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VETTED AND REAL-TIME PAPERS

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Covid Economics
Vetted and Real-Time Papers

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

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Ethics

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

Submission to professional journals

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

American Economic Journal, Applied Economics

American Economic Journal, Economic Policy

American Economic Journal, Macroeconomics

American Economic Journal, Microeconomics

American Economic Review

American Economic Review, Insights

American Journal of Health Economics

Canadian Journal of Economics

Econometrica*

Economic Journal

Economics of Disasters and Climate Change

International Economic Review

Journal of Development Economics

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Journal of Public Finance and Public Choice

Journal of Political Economy

Journal of Population Economics

Quarterly Journal of Economics

Review of Corporate Finance Studies*

Review of Economics and Statistics

Review of Economic Studies*

Review of Financial Studies

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

Covid Economics
Vetted and Real-Time Papers

Issue 76, 23 April 2021

Contents

Why did bank stocks crash during COVID-19?[1]

Viral V. Acharya,[2] Robert Engle[3] and Sascha Steffen[4]

We study the crash of bank stock prices during the COVID-19 pandemic. We find evidence consistent with a "credit line drawdown channel". Stock prices of banks with large ex-ante exposures to undrawn credit lines as well as large ex-post gross drawdowns decline more. The effect is attenuated for banks with higher capital buffers. These banks reduce term loan lending, even after policy measures were implemented. We conclude that bank provision of credit lines appears akin to writing deep out-of-the-money put options on aggregate risk; we show how the resulting contingent leverage and stock return exposure can be incorporated tractably into bank capital stress tests.

1. Introduction

The pandemic and subsequent government-imposed lockdowns put the liquidity-insurance function of banks for the U.S. economy to a real-life test, as firms' cash flows dropped as much as 100%, while operating and financial leverage remained sticky. As a consequence, U.S. firms with pre-arranged credit lines from banks drew down their undrawn facilities at a far greater intensity than in past recessions. Panel A of Figure 1 shows a sharp acceleration of credit-line drawdowns of publicly listed U.S. firms since March 1, 2020.[1] Within three weeks, public firms drew down more than USD 300bn, with drawdowns particularly concentrated among riskier BBB-rated and non-investment-grade firms.[2] Recent data shows that firms benefited from having access to credit lines during the pandemic when capital market funding froze (*e.g.*, Acharya and Steffen, 2020a; Chodorow-Reich *et al*., 2020; Greenwald *et al*., 2020). Banks, however, faced unprecedented aggregate demand for credit-line drawdowns when the pandemic broke out at the beginning of March 2020. Since then, banks' share prices have persistently underperformed those of non-financial firms (Panel B of Figure 1).[3]

We investigate causes and consequences of the crash of bank stocks during the COVID-19 pandemic and highlight a central role played by bank credit line drawdowns. Specifically, we ask what aspects of drawdowns – and the attendant spillovers – during the COVID-19 episode are different or similar compared to the 2008-2009 global financial crisis (GFC). Importantly, what are the possible transmission channels through which the drawdowns affected bank stock returns and ultimately banks' intermediation functions for the real

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[1] Ford Motor Company was one of the largest U.S. firms to draw down its credit lines in March 2020, withdrawing USD 15.4bn (Appendix I shows the SEC filings). It was still BBB- rated by S&P at this time. With USD 20bn in cash, credit lines make up a large part of its overall liquidity. Based on its loan contracts, Ford pays 15bps in commitment fees for any dollar-undrawn credit and 125bps once credit lines have been drawn down. Ford thus paid USD 23.1mn as long as the credit line was undrawn, and USD 192.5mn annually once the credit line was fully utilized. Importantly, once Ford was downgraded to non-investment grade, commitment fees increased to 25bps and credit spreads to 175bps, an increase of 67% and 40%, respectively.

[2] Li et al. (2020) show – using call report data – that drawdowns amounted to more than USD 500bn, likely because of private firms, even further increasing the pressure on bank balance sheets.

[3] Bank stock prices hardly recovered even after the monetary and fiscal measures (*i.e.*, after 3/23/2020) until the end of Q2 2020. However, average stock returns increased about 17% during this period (relative to a mean decline of 65% in the month before).

Figure 1. Cumulative drawdowns and bank stock prices

Panel A shows the cumulative credit line drawdowns of U.S. firms over the March 1, 2020 to July 1, 2020 period in billion USD. Panel B shows the stock prices of U.S. firms by sector, specifically firms from the energy, banking and other sectors, since Jan 1st, 2020. All variables are defined in Appendix II.

Panel A. Cumulative drawdowns (in USD bn)
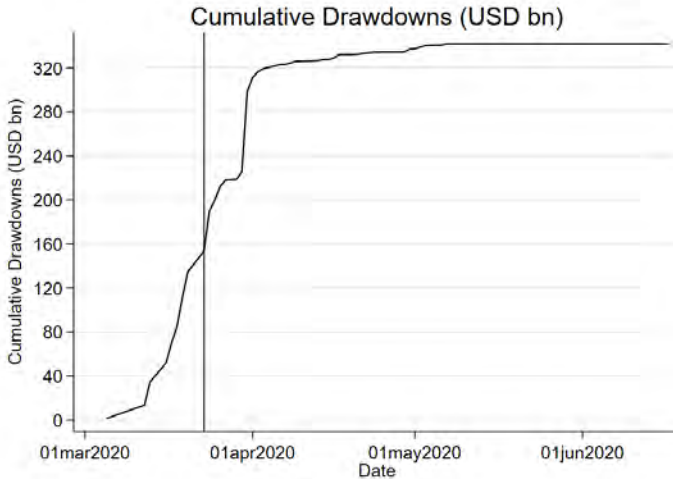
Panel B. Stock prices of banks vs. non-financial firms
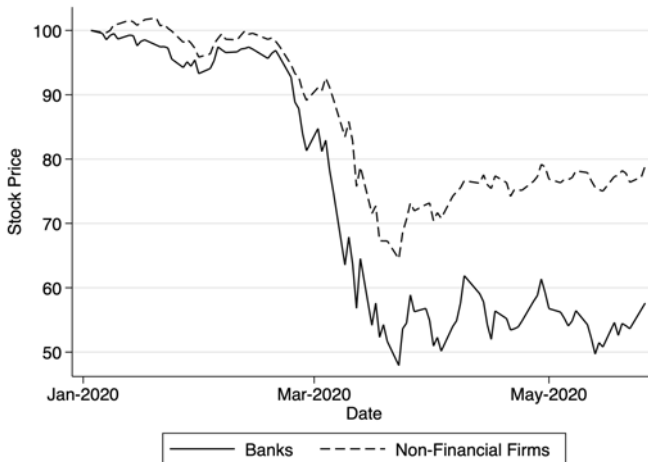
economy? Do these channels relate to the changing nature of bank regulatory standards between

the global financial crisis and the pandemic? And, if yes, how can regulation incorporate such

risks to safeguard against them in future?

These are first-order questions given the important role of banks in providing liquidity to firms and markets. To preview our results, not only did credit line drawdown rates intensify during COVID-19 compared to the GFC period and adversely impact bank stock returns, but the transmission channels also appear to be different: while funding liquidity risk of banks was a major concern during the GFC, bank capital became the binding constraint during the COVID-19 pandemic.

We construct a new measure of the balance-sheet liquidity risk of banks defined as *undrawn commitments plus wholesale finance minus cash or cash equivalents (all relative to assets)*.[4] We show that our measure of the liquidity risk of banks is important to understand the decline of bank stock prices during the first phase of the pandemic, *i.e.* from 1/1/2020 until 3/23/2020, before decisive monetary and fiscal support measures were introduced. During this phase of the pandemic, stock prices of banks with high balance-sheet liquidity risk underperformed relative to those of banks with low balance-sheet liquidity risk, controlling for market beta and key bank performance measures (capitalization, asset quality, profitability, liquidity and investments).[5] A one-standard-deviation increase in liquidity risk decreased stock returns by about 5% during this period, or 7.4% of the unconditional mean return.

We also posit alternative explanations for the observed relation between bank credit line exposure and stock returns. Liquidity risk through the provision of credit lines is likely correlated with bank portfolio composition. Specifically, credit lines in a time of stress tend to

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[4] We develop and use a comprehensive measure of liquidity risk because the relative importance of its components (unused C&I commitments or wholesale funding) might change over time. For example, bank reliance on wholesale funding has continued to decline since the global financial crisis while unused C&I loans have increased over 2017-2019.

[5] In contrast to bank capital, there is no consensus in the literature on how to measure liquidity, and those measures that have been used follow different concepts. For example, Deep and Schaefer (2004) use the difference between scaled liquid assets and liabilities, focusing on on-balance-sheet components of liquidity. Berger and Bouwman (2009) construct a comprehensive liquidity measure using on- and off-balance-sheet components. Both measures follow the concept of liquidity *creation*. Our measure focuses on liquidity *risk*, particularly during aggregate economic downturns, through credit lines and short-term wholesale funding. Bai *et al.* (2018) use on- and off-balance-sheet items to construct a measure of liquidity risk incorporating current market liquidity conditions. While their measure is more complex and reacts (contemporaneously) once market liquidity conditions deteriorate, our measure is a relatively simple (*ex-ante*) measure of bank exposure to liquidity risk. We compare our measure to both previous measures, highlighting similarities and differences in section 8 of this paper.

be drawn down from riskier borrowers, who are in a greater need of liquidity. That is, banks facing larger drawdowns may be working with riskier borrowers and in industries more vulnerable to economic crises.

We address this confounding hypothesis in a variety of ways. First, we control for portfolio risk using regulatory data from call reports including, *e.g.*, non-performing loans, real estate exposure, warehousing activities of dealer-banks, and the presence of large derivative portfolios. We also control for a measure for the market's perception of bank risk, such as an equity beta and a bank's distance-to-default. Importantly, we include detailed data on bank loan portfolio composition to isolate the effect of credit line exposures on bank stock returns. Several industries came under severe stress during the pandemic (*e.g.*, to the retail, hotel and leisure sectors). Exposure to oil prices also emerged as an important risk that might have contributed to the crash of bank stocks.[6] Moreover, bank exposures to retail credit line commitments and consumer loans were also at risk of losses when unemployment rates and furloughs rose. Using detailed bank-loan-level exposure data to these sectors sourced from the Dealscan database, we show that while these risk factors do significantly affect bank stock returns, they appear to be almost orthogonal to balance-sheet liquidity risk. Furthermore, when we include measures of a bank's capital shortfall conditional on a severe market correction (for example, *SRISK*[7], which relies in turn on *LRMES*, a measure of stock returns conditional on market downturns), which do not take into account the role of undrawn credit lines, the explanatory power for bank stock returns remains unaffected.

To summarize, while other factors are important in understanding the performance of bank stock prices at the beginning of the pandemic, the aggregate drawdown risk associated

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[6] The energy sector was severely hit when on March 9, 2020 oil prices dropped by more than 20% on a single day. Both Saudi Arabia and Russia, two of the world's largest oil producers, decided to increase their oil output considerably having failed to reach an agreement with OPEC on possible production cuts. After this oil price shock, oil price volatility increased by more than six times (to more than 100% on an annualized basis) and energy stocks crashed. Banks are heavily exposed through loans provided to this sector.

[7] See NYU Stern Volatility & Risk Institute, https://vlab.stern.nyu.edu/welcome/srisk, Acharya et al. (2016) and Brownlees and Engle (2017) for definition and estimation of LRMES and SRISK.

with bank credit lines does not appear to be captured in traditional measures of bank exposure or systemic risk. That is, a bank's "contingent leverage" associated with aggregate drawdowns is akin to a deep out-of-the-money put option that is neither captured by a bank's stock beta nor by its (long-run) marginal expected shortfall (MES), or is possibly captured only ex-post, i.e. with a lag, as the event causing aggregate drawdowns unfolds.

We then show that this cross-sectional explanatory power of balance-sheet liquidity risk for bank stock returns is *episodic* in nature. Using separate cross-sectional regressions during the months of January 2020, February 2020 and during the 3/1/2020 to 3/23/2020 period, we show that liquidity risk explains stock returns only during the last period, when firms' liquidity demand through credit line drawdowns becomes highly correlated, but not before. We then employ time-series tests for bank stock returns to shed further light on this result. Interacting our bank-level liquidity risk measure with the aggregate measure of realized cumulative credit line drawdowns, we show that (daily) bank stock returns are significantly lower when aggregate drawdowns in the economy increase and banks have more balance-sheet liquidity risk. Further, stock returns for banks with greater liquidity risk are lower, particularly when drawdowns of riskier firms accelerate. Finally, these effects reverse only after monetary policy and fiscal policy measures. There is a reversal of undrawn C&I credit lines on banks' balance sheets in Q2 and Q3 2020, but not to pre-COVID-19 levels. Consistently, we find that the episodic explanatory power of balance-sheet liquidity risk for bank stock returns also reverses following policy measures.[8]

We confirm that the episodic co-movement of stock returns and the balance-sheet liquidity risk of banks is not specific to aggregate drawdown risk during the pandemic, but was also a feature of the global financial crisis (GFC) during 2007 to 2009. We use the same cross-

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[8] Interestingly, the Fed already conducted large interventions in the repo market on 3/12/2020. The OIS-spread, a measure for liquidity conditions in financial markets, reverted already following these interventions. They were, however, insufficient to stop the further decline of bank stock prices suggesting that liquidity was not the binding constraint for banks at the beginning of the COVID-19 pandemic.

sectional tests as before and run them quarterly over the Q1 2007 to Q1 2009 period. We show that liquidity risk for banks ignited in Q3 2007, *i.e.*, in the first phase of the GFC when the Asset Backed Commercial Paper (ABCP) market froze, as documented in Acharya *et al*. (2013). Liquidity risk remained priced in the cross-section of bank stock returns (even increased in economic magnitude) until the end of Q2 2008. The Federal Reserve and the U.S. government responded to the economic fallout of the Lehman Brothers default with a variety of measures to support the banking sector, following which we do not see any effect of liquidity risk on bank stock returns. Acharya and Mora (2015) show that banks had deposit shortfalls relative to credit line drawdowns during the GFC, unlike during the pandemic. In other words, the episodic nature of liquidity risk contributing to bank stock returns during the pandemic finds similar undertones during the GFC. However, and importantly, the bank stock price decline at the beginning of the COVID pandemic was caused by credit line drawdown risk, while it was caused by rollover risk (wholesale finance) during the GFC period. Our liquidity risk measure for banks spans both of these risks.

Next, we examine the reasons why bank stock prices were particularly sensitive to undrawn C&I credit lines when the pandemic broke out. Does funding liquidity to source new loans become a binding constraint for banks as deposit funding does not keep pace with credit line drawdowns (the "funding channel")? Or, does the drawdown of credit lines lock up bank capital against term loans and impair bank loan origination, preventing banks from making possibly more profitable loans (the "capital channel")?[9] The capital channel is driven by

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[9] For the banks that provided credit lines to Ford Motors (as described in our introductory example in footnote 1 above), these commitments were (in aggregate) a USD 15.4bn off-balance-sheet C&I loan commitment as of 12/31/2019. The capital treatment of their commitment depends on whether banks follow the standardized (SA) or internal ratings-based (IRB) approach for credit risk. Under Basel III, the standardized approach differentiates between irrevocable and revocable commitments. Revocable commitments carry a credit conversion factor (CCF) of 10% and irrevocable commitments (with a maturity of more than 12 months) a CCF of 50%. Assuming an 8% capital requirement, an undrawn credit line thus requires funding in the range of 0.8% to 4% for banks using the SA. For IRB banks – as applies to most of our sample banks – the CCF might be considerably lower (Behn *et al*., 2016). In other words, a bank might need to fund 90% or more of the required capital when a credit line is drawn down and becomes a balance-sheet loan, which adversely impacts other business activities, particularly in an aggregate downturn.

borrower risk: drawn credit lines become term loans that may default in future, need to be funded with capital, and in particular, capital requirements increase with borrower risk.

To distinguish between these channels, we construct two proxies: (1) *Gross drawdowns* as the percentage change in credit line drawdowns; and (2) *Net drawdowns* as the percentage change in drawdowns minus the change in deposit funding. Holding gross drawdowns fixed, our measure of net drawdowns helps us understand the importance of changes in bank deposits for bank stock returns. We find that while bank stock returns during 1/1/2020 to 3/23/2020 are particularly sensitive to gross drawdowns, they do not load significantly on net drawdowns. Importantly, a higher level of bank capital buffer attenuates the negative effect of gross drawdowns on stock returns.

Overall, these results suggest that at the onset of the pandemic bank capital and not bank liquidity appears to have been perceived as the binding constraint causing liquidity risk to adversely affect bank stock returns. In other words, the pandemic fallout for banks differs from that during the GFC when banks struggled on the liquidity front to meet drawdowns (Acharya and Mora, 2015).

The development of credits spreads at the beginning of the pandemic suggests that this phenomenon might have also affected loan market originations. We plot the time-series of credit spreads in the loan and bond market over the Q1 2019 to Q3 2020 period in Figure 2. In particular, we plot the loan-bond differential (Panel A of Figure 2) and find that that difference between loan and bond spreads increased from about 2.5% to 3.5% following the outbreak of COVID-19 and remained highly elevated, particularly driven by loans to riskier firms (Panel B of Figure 2). Bond spreads, however, reverted back almost to pre-COVID levels (not shown).

Figure 2. Loan vs. bond spreads

This figure shows the time-series difference of loan and bond spreads (Panel A) and splitting loans by rating classes (Panel B). The loan spread is calculated based on Saunders et al. (2021). The sample is based on all loans traded in 2020 that were traded in the U.S. Leveraged Loan Index (LLI) obtained from Leveraged Commentary and Data (LCD) and matched to secondary loan market trading data from Refinitiv. The sample thus comprises about 1,000 U.S. non-financial firms. 3% of the observations are unrated (based on S&P ratings), 25% are CCC-C rated, 54% are B rated, 15% BB rated and 3% BBB rated. Loans with a "D" rating are dropped from the sample (35 firms). Loan spreads are constructed using a weighted average (with facility amounts as weights). Bond spreads are constructed based on Gilchrist and Zakrajšek (2012) and obtained from the Federal Reserve website.

Panel A. Loan-bond-spread difference
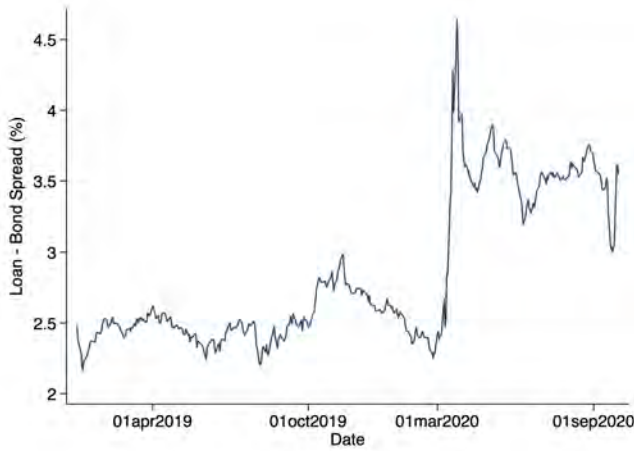
Panel B. Loan-bond-spread difference (by rating)
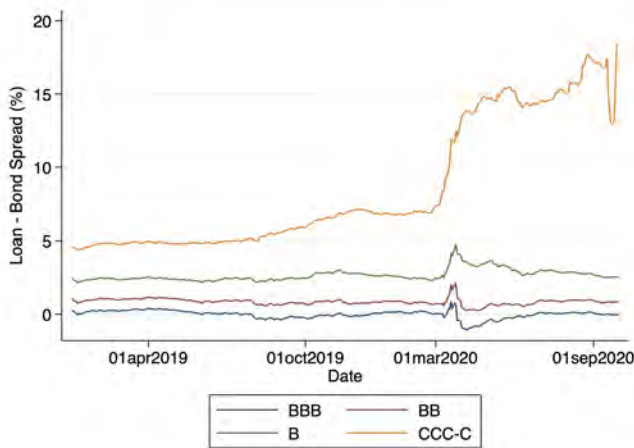
This is consistent with the interpretation that bank health was materially affected by the pandemic, and not just temporarily, impacting the access of firms to bank loans as well as the cost of bank credit.[10]

Investigating new loan originations, we find that banks with large credit-line drawdowns indeed significantly reduced their supply of newly issued loans.[11] We use a Khwaja and Mian (2008) estimator and aggregate our data at a borrower x bank x loan type x month level, collapse the sample into a pre- and post-COVID-19 period (where "post" is the period after 4/1/2020), and saturate the estimation with borrower x bank x loan type fixed effects. We show that both the number of loans as well as loan amounts are lower for banks with both higher gross and net drawdowns after the breakout of the COVID-19 pandemic. Importantly, when we estimate separately the effect on term loans and credit lines (using borrower x bank fixed effects), term loan originations are substantially lower for banks with higher gross drawdowns, whereas new credit line commitments decrease mainly for banks with higher net drawdowns. This confirms that gross drawdowns reduce the capital available to banks and thus term lending, whereas banks experiencing net drawdowns are reluctant to take on additional liquidity risk, but they can issue term loans as long as they have capital to provide for them. Overall, there appear to be long-term real consequences because of banks' contingent leverage materializing from a drawdown of credit lines during an aggregate shock.

A final key question is how can policy makers address aggregate drawdown risk in an *ex-ante* manner? Our results suggest that liquidity risk regulation is insufficient to address fully the consequences of aggregate credit line drawdown risk, as its consequences transmit to the real economy via bank capital channel, and therefore, regulators may have to (also) raise capital

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[10] The Senior Loan Officer Survey of the Federal Reserve also shows that at the end of Q3 2020, about 75% of loan officer reported a tightening of bank lending standards for small and medium/large firms.

[11] The theoretical literature argues that a key function of bank capital is to absorb risk, i.e., more capital facilitates bank lending. Bhattacharya and Thakor (1993), Repullo (2004), von Thadden (2004), and Coval and Thakor (2005), among others, argue that capital increases risk-bearing capacity. Allen and Santomero (1998) and Allen and Gale (2004) show that banks with less capital might have to dispose of illiquid assets when facing an adverse shock.

requirements as a function of exposure to such risk. One possible way is for regulators to add the effect of credit line drawdowns to stress tests and require banks to fund these exposures with equity.[12]

In our last step, we quantify the capital shortfall that arises due to banks' balance-sheet liquidity risk and show how it can be incorporated tractably into bank stress tests. Acharya *et al*. (2012), Acharya *et al*. (2016) and Brownlees and Engle (2017) developed the concept of SRISK, a measure of the capital shortfall of a stressed aggregate market correction (*e.g.*, 40% decline in the S&P 500 index), measured relative to an 8% requirement in terms of market value of equity to debt plus market value of equity. This measure, however, does not account for the impact of credit lines, which are off-balance-sheet or contingent liabilities. Given our results, such an impact can be broken down into two components. First, contingent liabilities enter banks' balance sheets as realized liabilities during periods of stress. Using drawdown data during the COVID crisis, the GFC and the 2000-2003 recession, we extrapolate the expected drawdown in a stress scenario with a 40% market correction based on each of these three stressed periods. We find the expected (incremental) drawdown rates in such a stress scenario to be in the range 11% to 23%. Using these expected drawdown rates, we calculate the additional equity capital that would be required to maintain adequacy against higher realized liabilities in periods of stress. Second, we have to account for the negative episodic effect of liquidity risk on bank stock prices during periods of stress. Using the loadings from our cross-sectional regressions of bank stock returns on balance-sheet liquidity risk during the COVID-19 crisis, we estimate the additional equity shortfall of banks based on their end of Q4 2019 market values of equity.

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[12] We find that banks do not account for aggregate drawdown risk in fees or spreads when initiating new loan contracts. Moreover, drawdowns do also not appear to be constrained through covenants. We investigate all loan amendments during the post-COVID period and find that not a single loan amendment was initiated through a covenant violation. On the contrary, banks and firms regularly negotiated a covenant relief period early in the pandemic. In other words, contractual mechanisms also did not attenuate aggregate drawdowns at the start of the pandemic.

Summing both components, we show that the additional capital shortfall for the U.S. banking sector as a whole due to balance-sheet liquidity risk amounted to more than USD 270bn as of 12/31/2019 in a stress scenario of a 40% correction to the global stock market with the top 10 banks contributing about USD 230bn. The incremental capital shortfall of the top 10 banks is about 1.5 times larger than the capital shortfall estimate without accounting for contingent liabilities.

The paper proceeds as follows. We first describe the related literature. In Section 3, we present the data. In Section 4, we describe our measure of balance-sheet liquidity risk and investigate the effect of liquidity risk on bank stock returns. We investigate the liquidity measure's components in Section 5. Section 6 analyzes the funding *vis-à-vis* the capital channel and also studies the consequences for the real economy. Section 7 illustrates how to incorporate episodic liquidity risk of bank balance sheets in stress tests and assess capital shortfalls. We provide a discussion of our results in section 8. Section 9 concludes.

2. Related literature

Our paper relates to the literature highlighting the role of banks as liquidity providers. Kashyap *et al*. (2002) proposed a risk-management motive to understand the unique role of banks as liquidity providers to both households and firms. As long as demand for deposits and loans is not too highly correlated, banks can pool both types of customers and hold less (costly) liquid assets. Gatev and Strahan (2006) build this idea and argue that banks can insure firms even against systematic declines in liquidity because of deposit inflows during crises. Ivashina and Scharfstein (2010) provide evidence of an acceleration of credit-line drawdowns during the 2007-2009 crisis as well as an increase in deposits. Acharya and Mora (2015) show that during the 2007-2009 crisis – in which the banking system itself was at the center of the crisis – banks faced a crisis as liquidity providers and could only perform this role because of significant support from the government. Li *et al*. (2020) show that during the COVID-19 crisis, aggregate

deposit inflows were sufficient to fund the increase in liquidity demand. Acharya and Steffen (2020b) use simulations based on drawdown scenarios from prior crises and arrive at similar conclusions. Kapan and Minoiu (2020) show that banks exposed to larger credit-line drawdowns reduce lending. None of these papers, however, explores the implications of banks as liquidity providers for bank stock returns when drawdowns affect bank capital availability for other intermediation functions, and especially when the realized risk is aggregate in nature.

There is a growing literature on the implications of COVID-19 for corporate finance, and the use of credit lines in particular. Chodorow-Reich *et al*. (2020) show that drawdowns of credit lines came exclusively from large firms during the first phase of the pandemic and document that banks did not honor commitments to smaller firms. Greenwald *et al*. (2020) also show that particularly large firms used their credit lines and banks with larger drawdowns reduced term lending to small firms more relative to other banks. Darmouni and Siani (2020) show that a large percentage of credit lines were repaid through bond issuances in Q2 and Q3 2020. By examining both gross drawdowns and net (of deposit inflows) drawdowns, we demonstrate that credit-line drawdowns reduce banks' franchise value because of binding capital constraints. While banks with higher gross drawdowns reduce term lending, banks with higher net drawdowns reduce credit line originations.[13]

Other papers consider stock price reactions to the COVID-19 pandemic, emphasizing the importance of financial policies (Ramelli and Wagner forthcoming), financial constraints and the cash needs of affected firms (Fahlenbrach, Rageth, and Stulz 2020), changing discount rates because of higher uncertainty (Gormsen and Koijen 2020, Landier and Thesmar 2020), and social-distancing measures (Pagano, Wagner and Zechner 2020). These papers focus on

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[13] Other papers explore the determinants of credit-line drawdowns in previous crises. Ivashina and Scharfstein (2010) document an acceleration of credit line drawdowns during the 2007-2009 crisis; their evidence is consistent with ours. Berg *et al*. (2016) show that credit lines are more likely to be used if a borrower's economic performance deteriorates, particularly for non-IG and unrated firms. Berg *et al*. (2017) show that U.S. firms' drawdown behavior is particularly sensitive to the overall market return. We show that pandemic drawdowns have been more intense but similar in spirit.

stock prices of non-financial firms, not banks. Demirguc-Kunt *et al*. (2020) investigate the bank stock market response to the COVID-19 pandemic and policy responses globally. They highlight that the effectiveness of policy measures was dependent on bank capitalization and fiscal space in the respective country. We focus instead on the implications of credit line drawdowns for bank stock returns and the consequences for bank lending.

Our paper also contributes methodologically to the literature on bank stress tests. After the 2007-2009 crisis, a variety of measures were developed to quantify the systemic risk of the banking sector. In addition to the SRISK measure of Acharya *et al*. (2012), Acharya *et al*. (2016) and Brownlees and Engle (2017), which we discussed in the introduction, Adrian and Brunnermeier (2015) develop the concept "CoVaR", which measures the risk to the financial system conditional on a bank being in distress. These measures, however, do not look at the role of contingent liabilities of banks or their episodic impact on bank returns; we show how these important considerations can be embedded into bank stress tests.

3. Data

We collect data for all publicly listed bank-holding companies of commercial banks in the U.S. To construct or main dataset, we follow Acharya and Mora (2015) and drop all banks with total assets below USD 100mn at the end of 2019 and also only keep those banks that we can match to the CRSP/Compustat database. All financial variables (on the holding-company level) are obtained from the call reports (FR-Y9C) and augmented with data sourced from SNL Financial. We keep only those banks for which we have all data available for our main specifications during the COVID-19 pandemic, which limits our sample to 127 U.S. bank-holding companies (accounting for about 80% of all outstanding credit lines).[14] All variables are explained below or in Appendix II.

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[14] Berger and Bouwman (2009), among others, document that off-balance-sheet credit commitments are important for large banks, but not medium-sized and small banks. The smaller number of banks in our dataset is a

We obtain daily stock returns for our sample banks from CRSP. We manually match these banks to the Thomson Reuter Dealscan database to obtain loan-level exposure data for the banks in our data set. For some tests and statistics, we use secondary market data about different industry sectors (*e.g.*, the oil or retail sector) from Refinitiv. We obtain information about a bank's systemic risk from the Volatility and Risk Institute at NYU Stern. Other market information is downloaded from Bloomberg (*e.g.* oil volatility (CVOX), VIX, S&P 500 market return).

4. Can balance-sheet liquidity risk explain bank stock returns?

4.1. Balance-sheet liquidity risk of banks

To construct our measure of balance-sheet liquidity risk, we collect bank balance sheet information as of Q4 2019 from call reports and construct three key variables associated with bank liquidity risk following Acharya and Mora (2015): (1) *Unused Commitments*: The sum of credit lines secured by 1-4 family homes, secured and unsecured commercial real estate credit lines, commitments related to securities underwriting, commercial letter of credit, and other credit lines (which includes commitments to extend credit through overdraft facilities or commercial lines of credit); (2) *Wholesale Funding*: The sum of large time deposits, deposits booked in foreign offices, subordinated debt and debentures, gross federal funds purchased, repos, and other borrowed money; (3) *Liquidity*: The sum of cash, federal funds sold and reverse repos, and securities excluding MBS/ABS securities. All variables are defined in Appendix II.

We construct a comprehensive measure of bank balance-sheet liquidity risk (*Liquidity Risk*):

$$Liquidity\ Risk = \frac{Unused\ Commitments + Wholesale\ Funding - Liquidity}{Total\ Assets}$$

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consequence of changes in reporting requirements over time (*i.e.* an increase in the size threshold above which banks have to provide specific information).

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Figure 3. Bank balance-sheet liquidity risk

This figure shows the time-series of balance-sheet *Liquidity Risk* over the Q1 2010 to Q3 2020 period. We measure *Liquidity Risk* as undrawn commitments plus wholesale finance minus cash or cash equivalents (all relative to assets). All variables are defined in Appendix II.

Panel A. Liquidity risk
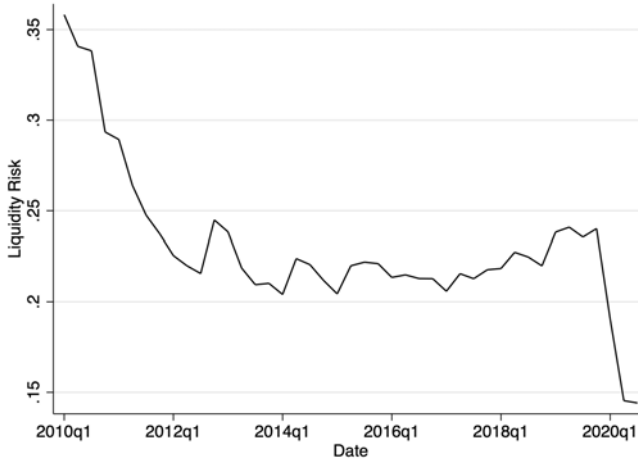
Panel B. Components of liquidity risk
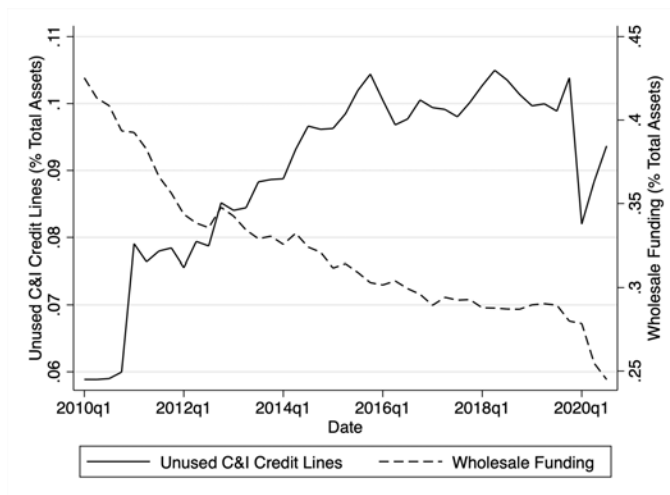
Figure 3 shows the time-series of the mean of *Liquidity Risk* (using our sample banks and weighted by total assets) quarterly since January 2010 as well as its components, i.e. *Unused C&I Credit Lines* and *Wholesale Funding*, both relative to total assets. *Liquidity Risk* has decreased since Q1 2010 to a level of about 20% relative to total assets (Panel A of Figure 3). In 2017, *Liquidity Risk* started to increase until Q4 2019, *i.e.* before the start of the COVID-19

pandemic. At the beginning of the pandemic in Q1 2020, liquidity risk dropped about 40% and continued to decline somewhat in Q2 and Q3 of 2020.

Panel B of Figure 3 shows the components. The decrease is driven by the declining share of wholesale funding relative to total assets that accelerated during the COVID-19 pandemic. Since 2017, the marginal increase in the importance of unused C&I loans has been larger than the marginal decline in wholesale funding exposure and *Liquidity Risk* started to increase again. The large decline of *Liquidity Risk* during the first quarter in 2020 was driven by the decrease in unused C&I credit lines consistent with the increase in drawdowns documented in Figure 1 above. We saw an immediate reversal of *Unused C&I Credit Lines* in Q2 and Q3 2020; however, not to pre-COVID-19 levels, pointing to a partial repayment of credit lines by U.S. firms. In Online Appendix B, we show that particularly non-investment grade rated firms did not repay their credit lines, likely as they only gradually regained access to capital markets as documented by Acharya and Steffen (2020). Banks experience only limited capital relief when high-quality firms repay their credit lines, with possible implications for their lending and investment activities. We investigate the importance of unused C&I credit lines for the stock price crash of U.S. banks as well as their lending activities further in this paper.

4.2. Methodology

To show that balance-sheet liquidity risk is priced in the cross-section of bank stock returns, we run the following ordinary-least-squares (OLS) regressions:

$$r_i = \alpha + \gamma LiquidityRisk_i \; + \textstyle\sum \beta \, X_i + \varepsilon_i \tag{1}$$

We compute daily excess returns (r_i), which we define as the log of one plus the total return on a stock minus the risk-free rate defined as the one-month daily Treasury bill rate. *X* is a vector of control variables (*e.g.*, bank balance-sheet characteristics) that have been shown to affect

bank stock returns. All control variables are measured at the end of 2019 and capture key bank performance measures (*capitalization*, *asset quality, profitability, liquidity and investments*) that prior literature has shown to be important determinants of bank stock returns (*e.g.*, Fahlenbrach et al., 2012; Beltratti and Stulz, 2012). More specifically, these variables include among others: a bank's *Equity Beta*, constructed using monthly data over the 2015 to 2019 period and the S&P 500 as market index, the natural logarithm of total assets (*Log(Assets)*), the non-performing loans to loan ratio (*NPL/Loans*), the equity-asset-ratio (*Equity Ratio*), *Non-Interest Income*[15], return-on-assets (*ROA*) and the deposit-loan-ratio (*Deposits*). All variables are described in detail in Appendix II and are shown in the regression specifications in the sections below. Standard errors in all cross-sectional regressions are heteroscedasticity robust.

