COVID-19 Induced Shocks and Uncertainty

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COVID-19-INDUCED SHOCKS AND UNCERTAINTY*

Mirela Miescu† Raffaele Rossi‡

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Abstract

Using statistical identification, we extract a COVID-19-induced shock by exploiting large daily jumps in financial markets caused by news about the pandemic. This shock depresses economic and financial indicators, increases risk and uncertainty measures, has sizeable distributional effects, and hits most harshly those industries relying on face-to-face interactions. Impulse response function analysis across various identification strategies leads us to interpret the statistical COVID-19-induced shock as a structural uncertainty shock.

Keywords: COVID-19, Uncertainty Shocks, Heteroskedasticity, Daily SVAR.

JEL codes: D12, E21, E32, H12.

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†**Miescu**: Lancaster University, UK. Email: m.miescu@lancaster.ac.uk;

‡**Rossi**: University of Manchester, UK. Email: raffaele.rossi@manchester.ac.uk.

1 Introduction

Understanding and measuring the causal effects of the COVID-19 pandemic is a primary goal for economists and policy-makers alike. However, this has proven to be a daunting task both from an empirical as well as from a theoretical point of view. From an empirical point of view, during the first wave of the pandemic, many confounding factors were happening contemporaneously, such as changes in expectations, policy interventions and sudden increases in uncertainty. This makes isolating causal effects of the pandemic very problematic. At the same time, the pandemic overall is not easily reconcilable with standard macroeconomic fundamentals, thus making it difficult to analyse it under the lens of off-the-shelf general equilibrium models. We address these issues and make two contributions. First, we exploit unexpected news and announcements related to the pandemic to extract a *COVID-19-induced* shock and estimate its short-run recessionary effects. Second, we propose an interpretation of COVID-19-induced shocks as structural uncertainty shocks. We analyse US daily data and cover the period between 13 January and 31 October 2020.

The COVID-19-induced shock is extracted with a statistical procedure within a VAR by combining a daily dataset of economic indicators, see Chetty et al. (2020), with the information content around days with large jumps in financial markets directly caused by COVID-19-related news and announcements, as reported by Baker et al. (2020) and major national newspapers.[1] In particular, we show that around these event days, the volatility of the system is higher than during non-event days, and that this difference can be attributed to a single, orthogonal shock – the 'COVID-19-induced' shock. This procedure is commonly known in the applied-macroeconomic literature as identification by heteroskedasticity (see Rigobon, 2003, Wright, 2012 and Gürkaynak et al., 2020).

At its core, we exploit the lumpy nature of relevant news and announcements relating to the pandemic as a source of statistical identification. Our key identifying assumption

[1]Daily economic data used in our empirical exercise are from the Economic Tracker, available at `tracktherecovery.org`. Categorisations of stock market jumps can be found at `stockmarketjumps.com`.

is that during our selected dates, the COVID-19-induced shocks are heteroskedastic with a particularly high variance, while other contemporaneous shocks are not. As such, this identification method allows for the possibility that other shocks occur on the same days beside the COVID-19-induced shock as long as their variances are unchanged in these and other days.[2]

Based on this approach, our paper's main empirical insights consist of showing that during the pandemic, a COVID-19-induced shock has: i) significant contractionary effects on economic and financial aggregates; and ii) important distributional and sectoral effects. At the aggregate level, we show that an unexpected COVID-19-induced shock that contracts the S&P 500 Index by 1 percent depresses standard economic indicators such as employment (-0.3 pc), private expenditure (-0.6 pc) and small business revenues (-0.6 pc). Furthermore, we find that the same shock increases risk and uncertainty measures such as the VIX index (+5 pc), the Economic Policy Uncertainty Index (+2.2 pc), TED (+1.7 bp) and BAA (+2.7 bp) spreads, and it depresses the global stock price index MSCI (-0.8 pc).

At the distributional level, we show that a COVID-19-induced shock reduces employment of poor households almost twice as much as that of rich households, by -0.4 pc vs. -0.2 pc, respectively. In contrast, the contraction in private expenditure is almost 50 percent higher for rich households (-0.6 pc) relative to poor ones (-0.4 pc). Moreover, as expected, we find that industries that rely heavily on face-to-face interactions, such as 'entertainment and hospitality', suffer a reduction in revenues that is between two and three times larger than the reduction in revenues experienced by industries which can operate remotely, such as 'business services'.

Next, we provide evidence that our statistically identified COVID-19-induced shock can be interpreted as a structural uncertainty shock. This link between statistical and

[2] Furthermore, unlike announcements relating to monetary policy events, announcements relating to the pandemic are not fixed at a scheduled time and are most likely scattered during the event days. For this reason, it is not feasible to create a few-minutes window in stock price movements around a specific announcement and use it as an instrument in a Proxy-SVAR fashion.

structural analysis is important because it allows us to study the dynamics of the current pandemic under the lens of standard economic fundamentals. Our structural interpretation can also be useful for modeling and calibrating large general equilibrium models of the pandemic, such as augmented SIR models, which are particularly insightful for macroeconomic as well as for microeconomic policy counterfactual.

Overall, we believe that our link between COVID-19 and uncertainty is reasonable on at least two counts. First, the current COVID-19 pandemic is increasing uncertainty about most, if not all, aspects of our lives. To mark this, measures of macroeconomic, financial and economic policy uncertainty all spiked at the onset of the pandemic (see Cascaldi-Garcia et al., 2020). Thus, it comes natural to link the pandemic news to uncertainty, as reported in the survey evidence of Dietrich et al. (2020), Coibion et al. (2020a), Binder (2020), Fetzer et al. (2020), and subsequent contributions.[3] Second, impulse response functions to COVID-19-induced shocks (*e.g.* the large and systematic increase in the VIX index; hump-shaped recessionary effects; and increased risk factors), are reminiscent of an uncertainty shock as is typically found throughout the empirical and theoretical literature, e.g. Leduc and Liu (2016), Basu and Bundick (2017) and Cascaldi-Garcia and Galvao (2020).

To assess our conjecture, we estimate a structural uncertainty shock on the whole sample under consideration by adopting two popular (and somewhat complementary) identification methods, *i.e.* Cholesky as in Altig et al. (2020) and Sign-Restriction as in Uhlig (2005). We find that the COVID-19-induced shocks and structural uncertainty shocks have a high correlation (0.86) and generate qualitatively and quantitatively comparable dynamic responses of key financial and economic indicators. This holds true both for the aggregate variables as well as for the distributional ones. These results are surprising because the identification schemes adopted to extract the COVID-19-induced shocks

[3]There was unprecedented uncertainty about the health consequences and the mortality of the virus; the ability and resources of healthcare systems to manage this exceptional emergency; the speed and effectiveness of a safe and reliable vaccine; social distancing, market lock-downs, and school closures; the depth and persistence of the economic downturn; and the speed and effectiveness of economic policy interventions, *inter alia*.

and the ones for uncertainty shocks are completely different. Interestingly, we reach the same results when we control for potential overlapping information between first-moment shocks, such as agents' confidence and our measure of uncertainty. As such, our findings strongly suggest that the COVID-19-induced shock and the structural uncertainty shock capture the same economic innovation.

Relation to the literature. Our paper relates to two strands of the emerging literature on the economic consequences of the COVID-19 pandemic. First, we provide causal empirical evidence about the short-run effects of news and announcements about the pandemic. On this, our paper closely relates to the literature that employs high-frequency data to measure the economic repercussions brought by the COVID-19 pandemic. At the aggregate level, Baek et al. (2020) measure the labour market effects of Stay-at-Home orders in the US and find that it caused around a quarter of all unemployment insurance claims between mid-March and beginning of April 2020. Using a newly compiled weekly economic indicator, Lewis et al. (2020) find that the pandemic had a significant contractionary effect on the US economy during the early weeks of the outbreak. Coibion et al. (2020a,b) use a repeated large-scale household survey and analyse the recessionary effects of the pandemic and lockdowns on employment, consumption and macroeconomic expectations. At the distributional level, Chetty et al. (2020) use a newly built daily dataset of economic indicators for the US and find that the pandemic outbreak had a stronger impact on the employment of the poor and the consumption expenditure of the rich. Hacioglu et al. (2020) find similar results in a weekly dataset of the UK.

We depart from this literature as we estimate the short-run causal effects of COVID-19 by exploiting large jumps in the stock markets that we combine with daily economic indicators of the pandemic. As such, we can rely on standard high-frequency time-series techniques, and analyse both the aggregate as well as the distributional short-run effects of the pandemic.

Second, we contribute to the literature that analyses the current pandemic under the lens of structural uncertainty shocks, *e.g.* Baker et al. (2020), Ludvigson et al. (2020), Cox et al. (2020), Dietrich et al. (2020), Caggiano et al. (2020), Pellegrino et al. (2021) and subsequent contributions. The closest contributions to our paper can be found in Baker et al. (2020) and Altig et al. (2020). These papers calibrate the size of the uncertainty shock on the jumps of the VIX index observed during the pandemic, and then they back out the contractionary effects of a 'COVID-19-induced' uncertainty shock either in a post-1980s quarterly model of economic disasters (Baker et al., 2020) or in a post-1960s monthly Cholesky-VAR (Altig et al., 2020).

We differ from these papers on two important aspects. First, we estimate a SVAR at daily frequency on a sample period during the first part of the pandemic (Jan-October 2020). This approach can isolate a precise quantitative understanding of the transmission mechanism of the uncertainty shocks and can also help avoid potential issues of structural breaks in the data post-January 2020 (see Lenza and Primiceri, 2020). Second, our methodology allows us to draw a formal link between COVID-19 related news and announcements and uncertainty shocks. This link is generally treated as an *ex-ante* assumption by the cited literature.

However, our approach comes at the cost of analysing a restricted number of variables (those available at daily level) and our inference is only valid for the sample under consideration, *i.e.* Jan-October 2020.[4] Furthermore, given that we use daily data, we cannot extend the sample prior to January 2020 as, before this date, economic indicators such as expenditure and employment do not exist at a daily frequency. Therefore our results only cover the short-run effects of uncertainty shocks. In this sense, data availability allows us to provide an important, unique and high-frequency (yet only partial) perspective of the economic effects of uncertainty shocks which arose during the pandemic.

The remainder of the paper is as follows: Section 2 describes the statistical technique used in the paper to extract our COVID-19-induced shock. Section 3 gives a brief descrip-

[4]Given that our dataset ends in October, our estimates should not be influenced by the change in presidency, and as such, all our results are conditional on having Trump as POTUS.

tion of our dataset. Section 4 reports our empirical results for the COVID-19-induced shock, both at the aggregate and distributional levels, while Section 5 presents our link between the statistical COVID-19-induced shock and the structural uncertainty shock. Finally, Section 6 concludes.

2 The COVID-19-Induced Shock

Here we outline the empirical model used to extract our COVID-19-induced shock. We estimate a VAR at daily frequency by combining heteroskedasticity identification as in Rigobon (2003), Wright (2012) and subsequent contributions, with standard Bayesian techniques (see also Miescu and Mumtaz, 2020). A description of the latter can be found in Appendix A.

The starting point of our analysis is a reduced-form VAR of order P, written as:

$$Y_t = X_t \beta + \mu_t, \tag{1}$$

where Y_t is $1 \times N$ matrix of endogenous variables, $X_t = [X_{t-1}, .., X_{t-P}, 1]$ is a $1 \times (NP+1)$ matrix of regressors, and β is a $(NP+1) \times N$ matrix of coefficients. Finally μ_t is a $1 \times N$ vector of reduced-form residuals. Identification of meaningful shocks amounts to finding a mapping Γ between the prediction errors μ_t and a vector of mutually orthogonal shocks ϵ_t, i.e.

$$\mathbf{\Gamma} \epsilon_t = \mu_t, \tag{2}$$

where $\mathbf{\Gamma}$ is a $N \times N$ non-singular matrix of coefficients that satisfy $E(\mu_t \mu_t') = \mathbf{\Gamma}\mathbf{\Gamma}'$. The identification of our COVID-19-induced shock within the vector ϵ_t exploits the following two testable assumptions: i) the volatility of the system on those days in the sample (event days) when large jumps ($\geq$ 2.5 pc) of the S&P 500 index are due to news and announcements about the pandemic is different, *i.e.* higher than on other days (non-event days); and ii) that the difference in volatility between event and non-event days is explained by a single orthogonal shock. We label this shock as *COVID-19-induced* shock.

