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Short and Variable Lags

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Abstract

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We study the transmission of monetary policy shocks using daily consumption, corporate sales and employment series. We find that the economy responds at both short and long lags that are variable in economically significant ways. Consumption reacts in one week, reaches a local trough in one quarter, recovers, and declines again after three quarters. Sales follow a similar pattern, but the initial drop, while delayed (one month), is deeper. In contrast, employment falls monotonically for five quarters albeit with a smaller impact reaction. We show that these short lags are masked by time aggregation at lower —quarterly— frequencies.

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"Monetary actions affect economic conditions only after a lag that is both long and variable" (Friedman, 1961).

1 Introduction

Milton Friedman's dictum is, to this day, firmly ingrained in the minds of both academics and policymakers. Decades of research and policymaking have shown that the transmission mechanism of monetary policy is no doubt complex, playing out over multiple channels and unfolding at medium to long horizons, as Friedman emphasized early on. This does not mean, however, that monetary policy actions have no impact already in the short run—nor that there is no value in discerning this impact for research and policymaking.

In this paper, we deploy novel, daily-frequency indicators of aggregate consumption, corporate sales and employment in Spain, together with state-of-the-art high-frequency monetary policy shock identification for the Euro Area, to revisit Friedman's dictum. Our contribution offers a new perspective on it. We show that the impact of monetary policy shocks can be detected already within days, rather than months, quarters or years. Specifically, we find that five days following a contractionary monetary policy shock, aggregate household consumption starts to decline. At a daily frequency, this decline is sustained, reaching a local trough 93 days from the shock, with consumption falling by 0.35%, followed by another trough at 330 days in which consumption falls by approximately 0.4%. Corporate sales – to households and other firms – react more slowly than consumption, but follow a similar pattern. Their response is statistically significant after 30 days; sales recover about six months from the shock, but fall again in the fourth quarter. Further, we find that this gross output adjustment, while somewhat slower than that of consumption, is nevertheless larger in magnitude, with a decline of 0.72% at trough taking place 102 days after the shock. Finally, while a response of aggregate employment is statistically detectable early on, relative to consumption and sales, the response of employment is initially much smaller, and its decline smoother and steadier. Employment reaches its trough 459 days after the shock, with a fall of 0.25%.

Our empirical findings qualify the conventional wisdom. The troughs of consumption, sales and employment at long lags corroborate the notion that the total effects of monetary policy actions fully unravel at relatively long horizons—in line with Friedman's dictum. However, monetary shocks are transmitted to the economy already over very short and variable horizons—with household consumption responding non-negligibly within days and corporate sales responding subsequently, within the first month fol-

lowing a shock. Vis-à-vis the response in the goods markets – economically significant already at short lags – the response of the labor market unwinds significantly only at longer horizons.

Leveraging the high-frequency nature of our data, we also bring to light consequential issues in time aggregation, which may weigh on the conventional wisdom. To do so, we run our empirical model on series that aggregate our daily data to the monthly or quarterly frequencies. We show that aggregating our data into lower frequency alters the empirical response of consumption, sales and employment to monetary policy shocks, blurring economically relevant results. Specifically, we find that aggregating daily into monthly data does not prevent our model from detecting a short-run impact of monetary policy - in the first month.[1] However, when aggregating up to the quarterly frequency, the model only detects a statistically significant impact of policy shocks at much longer lags.[2] In particular, a researcher with access to our quarterly-frequency data would conclude that consumption, sales and employment react 3 to 4 quarters after the shock. This finding suggests that at least some 'only after long and variable lags' conclusions may be led astray by the frequency of data most commonly available in macroeconometric work.

Finally, we exploit the cross-sectional richness of our data to disaggregate the high-frequency response of consumption and sales to monetary policy shock by categories and sectors of activity, respectively. We find that the bulk of the high-frequency response of aggregate consumption is accounted for by contractions in the consumption of food away from home and accommodation, recreation and culture, clothing and footwear, furniture and household equipment, and transportation (a category that includes car purchases). This is consistent with the notion that final demand adjusts on impact via a reduction of discretionary/luxury goods consumption, and the postponement of durable goods consumption. In contrast, necessities like food at home, housing and utilities, communication, and health barely adjust following a contractionary monetary policy shock.[3] Correspondingly, the response of sales is concentrated in sectors whose production is predominantly classified as durable consumption — transportation, manufacturing of furniture and textile. Moreover, in sectors more closely linked to household final de-

[1] We find similar results when aggregating daily at the weekly frequency, see Online Appendix B.4.

[2] It is worth stressing that using higher frequency data does not necessarily lead to better identification of monetary policy transmission. It is important to ensure the data are reliably collected and not very noisy, as pointed out by Christiano and Eichenbaum (1987). In both respects, our high-frequency consumption, sales and employment series sourced from Buda et al. (2022), administrative tax and social security data, respectively, ensure high standards.

[3] Likewise, consumption demand falls significantly across all types of payments — purchases by Transfers (associated to car purchases), Cash, Credit Card and Cards offline display the same pattern as consumption. The notable exception is Direct Debit, typically associated to consumption commitments and adjusted only infrequently, and Cards online. See Online Appendix C.

mand, such as wholesale and retail trade, the response of sales is quick—after just 16 days. Instead, the delayed response of aggregate sales relative to consumption is driven by upstream sectors, such as energy and construction, which only fall significantly after 70 days from the shock.

Literature. Our paper relates to, and brings together, three distinct literatures. The first is the rapidly growing empirical literature on high-frequency identification of monetary policy shocks, pioneered by Kuttner (2001) and Gürkaynak, Sack and Swanson (2005). An example relevant to our study is Altavilla et al. (2019), who built the database of monetary surprises around policy announcements for the EA that we use in our paper. Since the seminal work of Gertler and Karadi (2015), the literature has aggregated monetary surprises to lower frequencies such as monthly, quarterly and even yearly horizons (Almgren et al. (2022), Cloyne, Ferreira and Surico (2020) and Holm, Paul and Tischbirek (2021), respectively). Our contribution is to align the monetary identification with the time-frequency of economic indicators, thus enabling better identification of the monetary transmission. Relatedly, we should stress that the effects of monetary policy at short lags have already been documented by papers within this literature working with monthly frequency series. For example, same-month responses have been documented by Jarociński and Karadi (2020) and Miranda-Agrippino and Ricco (2021), focusing on GDP and industrial production for the EA and U.S., respectively. This suggests some of the long lags conclusions may have been the result of alternative identification strategies of monetary policy shocks. We contribute to the literature by providing detailed evidence on when and how these effects come into play within the month. The fast reaction of real economic activity to monetary policy shocks we document is perhaps not wholly surprising, given recent advances in the literature. Firstly, we know financial markets react immediately to monetary policy shocks (Gürkaynak, Sack and Swanson, 2005 and Swanson, 2021) and there is mounting evidence that relevant interest rates for households, such as mortgage rates, react within weeks (Gorea, Kryvtsov and Kudlyak, 2022). Secondly, there is increasing evidence at odds with the notion that households are inattentive to monetary policy developments. Notably, Lewis, Makridis and Mertens (2019) documents that public confidence in the state of the economy reacts *instantaneously* to surprises to the Federal Funds target rate.

Second, our paper builds on a fast-expanding literature on high-frequency indicators of economic activity, motivated (especially during the COVID-19 pandemic) by the need to support policy decision making in a rapidly-changing environment. Examples of weekly indicators are Eraslan and Götz (2021), Baumeister, Leiva-León and Sims (2021)

or Lewis et al. (2022), while examples of daily indicators are Diebold (2020) and Rua and Lourenço (2020). Concurrently and relatedly, there has been a surge in the usage of naturally occurring transaction-based data to measure economic dynamics at a high frequency (see, e.g., Andersen et al. (2021), Andersen et al. (2022), Bounie et al. (2020), Chetty et al. (2020) and Ganong and Noel (2019)). Related to our study, Grigoli and Sandri (2023) uses credit card provided by Fable Data for Germany to study how monetary policy shocks impact card expenditures at a daily frequency. A key advantage of using the universe of bank-transaction data, as we do in our paper, is that the consumption measure is much more accurate and comprehensive than measures derived from a specific method of payments (such as credit-card payments), a point stressed by Buda et al. (2022). Specifically, our measure of high-frequency consumption, constructed following the same procedures as in Buda et al. (2022) and appropriately aggregated up to quarterly frequency, matches well the consumption series in Spanish national accounts.[4]

Last but not least, our paper is related to a smaller but foundational literature on the consequences of time aggregation. Early seminal work on the theoretical properties of econometric modeling with temporally aggregated data includes Amemiya and Wu (1972), Sims (1971) and Geweke (1978). Marcet (1991) analyzes the consequences of time aggregation for forecasting. The relevance of temporal aggregation bias for a classical empirical question in macroeconomics – whether money growth granger causes inflation – is discussed by Christiano and Eichenbaum (1987) and Stock (1987). A recent re-visitation of this question is by Jacobson, Matthes and Walker (2022), who show that a temporal aggregation bias plays a non-secondary role in explaining the price puzzle typically found when estimating the impact of monetary policy shocks on inflation, relative to other rationalization (e.g., the 'FED information channel'). Our contribution is to offer empirical evidence suggesting that the 'long and variable' lags of monetary policy – across a number of key real outcomes – may be a byproduct of time aggregation.

In section 2 we describe the data and the methodology we use. We present our empirical findings, including extensive robustness analysis, in section 3. Section 4 concludes.

[4] A study relying on the same data is Cardoso et al. (2022) focused on the impact of inflation on households' balance sheets.

2 Data and Methodology

2.1 Data

We deploy three novel daily measures of economic activity in Spain, each of which we smooth by taking a 90-day backward-looking moving average to deal with noise and seasonality issues inherent to daily data.

Consumption Our proxy for daily aggregate consumption is derived from the universe of bank transactions recorded in the Spanish retail accounts of Banco Bilbao Vizcaya Argentaria (BBVA). Specifically, we construct a daily counterpart to the quarterly and annual series for aggregate consumption of private households reported in Buda et al. (2022). The latter build a detailed consumption panel for 1.8 million BBVA retail customers based on 3 billion individual transactions—covering all card transactions, cash withdrawals, regular direct debits and occasional transfers, from the 1st of April, 2015 till the 31st of December, 2021. The consumption panel is obtained by (i) constructing a representative sampling frame of the Spanish adult population and; (ii) applying national accounting principles to individual account outflows in order to isolate consumption expenditures. Expenditures are classified according to official COICOP consumption categories based on the available extensive meta-data associated to each transaction. Overall, as Buda et al. (2022) show, the aggregates implied by our series match well the official quarterly aggregate consumption series in both levels and growth rates: the implied level of aggregate consumption is, on average, within 1% of its official national accounts counterpart and the correlation of quarter-on-quarter growth rates across the two series is 0.987. Since we focus on daily frequencies, we take a stand on expenditures recurring at lower frequencies—such as monthly (imputed) housing services and the payment of regular utility bills, on direct debit. We distribute these expenditures uniformly across all days of the month, assuming a regular service flow to households. Aggregate consumption is deflated using the Spanish Consumer Price Index (CPI), while consumption category sub-aggregates are deflated using a CPI at the COICOP level.[5]

Sales Our daily sales variable is publicly available from the Spanish Tax Authority. The Tax Authority compiles the series from daily Value Added Tax (VAT) declarations by firms, reporting their domestic sales transactions (which form the tax base for VAT) on the day. This gross output measure reflects final sales to Spanish households or tourists;

[5]Further details on this consumption data are provided in the Online Appendix A

sales of investment goods to Spanish firms and households; and sales of intermediate goods to other Spanish firms. Only large firms or conglomerates – those with a turnover of 6 million Euros or above in the preceding year – are legally required to supply their domestic sales information daily. According to the Spanish Tax Authority (Agencia Tributaria, 2023), in 2019, the number of firms reporting daily sales was as high as 60,000 firms (out of a universe of 3.8 million VAT paying entities), accounting for about 70% of domestic sales by all firms in the same year. The Spanish Tax Authority also releases series disaggregated by NACE sector.[6] The series are adjusted for calendar and other effects by the Tax Authority. We use data from the earliest available date, July 1st 2017, till December 31st, 2021. We deflate these series following the Spanish Tax Authority recommendations: we deflate Manufacturing and Construction sectors with their respective Producer Price Indices; Wholesale and Retail Trade, and Transportation and Storage with their respective CPI; and use the CPI of the Services aggregate to deflate the remaining (service) sectors.

Employment We source a publicly available administrative series from the Spanish Ministry for Inclusion, Social Security and Migration, giving the total number of workers registered in the Spanish Social Security system on any given day; see Ministerio de Inclusión (2023). With a few exceptions, enrollment in the Social Security system is mandatory for all employer-employee contracts in Spain. The series is updated daily from Monday to Friday, and reports the stock of employment in each day obtained by netting out job destruction (labor contracts ending on the day) from job creation (new labor contracts registered with the social security system).[7] The daily employment series in this paper starts from April 1st, 2015 till December 31st, 2021.

2.2 Methodology

Identification We identify monetary policy shocks by constructing an external instrument using high-frequency changes in asset prices around ECB policy announcements, as in Gürkaynak, Sack and Swanson (2005) and Gertler and Karadi (2015). We derive these shocks based on the Euro Area Monetary Policy Database (EA-MPD), compiled by Altavilla et al. (2019). The EA-MPD record changes in prices and yields for different asset classes and maturities during ECB's monetary policy announcements. Following

[6]The available series account for at least 50% of each sector-level sales, with the exceptions of 'Hospitality Services' and the residual, catch-all, sector labelled 'Remaining Activities' (firms concentration is low in these sectors). We drop these two sectors from our analysis.

[7]For Saturdays and Sundays, we assume that the number of workers registered is the same as the previous Friday. See Online Appendix A for further details on the employment data.

Gertler and Karadi (2015) and Miranda-Agrippino and Ricco (2021), we focus on changes in the 1-year yield around the entire monetary event window—before the press conference till after the press conference Q&A. The use of 1-year yield is meant to broadly capture all the different policy actions available in the monetary policy toolkit—changes in short-term interest rates, forward guidance and quantitative easing.

A growing concern in the literature is that the central bank "information channel" can pollute monetary policy shocks (see, for example, Nakamura and Steinsson (2018), Jarociński and Karadi (2020) and Miranda-Agrippino and Ricco (2021)). To control for the information channel, we identify pure monetary policy shocks using the "poor man" sign restrictions adopted by Jarociński and Karadi (2020). Specifically, we exclude shocks such that, during the announcement window, the policy surprise (e.g., unanticipated contraction) has the opposite sign as the reaction of the stock price (a gain). This amounts to assuming that any movement in the stock market around a policy announcement window that is theoretically inconsistent with the expected effects of the monetary policy shock should be fully attributed to new information about the economy conveyed by policymakers during the announcement.