4.3. Descriptive evidence

We first investigate graphically whether differences in *ex-ante* liquidity risk across banks can explain their stock price development since the outbreak of COVID-19. We classify banks into two categories, with high or low balance-sheet liquidity risk using a median split of our *Liquidity Risk* variable. We then create a stock index for each subsample of banks indexed at 1/2/2020 using the (market-value weighted) average stock returns of banks in each sample. The difference between both subsamples is shown in Panel A of Figure 4. Bank stock prices collapsed as the COVID-19 pandemic started at the beginning of March 2020. Consistent with the idea that liquidity risk explains bank stock return, we find that banks with higher liquidity risk perform worse than other banks. In Panel B of Figure 4, we show bank stock returns on our measure of *Liquidity Risk*. The regression line through the scatter plot has a negative (and statistically significant) slope. That is, banks with higher *Liquidity Risk* had lower stock returns in the cross-section of our sample banks.

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[15] Demsetz and Strahan (1997) use non-interest income to net interest income ratio as a measure how bank holding companies rely on off-balance sheet activities more broadly (*e.g.* through derivatives contracts).

Figure 4. Stock prices and liquidity risk of U.S. banks

This figure shows stock prices of U.S. banks with *Low* or *High Liquidity Risk*. We measure *Liquidity Risk* as undrawn commitments plus wholesale finance minus cash or cash equivalents (all relative to assets) and use a median split to distinguish between banks with *Low* vs. *High Liquidity Risk*. Panel A shows the stock prices of both group of banks indexed at Jan 1, 2020, Panel B shows the difference between the stock prices (in percentage point). Panel B plots bank stock returns during the March 1 – March 23, 2020 period on *Liquidity Risk*. All variables are defined in Appendix II.

Panel A. Bank stock returns
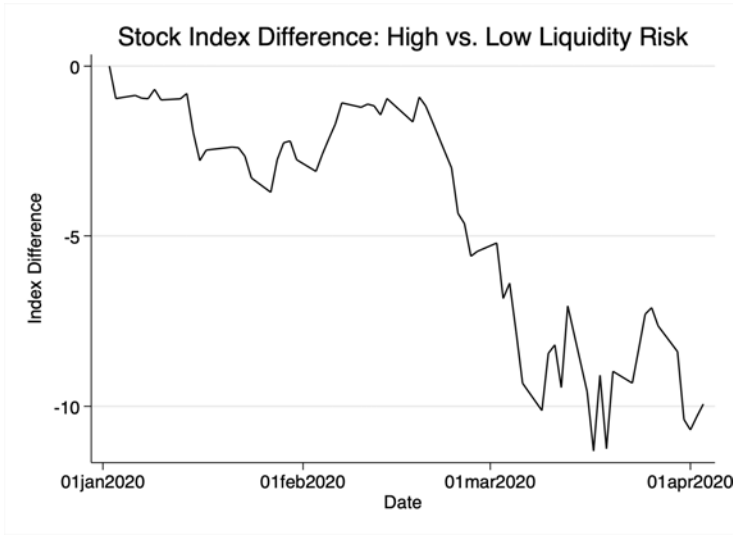
Panel B. Bank stock return and liquidity risk
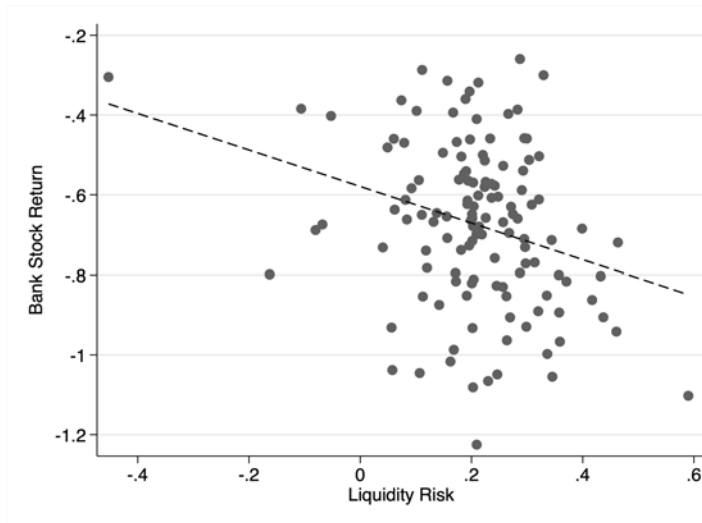
Panel A of Table 1 shows the stock returns of the firms in our sample for different periods, January 2020, February 2020 and the 3/1/2020 to 3/23/2020 period, and we calculate excess returns over these time periods. The average excess return is negative in all periods, ranging from -7.9% in January 2020 to -47.1% during the period 3/1/2020 to 3/23/2020 (and even -67.5% from 1/1/2020 to 3/23/2020).

Table 1. Descriptive statistics
Table 1 shows descriptive statistics of the variables included in the cross-sectional regressions. All variables are defined in Appendix II.

Panel A. Bank stock returns

Variable	Obs.	Mean	Std. dev.	Min	Max
Return January 2020	127	-0.079	0.039	-0.181	0.024
Return February 2020	127	-0.125	0.037	-0.194	0.011
Return 3/1-3/23 2020	127	-0.471	0.184	-1.084	-0.131
Return 1/1-3/23 2020	127	-0.675	0.204	-1.225	-0.260

Panel B. Bank characteristics

Variable	Obs.	Mean	Std. dev.	Min	Max
Liquidity Risk	127	0.209	0.128	-0.453	0.590
Unused LC / Assets	127	0.081	0.051	0.000	0.263
Liquidity / Assets	127	0.117	0.079	0.029	0.513
Wholesale Funding / Assets	127	0.132	0.075	0.013	0.544
Beta	127	1.173	0.310	0.390	2.313
NPL / Loans	127	0.007	0.007	0.000	0.044
Non-Interest Income	127	0.227	0.118	0.005	0.732
Log(Assets)	127	16.785	1.267	14.638	21.712
ROA	127	0.012	0.003	0.003	0.020
Deposits / Loans	127	1.124	0.338	0.756	4.272
Income Diversity	127	0.445	0.213	0.010	0.993
Distance-to-Default	127	3.648	0.522	1.859	5.060
Loans / Assets	127	0.702	0.113	0.196	0.899
Deposits / Assets	127	0.766	0.062	0.549	0.874
Idiosyncratic Volatility	127	0.202	0.044	0.121	0.417
Real Estate Beta	127	0.555	0.193	-0.266	1.136
Primary Dealer	127	0.031	0.175	0.000	1.000
Derivatives / Assets	127	0.648	2.515	0.000	19.565

Panel B of Table 1 shows descriptive statistics of bank characteristics as of Q4 2019. In addition to the control variables used in our regression, we also provide summary statistics of *Liquidity Risk* and its components. All these risk measures appear to be economically relevant. For example, the average *Liquidity Risk* is 0.209, the average bank has unused C&I loan commitments of about 8.1% relative to total assets, and the average wholesale funding-asset-

ratio is 13.2%. The average bank has a beta of 1.2 measured against the S&P 500 (*i.e.* it broadly resembles the U.S. economy) and a capitalization (equity-asset ratio) of 12%. We have omitted a discussion of the other variables but include their summary statistics to facilitate the interpretation of our estimates in the coming sections.

4.4. Multivariate results

The estimation results for regression (1) are reported in Panel A of Table 2.

As a dependent variable we use bank stock returns measured as excess returns in 1/1/2020 to 3/23/2020, *i.e.* the first phase of the current COVID-19 pandemic and before the decisive fiscal and monetary interventions. In column (1), we only include *Liquidity Risk* and *Equity Beta* and show that banks with a higher *ex-ante* balance-sheet liquidity risk and (as expected) high beta have lower stock returns during this period. When we add the different control variables, the coefficient of *Liquidity Risk* becomes, if anything, economically stronger and the explanatory power of the regressions increases as well (by more than 50% from column 1 to column 5). Economically, a one standard deviation increase in *Liquidity Risk* reduces stock returns during this period by about 5%. The other control variables behave as expected (focusing on those that turn out to have significant explanatory power): banks with more non-performing loans (*NPL/Loans*), lower return-on-assets (*ROA*), lower *Distance-to-Default* and higher deposit ratios (Deposits/Assets) have lower stock returns during this period.[16]

A possible explanation for bank stock returns during this period could be a large exposure to the real estate sector (as measured using a *Real Estate Beta*), large warehouses as banks act as dealer banks (*Current Primary Dealer Indicator*) or larger derivative portfolios (*Derivates/Assets*). Our regressions show, however, that stock returns do not load significantly on these factors (columns 3 to 4) once the other control variables are accounted for.

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[16] Gatev and Strahan (2006) show that banks with large credit-line commitments are also high deposit banks.

Table 2. Liquidity risk and bank stock returns

This table reports the results of OLS regressions of U.S. bank' beta adjusted stock returns over the 1/1/2020 – 3/23/2020 period with different set of control variables. Panel A shows baseline results sequentially adding control variables (as described in Table 1 and defined in Appendix A). P-values based on robust standard errors are in parentheses. All variables are defined in Appendix II.

	(1)	(2)	(3)	(4)	(5)
Liquidity Risk	-0.363***	-0.341*	-0.526**	-0.538**	-0.531**
	(0.003)	(0.072)	(0.010)	(0.016)	(0.011)
Equity Beta	-0.266***	-0.271***	-0.122	-0.123	-0.107
	(0.000)	(0.000)	(0.112)	(0.113)	(0.186)
NPL / Loans		-6.641***	-4.728**	-4.671**	-3.618*
		(0.001)	(0.034)	(0.050)	(0.095)
Equity Ratio		0.206	-1.017	-0.996	-0.758
		(0.790)	(0.240)	(0.294)	(0.440)
Non-Interest Income		0.0231	-0.218	-0.212	-0.148
		(0.894)	(0.368)	(0.405)	(0.564)
Log(Assets)		0.00892	-0.0299*	-0.0295	-0.0444*
		(0.588)	(0.097)	(0.169)	(0.065)
ROA		8.735	13.56**	13.41**	11.83*
		(0.110)	(0.041)	(0.048)	(0.090)
Deposits / Loans		0.0262	0.0289	0.0279	0.0445
		(0.594)	(0.631)	(0.654)	(0.496)
Income Diversity			0.191	0.189	0.133
			(0.198)	(0.217)	(0.386)
Distance-to-Default			0.0695**	0.0722*	0.0781**
			(0.045)	(0.052)	(0.030)
Loans / Assets			0.115	0.128	0.149
			(0.735)	(0.713)	(0.672)
Deposits / Assets			-0.841**	-0.815*	-0.938*
			(0.038)	(0.094)	(0.061)
Idiosyncratic Volatility			-0.733	-0.733	-1.232***
			(0.156)	(0.169)	(0.008)
Real Estate Beta			-0.00554	-0.00561	0.126
			(0.968)	(0.968)	(0.326)
Current Primary Dealer Indicator				-0.0652	0.0557
				(0.677)	(0.748)
Derivatives / Assets				0.00551	-0.00722
				(0.626)	(0.595)
Credit Card Commitments /Assets					0.580
					(0.154)
Consumer Loans / Assets					0.162
					(0.699)
R-squared	0.243	0.334	0.392	0.392	0.424
Number obs.	127	127	127	127	126

It could also be that those banks with high unused C&I credit lines are also those with high retail credit card commitments. Given the potential stress in the retail sector due to *e.g.* lay-offs and furloughs, our *Liquidity Risk* measure might pick up these effects. We collect each bank's exposure (though we could not identify this clearly for one bank in our sample) to off-balance-sheet credit card commitments and add this to our regression model. This variable does not enter significantly in our regression (column 5), more importantly, the coefficient on

Liquidity Risk remains unchanged. Using on-balance-sheet *Consumer Loans / Assets* does not change our results either.

5. Understanding balance-sheet liquidity risk of banks

Our previous results show that the liquidity risk of banks helps to explain bank stock returns during the first phase of COVID-19. The pandemic started in western economies at the beginning of March 2020; before then, firms had no problems accessing liquidity. But at the beginning of March 2020, it became a major concern for most firms (*e.g*., compare the increase in aggregate drawdowns in Figure 1 above).[17] Does liquidity risk also become apparent as an explanatory risk factor when aggregate drawdown risk increased? Which components of *Liquidity Risk* matter and how important are undrawn C&I credit lines relative to, *e.g.* wholesale funding, during the COVID-19 pandemic? Did the fiscal and monetary response help attenuate aggregate drawdown risk? And, is this pattern unique for the COVID-19 pandemic or do we observe the same dynamic repeatedly during episodes of aggregate drawdown risk? These are the questions we set out to address in this section.

5.1. Does balance-sheet liquidity risk have an impact on bank stock returns?

Panel A of Table 3 shows the estimation results from equation (1) separately for the three periods.

The coefficient estimates for January 2020 are shown in columns 1 to 2, for February 2020 in columns 3 to 4 and for the 3/1/2020 to 3/23/2020 period in column 5 to 6, with and without the control variables described above. During the first two months in 2020, bank stock returns do not load significantly on liquidity risk. However, during the March 1 to March 23 period, it emerges as an important risk factor, *i.e*., banks with higher balance-sheet liquidity risk had significantly lower stock returns during this period. The coefficient increases from -0.05 (January 2020) to -0.472 (3/1/2020 to 3/23/2020). At the same time, the R^2 more than

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[17] Refinitiv surveyed banks as to the key risks (investment grade) corporate clients were concerned about in March 2020. The key risks mentioned include cash flow impact, availability and access to liquidity, and access to future capital, highlighting the aggregate demand for credit-line drawdowns at the beginning of the pandemic.

doubles from January to March 2020 suggesting that *Liquidity Risk* has substantially more explanatory power after COVID broke out.

Table 3. Liquidity risk and bank stock returns by month

This table reports the results of OLS regressions of U.S. bank' realized stock returns during January 2020 (columns (1)-(2), February 2020 (columns (4) to (4)) and 1-23 March 2020 (columns (5) to (6)). Regressions with control variables are based on column (4) in Panel A of Table 2. Panel B reports the results of the regression of U.S. banks' daily stock returns on *Liquidity Risk* interacted with natural logarithm of cumulative drawdowns from credit line by U.S. firms until this day over the 1 – 23 March 2020 period. We include all firms (column (1)), the BBB-rated firms only (column (2)), then focus on non-investment grade rated firms (column (3)) and then on unrated firms (column (4)). We always include the contemporaneous return of the S&P 500 and bank fixed effects. P-values based on robust standard errors are in parentheses. All variables are defined in Appendix II.

Panel A. Cross-sectional test

	(1)	(2)	(3)	(4)	(5)	(6)
	January 2020		February 2020		3/1-3/23/2020	
Liquidity Risk	-0.0254	-0.0521	-0.0001	-0.0138	-0.338***	-0.472**
	(0.231)	(0.208)	(0.997)	(0.739)	(0.002)	(0.020)
Equity Beta	-0.0112	-0.0200	-0.0404***	-0.0002	-0.214***	-0.103
	(0.362)	(0.212)	(0.000)	(0.985)	(0.002)	(0.190)
Controls		Yes		Yes		Yes
R-squared	0.0167	0.157	0.113	0.282	0.211	0.359
Number obs.	127	127	127	127	127	127

Panel B. Time-series test

Dependent Variable: Banks' Daily Stock Returns (3/1 – 3/23/2020)				
	(1)	(2)	(3)	(4)
Liquidity Risk x Log(Cumulative Total Drawdowns)	-0.007**			
	(0.031)			
Liquidity Risk x Log(Cumulative BBB Drawdowns)		-0.017***		
		(0.002)		
Liquidity Risk x Log(Cumulative NonIG Drawdowns)			-0.0091**	
			(0.024)	
Liquidity Risk x Log(Cumulative Not Rated Drawdowns)				-0.014***
				(0.01)
S&P 500	1.194***	1.203***	1.193***	1.193***
	(0.000)	(0.000)	(0.000)	(0.000)
Bank Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.632	0.630	0.632	0.630
Number obs.	2595	2465	2595	2465

Time-series evidence. Using time-series regressions, we show aggregate drawdowns can explain bank stock returns with high *ex-ante* exposure to *Liquidity Risk* during the 3/1/2020 to 3/23/2020 period. We run the following time-series regression:

$$r_{i,t} = \alpha + \gamma LiquidityRisk_i \ x \ Drawdowns_t + \beta \ r_{S\&P,t} + \mu_i + \varepsilon_{i,t} \qquad (2)$$

We interact *Liquidity Risk* with the natural logarithm of the realized daily aggregate credit line drawdowns (*Log(Cumulative Total Drawdowns)*) and add the daily realized return of the S&P 500 stock index ($r_{S\&P,t}$) as well as a bank fixed effect (μ_i). We use Newey-West standard errors. The results are reported in Panel B of Table 3.

Column 1 shows total aggregate credit-line drawdowns. We aggregate credit-line drawdowns across BBB-rated firms (column 2, non-investment-grade rated firms (column 3) and unrated firms (column 4).[18] Bank (daily) stock returns are significantly lower when aggregate drawdowns in the economy increase and banks have more balance-sheet liquidity risk. Stock returns for banks with greater liquidity risk are lower, particularly when drawdowns of riskier firms accelerate. Overall, both our cross-sectional and time-series tests suggest that bank balance-sheet liquidity risk can episodically explain bank stock returns, emerging in an aggregate downturn with an increase aggregate liquidity demand for credit lines.

5.2. Components of liquidity risk and bank stock returns

Figure 2 shows that *Liquidity Risk* decreased since the global financial crisis but has increased again since 2016. This increase is driven by a surge in unused C&I credit lines, while wholesale funding (a major driver of liquidity risk during the GFC) continued to decrease relative to total assets. In the next step, we split *Liquidity Risk* into its components to investigate their differential impact on bank stock returns during the first phase of the pandemic. The results are reported in Table 4. We include all control variables described in model (5) in Panel A of Table 2.

We first include only *Unused C&I Loans / Assets* (column 1), then add *Liquidity / Assets* (column 2) and then add *Wholesale Funding / Assets* (column 3) to the regression model. The results suggest that *ex-ante* balance-sheet liquidity risk of banks is driven by banks' exposure to unused C&I loans. Bank stock returns load significantly on this factor while the coefficients

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[18] Due to the high correlations between cumulative credit-line drawdowns across different rating classes, common variance inflator tests reject using them together in a single regression.

on both wholesale funding and liquidity are economically small and statistically insignificant. In other words, banks' exposure to unused C&I loans are key to understanding bank stock returns during the early stages of the pandemic.

Table 4. Components of liquidity risk and portfolio risk

This table reports the results of OLS regressions of U.S. bank' beta adjusted stock returns over the 1/3/2020 – 3/23/2020 period on the different components of Liquidity Risk with control variables as in column (4) in Panel A of Table 2. We add the different components sequentially in columns (1)-(3) and add exposure to the oil & gas industry (column (4)) and other sectoral exposures (to hotel, leisure and retail industry) as additional control variables (column (5)). We add *SRISK/Assets* as additional control (column (6)). All oil & gas and sectoral exposures are based on loans reported in DealScan and thus available only for a subset of banks. SRISK is only available for banks in the vlab database. These regressions include a dummy for banks for whom we do not find exposure data (unreported). P-values based on robust standard errors are in parentheses. All variables are defined in Appendix II.

	A. Components of liquidity risk			B. Portfolio risk		
	(1)	(2)	(3)	(4)	(5)	(6)
Unused C&I Loans / Assets	-1.278***	-1.308***	-1.383***	-1.148**	-1.012**	-1.278***
	(0.002)	(0.002)	(0.001)	(0.013)	(0.043)	(0.002)
Liquidity / Assets		0.284	0.293	0.204	0.153	0.347
		(0.376)	(0.357)	(0.541)	(0.642)	(0.273)
Wholesale Funding / Assets			-0.349	-0.401	-0.349	-0.290
			(0.430)	(0.376)	(0.440)	(0.462)
Equity Beta	-0.140**	-0.135*	-0.124*	-0.107	-0.122*	-0.0841
	(0.043)	(0.052)	(0.089)	(0.132)	(0.096)	(0.205)
Oil Exposure				-2.187***	-2.000**	
				(0.009)	(0.012)	
Other Sectoral Exposures					-4.763	
					(0.194)	
SRISK /Assets						-7.173**
						(0.016)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.386	0.390	0.393	0.417	0.425	0.450
Number obs.	127	127	127	127	127	127

5.4. Bank portfolio composition

An alternative explanation for our results could be that liquidity risk through the provision of credit lines is correlated with bank portfolio composition, which our current proxies for bank risk might not fully capture. Specifically, exposure to oil price risk is an important (macro) risk factor that might have also contributed to the crash of bank stocks. After the oil price shock on March 9, 2020, the market performance of the oil & gas sector

considerably deteriorated.[19] But also other sectors were particularly impacted by the pandemic, *e.g.*, the retail, leisure, and hotel & gaming industry. Banks with large exposures to these sectors might experience larger stock price declines that could correlate with *Liquidity Risk*. We evaluate a bank's exposure to the oil & gas and other sectors using its loan exposures as of 12/31/2019. We obtain this data from Thomson Reuters LPC and allocate loan amounts among syndicate banks following the prior literature (*e.g.*, Ivashina, 2009). We construct a new variable, *Oil Exposure / Assets*, which is the sum of a bank's active loan exposures to oil & gas firms scaled by total assets. Similarly, we construct a similar measure of exposures to firms in the retail, leisure, and hotel & gaming industry, add all these exposures and scale them by total assets (*Other Sectoral Exposures / Assets*).

In columns 4 and 5, we add oil exposure and other sectoral exposure to the hotel, leisure and retail industry (all scaled by total assets) to the regression model. All oil & gas and sectoral exposures are based on loans reported in DealScan and thus are available only for a subset of banks, therefore we include a dummy for those banks for which we could not find exposure data (unreported). The results show that banks with larger exposures to oil and the other sectors experienced lower stock returns during the first phase of the pandemic. Stock returns still load significantly on *Unused C&I Loans / Assets*.[20] In column 6, we add *SRISK/Assets* as an

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[19] We provide some descriptive evidence consistent with this in Online Appendix A. Figure A.1 shows the performance of the oil & gas sector *vis-à-vis* other sectors directly affected by the pandemic (*e.g.*, retail, leisure and hotel & gaming) using returns from loans traded in the secondary market in these sectors. While the returns in the loan market declined substantially in all sectors, the loan return of oil & gas and mining firms significantly underperformed the other sectors even after the announcement of the interventions by the Fed on March 23, 2020. Figure A.2 shows the time-series of oil-price volatility using the CVOX oil price volatility index. While oil price volatility increases episodically during economic downturns (*e.g.*, during the global financial crisis (GFC) of 2007 to 2009), the European sovereign debt crisis (2011-2012), and the oil & gas crisis in 2015-2016), volatility increased by more than six times (to more than 100% on an annualized basis) around March 9, 2020 and energy stocks crashed.

[20] We also compute a bank's beta with respect to the oil and other sectors (based on Fama-French (FF) 49 industry portfolios) over the 12-month period prior to the pandemic and included these betas as proxy for bank exposures. *E.g.*, the correlation between the beta with respect to the oil sector and banks' Dealscan exposure to the oil sector is about 50%. Estimating regression (1) using the beta does not change the coefficient of *Liquidity Risk*. Interaction the exposure beta with the realized performance in March of the same FF industry portfolio does not change our results.

additional control.[21] These regressions also include a dummy for banks for which we do not find exposure data or no SRISK (unreported). Banks with higher systemic risk have lower stock returns but the coefficient on *Unused C&I Loans / Assets* does not change. Overall, liquidity risk from undrawn credit lines appears to be almost orthogonal to bank portfolio risk.

5.4. Reversal of the effect of liquidity risk on bank stock prices

Our previous tests show that liquidity risk explains bank stock returns during the first few weeks of the COVID-19 pandemic, *i.e.* before the monetary and fiscal response in the U.S. toward the end of March 2020. In a related paper, Acharya and Steffen (2020) show that capital market funding became immediately available after the Federal Reserve interventions on 3/23/2020, stopping the credit line drawdowns for all but the riskier firms as bond market access still eluded them. Aggregate demand for credit-line drawdowns attenuated after the interventions. Importantly, Figure 2 above suggests that high-quality firms have repaid credit lines, leading to a reversal of unused C&I credit lines on bank balance sheets. We thus investigate whether we observe a similar reversal in bank stock prices following the Fed interventions in March 2020.

Panel A of Table 5 shows descriptive statistics of bank stock returns in April, May and June 2020 and during the 3/24/2020 to 6/30/2020 period. On average, the stock prices of our sample banks increased about 18% over the entire period, which is small given the mean drop of 67% during the 1/1/2020 to 3/23/2020 period. In other words, bank market capitalization has, on average, hardly improved during this period.

We show the results from regressions of bank stock return on *Liquidity Risk* and its components and all control variables used before in Panel B of Table 5. Columns 1 and 2 show the results for April and May 2020. While the coefficient of *Liquidity Risk* is positive, it does not significantly enter into the regression. The effects somewhat increase in June 2020 and become statistically significant (column 3) but are driven largely by banks with high *ex-ante*

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[21] Cai et al. (2018) show that SRISK correlates with bank portfolio composition and common asset exposure of banks.

unused C&I lines of credit (column 4). The results become less noisy when measuring stock returns over the 3/24/2020 to 6/30/2020 period and also become economically larger (columns 5 and 6). That is, stock prices of those banks that have experienced a large decline in stock price during the first weeks of the pandemic recover somewhat in the period after the Fed interventions. The control variables (not reported) show a similar reversal.

Table 5. Reversal of liquidity risk
Panel A reports descriptive statistics of bank stock returns for the months April, May and June 2020 (i.e. after the Federal Reserve Intervention on 3/23/2020). Panel B reports the results of OLS regressions of U.S. bank' realized stock returns on *Liquidity Risk* and its components during each of these months (columns (1) – (4)) and then for the period 3/24/2020 – 6/30/2020 (columns (5) and (6)). Control variables as in column (5) in Panel A of Table 2 are included. P-values based on robust standard errors are in parentheses. All variables are defined in Appendix II.

Panel A. Descriptive statistics of bank stock returns

	Obs.	Mean	Std Dev	Min	Max
Return April 2020	127	.1140058	.0878647	-.0997281	.385558
Return May 2020	127	-.039326	.080453	-.4542235	.2228914
Return June 2020	127	.0119836	.0528534	-.1546759	.1514292
Return 3/24-6/30/2020	127	.1793604	.1639635	-.3437108	.6509989

Panel B. Pricing of liquidity risk

	(1)	(2)	(3)	(4)	(5)	(6)
	Apr 20	May 2020	June 2020		3/24/-6/30/2020	
Liquidity Risk	0.0876	0.0626	0.103*		0.349	
	(0.466)	(0.433)	(0.089)		(0.108)	
Unused C&I Loans / Assets				0.282**		1.048***
				(0.028)		(0.002)
Liquidity / Assets				-0.0920		0.0260
				(0.461)		(0.949)
Wholesale Funding / Assets				-0.0185		1.206***
				(0.908)		(0.004)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.284	0.275	0.154	0.174	0.304	0.358
Number obs.	127	127	127	127	127	127

Taken together, our results so far show that liquidity risk episodically explains bank stock returns. Banks with high liquidity risk experience a stock price decline during the first phase of the COVID-19 pandemic, *i.e.* during a period of high aggregate liquidity demand for bank credit lines of firms, but not before. This relationship even reverses when capital market funding became available after policy stabilization measures were put in place.

5.4. Balance-sheet liquidity risk of banks during the global financial crisis (2007-2009)

Are these effects specific to the COVID-19 pandemic or did liquidity risk also episodically explain stock returns during other times of aggregate risk? To understand whether this effect occurs more generally during aggregate economic downturns, we first plot the stock prices of banks with high vs. low *Liquidity Risk* over the 2007 to 2009 period in Figure 5.

Figure 5. Stock prices and liquidity risk of U.S. banks (2007-2009)

This figure shows stock prices of U.S. banks with *Low* or *High Liquidity Risk* for the Jan 2007 to Jan 2010 period. We measure *Liquidity Risk*.as undrawn commitments plus wholesale finance minus cash or cash equivalents (all relative to assets) and use a median split to distinguish between banks with *Low* vs. *High Liquidity Risk*. Panel A shows the stock prices of both group of banks indexed at Jan 1, 2007, Panel B shows the difference between the stock prices (in percentage point). All variables are defined in Appendix II.
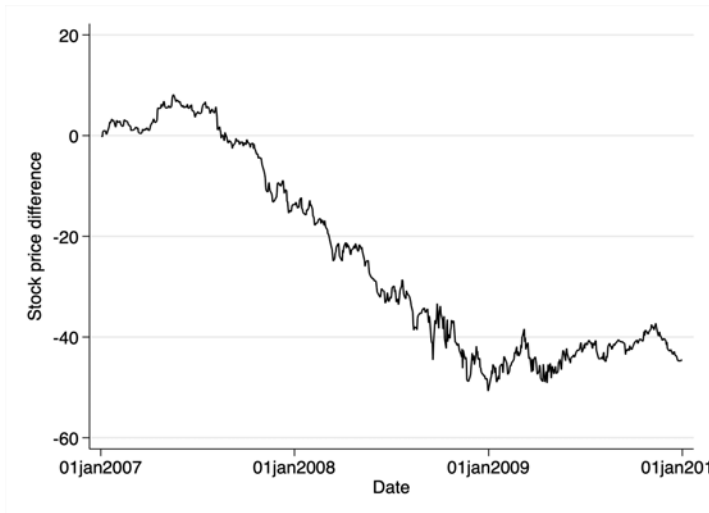
We plot the difference in the stock price of banks with high vs. low *Liquidity Risk* indexed at January 1, 2007. The difference in the stock price performance between the two groups of banks is even more pronounced than during the COVID-19 crisis. Stock of banks with high *Liquidity Risk* fell by about 40% more than banks with low liquidity risk between Q2 2007 and Q3 2008. The stock price performance was then similar until the end of 2009.

We construct our variables at the end of Q4 2006 for our regressions in 2007 and at the end of Q4 2007 for the regressions in 2008 and 2009, and estimate equation (1) quarterly over the Q1 2007 to Q1 2009 period. The estimation results are reported in Table 6.

Table 6. Liquidity risk and bank stock return during the Global Financial Crisis

This table reports the results of OLS regressions of U.S. bank' realized stock returns separately for each quarter during the Q1:2007 to Q4:2009 period. We show the estimates of the coefficients of the *Equity Beta* of a bank with the S&P 500 (measured monthly over the 2002-2006 period for tests in 2007 and measured monthly over the 2003-2007 period for tests in 2008/9), but include also all other control variables shown in Panel A of Table 2 (column (5)). P-values based on robust standard errors are in parentheses. All variables are defined in Appendix II.

Panel A. Liquidity risk and bank stock returns

	(1) Q1 2007	(2) Q2 2007	(3) Q3 2007	(4) Q4 2007	(1) Q1 2008	(2) Q2 2008	(3) Q3 2008	(4) Q4 2008	(5) Q1 2009
Liquidity Risk	0.0118	-0.00262	-0.0727**	-0.153***	-0.160**	-0.262***	0.0469	-0.102	-0.00628
	(0.745)	(0.962)	(0.046)	(0.002)	(0.017)	(0.000)	(0.644)	(0.386)	(0.956)
Equity Beta	-0.00720	-0.0117	0.0114	-0.0389	0.0377	-0.0707	0.0299	-0.0586	-0.149
	(0.612)	(0.588)	(0.439)	(0.167)	(0.073)	(0.008)	(0.336)	(0.080)	(0.000)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.030	0.030	0.084	0.173	0.097	0.326	0.338	0.201	0.301
Number obs.	225	225	225	225	237	237	237	237	237

Panel B. Components of liquidity risk

	(1) Q3 2007	(2) Q4 2007	(3) Q1 2008	(4) Q2 2008
Unused C&I Loans / Assets	-0.222**	-0.0263	-0.360***	-0.188
	(0.013)	(0.864)	(0.000)	(0.375)
Wholesale Funding / Assets	-0.0360	-0.151**	-0.0436	-0.162*
	(0.519)	(0.037)	(0.602)	(0.077)
Liquidity / Assets	0.0678	0.277***	0.171	0.523***
	(0.363)	(0.002)	(0.125)	(0.000)
Equity Beta	0.0247	-0.0622	0.0355	-0.0779
	(0.108)	(0.030)	(0.087)	(0.003)
Controls	Yes	Yes	Yes	Yes
R-squared	0.104	0.221	0.123	0.339
Number obs.	225	225	237	237

In Panel A of Table 6, we confirm that liquidity risk also episodically explained bank stock returns during the GFC, *i.e.*, during the 2007 to 2009 period. Liquidity risk for banks rose

in Q3 2007, *i.e.*, in the first phase of the GFC, when the Asset Backed Commercial Paper (ABCP) market froze as documented in Acharya *et al*. (2013). Thereafter, liquidity risk remained priced in the cross-section of bank stock returns (and even increased in economic magnitude) until the end of Q2 2008. The Federal Reserve and U.S. government responded to the economic fallout of the Lehman Brothers default with a variety of measures to support the liquidity of the banking sector including large guarantee programs, following which we do not see any effect of liquidity risk on bank stock returns.

In Panel B of Table 6, we split *Liquidity Risk* into its components. While unused C&I credit lines are clearly important, the results also show that wholesale funding exposure as well as having access to liquidity (*i.e.* cash) impacts bank stock returns, highlighting that a holistic measure of balance-sheet liquidity risk is useful. Otherwise we would force an average effect across banks for individual components.

Overall, episodes in which the balance-sheet liquidity risk of banks explains their stock returns seem to occur more broadly in aggregate economic downturns, when an aggregate liquidity demand for bank credit lines emerges.

6. Understanding the mechanisms: Funding versus bank capital

In this section, we investigate the mechanisms as to the effect of balance-sheet liquidity risk on bank stock returns during the COVID-19 pandemic. Does funding liquidity to source new loans become a binding constraint for banks when deposit funding dries up (the "funding channel")? Or, does the drawdown of credit lines lock up bank capital against term loans and impair bank loan origination, preventing banks from making possibly more profitable loans (the "capital channel")?

6.1. Net versus gross credit-line drawdowns and bank stock returns

To distinguish between the funding and capital channels, we construct two measures based on actual drawdowns experienced by our sample banks during the first quarter in 2020. *Gross*

Drawdowns are defined as the percentage change of banks' off-balance-sheet unused C&I loan commitments between Q4 2019 and Q1 2020 using call report data. Ivashina and Strahan (2012) and Li *et al.* (2020) show that lagged unused C&I credit commitments are a good predictor for changes in banks' C&I loans. We construct a second proxy, *Net Drawdowns*, which is defined as the absolute change in banks' unused C&I commitments minus the change in deposits (all relative to total assets) over the same period. Holding gross drawdowns fixed, our measure of net drawdowns helps us understand the importance of changes in bank deposits on bank stock returns. In other words, *Gross Drawdowns* proxies for the importance of capital, while *Net Drawdowns* is a proxy for the importance of bank deposit funding; both measures help us identify the importance of the funding vs. capital channel.

We plot the time-series of both measures since Q1 2010 in Figure 6. Panel A of Figure 6 shows the evolution of *Gross Drawdowns*. While *Gross Drawdowns* have been relatively stable since 2015, we observe a sudden increase in credit-line drawdowns by about 13.5% from Q4 2019 to Q1 2020. As observed for banks' off-balance-sheet levels of unused C&I loans, gross drawdowns had already reverted back to pre-COVID-19 levels by the end of Q2 2020.

Panel B of Figure 6 displays the development of *Net Drawdowns* since Q1 2010. *Net Drawdowns* have been relatively stable since 2015 and decreased by about 5% in Q1 2020. In other words, the change in deposits during the first quarter 2020 has been larger than the change in unused C&I commitments, suggesting that funding of new loans should not be a binding constraint for banks. Similar to gross drawdowns, net drawdowns also returned to pre-COVID-19 levels over the next two quarters (*i.e.* in Q3 2020).