Briefly, the identification exploits the lumpy and otherwise unpredictable nature of important events related to the COVID-19 pandemic, so that the days on which they happen are effectively random dates on the calendar. If this is true, then the variance of all other orthogonal shocks in vector ϵ_t should be the same on these and on other days. Crucially, the conditional variance of the other shocks can vary from day to day as long as their average variance is the same on event and non-event days.

Then, by defining Σ_H and Σ_L as the variance-covariance matrices of the reduced form errors on events and non-events days and σ_H^2 and σ_L^2 as the variances of the COVID-19-induced shocks on event and non event days, respectively, we can transform equation (2) as:

$$\Sigma_H - \Sigma_L = \Gamma_1 \Gamma_1' \left(\sigma_H^2 - \sigma_L^2 \right). \tag{3}$$

This enables us to recollect the vector Γ_1, which suffices to identify our COVID-19-induced shock.[5] Given that we are not interested in identifying any other orthogonal shock, we do not need to impose any further structure on Γ. It should be emphasised that the estimated coefficients in Γ_1 are still consistent in the case that heteroskedasticity is misspecified in the model, e.g. GARCH (see Rigobon, 2003 for further details).

From a statistical point of view, we proceed as follows. We estimate the parameters of our VAR via standard Bayesian techniques. Then, we compute within the same iteration, the sample variance-covariance matrices of the VAR residuals on event, i.e. $\hat{\Sigma}_H$, and non-event days, i.e. $\hat{\Sigma}_L$. Finally, we estimate the vector $\hat{\Gamma}_1$ of parameters corresponding to our COVID-19-induced shock, as a standard minimum distance problem, *i.e.*

$$\begin{aligned} \Gamma_1 = \arg\min_{\Gamma_1} & \left[vech\left(\hat{\Sigma}_H - \hat{\Sigma}_L\right) - vech\left(\Gamma_1 \Gamma_1'\right) \right]' \left[\hat{V}_L + \hat{V}_H\right]^{-1} \times \\ & \left[vech\left(\hat{\Sigma}_H - \hat{\Sigma}_L\right) - vech\left(\Gamma_1 \Gamma_1'\right) \right], \end{aligned} \tag{4}$$

[5]Given that $\Gamma_1 \Gamma_1'$ and $\left(\sigma_H^2 - \sigma_N^2\right)$ are not separately identified, we can impose the normalisation $\sigma_H^2 - \sigma_L^2 = 1$ without any loss of generality. Furthermore, our notation implies that the COVID-19-induced shock is ordered first, but this is just for notational convenience, since the ordering of variables is irrelevant.

where $\hat{V}_H$ and $\hat{V}_L$ are the sample estimates of the variance-covariance matrices of $vech(\hat{\Sigma}_H)$ and $vech(\hat{\Sigma}_L)$, respectively.

3 The Data

This section describes the data used in our econometric exercise. We work at a daily frequency (business days) and our sample covers the period 14/1/2020 to 31/10/2020. We now briefly present the data used and refer the reader to Appendix B for a detailed description of the dataset. Our data come from three distinct sources. First, we collect readily available daily financial data such as the S&P 500 index, the VIX index *et cetera*. Second, we use publicly available daily data on a set of economic indicators such as employment and private spending at the granular level, built using anonymised data from several private companies, such as credit card processors and payroll firms (see Chetty et al. (2020) and Appendix B for further details). Third, we select the list of days/events necessary for our identification by exploiting the newspaper-based dataset presented in Baker et al. (2020), which covers our data sample.

In the original dataset, Baker et al. (2020) examine next-day newspaper explanations for each daily movement in the U.S. stock market greater than 2.5 percent and classify the journalists' explanations for the sudden stock market movements into sixteen categories. The underlining observation is that large stock market jumps always attract media coverage in major newspapers on the very same night or on the following day. Then we classify as event days the episodes in the Baker et al. (2020) dataset within our sample that have as a primary cause news and announcements about the COVID-19 pandemic. These include, for example, large stock market movements due to pandemic fears in January and February, the ramp up of COVID-19 infections in March, but also the success of lockdown measures in April and hopes for the roll-out of the COVID-19 vaccine in May.[6,7]

[6] Further complementary explanation about the stock market events can be found in stockmarketjumps.com.

[7] Interestingly, although they attracted a lot of media attention, President Trump's announcements of

In order to improve our identification, we remove those days when important policy or macroeconomic announcements were made (such as 3*rd* of March and 29*th* of April) and events without a clear classification. In this way we isolate sixteen event days between January and October 2020, the last month presented in the Baker et al. (2020) dataset. The list of events with a brief description as reported in Baker et al. (2020) can be found in Table 1.

Table 1 – Stock market jumps due to Coronavirus news and announcements as reported by Baker et al. (2020).

Date	S&P 500 Jump	Brief Explanation
24/02/2020	-0.034	Pandemic fears
25/02/2020	-0.030	Pandemic fears
27/02/2020	-0.044	Pandemic fears
05/03/2020	-0.034	Pandemic fears
11/03/2020	-0.049	Worsening COVID-19 infections
12/03/2020	-0.095	COVID-19 infection surge
16/03/2020	-0.120	COVID-19 infection surge
18/03/2020	-0.052	COVID-19 infection surge
01/04/2020	-0.044	COVID-19 infection surge
06/04/2020	0.070	Success of COVID-19 lockdowns in Europe
08/04/2020	0.034	Pandemic slowdown in Europe and the US
14/04/2020	0.031	Reopening possible in the US
17/04/2020	0.027	COVID-19 drugs trial
18/05/2020	0.032	Hopes for COVID-19 vaccine
11/06/2020	-0.059	COVID-19 infection surge
24/06/2020	-0.026	COVID-19 infection surge
28/10/2020	-0.035	COVID-19 fears

4 The Empirical Evidence

This section presents the results pertaining to the COVID-19-induced shocks. First, we describe our benchmark econometric model and discuss a number of issues related to the validity of our identification. Second, we present a set of estimated impulse response

unproven COVID-19 treatments (19th - 21st of March 2020) did not cause large movements in stock prices. This is different to Trump's announcement of a COVID-19 aid package on the 13th of March, which caused an increase in stock prices of 9.3%. However, we do not include this date in our list as it is a policy event.

functions (IRFs) to COVID-19-induced shocks for aggregate, distributional and industry-level variables.

The Benchmark Model. Our econometric specification consists of a five-variable VAR comprising of financial and economic indicators, i.e.

$$X_t = [ln\,(VIX_t)\,,\, ln\,(S\&P500_t)\,,\, R_t,\, C_t,\, Emp_t]\,, \tag{5}$$

where $ln\,(VIX_t)$ is the (log of) VIX index, a popular financial indicator, commonly used as a proxy for forward looking economic uncertainty, e.g. Bloom (2009) and Baker et al. (2016).[8] $ln\,(S\&P500_t)$ is the (log of) the S&P 500 Index, the main US stock market indicator. It is meant to capture a number of first-order effects, given its forward-looking nature and the amount of information it contains. (R_t) is the 1-Year Treasury Constant Maturity Rate (DGS1). As argued by Gertler and Karadi (2015), this variable is an appropriate proxy for monetary policy when the Federal Fund Rate is stuck at zero, as in the sample under consideration. C_t is private expenditure and is the most common economic indicator used to capture aggregate demand conditions. It is reported as the 7-day moving average, seasonally adjusted credit/debit card spending relative (in percent deviation) to January 2020. Finally, Emp_t is employment and is meant to capture labour market conditions. This series is based on firm-level payroll data. Like the data on expenditure, it is reported as the 7-day moving average, seasonally adjusted relative (in percent deviation) to January 2020. Despite its daily frequency, our benchmark VAR specification includes a set of variables commonly used in applied works, *e.g.* Baker et al. (2016). The sample is consistently kept between 14/01/2020-30/10/2020, and the lag structure is equal to ten, *i.e.* two working weeks.[9]

[8] We use the VIX index in logs in order to smooth its variance that displays extreme spikes during the sample under consideration. Furthermore, by taking logs we have a clear interpretation in percent terms of the IRFs of the VIX index. Finally, the results remain, for all practical purposes, identical in an alternative model with the VIX index in levels (result available upon request).

[9] Although the curse of dimensionality is less of a problem in our Bayesian framework, we experiment with different lag structures (5, 21), and the results are for all practical purposes unchanged (see

Validation of our identification. Our identification strategy is based on two requirements. First, we require that event and non-event days are different with respect to their variance-covariance matrix of reduced-form residuals, that is $\Sigma_H \neq \Sigma_L$. This is essential to achieve identification as it signals heteroskedasticity on event days. We verify this requirement by computing for each saved draw in the Gibbs-sampler, the statistical distance

$$\hat{\Pi}_1 = vech\left(\hat{\Sigma}_H - \hat{\Sigma}_L\right) vech\left(\hat{\Sigma}_H - \hat{\Sigma}_L\right)'. \tag{6}$$

If the two variance-covariance matrices are not statistically different, we should obtain posterior distributions concentrated around zero. Figure 1 (left-quadrant) shows that this is not the case, as the Kernel distribution is not centered at zero. This brings favourable evidence to our identification assumption.

Second, we require that the difference in the variance-covariance matrices can be factored in the form of $\Gamma_1\Gamma_1'$, i.e. $\Sigma_H - \Sigma_L = \Gamma_1\Gamma_1'$. This would indicate that the difference in the variance-covariance matrices between event and non-event days can be explained by one orthogonal shock, the COVID-19-induced shock. We verify this requirement by computing, for each saved draw, the statistical distance

$$\hat{\Pi}_2 = \left[vech\left(\hat{\Sigma}_H - \hat{\Sigma}_L\right) - vech\left(\hat{\Gamma}_1\hat{\Gamma}_1'\right)\right]' \left[vech\left(\hat{\Sigma}_H - \hat{\Sigma}_L\right) - vech\left(\hat{\Gamma}_1\hat{\Gamma}_1'\right)\right]. \tag{7}$$

The identification assumption is verified if the posterior distribution of Π_2 is concentrated around zero, which Figure 1 (right-quadrant) suggests is the case.

As in Wright (2012), we can also test our two identification hypotheses via a standard Wald test (a slight statistical abuse under our Bayesian approach). In this case, for the first hypothesis, i.e. that the system is more volatile on event days, we test the null that $\Sigma_H = \Sigma_L$. For this, we use the posterior median from the statistic

$$\hat{\Omega}_1 = \left[vech\left(\hat{\Sigma}_H - \hat{\Sigma}_L\right)\right]' \left[\hat{V}_L + \hat{V}_H\right]^{-1} \left[vech\left(\hat{\Sigma}_H - \hat{\Sigma}_L\right)\right], \tag{8}$$

Appendix C, Figure C.2).

