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Abstract

We examine the transmission of monetary policy shocks to the macroeconomy at high frequency. To do this, we build daily consumption and investment aggregates using bank transaction records and leverage administrative data for measures of daily gross output and employment for Spain. We show that variables typically regarded as "slow moving", such as consumption and output, respond significantly within weeks. In contrast, the responses of aggregate employment and consumer prices are slow and peak at long lags. Disaggregating by sector, consumption category and supply-chain distance to final demand, we find that fast adjustment is led by downstream sectors tied to final consumption—in particular luxuries and durables—and that the response of upstream sectors is slower but more persistent. Finally, we find that time aggregation to the quarterly frequency alters the identification of monetary policy transmission, shifting significant responses to longer lags, whereas weekly or monthly aggregation preserves daily-frequency results.

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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 fully unfolding at medium to long horizons, as Friedman emphasized early on. Complementing Friedman's dictum, a widespread view in policy circles holds that monetary policy transmission can be envisioned as a two-stage process: at first, it affects asset prices, financial conditions and expectations; over time, these slowly drive key real aggregates and inflation with increasing intensity. Together with Friedman's dictum, this view challenges theoretical monetary models, where in standard specifications the largest response of both financial and real variables to monetary policy takes place on impact—motivating the introduction of frictions (e.g., adjustment costs) and/or behavioral elements (e.g., habit formation) that help rationalize delays in the real effects of policy measures (see the discussion in, e.g., Woodford 2003 and Mackowiak and Wiederholt 2009).

In this paper, we assemble a novel, high-frequency and comprehensive dataset for Spain and reassess the received wisdom on the short lags of monetary policy. Relying on series of policy shocks obtained by applying high-frequency identification and local projections, we study the response of measures of daily demand and output in the days, weeks and months following monetary policy disturbances, at aggregate and disaggregated levels, up to a yearly horizon. Our granular analysis reveals: (*i*) that the impact of monetary policy shocks on what are typically considered "slow-moving" aggregates, like gross output and consumption quantities, can be detected already within days and weeks, rather than quarters or years; (*ii*) that this fast adjustment is led by the output response of downstream sectors closer to final demand and, within those, especially activities focused on the production of durables and luxury consumption goods, while sectors sitting upstream in the economy's production network—together with aggregate prices and employment—respond only at longer lags and (*iii*) that this fast adjustment of output and final demand is detectable at daily, weekly and monthly frequencies but, due to time aggregation biases, not with the more typically available quarterly frequency data.

Our first contribution is the construction and use of novel high-quality daily proxies for four key macroeconomic aggregates in Spain: aggregate gross output, aggregate consumption, aggregate investment and aggregate employment. To do this, we leverage unfettered access to tens of millions of both retail and corporate transactions recorded by one of the largest private banks in Spain and combine it with newly available tax and social security data tabulations by the Spanish government. In particular, we construct daily consumption and investment series from the bottom up, from bank transactions associated with the universe of, respectively, household and corporate accounts of Banco Bilbao Vizcaya Argentaria (BBVA). We complement this with newly assembled daily data on corporate sales—which we take as a proxy for daily gross output—and aggregate employment as recently compiled by, respectively, the Spanish Tax Authority based on VAT declarations, and the Spanish Ministry for Inclusion, Social Security, and Migration in the course of tracking public pension obligations. Finally, the coverage and size of the underlying data is such that we are also able to construct and exploit a number of informative disaggregated series, which we additionally complement with more traditional high-frequency financial prices and a large number of other variables at the monthly frequency.

Based on our daily series, our second contribution consists of documenting that monetary policy has economically and statistically significant effects on *aggregate* real economic activity already within weeks from a policy innovation. Major components of demand and gross output, conventionally classified as "slow moving" (Bernanke, Boivin and Eliasz, 2005), closely track the fast response of financial variables and expectations.

Our baseline results show that gross output exhibits a statistically significant decline within one week of a contractionary monetary policy shock. The decline reaches a local trough of -0.75% 68 days after a one standard deviation shock, and stabilizes thereafter before declining again roughly 240 days after the monetary policy shock. Its global trough, -0.83%, is attained at day 330. Consumption follows a broadly similar pattern, with a first trough at 45 days after the monetary policy shock, followed by a spell of stabilization and then again a contraction at long lags. Relative to sales, the consumption response is smaller, with a first trough at -0.45%. Investment responds along similar lines as consumption and output. Relative to these variables, the short-lag investment response is noisier (but still detectable), and stronger and more persistent at longer horizons, 8 months after the shock. However, the aggregate employment response, while statistically detectable early on, is initially very contained and, relative to the other three variables, is smoother and steadier. Its strongest response within the first year is precisely at day 365, with a cumulative decline of -0.18%.

Overall, this evidence questions conventional views, holding that there is a sharp distinction between slow vs. fast moving variables, and between a financial and a real phase as sequential stages in the transmission of monetary policy discussed by, e.g., Burr and Willems (2024). Over the first six months after a monetary impulse, consumption, gross output and investment respond sharply, then stabilize, although not necessarily in sync, before aligning in a contraction over a longer horizon. Yet our results by no means contradict Friedman's dictum. As we also show empirically, the short lags in demand and gross output translate into a slow, smooth but progressively deeper response of employment, mirrored by the price level—consistent with the idea that monetary policy impinges on its two key policy targets, employment and inflation, with long lags. Long lags, in other words, appear to be driven by the slow transmission of the contraction in demand and gross output, rather by a slow response of these variables to monetary impulses.

Turning to disaggregated series, our third contribution consists of documenting heterogeneity in the propagation of monetary shocks at short lags across categories of consumption, gross output by sector and by sector upstreamness. We show that the response of the demand and supply of goods traditionally considered more responsive to monetary policy—such as durables and luxuries, e.g., transport, clothing, and furnishing—is not only sharp, but also fast: it is significant at very short lags. The same pattern characterizes services such as health, education, and restaurants. Conversely, we find no short lags in the response of housing services and utilities, communication, and food and beverages ("food at home"), which remain insignificant, or is even positive, across all horizons. Building on Antràs et al. (2012), we also classify sectoral activity depending on the position of the sector in the production network, distinguishing between upstream and downstream sectors. We show that the response of downstream sectors, more closely tied to final demand, is both faster (statistically significant in about one month) and much larger (3 times) when compared to upstream sectors. Mirroring fast-moving consumption and investment, downstream sales stabilize in the second quarter, before contracting further at long lags. In contrast, the contraction of upstream sectors, providing general purpose inputs for the production of goods and services, is somewhat slower, becoming statistically significant 60 days after the shock, but much more persistent. The differences in these responses suggest a pattern of gradual upstream transmission of an initial downstream, final demand adjustment to a monetary policy impulse—lending empirical support to an emerging view that production networks and supply chains may be critical in the transmission of monetary policy.

Finally, we exploit the unique features of our high-frequency dataset to contribute empirical evidence on "time aggregation bias", the subject of a long-standing foundational literature analyzing whether and why the infrequent measurement of economic variables may compromise the validity of empirical findings. We show that aggregating our daily data into quarterly frequency—the frequency at which macro variables have commonly been available to researchers—alters the empirical responses of sales, consumption and investment to monetary policy shocks, blurring economically relevant results. At the same time, we find that aggregating daily into weekly and monthly frequencies preserves daily-frequency results. In other words, a researcher with access to only a quarterly-frequency aggregation of our data would not be able to detect a same-quarter response—-which would be instead detectable with data at the daily, weekly or monthly frequency. Said researcher would therefore wrongly conclude that consumption and gross output only start reacting "slowly" in the second quarter following the shock. This finding suggests that sharp classifications of variables as slow and fast moving in response to monetary shocks may be misdirected by the frequency of data most commonly available in macroeconometric work.

In light of our results on time aggregation, showing that monthly frequency analysis largely preserves the results obtained with our daily series, in the last part of the paper we extend our analysis to a large set of variables only available at monthly frequency. These extensions allow us to validate our baseline results and show robustness to different, longer, samples. In particular, we first show that key macro-aggregates respond in tandem with forward-looking expectations and sentiment surveys, so that the observed actions taken by agents—in (quick) response to monetary policy shocks—match both the timing and content of their belief updates. Second, turning to slow moving variables, we show that the behavior of monthly CPI—both aggregate and by categories—is also slow to respond to monetary policy shocks, not unlike that of daily and monthly employment and that the slow adjustment in the latter mostly reflects the slow reaction of permanent employment contracts. Third, we use our rich monthly dataset to assess the response of alternative data sources and proxies of gross output, investment and consumption in longer samples.

In addition, we conduct extensive robustness exercises to our baseline methodological and sample choices. In particular, we demonstrate robustness to a wide variety of alternative seasonality and smoothing procedures, as well as to alternative monetary policy shock series. We also deploy alternatives to our simple local projections baseline methodology and consider robustness to alternative treatments of the COVID-19 pandemic period.

Literature. Our paper relates to four different strands of the literature. 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 Jarociński and Karadi (2020), who built the database of monetary surprises around policy announcements for the Euro Area (henceforth EA) that we use in our paper. Our contribution is to document the transmission of monetary policy to real variables at the daily frequencies. As discussed in the text and appendix, lining up the frequency of shocks with the frequency of data allows us to avoid known issues brought about by the need to time-aggregate shocks identified at high frequency, to the lower frequency at which macro variables are commonly available—see Gertler and Karadi (2015) and Ramey (2011).[1]

Relatedly, our analysis builds on a consolidated body of studies showing that financial markets react immediately to monetary policy shocks (Gürkaynak, Sack and Swanson, 2005 and Swanson, 2021) and providing evidence that relevant interest rates for households, such as mortgage rates, react within weeks (Gorea, Kryvtsov and Kudlyak, 2022). Recently, some studies have stressed evidence at odds with the notion that households are inattentive to monetary policy developments. In particular, Lewis, Makridis and Mertens (2019) documents that public confidence in the state of the economy reacts *instantaneously* to surprises about the Federal Funds target rate. Finally, for the EA and the US, respectively, Jarociński and Karadi (2020) and Miranda-Agrippino and Ricco (2021), document same month response of (interpolated) GDP and industrial production.

Secondly, 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, 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), Buda et al. (2022), Chetty et al. (2020) and Ganong and Noel (2019)). Concerning households' consumption, 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. One advantage of using the universe of bank-transaction data, as we do in our paper, is that our consumption measure is much more accurate and comprehensive than measures derived from a specific method of payment (such as credit-card payments); see Buda et al. (2022)

[1] The literature has routinely aggregated monetary surprises to lower frequencies such as monthly, quarterly and even yearly horizons (Gertler and Karadi (2015), Almgren et al. (2022), Cloyne, Ferreira and Surico (2020) and Holm, Paul and Tischbirek (2021), respectively)

for extensive discussion in the context of BBVA transaction data.[2] Most crucially, relative to the literature, we are able to additionally produce and study a new daily investment series.

Thirdly, our findings lend empirical support to the idea that production networks play an important role in mediating the transmission of monetary policy to real economic activity. Following the seminal work of Basu (1995), the early contributions of Carvalho (2006) and Nakamura and Steinsson (2010) – and, more recently, that of Pasten, Schoenle and Weber (2020) – developed calibrated multi-sector environments in order to quantitatively understand the role of heterogeneous price stickiness and intermediate input linkages in the transmission of monetary policy shocks. Closer in spirit to our study, both Ozdagli and Weber (2017) and Ghassibe (2021) seek to empirically identify the role of these production networks in amplifying the effects of monetary disturbances. In particular, similarly to Ozdagli and Weber (2017) we uncover strong patterns of upstream propagation of demand changes created by monetary shocks. Unlike Ozdagli and Weber (2017) however, we show that these patterns mediate the response of real sectoral output rather than that of financial markets and stock returns.

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 (2023), who show that a temporal aggregation bias plays a non-secondary role in explaining the "price puzzle" (with inflation raising in response to a contraction) typically found when estimating the impact of monetary policy shocks on inflation, relative to other rationalization (e.g., the 'FED information channel'). Consistent with this work, we find no price puzzle in our monthly data on inflation. 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 Sections 2 and 3 we describe the data and the methodology we use. Section 4 presents our baseline empirical findings unveiling short lags in aggregate real variables

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

response to monetary shocks. Section 5 shows how the response at short lags vary across goods categories and sectors, as well as across upstream vs downstream sectors. Section 6 discusses time aggregation. Extensions and robustness using monthly frequency data are presented in Section 7. Section 8 concludes.

2 Data

We assemble what is, to the best of our knowledge, the first dataset with daily, high-quality measures of aggregate gross output, consumption, investment and employment. To do this, we first leverage access to the universe of retail and corporate transaction records of one of the largest banks in Spain, BBVA, and construct novel series of daily aggregate consumption, daily aggregate investment, as well as daily consumption demand disaggregated by COICOP categories. Second, we complement this unique data with administrative daily series recently compiled by Spanish ministries and tax authorities: daily aggregate employment from Spanish Social Security records, and both aggregate and sectoral daily sales from Value Added Tax declarations by firms to the Spanish Tax Authority. Third, we further enrich our data by compiling more standard daily series on interest rates and stock returns as well as monthly data on consumer prices, housing prices, and forward-looking expectations and confidence indexes.[3]

2.1 Daily Economic Activity Data

In this section we detail the data sources and methods underlying the construction of our daily measures of real economic activity in Spain, which proxy for daily aggregate sales (or gross output), private consumption, private investment, and employment. We also review a host of monthly counterparts to our baseline daily measures. Although our series have varying starting dates, they all conclude in October 2023. Summary statistics for the various daily series are provided in Appendix A.2.

2.1.1 Aggregate Sales

Data on aggregate sales at daily frequency are publicly available through the Spanish Tax Authority. The Tax Authority compiles the series from daily Value Added Tax (VAT) declarations by firms, reporting their domestic sales transactions—the tax base

[3] Table A1 in the appendix provides an overview of measures, sources and time coverage for the data used in the paper.

for VAT—on each day.[4] This variable encompasses sales of final consumption goods to Spanish households and tourists, as well as sales of investment goods to Spanish firms and domestic firm-to-firm intermediate transactions. It can thus be taken as a proxy for daily gross domestic output.

Only large firms or conglomerates—specifically, those with a turnover of 6 million euros or above in the previous year—are legally required to supply their domestic sales information daily. According to the Spanish Tax Authority (Agencia Tributaria, 2023), the number of firms reporting daily sales in 2019 was approximately 60,000 (out of a universe of 3.8 million VAT-paying entities in Spain). Nevertheless, due to their size, these firms accounted for about 70% of domestic sales by all firms in the same year.

Based on the same information, the Spanish Tax Authority also releases daily series disaggregated by NACE sector, that we will use as proxies for sectoral daily (gross) output. 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', as a larger fraction of economic activity in these sectors is accounted for by smaller firms that are not obliged to report daily to the tax authority. Because of their low representativeness, we drop these two sectors from our analysis; see Appendix A.2 and Agencia Tributaria (2023) for details.

Following the recommendations of the Spanish Tax Authority, we deflate daily series using monthly price indexes. Specifically, we apply the same monthly deflator value to all observations in the month.[5] We deflate aggregate sales using the Spanish Consumer Price Index (CPI); Manufacturing and Construction sector sales using the respective producer price indexes (PPI); Wholesale and Retail Trade, and Transportation and Storage with appropriate disaggregated CPIs. Finally, we use the Services Price Index (SPI) to deflate the remaining (service) sectors.

Note that, based on the same VAT reporting data, the Spanish Tax Authority additionally compiles a range of *monthly* series, which we also employ in our analysis. These monthly series have two advantages. First, while the earliest available date for the daily sales series is July 1st, 2017, the monthly series start much earlier, in January 2000. Second, at monthly frequency, the Spanish Tax Authority provides sectoral breakdowns not

[4]With the introduction of the VAT immediate declaration system ("Suministro Inmediato de Información"), large taxpayers included in the system are required to send the Tax Agency the details of the billing records within four days of the issuance of an invoice. While comprehensive for most of Spain, this reporting system excludes firms with activity exclusively within the province of Navarra or the Basque Country, who report to their own regional tax agencies. In addition, activities in territories with no VAT – the Canaries, Ceuta and Melilla – are not represented.

[5]As an alternative, we also experimented with linear interpolation of the monthly price index series—the resulting real daily series are indistinguishable from our baseline.

available at daily frequency. Most importantly, it offers a breakdown of domestic gross output by use – distinguishing monthly domestic consumption, investment and intermediate input sales – supplementing it with a monthly export series.[6,7]

2.1.2 Consumption

We construct a proxy for daily aggregate consumption using the universe of bank transactions recorded in the Spanish retail accounts of Banco Bilbao Vizcaya Argentaria (BBVA)—following the methods developed by Buda et al. (2022). In particular, our measure is a daily counterpart to their proxy for quarterly and annual aggregate consumption of private households. Below we give a brief overview of the construction of our series and refer the reader to Buda et al. (2022) for further detail.

The underlying data source for our consumption data is the universe of bank account outflows—including all card transactions, cash withdrawals, regular direct debits and occasional transfers—of Spanish residents who hold a retail account with BBVA. These account outflow records are supplemented by extensive metadata on both bank clients and individual transactions. The baseline sample consists of 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 individuals who are self-employed.

Based on this data, the construction of a proxy for aggregate consumption involves two main steps, as detailed by Buda et al. (2022), that we follow closely. First, not every account outflow of these customers corresponds to a consumption expenditure: Buda et al. (2022) classify individual transactions as consumption, savings, investment or tax payments relying on the metadata associated to them, and following the national accounting principles from the European System of Accounts. The exceptions are cash withdrawals, which are assumed to serve exclusively for consumption expenditures.[8] Further, and again following European System of Accounts' recommendations, Buda et al. (2022) impute housing services to all customers. Second, since the population of BBVA retail customers differs from the Spanish adult population along observables, Buda et al. (2022) aggregate consumption by summing over individual consumption

[6] According to Agencia Tributaria (2023) (see the Table "Destination of Domestic Production at Basic Prices," in the National Accounts), the Spanish Tax Authority classifies domestic sales by calculating the proportion of the demand for intermediates, final consumption expenditure, and gross capital formation, based on national accounts data and in particular, the input-output matrix.

[7] In a further robustness check, we also use a standard monthly industrial production series for Spain as a proxy for manufacturing gross output.

[8] Analogously, difficulties in classifying the purpose of transactions by self-employed people explain why this group is excluded from our sample.

using Spanish population weights at the gender-age-neighborhood cell level.

While we follow closely Buda et al. (2022), our focus on daily frequencies requires us to take a stand on regular consumption expenditures happening at lower frequencies than daily. This includes monthly (imputed) housing services and the payment of regular utility bills, often on regular direct debits. We distribute these regular expenditures uniformly across all days of the month, assuming a regular service flow to households.

Overall, as Buda et al. (2022) show, the aggregates implied by this large scale consumption panel 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.

Further, exploiting metadata associated with each consumption transaction, Buda et al. (2022) show how to construct category-specific consumption series following the European COICOP system and distinguishing 11 consumption categories, which we also use here; see Appendix A.2 for a brief description of these consumption categories. As shown in Buda et al. (2022), the implied distribution for category-specific consumption shares matches well their official national accounts' counterpart.

As with daily sales series, consumption daily series are deflated using monthly price indexes—aggregate consumption is deflated with the CPI, while individual consumption category series are deflated using CPI at the COICOP level. Our sample starts in August 1st, 2015 and ends in October 30th, 2023. Finally, as mentioned above, note that we also have access to an alternative *monthly* consumption series, as assembled by the Spanish Tax Authority from VAT data; see the discussion in the previous subsection. This monthly data serves as an important robustness check, as it is compiled from wholly distinct and publicly available data source.

2.1.3 Investment

Our daily aggregate investment proxy is also constructed using transaction data from BBVA. Our starting point is now the universe of corporate accounts and corporate transactions at the bank. In particular, we have access to all corporate transfers, and all (reverse) factoring operations mediated by BBVA. The former corresponds to all direct firm-to-firm payments while the second, also known as confirming, is a form of supply chain finance service provided by the bank.

From this universe, we extract transactions that can be reasonably inferred to be a payment for goods and services. We do this by restricting to transactions that include the word 'invoice'—or variations thereof—in metadata associated with each transaction. Further, we only keep operations where we can identify both parties—sender and receiver—as firms. We implement this either because we can identify both parties as BBVA corporate customers or because we can identify the non-BBVA party in the Spanish Business Registry.[9] We further drop transactions involving financial sector firms, public sector firms or entities listed as non-profit foundations and eliminate all transactions where both parties belong to the same ownership group. Finally, note that while we are able to date each confirming transaction according to the original corporate invoice date, transactions stemming from direct firm-to-firm transfers are dated according to the date of the transfer of funds; they may therefore lag the original contract between firms. The resulting dataset comprises of 17.4 million transactions—roughly evenly split between corporate transfers and reverse factoring operations—among 1.9 million distinct corporate entities, occurring between April 2017 and October 2023.[10]

We are then faced with two hurdles. First, as is the case with retail customers, the population of BBVA corporate customers is a biased sample of the Spanish population of firms: we find that the sectoral coverage of BBVA does not coincide with the sectoral distribution of output in Spain. To address this problem, we apply sector-level weights such that the aggregate of yearly (total) corporate sales by sector within the BBVA sample matches the respective sectoral aggregate—yearly domestic sales of investment goods plus domestic sales of intermediate goods—as recorded in Spanish Input-Output tables, for each year.

The second hurdle is the difficulty in ascertaining whether a transaction between two firms corresponds to a trade in investment goods and services, as opposed to intermediate inputs. This is because, unlike the case of consumption above, the metadata associated to each transaction is not sufficient to classify corporate transactions according to use. To make progress, we broadly follow the solution proposed by the Spanish Tax Authority, which faces the same hurdle in compiling monthly investment series from corporate VAT data. Namely, we exploit Spanish national accounts' input-output tables to obtain, for each sector, the share of investment goods sales in total gross output. We then apply this share to all BBVA corporate transactions involving all firms in a sector, as sellers of goods and services, on any given day. Summing this across all firms and sectors gives our proxy for daily aggregate investment. Appendix A.3 presents the steps involved in constructing this daily investment series in more detail.

We benchmark our aggregate investment proxy by time-aggregating it at the monthly

[9]Information is available through the standard SABI – Iberian Balance Sheet Analysis System – firm-level dataset, which includes the near universe of all Spanish firms.

[10]Among the entities in our sample, about 1.1 million firms are BBVA corporate clients, while the remaining are externally matched via the corporate registry.

and quarterly frequency and comparing the resulting series with two series released by the Spanish authorities: quarterly national accounts investment and the above described monthly aggregate investment series published by the Spanish Tax Authority from VAT declarations, which we also use in the paper. As discussed in detail in Appendix A.3 our investment series tracks these two alternative lower frequency series reasonably well. The correlation of (the monthly aggregation of) our series and the monthly investment measure compiled by the Tax Authority is 0.70 when comparing year-on-year monthly growth rates. At the quarterly level, the correlation with the investment series from the national accounts (year-on-year growth rate) is much higher, 0.95.

We adjust our daily aggregate investment series for inflation similarly to how we handle sales and consumption: we apply the same monthly deflator to all daily observations in the month. To do this, we use the implicit price deflator obtained from monthly total investment series (nominal and real) provided by the Spanish Tax Authority.

2.1.4 Employment

For our measure of daily aggregate employment, we source a publicly available administrative series from the Spanish Ministry for Inclusion, Social Security and Migration, that records the total number of contracts registered in the Spanish Social Security system on any given day.

Enrollment in the Social Security system is mandatory for all employer-employee contracts in Spain, with the exception of a small number of private and public institutions which have historically maintained a separate pension system.[11] In total, the series represent about 99% of the employed population. We should note here that a worker may have more than one active contract in the system (e.g., someone maintaining two part-time jobs). Our series tracks the total number of active jobs registered in the Social Security system, rather than strictly the total number of employees.

The daily series is updated daily from Monday to Friday, and nets out job creation (new labor contracts registered with the social security system) from job destruction (labor contracts that have lapsed on that day) to obtain a daily series for the stock of employment contracts.[12] This daily employment series starts from August 3rd, 2015.

Finally, at the monthly frequency, the Spanish Ministry for Inclusion, Social Security and Migration additionally provides a breakdown of monthly aggregate employment

[11] The largest of these, by assets, are the Spanish Bar Association and the Basque Autonomous Government.

[12] For Saturdays and Sundays, we assume that the number of workers registered is the same as the previous Friday.

into aggregate permanent employment and aggregate fixed-term, or temporary employment, which we also use.

2.2 Other Data

As discussed above, we additionally compile more standard data on (i) interest rates, financial markets' and housing prices as well as consumer price indexes, both aggregate and by consumption category and (ii) a variety of expectations' data and confidence indicators.[13] We now briefly discuss each of these data.

Prices. For equities and housing we use, respectively, daily series data on stock prices from the IBEX35 index, and the monthly average price of housing per square meter from the Statistical Information Center for Notaries (CIEN). The daily data on stock prices begins on January 3rd, 2005 while the monthly data on house prices begins in January 2007. Our measures of borrowing rates are the 1-, 3-, 6-, and 12-month Euribor rates—all of which are available from January 4th, 1999—and the monthly home loan interest rates which are published by the Bank of Spain—available from June 2014.[14] Finally, the series for the monthly consumer price index for Spain and COICOP specific consumer price indexes both start in January 2012.

Expectations. We collect data on expectations for real activity, financial markets and prices from a variety of sources. We proxy for inflation expectations using market-based 1-, 5- and 10-year inflation-linked swaps for Spain, from Bloomberg. We gather consumer and business expectations on real activity in Spain from the European Commission's business and consumer surveys: industry, services, retail trade, construction, and consumer survey. Monthly Confidence Indicators (CIs), reflecting overall perception and expectations, are calculated separately for consumers and the four business sectors covered by the survey programme, and are available for Spain since January 2000. The monthly Economic Sentiment Indicator (ESI) and the Employment Expectations Indicator (EEI) are calculated based on a selection of questions from these surveys. We also make use of a recent consumer expectations survey, conducted by the European Central Bank since April 2020. This survey provides information on consumer expectations in Spain on various aspects of real activity and development in financial markets, such as expected mortgage interest rates and access to credit for the next 12 months.

[13] Recall that we provide a summary of the data collected and respective sources in Appendix A.1.

[14] We also collect data on credit quantitities rather than prices. Monthly credit volumes for households and for firms are available from the Bank of Spain and start in June 2010.

3 Methodology

For the remainder of the paper, we combine the high-frequency data introduced in the previous section with a standard high-frequency approach to monetary policy shock identification in order to obtain daily impulse responses to key macro variables. In this section, we review our baseline choices regarding shock identification schemes, seasonality and smoothing of data, and regression specification. Whenever possible, we opt for well-understood, off-the-shelf methods and assess robustness to our various baseline choices in Section 7 of the paper.

3.1 Identification

We identify the effects of monetary policy shocks on the macroeconomy by using series of monetary policy surprises for the Euro Area made publicly available by Jarociński and Karadi (2020). These daily monetary policy shocks are constructed from Euro Area high-frequency changes in financial assets around ECB policy announcements along the lines of Gürkaynak, Sack and Swanson (2005). Lining up the frequency of shocks with the frequency of data allows us to avoid known issues brought about by the time-aggregation of shocks, as discussed, for example, in Ramey (2011) and Jacobson, Matthes and Walker (2023).[15]

The off-the-shelf series by Jarociński and Karadi (2020) also addresses the growing concern in the literature whereby the central bank "information channel" can pollute monetary policy shocks (see, for example, Nakamura and Steinsson, 2018, Cieslak and Schrimpf, 2019, Jarociński and Karadi, 2020, and Miranda-Agrippino and Ricco, 2021). As argued by this literature, information effects could mute the negative stock market response to changes in the expected policy path, or even reverse its sign. To control for the information channel, Jarociński and Karadi (2020) combine standard high-frequency identification with sign restrictions in a Bayesian VAR. Specifically, sign restrictions are applied to changes in the interest rate and stock market, where pure monetary policy shocks are associated with a negative comovement between interest rates and stock prices, while information shocks are associated with positive comovement.[16] The up-

[15] See also our discussion of time aggregation in Section 6.

[16] For the US, the literature has proposed a number of alternative ways to orthogonalize monetary policy surprises from the information channel. For example, Miranda-Agrippino and Ricco (2021) orthogonalize monetary surprises by projecting market-based monetary surprises on their own lags, and on the central banks information set, as summarized by Greenbook forecasts. More recently, Bauer and Swanson (2023) were able to produce monetary policy shocks that control for the information channel using publicly available data only. Their work raises doubts about whether the central bank's superior knowledge or

dated version of the monetary policy shocks database by Jarociński and Karadi (2020) that we use in our analysis includes 293 ECB policy announcements from 1999 to 2023, with 63 of them occurring during our baseline sample from August 2015 to October 2023. Appendix B.1 presents descriptive statistics for our baseline series of monetary policy shocks.