We investigate the effect of gross and net drawdowns on bank stock returns more formally using the model specification and control variables from column 5 of Panel A of Table 2. Instead of *Liquidity Risk*, we use our two new proxies to understand the importance of the funding *vis-à-vis* the capital channel. Table 7 reports the results.

Figure 6. Net vs. gross drawdowns

This figure shows the time-series of *Gross Drawdowns* (Panel A) and *Net Drawdowns* (Panel B) over the Q1 2010 to Q3 2020 period. *Gross Drawdowns* is the percentage change in a bank's off balance sheet unused C&I loan commitments (measured during Q1 2020). *Net Drawdowns* are defined as the change in a bank's off balance sheet unused C&I loan commitments minus the change in deposits (all measured during Q1 2020) relative to total assets. All variables are defined in Appendix II.

Panel A. Gross Drawdowns
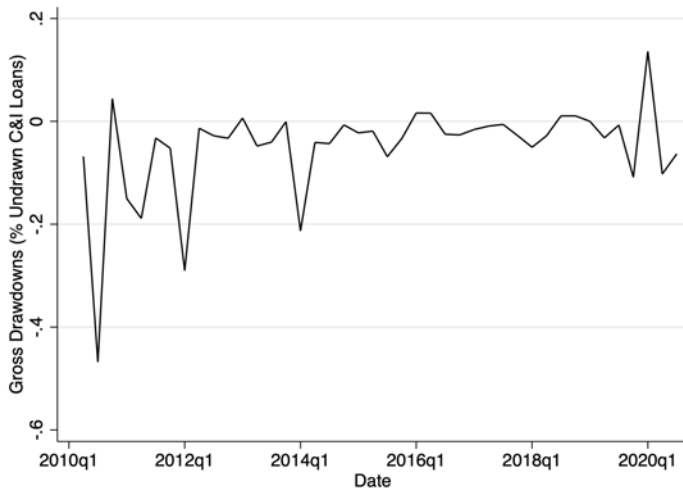
Panel B. Net Drawdowns
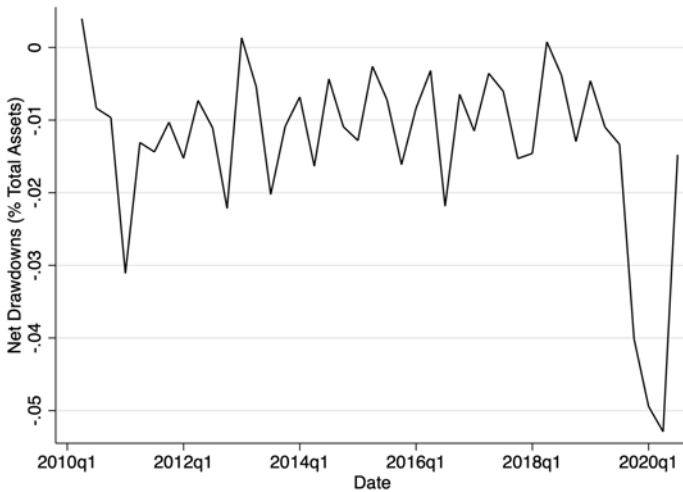
Table 7. Understanding the mechanisms: Funding versus capital

This table reports the results of OLS regressions of U.S. bank' realized stock returns during the 1/1/2020 to 3/23/2020 period on *Net Drawdowns* (column (1)) and *Gross Drawdowns* (column (2)) and control variables. *Net Drawdowns* are defined as the change in a bank's off balance sheet unused C&I loan commitments minus the change in deposits (all measured during Q1 2020) relative to total assets. *Gross Drawdowns* is the percentage change in a bank's off-balance sheet unused C&I loan commitments (measured during Q1 2020). Column (4) includes an interaction term of *Gross Drawdowns* with *Capital Buffer*. Column (5) includes an interaction term of *Net Drawdowns* with *Capital Buffer*. In column (6), we use the change in bank deposits (*Change Deposits*) instead of Net Drawdowns. Column (7) adds *SRISK/Assets* as additional control. SRISK is only available for banks in the vlab database. These regressions include a dummy for banks for whom we do not find SRISK (unreported). Control variables as in column (5) in Panel A of Table 2 are included. P-values based on robust standard errors are in parentheses. All variables are defined in Appendix II.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Net Drawdowns	0.0926		0.219	0.128	0.133		0.0866
	(0.885)		(0.736)	(0.844)	(0.815)		(0.889)
Gross Drawdowns		-4.457**	-4.593**	-3.929**	-4.485**	-4.276*	-4.172**
		(0.034)	(0.023)	(0.044)	(0.026)	(0.052)	(0.046)
Change Deposits / Assets						-0.0697	
						(0.904)	
Gross Drawdowns x Capital Buffer				1.588*			
				(0.084)			
Net Drawdowns x Capital Buffer					-0.109		
					(0.688)		
Change Deposits / Assets x Capital Buffer						0.175	
						(0.516)	
SRISK / Assets							-6.706*
							(0.071)
Capital Buffer				-1.442	-0.759	-0.944	
				(0.164)	(0.415)	(0.343)	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.353	0.378	0.379	0.393	0.381	0.383	0.424
Number obs.	127	127	127	127	127	127	127

We introduce both proxies sequentially in columns 1 and 2 and then together in column (3). The coefficient of *Net Drawdowns* is small and insignificant, while the coefficient of *Gross Drawdowns* is statistically significant and economically meaningful (column 2). A one-standard-deviation increase in *Gross Drawdowns* reduces bank stock returns by about 4.2%, which is large. In magnitude it is similar to our *Liquidity Risk* proxy used in Table 2 earlier in this paper. We include both proxies in column 3 and find that, holding gross drawdowns fixed, net drawdowns have still no significant effect on bank stock returns. That is, as variation in net drawdowns is driven by changes in bank deposits (holding gross drawdowns fixed), funding of drawdowns through bank deposits does not appear to be a binding constraint for banks.

In column 4, we observe the interaction between *Gross Drawdowns* and the *Capital Buffer*, which is the difference between a bank's equity-asset ratio and the cross-sectional average of the equity-asset-ratio of all sample banks in Q4 2019. A larger difference implies

that a bank has a higher capital buffer. The coefficient of the interaction term is positive and significant emphasizing that the negative effect of drawdowns on stock returns is attenuated if banks fund their credit line exposure with more capital. Consistently, the coefficient of the interaction term of *Capital Buffer* and *Net Drawdowns* is not significant (column (5)). Using the change in bank deposits (*Change Deposits*) instead of net drawdowns provides qualitatively the same results (column (6)). Finally, adding *SRISK/Assets* as additional control (column 5) does not change the coefficient of *Gross Drawdowns*, suggesting that SRISK does not capture systemic implications associated with aggregate credit-line drawdowns.

6.2. Implications for bank lending during the COVID-19 pandemic

What does balance-sheet liquidity risk mean for bank lending during the COVID-19 pandemic? The increase in loan vis-à-vis bond spreads documented in Figure 2 in the introduction suggests that bank health was materially affected by the pandemic, and not just temporarily, impacting the access of firms to bank loans as well as the cost of bank credit. Loan-level data shows that bank issuance of new corporate loans has indeed substantially declined since the start of the COVID-19 pandemic. It is a testable hypothesis that banks with more balance-sheet liquidity risk reduced lending more relative to other banks. Moreover, if banks' capital constraints matter, we expect (term loan) lending to be particularly sensitive to gross (but not to net) drawdowns.

We use data from Dealscan to investigate these important issues. We use data on new loan originations in January 2019 to October 2020 and divide our sample into a pre and post period, where post is defined as the period starting April 1, 2020 (Q2 2020), *i.e.* during the COVID-19 pandemic. In unreported tests, we collapse our sample at the bank x month level and show that banks with higher *Liquidity Risk* and higher *Gross Drawdowns* decrease lending in the post relative to the pre-period and relative to banks with lower exposures using bank and month fixed effects. *Net Drawdowns* have no effect on lending. Banks reduce lending particularly to riskier borrowers consistent with higher capital requirements associated with

these loans. However, while these tests are promising they do not allow us to control for loan demand. A plausible alternative explanation could be a reduction in loan demand due to lower investments from firms in a period characterized by high uncertainty. Another alternative explanation for a reduction in lending could be a loss of intermediation rents due to the low-interest-rate environment.

Methodology. We use a Khwaja and Mian (2008) estimator to formally disentangle demand and supply in a regression framework, investigating the change in lending of banks to the same borrower before and after the outbreak of the COVID-19 pandemic. We construct a new variable, $Loan_{i,b,m,t}$, which is the loan amount (or number of loans) issued to firm i by bank b as loan-type m in month t. As our data contains syndicated loans, we use all banks and their lending to firm i in a syndicate in the pre- and post-COVID-19 period. Absorbing loan demand shocks using borrower (η_i), x bank (η_b), x loan-type fixed effect (η_m), we can isolate the effect of balance-sheet liquidity risk on bank loan supply:

$$Loan_{i,b,m} = \beta_1 \times Post + \beta_2 \times DD_b \times Post + \left(\eta_i \times \eta_b \times \eta_m\right) + \varepsilon_{i,b,m}$$

Following Bertrand *et al*. (2004), we collapse our data on a *firm x bank x loan-type* level into a pre- and post-COVID-19 period to account for possible autocorrelation in the standard errors. $Loan_{i,b,m}$ is the natural log of the loan amount (or natural log of 1 plus the number of loans) issued to firm i by bank b as loan-type m. A negative β_2 implies that a bank with more exposure to drawdown risk (DD_b) – measured as either *Gross* or *Net Drawdowns* – decrease lending more than banks with less exposure during the COVID-19 pandemic after controlling for loan demand and other bank- and loan-specific effects via borrower x bank x tranche type fixed effects $\left(\eta_i \times \eta_b \times \eta_m\right)$. *Gross* and *Net Drawdowns* are measured over the Q1 2020 period and the post period starts, as explained above, in Q2 2020. We cluster standard errors on the borrower x bank x tranche level in all regressions.

Results. We provide results with the nat. log of loan amounts as dependent variable in Panel A of Table 8.

Banks that experienced larger gross drawdowns during Q1 2020 reduced lending more during the COVID-19 pandemic. The effect is highly statistically significant and economically large (column 1). A one-standard-deviation increase in *Gross Drawdowns* decreases loan amounts by 5%. While the effect of *Net Drawdowns* is also significant (column 2), its economic meaning is smaller than *Gross Drawdowns*. When including both proxies in the regression, we find that the coefficient of *Gross Drawdowns* becomes smaller and statistically insignificant (column 3). β_1 is negative and significant suggesting that bank lending has, on average, decreased after the outbreak of COVID-19 across all banks. A possible explanation is the loss of intermediation rents for banks at large.

This regression, however, might mask the fact that both proxies are important but that capital or liquidity might play different roles depending on whether or not the loan needs to be fully funded at origination. We thus split the sample into term loans (column 4) and credit lines (column 5) and run the same regressions. As expected, banks with larger *Gross Drawdowns* reduce term lending more post-COVID-19 and banks with larger *Net Drawdowns* reduce credit commitments. That is, banks that experience net drawdowns appear to be reluctant to take on additional liquidity risk. Banks, however, can make term loans as long as they have capital to provide for them. Gross drawdowns reduce the available capital and thus term lending. The economic magnitudes of both proxies are similar to columns 1 and 2. The statistical significance, however, is somewhat lower, as standard errors have increase, likely due to the smaller samples. [22]

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[22] Several papers provide evidence consistent with a reduction of banks' intermediation activity during COVID-19. Chodorow-Reich et al. (2020) and Greenwald et al. (2020) show that banks cut credit lines and term lending to small firms because of credit line drawdowns of large firms, likely due to capital constraints. Moreover, we show in the Online Appendix that loan spreads of small firms in secondary loan markets have significantly increased vis-à-vis spreads of large firms since the beginning of the pandemic, consistent with a loss of intermediation activity for small firms dependent on bank financing.

Table 8. Implications for bank lending during the COVID-19 pandemic

This table provides results of difference-in-differences regressions of the change in amount/number of loan issuance pre- and post-COVID-19 on credit line drawdowns. The analysis is based on data on firm-bank-loan type level between Jan 2019 October 2020 that is collapsed to a pre- and post-COVID-19 period (post is denoted as the period starting 4/1/2020). Panel A (B) shows the results using gross (net) drawdowns. The dependent variables are the natural log of 1 + the loan amount or the natural log of 1 + the number of loans issued. Columns (1)-(2) controls for the demand side with borrower fixed effects; column (3) additionally controls for the supply side with borrower x bank fixed effects; and column (4) additionally controls for tranche type effects with borrower x bank x tranche-type fixed effects. Detailed variable definitions can be found Appendix A. Standard errors are clustered at level of the fixed effect in each column. ***, **, * denote significance at the 1%, 5% and 10% level, respectively. All variables are defined in Appendix II.

Panel A. Loan amount

	(1)	(2)	(3)	(4) Term Loans	(5) Credit Lines
Post x Gross Drawdowns	-15.69***		-8.651	-16.35*	-3.129
	(0.001)		(0.117)	(0.070)	(0.657)
Post x Net Drawdowns		-5.676***	-4.267**	-5.232	-4.099*
		(0.001)	(0.028)	(0.141)	(0.078)
Post	-2.270***	-2.758***	-2.544***	-2.290***	-2.710***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Borrower x Bank x Loan Type FE	Yes	Yes	Yes		
Borrower x Bank FE				Yes	Yes
R-squared	0.200	0.200	0.200	0.186	0.208
Number obs.	17944	17944	17944	5770	12174

Panel B. Number of loans

	(1)	(2)	(3)	(4) Term Loans	(5) Credit Lines
Post x Gross Drawdowns	-2.609***		-1.710**	-2.315*	-1.086
	(0.000)		(0.040)	(0.067)	(0.323)
Post x Net Drawdowns		-0.824***	-0.545**	-0.741	-0.548*
		(0.001)	(0.048)	(0.137)	(0.098)
Post	-0.342***	-0.419***	-0.377***	-0.315***	-0.416***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Borrower x Bank x Loan Type FE	Yes	Yes	Yes		
Borrower x Bank FE				Yes	Yes
R-squared	0.223	0.223	0.223	0.189	0.240
Number obs.	17944	17944	17944	5770	12174

We find very similar results when using the nat. log of 1 plus the number of loans as the dependent variable. The economic magnitude of *Gross Drawdowns* and thus the relative importance of the capital *vis-à-vis* the funding channel is even more pronounced.

7. Contingent capital shortfall in a crisis

Balance-sheet liquidity risk of banks – mainly driven by undrawn credit lines – has severe implications on their ability to extend new loans, because it requires capital once these credit lines are drawn. How can policy makers address aggregate drawdown risk in an ex-ante

manner? One possible way is for regulators to add the effect of drawdowns to stress tests and require banks to fund these exposures with equity. In the last part of the paper, we quantify the capital shortfall that arises due to balance-sheet liquidity risk and show how balance sheet liquidity risk can be incorporated tractably into bank stress tests. Existing stress tests do not account for the impact of banks' contingent liabilities in times of stress. This is what we set out to do in this section.

7.1. Methodology

Capital shortfall in a systemic crisis (SRISK). SRISK is defined as the capital that a firm is expected to need if we have another financial crisis. Symbolically it can be defined as:

$$SRISK_{i,t} = E_t(Capital\ Shortfall_i | Crisis)$$

That is,

$$SRISK_{i,t} = E\ [k\ (Debt + Equity) - Equity\ |Crisis]$$
$$= K\ Debt_{i,t} - (1 - K)(1 - LRMES_{i,t})Equity_{i,t}$$

where $Debt_{i,t}$ is assumed to be constant between time *t* and *Crisis* over *t* to *t+h*. LRMES is the Long Run Marginal Expected Shortfall, approximated in Acharya *et al.* (2012) as $1 - e^{(-18 \times MES)}$, where MES is the one-day loss expected in bank i's return if market returns are less than -2% and Crisis is taken to be a scenario where the broad index falls by 40% over the next six months (h=6m). *K* is the regulatory capital ratio of 8%.

As described above, such an impact can be broken down into two components. First, off-balance-sheet (i.e., contingent) liabilities enter banks' balance sheets as loans and need to be funded with capital. Second, we also have to account for the effects on stock returns as demonstrated in our calculations above.

"Contingent" capital shortfall in a systemic crisis (SRISK-C). We calculate the capital shortfall of banks in a systemic crisis with contingent liabilities as follows:

$$SRISK_{i,t}^{C} = \text{Incremental } SRISK_{i,t}^{CL} + \text{Incremental } SRISK_{i,t}^{LRMES^{C}}$$

(i) $\text{Incremental } SRISK_{i,t}^{CL}$ recognizes that drawdowns of credit lines in crisis states represent contingent liabilities of banks ($Debt_{i,t+h}|Crisis \neq Debt_{i,t}$):

$$\text{Incremental } SRISK_{i,t}^{CL} = K\left[E\big[Debt_{i,t+h}|Crisis\big] - Debt_{i,t}\right]$$
$$= K \times E[\text{Drawdown rate} \mid Crisis] \times Unused\ Commitments_{i,t}$$

$E[\text{Drawdown rate} \mid Crisis]$ is estimated using past drawdown rates extrapolated for a market index fall of 40%.

(ii) $\text{Incremental } SRISK_{i,t}^{LRMES^{C}}$ recognizes that LRMES estimated using "small" (or local) - 2% market corrections in normal times does not account for the episodic effect of balance-sheet liquidity risk on bank stock returns:

$$\text{Incremental } SRISK_{i,t}^{LRMES^{C}} = (1 - K) \times \Delta LRMES_{i,t}^{C} \times Equity_{i,t},$$

where $\Delta LRMES_{i,t}^{C} = \hat{\gamma} \times Liquidity\ Risk_{i,t}$ and $\hat{\gamma}$ is the estimated episodic effect from our tests on balance-sheet liquidity risk.

7.2. Estimating the drawdown function

To calculate the expected percentage drawdown in a crisis, we use drawdown data during the COVID-19 pandemic as well as the GFC crisis and estimate the expected drawdown in a stress scenario with a 40% market correction for both stressed periods. We show plots of this exercise in Figure 7.

Figure 7. Credit line drawdowns and market returns

This figure plots the cumulative drawdown of credit lines of non-financial firms on the cumulative market return (using the S&P 500 as the market). In Panel A, we plot the cumulative quarterly drawdown rates during the COVID-19 pandemic (i.e. Q4 2019 and Q1 2020) and the GFC (i.e. the Q1 2007 to Q4 2009 period) on the respective quarterly S&P 500 returns. We also show the linear regressions for both periods. In Panel B of Figure 6, we use the lowest cumulative daily S&P 500 return within each quarter (instead of the quarterly return). All variables are defined in Appendix II.

Panel A. Quarterly drawdowns vs quarterly S&P 500 returns
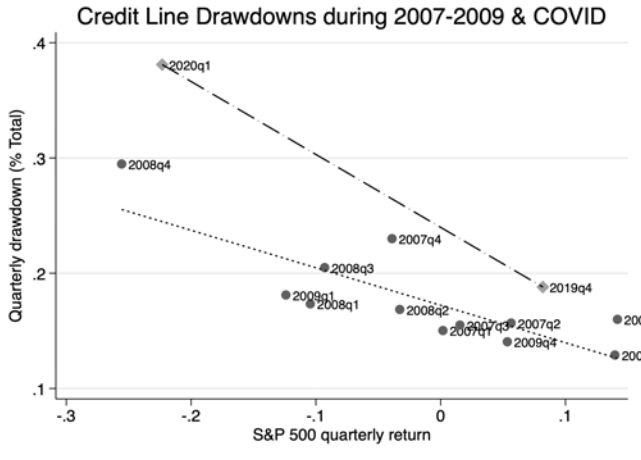
Panel B. Quarterly drawdowns vs lowest cumulative S&P 500 return in each quarter
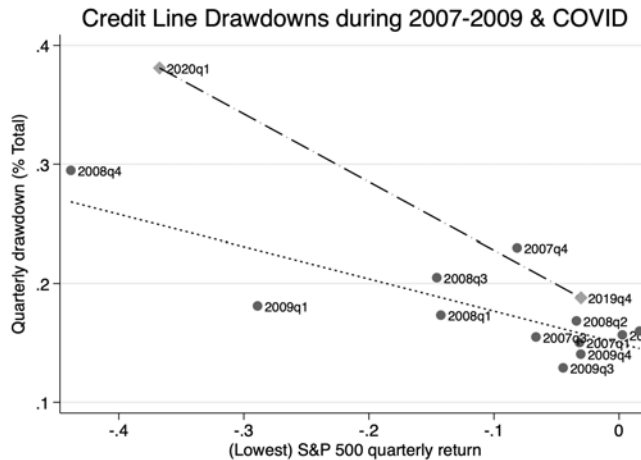
In Panel A of Figure 7, we plot the cumulative quarterly drawdown rates during the COVID-19 pandemic (*i.e.* Q4 2019 and Q1 2020) and the GFC (*i.e.* the Q1 2007 to Q4 2009 period) on

the respective quarterly S&P 500 returns. We also show the linear regressions for both periods. In Panel B of Figure 7, we use the lowest cumulative daily S&P 500 return within each quarter (instead of the quarterly return). This presentation has two advantages. First, it shows that for quarters with relatively low negative S&P 500 returns (*i.e.* "normal times"), drawdowns are somewhat clustered.[23] Second, drawdown decisions are arguably based on how bad a quarter has actually been rather than on the situation at the end of each quarter. We therefore calculate drawdown rates based on Panel B of Figure 7.

We find that the sensitivity of credit-line drawdowns to changes in market returns was higher during the COVID-19 pandemic (the slope coefficient, the β, is -0.57) compared with the GFC (the slope coefficient is -0.27). The projected drawdown rate in a market downturn of 40% (-40% x β) is thus also substantially higher in the COVID-19 pandemic (22.91% vs. 10.82%). A possible explanation of the differential impact on absolute drawdowns could be that corporate balance sheets were less impacted during the GFC, which originated in the banking and household sector. The COVID-19 pandemic, however, had an immediate effect on firms' balance sheets, resulting in elevated demand for liquidity from pre-arranged credit lines compared with the GFC.

The quarterly drawdown rates in both stress scenarios or crises are summarized together with the sensitivities of the drawdown rates in a market correction in Panel A of Table 9.

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[23] The intercept in the COVID-19 pandemic and the GFC are 17% and 15%, respectively.

Table 9. Credit line drawdowns and Incremental SRISK^{CL}

This table reports the predicted drawdown rates (*Drawdown Rate*) from credit lines in a stress scenario of 40% correction to the global stock market (Panel A) and the *Slope* of the drawdown function (compare Figure 6). In Panel B, we report the *Unused Commitments* (C&I loans), and the marginal required capital to fund the predicted drawdowns (Marginal SRISK) using all three (stressed) historical drawdown rates. *Incremental SRISK^{CL}* = Drawdown rate x 8% x Unused Commitments (C&I loans). *Debt* is total liabilities (from vlab). Panel C reports the calculation of *Incremental SRISK^{MES-C}* due to the sensitivity of bank stock returns to *Liquidity Risk* using the minimum (γ_{min}) and maximum (γ_{max}) sensitivity from different model specifications shown in prior tables. *MES-C$_{min}$ (%)* is calculated as *Liquidity Risk x γ_{min}*. *MES-C$_{min}$ ($)* is calculated as *Liquidity Risk x γ_{min} x MV*. Other variables are calculated accordingly. In Panel D, we show the Conditional SRISK (*SRISK-C*) which is the sum of *Incremental SRISK^{CL}* and *Incremental SRISK^{MES-C}*. All variables are defined in Appendix II.

Panel A. Estimating the drawdown rates in a stress scenario

			Slope	Drawdown Rate (S&P Return -40%)
Predicted	Quarterly	Q1 2020	-0.57	22.91%
Drawdowns	Quarterly	2007-2009	-0.27	10.82%

Panel B. Incremental SRISK^{CL}

Name	Unused C&I Commitments (USD mn)	Drawdown rate: 10.82%	Drawdown rate: 22.91%	Debt (USD mn)
JPMORGAN CHASE & CO.	273,278	2,365	5,009	2,496,125
BANK OF AMERICA CORPORATION	310,824	2,690	5,697	2,158,067
WELLS FARGO & COMPANY	198,316	1,717	3,635	1,748,234
CITIGROUP INC.	200,912	1,739	3,682	1,817,838
U.S. BANCORP	96,020	831	1,760	433,158
PNC FINANCIAL SERVICES GROUP, INC., THE	84,238	729	1,544	358,342
M&T BANK CORPORATION	9,260	80	170	109,692
FIFTH THIRD BANCORP	39,328	340	721	148,517
KEYCORP	33,070	286	606	129,380
CITIZENS FINANCIAL GROUP, INC.	33,682	292	617	142,497
	1,278,928	11,070	23,440	9,541,849
	1,434,367	12,416	26,289	10,759,335
	1,492,916	12,923	27,362	

Panel C. Incremental SRISK^{LRMESC}

Name	MV	LRMES	Liquidity Risk	γ_{min}	γ_{max}	LRMES^{Cmin}	LRMES^{Cmax}	Incremental SRISK ^{LRMESC}	
								LRMES$^C_{min}$	LRMES$^C_{max}$
JPMORGAN CHASE & CO.	437,226	43.4%	20.3%	-0.34	-0.54	6.9%	10.9%	30,276	47,766
BANK OF AMERICA CORPORATION	316,808	45.9%	25.7%	-0.34	-0.54	8.8%	13.8%	27,761	43,799
WELLS FARGO & COMPANY	227,540	44.9%	24.2%	-0.34	-0.54	8.2%	13.0%	18,768	29,610
CITIGROUP INC.	174,415	47.3%	37.1%	-0.34	-0.54	12.6%	19.9%	22,047	34,784
U.S. BANCORP	92,603	36.6%	46.3%	-0.34	-0.54	15.8%	24.9%	14,631	23,084
PNC FINANCIAL SERVICES GROUP, INC., THE	69,945	40.1%	39.9%	-0.34	-0.54	13.6%	21.5%	9,514	15,011
M&T BANK CORPORATION	22,400	38.7%	22.6%	-0.34	-0.54	7.7%	12.1%	1,724	2,720
FIFTH THIRD BANCORP	21,815	51.1%	29.9%	-0.34	-0.54	10.2%	16.1%	2,222	3,506
KEYCORP	19,936	45.2%	41.7%	-0.34	-0.54	14.2%	22.4%	2,834	4,472
CITIZENS FINANCIAL GROUP, INC.	17,654	48.3%	46.1%	-0.34	-0.54	15.7%	24.8%	2,772	4,374
Total (Top 10 Banks)	1,400,341							132,550	209,126
Total (Vlab Banks)	1,601,754							149,543	235,935
Total (All Sample Banks)	1,756,619							158,024	249,316

Panel D. SRISK^C

Name	SRISK (Q4 2019)		SRISK$^C_{min}$	SRISK$^C_{max}$
	w/o neg SRISK	w/ neg SRISK		
JPMORGAN CHASE & CO.	0	-27,848	32,641	52,775
BANK OF AMERICA CORPORATION	14,898	14,898	30,452	49,496
WELLS FARGO & COMPANY	24,425	24,425	20,485	33,245
CITIGROUP INC.	60,887	60,887	23,786	38,467
U.S. BANCORP	0	-19,352	15,462	24,843
PNC FINANCIAL SERVICES GROUP, INC., THE	0	-9,895	10,243	16,555
M&T BANK CORPORATION	0	-3,862	1,804	2,890
FIFTH THIRD BANCORP	2,067	2,067	2,562	4,227
KEYCORP	299	299	3,121	5,078
CITIZENS FINANCIAL GROUP, INC.	3,005	3,005	3,064	4,991
Total (Top 10 Banks)	105,581	44,623	143,621	232,566
Total (Vlab Banks)	111,135	36,680	161,958	262,224
Total (All Sample Banks)			170,947	276,678

7.3. Incremental SRISK due to credit line drawdowns

Using these expected drawdown rates, we calculate the equity capital that would be required to fund these new loans based on banks' unused commitments at the end of Q4 2019 ($Incremental\ SRISK_i^{CL}$). We use the Q4 2019 unused credit line commitments of banks and apply the drawdown rates calculated in the three different stress scenarios assuming a prudential capital ratio of 8%:

$$Incremental\ SRISK_i^{CL} = Drawdown\ rate\ \times\ 8\%\ \times\ Unused\ Commitments \qquad (4)$$

In Panel B of Table 9, we show the top 10 banks with the largest undrawn commitments as of Q4 2019 and report $Incremental\ SRISK_i^{CL}$ individually for each of these banks. We also report the total $Incremental\ SRISK^{CL}$ for the top 10 and for all banks in our sample. Overall, we find that $Incremental\ SRISK^{CL}$, *i.e*, the additional capital, amounts to about USD 36bn to USD 65bn depending on the estimates of the drawdown rate.

7.4. Incremental SRISK due to MES^C and contingent SRISK (SRISK^C)

We also accounte for the effect of liquidity risk on bank stock returns as demonstrated in our calculations above. Using the loadings from our regressions of bank stock returns on balance-sheet liquidity risk during the COVID-19 crisis (*i.e*, the γ in equation (2)), we estimate the additional (marginal) equity shortfall of banks based on their end of Q4 2019 market values of equity (MV), called the $Incremental\ SRISK_i^{LRMES^C}$:

$$Incremental\ SRISK_i^{LRMES^C} = (1 - k) \times MV_i \times LRMES_i^C$$
$$= (1 - k) \times MV_i \times \hat{\gamma} \times Liquidity\ Risk_i \qquad (5)$$

$LRMES_i^C$ is contingent marginal expected shortfall due to the impact of liquidity risk on bank stock returns. We report the *Incremental* $SRISK_i^{LRMES^C}$ in Panel C of Table 9.

We use a minimum and maximum loading (γ) estimated from different regressions based on equation (1) and calculate a range of $LRMES_{min}^C$ and $LRMES_{max}^C$, which is between 6.9% and 24.9%. The corresponding *Incremental* $SRISK_i^{LRMES^C}$ amounts to USD 158bn to USD 250bn.

In a final step, we calculate the conditional SRISK ($SRISK^C$) adding the two incremental SRISK components. Adding both components we show that the additional capital shortfall for the U.S. banking sector due to balance-sheet liquidity risk amounts to more than $300 billion as of 31 December 2019 in a stress scenario of a 40% correction to the global stock market, with the top 10 banks contributing USD 265bn. The incremental capital shortfall of the top 10 banks is about 1.6 times the SRISK estimate without accounting for contingent liabilities and the effect of liquidity risk.

Overall, our estimates show that the incremental capital shortfall in an aggregate economic downturn due to banks' contingent liabilities is sizeable, because it requires an additional amount of capital to fund the new loans on their balance sheets, and, importantly, because of an (even larger) incremental capital requirement due to an episodic impact of bank balance-sheet liquidity risk on bank stock returns.

8. Discussion

Finally, we discuss the robustness of our results and their extensions along three dimensions: (1) alternative liquidity proxies used in the literature; (2) pricing of contingent drawdown options through credit-line fees; and (3) the role of covenants during the pandemic.

8.1 Liquidity proxies

We propose and developed a new measure of balance-sheet liquidity risk as there is no consensus in the literature on how to measure liquidity risk. In this section, we compare our measure with two frequently used measures in the literature, the Berger and Bouwman (2009) liquidity creation measure (*BB*) and the Bai et al. (2018) liquidity risk measure (*LMI*). *BB* is a stock measure including banks' on and off-balance-sheet positions. In contrast, the *LMI* is a contemporaneous measure as it incorporates current market liquidity conditions (using the OIS - 3m Treasury Bill spread as a liability weight). We create two LMIs, one using liquidity conditions as of Q4 2019 (*LMI – 2019*) and one using the worst liquidity condition in March 2020 (*LMI – 2020*). We provide a more detailed discussion of the creation of the liquidity measures and the results in Online Appendix D. Below is a brief summary.

We estimate regression (1) using the alternative liquidity proxies. We find that the BB measure is negatively and significantly related to stock returns during the 3/1/2020 to 3/23/2020 period; however, the effect is somewhat moderate compared with both *Liquidity Risk* and *Unused C&I / Assets*. In unreported tests, we run a horse race of *Liquidity Risk* and both alternative liquidity measures in separate regressions. Both *LMI* and *BB* become small and insignificant, while *Liquidity Risk* remains negative and significant, suggesting that *Liquidity Risk* contains information not captured in these alternative liquidity proxies. *LMI – 2019* has only a small and statistically insignificant effect due to benign liquidity conditions in financial markets at the end of 2019. *LMI – 2020*, however, has a large, significant impact on stock returns and is also highly correlated with *Liquidity Risk*. This is consistent with the interpretation that a worsening of liquidity conditions in financial markets increases aggregate drawdown risk for banks, thereby increasing the value of the put option, which negatively impacts bank stock returns.

8.2. Credit line fees

Do banks price aggregate drawdown risk through fees and/or credit spreads when issuing new credit lines? In Online Appendix E, we investigate this question using all credit lines issued to U.S. non-financial firms over the 2010 to 2019 period, sourced from Refinitiv Dealscan. We first show that idiosyncratic drawdown risk (measured using a firm's realized equity volatility over the past 12 months) and systematic drawdown risk (measured using a firm's stock beta) are priced in both commitment fee (*AISU*) and spread (*AISD*). This is consistent with, for example, Acharya et al. (2013) and Berg et al. (2015).

However, while a higher *Bank Beta* and *LRMES* both somewhat increase the price of credit lines, *Liquidity Risk* or *Unused C&I / Assets*, on average, do not. Also, *SRISK / Assets*, which measures bank capital shortfall in times of aggregate market downturn, does not appear to be priced either. In other words, banks do not appear to be considering the deep out-of-the-money put option associated with aggregate drawdown risk when setting ex-ante price terms of credit lines. This may partly explain their need to fund aggregate drawdown risk with equity capital, as witnessed during the pandemic.

8.3. Covenants

Did covenants constrain drawdowns of credit lines at the beginning of the pandemic in March 2020, or later during the year when firms' financial situation had deteriorated? We follow the extant literature (*e.g.*, Roberts and Sufi, 2008) and use textual analysis to identify all loan amendments of publicly listed U.S. non-financial firms in SEC filings sourced from EDGAR from March to Q3 2020. We found that not a single loan amendment was initiated through a covenant violation. On the contrary, banks and firms regularly negotiated a covenant relief period (usually up to Q1 2021 or later) early in the pandemic to account for its fallout. In summary, credit line drawdowns (also by firms in the hardest-hit industries) did not appear to be constrained by possible covenant violations during the pandemic.

9. Conclusion

We document that the balance-sheet liquidity risk of banks is an explanation for the significant and persistent underperformance of bank stocks relative to other financial and non-financial firms during the COVID-19 pandemic. It explains both the cross-section and the time-series of bank returns during the pandemic but not before. This episodic impact of balance-sheet liquidity risk on bank stock returns is not unique to the COVID-19 crisis, and was also seen during the global financial crisis, *i.e.*, during the 2007 to 2009 crisis. That is, balance-sheet liquidity risk of banks affects bank stock prices during an aggregate economic downturn when firms' liquidity demand through credit-line drawdowns becomes highly correlated, but not before.

Importantly, bank stock returns react adversely to gross drawdowns rather than net drawdowns (which account for inflows in bank deposits), suggesting that bank capital is a binding constraint to intermediation activity by banks as perceived by markets. Consistently, we show that banks with large gross drawdowns reduce their immediate supply of term loans (not credit lines); banks with less deposit inflows, however, reduce credit line originations. We demonstrate how the episodic nature of credit-line drawdowns and balance-sheet liquidity risk can be incorporated tractably into bank stress tests.

