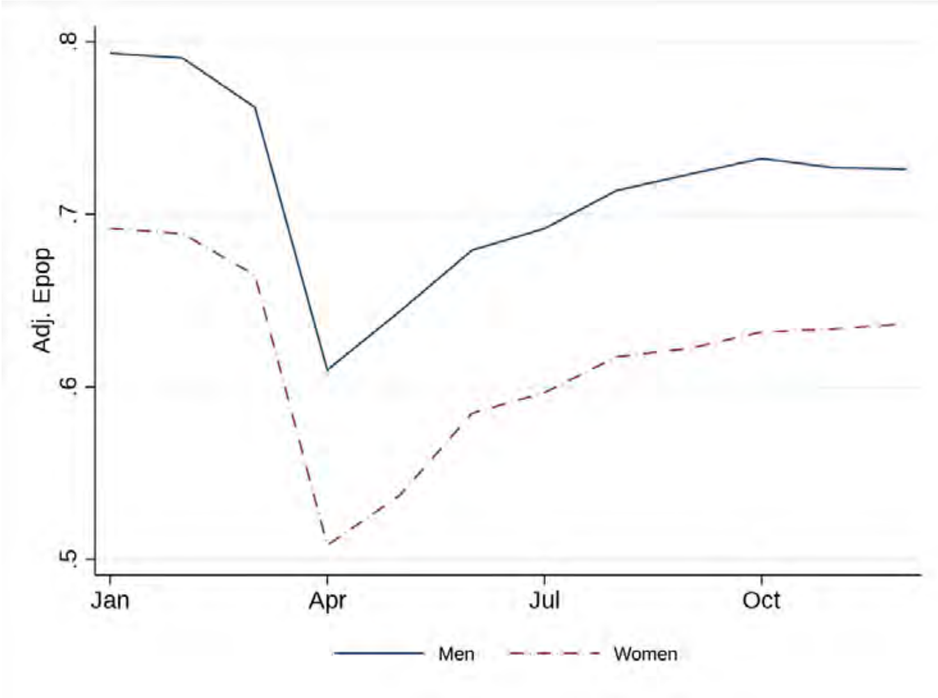

Figure 3A: Permanent Job Loser Share (of Population) by Occupational Wage Quartile

Figure 3B: Permanent Job Loser Share (of Population) by Race/Ethnicity

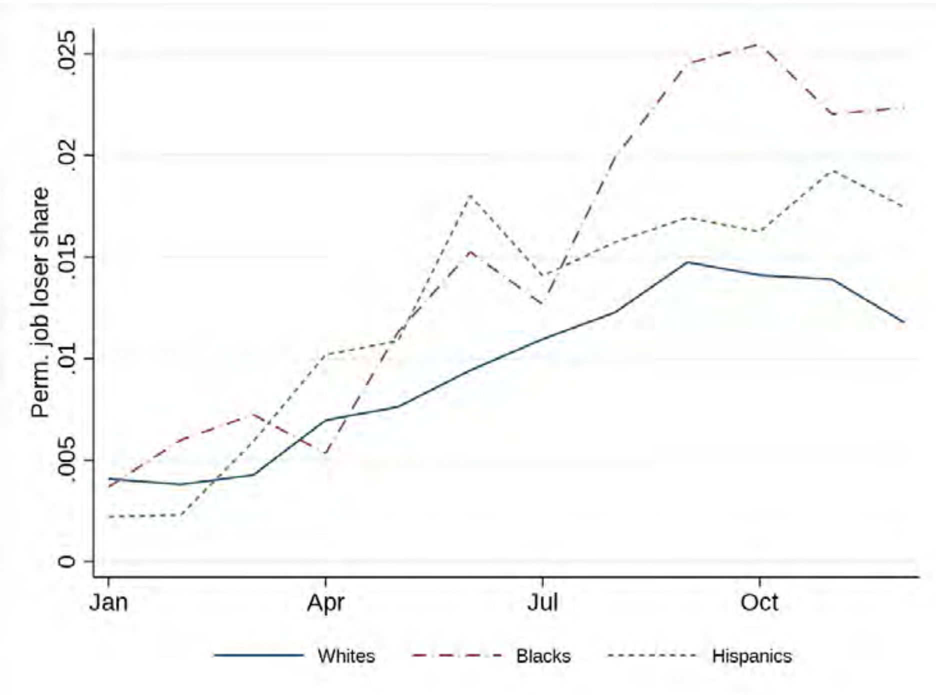

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Figure 3C: Permanent Job Loser Share (of Population) by Gender

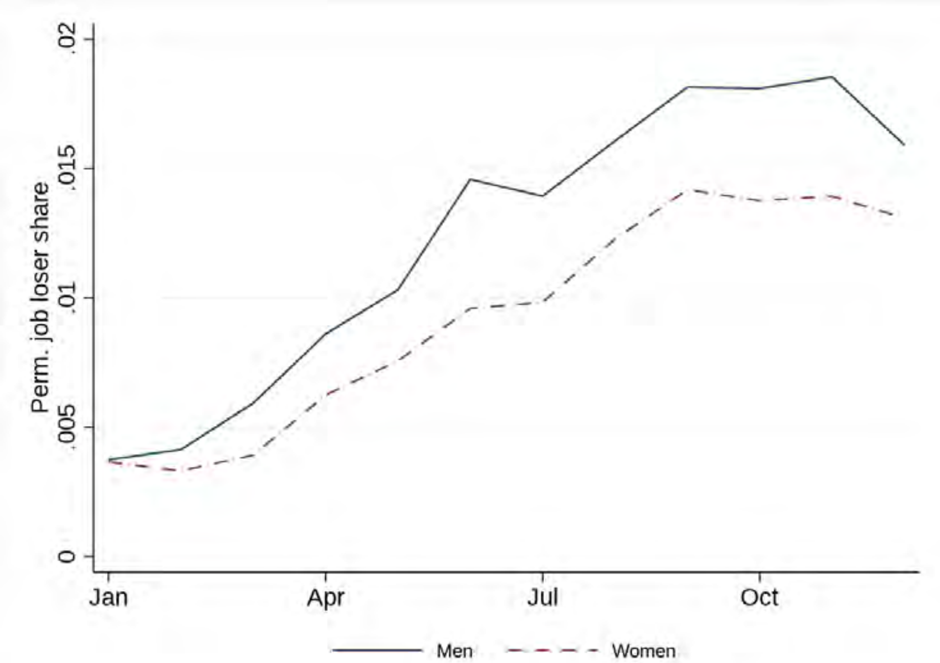

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Figure 4: Adjusted Employment Rate, by State COVID Group (Normalized to Jan. 2020)

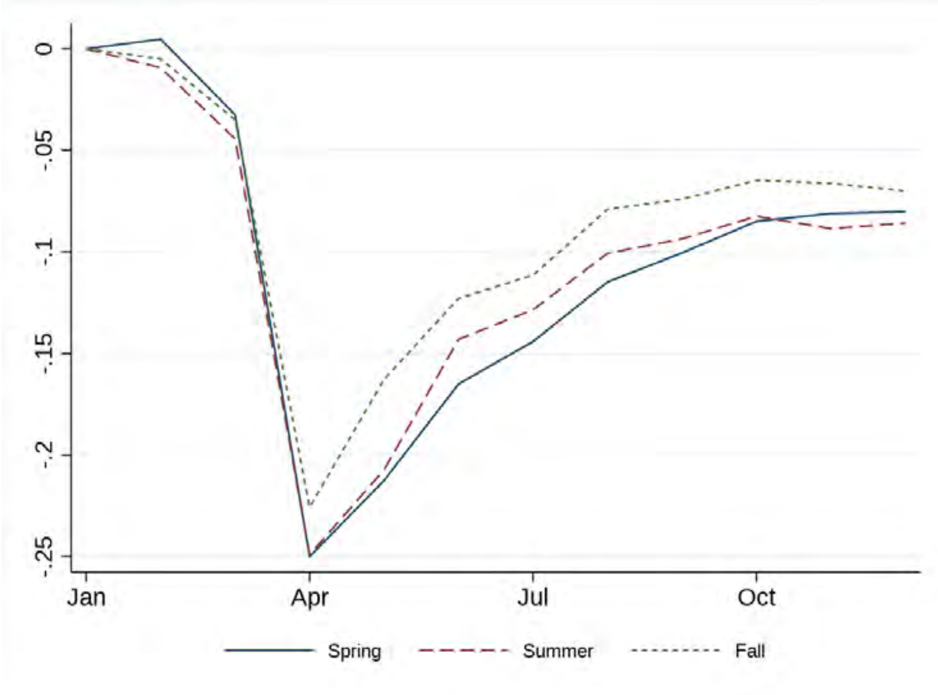

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Figure 5: Permanent Job Loser Share (of Pop.), by State COVID Group (Normalized to Jan. 2020)

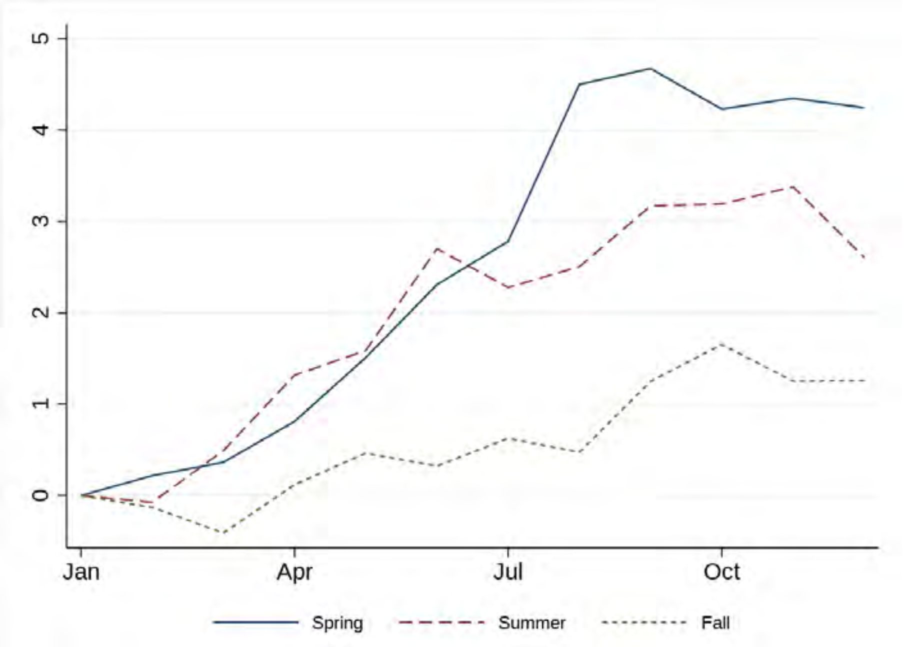

Figure 6A: Permanent Job Loser Share (of Pop.), by Occupational Wage Quartile, Spring Peak State (Normalized to Jan. 2020)

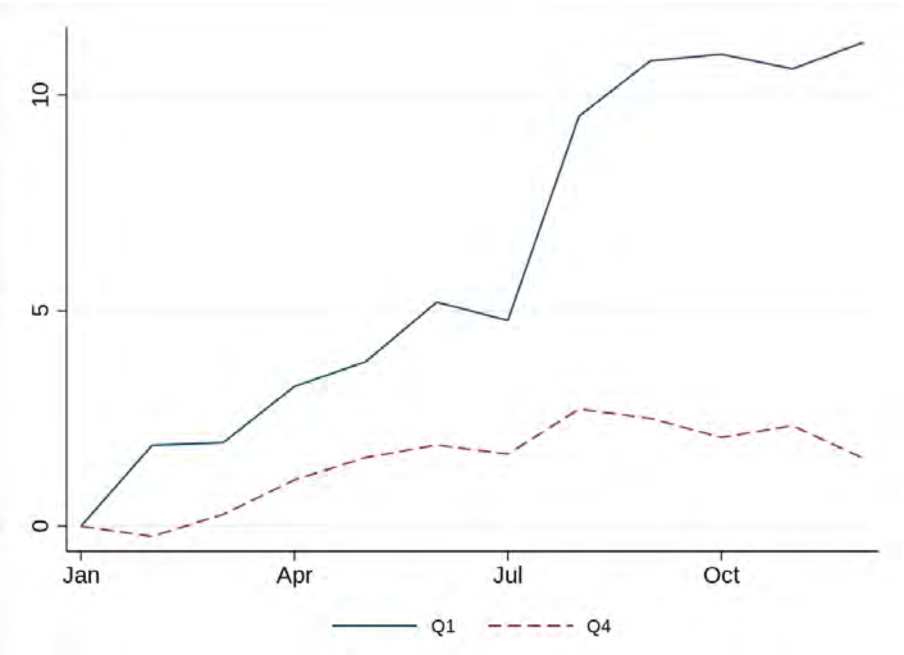

Figure 6B: Permanent Job Loser Share (of Pop.), by Race/Ethnicity, Spring Peak State (Normalized to Jan. 2020)

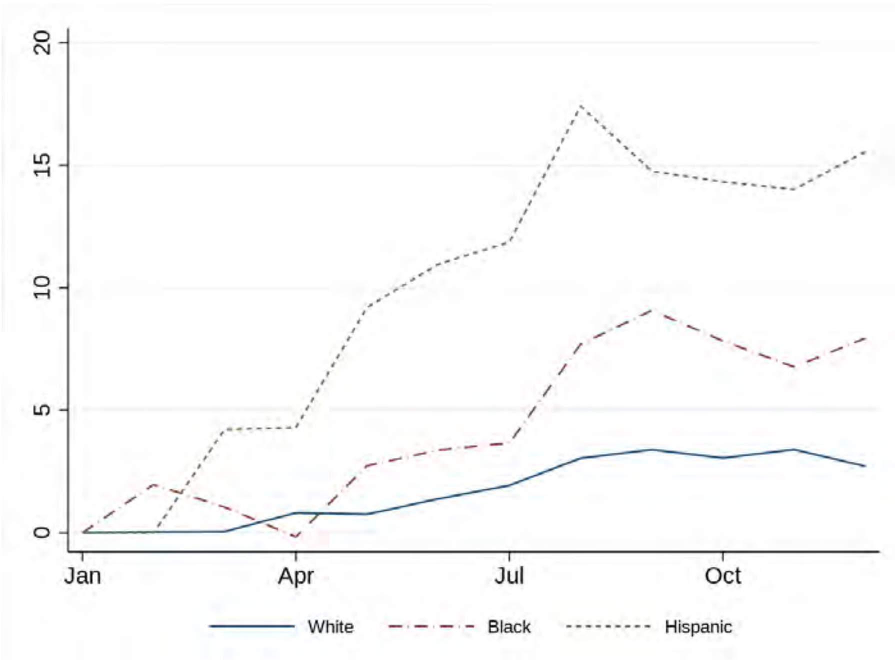

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Table 1A: Adjusted employment rates by Select Months: Demographic groups

	Feb	April	June	Oct	Dec
All	73.9%	55.8%	63.1%	68.1%	68.1%
Whites	75.6%	59.2%	66.7%	71.0%	71.0%
Blacks	69.8%	51.5%	57.1%	62.6%	62.3%
Hispanics	72.6%	49.9%	58.2%	65.0%	64.2%
Men	79.1%	61.0%	67.9%	73.2%	72.6%
Women	68.9%	50.8%	58.5%	63.2%	63.7%
Age 18–24	59.5%	37.1%	43.8%	53.8%	53.7%
Age 25–44	80.5%	62.2%	69.6%	74.2%	74.4%
Age 45–64	71.8%	55.5%	62.9%	66.6%	66.3%
Less than high school	55.1%	36.3%	43.3%	50.1%	49.6%
High school/some college	68.7%	47.4%	56.2%	62.4%	61.9%
Associate degree	78.1%	59.7%	67.8%	72.0%	71.3%
Bachelor's degree	82.3%	67.0%	71.7%	76.5%	77.1%
Graduate degree	86.5%	75.0%	80.6%	83.4%	83.8%

NOTE: Estimates show the adjusted employment rate, net of involuntary part-time workers, for each group in February, April, June, October, and December 2020. The adjusted employment rate captures the share of people employed but excluding those absent from work for "other reasons"; we further net out workers who are working part-time for economic reasons, either on a "usual" basis or the week prior to the survey. We believe this measure of employment best captures pandemic-related disruptions. Estimates have been seasonally adjusted via calendar month dummy regression for each group over 2015–2019. The underlying sample is civilian adults age 18–64.

SOURCE: Authors' calculations from the monthly CPS.

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Table 1B: Adjusted employment rates: Work groups

	Feb	April	June	Oct	Dec
All	73.9%	55.8%	63.1%	68.1%	68.1%
Managers & Professionals	95.3%	80.6%	86.8%	90.6%	91.7%
Service	88.6%	50.8%	65.2%	77.3%	75.5%
Sales & Administrative	90.7%	68.0%	76.5%	83.9%	84.5%
Agric., Construction, Installation, Maintenance, & Repair	93.2%	66.5%	78.5%	83.3%	83.4%
Production	90.0%	65.1%	78.0%	83.1%	82.8%
Agriculture & Mining	88.0%	80.7%	85.2%	83.5%	85.7%
Construction	93.7%	66.2%	79.4%	84.6%	83.5%
Manufacturing	93.1%	76.2%	84.7%	89.2%	91.5%
Trade	90.4%	66.0%	76.3%	83.3%	84.2%
Transportation & Utilities	92.0%	71.5%	77.7%	82.2%	82.1%
Information	93.8%	75.8%	81.7%	85.4%	83.6%
Finance, Insurance, & Real Estate	97.5%	84.2%	89.5%	93.1%	92.3%
Professional Services	92.3%	77.2%	84.1%	87.2%	87.9%
Education & Healthcare	93.9%	73.2%	83.8%	89.4%	89.8%
Arts, Accommodation, & Food	85.7%	38.3%	51.8%	68.3%	66.4%
Other Services	92.1%	52.0%	66.9%	79.6%	77.3%
Public Administration	96.0%	86.4%	90.9%	92.3%	94.4%
Hourly wage quartile 1	85.4%	51.2%	65.5%	75.4%	74.2%
Hourly wage quartile 2	92.3%	66.3%	77.1%	84.2%	84.6%
Hourly wage quartile 3	95.0%	74.7%	83.2%	88.4%	89.3%
Hourly wage quartile 4	96.4%	85.6%	89.9%	93.0%	93.9%
Teleworkable	94.2%	78.2%	84.9%	89.0%	89.6%
Non-teleworkable	91.0%	63.4%	74.9%	82.7%	82.7%

NOTE: See note to Table 1A. Wage quartiles are based on hourly occupational wages from Occupational Employment Statistics (2019) and are employment-weighted. "Teleworkable" occupations are as in Dingel and Neiman (2020). Note that occupation and industry are asked of the currently employed and those who reported working within the past 12 months (only for outgoing rotation groups for those out of the labor force), but in practice, relatively few individuals not in the labor force have a valid response for these questions, lower than transitions rates would imply should be eligible. Consequently, these numbers are likely biased upward from the truth.

