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MONETARY VERSUS MACROPRUDENTIAL POLICIES: CAUSAL IMPACTS OF INTEREST RATES AND CREDIT CONTROLS IN THE ERA OF THE UK RADCLIFFE REPORT

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Monetary Versus Macroprudential Policies: Causal Impacts of Interest Rates and Credit Controls in the Era of the UK Radcliffe Report

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ABSTRACT

We have a world of conjoined monetary and macroprudential policies. But can they function smoothly and with what effects? The evidence is quite limited, especially for advanced economies. We adopt a historical laboratory. From the 1950s to the 1980s, the UK used credit policy tools alongside conventional interest rate policy. These tools are similar to today's macroprudential policies. We document these tools and craft a new dataset to estimate their effects using modern high-frequency identification with a novel empirical strategy, Factor-Augmented Local Projection. Monetary policy acted mainly on inflation but credit policy acted primarily to modulate bank lending.

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Changes in the complex of interest rates . . . should not be precluded, . . . and should, as a rule, be regarded as secondary to the technique of rationing the volume . . . of credit . . . .

— John Maynard Keynes[1]

But, when all has been said on the possibility of monetary action and of its likely efficacy, our conclusion is that monetary measures cannot alone be relied upon to keep in nice balance an economy subject to major strains from both without and within. Monetary measures can help, but that is all.

— The Radcliffe Report[2]

1. Introduction: Macropru in the Rear-View Mirror

The Global Financial Crisis and its disappointing aftermath were widely viewed as a case of macroeconomic policy failure from which lessons must be learned. Yet, more than a decade on, agreement on the precise failures and, thus, the necessary lessons, has been elusive in many areas, especially regarding financial regulation and central banking. According to some prominent commentators (Bernanke 2009; Tucker 2011) the lack of a clear mandate for macroprudential policy was a key shortcoming and many of the regulatory reforms since have aimed to address this. Encouragingly, there has a substantial increase in the use of macroprudential policy tools over the past decade in both advanced economies and emerging market economies alike (see, e.g., Alam et al. 2019).

Despite this, the role of macroprudential policy remains fraught, with doubts about whether these tools should exist, if they work, and how they should be designed and used. For example, there remains no real consensus on whether credit booms are best addressed via tighter monetary policy or via macroprudential policies.[3] And while most advanced economies now have specific institutional arrangements to coordinate or discharge macroprudential policies,[4] and one prominent committee – the UK's Financial Policy Committee – is now a little over ten years old, some large countries including the United States, Australia and Japan have yet to deploy these tools at all.

[1] Keynes (1978), page 397.

[2] Committee on the Working of the Monetary System (1959), page 183.

[3] Facing a heating up of their housing markets last decade, Sweden and Norway took quite different policy actions. Sweden's Riksbank tried to battle this development using monetary policy tools only, raising the policy rate, and tipping the economy into deflation, as had been predicted by the dissident Deputy Governor Lars Svensson, who subsequently resigned. Across the border, the Norges Bank implemented some cyclical macroprudential policies to crimp credit expansion and moderate mortgage and house price booms and managed to avoid an out-turn like that in Sweden.

[4] Edge and Liang (2019) find that 47 countries have set up such Financial Stability Committees.

It seems likely to us that the lack of reliable empirical evidence on the impact of macroprudential policies is hindering their take-up. While the greater use of macroprudential tools over the past decade has spawned a large body of empirical research,[5] there remains a lack of longer-run empirical evidence, especially in advanced economies.

New evidence and methods This paper seeks a new analytical approach by bringing a novel and unique array of empirical evidence to the table based on past UK experience over a relatively long period. To that end, we turn to the last great era of central bank experimentation with instruments similar to those now being used for macroprudential policy: the postwar decades from the 1950s to the early 1980s when many types of new credit controls were put in play.

In these years – particularly the 1950s and 1960s – economic discourse and policymaking in the UK were dominated by Keynes' legacy. The Radcliffe Report is the most famous example of Keynes' followers' scepticism of using policy rates for macroeconomic control and it endorsed other instruments, notably credit controls, instead. In our examination of this period, which we dub the "Radcliffe era", we construct by hand new quantitative indicators over many decades on the application of such policies in the UK, including credit ceilings, hire-purchase controls, special deposits, and the so-called "Corset". We follow Hodgman (1973) and Miller (1973) in referring to instruments such as these as "credit controls" and use the term "credit policy" to describe these policies taken as a whole.

To evaluate the impacts of these policies, and to compare them with the impacts of the standard monetary policy tool of Bank Rate, we develop a new empirical approach, one that we refer to as Factor-Augmented Local Projection (FALP). Our approach uses the local projection (LP) method (Jordà 2005) with shocks identified using high-frequency financial market surprises to study macro responses in the spirit of Gertler and Karadi (2015). We join this with the factor augmentation approach that has been employed in the VAR literature (Bernanke, Boivin and Eliasz 2005) as a means to control for other information correlated with future macroeconomic developments, for more robust estimation in small samples. For robustness we also estimate LPs using an alternative and weaker identification assumption via the now widely-used heteroskedasticity-based estimation which takes into account the difference in signal and noise on (treatment) days with a policy action and (control) days without (Rigobon 2003; Rigobon and Sack 2004; Nakamura and Steinsson 2018; Känzig 2021). We also subject our results to a range of robustness tests, most of which give us little reason to doubt our main results.

[5] Araujo et al. (2020) summarise lessons from the literature, using a meta-analysis of 58 distinct empirical studies. Looking across these studies, they find that macroprudential policies have statistically significant effects on credit, albeit with considerable heterogeneity across instruments, and weaker effects on house prices. Moreover, they find these effects are stronger in emerging market economies.

To do all this, we have used archival data from the Bank of England and other primary sources to construct a new set of monetary and credit policy action dates and an index that measures the credit policy actions themselves. We also assembled high quality data for macroeconomic and financial variables, both quantitative and qualitative, which are crucially important to the credibility of our identification strategy and results. To that end we devoted a considerable length of time to transcribing data by hand from 1950s–80s copies of the *Financial Times* to gather the data for the high-frequency financial market surprises, specifically daily changes in gilt yields and in the equity prices of banks and financial companies.

We also note here that ours is one of the first papers to demonstrate the usefulness of high-frequency identification methods for macro-historical research, and the UK is a good experimental setting for this: the authorities were relatively transparent about their policy actions, unlike other countries at this time, e.g., the Federal Reserve (Cook and Hahn 1989).

Main findings What do we learn from this study – about these policies in their own time, and in terms of lessons for today? In one respect, our econometric evidence suggests that the policies of the Radcliffe era were actually a bit of a failure judged on their own terms, when we recall that policymakers at the time viewed credit policy as a substitute for monetary policy. That is, the Bank of England thought that it could use credit controls to fine-tune macroeconomic outcomes in line with the government's stated objectives for output, inflation, and so on. But we find only mixed evidence for this belief. Changes in credit controls did strongly depress lending, and did so more quickly and robustly than changes in Bank Rate; but their effects on output are somewhat less robust; and credit controls did not have a significantly negative impact on consumer prices. At the same time, we find that changes to the policy rate delivered more typical impacts of conventional sign and magnitude on output and inflation (with a lag), in accord with today's consensus views and empirical evidence. In other words, the two policies were complements not substitutes and operated in a somewhat orthogonal fashion on different targets.

Can these findings tell us anything about the usefulness of present-day macroprudential policies? There are many reasons to be wary of extrapolating lessons from the Radcliffe era to the present day. For one thing, it could be argued that the credit policies used in post-war Britain bear only a passing resemblance to the macroprudential policies of today. That said, hire-purchase regulation is not dissimilar to policies that regulate loan to value limits, the most commonly-used macroprudential tool today. Moreover, liquidity regulation is back in vogue with Basel III's Liquidity Coverage and Net Stable Funding ratios, and time-varying reserve requirements – close cousins of the Special Deposit and Corset tools analysed in this paper – are still widely used by developing countries for macroprudential purposes (see,

e.g., Cordella et al. 2014). Another concern is that the highly regulated financial system to which these tools were applied is largely unrecognisable from the world of satiated reserves, systemically important banks, and shadow banking which define the US landscape today. While this is certainly true of the US and other Anglo-Saxon countries, other countries – continental Europe, Japan and the rest of the world – remain largely bank based. Finally, it could be argued that the objectives of Radcliffe-era credit policies differed radically from the financial stability focus of macroprudential policies today. This is also true, although we note that Turkey's central bank recently employed a series of policies aimed at limiting credit availability to tame high inflation as a surrogate for tighter monetary policy.

Today, if a similar macroprudential policy were to operate as in the Radcliffe era, it might fairly be said that "it does exactly what it says on the tin". It would more strongly modulate credit, and yet would have weaker impacts on prices than conventional monetary policy. Since the underlying IRFs span different outcome spaces, a major claim by proponents of macroprudential policies now has at least some evidence behind it. That is, a non-duplicating but complementary mix of monetary and macroprudential policy tools might curb the credit cycle, and so moderate financial instability risks, without otherwise pushing the economy unduly off target, or less so than with only a pure monetary policy action like a leaning-against-the-wind rate hike.

2. Related literature

The effects of post-war credit controls in Britain At the time credit controls were being used, several British economists studied their macroeconomic effects, albeit using rather dated techniques. The majority of papers found that the application of credit controls was followed by lower bank lending, and although very few papers looked at the impact on overall activity, a handful found that hire-purchase controls reduced durable goods consumption.

According to Coghlan (1973), for every £1 of special deposits (similar to remunerated reserve requirements, see Section 4.1) called, lending fell by 62p, although this effect appeared to be temporary. Subsequently, Melitz and Sterdyniak (1979) reported evidence that an increase in reserve holdings by the commercial banks (which could be driven by a call for special deposits) was associated with a rise in the rate of interest they charged on bank lending. In a range of other studies, Norton (1969), Artis (1978) and Savage (1978) all found statistically significant and economically large impacts of credit ceilings on bank lending, while quite a number of papers reported that hire-purchase controls temporarily

depressed both hire purchase credit and consumption.[6]

This paper broadly confirms the results of this literature using modern techniques, but also goes far beyond it in terms of questions asked and responses examined.

The effects of post-war credit controls in other countries The modern literature on credit controls, which uses more sophisticated econometric techniques, remains small. Largely comprised of country-specific studies, it tends to find that credit controls depressed lending, while evidence on other effects is more sparse.

Romer and Romer (1993) identify five periods of credit policy restraint in post-WWII USA from the narrative evidence, which they call "credit actions". In response to a credit action, the credit spread rose and the ratio of bank lending to total credit fell, suggesting a bank lending channel. Industrial production also fell, although the estimate is not statistically significant. Elliott, Feldberg and Lehnert (2013) build on this by constructing a US credit policy index with more variation, but with almost all of the variation in policy actions concentrated in the 1960s and 1970s. The authors' preliminary analysis finds that tighter credit policies lowered consumer debt, while loosening did not raise it. Zdzienicka et al. (2015) extend this work, finding that both monetary and credit policy had a dampening effect on financial conditions, but the effect of credit policy was more immediate and shorter-lived.[7]

Elsewhere, Monnet (2014) studies the impact of post-WWII macroeconomic policy in France, using a narrative measure similar to that of Romer and Romer (1993). In periods of credit restraint, the French authorities used reserve and liquidity requirements and credit ceilings, as well as non-standard monetary measures such as individual bank discount ceilings. He finds that a tightening in non-price monetary and credit policy led to a fall in lending, output and prices and an improvement in the current account balance. In other work, Monnet (2016) describes the widespread use of similar credit policies in other European countries, with the notable exception of Germany. Sonoda and Sudo (2016) study Japan's experience of using credit controls called "Quantitative Restrictions". Although there is not much variation in the policy, the authors find that these controls reduced lending and output and raised prices. Emerging markets have for a long time continued to use credit controls, particularly reserve requirements. Glocker and Towbin (2015) examine the Brazilian experience and also find that activity falls and prices rise in response to a tightening in reserve requirements.

Our study complements this strand of work and goes beyond it in terms of the range of

[6] E.g., Ball and Drake (1963), Stone (1964), El-Mokadem (1973), Garganas (1975), Cuthbertson (1980), and, recently, Aron et al. (2012) and Scott and Walker (2017).

[7] Other papers including Koch (2015) study individual credit policies in the US such as Regulation Q.

responses to monetary and credit policy innovations and the techniques we use. Furthermore, Britain is a particularly good case to study for two reasons. First, it is probably the most comparable case study to most countries using monetary and macroprudential policies today. This is because, unlike in most European countries at the time, British monetary policy worked through relatively modern techniques (such as open market operations), but unlike the US (then and now), credit controls were not very prone to leakages because most household and business credit was intermediated through banks and near-banks subject to regulation. Second, we have excellent data, not least the archival evidence we exploit on the policies themselves, the financial market responses, and the banks' responses to these policies.

The effects of modern-day credit controls ("macroprudential policies") Our paper also intersects with the still nascent literature on the macroeconomic effects of macroprudential policy actions.

One recent such paper is by Kim and Mehrotra (2022). The authors estimate a panel vector autoregression model to analyse the transmission mechanism of macroprudential policy actions in 32 advanced and emerging market economies. Like us, they study an index of macroprudential policy measures broadly defined – their measure includes capital buffers, interbank exposure limits, concentration limits, loan-to-value ratios and reserve requirements. They find that macroprudential policy shocks – identified by assuming that macroeconomic variables do not respond within a quarter – influence GDP and CPI inflation in a similar way to monetary policy shocks. However, whereas the former mostly affect residential investment and household credit, monetary policy shocks have more widespread effects on the economy. These channels are influenced by macro-financial country characteristics such as the exchange rate regime and the level of financial development.

Another important contribution is by Richter, Schularick and Shim (2019). The authors study the macroeconomic impact of shocks to loan to value requirements, and also use a panel data approach with 56 advanced and emerging market economies. While their focus is therefore more narrow than Kim and Mehrotra (2022), they make two innovations: firstly, following Romer and Romer (1989) they identify policy shocks via a narrative approach as changes in loan to value requirements that are made without references to GDP and inflation; second, their method allows them to study the intensity of policy actions, rather than simply treating actions via an on-off dummy variable. They find that shocks to loan to value requirements have only modest and imprecisely estimated effects on GDP and an impact on the price level that is close to zero in most specifications.

Alam et al. (2019) also study the macroeconomic effects of changes in loan to value requirements. In contrast to Richter, Schularick and Shim (2019), they use a propensity-

score-based approach to identify exogenous changes in policy. They find that while changes in loan to value requirements have large effects on household credit, the impact on consumption is typically modest. They also condition on the size of policy changes and on the initial level of the loan to value limit, and find both diminishing per-unit effects from larger policy actions and a less favourable trade-off between resilience and macroeconomic costs when loan to value limits are already tight.

The effects of financial shocks Our paper also contributes to a broader debate about the macroeconomic impact of financial shocks. Whereas in the 1990s, the literature focussed on the bank lending and broad credit channels of monetary policy, more recently researchers have analysed financial shocks as an independent source of economic fluctuations.[8]

Several papers have focussed on the impact on firms. Gilchrist et al. (2017) analyse the impact of financial shocks on firms' pricing decisions in a customer markets model with financial frictions. They find that financing constraints act to inhibit firms from cutting prices following an adverse shock to avoid the need to resort to costly external finance – a channel they argue accounts for the muted decline in inflation in many countries following the 2008 financial crisis.

Other papers in the literature find that contractionary financial shocks reduce firms' ability to borrow, in turn reducing investment and output. Depending on the reaction of monetary policy, consumer prices can end up rising after a lag (Bernanke, Gertler and Gilchrist 1999; Gilchrist and Zakrajšek 2011). Firms also require finance for working capital, for instance because they need to pay for inputs of production before they receive payment for their product. Models of the working capital channel tend to find that a reduction in the credit supply forces firms to produce less, and in turn firms react by raising prices (Carlstrom, Fuerst and Paustian 2010; Gilchrist and Zakrajšek 2011). A closely related literature examines the macroeconomic impact of financial shocks via a household demand channel. Papers in this literature tend to focus on mortgage borrowing, and predict that financial shocks move output and consumer prices in the same direction (e.g., Iacoviello 2005). Another recent theoretical paper with relevance for our analysis is Bahadir and Gumus (2016), in whose setup controls do not directly affect mortgage lending as was true in the UK in the era we study.

While the literature contains evidence in favour of the existence of both the household demand and firm supply channels, only recently have researchers started to ask which channel dominates – though the answer might vary, of course, by time and place. Mian, Sufi and Verner (2020)'s work stresses the household demand channel as a dominant factor

[8]Indeed Romer and Romer (1993) was a response to other key papers in this literature such as Kashyap and Stein (1994).

in the US in the 1980s. However, our findings, and those of Glocker and Towbin (2015), Sonoda and Sudo (2016), and Meeks (2017),[9] find little response of consumer prices to tighter credit policy, suggesting perhaps no clearly dominant channel. In a similar vein, Barnett and Thomas (2014), who study bank lending and economic activity in post-WWII Britain using a structural VAR, conclude that "credit supply shocks behave more like aggregate supply shocks than aggregate demand shocks", while Bassett et al. (2014) report that lending standards shocks in the US in the 1990s and 2000s did not have an economically or statistically significant impact on prices.

3. Credit Policy in the Radcliffe Era

Why were credit controls used and how were they co-ordinated with monetary and other policies? Answering these questions is important both in designing an empirical strategy to analyse the effects of monetary and credit policies and in drawing conclusions for monetary and macroprudential policies today. There are three important pieces of historical context which shed light on these questions.[10]

First, policymakers were highly sceptical about using the central bank interest rate for macroeconomic control. Although the debate shifted in the 1970s under the influence of monetarism, it was not until the arrival of the Thatcher government in 1979 that conventional interest rate–based monetary policy became the primary tool of macroeconomic policy. This scepticism was most famously set out in the Report of the Committee on the Working of the Monetary System (1959), universally known as the Radcliffe Report. Commissioned by the Chancellor of the Exchequer, and the outcome of two years of hearings, the Report was a wide-ranging survey of the structure and operation of the UK monetary and financial system.

The central messages in the Radcliffe Report concerned the objectives and instruments of monetary policy. The number of scholarly citations it received within ten years of publication – around 2000, including a paper marking its tenth anniversary by Anna Schwartz (Schwartz 1969) – is testament to the stir it caused in academia.[11] Whether or not it led to immediate significant changes in UK policymaking, it merits a chapter in the official history of the Bank of England (Capie 2010), not least because it is an excellent guide to official thinking at the time. Ten years on from its publication, the Bank of England stated clearly that "the approach to policy has been similar to that of the Radcliffe Committee".[12]

[9] Meeks (2017) studies the macroeconomic impacts of microprudential regulation in the UK from the 1990s onwards.

[10] Excellent broader overviews of postwar UK monetary policy include Dimsdale (1991) and Howson (2004).

[11] Citation count from a Google Scholar search.

[12] Bank of England (1970).