Local Projections We estimate daily impulse response functions (IRFs) of consumption, sales and employment to monetary policy shocks up to the horizon H using local projections (LP) (Jordà, 2005). Horizon-h LP-IRFs are obtained from the OLS estimates, denoted $\hat{\beta}_h$, of the following linear regression:

$$y_{t+h} = \alpha_h + \beta_h shock_t + \sum_{\ell=1}^{p} \varphi_{h,\ell} y_{t-\ell} + \theta_h cases_t + \delta_h stringency_t + \varepsilon_{h,t}, \tag{1}$$

where y_{t+h} is the year-on-year consumption, sales or employment measures, and $shock_t$ is the monetary policy shock. Given that our sample includes the years of the COVID-19 pandemic, to account for its impact on the economy, we include two controls: $cases_t$ is the log of new confirmed cases of COVID-19, and $stingency_t$ is the log of the stringency index.[8] In the baseline specification, we estimate IRFs up to $H = 658$ days and include 90 lags of the endogenous variable, corresponding to up to two years after shock and one quarter of past information, respectively. Including dependent variable lags as controls is motivated by the findings by Montiel Olea and Plagborg-Møller (2021), who show that lag-augmenting local projections not only renders inference more robust, but also

[8]Covid cases data is compiled by the World Health Organization, and the stringency index is calculated by the University of Oxfords Coronavirus Government Response Tracker.

simplifies standard error calculations by avoiding residual serial correlation adjustment.[9] Finally, to facilitate the interpretation of the IRFs, we report the responses of variables in levels. To compute this, we cumulate the year-on-year growth rates.[10]

3 Empirical Findings

3.1 High-Frequency Transmission of Monetary Disturbances to Consumption, Sales and Employment

Figure 1 shows the effects of a one standard deviation contractionary monetary policy shock on total consumption (panel a); corporate sales (panel b) and employment (panel c). The graphs on the left column show the response of each variable over a 660-day horizon; the graphs on the right column zoom in on the response in the first 30 days.

The top panel of Figure 1 shows that consumption starts responding rather quickly: its decline is statistically significant five days after the shock hits the economy. The initial contraction reaches -0.35% after 93 days —- three months —- relative to the unconditional mean. After that, consumption recovers somewhat: the point estimate response to the shock, albeit still negative, becomes statistically insignificant. The recovery is temporary, however. After 240 days, the consumption response is again significantly negative and continues to decline until it reaches a global trough at approximately -0.4%, around one year after the shock. This global trough, which occurs at long lags, is now more persistent. The negative consumption response remains at approximately -0.4% for another 160 days, one year and a half after the monetary policy shock.

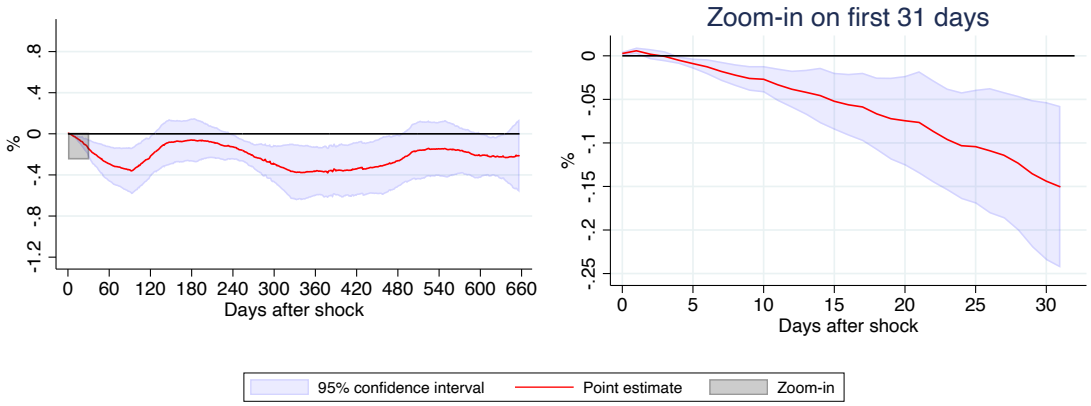
Corporate sales, in panel b, follow a broadly similar pattern, with an initial decline, then a rebound, and finally another fall—with two key differences. The first is the delay with which the contraction becomes significantly different from zero—initially, a 30-day delay instead of the 5-days delay observed for consumption. The contraction continues unabated thereafter, reaching a local trough at 102 days after the shock. After a rebound period, the sales response becomes again statistically negative at day 266 after the shock. Thus, the movements of corporate sales do mimic that observed for consumption, but with a lag of about 25 days. The second difference is the size of the response: in the first quarter after the shock, the contraction in sales is deeper than that of consumption. The initial trough is -0.72%, double that of consumption. These differences are not wholly

[9] Results are unchanged in the Online Appendix B.8 when computing standard errors using the Newey-West procedure (Newey and West, 1987).

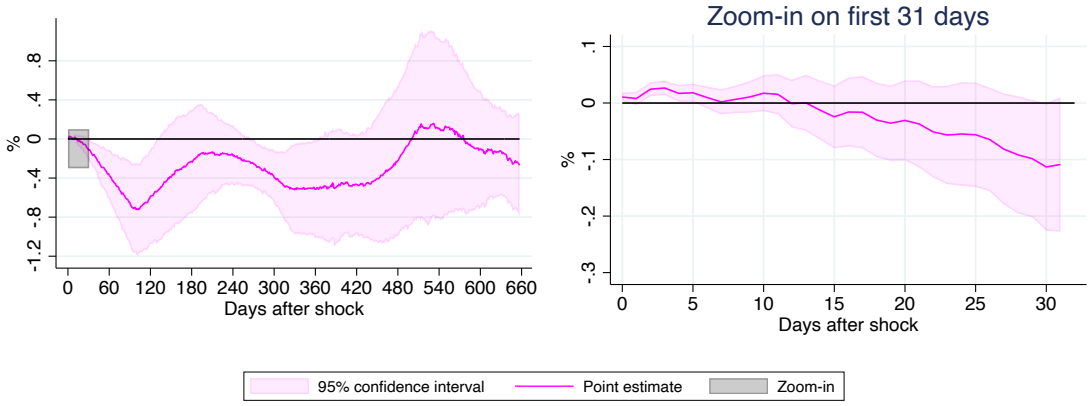
[10] Online Appendix E presents a detailed description of how we compute the IRFs in levels from year-on-year growth rates IRFs.

Figure 1: Daily response of economic activity variables to a monetary policy shock

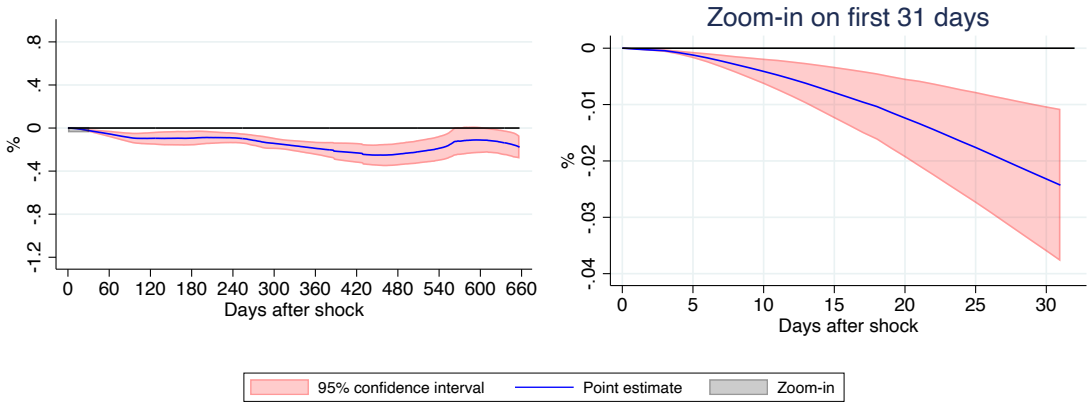
(a) Consumption



(b) Sales



(c) Employment



Notes: The monetary policy shock is one standard deviation. The responses are reported in levels. We obtain them by cumulating year-on-year changes. The 95% confidence intervals are computed from heteroskedasticity-robust standard errors.

unexpected. Final consumption demand by households accounts for only a portion of this gross output measure: VAT records also cover sales of investment goods and intermediates to other firms, and both are more volatile than consumption at the business cycle frequency.

Relative to both consumption and sales, the quantitative response of employment, in panel c, is less pronounced in the first quarter after the shock. Although the response flattens out temporarily, it remains significantly negative. Employment eventually drops persistently and significantly (in both statistical and economic terms) from day 240 to day 460, before recovering 550 days after the shock. Overall, the decline in employment takes much longer to fully materialize, and is much smoother and more persistent than the response of either consumption or sales. But the three series by and large align at long lags.

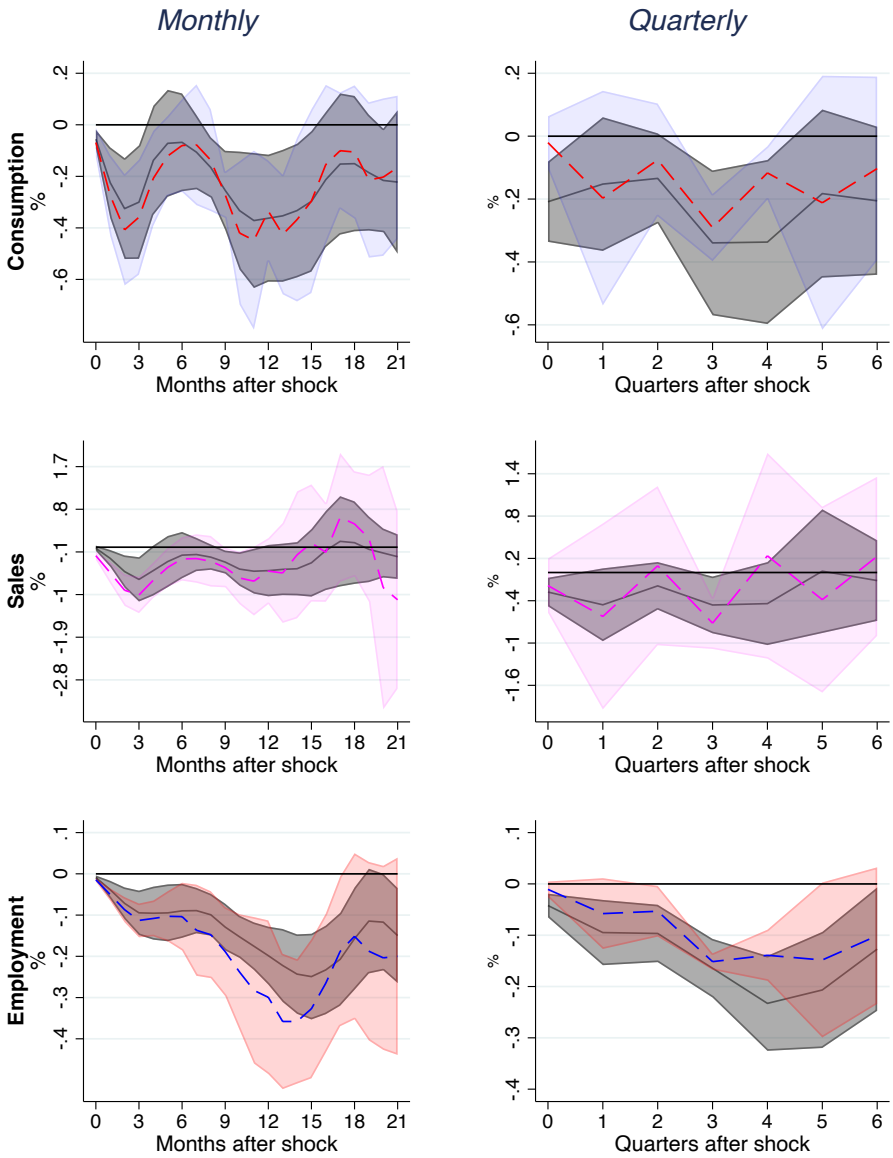
The takeaway is straightforward. Leveraging higher frequency data, we show that monetary policy shocks transmit to the economy at both short and long lags. The differences in the timing and intensity response of consumption demand, corporate sales and employment point to significant dynamic effects of monetary policy, raising a number of intriguing questions about the economics of monetary transmission. The initial drop in consumption[11] demand is followed, with a few weeks delay, by a much larger drop in sales. Neither however aligns with the upfront contraction in employment. The responses of consumption and employment become statistically indistinguishable, and negative at around -0.2%, only 420 days (14 months) after the shock—at this long lag, however, the response of sales has reverted to zero. The lagged and smoother response of employment may reflect labor market frictions (such as costs of firing or labor contracts) and/or the way firms react to the shock and subsequent changes in demand conditions.

3.2 Time Aggregation Tends to Hide the Short and Variable Lags

Most economic activity-related series are available only at monthly and quarterly frequencies. How would our results differ if we apply our empirical model to aggregation at monthly or quarterly frequency of our high-frequency data? In this subsection, we take advantage of our unique dataset to shed empirical light on potential issues arising from time aggregation of data. In particular, we aggregate (by averaging) our daily IRFs into monthly and quarterly IRFs, and then compare them with the IRFs estimated using

[11] With Spanish data, relying on different methods and samples, and focusing on nondurable consumption only, Slacalek, Tristani and Violante (2020) also finds evidence consistent with 1st quarter decline in consumption. Our findings suggest that their result is likely to be driven by a fall that starts just a few days and continues up to 93 days after the shock.

Figure 2: Monthly and quarterly response of consumption, sales and employment to a monetary policy shock



Notes: The monetary policy shock in this exercise is normalized to one standard deviation of monetary policy shocks at a daily frequency, so that its size is the same for the monthly and quarterly IRFs. The consumption (first row), sales (second row) and employment (third row) responses in levels are obtained from cumulating year-on-year changes. The 95% confidence intervals are computed from heteroskedasticity-robust standard errors. The left column shows the IRFs for the monthly frequency and the right column shows the IRFs for the quarterly. Dashed lines are the low-frequency data (monthly and quarterly) IRFs point estimates, while solid lines are the daily IRFs averages at the same monthly and quarterly frequencies.

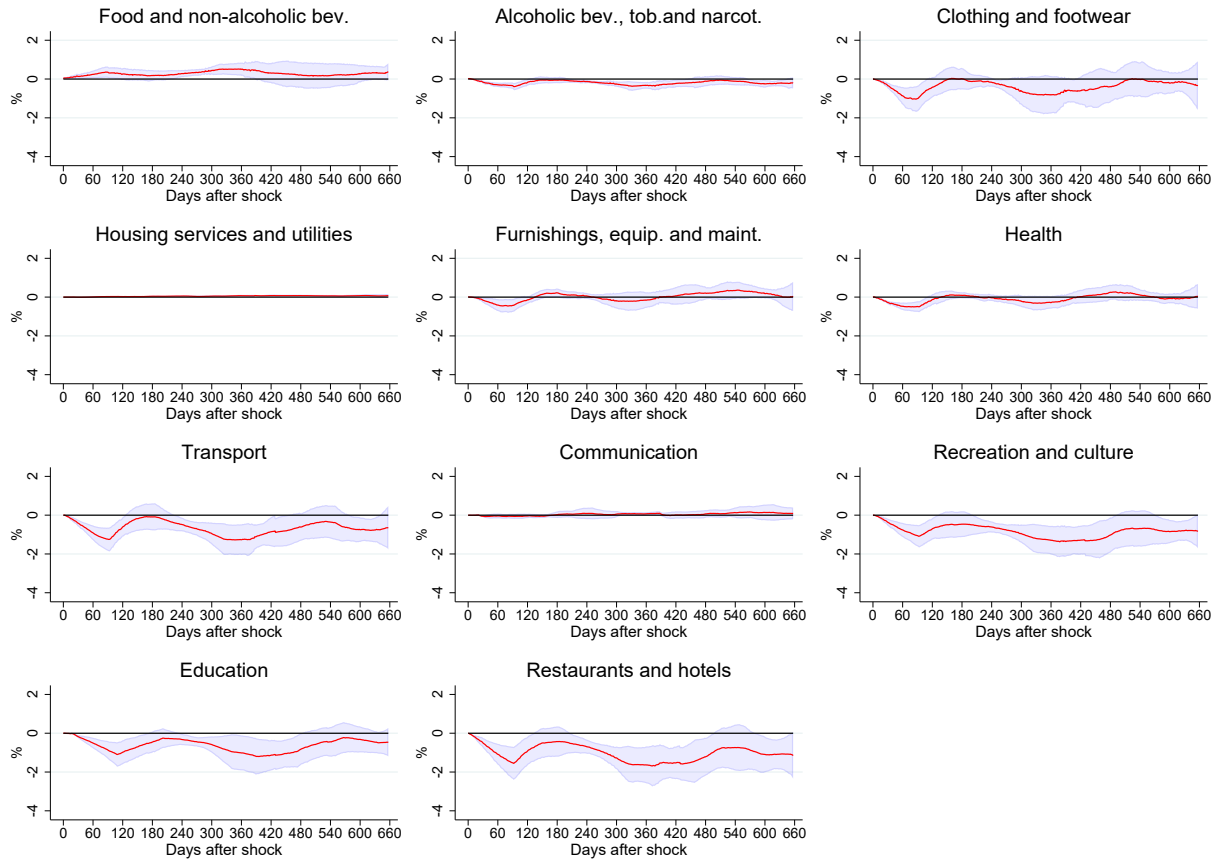
lower frequency, monthly and quarterly, series.