Darmouni and Siani (2020) show that U.S. non-financial firms issued bonds following the monetary policy and fiscal interventions in March 2020 and used the proceeds to repay credit lines. While a large proportion of credit lines has been repaid in Q2 and Q3 2020, corporate preference for cash of firms has remained high (Online Appendix B) and total debt on firms' balance sheet has substantially increased. The non-financial-sector leverage and exposure to capital markets has thus increased further during the COVID-19 pandemic. In other words, ex-ante aggregate drawdown risk of banks is again high (e.g., in case of another wave of the pandemic or due to another aggregate shock). In turn, the value of the put option in the form of bank credit lines for corporates and capital markets would be even more pronounced if liquidity conditions were to severely deteriorate. In summary, additional corporate leverage

accumulated during the initial phase of the pandemic has increased the likelihood of future impact on bank stock returns via the credit-line drawdown channel.
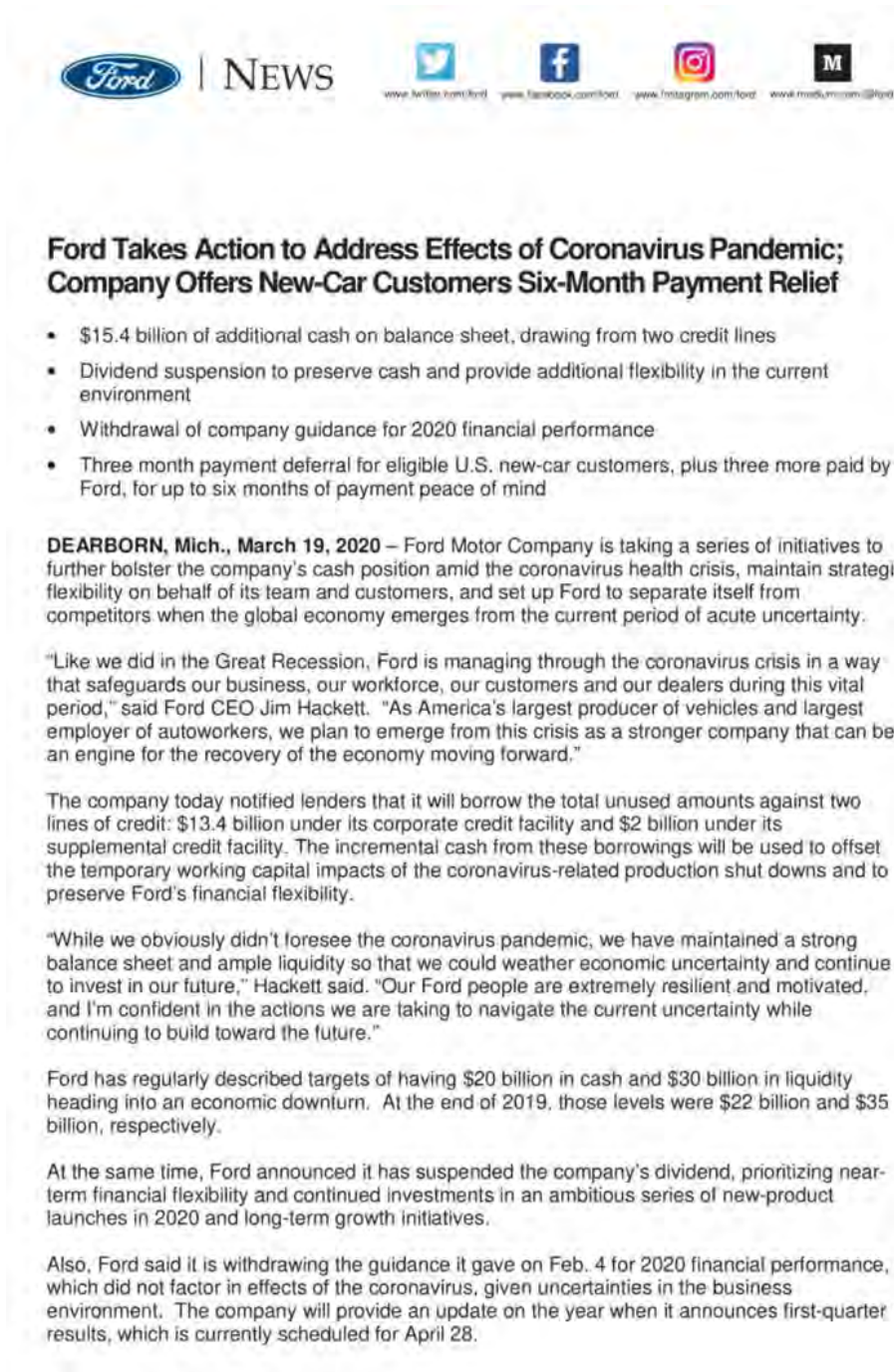
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Appendix I. Example – Drawdowns during COVID-19

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Ford Takes Action to Address Effects of Coronavirus Pandemic; Company Offers New-Car Customers Six-Month Payment Relief

- $15.4 billion of additional cash on balance sheet, drawing from two credit lines
- Dividend suspension to preserve cash and provide additional flexibility in the current environment
- Withdrawal of company guidance for 2020 financial performance
- Three month payment deferral for eligible U.S. new-car customers, plus three more paid by Ford, for up to six months of payment peace of mind

DEARBORN, Mich., March 19, 2020 – Ford Motor Company is taking a series of initiatives to further bolster the company's cash position amid the coronavirus health crisis, maintain strategic flexibility on behalf of its team and customers, and set up Ford to separate itself from competitors when the global economy emerges from the current period of acute uncertainty.

"Like we did in the Great Recession, Ford is managing through the coronavirus crisis in a way that safeguards our business, our workforce, our customers and our dealers during this vital period," said Ford CEO Jim Hackett. "As America's largest producer of vehicles and largest employer of autoworkers, we plan to emerge from this crisis as a stronger company that can be an engine for the recovery of the economy moving forward."

The company today notified lenders that it will borrow the total unused amounts against two lines of credit: $13.4 billion under its corporate credit facility and $2 billion under its supplemental credit facility. The incremental cash from these borrowings will be used to offset the temporary working capital impacts of the coronavirus-related production shut downs and to preserve Ford's financial flexibility.

"While we obviously didn't foresee the coronavirus pandemic, we have maintained a strong balance sheet and ample liquidity so that we could weather economic uncertainty and continue to invest in our future," Hackett said. "Our Ford people are extremely resilient and motivated, and I'm confident in the actions we are taking to navigate the current uncertainty while continuing to build toward the future."

Ford has regularly described targets of having $20 billion in cash and $30 billion in liquidity heading into an economic downturn. At the end of 2019, those levels were $22 billion and $35 billion, respectively.

At the same time, Ford announced it has suspended the company's dividend, prioritizing near-term financial flexibility and continued investments in an ambitious series of new-product launches in 2020 and long-term growth initiatives.

Also, Ford said it is withdrawing the guidance it gave on Feb. 4 for 2020 financial performance, which did not factor in effects of the coronavirus, given uncertainties in the business environment. The company will provide an update on the year when it announces first-quarter results, which is currently scheduled for April 28.

Appendix II. Variable definitions

Variable name	Definition	Source
Assets	Total Assets	Call Reports
Capital Buffer	Difference between a bank's equity-asset ratio and the cross-sectional average of the equity-asset-ratio of all sample banks in Q4 2019	Call Reports
Consumer Loans / Assets	Consumer loans (%Assets)	Call Reports
Credit Card Commitments / Assets	Unused credit card commitments (%Assets)	Call Reports
Credit Lines	Indicator if loan type within list:	Dealscan
Cumulative Total Drawdowns	Natural logarithm of the realized daily cumulative credit line drawdowns across all firms	8-K
Cumulative BBB Drawdowns	Natural logarithm of the realized daily cumulative credit line drawdowns across all BBB-rated firms	8-K
Cumulative NonIG Drawdowns	Natural logarithm of the realized daily cumulative credit line drawdowns across all NonIG rated firms	8-K
Cumulative Not Rated Drawdowns	Natural logarithm of the realized daily cumulative credit line drawdowns across all unrated firms	8-K
Current Primary Dealer Indicator	Indicator = 1 if bank is current primary dealer bank (https://www.newyorkfed.org/markets/primarydealers#primary-dealers)	NY Fed
Debt	Market value of bank liabilities (12/31/2019)	Vlab
Deposits / Assets	Deposits (%Assets)	Call Reports
Deposits / Loans	Deposits (%Loans)	Call Reports
Derivatives / Assets	Interest rate, exchange rat and credit derivatives (% Assets)	Call Reports
Distance-to-Default	Mean(ROA+CAR)/volatility(ROA) where CAR is the capital-to-asset ratio and ROA is return on assets	Call Reports
Drawdown Rate	Sensitivity of changes in credit line drawdowns to changes in the market returns (projected in a market downturn of 40%)	Capital IQ, 8-K, CRSP
Equity Beta	Constructed using monthly data over the 2015 to 2019 period and the S&P 500 as market index	CRSP
Equity Ratio	Equity (%Assets)	Call Reports
Gross Drawdowns	Percentage change of banks' off-balance sheet unused C&I commitments between Q4 2019 and Q1 2020	Call Reports
Idiosyncratic Volatility	Annualized standard deviation of the residuals from the market model	CRSP
Income Diversity	1 minus the absolute value of the ratio of the difference between net interest income and other operating income to total operating income	Call Reports
Incremental SRISK^{CL}	Equity capital that would be required to fund new loans based on banks' unused commitments (CL = credit lines) at the end of Q4 2019	Call Reports
Incremental SRISK^{LRMESC}	(Marginal) equity shortfall of banks based on their end of Q4 2019 market values of equity due to effect of liquidity risk on stock returns	Call Reports
Liquidity	The sum of cash, federal funds sold & reverse repos, and securities excluding MBS/ABS securities.	Call Reports
Liquidity Risk	Unused Commitments plus Wholesale Funding minus Liquidity (% Assets)	Call Reports
Loan	Either natural log of loan amount or natural log of 1+number of loans	Dealscan
Loans / Assets	Total loans (%Assets)	Call Reports
Log(Assets)	Natural log of Assets	Call Reports
LRMES	LRMES is the Long Run Marginal Expected Shortfall, approximated in Acharya et al. (2012) as $1-e^{((-18\times MES))}$, where MES is the one-day loss expected in bank i's return if market returns are less than -2%	Call Reports
LRMES^C	Contingent marginal expected shortfall due to the impact of liquidity risk on bank stock returns.	Call Reports, CRSP
MV	Market value of equity (12/31/2019)	Vlab
Net Drawdowns	Absolute change in banks' unused C&I commitments minus the change in deposits (% Assets) over the same period	Call Reports

Non-Interest Income	Non-interest-income (%Operating revenues)	Call Reports
NPL / Loans	Non-performing loans (%Loans)	Call Reports
Oil Exposure / Assets	Sum of a bank's active loan exposures to oil & gas firms (%Assets)	Dealscan
Other Sectoral Exposures / Assets	Sum of a bank's active loan exposures to the retail, leisure, and hotel & gaming industry (%Assets)	Dealscan
Post	Post is defined as the period starting April 1, 2020	
Real Estate Beta	Slope of the regression of weekly excess stock returns on the Fama and French real estate industry excess return in a regression that controls for the MSCI World excess return	CRSP
Return 1/1-3/23/2020	Cumulative stock return from January 1 to March 23, 2020; log excess returns are calculated as the $\log(1 + r - r_f)$, where r is the simple daily return (based on the daily closing price, adjusted for total return factor and daily adjustment factor), and r_f is the 1-month daily Treasury-bill rate	CRSP
Return January 2020	Cumulative stock return from January 1 to January 31, 2020	CRSP
Return February 2020	Cumulative stock return from February 1 to February 29, 2020	CRSP
Return 3/1-3/23/2020	Cumulative stock return from March 1 to March 23, 2020	CRSP
Return April 2020	Cumulative stock return from 01.04.-30.04.2020	CRSP
Return May 2020	Cumulative stock return from 01.05.-31.05.2020	CRSP
Return June 2020	Cumulative stock return from 01.06.-30.06.2020	CRSP
ROA	Return on assets: Net Income / Assets	Call Reports
S&P 500 Return	(Daily) excess return of the S&P 500 index; log excess returns are calculated as the $\log(1 + r - r_f)$, where r is the simple daily return (based on the daily closing price, adjusted for total return factor and daily adjustment factor), and r_f is the 1-month daily Treasury-bill rate	CRSP
SRISK	Bank capital shortfall in a systemic crisis as in Acharya et al. (2012)	Vlab
SRISK/Assets	SRISK scaled by total assets	Vlab and Call Reports
$SRISK^C$	Incremental $SRISK^{CL}$ + Incremental $SRISK^{LRMES-C}$	Call Reports
Term Loan	Indicator if loan type within list:	Dealscan
Unused C&I Commitments	Unused C&I credit lines	Call Reports
Unused Commitments	The sum of credit lines secured by 1-4 family homes, secured and unsecured commercial real estate credit lines, commitments related to securities underwriting, commercial letter of credit, and other credit lines (which includes commitments to extend credit through overdraft facilities or commercial lines of credit)	Call Reports
Wholesale Funding	The sum of large time deposits, deposited booked in foreign offices, subordinated debt and debentures, gross federal funds purchased, repos and other borrowed money.	Call Reports

Handle with care: Regulatory easing in times of COVID-19[1]

Fabián Valencia,[2] Richard Varghese,[3] Weijia Yao[4] and
Juan F. Yépez[5]

The policy response to the COVID-19 shock included regulatory easing across jurisdictions to loosen financial conditions by facilitating the flow of credit to the economy. Using an intraday event study, this paper examines how equity returns—a key driver in financial conditions—reacted to the announcement of these measures in a sample of 18 advanced economies and 8 emerging markets. The paper finds that the announcement of looser regulation overall contributed to easing financial conditions, but effects varied across sectors and tools. Financial regulatory easing led to lower valuations for financial sector stocks, and higher valuations for non-financial sector stocks, particularly for industries that are more dependent on bank financing. Furthermore, valuations declined and financial conditions tightened following announcements related to easier bank capital regulation while equity valuation rose and financial conditions loosened after those about liquidity regulation. Effects from non-regulatory financial measures appear to be generally more muted.

I. INTRODUCTION

Regulatory reforms implemented in the years after the global financial crisis allowed banks in many jurisdictions to enter the COVID-19 crisis with sizable capital buffers (IMF, 2020a). The Bank of International Settlements estimated that banks globally entered the crisis with roughly US$5 trillion of capital above their Pillar 1 regulatory requirements (Lewrick and others, 2020). The presence of these buffers and the exogenous nature of the COVID-19 shock allowed policymakers to embark on a significant easing of regulatory measures across jurisdictions as part of an unprecedented and wide-ranging policy support package (Figure 1).

Figure 1. Type and Count of Policy Responses to the COVID-19 Shock
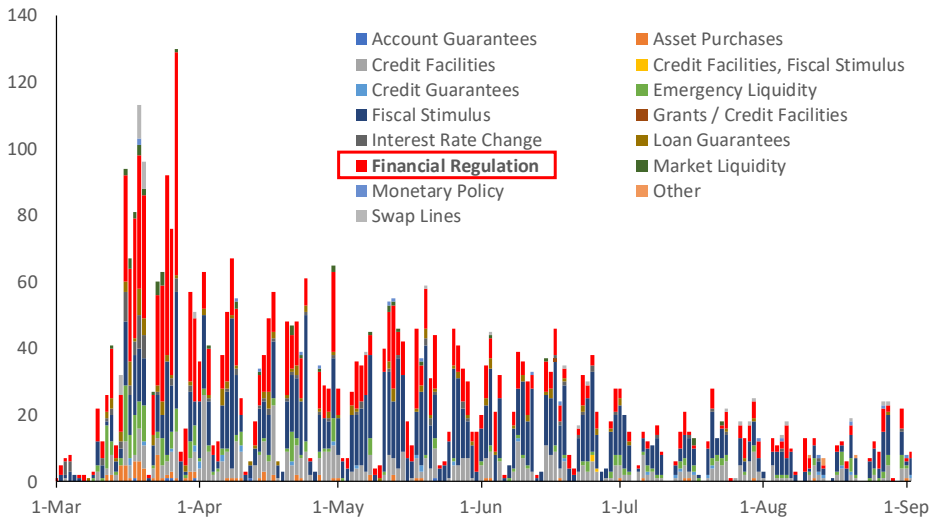
Note: Each bar in the figure shows the number of interventions in a given day, by policy type, in response to the COVID-19 shock for all the 66 jurisdictions that are reported in the COVID-19 Financial Response Tracker (CFRT) by the Yale Program on Financial Stability.

Almost half of these financial regulation measures were prudential in nature and as such, their objective has been to ensure the flow of credit to the economy and mitigate amplification effects of the initial shock stemming from binding regulatory constraints.[1] But prior to the COVID-19 shock, most of the literature studying the effects of financial regulation, in particular prudential policy, had focused on episodes of regulatory tightening (Araujo and others,

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[1] A prudential regulatory policy is implemented ex ante (before a shock) to mitigate risks and increase resilience to shocks. In this regard, policies such as asset purchases programs, loan payment holidays, and emergency liquidity schemes are non-regulatory or crisis management financial policies.

2020), resulting in relatively less understanding about the effects of easing. Relaxation measures following the COVID-19 shock offer a unique opportunity to shed light on the effects of regulatory easing and expand the knowledge in this area.

Against this backdrop, this paper contributes to the literature on financial regulatory policy by analyzing the effects of regulatory easing (mainly prudential policy) on financial conditions during COVID-19. Given the key role of stock prices in driving global financial conditions, the paper employs an intraday event study framework to estimate the response of stock prices to regulatory easing announcements. The initial set of information on financial policy announcements comes from the Yale COVID-19 Policy Tracker (CFRT). This initial list is first refined by restricting the sample to isolated policy announcements (i.e., excluding announcements that are part of a policy package or occur within the same day as other policy announcements or measures) that are financial in nature, and further augmented by collecting the precise hour for each relevant announcement. This step seeks to ensure the results can be attributed to the measure in question, which becomes more difficult to disentangle when the measure is part of a package. This process identifies 240 financial policy announcements—regulatory and non-regulatory related—from 42 jurisdictions from February 1 and July 31, 2020. [2]

Event studies have been commonly used in economics, including to measure the impact on the value of a firm in response to a change in the regulatory environment (Schwert, 1981). The efficient-markets/rational-expectations hypothesis' posits that security prices reflect all available information (see Muth 1961; Fama 1970; and Fama 1976). Therefore, if regulation has implications for the value of equities, the effects of regulation are impounded into prices at the time when they are first anticipated. From an econometric identification point of view, an event study allows also the possibility of isolating specific announcements over narrow time windows in order to mitigate reverse causality and simultaneity concerns.

The identification strategy employed in this paper, built around hourly stock price data, relies on the implicit assumption that financial policies are unlikely to be adjusted instantaneously to changes in stock prices within the same hour. Since the design and implementation of financial policy measures typically take more than just hours, and the empirical framework accounts for returns just prior to the announcements, the high frequency identification approach substantially mitigates any reverse causality concerns. By computing returns around a narrow window, this approach reduces the influence of other news on the estimates. The analysis also controls for global and country-specific covariates that could affect stock returns jointly with announcements, and hence could lead to mismeasurement. These include overlapping announcements occurring at the same time in any other jurisdiction, all announcements on the same day occurring in systemic jurisdictions (i.e. China, Euro Area, Hong Kong S.A.R., Switzerland, United Kingdom, and United States). The event study is implemented using the local projection method proposed by Jordà (2005) and estimates the response of MSCI sectoral stock returns to these policy announcements.

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[2] As it will be explained in Section III, only 26 jurisdictions are included in the analysis due to data limitations.

Beyond statistical identification, the focus on stock prices allows also to examine the role of regulatory relaxation in mitigating adverse asset price dynamics that follow a severe negative shock. These dynamics lead to asset price externalities and amplification of asset price spirals resulting from binding borrowing constraints (see for instance, Bernanke and Gertler 1989; Kiyotaki and Moore 1997; Mendoza 2010; Jeanne and Korinek 2010; Brunnermeier and Sannikov 2014; and Bianchi and Mendoza 2018). By relaxing regulatory constraints and facilitating the flow of credit in the economy, the expectation is that these externalities are mitigated. Yet this simple logic offers only one perspective, that of the positive effects of the policy actions on financial conditions, for a given level of banks' underwriting standards.

In practice, the effect of these policy announcements on stock prices depends also on how investors perceive these announcements to influence banks' risk-taking incentives. For example, an optimistic investor would expect that higher credit provision to non-financial firms would translate into higher future cash flows for banks from their assets, consequently increasing their equity value. On the other hand, a pessimistic investor could expect excessive risk-taking by financial sector firms through increasing leverage or weakening underwriting standards, which could cloud the prospects for future cash flows for financial firms.

This paper finds that news about regulatory easing led to a statistically significant reduction in financial sector stock returns in the hour immediately after the official announcement. This result could be a sign that investor sentiment towards the financial industry soured as markets priced in increased risk taking resulting from these policies.[3] Against this backdrop, it could be argued that investors, expecting inefficient credit expansions in response to regulatory easing and other policy support, may perceive an increase in the risk of a crash down the road, thus responding negatively to announcements on impact. This interpretation could be particularly fitting in the current environment given the magnitude and unprecedented nature of the COVID-19 shock, in which facilitating credit flows could come at the expense of deteriorating underwriting standards and increasing the risk of lending to zombie firms. [4] Contrary to the response from financial stocks, excess returns for non-financial stocks increased following the announcements of regulatory easing. Moreover, this increase was particularly larger in industries that depend more on bank credit indicating regulatory easing may have facilitated greater flow of credit to the economy through banks. These results point towards an emerging tradeoff stemming from the ongoing regulatory easing—policies introduced to facilitate credit availability may come at the expense of additional stress on the financial sector.

In terms of policy composition, the analysis shows that markets reacted negatively to announcements related to easier bank capital regulation and positively to those about liquidity regulation. For liquidity-based regulations, the effects on financial stock returns is negligible while the positive effect on non-financial stocks is positive and significant. The neg-

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[3] Previous studies have shown that higher (lower) bank capital is beneficial (detrimental) for bank shareholders, particularly during crisis episodes (Berger and Bouwman 2013; Cappelletti and others 2020; and Huang, de Haan, and Scholtens 2020). This result is also consistent with a strand of literature showing that credit expansions predict bank equity crash risk (Baron and Xiong 2017; and Gandhi 2018).

[4] Based on the historical relationship between bankruptcies and unemployment in the United States, Greenwood, Iverson, and Thesmar (2020) show that the pace of business bankruptcy can be expected to increase by 140 percent relative to their 2019 level.

ative reaction to the easing of bank capital regulation was not limited to financial sector stocks—valuations of non-financial stocks were also lower after easing of capital requirements. As shown in (Elenev, Landvoigt, and Van Nieuwerburgh, Forthcoming), bank shareholders can gain from tighter bank capital regulation, as higher capital requirements force banks to shift their capital structure to equity. In this regard, looser capital requirements would have an opposite effect (i.e., reduce equity valuations for banks). Also, in line with recent studies that look at the effect of financial policies on stock prices during the pandemic (Sever and others 2020; and Demirgüç-Kunt, Pedraza, and Ruiz-Ortega 2020), the analysis finds that non-regulatory financial measures (e.g. asset purchases, government credit guarantees, and emergency liquidity programs) did not have a statistical significant effect on equity valuations.

Overall, these results are consistent with the broad evolution of stock prices of financial firms vis-à-vis those of non-financial corporations since the onset of the crisis, whereby the former have significantly underperformed broad stock market indices (Demirgüç-Kunt, Pedraza, and Ruiz-Ortega 2020 for a detailed description of bank stock underperformance).

One caveat with high-frequency intraday event studies is that, while they can help with statistical identification, they cannot say much about the validity of the results beyond the window of observation. Therefore, to assess the economic significance of the effects estimated through the event study, we extend the analysis using a panel vector autoregression (PVAR) framework. PVARs capture dynamic relationship between regulatory announcements and financial conditions while maintaining the cross-country dimension of the event analysis. This is an important consideration over a longer time horizon. Specifically, we construct impulse-response functions (IRFs) of financial condition indices (FCIs) to regulation policy announcements. Given the lack of intraday FCIs, estimating the impact of regulation on financial conditions within a system is a more suitable approach, with the PVAR framework capturing possible feedback effects from movements in FCIs to regulatory decisions[5]. The results are consistent with the intraday analysis, showing that the easing of liquidity regulations supported FCIs while on the other hand the easing of capital regulations caused FCIs to tighten in a 30 day window following the announcement. The effects of liquidity and capital announcements on FCIs was particularly large in emerging market economies.

From a policy perspective, the findings suggest that the net effect of regulatory easing on financial conditions appears overall positive in the near term. At the same time, market reactions signal tradeoffs down the road, which can be interpreted as consistent with expected increased risk-taking, deterioration in underwriting standards, or continued lending to zombie firms by financial sector firms. These tradeoffs vary across tools, with a drop in equity returns mostly associated with easing of capital-related prudential regulation. In designing a road map for the roll-back of regulatory support, these results could suggest rolling back capital related regulations first to help rebuild buffers, once the recovery is on a firm footing. This of course implicitly assumes that the effects detected in this paper carry through symmetrically.

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[5]The economic impact of regulatory actions is likely to be observed over longer time horizons, therefore the assumption of strict exogeneity of regulation to market developments is likely to be violated when the window of analysis goes beyond a day.

If this is the case, the unwinding of regulatory easing should be done gradually to reduce the risk of a sudden tightening of financial conditions.

This paper contributes to three strands of literature. First, it is one of the first studies to explore the impact of regulatory announcements in response to COVID-19 on domestic financial markets using a sample of emerging markets and advanced economies. At the time of this draft, Demirgüç-Kunt, Pedraza, and Ruiz-Ortega (2020) is the only other study examining the same issues. This paper differs from the former in that the analysis relies on intraday data, which strengthens identification. Moreover, this paper looks at the effects of regulatory measures both on financial and non-financial industry level equity returns and overall financial conditions, to better document the transmission of policies. Along with the PVAR estimates, this allows the paper to provide a better sense of the macroeconomic relevance of these measures.

Second, this paper is related to a strand of literature that analyses the impact of policies (mainly fiscal and unconventional monetary policy announcements) deployed during the pandemic using event studies (e.g., Arslan, Drehmann, and Hofmann 2020; Gormsen and Koijen 2020; Sever and others 2020). These studies, however, do not investigate financial regulatory policies. Moreover, they are either specific to a certain jurisdiction or focus on a small sub-sample of EMs. This paper uses instead a broad sample of emerging markets and advanced economies using hand-collected intraday data leveraging the Yale's CFRT.

Third, the paper contributes to the growing literature on the effects of news conveyed in policy communication, by looking at regulatory announcements, while most of this literature has focused mainly on news about monetary policy (e.g., Cieslak and Schrimpf (2019); Gürkaynak, Sack, and Swanson (2005)).

The remainder of the paper is structured as follows. The next section presents the empirical strategy. Section III describes the database used for the event study and section IV documents the paper's main findings. A battery of robustness checks are presented in section V. Section VI concludes.

II. EMPIRICAL STRATEGY

This paper employs an event study framework to empirically examine the effect of regulatory easing announcements on stock returns. The identification strategy is built on three key elements. First, the analysis focuses exclusively on isolated events—those that are neither part of a package nor within the same day of any other announcements.[6] Second, it utilizes high-frequency hourly data to build a narrow intraday window around the announcement (one hour before and three hours after the announcement). The inclusion of the one hour prior to announcement return is to account for the fact that all information known up to that moment

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[6]However, announcements of policy packages consisting of similar prudential measures (e.g., packages reducing capital risk weighting factors and provision requirements) are included.

is expected to be already priced in by the markets. The high-frequency identification also mitigates reverse causality concerns as it is unlikely that prudential norms are systematically adjusted in response to hourly stock price movements. Finally, a tight event window makes it more feasible to control for all possible confounding external events, somewhat reducing simultaneity concerns. Specifically, all overlapping announcements occurring at the same time in all other jurisdictions and all regulatory announcements from systemic jurisdictions are accounted for in the empirical framework.

The empirical exercise starts by computing the dependent variable—cumulative excess sectoral equity returns. Excess return for sector i in jurisdiction c at time t is computed as the difference between the return of MSCI sectoral indices and the overall market return in a jurisdiction as depicted in equation (1) below.[7]

$$ExcessReturn_{i,c,t} = SectorReturn_{i,c,t} - MarketReturn_{c,t} \qquad \forall h \qquad (1)$$

The choice of sectoral return as the dependent variable is to aid the focus of analysis on the effects of asset price movements on financial conditions—given it being more representative of the economy as opposed to individual firm level return. The excess returns are further accumulated from one hour prior to the announcement to different horizons— that is hours after the announcement with the announcements occurring at h=0 (see equation (2)).

$$CumulativeExcessReturn_{i,c,t+h} = ExcessReturn_{i,c,t+h} - ExcessReturn_{i,c,t-1} \qquad \forall h \qquad (2)$$

With the cumulative excess returns on hand, the event study is implemented using Jordà (2005) local projections method. The local projection method allows for estimation of the cumulative excess stock return in response to a regulatory easing announcements at various horizons within the chosen window. Specifically, the analysis follows the baseline specification of the following form.

$$CumulativeExcessReturn_{i,c,t+h} = \beta_h Announcement_{c,t} + \delta_h X_{i,c,t} + \alpha_{c,h} + \gamma_{i,t,h} + \varepsilon_{i,c,t+h} \qquad \forall h \qquad (3)$$

The key explanatory variable of interest, $Announcement_{c,t}$, is the event dummy that takes a value of one at the hour of the announcement, and zero otherwise. The announcement hour is obtained by rounding the exact event time stamp obtained from official documents, news articles, or social media accounts to the closest full hour.

$X_{c,t}$ vector of global and country-specific covariates that control for any confounding factors that could affect stock returns, and hence could lead to mismeasurement of the economic

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[7]Section V presents robustness exercises using alternative equity return measures.

impact of the announcements. These include overlapping announcements occurring at the same time in any other jurisdiction, all announcements on the same day occurring in systemic jurisdictions (i.e. China, Euro Area, Hong Kong S.A.R., Switzerland, United Kingdom, and United States), and lagged return of the excess return measure. The first two control variables ensure that the results capture only the effect of domestic policy announcements by controlling for global developments. The third and final control—lagged cumulative excess return— is included so that the local projections are asymptotically valid in the presence of non-stationary data (Montiel Olea and Palgborg-Møller, 2020).

α_c and $\gamma_{i,t}$ are country and sector-time (sector-hour) fixed effects respectively. Country fixed effects control for any unobservable time-invariant country characteristics. More importantly, sector-time fixed effects control for all possible time-varying sector-level shocks. The sector-time fixed effects is a critical element in the identification strategy given the differential impact of COVID-19 shock across sectors over time.

Restricting the events to isolated announcements, utilising a narrow intraday event window, the choice of control variables, and the fixed effect combination provides an empirical framework that substantially mitigates endogeneity and omitted variable bias concerns when estimating the impact of announcements. The local projection method estimates provide the effect of announcements on impact and up to one hour later. β_h, proxies the economic impact of announcements and is the coefficient of interest. Therefore, β_0 and β_1 would capture the response of stock prices to announcements on impact and an hour after the announcement.[8] A positive (negative) coefficient would imply excess returns increased (decreased) following announcements.[9]

In addition, the empirical strategy also accommodates heterogeneity across types of instruments used by policymakers by disaggregating announcements by policy instruments – regulatory announcements (relaxation of capital regulations and liquidity measures) and non-regulatory announcements (credit support programs, emergency liquidity schemes, and other financial measures).

III. DATA

This section discusses the construction of the database of financial regulatory announcements that allows to exploit high frequency (i.e. intraday) data. This approach pins down the precise hour in which a specific announcement was made and subsequently estimate stock excess

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[8]The impulse response horizon does not go beyond one hour, as announcements typically occurred in the hour prior to markets closing, hence a larger horizon would entail going to the next trading day.

[9]The same specification is used to estimate the impact of announcements on financial stock price indices. In this case, equation (3) collapses to the following one sector form, where the sector-time fixed effect is replaced with time fixed effects.

$$CumulativeExcessReturn_{c,t+h} = \beta_h Announcement_{c,t} + \delta_h X_{c,t} + \alpha_{c,h} + \gamma_{t,h} + \varepsilon_{c,t+h} \qquad \forall h \qquad (4)$$

returns around the announcement, which is an advantage relative to the existing datasets of financial sector policy announcements that typically present data at a daily frequency. This section also presents (i) descriptive statistics on equity markets returns, (ii) a measure for sectoral bank-finance dependence, and (iii) a detailed description of the sample used in the empirical analysis.

A. Financial Regulatory Announcements

Using the COVID-19 Financial Response Tracker (CFRT) from Yale University, financial policy announcements are identified from February 2 until July 31 2020. The CFRT database collects and visualizes an array of policy responses during the pandemic, providing the links to the official communiques made available in the regulators' websites, with nearly universal coverage. The announcements include all policy actions, including the deployment of fiscal stimulus, monetary policy actions, asset purchase programs, credit facilities from multilateral institutions, and financial regulatory changes.

This paper extends the CFRT from Yale University by classifying the announcements by whether they are financial in nature and categorizing these policy actions by whether they constitute a relaxation of a financial regulation.[10] As a next step, announcements are also classified into sub-categories of regulatory policies such as changes in capital and liquidity requirements, limits on exposure, concentration, loan-to-value ratios, and postponement of financial reporting. Some of the capital regulation announcements were intended for financial firms to use the flexibility embedded in the regulatory frameworks (for instance the release of countercyclical capital buffers), therefore these measures are excluded from the analysis since they do not constitute a regulatory easing. Also, given that the focus of this paper is on regulatory easing, announcements of lower capital requirements that were accompanied by restrictions on dividend distributions are also excluded from the analysis. For completeness, non-regulatory financial announcements (such as emergency liquidity support, asset purchase initiatives, credit guarantees, and loan payment holidays) are also recorded.

Due to the scale and rapid developments of the pandemic, on several occasions regulators announced multiple policy measures in the same communiqué and/or during the same day.[11] In order to accurately identify the effects of financial measures and avoid confounding effects, financial announcements occurring on days in which other policy announcements were made (including fiscal, monetary, and/or other financial policy announcements) are dropped from the analysis. This in turn entailed parsing through all the announcements in the Yale's CFRT database in order to select only announcements of financial measures that occurred in isolation from other policy announcements.

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[10]A financial policy action is characterized as regulatory if it meets the taxonomy set forth in the joint IMF-World Bank staff position note on the regulatory and supervisory implications of COVID-19 for the banking sector (Narain and others, 2020).

[11]For example, the US Federal Reserve issued a communication on March 15, announcing the reduction of the reserve requirements to 0 percent; its commitment to purchase up to 500 USD billion in treasuries and 200 USD billion on mortgage backed securities; and encouraging banks to use their liquidity and capital buffers.

Further, for the events identified, the Yale's CFRT database is expanded by hand-collecting the precise timing of each announcement from the official press release. If the intraday time-stamp is not available in the official press releases, timestamps are obtained from announcements made through the social media accounts of national regulating agencies and/or local news reports, cross-checking all different sources where possible.[12] The choice of isolated announcements and identification of the precise timing of announcements is key to the identification strategy.

In total, the database includes more non-regulatory announcements (172) than regulatory (68) (Table 1). Regulatory announcements consist mainly of loosening of capital regulations, while non-regulatory announcements are more evenly distributed between credit support programs, emergency liquidity schemes, and other financial measures. In terms of sequencing, regulators responded to the COVID-19 shock by easing liquidity regulations before capital requirements in around 70 percent of jurisdictions that used both policy tools. The occurrence of regulatory announcements is almost evenly distributed between emerging markets (32) and advanced economies (36). On the other hand, the non-regulatory measures were most commonly observed in advanced economies.

Most of the financial policy announcements occurred in the first half of the year, with an important clustering around March and April, the period which was the onset of the pandemic and when governments around the globe implemented drastic containment measures (Figure 2). In the second half of the year, these containment measures were relaxed and economic activity started to recover, which explains the scant number of regulatory relaxations in this period. If anything, non-regulatory financial measures were used more often in the second half of 2020, with these measures consisting mainly of extension of government credit guarantees and liquidity support programs.

Figure 2. Number of Financial Regulation Easing Announcements During the Pandemic
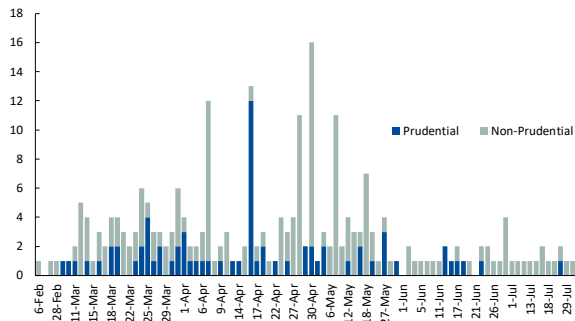
Source: Yale COVID-19 Policy Tracker (CFRT) and authors' calculations.

[12]In this case, the timestamp is obtained from the first instance of the announcement reported in news according to Factiva's global news database.