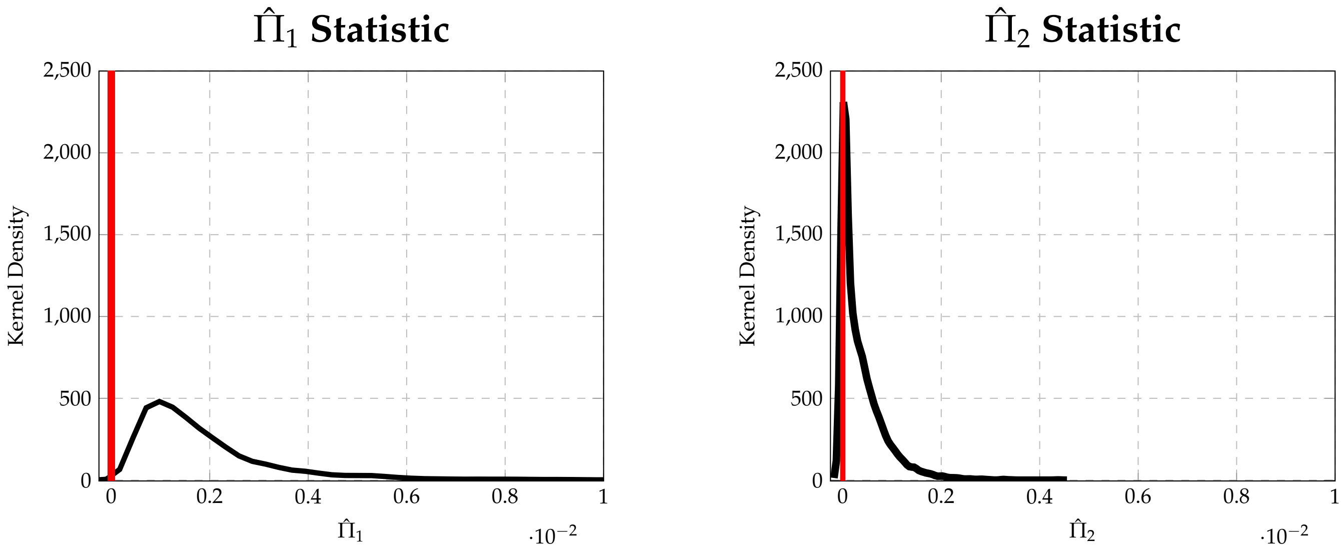

Figure 1 – Kernel density functions calculated on 5000 posterior draws of the statistics $\hat{\Pi}_1$, see equation (6), and $\hat{\Pi}_2$, see equation (7).

while for the second requirement, i.e. that the difference in volatility between event and non-event days can be attributed to a single orthogonal shock, we test the null that $\Sigma_H - \Sigma_L = \Gamma_1 \Gamma_1'$. For this, we use the posterior median of the statistic

$$\hat{\Omega}_2 = \left[vech\left(\hat{\Sigma}_H - \hat{\Sigma}_L\right) - vech\left(\hat{\Gamma}_1 \hat{\Gamma}_1'\right)\right]' \left[\hat{V}_L + \hat{V}_H\right]^{-1} \left[vech\left(\hat{\Sigma}_H - \hat{\Sigma}_L\right) - vech\left(\hat{\Gamma}_1 \hat{\Gamma}_1'\right)\right]. \tag{9}$$

Our identification is validated if we reject the first hypothesis and accept the second. In our baseline VAR, we find $\hat{\Omega}_1 = 38.8$ (p-value = 0.006) and $\hat{\Omega}_2 = 20.5$ (p-value = 0.15), so we reject the first hypothesis and accept the second, as desired. As such we bring further support for our identification scheme and for the presence of a single orthogonal shock explaining the difference in volatility between event and non-event days.[10]

IRFs of Aggregate Variables. Now we turn our attention to the analysis of the impulse response functions (IRFs). For each observable, we report the response of its posterior

[10] In Appendix C, Figure C.1, we also run a placebo-style exercise and show that if we randomise the event dates, our COVID-19-induced shock is not identified. This result further supports our choice of events in a sample characterised by the turbulent behaviour of financial markets.

median and the 68 and 90 credibility intervals to a COVID-19-induced shock scaled to lower the S&P 500 index by 1 percent. This scaling is without loss of generality and purely for expositional purposes.[11]

The COVID-19-induced shock has a prolonged contractionary effect on the financial markets as the S&P 500 index remains below its trend for around 40 working days (8 weeks). In the same fashion, the VIX index jumps on impact by around 4.2 percent and remains above its trend for about seven weeks (35 working days). The peak response in these two variables happens on impact and clearly reflects the forward-looking nature of financial markets. In Appendix C, Figure C.3, we show that world stock prices, i.e. the MSCI index, display a similar response to the S&P 500 index, reflecting the co-movement in the international financial variables (see Miranda-Agrippino and Rey, 2020).[12] Along the same line, in Appendix C, Figure C.3, we find that a COVID-19-induced shock increases significantly two standard measures of risk, the TED and the BAA spreads, whose peak effects happen two weeks after the shock and are around 2 and around 3 basis points for TED and BAA spreads, respectively.[13] Finally, in order to measure the effects of our COVID-19-induced shock on agents' expectations and confidence, in Appendix C, Figure C.3, we present results from VARs that include the Sentiment Index, a recent text-based measure of daily economic sentiment from economic and financial newspaper articles (see Shapiro et al., 2020). This index has been shown to correlate with a number of standard consumer confidence measures available at lower frequencies, such as the Michigan Consumer Sentiment Index. We find that a COVID-19-induced shock has a negative effect on agents' sentiment, with a peak effect of around 0.4 index-point three weeks after the shock. Interestingly, the response of the Sentiment Index to COVID-19

[11] While we also report the 90 percent credibility set, it is important to stress that the standard significance level within Bayesian settings is 68 percent (see Sims and Zha, 1999).

[12] For these extended variables, we plot the response of the observables added singularly one-by-one to the benchmark model in (5). For example, the IRF of the MSCI Index comes from a model where we add the MSCI Index to the set of observables in (5).

[13] The TED spread is the difference between the three-month Treasury bill and the three-month LIBOR based in US dollars. Put differently, the TED spread is the difference between the interest rate on short-term US government debt and the interest rate on interbank loans.

news and announcements is muted on impact.

The COVID-19-induced shock also generates a contraction in the 1-year Treasury rate, which approximates monetary policy. The peak response of around 1.1 basis points happens shortly after two weeks from the shock. The short delay in the response of the interest rate reflects the prompt policy actions taken by the monetary authority, both with conventional and unconventional instruments (see Bahaj and Reis, 2020 and Cox et al., 2020 among others), to news and announcements about the pandemic. In Appendix C, Figure C.3, we also show that the newspaper-based measure of economic policy uncertainty, the EPU index (see Baker et al., 2016), rises significantly with increases in COVID-19-induced shock. Interestingly, the peak effect on EPU happens slightly after the movements in the 1-year Treasury rate, probably signalling an increase in policy uncertainty around monetary policy interventions.

The last row of Figure 2 presents the response of private expenditure and employment to a COVID-19-induced shock. The main message is that these economic variables contract significantly in the short run to news and announcements about the pandemic. Employment, one of the main economic indicators of the labour market, decreases, with a maximal effect of 0.34 percent and 90 percent credibility set [-0.63;-0.02]. On the household side, we find that the maximal effect on private expenditure is around 0.46 percent and 90 percent credibility set [-0.85;-0.02]. In Appendix C, Figure C.3, we show that, consistently with the results on expenditure and employment, a COVID-19-induced shock contracts small-business revenues by around 0.6 percent and small business openings by around 0.5 percent.

These results are broadly consistent with the recessionary effects of the COVID-19 pandemic typically found in the literature, e.g. Baek et al. (2020), Lewis et al. (2020) and Coibion et al. (2020a). Our key contribution lies in combining high-frequency data with an event-study identification scheme. In this way, we can apply standard time-series techniques, and thus analyse the recessionary effects of news and announcements about the pandemic under the lens of VARs – the workhorse of empirical macroeconomics. This

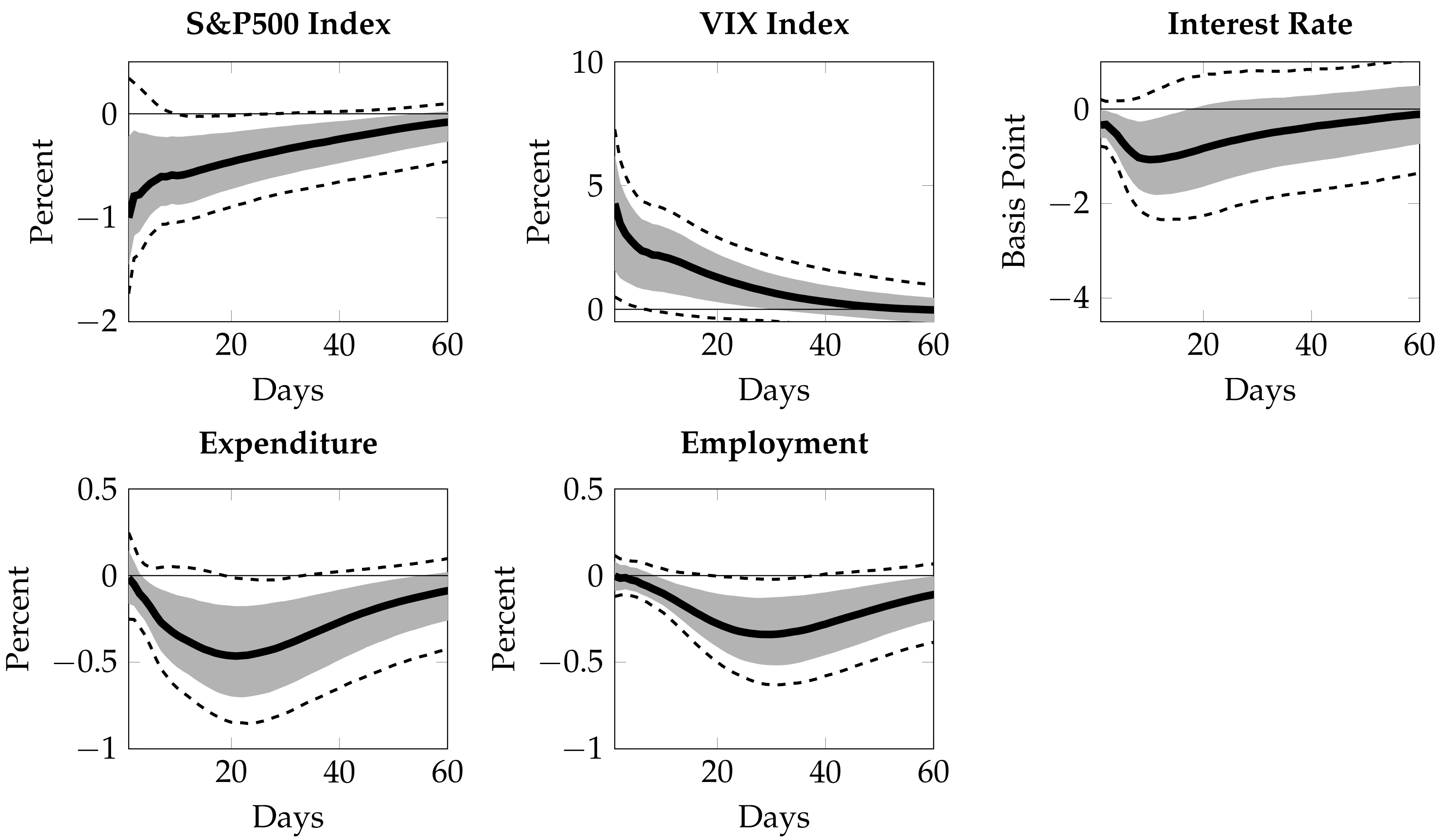

Figure 2 – IRFs to a COVID-19-induced shock lowering S&P 500 by 1 percent. Solid black line, median. Shaded areas and dotted lines are the 68 and 90 credibility sets, respectively.

allows us to study accurately the dynamic response of key economic and financial indicators and to recollect a precise transmission mechanism through an analysis of IRFs. It also enables us, by applying various identification schemes, to present a structural interpretation of the COVID-19-induced shock (see Section 5).

Results on Distributional/Sectoral Variables. Analysing the aggregate effects of a COVID-19-induced shock is important, as it sheds light on the short-run consequences of the pandemic from a macroeconomic perspective. As such, the results presented in the previous section can be informative to policy-makers for the setting of sound short-run macroeconomic policies. However, as is clear by now, exposure to the pandemic is extremely heterogeneous across different parts of the economy, e.g. Belot et al. (2020), van Dorn et al. (2020), Coibion et al. (2020b) and Chetty et al. (2020). For instance, strict lockdowns hit workers in manufacturing and in the service sectors very differently, or businesses like Amazon vs. the local bookshop.

Along these lines, in this Section we explore two forms of heterogeneity through which COVID-19-induced shocks could affect households' behaviour and their welfare: the first relates to income distribution and differences between richer and poorer areas; and the second relates to different business sectors such as business services, education and hospitality and expenditure categories, such as food services and transport. For the sake of brevity, we present the empirical findings in Table 2 where we include the peak effect of the median response along the IRFs, and the period when the peak effect materialises. Figure C.4, in Appendix C, reports the full set of IRFs for each observable in Table 2.

In order to be consistent among specifications, we proceed as follows. First, from the benchmark specification in (5), we keep three baseline variables, i.e. $[ln\left(VIX_t\right), ln\left(S\&P500_t\right), R_t]$. Then we add each variable in Table 2 to this set one-by-one. This is done to avoid interference in the econometric specifications between aggregate expenditure and employment and their subcategories. Thus, it should be understood that all of the results presented here come from a set of four-variable VARs. In all cases, the lag structure is kept at 10 as in the benchmark.

