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Pandemic Shocks and Household Spending

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Abstract

We study the response of daily household spending to the *unexpected* component of the COVID-19 pandemic, which we label as pandemic shock. Based on daily forecasts of the number of fatalities, we construct the surprise component as the difference between the actual and the expected number of deaths. We allow for state-dependent effects of the shock depending on the position on the curve of infections. Spending falls after the shock and is particularly sensitive to the shock when the number of new infections is strongly increasing. If the number of infections grows moderately, the drop in spending is smaller. We also estimate the effect of the shock across income quartiles. In each state, low-income households exhibit a significantly larger drop in consumption than high-income households. Thus, consumption inequality increase after a pandemic shock. Our results hold for the US economy and the key US states. The findings remain unchanged if we choose alternative state-variables to separate regimes.

Keywords: COVID-19, pandemic, consumption, smooth-transition model, state-dependence

JEL classification: E21, E32, I10

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I Introduction

The global spread of the COVID-19 pandemic since January 2020 led to a sharp contraction of economic activity in almost all economies affected by the virus. Between January and April, real personal consumption expenditures declined by more than 15%. With personal consumption expenditures accounting for 68% of US GDP in 2019, this decline in spending casts shadow on overall economic activity in 2020. Consumption recovered in May and June, partly driven by government transfers which led to an increase in real disposable income.

In this paper, we provide an analysis of the causal effect of the pandemic on household spending. Spending, like consumption in general, should mostly be driven by unexpected shocks. According to the theory of permanent income, predictable fluctuations in future income should prompt households to tap the capital market and smooth consumption, such that consumption exhibits very little fluctuations.[1] Initially, the spread of the pandemic might be considered unpredictable. After that, and in particular with the beginning of the second wave of infections in June, however, a large part of the development should have been predictable.

We look at the unexpected element of the pandemic and analyze how it affects spending decisions. We draw on forecasts of the number of fatalities due to COVID-19 in the US provided by Gu (2020) and contrast the one day-ahead forecast with the actual number of deaths. A positive forecast error is consistent with an under-prediction of the number of fatalities or a surprise in the severity of the pandemic, respectively. We refer to this series of unexpected deaths as a pandemic shock and use it as the key explanatory variable for household spending.

Our measure of household spending is provided by Chetty et al. (2020) and consists of debit and credit card transactions in the US. The key advantage of the data is timeliness. We can track spending on a daily frequency for the entire US economy as well as for US states. In a series of local projections, see Jordà (2005), we estimate the response of spending to a pandemic shock.

There are at least three channels through which a pandemic shock can affect spending. First, an adverse pandemic shock could prompt households to restrain consumption voluntarily. This is because the virus spreads through

[1] See Jappelli and Pistaferri (2010) for a survey of the field.

social interaction such as shopping in retail stores, dining or entertainment. Anxious households could reduce these activities even before official lockdown measures are in place.[2] Second, households might be barred from consumption due to a lockdown of selected activities or even shelter-in-place orders. An adverse pandemic shock makes these measures more likely. Third, households could perceive an unexpected change in future income and adjust their spending accordingly. Even if a household is not itself affected by the virus, the future of entire industries is at risk. Workers in the service sector, for example, cannot resort to working from home and experience a large drop in future income.[3]

While we cannot disentangle these transmission channels, we take account of an important property that all three channels have in common: the effect of a pandemic shock should be stronger if the virus spreads more rapidly. The more widespread the virus is, the larger the reluctance to shop offline, the more likely stricter lockdown measures and the more severe the drop in future income will be. Thus, the effect of the pandemic shock should depend on the position of the economy on the infection curve.

Therefore, we generalize our model and allow the pandemic shock to have regime-dependent effects. In our baseline setting, we chose the growth of the daily number of new infections as our state variable. This figure is omnipresent, especially in the media, and provides information on where the economy stands on the infection curve. The transition between states is driven by either a non-parametric model introduced by Born et al. (2020) or a parametric approach proposed by Auerbach and Gorodnichenko (2012).

We show that a pandemic shock originating when the number of new infections is growing fast has a strongly negative effect on spending. We find a significant drop of about 1% in spending after a pandemic shock of one standard deviation. The drop in consumption is consistent with recent macroeconomic models of the effect of income expectations and uncertainty on consumption, see Dietrich et al. (2020), or the feedback between the spread of the pandemic and macroeconomic aggregates, e.g. Eichenbaum et al. (2020). The pandemic shock explains more than 20% of fluctuations

[2] Goolsbee and Syverson (2020) show that consumer behavior during the pandemic is more driven by fear of infection than formal restrictions.

[3] In a survey conducted early in the pandemic, Binder (2020) finds that households expect an increase in unemployment due to the pandemic.

in spending.

If the shock occurs in a situation in which the virus spreads less rapidly, spending drops by 0.5% only with the peak response occurring after one week. Throughout the paper, we find that the nexus between spending and the pandemic shock is strongly depending on the underlying regime. In almost all cases, we can reject the null hypothesis of equal spending responses across regimes. We estimate the model not only for the whole US economy, but also for the 10 largest US states. Across all states, the regime-dependent sensitivity of spending to pandemic shocks is very similar. The results remain unchanged if we use alternative state variables such as the level of new infections rather than the growth rate of infections.

We also study the spending response across income quartiles. We use spending data for residents of ZIP codes with low, middle and high median household income. This allows us to estimate the response of household spending across income groups to a pandemic shock. The first two of the three transmission channels discussed before, voluntary and forced consumption restraint, should apply equally to high-and low-income households.

The third, however, should imply that low-income households reduce their spending by more compared to high-income households. This is because the drop in lifetime income should be particularly pronounced for low-productivity workers, e.g. workers in the service sector.[4] We do indeed find that in the regime with a strong growth of the number of infections, high-income households reduce their spending by 0.5%, while low-income households cut expenditures by 1%. This is remarkable because the initial fall in spending was larger for high-income households as documented by Chetty et al. (2020). Our results suggest that the economic burden of the pandemic in terms of consumption falls more on low-income households.[5] The difference in spending responses is highly statistically significant in both

[4] Even if the drop in income were equal across income groups, we expect the marginal propensity to consume (MPC) to be higher for low-income quartiles. In fact, Karger and Rajan (2020) track spending of recipients of governmental transfer payments during the COVID-19 pandemic. They find an MPC of 0.68 for hand-to-mouth consumers and 0.23 for savers.

[5] See Mongey et al. (2020) for an analysis of the effect of social distancing across workers. They find significant differences in the burden from social distancing.

regimes. Thus, the pandemic contributes to a growing of consumption inequality.

This paper contributes to the recent work on household behavior based on innovative datasets. In an early paper, Baker et al. (2020) use transaction-level data for the US in order to document the changes in consumption patterns after the outbreak of the coronavirus. Cox et al. (2020) extend this line of research and sheds light on the response of consumption and saving across the income distribution. Using transaction-level data from the largest Danish bank, Andersen et al. (2020) show that the decline in spending increases in the exposure of households to the economic consequences of the pandemic. Surico et al. (2020) use data from a fintech company based in the UK to track the behavior of spending. These authors also document the build-up of financial stress as well as consumption and income inequality across households. Carvalho et al. (2020) use six billion transactions of customers of Spain's second-largest bank to track consumption over the crisis. Coibion et al. (2020) estimate the effect of lockdowns on spending and household expectations based on survey data. They make use of the asynchronous timing of lockdown measures in order to identify a causal effect. The occurrence of the first corona infection is used to instrument local lockdown restrictions. They find that lockdown restrictions explain most of the fall in consumer spending since March 2020.[6]

Most of these papers, with the exception of Coibion et al. (2020), provide descriptive evidence based on massive new datasets or estimate the response of spending to observable events. Instead, we aim at estimating the sensitivity of spending to unexpected changes in the severity of the pandemic.

The paper is organized as follows. Section 2 presents the data and discusses the derivation of our pandemic shock. Section 3 lays out our estimation strategy. Our results are discussed in section 4. Section 5 presents results for alternative state variables. Section 6 concludes.

[6] Alexander and Karger (2020) analyze consumer spending and cellphone records in the US and show the causal effect of stay-at-home orders on spending.

II Data

To investigate the response of consumption to the unexpected spread of the pandemic, we rely on two data sets. The first contains information on daily household spending since the outbreak of the coronavirus and the second reports daily historical forecasts on the number of fatalities due to COVID-19.

A. *Household spending*

Throughout the paper, the dependent variable is a measure of household spending. We have daily observations ranging from April 3 up to July 26. We use the series provided by Chetty et al. (2020), which are open source and available at `https://tracktherecovery.org`.[7]

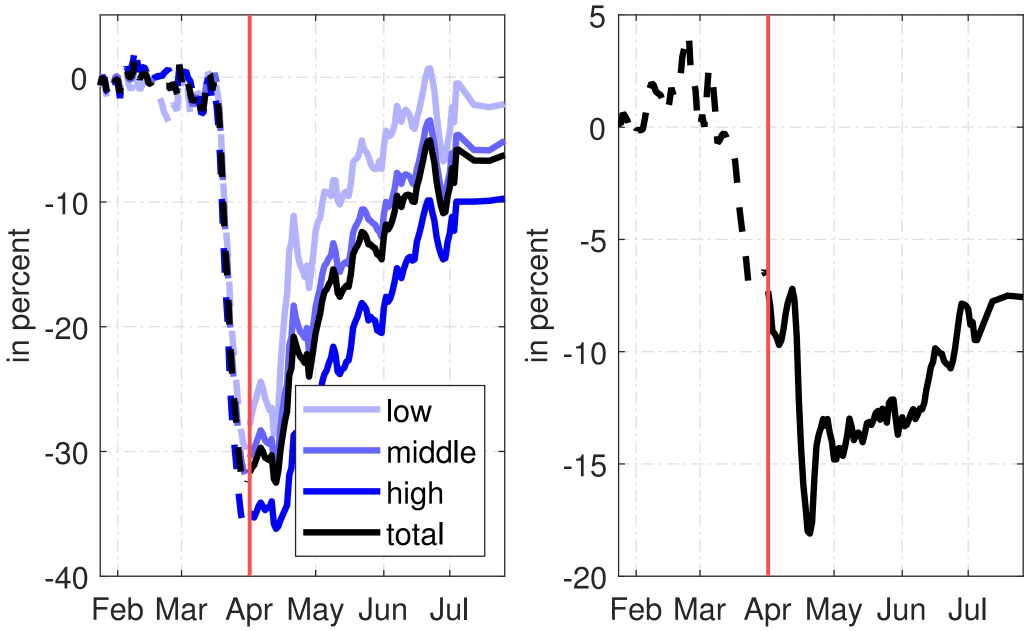
We also have spending broken down into ZIP codes with high, middle and low median income. Below, we will refer these subgroups of households as high and low income households, although we do not have information on household income, only on median income of the ZIP code of residency.