SOURCE: Authors' calculations from the monthly CPS.

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Table 2: 2020 Time Path of Select Employment Indicators, by Race/Ethnicity, Relative to January 2020

	Adjusted Employment Rate						*Permanent Job Loser Share of Population*					
	Overall		Diff: Blacks		Diff: Hispanics		Overall		Diff: Blacks		Diff: Hispanics	
March	-0.0212***	-0.0131***	-0.0087	-0.0048	-0.0277***	-0.0230***	0.0003	-0.0008	0.0011	0.0012	0.0036**	0.0040**
	(0.0019)	(0.0042)	(0.0070)	(0.0068)	(0.0058)	(0.0059)	(0.0006)	(0.0015)	(0.0026)	(0.0026)	(0.0018)	(0.0019)
April	-0.1620***	-0.2023***	-0.0247***	0.0039	-0.0661***	-0.0261***	0.0030***	0.0005	-0.0038	-0.0039	0.0048**	0.0046**
	(0.0026)	(0.0057)	(0.0089)	(0.0087)	(0.0074)	(0.0075)	(0.0007)	(0.0016)	(0.0026)	(0.0026)	(0.0020)	(0.0021)
May	-0.1284***	-0.1539***	-0.0340***	-0.0114	-0.0692***	-0.0445***	0.0044***	0.0022	0.0004	0.0000	0.0050**	0.0052**
	(0.0025)	(0.0055)	(0.0088)	(0.0086)	(0.0074)	(0.0075)	(0.0007)	(0.0017)	(0.0029)	(0.0029)	(0.0021)	(0.0022)
June	-0.0937***	-0.1035***	-0.0275***	-0.0129	-0.0506***	-0.0369***	0.0061***	0.0051***	0.0011	0.0006	0.0112***	0.0119***
	(0.0024)	(0.0053)	(0.0086)	(0.0084)	(0.0071)	(0.0073)	(0.0008)	(0.0018)	(0.0030)	(0.0030)	(0.0024)	(0.0025)
July	-0.0806***	-0.0783***	-0.0333***	-0.0256***	-0.0444***	-0.0372***	0.0065***	0.0047***	-0.0001	-0.0009	0.0082***	0.0075***
	(0.0023)	(0.0051)	(0.0084)	(0.0083)	(0.0069)	(0.0071)	(0.0008)	(0.0018)	(0.0030)	(0.0030)	(0.0023)	(0.0025)
Aug.	-0.0598***	-0.0573***	-0.0283***	-0.0223***	-0.0325***	-0.0277***	0.0087***	0.0088***	0.0060*	0.0049	0.0065***	0.0057**
	(0.0022)	(0.0048)	(0.0079)	(0.0077)	(0.0065)	(0.0067)	(0.0008)	(0.0018)	(0.0033)	(0.0033)	(0.0023)	(0.0025)
Sept.	-0.0477***	-0.0464***	-0.0326***	-0.0241***	-0.0231***	-0.0176***	0.0104***	0.0099***	0.0107***	0.0088***	0.0058***	0.0040*
	(0.0020)	(0.0044)	(0.0073)	(0.0072)	(0.0059)	(0.0061)	(0.0008)	(0.0018)	(0.0033)	(0.0033)	(0.0022)	(0.0024)
Oct.	-0.0440***	-0.0434***	-0.0298***	-0.0223***	-0.0197***	-0.0162***	0.0102***	0.0103***	0.0128***	0.0102***	0.0053**	0.0031
	(0.0020)	(0.0043)	(0.0071)	(0.0070)	(0.0057)	(0.0058)	(0.0008)	(0.0018)	(0.0033)	(0.0033)	(0.0021)	(0.0023)
Nov.	-0.0421***	-0.0367***	-0.0203***	-0.0132*	-0.0192***	-0.0154***	0.0102***	0.0114***	0.0104***	0.0085***	0.0097***	0.0080***
	(0.0020)	(0.0043)	(0.0071)	(0.0070)	(0.0058)	(0.0059)	(0.0008)	(0.0019)	(0.0032)	(0.0032)	(0.0023)	(0.0024)
Dec.	-0.0400***	-0.0400***	-0.0114	-0.0047	-0.0322***	-0.0259***	0.0082***	0.0096***	0.0079**	0.0063**	0.0099***	0.0086***
	(0.0020)	(0.0044)	(0.0071)	(0.0070)	(0.0060)	(0.0061)	(0.0008)	(0.0018)	(0.0031)	(0.0031)	(0.0023)	(0.0025)
Mean: Jan 2020	0.934	0.934	0.894	0.894	0.897	0.897	0.0079	0.0079	0.0145	0.0145	0.0078	0.0078
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

NOTE: The adjusted employment rate is the share of the population, age 18–64, employed, excluding those absent from work for "other" reasons, as well as those employed part-time involuntarily, either on a usual basis or just in the reference week of the survey. The permanent job loser share of the population is the fraction of the 18–64 year-old population that report being unemployed as a result of permanent job loss. Estimates for "Overall" reflect changes relative to January 2020 for racial groups except Blacks and Hispanics; estimates for Blacks and

Hispanics reflect the differential *relative* to the "Overall group. Estimates in columns {1,3,5}, {2,4,6}, {7,9,11}, and {8,10,12} come from four regressions, respectively. Controls include level and monthly interactions of four wage quartiles (based on occupation) and five education categories. Regressions are unweighted, but regressions using sample weights are qualitatively similar and available upon request. Data are first seasonally adjusted via regression adjustment (using data from 2015 to date), but estimates are shown are based on 2020 data only (n = 567,951). Standard errors robust to heteroskedasticity in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
SOURCE: Authors' calculations from the Current Population Survey.

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Table 3: Summary Statistics of State-Month Data

	Mean	Std Dev.	P25	P75
Adjusted Emp. Rate	0.656	0.065	0.615	0.698
Adj. Emp. Rate (normed)	−0.063	0.054	−0.091	−0.024
Permanent Unemp. Share	0.012	0.006	0.008	0.016
Perm. Unemp. Share (normed)	0.006	0.006	0.001	0.010
Ln Total Hours	17.88	1.01	17.06	18.55
Ln Total Hours (normed)	−0.086	0.088	−0.132	−0.028
New Case Rate	18.15	22.39	3.84	22.63
New Death Rate	0.293	0.383	0.070	0.364
Restrictions Index	0.450	0.252	0.313	0.625

NOTE: There are 510 observations across 51 states (including DC) and eight months (March through December). Normed values are differenced relative to the January level of the same state. Case and death rates are per 100,000 people. Restriction index ranges from 0 to 1. See text for precise definitions.

SOURCES: Chetty et al. (2020), Fullman et al. (2020), authors' calculations from the monthly CPS.

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Table 4: State-Level Employment Indicators and COVID Case Rates, Death Rates, and Economic Restrictions

	Adjusted Employment Rate				*Permanent Job Loser Share of Population*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
New case rate	0.0008*** (0.0001)	0.0004*** (0.0001)	0.0004*** (0.0001)	0.0003** (0.0001)	−0.0000 (0.0000)	−0.0000 (0.0000)	−0.0000 (0.0000)	−0.0000 (0.0000)
New case rate, *t-1*		0.0007*** (0.0001)	0.0004*** (0.0001)			−0.0001** (0.0001)	−0.0000 (0.0000)	
New case rate, *t-2*			0.0007** (0.0004)				−0.0001 (0.0000)	
New death rate	−0.0280*** (0.0057)	−0.0222*** (0.0049)	−0.0226*** (0.0048)	−0.0184*** (0.0049)	0.0010* (0.0006)	0.0004 (0.0005)	0.0004 (0.0005)	−0.0006 (0.0006)
New death rate, *t-1*		−0.0204*** (0.0043)	−0.0128*** (0.0034)			0.0019** (0.0007)	0.0013 (0.0008)	
New death rate, *t-2*			−0.0228*** (0.0049)				0.0020** (0.0008)	
Restriction index	−0.0625*** (0.0160)	−0.0493*** (0.0139)	−0.0473*** (0.0145)	−0.0464*** (0.0222)	0.0051** (0.0023)	0.0029 (0.0019)	0.0019 (0.0017)	0.0024 (0.0025)
Restriction index, *t-1*		−0.0115 (0.0131)	0.0022 (0.0091)			0.0026 (0.0022)	−0.0004 (0.0018)	
Restriction index, *t-2*			−0.0149 (0.0116)				0.0049* (0.0026)	
Cum case rate				0.0000 (0.0000)				−0.0000 (0.0000)
Cum death rate				−0.0003*** (0.0001)				0.00005*** (0.00001)
Cum restriction index				−0.0004 (0.0062)				0.0003 (0.0010)
Mean: Jan 2020	0.719	0.719	0.719	0.719	0.0067	0.0067	0.0067	0.0067
R^2	0.6645	0.6774	0.6833	0.69030	0.3313	0.3397	0.3485	0.3896

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NOTE: The adjusted employment rate is the share of the population, age 18–64, employed, excluding those absent from work for "other" reasons, as well as those employed part-time involuntarily, either on a usual basis or just in the reference week of the survey. The permanent job loser share of the population is the fraction of the 18–64 year-old population that report being unemployed as a result of permanent job loss. Each column is from a separate regression of state-level outcomes ranging from March through December 2020, for n = 510 observations in specifications without leads and 459 observations for specifications with leads. See text for precise definitions of covariates. Regressions are weighted by the number of individual observations contributing to each state-month cell; standard errors robust to heteroskedasticity and clustered on state in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

SOURCE: Chetty et al. (2020), Fullman et al. (2020), and authors' calculations from the Current Population Survey.

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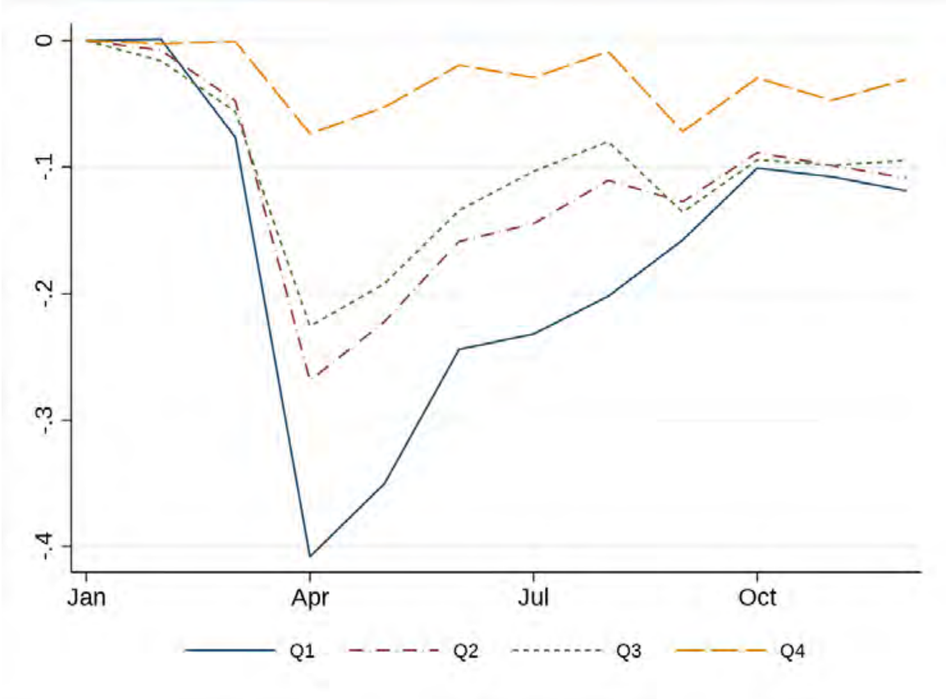
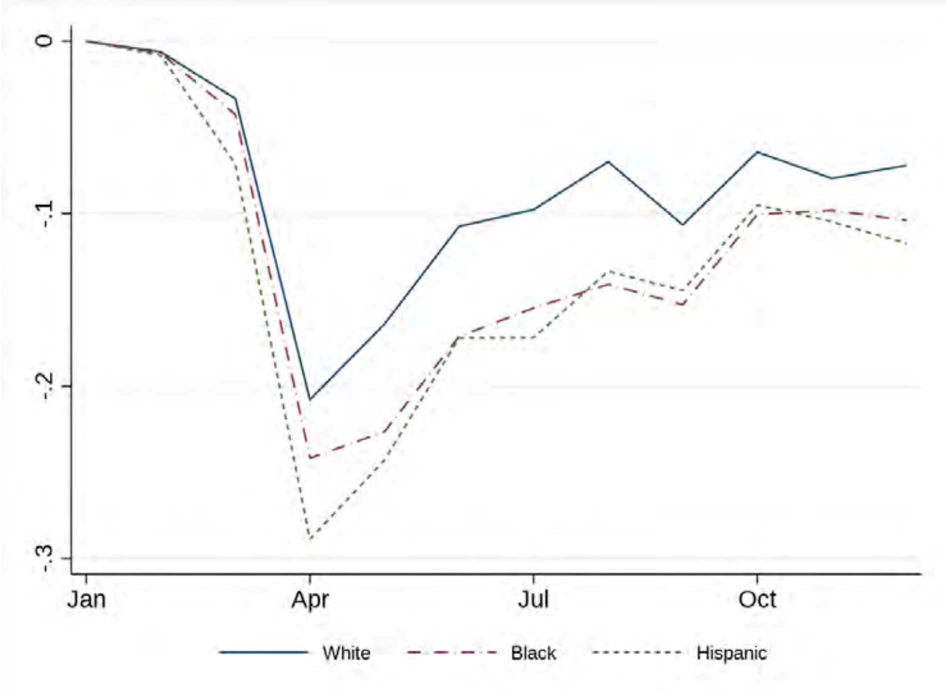
Appendix Figure 1A: Total Weekly Hours, by Occup. Wage Quartile (Norm. to Jan. 2020)



Appendix Figure 1B: Total Weekly Hours, by Race/Ethnicity (Norm. to Jan. 2020)



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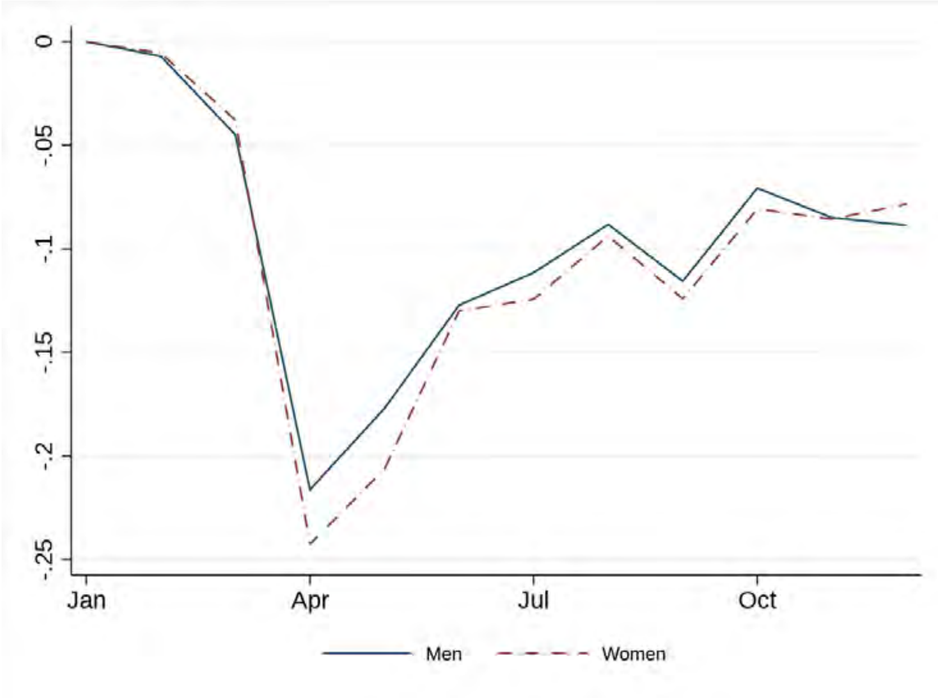
Appendix Figure 1C: Total Weekly Hours, by Gender (Norm. to Jan. 2020)