In Figure 2, we show the IRFs of aggregated (averaged) daily responses from our baseline, together with the IRFs obtained using lower frequency data for consumption, sales and employment. Results differ sharply at monthly vs. quarterly frequency. Working with data on a monthly frequency does not appear to make a difference. The point estimate of the consumption response using our aggregated daily series vs. monthly consumption nearly overlap—so do confidence intervals. For sales, the response of the monthly series is only slightly more negative in the first 5 months and nearly indistinguishable thereafter, up to 13 months after the shock. For employment, instead, the point estimates using monthly series align well over the first 6 months and subsequently tend to predict a more marked negative response. The overall pattern is nonetheless similar: both economically and statistically, the responses of all three variables tend to align well, so that monthly time aggregation does not seem to distort conclusions relative to the daily benchmark.

Using quarterly data, however, we find no significant same-quarter responses of consumption, sales or employment to monetary policy shocks, while we do so when using averaged daily responses. Quarterly aggregation shifts information in the data to lower frequencies: the first statistically significant response across all variables is detected only in the third quarter after the shock. Figure 2 also highlights differences in the persistence of responses: with time aggregation, the long lags seem less persistent—at longer horizons, the number of lags that are statistically significant is lower.

In sum, we find that time aggregation may confound the lags in the transmission of monetary policy, shifting information in the data to lower frequencies. This is a consequential empirical result for both theory and policy, stressing the need for further investigation into the root causes for this discrepancy. The problem may be relevant for a number of other variables. Complementing our own results, Jacobson, Matthes and Walker (2022) find that, in contrast to previous findings using lower-frequency data, the perverse response of daily inflation to high-frequency monetary policy shocks is short-lived, if present at all. More generally, as discussed in the Introduction, our results evoke the importance of classical results – in a literature straddling applied macroeconomics and time series – on the consequences of time aggregation. Echoing early findings by Christiano and Eichenbaum (1987), we conclude that our results lend empirical support to the concern that "temporal aggregation bias can be quantitatively important in the sense of significantly distorting inference" (Christiano and Eichenbaum, 1987, p.63).

Figure 3: Daily response of consumption by category to a monetary policy shock



Notes: The monetary policy shock is one standard deviation. The consumption responses in levels are obtained from cumulating year-on-year changes. See Buda et al. (2022) and the Online Appendix A for further details on how the consumption categories were constructed and on their cross section and time series characterization. The 95% confidence intervals are computed from heteroskedasticity-robust standard errors.

3.3 Heterogeneous Effects of Monetary Policy Shocks Across Consumption and Sales Categories

We now provide insight into the short and variable lags of monetary policy, studying the IRFs of disaggregated consumption and sales categories.[12] Transaction data - be it for sales or consumption - are dense enough in the cross-section to allow finer, disaggregated cuts that are typically unavailable at high-frequency (e.g., consumption series are often

[12]See Buda et al. (2022) for details on how the consumption categories are constructed; see Agencia Tributaria (2023) for further information on how the Spanish Tax Authority labels sectors of activity.

constructed from slow-moving survey data).

Results, shown in Figure 3, point to sharply heterogeneous responses, both in intensity and persistence, for different categories of final household consumption.Durable and semi-durable goods (clothing and footwear, and transport[13]) and luxury goods (restaurants and hotels, recreation and culture, and education)[14] are the two categories of consumption that fall more significantly in response to a contractionary monetary policy shock. The response of essential goods (such as food and non-alcoholic beverages, health-related and communication) is, instead, subdued. On average, durable and luxury goods decline between 1% and 2%; the response of essential goods ranges between -0.25% and 0%. Remarkably, the consumption of food and non-alcoholic beverages, and housing services and utilities actually displays a slight increase after the shock. We conjecture that the positive response in the category of food and non-alcoholic beverages reflects substitution away from restaurant consumption. The relatively small increase in housing services and utilities, which becomes significant 78 days after the shock with a point estimate of 0.025%, peaking at 0.08% approximately one year after the shock, is consistent with the findings by Dias and Duarte (2019), Corsetti, Duarte and Mann (2022), and Dias and Duarte (2022). These studies show that contractionary monetary policy shocks increase housing rents.

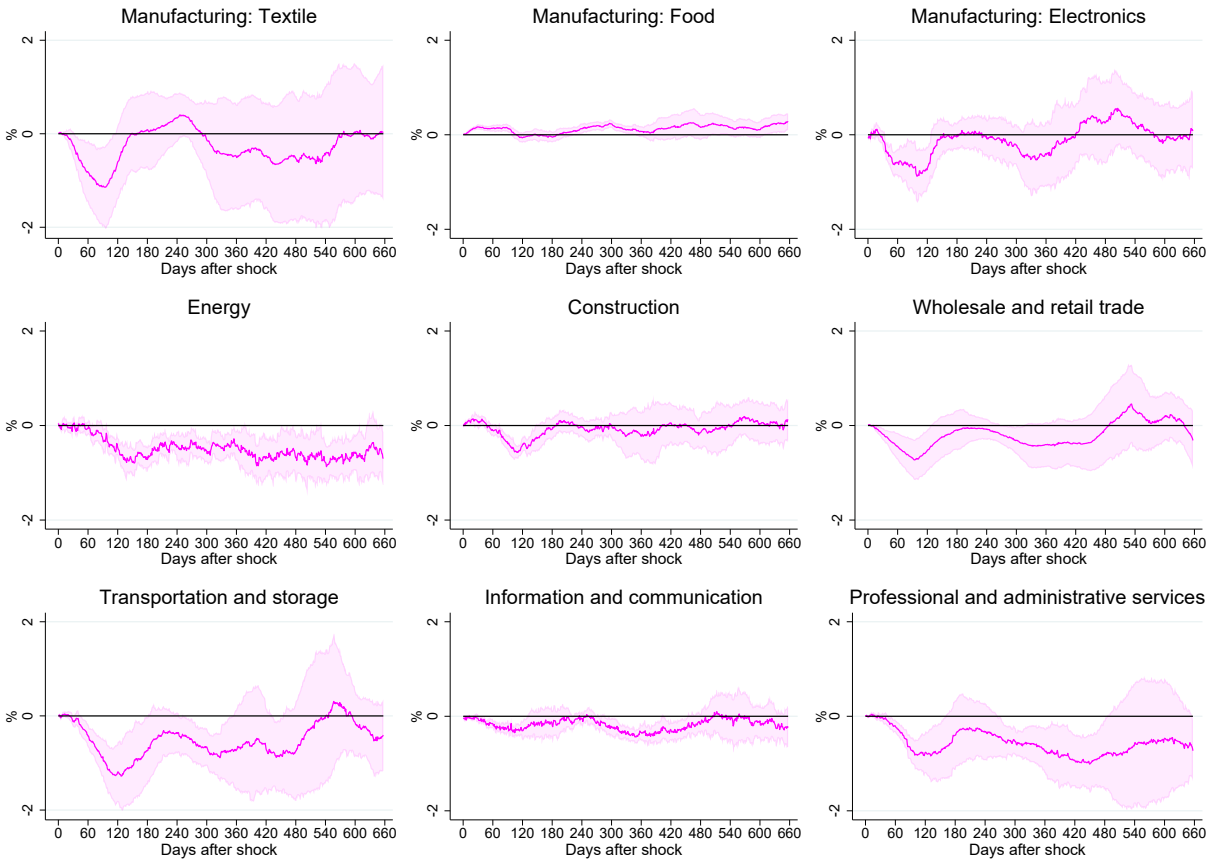
A second difference in the responses of essential goods vs. durable and luxury goods is the persistence of the contraction. While the demand for essential goods – however small – only declines in the first 120 days following the contractionary shock, the demand for luxury goods and durables (with the exception of furnishings, equipment and maintenance) falls until 480 days after the shock— with the contraction being relatively more persistent for luxury goods. Overall, these findings are in line with the literature that has long documented that the response of consumption to monetary policy shocks is mostly driven by the consumption of durable goods—see, e.g., Erceg and Levin (2006), Monacelli (2009), Sterk and Tenreyro (2018), and McKay and Wieland (2021).

We should note that the criteria used in constructing housing services and utilities and communication consumption may weigh on our findings—that the response for these two categories is not significant at short lags. This is because, for these series, the daily consumption is computed by either imputing or distributing the monthly/bi-monthly

[13]We classify the transport category as semi-durable because it includes not only vehicle purchases, but also expenditures related to transportation services—e.g. monthly public transportation pass—which are nondurable. Using a series specific to sales of vehicles, in the Online Appendix D we show that its response is very similar to the response of consumption of "Transport" services.

[14]In Spain, the large majority of education services are publicly provided at low or no cost to the end user. The transactions related to education in our data are mostly related to private education, which we consider a luxury good in the Spanish case.

Figure 4: Daily response of sales by sector to a monetary policy shock



Notes: The monetary policy shock is one standard deviation. The sales responses in levels are obtained from cumulating year-on-year changes. See the Online Appendix A for further details on sales sectoral classification. The 95% confidence intervals are computed from heteroskedasticity-robust standard errors.

payments over the days of the month/months. Two comments are in order. First, while the data construction procedure could create an artificial delay in the response, it would not prevent our model from eventually detecting a significant response. Second, the finding that, for housing services and utilities and communication, the responses are very small, can be cross-checked with the response for communication based on daily sales data (for the information and communication sector), that are not constructed following the same criteria. The fact that the empirical response of sales is also very small, suggests that our results are not determined by the data construction criteria.

The sectoral sales responses to a monetary shock, reported in Figure 4, draw a pattern that is broadly consistent with the response of consumption, especially in the case of

"Wholesale and retail trade", "Transportation and storage", "Information and communication" and "Professional and administrative services". Recall the key difference between the responses of aggregate consumption and aggregate sales: sales take longer to react to monetary policy shocks. This difference seems to be exacerbated for sectors that are more upstream in the production chain. For example, the "Energy" and "Construction" responses only become significantly negative after 70 days. Relatively less upstream sectors, such as "Transportation and storage", "Information and communication" and "Professional and administrative services", only fall significantly after 50 days. It is the sectors closer to the final household demand, "Manufacturing: Textile", "Manufacturing: Electronics" and "Wholesale and retail trade" that respond at shorter lags—they begin to decrease significantly after just 35, 37 and 16 days, respectively. Note that durable goods, highly represented in the sales of manufactured goods "Textile", "Electronics" and "Construction", are negative only at short lags, mirroring the consumption response of "Clothing and footwear" and "Furnishings, equip. and maint." displayed in Figure 3. Standing alone, instead, the response of "Energy" remains significantly negative over the 660 day horizon in the graph and the response of "Manufacturing: Food" is positive at short and long lags, following almost the exact same pattern as the response of food and non-alcoholic beverages consumption.

3.4 Robustness Checks

Monetary Policy Shocks We investigate whether our results are robust to the way we identify monetary policy shocks. First, in Online Appendix B.2 we show that our results do not depend on the poor man's approach to clean for the informational channel of monetary policy. We find that keeping all shocks produces virtually identical IRFs across all variables. Hence, the information channel of monetary policy does not appear to play a relevant role during our baseline sample, corroborating Altavilla et al. (2019) conclusions that the information channel is less relevant after 2014 in the EA. Second, we show our results are unchanged if we account for the information channel with shocks identified as in Jarociński and Karadi (2020) via sign restrictions. Finally, we show that our results are robust to using 3-month or 2-year instead of 1-year yields.

Seasonality In our baseline, we smooth the series by taking 90-day backward-looking moving averages before computing the year-on-year growth rates. In the Online Appendix B.1, we show that our results are robust to using a 30-day moving average instead. However, using daily data may still be problematic given, for example, holidays

falling on variable dates or differences in the number of days in leap years. In the Online Appendix B.4, we show that fitting all daily data in 53 weeks in each year, and estimating the regression at a weekly frequency does not change the results. We also show that our weekly results are robust to using the series in levels while controlling for seasonality directly with week-of-the-year dummy variables.

COVID-19 A considerable part of our sample (about one third) is affected by the COVID-19 pandemic outbreak. For this reason, in our regressions, we include as controls the stringency of the policy measures, such as the lockdown implementations, and the number of new COVID-19 cases—these controls should account for the large fluctuations in employment and especially consumption and sales that this abnormal period created. In the Online Appendix B.3, we reconsider our results omitting these controls (stringency and cases), and show that the overall conclusions are unaffected. Furthermore, during the pandemic, most countries approved significant government support packages to help households. In the same section of the Online Appendix, we show that including controls for household support does not change our results. More importantly, monthly industrial production (IP) data is available for Spain over a sample period that starts and ends before the COVID-19 outbreak—from 1999 to 2019. We find that the IRFs are very similar to the monthly responses of IP in our baseline sample (2016-2021). Furthermore, the response of GDP and IP at monthly frequency documented by Jarociński and Karadi (2020) and Miranda-Agrippino and Ricco (2021) for the EA and U.S., respectively, are similar in shape to our IRFs for consumption and sales.

Further Checks and Extensions In the Online Appendix, we also show our findings are robust to using an alternative number of lags of the dependent variable and that our main findings are not driven by how we deflate nominal consumption and sales expenditures. As for time aggregation issues, and consistently with our results, we further show that when we use official quarterly household consumption data, we no longer detect significant responses at short lags: the response becomes statistically significant only 2 quarters after the shock. Finally, in the Online Appendix B.5, we also find significant short lags when we extend our analysis to a weekly indicator of overall economic activity in Germany, compiled by the Bundesbank (Eraslan and Götz, 2021).

4 Conclusion

The increasing availability of high-frequency economic activity data yields a number of critical benefits for research and policymaking. One of them consists of creating new opportunities to study how monetary shocks drive economic dynamics at variable time horizons, and at a very disaggregated level. In this paper, we take one step into this new territory, in a study where the frequency of economic activity data is aligned with the frequency at which monetary policy shocks are identified around monetary policy announcement events.