Table 1. Number of Financial Regulation Easing Announcements

Jurisdiction	Regulatory				Non-Regulatory			
	Capital	Liquidity	Other	**Total**	Credit Support	Emergency Liquidity	Other	**Total**
Argentina	0	1	0	**1**	3	0	1	**4**
Australia	2	0	0	**2**	0	0	0	**0**
Austria	2	0	0	**2**	2	1	2	**5**
Belgium	1	0	0	**1**	2	1	3	**6**
Brazil	3	2	0	**5**	5	3	2	**10**
Canada	0	1	0	**1**	0	5	0	**5**
Chile	3	0	0	**3**	2	2	0	**4**
China	1	2	1	**4**	2	0	0	**2**
Colombia	0	1	0	**1**	1	3	0	**4**
Estonia	1	0	0	**1**	0	1	0	**1**
Finland	1	0	0	**1**	2	1	1	**4**
France	1	0	0	**1**	1	1	3	**5**
Germany	1	0	0	**1**	1	1	2	**4**
Greece	1	0	0	**1**	2	1	2	**5**
Hungary	2	0	0	**2**	0	1	1	**2**
India	0	0	0	**0**	2	11	0	**13**
Indonesia	0	1	1	**2**	3	0	0	**3**
Ireland	1	0	0	**1**	2	1	2	**5**
Israel	1	0	0	**1**	0	1	0	**1**
Italy	1	0	1	**2**	2	1	4	**7**
Japan	2	0	0	**2**	0	4	3	**7**
Korea	0	1	2	**3**	3	2	3	**8**
Malaysia	1	1	0	**2**	1	0	0	**1**
Netherlands	1	0	0	**1**	1	1	2	**4**
New Zealand	1	1	2	**4**	0	3	0	**3**
Nigeria	1	0	0	**1**	0	0	1	**1**
Norway	1	0	0	**1**	1	4	0	**5**
Peru	0	1	1	**2**	2	1	0	**3**
Philippines	2	1	0	**3**	1	3	2	**6**
Russian Federation	0	1	0	**1**	4	0	1	**5**
Singapore	0	0	1	**1**	4	0	2	**6**
South Africa	1	0	0	**1**	0	1	0	**1**
Spain	1	0	0	**1**	2	1	3	**6**
Sri Lanka	1	1	0	**2**	1	1	0	**2**
Switzerland	1	0	0	**1**	1	1	1	**3**
Turkey	1	0	0	**1**	3	3	0	**6**
Ukraine	2	0	0	**2**	2	1	0	**3**
United States	3	1	2	**6**	3	5	4	**12**
Total	**41**	**16**	**11**	**68**	**61**	**66**	**45**	**172**

Source: Yale COVID-19 Policy Tracker (CFRT) and authors' calculations.

B. Stock Market Returns

The paper's second set of data relates to stock market performance. Intraday equity price indices are obtained from Bloomberg. In particular, hourly data is collected for Morgan Stanley Capital International (MSCI) Indices, both overall stock market indices and industry indices, from February 1 until July 31 2020. MSCI uses the Global Industry Classification Standard (GICS), which classifies companies into 11 sectors.[13] Financial sector stock price indices are available for all of the economies in the sample, but for some other indices data are incomplete and only available for a couple of non-financial sectors. In order to have enough cross-sectional variation in the non-financial sector indices, the sample is constrained to jurisdictions that have data on at least four of the following sectors: energy, information technology, health care, consumer staples, industrials, and materials.[14]

The analysis uses excess returns of sector-specific stock market price indices relative to their domestic market. This measure is constructed by subtracting the returns of sector i's overall stock market index from sector i's specific return (i.e., $ReturnSector_{i,t} - ReturnMarket_t$).[15] Table 2 shows some descriptive statistics for the excess returns for the financial and the non-financial sectors.

Financial industries around the globe, to varying degrees, have been under stress throughout the pandemic. The intraday excess return of financial sector stocks was on average -1.5 basis points, with almost 85 percent of the jurisdictions in the sample showing negative excess returns. On the other hand, the stock performance for non-financial industries was broadly in line with broad market returns, with excess returns on average being close to zero.

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[13]The use of industry stock price indices over firm level data is to aid the focus of analysis on the effects of asset price movements on financial conditions—given that industry level indices are more representative of the economy as opposed to individual firm level returns.

[14]Results presented in the robustness check section show the effects of financial regulatory announcements on the unconstrained sample.

[15]As a robustness check, the exercise is also done with additional equity return measures. Results are presented in section V.C.

Table 2. Descriptive Statistics for Sectoral Excess Returns (in basis points)

Jurisdiction	Mean		St. Dev.		50th pctl		25th pctl		75th pctl	
	Financial	Non-Financial	Financial	Non-Financial	Financial	Non-Financial	Financial	Non-Financial	Financial	Non-Financial
Australia	-0.2	0.0	73.5	72.5	-0.9	0.2	-26.7	-29.3	25.4	29.3
Austria	-1.1	-0.3	41.3	77.1	0.0	0.0	-19.8	-33.6	17.7	29.2
Belgium	-2.6	1.7	46.0	55.4	0.0	0.0	-21.0	-20.9	15.9	24.2
Brazil	-2.2	-1.2	106.1	113.4	-2.0	-0.1	-54.7	-51.9	49.8	51.6
Canada	-0.4	0.5	25.0	59.1	-0.5	0.2	-13.3	-23.4	11.0	25.3
Chile	0.4	-14.7	51.2	164.0	3.0	-4.3	-27.2	-53.0	25.2	39.3
China	0.2	0.3	23.7	38.4	-0.2	0.1	-8.4	-11.5	6.6	11.3
Denmark	-2.5	-0.4	49.7	37.8	-2.2	0.0	-30.6	-17.7	25.1	16.5
Finland	-3.0	0.7	47.6	54.0	0.0	0.0	-24.0	-21.0	17.4	21.5
France	-2.2	0.6	42.9	42.1	-0.7	0.0	-21.9	-15.8	15.7	16.2
Germany	-0.5	0.4	28.5	30.1	-0.3	0.0	-13.8	-14.1	13.2	14.2
India	-1.2	0.0	39.6	38.4	0.0	0.0	-15.8	-16.5	12.2	15.0
Indonesia	-0.1	1.3	64.8	84.4	0.0	0.0	-19.4	-26.2	19.8	27.8
Ireland	-9.3	0.2	124.2	67.2	-1.0	0.0	-66.0	-26.3	48.2	25.5
Israel	-5.2	5.5	65.8	81.0	0.4	0.9	-29.8	-25.8	24.5	36.3
Italy	-0.6	-0.9	22.4	40.2	0.0	0.0	-13.4	-22.0	12.0	19.0
Japan	-0.5	0.0	22.8	23.0	0.0	0.0	-8.6	-6.9	6.4	7.0
Korea	-0.2	0.2	35.7	39.7	0.0	0.0	-16.3	-14.3	14.1	13.7
Malaysia	-1.6	1.6	24.5	42.5	0.0	0.0	-8.2	-9.4	4.8	11.3
Mexico	0.3	0.3	61.7	43.3	-1.4	1.2	-28.4	-20.3	30.7	21.1
Netherlands	-4.2	-0.1	67.7	45.4	-1.8	-0.1	-33.3	-17.7	25.0	17.7
Norway	-0.2	-0.2	34.8	49.0	0.0	0.0	-15.8	-19.3	16.0	18.2
Singapore	-0.4	0.8	13.9	44.4	-0.1	0.0	-7.7	-19.0	6.7	18.8
South Africa	0.7	0.3	51.2	84.3	1.1	0.0	-22.6	-35.7	24.5	37.1
Spain	-3.4	-1.9	50.4	68.0	-4.5	-2.6	-31.9	-31.4	24.3	26.9
Sweden	-0.4	0.2	20.8	38.7	0.0	0.0	-11.1	-14.7	9.1	14.5
Switzerland	-2.2	0.4	40.6	36.9	0.0	0.0	-18.0	-12.0	14.1	12.7
Thailand	-3.0	1.1	37.6	46.9	0.0	0.0	-17.8	-16.6	7.9	19.9
Turkey	-2.0	0.8	24.2	44.7	-1.6	-1.1	-13.6	-20.2	8.5	18.1
United Kingdom	-0.1	0.2	31.2	47.4	-1.7	0.4	-16.6	-20.0	15.6	20.6
United States	-0.1	-0.6	30.7	38.2	-1.0	-0.4	-17.0	-14.7	16.1	13.8

Source: Bloomberg Financial L.P. and author calculations.

Data coverage for bank loan liabilities, however, is poor for most emerging market economies in the sample with data for over 90 percent of listed firms not available. Thus, the bank-finance dependence measure is based on US firms and is likely to be a lower bound (as explained in footnote 12), therefore it is treated as a structural characteristic of the corresponding industries (i.e. some industries are inherently more bank-dependent than others, irrespective of cyclical considerations). This measure allows the ranking of industries according to their reliance on bank finance and more importantly compute the stock market return of firms in sectors that are more dependent on bank-based finance relative to firms in sectors that are less bank dependent. Figure 3 plots the measure of bank dependence variable by GICS sectors included in the analysis. Healthcare sector has the largest reliance on bank finance at about 50 percent while consumer staples ranks the lowest among sectors at about 35 percent.

**Figure 3. US Sectoral Bank-finance Dependence
(bank loans as a percent of total sector liabilities)**
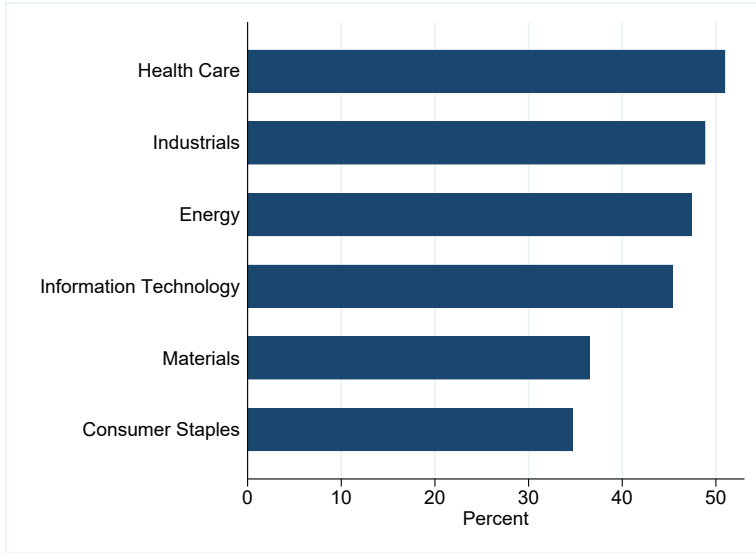
Sources: Bloomberg financial L.P. and authors' calculations.

C.   Jurisdiction Sample

The final sample contains policy announcements for 26 economies (18 advanced economies and 8 emerging markets), for which stock market and policy data is available (Table 3). The sample accounts for 78 percent of global GDP. For this constrained sample there are 51 announcements related to the easing of prudential regulations and 133 announcements related to non-regulatory policies. The next section presents the paper's main results.

**Table 3. Sample of Jurisdictions Used in the Analysis**

Australia	Chile	India	Japan	Singapore	United States
Austria	China	Indonesia	Korea	South Africa	
Belgium	Finland	Ireland	Malaysia	Spain	
Brazil	France	Israel	Netherlands	Switzerland	
Canada	Germany	Italy	Norway	Turkey	

IV. FINANCIAL POLICY EASING AND EQUITY MARKET REACTIONS

This section presents the estimates from equation (3), corresponding to the cumulative responses over a one-hour horizon with the 90-percent confidence intervals based on Driscoll and Kraay (1998) standard errors, which are robust with respect to heteroskedasticity as well as serial and cross-sectional correlation.

The market response to regulatory easing signals perceived trade-offs (Figure 4). The loosening of prudential policies leads to near a one percentage point decline in financial sector stock returns one hour after the announcement, with the effect being significant at the 90 percent level. The opposite is observed for non-financial equity returns, with stock prices outperforming aggregate indices immediately after the policy announcement. The net effect of regulatory easing on excess returns appears to have been overall positive.

Figure 4. Cumulative Impulse Responses of Excess Equity Returns to Financial Regulatory Announcements (Percentage points)
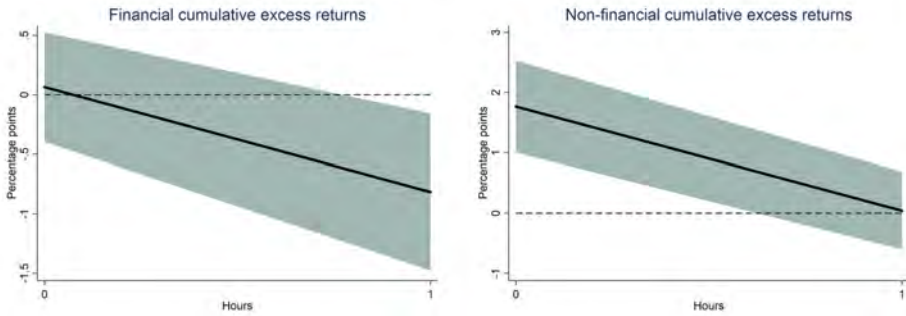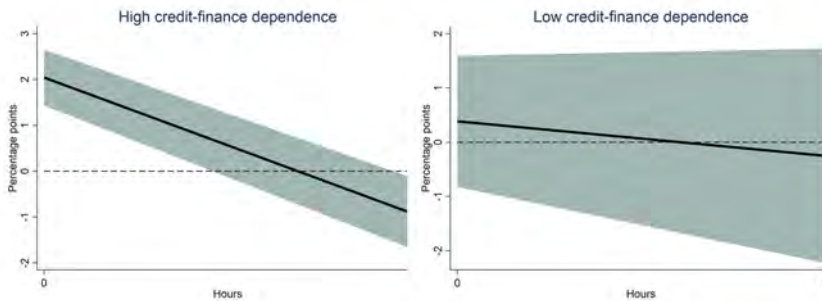

Note: Impulse responses obtained using Jordà (2005) local projection methods. Solid black line shows OLS point estimates. Teal shades are 90% confidence bands.

The negative effect of regulatory news on financial sector returns is in line with recent findings in the literature (Demirgüç-Kunt, Pedraza, and Ruiz-Ortega, 2020). This negative response associated with the easing of financial regulations could be interpreted as markets seeing these measures leading to an increase in risk-taking (e.g. by increasing leverage or weakening underwriting standards) that could cloud the prospects for future cash flows for financial sector firms, leading to lower equity value. In essence, under this interpretation, investors could be expecting credit expansion under weaker underwriting standards. Under such interpretation, the results are also related to the findings in Gandhi (2018), who shows that a one percent increase in aggregate credit growth is followed by a nearly three percent decline

in the excess return of bank stocks one year later. Baron and Xiong (2017) also document that credit booms are the best predictors for bank stock crashes.[16]

Markets perceive benefits accruing more among bank-dependent non-financial sectors. Figure 5 presents the response of excess returns for non-financial sector firms conditioning on the sector level measure of firms' dependence on bank-based financing discussed in the previous section. The sample is split into two groups. One group consists of industries with a high level of bank-credit dependence—above the 70th percentile. The other group consists of industries at or below the 30th percentile of the distribution of the bank-credit dependence metric (i.e., the share of total bank loans over total liabilities for firms in sector i). Following the announcement of prudential policies, equity excess returns react only in high bank-dependent sectors on impact.[17] As expected, equities for sectors that do not depend much on bank-finance were not significantly affected by these announcements.

Figure 5. Cumulative Impulse Responses of Excess Equity Returns of Non-Financial Industries to Financial Regulatory Announcements (Percentage points)

Note: Impulse responses obtained using Jordà (2005) local projection methods. Solid black line shows OLS point estimates. Teal shades are 90% confidence bands.

Policy composition matters, with market reactions varying with the type of regulation announced. Table 4 summarizes the effects of financial policy announcements on sectoral excess returns by instrument type, for regulatory and non-regulatory financial measures.

[16]This section shows effects on all financial sector firms include banks, insurance institutions, and other non-bank financial institutions. Section V.C shows that a similar response is observed when looking only at excess returns of bank stocks. The use of a financial industry stock price indices over bank level data is preferred as non-bank financial institutions are important in many jurisdictions and movements on the industry index would likely have a larger effect on overall financial conditions.

[17]The positive market response could be explained by the fact that the announced policies are expected to preserve the free flow of credit to cash-strapped firms, which in turn would allow them to maintain production, employment, and/or investment. Altavilla and others (2020) find that, in absence of easier prudential requirements put forward in Europe, the pandemic would lead to a significantly larger decline in firms' employment.

Equity excess returns drop after capital regulation easing announcements for financial and non-financial industries. [18] As explained before, the relaxation of capital requirements could lead to lower valuation for financial sector firms as markets expect increased risk taking by financial institutions. The decline in non-financial sector stocks is consistent with the interpretation of a potential credit crunch down the road. Excess returns on the financial sector decline immediately after capital announcements by 1.4 percentage points, and the effect being statistically significant at the 99 percent level. The loosening of capital requirements has also a large negative effect on non-financial equities.

This result is consistent with (Elenev, Landvoigt, and Van Nieuwerburgh, Forthcoming), which shows that bank shareholders can gain from tighter bank capital regulation, as higher capital requirements force banks to shift their capital structure to equity. In this regard, looser capital requirements would have an opposite effect (i.e., reduce equity valuations for banks). It can be argued that the negative equity price response to lower capital requirements could be driven by the information content of the policy action (e.g., the regulator signaling private information about the health of the financial system). However this interpretation would require that such information content works only through capital and not liquidity regulation, which is difficult to rationalize. [19] It is worth noting that financial institutions entered the COVID-19 crisis broadly in good health and with sizable buffers, therefore regulators lowering capital requirements could instead signal their confidence in the system (of its capacity to weather the shock).

There is an overall positive market reaction to lower liquidity requirements, for which the effects on financial stock returns are small and not statistically significant while the effect on non-financial firms is largely positive and statistically significant. Announcements of looser liquidity requirements (e.g., change in liquidity coverage ratios) led to an immediate spike in excess returns in non-financial stocks by around 1.9 percentage points, an effect that is statistically significant at the 99 percent level. Given the temporary nature of the COVID-19 shock and the unprecedented scale of monetary policy support in several key jurisdictions, it comes as no surprise that markets had a relatively more sanguine view regarding liquidity risks (as compared to solvency risks).[20] Interestingly, non-regulatory financial measures (e.g. asset purchases, government credit guarantees, and emergency liquidity programs) did not significantly affect sectoral excess returns. This result is in line with recent papers that look at the effect of financial policies on stock prices during the pandemic (Demirgüç-Kunt,

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[18]The results remain virtually unchanged if announcements related to countercyclical capital buffers were to be included, reflecting possibly the expectations for the release of these buffers as they are part of the built-in flexibility in regulatory frameworks following Basel III.

[19]Furthermore, regulators eased liquidity before capital regulations in around 2/3 of the jurisdictions included in the sample (see Section III). For these jurisdiction then, it is the loosening of prudential liquidity requirements that would have provided the negative information content.

[20]Although not reported, the effect of other regulatory policies (e.g. relaxation of loan-to-value requirements, postponement of financial reporting, and in some cases prudential capital flow measures) had a null effect on excess returns.

Pedraza, and Ruiz-Ortega, 2020; Schwert, 1981).[21] The lack of statistical significance for non-regulatory financial measures could be explained by the large degree of heterogeneity across measures in these group.

Table 4. Impact of Financial Announcements on Excess Equity Returns (at t=0)

VARIABLES	(1) Financial	(2) Financial	(3) Non-Financial	(4) Non-Financial
Regulatory	0.064		1.765***	
	(0.281)		(0.465)	
Non-regulatory	0.083		0.177	
	(0.224)		(0.192)	
Regulatory_capital		-1.392***		-1.284**
		(0.454)		(0.498)
Regulatory_liquidity		0.665		1.908***
		(0.482)		(0.428)
Non-regulatory_liquidity		0.052		0.134
		(0.144)		(0.239)
Non-regulatory_other		-0.069		-0.110
		(0.148)		(0.112)
Observations	139	173	553	617
R-squared	0.668	0.597	0.546	0.534
Country FE	YES	YES	YES	YES
Time FE	YES	YES	NO	NO
Sector-Time FE	NO	NO	YES	YES
Dependent variabale	Excess Return	Excess Return	Excess Return	Excess Return

Note: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

A. Regulation announcements and financial conditions

One caveat with intraday event studies is that, while they can help with statistical identification, they cannot say much about the validity of the results beyond the window of observation. In order to judge economic importance of the effects of financial regulation easing, this section looks into how a regulation easing announcement shock, differentiating between liquidity and capital announcements, would affect financial conditions. Financial conditions summa-

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[21] Interestingly, Sever and others (2020) shows that global factors seem to have had a more significant effect on domestic stock markets. In particular, the quantitative easing announcement by the Federal Reserve supported EM stock markets.

rize the cost of funding for firms and reflect the underlying price of risk in the economy[22]. Therefore, the large intraday equity price movements following the financial regulation announcements described in the previous section, could be economically important as stock prices have been key drivers of global financial conditions (IMF, 2020b). The exercise estimates the effect of regulation changes onto financial conditions by analyzing the responses of financial condition indices (FCIs) to regulation announcements using a panel VAR (PVAR) framework.[23]

The PVAR follows closely Towbin and Weber (2013) and captures the dynamic relationship of stock market returns and changes in FCIs in a sample of 24 economies with daily data from January 1 until July 31, 2020. The FCI index, constructed by Goldman Sachs, is available at a daily frequency and provides the largest country coverage. Following Burnside, Eichenbaum, and Fisher (2004) and Cavallo (2005) the policy announcement dates are embedded in the PVAR model. The regulation announcements are the same ones used in the previous sub-section, and described in detail in Section III. Interaction terms allow the model's coefficients to vary deterministically with the jurisdiction income classification (i.e. advanced economy or emerging market). The recursive interacted PVAR has the following form:

$$
\begin{pmatrix} 1 & 0 \\ \alpha_{0,it}^{2,1} & 1 \end{pmatrix} \begin{pmatrix} Announcement_{i,t} \\ FCI_{i,t} \end{pmatrix} = \sum_{l=1}^{L} \begin{pmatrix} \alpha_l^{1,1} & 0 \\ \alpha_{l,it}^{2,1} & \alpha_{l,it}^{2,2} \end{pmatrix} \begin{pmatrix} Announcement_{i,t-l} \\ FCI_{i,t-l} \end{pmatrix} + U_{it}
$$

where $Announcement_{i,t}$ is a dummy variable that equals to 1 on the day of the regulation announcement (and 0 otherwise); $FCI_{i,t}$ is the daily FCI estimated by Goldman Sachs. U_{it} is a vector of uncorrelated i.i.d. shocks. L denotes the number of lags. The coefficients $\alpha_{l,it}^{j,k}$ are allowed to vary deterministically as a function of the income level through the inclusion of an interaction term (a 0/1 dummy variable, which equals to 1 if the jurisdiction is an emerging market):

$$
\alpha_{0,it}^{j,k} = \beta_{0,1}^{j,k} + \beta_{0,2}^{j,k} \times Income_i
$$

Each equation in the system is estimated using ordinary least squares (OLS), with two lags, selected using the Schwartz Criterion. As the impulse responses are non-linear functions of the OLS estimates, standard errors are estimated using the bootstrap procedure proposed by Runkle (1987) summarized in Towbin and Weber (2013).

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[22] Adrian and Liang (2018), for example, argue that accomodative policies can create an intertemporal tradeoff between improving current financial conditions at a cost of increasing future financial vulnerabilities

[23] Given that the lack of intraday FCIs, the use of a PVAR framework is preferred over the local projection method, given that the PVAR allows to capture possible feedback effects from movements in FCIs to regulatory decisions. Since the economic significance of regulation is likely to be observed over longer time horizons, the assumption of strict exogeneity of regulation to market developments is likely to be violated when the window of analysis goes beyond a day.

It is common in the literature on the effects of policy shocks based on VAR models to impose the restriction that outcome variables (e.g., output, inflation, etc.) react immediately to policy shocks, whereas policy variables do not react contemporaneously to other shocks in the system. This identifying assumption is the standard Cholesky decomposition with the regulation policy variable ordered first in the VAR. The analysis presented in this subsection employs the same identification strategy, as intricacies in the design and deployment of prudential measures would somewhat limit the contemporaneous reaction of regulation. This timing restriction is plausible given the use of daily frequency data in the analysis.

In line with the results from the intraday analysis using equity prices, the effects of regulatory easing announcements on FCIs vary depending on the type of policy measure (Figure 6). Announcements of lower capital requirements led to a tightening of FCIs on impact, with the effect peaking during the first 5 days after the announcement. Although the effect of lower liquidity requirements on FCIs is not statistically significant on impact, this type of regulation easing eventually led to a significant loosening of FCIs. Furthermore, the effects are highly persistent, which highlights the important economic implications that these type of regulations can have.

Figure 6. Impulse Responses of Financial Conditions to Financial Regulatory Announcements
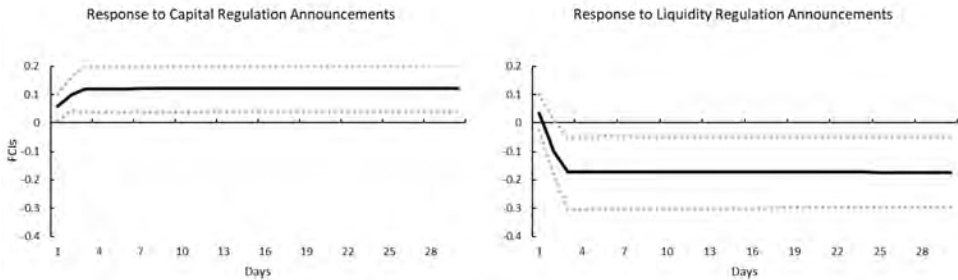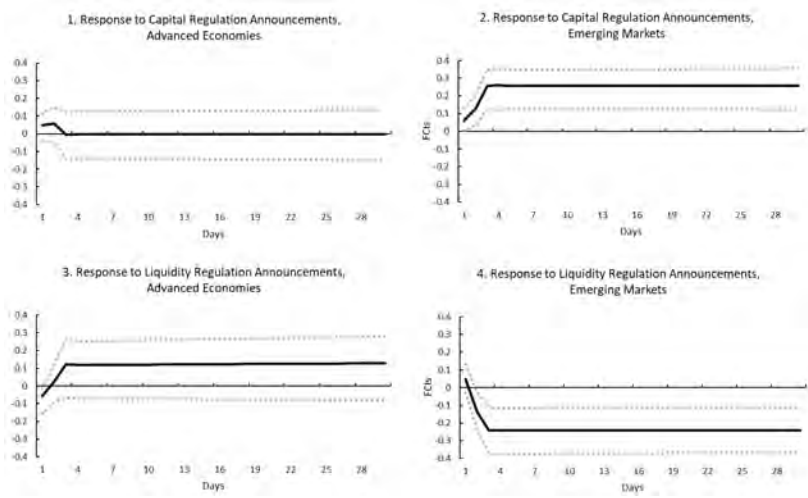

Note: Impulse responses using a PVAR framework with a recursive ordering with regulation announcement dummies ordered first. Solid black lines represents OLS point estimate. Dashed lines are one standard deviation confidence bands. A positive (negative) value denotes a tightening (loosening) of financial conditions.

Splitting the sample into advanced economies (AEs) and emerging markets (EMs) shows that regulation easing had the largest impact in EMs (Figure 7). For both income groups, announcements related to lower capital requirements tightened FCIs on impact, however the effect is short-lived and not statistically significant for AEs ((Figure 7, panel 1). In contrast, for EMs the effect of capital regulation announcements is very persistent, reaching its peak in the first week after impact (Figure 7, panel 2). Liquidity easing announcements loosen FCIs in AEs on impact, but the effect on FCIs turns statistically insignificant in the days after impact (Figure 7, panel 3). The effect is much larger and significant (both economically and

statistically) in EMs, with announcements of lower liquidity requirements leading to a large and persistent loosening of financial conditions (Figure 7, panel 4).

Figure 7. Impulse Responses of Financial Conditions to Financial Regulatory Announcements by Income Group

Note: Impulse responses using a PVAR framework with a recursive ordering with regulation announcement dummies ordered first. Solid black lines represents OLS point estimate. Dashed lines are one standard deviation confidence bands. A positive (negative) value denotes a tightening (loosening) of financial conditions.

V. ROBUSTNESS

This section performs a battery of robustness checks comprising: (i) expanding the sample to include jurisdictions with at least one non-financial industry stock market index, (ii) using alternative measures of equity returns, and (iii) using bank-level stock returns instead of an aggregate financial sector index. All the results remain broadly unchanged.

A. Expanded sample

Figure 8 presents the responses of the excess stock market returns for the financial and non-financial sectors for an expanded sample, which includes all jurisdictions with at least one non-financial industry sector index. If anything, trade-offs intensify in the larger sample. The response of financial sector equity becomes more significant than in the baseline. For non-financial industries, the response is considerably more limited on impact, and becomes

negative one hour after the announcement—albeit not significantly. These results suggest that the tradeoffs stemming from COVID-19 related financial regulations were more intense in jurisdictions with smaller and less liquid financial sectors. [24]

Figure 8. Cumulative Impulse Responses of Excess Equity Returns to Financial Regulatory Announcements, Expanded Sample (Percentage points)
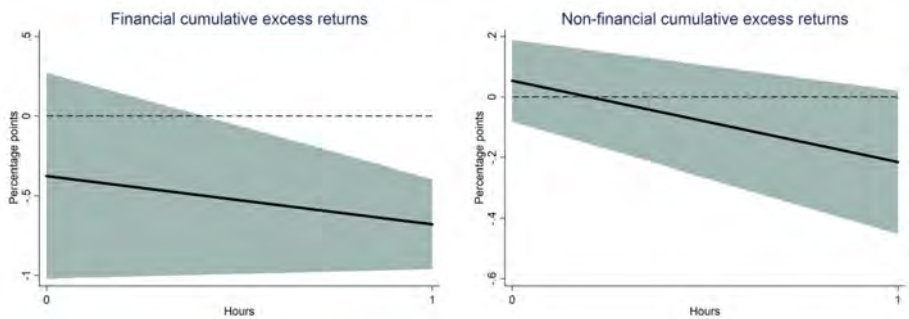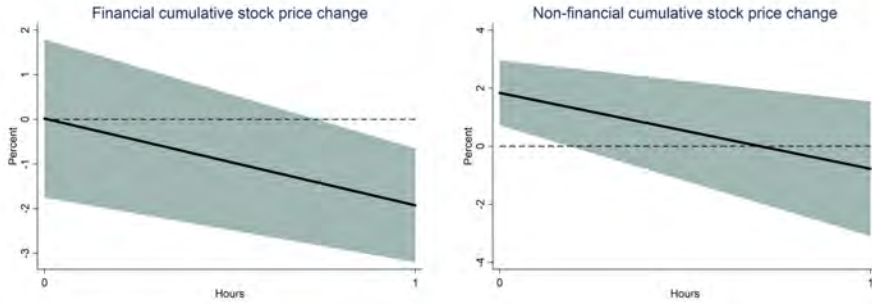

Note: Impulse responses obtained using Jordà (2005) local projection methods. Solid black line shows OLS point estimates. Teal shades are 90% confidence bands.

B.   Alternative equity return measures

The results are also robust to using alternative metrics for equity returns. Figure 9 shows that equity prices, measures in hourly percent changes, declined by almost 2 percent in the hour after announcements of regulatory easing for financial sector firms. In line with the baseline results, equity prices for non-financial industries also significantly increase on impact.

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[24]Demirgüç-Kunt, Pedraza, and Ruiz-Ortega (2020) show that in jurisdictions that are not part of the Basel Committee on Banking Supervision, prudential measures are accompanied by large declines in banks' stock prices.

Figure 9. Cumulative Impulse Responses of Equity Prices to to Financial Regulatory Announcements (Percent)
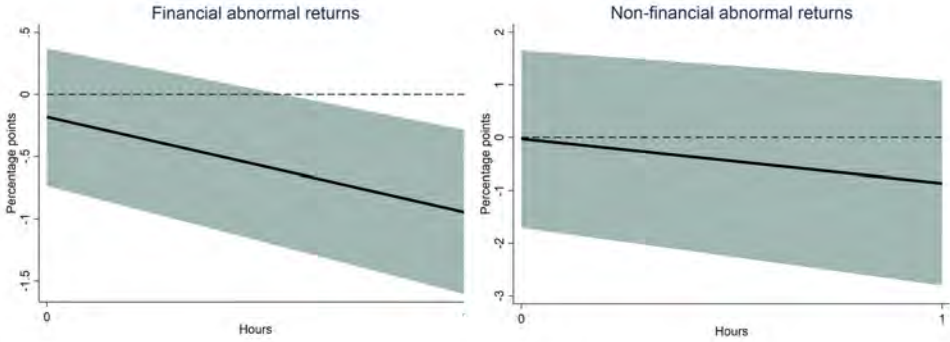

Note: Impulse responses obtained using Jordà (2005) local projection methods. Solid black line shows OLS point estimates. Teal shades are 90% confidence bands.

Similar results are also obtained when the dependent variable is a market model for abnormal returns (figure 10). The accumulated abnormal returns are obtained by estimating the difference between realized returns and the expected returns implied by the following market model:

$$EquityReturn_{i,t} = \alpha_t + \beta_i MarketReturn_{i,t} + \varepsilon_{i,t} \tag{5}$$

where the error term $\varepsilon_{i,t}$ is the abnormal return for sector i at time t. In line with the baseline results, regulatory easing announcements lead to decline in abnormal returns in the hour after impact. However, explained in part by the large heterogeneity across industries and economies, the estimates for the effect of regulatory easing on the abnormal returns of non-financial firms are very noisy, resulting in very wide confidence bands around the OLS point estimates.

**Figure 10. Cumulative Impulse Responses of Abnormal Eq-
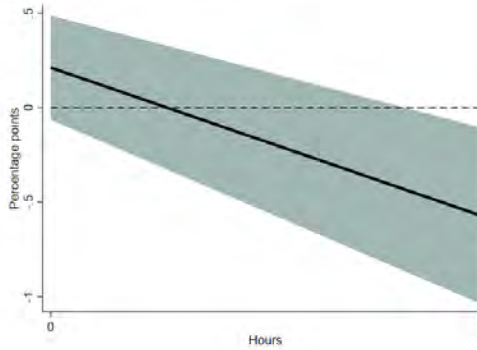uity Returns to to Financial Regulatory Announcements
(Percentage points)**

Note: Impulse responses obtained using Jordà (2005) local projection methods. Solid black line shows OLS point estimates. Teal
shades are 90% confidence bands.

C. Bank equity returns

Given that in some jurisdictions equities for non-bank financial institutions could be an im-
portant component in financial sector indices, we also isolate the response of banks, as these
institutions are likely to be the ones most affected by changes in regulation. Figure 11 shows
the average response of banks' excess stock return, with these being very similar to the re-
sponses of the financial equity indices used in the baseline specification (i.e. a significant
negative excess return one hour after the announcement). These results suggest that the re-
sponses of financial sector equity indices were mainly driven by bank stock prices.

**Figure 11. Cumulative Impulse Response of Bank's Excess
Equity Returns to to Financial Regulatory Announcements
(Percentage points)**

Note: Impulse responses obtained using Jordà (2005) local projection methods. Solid black line shows OLS point estimates. Teal shades are 90% confidence bands.

VI. Conclusion

Despite the negative effect on financial sector stock prices, the effect of regulatory easing on non-financial sector equity returns appears overall positive, particularly in jurisdictions with large and liquid financial markets. Together with other policies, looser financial regulations helped contain amplification of the COVID-19 shock, at least in the near term. However, the market reaction signals important trade-offs and the need to handle these policies with care. There has been a generalized negative market reaction to easier bank capital regulation, possibly suggesting the expectation of increased risk-taking by financial firms. In contrast, news about financial regulation easing positively affected non-financial sector returns, with markets seeing these measures as being conducive to looser borrowing constraints and financial conditions for firms, supporting employment and production, at least in the near-term. The effects of looser regulations, and the difference between the effects of capital and liquidity regulation on financial conditions, is particularly large in emerging markets. This suggests that the downside risk from depletion of capital buffers is perceived to be significant while, on the other hand, liquidity regulation easing was successful in lowering funding costs and boosting earnings among financial sector firms in emerging markets.