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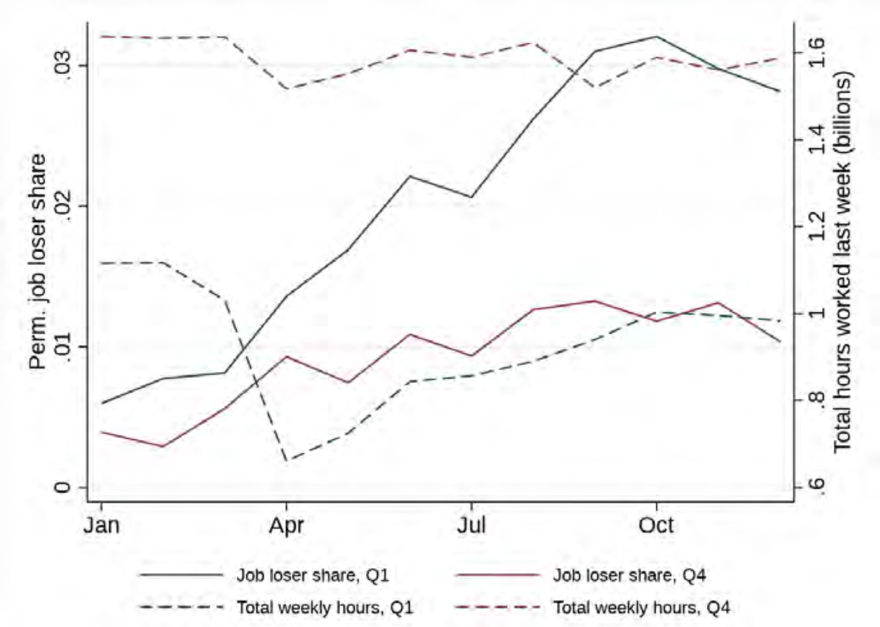
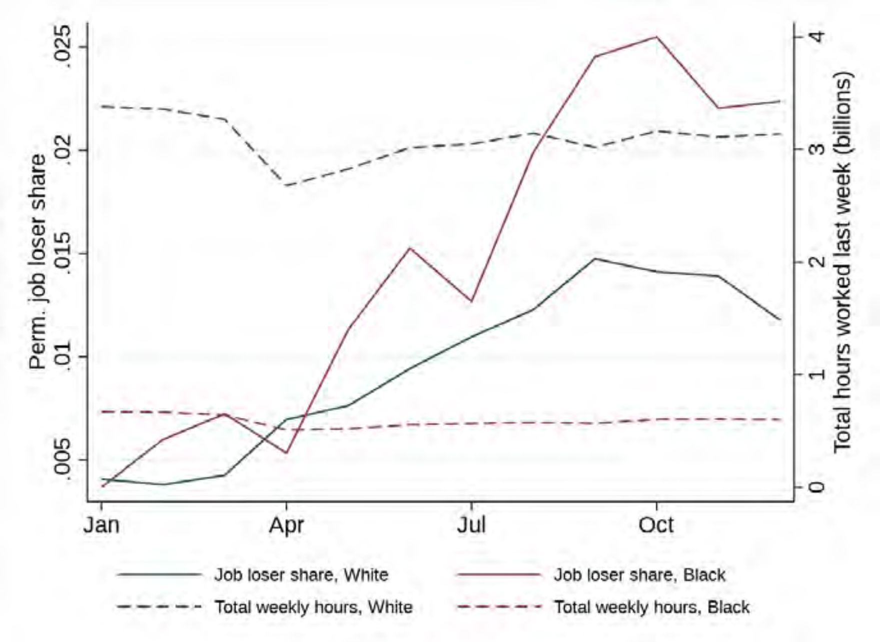
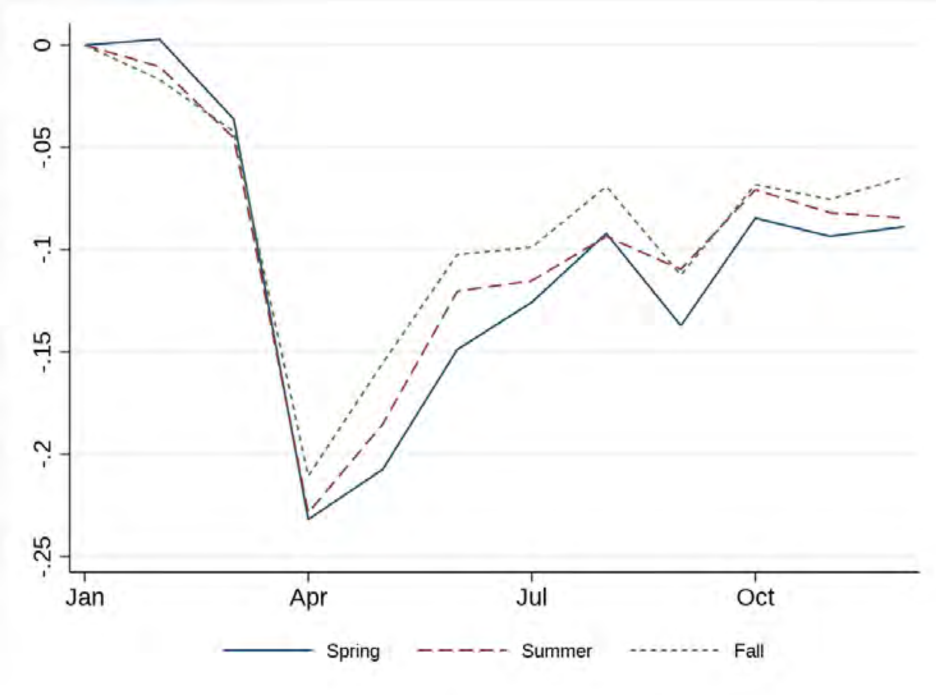
Appendix Figure 2A: Job Loser Share of Population and Total Weekly Hours, by Select Wage Quartile



Appendix Figure 2B: Job Loser Share of Population and Total Weekly Hours, by Race



Appendix Figure 3: Total Weekly Hours, by State COVID Group (Normalized to Jan. 2020)



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Appendix Table 1: State Groups by COVID Caseload Peak Timing

Spring States	Summer States	Fall States
Colorado	Alabama	Alaska
Connecticut	Arizona	Indiana
Delaware	Arkansas	Kentucky
District of Columbia	California	Maine
Illinois	Florida	Montana
Iowa	Georgia	New Hampshire
Louisiana	Hawaii	North Dakota
Maryland	Idaho	Oregon
Massachusetts	Kansas	South Dakota
Michigan	Mississippi	Vermont
Minnesota	Missouri	West Virginia
Nebraska	Nevada	Wisconsin
New Jersey	New Mexico	Wyoming
New York	North Carolina	
Pennsylvania	Ohio	
Rhode Island	Oklahoma	
Virginia	South Carolina	
	Tennessee	
	Texas	
	Utah	
	Washington	

SOURCE: Authors' calculations from COVID case rates as provided by Opportunity Insights: https://github.com/OpportunityInsights/EconomicTracker.

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Appendix Table 2A: Permanent Unemployed Share of Population: Demographic groups

	Feb	April	June	Oct	Dec
All	0.37%	0.74%	1.21%	1.59%	1.45%
Whites	0.38%	0.70%	0.94%	1.41%	1.18%
Blacks	0.60%	0.53%	1.53%	2.55%	2.24%
Hispanics	0.23%	1.02%	1.80%	1.62%	1.75%
Men	0.41%	0.86%	1.46%	1.81%	1.59%
Women	0.33%	0.62%	0.96%	1.38%	1.31%
Age 18–24	0.49%	0.92%	1.40%	1.23%	1.16%
Age 25–44	0.39%	0.79%	1.40%	1.76%	1.64%
Age 45–64	0.31%	0.62%	0.93%	1.53%	1.34%
Less than high school	0.59%	0.94%	1.09%	1.82%	1.38%
High school/some college	0.43%	0.68%	1.31%	1.82%	1.79%
Associate degree	0.30%	0.68%	0.93%	1.36%	1.26%
Bachelor’s degree	0.35%	0.89%	1.47%	1.58%	1.36%
Graduate degree	0.17%	0.69%	0.75%	1.08%	0.85%

NOTE: Estimates show the share of the population reporting permanent layoff, for each demographic group, in February, April, June, October, and December 2020. We believe this measure of unemployment best captures long-term pandemic-related disruptions. Estimates have been seasonally adjusted via calendar month dummy regression for each group over 2015–2019. The underlying sample is civilian adults age 18–64.

SOURCE: Authors’ calculations from the monthly CPS.

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Appendix Table 2B: Permanent Unemployed Share of Population: Work groups

	Feb	April	June	Oct	Dec
All	0.37%	0.74%	1.21%	1.59%	1.45%
Managers & Professionals	0.36%	0.89%	1.11%	1.31%	1.20%
Service	0.51%	1.10%	2.16%	2.87%	2.84%
Sales & Administrative	0.58%	1.04%	1.76%	2.22%	1.82%
Agric., Construction, Installation, Maintenance, & Repair	0.14%	0.79%	1.30%	1.75%	1.99%
Production	0.80%	0.88%	1.98%	2.99%	2.58%
Agriculture & Mining	0.66%	0.54%	1.51%	1.96%	2.05%
Construction	0.13%	1.03%	1.17%	1.56%	2.08%
Manufacturing	0.63%	0.71%	1.80%	2.23%	1.65%
Trade	0.66%	1.25%	1.94%	2.41%	1.89%
Transportation & Utilities	0.42%	0.75%	2.06%	2.01%	2.27%
Information	0.58%	1.29%	1.11%	2.31%	2.84%
Finance, Insurance, & Real Estate	0.00%	1.14%	0.82%	1.71%	1.52%
Professional Services	0.88%	1.15%	1.69%	2.17%	1.66%
Education & Healthcare	0.28%	0.44%	0.96%	0.97%	0.92%
Arts, Accommodation, & Food	0.53%	1.90%	3.36%	4.53%	4.79%
Other Services	0.42%	1.08%	1.45%	2.65%	2.21%
Public Administration	0.21%	0.26%	0.00%	0.65%	0.35%
Hourly wage quartile 1	0.77%	1.36%	2.22%	3.21%	2.82%
Hourly wage quartile 2	0.55%	0.87%	1.70%	2.27%	2.11%
Hourly wage quartile 3	0.26%	0.63%	1.18%	1.39%	1.44%
Hourly wage quartile 4	0.29%	0.93%	1.09%	1.18%	1.04%
Teleworkable	0.41%	0.94%	1.31%	1.73%	1.30%
Non-teleworkable	0.50%	0.94%	1.70%	2.21%	2.25%

NOTE: See note to Appendix Table 2A. Wage quartiles are based on hourly occupational wages frm Occupational Employment Statistics (2019) and are employment-weighted. "Teleworkable" occupations are as in Dingel and Neiman (2020). Note that occupation and industry are asked of the currently employed and those who reported working within the past 12 months (only for outgoing rotation groups for those out of the labor force), but in practice, relatively few individuals not in the labor force have a valid response for these questions, lower than transitions rates would imply should be eligible. Consequently, these numbers are likely biased upward from the truth.

SOURCE: Authors' calculations from the monthly CPS.

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Appendix Table 3: 2020 Time Path of Additional Employment Indicators, by Race/Ethnicity, Relative to January 2020

	Weekly Hours					
	Overall		Diff: Blacks		Diff: Hispanics	
March	−0.97***	−0.60**	−0.07	0.05	−0.66**	−0.66**
	(0.12)	(0.24)	(0.38)	(0.37)	(0.30)	(0.31)
April	−5.64***	−7.48***	−0.82**	0.07	−2.09***	−0.90***
	(0.13)	(0.27)	(0.42)	(0.41)	(0.34)	(0.34)
May	−4.36***	−5.78***	−1.33***	−0.67	−2.11***	−1.59***
	(0.13)	(0.27)	(0.42)	(0.41)	(0.34)	(0.34)
June	−3.13***	−2.96***	−0.57	−0.37	−1.09***	−1.38***
	(0.13)	(0.27)	(0.42)	(0.41)	(0.34)	(0.34)
July	−2.95***	−1.69***	−0.29	−0.50	−0.74**	−1.48***
	(0.13)	(0.27)	(0.42)	(0.41)	(0.34)	(0.34)
Aug.	−2.10***	−1.71***	−0.69*	−0.80**	−0.43	−1.04***
	(0.13)	(0.26)	(0.41)	(0.40)	(0.32)	(0.33)
Sept.	−2.88***	−3.07***	−0.74*	−0.72*	0.06	−0.44
	(0.12)	(0.24)	(0.38)	(0.37)	(0.30)	(0.31)
Oct.	−1.62***	−2.02***	−0.65*	−0.40	−0.16	−0.36
	(0.12)	(0.24)	(0.38)	(0.37)	(0.29)	(0.30)
Nov.	−1.76***	−1.93***	−0.27	−0.08	−0.23	−0.49
	(0.12)	(0.24)	(0.37)	(0.37)	(0.29)	(0.30)
Dec.	−1.50***	−2.33***	−0.27	−0.05	−0.95***	−0.83***
	(0.12)	(0.25)	(0.38)	(0.37)	(0.30)	(0.31)
Mean: Jan 2020	36.2	36.2	33.7	33.7	33.8	33.8
Controls	No	Yes	No	Yes	No	Yes

NOTE: See note to Table 2. Weekly hours are hours worked in the reference week, including zeros for the non-employed, if they were recent labor force participants who listed an occupation (this includes the unemployed and those out of the labor force in outgoing rotation groups who worked within the past 12 months).
SOURCE: Authors' calculations from the Current Population Survey.

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Inflation with Covid consumption baskets[1]

Alberto Cavallo[2]

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The Covid-19 Pandemic led to changes in expenditure patterns that can introduce significant bias in the measurement of Consumer Price Index (CPI) inflation. Using publicly-available data on card transactions, I update the official CPI weights and re-calculate inflation with Covid consumption baskets. I find that the US CPI underestimated the Covid inflation rate, as consumers spent relatively more on food with positive inflation, and less on transportation and categories experiencing deflation. The bias peaked in May, when US Covid annual inflation was 0.95% compared to just 0.13% in the CPI and low-income households were experiencing nearly twice as much inflation as those at the top of the income distribution. I find similar evidence of higher Covid inflation in 12 of 19 additional countries.

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1 I am grateful to Florencia Hnilo for excellent research assistance, to John Friedman for sharing the Opportunity Insights data, to Caroline Coughlin and Manuel Bertolotto for help with the CPI data, and to Rafael Di Tella, Marshall Reinsdorf, Dan Sichel, Xavier Jaravel, and Pete Klenow for helpful comments and suggestions. Financial support for this paper was provided by Harvard Business School.
2 Harvard Business School.

1 Introduction

The Covid-19 Pandemic has led to lockdowns, mobility restrictions, and social-distancing rules that dramatically changed consumer expenditure patterns in many countries, as documented by Chetty et al. (2020) and Carvalho et al. (2020).[1] In particular, consumers are spending less on transportation, hotels, restaurants, and recreation, while expenditures on food and other groceries have increased in both absolute and relative terms.

These sudden changes in expenditure patterns can introduce significant biases in the Consumer Price Indices (CPIs) used to measure inflation, as noted theoretically by Diewert and Fox (2020) and Soloveichik (2020). A major concern is that most National Statistical Offices (NSOs) update the CPI basket weights once a year with lagged expenditure data. The US Bureau of Labor Statistics (BLS), for example, updated the weights in December 2019 using expenditure information collected back in 2017-2018.[2] While this practice may be reasonable in normal times, it can obscure changes in inflation dynamics and inequality during the Pandemic.

In this paper, I quantify the impact that changes in Covid expenditure patterns had on the measurement of CPI inflation during the Pandemic. Relying on publicly-available data from credit and debit card transactions, I update official CPI weights and build alternative "Covid Basket" price indices in 20 countries from March to September 2020.

I start with the US, where daily Covid expenditures are published by the Opportunity Insights (OI) Economic Tracker at Harvard and Brown University, described in Chetty et al. (2020). I find that US Covid inflation was significantly higher for the all-items CPI for the first three months of the Pandemic, as social-distancing rules and preferences induced more consumer expenditure in food and groceries (where prices were increasing) and prevented spending in categories such as transportation (where there was significant deflation). By May, the annual inflation rate of the US Covid index was 0.95%, compared to only 0.13% of the official CPI (all-items, US city average, not seasonally adjusted). The difference narrowed in the following months, but by September the US Covid index still had an annual rate of 1.90%, compared to 1.41% in the fixed-basket CPI. Furthermore, I find a similar bias in the Core CPI, after excluding food and energy.

Next, I use the BLS consumer expenditure (CEX) survey to build income-specific CPI weights and show that Covid inflation was higher for low-income households, who traditionally spend relatively more on food and less on transportation. The difference between the bottom and top quintiles of the income distribution peaked in May, when the low-income inflation rate was 1.12% compared to just 0.57% for high-income households. By September, low-income households were still experiencing 0.26% more annual inflation. This gap was driven by the initial differences in basket weights across income groups, rather than by the relative changes experienced during the Pandemic, suggesting that even small discrepancies in upper-level expenditures can have a significant impact on inflation inequality during events like Covid, when there are sudden changes in relative inflation rates at the sector level.

Finally, I provide estimates of the Covid CPI rates in 19 other countries. Due to data limitations, in most cases I update the official CPI weights with the US Covid expenditure patterns, with the exception of nine European countries where I use credit and debit card spending estimates from Spain computed by Carvalho et al. (2020). Consistent with the US results, in 12 countries I find that the Covid-basket inflation rate was higher than that of the official CPIs. The magnitude of the difference varies greatly by country, and is largest in places experiencing more food inflation, such as Brazil.

These results have important implications for policy-maker trying to respond to the crisis. First, they suggest that the cost of living was higher than estimated by the official data, with welfare

[1] See also Baker et al. (2020), Andersen et al. (2020), Dunn et al. (2020), and Coibion et al. (2020).

[2] Most NSOs compute a Lowe Index formula at upper levels of aggregation. This introduces small adjustments that account for relative price changes across categories every month, but they have little impact on the basket weights because quantities are assumed to be fixed. See of Labor Statistics (2020a) for an archive of BLS weights over time.

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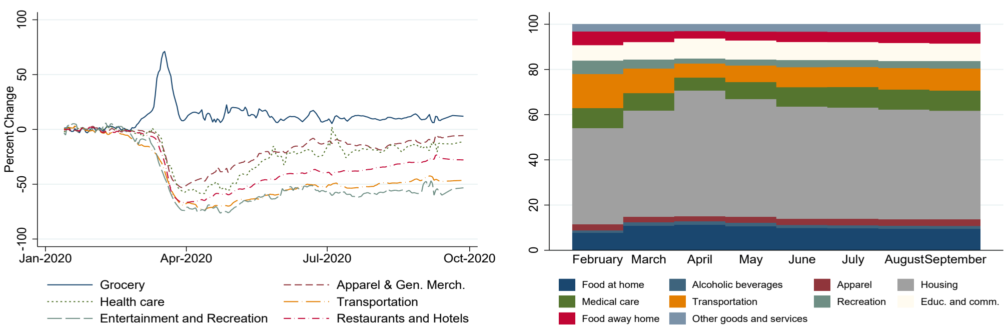
effects that are particularly relevant for low-income households. Second, they can help explain why consumer inflation expectations have increased in many countries, consistent with the recent literature that shows that consumers use their purchasing experiences to form expectations about the future.[3] Third, they reinforce the fact that, despite the collapse in output, there has been little disinflation during Covid. This is consistent with the view that the Pandemic combines a negative demand shock with supply disruptions that are putting upward pressure on prices in many sectors.