Because the original data on spending exhibits substantial periodic fluctuations across days, the publicly available series are 7-day moving averages in order to smooth daily fluctuations. Furthermore, data on consumer spending exhibits strong weekly fluctuations which are autocorrelated across years. To account for this, Chetty et al. (2020) divide all spending series by its corresponding value from 2019. Lastly, the seasonally adjusted data are indexed to its pre-pandemic level, namely the mean of the 7-day moving average from January 8-28.[8] Hence, our series are given in percent, such that a value of two percent in t corresponds to an increase of spending by two percent relative to its average value in January.

[7] Chetty et al. (2020) collect the data on spending from Affinity Solutions Inc. This company aggregates information on credit and debit card spending. The data is available for nation-wide aggregates as well as for each US state.

[8] It should be mentioned that the spending data for the entire sample is available on a daily basis. Since July 5th, however, it is based on the average of the last 7 days. More precisely, the daily data available from July 6th onward is interpolated as line segments connecting the weekly data points. For more details on the construction of the data, see Chetty et al. (2020).

Figure 1: Total Spending and Spending by Income Quartile



Notes: The left panel shows the difference of actual spending relative to its level in January 2020 by customers living in ZIP codes with different income classes, namely high (top quartile) median income, middle (middle two quartiles) median income as well as low (bottom quartile) median income. The right panel shows the difference in spending of customers living in ZIP codes with high median income and customers living in ZIP codes with low median income. In both samples, the start of our estimation sample (April 3rd) is highlighted by the red vertical lines.

The left panel of Figure (1) shows that for all income households, spending fell sharply in mid-March, when the National Emergency was declared. In early April, spending fell by 36.4% for high-income households, 32% for low income households and 29.8% for middle income households. The right panel shows relative spending, i.e. the difference between spending of high and low income households. The reversion of spending to the pre-pandemic level differs remarkably, with the level of low-income households being almost back to the pre-pandemic level. Spending from high-income households fell more and recovered less - a finding that we need to keep in mind because below we show that the sensitivity of high-income households to pandemic shocks is actually smaller than that of low-income households.

B. *The surprise number of fatalities*

Consumption should respond to the unexpected severity of the pandemic. Hence, in order to investigate the consumption response, we need a series of the surprise component of the pandemic. We formulate the surprise in terms of the unexpected number of fatalities due to COVID-19, i.e. the difference between expected and realized deaths.

We retrieve daily real-time projections on deaths and the unrevised reported number of deaths due to COVID-19 in the US from Gu (2020). This data is open source and can be downloaded from www.covid19-projections.com. The author takes a (machine learning) data-driven approach rooted in epidemiology to forecast infections and deaths from the coronavirus epidemic in the US (and around the world).[9] These forecasts have been covered by almost all major US media outlets.

Importantly, we do not only have the latest forecast, but also the historical forecasts. The forecasts are updated on a daily basis. We use this data to derive a pandemic shock, i.e. the unexpected number of deaths due to COVID-19. To do so, denote $\mathbf{d}_{t|t-1}$ the forecast made in $t-1$ for deaths occurring in t. Thus, we focus on one day-ahead forecasts. Our pandemic shock is calculated as the difference between the actual outcome for t and the forecast number of deaths, that is

$$\mathbf{e}_t = \mathbf{d}_t - \mathbf{d}_{t|t-1}. \tag{1}$$

That is, our pandemic shock is the difference of today's number of reported deaths and yesterday's forecast for today. Notice that the number of reported deaths exhibits transient drops on weekends, typically followed by increases during the week. We therefore purify our shock by regressing the shock on a set of dummies for each day of the weak. Formally, we regress

$$\mathbf{e}_t = \gamma \mathbf{D}_t + \phi \mathbf{e}_{t-1} + \varepsilon_t. \tag{2}$$

Note that the estimated residuals for ε_t can be interpreted as the pandemic shock which cannot be explained neither by the set of daily dummies captured in $\mathbf{D}_t$ nor by yesterday's forecast error.[10]

Table (1) reports some descriptive statistics for both the raw shock $\mathbf{e}_t$ and

[9] Details on the forecasting model, including assumptions on the model parameters are available at https://covid19-projections.com/model-details.

[10] We checked whether other control variables have explanatory power, including the daily number of cases and a lag polynomial for up to seven lags. However, it turns out that the dynamics jointly explained by these variables is negligible. We therefore exclude them from our regression.

Table 1: Descriptive Statistics for Shocks

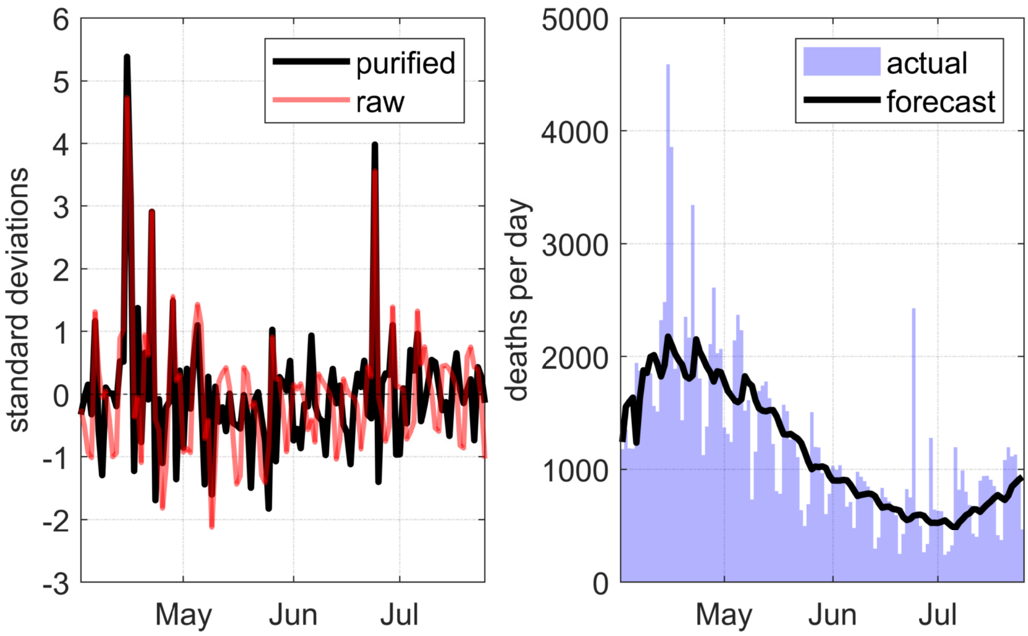
				RAW SHOCK			
MIN	MAX	MEAN	MEDIAN	5^{th}	95^{th}	Q-STAT.	$p-$VAL.
-1007	2416	52.86	63	-587.1	745.40	184.65	0.000
				PURIFIED SHOCK			
MIN	MAX	MEAN	MEDIAN	5^{th}	95^{th}	Q-STAT.	$p-$VAL.
-632.10	1865.36	0.00	-29.68	-468.59	465.68	10.61	0.717

Notes: Numbers are in deaths per day. Shocks are calculated based on unrevised real-time data. The last two columns report Q-statistics and $p-$values for a Ljung-Box test with the null hypothesis of zero autocorrelation up to 14 lags.

the purified shock ε_t. It is noteworthy that a large fraction of outliers can be explained by our daily dummies. This can be seen because for the purified shock, the 5^{th} and 95^{th} percentiles are much closer to zero than for the raw shock. Also for the minimum and maximum values of our shock, a notable fraction seems to be grounded on the seasonal patterns that is apparent in the reported number of deaths. The purified shock is almost iid and has no serial correlation for up to fourteen lags. Finally, in order to interpret our shock in terms of standard deviations, we subtract the mean and divide the series by the sample standard deviation.

Figure (2) shows the underlying data we use to derive the shock as well as our shock series. Starting with the right panel, the bars show the actual daily reported number of deaths over time. The black solid line corresponds to the one-step ahead forecasts. One can immediately recognize the seasonal pattern mentioned before. The reported number of deaths increased up to 2000 per day until the end of April and started to steadily decrease afterward, with daily deaths (on average) below 500 by the end of June. However, since early July, the number of daily deaths started to increase again. Interestingly, the forecasts follow the overall direction of the actual number of reported deaths with forecasts errors being either positive or negative equally likely. The left panel shows the raw and the purified shock series constructed as described above. While the raw series clearly exhibits seasonal patterns, the purified shock series now looks very much like an iid process. Moreover, especially from May onward, we can now see that a significant fraction of the swings disappears when taking seasonality into account.

Figure 2: Raw vs Purified Shock



Notes: The left panel shows the raw shock calculated as $\mathbf{e}_t = \mathbf{d}_t - \mathbf{d}_{t|t-1}$ as well as the shock after our purification procedure, i.e. ε_t. The right panel shows the reported number of deaths per day (in real-time) of people infected with the coronavirus (purple bars) as well as the real-time one-step-ahead forecasts.

III Methodology

We investigate the effects of pandemic shocks via local projections as proposed by Jordà (2005). Local projections provide a flexible framework and are easy to implement. Moreover, they offer a straightforward way to condition the short-run effects of pandemic shocks on the state of the pandemic.

A. *Setup*

The linear model of departure reads

$$y_{t+h} = \alpha_h + \beta_h \varepsilon_t + \delta_h t + \gamma_h \mathbf{x}_t + \varphi_h \mathbf{D}_t + u_{t+h}, \tag{3}$$

where y_{t+h} is the response of the dependent variable at time $t+h$ to a shock ε_t occurring in t. In our model, the dependent variable is household spending and ε_t is the pandemic shock introduced before. The coefficient α_h corresponds to a fixed effect at horizon h and δ_h measures the effect of a deterministic linear trend. The vector γ_h contains the effects of the lagged endogenous variable and other control variables (including our shock) at horizon h captured in the vector $\mathbf{x}_t$ and φ_h contains the effects of daily dummy variables. Finally, u_{t+h} is assumed to have a zero mean and a (strictly) positive variance.