We start the analysis from the employment indicators. The main result is that employment in low-income (bottom quartile) areas decreases almost twice as much as that in high-income areas (top quartile), i.e. -0.40 vs -0.23 percent, respectively. Both responses are significant at 90 percent. There are at least two reasons that can explain this result. First of all, some sectors are traditionally more populated by high-income workers, *e.g.* business services, and could continue to operate within the pandemic as they require less person-to-person contact. This is also confirmed by the relatively small loss of revenues by small businesses operating in this sector (see Part B of the Table). Second, it is natural to expect that the employment status of high-income workers, being on average more skilled, is in general less exposed to business-cycle risk and fluctuations – a standard finding in macro/labour studies (see inter alia, Solon et al., 1994 and Bils et al., 2012).

On the expenditure side, we find that the peak contraction on spending in high-income areas is almost 50 percent larger than in poorer areas, i.e. -0.58 vs. -0.39 percent, respectively. First of all, there is compelling evidence that households at the top of the income distribution finance their consumption out of asset ownership (Lettau et al., 2019), whose returns decreased sharply in the face of a COVID-19-induced shock. This effect might be only mitigated by portfolio rebalancing as found in the survey evidence presented in Coibion et al. (2020a). Second of all, it appears that the decrease in expenditure happened in categories that were simply not available during the lockdown and where rich households spend traditionally more, such as food services and entertainment (Part C of the table). Conversely, categories where poor households spend relatively more, *i.e.* groceries, increase in the face of a COVID-19-induced shock. Interestingly, also the contractions of small-business openings follow the same pattern as the expenditure variable, and they appear more severely affected by COVID-19-induced shocks in rich rather than poor areas.

Moving to the sectoral analysis in Part B of the Table, we find a clear, although not surprising, pattern. Industries that rely less on face-to-face and personal interactions suffer less from COVID-19-induced shocks relative to industries where the nature of the industry requires face-to-face interactions. For instance, the Professional and Business Service Industry (NAICS 60) recorded a smaller decrease in terms of employment, revenues and business opening than the Leisure and Hospitality Industry (NAICS 70) or the Education and Health Services (NAICS 65). This is a specific feature of the pandemic and differs sharply from the firm level-response at business cycle frequencies before January 2020 when the main discriminant factor was instead firm financial exposure, e.g. Gilchrist et al. (2014) and Alfaro et al. (2018).

One possible concern relating the results presented in this section is the potential role of spillovers and feedback among distributional and industry variables, *e.g.* income quan-

Table 2 – Peak Effects on Distributional and Sectoral Variables. Asterisks * and ** mean 68 and 90 percent significance, respectively.

Part A: Distribution

Variable	Peak Effect	Period (in weeks)
Employment, Aggregate	-0.34**	6
Employment, High Income	-0.23**	5
Employment, Mid Income	-0.34**	6
Employment, Low Income	-0.40**	6
Expenditure, Aggregate	-0.47**	5
Expenditure, High Income	-0.58**	5
Expenditure, Mid Income	-0.45**	5
Expenditure, Low Income	-0.39**	6
Small Business Revenue, Aggregate	-0.63**	5
Small Business Revenue, High Income	-0.64**	5
Small Business Revenue, Mid Income	-0.63**	5
Small Business Revenue, Low Income	-0.64**	4
Small Business Openings, Aggregate	-0.49**	6
Small Business Openings, High Income	-0.53**	6
Small Business Openings, Mid Income	-0.49**	6
Small Business Openings, Low Income	-0.44**	6

Part B: Sectors

Variable	Peak Effect	Period (in weeks)
Employment,Trade, Transportation and Utilities	-0.31**	6
Employment, Professional and Business Services	-0.20**	6
Employment, Education and Health Services	-0.31**	6
Employment, Leisure and Hospitality	-0.82**	6
Revenues, Trade, Transportation and Utilities	-0.48**	6
Revenues, Professional and Business Services	-0.36**	5
Revenues, Education and Health Services	-0.94**	6
Revenues, Leisure and Hospitality	-0.72**	5
Business Openings,Trade, Transportation and Utilities	-0.43*	5
Business Openings, Professional and Business Services	-0.18*	2
Business Openings, Education and Health Services	-0.50**	5
Business Openings, Leisure and Hospitality	-0.39*	5

Part C: Expenditure Categories

Variable	Peak Effect	Period (in weeks)
Accommodation and Food Service	-1.78**	5
Arts, Entertainment, and Recreation	-0.87**	5
General Merchandise Stores	-1.48**	5
Grocery and Food Store	0.75*	1
Health Care and Social Assistance	-0.91**	5
Transportation and Warehousing	-0.79*	5

tiles and/or different sectors. Controlling for these effects could potentially be important. At the same time, augmenting the VAR with extra variables can be problematic for the curse of dimensionality, as by increasing variables we reduce the degrees of freedom in our VARs. Table C.1 in Appendix C presents the distributional results where we control for spillovers and feedback within each income or industry categories. We do so by augmenting the VAR models with either all the income quantiles, the industries or with the expenditure categories. For example, we run a single VAR with the employment variables by including all income quantiles. Similarly, we run a single VAR with all the expenditure categories. Benchmark variables, lag structures and samples are kept as before. Reassuringly, all the qualitative results are unchanged while we find some marginal quantitative differences. Unsurprisingly, credibility sets enlarge (given the fewer degrees of freedom) and, as a result, most results are only significant at 68 percent.

Interestingly, our findings are consistent with the descriptive evidence provided by Chetty et al. (2020) for the US, by Hacioglu et al. (2020) for the UK and subsequent contributions. Like us, these analyses find that the largest drop in earnings happens in poor household areas, while the biggest reduction in spending is recorded in rich areas. These papers also report, as we do, that the effects of the COVID-19 pandemic on business activities crucially depend on how a specific industry relies on in-person interactions. Our findings are also consistent with the interpretation of the COVID-19 pandemic as a large industry and sector reallocation shock (see Barrero et al., 2020). Along the same line, large surveys evidence presented in Coibion et al. (2020a) and Coibion et al. (2020b) find that expenditure categories that recorded the largest drop are those, such as entertainment and transport, where social distancing is more difficult.

5 A Structural Interpretation to COVID-19-Induced Shocks

What we have done so far is to analyse the transmission mechanism of a COVID-19-induced shock on a set of aggregate and distributional variables. The results obtained are important as they shed light on the short-run causal effects of news and announcements about the pandemic. Of course, one serious drawback of our analysis is that the identified shock does not have a clear structural interpretation as its origin is purely statistical. For this reason, it is difficult to connect our COVID-19-induced shock to standard macroeconomic fundamentals. Here we show that our statistically identified shock can be interpreted as structural uncertainty shock – a 'COVID-19-induced' uncertainty shock.

There are several pieces of evidence that point to this interpretation. First, the current pandemic has brought about an unprecedented level of uncertainty about all aspects of our lives. For this reason, standard measures of macroeconomic, financial, and economic policy uncertainty all spiked at the onset of the COVID-19 pandemic (see Cascaldi-Garcia et al., 2020). Thus it is natural to seek to link COVID-19-induced shocks to uncertainty shocks.

Second, the IRFs to our COVID-19-induced shock, *i.e.* the impact responses of VIX and S&P 500 indexes and hump-shaped recessionary effects on economic indicators, closely resemble those of an uncertainty shock typically found within the empirical literature, *e.g.* Caldara et al. (2016) and Basu and Bundick (2017), while they are inconsistent with 'news'-type shocks, *e.g.* Cascaldi-Garcia and Galvao (2020). Third, the zero impact responses of the Sentiment Index and various credit market indicators (BAA and TED Spread) to our COVID-19-induced shock lead us to exclude other potential first-order structural interpretations such as 'expectation' or 'financial' shock.

Finally, the empirical findings of our statistical identification are also consistent with a broad range of general equilibrium models commonly used to study the transmission mechanism of uncertainty shocks. For example, our COVID-19-induced shocks can be mapped into models of effective demand featuring labour market frictions and nominal rigidities, *e.g.* Leduc and Liu (2016). In this type of model, sticky prices (or wages) magnify the effects of uncertainty shocks on the unemployment rate through declines in aggregate private demand. This decrease in aggregate demand spills over into the labour market by additionally reducing the value of new employment matches. As a result, firms post fewer job vacancies, thus pushing the unemployment rate up and output further down. Monetary policy reacts to these contractionary effects by cutting the policy rate. Thus, like our COVID-19-induced shock, an uncertainty shock shrinks economic indicators both on the demand and the supply side of the economy, *i.e.* employment and consumption, and triggers a cut in the policy rate.[14]

In order to check our conjecture, we proceed in two steps (see Kurmann and Otrok, 2013). First, we identify a structural uncertainty shock from unexpected movements in the VIX index in model (5) by imposing zero restrictions on the contemporaneous matrix Γ, *i.e.* Cholesky factorisation. With this approach, the ordering of the variables in the VAR matters for the underlying timing of the causality of the shocks. On this course, we follow the standard approach in the literature, *e.g.* Basu and Bundick (2017) and Altig et al. (2020), and identify the uncertainty shock by ordering the VIX index first. We thus assume that the VIX index does not respond on impact to any structural shock in the system other than to itself. Given the daily frequency of our empirical model, we believe that this timing assumption is reasonable and not too restrictive. Second, we compare the IRFs to the structural uncertainty shock and those from the statistical COVID-19-induced shock both on aggregate variables as well as on distributional ones.

Figure 3 reports the IRFs for the uncertainty shock with the Cholesky identification scheme and the median from the statistical COVID-19-induced shock. The main result from this exercise is that the COVID-19-induced shock and the structural uncertainty shock generate comparable dynamic responses of key financial and economic indicators. This holds true for the median responses as well as for the credibility sets. Interestingly, we obtain the same correspondence between COVID-19-induced shock and uncertainty with distributional variables, see Figure D.1 in Appendix D. This close similarity is surprising because the identification scheme adopted to identify the uncertainty shock is completely different from the statistical approach used in the benchmark model of Sec-

[14]Broadly speaking, our COVID-19-induced shocks are also consistent with supply-side models, *e.g.* Bloom (2009), whereas the recessionary effects of higher uncertainty occur because firms temporarily pause their investment and hiring for precautionary motives.

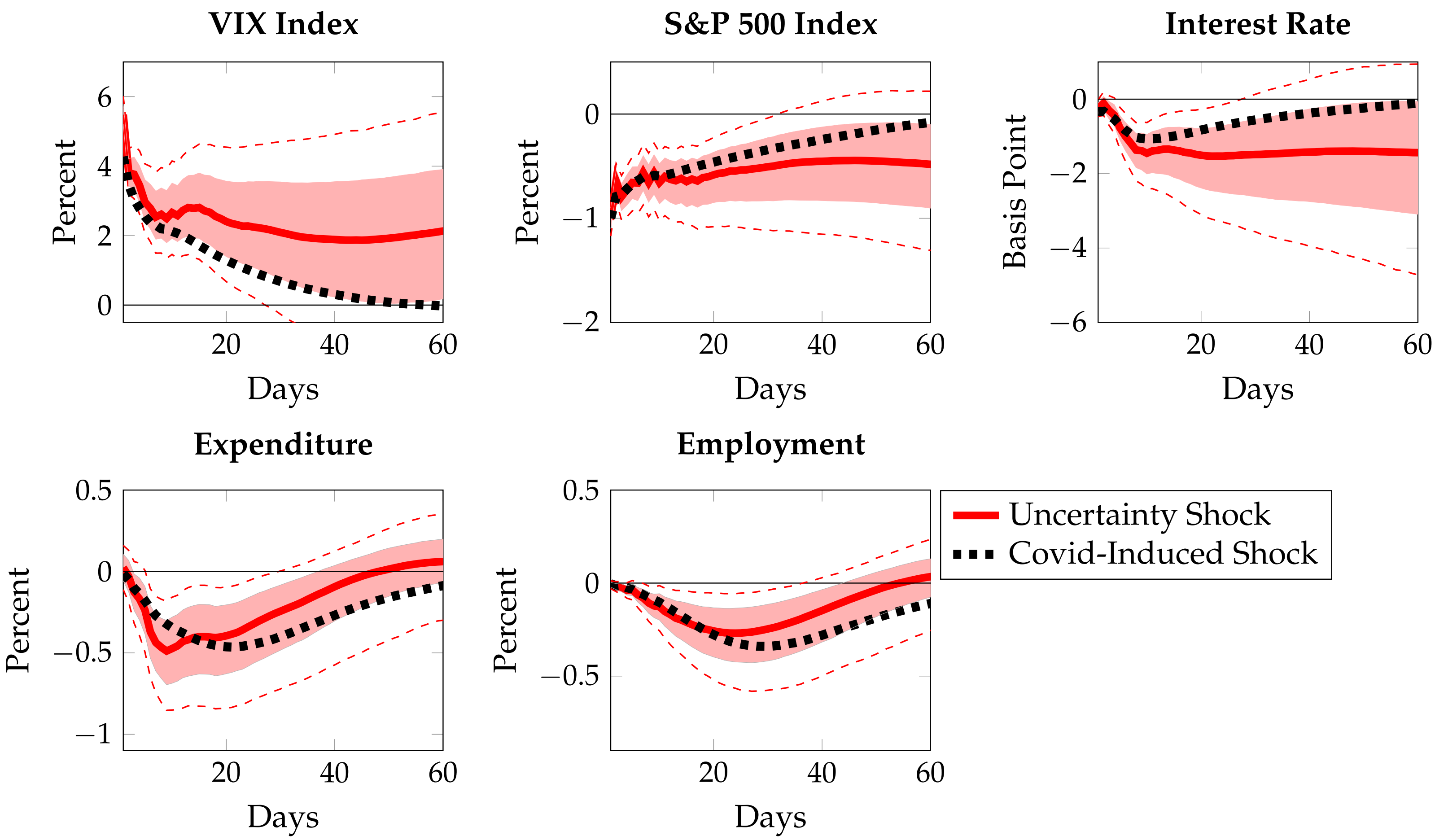

Figure 3 – IRFs to a uncertainty shock lowering S&P 500 by 1 percent, Cholesky structural identification. Solid red lines identify the median for the Cholesky identification, while shaded areas and broken lines represent the 68 and 90 percent credibility sets, respectively. Dashed black line, median response to the benchmark COVID-19-induced shock obtained with statistical identification.

tion 2. Hence, there is no *ex-ante* technical reason to expect that the two shocks capture the same economic innovation.