More generally, my findings show that the increasing availability of high-frequency expenditure data provides a simple and effective way to build price indices that can adjust for sudden changes in consumption baskets, significantly improving the accuracy of inflation statistics during times of crisis.[4]

2 Data and Methodology

To build the "Covid weights," I start with daily measures of the change in US consumption across sectors since January 2020, available at the Opportunity Insights (OI) Tracker[5]. These estimates, shown in Figure 1(a), are produced using transactional data collected from credit and debit card transactions in the US, as described by Chetty et al. (2020). They show that consumer spending quickly dropped by up to 70% in most categories by the end of March. Over time, expenditures in "Apparel", "General Merchandise", and "Health Care" slowly recovered, but spending in "Transportation" and "Entertainment and Recreation" were still about 50% below pre-pandemic levels by September. The only category where spending increased was "Groceries", peaking in late March and remaining about 10% above pre-pandemic levels in the following months.



(a) Consumer Spending (Opportunity Insights)

(b) Covid Basket Weights

Figure 1: Consumer Spending and CPI Basket Weights During the Pandemic

Notes: Figure (a) shows the expenditure change across categories of goods and services in the US since January 2020. These estimates are computed by Chetty et al. (2020) using data collected from credit and debit card transactions. The data is publicly available at the Opportunity Insights (OI) "Track the Recovery" website (tracktherecovery.org). Figure (b) shows the Covid basket weights estimated by combining the data in (a) with the official CPI weights from the Bureau of Labor Statistics.

I combine these estimates with official CPI data from January 2019 to September 2020, obtained from the official NSO in each country, including the Bureau of Labor Statistics in the US. In all

[3]See Coibion and Gorodnichenko (2015), Cavallo et al. (2017), and D'Acunto et al. (2019)
[4]All the data, code, and updated results from this paper are available at projects.iq.harvard.edu/covid-cpi.
[5]See tracktherecovery.org

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cases, I use the upper-level sector series that compose the headline CPI (all-items, not-seasonally adjusted), as well as the latest available expenditure weights for each of these sectors in the official CPI.

The matching of the OI categories with the CPI sectors requires some assumptions.[6] To improve the correspondence, I split the US CPI for "Food and Beverages" into three additional subcategories. About six categories are then matched across datasets. For "Food at Home" and "Alcoholic Beverages," I use the OI "Grocery" category. For "Food Away from Home," I use the OI category for "Restaurants and Hotels." For "Other Goods and Services," I assume that the expenditure changes are equal to those of the whole OI basket. Finally, for "Housing" and "Education and Communication," I assume that expenditures in these categories have not changed, which seems a reasonable assumption during the first months of the Pandemic.

To estimate the expenditure shares in the Covid basket, I start with the latest official CPI weights and multiply them by the average percentage change in the corresponding expenditure category each month. The new weights are then re-computed as a share of the total, to account for the fact that total expenditure is also falling over time.

Formally, the Covid weights are given by:

$$s_t^i = \frac{P_t^i Q_t^i}{\sum_i P_t^i Q_t^i} = \frac{s_0^i \Delta e^i}{\sum_i s_0^i \Delta e^i} \tag{1}$$

where P_t^i and Q_t^i are the prices and quantities of CPI category i at time t, and $\Delta e^i = \frac{P_t^i Q_t^i}{P_0^i Q_0^i}$ is the change in expenditure. Equation 1 highlights the fact that these are *relative* weights, so the importance of a category in the basket can change even when its expenditure is not affected.

Finally, the CPI and Covid price indices are computed using the weighted sum of the changes in the official CPI sectoral indices, using weights s_0^i and s_t^i, respectively. Note the fixed-basket CPI is Laspeyres index, which traditionally results in *higher* inflation because it does not allow for the possibility that consumers shift their spending away from categories experiencing relatively more inflation. By using more current expenditure weights in the Covid Index, I am allowing for this possibility, which makes my results with the Covid basket more surprising.[7]

3 Impact on US Inflation

In this section, I look at the impact in the US for the all-items CPI, extend the analysis to the Core CPI, and discuss potential welfare implications by comparing Covid inflation for both low and high-income households.

3.1 All-items CPI

The all-items CPI for urban consumers (CPI-U) is the main "headline" measure of inflation in the US. Figure 2(a) shows the impact that the changes in expenditure shares across categories have on this index for every month since the Pandemic started.

During the first three months of the Pandemic, from March to May, the Covid CPI experienced significantly less deflation. In March, when the Pandemic first hit the US, the Covid index had only half the deflation shown by the fixed-basket CPI. In April, the difference became even larger, with the Covid CPI falling by only -0.09% compared to a fall of -0.69% in the CPI. Interestingly,

[6]See details in the Appendix

[7]The Covid index is not a Paasche index because I am not fixing the basket weights to the last period. Instead, my method is closer to the "Chained CPI" produced by the BLS (C-CPI-U). Unfortunately, the BLS can only update expenditure weights gradually, which results in a preliminary C-CPI-U index that does not fully reflect spending patterns until a year later, when a final version is published. In fact, in the Appendix I show that the C-CPI-U has had less inflation during the Pandemic than the CPI-U, the benchmark all-items CPI used in this paper.

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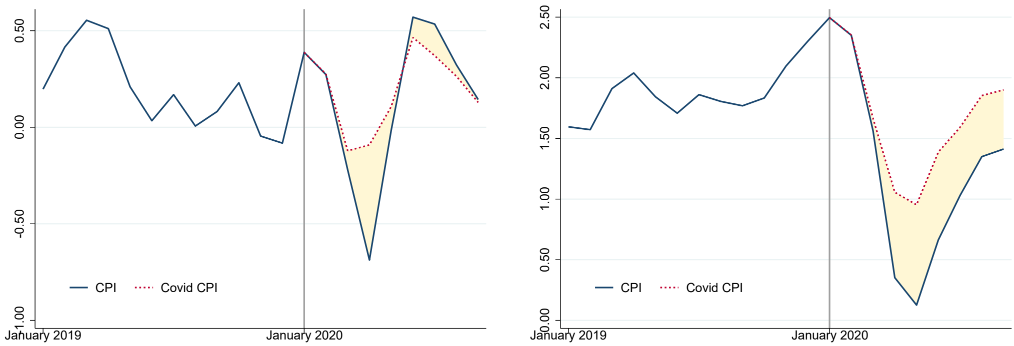

(a) Monthly Inflation Rate (%) (b) Annual Inflation Rate (%)

Figure 2: US Inflation During the Covid Pandemic

Notes: These graphs show the all-items, US city average, not seasonally adjusted CPI, and an equivalent index constructed using estimates of the consumption expenditure shares under lockdown.

that month the trend in the Covid CPI was already rebounding. In May, the Covid CPI has a positive inflation rate, while the CPI was still experiencing some deflation.

The following three months, from June to August, the direction of the CPI bias was reversed, and inflation started being *lower* with the Covid basket. In particular, in June and July, the fixed-basket CPI was assigning too much weight to the transportation sector, where prices were now rebounding, even though expenditure levels remained significantly below pre-pandemic levels.

Although the sign and magnitude of the bias changed over time, the annual inflation rate for the Covid index has been consistently higher than that of the fixed-basket CPI, as shown in Figure 2(b). The difference was largest in May, when Covid inflation was 0.95% compared to just 0.13% in the CPI. By September, the Covid index was still experiencing an annual inflation rate equal to 0.95% compared to only 0.13% in the official CPI.

To understand why the Covid index has more inflation, consider the CPI sectors and weights shown in Table 1 for April 2020, when the difference was largest. The first column shows a comprehensive list of all the CPI categories that compose the all-items index. The second column shows the monthly CPI sector inflation for that month. The third and fourth columns show the CPI and Covid weights. Finally, the last two columns show the incidence that each category has on the total inflation rate. The incidence is the monthly inflation rate multiplied by the weight, so that the sum of all the numbers in the last two columns equals the -0.69% and -0.09% monthly inflation rates for CPI and Covid CPI during April.

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CPI Category	Monthly CPI Inflation	Weight CPI	Weight Covid CPI	Incidence CPI	Incidence Covid CPI
Food at Home	2.67	7.58	11.28	0.20	0.30
Alcoholic Beverages	0.30	1.02	1.52	0.00	0.00
Apparel	-4.38	2.81	2.20	-0.12	-0.10
Housing	-0.03	42.11	55.80	-0.01	-0.02
Medical Care	0.28	8.83	5.60	0.02	0.02
Transportation	-4.97	15.74	6.25	-0.78	-0.31
Recreation	-0.27	5.82	2.23	-0.02	-0.01
Education and Communication	0.13	6.77	8.97	0.01	0.01
Food Away from Home	0.15	6.19	3.13	0.01	0.00
Other Goods and Services	-0.04	3.13	3.03	0.00	0.00

Table 1: US CPI Weights and Incidence - April 2020

Notes: The CPI weight is the share of expenditure in a given category over total expenditures. Note that categories that experience no change in spending over time can have higher Covid weights as a share of the decreasing total expenditure basket. The incidence is the monthly inflation rate multiplied by the weight. The sum of all the category incidence numbers is equal to the monthly inflation rate.

Table 1 shows that the US Covid inflation rate was higher in April mainly because there was more weight in categories that had a positive inflation rate, and less weight in categories experiencing significant deflation. In particular, the weight for "Food at Home" rose from 7.58% to 11.28%, increasing the incidence of this category by 0.10%. At the same time, the weight for "Transportation" fell from 15.74% to 6.25%, increasing the incidence on the total monthly inflation rate by about 0.47%. The weights of "Housing" and "Education and Communication" also rose significantly. However, these two categories had little impact on Covid inflation so far because their sectoral inflation rates are close to zero.

3.2 Core CPI and PCE Index

Although much of the basket bias comes from the changes in spending on food and fuel, there is also higher Covid inflation in the Core CPI index that excludes these categories, as shown in Figure A2 in the Online Appendix.[8] The difference was largest in the first three months of the Pandemic, and by September the annual inflation rate for the Covid basket Core index was 1.98% compared to the 1.74% in the BLS Core.

The reason for the higher Core inflation is that the Covid basket puts less weight on non-energy transportation categories that were having significant deflation in April and May, such as "Public Transportation" and "New and Used Motor Vehicles." Although the magnitude of the bias is smaller with the Core, its effects may be more persistent because expenditures in transportation are taking longer to recover, as shown by the consumption patterns in Figure 1(a).

An alternative Core index in the US is the Personal Consumption Expenditures (PCE) deflator, used by the Federal Reserve for its official inflation target. There are many methodological differences with the CPI, but a key distinction is that the PCE is a chained index that tries to more frequently account for changes in expenditures using the Census Retail Trade Survey. Un-

[8]To build the Core indices, I exclude all food series and split the "Housing" and "Transportation" series to remove their energy components. I also made similar assumptions for the consumer spending patterns at the category level, with details provided in the Appendix.

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fortunately, many sectors can only be adjusted on a quarterly basis, introducing delays.[9] Indeed, a comparison between the CPI and PCE Core indices shows that there was almost identical deflation in the Core CPI and PCE indices in March and April, suggesting that the PCE Core also underestimated the level of Covid inflation during that time.[10].

3.3 Impact by Income Level

My findings imply that the cost of living for consumers is rising faster during the Covid crisis than what the official CPI suggests. This can, in turn, can have different welfare implications across income groups, depending on how much households spent during the crisis in categories such as food and transportation.

A large literature has studied how inflation varies across income levels. Earlier papers such as Hobijn and Lagakos (2005) compared expenditures at upper levels of aggregation and found small differences in inflation rates across income groups. In more recent years, Kaplan and Schulhofer-Wohl (2017), Argente and Lee (2017), and Jaravel (2019) used scanner data to study mechanisms that can increase the inflation experienced by low-income households within narrower categories of goods. For example, Jaravel (2019) found that annual inflation in the US for households in the bottom income quintile was on average nearly 0.4% higher for the period 2004-2015. For the Covid Crisis, Jaravel and O'Connell (2020) used UK scanner data to show that inflation increased for most households, but they found only modest differences in inflation rates across socio-demographic groups.

To study the impact for different households in the US, I construct expenditure weights for the lowest and highest quintiles of the household income distribution. These weights are not published by the BLS, so I estimate them with data from the 2018 BLS Consumer Expenditure Survey (CEX). The initial weights, shown in Table A5 in the Appendix, reflect that low-income households spend relatively more on "Food and Beverages", "Housing", "Medical Care", and "Other goods and Services", and relatively less in "Transportation" and other categories. I update these weights during Covid using monthly spending patterns provided by Opportunity Insights for the same income quintiles, and re-estimate the inflation rate experienced by each group during the crisis.[11]

Figure 3 shows the annual inflation rate for each income-level Covid index, as well as the benchmark official and Covid CPIs. During 2019, low-income households were already experiencing more inflation due to the fact that they spend relatively more on food. After March 2020, the Pandemic increased the difference. With Covid weights, the low-income households had an annual inflation rate of 1.12% in May 2020, compared to just 0.57% for high income households. The difference narrowed in the months that followed, but by September the Covid inflation rate for low-income households was still higher, at 1.99% compared to 1.73% for high-income families.

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[9]See of Economic Analysis (2014).
[10]See Figure A3 in the Online Appendix
[11]More details on these weights and their construction are provided in the Appendix.

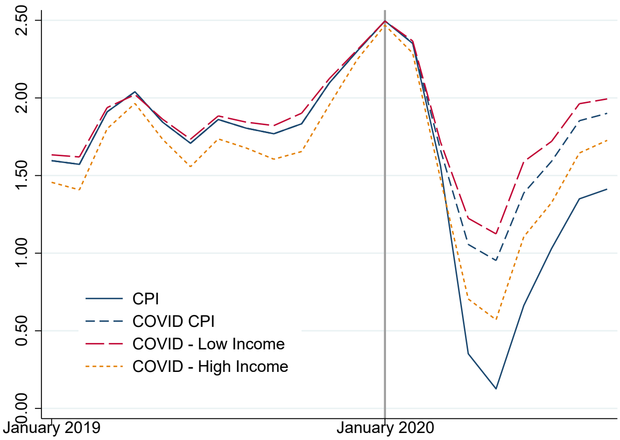

Figure 3: US Annual Inflation with Covid Expenditure Baskets

Notes: The CPI and Covid CPI are plots of the same indices shown in Table 1. The Covid Low (High) Income index uses CEX expenditure weights for households in the lowest (highest) quintile of the income distribution. These weights remain constant for 2019, and after January 2020, are updated using the changes in spending patterns for equivalent quintiles computed by Opportunity Insights. See the Appendix for details.

The changes in consumption patterns during Covid increased the inflation rate for both income groups, consistent with the UK results in Jaravel and O'Connell (2020). But the fact that low-income households spend relatively more on food, and less on transportation, made the Covid inflationary impact greater for those at the bottom of the income distribution. This was mainly driven by the initial differences in basket weights across income groups, rather than by the specific changes experienced during the Pandemic.[12] This suggests that even small differences in upper-level expenditure weights can have an impact on inflation inequality during events such as Covid.[13]

4 Impact in other Countries

In this section, I extend my analysis to 19 additional countries: Argentina, Brazil, Canada, Chile, Colombia, France, Germany, Greece, Ireland, Italy, Japan, Korea, Netherlands, Russia, Southafrica, Spain, Turkey, UK, and Uruguay. All these countries use the COICOP classification system, which is different from the one applied by the BLS in the US, but the category matching process and assumptions are very similar, as shown in the Appendix.

There is still no publicly-available Covid expenditure data in all these countries, so in most cases I simply assume that the Pandemic caused a similar change in consumption patterns as seen in the US data. This is clearly a rough approximation because spending patterns could be influenced by differences in infection rates, lockdown intensity, and the overall timing of the crisis. However, for a set of nine European countries where the timing of the Covid crisis was particularly different from

[12]In fact, the increase in inflation of the Covid-basket index relative to a fixed-basket index is *smaller* for low-income households, because their changes in spending patterns have been less persistent over time (as documented by Chetty et al. (2020). In other words, inflation inequality is greater during Covid, but it is actually smaller than what would be measured with a fixed-basket index. See the Appendix for details.

[13]By contrast, differences in expenditure patterns at lower levels of aggregation may matter more in the long-run, as documented in Jaravel (2019).

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the rest of the world, I use expenditure estimates from Spain computed by Carvalho et al. (2020).[14] Furthermore, there are many similarities in the Covid spending patterns from both data sources, particularly with the increase in spending for food at home and the collapse of expenditures for transportation, which matter the most for the inflation results presented below.[15]

Table 2 shows the CPI and Covid annual inflation rates for all countries in September 2020. Detailed weights and inflation dynamics in each country are shown in the Appendix.

	Annual Inflation (12-month change, %)		
Country	CPI	Covid CPI	Difference
Brazil	3.20	4.17	0.97
Uruguay	9.93	10.61	0.68
Chile	3.01	3.63	0.62
Korea	0.94	1.46	0.52
US	1.41	1.89	0.48
France	0.15	0.61	0.46
Southafrica	2.93	3.32	0.40
Spain	0.02	0.34	0.32
Japan	0.19	0.44	0.24
Colombia	1.96	2.14	0.18
Canada	0.43	0.58	0.15
Russia	3.66	3.76	0.10
Greece	-1.48	-1.42	0.06
UK	0.66	0.59	-0.06
Argentina	37.02	36.90	-0.13
Italy	-0.06	-0.21	-0.15
Ireland	-1.32	-1.64	-0.32
Netherlands	1.09	0.75	-0.34
Turkey	10.84	10.40	-0.44
Germany	-0.30	-0.88	-0.58

Table 2: CPI and Covid Inflation in September 2020

Notes: The top panel shows countries where the Covid inflation is higher than the fixed-basket CPI. The bottom panel shows countries where the Covid inflation is lower than the fixed-basket CPI. Covid inflation rates are constructed using official CPI weights in each country updated by the relative changes across categories observed in US data. Details on the incidence of CPI categories on the monthly inflation rate in each country are shown in the Appendix.