The Radcliffe Report's prescriptions for the objectives of policy were not controversial. Above all, policymakers should aim to achieve full employment, stable prices, and external balance, although the Committee recognised that there may be trade-offs between these objectives. However, viewed from today's perspective, the Committee's views on the instruments of policy were deeply unorthodox (Batini and Nelson (2009) give an excellent account of the development of monetary policy doctrine over the period of our study). It argued that other financial assets were good substitutes for money, so an increase in money would fall predominantly on demand for financial assets (and hence interest rates) rather than demand for goods. In favour of this, it cited survey evidence which it claimed showed that inventories and investment were not particularly responsive to the short-term interest rate. Or, to paraphrase using a simple model of the time, it believed that the IS curve was steep (Howson 2004). While a steep IS curve does not imply that traditional monetary policy is ineffective, this belief did lead to a view that large movements in short-term interest rates would be needed for a given impact on output.

A second important piece of historical context cemented the aversion to using short-term interest rates. This is that the post-WWII public debt to GDP ratio was extremely high – over 150 per cent of GDP at the start of our study and only falling below 50 per cent by the mid-1970s. Given the public debt position and the fact that a significant portion was owed to foreigners, the Treasury was extremely keen to avoid macroeconomic policies which would raise the debt service burden (Hodgman 1973; Fforde 1992; Allen 2014). Although the Radcliffe Committee refrained from giving a clear view on how the macroeconomy should be controlled if not via conventional monetary policy, contemporary commentators agreed that fiscal policy was to be preferred (see, e.g., Gurley 1960; Kaldor 1960).

Finally, the exchange rate regime – the third key piece of context about the period – is instrumental in understanding why credit controls were used. Britain had a dollar peg until 1972, and even after the end of the Bretton Woods regime, the exchange rate was still heavily managed, particularly before and after the 1976 crisis. Britain had some interest rate independence under the trilemma, given the bite of Bretton Woods era cross-border capital controls. But the leeway was limited, and the economy lurched from one balance of payments crisis to another (Schenk 2010; Kennedy 2018; Naef 2022). Given political constraints and the high debt burden the authorities were often unable or unwilling to run sufficiently tight conventional monetary policy to defend the foreign exchange reserves position. It could also be difficult to achieve the political consensus to tighten fiscal policy sufficiently in the heat of a crisis. The authorities naturally looked for alternatives, hence the attractiveness of a distinct set of credit policy tools.

The choice of instrument was a source of ongoing friction between the Treasury and the Bank of England over this period. This tension led, in Kareken (1968)'s view, to a kind of

game of chicken, where the two authorities were inclined to put off tighter policy until the last minute (leading to "stop–go" policy) as the Treasury did not want higher interest rates and the Bank did not want to apply controls. The reason for the Bank of England's distaste of credit controls is clear – they were difficult to administer, required ongoing attention to limit leakages, and above all the banks (to which the Bank was close) did not like them. In the end, the authorities often responded with a policy package which included higher Bank Rate, tighter credit controls, tax rises, and other tools such as incomes policies.[13]

Evidence of the stop–go pattern is seen in Figure 1 which shows key indicators used in our analysis: manufacturing output and retail sales; consumer price inflation; bank lending; and Bank Rate. There were pronounced fluctuations in all variables over the period, at an unusually high frequency. Real activity exhibited very short cycles: activity peaked locally in 1953, 1955, 1957, 1960, 1964, 1968, 1973, 1976, 1978–79, and 1982. But volatility clearly stepped up in the 1970s and this has some bearing on our empirical strategy.

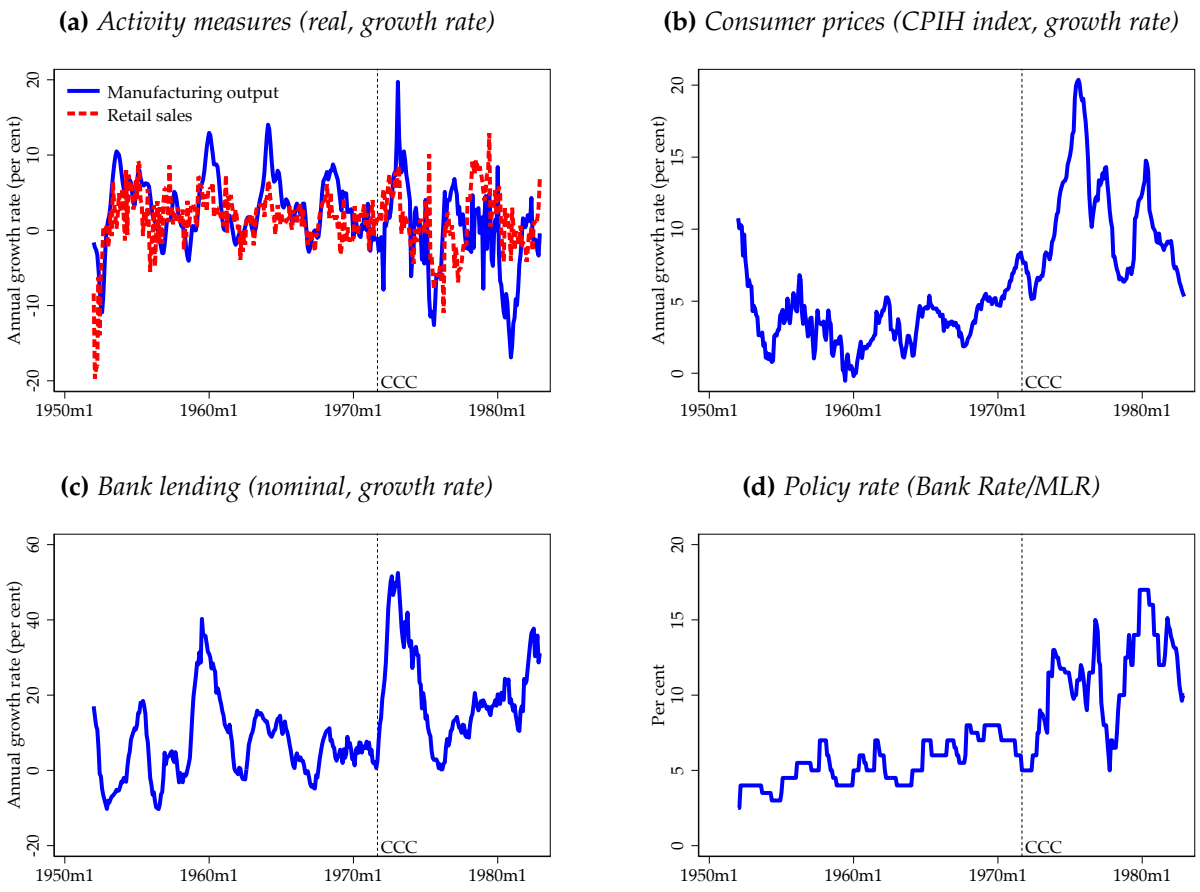
A change of regime in 1971 The Competition and Credit Control (CCC) reform of September 1971 marked the start of a new regime, with a liberalised commercial banking sector and significantly less reliance on credit controls (Goodhart 2015). Following years of growing dissatisfaction on the part of the financial sector and the Bank of England, the reform was intended to mark the permanent end of credit controls (with the partial exception of special deposits, as explained below). In addition, the reform abolished the banking cartel. In the words of an observer, CCC was a "radical and still instructive deregulatory experiment. . . The Bank of England scrapped quantitative ceilings on bank lending in favour of indirect controls, such as balance sheet ratios" (Plender 2024).

Alongside Chancellor Barber's highly expansionary 1972 "dash for growth" Budget, CCC unleashed a vigorous boom in both bank lending and output. Sterling was floated in 1972 and, after a lag, inflation started its rapid ascent to a peak of over 20%. The credit boom ended in the secondary banking crisis of 1973–74. Given Prime Minister Heath's opposition to higher interest rates, there was a partial return to credit policy. However, from this point on the controls used were less aggressive than in the pre-CCC period. Thus, CCC is seen as "a decisive break with the prior system of maintaining direct controls over bank lending. . . .despite a partial reversion towards a direct control system in the guise of the 'corset' . . . the direction of travel towards a more liberal, market-based system, remained" (Goodhart 2015, p. 235).

[13] A series of papers written in the 1960s and 70s examined the authorities' reaction function and confirmed that Bank Rate, credit controls and tax rates were all used forcefully in response to movements in foreign exchange reserves and also in reaction to unemployment and consumer prices (Fisher 1968, 1970; Pissarides 1972; Coghlan 1975). Interestingly, Pissarides (1972) finds some evidence of sluggish policy adjustment (possibly because of political costs of changing policy).

Figure 1: *Behaviour of key macroeconomic variables over the period*

The figure presents time series of key macroeconomic indicators over our sample period.



Notes: See text. The vertical dashed line marks the 1971 inception of Competition and Credit Control (CCC).

For this reason, we think an important breakpoint in the macroeconomic regime, in policy and other respects, is indicated in 1971, as shown in the figure. But not only did CCC appear; coincidentally, within a short time the OPEC oil shocks, accelerating wage-price inflationary dynamics, industrial strife, political instability and other factors combined to produce a new and different high-volatility regime for macroeconomic and financial variables. Inflation, which had been in the 0–10 per cent range for most of the 1950s and 60s then rose rapidly and peaked at over 20 per cent in 1975.[14]

But there is also evidence that other aspects of policy changed in the 1970s. Narrative accounts stress that British authorities adopted monetary targets as a constraint on policy

[14]The surge in inflation from 1973 to 1975 was not helped by the policy-induced indexation of wages to retail prices which came in to operation around the same time as the first the oil shock (Miller 1976).

in the 1970s and although the exact timing is disputed,[15] there is broader consensus that they were not effective constraints.[16] Instead, balance of payments concerns still played an important role, particularly in 1976 when an IMF loan was accompanied by a significant policy tightening, and in 1977 when the Bank of England cut the policy rate and accumulated reserves rapidly in an effort to prevent sterling appreciation.

Beyond the well known narrative evidence, we also find evidence of a change in regime when we apply our high-frequency identification methods below (see Figure 4, especially the second row). After 1971, volatility increases, especially for the bank equity prices we use for credit policy surprises. We also discuss how, after the CCC regime break, the exact functioning of credit policies becomes less clear, and more conflated with monetary policy. These changes all tend to blur empirical findings when we extend the sample after CCC. This would undercut identification approach, so all of our baseline results will be based on the pre-CCC period, i.e., the years before 1971. We also show results for the full sample as a robustness check.

4. Description and Use of Credit Policy Tools

In this section we set out the details and our measurement of the four main cyclical credit policy tools used in Britain in the era we study:

- Hire-purchase controls;
- Special deposits;
- Credit ceilings; and
- The supplementary special deposits scheme (the "Corset").

Table 1 provides a summary of these tools, including the authority responsible for setting each tool, the institutions to which they applied, and their use over the period we examine. Banks were also subject to various other liquidity requirements, including a uniform minimum liquidity ratio and cash ratio (Bank of England 1962*a*).[17] However, the calibration

[15] Needham (2015) argues that informal monetary targets came in as early as 1971, in contrast to Capie (2010) who puts the date at 1976 when the first hard public commitment was made.

[16] Indeed Nelson and Nikolov (2004) attribute the Great Inflation in the UK to monetary policy neglect.

[17] From 1946, the clearing banks agreed to maintain a minimum cash-to-deposit ratio of 8%. From 1951, the clearing banks agreed to maintain a liquid asset ratio of reserves, call money and Treasury and commercial bills of 28%–32% of their deposit liabilities; from 1957, this agreement was made more precise when the liquid asset ratio was set at a minimum of 30%; in 1963, this was reduced to 28%. This system was replaced by a single reserve asset ratio of 12.5% in 1971. The definition was in between the cash and liquid asset ratios and turned out to constitute a modest easing of the constraint on most banks. In 1981 the reserve asset ratio requirement was abandoned.

Table 1: *A summary of credit policy tools used by the UK authorities*

Policy instrument	Description	Responsible authority	Scope	Usage
Hire-purchase controls	Restrictions on terms of hire purchase (instalment) lending for different categories of consumer goods, including minimum down payments and maximum repayment periods	Board of Trade (UK government)	Market-wide	Used through the 1950s, 1960s and 1970s, until their removal in 1982
Credit ceilings	Short-term quantitative ceilings on the level of credit extended to the private sector and overseas. Export finance usually excluded and lending to households and hire purchase lenders usually particularly discouraged	Bank of England	London and Scottish clearing banks and other listed banks; restraint letters also sent to finance houses, acceptance houses and discount market participants	Scheme used on various occasions in all three decades until abolition in 1971
Special deposits	Requirements to place interest-bearing deposits at the Bank of England	Bank of England	London and Scottish clearing banks; all retail banks from 1971	First used in April 1960, used frequently in 1960s and 1970s
Supplementary special deposits scheme (the "Corset")	Requirements to place non-interest bearing deposits at the Bank of England if interest-bearing eligible liabilities grew faster than a specified rate	Bank of England	All listed banks and deposit-taking finance houses; small institutions and Northern Irish banks were exempt	Scheme activated on three occasions: December 1973; November 1976; June 1978

of these requirements was largely unchanged over the period, and so they have been excluded from this analysis.

4.1. Description of credit policy tools

Hire-purchase controls By the late 1950s, hire purchase, the practice of purchasing durable goods via instalment credit, had become an increasingly important source of finance for commercial vehicles, cars, and other consumer durables. For instance, by the late 1950s, the number of hire purchase contracts signed for financing new and second-hand car purchases was around 50% larger than the number of new car registrations (Cuthbertson 1961). The Board of Trade, a government department, exercised control over the terms of hire purchase credit by stipulating minimum down payments and maximum repayment periods for different categories of goods. These controls were used actively for much of the 1950s, and despite the Radcliffe Report's verdict that they were suitable for use only for short periods at times of emergency, continued to be used throughout the 1960s and 70s until their removal in 1982.

The principal advantage of hire-purchase controls lay in the reach they provided beyond the clearing banks to the specialist hire purchase finance companies that funded approximately three quarters of the stock of hire purchase debt. While the larger finance houses took some advances from banks, the majority of their funding came from issuing

deposits.[18] Controls on clearing banks' lending therefore had only a small effect on the provision of hire purchase credit by finance companies. A second purported advantage of hire-purchase controls was that they could be targeted: policy changes were frequently directed at particular classes of good (e.g., cars, furniture, etc.).

How important was hire-purchase credit in the aggregate? Relative to all forms credit, it represented a nontrivial share of lending. Based on data for June 1962 we found that total hire purchase credit was £887 million (mainly from hire purchase finance houses, the largest of which was United Dominions Trust). At that time the total of all bank lending was £5,044 million. Thus, the two together added up to a total credit supply of £5,931, of which hire-purchase credit accounted for a share of about $\frac{1}{7}$.

Credit ceilings Credit ceilings were used frequently from 1955 to 1971 as an emergency tool to reduce aggregate demand when balance of payments deficits reached crisis proportions. The modalities of these ceilings were typically set out in letters from the Governor of the Bank to the main banking and finance associations. The first such request was made in July 1955, with the banks asked to reduce their lending by 10 per cent by the end of the year. Typically, the ceilings were accompanied by a request to focus restraint on lending to the household sector and to maintain or expand lending to export or import-competing sectors. Usually the banks complied with the ceilings, but on one occasion (in 1969) the banks were fined for exceeding a ceiling.[19] The scope of the ceilings varied over time in terms of the institutions and types of lending covered, but it was always wide.

The practical difficulties of implementing credit ceilings were colossal. One problem lay in extending the scope of the restraint beyond the clearing banks to relevant non-bank financial intermediaries. Restraint letters were sent to the Finance Houses Association, the Acceptance Houses Committee, and the London Discount Market Association – but the Bank had no formal power to enforce its requests to these non-banks. A second problem arose when the clearing banks repeatedly overshot their lending targets, a fact that they plausibly put down to customers making use of existing credit facilities (Goodhart 2015).[20]

Special deposits Special deposits were introduced as a less intrusive replacement for ceilings in 1958. They were akin to a remunerated reserve requirement. Banks were requested to place varying amounts of deposits at the Bank of England, in an amount

[18] In June 1962, deposits accounted for nearly two thirds of large finance houses' liabilities; smaller finance houses, by contrast, were more reliant on bank advances to fund their balance sheets (Bank of England 1962*b*).

[19] On 31 May 1969, the Bank announced that the rate of interest payable on special deposits would be halved until the ceiling at the time was met.

[20] The lending targets had no statutory basis, as was common with most financial regulation of the time. According to Capie (2010), the possibility of issuing a formal directive was raised, but dropped for fear of "changing the nature of the relationship between the Bank and the banks".

proportional to their deposit bases, at an interest rate close to the Treasury bill rate.[21] Banks were given about 60 days on average to meet requests for additional deposits, and about 20 days to comply when policy was loosened. The scheme initially applied to the London and Scottish clearing banks only but was broadened in 1971.

The tool was intended to influence banks' ability to expand credit by acting on their minimum liquidity ratios: special deposits did not count as liquid assets for the purposes of calculating these ratios, so a bank whose ratio was near the minimum when the tool was applied would be forced either to curtail lending or sell investments (typically government securities). Over time, the authorities tried to restrict banks' freedom in adjusting to these calls by requesting that they were not met by selling investments. The tool was first used in April 1960 and, despite being initially seen as "a temporary arrangement" (Capie 2010), was frequently used until its abandonment in 1980.

Between 1972 and 1978 special deposits were used in quite a different way to the pre-CCC period. These were years when the Bank of England set the policy rate – called the Minimum Lending Rate from 1972 – as a mark-up on the three-month Treasury Bill rate. Special deposits were used as one of the instruments to influence the market rate: by calling for special deposits, the Bank would raise banks' demand for central bank money. This contrasted with the pre-CCC period when special deposits had no effect on Bank Rate, the rate at which the Bank lent to the money market, which was set directly by the authorities. It is therefore more difficult to separate monetary from credit policy in the post-CCC period.

Supplementary special deposits (the "Corset") The supplementary special deposits scheme – otherwise known as the "Corset" – was a second attempt to replace ceilings after they were brought back in the 1960s. First used in 1973, it was a system of non-remunerated reserve requirements tied to the rate of growth of each bank's interest-bearing sterling deposits or "eligible liabilities".[22] If eligible liabilities grew faster than the prescribed rate, banks were required to place non-interest-bearing supplementary special deposits with the Bank. The deposit requirements were progressively larger the greater the overshoot over the prescribed rate, ranging from 5 to 50 per cent of the incremental excess eligible liability growth. This had the effect of forcing banks either to accept lower profits or even losses on their marginal lending, or else to widen their lending spreads. The scheme applied to all listed banks and deposit-taking finance houses operating in the United Kingdom, but small institutions and Northern Irish banks were made exempt.

[21] There was no statutory basis for these requests either.

[22] Interest-bearing sterling deposits were taken to be the marginal source of funding to meet fluctuations in the demand for credit. For further detail, see Bank of England (1982).

Although it was in place for several years, banks' eligible liabilities were well below the limit for most of this time. The limit was only significantly exceeded in 1980 after the planned abolition of the "corset" had been announced. Here again this period saw the credit policy regime drifting into a less strict configuration. But we emphasize that this post-CCC period is excluded from our baseline sample.

4.2. Creating an aggregate credit policy index

To analyse the effects of the credit policy instruments, we sum up the overall stance of credit policy by creating an aggregate index. We create this index in two steps: first, we code subindices that characterise the stance of each of the four instruments listed above; second, we summarise variation across the subindices by combining them using a simple weighting scheme.