Our vector $\mathbf{D}_t$ in (4) includes the stringency index provided by researchers from the University of Oxford as well as two dummy variables to account for (1) the stimulus payment under the CARES act that started in April 15, the Paycheck Protection Program signed into law by President Trump on April 24, and (2) the three FOMC meetings since April.[11] In our baseline setting, $\mathbf{x}_t$ includes one lag of the endogenous variable, one lag of the Economic Policy Uncertainty Index (EPU) as well as one lag of our structural shock. This lag structure is the recommendation of the Bayesian Schwartz Criterion.[12]

The model presented before is linear. We now generalize the model to allow for state-dependent effects, that is we condition the impact of the shock on different regimes. Our preferred version throughout this paper conditions the response on the growth rate of new infections. Therefore, we estimate a smooth transition model of the form

$$y_{t+h} = F(z_t)\left(\alpha_h^I + \beta_h^I \varepsilon_t + \gamma_h^I \mathbf{x}_t\right) + (1 - F(z_t))\left(\alpha_h^{II} + \beta_h^{II} \varepsilon_t + \gamma_h^{II} \mathbf{x}_t\right) + \delta_h t + \varphi_h \mathbf{D}_t + u_{t+h}, \tag{4}$$

where the fixed effects, the effects of controls and the lagged endogenous variable captured in $\mathbf{x}_t$ as well as the effect of our shock are now allowed to differ across regimes I and II at each horizon h, respectively. That is, the indicator function $F(z_t)$, which lies between 0 and 1, determines the weight of each regime, whereby $F(z_t)$ depends on outcomes of the state variable z_t, which in our case is the growth rate of new infections.

In effect, the response of our endogenous variables to a shock is a weighted

[11] The stringency index is meant to measure the strictness of policies restricting people's behavior and lies between 1 and 100. The data is available on a daily frequency at https://www.bsg.ox.ac.uk/research/research-projects/coronavirus-government-response-tracker. The index is aggregated from 17 indicators of government responses, economic policies and health system policies.

[12] The results are insensitive to using the Akaike Information Criterion instead.

average of regimes I and II conditional on z_t and reads

$$\left.\frac{\partial y_{t+h}}{\partial \varepsilon_t}\right|_{z_t} = F(z_t)\beta_h^I + (1 - F(z_t))\,\beta_h^{II}. \tag{5}$$

In the next subsection, we will describe the specification of $F(z_t)$ in detail. However, it is important to note that our framework allows us to easily compare the sensitivity to shocks across both regimes, without making explicit assumptions (as in the case of VAR models) on the economy staying in either regime I or II. That is, we can draw inference on the difference between β_h^I and β_h^{II} based on $t-$type tests.

B. *State-Dependent Dynamics*

Our approach follows Born et al. (2020) and relies on specifying the transition function $F(z_t)$ based on the empirical cumulative density function (CDF)

$$F(z_t) = \frac{1}{T}\sum_{t=1}^{T} \mathbb{1}_{z_j<z_t}, \tag{6}$$

where T is the sample size and $\mathbb{1}_{z_j<z_t} = 1$ if $z_j < z_t$ and zero otherwise. That is, $\mathbb{1}_{z_j<z_t}$ denotes the indicator function of the event $z_j < z_t$. We refer to this approach as non-parametric, as we do not need to specify parameters driving the transition.

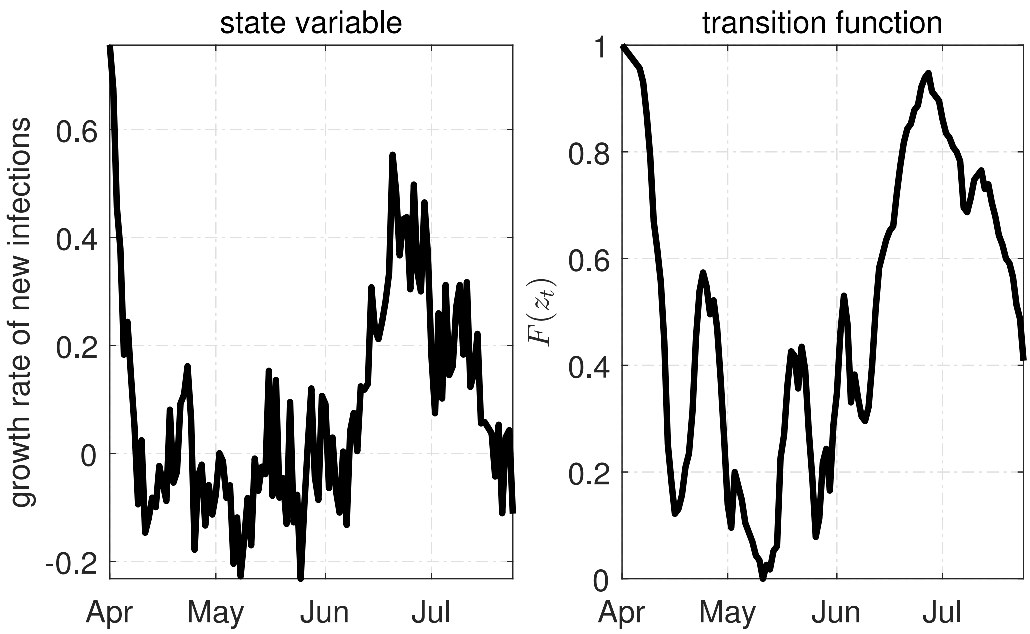
We choose the weekly growth rate of new infections as our state variable. Since the outbreak of the pandemic, numbers on new infections are reported every day in almost all media outlets. Public policies geared towards "flattening of the curve" made this statistic particularly popular. The left panel of Figure (3) shows the weekly growth rate of new infections with the coronavirus over time. The right panel shows the transition functions $F(z_t)$ based on the empirical cumulative density function over time.[13]

Starting with the left panel, after a well-pronounced decline with the growth rate falling from 60% to almost zero throughout mid of April, the growth

[13]In order to further get rid of noise in the data, we take a 7-day moving average before calculating the transition function. It must be stressed, however, that we get exactly the same results if we abandon the moving average and use the un-smoothed growth rate.

rate of new infections fluctuated stable around zero until the beginning of June. Since then, however, we observe a strong increase in the growth rate with an increase in cases of above 40% in mid June, which declines again at the end of our sample.

Figure 3: Deriving the state variable



Notes: The left panel shows the weekly growth rate of daily cases of new infections with the coronavirus that causes COVID-19. The right panel shows the transition functions $F(z_t)$ based on parametric approach drawing on the empirical cumulative density function.

The right panel of Figure (3) shows the resulting transition function calculated as described above. While we see a sharp fall of $F(z_t)$ at the beginning of our sample, saying that the economy swiftly moves from regime I to regime II, the sudden rise in daily cases translates into a fast reversion from regime II to regime I from mid June onward. As a result, we observe that a high weight is attached to regime I throughout June and July.

C. *Inference*

We regress the dependent variable at different horizons on the same set of control variables. This will likely result in autocorrelated residuals. In order to calculate standard errors that account for the possibility of serially correlated residuals both within and across equations, we follow the strategy of Ramey and Zubairy (2018) and Tenreyro and Thwaites (2016) and estimate seemingly unrelated equations as proposed by Driscoll and Kraay

(1998). That is, we estimate the parameters of interest of each equation separately and, in a second step, average the moment conditions across horizons $h = 0, ..., H$ when deriving Newey-West standard errors. As a result, Driscoll and Kraay (1998) standard errors account for autocorrelation across both, time t and horizons h.

Finally, we follow standard practice (see Jordà, 2005) and set the maximum autocorrelation lag for the Newey-West procedure to $L = h + 1$.[14]

IV Results

In this section, we first set out our baseline results. In the baseline setting, the idea is to uncover possible asymmetries across regimes in the responses of consumer spending to a standardized pandemic shock. That is, the baseline regression focuses on the effects of pandemic shocks conditional on the state of the infection curve. The sample size covers data from April 3 to July 26, consisting of 115 observations. After adjusting for leads and lags, the effective sample size starts in April 4 and ends in July 12 and, hence, consists of 100 observations. The section also reports results for different income levels as well as for the 10 largest US states.

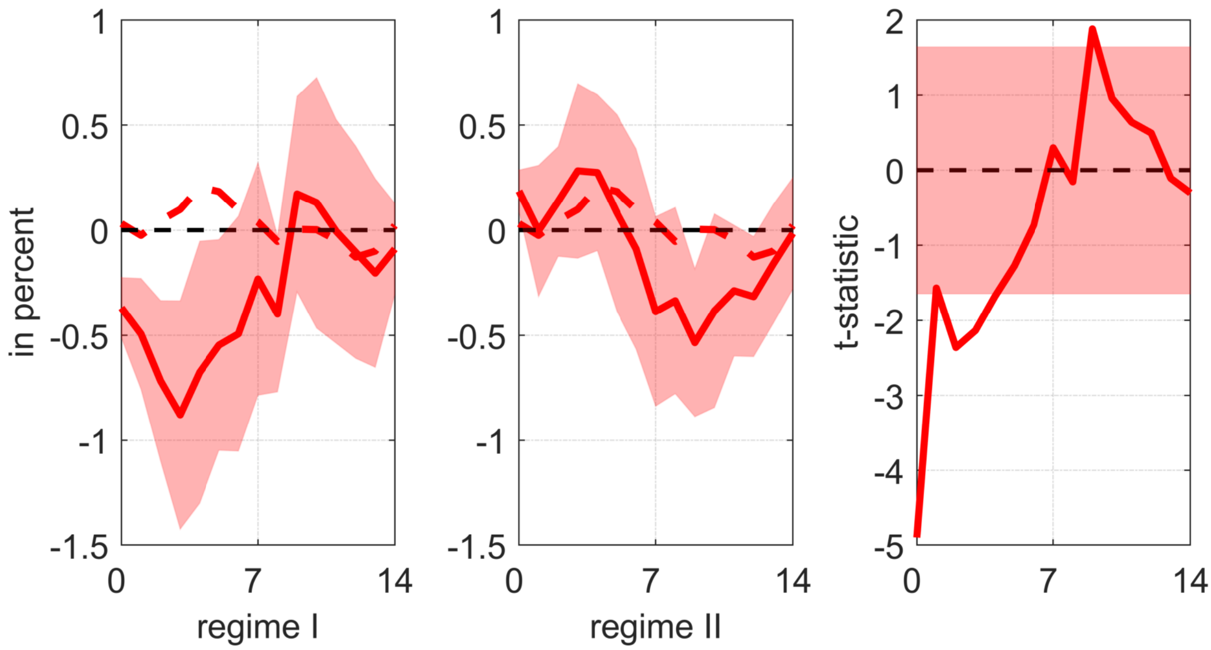
A. *Baseline Results*

Figure (4) shows the state-dependent impulse responses of total spending following a pandemic shock. Remember that throughout the paper, all spending variables are given in percent change relative to the average level of January. That is, a value of one corresponds to an increase in spending of one percent relative to January. In the left column, the red-solid line depicts the impulse response coefficients in regime I following a pandemic shock. Regime I corresponds to a situation with a high growth rate of new infections. The shaded area corresponds to the 90 percent confidence bands

[14] Note that for each horizon h, our null hypothesis is that $H_0 : (\beta_h^I - \beta_h^{II}) = 0$. Since we test the same null hypothesis for each $h = 0, ..., H$, one could argue that our t−statistics will result in a multiple testing problem as we test $H+1$ null hypotheses at a significance level α and in effect would - on average - reject αn true hypotheses. However, as pointed out by Tenreyro and Thwaites (2016), the multiple testing problem is negligible when the t−statistics for adjacent horizons are correlated, which is what we will see when we discuss our results.

based on Driscoll-Kraay standard errors. For the purpose of comparison, we also report the corresponding coefficients from the linear model (red-dashed line). Accordingly, the second column reports the corresponding values for regime II, i.e. the regime with a modest growth rate of new infections. The third column shows the t-statistics testing the null hypothesis $H_0 : \beta_h^I - \beta_h^{II} = 0$ for adjacent horizons $h = 0, ..., H$, where the shaded area covers the t−critical values for a 90 percent confidence interval, i.e. ±1.645.