In Section 7.3.3, for robustness, we consider two alternative series of monetary policy shocks using the Euro Area Monetary Policy Event-Study Database. The first series involves observed 1-month Overnight Indexed Swap (OIS) changes around policy announcements, controlling for the 'information channel' by excluding instances where the sign of the OIS change matches that of the euro STOXX50E index stock price change. The second series is an updated version of the Policy Target factor by Altavilla et al. (2019), focusing on the short end of the yield curve and derived using factor analysis based on changes in the yield curve.

3.2 Seasonal Adjustment and Smoothing of Daily Time Series

Seasonally adjusting daily series faces at least three challenges. Firstly, daily data is more sensitive to calendar effects, such as the different number of working days or moving holidays. Secondly, one needs to purge not only the more typical predictable seasonal patterns, but also within-month and within-weekly regular variation. Thirdly, noise is more pervasive in daily series, be it because true irregular variation in data is not time-averaged away or because measurement error may be heightened at high frequencies.

To deal with these challenges, for our baseline treatment of data, we opt for what is arguably the simplest and most transparent two-step approach. In the first smoothing step—to deal with the effects of daily noise, irregular events and moving holidays—we apply a 30-day backward-looking moving average to the series. In the second step, we compute year-on-year growth rates differencing out periodicity in data at the daily level. Throughout the paper, almost all daily series are seasonally adjusted using this baseline data transformation; the exception are financial-market variables, which remain in levels or log-levels and are not seasonally adjusted.

Finally, Section 7.3.2 shows that applying a range of alternative, more sophisticated methods to deseason and smooth daily data, produces results that are qualitatively and quantitatively similar to those derived under our simpler, baseline approach.

a violation of the assumption of rational full-information expectations is the source of the information contained in monetary surprises.

3.3 Local Projections

For our variables of interest, we estimate daily impulse response functions (IRFs) 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 $\hat{\beta}_{h,0}$, of the following linear regression:

$$y_{t+h} = \alpha_h + \sum_{\ell=0}^{k} \beta_{h,\ell} shock_{t-\ell} + \sum_{\ell=1}^{p} \varphi_{h,\ell} y_{t-\ell} + \theta_h cases_t + \delta_h stringency_t + \varepsilon_{h,t}, \tag{1}$$

where y_{t+h} is the dependent variable of interest, and $shock_t$ is the monetary policy shock at time t. At the daily or weekly frequency, we find no significant autocorrelation in the monetary policy shocks, and because of that, we do not include lags, $k = 0$. However, when monetary policy shocks are aggregated to monthly or quarterly frequency, we find significant shock autocorrelation, that only dies out after 6 months. Hence, for monthly and quarterly series we include six and two lags of monetary policy shocks in our set of control variables, respectively.

In the baseline specification, we estimate IRFs up to $H = 364$ days (one year after the shock) and include 90 lags of the endogenous variable (a quarter of past information). The 364-day horizon is convenient because our variables are measured in year-on-year growth rates. Hence, the responses of the IRFs up to one year can be directly interpreted as changes in levels; see Appendix B.2 for further detail. The inclusion of lags of the dependent variable as controls is motivated by Montiel Olea and Plagborg-Møller (2021), who show that lag-augmenting local projections both renders inference more robust and simplifies standard error calculations, by avoiding the need to adjust for residual serial correlation. We compute 68% and 90% confidence intervals from heteroskedasticity-robust standard errors.[17]

Since our baseline sample includes the period of the COVID-19 pandemic, we adopt the approach advocated by Schorfheide and Song (2024) (also discussed in Lenza and Primiceri, 2022) and drop observations between March 14th, 2020 and October 30th, 2020 when estimating our baseline local projections.[18] In addition, we include two COVID

[17]Results are unchanged when computing standard errors using the Newey-West procedure (Newey and West, 1987).

[18]Lenza and Primiceri (2022) alternatively allow for time varying volatility in the residuals and obtain very high volatility estimates during COVID, so that effectively COVID observations contribute little to the estimation. Our strategy is more conservative, excluding any effect of extreme observations on our LP estimates altogether. Second, note that we do allow for COVID data to appear as lagged controls (for post-COVID observations for the dependent variable which we retain) in order to preserve the autocorrelation

controls: $cases_t$ is the log of new confirmed cases of COVID-19, and $stingency_t$ is the log of the stringency index.[19] Section 7.3.4 presents robust checks using pre-COVID-19 sample periods, demonstrating that the full sample's baseline method for addressing COVID-19 produces results comparable to those from samples ending before the COVID-19 crisis takes place.

Finally, for robustness, note that we will also consider two variations on our baseline LP estimation procedure. First, by utilizing direct monetary policy shocks from Jarociński and Karadi (2020), our baseline specification sidesteps instrumental variable approaches to LP. Nevertheless, when conducting robustness assessments that consider alternative disturbances—specifically, the OIS 1-month and the Policy Target Factor, which are more suitably considered as instrumental variables rather than explicit shocks—we do offer estimates utilizing both LP and LP-IV methods. As we will discuss in Section 7.3.3 and Appendix G.2, results are robust. Second, instead of smoothing the data as per the previous subsection, one may opt instead to smooth the local projection itself by resorting to the Smooth-LP methods in Barnichon and Brownlees (2019), who use generalized ridge estimation in place of OLS for LP. The results using this alternative approach are presented in Section 7.3.2.

4 Slow vs. Fast Moving Variables in the Transmission of Monetary Policy: Novel Evidence at High Frequency

According to a widely held view in the community of central bankers, monetary impulses transmit to the economy by affecting financial markets quickly, on impact—namely, borrowing conditions, expectations and stock prices—but only slowly affect production, investment and consumption decisions by firms and households, with the corresponding macro aggregates responding only after long lags; see, for example, Burr and Willems, 2024 for an institutional account of monetary policy transmission as a two-stage sequential process of fast vs. slow adjustment.[20] In turn, this view resonates with the hypothesis in the academic literature, that some real variables can be expected to react

structure in data. We thank Giorgio Primiceri for discussions on this topic.

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

[20] According to the Bank of England's Quarterly Bulletin (Burr and Willems, 2024), the "Monetary Transmission Mechanism can be broken down into a first stage, the pass-through from Bank Rate to various asset prices and other interest rates, and a second stage governing how financial conditions affect macroeconomic outcomes. First-stage transmission is typically rapid, thanks to the fast speed at which financial markets react to news, whereas the second stage takes more time."

more slowly than others. In the taxonomy of the seminal paper by Bernanke, Boivin and Eliasz (2005), the list of slow-moving variables spans employment, consumption, personal income, hourly earnings, CPI as well as industrial production. A large literature has built on this received wisdom, either contributing evidence, or using it for identification purposes. In this section, we leverage our novel high-frequency data to revisit this received wisdom, producing granular evidence on the short and long lags of the transmission of monetary policy across a set of financial and real, aggregate variables.

4.1 Setting the Stage: Interest Rates and Inflation Expectations

To set the stage of our analysis, in Figure 1 we show the responses to a contractionary monetary shock of key interest rates, asset returns and asset-price-based inflation expectations—the variables defining the first stage of the transmission mechanism according to the consensus view. These IRFs—and all that follow—are calculated relative to one standard deviation contractionary monetary policy shock.

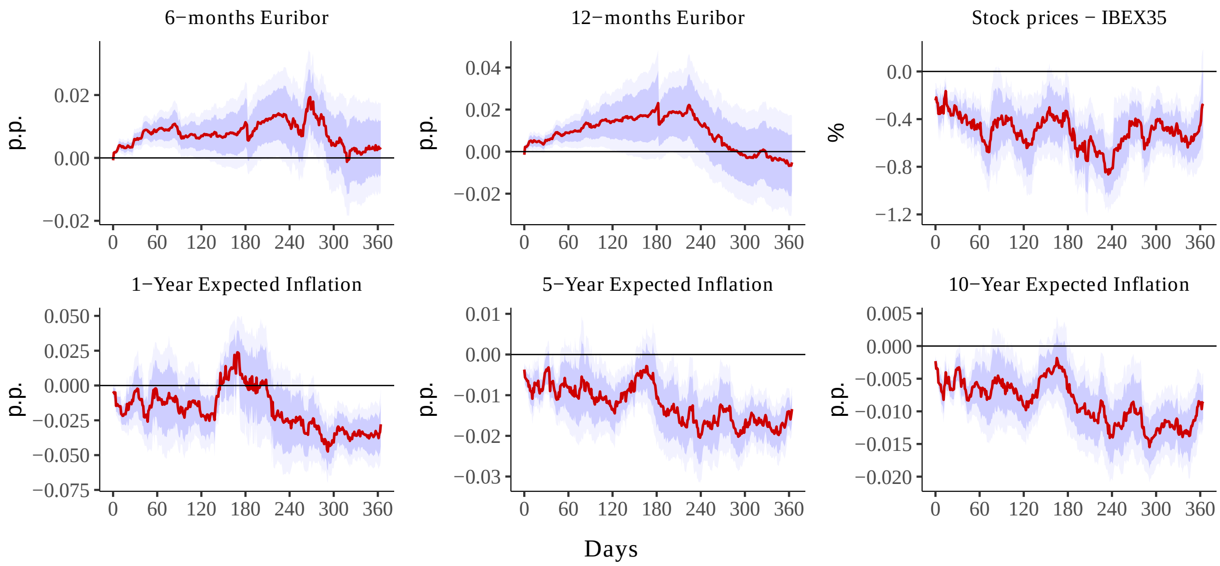
The daily responses of key interest rates, the 6- and 12-month Euribor, are shown in the first two panels (top row) of this figure.[21] Both rates rise by roughly 1bp within the first 60 days following a monetary policy shock; the 12 month Euribor displays a continued rise thereafter. Correspondingly, the benchmark stock market index for Madrid's stock exchange contracts—shown in the third panel (top row)—continuously and significantly over the first 90 days from the shock, by about 0.5 percentage points, remaining around that level for the rest of the year.

The second row of the figure shows the daily responses of inflation expectations using 1-, 5- and 10-year inflation-linked swaps for Spain. Consistent with a contractionary shock, these market-based inflation expectations decline in the first 30 days from the shock. While this decline happens across the term structure, it is quantitatively stronger for 1-year ahead expectations, with a drop of about 2bp. Further, as we will show below using available price data at the monthly frequency, these inflation expectations appear to be roughly on target when compared to the CPI's one-year ahead response.

Thus, key interest rates, asset prices and expectations all move *fast*: they respond on impact, with economically and statistically significant reactions observed at high frequency, within the first few days and weeks after the monetary policy shock. Importantly, as anticipated above, these results are not specific to Spain, our sample period or particular methodological choices. They are qualitatively and quantitatively consistent with a large literature documenting similarly fast and sizeable reactions of financial

[21]It is worth noting that these are the reference rates for most variable-rate mortgage contracts in Spain, themselves the most common contract in Spain.

Figure 1: Daily response of financial markets to a monetary policy shock



Notes: Financial market series are not seasonally adjusted. LP impulse response functions to a one standard deviation monetary policy shock. The responses are reported in levels. The confidence intervals are computed from heteroskedasticity-robust standard errors. Darker-shaded areas are the 68% confidence intervals, and lighter-shaded areas are the 90% confidence intervals. The sample starts in August 1st, 2015 and ends in October 30th, 2023. The monetary policy shock standard deviation is 3.7bp.

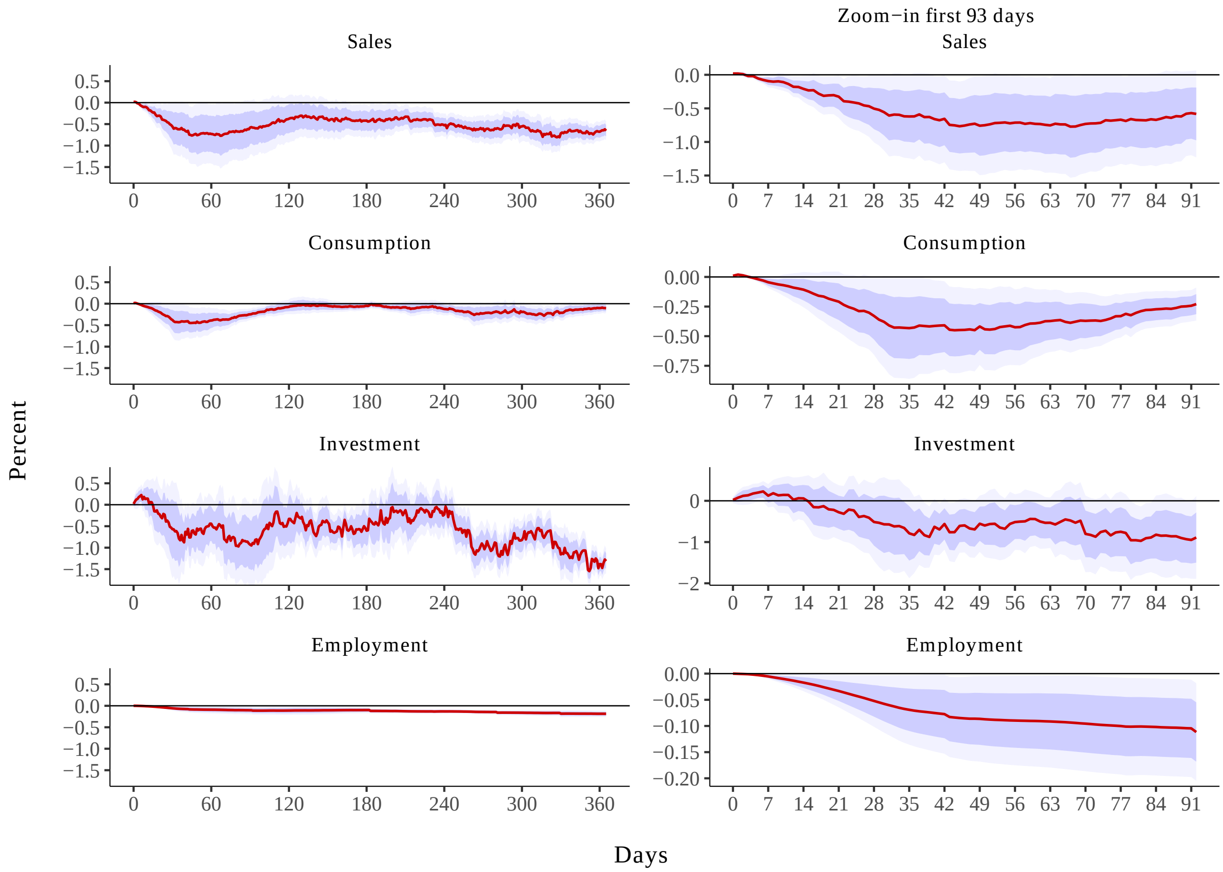
markets across different countries, time-periods and monetary policy shock measures;[22] Appendix C.1 further benchmarks our findings here, showing they are consistent with a broad range of published estimates. As we now show, however, these are not the only classes of variables quickly set in motion by monetary policy.

4.2 Four Key Daily Measures of Economic Activity

Our baseline evidence on the effects of monetary policy on the real economy is presented in Figure 2. This figure shows the responses to a monetary policy shock of four daily measures of real economic activity: corporate sales, proxying for gross output (first row); two major components of aggregate demand, consumption (second row) and investment (third row); and employment (fourth row). The graphs on the left column of

[22]See, for example: Jarociński and Karadi (2020) who documents monthly IRFs of the German one-year government bond yield and the Euro-Area stock index using a sample from January 1998 to December 2016, with a comparable shock magnitude of 3.5bp; Jarociński (2024) for their estimates of daily IRFs of financial variables in response to monetary policy shocks for the United States; Swanson (2021) for their IRF of 6-months US treasury yields; Miranda-Agrippino and Ricco (2021) for their IRFs of the 1 year T-bond and the S&P500; and Altavilla et al. (2019) for the impact of monetary policy shocks on 6-months and 1-year German yields.

Figure 2: Daily response of real activity to monetary policy shock



Notes: LP impulse response functions to a one standard deviation monetary policy shock. The responses are reported in levels. The confidence intervals are computed from heteroskedasticity-robust standard errors. Darker-shaded areas are the 68% confidence intervals, and lighter-shaded areas are the 90% confidence intervals. The sample ends in October 30th, 2023. The sample starts one year after the sample start of each series reported in Table A1. For sales and investment—the shorter series—the monetary policy shock standard deviation is 4.1bp, while for consumption and employment it is 3.7bp.

the figure show the response of each variable over a 365-day horizon; the graphs on the right column zoom in on the response of each variable during the first 93 days following a monetary policy shock.

Our broadest measure of economic activity, real corporate sales, encompasses final consumption, investment and intermediate inputs. As shown in the top panel of Figure 2, economic activity responds rather quickly: its decline is statistically significant (at the 90% confidence level) a week after the shock hits the economy. The contraction reaches a local trough of -0.75% 68 days after the shock and stabilizes thereafter, before declining again roughly 240 days after the monetary policy shock. Its global trough, -0.83%, is attained at day 330.

Relative to our measure of gross output, the response of aggregate consumption, shown in the second row of Figure 2, is somewhat muted, but far from "slow." Indeed, it follows a broadly similar pattern: it significantly contracts at short lags, stabilizes and then again falls at longer lags. Relative to corporate sales, the short-run through in consumption occurs at a slightly shorter horizon, 45 days after the monetary policy shock, and is 70% smaller, at -0.45%. The response turns economically and statistically insignificant 120 days after the monetary policy shock. The second spell of contraction, later on over the year horizon, is relatively shallow—attaining a (significant) local trough of -0.29% 318 days after the shock.

The same broad pattern characterizes the response of our proxy for aggregate investment, displayed in the third row of Figure 2. The response of investment is stronger than consumption and sales: our point estimates imply a statistically significant short-run contraction of -0.88% at the 40 day mark. Nonetheless, relative to the other series, the response of investment is noisier. The higher noise in this series may reflect the nature of investment spending—spikier than consumption and sales at the daily level—as well as higher measurement error in its construction. The contraction is clearer and more persistent at longer horizons, starting at 8 months after the shock. The global trough is as deep as -1.54%, 353 days after the monetary policy shock.

Finally, compared with gross output, consumption and investment, the quantitative response of employment, shown in the bottom panel of the figure, is qualitatively and quantitatively different. It is smoother and much slower. It remains economically negligible at short horizons: three months after the monetary policy shock, the response of employment, while statistically significant at 90% level, is only -0.11%. The response flattens out thereafter, resuming its decay at roughly 200 days after the shock. Its strongest response within the first year is precisely on day 365, with a cumulative decline of -0.18%.

4.3 Taking Stock: Slow-moving Variables vs. the Slow Transmission of Short Lags

The key fact unveiled in this section is that monetary policy has economically and statistically significant effects on real economic activity already within weeks from a policy innovation. Major components of demand and gross output, typically considered slow in reacting, in fact closely track the "fast" response of asset prices and expectations to a monetary policy shock. Our evidence thus casts a different light on the consensus view of monetary transmission, as well as on popular labelling of variables into slow- and fast-moving used in identification.

Nonetheless, our evidence by no means contradicts Friedman's dictum. The response of employment is smooth and becomes economically significant at longer lags—when gross output, consumption and investment also align in a contraction. In Section 7.2, using monthly data, we will show that the response of employment is actually mirrored by CPI inflation; see Figure 10 therein. Arguably, employment and the CPI are the two variables that matter the most for central banks: our evidence shows that monetary policy drives them down in tandem (along with the other three aggregates in Figure 2) at longer lags.

One natural conclusion prompted by these results is that the long lags of monetary policy are not rooted in a generic "slow response of real variables". Rather, they reflect mechanisms that slow down the transmission of a contraction in demand and gross output, already significant at short lags, into employment and, as we will see below, inflation.

5 Monetary Transmission Across Goods and Sectors

In this section, we exploit the rich information in our dataset to study the heterogeneity in the short-lag response across subcategories of demand and gross output. In particular, we are interested in establishing whether, underlying our aggregate responses, there are significant and economically meaningful differences in the response to a monetary shock across (i) subcategories of final consumption demand for goods, contrasting differences in the response of durables and luxuries vs. that of non durables and necessities) and; (ii) disaggregated gross output responses, contrasting differences in the response of final demand, downstream sectors, serving goods and services to households, to upstream sectors, specialized in the sales of intermediate and investment goods to other producers in the economy.

5.1 Response Lags Across Goods Categories: Final Demand for Durables and Luxuries Responds Fastest

Our data on consumption is dense enough in the cross-section to allow finer, daily disaggregated cuts that are typically unavailable at high-frequency. Specifically, we are able to decompose our aggregate consumption series at the 1-digit COICOP category level, obtaining eleven consumption series; see Appendix A.2 for a listing of the disaggregated consumption categories.

The results of our disaggregated analysis are shown in Figure 3. The categories of consumption goods that fall significantly in the immediate aftermath of a contractionary monetary policy shock are durable or semi-durable goods and luxury goods. The durable or semi-durable goods include clothing and footwear, transport[23] and, to a lesser extent, furnishing, equipment and maintenance. The luxury goods include restaurants and hotels, recreation and culture, health and education.[24] In contrast, the response of necessities—including food and non-alcoholic beverages, housing services and utilities and communication—remains subdued at all horizons within the first year after a monetary policy shock.[25] The upshot of these heterogeneous responses is that, on average, durable and luxury goods and services decline between 1% and 2% within the first 60 days from the shock. The response of essential goods instead ranges between -0.25% and 0%. In fact, the consumption of food and non-alcoholic beverages displays a slight rise, consistent with substitution away from restaurants and hotels (i.e., from "food away from home").

Overall, these findings provide a detailed high-frequency counterpart to the literature documenting that the response of consumption to monetary policy shocks is disproportionately driven by the response of durable goods consumption—see, e.g., Erceg and Levin (2006), Monacelli (2009), Sterk and Tenreyro (2018), and McKay and Wieland (2021).[26] We show that the contraction in the demand for these goods is not only sharp, but also fast—it is significant at very short lags.

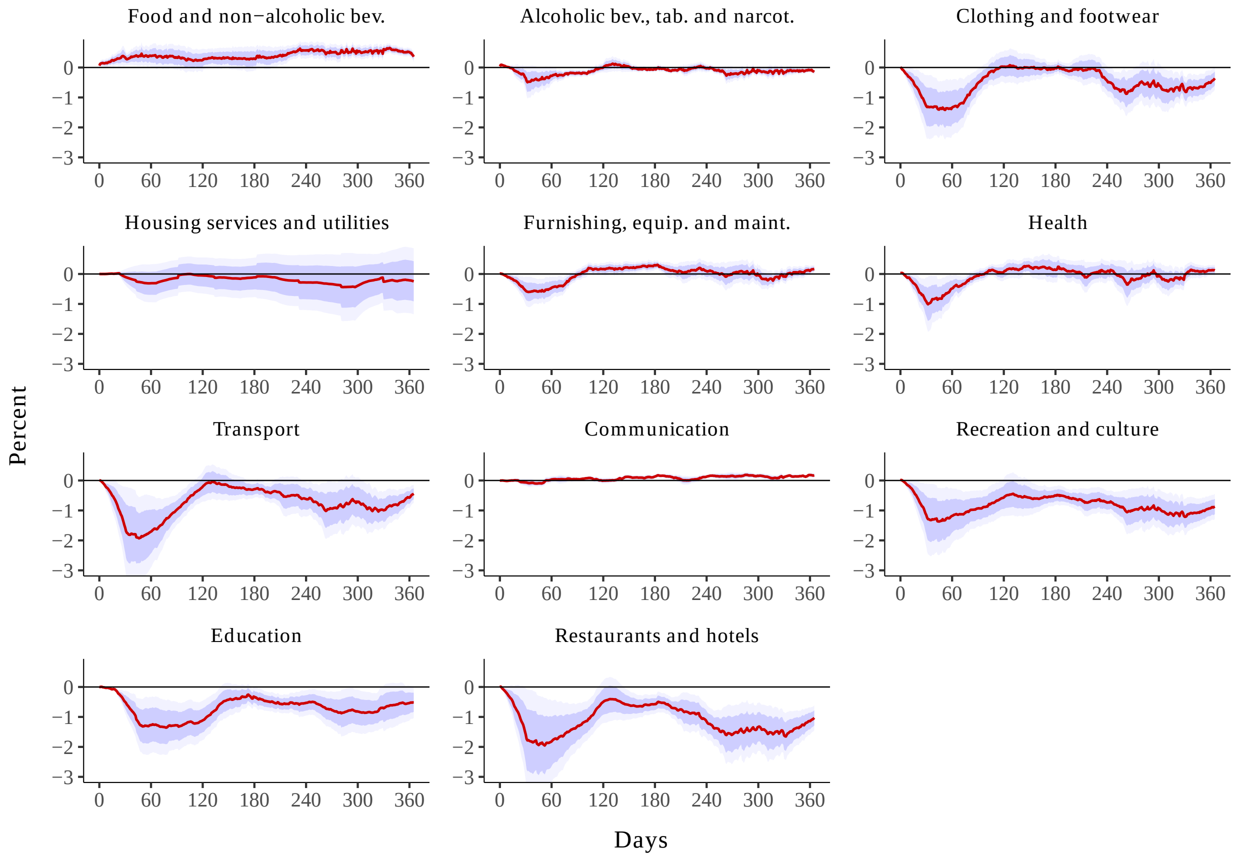
[23]We classify the transport category as semi-durable because it includes both durable vehicle purchases and a mix of expenditures on transportation services—e.g., taxi rides or monthly public transportation pass— that are either non-durable (the service flow of a taxi ride extinguished within the day) or semi-durable (the service flow of a transportation pass lasts typically for a month). Using an independent monthly series specific to sales of vehicles, in Appendix D we show that its response is very similar to the response of consumption of "Transport" services.

[24]In Spain, the large majority of education and health services are publicly provided by the state at low or no cost to the end user. The transactions related to education in our data are mostly generated by spending on private education and private health services, which in Spain can be considered luxury goods.

[25]We should note that the criteria used in constructing housing services and utilities and communication consumption may weigh on these findings. This is because, for these series, the daily consumption is computed by either imputing rents or distributing the monthly/bi-monthly utility 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 obtained from 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.

[26]The point is well-understood in the central bank community, as exemplified by the following quote: "Our staff estimate(s) that spending on durable goods and luxury goods declines more than it does on non-durables, services, and necessity goods" (Lane, 2024).

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



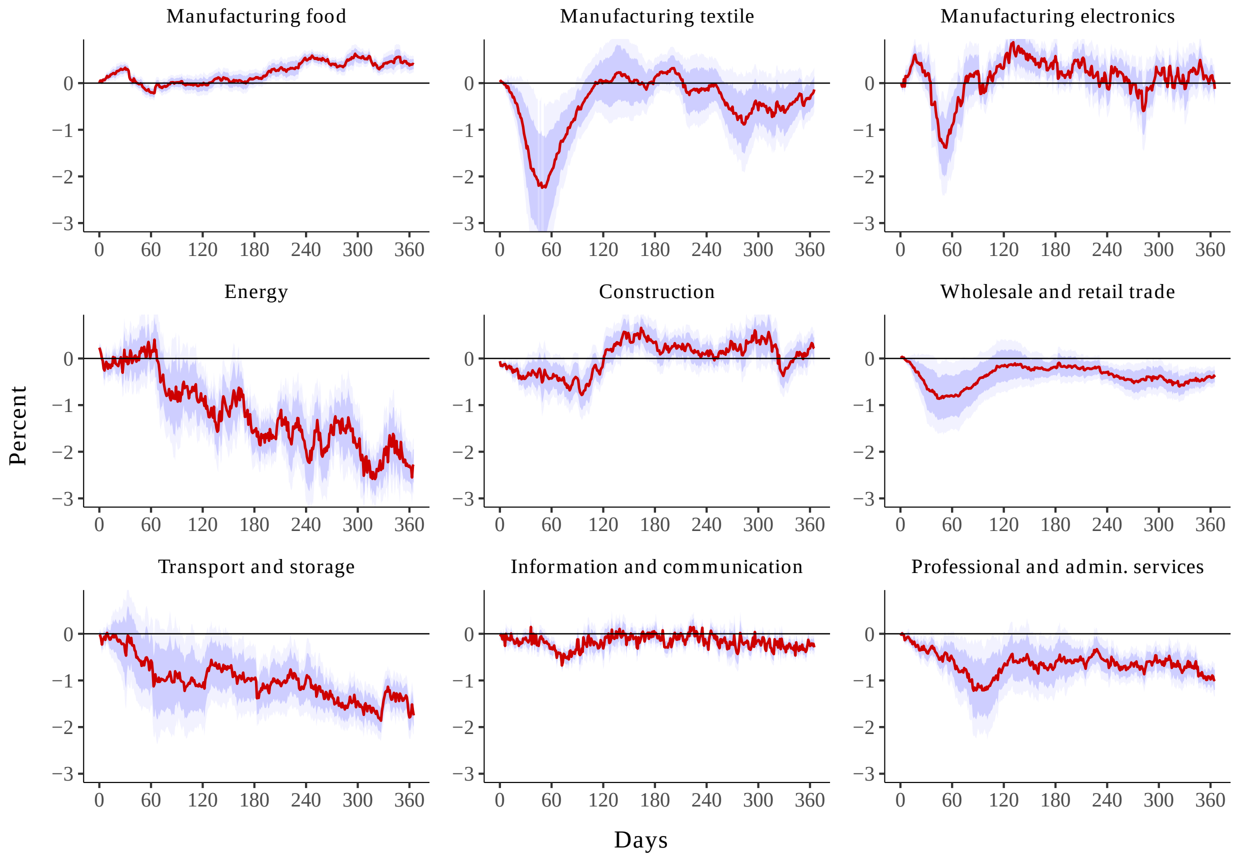
Notes: LP impulse response functions to a one standard deviation monetary policy shock. The responses are reported in levels. The confidence intervals are computed from heteroskedasticity-robust standard errors. Darker-shaded areas are the 68% confidence intervals, and lighter-shaded areas are the 90% confidence intervals. The sample starts in August 1st, 2016 and ends in October 30th, 2023. The monetary policy shock standard deviation is 3.7bp. See Buda et al. (2022) and Appendix A for further details on how the consumption categories were constructed and on their cross section and time series characterization.