Coding the individual credit controls It is straightforward to characterise the stance of special deposits, as the tool was varied on a continuous scale.[23] The source for the data is the *Bank of England Quarterly Bulletin* (various issues).

For hire-purchase restrictions, we create a continuous subindex following the same method as Williams (1972) and Cuthbertson (1980). This combines the minimum down payment and the maximum repayment limit (in months):

$$HP_t^i = d_t^i + \left(\frac{100 - d_t^i}{T_t^i} \right),$$

where i is the type of good (cars, commercial vehicles, radios and televisions, or furniture), d_t^i is the required down payment on good i, and T_t^i is the maximum repayment period on good i in months. The final subindex is the sum of all the HP_t^i weighted by the amount of credit subject to hire-purchase controls financing good type i. Credit conditions for durable good i are therefore tighter when either the d^i increases or when T^i falls.

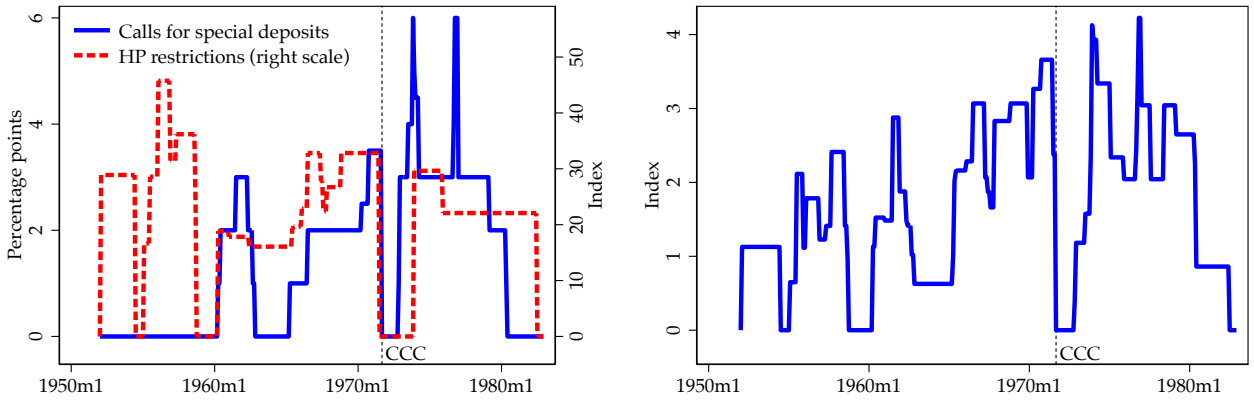
This index captures the idea that hire-purchases restrictions operated by affecting both the required downpayment on durables purchases and via the implied debt service burden. One appealing feature of the formulation we employ is that it captures the interaction between these two channels in that tighter downpayment requirements mean borrowers require greater liquidity up front, but their debt service burden thereafter is lower. In practice, changes in downpayment requirements dominate the series. The data on minimum

[23] One complication is that banks were given around 60 days' notice of calls for special deposits before they were binding. So we could use either announcement dates or implementation dates in our subindex. We choose the former as we expect that forward-looking banks would have begun adjusting their portfolios immediately after announcements.

Figure 2: *Time series of credit policy indices*

The left panel shows the special deposit rates set by the Bank of England (blue line) and our index of hire-purchase restrictions set by the Board of Trade (red line). The right panel shows the credit policy index.

(a) *Special deposit rate and hire-purchase controls index* **(b)** *Aggregate credit policy index*



Notes: See text. The vertical dashed line marks the 1971 inception of Competition and Credit Control (CCC).

down payments and maximum repayment periods are taken from the Crowther Report and the *National Institute Economic Review*.[24] For the weighting, we use fixed weights from the Crowther Report on credit extended in 1966 (around the middle of our sample).[25]

Figure 2a presents the time series of these first two continuous subindices, hire-purchase controls and special deposits.

We code the other tools in our set – credit ceilings and the supplementary special deposits scheme – using a binary 0–1 index. Decisions about ceilings were not always made in public, so we have consulted the Bank of England archives. For example, in December 1969, the Bank of England told the banks privately that they would not be held to the publicly-announced credit ceiling, so we code this and subsequent months "0" until the ceiling was next activated in April 1970.[26] Tables 2 and 3 set out the policy changes that occurred and the mapping from these into index values.

Aggregating the subindices We next aggregate the subindices because we expect the macroeconomic impacts to be similar and because the resulting credit policy index gives us more variation to exploit than would the four individual subindices.[27]

[24] Crowther (1971*a*), National Institute of Economic and Social Research (1960) and National Institute of Economic and Social Research (1984).

[25] Crowther (1971*b*).

[26] Extract from Mr. Hollom's memo dated 1.1.70 on his talk 31.12.69 with Mr. Wilson and Mr. Piper (C.L.C.B.), 1st January 1970, Bank of England Archive 3A8/6

[27] A macroeconomic model would likely treat all of these controls as an increase intermediation costs.

Table 2: *Classifying credit ceiling notices*

Date	Description of the policy	Classification
1955 Jul 25	Chancellor asks the banks to "achieve a positive and significant reduction in the total of bank advances outstanding", which is interpreted as a 10% reduction before the end of the year	1
1955 Dec 31	End of the ceiling	0
1957 Sep 19	Chancellor asks the banks that "the average level of bank advances during the next twelve months should be held at the average level for the last twelve months"	1
1958 Jul 31	End of the ceiling	0
1961 Jul 25	Governor's letter to Committee of London Clearing Bankers: the level of advances at the end of the year should not exceed that of June"	1
1961 Dec 31	End of the ceiling	0
1965 May 5	Governor's letter to Committee of London Clearing Bankers: advances to private sector should not increase more than 5% during twelve months to March 1966. Letters also sent to other main banking associations	1
1966 Feb 1	Governor's letter to Committee of London Clearing Bankers: until further review, advances, acceptances and commercial bills should not rise above March 1966 levels. Letters also sent to other main banking associations	1
1967 Apr 11	Chancellor's Budget statement: ceiling on lending to private sector would be discontinued forthwith for London and Scottish clearing banks, and discontinued for other banks as and when suitable new arrangements had been worked out	0
1967 Nov 19	Government announced new measures of credit policy: lending at the latest date for which figures are available will become the ceiling until further notice	1
1968 May 23	Bank credit restriction notice: the restrictions requested last November should be modified so as to achieve a greater reduction in lending to non-priority borrowers than has so far taken place. Banks are asked not to allow lending to exceed 104% of the Nov 1967 level (i.e., roughly current level) until further notice	1
1968 Aug 30	Bank credit restriction notice: lending has fallen below the 104% ceiling; current restrictions must continue to be rigorously enforced over the period ahead	1
1968 Nov 22	Bank credit restriction notice: the clearing banks are now asked to reduce lending to 98% of its mid-Nov 1967 level by mid-Mar 1969; other banks are asked to ensure their lending does not exceed 102% of its Nov 1967 level by Mar 1969	1
1969 Jan 31	Deputy Governor's letter to Committee of London Clearing Bankers: little progress has yet been made towards meeting the revised target for private sector lending set last November; we must now re-emphasise the importance to achieving this target	1
1969 May 31	Bank credit restriction notice: latest data show sharp reversal in progress in meeting lending ceiling; Bank has decided to halve the rate of interest payable on special deposits from June 2 until banks comply with the ceiling	1
1969 Dec 31	Bank of England informs the Committee of London Clearing Bankers in private that it would be content to see lending growth	0
1970 Apr 14	Chancellor's Budget statement: a gradual and moderate increase in lending that has hitherto been subject to ceilings would be consistent with economic objectives; bank lending should not rise by more than about 5% over 12 months from Mar 1970	1
1970 Jul 28	Bank credit restriction notice: latest data indicate lending is increasing at a faster pace than April guidance. Clearing and Scottish banks reminded that April guidance remains in force and have been asked to reduce lending growth accordingly	1
1971 Mar 30	Chancellor's Budget statement: intention to put forward proposals to change techniques of monetary control, but for now necessary to maintain guidelines on lending. Clearing and Scottish banks asked that lending should not rise to mid-Jun 1971 by more than 7.5% above its mid-Mar 1970 level	1
1971 Jun 30	Bank notice: proposed changes in monetary policy techniques not yet complete, so necessary to extend the guidelines on lending. Clearers asked that lending in mid-Sep 1971 should not exceed its mid-Mar 1970 level by more than 10%. Similar restraint applied to other banks	1
1971 Sep 16	Quantitative ceilings abandoned as part of Competition and Credit Control reform package	0

Source: Bank of England *Quarterly Bulletin*, Bank of England Archive, and authors' calculations.

It is not obvious how to weight the four controls. We consulted the clearing bank archives to look for evidence on how the banks reacted to them. Interestingly, the banks appear to have reacted to the different controls in a similar way. Following the application of a ceiling in 1955, Westminster told its branch managers that "in general, no new or increased advances can be made unless they are specifically required for the production of exports,

Table 3: *Classifying announcements under the supplementary special deposits scheme*

Date	Base period	Permissible growth, per month	Rate of deposit	Classification
1973 Dec 17	Oct–Dec 1973	8% over first 6 months; 1.5% thereafter	Until Nov 1974: 5% for excess of up to 1pp 25% for excess of 1–3pp 50% for excess of over 3pp From Nov 1974: 5% for excess of up to 3pp 25% for excess of 3–5pp 50% for excess over 5pp	1
1975 Feb 28	—	Scheme terminated	—	0
1976 Nov 18	Aug–Oct 1976	3% over first 6 months; 0.5% thereafter	As from Nov 1974	1
1977 Aug 11	—	Scheme terminated	—	0
1978 Jun 8	Nov 1977–Apr 1978	4% during Aug – Oct 1978; 1% thereafter	As from Nov 1974	1
1980 Jun 18	—	Scheme terminated	—	0

Source: Bank of England *Quarterly Bulletin* and authors' calculations.

for the saving of essential imports, for necessary seasonal commitments or for the essential requirements of the defence programme. In particular, advances should not be made for capital expenditure or for stockpiling or for increasing production of consumer goods".[28] Following the call for special deposits in April 1960, National Provincial instructed its branch managers that "advances which are regarded as analogous to the 'Personal Loans' of our competitors must in future be granted in very exceptional cases" and that applications for advances to fund capital expenditure should be referred elsewhere.[29] In December 1973, Barclays reacted to the "Corset" announcement by informing branch managers that it was prepared to see a rise of 5% in borrowing limits in priority sectors (industry, agriculture, and construction) but wanted to see a 10% reduction in lending to non-priority sectors (banks and other financial companies, property companies, and households).[30]

Hire-purchase controls of course did not directly apply to the banks, but even so, they sometimes amended their lending policies accordingly as for example in the case of Midland Bank in 1960 which told its branch managers that "it is important that attention should be paid to the provisions of the Hire Purchase and Credit Sale Orders in those cases where they would apply had the transaction been arranged through hire-purchase finance instead of by Personal Loan".[31] Interestingly, from the perspective of the bank lending channel literature cited above, we found no evidence of banks reacting to changes in Bank

[28] "Advance Policy", Circular A.54-1955, 3rd August 1955, Royal Bank of Scotland Archives WES/1302/14.
[29] "Advances", Circular No. 1960/10, 29th April 1960, Royal Bank of Scotland Archives NAT/914/10.
[30] "Advances", Head Office Circular No. A.9,955, 21st December 1973, Barclays Archives 29/1603 .
[31] "Personal Loans Service", S.A.126/1960, 28th May 1960, HSBC Archives UK 1493.

Rate other than via interest rates on loans and deposits.[32]

On the basis of this evidence, and in an effort to keep things simple, we give each of the four controls an equal weight in the credit policy index. Specifically, we divide the hire-purchase and special deposit subindices by their mean values when they are on, so that each of the four controls has a mean value of 1 for periods when they were active. Then we sum the four subindices and divide by the resulting series' standard deviation. The resulting unweighted aggregate index is presented in Figure 2b.[33] Of course, this weighting does contain a healthy dose of judgement, so we use different weighting schemes in robustness tests described in Section 6.5.

4.3. Usage and co-movement of monetary and credit policies

One of our concerns, anticipating the econometric analysis to come, is whether monetary and credit policy were mostly announced or implemented together, as a package, or whether they were mostly employed at different times. At low frequency, they were used systematically and were tightened and loosened somewhat in sync. e.g., on an annual basis. At higher frequency, in the monthly data that we study, this was not the case.

To compare monetary and credit policy actions in our sample, Figure 3a shows the time series data for *annual* changes in Bank Rate and *annual* changes in our aggregate credit policy index. Monetary and credit policies were clearly used jointly over the period, but the relationship is far from a one-to-one correspondence even when we look at *annual* changes. However, our empirical analysis will be conducted using monthly data and at this frequency the two policies are seen to be used more independently. The correlation between monthly changes in these variables is much lower still, with a contemporaneous correlation coefficient of 0.3, as seen in Figure 3b.

Table 4 presents a range of sample statistics on the usage of these policy tools. Over the full sample, changes in the credit policy index are evenly distributed between tightenings and loosenings, whereas the distribution of changes in Bank Rate is skewed, with almost twice as many loosenings as tightenings. Bank Rate hikes are typically twice as large as cuts, however. Consistent with Figure 3a, the instruments were often adjusted in the same direction: over the full sample, there were 22 within-month moves in the same direction;

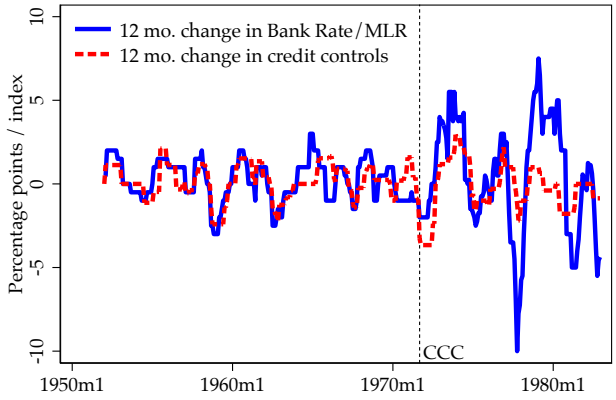
[32] While far from decisive evidence, this supports Romer and Romer (1993)'s claim that the bank lending channel of monetary policy is not quantitatively important.

[33] One argument supporting our approach is that it is one taken by many other authors studying the effects of similar policies. For example, in their comprehensive analysis of contemporaneous macroprudential policies, Cerutti, Claessens and Laeven (2017) calculate a macroprudential policy index as the simple unweighted sum of 12 underlying policies. A similar approach is taken in studies of macroprudential policy by Akinci and Olmstead-Rumsey (2018), Kim and Mehrotra (2022), Kuttner and Shim (2016), and Biljanovska et al. (2023). Another argument is conceptual: in a macroeconomic model, one would expect each of these policies to operate by changing the shadow cost of borrowing, depressing lending, and reducing real economic activity.

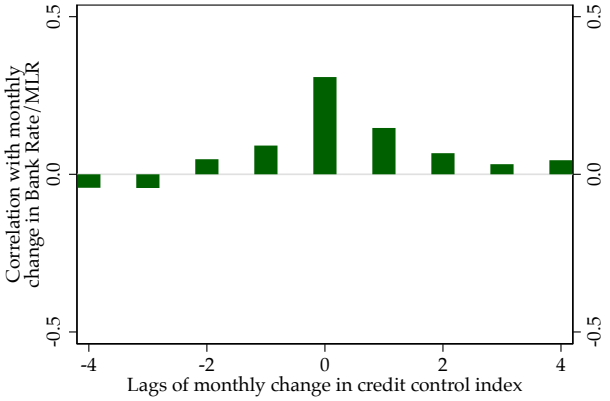
Figure 3: *The co-movement of monetary policy and credit policy*

The upper panel presents twelve-month changes in Bank Rate (blue line) and the credit policy index (red line). The lower panel presents the cross-correlation between the monthly change in Bank Rate/MLR and leads and lags of the month change in the credit policy index.

(a) *Co-movement of Bank Rate/MLR and the aggregate credit policy index*



(b) *Lead-lag relationship between Bank Rate/MLR and the aggregate credit policy index*



Notes: See text. The vertical dashed line marks the 1971 inception of Competition and Credit Control (CCC).

Table 4: *Summary statistics on the usage of monetary and credit policy tools*

Bank Rate	Standard deviation	3.5
	Number of tightenings	38
	Average tightening size	1.4
	Number of loosenings	72
	Average loosening size	0.6
Credit policy index	Standard deviation	1.0
	Number of tightenings	28
	Average tightening size	0.7
	Number of loosenings	28
	Average loosening size	0.7
Both policies	Months with moves in same direction	22 (42)
	Months with moves in opposite direction	3 (6)
	Months when only one of the two moves	116
	Months when neither of the two moves	230

Notes: Authors' calculations. See text. Figures in parentheses indicate the number of months of policy actions which take place within the same calendar quarter, even if the exact month the action takes place does not coincide. Note that there are 371 months in our dataset.

the policies were moved in opposite directions on only 3 occasions. (These corresponding figures are 42 same direction moves and 6 opposite direction moves if we consider instead moves within the same calendar quarter.) However, there were still 116 cases where one policy moved but not the other, indicating little correlation overall, which is essential if we are to separate the impacts of the two policies.

5. Identification Strategy and Data

The primary goal of this paper is to estimate the effects of monetary and credit policies on the macroeconomy and the financial system. Our empirical approach is motivated by three challenges we face in achieving this goal. The first – measuring the stance of credit policy – is discussed in Section 4.2.

This section sets out two further challenges and explains how we overcome them, in so doing introducing *Factor-Augmented Local Projections* (FALPs) into the literature.[34] The FALP approach is the one we use to obtain the baseline estimates in our paper. We will also describe an extension we employ as our principal robustness check, using identification through heteroskedasticity (Rigobon 2003; Rigobon and Sack 2004; Nakamura and Steinsson 2018; Känzig 2021). Finally, in this section we also present our new monthly macrofinancial data set for the UK, which we hope will be used widely by other researchers.

The second challenge is that policy responded at least to some degree to actual or prospective macroeconomic developments. To estimate causal effects, we construct measures of the exogenous components of policy changes. We do so using changes in asset prices on the days of policy actions, following the literature on high-frequency identification (Gertler and Karadi 2015). The asset prices we use are as follows. Following standard practice for monetary policy we use the change in the 1-year gilt yield, around the announcement window. For credit policy, there is no standard so we employ the average percentage change in the equity prices of a basket of banks and finance companies. The justification for this choice is that if a change in credit controls is binding then it should impact the share prices of these companies. When both policies change we control for both.

The third challenge is that monetary and credit policy were quite often used together and also in combination with other macroeconomic policies. As described in Sections 3 and 4, there were 25 occasions when monetary and credit policies were used together;[35] there were also 23 occasions when fiscal policy was used with one or both of them. To reflect this, we control for other policy shocks in our main specification. In principle, this should be sufficient to eliminate any bias. But if we have mismeasured the policies, any correlation between the policy variables may be a source of bias. To check for this, we dummy out months with more than one type of policy action in a further set of robustness tests. Other policies (e.g., balance of payments policies) could also be a source of bias, so we follow the same dummy strategy in another robustness test.