In terms of composition, tradeoffs appear to be small for liquidity measures as equity prices increased in response to easier liquidity regulation for financial and non-financial firms. However, lower capital requirements led to a significant decline in equity prices for financial and non-financial industries alike. The analysis did not find statistically significant responses of stock prices to the relaxation of other financial regulations, such as concentration requirements or borrower-based measures, or non-regulatory financial measures. This could in part

be explained by the larger heterogeneity among policy measures in this class. Finally, results presented in this paper could help inform the design of plans to roll back regulatory easing, for instance by rolling back capital related regulations first to help rebuild buffers, once the recovery is on a firm footing. This of course implicitly assumes that the effects detected in this paper carry through symmetrically. If this is the case, the rollback of regulation should be gradual in order to prevent a sudden tightening of financial conditions.

While the regulatory response to the COVID-19 shock provides insights on how regulation could affect the financial system and the economy, caution should be exercised in drawing broader lessons applicable to future episodes when the shock might be of different, non-exogenous nature. As the crisis evolves more work on the effects of the recent regulatory easing will also help better understand its effects beyond the near term and inform possible regulatory reforms in the future, particularly with regards to the composition of regulatory capital buffers (e.g. conservation, countercyclical, etc.). Finally, additional work is also needed to understand the effects of specific non-regulatory financial policies (e.g., asset purchase programs), as the ambiguous effects of these measures (in this and other studies) could be a result of the bunching of different policies and strategies together.

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Gender and employment in the COVID-19 recession: Cross-country evidence on "she-cessions"[1]

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Early evidence on the pandemic's effects pointed to women's employment falling disproportionately, leading observers to call a "she-cession." This paper documents the extent and persistence of this phenomenon in a quarterly sample of 38 advanced and emerging market economies. We show that there is a large degree of heterogeneity across countries, with over half to two-thirds exhibiting larger declines in women's than men's employment rates. These gender differences in COVID-19's effects are typically short-lived, lasting only a quarter or two on average. We also show that she-cessions are strongly related to COVID-19's impacts on gender shares in employment within sectors.

I. INTRODUCTION

The pandemic is having very unequal effects across socioeconomic groups with vulnerable workers more at risk of losing their jobs (Chetty and others 2020, IMF 2021). More specifically, a growing literature has been examining the differences in labor market consequences of the COVID-19 crisis by gender. Many recent studies have argued that the crisis is causing a "she-cession," where women's labor market outcomes and prospects have deteriorated disproportionately (Albanesi and Kim 2021, Alon and others 2020, Alon and others 2021, Caselli and others 2020, Fabrizio and others 2021, and Shibata 2020, among others). This pattern contrasts with the "man-cession" observed after the global financial crisis in which men appeared to be much more heavily impacted, according to U.S. studies (Wall 2009; Hoynes, Miller, and Schaller 2012).

There has been vigorous debate on the underlying drivers of the COVID-19 crisis's asymmetric labor market impact by gender, with several factors cited. First, women's employment shares are higher in contact-intensive sectors which have been more severely affected (Mongey, Pilossoph, and Weinberg 2020 and Albanesi and Kim 2021). Second, women tend to carry a higher childcare burden when schools are closed (Adams-Prassal and others 2020, Fuchs-Schündeln and others 2020, Hupkau and Petrongolo 2020, Russell and Sun 2021, and Zamarro and Prados 2020). Third, women are more often employed in temporary and part-time jobs that are more at risk of being terminated in a downturn (Petrongolo 2004 and Bahn and Sanchez Cumming 2020). All these factors contributed to the she-cession in the first months of the pandemic, with women's unemployment rising disproportionately more than that of men in several countries.

While most studies discussing the pandemic's economic consequences by gender focus on the U.S. or a few advanced economies, this paper examines the labor market impacts of the COVID-19 crisis by gender at quarterly frequency for a large panel of 30 advanced economies and 8 emerging market economies, making three key contributions. First, we document the extent and persistence of COVID-19 crisis-related she-cessions across countries within 2020, where a she-cession is called for a quarter in 2020 when there is a negative difference between women's and men's employment rate (employment-to-population ratio) changes compared with 2019 (that is, women are losing more jobs than men as a share of population). Second, we quantify the role of sectoral employment composition in gender differences in labor market outcomes. Third, we further elaborate on recent labor market dynamics by gender, examining the relative importance of the intensive versus extensive margins by gender (average hours worked versus employment) and movements in unemployment versus labor force participation that underlie employment changes by gender. Our paper is related to contemporaneous work by Alon and others (2021), which takes a similar cross-country approach, but focuses on the cyclicality of women's and men's hours-worked and employment.

We find that over half to two-thirds of the countries in our sample experienced she-cessions in 2020:Q2, depending on whether the gap change is gauged as a percentage point or percent change. By contrast, only about 8 percent of the countries in our sample experienced a she-cession during the depths of the global financial crisis (2009:Q2). Moreover, the COVID-19 crisis she-cessions tend to be short-lived, fading by 2020:Q3 for three-fifths to two-thirds of countries.

The analysis of sectoral employment patterns reveals that COVID-19 she-cessions are associated with drops in women's sectoral employment shares, particularly in sectors accounting for more of women's total employment. For countries where we have sectoral employment by gender, almost all of those which experienced she-cessions were predominantly driven by declining women's shares in employment within sectors.

The employment rate is a standard measure of overall labor market conditions that captures the extensive margin of labor utilization. However, it can mask changes in other margins that may be of interest. The employment rate misses labor market adjustments in hours worked (the intensive margin of labor), which could be significant with the widespread deployment of short-term work schemes to preserve job links while enabling reductions in employers' labor costs through reductions in working hours (Giupponi and Landais 2020; IMF COVID-19 Policy Tracker). The labor force participation margin may be particularly important in understanding the consequences of the COVID-19 recession by gender.[2] As noted before, women may have opted out of the labor market in greater numbers to care for their children who are not in school. Moreover, early in the COVID-19 recession, entire sectors had to shut down, leading some laid-off workers to simply cease participating in the labor market (Coibion, Gorodnichenko, and Weber 2020).[3]

We find that men have typically seen average hours worked fall by more than women in our sample of countries. She-cession gender gap changes in the first three quarters of 2020 are far more evident along the extensive margin (employment) than the intensive margin (average hours worked). She-cession gender gap changes more often reflected larger declines in women's than men's labor force participation rates than relative rises in women's unemployment rates. The average narrowing of she-cession gender gap changes in employment as of 2020:Q3 appears to arise from both relative improvements in labor force participation by women and shrinking of unemployment rate differences.

The consequences for job losers can be dire, as they face earnings losses and difficulties in finding a job after unemployment spells (IMF 2021). Policies to support adversely impacted workers over the near term—such as wage subsidies—can help mitigate these losses. Women's employment may also be particularly sensitive to measures that help alleviate childcare burdens (Vuri 2016). Even after the pandemic abates, some of the effects on the structure of employment may persist, with some sectors permanently shrinking and others growing. For these persistent effects, the speed with which economies can reemploy and reallocate impacted workers across sectors—for example, through training and hiring incentives—will determine how long-lived the effects on employment are. Recent studies pointing to the productivity increases associated with gender diversity at all levels of the workforce suggest that targeting gender gaps specifically could generate a double benefit: improvements in overall labor market conditions and a productivity boon (Elborgh-Woytek and others 2013; Gonzales and others 2015).

There are some potential channels by which the COVID-19 recession could have long-lasting, asymmetric adverse effects on women's prospects that may not be visible in recent employment trends. Some recent survey results suggest that women are more likely to be rethinking career decisions in light of the pandemic (Romei 2021), which could mean that the crisis leads to a future drop in women's labor force participation. Moreover, career interruptions that fall more heavily on women from the COVID-19 crisis could adversely impact their longer-term earnings and employment prospects (Albrecht and others 1999, Aisenbrey, Evertsson, and Grunow 2009).

The rest of the paper is organized as follows. Section 2 describes the data and the definition of she-cession used throughout the paper. Section 3 presents the baseline results on employment rates by gender. Section 4 explores sectoral employment differences as a driver of COVID-19 she-cessions.

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[2] Blau and Khan (2013) and Goldin (2015) find that women tend to have more fragile labor force attachment and be at greater risk of dropping out of the labor force when they experience adverse shocks.

[3] Based on a large-scale survey of US households, Coibion, Gorodnichenko, and Weber (2020) find that many workers who lost their job during the COVID-19 recession are not actively looking for jobs, suggesting a large fall in labor force participation.

Section 5 examines gender gaps along alternative labor margins, including hours worked, labor force participation, and unemployment. Section 6 reports some robustness exercises using an alternative she-cession measure. Section 7 concludes.

II. SHE-CESSIONS: DATA AND DEFINITION

Our analysis relies on a mix of OECD and Eurostat labor market data, harmonized within analytical exercise at quarterly frequency. The employment rate (employment-to-population ratio) and the unemployment rate (number of unemployed relative to the labor force, defined as the sum of employed and unemployed workers) by gender and age are available for 38 countries at the quarterly frequency from the OECD, with 30 advanced economies and 8 emerging market economies in the sample. For the sectoral analysis of employment by gender, quarterly data from Eurostat are used, covering 19 European countries with populations over 2 million inhabitants. Average hours worked are also available for European countries only. The analysis ends in 2020:Q3 due to data availability. Table A1 shows the list of countries and the samples used across the different exercises.

We start by presenting the evolution of the employment rate by gender over 2020, with respect to 2019. There is a sharp drop in the median country's employment rate in 2020:Q2 for both women and men (Figure 1, panel a). This is not surprising as this period coincides with the beginning of the pandemic and the introduction of strict containment measures by most countries in our sample. Women's employment rate falls by around 1.7 percentage points and men's by around 1.6 percentage points from their average levels in 2019. When gauged using the percent change in the employment rate relative to 2019, the declines are about 2.5 percent for women and about 2 percent for men, suggesting a larger difference if base effects (lower preexisting employment rates for women) are incorporated (Figure 1, panel b).

Figure 1. Employment Rate By Gender Over 2020
(Deviation from 2019 average)

Panel (a) - Percentage point change

Panel (b) - Percent change
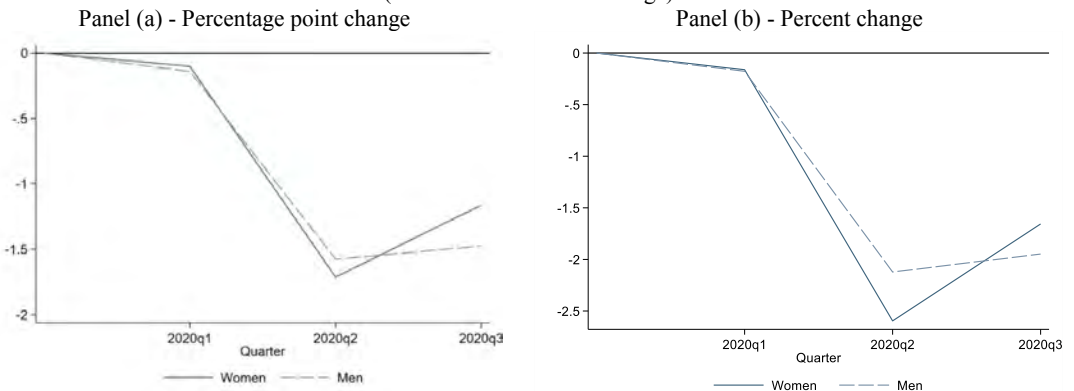
Note: The chart reports the evolution of the ratio of employment to population by gender with respect to their 2019 averages. The sample includes 38 advanced and emerging market economies. Panel (A) reports the percentage point deviation as shown Equation (1) and Panel (B) exhibits the percent change as shown in Equation (2).

While the trough of the two median series appears to be in 2020:Q2, women's employment rate tends to recover faster in 2020:Q3. This might be due to reopening of some of the hardest hit sectors, such as hospitality and personal care, in 2020:Q3 as containment measures were relaxed. Similarly, in many of the countries in our sample, schools reopened in the third quarter of 2020. Since 2020:Q2

represents the trough of the average employment series for both men and women, we focus on labor market differences.

We consider two possible definitions for a she-cession, based on alternative measures of the change in employment rates by gender. The first is based on the percentage point difference in the change in women's and men's employment rates:

$$\Delta e_{t,2019avg}^{Diff,W-M} = \left(e_t^W - e_{2019avg}^W\right) - \left(e_t^M - e_{2019avg}^M\right) \qquad (1)$$

where $e_{2019avg}^W$ and $e_{2019avg}^M$ are the average employment rates in 2019 based on quarterly data for women and men respectively and e_t^W and e_t^M are the employment rates by gender by quarter in 2020. The second is based on the difference in the percent changes of women's and men's employment rates:

$$\Delta e_{t,2019avg}^{DiffRatio,W-M} = \left(\frac{e_t^W}{e_{2019avg}^W}\right) - \left(\frac{e_t^M}{e_{2019avg}^M}\right) \qquad (2)$$

where all the components are defined as above. The measure in equation (1) is the absolute difference in employment rate changes or absolute gender gap change, while that in equation (2) captures the relative difference in employment rate changes or relative gender gap change, since it incorporates possible base effects from women's typically lower employment rates. With either measure, a she-cession is called if the gender gap change ($\Delta e_{t,2019avg}^{Diff,W-M}$) is negative (women's drop in employment is greater than men's).

III. The Extent and Persistence of COVID-19 She-cessions Across Countries

Focusing on the she-cession gender gap change in the trough observed in 2020:Q2, we see a striking degree of heterogeneity across countries with just over half of the sample experiencing she-cessions according to the absolute gender gap change measure (Figure 2). This pattern also appears within the subset of emerging market economies in our sample, with Poland, Hungary, Lithuania, and Colombia experiencing a she-cession while Turkey, South Africa, Russia, and Chile saw men's employment rates hit harder than women's. Moreover, there is significant variation in the severity of she-cessions, with Canada and Iceland having absolute gender gap changes of more than 1 percentage point, followed by Poland, Sweden, Finland, New Zealand, and Lithuania.

About 40 percent of the economies experiencing she-cessions had she-cession gender gap changes of a half percentage point or more (or about a fifth of all economies in the sample). She-cessions are not a universal phenomenon and moreover, their severity differs markedly. There is a high degree of heterogeneity in experiences, across both advanced and emerging market economies.

The broad pattern of she-cessions is also evident if the relative gender gap change measure in equation (2) is used (Figure 3). By this metric, about two-thirds of economies were experiencing she-cessions in 2020:Q2. The higher incidence captured by the difference in percent change of employment rates reflects the base effects of women's typically lower employment rates.

Figure 2. She-cession Absolute Gender Gap Changes Across Economies (2020:Q2)
(Percentage point change)
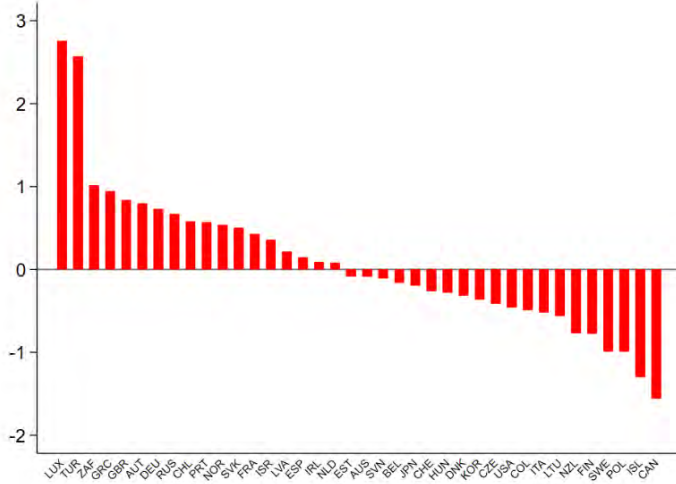
Note: The chart reports the gap in employment rate changes between women and men as defined by Equation (1). A negative value corresponds to a she-cession.

Figure 3. She-cession Relative Gender Gap Changes Across Economies (2020:Q2)
(Percent change)

Note: The chart reports the gap in employment rate changes between women and men as defined by Equation (2). A negative value corresponds to a she-cession.

We now analyze the persistence of the she-cession through 2020. Figure 4 reports the median and the 25th and 75th percentiles of the gender gap change (absolute or relative) up to the third quarter of 2020 (due to data availability). First, we observe that the median country in our sample experienced a she-cession in 2020:Q2, but these subsided for the median country by 2020:Q3 for both measures. Second, as stressed before, there is sizable dispersion across countries.

Figure 4. COVID-19 She-cessions Over Time

Panel (a) - Percentage point change   Panel (b) - Percent change
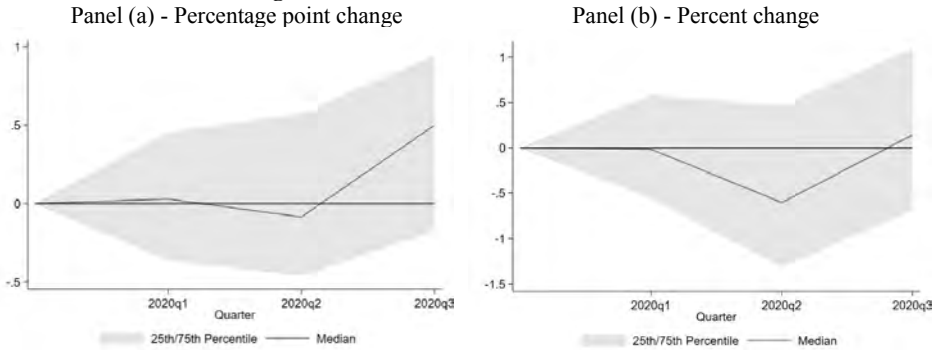
Note: The chart reports the evolution of the gap between women and men employment to population ratio with respect to the 2019 average. The solid line corresponds to the median and the shaded areas to the interquartile ranges. The sample includes 38 advanced and emerging market economies.

Unpacking these patterns, Table 1 shows that 53 percent of countries in our sample experienced a she-cession in 2020:Q2. By 2020:Q3, however, only 32 percent of the sample countries were in a she-cession, where the gender gap change remained negative. A similar pattern is visible when focusing only on emerging market economies, with 50 percent of countries in she-cession in the second quarter and around 38 percent by the third quarter. However, even as the COVID-19 shock's exacerbation of the gender gap abates, the preexisting gender gaps between the levels of men's and women's employment rates may still remain.

This picture constrasts with the one that emerges from the analysis of the global financial crisis. When we do the same exercise using the absolute gender gap change, but comparing 2009:Q2 with the 2007 average, we find that only 8 percent of countries experienced a she-cession. By 2009:Q3 only 5 percent of the countries in our sample were in a she-cession. This evidence is consistent with studies of the US labor market during the global financial crisis which found that men were more heavily impacted by the crisis (Wall 2009 and Hoynes, Miller, and Schaller 2012).

Table 1. Fraction of Economies with She-cessions
(based on absolute gap change)

Quarter	All	AEs	EMs
2020:Q2	0.53	0.53	0.50
2020:Q3	0.32	0.30	0.38

Note: The table reports the fraction of countries with negative absolute gender gap changes (Equation (1) she-cessions) by quarter for the full sample.

The relative gender gap change metric shows a similar pattern over time, with 68 percent of countries in our sample in 2020:Q2 experiencing a she-cession (Table 2). This declines to about 45 percent by

2020:Q3. The reduction in she-cession incidence over time also holds across economy groups, but with a larger drop showing up in advanced economies.

Table 2. Fraction of Economies with She-cessions
(based on relative gap change)

| Quarter	All	AEs	EMs
2020:Q2	0.68	0.63	0.88
2020:Q3	0.45	0.37	0.75

Note: The table reports the fraction of countries with negative relative gender gap changes (Equation (2) she-cessions) by quarter for the full sample.

IV. THE ROLE OF SECTORAL WORKFORCE COMPOSITION IN THE COVID-19 PANDEMIC'S ASYMMETRIC EMPLOYMENT IMPACTS BY GENDER

Two factors could be driving the uneven impact of the current crisis on women: (i) women tend to have higher preexisting employment shares in sectors particularly hit by the COVID-19 crisis, and (ii) within a given sector women tend to experience worse labor market outcomes. To better understand the role of the gender distribution of employment across sectors, we investigate sectoral employment composition by gender for the set of countries in our sample for which we have quarterly employment data broken down by gender and sector (Table A1). The sample includes 20 European countries.

Ordered by the pre-crisis sectoral share of women's employment, we observe that some of the sectors experiencing the largest drops in employment accounted for substantial shares of women's employment. More specifically, the ratio of employment to population in sectors with a larger share of women's employment fell by about 0.11 percentage points on average from their average level in 2019 to 2020:Q2, while sectors with a higher share of men's employment fell slightly less, by an average of 0.07 percentage points as of 2020:Q2. For instance, the accommodation and food sector, which accounts for 6.5 percent of women's employment, saw its employment-to-population ratio decline by a 0.62 percentage points on average during the pandemic. The wholesale and retail sector, accounting for 13.7 percent of women's employment, saw its ratio drop by 0.42 percentage points by 2020:Q2. By contrast, the construction sector's employment-to-population ratio, where 11.1 percent of employed men have jobs, only fell by 0.17 percentage points on average. These dynamics suggest that sectors employing more women typically experienced worse outcomes compared with sectors accounting for larger shares of men's employment (Figure 5).

Figure 5: Sectoral Employment Changes as of 2020:Q2 and Pre-Crisis Sectoral Shares of Women's Employment
(Percentage points, left and percent, right)
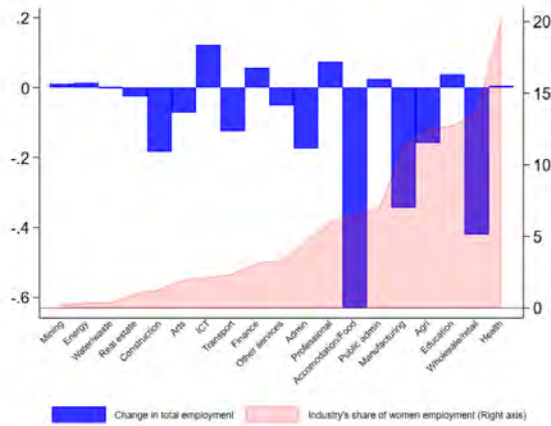
Note: The chart reports the change in employment-to-population ratios between 2020:Q2 and 2019 and the share of women's employment in each NACE Rev. 4 sector as of 2019. The reported employment changes and sectoral shares are weighted averages of underlying country-by-country changes and sectoral shares with total employment used as weights.

Figure 6 shows that women in some sectors have been harder hit than men. An extreme example is the health sector, where employment for men increased by 0.14 percentage points, while women's employment fell by 0.14 percent. Conversely, women's employment increased within construction and public administration, while it fell for men. Overall, in 9 sectors women's employment fell by more than men's, while the opposite is true in 10 sectors.

Figure 6: Sectoral Employment Changes By Gender (2020:Q2 versus 2019)
(Percentage points)

Panel (a) – By Gender

Panel (b) – absolute difference (Women *less* Men)
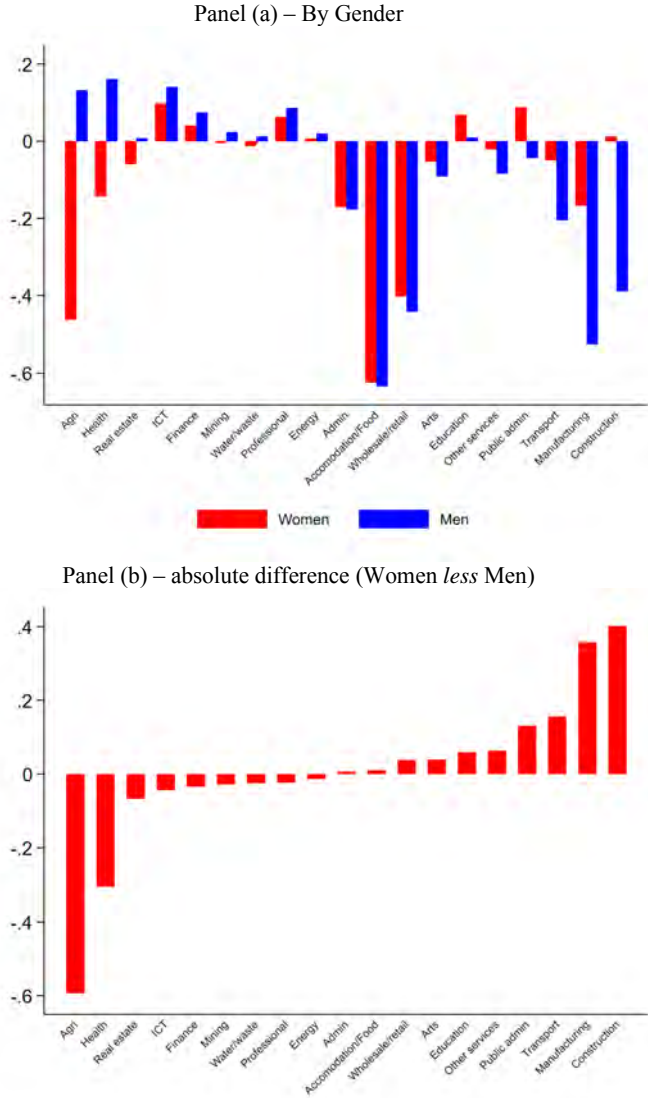
Note: The chart reports the employment change in 2020:Q2 with respect to the average in 2019 by NACE Rev. 4 sector. Panel (A) shows the percent changes in employment by gender. Panel (B) shows the average gender gap changes. The reported employment changes are weighted averages of underlying country-by-country changes with total employment used as weights.

To quantify the roles of changing gender shares and sectoral employment changes, we focus on the absolute gender gap change during the pandemic and decompose it into two components:

$$(e_{t+1}^w - e_t^w) - (e_{t+1}^m - e_t^m)$$

$$= \left[\left(e_{t+1}^w - \sum_s \omega_{s,t} \left(\frac{E_{s,t+1}}{P_{t+1}^w} \right) \right) - \left(e_t^w - \sum_s \omega_{s,t} \left(\frac{E_{s,t+1}}{P_{t+1}^w} \right) \right) \right]$$

$$- \left[\left(e_{t+1}^m - \sum_s (1 - \omega_{s,t}) \left(\frac{E_{s,t+1}}{P_{t+1}^m} \right) \right) - \left(e_t^m - \sum_s (1 - \omega_{s,t}) \left(\frac{E_{s,t+1}}{P_{t+1}^m} \right) \right) \right]$$

$$= \underbrace{\left[\left(\sum_s \omega_{s,t} \left(\frac{E_{s,t+1}}{P_{t+1}^w} - \frac{E_{s,t}}{P_t^w} \right) \right) - \left(\sum_s (1 - \omega_{s,t}) \left(\frac{E_{s,t+1}}{P_{t+1}^m} - \frac{E_{s,t}}{P_t^m} \right) \right) \right]}_{\text{change in sectoral employment}}$$

$$+ \underbrace{\left[\left(\sum_s (\omega_{s,t+1} - \omega_{s,t}) \left(\frac{E_{s,t+1}}{P_{t+1}^w} \right) \right) - \left(\sum_s (\omega_{s,t} - \omega_{s,t+1}) \left(\frac{E_{s,t+1}}{P_{t+1}^m} \right) \right) \right]}_{\text{change in gender share}} \quad (2)$$

where e_t^w and e_t^m denote women's and men's employment rates at time t, $\omega_{s,t}$ denotes women's share of employment in sector s at time t, $E_{s,t+1}$ is total employment in sector s at time $t+1$, and P_{t+1}^w and P_{t+1}^m are the working-age populations of women and men, respectively, at time $t+1$. For the COVID-19 crisis, we take t to indicate the average value of the variable over 2019 and $t+1$ to indicate the value of the variable in 2020:Q2, giving us the difference in the percentage point change of employment rates by gender over the first half of 2020. Recall that the employment rate for a given population $x \in \{w, m\}$ is defined as $e_t^x = \frac{E_{t+1}^x}{P_{t+1}^x}$ and note that $E_{t+1}^w = \sum_s \omega_{s,t+1} E_{s,t+1}$ and $E_{t+1}^m = \sum_s (1 - \omega_{s,t+1}) E_{s,t+1}$.

The first squared bracket on the righthand side then represents the employment effect stemming from *changes in sectoral employment* holding fixed women's employment shares by sector. This captures the consequences of women's higher preexisting employment shares in sectors particularly hit by the COVID-19 crisis. The second squared bracket on the righthand side represents the effect of *changes in gender shares* of employment within sectors. This captures the extent to which women's employment is hit harder than men's within sectors.

Figure 7 shows this decomposition for each country in our sample. The grey dot corresponds to the gender gap change, the blue bar to the component due to the change in gender share, and the red one to the component due to the change in sectoral employment. A negative value in this chart indicates that the relevant factor contributed to a more negative impact for women than for men, while a positive factor means that the relevant factor contributed to a more negative impact for men than women. Consistent with the previous analysis, about half of the countries (9 out of 20 in the sample) appear to be in she-cession—that is, the change in the overall gender gap in employment rates is negative. In 8 countries out of these 9 countries, changes in gender shares of sectoral employment are the driving factor of the she-cession. Annex Figure A.1 shows the same decomposition for the period

2019 (average) to 2020:Q3 (rather than 2020:Q2). For this period 7 out of 18 countries[4] appears to be in she-cession, and in 5 out of these 7 countries changes in gender shares of sectoral employment are the driving factor of the she-cession. Similar patterns are also visible with a sectoral decomposition for the relative gender gap change (Annex Figure A.2) for the restricted number of countries (Table A1). For these 12 countries appear to be in she-cession, and in 10 out of these 12 countries changes in gender shares of sectoral employment are the driving factor of the she-cession.

Figure 7: Decomposition of the COVID-19 Crisis's Absolute Gender Gap Change, 2019-2020:Q2
(Percentage points)
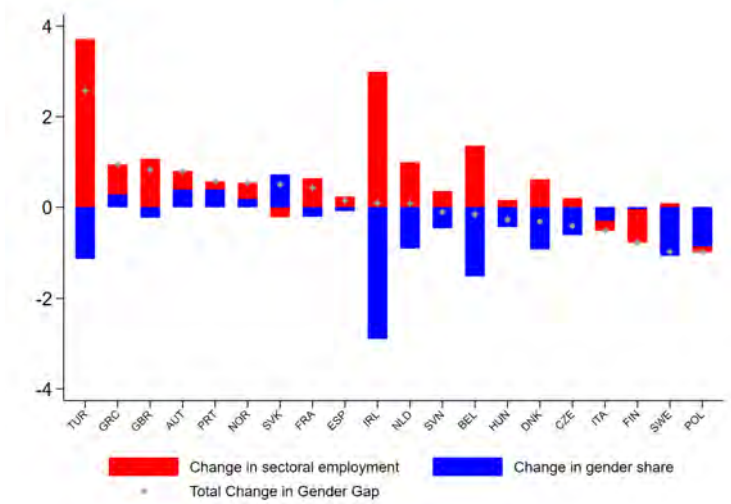
Note: The chart reports the two components which sum to the average gender gap change by country as defined by Equation (2). Based on restricted sample (Table A.1).

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[4] For the decomposition of the gap from 2019 (average) to 2020:Q3 only 18 countries are included due to data availability for 2020:Q3.

V. MARGINS OF LABOR MARKET CHANGES: HOURS WORKED, UNEMPLOYMENT, AND LABOR FORCE PARTICIPATION

As mentioned in the introduction, the employment rate might miss some important aspects of labor markets' weakness, as it does not include workers that are still employed but experience cuts in working hours. To address this concern, we examine average hours worked, or the intensive margin of employment. Another important aspect is whether workers that are laid-off or quit their jobs look for a new job (unemployment), or they exit the labor force. As noted earlier, the literature has stressed that women have a higher average likelihood of exiting the labor force than men. If the COVID-19 recession has pushed women out of the labor force more than men, then focusing solely on the unemployment rate could understate the impacts on women, and as a result, the extent of she-cessions across countries.[5]

Contrary to our prior, men experienced a larger reduction in average hours worked than women in most countries, leading the median gender gap change for average hours worked relative to 2019 to rise (Figure 8, panel A). This average behavior also appears to be relatively common in the sample, with the 25th percentile of the indicator well above zero. The implication is that the she-cessions are largely phenomena related to the extensive margin of employment.

As remarked earlier, the change in the employment rate is equal to the sum of changes in labor force participation rate minus changes in the unemployment rate (with some weights). When looking at these two indicators separately, it becomes clear that the she-cessions—as measured using the absolute gender gap change—tend to be more related to the relative declines in women's labor force participation than relative rises in their unemployment rates (Figure 8, panels b and c). This becomes clear when the absolute gender gap change in employment rates is decomposed into these two margins—relative drops in women's labor force participation rate are more numerous and their contribution to negative gender gap changes in employment tend to be larger in magnitude than that from unemployment rises (Figure 9).

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[5] Note that the analysis of hours worked uses the same sample of 20 countries as our decomposition exercise in Section 4 due to data availability.

Figure 8: Absolute Gender Gap Changes in Labor Market Margins
(Percentage points)

Panel (a) – Average hours worked

Panel (b) – Unemployment rate

Panel (c) – Labor force participation rate
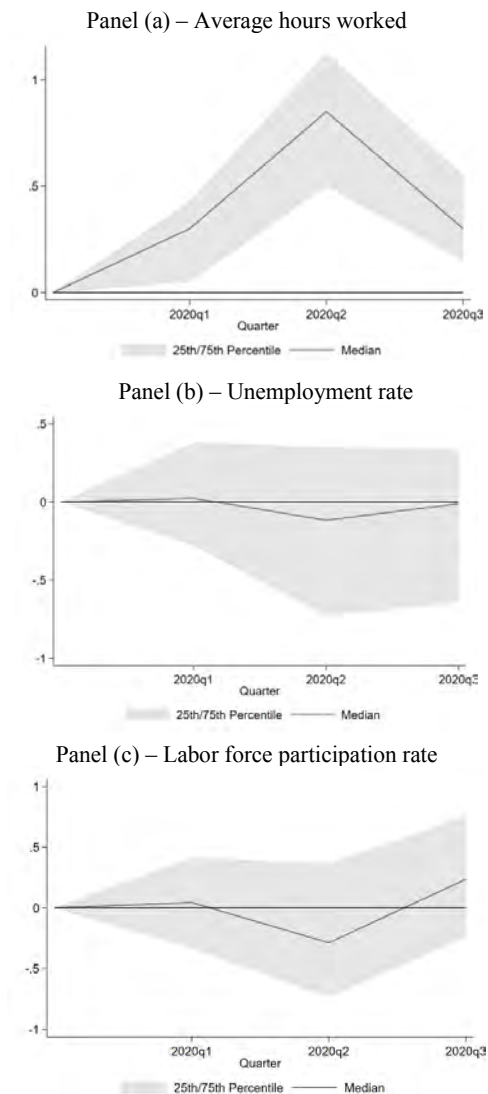
Note: The chart reports the evolution of the gap between women and men for three labor market outcomes with respect to the 2019 average. The solid line corresponds to the median and the shaded areas to the interquartile ranges. The sample includes 20 EU countries in panel (A) and the full sample of 38 advanced and emerging countries in panels (B) and (C).

Figure 9: She-cession Absolute Gender Gap Change by Unemployment and Labor Force Participation Contributions
(Percentage points)
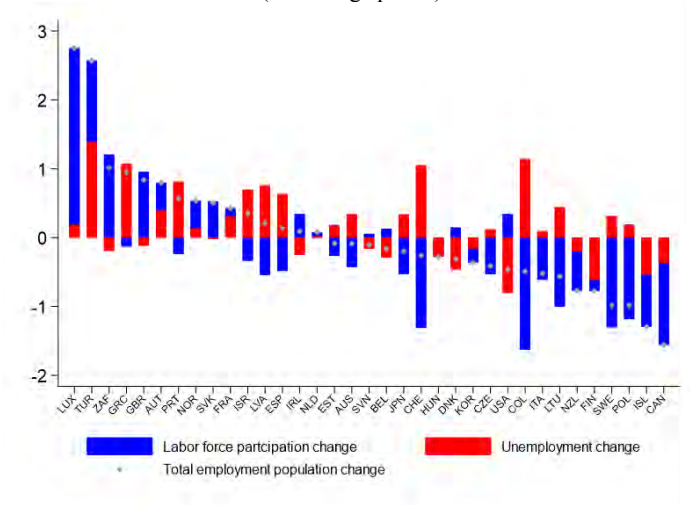
Note: The total absolute gender gap changes shown here correspond to Figure 2.