5.2 Response Lags Across Sectors: Downstream, Final demand Sectors Respond Fastest

The VAT-sales data underlying our gross output proxy can also be usefully disaggregated in order to further inspect the heterogeneous transmission of monetary policy shocks. In total, we have 20 disaggregated daily sectoral sales series made available by the Spanish Tax Authority; see Appendix A.2 for a full listing and associated descriptive statistics.

We start by considering a subset of nine sectors that best match the consumption categories studied above. Results for these sectors, shown in Figure 4, suggest that the responses to a monetary shock for sectors that are closer to the final demand by

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



Notes: LP impulse response functions to a one standard deviation monetary policy shock. The responses are reported in levels. The confidence intervals are computed from heteroskedasticity-robust standard errors. Darker-shaded areas are the 68% confidence intervals, and lighter-shaded areas are the 90% confidence intervals. The sample starts in July 1st, 2018 and ends in October 30th, 2023. The monetary policy shock standard deviation is 4.1bp. See Appendix A for further details on sales sectoral classification.

households are broadly consistent with the short-lag response of consumption—when not slightly stronger. Specifically, wholesale and retail broadly track the response of aggregate consumption in Figure 2, with a decline of about 1% in the first 60 days of a monetary policy shock. The response of textile manufacturing mirrors the adjustment in consumption of clothing and footwear in Figure 3, with a marked, deeper trough during the second post-shock month. Other sectors where durable goods are highly represented in sales, such as electronics manufacturing or construction, also display patterns similar to the disaggregated consumption series, if only a bit nosier, with negative responses only in the short run. Finally, sales of the food manufacturing sector or information and communication display economically small responses, consistent with the patterns observed for the consumption of food and non-alcoholic beverages or communications expenses, respectively. In contrast, the responses of energy, transportation and storage or professional and administrative services are slower, but deeper and more protracted, with troughs at roughly 300 days from the monetary policy shock.

Thus, despite the underlying differences in data sources and coverage – our consumption measures being computed from banking transactions by private households and firms' sales measures from VAT tax declarations – we reassuringly obtain comparable patterns to those in the previous subsection. Importantly, the results here further suggest that the responses of upstream sectors, providing general purpose inputs for the production of goods and services, may be different from downstream sectors.

We submit this observation to a formal test. To do so, we return to the full daily disaggregated sales data for 20 sectors and bridge the Spanish Tax Authority sales sectoral classification with the INE Input-Ouput matrix to compute an upstreamness indicator following Antràs et al. (2012); Appendix D.1 provides details on how we bridge the sectoral classification of the Spanish Tax Authority with the INE IO sectoral classification, and on how we derive our upstream vs. downstream sectoral classification of the sales data. Based on this indicator, we classify a sector as upstream—far from final demand by households—or downstream depending on whether it scores above or below the empirical average of the indicator for all sectors.[27] As shown in the Appendix, in our classification, overall energy, transportation and storage, and professional and administrative services are accurately considered upstream sectors.

We then estimate the following panel Local Projection:

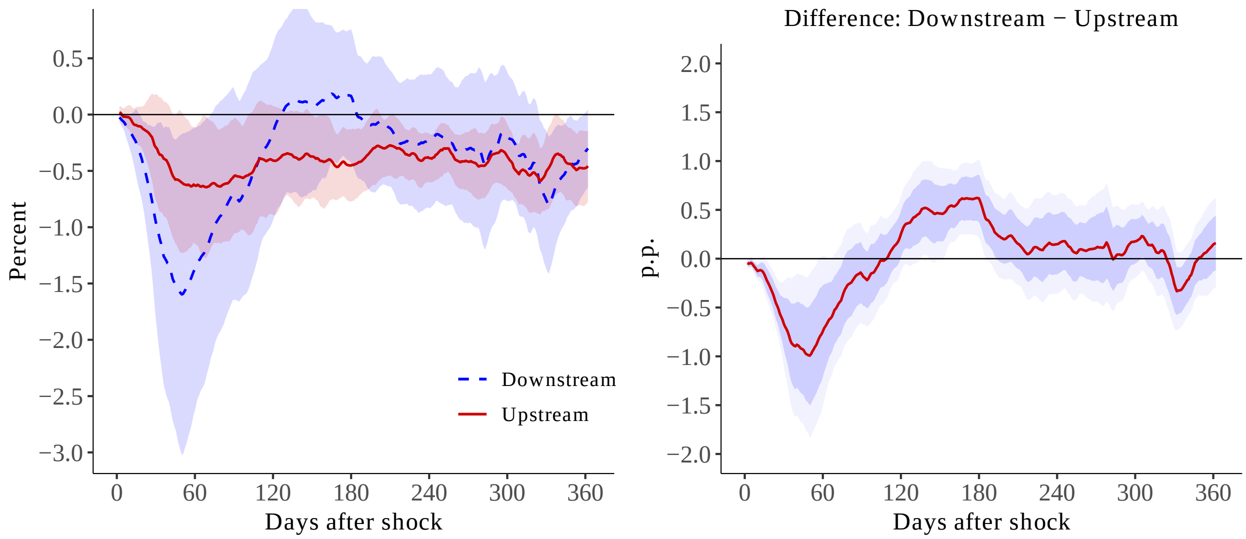
$$\begin{aligned} y_{t+h,s} = \alpha_{h,s} + \sum_{\ell=0}^{k} \beta_{h,\ell} shock_{t-\ell} + \sum_{\ell=0}^{k} \gamma_{h,\ell} shock_{t-\ell} \times up_s \\ + \sum_{\ell=1}^{p} \varphi_{h,\ell} y_{t-\ell,s} + \theta_h cases_t + \delta_h stringency_t + \varepsilon_{h,t}, \end{aligned} \quad (2)$$

where $\alpha_{h,s}$ is the sector fixed-effect and up_s is a dummy variable that takes value one if the sector is classified as upstream and zero if it is classified as downstream. This equation is the panel version of equation (1), with an added interaction term of the monetary policy shock using the upstream dummy. We estimate it with fixed-effects: $\hat{\beta}_{h,0}$ gives us the estimated response of sales at horizon h for downstream sectors, while $\hat{\beta}_{h,0} + \hat{\gamma}_{h,0}$ gives us the estimated response of sales at horizon h for upstream sectors.

Results are shown in Figure 5. The left-hand panel shows that the response lag of

[27] Our results were consistent under an alternative sector classification, considering upstream if above the third quartile and downstream if below the first quartile.

Figure 5: Upstream vs. downstream sectoral sales to a monetary policy shock



Notes: Left panel displays the LP impulse response functions to a one standard deviation monetary policy shock. The responses are reported in levels. The confidence intervals are computed from heteroskedasticity-robust standard errors. Lighter-shaded areas are the 90% confidence intervals. The sample starts in July 1st, 2018 and ends in October 30th, 2023. The monetary policy shock standard deviation is 4.1bp. See Appendix D.1 for further details on sales upstream (red line and band) vs. downstream (dashed blue line and blue band) sectoral classification. Right panel presents the difference between upstream and downstream sales responses together with the 68% (lighter-shaded areas) and 90% (darker-shaded areas) confidence intervals of this difference.

downstream sales—closer to final consumption demand—is short, statistically significant in about a month, and, compared to upstream sales, much stronger on impact.[28] The short-run trough of downstream sectors is almost three times deeper relative to the upstream sectors. Mirroring the pattern in the response of consumption that we have documented previously, downstream sales stabilize in the second quarter, and the contract further at long lags. Conversely, the response lag of upstream sectors is somewhat slower—it becomes statistically significant 60 days after the shock. The upstream response is, however, very persistent and the size of the contraction remains stable at longer lags. Finally, the right-hand panel of the figure plots the difference between the two groups of sectors, highlighting the statistically significant deeper short-run response of the downstream part of the economy during the first quarter after the monetary policy shock, with the two series aligning over longer—6 months and above—lags.

Overall, these results provide a distinctive perspective on the short-term transmis-

[28]We explored the monthly sales data beginning in 2000 and confirmed that, in the pre-COVID sample ending in 2019, downstream sectors exhibit stronger contemporaneous responses to monetary policy shocks than upstream sectors. However, differences in sectoral classification between the monthly and daily sales data constrain the comparability of this analysis.

sion of monetary policy. The contraction in upstream industries in the economy lags the strong and quick downstream response, suggesting a pattern of gradual (usptream) transmission of an initial downstream, final demand adjustment to a monetary policy impulse. At the same time, once underway, our results suggest that the gross output response of upstream industries is more persistent. Taken together, the significant differences in these responses also point to the potential usefulness of explicitly considering production networks as an important determinant of the transmission of monetary policy shocks; see, for example, Ozdagli and Weber (2017) and Ghassibe (2021) for early empirical findings along these lines.

6 Time Aggregation

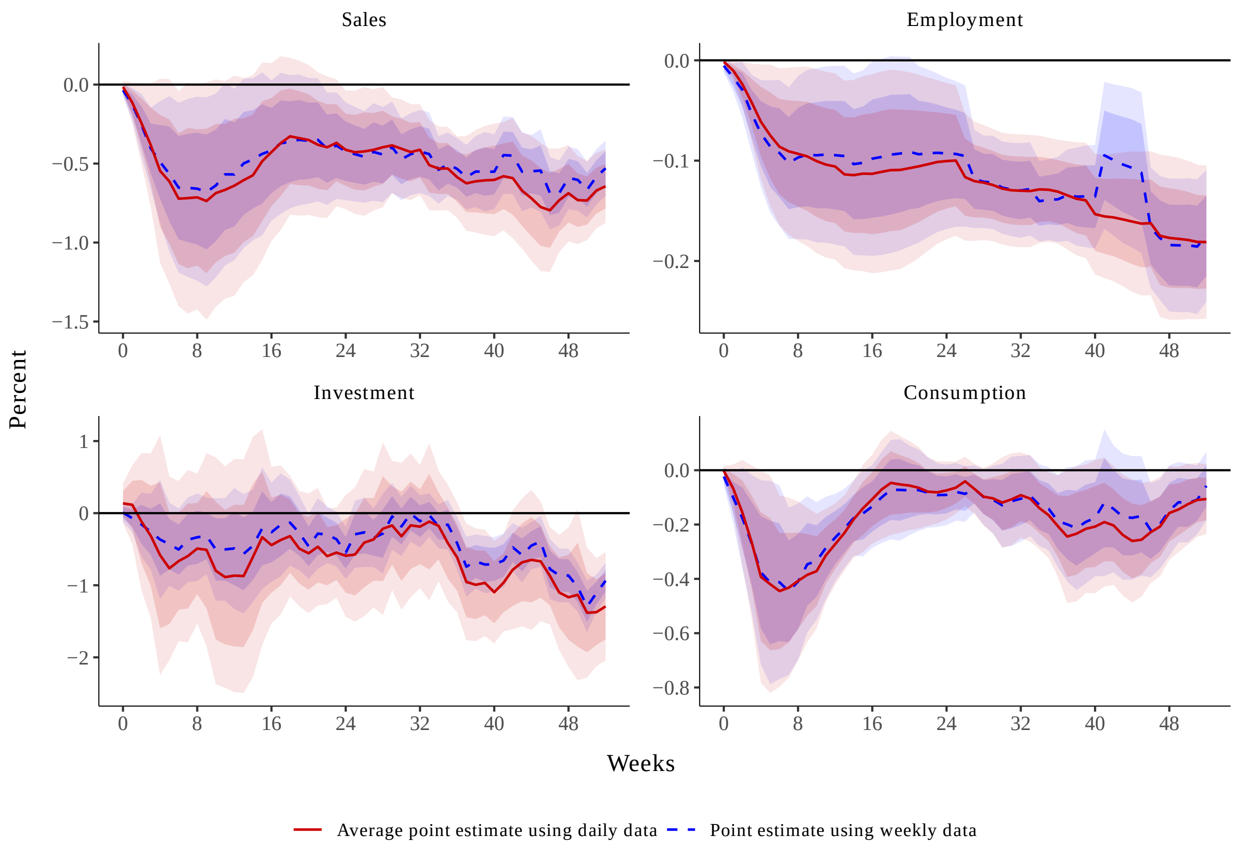
Unlike the high-frequency data we assemble in this paper, empirical measurements of continuous aggregate processes are typically available only relatively infrequently, at monthly or quarterly intervals at best. This, in turn, raises the well-known possibility of 'time aggregation biases,' whereby infrequent measurement may compromise the validity of empirical findings.[29] For example, in one of the first empirical assessments of the importance of such biases, Christiano and Eichenbaum (1987) already conclude that "temporal aggregation bias can be quantitatively important in the sense of significantly distorting inference" regarding the dynamic relation between money and output.[30] These issues have become newly relevant in the context of the modern literature based on high-frequency identification of shocks, as these shocks typically need to be "time aggregated" into the lower frequency at which economic series of interest are available. Thus, while Gertler and Karadi (2015) note that simply cumulating – by summing over time – high frequency surprises may be problematic whenever the source of disturbances is not periodic or when disturbances tend to take place at the end of the low-frequency period, Ramey (2011) points out that solutions to this problem, such as averaging across periods, may in turn generate predictability and serial correlation of shocks.

In this section, we leverage our high-frequency dataset—and our ability to line up the frequency of shocks with that of data—to shed empirical light on potential issues arising from time aggregation of data. We propose to do so by comparing two sets of estimates. The first is obtained by time-aggregating—by averaging—our daily *local pro-*

[29] This is a classical question in economics and econometrics; see, e.g., the seminal contributions of Amemiya and Wu (1972), Sims (1971) and Geweke (1978), Marcet (1991) among others.

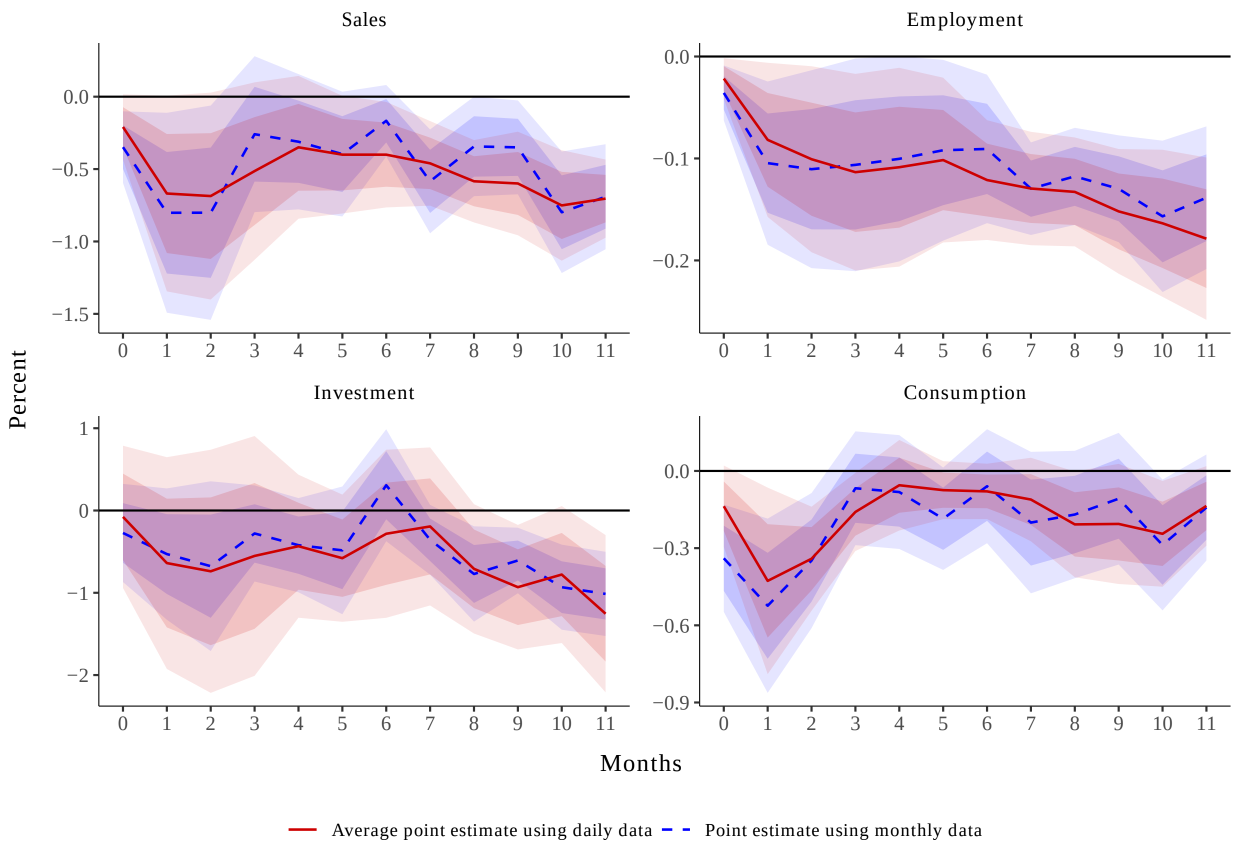
[30] See also the corresponding comment by Stock (1987).

Figure 6: Time aggregation: Weekly responses



Notes: LP impulse response functions to a one standard deviation monetary policy shock. The responses are reported in levels. The confidence intervals of time-aggregated data LP's are computed from heteroskedasticity-robust standard errors, while the confidence intervals of time-aggregated daily LP's are computed based on averaged standard errors that take into account the variance-covariance structure of the daily response estimates across horizons. Darker-shaded areas are the 68% confidence intervals, and lighter-shaded areas are the 90% confidence intervals. The sample ends in October 30th, 2023. The sample starts one year after the sample start of each series reported in Table A1. For sales and investment—the shorter series—the monetary policy shock standard deviation is 4.1bp, while for consumption and employment it is 3.7bp. Clockwise, we display local projections for total sales, employment, consumption and investment. Dashed lines are the implied low-frequency aggregated data (weekly) LP point estimates, while solid lines are the weekly averages of daily LP estimates in our baseline.

Figure 7: Time aggregation: Monthly responses



Notes: LP impulse response functions to a one standard deviation monetary policy shock. The responses are reported in levels. The confidence intervals of time-aggregated data LP's are computed from heteroskedasticity-robust standard errors, while the confidence intervals of time-aggregated daily LP's are computed based on averaged standard errors that take into account the variance-covariance structure of the daily response estimates across horizons. Darker-shaded areas are the 68% confidence intervals, and lighter-shaded areas are the 90% confidence intervals. The sample ends in October 2023. The sample starts one year after the sample start of each series reported in Table A1. For sales and investment—the shorter series—the monetary policy shock standard deviation is 4.1bp, while for consumption and employment it is 3.7bp. Clockwise, we display local projections for total sales, employment, consumption and investment. Dashed lines are the implied low-frequency aggregated data (monthly) LP point estimates, while solid lines are the monthly averages of daily LP estimates in our baseline.

jection estimates at the weekly, monthly and quarterly horizons.[31] The second is obtained from local projections based on time-aggregated versions of our *daily data*—at weekly, monthly and quarterly frequency—where we average the high frequency monetary policy disturbances. As noted above (and in Section 3.3), autocorrelation arises in monetary policy surprises following their aggregation to lower frequencies. Consequently, we incorporate six lags for monthly frequencies and two for quarterly frequencies of monetary policy shocks into our control variables.

For our two new series, averaged daily impulse responses and direct lower-frequency impulse responses estimates, in Figures 6, 7 and 8, we plot the implied responses of gross output, consumption, investment and employment, at the weekly, monthly and quarterly frequencies, respectively.

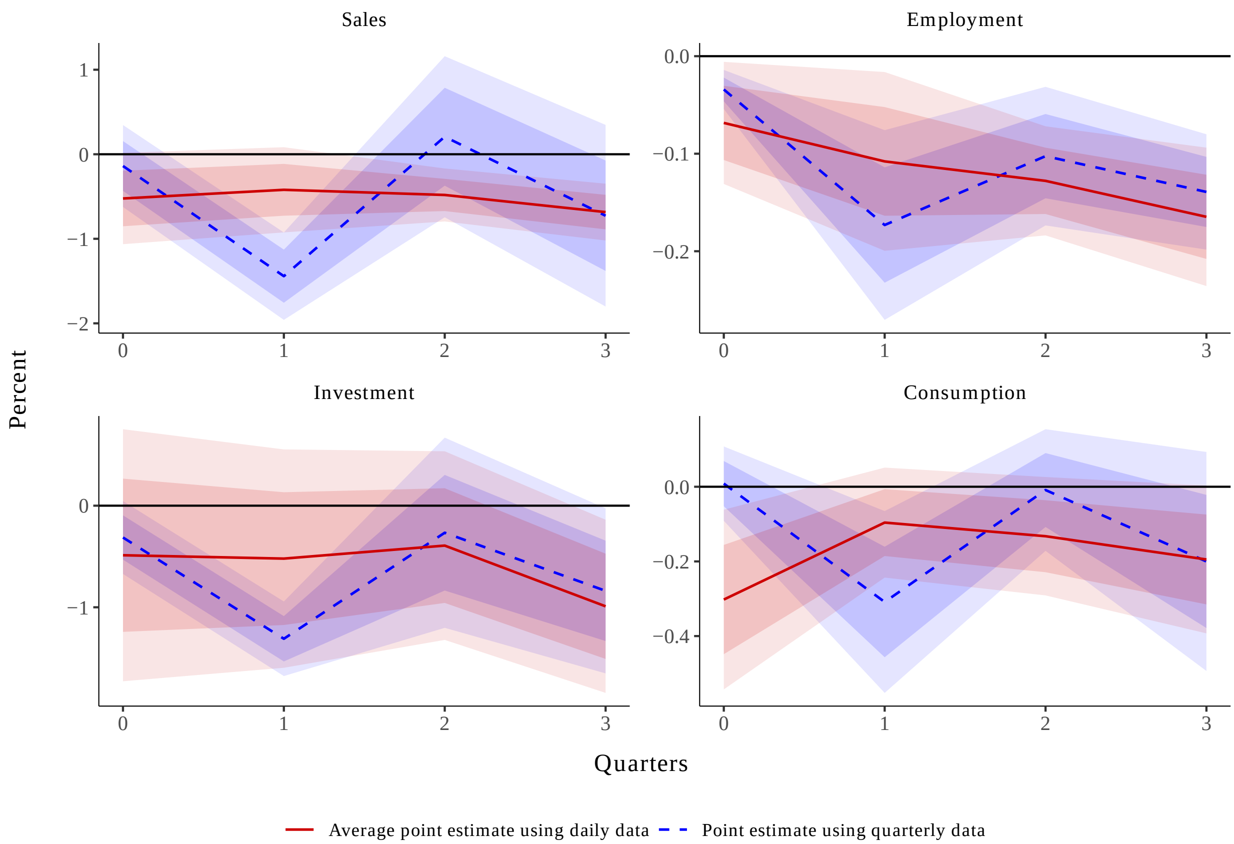
As shown in Figures 6 and 7, for all variables, the point estimates of the response using our aggregated daily series and using weekly or monthly aggregated data are close to each other—the confidence intervals nearly overlap. Patterns are similar in the two estimates: the responses of consumption and sales are significant in the first two months of a monetary policy shock; the short-run contraction is noisier for investment, but align over a large portion of the year horizon; the responses of employment is economically smaller, but quite persistent and significant over the horizon of our analysis. In our data, time aggregation is not an issue in studies employing monthly or higher frequency series.

However, time aggregation *does* make a difference at the quarterly frequency—as shown in Figure 8. Using quarterly data, we find no significant same-quarter responses of gross output and consumption, while we do so when using averaged daily responses at a quarterly frequency. In our data, quarterly aggregation shifts information to lower frequencies—responses become statistically significant only in the second quarter after the shock. It thus alters both the impact response to the shock and the overall time profile of the implied local projections during the first year after a shock.

Overall, our results above suggest that whenever researchers rely on monthly or higher frequency data, time aggregation biases are likely not a first-order issue for the large and fast-expanding literature using high-frequency identification of monetary policy shocks. However, at lower frequencies, our evidence suggests that time aggregation

[31] Instead of displaying all IRFs at low(er)-frequencies, in Appendix E.1 we present an alternative approach where we plot the same low-frequency LP IRFs at a daily resolution; that is we plot, say, a LP based on aggregated monthly data against a daily axis, allowing us to superimpose our daily baseline local projection estimates (rather than averaging these to the montly frequency as in main text). Comparisons may be harder visually. Yet, this alternative method highlights the finer details of high-frequency data, especially in investment, where responses become significant around 45 days after shock. It also highlights that sales, consumption and employment respond primarily at the end of the month, reinforcing the value of daily data granularity.

Figure 8: Time aggregation: Quarterly responses



Notes: LP impulse response functions to a one standard deviation monetary policy shock. The responses are reported in levels. The confidence intervals of time-aggregated data LP's are computed from heteroskedasticity-robust standard errors, while the confidence intervals of time-aggregated daily LP's are computed based on averaged standard errors that take into account the variance-covariance structure of the daily response estimates across horizons. Darker-shaded areas are the 68% confidence intervals, and lighter-shaded areas are the 90% confidence intervals. The sample ends in October 30th, 2023. The sample starts one year after the sample start of each series reported in Table A1. For sales and investment—the shorter series—the monetary policy shock standard deviation is 4.1bp, while for consumption and employment it is 3.7bp. Clockwise, we display local projections for total sales, employment, consumption and investment. Dashed lines are the implied low-frequency aggregated data (quarterly) LP point estimates, while solid lines are the quarterly averages of daily LP estimates in our baseline.

may confound the lags in the transmission of monetary policy, shifting information in the data. This result is relevant for the large body of academic and applied literature that routinely aggregates identified monetary policy shocks around policy announcements to quarterly, or yearly frequencies. The reliance on quarterly data in many classical studies of monetary policies may explain why short lags went long missed in the literature: the responses are not invariant to the frequency of data employed in the analysis.[32] These results both echo the early conclusions of Christiano and Eichenbaum (1987)—who found that the dynamic correlation between money and output is altered when going from monthly to quarterly frequency data—and complement the contemporary findings of Jacobson, Matthes and Walker (2023) who find that the salience of the so-called 'price-puzzle' is significantly dampened when analyzing high frequency data.

7 Extensions and robustness

In this section, we extend our empirical study and conduct a series of robustness exercises taking advantage of a larger dataset with variables that are available at the monthly, but not higher, frequency. In doing so, we build on our time aggregation result, that short lags are preserved when going from daily to monthly.

We start by completing our analysis of the short-run response to monetary policy focusing on financial conditions, sentiment indicators, and expectations. We then turn attention to the response of inflation, which, as we will show, is crucial to characterize the long lags of monetary policy in the aggregate. In the last part of the section, we focus on four robustness exercises.