Our empirical analysis suggests that the time aggregation of economic activity and monetary policy shocks may alter the identification of monetary policy transmission, shifting the empirical response to longer lags. The issues in time aggregation we document in our paper are relevant to a large body of a modern literature that routinely aggregates identified monetary policy shocks around policy announcements to monthly, quarterly or yearly frequencies. In these cases, time aggregation may severely weigh on the result and limit our ability to identify monetary transmission. As we show in the text, transaction data also hold promise in shedding light on the heterogeneous impact of monetary policy, as it can speak to rich cross-sectional heterogeneity at a high frequency.

Looking ahead, the increasing availability of high-frequency data should make it possible to extend and replicate the analysis across countries and over longer time series. This new research may well find heterogeneity in the response—as a function of, say, the type of mortgage prevailing in the country (in Spain, variable rate mortgages are predominant), e.g., see Corsetti, Duarte and Mann (2022). While results may differ depending on the sample, we conjecture that, in line with both our findings and the many recent studies highlighted in the introduction, short lags will still be detected. Overall, our findings present a challenge to both theoretical and empirical work on the transmission of monetary shocks, underscoring the need to investigate how these shocks affect different components of demand at different time horizons, and how demand eventually drives overall economic activity.

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Online Appendix for "Short and Variable Lags"

Contents

A Data **A-1**

A.1 Descriptive Statistics A-1

A.2 Additional Data Details A-5

B Robustness Checks **A-7**

B.1 Different Specifications A-7

B.2 Different Monetary Policy Shocks A-10

B.3 Impact of COVID-19 . . . . . A-15

B.4 Weekly Impulse Response Functions A-18

B.5 Other Economic Indicators A-21

B.6 Including 2022 Monetary Policy Shocks A-27

B.7 Nominal Responses A-28

B.8 Heteroskedasticity and Autocorrelation Consistent Standard Errors A-28

C Consumption Response by Payment Type **A-30**

D Other Sectoral Sales Responses **A-32**

E Recovering impulse-response functions in levels from impulse-response functions in year-on-year growth rates **A-34**

A Data

A.1 Descriptive Statistics

Table A1 shows the descriptive statistics for total consumption, total sales and employment growth rates. In our baseline sample, total real consumption grew on average 2.69% yearly, total real sales 0.89%, and employment 2%—halfway between sales and consumption growth rates. We also note that, as expected, employment is the least volatile time series, sales the most volatile series. This is expected because employment is typically slow-moving and sales are a gross output measure that includes intermediate

inputs and investment expenditures, which are known to be more volatile than consumption in low-frequency data. Finally, reflecting the COVID-19 pandemic crisis, there are large variations in our baseline sample with consumption and sales falling by 20.5% and 33%, respectively. Note that in our main findings, consumption and sales fall at most by 0.4% and 0.8% following one standard deviation contractionary shock. This suggests that monetary policy shocks played a very minor role in determining consumption and sales total variation in our sample—a result commonly found in the literature.

Table A1: Descriptive statistics of the main variables

	Mean	SD	Min	Max
Total consumption	2.69%	5.99%	-20.49%	20.10%
Total sales	0.89%	12.23%	-33.39%	30.35%
Employment	1.99%	2.30%	-4.54%	4.22%

Notes: Consumption, Sales and Employment measured as YoY growth rates of their 90-day moving averages. Consumption and Sales are deflated using the overall Consumer Price Index.

Turning to the disaggregated consumption and sales series, Table A2 lists the COICOP consumption subaggregates used in this paper, while Table A3, shows summary statistics by consumption category. We note that, as expected, the weighted average (by spending volume) of the categories imply an aggregate consumption growth rate of 2.62%, consistently with Table A1. Finally, Table A4 shows the NACE classification of sales data from the the Spanish Tax Authority. Descriptive statistics for each sector are presented in Table A5. In our sample, we note that some sectors experienced negative mean real growth rates in sales: textile, electronics, transportation and storage and professional and administrative services. Finally, again the weighted average growth of these sales categories aggregates to the total sales growth in Table A1.

Table A2: COICOP consumption categories (two-digit)

Category	Description
01	Food and Non-Alcoholic Beverages
02	Alcoholic Beverages, Tobacco, and Narcotics
03	Clothing and Footwear
04	Housing, Water, Electricity, Gas, and Other Fuels
05	Furnishings, Household Equipment, and Routine Household Maintenance
06	Health
07	Transport
08	Communication
09	Recreation and Culture
10	Education
11	Restaurants and Hotels

Notes: This table displays the 11 COICOP categories we use for classifying consumption transactions. In line with the Spanish Statistical Office, we use the European COICOP system in place of the international COICOP system. The main difference is that the latter has two separate categories for Insurance and financial services and Personal care, social protection and miscellaneous goods and services which in ECOICOP are merged into a single Miscellaneous Goods and Services category.

Table A3: Descriptive statistics, COICOP consumption categories (two-digit)

Two-Digit Category	Mean	SD	Min	Max
01	10.78%	9.95%	-12.34%	35.14%
02	4.27%	8.68%	-19.00%	28.45%
03	0.38%	16.20%	-67.92%	57.06%
04	0.96%	1.84%	-6.52%	3.61%
05	4.09%	8.82%	-25.43%	35.38%
06	11.77%	9.12%	-16.88%	35.79%
07	6.20%	18.36%	-69.99%	65.06%
08	3.67%	7.35%	-12.62%	26.32%
09	2.33%	17.93%	-64.76%	45.14%
10	4.01%	17.74%	-63.93%	61.01%
11	4.27%	21.99%	-84.89%	74.04%

Notes: Consumption Categories measured as YoY growth rates of their 90-day moving averages. Categories are deflated using the Consumer Price Index at the respective COICOP level.

Table A4: Sales sectors, NACE code, and description

Sector	NACE code	Description
Manufacturing: Textile	C13 + C14 + C15	Manufacture of textiles, wearing apparel and leather products
Manufacturing: Food	C10	Manufacture of food products
Manufacturing: Electronics	C26 + C27	Manufacture of computer, electronic, optical products and of electrical equipment
Energy	D	Electricity, gas, steam and air conditioning supply
Construction	F	Construction
Wholesale and retail trade	G	Wholesale and retail trade; repair of motor vehicles and motorcycles
Transportation and storage	H	Transportation and storage
Information and communication	J	Information and Communication
Professional, scientific and administrative	M + N	Professional, scientific and technical activities

Table A5: Descriptive statistics of sales by sector

Sector	Mean	SD	Min	Max
Manufacturing: Textile	-3.86%	19.11%	-64.96%	49.19%
Manufacturing: Food	0.28%	4.60%	-15.59%	7.17%
Manufacturing: Electronics	-6.18%	14.74%	-37.42%	31.73%
Energy	4.37%	23.90%	-35.55%	63.90%
Construction	5.31%	10.69%	-23.06%	28.82%
Wholesale and retail trade	1.51%	10.94%	-31.85%	31.71%
Transportation and storage	-3.55%	21.81%	-62.88%	40.28%
Information and communication	1.17%	7.61%	-16.47%	17.83%
Professional and administrative services	-0.79%	15.56%	-40.20%	31.26%

Notes: Sales by sector measured as YoY growth rates of their 90-day moving averages. Manufacturing and construction are deflated with the Producer Price Index; Wholesale and Retail Trade and Transportation and storage with the Consumer Prices Index and the remaining (service) sectors with the Service Price Index.

A.2 Additional Data Details

Consumption The following provides a brief overview of the consumption data used in this paper—the interested reader is referred to Buda et al. (2022) for a detailed discussion.

The underlying data source of Buda et al. (2022) is the universe of bank account outflows of Spanish residents who hold a retail account with BBVA. This is supplemented by extensive metadata on both bank clients and individual transactions. Specifically, the baseline sample consists of the observed bank transactions for 1.8 million BBVA 'active customers' – defined as bank clients that made at least ten consumption-related transactions in each quarter of the sample – excluding any self-employed individuals.

Based on this data, the construction of a proxy for aggregate consumption involves two main steps. First, since not every account outflow of these customers represents a consumption expenditure, Buda et al. (2022) relies on the metada associated to each transaction, to implement national accounting principles from the European System of Accounts and classify whether an individual transaction corresponds to consumption—as opposed to, e.g., savings, investment or tax payments. The exception to this are cash withdrawals which are assumed to serve for consumption expenditures alone. Further, and again following European System of Accounts' recommendations, Buda et al. (2022) additionally impute housing services to all customers. This is done by extrapolating – from a sub-sample of renters – the observed relation between observed rents and location, utilities' spending and household income.

Second, since the population of BBVA retail customers differs from the Spanish adult population along observables, Buda et al. (2022) obtain aggregate consumption, by summing over individual consumption using sample weights at the gender-age-neighborhood cell level.

Finally, further exploiting metadata associated to each consumption transaction Buda et al. (2022) show how to construct category-specific consumption series, following the European COICOP system. As shown in Buda et al. (2022), the implied distribution for category-specific consumption series matches well its official national accounts' counterpart.

Employment The data includes all working population except some self-employed in regulated professional associations (such as lawyers, architects or engineers) who may opt out of the main Social Security System. These account for less than 1% of the employed population. Further note that a given worker may have more than one active contract in the system (e.g. someone maintaining two part-time jobs). Therefore this se-

ries tracks the total number of active jobs registered in the Social Security system rather than the total number of employees.

B Robustness Checks

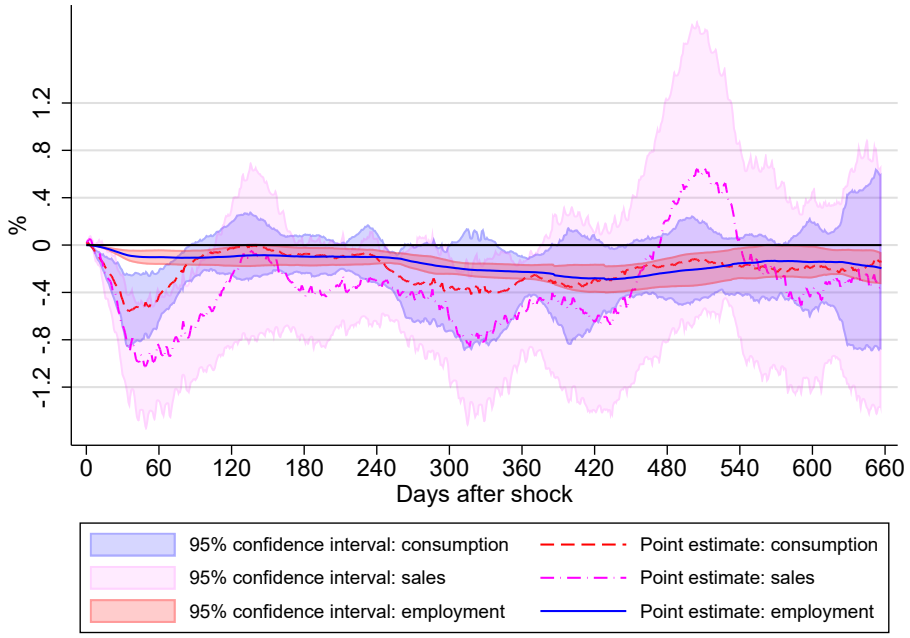
B.1 Different Specifications

The main results are robust to a number of alternative specifications of the empirical model. Here, we present details for two robustness exercises in which we change, in turn, (i) the moving-averages used to smooth consumption, sales and employment prior to taking their year-on-year growth rates, and (ii) the lags included in the main regressions.

In Figure B1 we show the impulse responses when, instead of computing the 90-day moving averages of consumption, sales and employment before taking their year-on-year growth rates, we use a 30-day moving average. The responses become only slightly noisier for consumption and sales; in particular our short run characterization remains unchanged.

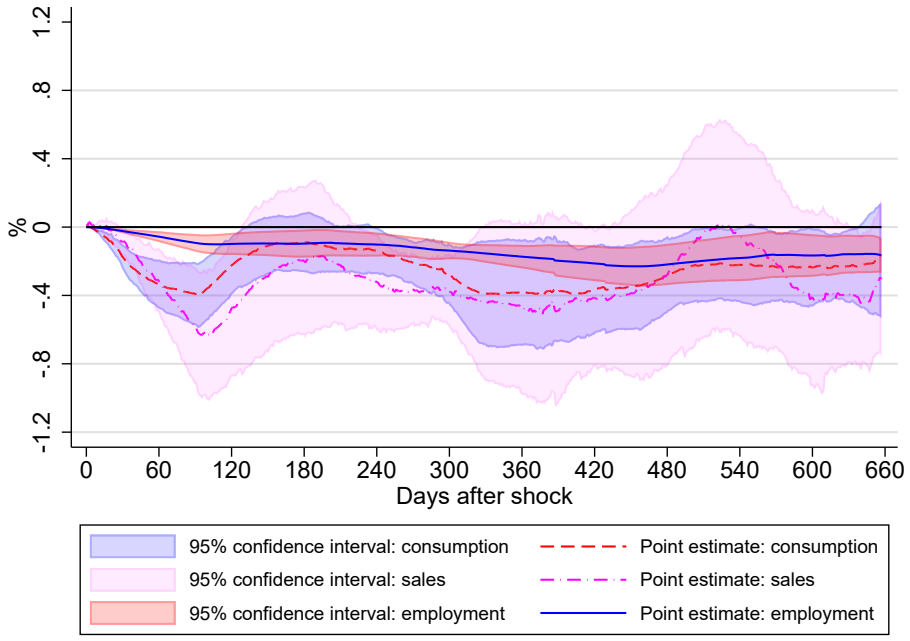
Additionally, we calculate the local projections with a different lag specification on the regression — 30 days instead of 90, as in the baseline case — and show that results are robust to these changes. In Figure B2, we show the impulse responses for this case. Again, results remain practically unchanged.

Figure B1: Daily response of consumption, sales and employment to a monetary policy shock, 30 day moving average



Notes: The monetary policy shock is one standard deviation. The consumption, sales and employment responses in levels are obtained from cumulating year-on-year changes. The 95% confidence intervals are computed from heteroskedasticity-robust standard errors. This specification takes a 30-day moving average (instead of the baseline 90-day) of the endogenous variables before computing their year-on-year growth rates.

Figure B2: Daily response of consumption, sales and employment to a monetary policy shock, controlling for 30 lags of endogenous variable



Notes: The monetary policy shock is one standard deviation. The consumption, sales and employment responses in levels are obtained from cumulating year-on-year changes. The 95% confidence intervals are computed from heteroskedasticity-robust standard errors. This specification includes in the regression 30 lags (instead of the baseline 90 lags) of the endogenous variables.