[34] We also note that, even though the papers were written independently, in a similar spirit to our paper the survey chapter by Ramey (2016) (p. 41 and Figure 3.2B) also briefly explores among several identification schemes the idea of combining recursive LPs with FAVAR controls.

[35] 22 in the same direction and 3 in opposite directions from each other.

First stage: constructing policy innovation series In the first stage, we regress the raw policy variable, Bank Rate or our credit policy index (indexed by i), on the relevant one-day market surprises (1-year gilt yield change or bank equity price change, respectively), P lags of factors, P lags of the policy variable itself, and P lag differences of the response variable $\Delta y_{n,t}$ (indexed by n), which appears as a long difference in the 2nd stage below, such that

$$\begin{aligned}\Delta\text{policy}_{i,t} = \quad & \alpha_{n,i} + \beta_{n,i}\Delta\text{market}_{i,t} + \textstyle\sum_{p=1}^{P}\sum_{m=1}^{M}\zeta_{n,i,p,m}F_{m,t-p} \\ & + \textstyle\sum_{p=1}^{P}\theta_{n,i,p}\Delta\text{policy}_{i,t-p} + \sum_{p=1}^{P}\phi_{n,i,p}\Delta y_{n,t-p} + \epsilon_{n,i,t}\,, \end{aligned} \tag{1}$$

where $\text{policy}_{i,t}$ refers to our measures of monetary or credit policy, and $F_{m,t}$ is one of a number of macroeconomic factors indexed by m. Note that all of the controls on the right-hand side of this regression also appear in the second-stage regression below. From this first stage we construct a fitted value $\widehat{\Delta\text{policy}}_{n,i,t}$ which we treat as the identified exogenous policy surprise, which is then used in the second stage described below.

The market surprises are constructed from hand-collected financial market data in newspapers, as described below. The macroeconomic factors are estimated from a large data set, also described below, using principal component analysis. We use information criteria developed by Bai and Ng (2002) to guide our choice of factors. Their first three information criteria suggest at least three factors. We use the first $M = 4$ principal components in our main specification but three and five in robustness exercises. We use $P = 12$ lags as our baseline, with 6 and 18 in robustness exercises.

Second stage: Factor-Augmented Local Projection (FALP) In the second stage, we regress macroeconomic response variables of interest on the policy innovations estimated in the first stage, along with a rich set control variables. Here our LP specification is

$$\begin{aligned}\Delta y_{n,t,t+h} = \quad & \iota_{h,n} + \beta_{h,i,n}\widehat{\Delta\text{policy}}_{n,i,t} + \textstyle\sum_{p=1}^{P}\sum_{m=1}^{M}\zeta_{h,n,p,m}F_{m,t-p} \\ & + \textstyle\sum_{p=1}^{P}\theta_{h,n,i,p}\Delta\text{policy}_{i,t-p} + \sum_{p=1}^{P}\phi_{h,n,p}\Delta y_{n,t-p} \\ & + \textstyle\sum_{j\neq i}\xi_{h,n,j}\Delta\text{market}_{j,t} + \psi_{h,n,i}\Delta\text{fiscal}_t + \eta_{n,t,t+h}\,, \end{aligned} \tag{2}$$

where $\Delta y_{n,t,t+h}$ is the change in a response variable (indexed by n) between t and $t+h$, and $\beta_{h,i,n}$ is the LP impulse-response coefficient of interest (the $\beta_{h,i,n}$ coefficients are indexed by horizon h, impulse variable i, and outcome variable n).

Identification relies on a timing assumption that policy can affect outcomes in the same month, but within-month macroeconomic conditions do not affect the policy surprises. One potential concern is that, in response to policy announcements market participants made inferences about the current state of the economy (the information effect). We do not

believe this was the case in the UK over this period, because we tested and confirmed that market surprises were not significantly correlated with forecasts made by the Treasury in the same month as the announcement.[36]

The other controls consist of, as before, P lags of factors, P lags of the policy variable itself, and P lag differences of the response variable $\Delta y_{n,t}$ (indexed by n), plus, in addition, a control for the contemporaneous change in the other policy monetary/credit policy variable and a control for the contemporaneous change in fiscal policy discussed in Section 5.1. Standard errors are calculated using the HAC estimator in the baseline results (Olea and Plagborg-Møller 2021), and using the bootstrap in the robustness appendix.

5.1. Data

Constructing a high quality data set was essential to implementing our identification strategy. This took considerable effort, and roughly two thirds of our 94 factor variables were copied by hand, at least in part, from archival copies of statistical publications or newspapers. Beyond this paper, we hope other researchers can use these data in future.

Policy variables Bank Rate and its replacement, Minimum Lending Rate (MLR), are sourced from the Bank of England website. The sources and construction of the credit policy index are described in Section 4. The fiscal policy variable is the well-known narrative fiscal policy shock measure constructed by Cloyne (2013).

Macro and financial variables used in responses and factors We have also constructed a new monthly macroeconomic data set for the UK from 1951 to 1982. This 94 variable data set was deployed using principal components analysis to estimate the factors used as additional controls in the local projections. The data span most of the key series which policymakers followed then and now, although because the data set is monthly it is somewhat light on measures of demand and output, many of which are only produced at quarterly frequency. Table A1 in Appendix A lists the full data set used to construct the factors in the FALPs, and describes the transformations we performed before estimating the factors. Series were seasonally adjusted when necessary using Census X13.

The series in the monthly data set were pulled together from a wide variety of intermediate sources, although a high proportion of the series was originally published by the Central Statistics Office (CSO) or the Bank of England. The most frequently used secondary sources were Thomas and Dimsdale (2017) and Refinitive Datastream.[37]

[36] See our earlier working paper for information on the collection of these forecasts.

[37] Other secondary sources were Capie and Webber (1985), Denman and McDonald (1996), Huang and Thomas (2016), O'Donoghue, McDonnell and Placek (2006), and Reinhart and Rogoff (2004).

High-frequency financial market data To facilitate our high-frequency identification of shocks we need appropriate historical UK financial market price data from the 1950s to the 1980s. Understandably, this is quite a tall order in a period so long ago. Intraday data do not exist at all. And daily frequency data, at a minimum, would require of us hand collection for the most part. Nonetheless, we set out to achieve this goal.

To gauge the surprise impact of monetary policy surprises we sought the percentage point change in the 1-year gilt yield around policy announcements. This required two considerable steps. First, no pre-collated daily gilt yield data existed at all in our sample, and even in newspaper sources no constant-maturity 1-year gilt yield series were published at that time. Consequently, we had to first obtain an estimate of the end-of-day yield curves from raw bond coupon, price, and maturity data; we take this from the Ph.D. dissertation of Bush (2024), where data were hand collected from original sources in the Financial Times (and occasionally other newspapers when the FT was on strike and unavailable). Based on the fitted curves we then calculated and collated the change in yield at the 1-year maturity point on all monetary policy dates in our sample period (the "treatment" subsample of months). We also collected a parallel monthly sample of yield changes at the same 1-year maturity point on a set of non-policy dates (the first trading day of the month, the "control" subsample of months) for use in our main robustness exercise with the method of identification through heteroskedasticity, described in the next subsection (see, e.g. Känzig 2021). With this in hand we can then obtain a new time-series of identified monetary policy shocks for our sample period in the manner of Rigobon and Sack (2004).

To gauge the surprise impact of credit policy surprises we collected per cent changes (sign-reversed) in the end-of-day stock prices of banks and finance companies, as we reason that these would be the financial market prices that would be most strongly affected by binding changes in credit policy. For this exercise, at the start of our period, given the lack of other data, we had no option other than hand-collecting quotes of six public equities: the "Big 5" banks, consisting of Barclays, Lloyds, Midland, National Provincial, and Westminster, and the largest hire-purchase house, United Dominions Trust. We equally weighted the six share prices (which shrank to just five following the 1968 merger of National Provincial Bank and Westminster Bank). Here again we relied on the same newspaper sources as above for the pre-CCC sample. However, after 1971, in the post-CCC period we extended this series forward using the daily FTSE Banks index, which exists from 1969 onwards, and which provides acceptable continuity as it includes the "Big 4" banks and UDT. We also again collected a parallel monthly sample of price changes on a set of non-policy dates for identification through heteroskedasticity.

Finally, a note on timing. The Bank's archival records indicate that not all monetary and credit policies were announced early enough in the day to impact the end-of-day market

quotes. Consequently, in our baseline results we rely on the market reports in the Financial Times to learn when the news hit, and we use the following-day change when this is indicated by the narrative. In our pre-CCC baseline sample, there were 7 credit policy dates (out of 37) when this was the case, and 2 monetary policy dates (out of 40). However, in the case of credit policy there were also 4 days when the market reports were uninformative, and we simply dropped those policy dates from our baseline. As a robustness check, we then repeated the estimation using all raw policy dates, dropping none, and ignoring the FT market report by assuming that all news hit the market on the announcement day.

Main robustness exercise: identification through heteroskedasticity One concern with our baseline FALP specification is that the one-day window used to construct market surprises may be so wide as to include market reactions to other news or events, in addition to the policy action itself. As noted, we cannot turn to intra-day data, but a solution to this problem proposed in the literature is to employ identification through heteroskedasticity (see, e.g. Nakamura and Steinsson 2018). So for our main robustness exercise, we employ this method, specifically as in, e.g., Rigobon and Sack (2004), using the implementation with instrumental variables (IV).

In this approach the factor-augmented local projection we estimate using LP-IV is

$$\begin{aligned}\Delta y_{n,t,t+h} = \; & \iota_{h,n} + \beta_{h,i,n} x_{i,t} + \textstyle\sum_{p=1}^{P} \sum_{m=1}^{M} \zeta_{h,n,p,m} F_{m,t-p} \\ & + \textstyle\sum_{p=1}^{P} \theta_{h,n,i,p} \Delta \text{policy}_{i,t-p} + \sum_{p=1}^{P} \phi_{h,n,p} \Delta y_{n,t-p} + \eta_{n,t,t+h} \,, \end{aligned} \tag{3}$$

applied to treatment T and control C subsamples, where $\Delta y_{n,t,t+h}$ is again the change in a response variable, and $\beta_{h,i,n}$ is again the response coefficient of interest (cf. Känzig 2021).

The key independent variable is defined as $x_{i,t} = T_{i,t}\Delta\text{market}_{i,t} + (1 - T_{i,t})\Delta\text{market}_{i,t}$ which is instrumented using the variable $z_{i,t} = T_{i,t}\Delta\text{market}_{i,t} - (1 - T_{i,t})\Delta\text{market}_{i,t}$ where $T_{i,t}$ is an indicator equal to 1 when the month t is in the treatment T subsample of size N_T (i.e., a policy action i occurs in that month), and $(1 - T_{i,t})$ is an indicator for when the month t is in the control C subsample of size N_C (other months).

Four further comments. First, in our case the treatment and control subsamples are of unequal size so the regression is estimated with observation weights $\omega_{i,t} = T_{i,t}N_T^{-1/2} + (1 - T_{i,t})N_C^{-1/2}$. Second, in a few cases there is more than one policy action in a given month, and in that case we only use the market surprise associated with the first action in the month. Third, in regression (3) we no longer control for other contemporaneous policies since in the Rigobon method these can be treated as simply another source of background noise. Fourth, identification relies on a higher variance of price changes in the treatment sample versus the control sample; in the next section, we show that this condition holds.

6. Main Results

In this section, we describe the main results of the paper. We first present the monetary and credit policy surprises that we have constructed, as described above, and we examine the own responses of the variables. We then move onto the main results, the responses of lending, real activity, and inflation to the monetary and credit policy surprises. We present two sets of results in the main text: our baseline results using FALP and our main alternative robustness exercise using identification through heteroskedasticity. We also summarise here in the main text the results of a battery of robustness tests including those described above. A Supplementary Appendix presents full results from these exercises.

6.1. Monetary and credit policy surprises

In Section 5.1, we described how we constructed two monetary and credit policy innovations series, using a first-stage regression with the market surprise $\Delta\text{market}_{i,t}$. The top row of Figure 4 shows these surprises, the 1-day changes in the 1-year yield and bank equity prices on days of monetary and credit policy actions, respectively.

The first point to make is that the shocks are big in absolute terms, when we have in mind a comparison with more modern examples of similarly constructed policy surprises. The second point is that the market surprises explain a large share in the low-horizon variation in the "own" response of Bank Rate and the credit policy index. In other words there were large exogenous components of monetary and credit policies in these surprises on policy announcement days.

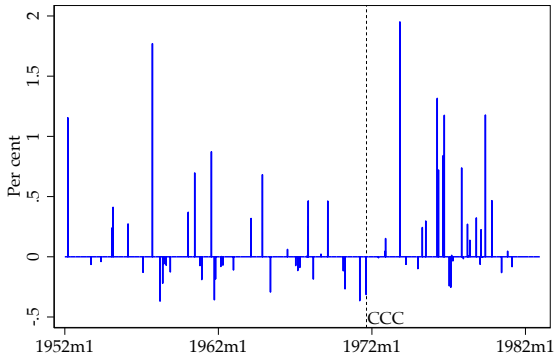
At first sight, this might be surprising: we have noted in our historical narrative that these policies were used systematically, e.g., in response to balance of payments concerns. Even so, our narrative also demonstrated that the idiosyncratic and political nature of the authorities' decisions about the individual instruments used in any given event may explain why market participants were often surprised by movements in both Bank Rate and individual credit policy tools.

Finally, as can be seen in the second row of the figure, the variation in asset prices on monetary and credit policy days is large relative to the variation on non-policy days. This is most clearly the case in our baseline sample, for the pre-CCC period, since after that point the background noise grows, as we noted earlier. In the pre-CCC daily sample, the standard deviation of one-day percentage point changes in 1-year gilt yields was 0.44 on monetary policy (MP) days and 0.04 on non-MP days. The standard deviation of one-day percentage changes in bank stock prices was 1.37 on credit policy (CP) days and 0.76 on non-CP days.

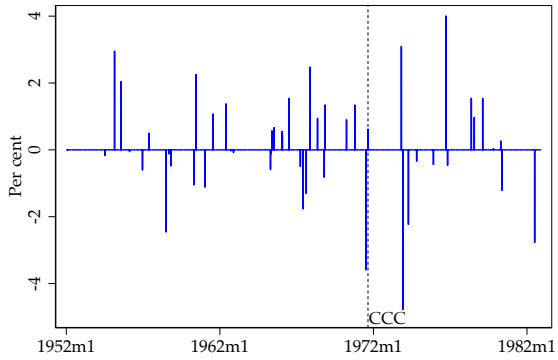
Figure 4: *Monetary and credit policy surprises*

The first two rows presents daily changes in one year gilt rates (monetary policy surprises) and bank stock prices (credit policy surprises) on policy and non-policy days. The third row presents the density estimates, using an Epanechnikov kernel, of 1-day changes in treatment and control day subsamples (one per month).

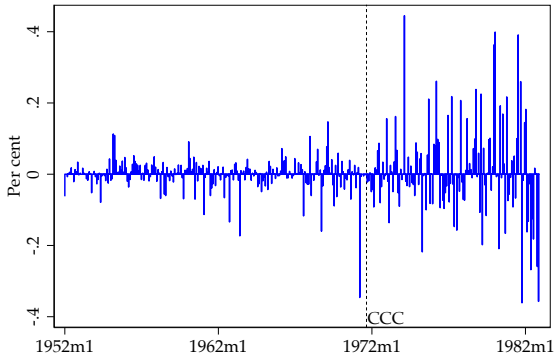
(a) *Monetary policy surprises on MP days, full sample*



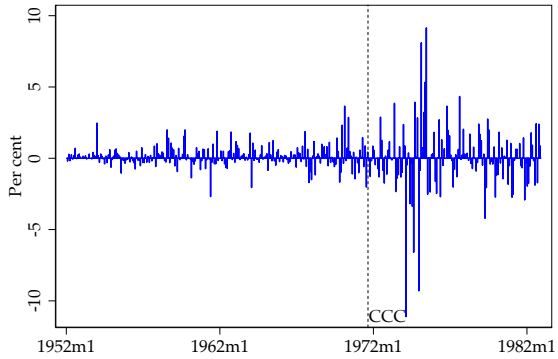
(b) *Credit policy surprises on CP days, full sample*



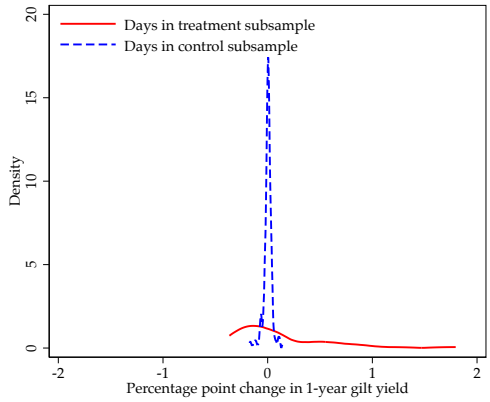
(c) *Gilt rate changes on non-MP days, full sample*



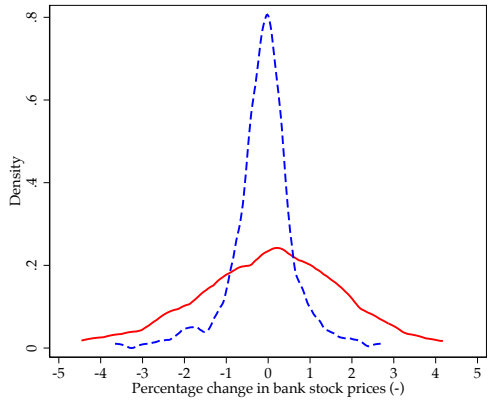
(d) *Bank stock price changes on non-CP days, full sample*



(e) *Monetary policy surprises, kernel density plots, baseline sample (pre-CCC)*



(f) *Credit policy surprises, kernel density plots, baseline sample (pre-CCC)*



Notes: See text. The vertical dashed line marks the 1971 inception of Competition and Credit Control (CCC).

The differences in these two distributions for what we have called the treatment and control subsamples of months is most clearly visible in the final row of Figure 4, which shows kernel density estimates for the pre-CCC sample. Two observations follow from this. First, the amount of background noise in the market surprises we use in the baseline FALP specification does not appear to be large. Second, the substantial differences in the variances in the treatment and control samples means that we do not suffer from weak instrument problems when we use the Rigobon method of identification through heteroskedasticity.

6.2. Policy own responses and cross responses

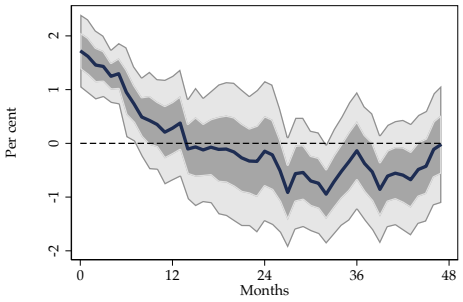
Given the estimated policy surprises that we have just constructed, we first look at the response of monetary and credit policy measures themselves to these surprises, as shown in Figure 5, which displays LP estimates of impulse responses from the baseline FALP regression (1).

Figure 5: *Responses of Bank Rate and the credit policy index to monetary and credit policy surprises*

The bold line shows the mean estimated response of each variable to contractionary monetary policy (first column) and credit policy (second column) surprises. The dark shaded region shows the ± 1 s.e. HAC confidence interval; the light shaded region shows the ± 2 s.e. HAC confidence interval.