Figure 4: Response of Total Spending



Notes: The first column shows the impulse response coefficients (red-solid) β_h^I for $h = 0, ..., H$ in regime I following a pandemic shock (one standard deviation), the second column shows the corresponding impulse response coefficients β_h^{II} in regime II. In both cases, the red-shaded area corresponds to the 90 percent confidence interval relying on Driscoll-Kraay standard errors. The red-dotted lines in the first two columns correspond to the impulse response coefficients from the linear model without allowing for state-dependent effects. The third column shows the t−statistics testing the null that $H_0 : \beta_h^I - \beta_h^{II} = 0$ for each horizon using the Driscoll-Kraay method. The red-shaded area covers the t−critical values for a 90% confidence interval, i.e. ±1.645.

In this context, it is important to stress that a perfectly symmetric transmission of pandemic shocks would imply that $\beta_h^I = \beta_h^{II}\ \forall h = 0, ..., H$. In other words, a pandemic shock as identified in the previous section would have the same effects across both regimes. Contrary to this, we would refer to asymmetric effects when the difference between β_h^I and β_h^{II} is significantly different from zero.

Starting with the results from the left panel, i.e. the impulse response coefficients in regime I, we see a significant drop in total spending. That is, following a pandemic shock, total spending falls by about 0.5 percent on impact relative to its average value in January. Spending decreases even further on subsequent days and peaks at a decrease of 0.8% three to four days after the shock. Afterwards, total spending starts to steadily revert to its mean which is reached after eight days. In other words, having recognized the pandemic shock as bad news, households respond with a significant decline in aggregate spending when the reported daily number of new infections is relatively high.

However, we see a different pattern in regime II, i.e. when the growth rate of new infections is relatively small. Following a pandemic shock of the same size, spending remains unchanged for the first week. After that, we find a drop by about 0.5%. The t–statistics in the right panel shows that the difference between the response in regime I and regime II is significantly different from zero for the first five days. This being said, we reject the null hypothesis of symmetric effects and find strong evidence for a regime-dependent response of spending to a pandemic shock.

As discussed in the introduction, the effect of the pandemic on household spending, whether it works through voluntary or force consumption restraint or an unexpected fall in lifetime income, should increase in the spread of the pandemic. If few people are affected by the virus, the need to reduce spending, either voluntarily or through governmental restrictions, remains limited. Likewise, the drop in lifetime income remains small since a shock does not call entire industries or job profiles into question. If, in contrast, the number of infections is large, the shock should have stronger effects. Our results are consistent with this notion because the effect of the shock is significantly larger in regime I compared to regime II. As we will see now, the shock impact across income quartile is also consistent with this. Low-income households work more in contact-intensive jobs. With low-income household bearing the burden of social distancing, the future of these jobs is uncertain in a situation with many infections, while high-income households have jobs in which social distancing is possible. The impact of the shock in regime I should therefore be larger for low-income compared to high-income households.

Next, we take a closer look on the response of spending and investigate how the impulse responses differ across income quartiles. This is possible because we have data on spending by customers living in ZIP codes with different income levels, namely high (top quartile) median income, middle (middle two quartiles) median income as well as low (bottom quartile) median income. This encourages us to estimate the response of spending to our pandemic shock across different income classes.

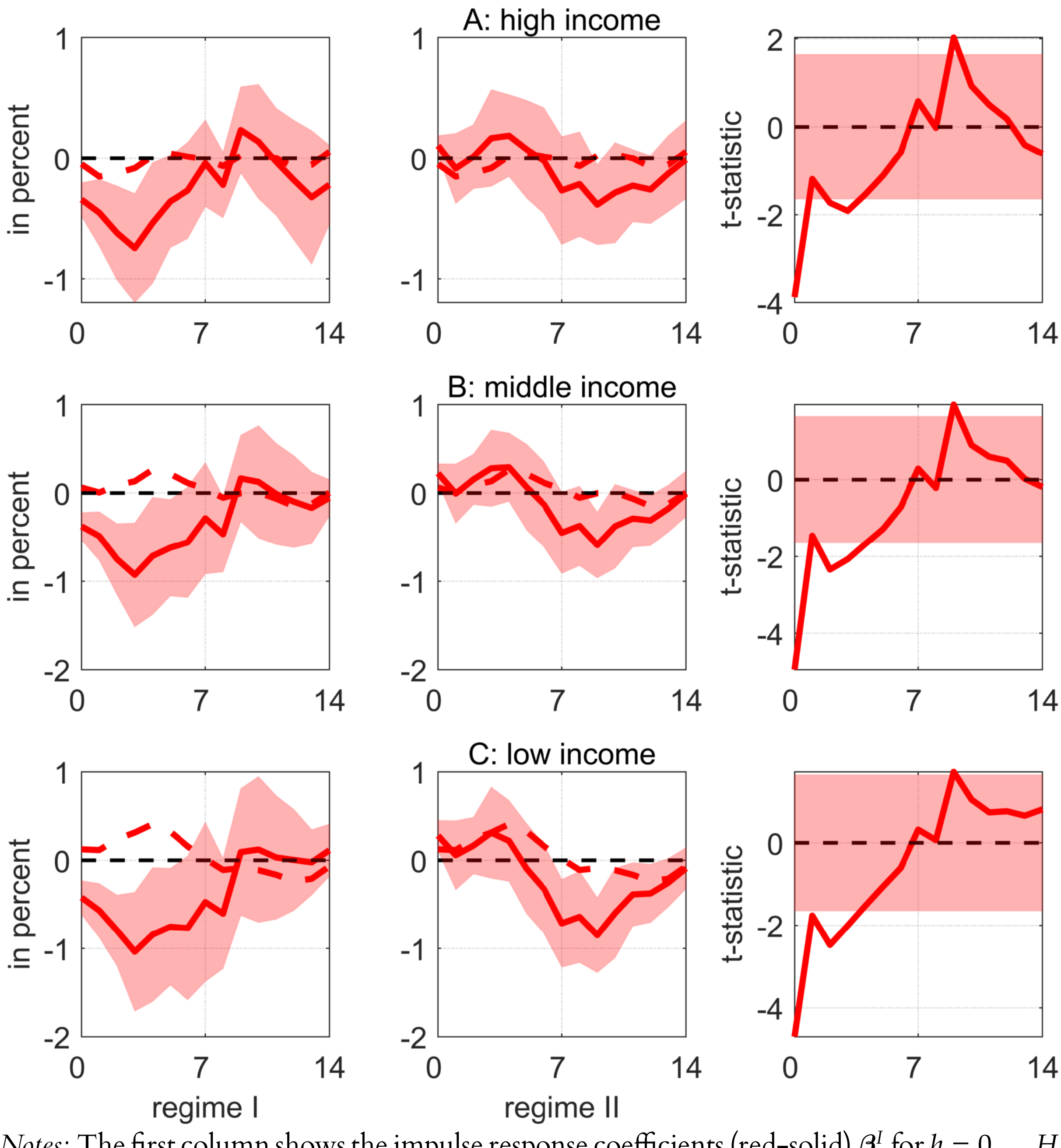
Figure (5) reports the results for spending of all three different income categories. Starting with panel A, it stands out that, prompted by a pandemic shock, high-income households significantly reduce spending when the growth rate of new infections is relatively high (regime I). After four days, spending drops by about 0.8% and starts to revert to its mean which is reached after one week. The reaction to the same shock has no significant effect in regime II, i.e. when the spread of the virus is slower.

We have a similar picture in panels B and C. While the qualitative picture is similar to the one of high-income households, it stands out that the response of households in regime I seems to be negatively correlated with lifetime income. That is, we see a larger drop in spending for middle-income households and an even larger drop for low-income households. In regime I, spending of low-income households drops by 1% percent, i.e. low-income households are more sensitive to the shock than high-income households.

For all income groups, the response to a pandemic shock depends strongly on where the economy is at the infections curve. Our results indicate that over the first week after the shock the response is much stronger in regime I and is significantly different from the response in regime II. For each income quartile, we cannot reject the null that $\beta_h^I = \beta_h^{II}$ over the first few days considered. Hence, the response of household spending is asymmetric across regimes.