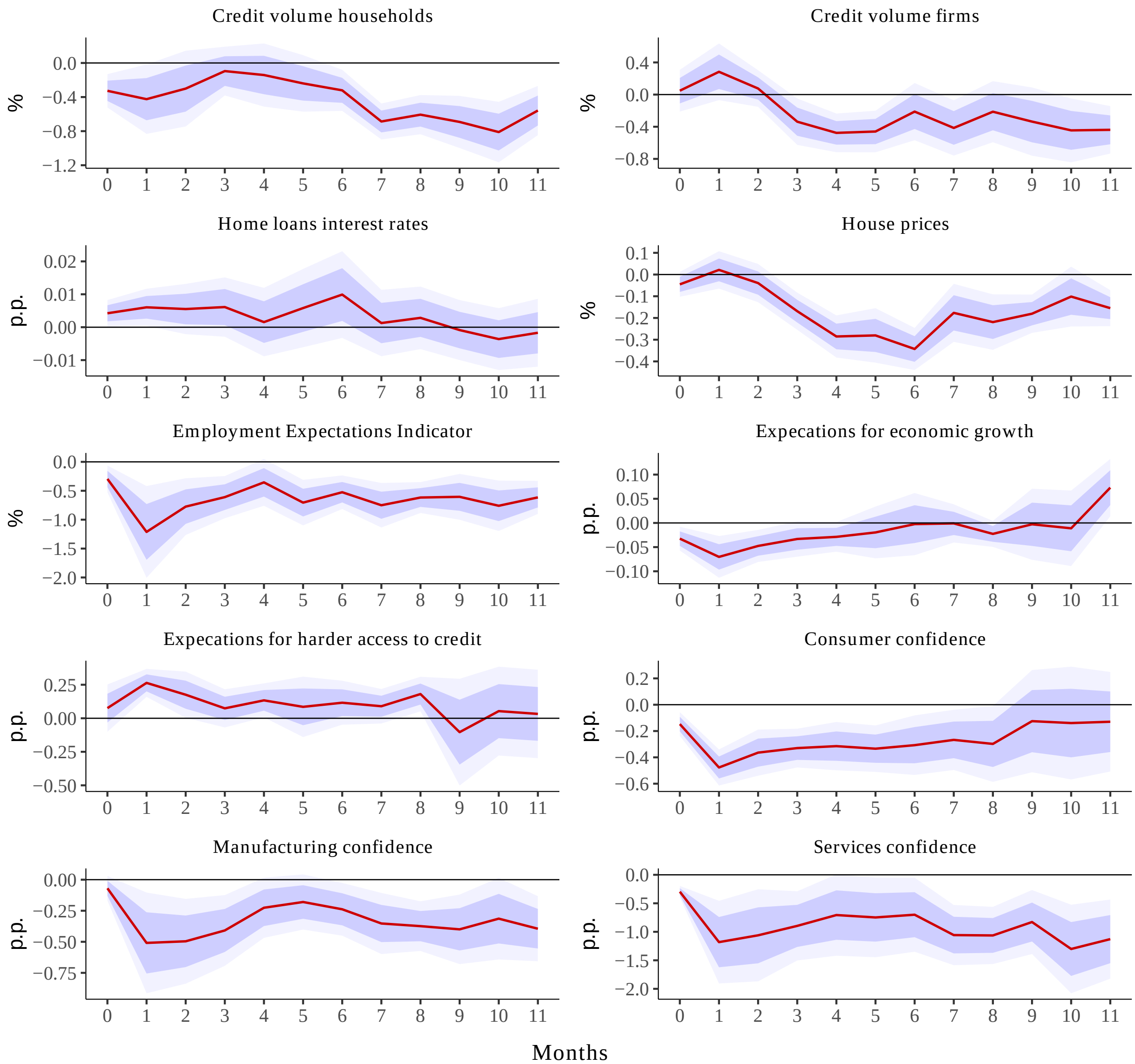
7.1 What else responds fast? Credit Conditions, Sentiments, and Expectations

We complete the picture of fast responding variables by bringing forward evidence, at monthly frequency, on the transmission of monetary policy to the volume of credit to firms and households, mortgage rates and house prices, as well as survey-based sentiment indicators concerning access to credit, expectations of economic conditions in the future.[33]

[32] It follows that differences in the empirical results on monetary transmissions across different contributions may not be driven exclusively by differences in the identification strategy—but may be a function of the frequency of the data employed in the analysis.

[33] Appendix F.2 presents supplementary evidence on the transmission of monetary policy shocks to other real activity, expectations, and confidence monthly indicators.

Figure 9: Monthly IRFs of prices to a monetary policy shock



Notes: LP impulse response functions to a one standard deviation monetary policy shock. The responses are reported in levels. The confidence intervals are computed from heteroskedasticity-robust standard errors. Darker-shaded areas are the 68% confidence intervals, and lighter-shaded areas are the 90% confidence intervals. For all series—except for expectations for economic growth and for harder access to credit which start in April 2020—the sample starts in August 2016 and ends in October 2023. The monetary policy shock standard deviation is 4.2bp for the full sample series and 3.6bp for the two series that start in April 2020.

We start by displaying, in Figure 9, the responses of credit volumes. Credit to households declines by about 0.42% already in the first month after the monetary policy shock, attaining a deeper trough, at about -0.81%, at 10 months. In contrast, the volume of credit to firms contracts significantly only from the second quarter on after the shock, with a decline of -0.4% thereafter. Notably, the impact response is a mild expansion, which may reflect temporary higher credit needs. Vis-à-vis these different patterns, the expectations of access to credit, capturing borrowing conditions, deteriorate on impact and remain pessimistic over 9 months—see the fourth row of the figure. Credit markets respond early, contributing to the buildup of the effects of monetary policy in the aggregate.

In line with the deterioration of credit conditions, house prices, shown in the second row of the figure, drop significantly from the third month from the shock on—with a trough of -0.34 percentage points at month 6. The empirical response of mortgage rates, while also consistent, is more muted and shorter lived. Mortgage rates rise on impact, with a peak response of 1bp, and remain elevated for 6 months after the shock, turning insignificant in the second half of the year.

Short lags are apparent in the fast, strong response of all indicators of confidence, shown in the bottom three rows of Figure 9, reporting IRFs for the expectations of employment and economic activity (third row), consumer confidence (fourth row, second column), manufacturing and service confidence (bottom row). Together, these figures suggest an immediate, generalized deterioration of sentiment anticipating lower economic activity, income and employment opportunities. In Appendix F.2 we further document that survey respondents indeed expect higher unemployment and lower income and house prices.[34]

To conclude, the evidence in this section suggests that, at short lags, key actions of firms and households following monetary policy surprises—in the form of consumption, investment and gross output responses, as in our baseline results—are consistent with both (i) rapid observed responses in these agents' forward-looking sentiments and expectations and (ii) deteriorating credit conditions and borrowing costs.

7.2 The long lags in inflation match employment

Using monthly data, we can match the response of employment with that of inflation—CPI data being available only at the monthly frequency. Thus, in the first row of Figure 10 we restate the response of monthly employment, obtained by aggregating our daily data at the monthly frequency (left panel), together with the response of the CPI (right panel). Note that the response of employment is very close to our baseline in Figure 2, in line with our results on time aggregation above.

The key result here is that, similarly to employment, the CPI decline is only gradual. Despite the CPI dropping significantly on impact—there is no "price puzzle" in our

[34] It is also worth stressing that these expectations are comparable across survey respondents whether they work in the construction and retail sectors, or in manufacturing and services.

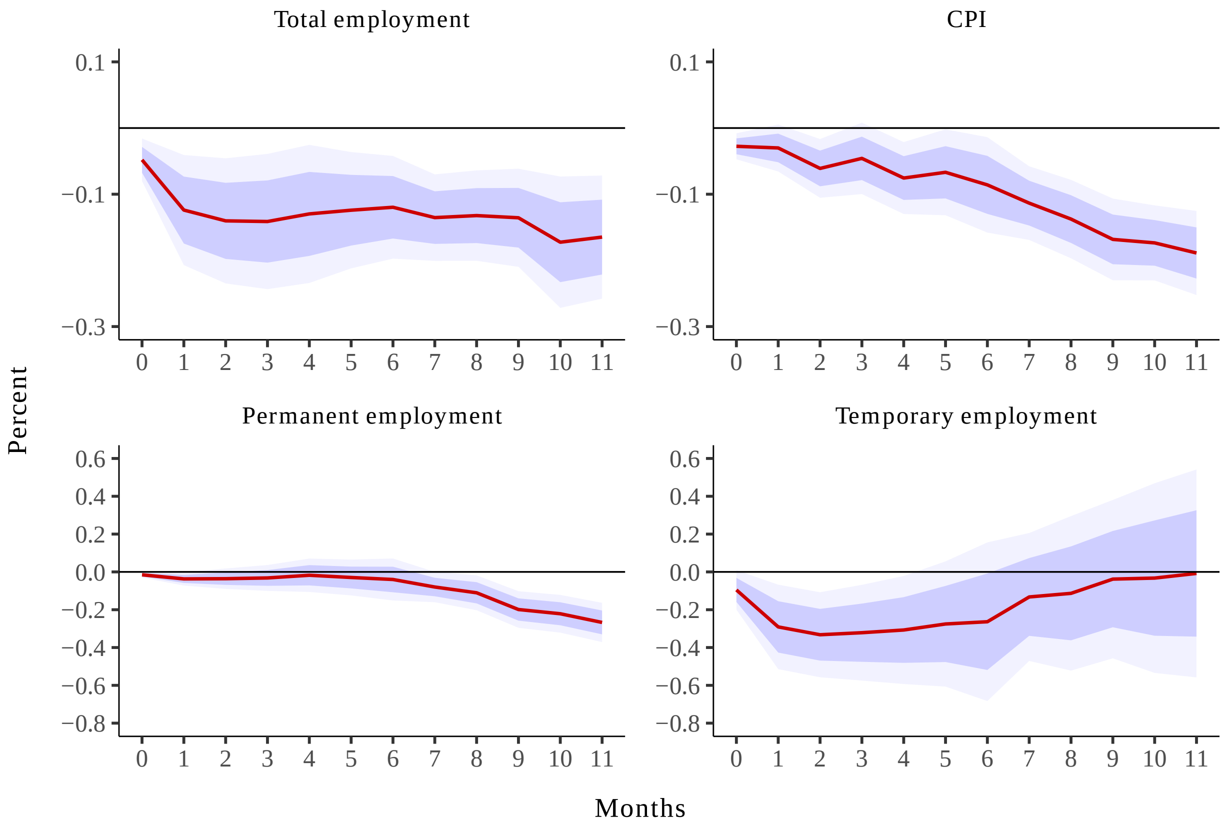
results—its short-run response is economically small and slow to set in, relative to the global trough of -0.2 percentage points taking place 11 months after a monetary policy shock. Further, at this monthly frequency, we can also study the response of disaggregated CPI inflation across eleven COICOP categories. As shown in Appendix F.1, for 9 of these categories, we find that prices appear not to respond significantly, if at all, at short lags. Instead, broader-based declines set in in the second part of the year.[35] Disaggregated patterns then cumulate up to a smooth and protracted decline in the aggregate CPI, capturing the aggregate effects of monetary policy on the target measure of inflation. This substantiates our earlier conclusion: while headline macro aggregates do respond fast, the effects of monetary policy on, arguably, the key variables of interest to central bank—employment and inflation—do materialize at only long lags, in line with the consensus view.

The availability of disaggregated series at monthly frequency also allows us to elaborate further on the transmission of monetary policy on employment. To do so, we rely on two publicly available series of the Spanish Social Security data, one for temporary (fixed-term) contracts, the other for permanent (no fixed-term declared) contracts. The responses of these two series to a policy shock, shown in Figure 10, are markedly different. The response of permanent employment, which accounts for about 84% of contracts in Spain, is muted at short lags—it becomes significant in statistical and economic terms in the last months of the year. In contrast, temporary employment adjusts on impact, with a contraction that lasts for around three months after the shock. The response in total employment shown in the same figure results from the combined dynamics of these two series: the smooth contraction at longer lags is dominated by the loss of jobs with permanent contracts, which account for the largest share of employment; the smaller contraction at short lags results from the drop in temporary employment. Our evidence thus suggests that, in economies with a higher share of temporary/flexible contracts, the response of total employment at short and long lags may well reflect significant composition effects.

Overall, the sluggish response of CPI and employment (in particular that of permanent employment) suggests that adjustment frictions in both labor markets and price updating play a role in the transmission of an otherwise fast final demand response—already non-negligible at short lags—to the overall adjustment of prices and inputs, only

[35] In detail, in Appendix F.1 we report that the short-run CPI responses are driven mainly by contractions in the CPI associated to Transport and Housing and Utilities. Food and non-alcoholic beverages, Alcoholic beverages and tobacco, Furnishings, Equipment and maintenance, and Restaurants and hotels only respond significantly later in the year, 6 months after the monetary policy shock. Finally, we find that even at the one year horizon the response for the CPI of Health, Communication and Education Services remains insignificant.

Figure 10: Monthly IRFs of employment and CPI



Notes: LP impulse response functions to a one standard deviation monetary policy shock. The responses are reported in levels. The confidence intervals are computed from heteroskedasticity-robust standard errors. Darker-shaded areas are the 68% confidence intervals, and lighter-shaded areas are the 90% confidence intervals. The sample starts in August 2015 and ends in October 2023. The monetary policy shock standard deviation is 3.7bp.

substantial at longer lags.

7.3 Robustness

We now focus on four robustness exercises. First, we validate our results on alternative monthly data sources, considering the response of industrial production, trade and demand variables. Second, we consider alternative seasonality and smoothing adjustments. Third, we employ alternative series of monetary policy surprises proposed in the literature. Fourth, we analyse alternative samples and subsamples that do not overlap with the COVID-pandemic. We demonstrate that our baseline findings are robust to all these variants in methods, data sources, shock measures, samples and the inclusion (or not) of the COVID-19 pandemic in our baseline sample.

7.3.1 Short-Lags in Industrial Production and Demand

Industrial production is the most commonly available proxy indicator of economic activity at monthly frequency—unsurprisingly, widely employed in the analysis of the transmission of monetary policy. It serves as an alternative proxy to our gross output VAT sales series (particularly for manufacturing and goods producing sectors) while allowing us to compare our short-run responses to those reported elsewhere, for other economies. In Figure 11, first panel, we show that, following a monetary shock, monthly industrial production in Spain also contracts at short lags, declining by -0.29% on impact and by 0.78% at the 1 month horizon. At longer lags the contraction first moderates, then turns significant again through month seven. Importantly, this same on-impact, short-lag response of industrial production to a monetary shock is by no means specific to Spanish data; working with US Industrial Production data, Miranda-Agrippino and Ricco (2021) and Bauer and Swanson (2023) report similar short-run responses, consistent with the evidence here.

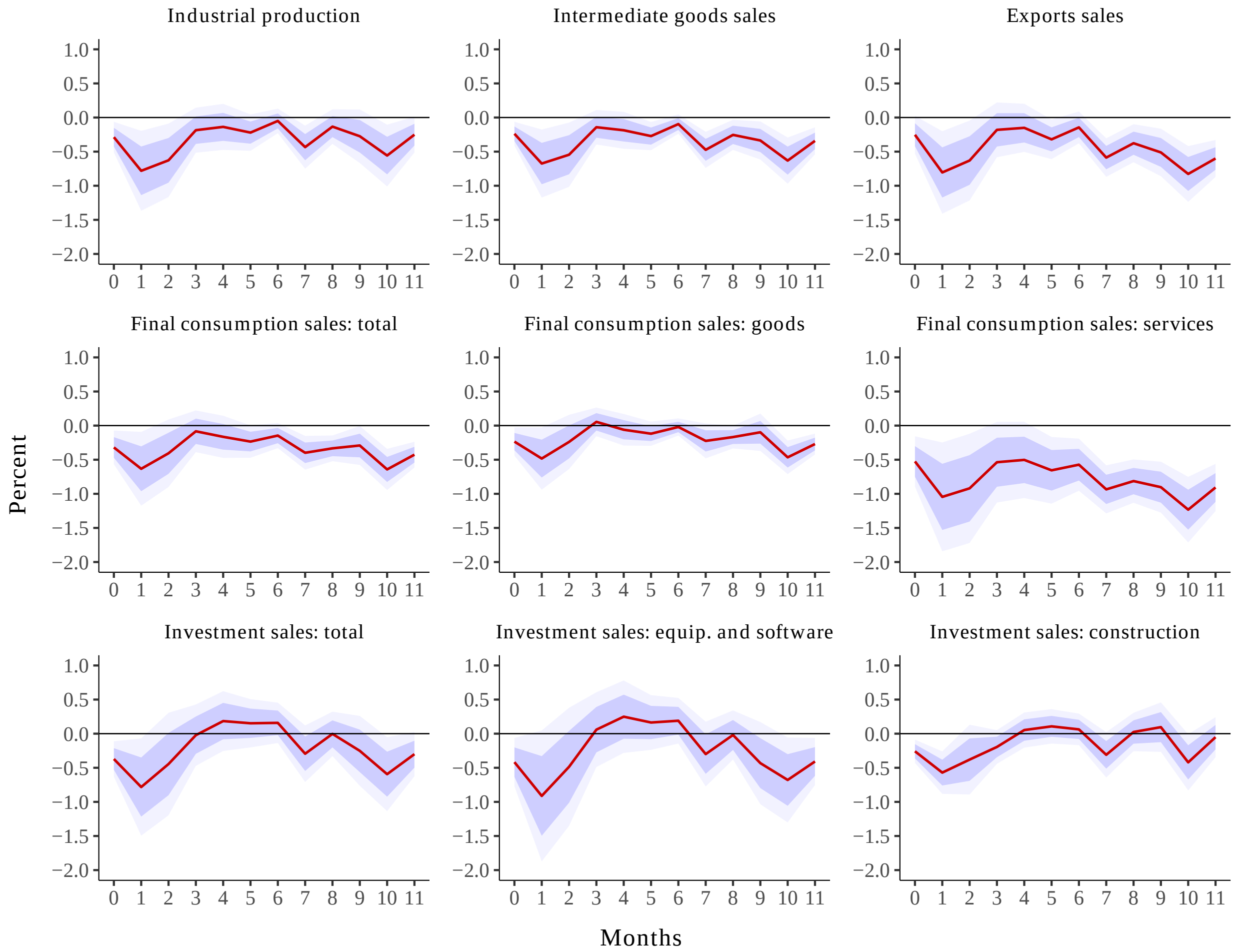
Using data at monthly frequency, we can also re-analyze and extend our headline findings regarding consumption and investment demand. In particular recall that, as discussed in Section 2, the Spanish Tax Authority—based on monthly tabulations of VAT sales' declarations by large corporations— additionally compiles series that (*i*) disaggregate the gross domestic output series into aggregate consumption, aggregate investment and aggregate intermediate input sales and (*ii*) tabulate external sales, i.e., export and import series.[36] Thus, at the monthly frequency, we have an alternative data source to our baseline, transaction-based, consumption and investment series. These series also mitigate concerns related to the impact of daily noise in our high-frequency data.

Results are shown in Figure 11. The sales of intermediate goods and services and exports sales essentially display the same pattern of the response of the Spanish Industrial Production Index—see the top row of the figure.[37] The responses of the VAT-derived monthly final consumption measure—as well as its disaggregation into consumption goods and consumption services (in the second row of the figure)—are also consistent with our daily baseline findings. A drop in the final consumption sales series happens within the first month after the contractionary monetary policy shock, reaching a local trough in the following month. A second local trough follows later in the year, around 10 months after the shock. The estimated size of the short-lag response is also very similar

[36] These series, sourced from the External Trade in Spain Statistics (AEAT - Spanish Tax Agency) are direct inputs to the compilation of quarterly national accounts by Spain's National Statistical Institute.

[37] The IR of import sales display the same pattern as export. We do not shown the corresponding plot to avoid redundancy.

Figure 11: Monthly IRFs of industrial production and sales



Notes: LP impulse response functions to a one standard deviation monetary policy shock. The responses are reported in levels. The confidence intervals are computed from heteroskedasticity-robust standard errors. Darker-shaded areas are the 68% confidence intervals, and lighter-shaded areas are the 90% confidence intervals. The sample starts in August 2015 and ends in October 2023. The monetary policy shock standard deviation is 3.7bp.

to our baseline results. Looking at the disaggregated series, the brunt of the adjustment is borne out by the consumption of services—broadly in line with our findings in Figure 3, documenting significant short-run adjustment in spending on education, health, recreation and culture, and restaurants and hotels—all of which are consumption services.

The third and final row of Figure 11 reports the response of the monthly investment series derived from VAT data, both the aggregate (first column), and disaggregated into two broad categories: investment in equipment and software (second column) and investment in construction (third column). Relative to our results based on daily data, local projections based on these series are significantly less noisy. Qualitatively, the figure confirms the pattern discussed in Sections 4 and 5.2.[38] Relative to consumption, the short-lag response of investment is again stronger —primarily driven by spending on equipment, maintenance and software. The muted response in construction investment is consistent with the daily gross output response of construction firms in Figure 4.

7.3.2 Seasonal Adjustment and Smoothing of Daily Series

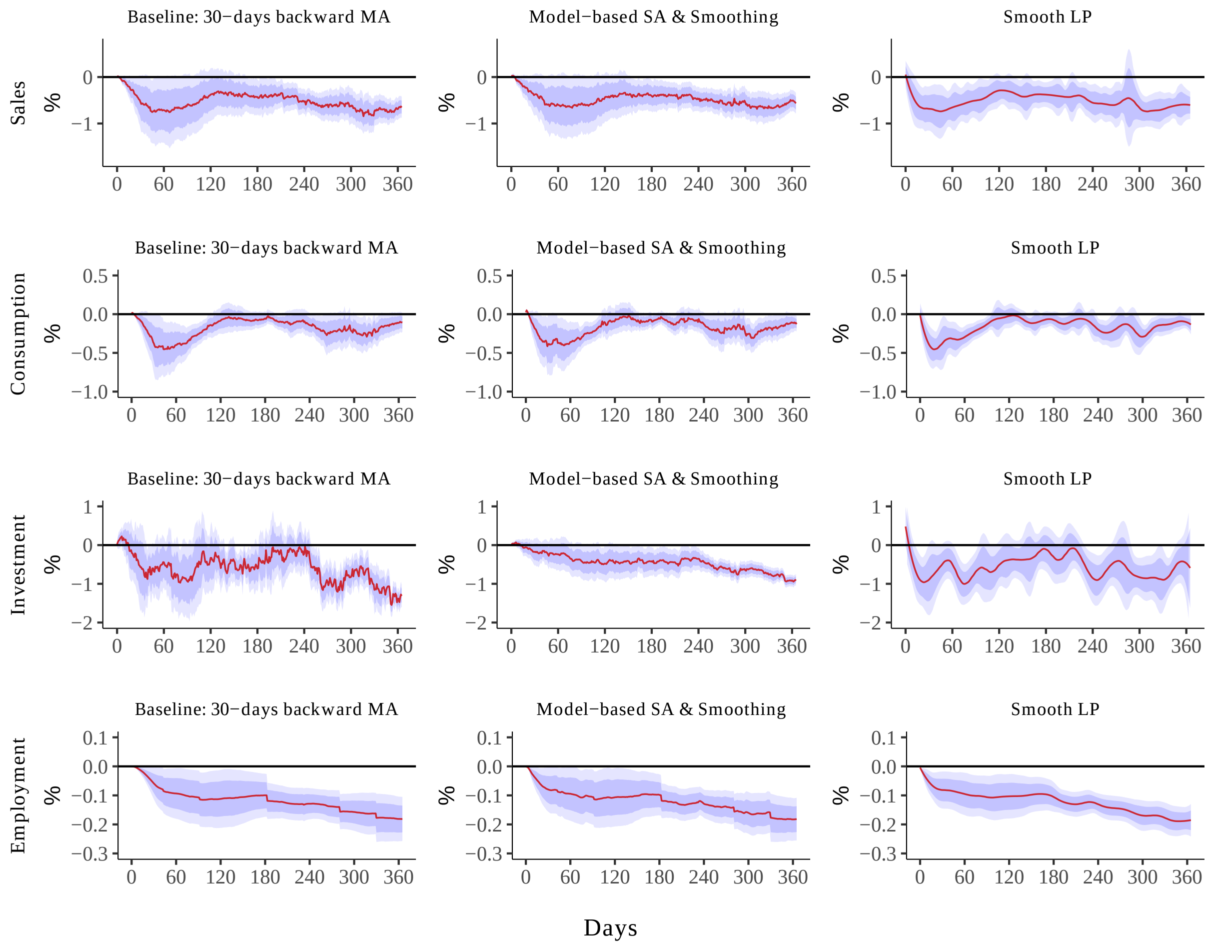
As discussed in Section 3.2, working with raw daily data presents challenges. First, the periodic structure of daily and weekly data is less standard, and calendar effects together with (possibly moving) holidays need to be accounted for. Second, daily and weekly series are typically much noisier than lower frequency series. This is either because the construction of lower frequency series entails smoothing—through time averaging—of intrinsically high-volatility series; or because measurement error is larger at the daily and weekly frequencies. Recall that to obtain our baseline results we deployed what is arguably the most transparent and elementary method to deal with these two challenges: a 30-day moving average to smooth over daily noise coupled with a daily year-on-year growth rate transformation, differencing out day-of-the-month effects. In this section, we assess the robustness of our baseline results to employing alternative methods to deal with seasonality, calendar effects, and noise.

The first alternative we consider is an unobserved components approach that econometrically decomposes the raw observed daily series into distinct components—trend-cycle, seasonal and irregular—with flexible laws of motion for each component; see, for example, (Harvey, 1989) for a review. By directly estimating both seasonal and irregular (i.e. daily noise) components, unobserved components methods provide a joint solution to seasonality and noise smoothing challenges: once estimated, it suffices to subtract both components from the raw observed series in order to obtain a seasonally adjusted, de-noised series. In particular, we follow the Spanish Tax Authority's own suggestion (itself based on Cuevas, Ledo and Quilis, 2021 treatment of seasonality and noise in daily VAT sales), and implement a variant of the "Trigonometric seasonality, Box-Cox transformation, ARMA innovations, Trend and Seasonality" (TBATS) unobserved components model proposed in De Livera, Hyndman and Snyder (2011).[39]

[38] An immediate contraction, with a short run trough in the second month after the shock, is followed by a spell of stabilization, and then again by a decline towards the end-of-year.

[39] We proceed in two steps. First, we implement a standard pre-processing step where we remove calendar and other deterministic effects (such as day-of-the-week) via a multiplicative seasonal ARIMA model with automatic detection for outliers and with Spanish public holidays dummies included. Second, we estimate the (stochastic) seasonal and irregular components with TBATS, following closely the specification discussed in Cuevas, Ledo and Quilis (2021): we apply a log-transformation, impose no trend damping, and include weekly, monthly and annual seasonal components with a periodicity of 7, 30.4375 and 365.25

Figure 12: Daily response of real activity to monetary policy shock under alternative seasonal and smoothing procedures



Notes: Responses to a one standard deviation monetary policy shock. The responses are reported in levels. The confidence intervals are computed from heteroskedasticity-robust standard errors. Darker-shaded areas are the 68% confidence intervals, and lighter-shaded areas are the 90% confidence intervals. The sample ends in October 30th, 2023. The sample begins one year after the start of each series reported in Table A1. For sales and investment (the shorter series) the monetary policy shock standard deviation is 4.1bp, while for consumption and employment it is 3.7bp. The figure compares the effects of three seasonal adjustment and smoothing methods: a 30-day backward moving average (left column), a model-based seasonal adjustment with smoothing (middle column), and a model-based seasonal adjustment but with responses estimated via SLP (right column).

The second alternative we consider explicitly separates and applies different solutions to the treatment of seasonality and the smoothing of daily noise. In particular, we retain the unobserved component approach discussed above to deal with seasonal and

days, respectively. We then remove both the estimated seasonal and irregular components from the original series. Finally, we compute year-on-year growth rates on this filtered series; this last transformation is not strictly necessary (as it was in our baseline method), but facilitates comparison of results.