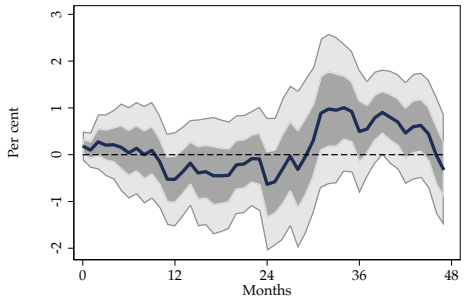
Responses to monetary policy:

(a) *Impulse: monetary policy, Response: Bank Rate*

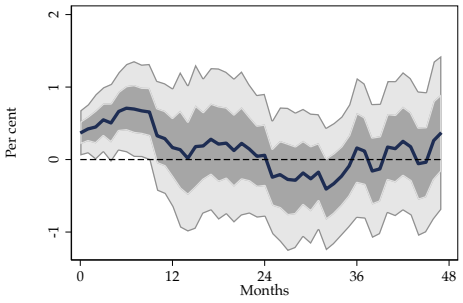


Responses to credit policy:

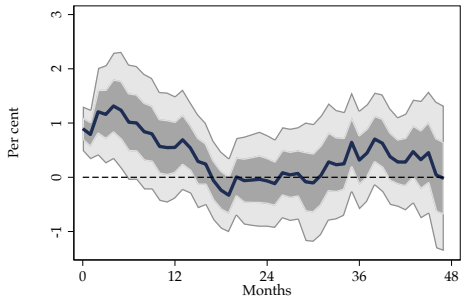
(b) *Impulse: credit policy, Response: Bank Rate*



(c) *Impulse: monetary policy, Response: credit index*



(d) *Impulse: credit policy, Response: credit index*



Notes: The monetary policy surprise is a one percentage point rise in the one year gilt rate. The credit policy surprise is a three per cent fall in bank stock prices. See text for further details.

In all the impulse response functions that follow, we scale the size of the monetary policy shock to be equal to a 1 percentage point increase in the one year gilt yield. In order to make the monetary and credit policy results more comparable, we scale the credit policy shock to be equal to a (minus) 3 percent change in bank stock prices. This scaling seems appropriate because is ensures that, on treatment days, each scaled policy surprise has roughly the same variance in our baseline pre-CCC sample.

Figure 6 shows that, as expected, monetary and credit policy surprises raised Bank Rate and the credit policy index respectively on impact. The contemporaneous impact of the monetary policy surprise was a 1.9 percentage increase in Bank Rate. The response to the credit policy surprise was a 1 unit increase in the credit policy index. Both surprises caused "own responses" that persisted for about one year.

The top right subfigure shows that credit policy surprises had no impact on Bank Rate and the top left subfigure shows that monetary policy surprises caused the credit policy index to rise by about half a unit. These "cross-responses" are useful for interpreting the impulse responses that follow: the responses to monetary policy surprises may partly reflect movements in credit policy, while the responses to credit policy surprises are unlikely to reflect movements in monetary policy.

6.3. The macroeconomic impact of monetary and credit policy surprises

We now look at the response of key macroeconomic variable to our policy surprises, as shown in Figure 6, which again displays LP estimates of impulse responses from the baseline FALP regression (1).

The left column of Figure 6 presents our FALP-estimated impulse responses of key macroeconomic variables following a one-unit monetary policy surprise as described above.

We estimate that there was a modest and short-lived decline in bank lending in response. Manufacturing output also fell a little, 1 to 2 per cent, but returning to its original level within four years. Retail sales are very volatile but also show a modest decline. The consumer price level responded more slowly; prices were broadly unchanged for 10 months, and then began to decline persistently thereafter.

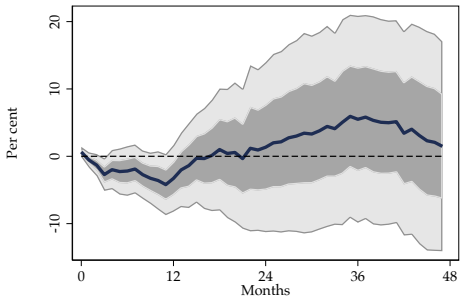
The results are broadly in line with the empirical literature on the effects of monetary policy. After adjusting for the standard deviation, the estimated responses for output and inflation are qualitatively similar but have somewhat smaller impacts than the estimates by Cloyne and Hürtgen (2016) for Britain in the period 1975 to 2007. The shape of our IRFs for output and inflation are very similar to those in the U.S. literature such as in Bernanke, Boivin and Eliasz (2005) and Romer and Romer (2004), but the peak impacts are somewhat smaller in magnitude.

Figure 6: *Responses of key variables to monetary and credit policy surprises, FALP*

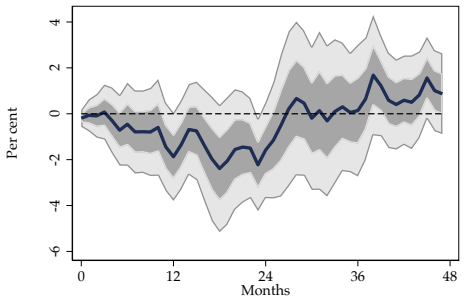
The bold line shows the mean estimated response of each variable to contractionary monetary policy (first column) and credit policy (second column) surprises. The dark shaded region shows the ± 1 s.e. HAC confidence interval; the light shaded region shows the ± 2 s.e. HAC confidence interval.

Responses to monetary policy:

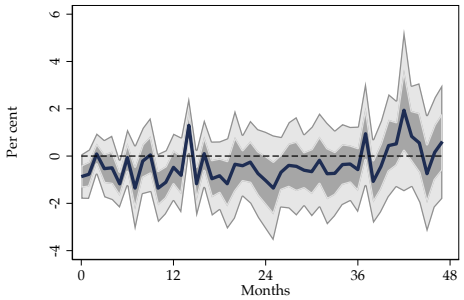
(a) *Impulse: monetary policy, Response: lending*



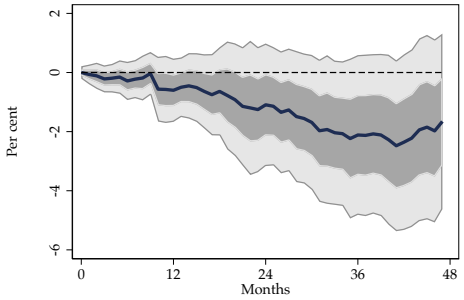
(c) *Impulse: monetary policy, Response: mfg. output*



(e) *Impulse: monetary policy, Response: retail sales*

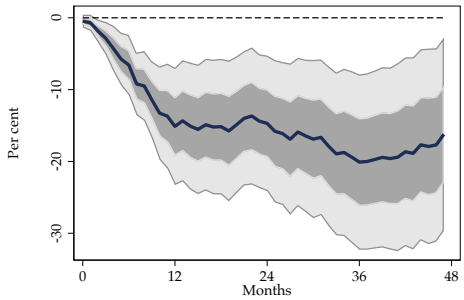


(g) *Impulse: monetary policy, Response: CPIH*

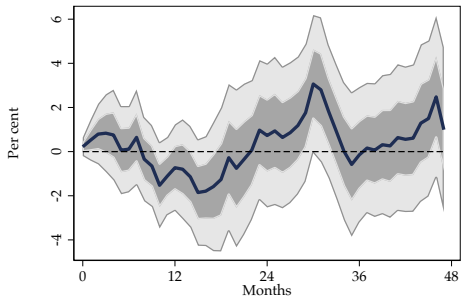


Responses to credit policy:

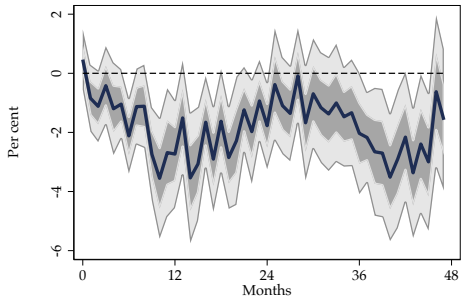
(b) *Impulse: credit policy, Response: lending*



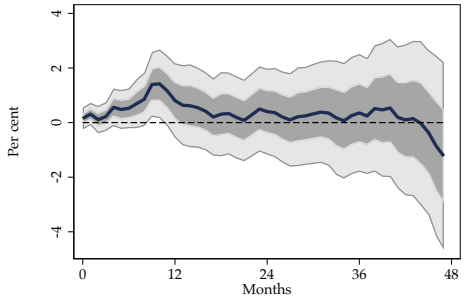
(d) *Impulse: credit policy, Response: mfg. output*



(f) *Impulse: credit policy, Response: retail sales*



(h) *Impulse: credit policy, Response: CPIH*



Notes: The monetary policy surprise is a one percentage point rise in the one year gilt rate. The credit policy surprise is a three per cent fall in bank stock prices. See text for further details.

We interpret our results as running contrary to the Radcliffe Committee's doubts about the efficacy of conventional monetary policy: our findings indicate that, even in that distant era, monetary policy did transmit weakly to output and quite strongly to prices. Moreover, the causal impacts we identify are similar to the consensus impacts seen in the contemporary macro literature.

The right column of Figure 6 presents our FALP-estimated impulse responses of key macroeconomic variables following a one-unit credit policy surprise as described above.

In response, bank lending is estimated to have declined persistently and considerably, falling by around 15 per cent after a year. Manufacturing output is estimated to have changed very little. However we do find evidence of a stronger decline in another measure of real activity, retail sales. And there was no discernible impact on consumer prices. In section 6.6 below we explain why credit policy may have had these particular effects.

The numerous robustness exercises presented in the Supplementary Appendix do not give us cause to doubt our qualitative results, but we discuss these below in section 6.5.

6.4. Robustness: Identification through heteroskedasticity

Figure 7 shows the same impulse responses, but this time using identification through heteroskedasticity. Our central findings – that monetary policy had a larger negative impact on consumer prices and credit policy had a larger negative impact on lending – remain.

Despite the high signal to noise ratio in our policy surprise measures, there are some noticeable differences in some of the responses. The most striking change for monetary policy is that the consumer price response is much larger in size and peak impacts on real activity have also roughly doubled in size. Somewhat puzzlingly, the lending and real activity responses rebound into (slightly) positive territory after around two years. The short-term response of lending to credit policy surprises is of a similar size, but it is now estimated to be less persistent. And, puzzlingly, manufacturing output is estimated to rise significantly between the one and three year horizons.

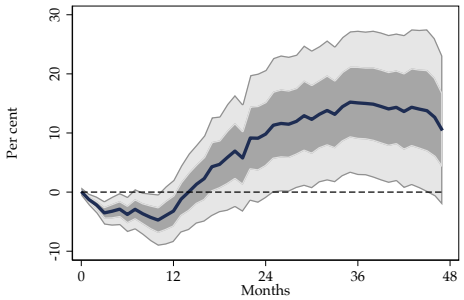
Comparing these results to the baseline FALP results sheds some light on potential biases. It is unsurprising that the estimated impacts of monetary policy on real activity and consumer prices are larger using identification through heteroskedasticity. Movements in market yields not connected to monetary policy are likely to reflect good news about the economy which in turn is correlated with higher future output and prices. In contrast, the sign of any bias in the credit policy is unclear, *a priori*. One might expect both policy and non-policy news which lowers bank stock prices also to presage lower lending and possibly activity. The sign of any bias is a function of the relative impacts and the relative variances of policy and non-policy shocks.

Figure 7: *Responses of key variables to monetary and credit policy surprises, identified via heteroskedasticity*

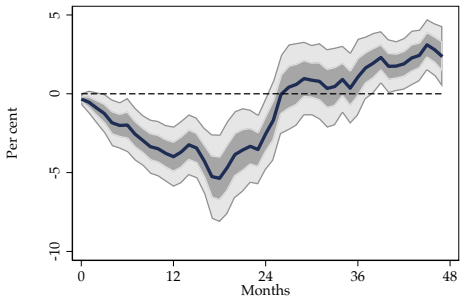
The bold line shows the mean estimated response of each variable to contractionary monetary policy (first column) and credit policy (second column) surprises. The dark shaded region shows the ± 1 s.e. HAC confidence interval; the light shaded region shows the ± 2 s.e. HAC confidence interval.

Responses to monetary policy:

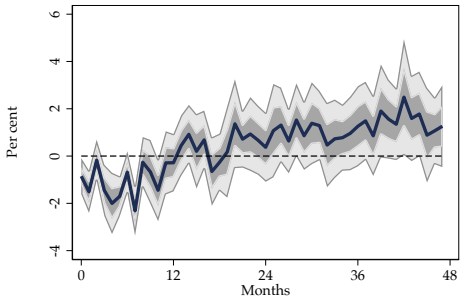
(a) *Impulse: monetary policy, Response: lending*



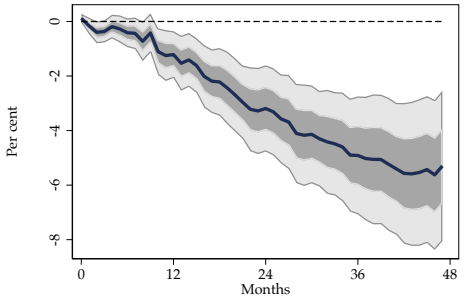
(c) *Impulse: monetary policy, Response: mfg. output*



(e) *Impulse: monetary policy, Response: retail sales*

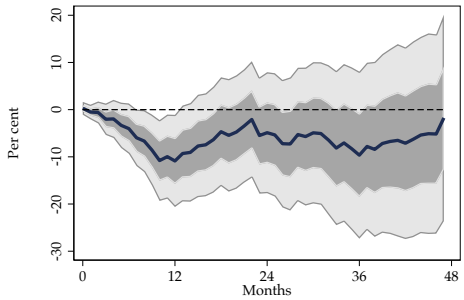


(g) *Impulse: monetary policy, Response: CPIH*

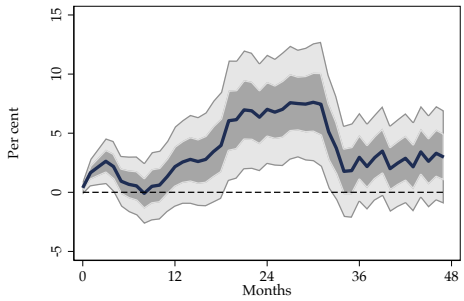


Responses to credit policy:

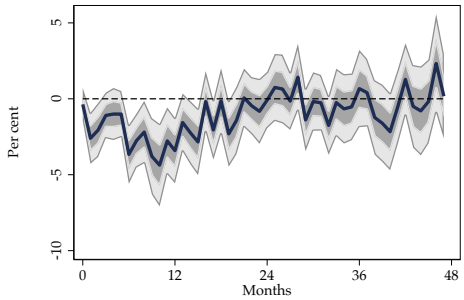
(b) *Impulse: credit policy, Response: lending*



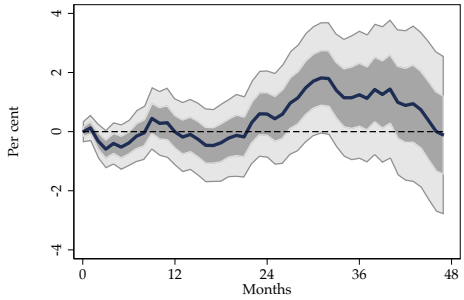
(d) *Impulse: credit policy, Response: mfg. output*



(f) *Impulse: credit policy, Response: retail sales*



(h) *Impulse: credit policy, Response: CPIH*



Notes: The monetary policy surprise is a one percentage point rise in the one year gilt rate. The credit policy surprise is a three per cent fall in bank stock prices. See text for further details.

6.5. Robustness: Other checks

We conduct a very large set of other robustness checks for our baseline FALP estimation, but these results are too large and cumbersome to present in the main text, so the 12 variations are shown in the robustness appendix, in Appendix B.

There, a sequence of exhibits, Figures B1 to B10, shows robustness exercises for each of ten impulse responses where the LPs are calculated under the following alternative samples or specifications, where the appendix contains exact details. First we present the baseline (FALP) and then the Rigobon (FALP with identification through heteroskedasticity), as described above, to provide a reference. All the remaining 12 robustness exercises then show variations built around the baseline (FALP).

In the first exercise we extend the sample to the full period when controls were used, 1952 to 1982, i.e., including the post-CCC period, but without the highly volatile 1973–75 window. The next three exercises vary the definition of credit policy, by excluding certain types of policies one at a time: we first drop hire-purchase regulations, then we drop credit ceilings, then we drop special deposits. The next three exercises concern timing. First, we disallow our use of the FT market report to fix the time when news hit the market, instead using just the policy announcement date itself. Then we disallow events with policy packages consisting of combinations of monetary/credit/fiscal policy events. Then we disallow events with policy packages consisting of combinations of balance of payment alongside either monetary or credit policy events. The next four robustness checks relate to the saturation of the specification. We first change from the baseline of 4 factors, to 3 factors and then 5 factors. Then, we change from the baseline of 12 lags, to 6 lags and then 18 lags. Finally, the twelfth and last figure shows the baseline with bootstrap confidence intervals.

Most importantly, across all of these many exercises we find that our central results are robust. In all variations, the estimated responses show that contractionary monetary policy shocks depressed consumer prices in the medium term. In all variations but one (when we drop ceilings from the credit policy index), the results show that tighter credit policy failed to reduce consumer prices. Meanwhile, the impact of credit policy on the lending to GDP ratio is robust across all variations and in only one of the variations did tighter monetary policy have a significantly negative impact. Interestingly, the positive response of manufacturing output to tighter credit policy varies considerably across the robustness exercises, while the negative impact on retail sales is noticeably more robust.

Finally, Figure B11 shows the forecast error variance decompositions for our baseline using the method of Gorodnichenko and Lee (2020). The figure shows that where we find significant responses in the LPs above we also generally find a substantial contribution of identified shocks to the variation of forecast errors at different horizons

6.6. Discussion of the responses to credit policy

The main results above, and the main robustness exercise, produced fairly standard estimated responses to monetary policy. However, the estimated responses to credit policy are more novel, and there is less literature to compare them to, so we spend some time discussing the plausibility of these estimates.

As we explained in the narrative discussion earlier, the authorities often accompanied credit tightening policy actions with instructions, or moral suasion, to focus reductions in lending on the household sector and to protect credit to export and import-competing sector. Furthermore, we also presented evidence that banks acted on these instructions. It is therefore unsurprising to see that retail sales declined in response to tighter credit policy. Furthermore, contemporaneous accounts would support the idea of a sizable impact on retail sales. For example, the Retail Distributors Association evidence to the Radcliffe Report stated that "undoubtedly when initial payments [on purchases financed by hire purchase] are raised and the periods of repayment shortened there is an immediate fall in hire purchase sales".[38] In 1969, the *Times* reported a 38 per cent fall in hire purchase sales of vehicles between November and December 1968, concluding that the falls were "a clear indication that the Government's emergency November hire purchase restrictions are having their intended effect" (Smith 1969).

The response of manufacturing output was less clear. In particular, we find that contractionary credit policy surprises had either no effect on manufacturing output (our baseline), or led it to increase with a lag (our heteroskedasticity-based identification). This may, we conjecture, have reflected the impact of instructions to banks that lending to export-focused or import-competing sectors should be protected. There is at least anecdotal evidence that some firms were directly affected by credit controls, as opposed to indirectly via a demand channel. This is a topic for further research.