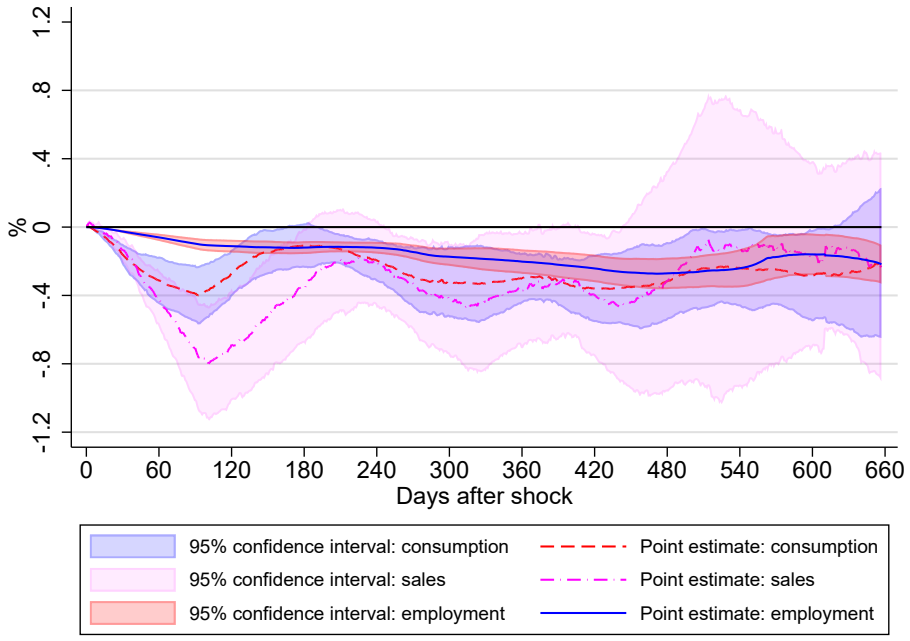
B.2 Different Monetary Policy Shocks

The baseline identification of pure monetary policy shocks in Jarociński and Karadi (2020) uses sign restrictions as opposed to the simpler poor man's sign approach. We should note here that, in the exercises performed by Jarociński and Karadi (2020), the two procedures lead to similar impulse responses despite producing different series of shocks. As a robustness exercise, we re-estimate our consumption, sales and employment impulse responses using the baseline shocks identified in their paper. In line with their results, Figure B3 shows that the impulse responses remain mostly unchanged. The main difference is that the responses of all variables at long lags become more persistent—i.e. the responses are significantly negative at longer lags.

We choose the 1-year Overnight Index Swap rate as our baseline because it captures the various instruments available in the monetary authority toolkit—conventional changes in short-term interest rates and unconventional ones in the form of forward guidance and quantitative easing. Hence, the OIS1Y can be seen as a weighted average of the various fundamental dimensions of monetary policy shocks. Nevertheless, we show that our results are unchanged if we use shorter, 3-months OIS, or longer, 2-year OIS, rates. In Figure B4, we show that impulse responses are robust to using shocks to shorter maturities interest rates—using the changes in the 3-months Overnight Index Swap rates. Using the shorter interest rate, we note that the responses estimates are more precise in the first 420 days after the monetary policy shock. In Figure B5, we show that impulse responses are robust to using shocks to longer maturities interest rates—using the changes in the 2-year Overnight Index Swap rates. We find the responses are similar to the baseline estimates.

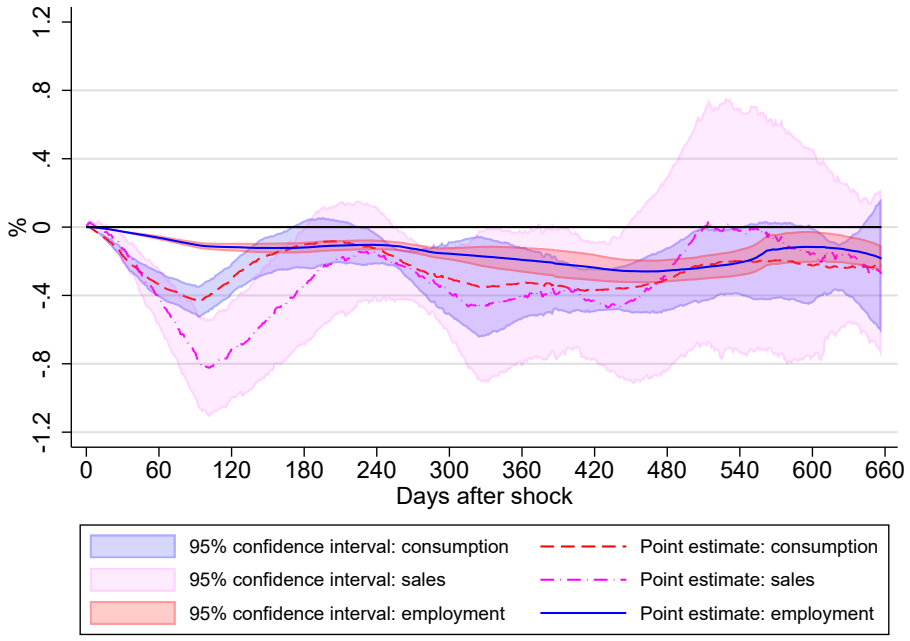
In addition, in Figure B6 we show that our results are not dependent on the poor man's approach. That is, if we keep all shocks in our sample, our IRFs are mostly unchanged. This is expected given that Altavilla et al. (2019) have shown that the new informational content of monetary policy announcements was mostly insignificant after 2014 when our sample starts in 2016.

Figure B3: Daily response of consumption, sales and employment to a monetary policy shock, Jarociński and Karadi (2020) shocks



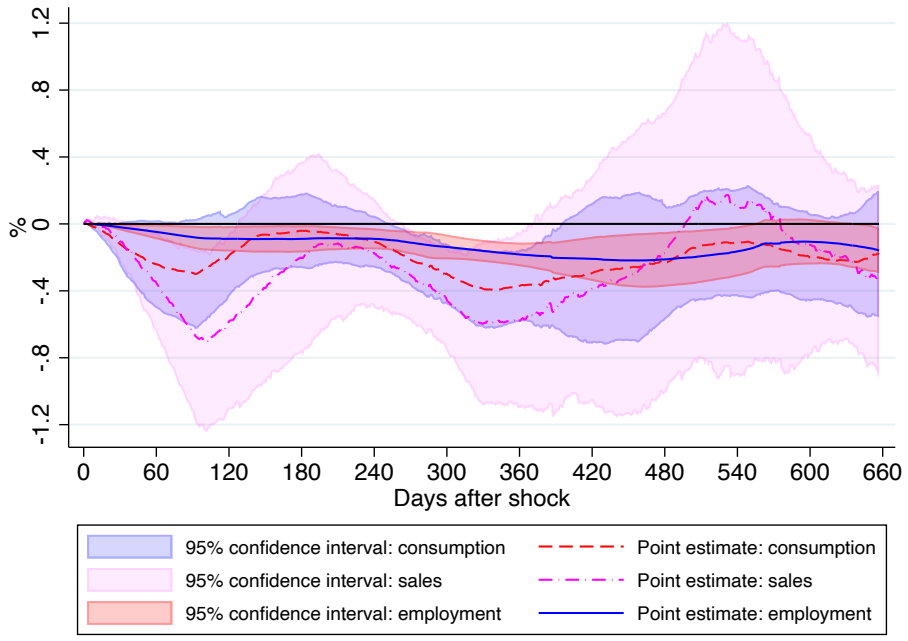
Notes: One standard deviation monetary policy shock. Jarociński and Karadi (2020) shocks series goes only until the beginning of June 2020. The consumption, sales and employment responses in levels are obtained from cumulating year-on-year changes. The 95% confidence intervals are computed from heteroskedasticity-robust standard errors.

Figure B4: Daily response of consumption, sales and employment to a monetary policy shock, shocks to the 3-months Overnight Index Swap rates



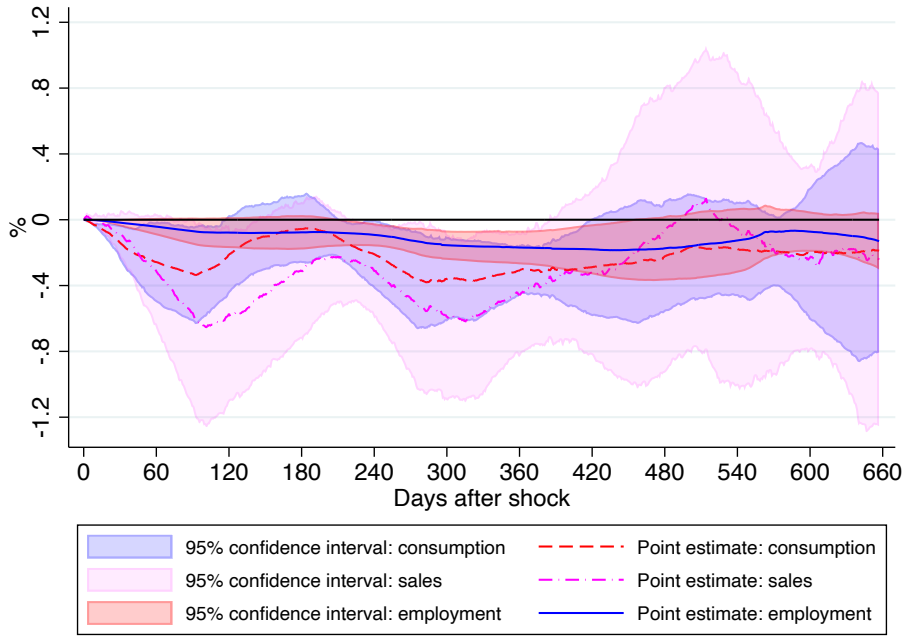
Notes: One standard deviation monetary policy shock. Monetary policy shocks identified by high-frequency movement of OIS3M rates around policy announcements. The consumption, sales and employment responses in levels are obtained from cumulating year-on-year changes. The 95% confidence intervals are computed from heteroskedasticity-robust standard errors.

Figure B5: Daily response of consumption, sales and employment to a monetary policy shock, shocks to the 2-years Overnight Index Swap rates



Notes: One standard deviation monetary policy shock. Monetary policy shocks identified by high-frequency movement of OIS2Y rates around policy announcements. The consumption, sales and employment responses in levels are obtained from cumulating year-on-year changes. The 95% confidence intervals are computed from heteroskedasticity-robust standard errors.

Figure B6: Daily response of consumption, sales and employment to a monetary policy shock, all shocks (no poor man's approach) to the 3-months Overnight Index Swap rates



Notes: One standard deviation monetary policy shock. Monetary policy shocks identified by high-frequency movement of OIS3M rates around policy announcements. The consumption, sales and employment responses in levels are obtained from cumulating year-on-year changes. The 95% confidence intervals are computed from heteroskedasticity-robust standard errors.

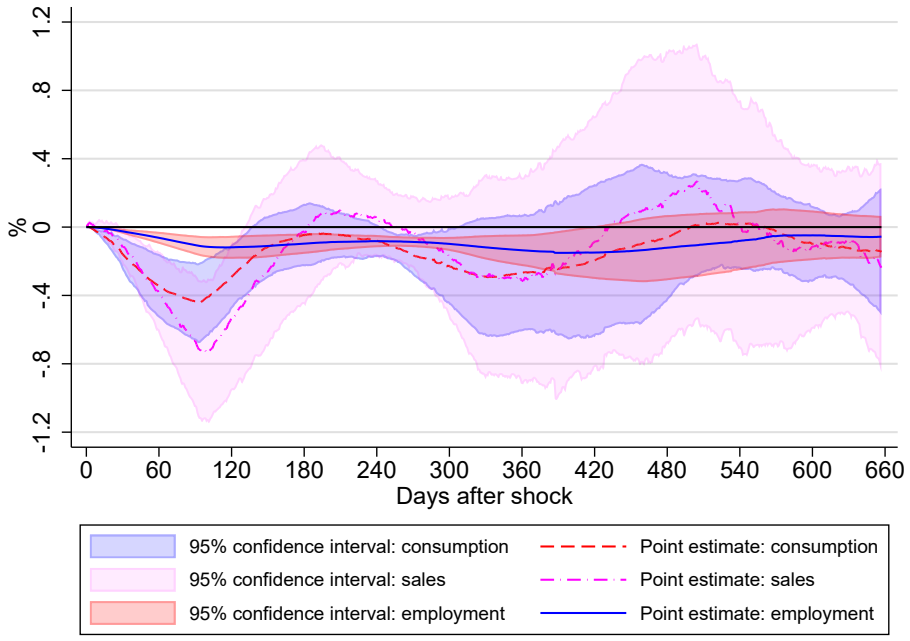
B.3 Impact of COVID-19

A considerable part of our sample (about one third) is affected by the COVID-19 pandemic outbreak. For this reason, in our regressions, we include as controls the stringency of the policy measures, such as the lockdown implementations, and the number of new COVID-19 cases—these controls should account for the large fluctuations in employment and especially consumption and sales that this abnormal period created. Here, we reconsider our results omitting these controls ($stringency_t$ and $cases_t$). Figure B7 shows that the overall conclusions on the "short and variable" lags of monetary transmission are unaffected—at long lags the recovery of both consumption and employment is much faster.

During the pandemic, most countries approved significant government support packages to help households. It might also be important to control for the size of these packages. To do so, we control for the size of the government support in Spain, using the University of Oxford's Coronavirus Government Response Track. This index tracks and compares a set of different policy responses to tackle COVID-19 by different governments and countries. Policies are ranked to reflect the degree of government intervention. The index starts in 1 January 2020, and is available for more than 180 countries. Figure B8 shows that including controls for household support does not change our results.

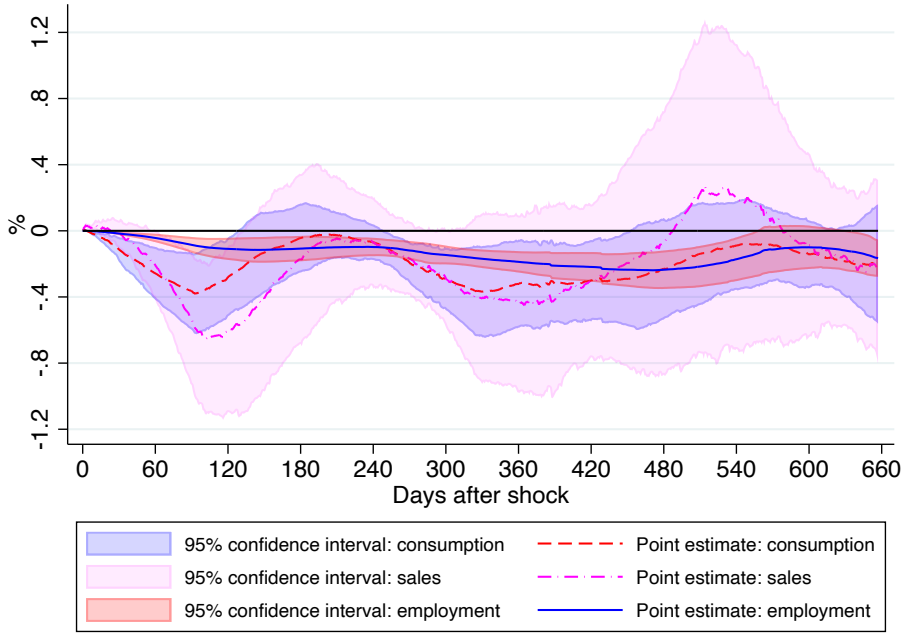
Finally, note that in section B.5 below, we also show that the IRFs of monthly Spanish Industrial Production (IP) are consistent with the (monthly) results we obtain for consumption and sales. This is relevant, as these official monthly series are available for a longer time period, thus enabling a comparison of responses across our baseline sample and a long pre-COVID period: we find statistically indistinguishable responses of IP, for both our baseline sample period and a pre-COVID sample. This additional check again suggests that our results are not driven by the COVID disruption.

Figure B7: Daily response of consumption, sales and employment to a monetary policy shock, excluding COVID-19 pandemic controls



Notes: One standard deviation monetary policy shock. This exercise excludes the control variables $stringency_t$ and $cases_t$, controlling only for the lagged endogenous variable. The consumption, sales and employment responses in levels are obtained from cumulating year-on-year changes. The 95% confidence intervals are computed from heteroskedasticity-robust standard errors.