To further reinforce our results on the similarity between the uncertainty shock and our COVID-19-induced shock, we extract the time series of each of the two shocks and plot them together.[15] As Figure 4 shows, the two shocks move closely together with a

[15] Our COVID-19-induced shock is identified up to a scale. For this reason, we extract it by applying the transformation method proposed by Mertens and Ravn (2013).

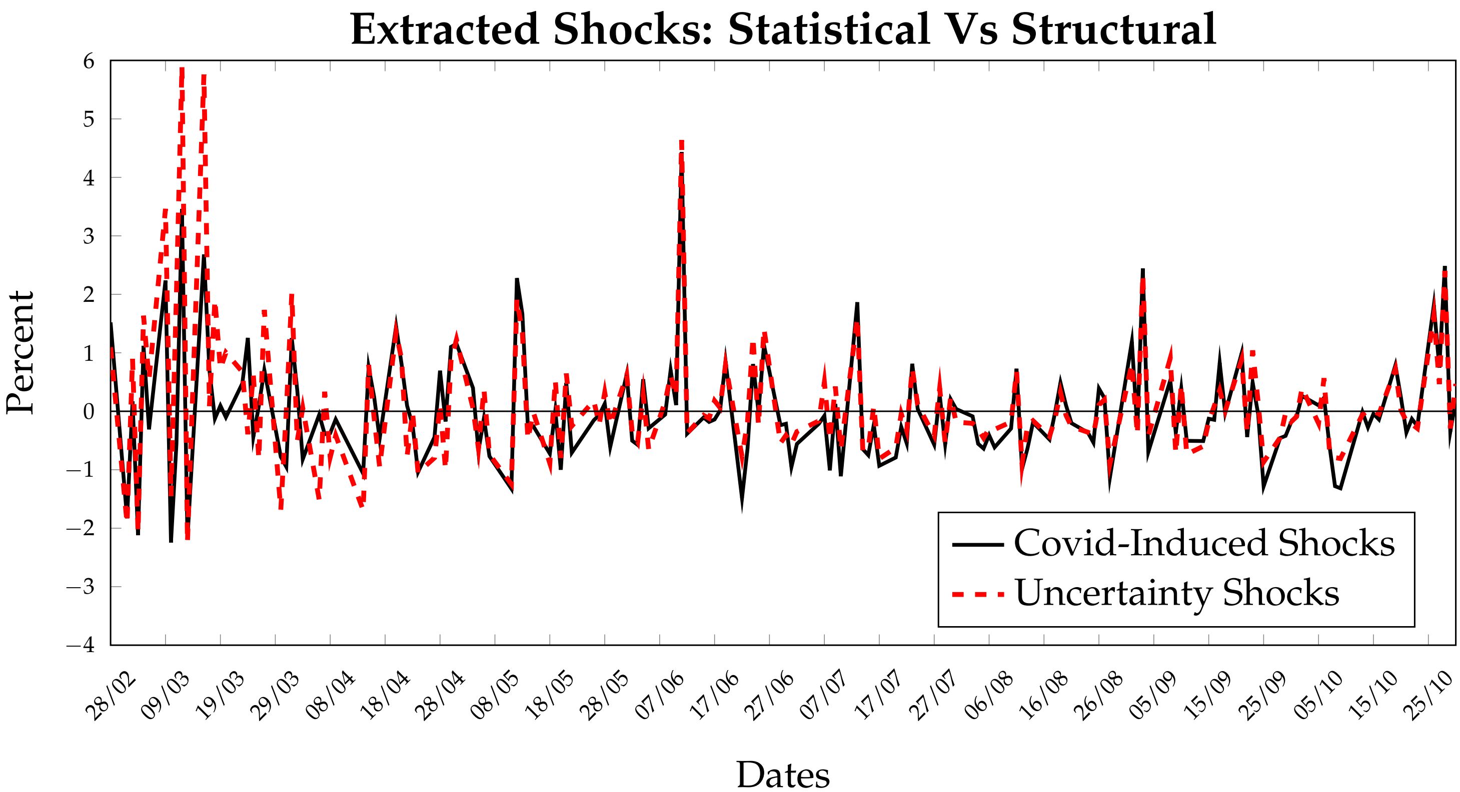

Figure 4 – Shock series (median across saved draws). Solid black line, statistical COVID-19-induced shocks, dashed, uncertainty shocks. Note that the series of the shocks starts in February as we lose some observations due to the calculation of the priors and to the lag structure of the estimated VARs.

correlation coefficient between the two of 0.86. Overall, our results strongly suggest that our statistically identified COVID-19-induced shock can be interpreted as a structural uncertainty shock.

Additional Checks. In Appendix D, Figure D.2, we show that the same link between COVID-19-induced surprises and uncertainty holds with a sign-restriction approach, *e.g.* Uhlig (2005) and subsequent contributions.

This identification scheme consists of specifying the sign of the IRFs responses of some variables included in model (5). Relative to Cholesky, the advantage of the sign-restriction approach is that timing assumptions on the contemporaneous impact matrix of the shocks are not necessary. Instead, restrictions which are often used implicitly, consistent with the conventional view, are made more explicit. Given the nature of the shock that we aim to identify, we impose that the uncertainty shock has a positive impact response on the VIX index and a negative impact response on the S&P 500 index, while we remain agnostic about the sign of the other observables in the model. It is important to note that, contrary to the Cholesky identification, the sign-restriction delivers a set of equally likely impulse responses rather than point identified estimates (see Baumeister and Hamilton, 2015, 2020). In this sense, the sign-restriction approach gains generality in some dimensions and loses in others. Most importantly, our results are largely unchanged under the two identification schemes.

A further potential concern of our analysis is whether and to what extent our IRFs, both here and in our statistical identification, simply reflect 'bad news' rather than uncertainty shocks. Including the S&P500 index in our benchmark model should mitigate this concern given that financial markets are forward-looking and stock prices incorporate many sources of information. Our baseline VAR also includes other 'first-moment' variables: employment, expenditure, and the interest rate. Still, our structural shock to the VIX index could be contaminated by first-moment information not captured by these variables.

To investigate this issue, we also consider VARs that include the Sentiment Index, our best measure of consumer confidence available at daily frequency (see Shapiro et al., 2020). In particular, we estimate jointly an uncertainty and a sentiment shock with a Cholesky identification scheme. As ordering, we identify the uncertainty shock 'after' the sentiment shock. By imposing this identification order, we clean our uncertainty shock of first-order ('confidence' or 'bad news') contemporaneous contamination effects. The results from this experiment are presented in Appendix D, Figure D.3 and show that our conclusions are, for all practical purposes, unchanged.

6 Conclusions

This paper provides novel causal evidence on the short-run effects of unexpected news and announcements about the pandemic, *i.e.* a COVID-19-induced shock. We analyse a set of daily economic and financial variables within a VAR on US data, over the sample January-October 2020. We find that a COVID-19-induced shock has large contractionary effects on key economic indicators such as employment, spending and business revenues, as well as standard financial indicators, such as the S&P 500 index, uncertainty and credit spreads. We also provide evidence of important distributional effects. Employment appears to be decreasing more in poor areas while the opposite is true for private spending. Crucially, we find that exposure to COVID-19-induced shocks is highly heterogeneous at the sectoral level whereupon those industries that rely heavily on face-to-face interactions, such as entertainment and hospitality, see a reduction in their revenues over two times larger than those industries which can conduct businesses remotely, such as business services.

Furthermore, using two identification schemes (Cholesky and Sign-Restriction), we show that our statistically identified COVID-19-induced shock can be interpreted as a structural uncertainty shock. Our interpretation holds both for aggregate financial and economic indicators as well as for distributional ones.

We believe there are several interesting avenues for future research. First, as more daily data become available, one could expand the analysis presented here, for example on international trade. Another interesting avenue of research is to understand if a COVID-19-induced shock has asymmetric effects with 'good' as opposed to 'bad' COVID-19 news and announcements, or during the second wave of the pandemic. Finally it would be interesting to analyse more deeply the distributional effects of a COVID-19-induced shock, with particular focus on precautionary savings and portfolio rebalancing and their relation with the response of earnings and expenditure.

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A The Econometric Methodology

The Bayesian Algorithm. The baseline model is defined as:

$$Y_t = X_t\beta + \mu_t \tag{A.1}$$

where Y_t is $1 \times N$ matrix of endogenous variables, $\underbrace{X_t}_{1\times(NP+1)} = [X_{t-1}, .., X_{t-P}, 1]$ denotes the regressors in each equation and β is a $(NP+1) \times N$ matrix of coefficients. The error term is heteroscedastic:

$$\mu_t \tilde{} N\left(0, \Sigma_H\right) \text{ periods of CIU events}$$
$$\mu_t \tilde{} N\left(0, \Sigma_L\right) \text{ all other periods}$$

We use a natural conjugate prior for the VAR parameters implemented via dummy observations,see Bańbura et al. (2010):

$$Y_{D,1} = \begin{pmatrix} \frac{diag(\gamma_1\sigma_1...\gamma_N\sigma_N)}{\tau} \\ 0_{N\times(P-1)\times N} \\ .... \\ diag\left(\sigma_1...\sigma_N\right) \\ .... \\ 0_{1\times N} \end{pmatrix}, and\ X_{D,1} = \begin{pmatrix} \frac{J_P \otimes diag(\sigma_1...\sigma_N)}{\tau} & 0_{NP\times 1} \\ 0_{N\times NP+1} & \\ .... & \\ 0_{1\times NP} & I_1 \times c \end{pmatrix} \tag{A.2}$$

where γ_1 to γ_N denote the prior mean for the coefficients on the first lag, τ is the tightness of the prior on the VAR coefficients and c is the tightness of the prior on the constant. In our application, the prior means are chosen as the OLS estimates of the coefficients of an AR(1) regression estimated for each endogenous variable. We set $\tau = 0.1$. The scaling factors σ_i are set using the standard deviation of the error terms from these preliminary AR(1) regressions. Finally we set $c = 1/10000$ in our implementation indicating a flat prior on the constant. We also introduce a prior on the sum of the lagged dependent variables by adding the following dummy observations:

$$Y_{D,2} = \frac{diag\left(\gamma_1 \mu_1 ... \gamma_N \mu_N\right)}{\lambda},\ X_{D,2} = \left(\begin{array}{cc} \frac{(1_{1\times P}) \otimes diag(\gamma_1 \mu_1 ... \gamma_N \mu_N)}{\lambda} & 0_{N\times 1} \end{array} \right) \tag{A.3}$$

where μ_i denotes the sample means of the endogenous variables calculated using AR(1) preliminary regressions. We set a loose prior of $\lambda = 10000\tau$.