In the top panel, I list the countries where the Covid Inflation rate is higher than that of the official CPI, ranked by the percentage point difference. Consistent with the US results, in these 12 countries the higher Covid inflation rate is driven by an increase in expenditure weights for "Food and Beverages," which was having more inflation, and a decrease in the weight of "Transportation," which was having significant deflation. Brazil is at the top because the divergence in these two sectoral inflation rates was larger and more persistent over time.

The bottom panel shows that some countries appear to have *less* inflation with the Covid basket. In these cases, there is no common explanation across countries. For example, in Germany the Covid index has less weight on "Recreation and Culture," a category with a surprisingly high

[14]These data are published at https://www.bbvaresearch.com/en/special-section/charts/

[15]See the Appendix for a comparison of Covid spending patterns in the US and Spain.

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inflation rate of 4.23% during April. In the Netherlands, instead, there was less Covid inflation because of a 7% spike in "Restaurants and hotels" that same month. Understanding the specific inflation dynamics within each of these countries is outside the scope of this paper, but these results highlight the fact that the Covid basket bias described in this paper depends not only of the changes in the basket weights but also on the sectoral inflation rates experienced by each country.

My results outside the US are only approximations to the true Covid inflation rates in these countries, but they still suggest where there might be larger biases in measured CPI inflation. Reinsdorf (2020) applies a similar methodology and weights in 83 countries and finds that underestimation of inflation during the early months of the pandemic occurs in nearly all regions of the world. Recognizing the importance of these potential distortions, some NSOs have recently started to produce experimental indices with ad-hoc Covid adjustments, as in ONS (2020).[16]. Future research papers could help expand these efforts by computing Covid expenditure weights from high-frequency transactional data collected in each of these countries.

5 Discussion and Other Potential Biases

As noted in Section 2, my findings go in the opposite direction of the *upward* CPI substitution bias that is well-documented in the literature. The usual claim is that the fixed-basket CPI does not take into account how consumers shift spending away from categories with relatively higher inflation and into categories experiencing more deflation. With the updated Covid weights, I am explicitly allowing for this to happen, so why do I find even higher inflation? In other words, why is this expenditure switching not happening during Covid?

The answer may lie within the characteristics of the Covid shock itself. One possibility is that the sectoral inflation differences during Covid are mostly driven by relative demand shocks (people consuming more groceries and less transportation because they have to stay home), whereas in normal times they might be driven by relative supply shocks (with a move *along* the demand curve in response to the change in prices). Large demand shocks were clearly important in this crisis, but there is also evidence that supply disruptions have played a significant role in some sectors.[17] Another possibility is that Covid made demand more inelastic in some sectors. Indeed, it is reasonable to expect consumers to be less responsive to price changes in times of lockdowns and social-distancing, when they are forced to consume food at home even if prices rise, or are unable or unwilling to travel even if fuel prices collapse. In fact, these two explanations are not mutually exclusive, and are both likely playing a role during Covid.

A third possibility is that consumers could be making the expenditure switching *within* the ten categories that I study in this paper; for example, by buying cheaper varieties of food products. I do not have access to more detailed expenditure patterns in the US to rule this out. However, Jaravel and O'Connell (2020) explored this possibility using supermarket scanner data in the UK (with prices and quantities at the product level) and found a similar increase in Covid inflation with both fixed-basket and chained price indices (which adjust the expenditure basket over time). Their result suggests that there was little within-sector expenditure switching during the first few months of the Pandemic, at least in countries like the UK.

Furthermore, there are other Covid-related measurement challenges at lower levels of aggregation the could reinforce the *downward* bias in the CPI. In particular, Diewert and Fox (2020) and Soloveichik (2020) describe the *disappearing goods* bias, which occurs when some products' prices are no longer available to construct elementary price indices, at the most disaggregated level of the CPI. In fact, the BLS reported that the share of products with missing prices in the US CPI rose from 14% in April 2019 to 34% in April 2020. In part, this reflects the challenges of collecting data during this period (the BLS suspended physical data collections in March), but some prices

[16]See also Dixon (2020)

[17]See Cavallo and Kryvtsov (2020).

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are likely missing due to the stock-outs that resulted from the surge in panic-buying and supply disruptions caused by the Pandemic.[18] Diewert and Fox (2020) note that the out-of-stock products are likely to have higher market-clearing prices than those for continuing goods, potentially introducing an additional downward bias on the measured CPI that reinforces the results in my paper. For the UK, Jaravel and O'Connell (2020) estimate that the reduction in product variety due to Covid is equivalent to approximately 0.85% additional inflation.

Finally, the Pandemic is also likely to introduce an *outlet* bias, as a large share of total spending moves online. Cavallo (2017) shows that multi-channel retailers tend to have identical prices offline and online, so the data collected for this type of retailer are not likely affected. However, the use of online delivery platforms such as Instacart and Shipt in the US, has soared during the Pandemic. Most of the retailers participating in these platforms disclose that they have higher prices than in their physical stores.[19] If this is not accounted for in the data sampling methodology used by the NSO, the change in outlets could introduce another downward bias in the CPI, reinforcing the results in my paper.

6 Conclusion

There is growing awareness among academics, central bankers, and financial analysts about the challenges of measuring and interpreting inflation data during the Pandemic.[20] A major concern is that consumption patterns were greatly affected by the lockdowns and social-distancing behaviors, introducing a potential bias into the measurement of inflation with traditional fixed-basket CPIs.

Using estimates of the changes in consumer spending during the Pandemic, obtained from credit and debit card transactions by Chetty et al. (2020) and Carvalho et al. (2020), I study the impact of Covid expenditure baskets on CPI inflation in 20 countries. In 13 of those countries, I find that the Covid price index has more inflation than the official CPI. In the US, the impact was most significant in the first three months of the Pandemic, because consumers spent more on food and categories experiencing inflation, and less on transportation and related categories with significant deflation. By May, the US annual inflation rate was 0.95% with the Covid basket, compared to only 0.13% with the official CPI. The difference narrowed in the following months, but by September the Covid CPI still had 0.49% higher annual inflation. Furthermore, I show that the Covid basket bias was also present in the US Core CPI, because consumers were spending less on non-energy transportation and recreation categories. More importantly, I find that Covid inflation affected low-income households the most, leading to a sudden increase in inflation inequality in the first months of the Pandemic.

These results have important implications for policy-makers. First, they imply that the cost of living for consumers was higher than what was measured by the official data. The welfare implications are more relevant for lower-income households, and extend to many countries, particularly those experiencing a divergence in sectoral inflation rates. Second, my results could help explain the sudden increase in consumer inflation expectations, as reported in the Michigan Survey of Consumers for the US.[21] This is consistent with a recent literature that finds that consumers use their own purchasing experiences to form expectations about future inflation, as in Coibion and Gorodnichenko (2015), Cavallo et al. (2017), and D'Acunto et al. (2019). Third, my results highlight the fact that inflation has been relatively stable in this crisis, particularly when we take into account the changes in expenditure patterns. This supports the view that supply disruptions are putting upward pressure on prices in many sectors, compensating for the effects of the negative demand shocks. Understanding the pricing impact of these supply shocks is likely to be an important area for future research on Covid inflation dynamics.

[18] See of Labor Statistics (2020b)

[19] See Instacart (2020) and Shipt (2020)

[20] See Diewert and Fox (2020), Tenreyro (2020), Lane (2020), and Wolf (2020).

[21] See Curtin (2020)

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More generally, my findings suggest that the public availability of high-frequency expenditure data may give NSOs an effective way to build price indices that can adjust to sudden changes in consumption patterns, significantly improving inflation measurement during times of crisis.

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The COVID-19 she-cession: The employment penalty of taking care of young children[1]

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The COVID-19 outbreak and the measures to contain the virus have caused severe disruptions to labor supply and demand worldwide. Understanding who is bearing the burden of the crisis and what drives it is crucial for designing policies going forward. Using the U.S. monthly Current Population Survey data, this paper analyzes differences in employment responses between men and women. The main finding is that less educated women with young children were the most adversely affected during the first nine months of the crisis.The loss of employment of women with young children due to the burden of additional childcare is estimated to account for 45 percent of the increase in the employment gender gap, and to reduce total output by 0.36 percent between April and November 2020.

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I. Introduction

The COVID-19 pandemic triggered an unprecedented economic crisis that required unique actions to contain the spread of the virus. Due to the infectious nature of the virus, governments had to impose lockdowns restricting economic activity and closing schools to contain its spread. Economic sectors requiring face-to-face interactions to operate, called social sectors, were hit the hardest, and so were their workers.

Women have been particularly impacted by the crisis for many reasons. Women's employment is highly concentrated in social sectors.[1] For example, in the United States, 66 percent of total female workers were employed in social sectors as of January 2020. Women are also traditionally more likely to be in charge of housework and taking care of children. In the United States, before the crisis, women spent 60 percent more time doing unpaid work than men (Alonso and others., 2019). Lockdowns and school closures have dramatically increased housework, especially for families with young children.

This paper investigates the impact of the pandemic on employment across industries, occupations, education levels, and family structures, during the first nine months of the crisis (from April to December). Using U.S. monthly Current Population Survey (CPS) data, we identify less educated women with children under 12 years old as the hardest-hit workers. To confirm this descriptive statistic, we perform an empirical investigation using a linear probability model of the individual likelihood of employment that controls for differences in sectoral employment, occupation, age, race, marital status, education, and geography.

We find that being a woman with at least one child under 12 years old reduced the probability of being employed by 3 percentage points on average compared to a man with similar characteristics during the first nine months of crisis. In contrast, we find that being a woman without a child under 12 years old reduced the probability of being employed by 1 percentage point, compared to a man with similar characteristics (less than half of the impact on women with a child under 12 years old). This result suggests that the risk of infection and intervention measures such as school closures that increased the childcare at home are key drivers in the employment gender gap observed during the COVID-19 crisis. Further, we perform a decomposition exercise and find that the extra impact on women with young children explains 45 percent of the total employment gender gap between April and December. This is a large share since employed women living with children under 12 account for only 25 percent of women's total employment before the crisis. We also find that race and level of education play an important role, as African American women with young children and less educated women with young children are among the most affected by the crisis.

Finally, to quantify the overall economic cost associated with the extra-childcare burden on female employment induced by the pandemic and measures to contain it, we use a

[1]Social sectors are defined as in Shibata (2020). Industries are considered as social if their output requires interpersonal interaction to consume, for example air transportation, veterinary services, and hospitality and tourism.

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production model calibrated to the United States. In this model, aggregate labor input is a CES combination of men's and women's total hours worked, assuming an incomplete substitutability between men and women. Estimating a counterfactual employment series for women with young children that cancels the extra burden (in employment terms) on this group of women, we find that the effect of the additional childcare on the employment of women with young children reduced total U.S. output by 0.36 percent between April and November 2020. This estimate is a lower bound, since it abstracts from other possible short-term output losses, for example, from school closures such as those related to school employees or suppliers to schools.

Our findings contribute to the growing literature on the employment effects of the COVID-19 crisis that has focused on the beginning of the pandemic (Adams-Prassl and others, 2020; Alon and others, 2020; Montenovo and others, 2020); and Shibata, 2020). Our main contribution relative to this literature is that we identify women with young children as being impacted harder by the crisis using data for the entire year of 2020. Our work suggests that the extra childcare needs played an essential role in explaining the increased in the gender employment gap since the onset of the pandemic, a fact also confirmed in a contemporaneous paper (Albanesi and Kim, 2021). Our findings support some early conjectures about the impact of the crisis on gender inequality (Dingel and others. 2020; Fabrizio and others, 2020; Georgieva and others, 2020; and Gates 2020). Our paper also contributed to literature that quantifies the impact of the employment gender gap on the economic recovery (Alon and others, 2020).

The rest of the paper is organized as follows. Section II provides a brief literature review about the impact of COVID-19 on female employment and its drivers. Section III.A provides an overview of developments in employment by gender in the U.S. since the onset of the crisis. Section III.B zooms on differences on education, sectors, and occupations. Section III.C presents the formal empirical analysis. Section IV estimates the economic costs of extra childcare and other unidentified factors that increased the gender employment gap during the crisis. Section V concludes.

II. COVID-19 AND FEMALE EMPLOYMENT: RELATED RESEARCH

Unlike previous recessions, the COVID-19 crisis employment losses have been larger for women than for men. In the literature, two main drivers have been identified that would explain why we are facing a she-cession:[2] (i) lockdown measures and fear of contagion have mostly affected sectors/occupations with a high concentration of female workers; and (ii) the closures of schools and daycare centers, and the implementation of remotely learning have increased childcare needs forcing many parents, particularly mothers, to choose between keeping their jobs or taking care of children.

[2]She-cession is a colloquial term that indicates that the crisis impacted women's employment more than men. The term is used in opposition to the use of mancession expression during the 2008 global financial crisis.

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The COVID-19 pandemic has affected industries and jobs that require direct contact, and women's employment is concentrated in these sectors. An extensive literature has shown that women's occupational and sectoral employment has contributed to increased unemployment relative to men. Adams-Prassl and others. (2020), using real-time survey evidence from the United Kingdom, the United States, and Germany in March and April 2020, find that workers who have non-teleworkable occupations are more likely to have lost their jobs; however, they find that occupation fixed effects and the percentage of task one can do from home cannot explain the total increase in the employment gender gap. Shibata (2020) and Montenovo and others. (2020), using CPS from the first months of the pandemic find that in the United States, women were more affected than men during the pandemic and part of this difference is attributable to sectors and occupations employment. Alon and others (2020) using early data from pandemic argue that women employment loss is caused by sectoral employment and childcare needs.

Women are also traditionally the primary caregiver. As documented by Alon and others (2020), before the crisis, among married parents in the United States who both work full time—represent 44 percent of married couples with children—women provided about 60 percent of childcare: men perform 7.2 hours of childcare per week versus 10.3 hours for women. When the needs of childcare increase (like during this crisis), women are more likely than men to give up their job to take care of children. Zamarro and Prados (2020), for example, find that, in the United States, women have carried a heavier load than men in the provision of childcare during the COVID-19 crisis, even while still working. Hupkau and Petrangolo (2020) find that in United Kingdom., mothers took on a larger share of increased childcare needs, even though fathers became the primary childcare providers in an important share of households. Russel and Sun (2020), using state-level variations in the United States, show that childcare center closures or imposed class size restrictions increased the unemployment of mothers of small children. Using novel mobility indicators for Italy, Portugal, and Spain at the provincial level, Caselli and others (2020a) provide further evidence that school closures and other lockdown measures have also impacted women more than men. Furthermore, using data for 128 countries, Caselli and others (2020b) show that lockdown measures tend to have statistically significant negative effect of mobility in particular for women, less educated and minorities. Beyond childcare, women are also more likely than men to provide care to others in need, including elderly and disable (American Psychological Association, 2011).

Beyond these main factors, there is also some evidence that fear of the virus is higher among women, in particular women in jobs where they would have to take additional risks in Italy (Benassi and others, 2020) and in the United States that women are more likely to perceive their working environment as riskier than men (Covington and Kent, 2020).

III. What do the Data tell Us? An Empirical Analysis

We use monthly U.S. microdata from the Current Population Survey (CPS) between January and December 2020 collected from the IPUMS-CPS database (Flood and others, 2020). The CPS is jointly sponsored by the U.S. Census Bureau and the U.S. Bureau of Labor

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Statistics (BLS) and is the primary source of labor force statistics for the population of the United States. Nationwide, comprehensive interviews of approximately 60,000 households (covering about 150,000 individuals) collect information about workers' labor force and employment status, industry, occupation, demographics, and family structure.[3] This rich dataset allows us to investigate the short-term gendered effects of the pandemic across a variety of worker types.

A. Labor Market During the Pandemic: A Gender Perspective

We begin by documenting developments in total hours worked by gender since the onset of the crisis. For each month, we select all individuals assigned as employed with positive hours worked.

Figure 1 shows that total hours worked plummeted more for women than for men in April 2020 and have partially recovered afterward at a slower pace for women than for men. Furthermore, decomposing total hours worked in average hours worked and employment, Figure 1 shows that the reduction in total hours worked reflected mostly a loss of jobs rather than a reduction of hours per worker, a regularity observed in past recessions. However, a distinguished aspect of the pandemic recession is that employment losses for women have been larger than for men.

Figure 1. Transition of Total Hours Worked by Gender, January-December 2020

Sources: Current Population Survey (CPS); authors' calculations.
Note: January 2020 is the reference point. The total hours series is plotted as the logarithm of total hours worked in the current month divided by the total hours worked in January. Formally, let H_t be the total hours worked in month t, and assume that $t = 0$ represents January. Then, the plotted total hours series is given by $\log(H_t/H_0)$. Note that this series can be decomposed into the contributions of employment and average hours worked. Let E_t and h_t be the employment level and average hours worked, respectively, in month t. Since, by definition, $H_t = h_t E_t$ for all t, then $\log(H_t) = \log(h_t) + \log(E_t)$ for all t. This equality also holds in January, so $\log(H_0) = \log(h_0) + \log(E_0)$. By subtracting one equality from the other, we get $\log(H_t/H_0) = \log(h_t/h_0) + \log(E_t/E_0)$. The bars in the figures represent the two terms on the right-hand side of the last equality.

[3]The CPS does not publish earnings data monthly.