Figure B8: Daily response of consumption, sales and employment to a monetary policy shock, controlling for government response



Notes: One standard deviation monetary policy shock. This exercise includes a control for the Spanish government response, an index created by the University of Oxford's Coronavirus Government Response Track. The consumption, sales and employment responses in levels are obtained from cumulating year-on-year changes. The 95% confidence intervals are computed from heteroskedasticity-robust standard errors.

B.4 Weekly Impulse Response Functions

In the main text, we show that significant 'short and variable' lags of monetary policy impacts on economic activity are no longer detected by our model when data are aggregated to a quarterly frequency. Aggregating to a monthly frequency instead did not seem to change our main conclusions — short-and-variable lag results were also statistically significant at impact in the monthly exercise. In Figure B9 we show that the same holds using weekly aggregation, with short-run responses being statistically significant as in the daily exercises.

An additional concern is that daily data may present calendar patterns that influence the impulse responses. The task of controlling for these effects at a daily frequency is quite complex. First, because each fourth year has a different number of days, computing year-on-year growth rates is not as precise as, for example, with weekly data. Second, some holidays fall on a different calendar day, requiring a precise identification of each one of them in different years. Finally, daily series also present intra-weekly seasonality, especially when including weekend days, which should also be taken into account. As discussed in the text, in our main exercises we control for holidays and leap year issues by taking the 90-day moving average and the year-on-year growth rates of variables.

However, aggregating consumption, sales and employment to a weekly frequency and computing their YoY (52 weeks) growth rates would also attenuate such concerns.[1] This way of dealing with seasonality at the weekly frequency is also followed by Lewis, Mertens and Stock (2020). In Figure B9, we had already shown that the IRFs that have exactly the same patterns as the IRFs based on daily data,[2] suggesting that calendar effects are not driving our results (which was already clear from the dynamics in Figure B9).

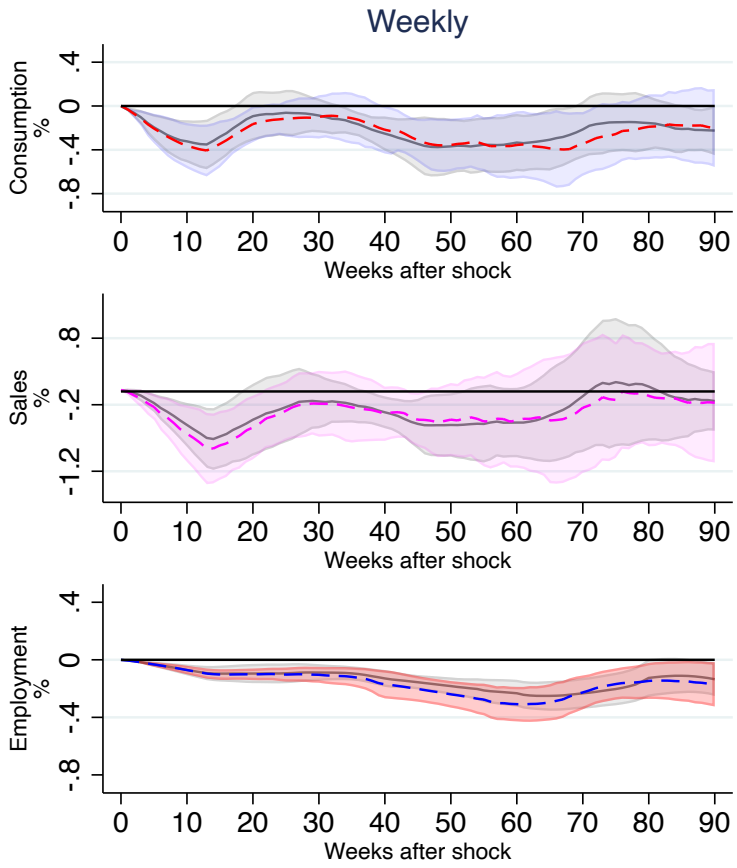
Given that the baseline exercises use the year-on-year growth rates of the endogenous variables, we get responses in percentage by accumulating IRFs as shown in E. However, we can also show that results are robust to using series in (log) levels and controlling for seasonality directly with a dummy variable tracking weeks in each year, as follows:

$$y_{t+h} - y_{t-1} = \alpha_h + \beta_h shock_t + \sum_{\ell=1}^{p} \varphi_{h,\ell} \Delta y_{t-\ell} + \theta_h cases_t + \delta_h stringency_t + week_h + \varepsilon_{h,t}, \quad (1)$$

[1] The trade-off of estimating the local projections at a weekly frequency to deal with seasonal effects is that, as documented in the paper, aggregation bias can be a problem even when moving from a daily to a weekly frequency.

[2] At a weekly frequency, the implicit standard deviation of the shocks is higher than the standard deviation of the same shocks, but computed at a daily frequency. Given that IRFs are normalized to a one standard deviation shock, this explains the higher magnitude of responses when using weekly data.

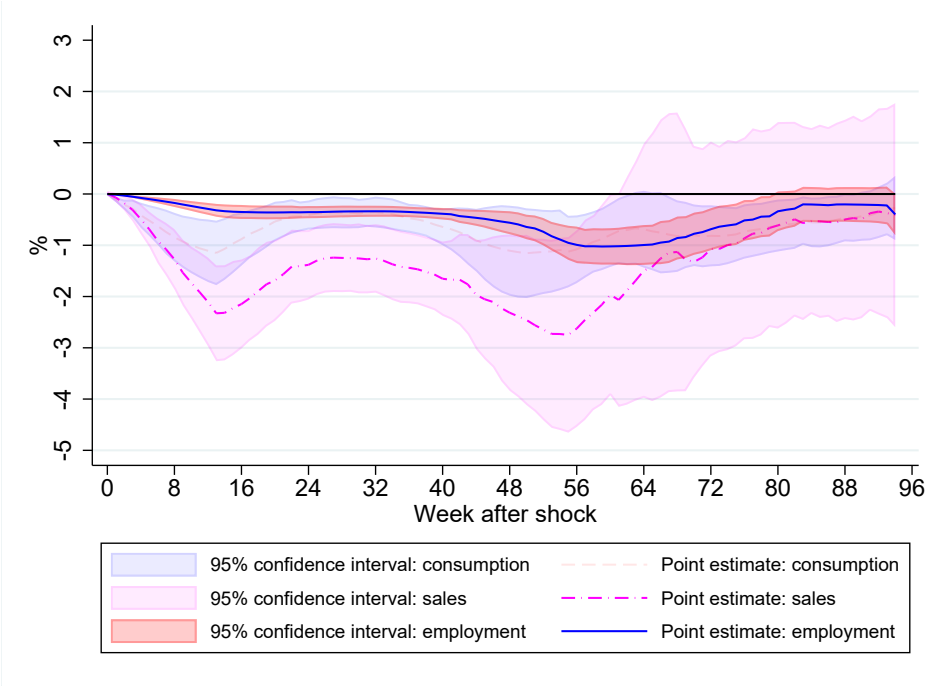
Figure B9: Weekly response of consumption, sales and employment to a monetary policy shock: aggregation to lower frequency



Notes: The monetary policy shock in this exercise is normalized to one standard deviation of monetary policy shocks at a daily frequency so that its size is the same for the weekly IRFs. The consumption, sales and employment responses in levels are obtained from accumulating year-on-year changes. The 95% confidence intervals are computed from heteroskedasticity-robust standard errors. Dashed lines are the low-frequency data (weekly) IRFs point estimates, while solid lines are the daily IRFs averages at a weekly frequency.

where *week* a dummy variable controlling for the week of the year. As shown in Figure B10 the results from this specification are similar to the results from the specification in the main exercises. As found in previous robustness checks, the main difference is, again, that the responses of all variables at long lags become more persistent—i.e. they present more lags where the responses are significantly negative.

Figure B10: Weekly response of consumption, sales and employment directly in levels to a monetary policy shock



Notes: The monetary policy shock is one standard deviation. Local projections include 26 lags of weekly endogenous variable. Responses are in percentage deviation in levels. The 95% confidence intervals are computed from heteroskedasticity-robust standard errors.

B.5 Other Economic Indicators

For external validity, we now compare our results with other economic activity indicators. We begin by studying the response to a monetary policy shock of the German Weekly Activity Indicator (WAI), compiled by the Bundesbank (Eraslan and Götz (2021)). The variables used to construct this German indicator include monthly industrial output and quarterly GDP, together with nine high-frequency indicators which are recorded on a weekly basis. When the variables have a daily frequency, they are aggregated by taking averages, to a weekly frequency. The high-frequency indicators are: "electricity" and "toll" (road charge), which capture production and trade, respectively; "Flights", as a proxy for global activity; "G-unemployment", "G-short-time work" and "G-state support", which are derived from Google search queries; "Pedestrian frequency" and "credit card payments", which capture parts of consumer behaviour; "Air pollution" which serves as a metric for the mobility sector. Variables are seasonal- and calendar-adjusted in advance and the index is reported as a trend-adjusted rolling 13-week growth rate. To estimate WAI responses to monetary policy shocks, we use the sample period as for the baseline exercises and normalize the monetary policy shocks to have the same standard deviation as in the baseline daily exercises. As Figure B11 shows, the IRF for the German WAI displays a similar pattern to the IRFs of consumption and sales we report in the paper.

In Figure B12, we compare our results with results using the monthly Spanish industrial production. For the same sample period as our main exercise, we find that the IRF has a similar pattern to that of corporate sales—we detect the presence of significant short and long lags. In addition, since industrial production data is available for a sample period that starts and ends before the COVID-19 outbreak, we also perform the same exercise but using a sample period from 1999 to 2019. Figure B13 shows that the industrial production IRFs are virtually the same across the two samples. Hence, our findings of short that variable lags in monetary policy transmission do not seem to be a byproduct of our baseline sample.

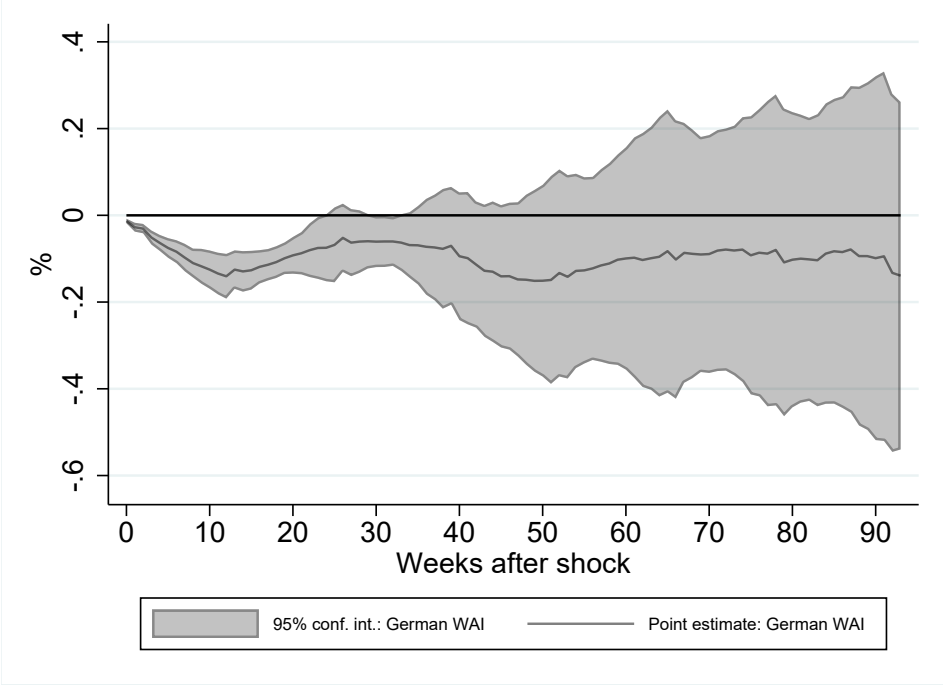
In Figure B14, we plot the IRF of quarterly household consumption from Spanish national accounts data[3], together with the response of our consumption data (aggregated to a quarterly frequency). As shown by Buda et al. (2022), the two raw series are highly correlated. Not surprisingly, the IRFs are also strikingly similar: the responses are statistically significant 2 and 3 quarters after the shock takes place. This exercise thus serves two main purposes: the first is external validity: both the official data for consumption

[3]For comparability with our results based on transaction data, we use non-seasonally adjusted household consumption, and compute the year-on-year growth rates.

and our proxy based on transaction data, when used at the same frequency, yield almost exactly the same responses to monetary policy shocks; the second refers to time aggregation: we only find long lags also when using official lower-frequency data—a result that strengthens the case for studying the transmission of monetary policy with higher-frequency data.

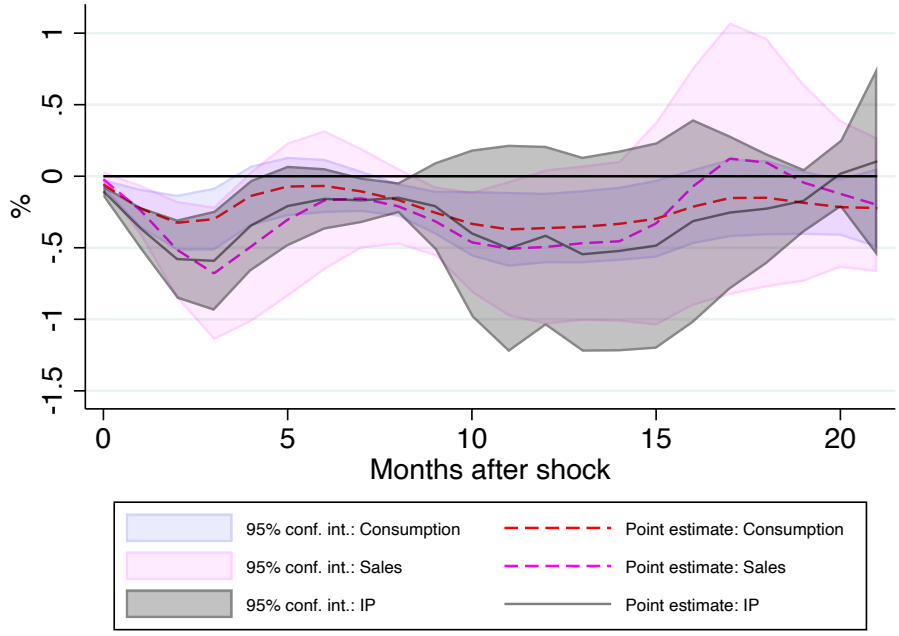
In Figure B15 we plot the quarterly IRFs of the consumption series from the national accounts, distinguishing non-durables consumption and durables consumption. As shown in the text, we find that the high-frequency response of consumption is mostly driven by the decline in durable goods consumption. The same is true when using official data: the negative response of household consumption is driven by the decline of durable goods consumption, 2 and 3 quarters after the shock. The response of non-durables consumption is not statistically significant.