The baseline VAR model is estimated via Gibbs sampling. Conditional on Σ_H and Σ_N, the posterior distribution of $b = vec\left(\beta\right)$ is normal with mean M^* and variance V^* where

$$V^* = \left(\sum_{t=1}^{T} \left(R_t^{-1} \otimes X_t X_t' \right) + S_0^{-1} \right)^{-1} \tag{A.4}$$

$$M^* = V^* \left(vec\left(\sum_{t=1}^{T} \left(X_t Y_t' R_t^{-1} \right) \right) + S_0^{-1} \tilde{\beta}_0' \right) \tag{A.5}$$

where $R_t = \Sigma_H$ over periods characterized by the financial shock and $R_t = \Sigma_N$, otherwise. The prior for the VAR coefficients based on dummy observations is $N\left(\tilde{B}_0, S_0\right)$. Conditional on a draw for β, the conditional posterior for $\Sigma_i, i = 0, 1$ is inverse Wishart: $IW\left(\mu_i' \mu_i + s_0, T + t_0\right)$ where μ_i denotes the residuals associated with period of CIU shock when $i = 1$ and all other periods when $i = 0$. The prior for the VAR error covariance implied by the dummy observations is $IW\left(s_0, t_0\right)$.

The estimation of the three identification approaches in Section 5 relies as well on Bayesian techniques. Specifically, for the PF and Choleski approaches we follow Caldara et al. (2016) and impose Minnesota priors choosing optimally the hyperparameters that maximize the marginal data density. In the sign restriction scheme we impose a standard tightness of the Minnesota prior of 0.1 as in the statistical approach. The lag is set to 10 in all specifications.

B Description of the Data

1. **Daily Financial Data**

   - the S&P500 index at daily frequency, transformed in logs. FRED link https://fred.stlouisfed.org/series/SP500
   - the VIX index at daily frequency, transformed in logs. FRED link https://fred.stlouisfed.org/series/VIXCLS.
   - the DGS1 index is the 1-year Treasury Constant Maturity Rate, FRED link https://fred.stlouisfed.org/series/DGS1
   - the MSCI world index is a cap-weighted world stock market index, FRED link https://uk.finance.yahoo.com/quote/MSCI/history?p=MSCI
   - the TED spread is the difference between the three month Treasury bill and the three-month LIBOR based index, FRED link https://fred.stlouisfed.org/series/TEDRATE
   - the BAA Spread isMoody's Seasoned Baa Corporate Bond Yield Relative to Yield on 10-Year Treasury Constant Maturity, FRED link https://fred.stlouisfed.org/series/BAA10Y
   - the EPU index is the economic policy uncertainty index by Baker et al. (2016), transformed in logs. Link https://www.policyuncertainty.com

2. **Daily Economic indicator**, tracktherecovery.org

   (a) Spending data from Affinity Solutions: Aggregated and anonymized purchase data from consumer credit and debit card spending. Spending is reported based on the ZIP code where the cardholder lives, not the ZIP code where transactions occurred.

      - Aggregate Spending: Seasonally adjusted credit/debit card spending relative to January 4-31 2020 in all merchant category codes, 7 day moving average.

- Spending in ‘accomodation and food service’ category: Seasonally adjusted credit/debit card spending relative to January 4-31 2020 in accomodation and food service (ACF) MCCs, 7 day moving average, 7 day moving average.
- Spending in arts et cetera: Seasonally adjusted credit/debit card spending relative to January 4-31 2020 in arts, entertainment, and recreation (AER) MCCs, 7 day moving average.
- Spending in general merchandising stores: Seasonally adjusted credit/debit card spending relative to January 4-31 2020 in general merchandise stores (GEN) and apparel and accessories (AAP) MCCs, 7 day moving average.
- Spending in grocery and food store: Seasonally adjusted credit/debit card spending relative to January 4-31 2020 in grocery and food store (GRF) MCCs, 7 day moving average.
- Spending in health care services: Seasonally adjusted credit/debit card spending relative to January 4-31 2020 in health care and social assistance (HCS) MCCs, 7 day moving average.
- Spending in transportation and warehousing: Seasonally adjusted credit/debit card spending relative to January 4-31 2020 in transportation and warehousing (TWS) MCCs, 7 day moving average.
- Spending high income households: Seasonally adjusted credit/debit card spending by consumers living in ZIP codes with high (top quartile) median income, relative to January 4-31 2020 in all merchant category codes (MCC), 7 day moving average.
- Spending middle income households: Seasonally adjusted credit/debit card spending by consumers living in ZIP codes with middle (middle two quartiles) median income, relative to January 4-31 2020 in all merchant cat-

egory codes (MCC), 7 day moving average.

- Spending low income households: Seasonally adjusted credit/debit card spending by consumers living in ZIP codes with low (bottom quartiles) median income, relative to January 4-31 2020 in all merchant category codes (MCC), 7 day moving average.

(b) Employment data from Paychex, Intuit, Earnin and Kronos: Number of active employees, aggregating information from multiple data providers. This series is based on firm-level payroll data from Paychex and Intuit, worker-level data on employment and earnings from Earnin, and firm-level timesheet data from Kronos. All data is daily, presented as a 7-day moving average, as percent deviation relative to January 4-31 2020.

- Aggregate employmeny: Employment level for all workers.
- Empoyment low income households: Employment level for workers in the bottom quartile of the income distribution (incomes approximately under $ 27,000).
- Empoyment middle income households: Employment level for workers in the middle two quartiles of the income distribution (incomes approximately $27,000 to $60,000).
- Empoyment high income households: Employment level for workers in the top quartile of the income distribution (incomes approximately over $60,000).
- Employmeny NAICS supersector 40: Employment level for workers in trade, transportation and utilities.
- Employmeny NAICS supersector 60: Employment level for workers in professional and business services .
- Employmeny NAICS supersector 65: Employment level for workers in education and health services.

- Employmeny NAICS supersector 70: Employment level for workers in leisure and hospitality.

(c) Small business openings and revenue data from Womply. Small business transactions and revenue data aggregated from several credit card processors. Transactions and revenue are reported based on the ZIP code where the business is located. Number of small businesses open, as defined by having had at least one transaction in the previous 3 days.

- Small business openings, aggregate: Percent change in number of small businesses open calculated as a seven-day moving average seasonally adjusted and indexed to January 4-31 2020.
- Small business openings, high income areas: Percent change in number of small businesses open calculated as a seven-day moving average seasonally adjusted and indexed to January 4-31 2020 in high income (quartile 4 of median income) ZIP codes.
- Small business openings, middle income areas: Percent change in number of small businesses open calculated as a seven-day moving average seasonally adjusted and indexed to January 4-31 2020 in middle income (quartiles 2 3 of median income) ZIP codes.
- Small business openings, low income areas: Percent change in number of small businesses open calculated as a seven-day moving average seasonally adjusted and indexed to January 4-31 2020 in low income (quartile 1 of median income) ZIP codes.
- Small business openings, NAICS supersector 40: Percent change in number of small businesses open calculated as a seven-day moving average seasonally adjusted and indexed to January 4-31 2020 in transportation.
- Small business openings, NAICS supersector 60: Percent change in number of small businesses open calculated as a seven-day moving average

seasonally adjusted and indexed to January 4-31 2020 in professional and business services.

- Small business openings, NAICS supersector 65: Percent change in number of small businesses open calculated as a seven-day moving average seasonally adjusted and indexed to January 4-31 2020 in education and health services .
- Small business openings, NAICS supersector 70: Percent change in number of small businesses open calculated as a seven-day moving average seasonally adjusted and indexed to January 4-31 2020 in leisure and hospitality.
- Small business revenues, aggregate: percent change in net revenue for small businesses, calculated as a seven-day moving average, seasonally adjusted, and indexed to January 4-31 2020.
- Small business revenues, high income areas: Percent change in net revenue for small businesses, calculated as a seven-day moving average, seasonally adjusted, and indexed to January 4-31 2020 in high income (quartile 4 of median income) zipcodes.
- Small business revenues, middle income areas: Percent change in net revenue for small businesses, calculated as a seven-day moving average, seasonally adjusted, and indexed to January 4-31 2020 in middle income (quartiles 2 3 of median income) zipcodes.
- Small business revenues, low income areas: Percent change in net revenue for small businesses, calculated as a seven-day moving average, seasonally adjusted, and indexed to January 4-31 2020 in low income (quartile 1 of median income) zipcodes.
- Small business revenues, NAICS supersector 40: Percent change in net revenue for small businesses, calculated as a seven-day moving average, sea-

sonally adjusted, and indexed to January 4-31 2020 in transportation.

- Small business revenues, NAICS supersector 60: Percent change in net revenue for small businesses, calculated as a seven-day moving average, seasonally adjusted, and indexed to January 4-31 2020 in professional and business services.
- Small business revenues, NAICS supersector 65:: Percent change in net revenue for small businesses, calculated as a seven-day moving average, seasonally adjusted, and indexed to January 4-31 2020 in education and health services.
- Small business revenues, NAICS supersector 70: Percent change in net revenue for small businesses, calculated as a seven-day moving average, seasonally adjusted, and indexed to January 4-31 2020 in leisure and hospitality.

3. **Other Series**

- Sentiment index is the Daily News Sentiment Index, a high frequency measure of economic sentiment based on lexical analysis of economics-related news articles, see Shapiro et al. (2020), link `https://www.frbsf.org/daily-news-sentiment-index/`.
- Stock market jumps is a collection of events about jumps of the S&P 500 index as reported and descripbed by Baker et al. (2020), link `https://www.stockmarketjumps.com/data/`.

C Tables and Figures, COVID-19-Induced Shock

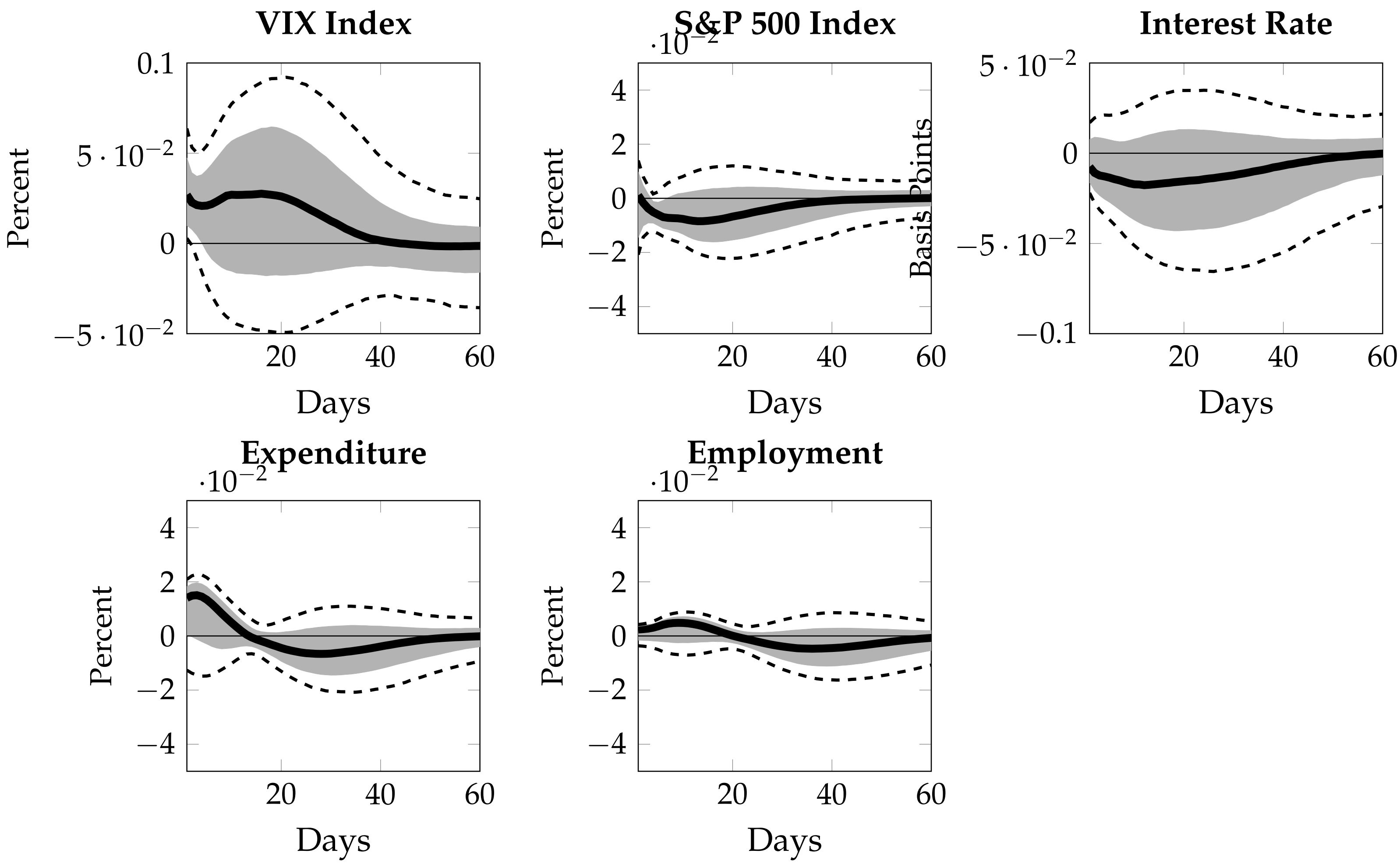

Figure C.1 – IRFs to a COVID-19-induced shock, random event dates. Solid black lines represent the median while shaded areas and broken lines represent the 68 and 90 percent credibility sets, respectively.