These findings can be rationalized based on the notion that the fall in lifetime income as a result of a pandemic shock is larger for low-income households. Workers in the service and hospitality sector, for example, face uncertainty about whether and when they can return to their old jobs. In addition, our results resemble what is found in literature dealing with the nexus between household characteristics and the marginal propensity to consume (MPC). Aggregate MPC is typically found to depend on how aggregate shocks are distributed across households (see, for instance, Carroll et al., 2017; Carroll, 2009; Gelman, 2020; Calvet and Comon, 2003). In this context, higher marginal propensities to consume as typically found in the

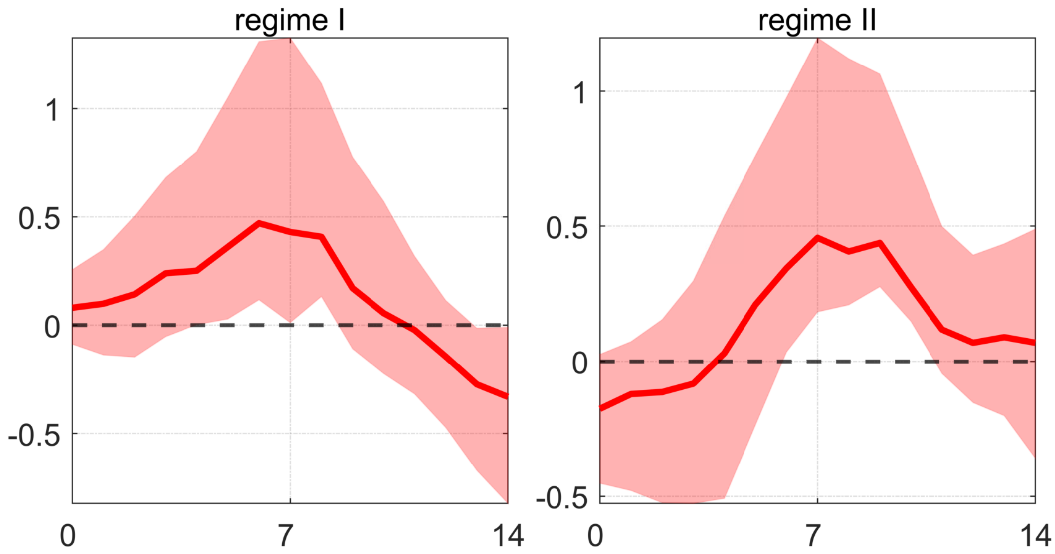
Figure 5: Response of Spending by Income Quartile



Notes: The first column shows the impulse response coefficients (red-solid) β_h^I for $h = 0, ..., H$ in regime I following a pandemic shock (one standard deviation), the second column shows the corresponding impulse response coefficients β_h^{II} in regime II. In both cases, the red-shaded area corresponds to the 90 percent confidence interval relying on Driscoll-Kraay standard errors. The red-dotted lines in the first two columns correspond to the impulse response coefficients from the linear model without allowing for state-dependent effects. The third column shows the t−statistics testing the null that $H_0 : \beta_h^I - \beta_h^{II} = 0$ for each horizon using the Driscoll-Kraay method. The red-shaded area covers the t−critical values for a 90% confidence interval, i.e. ±1.645.

literature can explain why our pandemic shock has a larger impact on lower-income households. In the context of the COVID-19 pandemic, Karger and Rajan (2020) show an MPC of 0.68 for hand-to-mouth consumers and 0.23 for households with access to assets. [15]

Figure 6: Response of Relative Spending (High Income - Low Income)



Notes: Difference of estimated coefficients $\beta_h^{high,I} - \beta_h^{low,I}$ in regime I and $\beta_h^{high,II} - \beta_h^{low,II}$ in regime II. The shaded areas cover the 5^{th} and 95^{th} percentiles from the distribution of the block bootstrap procedure as described in the text.

The previous graph revealed a significant state-dependence of the spending responses. However, we could not infer whether the response of high-income households is significantly different from low-income households. To shed light on the responses across quartiles, we proceed as follows: we generate 2,000 samples of contiguous blocks (with replacement) of four consecutive observations each. Within each replication, for each $h = 0, ..., H$ we then estimate the impulse response coefficients and calculate the sign of $\beta_h^{high,j} - \beta_h^{low,j}$ in regime j =I, II.[16] We then use the distribution of our boot-

[15]Explanations include households' wealth or employment status and the accompanying heterogeneity with respect to liquidity constraints.

[16]One difficulty in our application is that our set of control variables includes two dummies which have mostly 0-entries. It is therefore likely that inverting the matrix of right-hand side variables is not possible due to multicollinearity. To overcome this issue, we add another step and make sure that each bootstrap sample contains at least once those observations (not blocks) where the dummy variables are equal to one. From a practical point of view, this should not be a problem, since the dummy variables only improve the in-sample fit.

strap and report the 5^{th} and 95^{th} percentiles.

We show the results in Figure (6). Starting with the left panel, i.e. regime I, we find a significantly positive difference, which peaks at about 0.5% after six days. To interpret this finding, recall that the actual response for both income quartiles was negative. Hence, the positive value means that, following a pandemic shock, low-income households reduce spending significantly more than high-income households. The results are consistent with the view that the pandemic prevents low-income households from returning to their jobs, while high-income households can reconcile their jobs with the necessary degree of social distancing. As a result, the drop in permanent income is larger for low-income households. Also in regime II, i.e. when the number of new infections is growing less strongly, we find that the drop in spending is significantly stronger for low-income households. Hence, a pandemic shock prompts an increase in consumption inequality.

B. *The Quantitative Significance of Pandemic Shocks*

So far our results imply that spending is significantly responsive to our identified pandemic shock. However, we do not yet know the overall quantitative significance of our shock. If the shock we have identified is indeed an important driver of consumer spending, this should also be reflected in the variance of the forecast errors. In this section, we therefore apply the strategy of Gorodnichenko and Lee (2019) for forecast error variance decompositions (FEVDs) within the local projection framework and assess the contribution of our pandemic shock to the variation of forecast errors at different horizons. In a first step, we estimate the same model as before, but this time we leave out the contemporaneous effect of the shock

$$y_{t+h} = F(z_t)\left(\alpha_h^I + \gamma_h^I \mathbf{x}_t\right) + (1 - F(z_t))\left(\alpha_h^{II} + \gamma_h^{II} \mathbf{x}_t\right) + \delta t + \varphi_h \mathbf{D}_t + u_{t+h}. \quad (7)$$

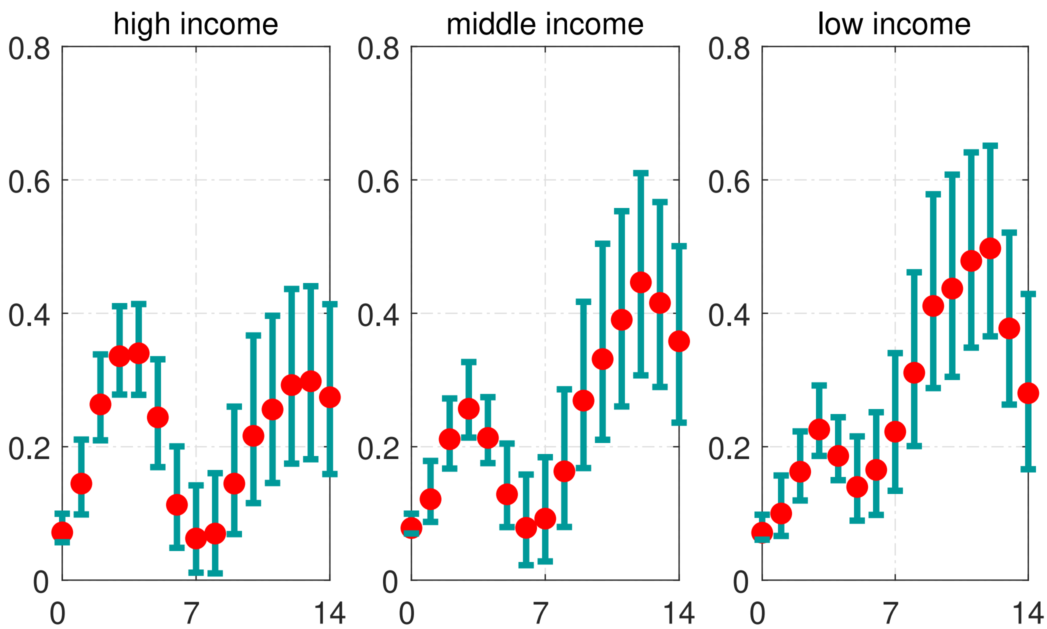
In a second step, we take the estimated forecast errors $\widehat{u}_{t+h}$ and regress them on the shock ε_t occurring between t and $t+h$ while accounting for our regimes I and II from our baseline setting

$$\widehat{u}_{t+h} = F(z_t)\left(\omega_0^I \varepsilon_t + \cdots + \omega_h^I \varepsilon_{t+h}\right) + (1 - F(z_t))\left(\omega_0^{II} \varepsilon_t + \cdots + \omega_h^{II} \varepsilon_{t+h}\right) + \eta_{t+h}, \quad (8)$$

where ω_j^i for $j = 0, \ldots, h$ and i = I, II measures the state-dependent effect of the pandemic shock on the estimated forecast error. Note that the coefficient of determination of this regression gives us the share of the forecast error variance which is explained by our pandemic shock. As shown by Gorodnichenko and Lee (2019), the R^2-method of the above regression is a natural estimator of the population share of variance explained by the future innovations ε_t in the total variations of our endogenous variable.

Inference is based on the distribution of the R^2s from a block bootstrap procedure including a bias-correction step as recommended by Gorodnichenko and Lee (2019).[17]

Figure 7: FEVD by Income Quartiles



Notes: Explained share of forecast error variance after the bias-correction procedure (red dots) and the 5^{th} and 95^{th} percentiles of the distribution of the block bootstrap procedure

Remember that we do not have a VAR-based benchmark for our local projection-based FEVD, which is due to the novelty of the data. However, theoretically, and based on our results so far, we expect pandemic shocks to

[17] To do so, we generate $B = 2000$ samples consisting of contiguous blocks of four consecutive observations each. Our bias is calculated as the difference between the mean over all bootstrap-based $R^{2,(b)}$ and the R^2 from our baseline procedure, i.e. $\text{bias}_h = B^{-1} \sum_{b=1}^{B} R^{2,(b)} - R^2$. Hence, our bias-corrected variance decomposition reads $R^{2,bc} = R^2 - \text{bias}_h$. As in the previous section, we adjust our bootstrap algorithm and manually add the dummy observations equal to one in our bootstrap samples to improve the robustness of our estimates.

be a major driver of fluctuations in household spending. This is what we see in Figure (7), which shows the estimated share of the forecast error variance that can be explained by our pandemic shock by income quartiles. The red dots correspond to the explained share of the forecast error variance. The green bars cover 90% of the distribution the R^2s obtained by our bootstrap procedure. For all groups, our pandemic shock seems to be an important driver of spending. Over the first week, our shock explains up to above 20% of the forecast error variance. While we observe a drop in the explained share for all income quartiles after the first week (especially for high-income households), the quantitative significance increases sharply and reaches its maximum after 12 days or so. We see again that spending for low-income households is most responsive to our pandemic shock, with an explained share of above 40% after nearly two weeks. Interestingly, the 5^{th} percentiles are above zero across all income quartiles and for all horizons considered. Our results therefore point to an important role of the pandemic shock in the variation of consumer spending. While after 12 days about 29% of the forecast variance can be explained for high-income households, this share is almost twice as large for low-income households. In other words, our results imply that for low-income households all other shocks together have a lesser role in the fluctuations of spending than our pandemic shock alone.