Figure B11: Response of German Weekly Activity Indicator (WAI) to a monetary policy shock



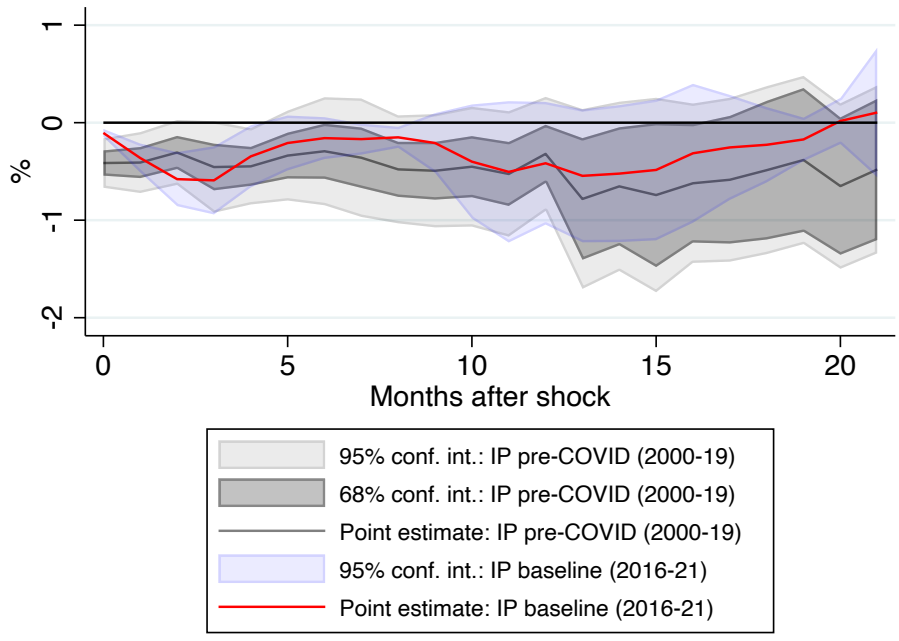
Notes: One standard deviation of monetary policy shocks at a daily frequency. The WAI response in levels is obtained from cumulating quarter-on-quarter changes. The 95% confidence intervals are computed from heteroskedasticity-robust standard errors.

Figure B12: Response of industrial production, consumption and sales to a monetary policy shock



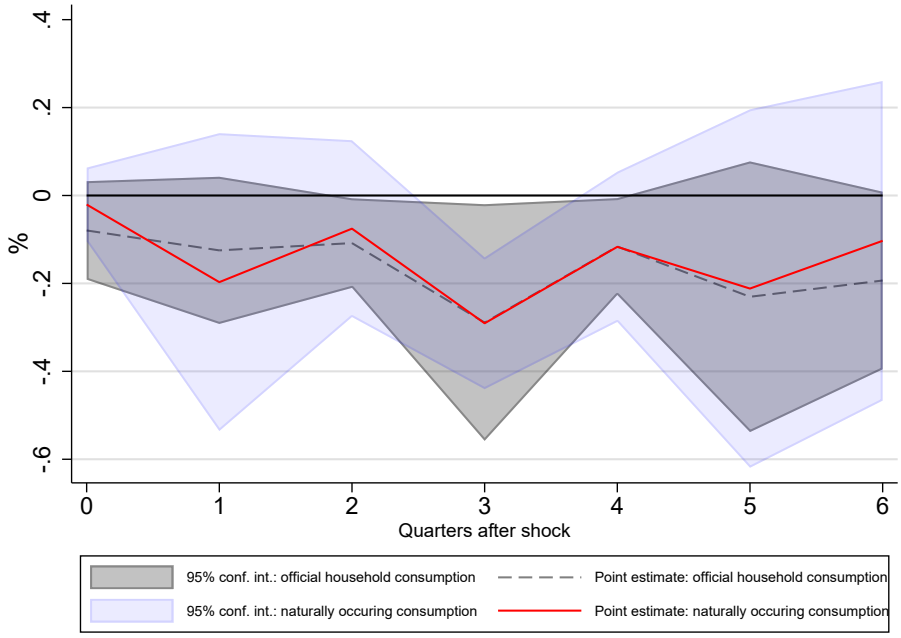
Notes: One standard deviation of monetary policy shocks at a daily frequency. The 95% confidence intervals are computed from heteroskedasticity-robust standard errors. Monthly consumption and sales IRFs are the response of the daily exercise averaged to the monthly frequency (similarly to Figure B9 for the weekly frequency).

Figure B13: Response of industrial production to a monetary policy shock (pre-COVID—1999-01 to 2019-12— sample) vs. industrial production's IRFs (baseline sample)



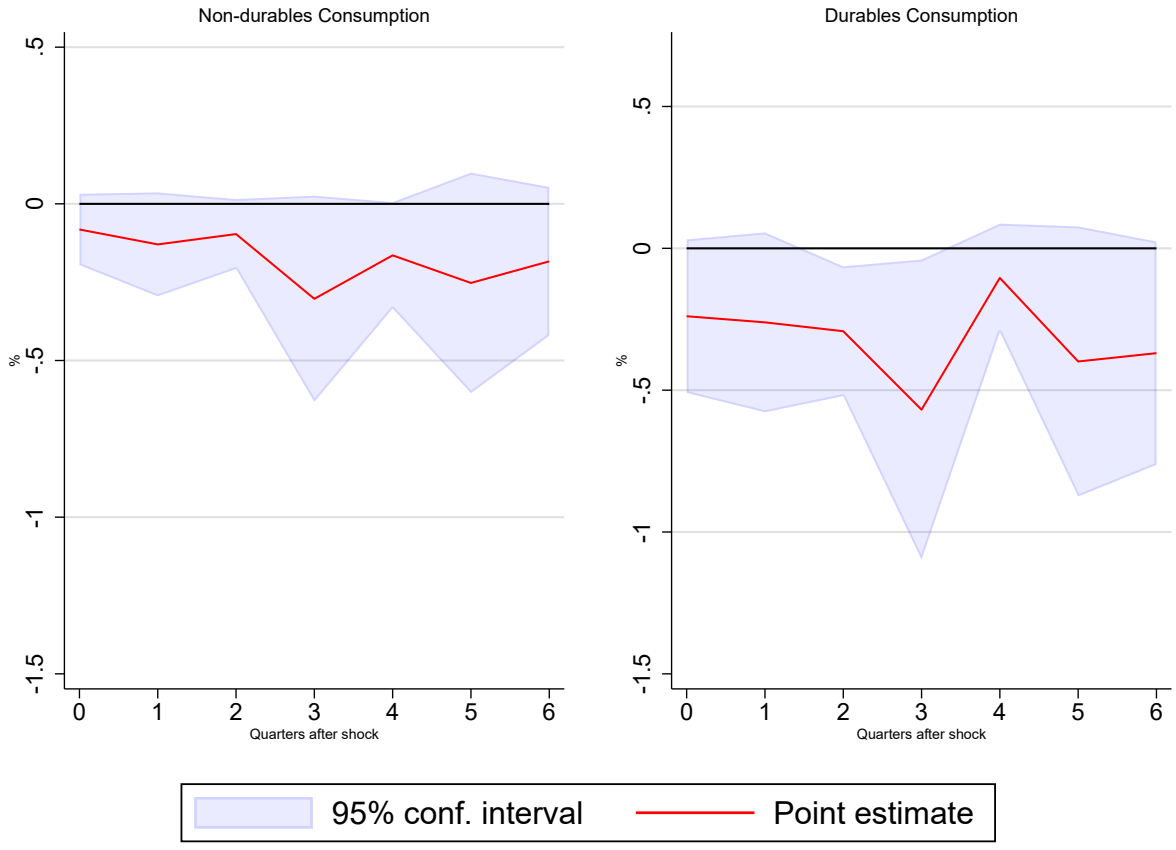
Notes: One standard deviation monetary policy shock. Confidence intervals are computed from heteroskedasticity-robust standard errors.

Figure B14: Response of quarterly national accounts household consumption data vs. our baseline consumption data to a monetary policy shock



Notes: One standard deviation of monetary policy shocks at a daily frequency. Both our baseline consumption data (aggregated to a quarterly frequency) and the official national accounts consumption responses in levels are obtained from accumulating year-on-year changes. The 95% confidence intervals are computed from heteroskedasticity-robust standard errors. Official Household Consumption data used from 2010Q1:2021Q4.

Figure B15: Response of quarterly national accounts Household Consumption of Non-Durable and Durable Goods to a monetary policy shock



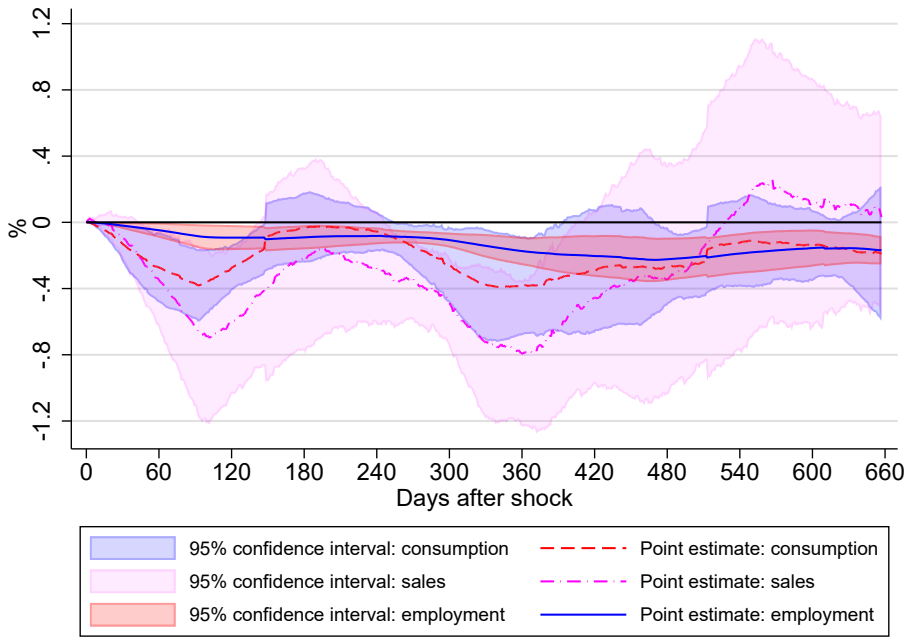
Notes: The monetary policy shock in this exercise is normalized to one standard deviation of monetary policy shocks at a daily frequency. The 95% confidence intervals are computed from heteroskedasticity-robust standard errors. Official Household Consumption data used from 2010Q1:2021Q4.

B.6 Including 2022 Monetary Policy Shocks

The dataset of Altavilla et al. (2019) that we use to identify our monetary policy shocks only reports interest rate movements around the ECB's announcement windows until the end of 2021. We extend our analysis beyond the time span covered in the dataset of Altavilla et al. (2019), constructing the monetary policy surprises in OIS3M, 1Y and 2Y for the first half of 2022. To do so, we use Refinity's proprietary data and document stock market and interest rates movements. Then we identify pure monetary policy shocks relying on the same "poor man" sign restriction approach used throughout the paper. Figure B16 shows the IRFs based on the extended sample, including half of 2022. As we can see, results are practically unchanged.

Even though the results are similar when we include the first half of 2022, the fast pace of monthly consumer price increases makes it more challenging to adjust for inflation. For this reason, our baseline sample stops at the end of 2021.

Figure B16: Daily response of consumption, sales and employment to a monetary policy shock, sample until June 2022

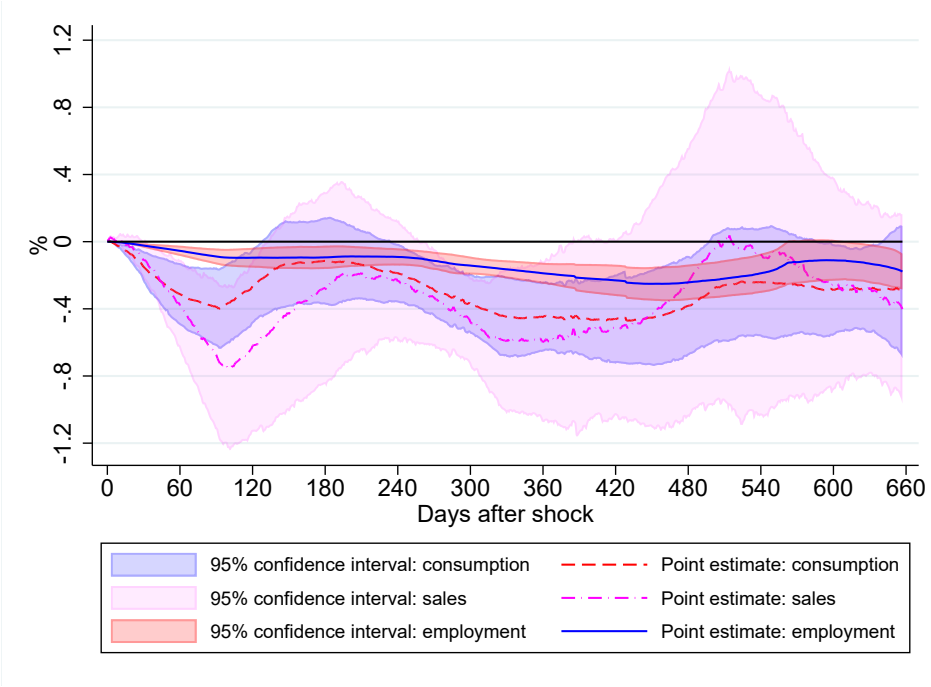


Notes: The monetary policy shock is one standard deviation. The consumption, sales and employment responses in levels are obtained from cumulating year-on-year changes. The 95% confidence intervals are computed from heteroskedasticity-robust standard errors.

B.7 Nominal Responses

In this section, we show that our IRFs are robust to considering nominal consumption and sales instead of their real counterparts. Figure B17 shows the responses of nominal consumption and sales together to those of employment (for comparison with the main exercises). The results are virtually the same as the real responses of consumption and sales. The fact that our main findings do not depend on how we deflate the nominal series suggests that adjustments in prices in response to monetary policy shocks do not seem to play a role in our "short and variable" lags findings.

Figure B17: Daily response of nominal consumption and sales and employment



Notes: The monetary policy shock is one standard deviation. The consumption, sales and employment responses in levels are obtained from cumulating year-on-year changes. The 95% confidence intervals are computed from heteroskedasticity-robust standard errors.

B.8 Heteroskedasticity and Autocorrelation Consistent Standard Errors

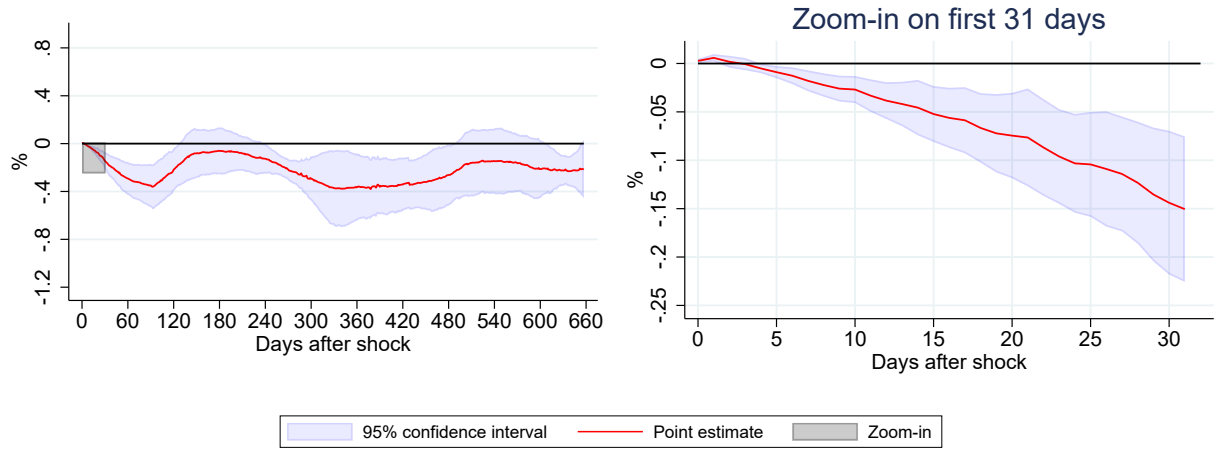
In this section, we analyze if our IRFs heteroscedasticity-robust standard errors in our lag-augmented local projections are similar to those computed using heteroscedasticity and autocorrelation consistent (HAC) estimators. Throughout the paper, following Mon-

tiel Olea and Plagborg-Møller (2021), we relied on the lag augmentation of the regressions to account for autocorrelation, and therefore only employed heteroscedasticity-robust estimators.