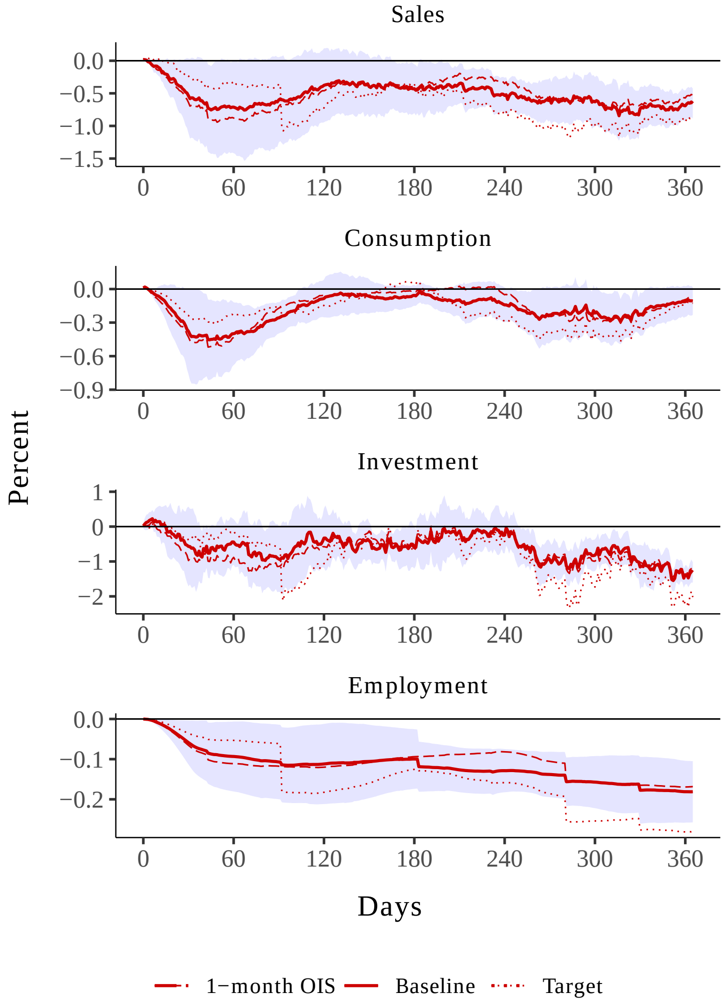
calendar effects only. However, we follow an alternative approach to deal with smoothing: instead of de-noising the series—either via moving average (as in our baseline) or removing the irregular component of the time series—we consider smoothing the local projection itself. We thus follow a two-step procedure where we first purge the raw series from calendar and seasonal—but not irregular—components, and then estimate Smooth Local Projections (SPL) on the deseasonalized data, using penalized B-splines—a popular nonparametric method for impulse response estimation - as proposed by Barnichon and Brownlees (2019).[40]

In Figure 12, we present the responses of daily sales, consumption, investment, and employment to a monetary policy shock, using these different seasonal adjustment and smoothing methods. In the left column, we reproduce our baseline findings, relying on a year-on-year transformation and a backward-looking 30-day MA. In the middle column (labeled Model-based SA & Smoothing), results are conditional on applying the TBATS unobserved components methodology, removing both seasonal and irregular components and then estimating standard Local Projections. In the right column (labeled Smooth LP), results are conditional on seasonal adjustment using TBATS and then estimating Smooth rather than standard Local Projections. Reassuringly, the responses of sales, consumption and employment are similar across all three approaches. Some differences are noticeable for the response of investment, especially for the model-based approach. This most likely reflects the fact that the incidence of noise in the raw data underlying the investment series is stronger relative to the other series. As a result, relative to our baseline and the SLP method, the model-based approach tends to interpret short-run investment reactions as noise (while still capturing medium- to long-run reactions).

For completeness, in Appendix G.1, we extend our robustness analysis to allow for further methodologies. Specifically, we systematically explore combinations of alternative seasonal adjustment methods—ranging from no adjustment at all to alternative econometric approaches—with alternative smoothing procedures—ranging from no smoothing at all, to moving averages over different windows to exponential smoothing. Overall, we again conclude that our baseline results are qualitatively and quantitatively robust to a range of alternative seasonal and calendar adjustments and alternative smoothing methods.

[40]By smoothing the Local Projection estimates, SLPs reduce the variability of the IRF estimates while preserving the flexibility of the LP method. Barnichon and Brownlees (2019) demonstrate, through simulations and empirical applications, that SLP provides more accurate and smoother IRF estimates than standard LP, especially when the data are noisy.

Figure 13: Daily response of real activity to alternative monetary policy shocks



Notes: LP impulse response functions to a one standard deviation monetary policy shock. The responses are reported in levels. The confidence intervals are computed from heteroskedasticity-robust standard errors. Lighter-shaded areas are the 90% confidence intervals. The sample ends in October 30th, 2023. The sample starts one year after the sample start of each series reported in Table A1. For sales and investment—the shorter series—the baseline, target and poor man's 1-month OIS monetary policy shock standard deviation is 4.1bp, 3.8bp and 3bp, respectively; while for consumption and employment it is 3.7bp, 3.6bp and 2.9bp, respectively.

7.3.3 Monetary Policy Shocks

Our baseline results rely on the methodology proposed by Jarociński and Karadi (2020) to identify monetary policy shocks in the Euro-Area. As discussed in Section 3.1, this scheme combines high-frequency identification with sign restrictions in a Bayesian VAR to control for the information effect. This choice ensures that the monetary policy shock series we use is standard, off-the-shelf and publicly available. In this section, we analyse the robustness of our baseline results to the use of alternative methods to identify monetary disturbances.

We construct two series of alternative monetary policy shocks, both making use of the publicly available Euro Area Monetary Policy Event-Study Database (EA-MPD) provided by Altavilla et al. (2019), containing intraday asset price changes around monetary policy events. The first shock series is the observed 1-month Overnight Indexed Swap (OIS) change around policy decision announcements. Unlike our baseline monetary policy shock series—which is estimated with Bayesian methods—this alternative is directly observable in data. In this case, to control for the information channel, we resort to the "poor man's approach" proposed by Jarociński and Karadi (2020), which consists of excluding observations whenever the sign of the change in the 1-month OIS is the same as the change in the Euro STOXX50E Index.

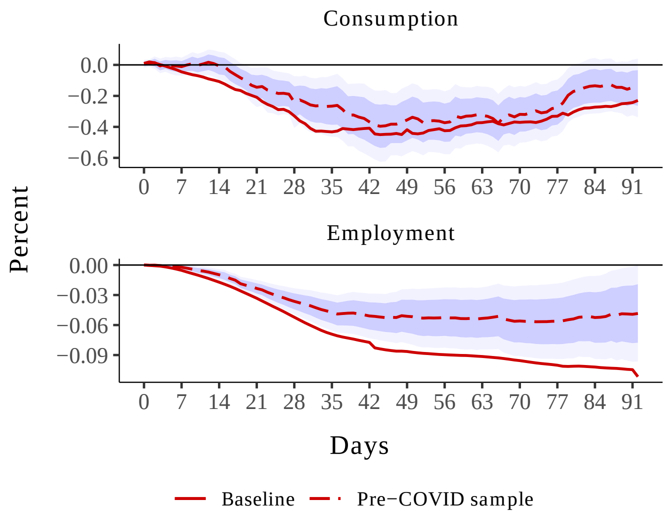
The second series of shocks is the Policy Target factor proposed by Altavilla et al. (2019)—which we have extended to cover our baseline sample period. The Policy Target factor refers to the immediate market response to changes in the ECBs main policy rate, typically announced during the policy decision release. This factor predominantly affects short-term interest rates, causing significant movements at the short end of the yield curve, while having minimal impact on longer-term rates. To construct this alternative series, we follow the methodology proposed in Altavilla et al. (2019), consisting of applying factor analysis to the observed changes in the yield curve. This series of alternative shocks, focusing exclusively on short-term yield curve changes, allows for an assessment of how much unconventional monetary policy shocks, more common before COVID, affect our baseline findings, compared to the more recent emphasis on conventional monetary policy shocks during the latest tightening cycle.

In Figure 13, we show the responses of sales, consumption, employment and investment to these two alternative monetary policy shock measures. Results are very similar to those obtained with our baseline monetary policy shock: the dynamics implied by the estimated local projections track our baseline results closely, with the responses under alternative shock series contained within the baseline confidence intervals.

Finally, we note that, differently from the shocks used in our baseline, these alternative series are arguably better thought of as instruments for monetary policy surprises, rather than actual monetary policy shocks. Indeed, for these shock series, Stock and Watson (2018) argue that a LP-Instrumental Variable approach is to be preferred to using instruments as direct measurement of monetary shocks. To demonstrate robustness to this argument, in Appendix G.2 we also show that the responses obtained from LP-IV are similar to those reported in Figure 13.

Overall, we conclude that our baseline findings on the monetary transmission to daily

Figure 14: Daily response of real activity before COVID-19



Notes: LP impulse response functions to a one standard deviation monetary policy shock. The responses are reported in levels. The confidence intervals are computed from heteroskedasticity-robust standard errors. Darker-shaded areas are the 68% confidence intervals, and lighter-shaded areas are the 90% confidence intervals. The sample ends in December 2019. The sample begins one year after the start of each series reported in Table A1. To make responses comparable to the baseline responses (solid line) reported in Figure 2, the monetary policy shock size is 3.7bp.

real activity are robust to alternative methods to identify monetary policy shocks.

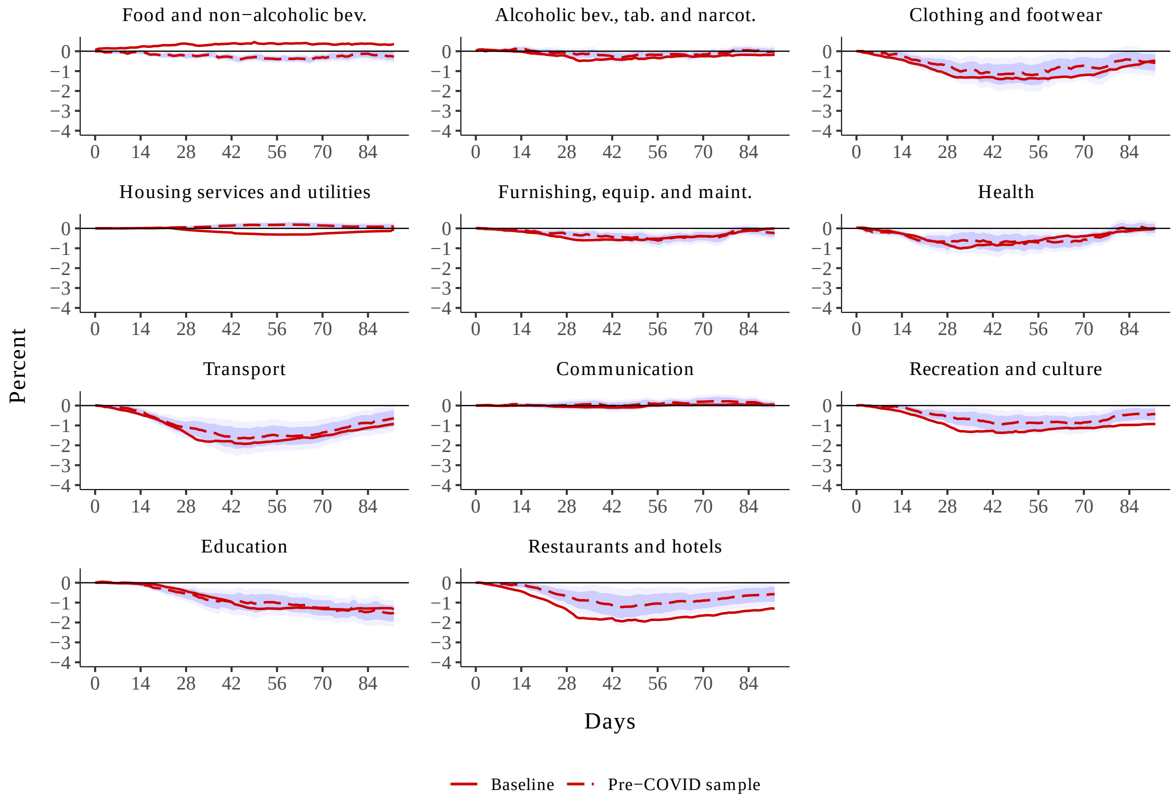
7.3.4 COVID-19

To ensure that the extreme observations and idiosyncratic dynamics observed during COVID-19 do not distort our estimates, in our baseline methods we have followed the the approach discussed in Schorfheide and Song (2024) and Lenza and Primiceri (2022) and dropped pandemic-period observations. Here we conduct two robustness checks that further suggest that COVID-19 is not driving our baseline results.

First, we use the longest of our daily series—consumption (total and disaggregated by COICOP categories) and employment— to estimate the transmission of monetary policy shocks on a COVID-19 free sample, ending in December 2019. Even for these longer series, this sample restriction implies that we only have three and a half years of data: thus, we only have enough variation in the data to estimate responses up to 93 days after a monetary policy shock. At this horizon, local projection estimates in this pre-COVID sample are based on 24 monetary policy shocks, instead of the 46 shocks in the our full sample.[41]

[41] To be clear, extending this exercise to sales and investment is not feasible as the respective samples are significantly shorter.

Figure 15: Daily response COICOP consumption categories before COVID-19



Notes: LP impulse response functions to a one standard deviation monetary policy shock. The responses are reported in levels. The confidence intervals are computed from heteroskedasticity-robust standard errors. Darker-shaded areas are the 68% confidence intervals, and lighter-shaded areas are the 90% confidence intervals. The sample ends in December 2019. The sample begins one year after the start of each series reported in Table A1. To make responses comparable to the baseline responses (solid line) reported in Figure 3, the monetary policy shock size is 3.7bp.

Figure 14 illustrates our findings for aggregate daily consumption and employment in this pre-COVID-19 subsample. Overall, the short-lags for aggregate consumption and employment in the pre-COVID-19 sample are qualitatively similar to our baseline, and only slightly attenuated quantitatively. This suggests that our baseline results are not driven by dynamics specific to COVID-19. A similar conclusion is reached when looking at the responses of disaggregated daily consumption broken down by COICOP categories; this is depicted in Figure 15. Generally, the responses by consumption category in the shorter pre-COVID-19 sample remain consistently aligned with the corresponding responses (in the initial 93 days after a monetary policy tightening) estimated using our baseline sample.

For our second robustness check, we rely on monthly data spanning a longer sample period, which allows us to analyse pre-COVID subsamples. In particular, we deploy monthly data introduced in Section 7.3.1 above, and analyze the transmission of monetary policy to monthly total sales, consumption, investment, and employment – our baseline variables – and ten other real economic indicators, over the longest available pre-COVID sample possible, running from either January 2000 (most variables) or January 2012 (employment variables) to December 2019. Further details about specific series use together with our results are detailed in Appendix G.3, where we show that the short lag response of these key real variables in this longer, monthly pre-COVID-19 sample (excluding COVID-19) are qualitatively and quantitatively consistent with our baseline findings (which include the COVID period).

Overall we conclude that our baseline findings are robust, and that monetary transmission at short lags is stable across sub-samples and not driven by spurious COVID dynamics.

8 Conclusion

We study the effects of monetary policy at short lags, assembling a high-quality high-frequency dataset on a large set of variables spanning demand, output, asset prices, expectations and confidence, combining different sources. Our rich granular dataset allows us to unveil the extent and scope of the real effects of monetary policy in the very short run across variables that are traditionally considered slow-moving.

Our findings suggest that future research, both theoretical and empirical, would benefit from redirecting focus from mechanisms that delay the transmission of shocks and financial variables to aggregate demand, toward those that slow the transmission of rapid demand and output responses to adjustments in labor, upstream intermediate inputs, and prices. Both transaction-level and administrative microdata offer promising avenues for empirically investigating these transmission mechanisms.

Our analysis also shows that the time aggregation of economic activity and monetary policy shocks may distort the identification of monetary policy transmission, shifting the empirical response to longer lags. The issues in time aggregation we document in our paper are therefore relevant to a large empirical literature that routinely aggregates identified monetary policy shocks around policy announcements to quarterly or yearly frequencies: temporal aggregation at these lower frequencies may significantly impair the identification of monetary transmission mechanisms. However, our findings also suggest that monthly frequency data provide sufficient granularity to capture the short transmission lags of monetary policy. This is particularly relevant given the increasing availability of monthly aggregate measurements which should, in turn, facilitate the replication of our findings across different countries.

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Online Appendix

A Appendix for Section 2

A.1 Summary of Data Collection and Sources

Table A1 presents an overview of the data used in the main body of the paper, omitting disaggregation of the different series of gross output and consumption by subcategories. Note that while different series have different starting dates, all end in October 2023. For the prices block of the dataset, we only have monthly data; for other variables, we have both daily and monthly data.

Variable	Proxy	Source	Frequency	Start date
Real activity				
Gross output	Sales	Spanish Tax Authority	Daily / Monthly	July 1st, 2017 / January 2000
	IP	INE	Monthly	January 2000
Consumption	Private consumption	BBVA	Daily	August 1st, 2015
	Private consumption	Spanish Tax Authority	Monthly	January 2000
Investment	Investment	BBVA	Daily	April 6th, 2017
	Investment	Spanish Tax Authority	Monthly	January 2000
Employment	Employment	Spanish Social Security	Daily	August 3rd, 2015
Financial Markets				
Interest rate	Euribor	European Money Markets Institute	Daily	January 4th, 1999
	Interest rates for housing	Bank of Spain (Statistics Bulletin)	Monthly	January 2003
Stock prices	IBEX35	Bloomberg	Daily	January 3rd, 2005
Prices				
Consumer prices	CPI	INE	Monthly	January 2000
Housing prices	Average price per square meter	CIEN	Monthly	January 2007
Expectations				
Inflation expectations	Inflation-linked swaps	Bloomberg	Daily	June 3rd, 2004
Real activity expectations	Consumer sentiment indicators	EU Commission	Monthly	January 2000
	Business sentiment indicators	EU Commission	Monthly	January 2000
	Consumer expectations	ECB	Monthly	April 2020
Financial markets expectations	Consumer expectations	ECB	Monthly	April 2020

Table A1: Data overview

A.2 Descriptive Statistics

Table A2 reports descriptive statistics of year-on-year growth rates of 30-day backward-looking moving averages for real sales, consumption, investment and employment. In our baseline sample, on average total real sales increased 3.09%, total real consumption 2.41%, real investment 6.72% and employment 2.31%. Employment is the least volatile time series, while investment is the most volatile series, in line with the standard ranking of volatilities in lower frequency data. Reflecting the effects of the COVID-19 pandemic, in our baseline sample the series of sales and consumption exhibit large variations, with a maximum contraction as deep as, respectively, -41.7% and -29.5%. Note nonetheless that, in our findings, sales and consumption fall at most by 0.8% and 0.4% in response to a contractionary monetary shock. This suggests that monetary policy shocks played a very minor role in determining the total variation in consumption and sales in our sample, as commonly found in the literature.

Table A2: Descriptive statistics of the main variables

	Mean	SD	Min	Max
Sales	3.09	13.02	-41.68	39.93
Consumption	2.41	7.08	-29.48	27.05
Investment	6.72	18.09	-50.40	53.59
Employment	2.31	2.21	-4.74	5.07

Notes: Sales, consumption, investment and employment are measured as YoY growth rates of their 30-day moving averages. We deflate daily sales and consumption using the monthly Consumer Price Index (CPI); and daily investment using the monthly investment sales price deflator used by the Spanish Tax Authority.

Turning to the disaggregated consumption and sales series, Table A3 lists the COICOP consumption subaggregates that we use in our analysis, while Table A4 reports summary statistics. Table A5 lists the NACE classification of sales data by sector from the Spanish Tax Authority— the corresponding descriptive statistics are presented in Table A6. Note that in our sample, seven sectors—beverages and tobacco, textile, paper, chemical industry, pharmaceutical, rubber and plastics, and electronics—experienced negative mean real growth rates in sales. The weighted average growth of sales by category aggregates to the total sales growth in Table A2.

Table A3: 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 A4: Descriptive statistics, COICOP consumption categories (two-digit)

Two-Digit Category	Mean	SD	Min	Max
01	8.54	12.73	-14.75	51.22
02	2.05	9.72	-30.33	40.80
03	2.69	19.42	-59.48	121.79
04	0.37	9.51	-22.01	22.27
05	4.42	11.42	-32.95	57.58
06	11.43	17.35	-45.51	115.21
07	7.65	28.11	-70.63	204.80
08	0.77	5.69	-10.31	20.80
09	3.48	18.26	-60.60	101.09
10	5.44	20.30	-53.35	115.33
11	7.04	27.84	-71.52	199.21

Notes: Consumption categories are measured as YoY growth rates of their 30-day moving averages. Categories are deflated using the CPI at their corresponding COICOP category.

Table A5: Sales sectors, NACE code, and description

Sector	NACE code	Description
Manufacturing: Food	C10	Manufacture of food products
Manufacturing: Beverages and tobacco	C11 + C12	Manufacture of beverages and tobacco
Manufacturing: Textile	C13 + C14 + C15	Manufacture of textiles, wearing apparel and leather products
Manufacturing: Paper	C17 + C18	Paper industry and graphic arts
Manufacturing: Chemical industry	C20	Chemical industry
Manufacturing: Pharmaceutical	C21	anufacturing of pharmaceutical products
Manufacturing: Rubber and plastics	C22 + C23	Manufacturing of rubber and plastics and other non-metallic mineral products
Manufacturing: Metallurgy	C24 + C25	Metallurgy and manufacturing of iron, steel, ferroalloys, and metal products (except machinery and equipment)
Manufacturing: Electronics	C26 + C27	Manufacture of computer, electronic, optical products and of electrical equipment
Manufacturing: Motor vehicles, trailers, and semi-trailers	C29	Manufacturing of motor vehicles, trailers, and semi-trailers
Manufacturing: Furniture	C16 + C31	Wood and cork industry; manufacturing of furniture
Manufacturing: Machinery	C28 + C30 + C33	Manufacturing of machinery and equipment; manufacturing of other transport equipment; repair and installation of machinery and equipment
Manufacturing: Coking and oil refining	C19 + C32	Coking and oil refining; other manufacturing industries
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
Hospitality	I	Lodging, food and beverage services, event planning, theme parks, travel agency, tourism, hotels, restaurants, nightclubs, and bars
Information and communication	J	Information and Communication
Professional, scientific and administrative	M + N	Professional, scientific and technical activities

Table A6: Descriptive statistics of sales by sector

Sector	Mean	SD	Min	Max
Manufacturing: Food	0.41	10.45	-29.15	26.86
Manufacturing: Beverages and tobacco	-0.76	15.67	-46.51	70.95
Manufacturing: Textile	-0.69	25.96	-71.27	193.32
Manufacturing: Paper	-0.73	7.74	-23.02	32.65
Manufacturing: Chemical industry	-2.22	8.49	-23.35	27.99
Manufacturing: Pharmaceutical	-2.42	14.87	-33.04	40.49
Manufacturing: Rubber and plastics	-0.07	15.99	-45.12	86.23
Manufacturing: Metallurgy	0.33	18.73	-51.51	107.46
Manufacturing: Electronics	-2.59	19.55	-58.50	110.49
Manufacturing: Motor vehicles, trailers, and semi-traile	9.38	83.27	-91.97	963.87
Manufacturing: Furniture	2.61	21.90	-53.44	147.43
Manufacturing: Machinery	1.82	18.92	-38.82	59.51
Manufacturing: Coking and oil refining stries	0.55	26.41	-68.05	100.81
Energy	12.05	42.51	-55.96	145.36
Construction	4.81	17.65	-32.89	60.58
Wholesale and retail trade	3.91	12.23	-36.17	56.41
Transport and storage	5.92	28.93	-48.11	96.33
Hospitality	31.32	76.50	-91.92	300.83
Information and communication	1.42	9.44	-22.20	39.05
Professional and administrative services	3.29	14.40	-46.26	51.75

Notes: Sales by sector measured as YoY growth rates of their 30-day moving averages. Manufacturing and Construction sectors are deflated using the closest producer price indexes (PPI); Wholesale and Retail Trade, and Transport and Storage with the closest disaggregated CPI. Finally, we use the Services Price Index (SPI) to deflate the remaining (service) sectors.

A.3 Additional Data Details: Investment

Our baseline daily series on sales and employment, along with discussions of the underlying data sources and methods, are publicly available from Spanish public institutions – e.g. the national Tax Authority or the Ministry for Social Security. Our daily aggregate consumption, is instead discussed in detail by Buda et al. (2022). Our daily aggregate investment series, instead, has not been previously released or analysed. Hereafter, we complement the discussion in the main text with additional details on the steps taken to construct this series, and compare it to other investment series available at lower frequency from the national accounts or tax declarations.

Recapping the discussion in the main text, we evaluate all transfers and reserve factoring[42] operations mediated by BBVA. We keep only transactions where both parties can be identified as firms, either because both ends of the transaction are linked to BBVA corporate accounts, or because the BBVA party is a firm and we can identify the counterpart as another firm through name-matching with the SABI dataset—which includes the universe of all Spanish firms. In either case, both firms are always associated with a CNAE sector code. We include only transactions related to a payment for goods or services, as can be typically be inferred from presence of the keyword 'invoice' or variations thereof in the concept of the payment. We eliminate all transactions where both parties belong to the same ownership group (as the transaction may reflect transfers of funds and not a purchase). Likewise, we eliminate all transactions where one of the parties belongs to the financial sector, the public sector, or a non-profit foundation.

For each of these firm-to-firm transactions in our dataset, we then assign the probability that the transaction refers to the purchase of an investment good. As explained in the main text, we do so by classifying each firm (either selling or purchasing) in a sector, reweighing our data and exploiting the share of investment spending in each sector derived from the 2019 Input-Output table for Spain, made available by INE.[43] Below we provide further discussion of how we transform our data to make sure that (a) they are representative of aggregate corporate sector in the Spanish economy and (b) they appropriately capture investment spending

Formally, let Y_i^j stand for the sales of intermediate goods from sector i to j, and I_i for the sales of investment goods by firms in sector i, both being the sum of the respective annual flows during 2019 as recorded in the I-O table. Moreover, let Z_i^j denote the sum of all sales from sector i to firms of sector j recorded in BBVA during 2019 that have passed the selection procedure described above. Finally, let Z_i denote the total corporate sales –

[42] In Spanish banking these are also called "Confirming" operations.

[43] Tabla Origen Destino 2019 from INE. https://www.ine.es/daco/daco42/cne15/cne_tod_19.xlsx

recorded by BBVA – by firms of sector i.

Concerning the first issue above in (a), the sectoral distribution of BBVA corporate clients maybe biased relative to the Spanish sectoral distribution of activity. This will be the case whenever the relative size of the sectors as mediated by BBVA $\left(\frac{Z_i}{\sum_{\forall k} Z_k}\right)$ is different from the IO table $\left(\frac{Y_i+I_i}{\sum_{\forall k}(Y_k+I_k}\right)$. We correct the potential bias by reweighing our data as follows:

$$\phi_i = \frac{Z}{Y+I} \times \frac{\frac{Z_i}{Z}}{\frac{Y_i+I_i}{Y+I}} \tag{3}$$

The first factor corrects the size of the recorded sales across firms in the BVVA account ($Z = \sum_{\forall k} Z_k$) and expands it to cover all sales in the Spanish economy ($Y + I = \sum_{\forall k}(Y_k + I_k)$). The second factor corrects the relative size of the sector in BBVA with the size of the sector in the Spanish economy. This correction implies that $\frac{Z_i^j}{\phi_i}$ is now an estimate of what we would expect to be the total sales from sector i to j in Spain, by projecting the amount observed in the BBVA data with appropriate weights.

Concerning the second issue in (b), we need to estimate the probability that BBVA transactions between two firms (in any two sectors) reflects the sale of an investment good. Recall that from official input-output data, we know the total sales of intermediate goods from i to j in a given year, Y_i^j. Thus, letting δ_i^j denote the percentage of sales on investment goods, we can write $(1-\delta_i^j)Z_i^j\frac{1}{\phi_i} = Y_i^j$ such that:[44]

$$\delta_i^j = 1 - \frac{Y_i^j}{Z_i^j}\phi_i. \tag{4}$$

The investment sales from sector i to j attributed by our algorithm is then simply:

$$\hat{I}_i^j(t) = \delta_i^j \times Z_i^j(t) \tag{5}$$

where $Z_i^j(t)$ is the sum of all sales during day t by firms of sector i to firms of sector j mediated by BBVA. Summing up, we obtain our proxy for aggregate investment on a given day t:

$$\hat{I}(t) = \sum_{\forall i,j} \hat{I}_i^j(t) \tag{6}$$

to which we apply standard filters for outliers.

[44]We further check that the implied δ_i^j estimate is a number between 0 and 1 and renormalize the data whenever this is not the case.

We build this high-frequency indicator of investment precisely because there are no high-frequency measures from administrative records that we can use for our purposes. We can however validate our series by aggregating our data to lower frequencies and comparing it to two investment series released by Spanish authorities: the quarterly series from the national accounts – released by the National Statistics Institute – and the monthly aggregate investment series compiled by the Spanish tax authority (AEAT), based on tax declarations of corporate sales (from VAT records) for large Spanish firms with sales of more than 6 million annual euros. As discussed in the main text, although the number of firms declaring daily sales for VAT purposes is only around 1% of firms, they cover over 60% of total sales. Note that, similarly to our own procedure above, the latter monthly proxy for aggregate investment also relies on the Spanish Input-Output tables in order to classify corporate sales as investment.