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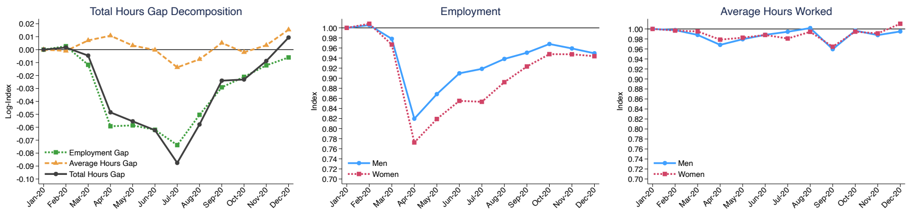
Figure 2 shows the gender gap in total hours worked decomposed by average hours worked and employment. The larger gender gap in total hours worked also reflects mostly a greater loss of jobs for women for most of the period.

Figure 2. Decomposition of the Gender Gap in Total Hours Worked, January-December 2020



Sources: Current Population Survey (CPS); authors' calculations.
Note: As in Figure 1, the employment and average hours series are plotted as the ratio between the current month value and the value in January. Formally, for each gender, we are plotting E_t/E_0 in the middle figure and h_t/h_0 in the right-most figure. For the total hours gender gap decomposition, first define $G_{H,t} \equiv H_{f,t}/H_{m,t}$ as the gender gap in total hours, $G_{E,t} \equiv E_{f,t}/E_{m,t}$ as the gender gap in employment, and $G_{h,t} \equiv h_{f,t}/h_{m,t}$ as the gender gap in average hours. Then, from the definition of the gaps, we have that $G_{H,t} = G_{h,t} \times G_{E,t}$ for all t. By taking the logarithm of the previous equality, this gives $\log(G_{H,t}) = \log(G_{h,t}) + \log(G_{E,t})$ for all t. In particular, this equality also holds in January, so we have that $\log(G_{H,0}) = \log(G_{h,0}) + \log(G_{E,0})$. By subtracting one equality from the other, we have $\log(G_{H,t}/G_{H,0}) = \log(G_{h,t}/G_{h,0}) + \log(G_{E,t}/G_{E,0})$. The three elements of the last equality are plotted in the left hand-side chart.

This change in employment reflects both an increase in unemployment and a decline in labor force participation. Total unemployment rose sharply in April. The rise in unemployment was larger among women than men, generating an increase in the unemployment gender gap. The unemployment gender gap picked in April and declined afterward, almost closing in December. Male and female labor force participation declined sharply in April recovering gradually between April and July and declining again afterwards. In contrast to trends in employment, trends in male and female labor force participation were similar, leading to larger increase in women out of labor force since female labor force participation was lower than men in January.

B. What Drives the Widened Gender Employment Gaps during the Crisis

In this section, we zoom in on workers' characteristics to gather further information on who lost jobs, and why, during the crisis. We group workers according to their education level (college or no college degree), race (white or African-American), industry (262 in total), occupation (525 in total) and family features (parents with or without young children). For the industry and occupation labels, we use the classifications of social industries and teleworkable occupations as in Shibata (2020).[4] Industries are considered social if their output requires

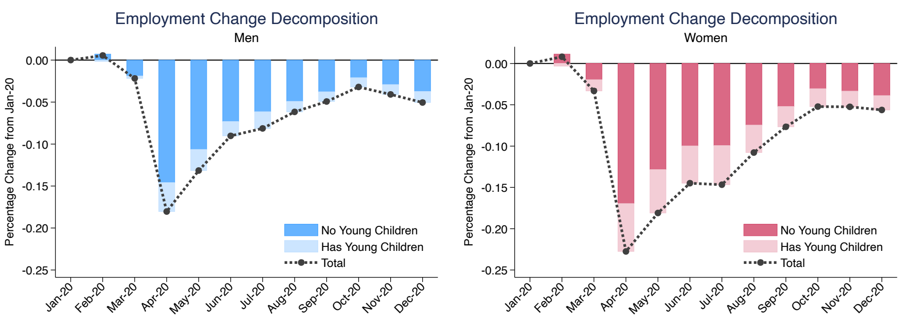
[4]Shibata (2020) adapted to the CPS the social industry classification proposed by Kaplan and others (2020) and the teleworkable classification proposed by Dingel and Neiman (2020) and Mongey and others. (2020). We thank the author for

(continued…)

interpersonal interaction to consume. Occupations are labeled as teleworkable if workers are able to work remotely. Finally, within each group, we categorize workers according to the presence or absence of young children in the household (at least one child younger than 12 years old).

We compare women and men's employment trajectories within each group of workers using January 2020 as the reference point.[5] We start by looking at the contribution to overall employment losses of women and men that came from individuals with and without young children in the household (Figure 3). For both women and men, the largest share of the employment change is explained by individuals without young children, as they represent a larger share of the population. However, in every month since the onset of the crisis, the share of job losses explained by individuals with young children is higher among women than men. From April to December, this figure averages to 32 percent for women, while 24 percent for men.

Figure 3. Employment Change Decomposition, January-December 2020



Sources: Current Population Survey (CPS); authors' calculations.
Note**:** The total employment series are plotted as the percent change from January to the current month. Formally, for each gender, we are plotting $\%\Delta E_t = (E_t - E_0)/E_0$ as the black dashed line. Let $E_{k,t}$ be the employment level of individuals with young children status k in month t, where $k = 0$ refers to those without young children and $k = 1$ to those with young children. Note that $E_t = E_{k=0,t} + E_{k=1,t}$ for every month t. Similarly, define $\%\Delta E_{k,t} = (E_{k,t} - E_{k,0})/E_{k,0}$ as the percent change for individuals with children status k. Also, let $W_k = E_{k,0}/E_0$ be the employment weight in January of those with children status k. Then, we can decompose the total employment change series as $\%\Delta E_t = W_{k=0} \times \%\Delta E_{k=0,t} + W_{k=1} \times \%\Delta E_{k=1,t}$. The darker bars represent the first term on the right-hand side of the decomposition, and the lighter bars represent the second term.

Focusing on the level of education, Figure 4 shows that women and men without college education have experienced a large decline in employment during the crisis compared with educated workers. This is particularly the case for women with young children, which have

kindly providing the data containing the CPS codes for industries and occupations already labeled as social and teleworkable, respectively.

[5]See Note in Figure 2 for a description of how the series were constructed.

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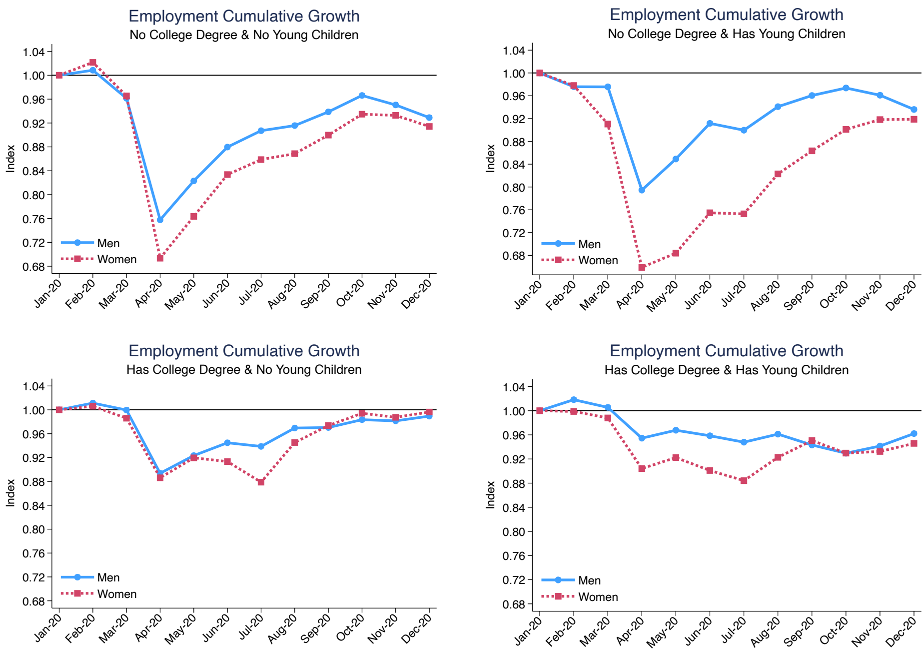
largely contributed to increase the employment gender gap. The employment gender gap among men and women without a college education and with young children is more than three times the employment gender gap experienced by men and women with a college education independently of children, and men and women without a college education and without children. Considering the high correlation between education and income levels, evidence suggests that the crisis is not only increasing the employment gender gap but is also exacerbating income inequality.

Figure 4. Employment Developments by Education Level, January-December 2020



Sources: Current Population Survey (CPS); authors' calculations.
Note: We are plotting the employment cumulative growth using January 2020 as the reference point. Formally, each point represents the ratio between the employment level of the current month and the corresponding value in January 2020.

We also look at the pattern of employment focusing on differences between race, in particular white and African-American men and women with young children. Figure 5 shows that African-American women were the most affected compared to all others.

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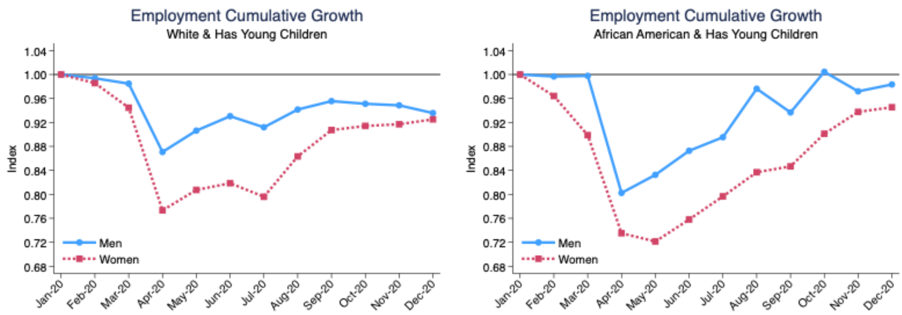
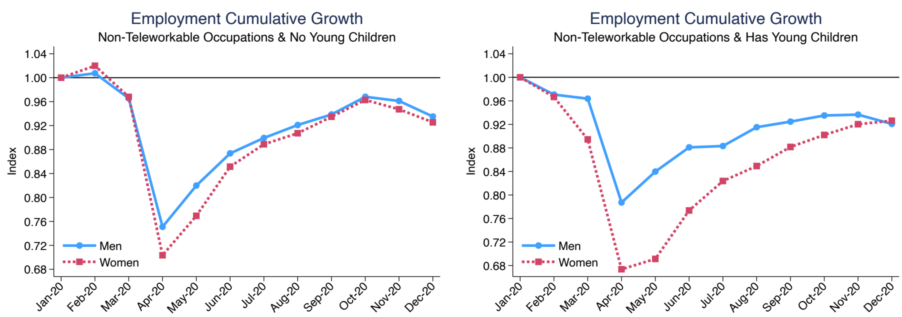
Figure 5. Employment Fluctuations by Race, January-December 2020



Sources: Current Population Survey (CPS); authors' calculations.
Note: We are plotting the employment cumulative growth using January 2020 as the reference point. Formally, each point represents the ratio between the employment level of the current month and the corresponding value in January 2020. In the appendix we present similar charts for white and African American workers without young children.

We now focus on the groups most likely to suffer large employment losses during the crisis, namely workers who hold non-teleworkable occupations or are involved in social sectors. A similar pattern is observed among workers unable to work from home (Figure 6). Employment disparities by gender are quite different between those with and without young children. In particular, mothers of young children with jobs requiring in-person interactions have experienced a much slower employment recovery compared with women without young children and to fathers of young children holding similar jobs.

Figure 6. Employment Fluctuations in Non-Teleworkable Occupations, January-December 2020



Sources: Current Population Survey (CPS); authors' calculations.
Note: We are plotting the employment cumulative growth using January 2020 as the reference point. Formally, each point represents the ratio between the employment level of the current month and the corresponding value in January 2020. In the appendix we present similar charts for teleworkable occupations.

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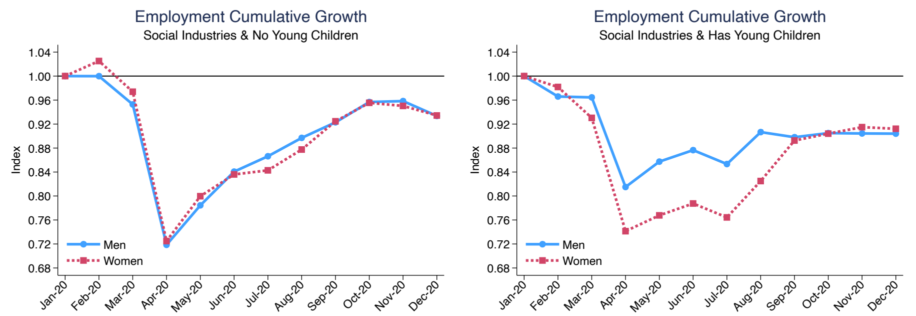
Lastly, we analyze employment development in social industries (Figure 7). Interestingly, among workers without young children, men and women involved in the same industries have experienced similar job losses, suggesting that the type of industry has been a key driver of the evolution of employment among workers without children. This has not been the case for female workers with young children, who have experienced a protracted loss of employment over several months since April compared to all other workers in similar industries.

Figure 7. Employment Fluctuations in Social Industries, January-December 2020



Sources: Current Population Survey (CPS); authors' calculations.
Note: We are plotting the employment cumulative growth using January 2020 as the reference point. Formally, each point represents the ratio between the employment level of the current month and the corresponding value in January 2020. In the appendix we present similar charts for non-social industries.

In summary, data analysis shows that women with young children have been disproportionately affected compared with other women and men in terms of employment losses, though also women without children have witnessed to a certain extent a larger loss in employment than men with similar characteristics. This points to additional childcare that women had to provide as an important driver of the increased employment gender gap. Notably, among women with young children, the less educated have experienced the largest loss of employment, suggesting that the pandemic and related lockdown measures such as school closures are not only increasing the gender employment gap but also income inequality.

C. Measuring Women's Employment Penalty: A Regression Approach

The evidence so far points to a significant employment penalty for women throughout the pandemic, especially for the mothers of young children. In this section, we corroborate these results with empirical evidence. Our approach is to run monthly linear probability models (LPM) of the employment probability on a female dummy and a set of controls.[6] Furthermore, to fully capture differentiated effects coming from the presence of young children, we run the regressions separately for those with and without young children in the household. The set of

[6]As regressions are estimated for each month separately, our results are being controlled for any seasonal variations that may have occurred during the period or that occur regularly.

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controls contains six age groups, three racial categories, six education groups, a married dummy, 17 industry groups, 12 occupation groups, a social industry dummy, a teleworkable occupation dummy, and a dummy for each state. Since regressions are performed separately for those with and without young children, the estimates of the female dummy and controls are allowed to vary between these two groups.[7]

To estimate the regression models, we considered the sample of individuals assigned by the CPS as employed, unemployed, and out of the labor force but not retired. Note that the CPS records information about industry and occupation only for the employed and unemployed, but not for those out of the labor force. To get around this and keep those out of the labor force in the sample, we created a specific industry/occupation classification for that group.

To formalize our regression specification, we index individuals by i, months by t, and the young children status by k. Let $E_{i,t,k}$ be the employment indicator, $F_{i,t,k}$ the female indicator, and $\mathbf{X}_{i,t,k}$ the vector of controls. Then, our regression equation can be stated as

$$\Pr\left(E_{i,t,k} = 1 \mid F_{i,t,k}, \mathbf{X}_{i,t,k}\right) = \alpha_{t,k} + \beta_{t,k} F_{i,t,k} + \boldsymbol{\gamma}'_{t,k}\mathbf{X}_{i,t,k} + \varepsilon_{i,t,k}, \qquad (1)$$

where $\varepsilon_{i,t,k}$ is the error term. We start by inspecting the estimates of the $\beta_{t,k}$ coefficients, which can be interpreted as the average marginal effects (AME) of being a female on the likelihood of employment. First, we want to look at the sign of the estimates. Negative values would confirm the women's employment penalty. Second, we want to compare how the estimates change over the months. More negative values since April would suggest higher penalties for women since the onset of the pandemic. Third and finally, we want to compare the sizes of the coefficients across the children or no children statuses. More negative values for the young children estimates would indicate that the required additional care for children by mothers could be a key factor for widening gender differences.

The estimates confirm findings in the previous section (Figure 8). Before the systematic lockdowns, the estimates for those without young children have no statistical significance, indicating no significant employment penalty for women. Women with young children, on the other hand, already faced a certain degree of employment penalty before the pandemic. However, with the advent of lockdowns, all the estimates become highly significant and negative, corroborating the story that severe employment penalties on women were triggered during the recession. Furthermore, there are considerable differences in the sizes of the estimated coefficients across the two children status. The estimated coefficients for the sample with young children are more negative than the ones for the sample without young children.

[7] Due to the lack of data availability, the analysis does not account for the potential impact of the UI benefits that workers with children younger than 17 received during the crisis, which could have created incentives for those individuals to delay searching for a job.

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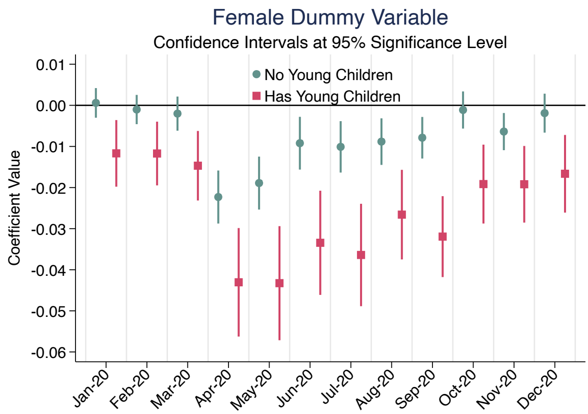
This evidences that having young children at home indeed contributed to the aggravation of employment gender disparities.