Examining other labor market indicators based on the relative gender gap change suggests similar patterns to the baseline seen with the absolute gender gap change—relative labor force participation fell on average in 2020:2, while the relative unemployment rate for women actually rose somewhat (Figure 10). Both patterns reverse by 2020:Q3 on average, Average hours worked also displays a similar pattern to the baseline, consistent with the extensive margin being most relevant for COVID-19 she-cessions.

Figure 10. Relative Gender Gap Changes in Labor Market Margins
(Percent change)

Panel (a) – average hours worked

Panel (b) - unemployment rate

Panel (c) – labor force participation rate
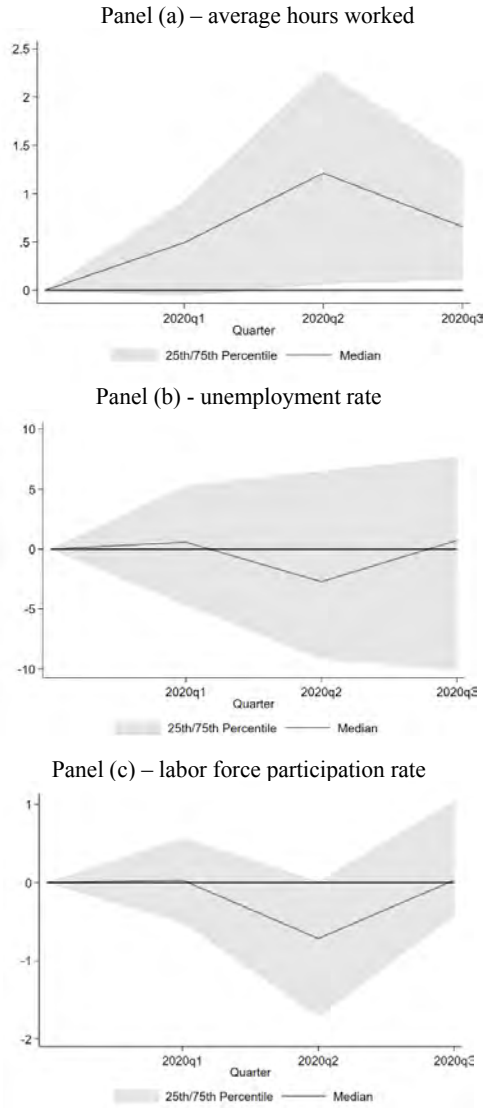
Note: The chart reports the evolution of the gap between women and men for four labor market outcomes with respect to the 2019 average. The solid line corresponds to the median and the shaded areas to the interquartile ranges. The sample includes 38 advanced and emerging countries in panels (b) and (c) whereas it includes 19 European union countries in panel (a).

VI. CONCLUSION

Unusual features of the COVID-19 pandemic recession—widespread lockdowns of the economy, school closures, and large hits to contact-intensive sectors—led to concerns of highly disproportionate, adverse impacts on women workers. Examining a large panel of 38 advanced economies and emerging markets through 2020, we find a high degree of heterogeneity across countries in the incidence and severity of she-cessions, where women's employment falls more than men's, with more than half to two-thirds of the countries experiencing a she-cession in 2020:Q2 (the trough of the pandemic recession). Moreover, she-cessions tend to be short-lived, with between three-fifths to two-thirds of countries out of she-cession by 2020:Q3 (that is, preexisting gender gaps are not further worsening).

For countries where sectoral employment by gender is observed, she-cessions were predominantly driven by declining women's shares in employment within sectors. This is more important for sectors which account for more of women's employment.

COVID-19 she-cessions are also more clearly phenomena related to the extensive margin (employment), as the gender gap change in average hours worked actually rose on average (that is, for women who keep their jobs, their hours decline tended to be less than that of men). Moreover, much of the relative employment decline reflects women's greater propensity to exit the labor force than men, rather than a shift into outright unemployment. The relative drop in women's labor force participation could partly reflect the greater impact of the crisis on mothers, particularly those who are lower-income and lower-skilled, as childcare burdens increased with the crisis (Fabrizio, Gomes, and Tavares 2021).

Some of the cross-country differences in the gender gap changes with the COVID-19 recession may reflect deeper structural factors at play, including the gender composition of sectoral employment, the availability of affordable childcare alternatives, and employment regulations with differential impacts by gender. To reduce these gaps, policymakers could aim to ensure that affordable and reliable childcare options are available (whether privately or publicly provided), that family leave is available for equitable use by men and women (recognizing evolving gender roles), and that there is flexibility in work hours as job requirements allow.

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Table A1. List of countries by section

	Country	iso3code	AEs	EMs	Full Sample	Restricted Sample
1	Australia	AUS	1	0	1	
2	Austria	AUT	1	0	1	1
3	Belgium	BEL	1	0	1	1
4	Canada	CAN	1	0	1	
5	Chile	CHL	0	1	1	
6	Colombia	COL	0	1	1	
7	Czech Republic	CZE	1	0	1	1
8	Denmark	DNK	1	0	1	1
9	Estonia	EST	1	0	1	
10	Finland	FIN	1	0	1	1
11	France	FRA	1	0	1	1
12	Germany	DEU	1	0	1	
13	Greece	GRC	1	0	1	1
14	Hungary	HUN	0	1	1	1
15	Iceland	ISL	1	0	1	
16	Ireland	IRL	1	0	1	1
17	Israel	ISR	1	0	1	
18	Italy	ITA	1	0	1	1
19	Japan	JPN	1	0	1	
20	Korea	KOR	1	0	1	
21	Latvia	LVA	1	0	1	
22	Lithuania	LTU	0	1	1	
23	Luxembourg	LUX	1	0	1	
24	Netherlands	NLD	1	0	1	1
25	New Zealand	NZL	1	0	1	
26	Norway	NOR	1	0	1	1
27	Poland	POL	0	1	1	1
28	Portugal	PRT	1	0	1	1
29	Russia	RUS	0	1	1	
30	Slovak Republic	SVK	1	0	1	1
31	Slovenia	SVN	1	0	1	1
32	South Africa	ZAF	0	1	1	
33	Spain	ESP	1	0	1	1
34	Sweden	SWE	1	0	1	1
35	Switzerland	CHE	1	0	1	
36	Turkey	TUR	0	1	1	1
37	United Kingdom	GBR	1	0	1	1
38	United States	USA	1	0	1	
					38	20

ANNEX: ROBUSTNESS ON THE ROLE OF SECTORAL WORKFORCE COMPOSITION

In this annex we conduct two robustness checks of the decomposition exercise conducted in Section IV of the main paper. First, we decompose the total change in the gender gap from 2019 (average) to 2020:Q3 rather than from 2019 (average) to 2020:Q2 as done in Section IV of the main text. Second, we decompose the change in the total gender gap from 2019 (average) to using 2020:Q2 using the relative gender gap change.

Figure A1 shows the decomposition of the absolute change in the gender gap from 2019 to 2020:Q3 for each country in our sample. The grey dot corresponds to the gender gap change, the blue bar to the component due to the change in gender share, and the red one to the component due to the change in sectoral employment. A negative value in this chart indicates that the relevant factor contributed to a more negative impact for women than for men, while a positive factor means that the relevant factor contributed to a more negative impact for men than women. For this period 7 out of 18 countries (instead of 9 out of 20 counties) appear to be in she-cession—that is, the change in the overall gender gap in employment rates is negative.[6] In 5 countries out of these 7 countries, changes in gender shares of sectoral employment are the driving factor of the she-cession.

Figure A1: Sectoral Decomposition of the Absolute Gender Gap Change, 2019-2020:Q3
(percentage points)
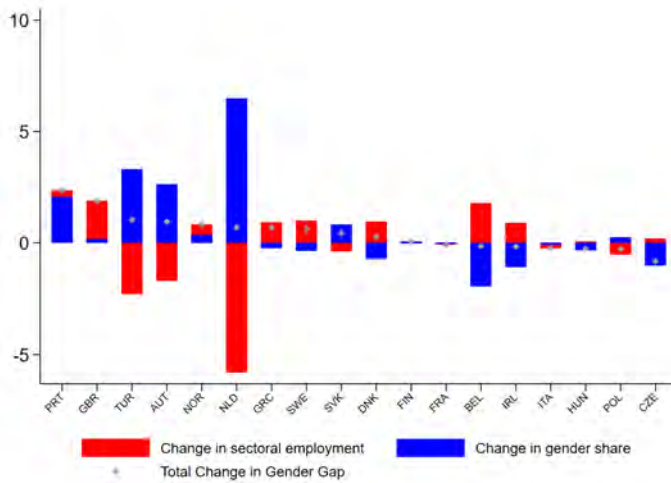
Note: The chart reports the two components which sum to the average gender gap change by country as defined by Equation (2). Based on restricted sample (Table A.1).

Figure A2 shows the decomposition of the relative change in the gender gap from 2019 to 2020:Q2 using the relative gender gap change from equation (2). This alternative gender gap is decomposed into changes stemming from (i) the change in sectoral employment, and (ii) the change in gender share using the equation in (A.1).

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[6] For Figure A2 only 18 countries are included due to data availability for 2020:Q3.

$$(e_{t+1}^w - e_t^w)/e_t^w - (e_{t+1}^m - e_t^m)/e_t^m$$

$$= \left[\left(e_{t+1}^w - \sum_s \omega_{s,t} \left(\frac{E_{s,t+1}}{P_{t+1}^w} \right) \right) - \left(e_t^w - \sum_s \omega_{s,t} \left(\frac{E_{s,t+1}}{P_{t+1}^w} \right) \right) \right] / e_t^w$$

$$- \left[\left(e_{t+1}^m - \sum_s (1 - \omega_{s,t}) \left(\frac{E_{s,t+1}}{P_{t+1}^m} \right) \right) - \left(e_t^m - \sum_s (1 - \omega_{s,t}) \left(\frac{E_{s,t+1}}{P_{t+1}^m} \right) \right) \right] / e_t^m$$

$$= \underbrace{\left[\frac{\left(\sum_s \omega_{s,t} \left(\frac{E_{s,t+1}}{P_{t+1}^w} - \frac{E_{s,t}}{P_t^w} \right) \right)}{e_t^w} - \frac{\left(\sum_s (1 - \omega_{s,t}) \left(\frac{E_{s,t+1}}{P_{t+1}^m} - \frac{E_{s,t}}{P_t^m} \right) \right)}{e_t^m} \right]}_{\text{change in sectoral employment}}$$

$$+ \underbrace{\left[\frac{\left(\sum_s (\omega_{s,t+1} - \omega_{s,t}) \left(\frac{E_{s,t+1}}{P_{t+1}^w} \right) \right)}{e_t^w} - \frac{\left(\sum_s (\omega_{s,t} - \omega_{s,t+1}) \left(\frac{E_{s,t+1}}{P_{t+1}^m} \right) \right)}{e_t^m} \right]}_{\text{change in gender share}} \quad (A.1)$$

where variables are notated similarly to in Section IV of the main text. According to Figure A.2, 12 out of 20 countries appear to be in she-cession. In 10 countries out of these 12 countries, changes in gender shares of sectoral employment are the driving factor of the she-cession.

Figure A2: Sectoral Decomposition of the Relative Gender Gap Change, 2019-2020:Q2
(percentage points)
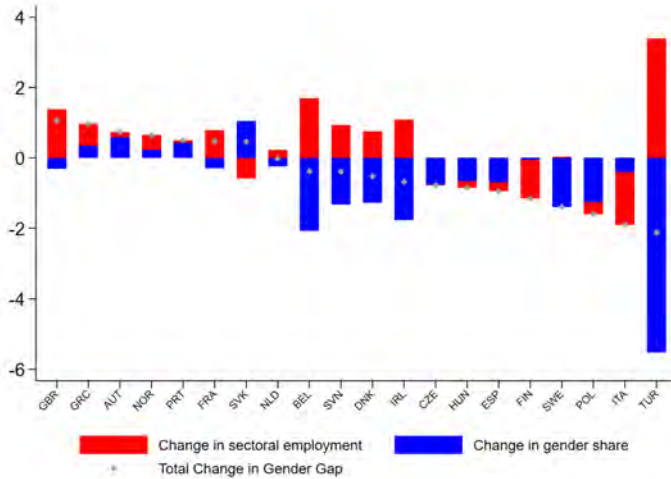
Note: The chart reports the two components which sum to the average gender gap change by country as defined by Equation (2). Based on restricted sample (Table A.1).

COVID-19 and stay-at-home orders: Identifying event study designs with imperfect testing[1]

Jaedo Choi,[2] Elird Haxhiu,[3] Thomas Helgerman,[4] Nishaad Rao[5] and Taeuk Seo[6]

This paper estimates the dynamic effect of Stay-At-Home (SAH) orders on the transmission of COVID-19 in the United States. Identification in this setting is challenging due to differences between real and reported case data given the imperfect testing environment, as well as the clearly non-random adoption of treatment. We extend a Susceptible-Infected-Recovered (SIR) model from Epidemiology to account for endogenous testing at the county level, and exploit this additional structure to recover identification. With the inclusion of model-derived sufficient statistics and fixed effects, SAH orders have a large and sustained negative effect on the growth of cases under plausible assumptions about the progression of testing. Point estimates range from a 44% to 54% reduction in the growth rate of cases one month after a SAH order. We conclude with a discussion on extending the methodology to later phases of the pandemic.

1 Introduction

It is difficult to exaggerate the severity of the COVID-19 pandemic in the United States. After "flattening the curve" three separate times since April, new daily cases are once again on the rise (see Figure 1). Even as we approach a spring season with cautious optimism and a set of vaccines on hand, it is important to understand how effective existing policy tools are at stopping the virus given uncertainty surrounding both vaccine take-up and protection offered by the shot against newer variants of COVID-19.[1] One policy used widely during the initial propagation of the disease was the Stay-At-Home (SAH) order, where states require citizens to remain at home unless they need to leave for an essential activity.[2] While the aggregate time-series evidence suggests that these policies were effective at arresting the growth rate of new cases, credible causal estimation of treatment effects on disease spread remains challenging due to a number of identification challenges.

The most difficult of these is related to non-classical measurement error: we do not expect *actual* COVID-19 cases to match those *reported* by local health authorities. We could use standard econometric methods if this measurement error is classical, but as infections are almost surely systematically under-reported, we cannot reasonably maintain that this error is mean zero. Overcoming this issue therefore requires a theoretical framework relating observed positive cases to actual infections.[3] We augment a standard Susceptible-Infected-Recovered (SIR) model from Epidemiology with endogenous testing and derive assumptions about variation in testing capacity over time and across space that must be made in order to conduct valid inference. The SIR model governs the evolution of cases and disciplines all of our estimating equations, while the theory on testing reveals how different fixed effects amount to substantive assumptions about testing capacity.

The second challenge to credibly identifying the effect of SAH orders on disease spread is the non-random adoption of treatment. All else equal, states with higher rates of spread are more likely to implement the policy on a given day. Simple comparisons then suggest that SAH orders *increase*

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[1]This is especially true given the slow roll-out of the vaccine in countries other than the United States.

[2]Starting on March 19th with California and concluding with South Carolina on April 7th, 42 states enacted a SAH order during the sample period ending on April 30th. We maintain an absorbing treatment assumption throughout, and show robustness to dropping Montana and Colorado which removed their respective SAH orders a number of days before the last day in our sample.

[3]Yang et al. (2020) estimates the active prevalence of COVID-19 based on an SIR model that allows for the fact that testing is skewed towards symptomatic individuals. They find that accounting for this reveals that the prevalence of COVID-19 could be up to three times higher in the United States, highlighting the importance of considering testing and how it relates to caseload data. However, the paper focusses on the extent of spread, not the effect that a policy such as stay-at-home orders would have on this prevalence.

the spread of COVID-19, an implausible conclusion given our knowledge of disease transmission. The recursive structure of the SIR model implies a sufficient statistic that captures this underlying heterogeneity: the lag of cumulative cases is sufficient to determine the current evolution of daily cases, in the absence of policy. A parallel trends assumption on the evolution of daily cases across different cohorts of SAH adopters then identifies potential dynamic effect of SAH orders. Pre-trends tests in our model-derived estimating equations can then be used to assess the validity of this parallel trends assumption in the *post*-period, which we find strong support for. [4]

Figure 1: New Cases of COVID-19 Reported to the CDC
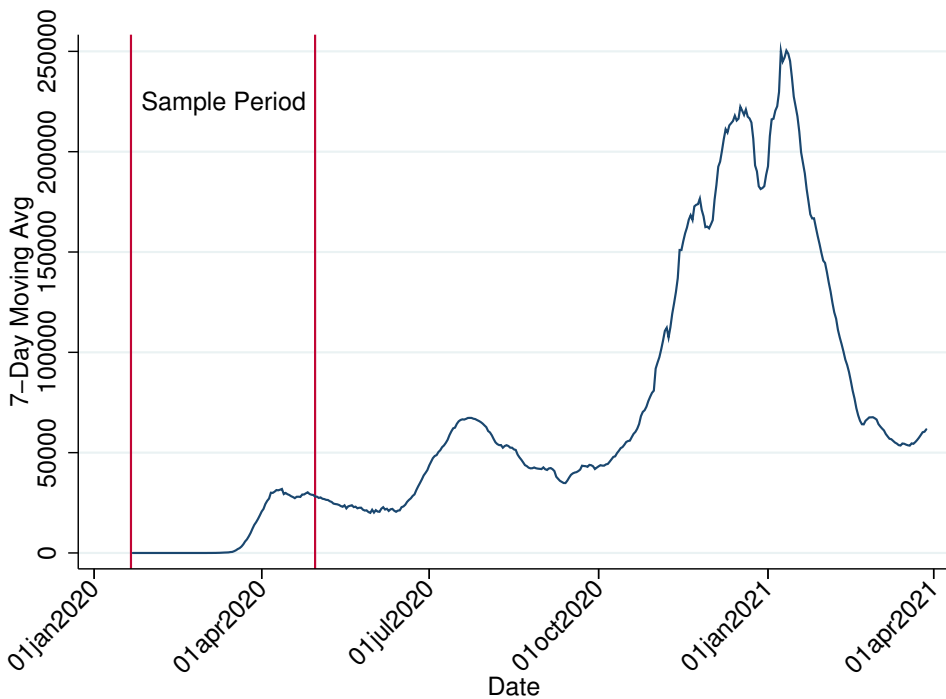
This figure plots the 7-Day Moving Average of daily new cases of COVID-19 reported in the U.S. Data was

collected from the CDC on 3/30/2021 at

`https://www.cdc.gov/coronavirus/2019-ncov/cases-updates/cases-in-us.html`

Despite these unique challenges to identification, there has been an explosion of economic re-

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[4]This is subject to the additional assumption that dynamic treatment effects associated with a SAH do not vary *too much* across different cohorts adopting treatment (Sun and Abraham (2020)). We believe this is reasonable given the short time period between the first and last adopter (20 days), but estimates assessing robustness are forthcoming.

search focusing on the effectiveness of policy responses: as of the time of writing, more than 380 pandemic related working papers have been uploaded to NBER; and *COVID Economics* has published 468 papers on the economics of COVID-19, with at least 20 focusing specifically on the effects of lock-down measures. Early work based on US data includes Lyu and Wehby (2020), Dave et al. (2020) and Fowler et al. (2020), which all focus on the treatment effect of SAH orders, but each use specifications with differing outcome variables and fixed effects. Each find significant effects with lock-downs, but notable pre-trends suggest the parallel trends identifying assumption is suspect, at least as specified.[5] Similar specifications have been used to compare policies across countries using cross-country regression analysis (Alfano and Ercolano (2020), Bonardi et al. (2020)). Research since then has expanded to look at the effects of different policies; for example, Isphording et al. (2020) study the impact of public health informed school re-openings in Germany on the spread of COVID-19. Further, Schlosser et al. (2020) focus on how geographic mobility interacts with SAH orders to reduce spread.[6]

Related work to ours includes Allcott et al. (2020), which derives event study specifications from the SIR model in a similar fashion. However, we explicitly deal with the problem that observed data is the endogenous outcome of testing and show how different sets of fixed effects are structurally related to different assumptions on testing capacity. Some practitioners focus on simulating the SIR model to derive estimates of effect size (as in Giordano et al. (2020)), while we estimate it using a specification from the model and minimal data requirements. We derive the *additional* assumptions on the evolution of testing capacity over time and across space needed to properly interpret fixed effects estimators of this effect.

Finally, Chernozhukov et al. (2020) construct an SIR model that is similar in spirit to ours but written in continuous time. They find an expression for the growth rate of confirmed cases, which they approximate using a discrete time difference equation. They proceed by assuming that the growth of testing capacity[7] is a linear function of the growth of the number of tests administered. Their equation (10) restricts the growth rate in the number of confirmed cases to be positively

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[5]Their estimates naturally vary in size and interpretation: for instance, Fowler et al. (2020) find that the infections declined by 30% in the first week after the lock-down, while Lyu and Wehby (2020) estimate that there is a difference of 0.51 per 10,000 resident in cases after imposing lock-down in a state compared to its neighbor.

[6]They find that counties become more disconnected once they impose lock-downs, and there is a significant reduction in mobility, which also leads to a decline in disease spread.

[7]Defined here as the percentage of true cases reported by local public health officials.

related to the corresponding growth rate in tests administered in a linear fashion. We pursue an alternative approach in our main specifications by making assumptions on the *progression* of testing capacity itself; this amounts to restricting the variation in confirmed cases we permit to identify the effect of SAH on disease transmission. As such, these approaches are complementary ways of solving the challenge of making inference about real COVID-19 cases from reported ones.

We proceed as follows. Section 2 documents our data sources and implements the standard two-way fixed effects (TWFE) estimator, while section 3 derives alternative estimating equations based on the SIR model augmented with endogenous testing. This section also presents and discusses our main finding, that SAH orders adopted early in the pandemic had a strong and significant effect on curbing the spread of disease, as well as the identifying assumptions on testing and parallel trends needed to believe it. We conclude in section 4 with some extensions and robustness checks, as well as a discussion of how to interpret and adapt our framework during later phases of the pandemic.

2   Standard Event Study

At its core, this paper is concerned with the research question: "What is the effect of Stay At Home Orders on the spread of the COVID-19?" On face, this is a policy evaluation that seems to be easily answerable with standard econometric tools. This section attempts to evaluate the effect of SAH orders using a TWFE event study approach and illustrates its pitfalls in this context.

2.1   Data

To measure the spread of SARS-CoV-2[8], we collect county level data on both the number of positive cases of and deaths attributable to COVID-19 compiled by *The New York Times*. The data begins on January 21st, 2020, with the first reported case in the United States, and includes the cumulative number of cases and deaths for each county on each day through April 30th, at the time of writing. To compile this dataset, *The New York Times* collected historical information from local and state governments and health departments; as a result, these data are subject to the same limitations as this source material. First, there are cases in which state reports do not report cases separately by county, or the county of residence of an individual is simply reported as "unknown." Second,

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[8]Throughout, we interchangeably use the terms: COVID-19, SARS-CoV-2, the coronavirus, and the pandemic.

and more importantly, these data should be interpreted as the incidence of COVID-19 conditional on the level of testing at the county level. Preliminary estimates suggest that a large fraction of positive cases remain undetected (Jagodnik et al. (2020)), which we tackle explicitly in Section 4.

We collect the date each state implemented a SAH order from the *New York Times*.[9] In our data set, we count 42 states as implementing SAH orders (as well as Washington, D.C.); the eight that do not are Arkansas, Iowa, Nebraska, North Dakota, Oklahoma, South Dakota, Utah and Wyoming. Importantly, we do not count Oklahoma's "Safer-At-Home" order as an SAH order, as it only applied to the older and more vulnerable to serious infection. In addition, while several localities within Oklahoma, Utah and Wyoming issued their own SAH orders, we do not consider them as treated, as these policies were not implemented state-wide.

2.2 Empirical Specification

We observe C counties for T days in our data set. We define the Stay-At-Home event for county c, E_c, as the day on when the state in which that county resides imposed a stay at home order. Then, we can utilize the TWFE event study specification:

$$Y_{c,t} = \sum_{l=-7, l\neq-1}^{l=28} \mu_l \cdot \mathbb{1}\{t - E_c = l\} + \mu_{29+} \cdot \mathbb{1}\{t - E_c \geq 29\} + \alpha_c + \gamma_t + \varepsilon_{c,t} \qquad (1)$$

$Y_{c,t}$ is an unspecified outcome variable, as it is *ex-ante* unclear what transformation of positive cases is appropriate to use. In each county c, we count the total number of people who tested positive *by* day t, $T_{c,t}$. Then, to measure the daily number of new cases, we take first differences $\triangle T_{c,t} := T_{c,t} - T_{c,t-1}$. We set $Y_{c,t} = \ln\{\triangle T_{c,t} + 1\}$, noting that $\triangle T_{c,t}$ grows in an exponential manner during the beginning of a pandemic.

We use an event window of 1 week before the SAH order through 4 weeks after, omitting the day before the event. In addition, we include a term that bins together all days after 4 weeks to capture "long run" effects of treatment, though in practice for most states the data do not extend far beyond our event window. Finally, we include county and time fixed-effects to account for fixed differences across counties in spread and national trends in cases, respectively.

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[9]Data collected at `https://www.nytimes.com/interactive/2020/us/coronavirus-stay-at-home-order.html`
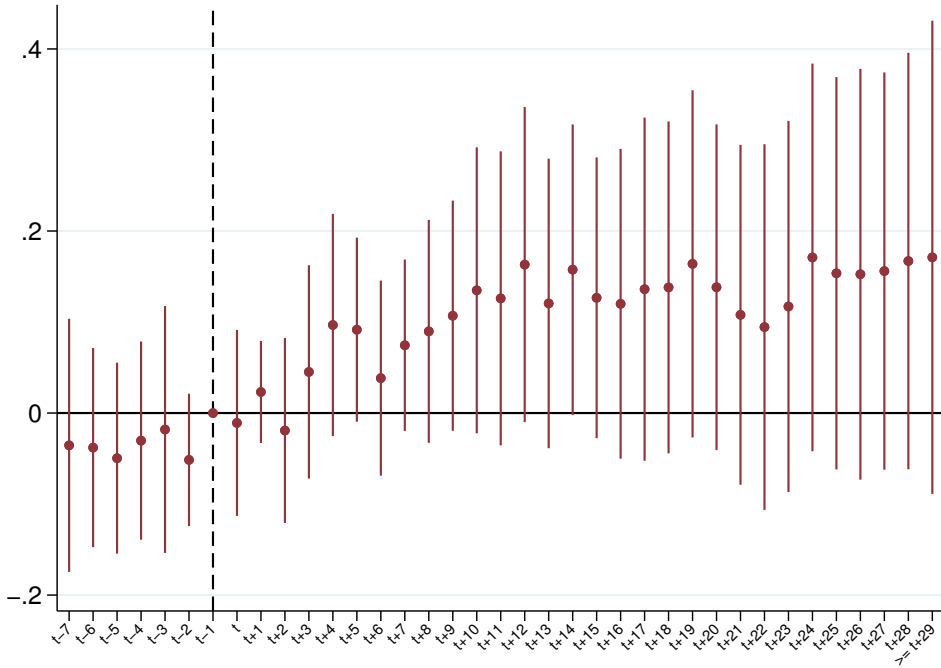
Figure 2: Standard TWFE Event Study Results

Figure plots point estimates and 95 percentile confidence interval of Equation (1). The dependent variable is the daily number of new infections. The regression model includes time and county fixed effects. Standard errors are clustered at the state level.

2.3 Results & Discussion

Figure 2 reports the results from estimating equation (1). The point estimates are noisy, especially around a month following the SAH order, but it is clear that this model estimates a weakly positive impact of SAH orders on positive cases.

These results strike us as suspect for two reasons. First, it seems extremely unlikely *ex-ante* that SAH orders increase the spread of SARS-CoV-2. A null estimate seems perfectly plausible, as it is unsettlingly easy to imagine Americans roundly ignoring this public health directive, reducing SAH orders to a placebo treatment in effect. A positive coefficient estimate seems more consistent with the interpretation that treatment is not randomly assigned conditional on the covariates included; rather, it is likely that the imposition of an SAH order is correlated with the growth rate of positive

cases in a state. This interpretation is bolstered by the second problem with these results: there is a clear pre-trend in the event coefficients preceding the SAH orders. This suggests that counties receiving earlier treatment are experiencing faster case growth than those receiving later treatment (or no treatment at all), which casts serious doubt on interpreting these results as causal.

In light of these issues, we focus on correctly modeling the expected spread of SARS-CoV-2 in the absence of treatment. Adding structure to our estimation strategy reduces dependence on the faulty assumption of random timing of SAH orders. To achieve this, we augment an epidemiological model with endogenous testing to inform which variables explain the unmitigated spread of the virus and how these variables enter the conditional expectation. Then, under the assumption that the restrictions imposed by our model are correct, we are able to recover causal identification.

3 Causal Evidence

3.1 SIR Model

To understand how we expect COVID-19 to spread, we utilize a simple discrete time SIR model.[10] An SIR model divides the population in each county c at time t into three groups: Susceptible ($S_{c,t}$), Infected ($I_{c,t}$) and Recovered ($R_{c,t}$). It then describes how the stock of these groups will change over time as a function of two key parameters. The model is described completely by the following set of difference equations:

$$\text{Susceptible} \quad : \quad S_{c,t} = S_{c,t-1} - \beta_{c,t} \cdot I_{c,t-1} \frac{S_{c,t-1}}{N_c} \tag{2}$$

$$\text{Infected} \quad : \quad I_{c,t} = (1 - \gamma) \cdot I_{c,t-1} + \beta_{c,t} \cdot I_{c,t-1} \frac{S_{c,t-1}}{N_c} \tag{3}$$

$$\text{Recovered} \quad : \quad R_{c,t} = R_{c,t-1} + \gamma \cdot I_{c,t-1} \tag{4}$$

The dynamics of the model are determined by the parameters $(\beta_{c,t}, \gamma)$. $\beta_{c,t}$ describes the rate of infection in county c at time t under the assumption of random mixing: in period t, every infected person from last period $I_{c,t-1}$ spreads the disease to $\beta_{c,t}$ people at random, of which the portion $\frac{S_{c,t-1}}{N_c}$ are susceptible, leading to $m_{c,t} := \beta_{c,t} I_{c,t-1} \frac{S_{c,t-1}}{N_c}$ new infections in period t. At the same time, some proportion γ of $I_{c,t-1}$ recover, while $(1 - \gamma)I_{c,t-1}$ individuals stay infected in period t.

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[10]Atkeson (2020) provides an excellent introduction to a continuous time SIR model for economists.

We can now consider how to identify policy effects separately from the county-specific growth rate $\beta_{c,t}$. We do this by taking logs and linearizing the model. We show that without making further restrictions on $\beta_{c,t}$ we cannot identify policy effects; in light of this, we outline two identification strategies under different restrictions on the progression of $\beta_{c,t}$.

3.2 Identifying Policy Effects

To begin, suppose that all of the model variables are observable. In this case, we can follow the derivations in Gupta et al. (2020). Early in an epidemic, it is reasonable to assume that almost all of the population is susceptible to the disease, i.e. $S_{c,t}/N_c \approx 1$. Taking the log of new cases yields

$$\ln(m_{c,t}) = \ln(\beta_{c,t}) + \ln(I_{c,t-1}) \tag{5}$$

Note that we can do this in a general sense only because of the recursive nature of the problem, i.e. because this relationship holds for all t within the framework of our model. New cases today are *only* a function of the growth rate of cases in a county, $\beta_{c,t}$, and the number of infected people in that county yesterday, $I_{c,t-1}$. The former is a structural parameter, and the latter is a variable that summarizes the state of the world going into the current period. This is the sense in which we consider infections yesterday a "sufficient statistic" to determine cases today.[11]

Of course, we do not believe that this equation holds exactly in practice; rather, we assume it is modeling the conditional mean of $\ln(m_{c,t})$ in the absence of policy. Formally, $\mathbb{E}[\ln(m_{c,t})|I_{c,t-1}] = \ln(\beta_{c,t}) + \ln(I_{c,t-1})$, and we define $\varepsilon_{c,t} := \ln(m_{c,t}) - \mathbb{E}[\ln(m_{c,t})|I_{c,t-1}]$, allowing us to rewrite 5 as

$$\ln(m_{c,t}) = \ln(\beta_{c,t}) + \ln(I_{c,t-1}) + \varepsilon_{c,t}$$

Now, let $\mu_{s(c),t}$ denote the impact of some state-level SAH order on the spread of the virus. Specifically, we model this treatment as lowering the growth rate of the virus by a fixed proportion in every county c in state s at time t. Then, the effective growth rate in county c at time t will be

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[11] A parallel can be drawn here with the motivation for using lagged variables as IVs in the macroeconomics literature. Current variables are only affected by structural parameters through state variables, making the exclusion restriction clear, at least in theory. State variables are also uncorrelated with unobservables that might affect the control variables by design, satisfying exogeneity. Likewise, the control variable in our case, i.e. the number of cases today, is determined completely by the number of infected cases yesterday and a structural parameter.

$\beta_{c,t}\mu_{s(c),t}$, and the progression of daily cases will be given by

$$\ln(m_{c,t}) = \ln(\beta_{c,t}) + \ln(\mu_{s(c),t}) + \ln(I_{c,t-1}) + \varepsilon_{c,t}$$

Proposition: State-level policy effects are not identified in the fully general SIR model.

Proof: Let $\{\beta_{c,t}, \mu_{s(c),t}, \gamma, F_{c,t}^e\}$ describe the progression of cases, where $\varepsilon_{c,t} \sim F_{c,t}^e$. Define $\beta'_{c,t}$ such that $\ln(\beta'_{c,t}) = \ln(\beta_{c,t}) + \ln(\mu_{s(c),t})$ and let $\mu'_{s(c),t} = 1$. Then, $\{\beta'_{c,t}, \mu'_{s(c),t}, \gamma, \varepsilon_{c,t}\}$ leads to the same distribution of observed daily cases.

In light of proposition 1, we know that we need to impose structure on the structural parameters $\beta_{c,t}$ in order to identify policy effects. We first consider the simplest assumption - that this growth rate does not vary over time:

$$\beta_{c,t} = \beta_c \qquad \text{(Assumption 1B)}$$

Under Assumption 1B, we can identify the treatment effect of a SAH order using a panel event study approach with only county fixed effects $\alpha_c = \ln(\beta_c)$:[12]

$$\ln(m_{c,t}) = \sum_{l=-7,l\neq-1}^{l=28} \mu_l \cdot \mathbb{1}\{t - E_c = l\} + \alpha_c + \ln(I_{c,t-1}) + \varepsilon_{c,t}$$

Unfortunately, this estimation is infeasible, as we are unable to observe neither $m_{c,t}$ nor $I_{c,t-1}$. We do not observe $m_{c,t}$ as we are limited in each period by existing testing capacity and infrastructure. We miss $I_{c,t-1}$ for two reasons. It is clear that total infections yesterday is a function of all previous new daily cases; that is, $I_{c,t-1} = f(m_{c,t-1}, ..., m_{c,t_0}; \beta, \gamma)$. As a result, any issues with testing in the past will impact our estimate of the stock of infections today. In addition, this variable is not equivalent to the stock of positive cases yesterday, even if testing is complete, as it does not include people who have recovered and can no longer pass the disease along to others. Therefore, to the extent that recovery data is incomplete, we will have issues measuring the number of individuals who are currently infected.

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[12]This section will omit the binned terms from our specifications for brevity.