Overall, we find that our series tracks both series reasonably well. Figure A1 plots the YoY growth rate of the monthly aggregation of our series (in black) versus the monthly AEAT series (in red) for the time period when both are available. The correlation of the two series is 0.70. In Figure A2 we compare the time series of our investment proxt aggregated to a quarterly frequency (in black) relative to the official series for private investment in the Spanish national accounts (in red). This figures has three panels. In panel a), we compare the levels (the correlation is 0.95), in panel b) the QoQ growth rates (the correlation is 0.90), and in panel c) the YoY growth rates (the correlation is 0.95). Notice that QoQ growth rates of the raw series may be driven by common seasonal patterns, which could inflate the correlation between series. However this is argument is less applicable to the exercise in (c), as year-on-year growth rates naturally difference our quarter-specific effects.

We conclude that our high-frequency series appears to be a reliable proxy indicator of investment.

Figure A1: Monthly aggregation of BBVA Investment Indicator and AEAT measure

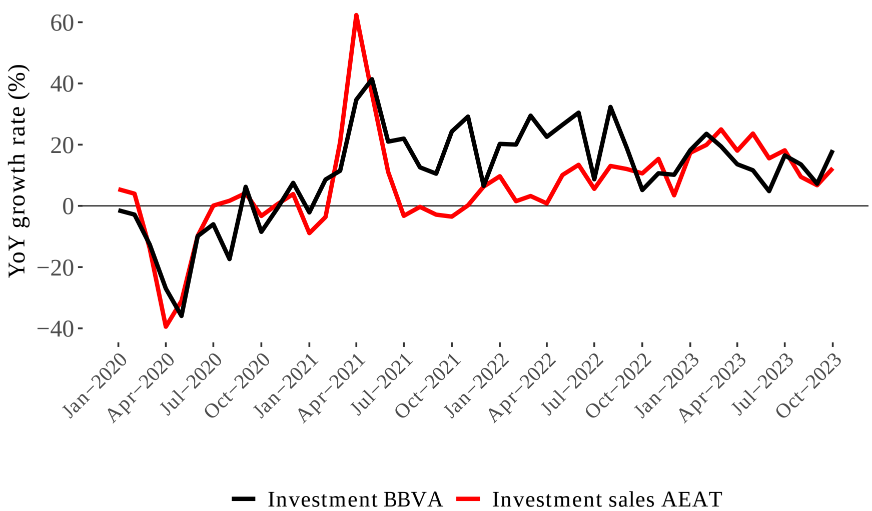
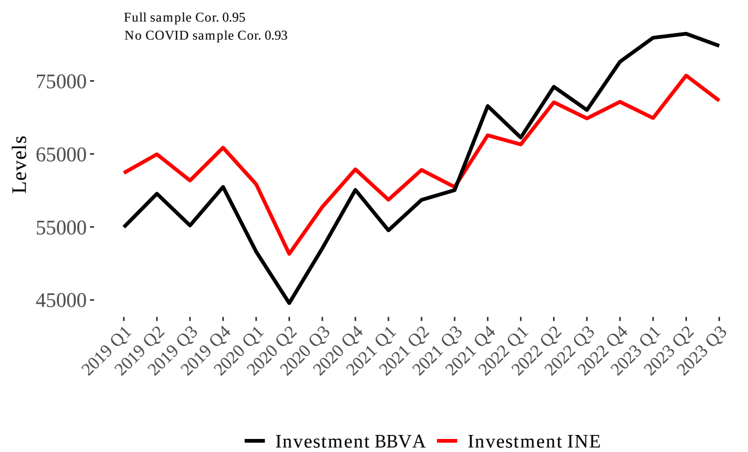
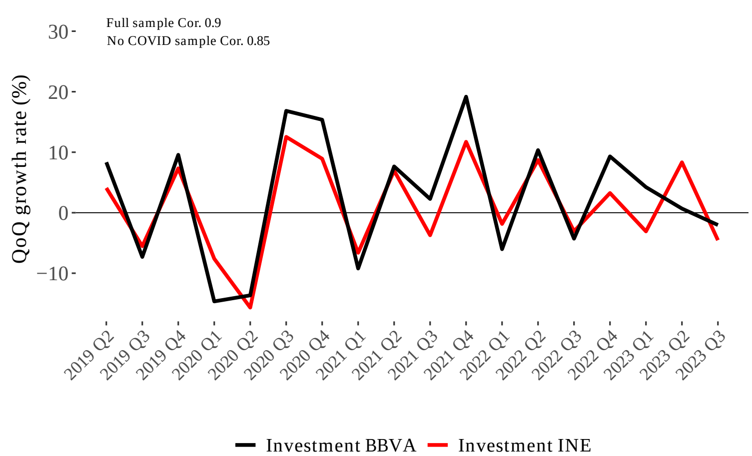


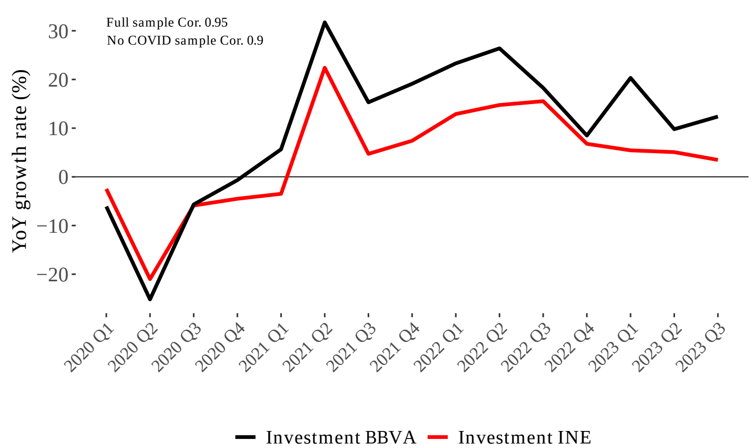
Figure A2: Quarterly series of BBVA Investment Indicator, and comparison with National Accounts



(a) Levels



(b) Quarter on Quarter Growth



(c) Year on Year Growth

B Appendix for Section 3

B.1 Monetary Policy Shocks: Descriptive Statistics

Table B1 presents descriptive statistics for the monetary policy shocks (MP) in two samples: the baseline sample and the full sample. The baseline sample starts on August 1, 2016 and ends in October 30, 2023, while the full sample starts in January 1, 2000, and ends in the same day as the baseline sample. The table compares the three different measures of MP shocks described in the text—JK (baseline), Target, and 1-month OIS—providing summary statistics for each.[45] The descriptive statistics indicate that the distribution of the JK MP shocks in the baseline sample is comparable to that of the full sample. In both samples, the JK MP shocks exhibit a distribution that is marginally skewed to the right. Turning to how the alternative monetary policy shocks compare to the baseline shocks, the Target descriptive statistics are quite similar, the 1-month OIS slightly different (a lower variance). One key reason for the discrepancy is the difference in the number of shocks in the series—24 against 55 in the baseline shock series. The difference follows from applying the poor man's approach, which removes shocks depending on the sign of the comovement between the change in the stock market price index and the change in interest rate around policy announcements.

Table B2 shows a significant correlation among different measures of European Central Bank (ECB) monetary policy surprises within the baseline sample. The 1-month OIS shows a robust positive correlation with both the JK measure (0.75) and the Target shock (0.51), indicating that directly observed market-based and constructed policy rate surprises are closely linked. Similarly, the correlation between the Target and the JK measure is 0.52. This correlation across the different measures of monetary policy surprises helps explain why our findings are consistent across them—see Figure 13.

Figure B1 plots the time series of monetary policy shocks from Jarociński and Karadi (2020), panel (a) for the baseline sample and panel (b) for the full sample. Fluctuations in monetary policy shocks are less pronounced in the first half of the sample compared to the second half. This is because in the the first half of the sample policy rates are at their effective (zero) lower bound— while the second half is characterized by the tightening cycle triggered by the sustained rise in inflation. One notable event is the significant contractionary surprise of approximately 19 basis points on March 12, 2020. These shock

[45] As explained in the section 7.3.3 in the main text, the JK (baseline) shock is from Jarociński and Karadi (2020), the other two monetary policy shocks (used in our robustness checks) are constructed based on the publicly accessible Euro Area Monetary Policy Event-Study Database (EA-MPD) created by Altavilla et al. (2019).

patterns observed in our baseline sample, coupled with the onset of the COVID crisis in March 2020, prompt us to perform sub-sampling robustness checks. These robustness checks are detailed in Section B.3, where we examine whether our main conclusions remain valid when restricting the sample to end in December 2019, before the start of COVID-19 and before the major contractionary shock.

Finally, note that, as demonstrated in panel (b) of Figure B1, the degree of variation and the occurrence of more extreme shocks are comparable in the baseline sample (2016-2023) and in the lnger sample.

Table B1: MP descriptive statistics

MP shock	N	Mean	Median	SD	Min	Max
Baseline sample						
JK (baseline)	55	0.90	0.31	3.74	-5.38	18.76
Target	55	0.55	-0.30	3.58	-4.96	21.13
1-month OIS	24	0.76	0.03	2.86	-4.45	10.57
Full sample						
JK (baseline)	224	0.51	0.20	3.29	-8.53	18.76

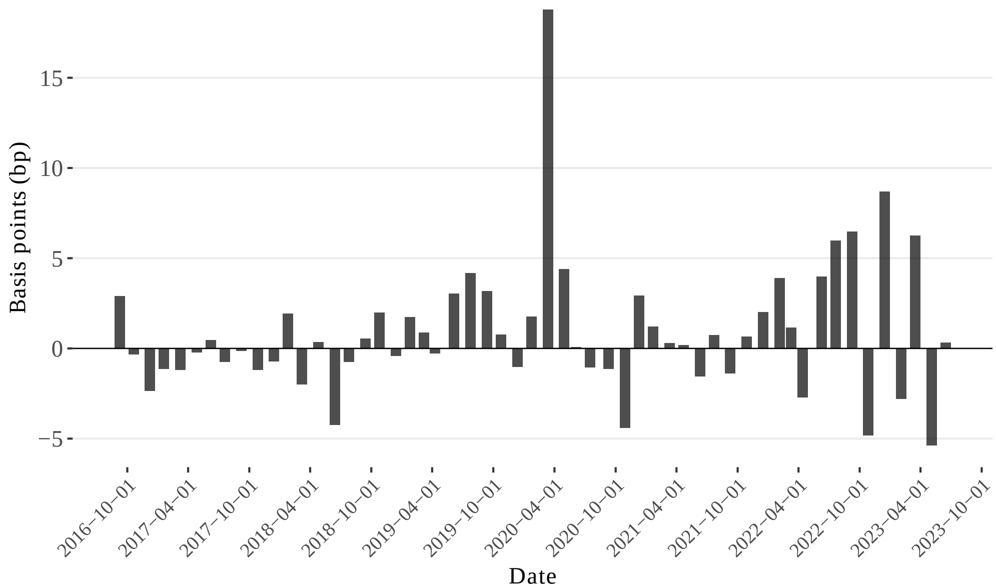
Notes: The baseline sample starts in August 1st, 2016 and ends in October 30th, 2023, while the full sample starts in January 1st, 2002 and ends in the same day as the baseline sample.

Table B2: MP cross-correlation in the Baseline sample

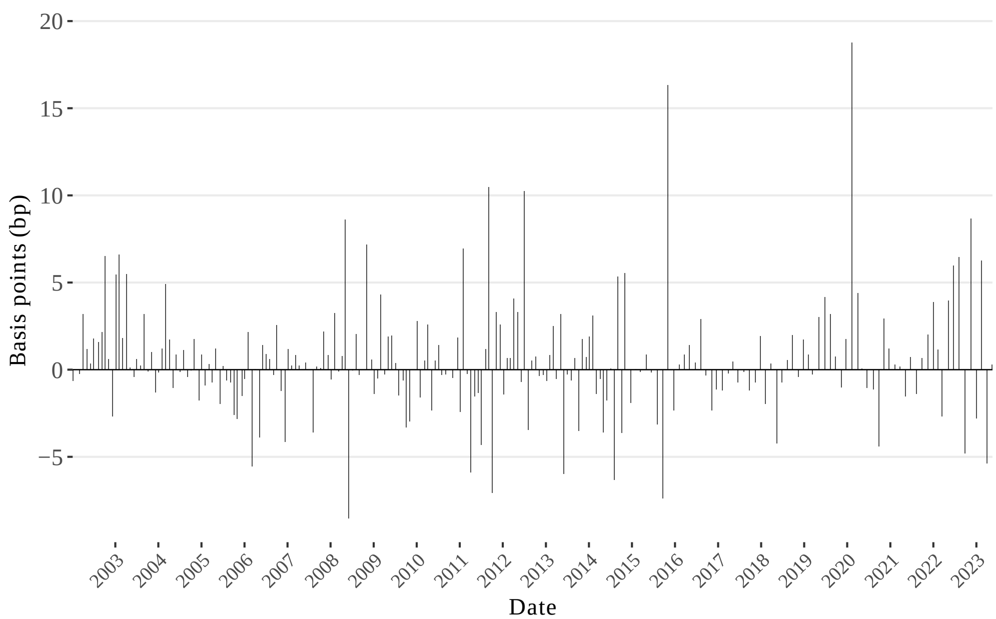
	1-month OIS	Target	JK (baseline)
1-month OIS	1.00	0.51	0.75
Target	0.51	1.00	0.52
JK (baseline)	0.75	0.52	1.00

Figure B1: Jarociński and Karadi (2020) monetary policy shocks

(a) Baseline sample



(b) Full sample



Notes: This figure shows the baseline monetary policy shocks (Jarociński and Karadi, 2020) time series we use in this paper. Panel (a) shows the time series of the baseline monetary policy shock in the baseline sample, while Panel (b) shows the time series of the baseline monetary policy shock in the full sample.

B.2 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:

$$y_t = \sum_{j=0}^{\infty} \phi_j \varepsilon_{t-j} + \eta_t, \tag{7}$$

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

$$IRF_h = \frac{\partial y_{t+h}}{\partial \varepsilon_t} = \phi_j. \tag{8}$$

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:

$$z_t = \sum_{j=0}^{\infty} b_j \varepsilon_{t-j} = \sum_{j=0}^{\infty} \phi_j \varepsilon_{t-j} - \sum_{j=0}^{\infty} \phi_j \varepsilon_{t-365-j}. \tag{9}$$

Hence, the impulse response of the YoY is given by

$$IRF_h^{YoY} = \frac{\partial z_{t+h}}{\partial \varepsilon_t} = b_j. \tag{10}$$

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

$$IRF_h^{YoY} = \frac{\partial z_{t+h}}{\partial \varepsilon_t} = b_j = \phi_j = \frac{\partial y_{t+h}}{\partial \varepsilon_t} = IRF_h. \tag{11}$$

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 $h \geq 365$,:

$$IRF_h^{YoY} = b_j = \phi_j - \phi_{j-365} \quad \text{for } j \geq 365. \tag{12}$$

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

$$IRF_h = IRF_h^{YoY} + IRF_{h-365}. \tag{13}$$

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

$$IRF_h = \begin{cases} IRF_h^{YoY} & 0 \leq h < 365 \\ IRF_h^{YoY} + IRF_{h-365} & h \geq 365 \end{cases} \tag{14}$$

C Appendix for Section 4

C.1 Empirical Evidence on High-Frequency Monetary Policy Shock Transmission to Financial Markets: a Comparative Review of the Literature

This subsection contrasts our results regarding monetary transmission to financial variables with established literature, focusing on response magnitudes and dynamics across various markets and time periods.

Focusing on the monetary policy event window and using our baseline sample, the F-statistics from our first-stage regression—focusing on the 6-month and 1-year German yields in response to baseline monetary policy shocks—are 28.42 and 16.33, respectively. These outcomes are consistent with the literature, for instance, see the findings by Altavilla et al. (2019). When examining the data on a monthly basis, our results align both qualitatively and quantitatively with those of Jarociński and Karadi (2020) concerning the one-year German bond yield and the Euro-Area stock index, as depicted in Figure 8B of their study.

In comparison to the euro area, there is a much larger number of studies focused on the United States. Starting with high-frequency evidence, Jarociński (2024) estimates daily IRFs of US financial variables in response to monetary policy shocks. According to the findings by this author, the response of the stock market and inflation expectations are similar in magnitude to ours: around -0.5% in the first 25 days for the S&P500, close to our estimate of around -0.4% for IBEX35, and between -0.01% and -0.02% on impact for the 5-year breakeven inflation (TIPS), which cumulates to between -0.04% and -0.05%, depending on the shock considered, within the first 25 days (this amounts to a cumulative response for expected inflation of around -0.01%, as in our estimates). For the 6-months US treasury yields, Swanson (2021) reports an IRF to an identified Federal funds rate shock that exhibits an increasing dynamic up to 120 days after the shock, similar to our estimated IRF for the euribor. Finally, Lewis (2023) finds that, empirically, the same day impact on financial variables may vary depending on the sample period and nature of the shock considered. For example, in the 1996-2019 sample, this author finds no significant impact of Fed funds rate shocks on the bond yields and the 10 years TIPS spreads. This is not the case (for this and other monetary policy shocks of different nature) in the sample 2009-2015.

Finally, at the monthly frequency, Miranda-Agrippino and Ricco (2021) report IRFs for the 1 year T-bond and the S&P500 similar in magnitude to our 12-months euribor and IBEX35 responses. After adjusting the magnitude of the shock for comparability, 6 months after the shock the response is about 0.015% for the 1 year T-bond and -0.18% for the S&P500. The first estimate is close to our finding for the 12-months euribor, the second is of the same order of magnitude as the response of IBEX35, around -0.4% for IBEX35.

D Appendix for Section 5

D.1 Upstream vs. Downstream Sectoral Classification

This section outlines how we bridge the sector classification used by the Spanish Tax Authority to compile their sales data with the 2015 Spanish INE Input-Output sector classification and how we calculate the upstreamness indicator and derive the sectoral upstream classification for the sales data.

Table D1 presents a mapping between the Spanish Tax Authority and the 2015 Spanish INE Input-Output sector classification. Out of the 64 sectors listed in the INE IO table, we can match 43 to the sales sector classification. Notably, health and education services remain unassigned, since there are no sales data recorded. Ultimately, we can link 20 sales sectors to the 43 sectors in the IO table, for which we can calculate an upstreamness indicator.

We adopt upstreamness metric proposed by Antràs et al. (2012), designed to accurately assess an industry position within the global production network. The upstreamness metric offers an insightful perspective on the relative distance of an industry from the final consumption phase, reflecting their involvement in early or intermediate stages in the supply chain. These authors show that this metric can be derived taking two distinct approaches, both anchored in input-output analysis.[46] Both methods produce an upstreamness measure that is always at least one for every industry. Higher values indicate a greater degree of upstreamness for that particular industry.

Table D3 reports the upstreamness indicator for both the bridged sales sectors and those IO sectors that could not be linked to the sales data. The second column, labeled "In Sales Data?", reports whether that industry is part of the sales data with a simple "Yes" or "No" response. The last column, Upstream, assigns a binary value (0 or 1) to indicate whether an industry is upstream (1)—upstreamness above the average of 2.20—or downstream (0)—below average. These findings align well with the results reported

[46]One method calculates upstreamness by iterating through the production process to capture the weighted average position of an industry output in the overall value chain. Using input-output tables, the approach tracks the flow of goods and services across different industries, taking into account both direct and indirect uses of an industry output. Upstreamness is calculated by considering the number of times an industry's output is employed as an intermediate input by other industries prior to its ultimate consumption. The second approach involves using a system of equations. This technique characterizes upstreamness by utilizing a group of linear equations to express the interconnections between various industries. It operates on the principle that industries which supply a considerable amount of their production to other upstream industries should also be considered more upstream. By solving these equations, the method assigns a measure of upstreamness to each industry, indicating its role and significance in the initial stages of production.

Table D1: Summary of the Spanish Tax Authority Sectors and their corresponding row/columns in the IO Table

Sectors Spanish Tax Authority	**Rows/Columns in IO Table**
C10 + C11 + C12. Food Industry, Beverage and Tobacco Manufacturing	5
C13 + C14 + C15. Textile Industry, Apparel, and Footwear Manufacturing	6
C17 + C18. Paper Industry; Graphic Arts	8, 9
C20. Chemical Industry	11
C21. Pharmaceutical Products Manufacturing	12
C22 + C23. Rubber and Plastic Products Manufacturing; Other Non-Metallic Mineral Products Manufacturing	13, 14
C24 + C25. Metallurgy; Manufacturing of Metal Products, except Machinery and Equipment	15, 16
C26 + C27. Manufacturing of IT, Electronic and Optical Products; Manufacturing of Electrical Equipment	17, 18
C29. Manufacturing of Motor Vehicles, Trailers, and Semi-Trailers	20
C16 + C31. Wood and Cork Industry; Furniture Manufacturing	7, 22
C28 + C30 + C33. Machinery and Equipment Manufacturing n.e.c.; Other Transport Equipment Manufacturing; Repair and Installation of Machinery and Equipment	19, 21, 23
C19 + C32. Coke Ovens and Oil Refining; Other Manufacturing Industries	10
D. Electricity, Gas, Steam, and Air Conditioning Supply	24
F. Construction	27
G. Wholesale Trade and Trade Intermediaries, Repair of Motor Vehicles and Motorcycles and Retail Trade except Motor Vehicles and Motorcycles	28, 29, 30
H. Transport and Storage	31, 32, 33, 34
I. Hospitality	36
J. Information and Communications	35, 37, 38, 39, 40
M+N. Professional and Administrative Activities	45, 46, 47, 48, 49, 50, 51, 52, 53

Notes: The first column of this table displays the sectoral classification used by the daily sales weekly report of the Spanish Tax Authority. The second column shows which row and column in the INE IO table corresponds to the sales sectors. Each row/column in the IO table is a sector of activity that can be mapped directly to the Statistical classification of economic activities in the European Community (NACE rev. 2). This mapping is presented in Table D2.

by Antràs et al. (2012) for Spain. The upstreamness values presented in the table range from 1.01 for residential care services to 3.87 for mining and quarrying, demonstrating the variation in industries positions within the supply chain.

Table D2: Correspondence between Spanish Input-Output Table Sectors and NACE Rev. 2 Industry Classification

Rows/Columns in IO table	Industries	NACE rev. 2
1	Crop and animal production, hunting and related service activities	01
2	Forestry and logging	02
3	Fishing and aquaculture	03
4	Mining and quarrying	05–09
5	Manufacture of food products, beverages and tobacco products	10–12
6	Manufacture of textiles, wearing apparel and leather products	13–15
7	Manufacture of wood and of products of wood and cork, except furniture; manufacture of articles of straw and plaiting materials	16
8	Manufacture of paper and paper products	17
9	Printing and reproduction of recorded media	18
10	Manufacture of coke and refined petroleum products	19
11	Manufacture of chemicals and chemical products	20
12	Manufacture of basic pharmaceutical products and pharmaceutical preparations	21
13	Manufacture of rubber and plastic products	22
14	Manufacture of other non-metallic mineral products	23
15	Manufacture of basic metals	24
16	Manufacture of fabricated metal products, except machinery and equipment	25
17	Manufacture of computer, electronic and optical products	26
18	Manufacture of electrical equipment	27
19	Manufacture of machinery and equipment n.e.c.	28
20	Manufacture of motor vehicles, trailers and semi-trailers	29
21	Manufacture of other transport equipment	30
22	Manufacture of furniture; other manufacturing	31–32
23	Repair and installation of machinery and equipment	33
24	Electricity, gas, steam and air conditioning supply	35
25	Water collection, treatment and supply	36
26	Sewerage; waste collection, treatment and disposal activities; materials recovery; remediation activities and other waste management services	37–39
27	Construction	41–43
28	Wholesale and retail trade and repair of motor vehicles and motorcycles	45
29	Wholesale trade, except of motor vehicles and motorcycles	46
30	Retail trade, except of motor vehicles and motorcycles	47
31	Land transport and transport via pipelines	49
32	Water transport	50
33	Air transport	51
34	Warehousing and support activities for transportation	52
35	Postal and courier activities	53
36	Accommodation, food and beverage service activities	55–56
37	Motion picture, video and television programme production, sound recording and music publishing activities; programming and broadcasting activities	59–60
38	Telecommunications	61
39	Computer programming, consultancy and related activities; information service activities	62–63
40	Financial service activities, except insurance and pension funding	64
41	Insurance, reinsurance and pension funding, except compulsory social security	65
42	Activities auxiliary to financial services and insurance activities	66
43	Real estate activities	68
44	Imputed rent of owner-occupied dwellings	68a
45	Legal and accounting activities; activities of head offices; management consultancy activities	69–70
46	Architectural and engineering activities; technical testing and analysis	71
47	Scientific research and development	72
48	Advertising and market research	73
49	Other professional, scientific and technical activities; veterinary activities	74–75
50	Rental and leasing activities	77
51	Employment activities	78
52	Travel agency, tour operator reservation service and related activities	79
53	Security and investigation activities; services to buildings and landscape activities; office administrative, office support and other business support activities	80–82
54	Public administration and defence; compulsory social security	84
55	Education	85
56	Human health activities	86
57	Social work activities	87–88
58	Creative, arts and entertainment activities; libraries, archives, museums and other cultural activities; gambling and betting activities	90–92
59	Sports activities and amusement and recreation activities	93
60	Activities of membership organisations	94
61	Repair of computers and personal and household goods	95
62	Other personal service activities	96
63	Activities of households as employers; undifferentiated goods- and services-producing activities of households for own use	97–98
64	Activities of extraterritorial organisations and bodies	99

Table D3: Upstreamness Indicator and Upstream vs. Downstream Classification of Bridged IO Sectors

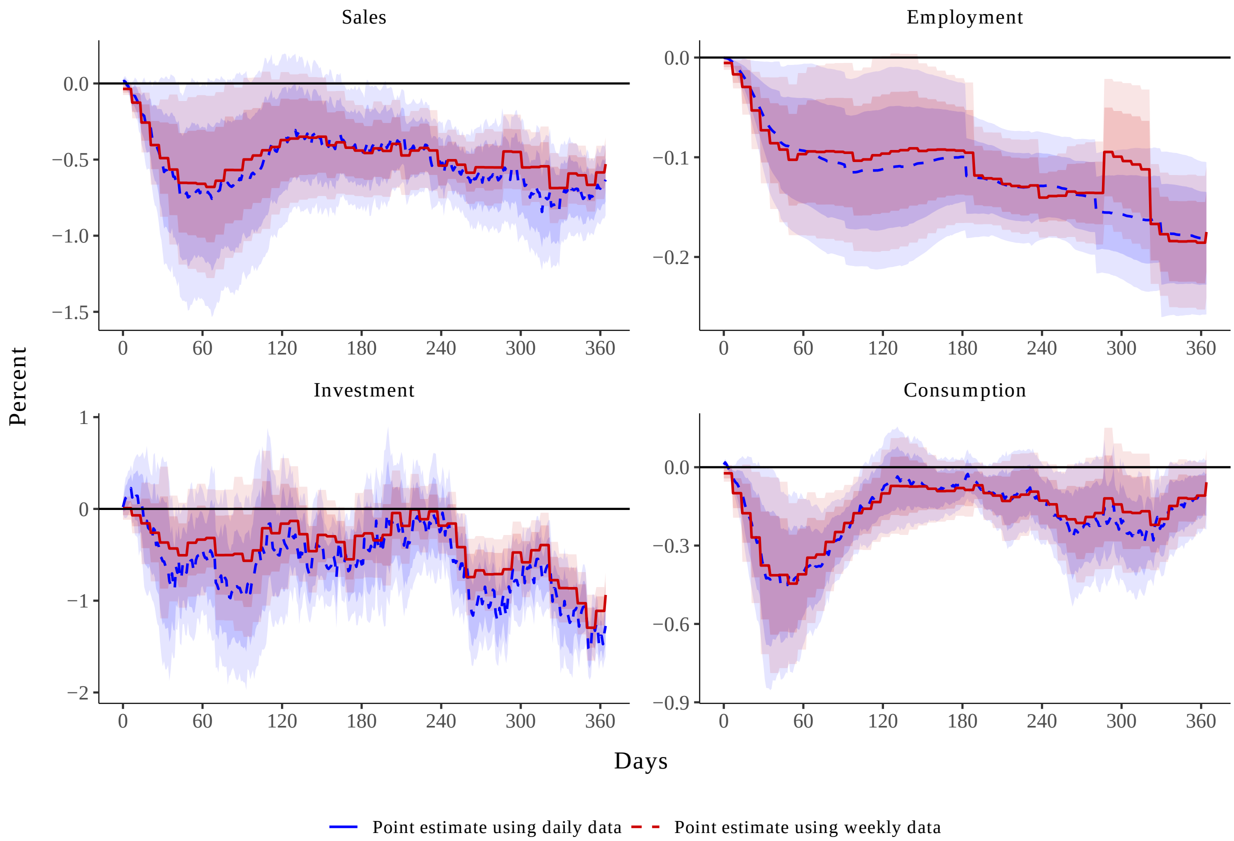
Industry	In Sales Data?	Upstreamness	Upstream
Residential care services; social work services without accommodation	No	1.01	0
Public administration and defense services; compulsory social security	No	1.06	0
Human health services	No	1.20	0
Education services	No	1.20	0
Other personal services	No	1.24	0
I. Hospitality	Yes	1.28	0
Creative, arts and entertainment services; libraries, archives, museums	No	1.42	0
Fish and other fishing products; aquaculture products	No	1.45	0
C21. Pharmaceutical Products Manufacturing	Yes	1.55	0
F. Construction	Yes	1.56	0
Sporting services and amusement and recreation activities	No	1.57	0
Real estate services	No	1.58	0
Services furnished by membership organizations	No	1.76	0
G. Wholesale Trade and Trade Intermediaries, Sale and Repair of Motor Vehicles and Motorcycles and Retail Trade, except Motor Vehicles and Motorcycles	Yes	1.86	0
Insurance, reinsurance, and pension funding services, except compulsory social security	No	1.89	0
Repair services of computers and personal and household goods	No	1.89	0
C13 + C14 + C15. Textile Industry, Apparel, and Footwear Manufacturing	Yes	2.02	0
C26 + C27. Manufacturing of IT, Electronic and Optical Products; Manufacturing of Electrical Equipment	Yes	2.09	0
C29. Manufacturing of Motor Vehicles, Trailers, and Semi-Trailers	Yes	2.16	0
Manufacturing of machinery and equipment n.e.c.; other transport equipment manufacturing; repair and installation of machinery and equipment	No	2.17	0
Natural water; water treatment and supply services	No	2.18	0
C16 + C31. Wood and Cork Industry; Furniture Manufacturing	Yes	2.19	0
J. Information and Communications	Yes	2.20	0
C10 + C11 + C12. Food Industry, Beverage and Tobacco Manufacturing	Yes	2.27	1
Services auxiliary to financial services and insurance	No	2.39	1
Financial services, except insurance and pension funding	No	2.58	1
C19 + C32. Coke Ovens and Oil Refining; Other Manufacturing Industries	Yes	2.65	1
Products of agriculture, hunting, and related services	No	2.76	1
M+N. Professional and Administrative Activities	Yes	2.78	1
H. Transport and Storage	Yes	3.09	1
Sewerage services; sewage sludge; waste collection and disposal services	No	3.11	1
D. Electricity, Gas, Steam, and Air Conditioning Supply	Yes	3.28	1
C22 + C23. Rubber and Plastic Products Manufacturing; Other Non-Metallic Mineral Products Manufacturing	Yes	3.31	1
Products of forestry, logging, and related services	No	3.33	1
C24 + C25. Metallurgy; Manufacturing of Metal Products, except Machinery and Equipment	Yes	3.53	1
C17 + C18. Paper Industry; Graphic Arts	Yes	3.55	1
Chemical industry	No	3.60	1
Mining and quarrying	No	3.87	1