Working capital problems were clearly affecting some firms in both 1955–56 and 1969–71. A letter to the *Financial Times* complained about the credit ceiling: "Can you imagine anything more absurdly futile? I asked my bank manager whether I was expected to pay my wages and various supplies with bits of straw and un-threshed grain while I waited to sell my corn many weeks if not months after the 31st December next?" (Hopton 1955). By 1969, the Confederation of British Industry was regularly asking manufacturers whether credit or finance was limiting output. Over the period 1969–71, an average of 8.9 per cent responded that output had been limited this way. This compares to a survey average of 4.3 per cent and an all-time peak of 9.4 per cent in the three years to July 2011, including the most acute phase of the Global Financial Crisis.

[38] Committee on the Working of the Monetary System (1960), page 141.

This array of evidence suggests that credit controls affected both aggregate demand and the potential supply capacity of the economy. The aggregate demand effects operated via lower consumer and fixed investment spending. The supply effects operated via a reduced capacity to finance working capital needs and, over time, a lower capital stock. These effects could explain our finding of no significant response of consumer prices if these demand- and supply-side effects were broadly offsetting.

7. Implications for Monetary and Macroprudential Policies Today

Even though credit policies may not have been used successfully around the time of the Radcliffe Report, policymakers today might be able to learn from the experience. Central banks are now grappling with the challenges of a new mission, one that may require them to use unusual and controversial macroprudential tools alongside interest rate policy. There is an active debate about the efficacy of macroprudential tools and the appropriate role for them alongside monetary policy in securing financial stability.[39]

Some of the macroprudential tools under consideration today are quite similar to the tools discussed in this paper. For example, liquidity regulations are similar to special deposits, while product tools such as loan-to-value regulations are similar to hire-purchase controls. And just as today, these tools were used alongside the policy rate, although today policymakers are seeking to achieve a different objective with macroprudential policy – namely, financial stability.

If macroprudential policies today work as credit controls did in the Radcliffe era, then the different macroeconomic impacts we estimate could be a useful guide for policymakers. First, we could conclude that lending can be controlled without sacrificing control of inflation. Second, macroprudential policy may give policymakers more immediate control over bank lending, compared to standard monetary policy.

At this juncture, there is uncertainty about how to measure systemic risk and hence the efficacy of macroprudential policy in achieving its main objective. Nevertheless, many studies have found that systemic banking crises are routinely preceded by deteriorations in non-financial private sector balance sheets (e.g., Borio and Lowe (2002); Schularick and Taylor (2012)). This evidence has been influential in policy circles: the Riksbank decided to tighten monetary policy because of concerns about the rising ratio of credit to GDP.

Given all this, we investigate the effects of both monetary policy and credit policy on the popular bank lending to GDP systemic risk proxy. We use this as our final response variable of interest in our estimation frameworks, and in the robustness checks. We find that, whereas the ratio of bank lending to GDP is estimated to decline persistently, considerably

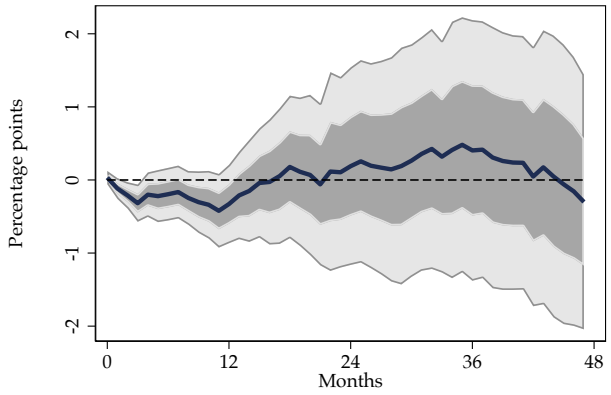
[39] E.g., Stein (2013); Williams (2014); Korinek and Simsek (2016); Svensson (2017); Aikman et al. (2018).

Figure 8: *Impact of policies on a financial stability risk indicator*

The bold line shows the mean estimated response of each variable to contractionary monetary policy (first column) and credit policy (second column) surprises. The dark shaded region shows the ± 1 s.e. HAC confidence interval; the light shaded region shows the ± 2 s.e. HAC confidence interval.

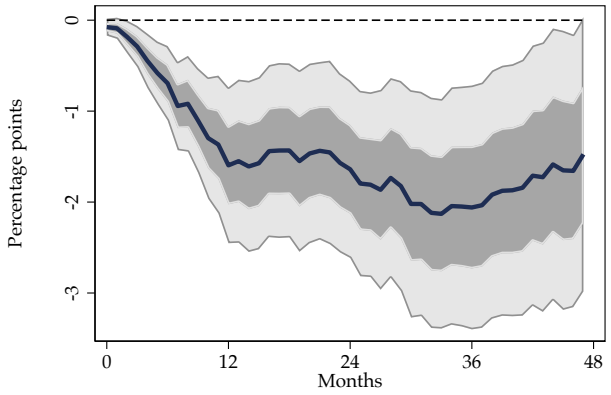
Responses to monetary policy:

(a) *Impulse: monetary policy, Response: lending to GDP*



Responses to credit policy:

(b) *Impulse: credit policy, Response: lending to GDP*



Notes: The monetary policy surprise is a one percentage point rise in the one year gilt rate. The credit policy surprise is a three per cent fall in bank stock prices. See text for further details.

and robustly following a credit policy tightening, the estimated response to a Bank Rate innovation is of marginal statistical significance in Figure 8.

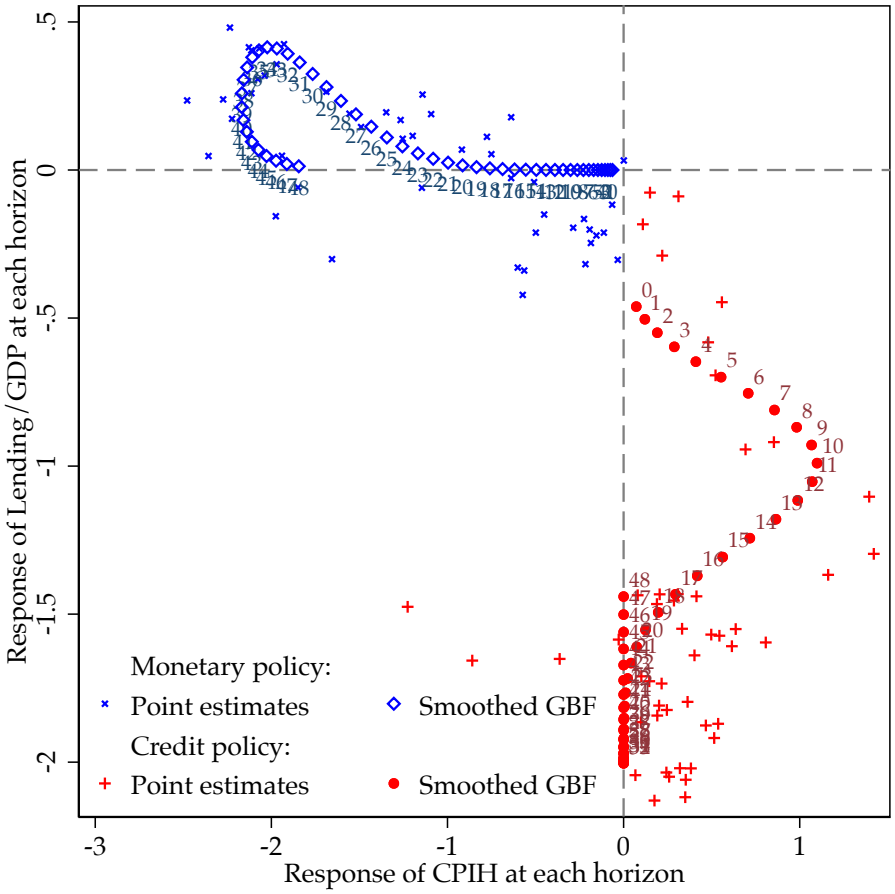
Although these results are subject to the Lucas Critique given credit policies were not used to limit systemic risk in the period we study, the findings suggest that credit policies may be better suited to curbing increases in the credit to GDP ratio than monetary policy. If smoothing fluctuations in the credit to GDP ratio is beneficial to financial stability, this points to directing macroprudential policy in the first instance at achieving financial stability aims, leaving monetary policy to control inflation.

A diagnostic tool How could policymakers evaluate the tradeoffs between monetary and credit policies revealed in our results? The many impulse responses shown as part of the baseline and robustness check results may make this harder to see, so we want to end with what we see as a useful diagnostic response, which can be applied to our results, as well as to other empirical work for different experiments in other times and places.

Ultimately, the authorities are usually concerned about how well they can achieve their financial stability goal, by deflecting the path of credit creation, which as we have just noted is typically gauged by the counterfactual path of the credit to GDP ratio under the policy P, versus the potential unwelcome side effects in the form of deflecting the counterfactual path of prices under the policy P and, hence, a miss on their inflation target.

Figure 9: *Diagnostic for the policy tradeoff: credit impact relative to inflation impact*

...



Notes: See text.

Figure 9 displays our diagnostic, for the two policies in the UK's Radcliffe Era, monetary policy and credit policy. For first MP, and then for CP, we scatterplot at each horizon the local projections estimates of price level and credit to GDP responses, on the horizontal and vertical axes, respectively. Since the point estimates are noisy, we focus on more reliable smoothed LP responses which employ dimension reduction using the established method of Gaussian basis functions (Barnichon and Matthes 2018).

The diagnostic clearly illustrates the disjoint impacts of the two policies, monetary policy and credit policy. Monetary policy acts mainly along the horizontal axis, modulating the price level, but having limited impact on lending to GDP. Credit policy acts mainly along the vertical axis, modulating lending to GDP, but having limited impact on the price level. The two policies, despite the Radcliffe view, were complements not substitutes.

8. Conclusion

In this paper, we use a novel econometric technique and a new data set to study the effects of monetary and credit control policies on macroeconomic and financial outcomes in the United Kingdom between the 1950s and early 1980s.

Increases in Bank Rate had robust negative effects on consumer prices, but moderated lending relatively little. By contrast, credit controls – liquidity requirements on banks, credit growth limits, and constraints on the terms of consumer finance – had a strong negative impact on lending but relatively little effect on inflation.

Overall, our estimates suggest that monetary and credit policies spanned different outcome spaces during this period. This result supports the notion that today's macroprudential tools, which are close cousins of the credit policies studied in this paper, might provide additional independent instruments to help central banks mitigate painful tradeoffs and better meet both their monetary and financial stability objectives.

Our impulse responses indicate that credit policies had moderating effects on a key modern-day indicator of financial system vulnerabilities, while the effects of monetary policy actions were less clear cut. The results therefore provide some support to the view that macroprudential policy is better suited to achieving financial stability goals than monetary policy.

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APPENDIX

A. Monthly Dataset

Table A1 shows the 94 variables in our monthly data set. Variables were seasonally adjusted and transformed where appropriate. The second column below shows the transformation, with '1' indicating no transformation and '2' indicating that the log difference was taken.

Table A1: *Variables used in estimating factors*

Domestic demand and output		
1	2	Index of production
2	2	Mining and quarrying output
3	2	Manufacturing output
4	2	Food, drink and tobacco output
5	2	Textiles output
6	2	Wood and paper output
7	2	Coke and petroleum output
8	2	Chemicals output
9	2	Pharmaceuticals output
10	2	Rubber and plastics output
11	2	Metals output
12	2	Computer and electronic output
13	2	Electrical equipment output
14	2	Machinery output
15	2	Transport equipment output
16	2	Other manufacturing output
17	2	Electricity and gas output
18	2	Water output
19	1	Number of days lost to industrial stoppages
20	2	Retail sales values
21	2	Retail sales values - food
22	2	Retail sales values - clothing
23	2	Retail sales values - durables
24	2	Retail sales values - other
25	2	Number of new vehicle registrations
26	2	Number of dwellings completed
Labour market		
27	2	Manufacturing employment
28	1	Unemployment rate
Balance of payments		
29	2	Goods export value
30	2	Goods import value
31	2	FX reserves
32	1	IMF lending to UK
33	1	Other official lending to UK
Financial markets		
34	2	£ per $
35	2	FF per $
36	2	DM per $
37	2	£ per $ (black market)
38	1	Dollar forward margin
39	2	Stock prices
40	2	Bank stock prices
41	1	Dividend yield
42	1	Treasury bill yield
43	1	20 year gilt yield
44	1	Consol yield
45	1	Corporate bond spread
46	1	Call rate (low)
47	1	Call rate (high)
Financial markets (cont.)		
48	1	Bank deposit rate
49	1	Trade bill rate
50	1	Bank bill rate
51	1	Mortgage rate
52	2	Gold price
53	2	Oil price
54	2	Commodity prices
Money, credit and banking		
55	2	BoE notes in circulation
56	2	Bank deposits at BoE
57	2	LCB deposits
58	2	LCB retail deposits
59	2	LCB advances
Prices		
60	2	Producer input prices
61	2	Producer output prices
62	2	Consumer prices (CPI)
63	2	CPIH: CPI plus owner occupiers' housing (OOH)
64	2	Consumer prices - food
65	2	Consumer prices - alcohol
66	2	Consumer prices - clothing
67	2	Consumer prices - housing exc. OOH
68	2	Consumer prices - housing inc. OOH
69	2	Consumer prices - furniture
70	2	Consumer prices - health
71	2	Consumer prices - transport
72	2	Consumer prices - communication
73	2	Consumer prices - recreation
74	2	Consumer prices - restaurants
75	2	Consumer prices - misc
76	2	Import prices
77	2	Export prices
78	2	Economy-wide average hourly wages
79	2	Manufacturing average hourly wages
80	2	Economy-wide average weekly wages
81	2	Manufacturing average weekly wages
Foreign variables		
82	2	US industrial production
83	2	US consumer prices
84	1	Federal Funds Rate
85	1	US Treasury bill yield
86	1	US long-term bond yield
87	2	German industrial production
88	2	German consumer prices
89	1	German Lombard rate
90	1	German overnight market rate
91	1	German ten year rate
92	2	French industrial production
93	2	French consumer prices
94	1	French short-term interest rate

B. Robustness

The following Figures B1 to B10 show robustness exercises for each of ten impulse responses.

The order of the figures is as follows:

- lending response to monetary policy impulse
- manufacturing output response to monetary policy impulse
- retail sales to monetary policy impulse
- CPIH response to monetary policy impulse
- lending to GDP response to monetary policy impulse
- lending response to credit policy impulse
- manufacturing output response to credit policy impulse
- retail sales to credit policy impulse
- CPIH response to credit policy impulse
- lending to GDP response to credit policy impulse

Within each figure the order of robustness checks is, by column, as follows, and details are provided in the main text:

- baseline (FALP)
- Rigobon (FALP with identification through heteroskedasticity)
- full sample
- HP regs dropped
- ceilings dropped
- special deposits dropped
- alternative credit policy dating (do not use FT market report)
- no MCF packages (exclude dates when more than one of monetary/credit/fiscal policy are changed)
- no balance of payment packages (exclude dates when BOP and either monetary/credit policy are changed)
- 3 factors
- 5 factors
- 6 lags
- 18 lags
- Bootstrap CIs

Finally, Figure B11 shows the forecast error variance decompositions.

Figure B1: *Robustness: lending response to monetary policy impulse*

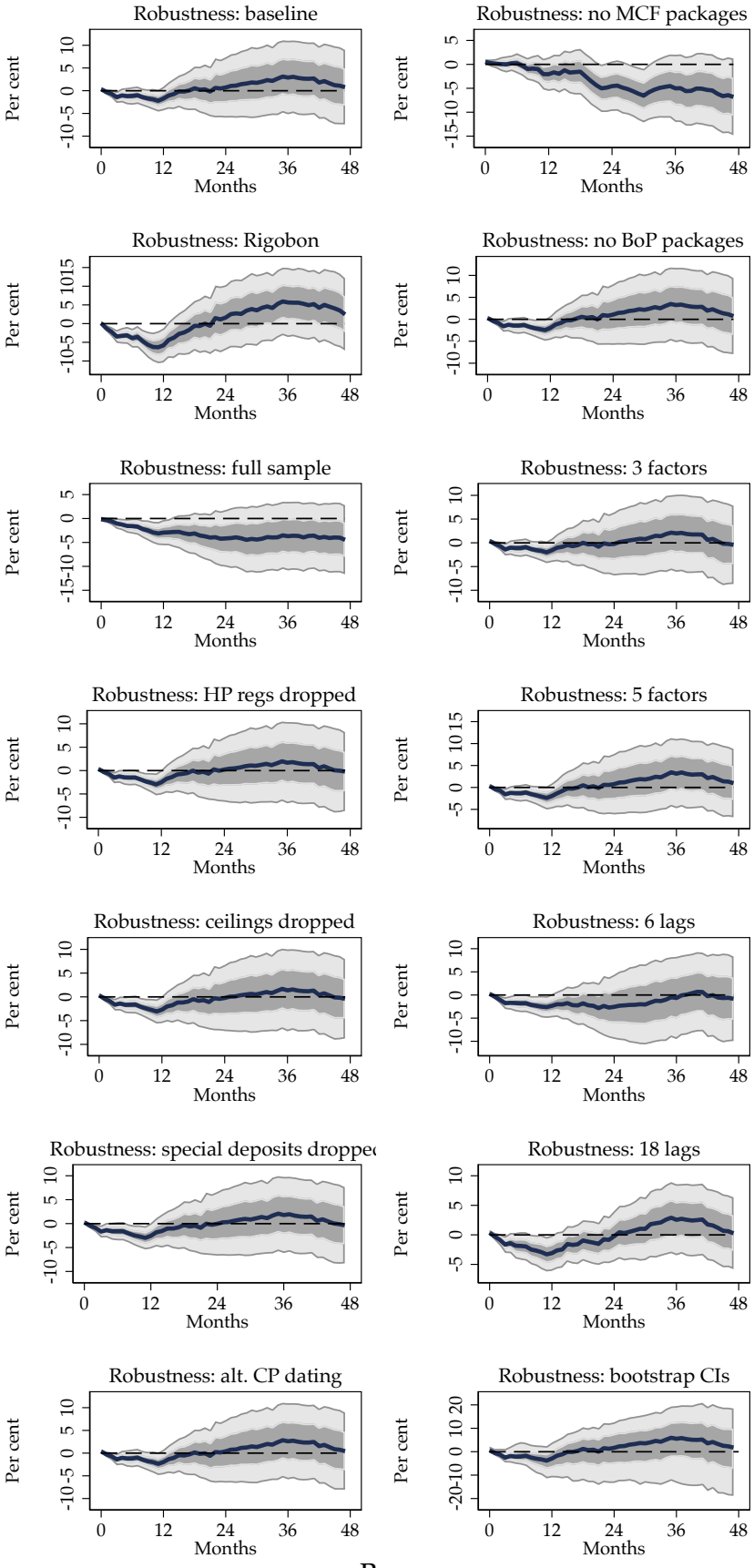


Figure B2: *Robustness: manufacturing output response to monetary policy impulse*