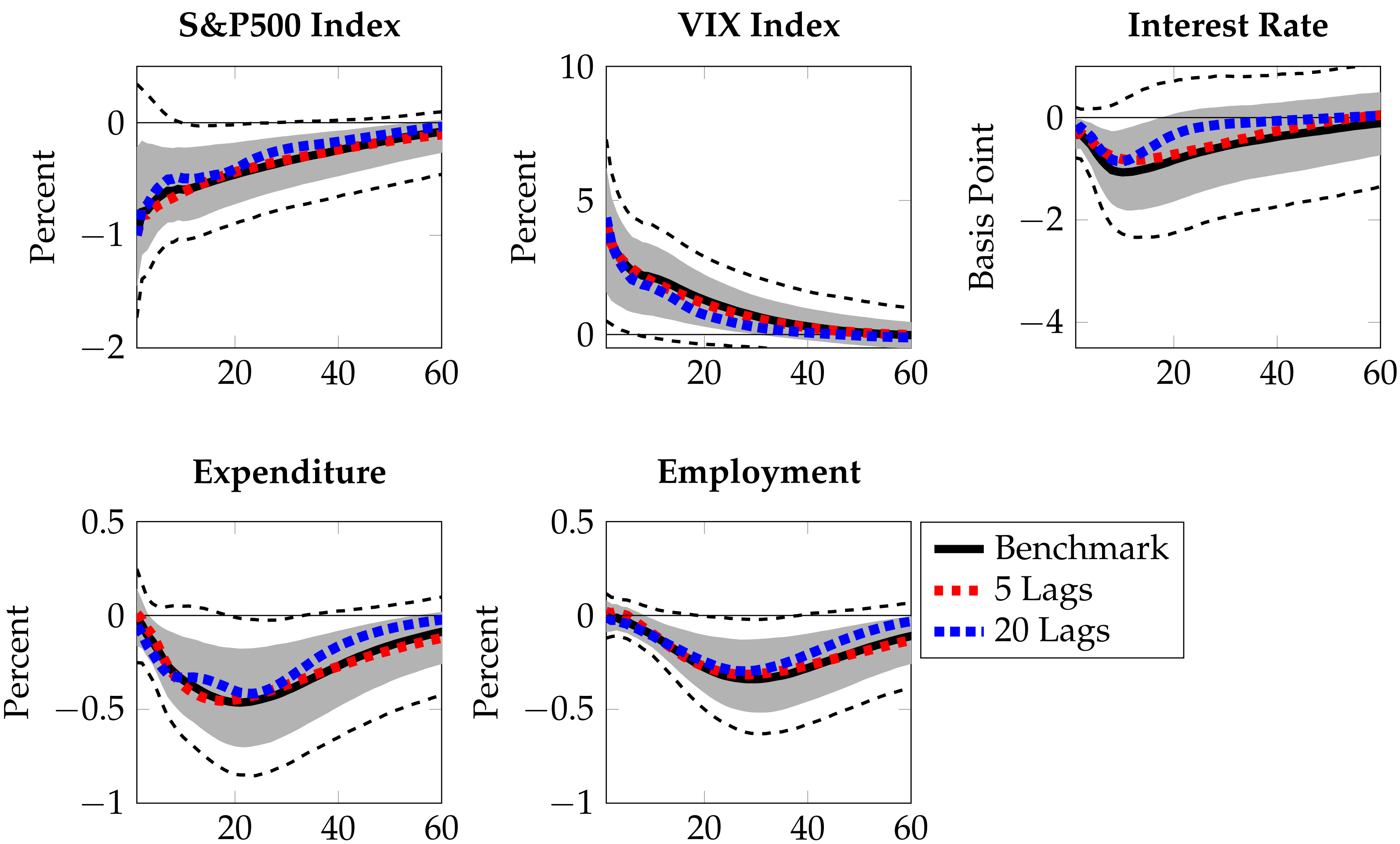

Figure C.2 – IRFs to a COVID-19-induced shock lowering S&P 500 by 1 percent. Different lag structure. Solid black line, median. Red dashed line, median 5 lags. Blue dashed line, median 21 lags. Shaded areas and dotted lines are the 68 and 90 credibility sets of the benchmark, respectively.

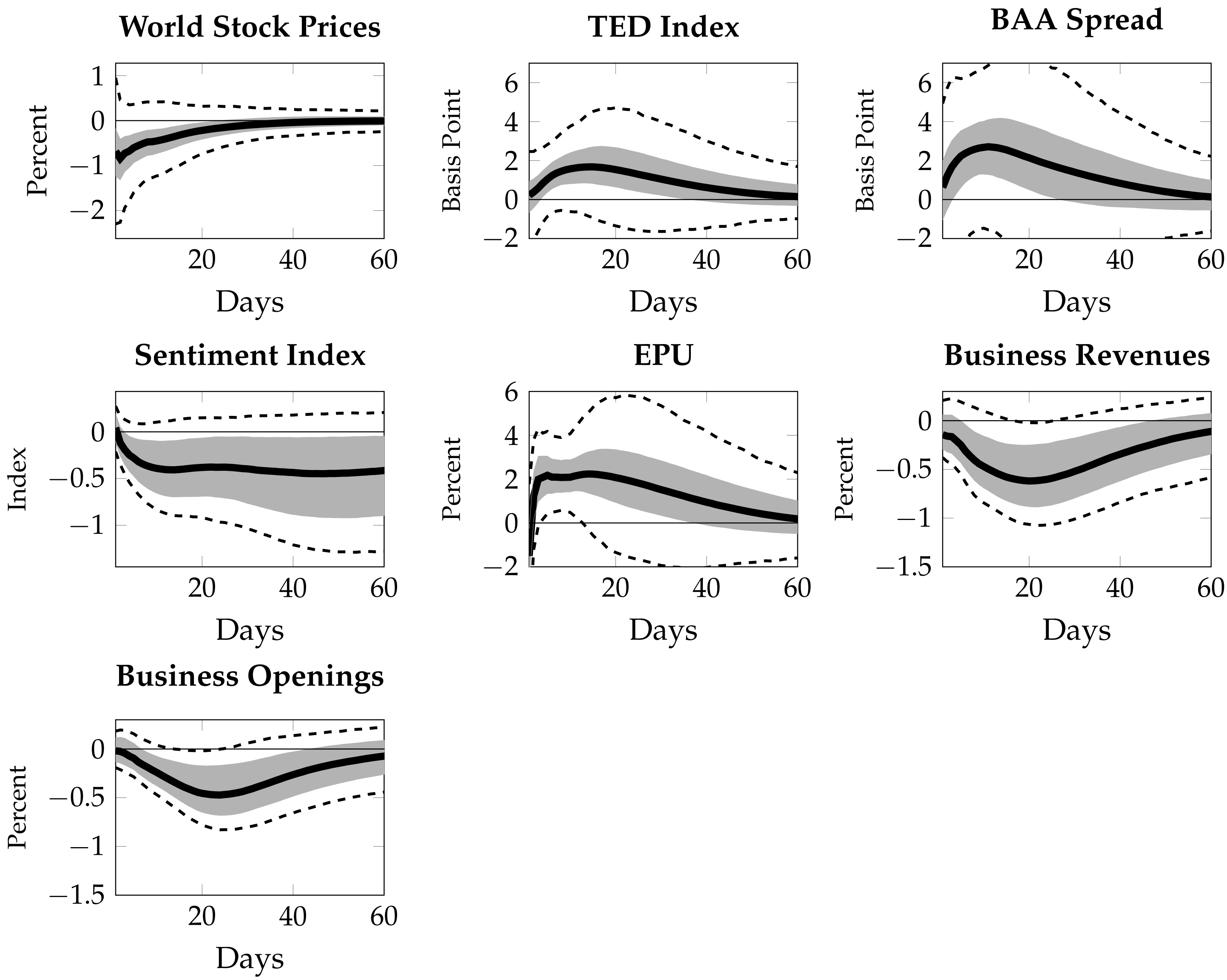

Figure C.3 – IRFs to a COVID-19-induced shock that lowers the S&P 500 Index by 1 pc. Black solid lines represent the median response. Shaded areas and dashed-lines represent the 68 and 90 percent credibility interval, respectively.

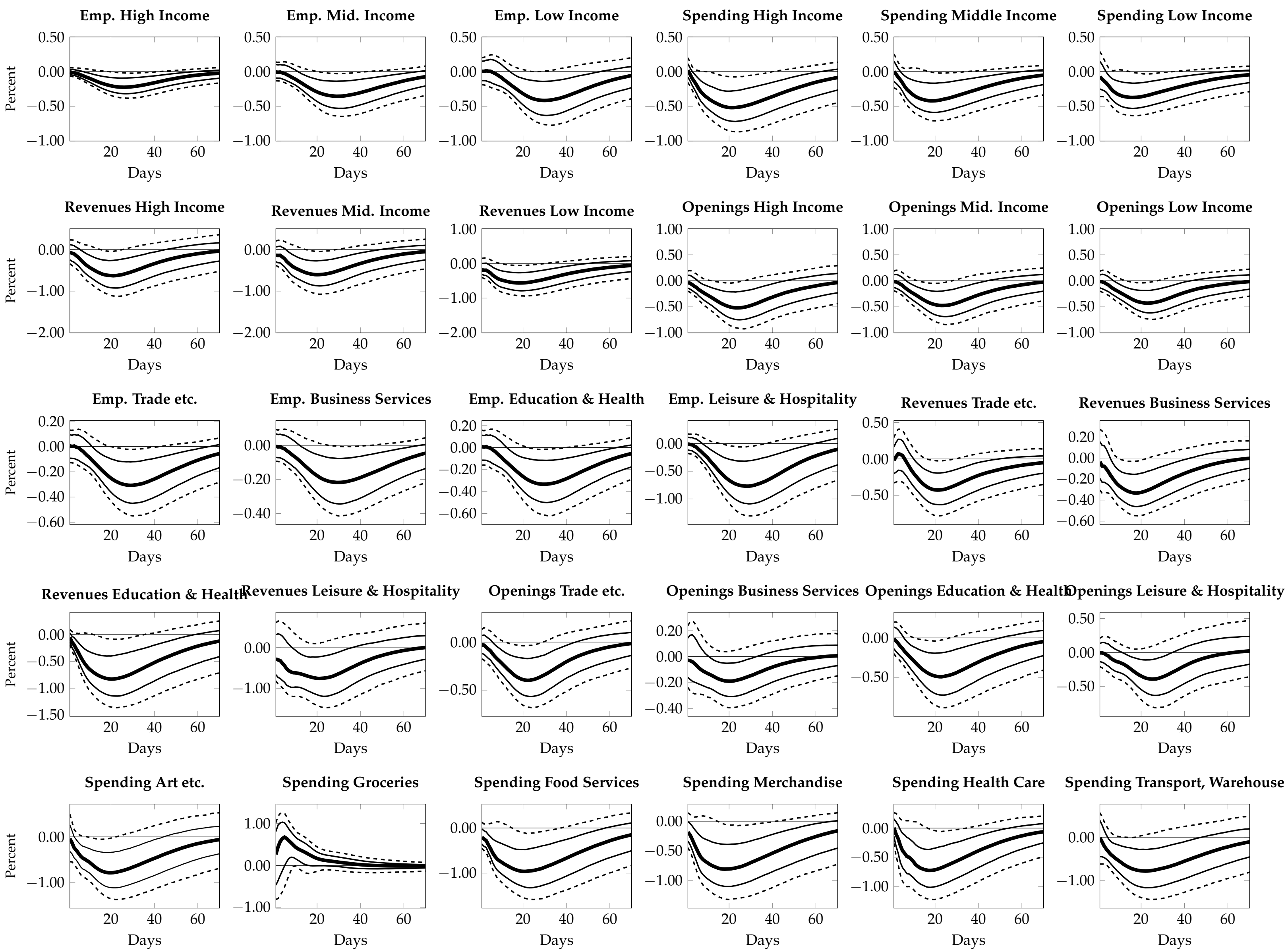

Figure C.4 – IRFs to a COVID-19-induced shock that increases reduces S&P500 by 1 percent. Solid and dashed lines represent the 68 and 90 percent credibility interval, respectively.