E Appendix for Section 6

E.1 Comparison of Lower-Frequency LP IRFs with Baseline Daily IRFs at Daily Frequency

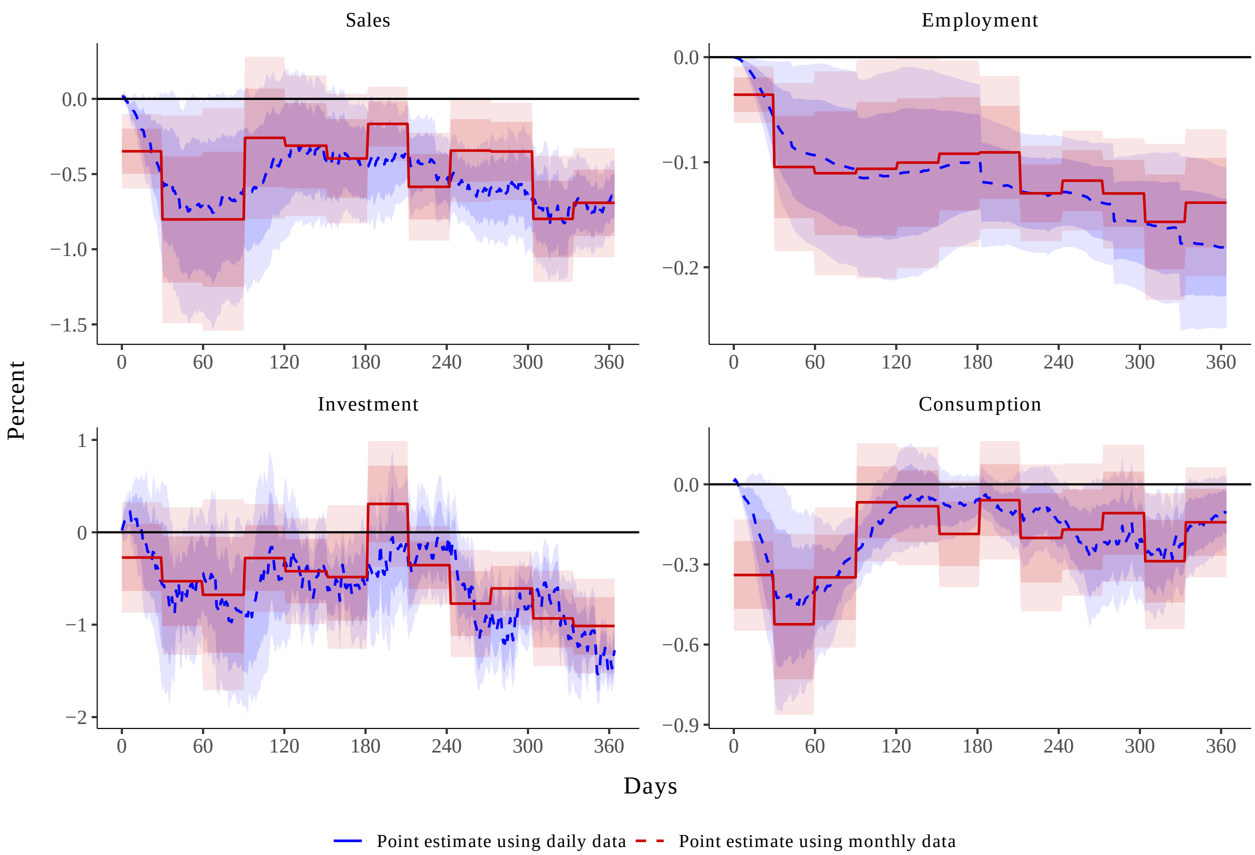
In the main text, we contrasted IRFs derived from low-frequency data with those from time-aggregated daily data LP IRFs, effectively situating all IRFs within a low-frequency context. Alternatively, IRFs estimated from LPs using low-frequency data can be combined into a mixed-frequency dataset, presenting all estimated IRFs at the daily frequency. Although comparing point estimates over the same period from different frequencies becomes visually challenging, it enhances the ability to appreciate the superior resolution offered by high-frequency estimates. In this section, we provide this alternative comparison for weekly, monthly, and quarterly frequencies in Figures E1, E2 and E3, respectively. Examining these figures, we derive conclusions similar to those in the main text. The IRFs of daily data aggregated into weekly and monthly frequencies closely follow the daily IRFs, whereas the quarterly data differ. In the first quarter, responses of sales, employment and consumption become insignificant while the were detectable at the weekly and monthly frequencies. Moreover, it underscores the importance of preserving the high resolution of the daily data. For investment, lower frequencies produce insignificant short lags in all low-frequency estimates, whereas our high-resolution daily IRFs detect significant responses at a confidence level 90% around 45 days after monetary policy shock. Furthermore, high resolution reveals that significant responses in sales, consumption, and employment typically materialize at the month's end, implying that the anticipated contemporaneous impact in monthly studies depends on whether the shock occurs at the start or end of the month.

Figure E1: Time Aggregation: Weekly responses



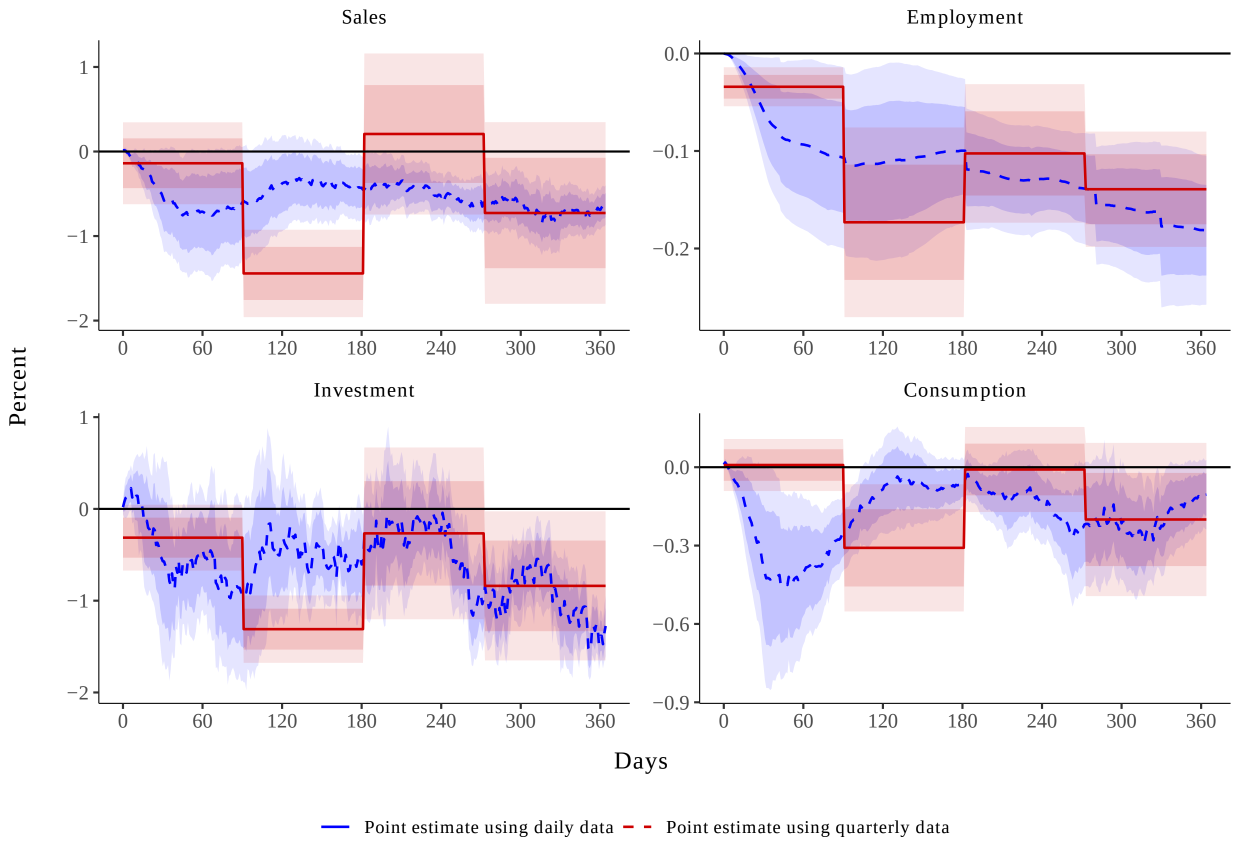
Notes: LP impulse response functions to a one standard deviation monetary policy shock. The responses are reported in levels. The confidence intervals are computed from heteroskedasticity-robust standard errors. Darker-shaded areas are the 68% confidence intervals, and lighter-shaded areas are the 90% confidence intervals. The sample ends in October 30th, 2023. The sample starts one year after the sample start of each series reported in Table A1. For sales and investment—the shorter series—the monetary policy shock standard deviation is 4.1bp, while for consumption and employment it is 3.7bp. Clockwise, we display LP for total sales, employment, consumption and investment. Dashed lines are the implied low-frequency aggregated data (weekly) LP point estimates, while solid lines are the baseline daily LP point estimates.

Figure E2: Time Aggregation: Monthly responses



Notes: LP impulse response functions to a one standard deviation monetary policy shock. The responses are reported in levels. The confidence intervals are computed from heteroskedasticity-robust standard errors. Darker-shaded areas are the 68% confidence intervals, and lighter-shaded areas are the 90% confidence intervals. The sample ends in October 30th, 2023. The sample starts one year after the sample start of each series reported in Table A1. For sales and investment—the shorter series—the monetary policy shock standard deviation is 4.1bp, while for consumption and employment it is 3.7bp. Clockwise, we display LP for total sales, employment, consumption and investment. Dashed lines are the implied low-frequency aggregated data (monthly) LP point estimates, while solid lines are the baseline daily LP point estimates.

Figure E3: Time Aggregation: Quarterly responses



Notes: LP impulse response functions to a one standard deviation monetary policy shock. The responses are reported in levels. The confidence intervals are computed from heteroskedasticity-robust standard errors. Darker-shaded areas are the 68% confidence intervals, and lighter-shaded areas are the 90% confidence intervals. The sample ends in October 30th, 2023. The sample starts one year after the sample start of each series reported in Table A1. For sales and investment—the shorter series—the monetary policy shock standard deviation is 4.1bp, while for consumption and employment it is 3.7bp. Clockwise, we display LP for total sales, employment, consumption and investment. Dashed lines are the implied low-frequency aggregated data (monthly) LP point estimates, while solid lines are the baseline daily LP point estimates.

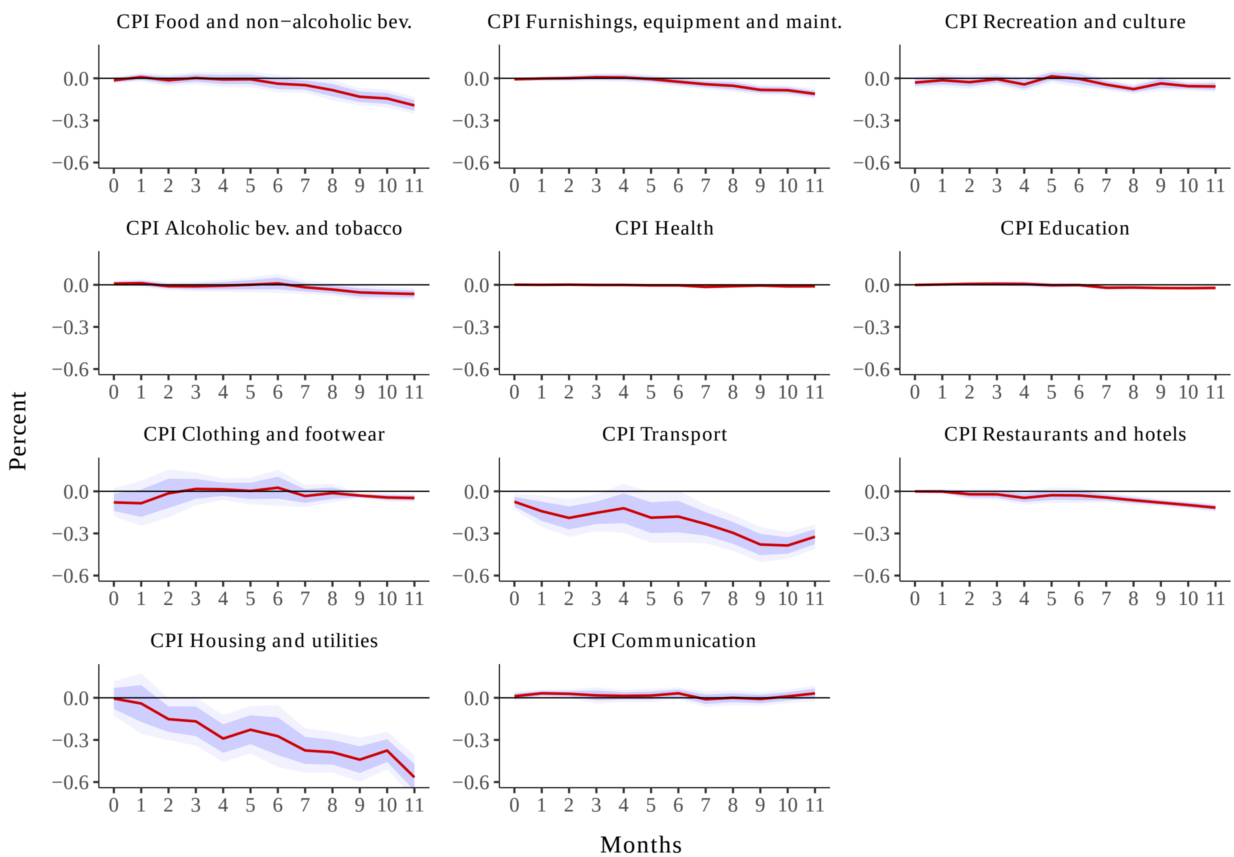
F Appendix for Section 7.2

F.1 The Transmission to CPI Disaggregated by COICOP Category

This appendix presents how consumer prices, categorized by COICOP, react to monetary policy changes on a monthly basis. In Figure F1, the impulse responses correspond to these CPI categories: food and non-alcoholic beverages, furnishings, equipment and maintenance, recreation and culture, alcoholic beverages and tobacco, health, education, clothing and footwear, transport, restaurants and hotels, housing and utilities, and communication.

Impulse responses are heterogeneous in economically sensible ways, potentially reflecting differences in the transmission mechanisms and/or the degree of price rigidity across categories. In response to a contractionary monetary policy shock, some categories of price fall markedly. For instance, the CPI for Housing and Utilities and the CPI for Transport experience a pronounced and persistent decline, reaching approximately -0.6% and -0.3%, respectively, by the 11th month. The CPIs for Food and non-alcoholic beverages, Alcoholic beverages and tobacco, Furnishings, equipment and maint., and Restaurants and hotels also decline significantly, albeit their decline is less steep and slightly lagged. In contrast, the CPI for Health, Education and Communication show minimal or no response to the monetary shock.

Figure F1: Monthly response of prices by COICOP category to a monetary policy shock



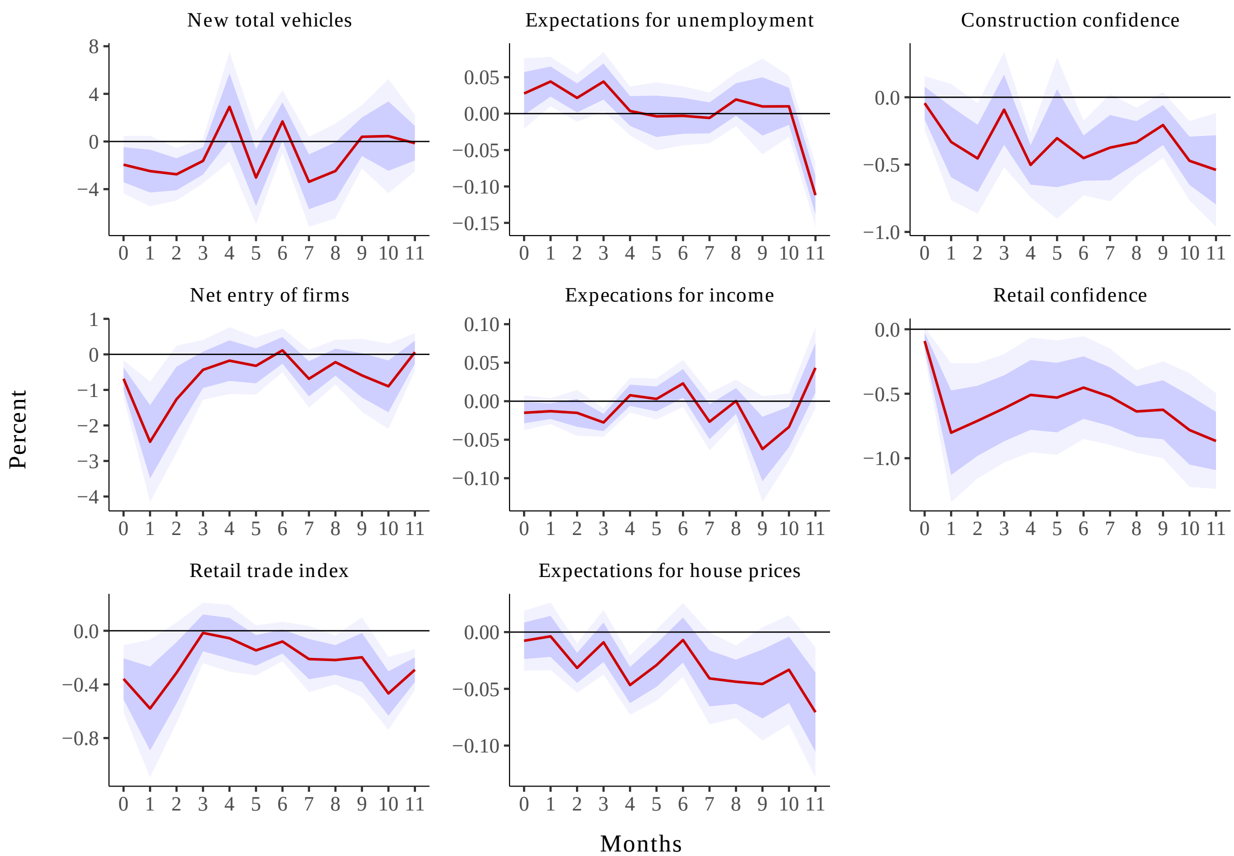
Notes: LP impulse response functions to a one standard deviation monetary policy shock. The responses are reported in levels. The confidence intervals are computed from heteroskedasticity-robust standard errors. Darker-shaded areas are the 68% confidence intervals, and lighter-shaded areas are the 90% confidence intervals. The sample starts in August 2015 and ends in October 2023. The monetary policy shock standard deviation is 3.7bp.

F.2 Further Evidence on Monetary Transmission Using Monthly Series

This section reports results on the monetary transmission to additional variables relative to the one discussed in the main text. We rely on monthly series focusing on the baseline sample period. Results are shown by Figure F2. The new series include three additional real activity indicators: total new vehicles, net entry of firms, and a retail trade index; three additional expectation variables, concerning unemployment, income, and house prices; and two additional confidence variables, related to the construction and retail sectors. Data on total new vehicles is obtained from EUROSTAT's registrations of new motor vehicles in Spain; both the net entry of firms and the retail trade index are reported by the INE. Expectations regarding unemployment, income, and housing prices are drawn from the ECB's consumer expectations survey, conducted solely at the EA level since April 2020. Lastly, the EU Commission provides the confidence indicators for construction and retail for Spain.

Examining the real activity indicators, we note that each of them sharply decreases in the two months following a monetary policy tightening. New total vehicles and net entry of firms stabilize after this initial decline, whereas the retail trade index resumes its decline after the seventh month, which is consistent with our main findings for aggregate daily consumption. Expectations variables as group behave as expected, with households anticipating an increase in unemployment, a decrease in income, and a decline in housing prices. Finally, confidence falls persistently in both the construction and the retail sector.

Figure F2: Monthly response of other selected variables to a monetary policy shock



Notes: LP impulse response functions to a one standard deviation monetary policy shock. The responses are reported in levels. The confidence intervals are computed from heteroskedasticity-robust standard errors. Darker-shaded areas are the 68% confidence intervals, and lighter-shaded areas are the 90% confidence intervals. For all series—except for expectations for unemployment and for income which start in April 2020—the sample starts in August 2016 and ends in October 2023. The monetary policy shock standard deviation is 4.2bp for the full sample series and 3.6bp for the two series that start in April 2020.

G Appendix for Section 7.3

G.1 Further Results on Seasonality Adjustment and Smoothing of Daily Series

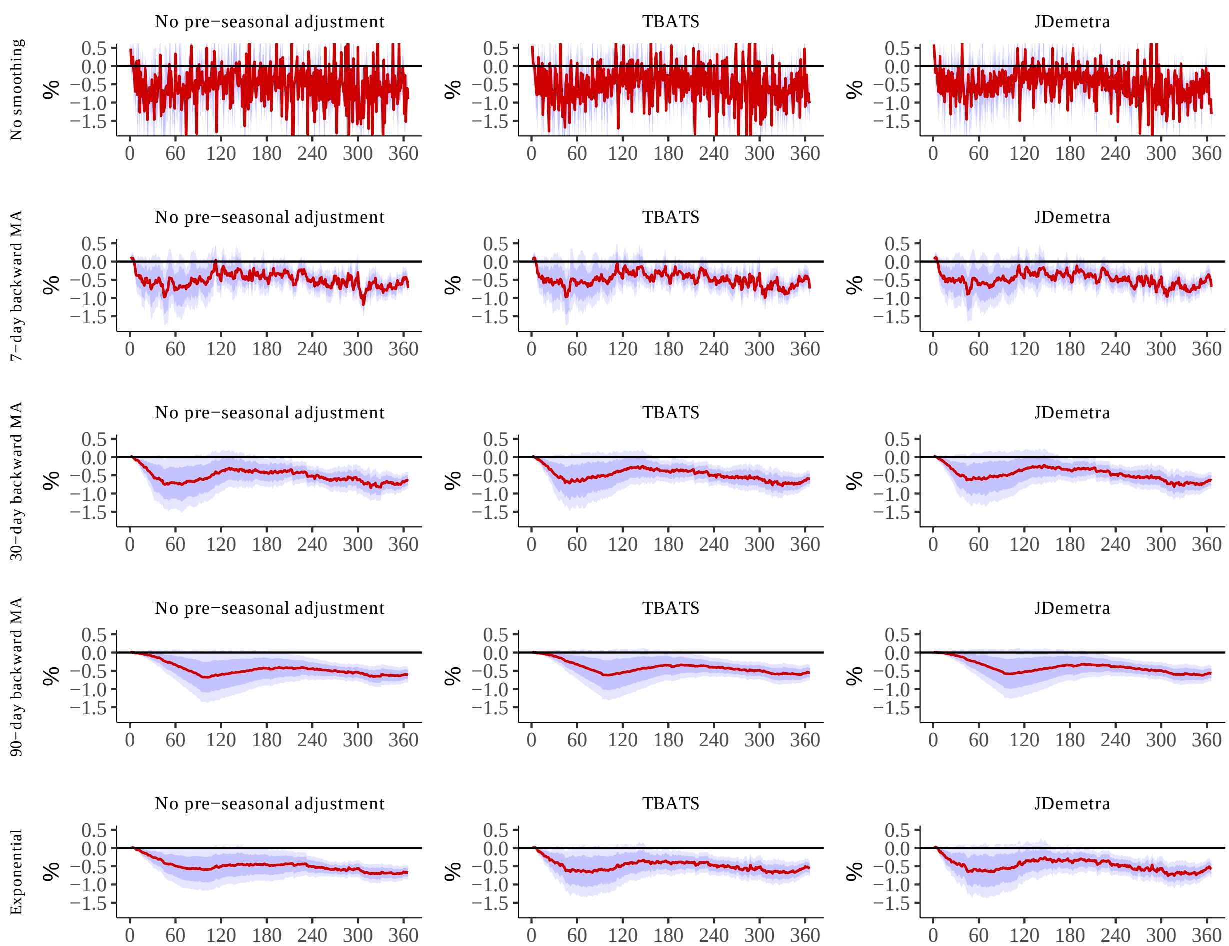
This appendix complements the analysis of seasonality and smoothing in Section 7.3.2. To start with, we explore the sensitivity of our results to specific changes in the smoothing method. Namely, we show results conditional on (i) not smoothing the data; (ii) setting the number of days of the backward MA in our poor man's approach to 7, 30 (baseline) and 90, respectively; and (iii) applying exponential smoothing. Next, for each smoothing procedure we show results conditional on (i) not seasonally adjustmenting the data; (ii) applying the TBATS procedure—shown in Section 7.3.2; and (iii) estimating a Fractional Airline Model (FAM)—a model-based seasonal adjustment method developed by the Bank of Belgium, as part of an R package with access to a Java library under development for the JDemetra+ program.[47] We will compare IRFs for all possible combinations of smoothing and seasonal adjustment techniques.

Figures F1, F2, F3, and F4 present our findings under various combinations of seasonal adjustment methods (three, one per column) and smoothing procedures (five, one per row), for sales, consumption, investment, and employment, respectively. To facilitate comparisons, in each figure, the graph on the third row of the first column reproduces our baseline IRFs, obtained relying on a 30-day backward-looking MA smoothing for all series without applying any direct seasonality adjustment prior to smoothing. A first takeaway from this comparative analysis is that our baseline results look remarkably similar to those obtained by applying a 30-day MA smoothing *after* seasonally adjusting the series (compare the graphs on third row of each figure). This suggests that our baseline approach appears to take care of the most significant seasonal patterns. A second point concerns the role of data smoothing. The first row of our figures shows that, with the exception of employment, seasonal adjustment on its own tends to produce noisy IRFs. Noise in turn tends to reduce the information content of our IRFs—in other words, noise can outweigh seasonality concerns when dealing with high-frequency series of sales, consumption, and investment, motivating smoothing. And yet, one needs to exercise caution in applying smoothing methods to the data. On one hand, smoothing reduces noise; on the other hand, it filters out short-run dynamics. Our graphs on the third row

[47] JDemetra+ is a tool for seasonal adjustment developed by the National Bank of Belgium in cooperation with the Deutsche Bundesbank and Eurostat in accordance with the Guidelines of the European Statistical System (ESS).