Thus, we conclude that our pandemic shock is a significant driver for all income groups, but especially for low-income households.

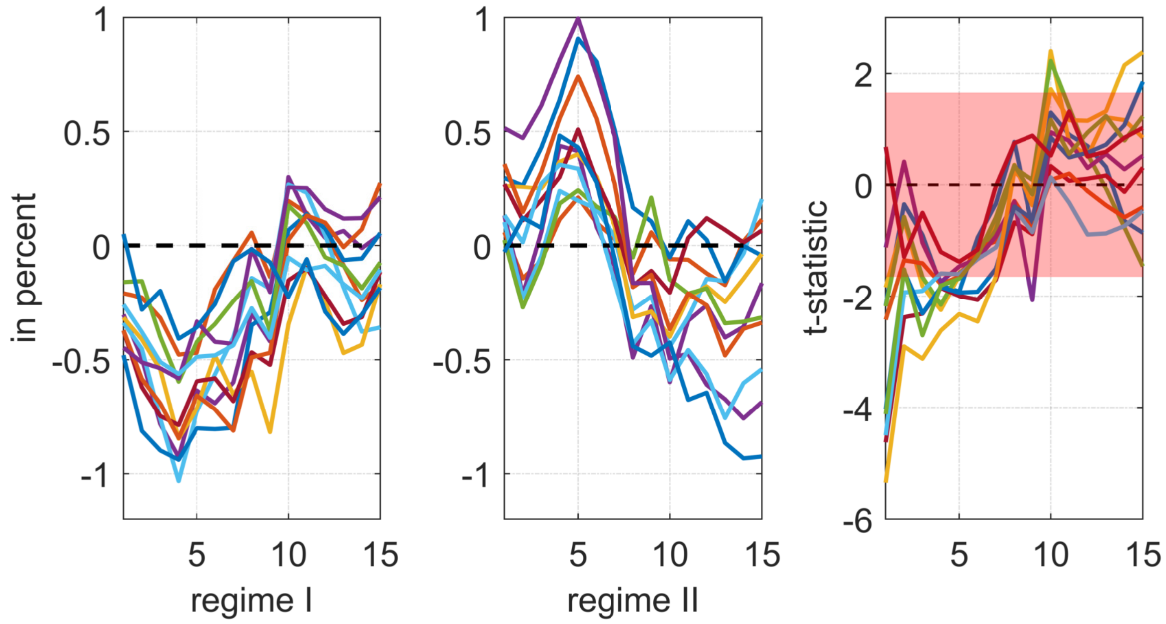
V Results on the State Level

Our data on spending is also available on the level of US states. We therefore repeat our exercise from the previous subsection and now investigate the responsiveness of spending for the ten states with the largest population.

We estimate the baseline model with spending in each of the 10 largest US states as the dependent variable. The driving variable remains the nation-wide pandemic shock and the state-variable is still the nation-wide growth of new infections.

Figure (8) shows the mean impulse responses of total spending following a pandemic shock for 10 states. It stands out that the qualitative pattern in regime I appears to be very homogeneous across all states. In regime I, for all states we observe a sharp drop in spending which peaks after four or five

Figure 8: Response on the State Level



Notes: The first column shows the impulse response coefficients β_h^I for $h = 0, ..., H$ in regime I following a pandemic shock (one standard deviation), the second column shows the corresponding impulse response coefficients β_h^{II} in regime II. The third column shows the t−statistics testing the null that $H_0 : \beta_h^I - \beta_h^{II} = 0$ for each horizon using the Driscoll-Kraay method. The red-shaded area covers the t−critical values for a 90% confidence interval, i.e. ±1.645.

days before it returns to its mean after two weeks. Also in regime II, the overall direction of the responses looks quite similar across all states. The third column shows that, for the first five days, in many cases we reject the null of equal responses across regimes.

While there are no error bands shown in Figure (8), Figure (9) shows the corresponding impulse response coefficients across states with ±1.645 standard deviations for selected periods, namely four, eight and twelve periods after the pandemic shock. For reasons of comparison, the transparent horizontal lines report the coefficients on the national level.

In almost all states, spending after four days is significantly reduced in regime I. In regime II, in contrast, we see an insignificant response for most states. Let us focus on two states, Michigan and New York, in which spending behaves differently than in most other states. Eight days after the occurrence of the shock in regime I, the drop in spending is sharpest in Michigan, while the response in New York is also well below the nationwide average response. Household spending in Michigan and New York deviates from the nationwide recovery after 12 days since spending in regime I is below spending in regime II.

VI Alternative State-Variables

In our baseline setting, we choose the growth rate of daily infections as our state variable. While figures about new infections are omnipresent in the media, the drawback of this state variable is that it does not necessarily provide information on where the economy stands on the infection curve. In fact, households may condition their spending behavior on the overall level of the infections curve, rather than the slope of the curve.

As a first alternative, we therefore repeat our estimation and take as a state variable an indicator variable which is 0 if the temporary peak of new infections is not yet reached and 1 if the total number of new infections decreases (alternative I). To do so, we set our indicator function to 1 until April 9th, to 0 from April 10th to June 8th, and to 1 again afterwards. These are roughly the cut-off dates that reflect a reversal of the current infection pattern.[18]

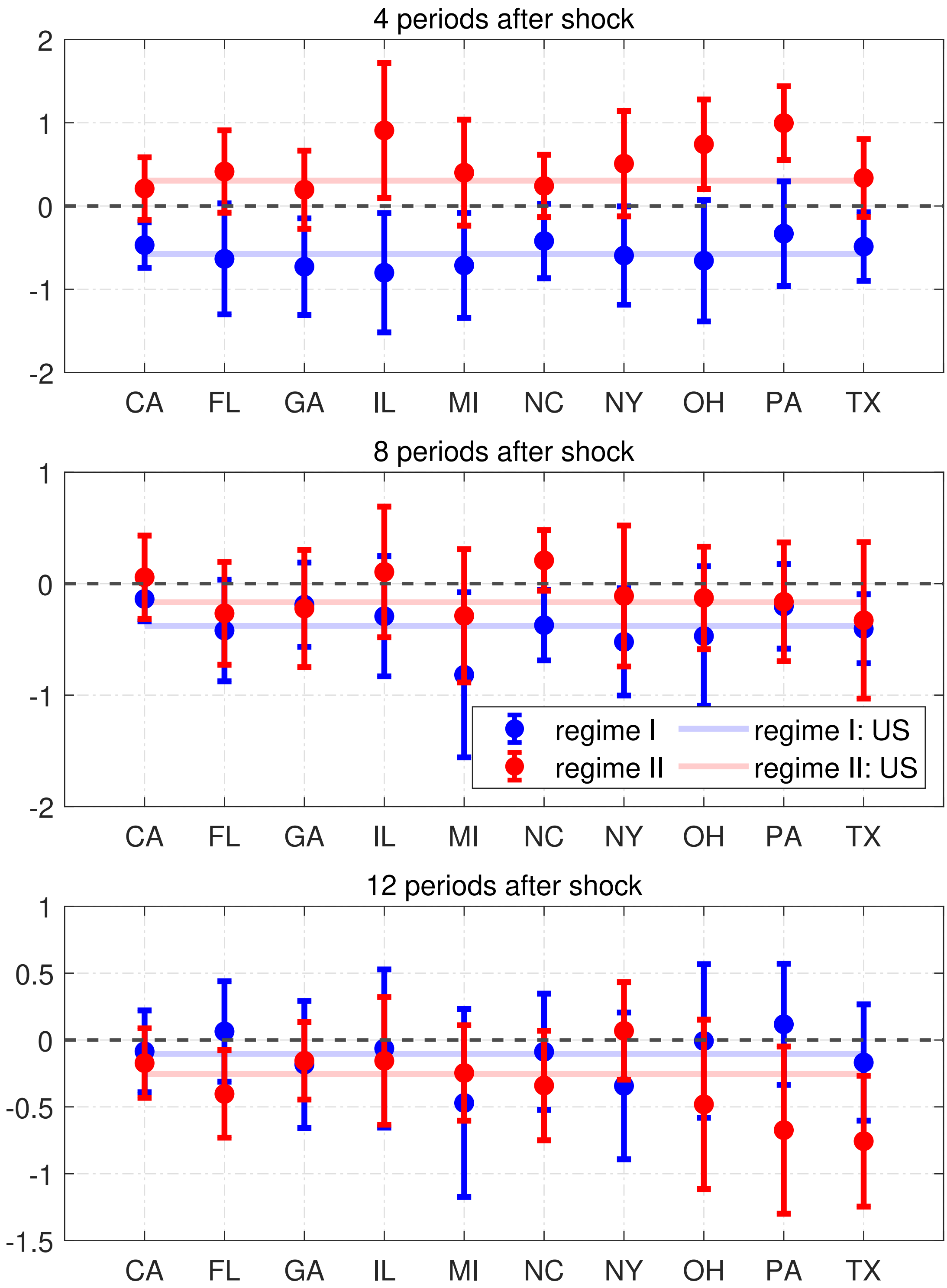
As a second alternative, we specify $F(z_t)$ as a logistic function of the form

$$F(z_t) = \frac{\exp\left(\kappa \frac{z_t - \mu}{\sigma_z}\right)}{1 + \exp\left(\kappa \frac{z_t - \mu}{\sigma_z}\right)}, \tag{9}$$

where μ is used to control the proportion of the sample the economy spends in either state, and σ_z is the sample standard deviation of the state variable z_t. The parameter κ controls how abruptly the economy switches from one state to the other following movements of the state variable. In other words, higher values of κ mean that small movements of the state variable suffice to induce a switch from one regime to the other. However, although the parametric approach has the disadvantage that we have to make explicit assumptions about the parameters determining the behavior of switching from one state to the other, this approach is well understood and relies on the idea

[18] Keeping anything else constant, we therefore replace $F(z_t)$ with an indicator variable $I(z_t)$, where $I(z_t)$ is equal to zero from April 10 to June 8 and equal to one otherwise.