D Tables and Figures, Uncertainty Shock

Table C.1 – Peak Effects on Distributional and Sectoral Variables, Various Models. Asterisks * and ** mean 68 and 90 percent significance, respectively.

Part A: Distribution

Variable	Peak Effect *baseline*	Peak Effect *with feedbacks*	Period (in weeks)
Employment, Aggregate	-0.34**	-0.34**	6
Employment, High Income	-0.23**	-0.17*	5
Employment, Mid Income	-0.34**	-0.28*	6
Employment, Low Income	-0.40**	-0.35*	6
Expenditure, Aggregate	-0.47**	-0.47**	5
Expenditure, High Income	-0.58**	-0.53*	5
Expenditure, Mid Income	-0.45**	-0.43*	5
Expenditure, Low Income	-0.39*	-0.38*	6
Small Business Revenue, Aggregate	-0.63**	-0.63**	5
Small Business Revenue, High Income	-0.64**	-0.56*	5
Small Business Revenue, Mid Income	-0.63**	-0.48*	5
Small Business Revenue, Low Income	-0.64**	-0.42*	4
Small Business Openings, Aggregate	-0.49**	-0.49**	6
Small Business Openings, High Income	-0.53**	-0.48*	6
Small Business Openings, Mid Income	-0.49**	-0.43*	6
Small Business Openings, Low Income	-0.44**	-0.38*	6

Part B: Sectors

Variable	Peak Effect *baseline*	Peak Effect *wit feedbacks*	Period (in weeks)
Employment,Trade, Transportation and Utilities	-0.31**	-0.30*	6
Employment, Professional and Business Services	-0.20**	-0.19*	6
Employment, Education and Health Services	-0.31**	-0.30*	6
Employment, Leisure and Hospitality	-0.82**	-0.78*	6
Revenues, Trade, Transportation and Utilities	-0.48**	-0.39*	6
Revenues, Professional and Business Services	-0.36**	-0.32*	5
Revenues, Education and Health Services	-0.94**	-0.81*	6
Revenues, Leisure and Hospitality	-0.72**	-0.77*	5
Business Openings,Trade, Transportation and Utilities	-0.43*	-0.33	5
Business Openings, Professional and Business Services	-0.18*	-0.16*	2
Business Openings, Education and Health Services	-0.50**	-0.43	5
Business Openings, Leisure and Hospitality	-0.39*	-0.35*	5

Part C: Expenditure Categories

Variable	Peak Effect *baseline*	Peak Effect *with feedbacks*	Period (in weeks)
Accommodation and Food Service	-1.78**	-0.70**	5
Arts, Entertainment, and Recreation	-0.87**	-0.49	5
General Merchandise Stores	-1.48**	-1.12**	5
Grocery and Food Store	0.75*	0.54*	1
Health Care and Social Assistance	-0.91**	-0.68*	5
Transportation and Warehousing	-0.79*	-0.52*	5

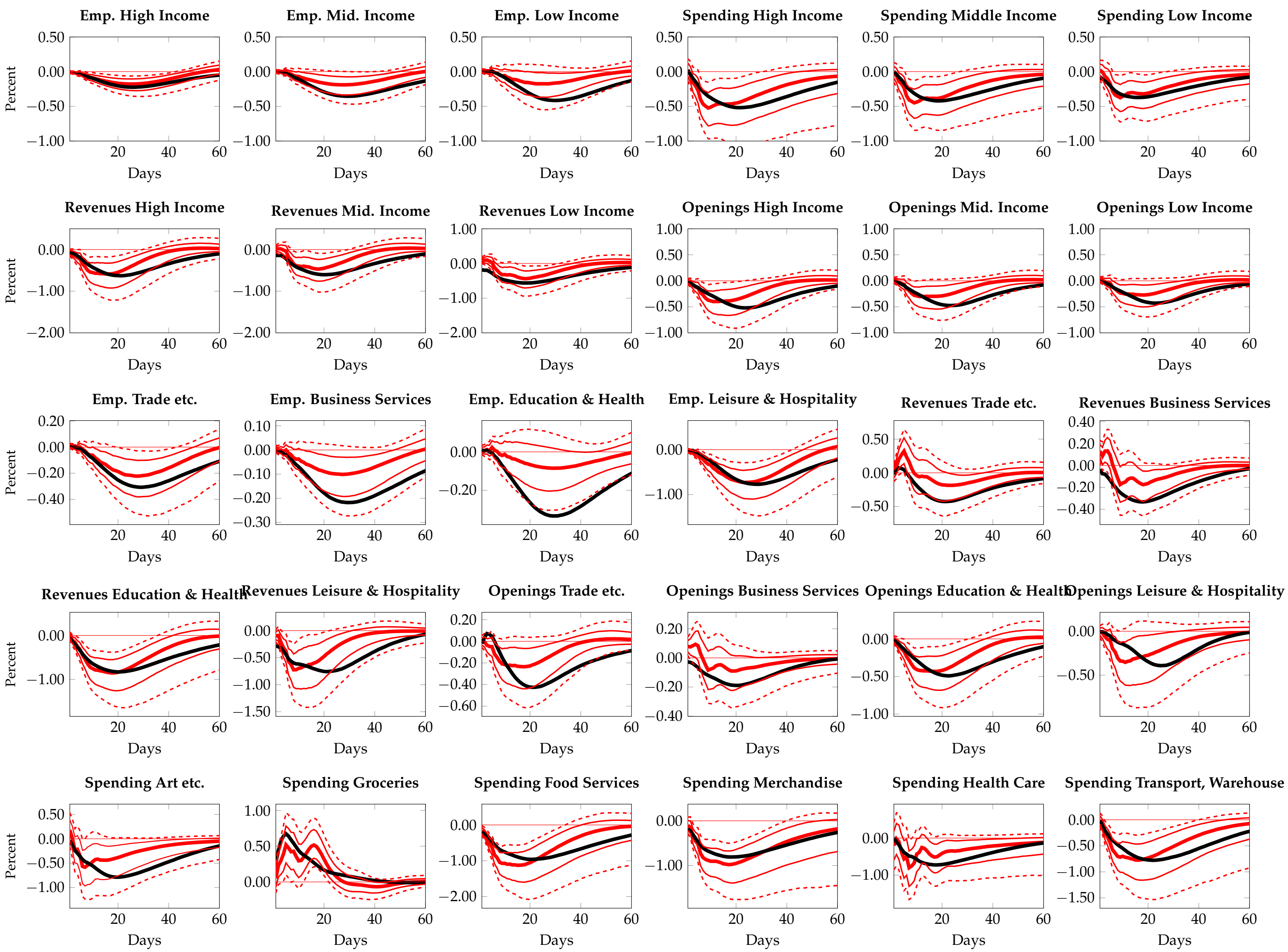

Figure D.1 – IRFs to a Uncertainty Shock that reduces S&P500 by 1 percent, Cholesky identification. Red solid and dashed lines represent the 68 and 90 percent credibility interval, respectively. Black thick lines represent the posterior median response from the statistical models, see Figure C.4.

Figure D.2 – IRFs to an uncertainty shock lowering S&P 500 by 1 percent, Cholesky, Sign-Restrictions and benchmark identifications. Solid red lines identify the median for the Cholesky identification, while shaded areas and broken lines represent the 68 and 90 percent credibility set from the same scheme, respectively.

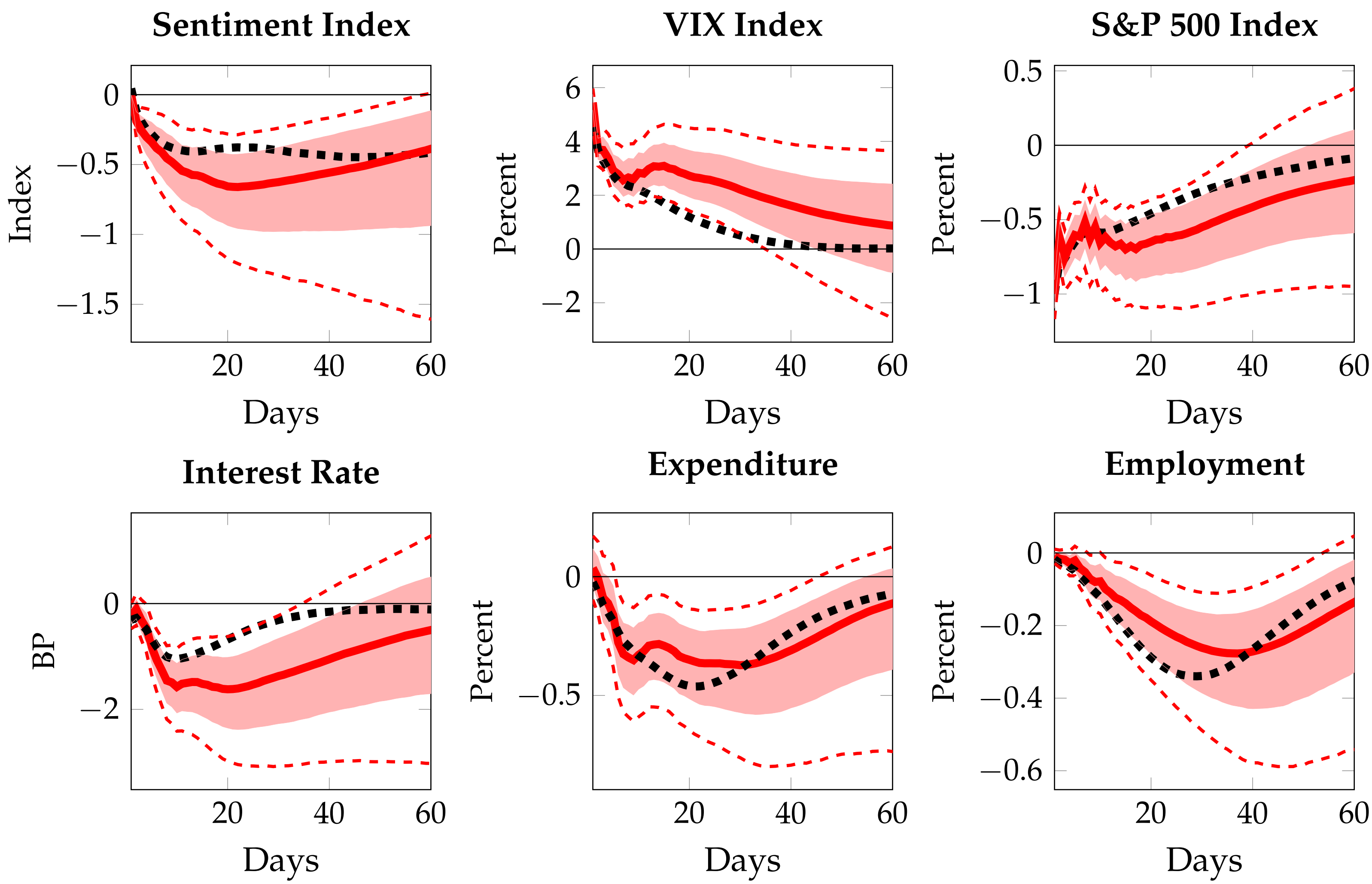

Figure D.3 – IRFs to an uncertainty shock lowering S&P 500 by 1 percent, ncertainty shock sequentially ordered after sentiment shock, Cholesky Identification. Red solid lines identify the median while shaded areas and broken lines represent the 68 and 90 percent credibility sets, respectively. Thick, dashed black line, median of the benchmark statistical identification, extended model, i.e. Baseline plus the Sentiment Index.