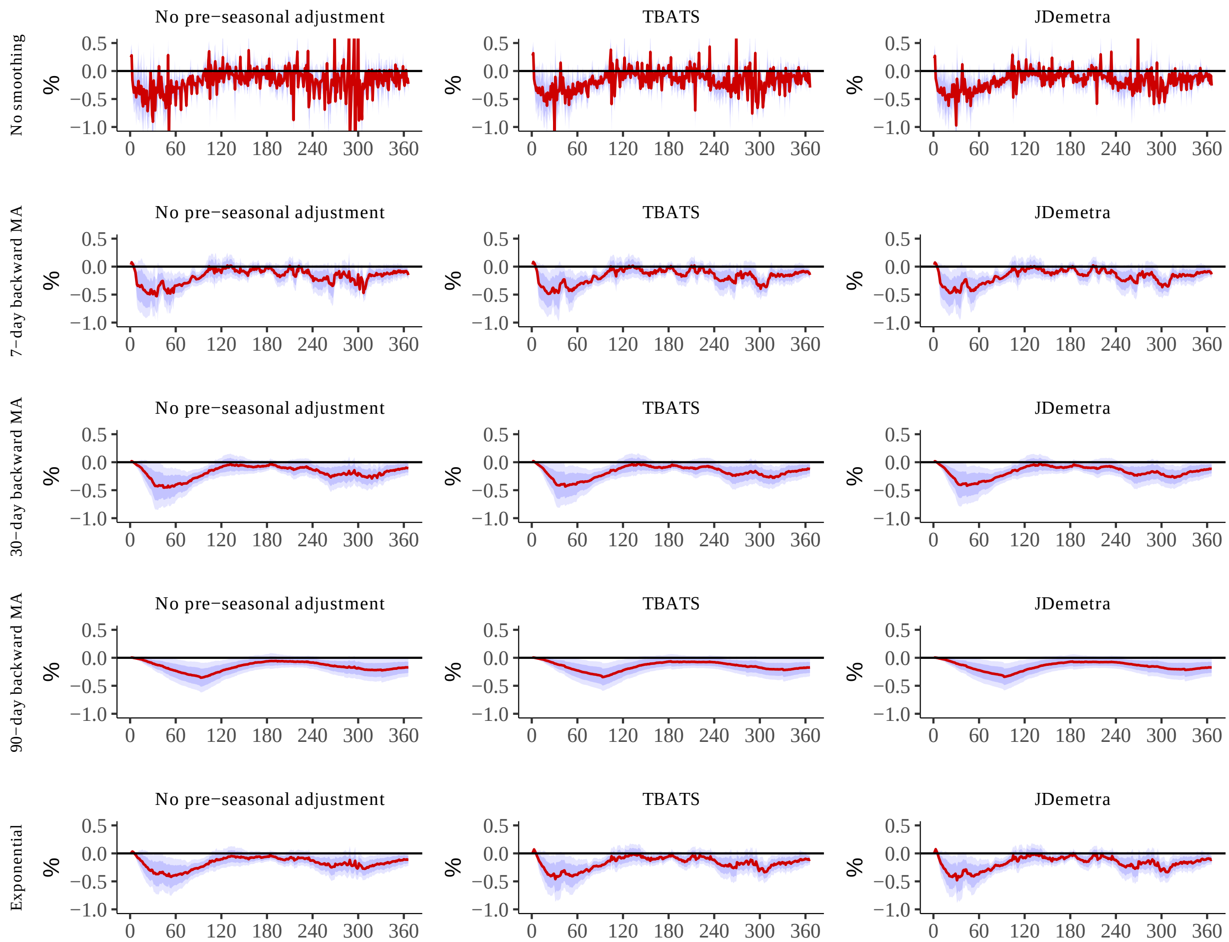
of the figures suggest that the 30-day MA exhibits smooth IRFs while swiftly adapting to changing conditions—both desirable outcomes. In contrast, a 7-day MA (second row of the figures) results in significantly volatile IRFs, while a 90-day MA (fourth row) tends to blur very short-lag responses in all variables—the analysis misses some of the fast responses. Both the SLP results and the results applying model-based smoothing with TBATS given in Section 7.3.2 in the main text, motivate the 30-day MA we adopt in our baseline.

Figure F1: Daily response to monetary policy shock under alternative seasonal and smoothing procedures: sales



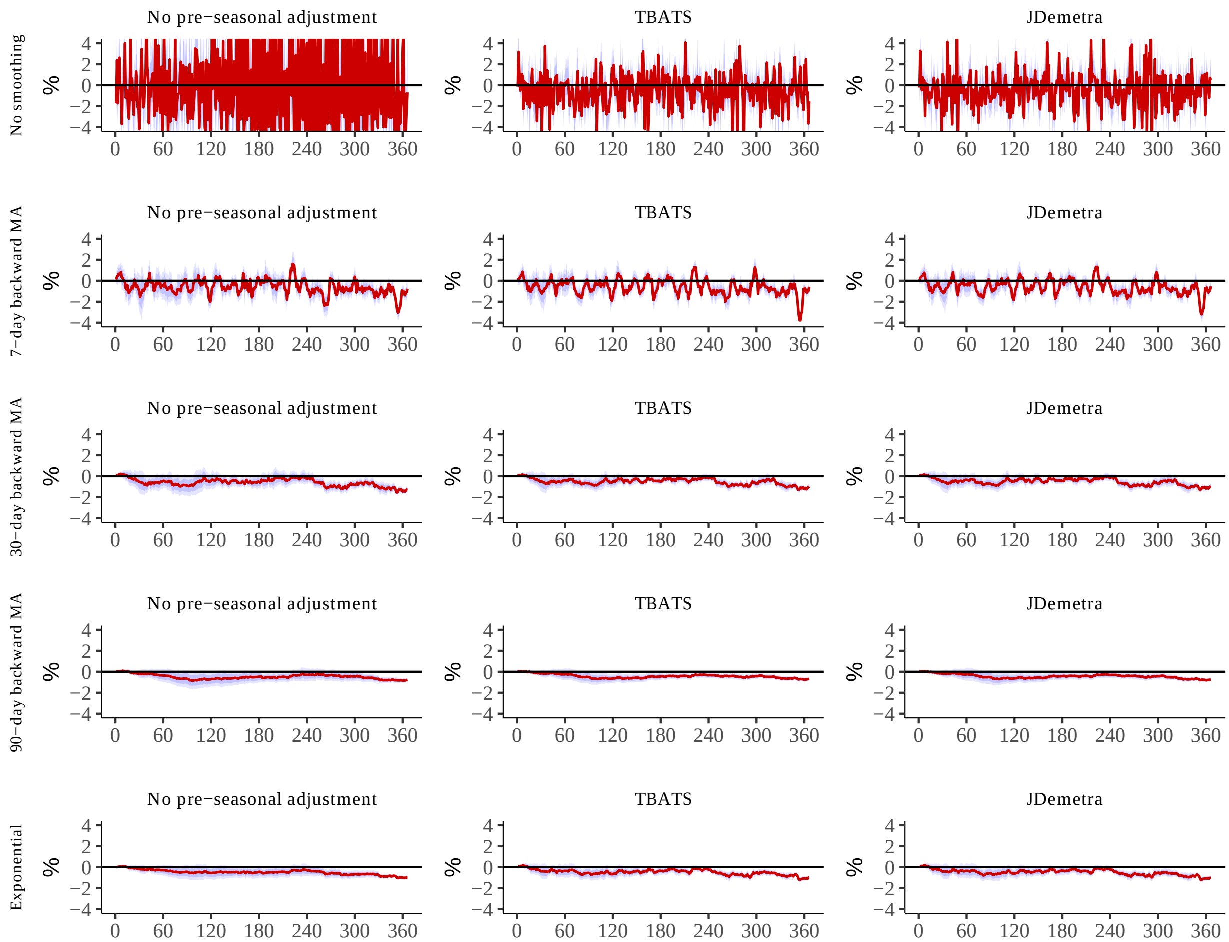
Notes: Responses to a one standard deviation monetary policy shock. The responses are reported in levels. The confidence intervals are computed from heteroskedasticity-robust standard errors. Darker-shaded areas are the 68% confidence intervals, and lighter-shaded areas are the 90% confidence intervals. The sample ends in October 30th, 2023. The sample begins one year after the start of each series reported in Table A1. The monetary policy shock standard deviation is 4.1bp. Across columns, the figure displays the responses under different seasonal adjustment procedures, and across rows, it presents responses under different smoothing methods.

Figure F2: Daily response to monetary policy shock under alternative seasonal and smoothing procedures: consumption



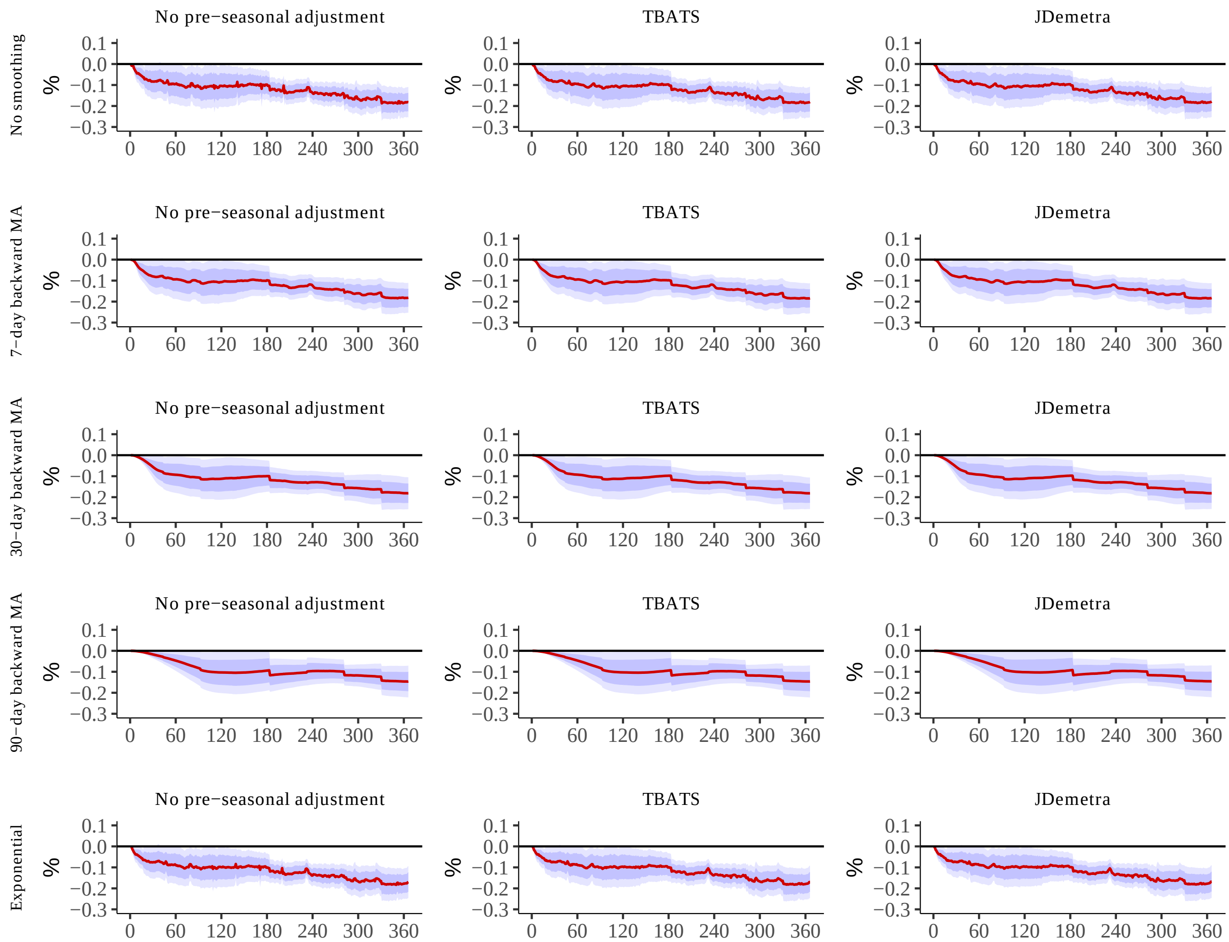
Notes: Responses to a one standard deviation monetary policy shock. The responses are reported in levels. The confidence intervals are computed from heteroskedasticity-robust standard errors. Darker-shaded areas are the 68% confidence intervals, and lighter-shaded areas are the 90% confidence intervals. The sample ends in October 30th, 2023. The sample begins one year after the start of each series reported in Table A1. The monetary policy shock standard is 3.7bp. Across columns, the figure displays the responses under different seasonal adjustment procedures, and across rows, it presents responses under different smoothing methods.

Figure F3: Daily response to monetary policy shock under alternative seasonal and smoothing procedures: investment



Notes: Responses to a one standard deviation monetary policy shock. The responses are reported in levels. The confidence intervals are computed from heteroskedasticity-robust standard errors. Darker-shaded areas are the 68% confidence intervals, and lighter-shaded areas are the 90% confidence intervals. The sample ends in October 30th, 2023. The sample begins one year after the start of each series reported in Table A1. The monetary policy shock standard deviation is 4.1bp. Across columns, the figure displays the responses under different seasonal adjustment procedures, and across rows, it presents responses under different smoothing methods.

Figure F4: Daily response to monetary policy shock under alternative seasonal and smoothing procedures: employment

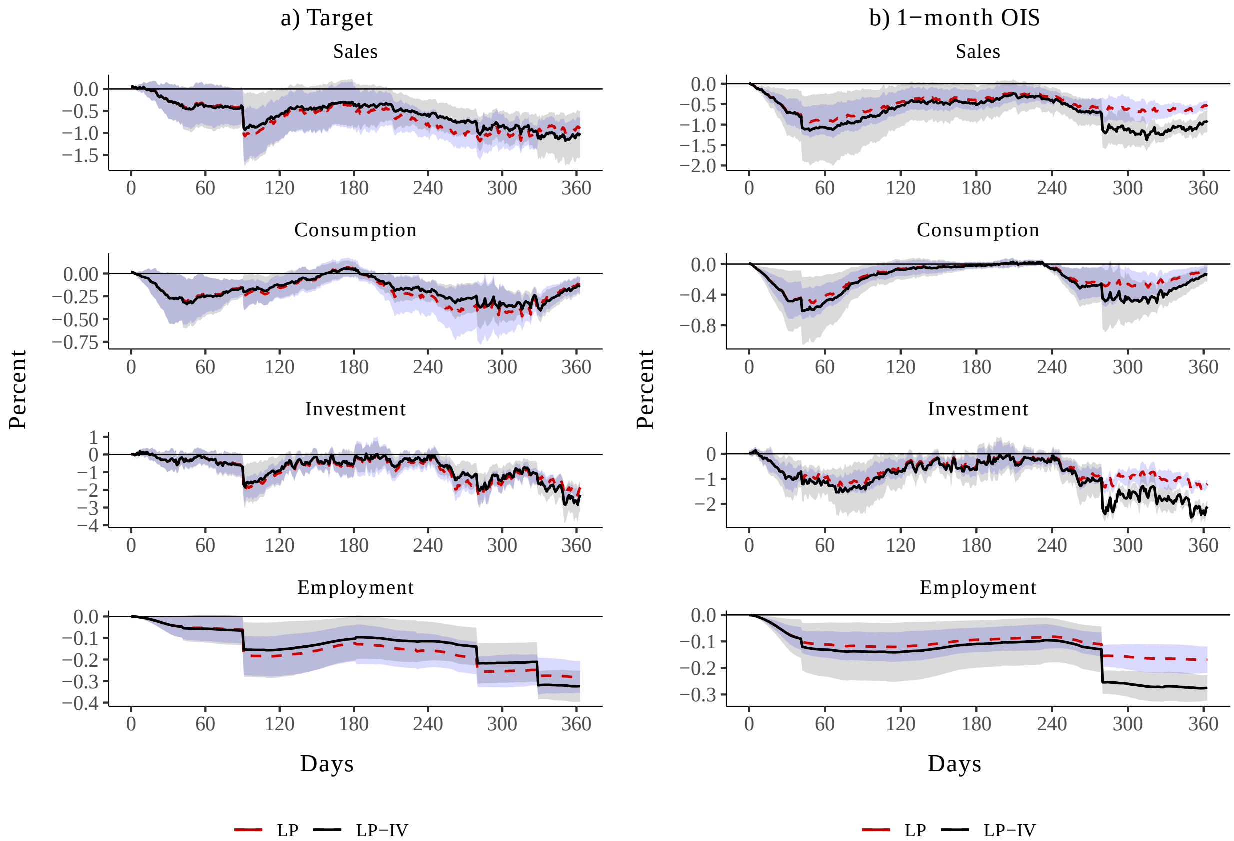


Notes: Responses to a one standard deviation monetary policy shock. The responses are reported in levels. The confidence intervals are computed from heteroskedasticity-robust standard errors. Darker-shaded areas are the 68% confidence intervals, and lighter-shaded areas are the 90% confidence intervals. The sample ends in October 30th, 2023. The sample begins one year after the start of each series reported in Table A1. The monetary policy shock standard is 3.7bp. Across columns, the figure displays the responses under different seasonal adjustment procedures, and across rows, it presents responses under different smoothing methods.

G.2 LP vs. LP-IV for Instruments for Monetary Policy Shocks

This appendix compares the Impulse Response Functions (IRFs) derived from Local Projections (LP) and LP with Instrumental Variables (LP-IV) for each of the two alternative monetary policy shock instruments found in Figure 13 of the main document. The instruments in question are the target and the 1-month Overnight Index Swap (OIS), adjusted for the information channel via the approach outlined by Jarociński and Karadi (2020). Unlike our baseline monetary policy shocks, these alternative series serve as instruments for monetary surprises rather than direct measures of monetary policy shocks. According to Stock and Watson (2018), in such situations, simple LP should be replaced by LP-IV methods. In Figure F5, the baseline sample analysis shows no significant difference between LP and LP-IV results over the horizons of up to 250 days after a contractionary monetary policy surprise.

Figure F5: IRFs derived from Local Projections (LP) and LP with Instrumental Variables (LP-IV) for two alternative monetary policy shock instruments



Notes: LP (red) and LP-IV (black) impulse response functions to a one standard deviation monetary policy shock. The responses are reported in levels. The confidence intervals are computed from heteroskedasticity-robust standard errors. Shaded areas are the 68% confidence intervals. The sample starts in August 2015 and ends in October 2023. For sales and investment—the shorter series—the target and poor man's 1-month OIS monetary policy shock standard deviation is 3.8bp and 3bp, respectively; while for consumption and employment it is 3.6bp and 2.9bp, respectively.

G.3 Further Results on Robustness Checks to COVID-19 using Monthly Data

This subsection uses monthly data to provide further robustness checks with respect to COVID-19. We analyze the transmission of monetary policy to total sales, consumption, investment, and employment (our baseline variables) and ten other real economic indicators, over the longest available Pre-COVID sample, running from January 2000 to December 2019. The ten real variables include final consumption sales (disaggregated by goods and services), investment sales (equipment and software, construction), intermediate goods sales, exports, imports, and both permanent and temporary employment, as well as industrial production.

The results for the baseline variables are shown in Figure F6, for the other variables in Figure F7. Overall, we find that the responses of the monthly economic indicators are consistently significant at short lags, corroborating the robustness of our findings in our baseline sample with daily data. In Figure F6, consumption sales respond on impact, then stabilize and fall again significantly in month 3 after the shock; total sales decline in month 1, investment in month 2. While total employment remains flat, the response of permanent employment (shown below) is smooth, similar to the baseline.

Coming to the disaggregated categories in Figure F7, we highlight the following.

- Final Consumption Sales.

  The response pattern of final consumption sales in goods and services mirrors that of total consumption sales (reproduced in Figure for comparison). Both are similar to our Baseline: a significant drop on impact in response to a monetary policy shock is followed by temporary stabilization before again a significant decline from month 3. An important difference between the response of sales of goods and the sales of services, however, is that the contraction in the latter is significant only at the 68% confidence level.

- Investment Sales.

  Investment sales overall exhibit a strong response to monetary policy shocks. The response is particularly strong for investment in the construction sector. In the long Pre-COVID sample, the decline in investment in construction is sharp and prolonged following the shock.

- Intermediate Goods Sales, Exports, Imports and Industrial Production.

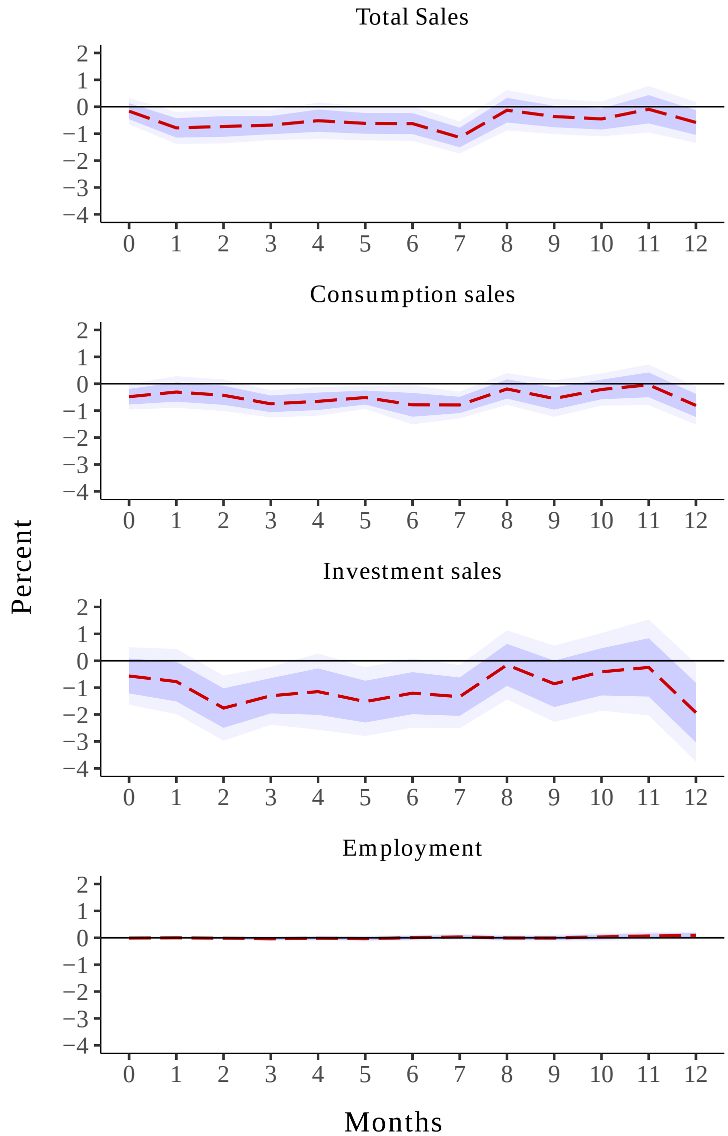
The response of intermediate good sales and export sales follows a similar pattern, with a significant decline starting in the first month after the shock—somewhat more pronounced for exports. Relative to these two series, imports display a significantly larger negative response, and is already significant on impact at the 68% level. Industrial production also responds in month 2, with a comparable magnitude and pattern to intermediate goods, export and import sales.

- Employment

  The impulse response of permanent employment closely follows the response of total employment in our baseline, with a significant if contained contraction the middle of the year. In the long pre-COVID-19 sample, however, temporary employment do not exhibit the pronounced fall at short lags we detect in our baseline and, most crucially, it exhibits a rise from the middle of the year, if only marginally significant (at the 68% confidence level) in month 12. The pattern in temporary employment may account for the difference in the response of total employment in the long pre-COVID-19 sample, relative to our baseline.

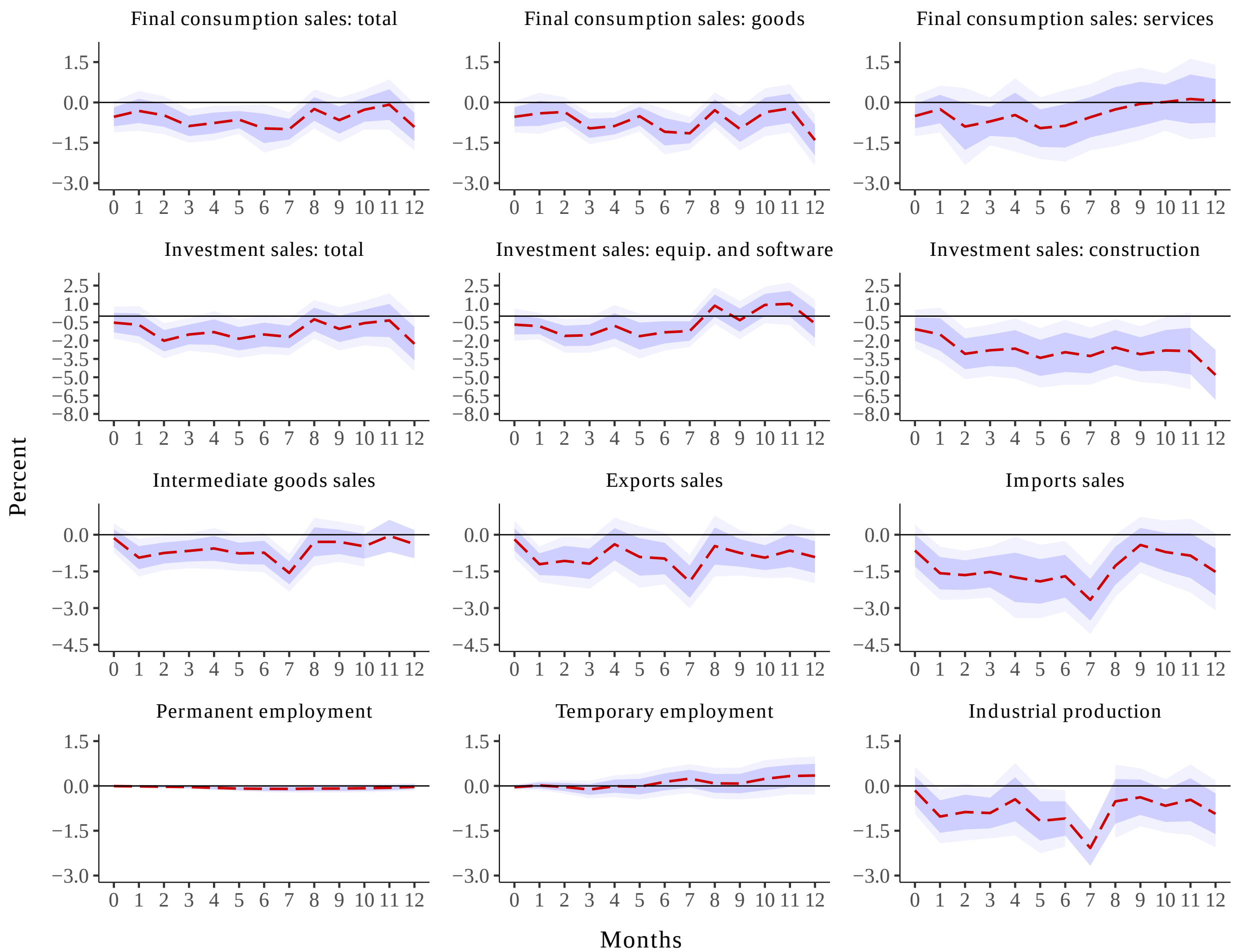
The results from our analysis at monthly frequency extended to a long Pre-COVID-19 sample demonstrates considerable robustness of our results on the short lags of monetary policy.

Figure F6: Monthly response of baseline real activity variables before COVID-19



Notes: LP impulse response functions to a one standard deviation monetary policy shock. The responses are reported in levels. The confidence intervals are computed from heteroskedasticity-robust standard errors. Darker-shaded areas are the 68% confidence intervals, and lighter-shaded areas are the 90% confidence intervals. The sample starts in January 2000 and ends in December 2019 and the monetary policy shock size is 3.7bp.

Figure F7: Monthly response of real activity before COVID-19



Notes: LP impulse response functions to a one standard deviation monetary policy shock. The responses are reported in levels. The confidence intervals are computed from heteroskedasticity-robust standard errors. Darker-shaded areas are the 68% confidence intervals, and lighter-shaded areas are the 90% confidence intervals. The sample starts in January 2000 and ends in December 2019 and the monetary policy shock size is 3.7bp.