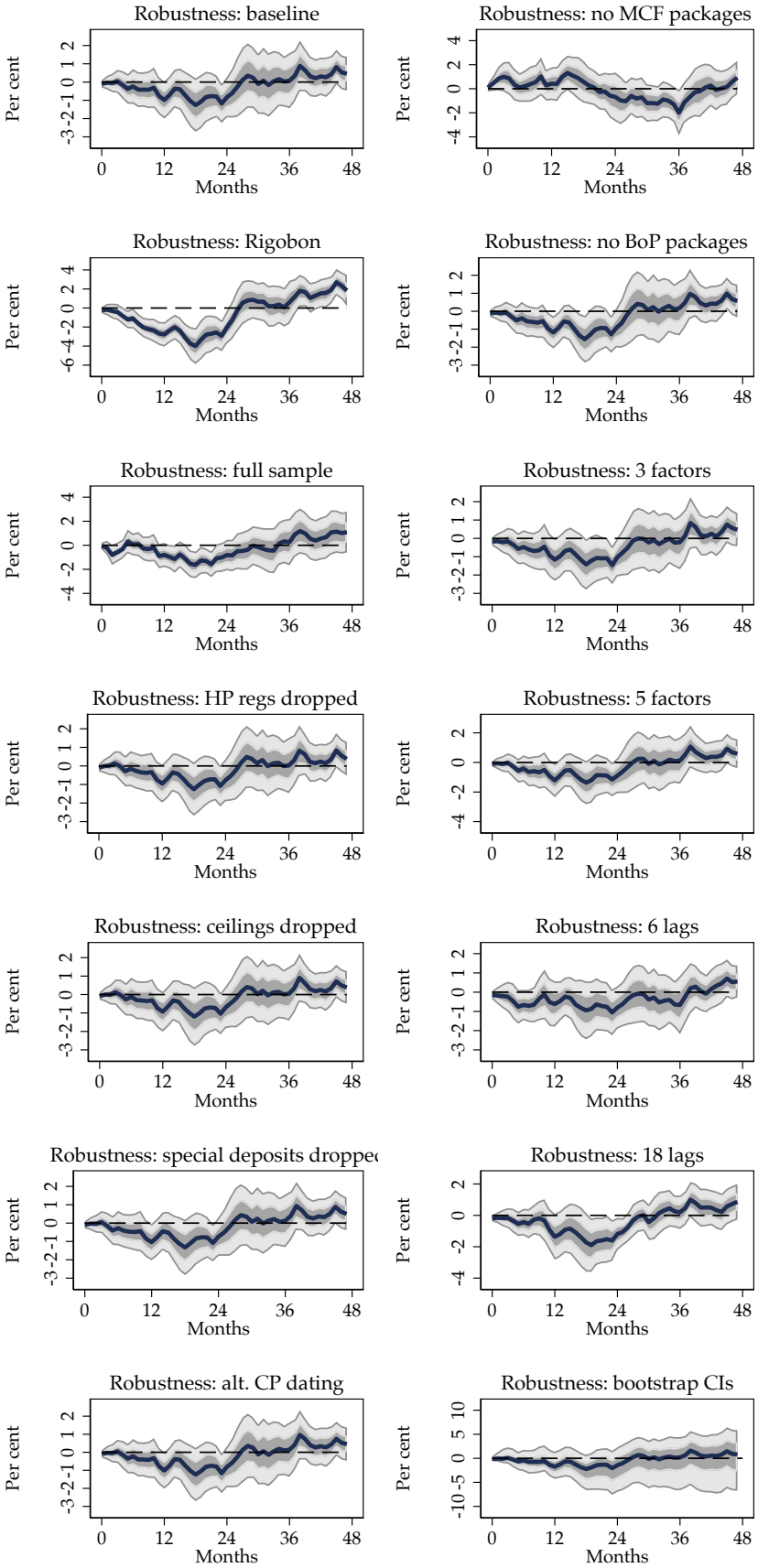


Figure B3: *Robustness: retail sales response to monetary policy impulse*

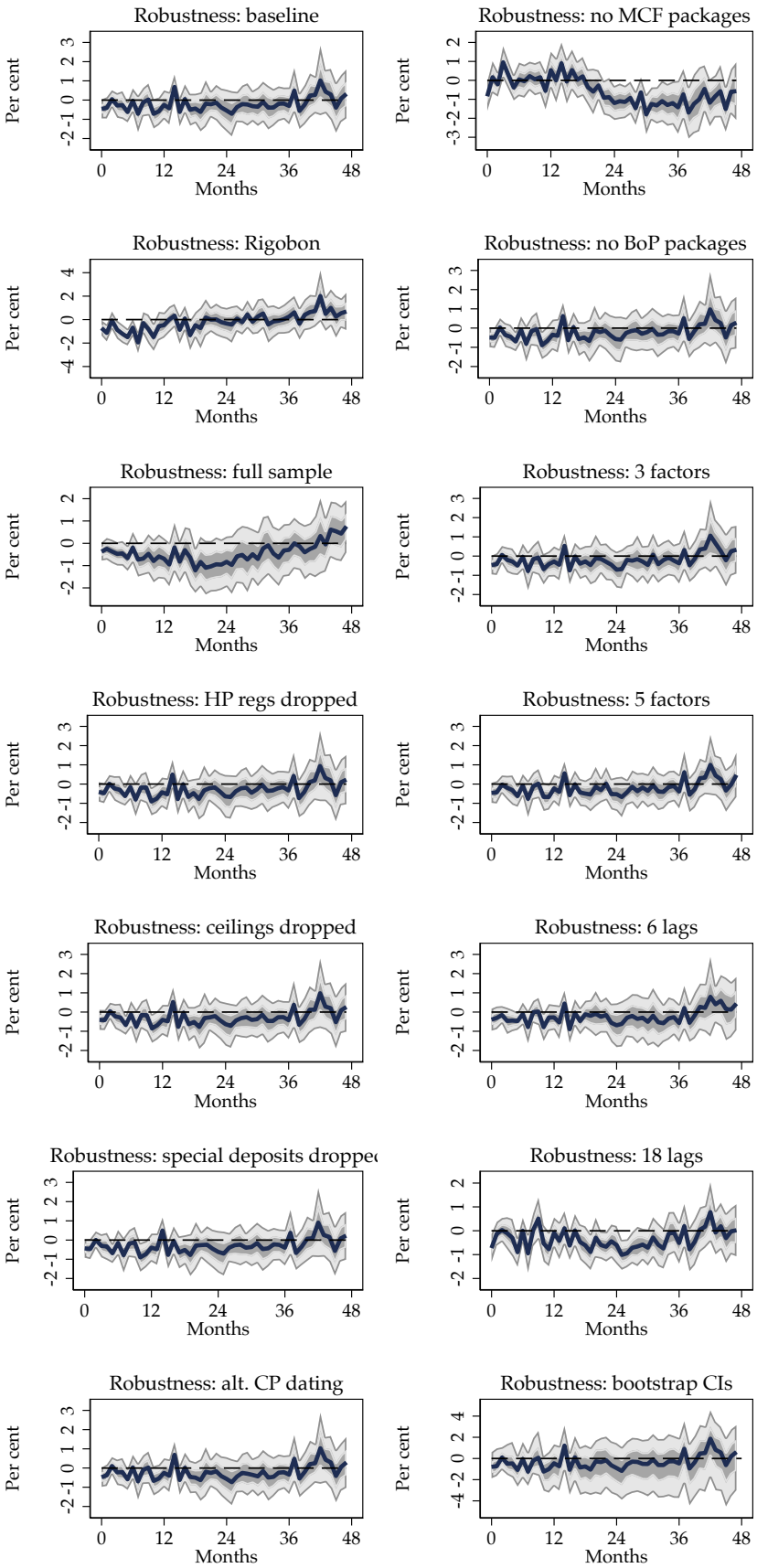


Figure B4: *Robustness: CPIH response to monetary policy impulse*

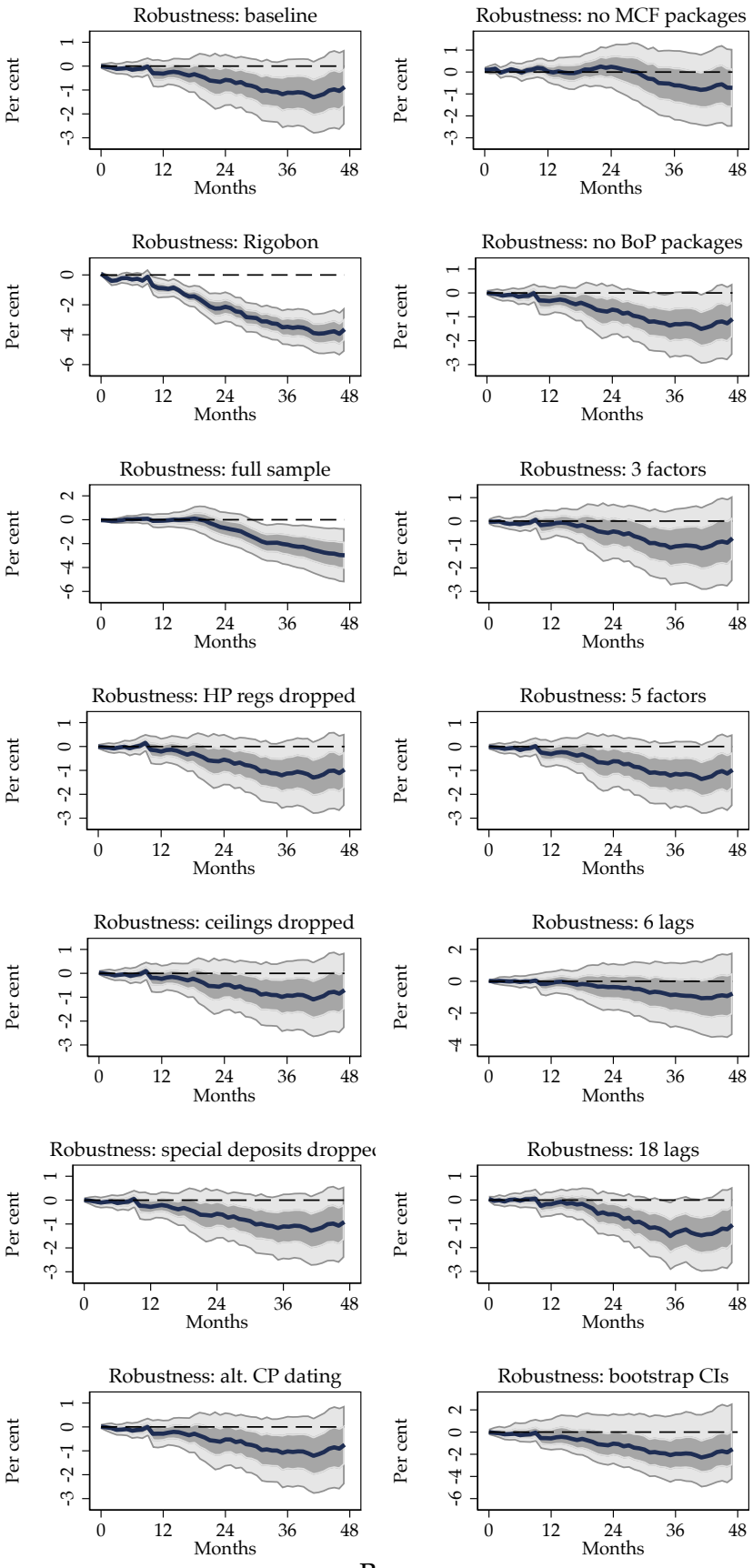


Figure B5: *Robustness: lending to GDP response to monetary policy impulse*

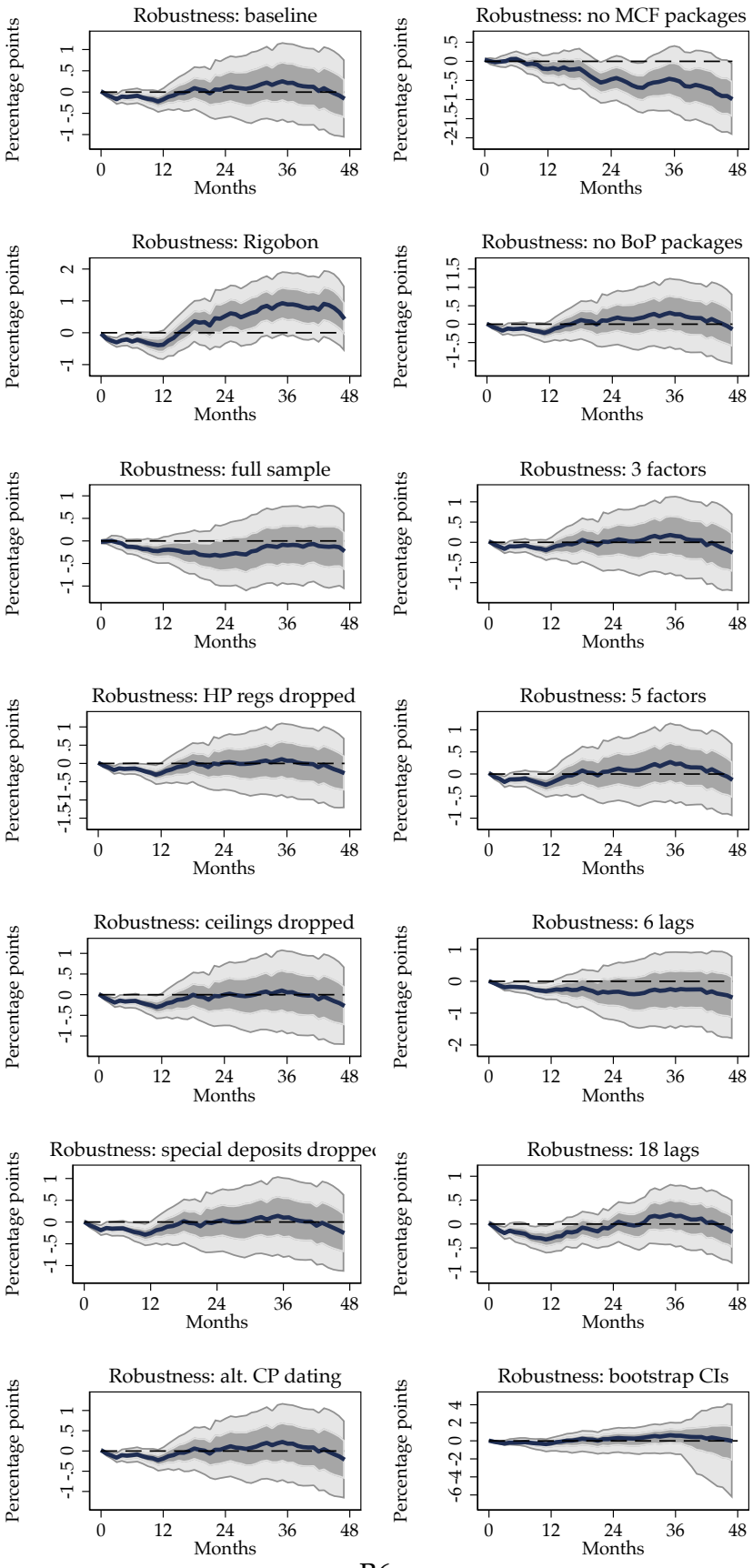


Figure B6: *Robustness: lending response to credit policy impulse*

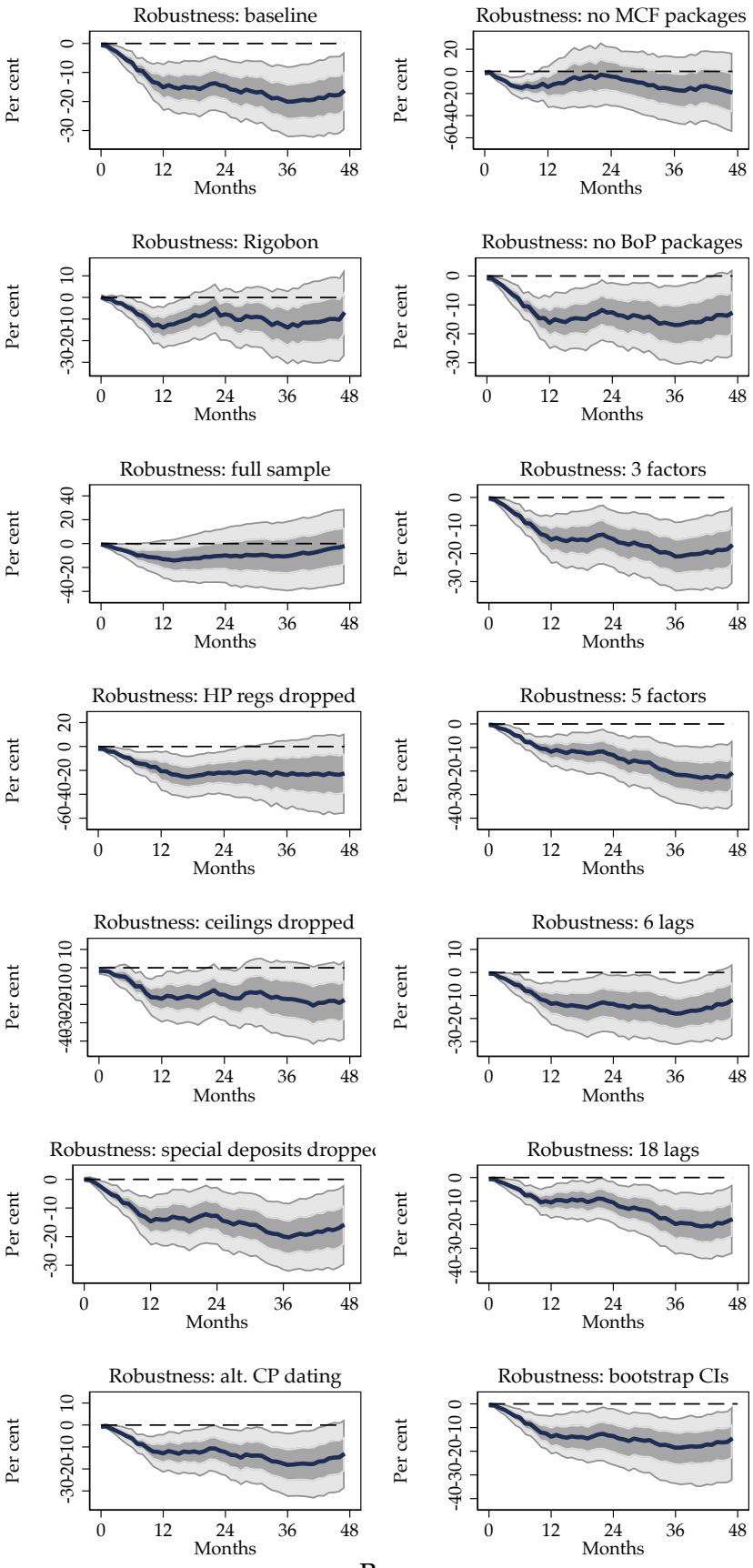


Figure B7: *Robustness: manufacturing output response to credit policy impulse*

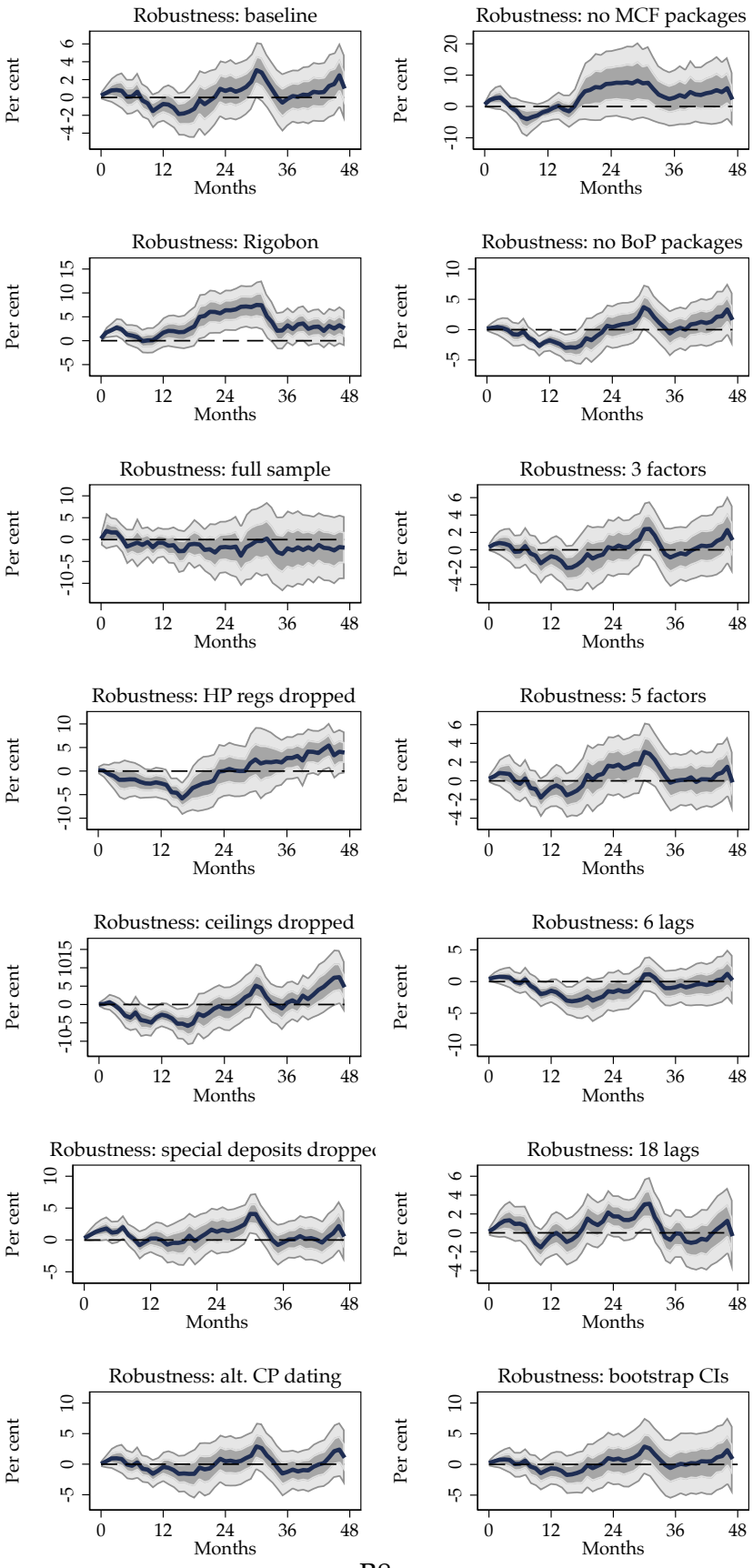


Figure B8: *Robustness: retail sales response to credit policy impulse*

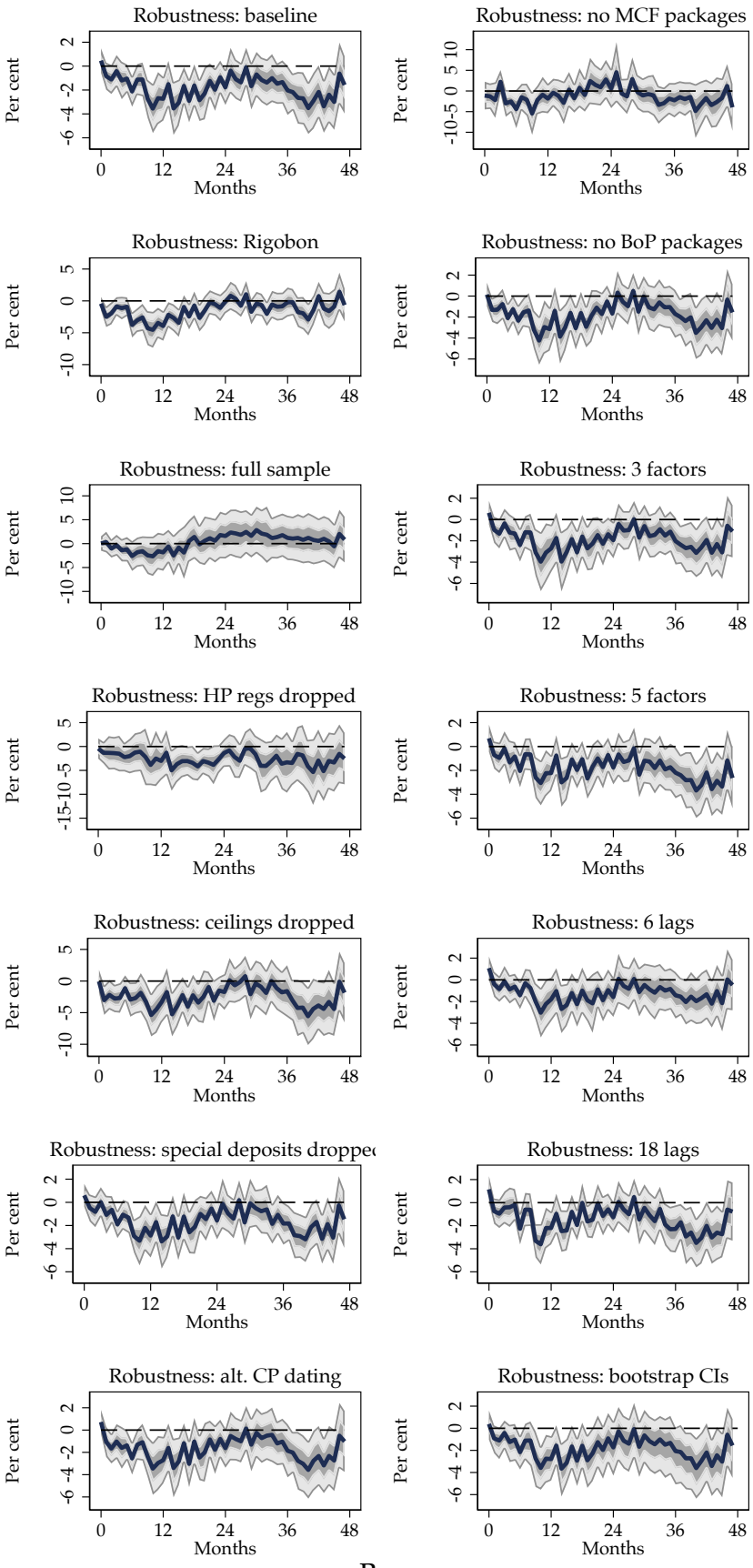


Figure B9: *Robustness: CPIH response to credit policy impulse*

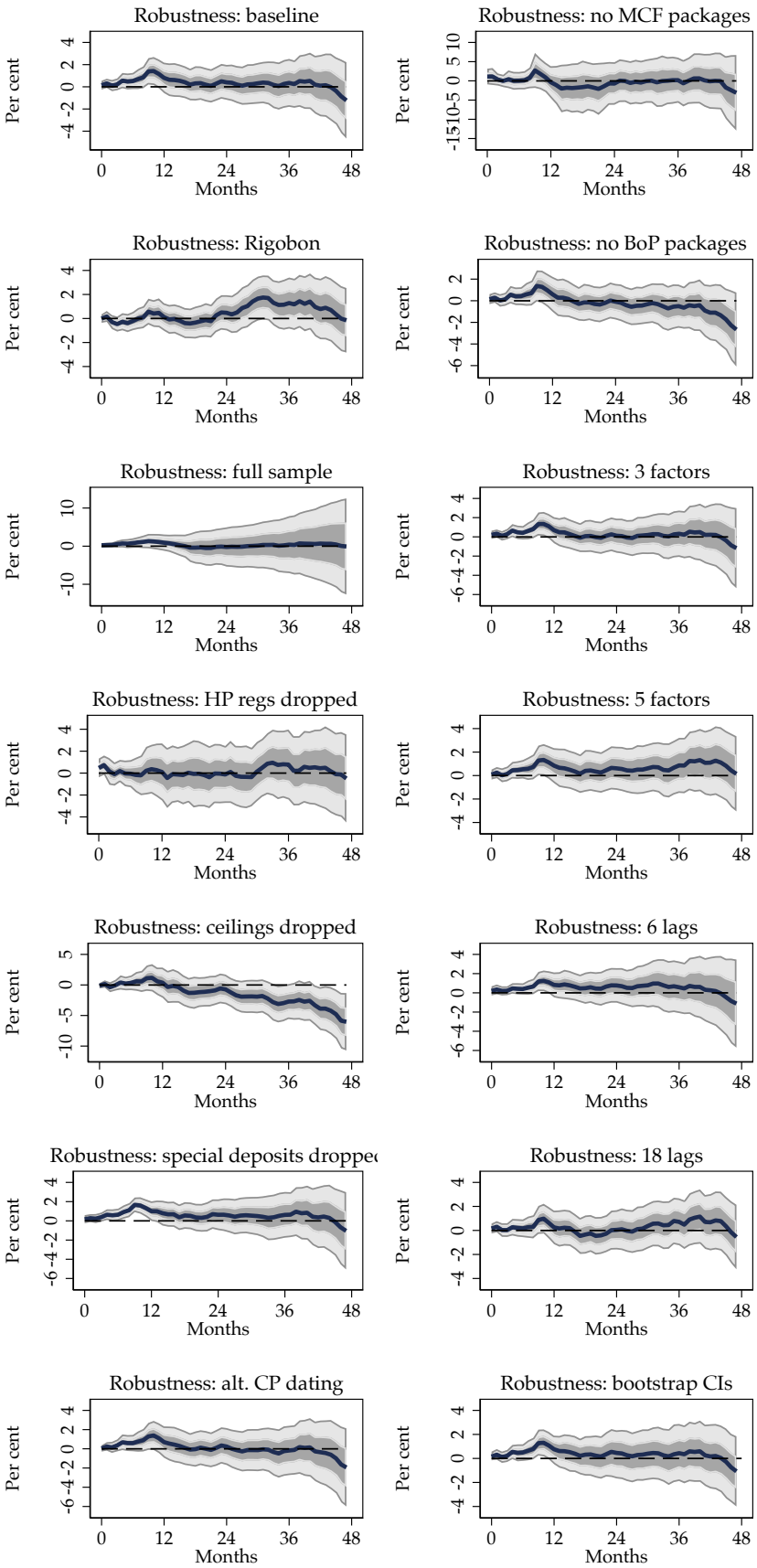


Figure B10: *Robustness: lending to GDP response to credit policy impulse*

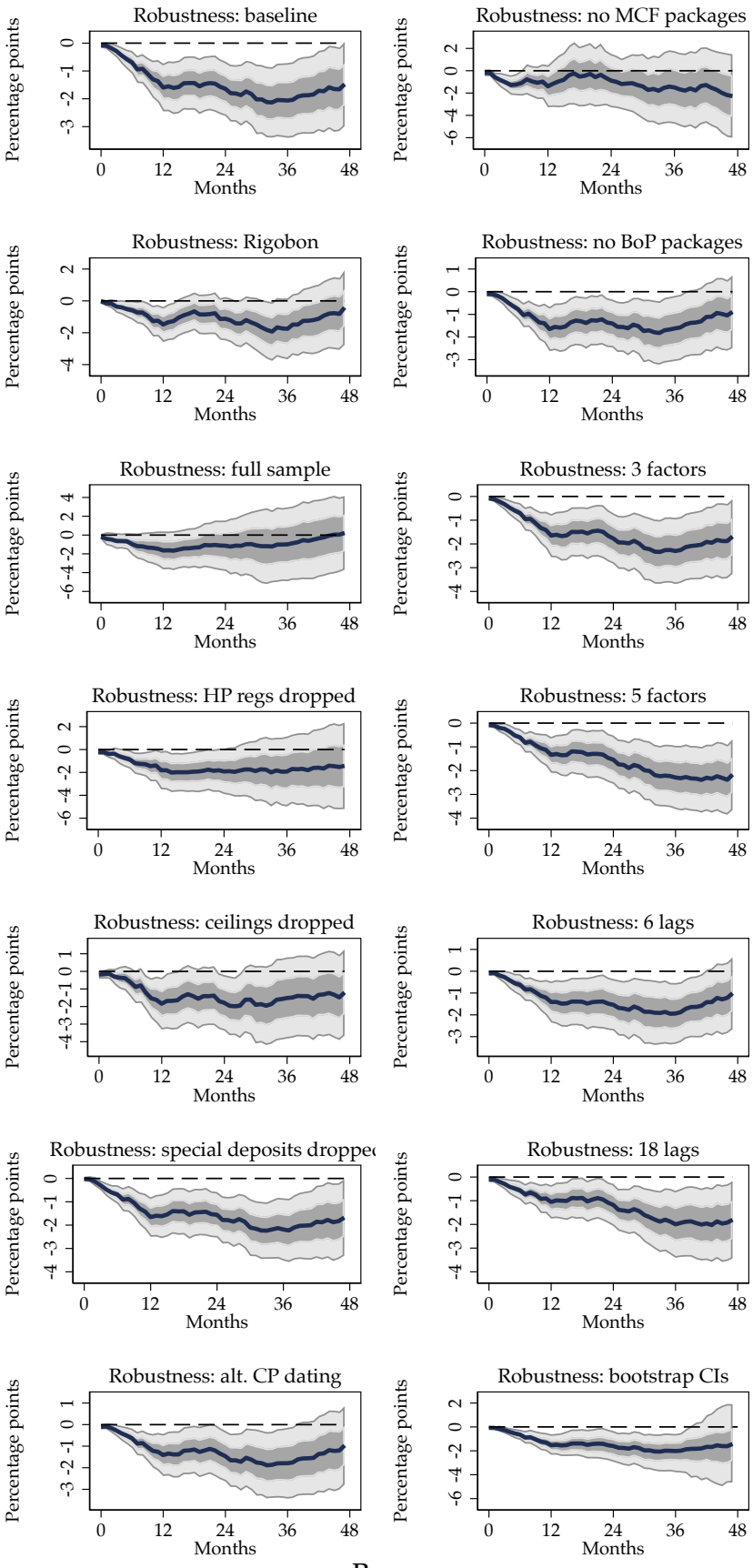
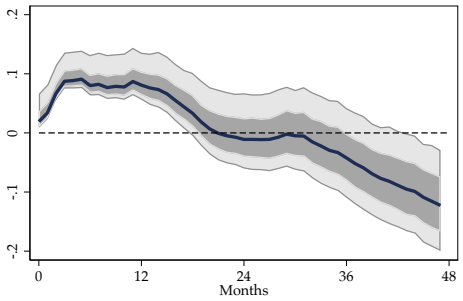