3.3 Testing

In light of this issue, we account for testing within our model.[13] Specifically, we add the variable $T_{c,t}$, the total number number of individuals who have tested positive in county c at time period t, to the model. Then, by making assumptions on the relationship between the underlying mechanisms of the SIR model and what we observe, we can recover the event study coefficients. We assume that, in each period, we are able to estimate some fraction $\tau_{c,t}$ of daily new cases in each county ($m_{c,t}$). In the general form of this model, $\tau_{c,t}$ can vary both across counties and over time. Using this new parameter, we can write $T_{c,t}$ using the recursion $T_{c,t} = T_{c,t-1} + \tau_{c,t} m_{c,t}$, which is equivalent to $\triangle T_{c,t} = \tau_{c,t} m_{c,t}$. The left hand side of this equation is now the number of daily new cases, which we observe. Taking logs of this relationship and utilizing (5) gives us that

$$\ln(\triangle T_{c,t}) = \ln(\tau_{c,t}) + \ln(\beta_c) + \ln(I_{c,t-1}) \tag{6}$$

We now need only find an expression for $\ln(I_{c,t-1})$ in terms of variables we observe. Adding together (3) and (4) and utilizing the definition of $m_{c,t}$ gives us that

$$I_{c,t} + R_{c,t} = I_{c,t-1} + R_{c,t-1} + m_{c,t} = \sum_{i=t_0}^{t} m_{c,i}$$

A similar recursion using positive tests allows us to write

$$T_{c,t} = T_{c,t-1} + \tau_{c,t} m_{c,t} = \sum_{i=t_0}^{t} \tau_{c,i} m_{c,i}$$

Unless we are willing to make the assumption that testing capacity is not evolving over time, we cannot solve for the general relationship between $T_{c,t}$ and $I_{c,t} + R_{c,t}$. Nevertheless, it is clear from these derivations that we can write down the reduced form relationship between these variables as $T_{c,t} = \tau_{c,t}^{RF} \cdot (I_{c,t} + R_{c,t})$, where $\tau_{c,t}^{RF}(\tau_{c,t}, ..., \tau_{c,t_0}; \beta_c, \gamma)$ is some unknown function of the evolution of testing capacity in county c. Taking logs of this relationship and rearranging gives us that

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[13]Berger et al. (2020) extends the basic SEIR model to include testing as well. They are able to show that a mixture of higher levels of testing and targetted quarantining can both reduce transmission and dampen the impact of the virus on the economy. However, theirs is a model that calibrates certain facts about COVID-19 (such as the infection rate or the quarantine rate) and therefore does not have en empirical strategy per se – which makes sense because they are attempting to recommend policy. However, we take the SEIR model as a starting point to inform our empirical analysis to assess the effectiveness of the stay-at-home orders.

$\ln(I_{c,t} + R_{c,t}) = \ln(T_{c,t}) - \ln(\tau_{c,t}^{RF})$. We can utilize this expression to rewrite (6) as

$$\ln(\triangle T_{c,t}) = \ln(\tau_{c,t}) + \ln(\beta_c) + \ln(I_{c,t-1})$$

$$= \ln(\tau_{c,t}) + \ln(\beta_c) + \ln(I_{c,t-1}) - \ln(I_{c,t-1} + R_{c,t-1}) + \ln(I_{c,t-1} + R_{c,t-1})$$

$$= \ln(\tau_{c,t}) - \ln(\tau_{c,t-1}^{RF}) + \ln(\beta_c) + \ln(T_{c,t-1}) + \{\ln(I_{c,t-1}) - \ln(I_{c,t-1} + R_{c,t-1})\}$$

Early in a pandemic, we would expect $\ln(I_{c,t-1}) - \ln(I_{c,t-1} + R_{c,t-1}) = \ln(\frac{I_{c,t-1}}{I_{c,t-1}+R_{c,t-1}})$ to be small. The size of this fraction depends on how fast the disease is spreading relative to the rate of recovery at the beginning of the pandemic - this is referred to as the basic reproduction rate $R_0 = \beta_c/\gamma$.[14] Intuitively, as the disease spreads initially with a high R_0, the stock of infected individuals grows much faster than the stock of recovered individuals. Further, since $\frac{\partial}{\partial x \partial y} \ln(\frac{x}{x+y}) < 0$, even as the stock of recovered individuals inevitably rises, this difference remains small. Utilizing the approximation $\ln(I_{c,t-1}) - \ln(I_{c,t-1} + R_{c,t-1}) \approx 0$, we can write:

$$\ln(\triangle T_{c,t}) = \ln(\tau_{c,t}/\tau_{c,t-1}^{RF}) + \ln(\beta_c) + \ln(T_{c,t-1}) \tag{7}$$

In principle, this motivates the event study design

$$\ln(m_{c,t}) = \sum_{l=-7, l \neq -1}^{l=28} \mu_l \cdot \mathbb{1}\{t - E_c = l\} + \ln(\tau_{c,t}/\tau_{c,t-1}^{RF}) + \alpha_c + \ln(T_{c,t-1}) + \varepsilon_{c,t}$$

Since this empirical design is informed directly by an SIR model, we can interpret μ_l in the same way as before. Notice that $\alpha_c = \ln(\beta_c)$ and μ_l both enter additively into the log of daily cases; as a result, properly transformed, μ_l gives the average change in β_c on day l after the implementation of a SAH order. Formally, $\ln(\beta_c) + \mu_l = \ln(\beta_c e^{\mu_l})$, so that $100 \cdot (1 - e^{\mu_l})$ gives the average percentage change in the growth rate across counties induced by SAH orders.

3.3.1 Relationship to Literature

Before continuing, it is worthwhile to briefly discuss the relationship between this model of imperfect testing and what has been developed in the empirical literature. As noted earlier, Chernozhukov

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[14]A medical metastudy (Liu et al. (2020)) finds the number to be within 1.4 to 6.49 with a mean of 3.28.

et al. (2020) derive a similar model: their equation (8) describing the evolution of confirmed cases is the continuous time analog of our equation $\triangle T_{c,t} = \tau_{c,t} m_{c,t}$. We derive their estimating equation within our discrete time framework to illustrate how our focus differs from theirs.

Taking the first difference of the natural log of our identity $\triangle T_{c,t} = \tau_{c,t} m_{c,t}$ for confirmed cases decomposes the growth rate of this variable into two sources, growth in testing capacity and spread of disease: $\triangle \ln(\triangle T_{c,t}) = \triangle \ln(\tau_{c,t}) + \triangle \ln(m_{c,t})$. Taking the first difference of equation 6 (and adding that $\beta_{c,t} = \beta_c$) reveals that $\triangle \ln(m_{c,t}) = \triangle \ln(I_{c,t-1})$, allowing us to write $\triangle \ln(\triangle T_{c,t}) = \triangle \ln(\tau_{c,t}) + \triangle \ln(I_{c,t-1})$. Maintaining our earlier assumption that $S_{c,t}/N_c \approx 1$, we can rearrange equation 3 from our SIR model to describe the progression of infections as $I_{c,t} = (1 + \beta_c - \gamma)I_{c,t-1}$. Taking logs and rearranging slightly yields $\triangle \ln(I_{c,t}) = \ln(1 + \beta_c - \gamma)$, which we can substitute into our expression for the growth rate of confirmed cases:

$$\triangle \ln(\triangle T_{c,t}) = \triangle \ln(\tau_{c,t}) + \ln(1 + \beta_c - \gamma) \approx \triangle \ln(\tau_{c,t}) + (\beta_c - \gamma)$$

This is the discrete time version of equation (10) in Chernozhukov et al. (2020), which provides a theoretical basis for their estimating equation. They proceed by assuming that the growth of testing capacity $\triangle \ln(\tau_{c,t})$ is a linear function of the growth of the number of tests administered, which is observable, and focus on modeling how $\beta_c - \gamma$ evolves in response to changes in behavior and information over time. In contrast, we assume that $\beta_c - \gamma$ is stationary and focus on making inferences when testing, $\triangle \ln(\tau_{c,t})$, evolves in a complicated manner over time. Equation 7 is better suited for this approach, so we return to our model of how daily positive cases evolve over time.

3.4 Identification

Unfortunately, at this point, we are still unable to identify μ_l. This stems from the fact that $\tau_{c,t}$ varies at the county-day level, so we cannot differentiate a rise in cases over time due to spread of the disease from changes in testing. We view this as the fundamental identification challenge stemming from endogenous testing. At this point we need to impose more structure in order to recover a useful estimating equation. We consider several different assumptions that allow us to identify μ_l, and proceed from the most to least restrictive.

3.4.1   Time-Invariant Testing

First, we could impose that

$$\tau_{c,t} = \tau_c \qquad \text{(Assumption 1T)}$$

is constant over time.[15] As a result, $T_{c,t} = \tau_c \sum_{i=t_0}^{t} m_{c,i} = \tau_c(I_{c,t} + R_{c,t})$, which gives us the intuitive result that $\tau_c = \tau_c^{RF}$, since we are simply reporting some fixed fraction of cases in every period. Then, 7 will simplify to $\ln(\triangle T_{c,t}) = \ln(\beta_c) + \ln(T_{c,t-1})$, which is the observed analog of (5). This result is important, as it establishes a sufficient condition under which we can estimate β_c as if we are observing the true number of cases.

Under (Assumption 1B) & (Assumption 1T), we can utilize an event study approach to identify the impact of a stay at home order. Our SIR model implies the following specification:

$$\ln(\triangle T_{c,t}) = \sum_{l=-7, l\neq -1}^{l=28} \mu_l \cdot \mathbb{1}\{t - E_c = l\} + \delta_{\text{lag}} \cdot \ln(T_{c,t-1}) + \alpha_c + \text{DOTW}_t + \varepsilon_{c,t} \qquad (8)$$

We use county level fixed effects to difference out the county specific growth rate, and we include cumulative cases yesterday to control for the expected number of cases today. Intuitively, this functions as a sufficient statistic (along with α_c) for what we expect daily new cases to be in the absence of a public health intervention. Lastly, we include 7 day of the week fixed effects DOTW_t to account for systematic differences in case reporting throughout the week.[16]

Figure 3(a) plots the results of this specification. The contrast with Figure 2 is immediately clear - we no longer have a pre-trend in daily new cases, and the effect of a SAH order is negative and significant. One month after the SAH order is implemented, our event coefficient $\mu_{28} = -.661$, implying a 48% average reduction in the spread of COVID-19. As noted previously, Liu et al. (2020) find that the average estimated R_0 was 3.28; our estimates imply that the reproductive rate at time $t + 28$ is $R_{t+28} = 1.70$, implying a substantial reduction but not elimination of spread.[17]

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[15]It is important to note that this assumption does *not* impose that testing capacity remains constant over time. Rather, we assume that we are identifying a fixed fraction of infected individuals, which implies that testing capacity is expanding at the same rate as the virus is spreading. As a result, this assumption is not inconsistent with the commonsense observation that more tests are being performed over time, though it might still not be appropriate.

[16]Including day-of-the-week fixed effects doesn't change our results much, but we notice that it shrinks the standard error bands around our estimates. This is to be expected given that we are essentially controlling for the variation that is happening because of the cyclicality in the reporting – it is well documented that there is a big drop in cases reported on weekends compared to the middle of the week, for instance.

[17]R_t would need to drop below 1 for spread to eventually stop, as this reflects a scenario in which each infected person is spreading the disease to less than 1 other person.

Figure 3: Causal Event Study Results
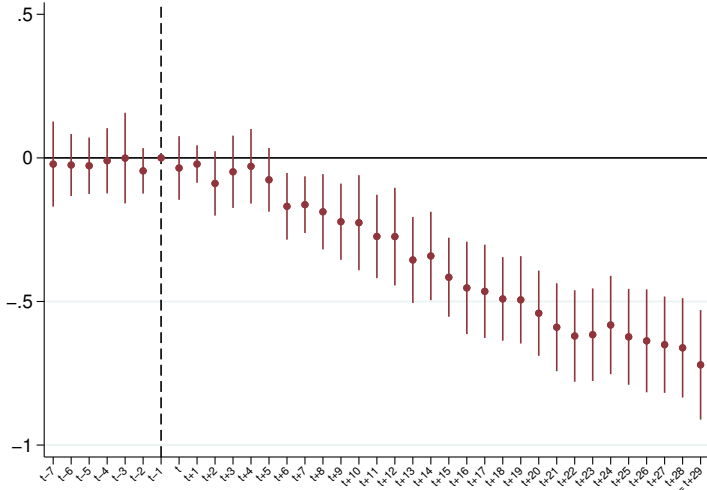
(a) Assumption 1B & Assumption 1T
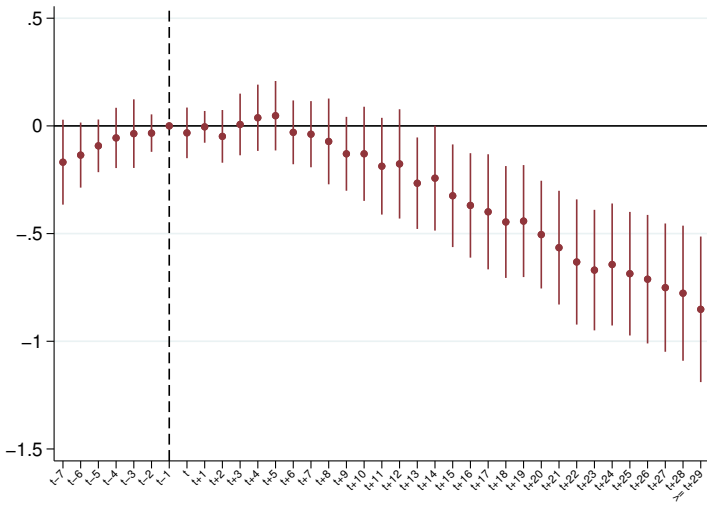
(b) Assumption 2B & Assumption 1T

Panel (a) plots the estimates and 95 percent confidence intervals for Equation (8). In all models, the dependent variable is the daily number of new infections, and county fixed effects and lagged stock of positive cases are controlled for. Panel (b) plots the estimates and 95 percent confidence intervals for Equation (10). Standard errors are clustered at the state level.

We view this simple model as having two major lessons. First, $\ln(T_{c,t-1})$ should be included as a control to correctly adjust for what cases are expected to be this period (as well as unit fixed effects). Second, if the determinants of case growth are specified properly, there is no need to include daily fixed effects, which are standard in the TWFE design. In practice, we find that including these fixed effects only serves to increase noise in the model, as a lot of important variation is soaked up, without a clear theoretical justification. The assumptions needed for this model to be correct are restrictive, so we now turn to estimation when testing can change differentially over time.

3.4.2 Identification Issues: Changing Testing Over Time

To begin, we could impose the similar but less restrictive assumption that $\tau_{c,t}$ varies over time but does not change *too* quickly. To illustrate, let $w(t)$ denote the week that day t is a part of and assume that $\tau_{c,t} = \tau_{c,w(t)}$ is determined entirely by $w(t)$ rather than t[18]. On face, this would seem to have solved the identification challenge, as we can now contribute intraweek variation in cases to spread, not changes in testing. However, even when the structural testing parameter does not vary at the daily level, the reduced form parameter will.

To see this, let $m_{c,w}$ denote the total number of new cases in week w,[19] and let $I_{c,w}$ and $R_{c,w}$ denote the total stock of infected and recovered individuals at the end of week w, respectively. Notice that we can rewrite $T_{c,t}$ as the weighted sum of weekly averages:

$$T_{c,t} = \sum_{w=0}^{w(t)} \tau_{c,w} m_{c,w} = \tau_{c,w(t)} m_{c,w(t)} + \sum_{w=0}^{w(t)-1} \tau_{c,w} m_{c,w}$$

$$= \tau_{c,w(t)}\{(I_{c,t} + R_{c,t}) - (I_{c,w(t)-1} + R_{c,w(t)-1})\} + \tau_{c,w(t)-1}^{INT}(I_{c,w(t)-1} + R_{c,w(t)-1})$$

Where the last equality utilizes the identity $m_{c,w(t)} = (I_{c,t} + R_{c,t}) - (I_{c,w(t)-1} + R_{c,w(t)-1})$ and implicitly defines $\tau_{c,w}^{INT} = T_{c,w}/(I_{c,w} + R_{c,w})$, which are fixed for all t' after week w. A bit of rearranging shows us that

$$T_{c,t} = \tau_{c,w(t)}(I_{c,t} + R_{c,t}) + \{\tau_{c,w(t)-1}^{INT} - \tau_{c,w(t)}\}(I_{c,w(t)-1} + R_{c,w(t)-1}) \tag{9}$$

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[18]This analysis remains unchanged for any different level of aggregation.

[19]Strictly speaking, $m_{c,w}$ is a function of t, as it gives the number of cases in week $w(t)$ on day t. We write $m_{c,w}(t) = m_{c,w}$ in the derivations in this section.

What this tells us is that testing today has an *affine* relationship with the stock of infected and recovered individuals, with coefficients that vary at the weekly level. As a result, since $\tau_{c,t}^{RF}$ imposes a *linear* relationship between these variables, it must vary at the daily level.

Unfortunately, we cannot escape this problem by assuming that testing does not vary too much across space, our other dimension of variation. Suppose testing capacity does not vary across state so $\tau_{c,t} = \tau_{s,w(t)}$. We can still write $T_{c,t}$ as the weighted sum of weekly averages, but since each county has a different path of daily new cases, the intercept still varies at the county-week level:

$$
\begin{aligned}
T_{c,t} &= \tau_{s,w(t)} m_{c,w(t)} + \sum_{w=0}^{w(t)-1} \tau_{s,w} m_{c,w} \\
&= \tau_{s,w(t)} \{ (I_{c,t} + R_{c,t}) - (I_{c,w(t)-1} + R_{c,w(t)-1}) \} + \tau_{c,w(t)-1}^{INT} (I_{c,w(t)-1} + R_{c,w(t)-1})
\end{aligned}
$$

This illustrates exactly how assumptions on testing impact the theoretical relationship between total population of ever-infected individuals $(I_{c,t} + R_{c,t})$ and the number of these individuals that we have identified $(T_{c,t})$. Any level of testing aggregation across time will be reflected in the intercept and slope terms, but any level of testing aggregation across space will only be reflected in the slope term. However, neither assumption or its combination, will give us that the relationship between these variables is linear *and* varies at a level higher than our level of observation.

3.4.3   Modeling the Affine Relationship

In light of this, we deal directly with the affine relationship identified earlier. Since assuming that testing only varies at the state level does not simplify the model, we maintain the previous assumption that testing capacity progresses at the county-week level, i.e. $\tau_{c,t} = \tau_{c,w(t)}$. We can rearrange (9) to obtain the following expression for $I_{c,t} + R_{c,t}$:

$$
\begin{aligned}
\tau_{c,w(t)}(I_{c,t} + R_{c,t}) &= T_{c,t} - \{ \tau_{c,w(t)-1}^{INT} - \tau_{c,w(t)} \}(I_{c,w(t)-1} + R_{c,w(t)-1}) \\
&= T_{c,t} - \frac{\tau_{c,w(t)} - \tau_{c,w(t)-1}^{INT}}{\tau_{c,w(t)-1}^{INT}} T_{c,w(t)-1}
\end{aligned}
$$

To simplify notation, let $\zeta_{c,w(t)} := -\frac{\tau_{c,w(t)} - \tau_{c,w(t)-1}^{INT}}{\tau_{c,w(t)-1}^{INT}}$, and define $T_{c,t}^* := T_{c,t} + \zeta_{c,w(t)} T_{c,w(t)-1}$. Substituting into the expression above, we now have that $\tau_{c,w(t)}(I_{c,t} + R_{c,t}) = T_{c,t}^*$.

Once again recalling (6), we can perform a similar derivation to obtain that

$$\ln(\triangle T_{c,t}) = \ln(\tau_{c,w(t)}) + \ln(\beta_c) + \ln(I_{c,t-1}) \approx \ln(\beta_c) + \ln(T^*_{c,t-1})$$

$$= \ln(\beta_c) + \ln(T_{c,t-1}) + \ln\left(1 + \frac{\zeta_{c,w(t-1)}T_{c,w(t-1)-1}}{T_{c,t-1}}\right)$$

Finally, we linearize the last term around 1 so that $\ln\left(1 + \frac{\zeta_{c,w(t-1)}T_{c,w(t-1)-1}}{T_{c,t-1}}\right) \approx \frac{\zeta_{c,w(t-1)}T_{c,w(t-1)-1}}{T_{c,t-1}}$.
This is appropriate when $\zeta_{c,w(t-1)}T_{c,w(t-1)-1} \ll T_{c,t-1}$, which relies on $\zeta_{c,w(t-1)}$ and $T_{c,w(t-1)-1}/T_{c,t-1}$
being small. Notice that $T_{c,w(t-1)-1}/T_{c,t-1} \leq 1$ by construction, so we need to assume like before
that testing capacity does not change drastically from week to week; formally

$$\tau_{c,t} = \tau_{c,w(t)} \text{ and } \tau_{c,w(t)} - \tau^{INT}_{c,w(t)-1} \approx 0 \qquad \text{(Assumption 2T)}$$

Under Assumption 2T, our model implies that

$$\ln(\triangle T_{c,t}) = \ln(\beta_c) + \ln(T_{c,t}) + \zeta_{c,w(t-1)-1}\frac{T_{c,w(t-1)-1}}{T_{c,t-1}}$$

Accordingly, we estimate policy effects using the event study specification

$$\ln(\triangle T_{c,t}) = \sum_{l=-7,l\neq-1}^{l=28} \mu_l \cdot \mathbb{1}\{t - E_c = l\} + \alpha_c + \delta_{\text{lag}}\ln(T_{c,t-1}) + \alpha_{c,w(t-1)-1}\frac{T_{c,w(t-1)-1}}{T_{c,t-1}} + \text{DOTW}_t + \varepsilon_{c,t}$$

$$(10)$$

We interact county-by-week fixed effects $\alpha_{c,w}$ with $T_{c,w(t-1)-1}/T_{c,t-1}$ to estimate the coefficient on
this term $\zeta_{c,w(t-1)-1}$, which varies at the county-by-week level.

Figure 3(b) plots the result of this specification. The pattern of event coefficients is different
here - there is a clear pre-trend leading up to and continuing past the event date, suggesting that the
growth rate of cases in treated counties is increasing relative to control areas. Nevertheless, despite
this, after a month counties with a SAH order have significantly decreased spread of COVID-19.
Around one month after the SAH order is implemented, our event coefficient $\mu_{28} = -.777$, implying
a 54% average reduction in the growth rate β_c and effective reproductive rate of $R_{t+28} = 1.50$.

3.5 Adding Mobility

3.5.1 Theory

So far, we have maintained Assumption 1B and held that the rate of infection β_c can differ across county but must remain constant over time. Clearly, we need to impose structure on $\beta_{c,t}$, as we could then not distinguish changes in the underlying spread from policy effects by the proposition established earlier. However, we can break $\beta_{c,t}$ into its component parts and introduce data to measure some of these components to estimate a model with weaker assumptions on the components we cannot observe.

To begin, we expand our basline model and now allow $\beta_c = \beta_{c,t}$ to change over time. Recall that β gives the expected number of susceptible individuals one infected person will pass the virus to. Following Arnon et al. (2020), we can decompose this rate into the product of the expected number of susceptible individuals one infected person will come in contact with and the probability that the virus is transmitted, conditional on contact. We define the former as the *contact rate* $\kappa_{c,t}$ and the latter as the *infection rate* $\theta_{c,t}$, giving us that

$$\beta_{c,t} = \kappa_{c,t} \cdot \theta_{c,t} \tag{11}$$

Taking logs of this equation reveals that $\ln(\beta_{c,t}) = \ln(\kappa_{c,t}) + \ln(\theta_{c,t})$, which enter additively into our estimating equation.

We assume that the contact rate can change over time, and we introduce new data in the next subsection to proxy for these changes. To recover identification, we assume that the infection rate is constant across time; that is, $\theta_{c,t} = \theta_c$. This rate is likely a function of two factors: biological factors specific to the disease, and the demographic make-up of the population. The former are constant across all counties,[20], but the latter need to be accounted for. Let $\mathbf{X}_c$ denote all demographic factors that determine the spread of COVID-19. To the extent that these determinants do not change over time, we can write $\theta_c(\mathbf{X}_c)$ as their aggregate impact on the infection rate, which we can account for using fixed effects estimation.

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[20]While there are documented mutations of SARS-CoV-2, it is unlikely that new strains are changing over time, within county, during our sample frame, in a manner that would seriously impact our results.

3.5.2 Data

We use the patterns data from SafeGraph[21] to measure mobility. Using anonymized data from mobile devices, the company has compiled daily visits to about 7 million points of interest (POI) located across the United States. The POI include a wide range of physical locations such as restaurants, retail and grocery stores.

Our mobility measure is very similar to weekly estimates used in Allcott et al. (2020), except that ours is at a daily frequency. On each calendar day, we aggregate visits to each POI using its FIPS code into a county level panel. For simplicity, we do not differentiate across types of establishments and focus on capturing widest range of foot traffic possible. While this increased frequency allows for greater time variation, it also creates strong day-of-the-week seasonality. To address the issue, we use a 7-day moving average of the POI visit counts as our baseline measure.

Our mobility measure closely captures decreased mobility across counties around March. To illustrate, we plot the time series of total POI visits in Los Angeles County of California and Washtenaw County of Michigan on Figures 4a and 4b. On March 13th, the White House declared the pandemic a National Emergency (US President, Proclamation (2020)) - as these figures illustrate, mobility declines markedly upon declaration of national emergency by the federal government ('NE') and continue to drift downwards following the announcement of stay-at-home orders ('SAH') in their respective states.

To match this empirical fact, we model the March 13th emergency declaration as a national-level information event that is responded to differentially by county. Formally, we modify equation (11) such that

$$\ln(\beta_{c,t}) = \ln(\kappa_c) + \mathbb{1}(t \geq \text{March 13}) \ln(\hat{\kappa}_{c,t}) + \ln(\theta_c) \qquad \text{(Assumption 2B)}$$

To be clear, we are assuming that the 7-day average contact rate is constant within county in the period leading up the March 13 emergency declaration. Then, following this date, we assume the contact rate is measured by our POI metric $\hat{\kappa}_{c,t}$, which we are able to include as a control. This

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[21]https://docs.safegraph.com/docs/weekly-patterns

Figure 4: Mobility Patterns in Selected Counties
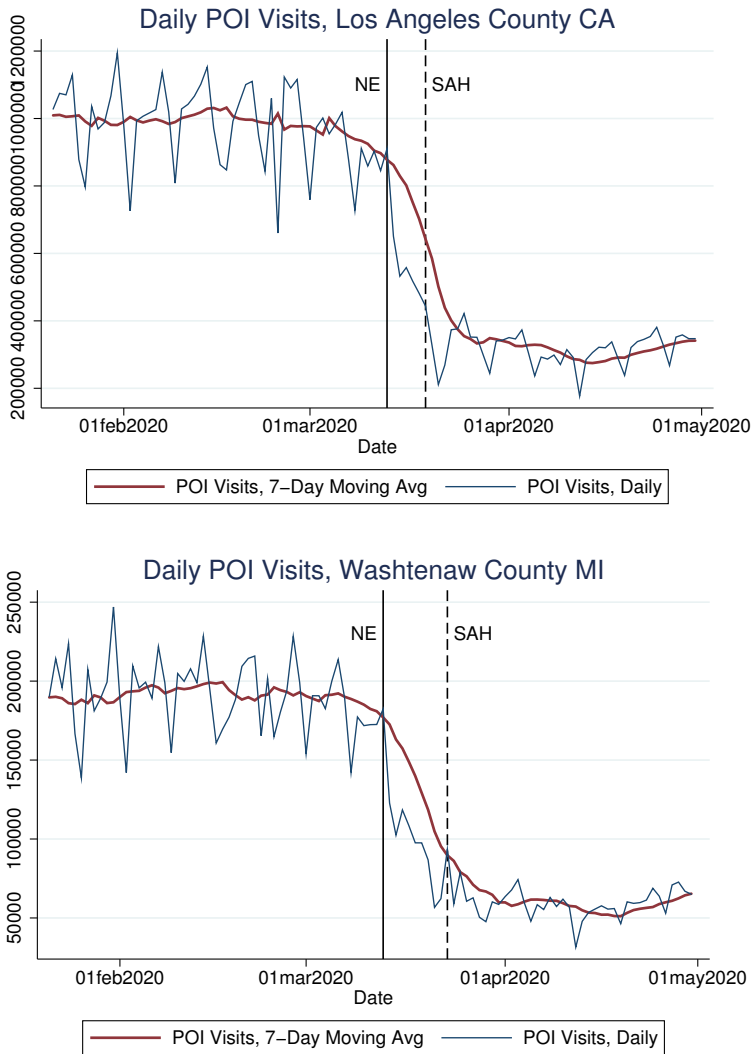
This figure plots the time series of total visits to points of interest (POIs) located in Los Angeles County of California and Washtenaw Coiunty of Michigan according to SafeGraph. 7-day moving averages of daily visits are in solid red lines, and the raw series are plotted in thinner blue lines. Solid vertical line is drawn on March 13 of 2020, when the federal government declared national emergency. Dashed vertical lines are denoted on dates when the governors of Michigan and California issued Stay-at-Home orders.

motivates the event study design:

$$\ln(\triangle T_{c,t}) = \sum_{l=-7, l\neq-1}^{l=28} \mu_l \cdot \mathbb{1}\{t-E_c = l\} + \alpha_c + \delta_{\text{lag}}\ln(T_{c,t-1}) + \delta_{\text{mob}}\mathbb{1}(t \geq \text{March } 13)\ln(\hat{\kappa}_{c,t}) + \text{DOTW}_t + \varepsilon_{c,t}$$

$$(12)$$

Where $\alpha_c = \ln(\beta_c) + \ln(\kappa_c) + \ln(\theta_c)$, maintaining Assumption 1T.

3.5.3 Results

Figure 5(a) plots our results when we control for POI visit mobility and impose Assumption 1T. The results are similar in Figure 5(b), which utilizes controls for both mobility (under Assumption 2B) and testing (under Assumption 2T). These results look very similar qualitatively to those without mobility controls. We still have the same upward pre-trend leading up to lockdown measures in counties under Assumption 2T, but there is a significant negative effect of SAH orders in both specifications. Our main results are therefore robust to accounting for a time varying $\beta_{c,t}$, at least as far as the time variation is captured by changes in mobility, which the literature and theory both suggest it should be. Quantitatively, our $\mu_{28} = -.589$ & $\mu_{28} = -.705$ in each respective specification, which implies a 44-50% reduction in the spread of cases.

4   Conclusion

We build a novel SIR model with endogenous testing to identify the effect of Stay-at-Home orders on COVID-19 spread. One insight is that the number of infections in the previous period (or a proxy for it) is a key control variable that must be included in the event study in order to properly estimate the impact of the policy. This is because the number of infections yesterday summarizes the state of the world going into the current period, and together with some structural parameters determines the spread of the virus today. Another key contribution is to show how different sets of fixed effects amount to different assumptions about the progression of a county's ability to detect the virus over time. Taking into account these two issues, and assuming a conditional parallel trends assumption holds, we find that SAH orders have a strong sustained negative effect on the growth of cases under various assumptions about the progression of testing, with point estimates varying from 44% to 54%.

Figure 5: Causal Event Study Results
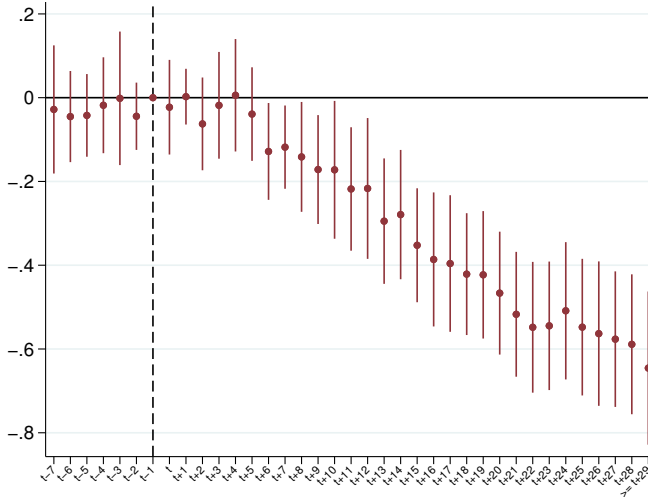
(a) Assumption 1B & Assumption 2T
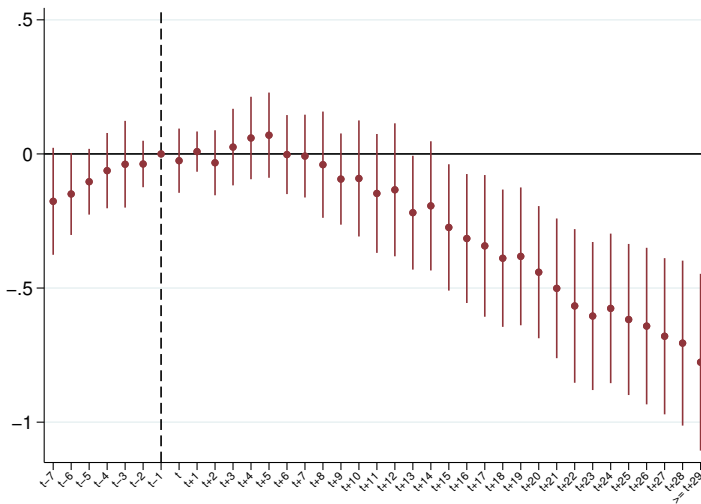
(b) Assumption 2B & Assumption 2T

Panel (a) plots the estimates and 95 percent confidence intervals for Equation (12). In all models, the dependent variable is the daily number of new infections, and county fixed effects and lagged stock of positive cases are controlled for. Panel (b) plots the estimates and 95 percent confidence intervals for Equation (12) with additional testing controls from Equation (10). Standard errors are clustered at the state level.

These estimates imply that SAH orders are a strong policy tool to eliminate the spread of COVID-19. However, it is important to note that initial estimates of R_0 suggest that reductions of $\beta_{c,t}$ of the magnitude we find will not eventually drive daily cases to zero. This suggests that Stay-at-Home Orders are best used in combination with other policy tools, such as mask mandates and contact tracing. Not only are these likely effective approaches in their own right, but they might augment the effects that we estimate as well. There is also a growing literature studying the costs and benefits of lockdown orders (e.g. Kaplan et al. (2020)), and while an economic model is needed to trade these off, we believe that our estimates add to this debate by precisely estimating the *benefits* of a lockdown from a public health perspective.

While our results are encouraging, it is unreasonable to expect that we can capture the full dynamics of an infectious disease with a simple SIR model. There is a nascent but deep literature exploring several important dimensions of the spread of COVID-19. We hope to augment this work by giving researchers the tools to inform their empirical approach using insights from Epidemiology rather than a standard econometric design. In particular, future research could expand on this basic model in two important directions.

First, while we note that $I_{c,t-1}$ (along with the county-specific growth rate) is a sufficient statistic for the future evolution of cases, this is only true in the context of our model. If the state policymaker has information outside of this model that informs their SAH timing decision (for example, that the disease is spreading among undergraduate students in a college town but not circulating outside of this subgroup), this timing might fail to be random conditional on knowing cumulative cases last period. Accordingly, future work ought implement a more complicated multi-group SIR model (see Acemoglu et al. (2020)), as well as a political economy model to explicitly address the non-random adoption of SAH orders.

Finally, it is important to note that the model we construct here is only appropriate at the beginning of a pandemic. As a result, any study interested in the current spread of COVID-19 should reconsider the assumptions made here. We sketch an outline of these assumptions and their plausibility in the present. First, we assume that almost every individual in the population is susceptible to disease, i.e. $S_{c,t}/N_c \approx 1$. This allows us to ignore the progression of the susceptible population in an (S)IR model. However, as this population drops, this assumption begins to

introduce important bias into estimates. If $S_{c,t}/N_c < 1$, then equation 5 becomes

$$\ln(m_{c,t}) = \ln(S_{c,t}/N_c) + \ln(\beta_c) + \ln(I_{c,t-1})$$

Due to the shape of the natural log function, significant drops in $S_{c,t}$ lead to a large negative bias in estimates of event coefficients. Second, we assume that the population of recovered individuals $R_{c,t}$ is small relative to to the population of infected people $I_{c,t}$. This obviously becomes inappropriate later in a pandemic; however, it is in principle possible to adjust for this with data on the number of recovered individuals or additional assumptions about the recovery rate. Third, recent months have seen the spread of newer mutations of COVID-19. If the infection rate $\theta_{c,t}$ differs across variants, additional assumptions are needed to model the change in this rate over time. For example, suppose that variant B overtakes variant A over the course of a month. Even if we assume that the infection rate is constant across the country and over time for each variant so that the series $\{\theta^A, \theta^B\}_{t=1}^{30}$, the true infection rate will be $\alpha_t \theta^A + (1 - \alpha_t)\theta^B$ where α_t measures the prevalence of each variant in the population. This setup would motivate the include of daily fixed effects to control for this changing composition.

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