As explained in Montiel Olea and Plagborg-Møller (2021), employing directly HAC standard errors is computationally intensive. Our deployment of daily data makes computing HAC standard errors computationally expensive—as the projected horizon increases, so does the time of estimation given that more lags are needed to take into account. With horizons going as far as 660 days, the time for estimation of the IRFs is considerably large. In Figure B18 we show the IRF for daily consumption using Newey and West (1987) estimators of standard errors. It took 4 complete days to compute the IRF with HAC standard errors for daily consumption alone, running our code in Stata 17 MP license on a PC 12-core server with 200 GB of RAM.

As expected given the findings of Montiel Olea and Plagborg-Møller (2021), the results do not change significantly. Consumption responds significantly after 5 days, which is the same as in our main exercises. The slight difference we observe in Figure B18 is that the second drop in consumption becomes statistically significant slightly faster.

Figure B18: Daily response of consumption using Newey-west estimators of standard errors



Notes: The monetary policy shock is one standard deviation. The consumption, sales and employment responses in levels are obtained from cumulating year-on-year changes. The 95% confidence intervals are computed from standard errors that are robust to heteroskedasticity and autocorrelation.

C Consumption Response by Payment Type

Consumption expenditure can also be linked to the type of payment employed in the transaction. By exploring the metadata information for each transaction, Buda et al. (2022) classifies each transaction mode into the categories presented in Table C1. As they discuss in their paper, card transactions make up the large majority of the number of transactions but not of total value. It is also worth stressing that transfers exclude any transaction related to rent payments. Table C2 reports descriptive statistics for consumption by payment type. Consumption using card online payments is clearly on a fast-growing tendency throughout our sample period, while direct debit and cash withdrawal are the only payment methods that show a negative average growth rate across our sample.

Table C1: Payment types

Type	Description
01	Credit Card
02	Direct Debit
03	Transfer
04	Cash Withdrawal
05	Card Online
06	Card Offline

Notes: Credit card transaction are the sum of the offline and online card transactions. Transfer payments exclude any transaction related to rent payments.

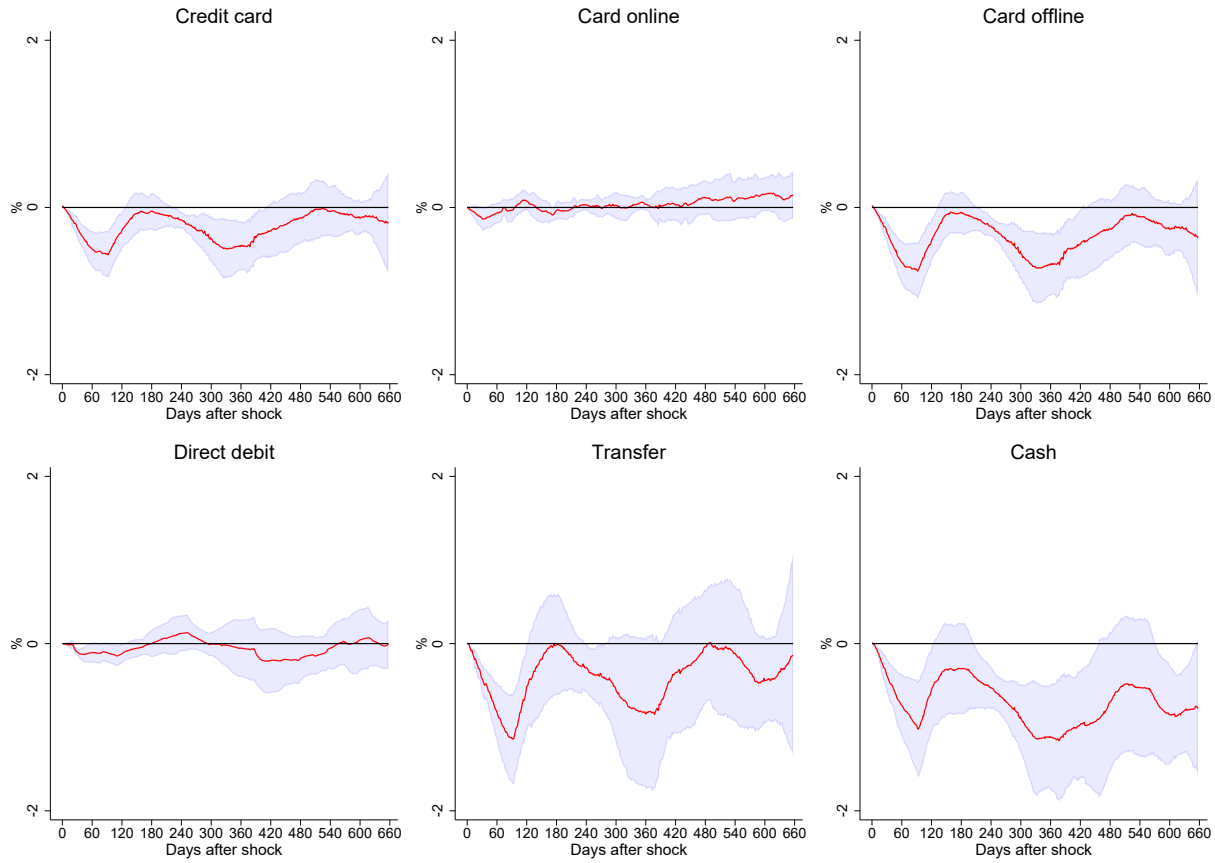
Table C2: Descriptive statistics, payment type

Payment type	Mean	SD	Min	Max
01	12.25%	8.67%	-19.54%	37.88%
02	-4.25%	5.49%	-20.26%	9.34%
03	8.90%	19.66%	-61.85%	62.03%
04	-0.49%	16.33%	-61.57%	41.74%
05	27.76%	4.13%	17.81%	37.97%
06	8.44%	10.95%	-33.08%	40.07%

Notes: Payment type measured as YoY growth rates of their 90-days moving averages.

Figure C1 shows the impulse responses of consumption by payment method. We find that, after a contractionary monetary policy shock, consumption demand falls significantly across most categories. Specifically, in the 90-day (three-months) time span

Figure C1: Daily response of consumption by payment type to a monetary policy shock



Notes: The monetary policy shock is one standard deviation. The responses in levels are obtained from cumulating year-on-year changes. Credit card transactions are the sum of offline and online card transactions. The 95% confidence intervals are computed from heteroskedasticity-robust standard errors.

following the shock, payments by "transfer" is the category that drops the most, by 1.2%, followed by payment by card offline and cash, both contracting by approximately 1%. The notable exceptions in terms of responses by means of payment are direct debits and cards online. Lack of response in direct debit is to be expected, since direct debit is typically linked to fixed monthly payments or consumption commitments. Payment methods and consumption categories are, of course, related to each other. By way of example, car purchases, included in the transport category, are typically paid by transfer. Not surprisingly, the response of payments by transfer is similar to the response of the transport category of consumption demand.

D Other Sectoral Sales Responses

There are other sectoral daily corporate sales that the Spanish Tax Authority publishes. Here, we show the IRFs of other interesting subcategories of manufacturing. Table D1 shows the description of these categories, while Table D2 shows their descriptive statistics. Figure D1 shows the impulse responses to a monetary policy shock.

Consistently with our IRFs for consumption categories, Furniture, Metallurgy and Vehicles have similar responses to shocks as the other sales categories — decrease significantly after about one month, but in these cases, they recover in the second quarter after the shock. Machinery, on the other hand, does not show any statistically significant response.

Table D1: Other sales category names and Spanish Tax Authority' description

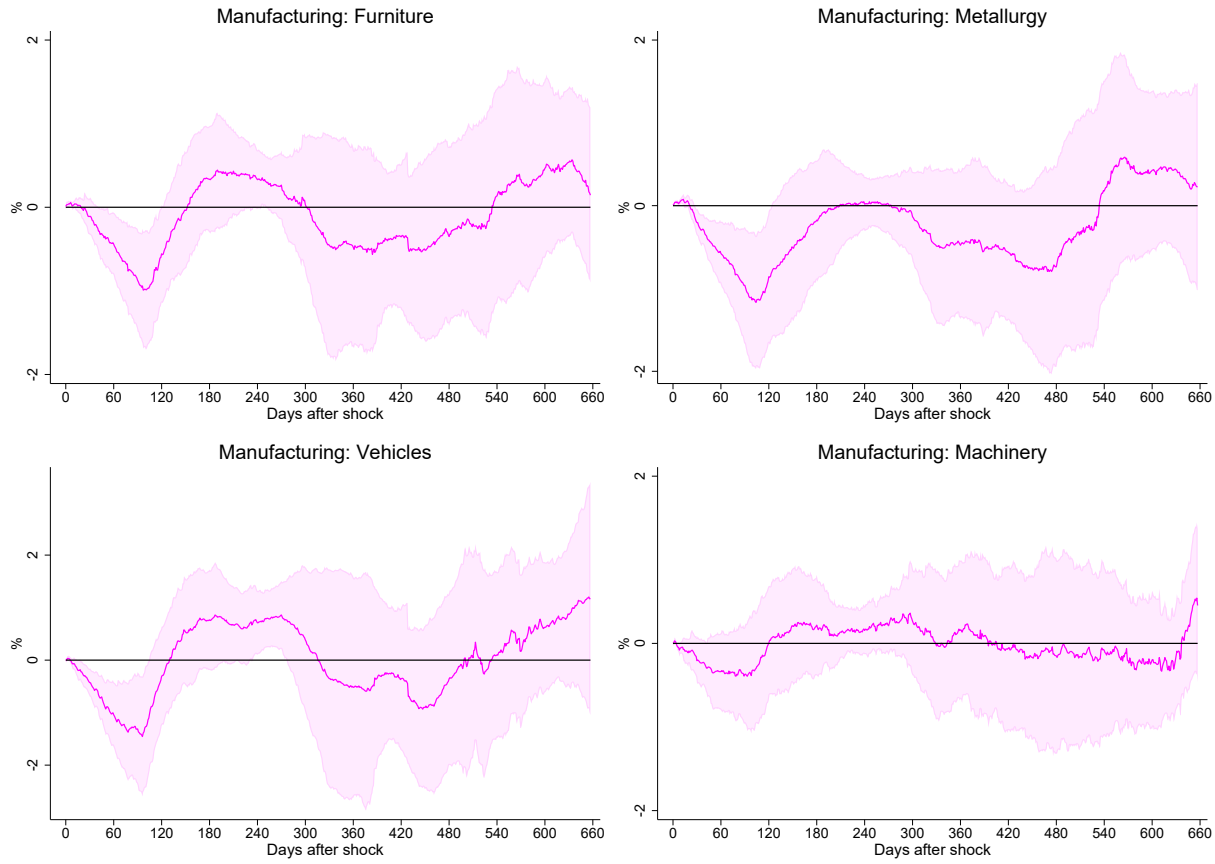
Category	NACE code	Description
Manufacturing: Furniture	C16 + C31	Manufacture of wood and of products of wood and cork and furniture
Manufacturing: Metallurgy	C24 + C25	Manufacture of basic metals and fabricated metal products, except machinery and equipment
Manufacturing: Vehicles	C29	29 - Manufacture of motor vehicles, trailers and semi-trailers
Manufacturing: Machinery	C28 + C30 + C33	Manufacture of machinery and equipment n.e.c., other transport equipment, and repair and installation of machinery and equipment

Table D2: Descriptive statistics, other sales categories

Sub-sector	Mean	SD	Min	Max
Manufacturing: Furniture	3.89%	16.61%	-48.23%	56.32%
Manufacturing: Metallurgy	-0.10%	19.01%	-54.57%	55.39%
Manufacturing: Vehicles	-7.05%	26.52%	-88.06%	62.34%
Manufacturing: Machinery	3.87%	14.11%	-34.87%	30.46%

Notes: Variables measured as YoY growth rates of their 90-day moving averages.

Figure D1: Daily response of other sales categories to a monetary policy shock



Notes: The monetary policy shock is one standard deviation. The responses in levels are obtained from cumulating year-on-year changes. The 95% confidence intervals are computed from heteroskedasticity-robust standard errors.

E Recovering impulse-response functions in levels from impulse-response functions in year-on-year growth rates

Consider a time series y_t in log-levels. Assuming y_t is covariance stationary, the Wold representation of the time series is given by:

$$y_t = \sum_{j=0}^{\infty} \phi_j \varepsilon_{t-j} + \eta_t, \tag{2}$$

with $\phi_0 = 1$ and $\sum_{j=1}^{\infty} \phi_j^2 < \infty$, and where ϕ_j are coefficients, ε_{t-j} are uncorrelated innovations, and η_t is a deterministic component.

An impulse response function is defined as the response of variable y to innovation ε_t at horizon $h = 0, 1, ..., H$. Given the Wold representation of y_t, we have that

$$IRF_h = \frac{\partial y_{t+h}}{\partial \varepsilon_t} = \phi_j. \tag{3}$$

Assuming the frequency of y_t be daily, we can define the year-on-year (YoY) growth rate as $z_t = y_t - y_{t-365}$. Since y_t is covariance stationary, so is z_t, and its Wold representation is given by:

$$z_t = \sum_{j=0}^{\infty} b_j \varepsilon_{t-j} = \sum_{j=0}^{\infty} \phi_j \varepsilon_{t-j} - \sum_{j=0}^{\infty} \phi_j \varepsilon_{t-365-j}. \tag{4}$$

Hence, the impulse response of the YoY is given by

$$IRF_h^{YoY} = \frac{\partial z_{t+h}}{\partial \varepsilon_t} = b_j. \tag{5}$$

We can recover the impulse response function of the variables in log-levels ϕ_j from YoY impulse response functions b_t. For $0 \leq h < 365$:

$$IRF_h^{YoY} = \frac{\partial z_{t+h}}{\partial \varepsilon_t} = b_j = \phi_j = \frac{\partial y_{t+h}}{\partial \varepsilon_t} = IRF_h. \tag{6}$$

The intuition behind this result is straightforward. Changes in YoY growth rates, $z_t = y_t - y_{t-365}$, that are induced by innovations between time 0 and 364 days ago can only be driven by y_t because y_{t-365} cannot be affected by future innovations. Now from 365 days forward, an innovation impacts y_t and y_{t-365}. For $h \geq 365$,:

$$IRF_h^{YoY} = b_j = \phi_j - \phi_{j-365} \quad \text{for } j \geq 365. \tag{7}$$

Hence, the impulse response in levels for $h \geq 365$ can be retrieved from the YoY IRF recursively according to

$$IRF_h = IRF_h^{YoY} + IRF_{h-365}. \tag{8}$$

In sum, the impulse response function in levels mapping to the YoY impulse response function is given by

$$IRF_h = \begin{cases} IRF_h^{YoY} & 0 \leq h < 365 \\ IRF_h^{YoY} + IRF_{h-365} & h \geq 365 \end{cases} \tag{9}$$

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