Figure 8. Regression Estimates for the Female Dummy Variable

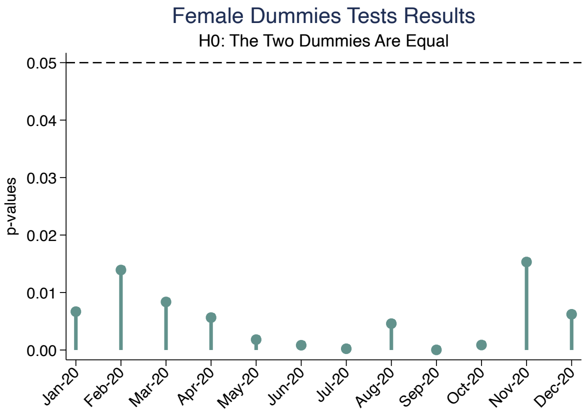


Sources: Current Population Survey (CPS); authors' calculations.
Note: We are plotting the estimates for the female dummy variables calculated from the logistic regressions described by equation (1). The points represent the estimated coefficients and the lines represent the confidence intervals at a significance level of 95 percent.

To provide further evidence on the different impacts of the crisis on women with and without young children, we test for the equality of the two female dummy coefficients each month separately.[8] We perform cross-model hypothesis tests using a seemingly unrelated estimation approach for a Wald test (Weesie, 1999). For each month, the null hypothesis is that the two female dummies are equal. The monthly p-values for the tests are presented in Figure 9. From January to December, all p-values are smaller than 5 percent, indicating that we can reject the equality of the dummies at the 5 percent level.

[8]The fact that some confidence intervals reported in Figure overlap does not necessarily imply that the two female dummies are not statistically different from each other; see Schenker and Gentleman (2001).

Figure 9. Testing the Equality of the Two Female Dummies



Sources: Current Population Survey (CPS); authors' calculations.
Note: We are plotting the p-values of the hypothesis tests in which we test for the equality of the two female dummy coefficients. The null hypothesis is that the two coefficients are equal. The tests are performed separately for each monthly regression.

The resulting figures quantify that, with the onset of the crisis, the female effect on the likelihood of employment has deteriorated in general, but much more for those with young children. We next take a closer look at the size of these differences before and after the pandemic (Figure 10). The numbers displayed between the two curves represent the differences in female AMEs between those with and without young children. Before the crisis, these differences were all less than or equal to 1.3 percentage points. However, after the start of the recession, although all AMEs became quite negative, the ones for those with young children decreased much more. The differences in all subsequent months are considerably higher than 1.3 percentage points (except for November), reaching more than 2 percentage points in five months.

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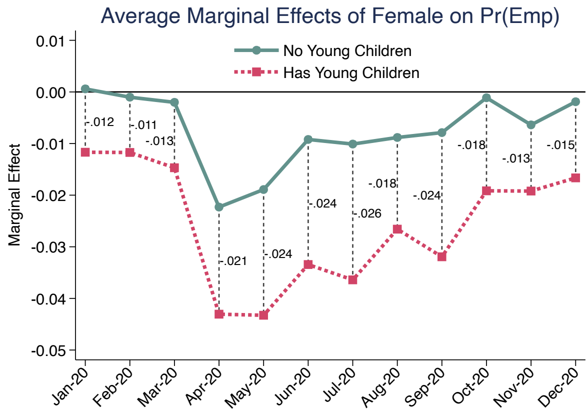
Figure 10. Comparison of Average Marginal Effects



Sources: Current Population Survey (CPS); authors' calculations.
Note: We are plotting the average marginal effects of being a female, with or without young children, on the probability of being employed. Each point can be read as follows: on average, for a certain month and young children status, being a woman increases/decreases the employment probability by the size of the plotted point.

To determine the magnitude of these figures, we compare what happened from March to April. In March, the female AME for those without young children was nearly zero, indicating there was no significant female penalty in the employment likelihood within that group. In April, the AME dropped sharply, to -0.022, indicating that, on average, women without young children had a probability of being employed of about two percentage points less than that of men. Within the group with young children, the drop in the AME was more pronounced. It went from -0.015 to -0.043, a difference of almost 3 percentage points. In summary, on average, mothers of young children began to experience a probability of being employed that was almost three percentage points lower than that of fathers of young children.

We further perform robustness checks using a sample containing only those employed and unemployed, excluding the individuals out of the labor force. We keep the same strategy of running monthly regressions separately for individuals with and without young children, using the same variables as our main exercise. In addition to the linear probability models, we also run logistic regressions for this sample.[9] The results are presented in the Appendix. For both the LPM and logistic models, the AMEs behave similarly to our main exercise. Additionally, the AME values are broadly similar in both models, though we can reject the

[9]Due to the lack of information on industry and occupation for individuals out of the labor force, we were unable to apply logistic regressions to the enlarged sample of all individuals in the labor force used in the LPM analysis.

equality of the two female dummies at 10 percent the level of significance for some months rather than at 5 percent for all months as in the LPM exercise.[10]

IV. Potential Economic Costs: Analytical Analysis

We now estimate the potential economic costs of the extra employment loss suffered by mothers of young children and women in general. A two-step analysis is performed. First, we create a counterfactual aggregate employment series assuming that women with young children had the same probability of keeping their jobs as women without children (relative to the men with similar children status). We account for changes in employment across combinations of industries, occupations, and levels of education. Second, using a model-based analysis, we use the counterfactual series of employment to quantify the output cost of employment gender gaps that emerged since the onset of the crisis. We then extend the exercise to simulate the output cost associated with the increase in the employment gender gap experienced also by women without young children (assuming that all women had the same probability of keeping their jobs as men with similar occupations, industries, level of education and children status since the crisis began).

A. Counterfactual Employment Analysis

We construct two counterfactual aggregate employment series for women that simulate two distinct scenarios in terms of women's employment. To do so, we draw on the results of the empirical analysis. The results point to generalized gender differences in employment growth, but much greater among those with young children at home. Therefore, we assume that the observed widened gender gaps can be broken down into two parts. The first one is observable among workers with and without young children and comes from factors other than the extra childcare needs or other factors related to the presence of children. We call this part the "general gap." The second part is observed exclusively among those with young children that has become particularly accentuated as school closures were imposed in most part of the country.[11] We call this second part the "extra childcare-need gap." The total gender gap in employment growth is interpreted as the sum of the "general gap" and the "extra childcare-need gap."

The first counterfactual series simulates the employment trajectory of women with young children as women with young children have experienced the same employment impact as women without young children relative to their male groups. In practice, we create this series by assuming that the "extra childcare-need gap" would be zero, and therefore the gender gaps among workers with young children would be equal to the ones we observe among workers

[10]We also estimated our main regression specification separately for those with young children between 0-5 and 6-12 years old. We did not find any significant differences between these groups.

[11]Using data from the Education Week Tracker that covers 907 school districts including the 100 largest schools district in the United States, and the largest district in each state. We find that in August, 68 percent of students were enrolled in remote learning, 19 percent in hybrid learning, and 13 percent in full in-person schools. In September, the distribution was 73 percent in remote learning, 14 percent in hybrid, and 13 percent full in-person. https://www.edweek.org/leadership/school-districts-reopening-plans-a-snapshot/2020/07

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without young children (this is done by adjusting only the employment of women with young children). The second counterfactual exercise simulates female employment growth as if the pandemic had affected women and men proportionately in the same way. In other words, this series is created by considering that gender gaps in employment growth would be zero.

Formally, for each month and gender, we group workers according to their industry, occupation, level of education, and young children status.[12] Let $E_{g,i,o,e,k,t}$ be the employment of the subgroup characterized by the gender g, industry i, occupation o, education e, young children status k, in month t. Assume that $g \in \{m, f, c\}$, where m means actual male workers, f means actual female workers, and c means counterfactual female workers, that we want to generate. Then, for each possible subgroup, the employment growth $G_{g,i,o,e,k,t}$ up to a given month considering January 2020 ($t = 0$) as the starting point is given by

$$G_{g,i,o,e,k,t} = \frac{E_{g,i,o,e,k,t}}{E_{g,i,o,e,k,0}} . \tag{2}$$

From the above growth rates, we can define the actual ($GAP^{A}_{i,o,e,k,t}$) and counterfactual ($GAP^{C}_{i,o,e,k,t}$) gender employment growth gaps, respectively, as

$$GAP^{A}_{i,o,e,k,t} = G_{m,i,o,e,k,t} - G_{f,i,o,e,k,t} \,, \tag{3}$$

$$GAP^{C}_{i,o,e,k,t} = G_{m,i,o,e,k,t} - G_{c,i,o,e,k,t} \,, \tag{4}$$

where $G_{c,i,o,e,k,t}$ is the counterfactual female employment growth that we want to generate.

In the first experiment, our counterfactual series aim to simulate the growth in employment of women with young children as if women with young children have experienced similar employment trends as women without young children. Conceptually, we estimate it by equating the counterfactual gender gap among those with young children ($GAP^{C}_{i,o,e,1,t}$) to the actual gender gap within those without young children ($GAP^{A}_{i,o,e,0,t}$):

$$GAP^{C}_{i,o,e,1,t} = GAP^{A}_{i,o,e,0,t} \,. \tag{5}$$

After some simple algebraic manipulation, we obtain the following intuitive expression for the counterfactual female employment:

$$E_{c,i,o,e,1,t} = E_{f,i,o,e,1,0}\big(G_{m,i,o,e,1,t} - (G_{m,i,o,e,0,t} - G_{f,i,o,e,0,t})\big) \,, \tag{6}$$

[12]To keep a reasonable amount of observations in each possible bin, we consider a college degree indicator for education level, a social indicator for industries, and a teleworkable indicator for occupations.

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which states that the counterfactual female employment is equal to the employment growth rate of men with young children minus the actual employment gender gap for those without young children, weighted by the women employment distribution in January 2020 ($t = 0$), where the right-hand side of the above equation is fully observable in the data.

Last, we aggregate the female counterfactual employment $E_{c,t}$ by summing over the female employment of women without children, from the data, and the counterfactual employment of women with children, calculated in equation (6). Therefore, for each month, an aggregated counterfactual series can be calculated by

$$E_{c,t} = \sum_{i,o,e} E_{c,i,o,e,1,t} + \sum_{i,o,e} E_{f,i,o,e,0,t} \,. \tag{7}$$

The second counterfactual series, as discussed, intends to simulate equal employment fluctuations among women and men. This is done by making the counterfactual gaps in gender employment growth equal to zero, that is, $GAP^{C}_{i,o,e,k,t} = 0$ for all combination of characteristics. By plugging the gap definition, equation (4), into the previous equality, we find that the counterfactual female employment series can be written as

$$E_{c,i,o,e,k,t} = E_{f,i,o,e,k,0} \times G_{m,i,o,e,k,t} \,, \tag{8}$$

where again, by construction, the actual and counterfactual series are equal in $t = 0$ for all characteristics. As before, all elements of the right-hand side of the above expression are observable in the data. But now we are constructing a new series for all female workers, regardless of the young children status. Then, the monthly aggregated series is given by

$$E_{c,t} = \sum_{i,o,e,k} E_{c,i,o,e,k,t} \,. \tag{9}$$

The results show that without the extra childcare burden, women's employment would have been on average 1.2 percentage points higher between April and December (left panel of Figure 11). The potential gains are heterogeneous across months, ranging from almost 2.2 p.p. in May to 0.17 p.p. in December. By closing the entire increase in the gender employment gap to the pre-crisis period, we find that women's employment would be 2.6 p. p. higher, on average, over the same period (right panel of Figure 11). The most considerable gains would have been in June (4.12 p.p.) and July (4.73 p.p.). As a result, by closing only the "extra childcare-need gap" we explain, on average, 45 percent of the "general gap" (1.2/2.6).[13]

[13]During June-August, many summer camps, which kids usually attend when they are out of school, were also closed due to the pandemic and related lockdown measures.

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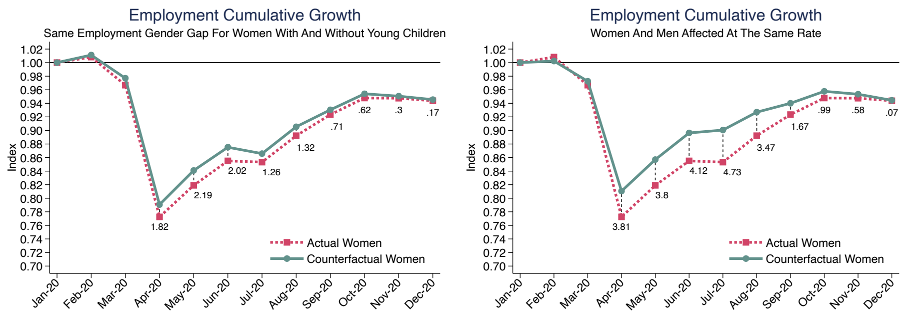
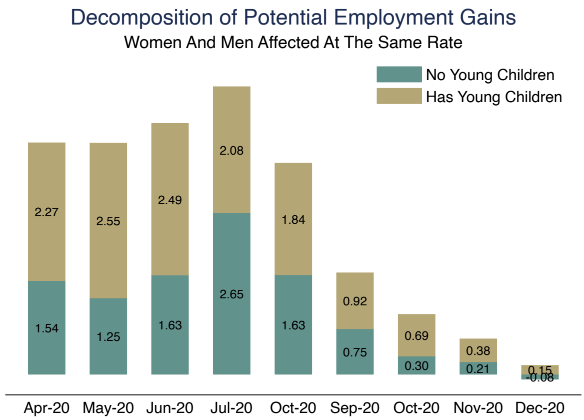
Figure 11. Actual and Counterfactual Women's Employment Fluctuations



Sources: Current Population Survey (CPS); authors' calculations.
Notes: On the left panel, we are plotting the employment cumulative growth using the counterfactual series calculated from equation (7). On the right panel, we are plotting the employment cumulative growth using the counterfactual series calculated from equation (9). All series are plotted using January 2020 as the reference point.

The potential gains from closing the entire increase in the gender gap are substantial. To better understand who would be benefiting more, we decompose the total employment gains into the gains from women with and without young children (Figure 12). We find that, on average, women with young children would account for 60 percent of the potential employment gains from April to December. This is a remarkable result since, in our sample, women with young children represent only 25 percent of total female employment.

Figure 12. Decomposition of Potential Gains When Closing the Entire Employment Gender Gap Increase



Sources: Current Population Survey (CPS); authors' calculations.
Notes: For each month, the sum of the numbers reported on each bar is equal to the spread value reported in the corresponding month on the right panel of Figure 11.

B. Economic Model: Description and Calibration

To assess the output impact of the pandemic-driven rise in the employment gender gap, we use a production-function model to simulate counterfactual output scenarios throughout the pandemic. In each month t, we assume that total output Y_t is produced using capital K_t and labor L_t as inputs into a Cobb-Douglas production function given by

$$Y_t = A_t K_t^{\alpha} L_t^{1-\alpha}\,, \tag{10}$$

where A_t is total factor productivity (TFP) and α is capital's share of output. The labor input is composed of women's and men's total hours worked combined into a CES aggregator given by

$$L_t = \left(\phi_{m,t} H_{m,t}^{\rho} + \phi_{f,t} H_{f,t}^{\rho}\right)^{1/\rho}\,, \tag{11}$$

where $H_{m,t}$ ($H_{f,t}$) is men's (women's) total hours worked, $\phi_{m,t}$ ($\phi_{f,t}$) is the weight on men's (women's) hours, and ρ captures the elasticity of substitution between the worked hours of men and women. We assume that $\phi_{m,t} + \phi_{f,t} = 1$. Note that we are allowing the hours' weights to vary over time. When simulating the model, our main focus is on the transition of output cumulative growth starting in January 2020 ($t = 0$), which can be written as

$$g_t = \frac{Y_t}{Y_0} = \frac{A_t}{A_0}\left(\frac{K_t}{K_0}\right)^{\alpha}\left(\frac{L_t}{L_0}\right)^{1-\alpha}. \tag{12}$$

The calibration of the model's parameters is done for the United States and follows the standards of the literature. We set the capital share α to 0.36, which is the 2019 figure estimated by the Conference Board Total Economy Database (TED).[14] The parameter ρ is set to 0.5 to match an elasticity of substitution of 2.0 following the estimates from Ostry and others (2018). We experiment with other values for the elasticity of substitution to assess the sensibility of our results.

To calibrate the total hours' weights in the CES aggregator, we first assume that workers are paid their marginal product every month, i.e., $w_{m,t} = \partial Y_t / \partial H_{m,t}$ and $w_{f,t} = \partial Y_t / \partial H_{f,t}$ for all t, where $w_{m,t}$ and $w_{f,t}$ are the hourly wage rates of men and women, respectively. Then, by solving for the women's weight from the female-to-male wage ratio, we get that

$$\phi_{f,t} = \frac{1}{\left(\frac{w_{m,t}}{w_{f,t}}\right)\left(\frac{H_{m,t}}{H_{f,t}}\right)^{1-\rho} + 1}. \tag{13}$$

[14]For the source, refer to https://conference-board.org/data/economydatabase.

The monthly hour ratios are calculated from the CPS microdata. The wage rate ratios are calculated using the median usual weekly earnings of full-time workers from the BLS. As the BLS only releases these numbers quarterly, we repeat the same value for all months in the same quarter.[15] Then, we can feed these data into equation (13) to backout $\phi_{f,t}$ and, consequently, the men's weight as $\phi_{m,t} = 1 - \phi_{f,t}$.

Finally, we calibrate the TFP series so that the model replicates the monthly growth rate of output from the IHS Markit Monthly Real GDP Index.[16] We use equation (12) to solve for the monthly TFP growth as a function of output, capital, and labor growth. The output growth data is given, and the labor growth series is calculated from the total hours' data and the already calibrated parameters. For capital growth, we make the conventional short-term assumption that capital is fixed and set its growth factor to 1 every month. Then, we can feed the output and labor figures into equation (12) to backout the monthly TFP growth series.[17]

C. The Output Costs of Gender Gaps

We conduct two different simulations by feeding our model with the counterfactual employment series from Section V.A. First, by using the counterfactual employment series for women generated by equation (7), we simulate output growth in a counterfactual scenario where women with young children have faced a similar employment trend as women without young children. Second, by using instead the counterfactual employment series generated by equation (9), we simulate output growth in a scenario where women and men would have been affected by the crisis at the same rate. Note that, in both experiments, we are considering the men's employment series as in the data.