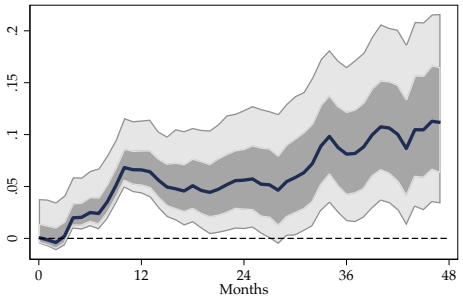
Figure B11: *Forecast error variance decompositions, FALP*

Responses to monetary policy:

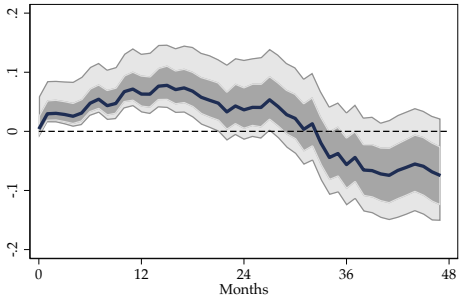
(a) *Impulse: monetary policy, Response: lending*



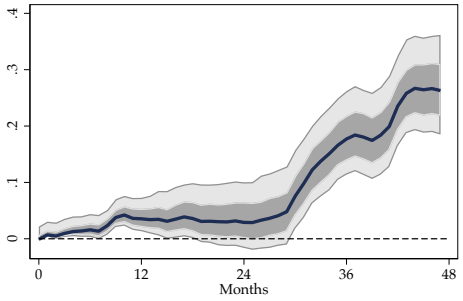
(c) *Impulse: monetary policy, Response: mfg. output*



(e) *Impulse: monetary policy, Response: retail sales*

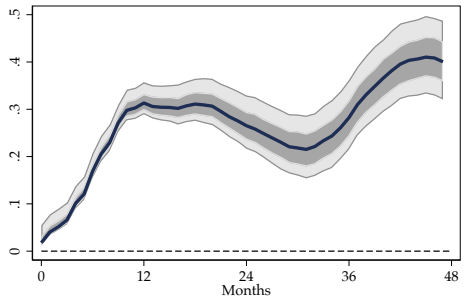


(g) *Impulse: monetary policy, Response: CPIH*

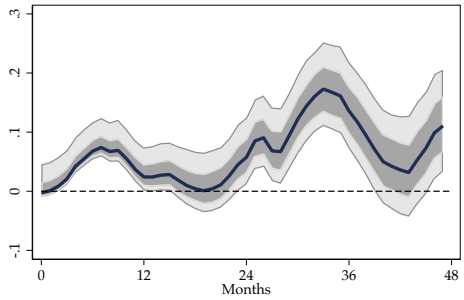


Responses to credit policy:

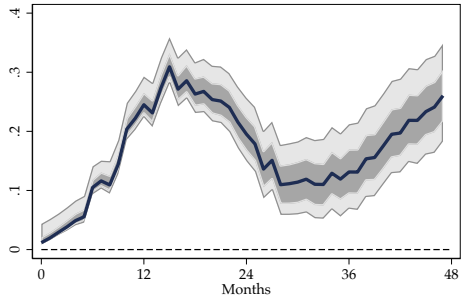
(b) *Impulse: credit policy, Response: lending*



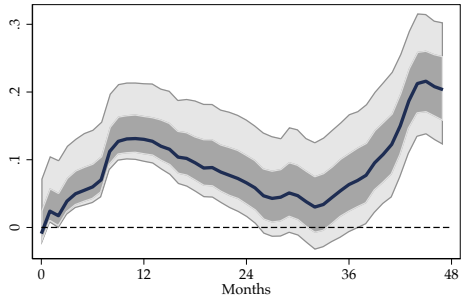
(d) *Impulse: credit policy, Response: mfg. output*



(f) *Impulse: credit policy, Response: retail sales*



(h) *Impulse: credit policy, Response: CPIH*



Notes: The monetary policy surprise is a one percentage point rise in the one year gilt rate. The credit policy surprise is a three per cent fall in bank stock prices. See text for further details.