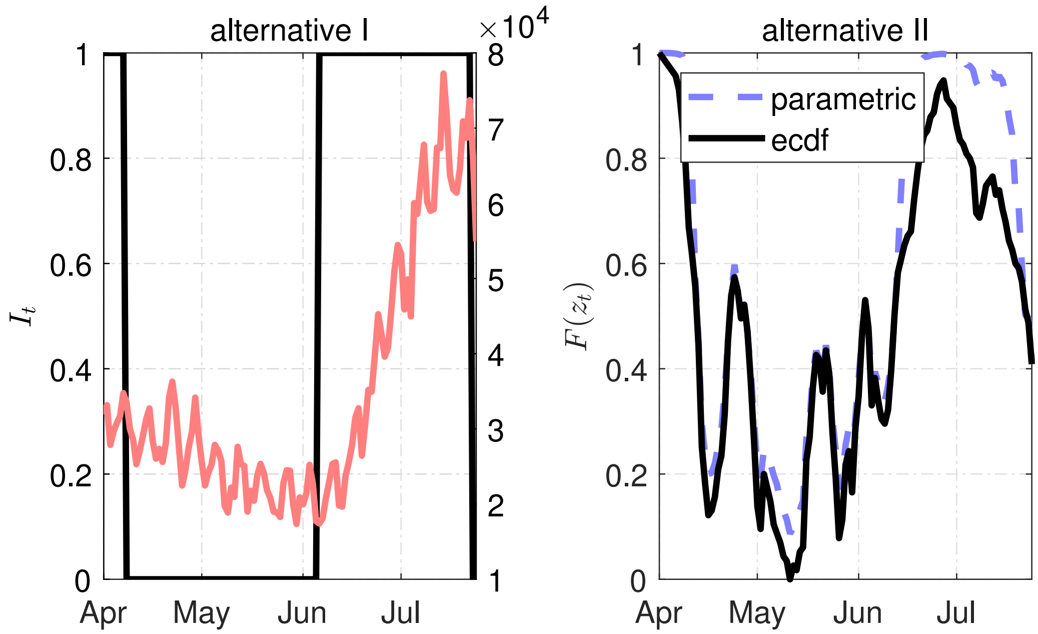
Figure 9: Response of Total Spending on the State Level



Notes: The dots correspond to the point estimates point estimates for regime I (red) and regime II (blue) after 4 periods (upper panel), 8 periods (middle panel) and 12 periods (bottom panel). The edges indicate 1.645 standard deviations in order to cover a 90% confidence interval, based on Driscoll-Kraay standard errors. The horizontal lines reflect the nation-wide effects in each regime.

of Granger and Terasvirta (1993) and is, among others, used in Auerbach and Gorodnichenko (2012), Ramey and Zubairy (2018) and Tenreyro and Thwaites (2016). We set $\kappa = 3$ which implies an intermediate degree of intensity of regime-switching and set $\mu = \text{med}(z_t)$. Figure (10) shows both

Figure 10: Alternative States



Notes: The left panel shows daily new infections (red-solid) and the corresponding alternative regimes as described in the main text. The right panel shows the alternative transition function obtained by the parametric approach as described in the text.

alternative regimes. The left panel shows the actual number of daily new infections and the distinction of regimes I and II as indicated by the black vertical lines on the cut-off dates. The right panel shows the transition function based on the parametric approach (blue-dashed). The alternative transition function looks very much like our baseline transition function, although we observe a higher weight of regime I at the end of our sample.

Figures (11) to (14) in the appendix show the corresponding impulse response functions for total spending and spending across income quartiles for both alternative regime classifications. It stands out that our results from the first alternative state look exactly like the results in the previous section. That is, total spending significantly decreases in regime I, i.e. when the growth rate of new infections is relatively high. The peak is reached after 3-4 days with a drop of 0.7 percent. Also the responses in regime II look exactly like those from our baseline results. Finally, we also observe a significant difference in the responses across regimes which is consistent with asymmetric effects. The responses across different income quartiles also look very much like in the benchmark model.

The results from our alternative state variable (alternative II) exhibit a similar picture. Both the shape of the impulse responses and the magnitudes of the effects are remain unchanged. This being said, our results indicate that spending reacts more strongly when the number of new infections is high. Again, we find that the difference across regimes I and II is again stronger for low-income households.

VII Conclusion

We provided evidence on the causal effect of unexpected news about the COVID-19 pandemic on spending of US households. Our first finding is that a pandemic shock, the forecast error about the number of fatalities, has a negative effect on spending: a surprise increase in the number deaths leads to a sharp reduction in expenditures. We also showed that this effect is depending on the position of the US economy on the infection curve. With the number of new infections increasing, the effect of a shock is much stronger. If the growth rate of the number of infections is small, in contrast, the pandemic shock has almost no effect. A second finding pertains to the effect across income quartiles. If the number of infections is increasing strongly, the shock prompts a much larger adjustment of spending from low-income households compared to high-income households. Hence, the pandemic shock increases consumption inequality.

Our results have two implications for economic policies designed to stabilize aggregate economic activity. First, policy should target low-income households more than high-income households. Spending of low-income households is particularly sensitive to a pandemic shock, and support packages will be more effective when targeting relatively poor households. Second, economic support through direct and indirect transfers should be conditioned on the state of the pandemic in order to stabilize consumption effectively. Transfers will be more effective when the number of infections is large, because in this state households would reduce spending the most.

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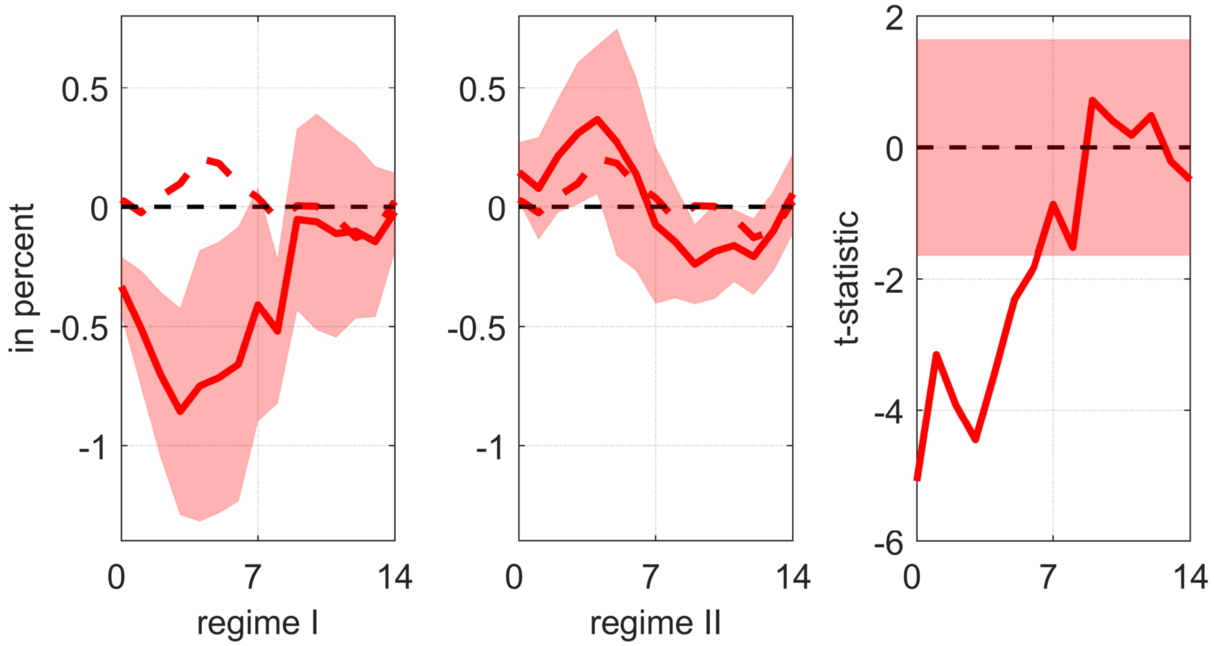
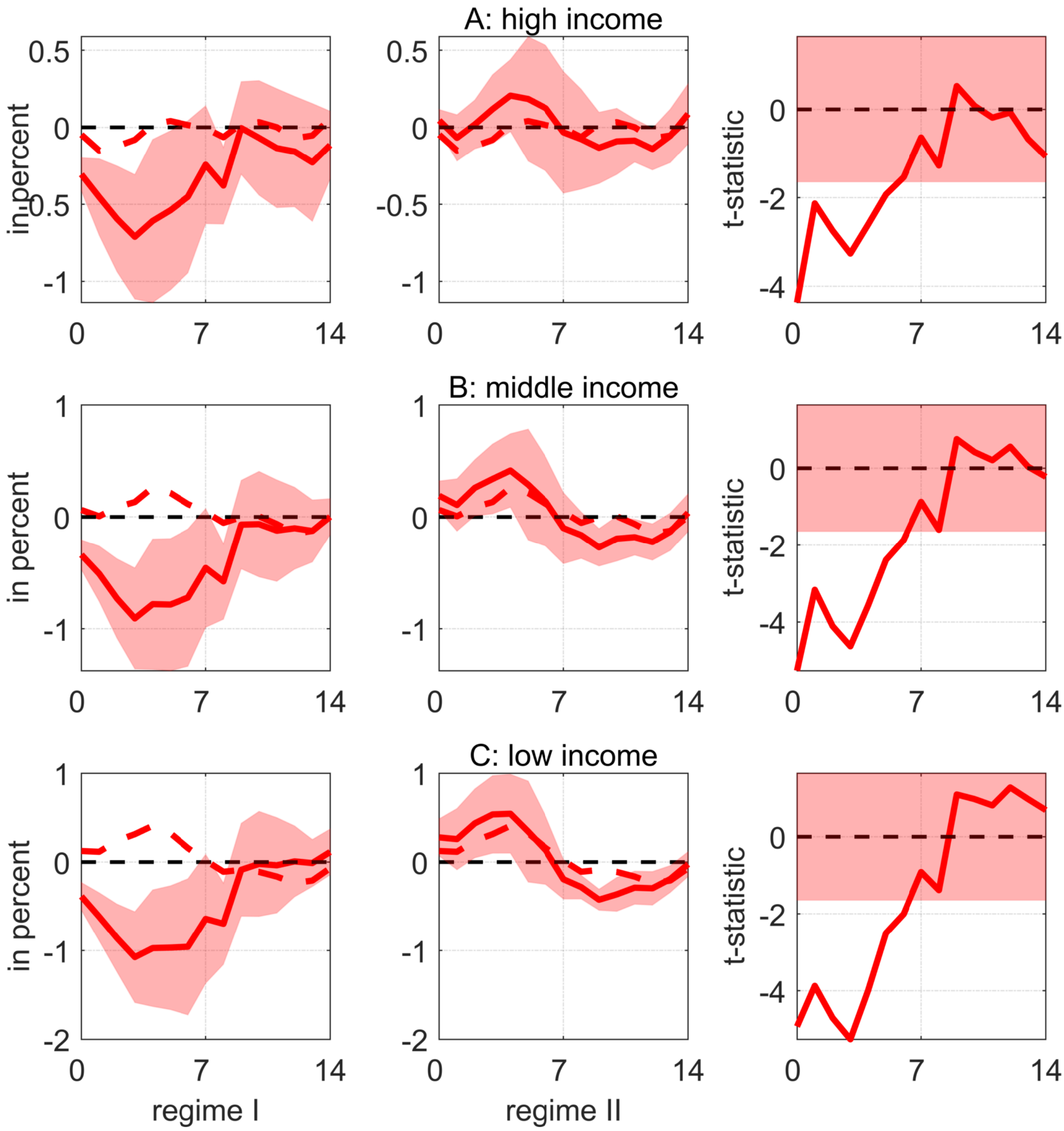
Figure 11: Response of Total Spending: alternative I