As described in the previous section, the model's labor input depends on the total hours worked of men and women. However, when running the simulations, we are only varying the female employment component of the labor input. To address this issue, we use the fact that the total hours worked is equal to the product of average worked hours and total employment. Formally, women's total hours can be described as $H_{f,t} = h_{f,t}E_{f,t}$, where $h_{f,t}$ is the average worked hours and $E_{f,t}$ is the employment level. Therefore, to generate the counterfactual total hours series for women, we simply substitute the actual employment component with the counterfactual one, keeping average hours worked as in the data. Formally, the counterfactual women's total hours worked is given by $H_{c,t} = h_{f,t}E_{c,t}$, where $E_{c,t}$ is calculated from equations (7) or (9) depending on the experiment.

[15]The male-to-female wage rate ratios for each quarter are as follows: 1.239 for Q1; 1.190 for Q2; 1.233 for Q3; 1.193 for Q4. For the source, refer to https://www.bls.gov/charts/usual-weekly-earnings/usual-weekly-earnings-over-time-total-men-women.htm.

[16]For the source, refer to https://ihsmarkit.com/products/us-monthly-gdp-index.html.

[17]Note that this calibration strategy ensures by construction that the output series generated by the model is equal to that observed in the data when we feed the model with actual total hours worked.

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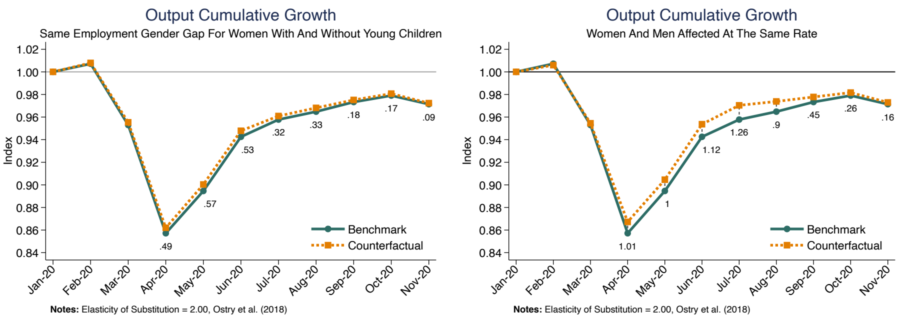
We calculate the total percentage loss in output, over the April-November period, generated by the benchmark relative to the counterfactual simulations. Note that the benchmark is the economy calibrated to replicate the data. To formally specify such a metric, let "B" and "C" be the labels of the variables of interest in the benchmark and counterfactual exercises, respectively. Then, the relative total percentage loss can be defined as

$$LOSS = 1 - \frac{\sum_{t=4}^{11} Y_t^B}{\sum_{t=4}^{11} Y_t^C} = \frac{\sum_{t=4}^{11}(Y_t^C - Y_t^B)}{\sum_{t=4}^{11} Y_t^C} = \frac{\sum_{t=4}^{11}(g_t^C - g_t^B)}{\sum_{t=4}^{11} g_t^C}, \tag{14}$$

where the last equality comes from dividing the numerator and denominator by the January output level. The loss metric is expressed by the numbers plotted in Figure 13. The results show that the total actual output, over the April-November period, was 0.36 percent lower relative to the scenario in which women with young children (relative to men with young children) were to face the same probability of keeping their jobs compared to women without young children (relative to men without young children). Also, relative to the scenario in which all women and men were affected by the crisis at the same rate, the total output was 0.81 percent lower during the same period.

Figure 13. Output Growth Simulations



Source: Authors' calculations.
Notes: We are plotting the output cumulative growth calculated from equation (13). All series are plotted using January 2020 as the reference point. Due to our calibration strategy, the output series generated by the benchmark exercise is equal to that observed in the data.

We finally perform a robustness check to assess the sensibility of our results to different values of the elasticity of substitution between women's and men's total hours worked. Our benchmark value was set to 2, following Ostry and others. (2018). We now consider two alternative figures: a larger one equal to 4.33 (Albanesi, 2020) and a smaller one equal to 1.7

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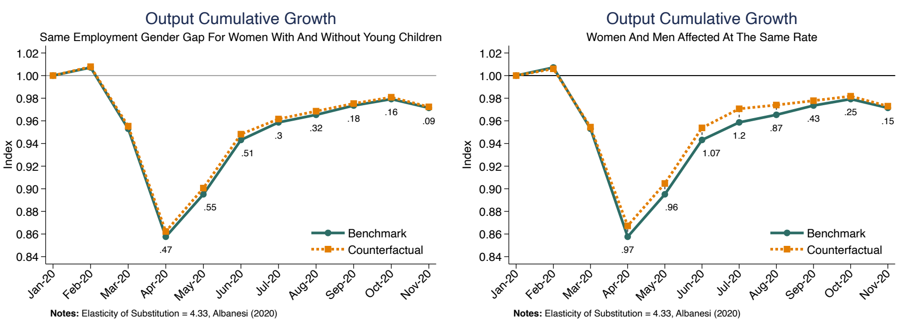
(Ghosh, 2018). Larger (smaller) values imply a larger (smaller) degree of substitution between men's and women's total hours worked.[18]

As expected, when we increase the degree of substitution between men’s and women’s total hours worked, the total output loss would be lower, equal to 0.34 percentage points, compared to 0.36 percentage points in the benchmark (left panel of Figure 14). For the scenario in which women and men are affected ate the same rate, the total output loss is estimated at 0.78 percentage points, 0.03 percentage points less than the benchmark figure (right panel of Figure 14). Alternatively, when the elasticity of substitution is lower, implying a larger complementary between men’s and women’s hours worked, the loss is slightly higher (Figure 15). The total output loss is estimated at 0.36 percentage points in the first scenario and 0.82 percentage points in the scenario where women and men are affected at the same rate.

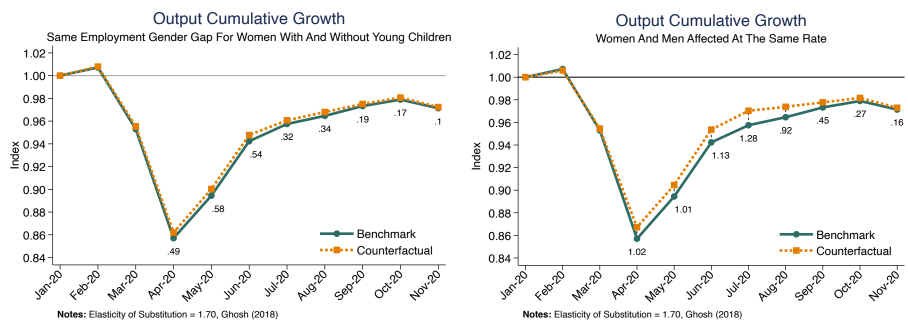
Figure 14. Robustness Checks, ES = 4.33



Source: Authors’ calculations.
Notes: We are plotting the output cumulative growth calculated from equation (13). All series are plotted using January 2020 as the reference point. Since we are not recalibrating the parameters when varying the elasticities of substitution, the output series generated by the benchmark exercise is no longer necessarily equal to that observed in the data.

[18] We do not recalibrate the parameters when performing the robustness exercises with different elasticities of substitution. Therefore, in these cases, the output series generated by the benchmark exercise is no longer necessarily equal to that observed in the data.

Figure 15. Robustness Checks, ES = 1.70



Source: Authors' calculations.
Notes: We are plotting the output cumulative growth calculated from equation (13). All series are plotted using January 2020 as the reference point. Since we are not recalibrating the parameters when varying the elasticities of substitution, the output series generated by the benchmark exercise is no longer necessarily equal to that observed in the data.

V. Conclusion

In this paper, using monthly household survey data for the United States, we analyze employment losses by population group over the first nine months of the COVID-19 crisis. Controlling for industry, occupation, and education level, we find that women with young children have been the most affected by the crisis. As schools closed at the onset of the crisis, this group of women experienced larger employment losses than other women and men with or without young children. These women also witnessed a milder recovery in employment than others over the subsequent months. Further, the less educated among these women experienced greater job losses. This suggests that the risk of infection and the measures adopted to contain it, including school closures, increased both gender and income inequalities. In addition, race seems to matter. In fact, African-American women with young children have lost more jobs than other workers.

Beyond the additional childcare burden, which account for 45 percent of the increase in the employment gender gap during the crisis, other factors also played a role. We find that women without young children have also experienced greater employment loss than men without young children working in similar industries and occupying jobs with same level of education. Another interesting finding that would require further investigation is that men with young children employed in social industries have kept their jobs more than other groups.

Next, we use empirical analysis to calculate a counterfactual series of female employment where women with young children had the same opportunities to hold their jobs as other women (relatively to men with similar children status). Feeding this counterfactual series into a production-function model, we find that the extra-childcare burden on female employment induced by the pandemic and measures to contain it, such as school closures, may have reduced total U.S. output by 0.36 percent between April and November 2020. This

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estimate neither includes other factors such as, for example, the job losses at schools themselves and employment spillovers to other (non-education) sectors, nor the possible reduction of children's human capital and future earnings (Fuchs-Schündeln and others., 2020), in particular for children of poor families (Agostinelli and others, 2020).

Our findings point to the importance of limiting the extra childcare on families, which is mostly affecting women, and prioritize measures that could alleviate such burden such as early reopening of schools. This requires investing in infrastructure and procedures to ensure a safe and sustainable reopening of schools, which should be a priority for governments. Decisions about vaccination and related priority groups should also take into consideration the urgency of school reopening. This is particularly important for countries where vaccines may not be rolled out for some time, such as in developing countries that are still in the process of procuring vaccines and making decisions regarding how to prioritize vaccine distribution.

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Appendix I: Sample Characteristics

Table 1. Employment Summary Jan-20

	No Young Children			Has Young Children			Total Sample		
	% M	% F	Share F	% M	% F	Share F	% M	% F	Share F
Occupation									
Non-teleworkable	67.7	54.8	41.9	64.8	55.7	43.9	67.0	55.0	42.4
Teleworkable	32.3	45.2	55.5	35.2	44.3	53.4	33.0	45.0	54.9
Total	*100*	*100*	-	*100*	*100*	-	*100*	*100*	-
Industry									
Non-social	54.8	34.1	35.7	60.7	33.0	33.1	56.3	33.9	35.0
Social	45.2	65.9	56.5	39.3	67.0	60.8	43.7	66.1	57.6
Total	*100*	*100*	-	*100*	*100*	-	*100*	*100*	-
Education									
No college degree	64.8	58.8	44.7	58.0	54.2	46.0	63.1	57.6	45.0
Has college degree	35.2	41.2	51.0	42.0	45.8	49.8	36.9	42.4	50.7
Total	*100*	*100*	-	*100*	*100*	-	*100*	*100*	-

Sources: Current Population Survey (CPS); authors' calculations.

Table 2. Employment Distribution by Young Children and Marital Status Jan-20

		Men (%)	Women (%)	Women (% share)
No young children	Not married	39.7	41.0	48.1
	Married	35.4	33.6	46.0
	Sub-total	*75.0*	*74.6*	-
Has young children	Not married	3.2	8.0	69.1
	Married	21.8	17.4	41.7
	Sub-total	*25.0*	*25.4*	-
Total		*100*	*100*	-

Sources: Current Population Survey (CPS); authors' calculations.

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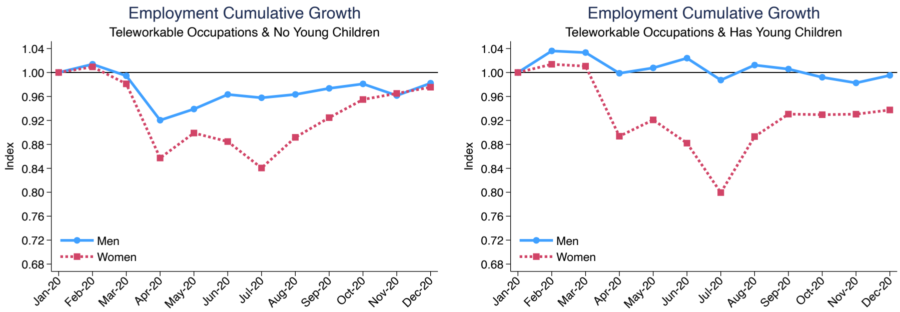
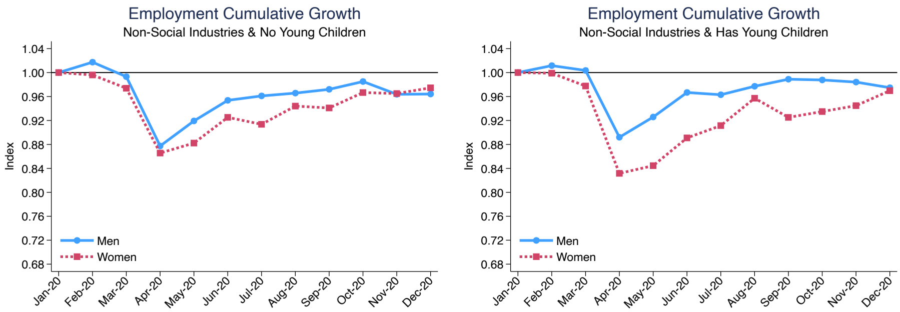
APPENDIX II: ADDITIONAL DATA ANALYSIS

Figure 1. Employment Fluctuations in Teleworkable Occupations



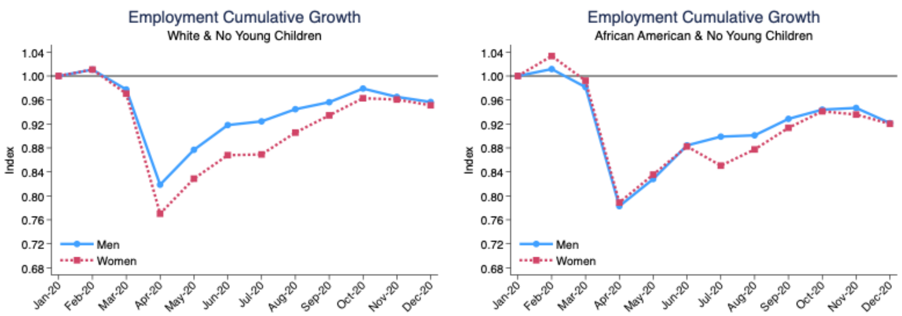
Sources: Current Population Survey (CPS); authors' calculations.
Note: We are plotting the employment cumulative growth using January 2020 as the reference point. Formally, each point represents the ratio between the employment level of the current month and the corresponding value in January 2020.

Figure 2. Employment Fluctuations in Non-social Industries



Sources: Current Population Survey (CPS); authors' calculations.
Note: We are plotting the employment cumulative growth using January 2020 as the reference point. Formally, each point represents the ratio between the employment level of the current month and the corresponding value in January 2020.

Figure 3. Employment Fluctuations by Race



Sources: Current Population Survey (CPS); authors' calculations.
Note: We are plotting the employment cumulative growth using January 2020 as the reference point. Formally, each point represents the ratio between the employment level of the current month and the corresponding value in January 2020.

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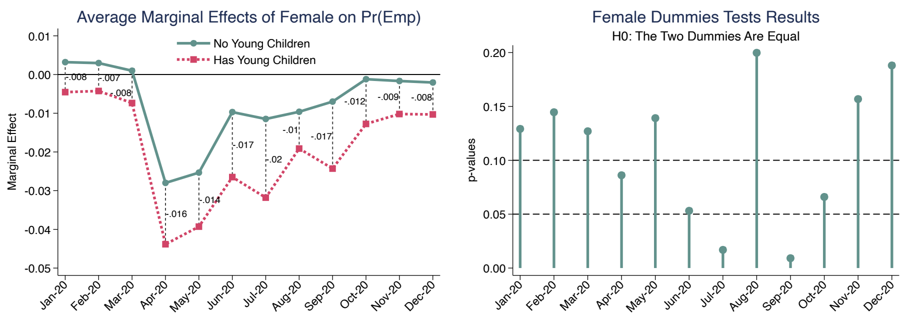
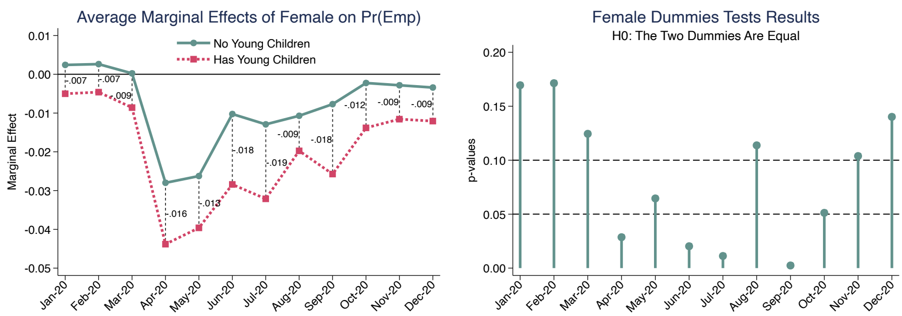
APPENDIX III: EMPIRICAL ROBUSTNESS EXERCISES

Figure 4. Linear Probability Models



Sources: Current Population Survey (CPS); authors' calculations.
Note: On the left panel, we are plotting the average marginal effects of being a female, with or without young children, on the probability of being employed. Each point can be read as follows: on average, for a certain month and young children status, being a woman increases/decreases the employment probability by the size of the plotted point. On the right panel, we are plotting the p-values of the hypothesis tests in which we test for the equality of the two female dummy coefficients. The null hypothesis is that the two coefficients are equal. The tests are performed separately for each monthly regression.

Figure 5. Logistic Regressions



Sources: Current Population Survey (CPS); authors' calculations.
Note: On the left panel, we are plotting the average marginal effects of being a female, with or without young children, on the probability of being employed. Each point can be read as follows: on average, for a certain month and young children status, being a woman increases/decreases the employment probability by the size of the plotted point. See Long and Freese (2014) for references on the marginal effects methodology for categorical dependent variables in non-linear regressions. On the right panel, we are plotting the p-values of the hypothesis tests in which we test for the equality of the two female dummy coefficients. The null hypothesis is that the two coefficients are equal. The tests are performed separately for each monthly regression.