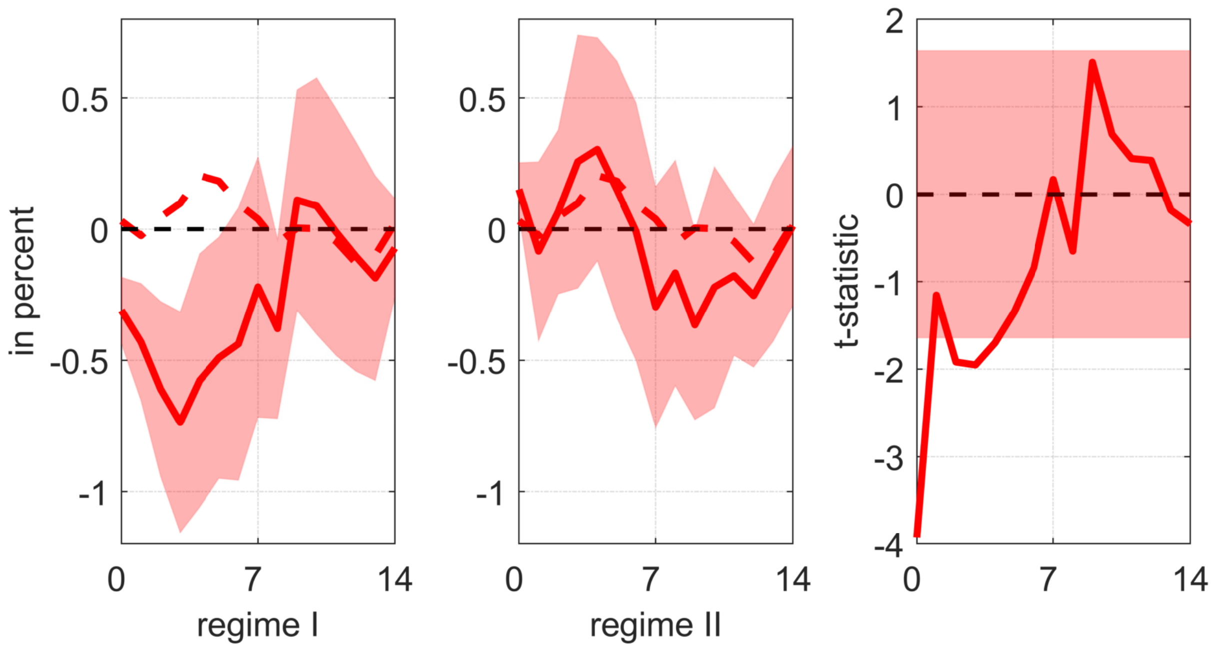
Notes: The first column shows the impulse response coefficients (red-solid) β_h^I for $h = 0, ..., H$ in regime I following a pandemic shock (one standard deviation), the second column shows the corresponding impulse response coefficients β_h^{II} in regime II. In both cases, the red-shaded area corresponds to the 90 percent confidence interval relying on Driscoll-Kraay standard errors. The red-dotted lines in the first two columns correspond to the impulse response coefficients from the linear model without allowing for state-dependent effects. The third column shows the t−statistics testing the null that $H_0 : \beta_h^I - \beta_h^{II} = 0$ for each horizon using the Driscoll-Kraay method. The red-shaded area covers the t−critical values for a 90% confidence interval, i.e. ±1.645.

Figure 12: Response of Spending Across Income Quartiles: alternative I



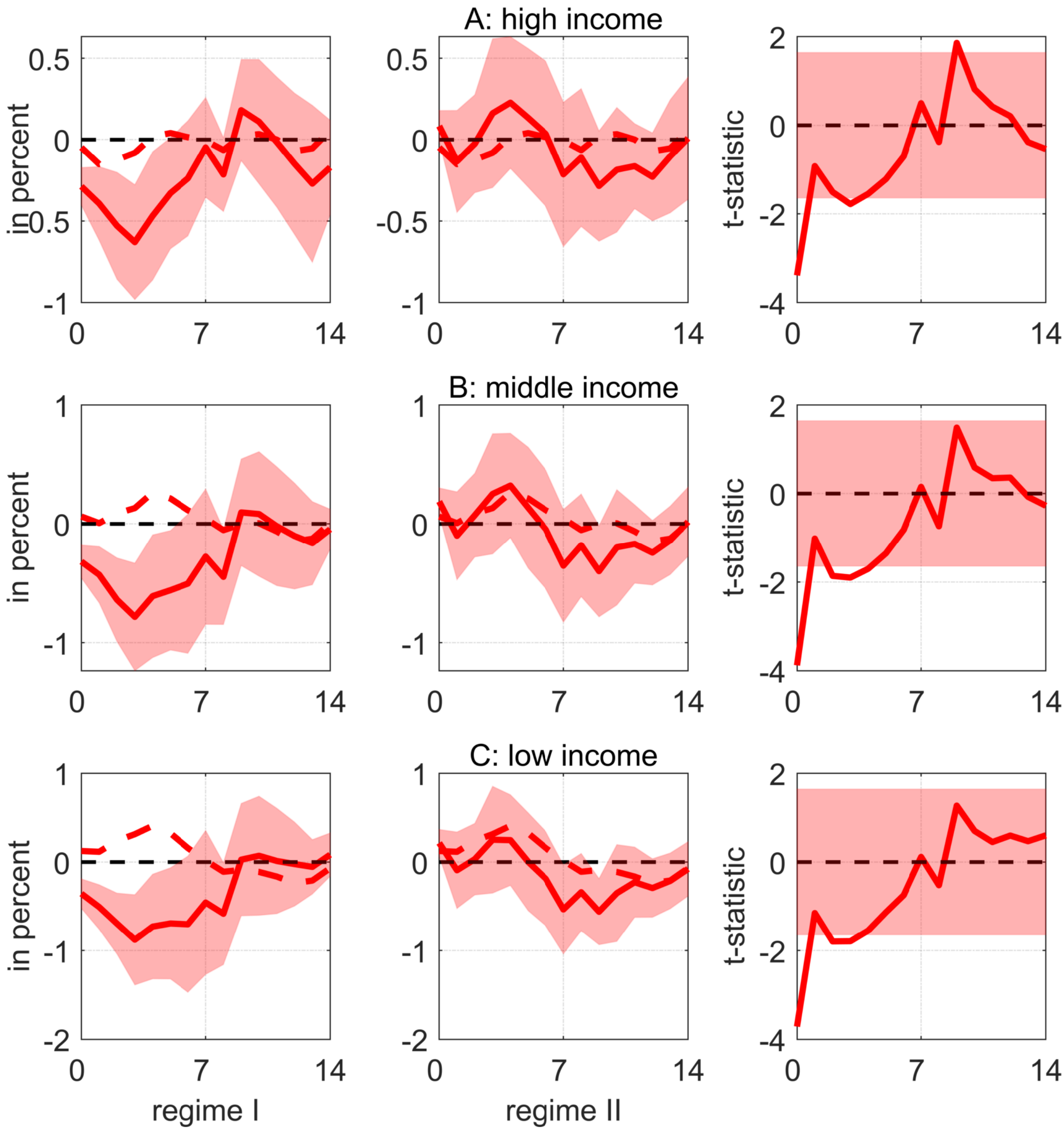
Notes: The first column shows the impulse response coefficients (red-solid) β_h^I for $h = 0, ..., H$ in regime I following a pandemic shock (one standard deviation), the second column shows the corresponding impulse response coefficients β_h^{II} in regime II. In both cases, the red-shaded area corresponds to the 90 percent confidence interval relying on Driscoll-Kraay standard errors. The red-dotted lines in the first two columns correspond to the impulse response coefficients from the linear model without allowing for state-dependent effects. The third column shows the t−statistics testing the null that $H_0 : \beta_h^I - \beta_h^{II} = 0$ for each horizon using the Driscoll-Kraay method. The red-shaded area covers the t−critical values for a 90% confidence interval, i.e. ±1.645.

Figure 13: Response of Total Spending: alternative II



Notes: The first column shows the impulse response coefficients (red-solid) β_h^I for $h = 0, ..., H$ in regime I following a pandemic shock (one standard deviation), the second column shows the corresponding impulse response coefficients β_h^{II} in regime II. In both cases, the red-shaded area corresponds to the 90 percent confidence interval relying on Driscoll-Kraay standard errors. The red-dotted lines in the first two columns correspond to the impulse response coefficients from the linear model without allowing for state-dependent effects. The third column shows the t−statistics testing the null that $H_0 : \beta_h^I - \beta_h^{II} = 0$ for each horizon using the Driscoll-Kraay method. The red-shaded area covers the t−critical values for a 90% confidence interval, i.e. ±1.645.

Figure 14: Response of Spending Across Income Quartiles: alternative II



Notes: The first column shows the impulse response coefficients (red-solid) β_h^I for $h = 0, \ldots, H$ in regime I following a pandemic shock (one standard deviation), the second column shows the corresponding impulse response coefficients β_h^{II} in regime II. In both cases, the red-shaded area corresponds to the 90 percent confidence interval relying on Driscoll-Kraay standard errors. The red-dotted lines in the first two columns correspond to the impulse response coefficients from the linear model without allowing for state-dependent effects. The third column shows the t–statistics testing the null that $H_0 : \beta_h^I - \beta_h^{II} = 0$ for each horizon using the Driscoll-Kraay method. The red-shaded area covers the t–critical values for a 90% confidence interval, i.e. ±1.645.