Wage Setting Patterns and Monetary Policy: International Evidence*

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Abstract

Systematic differences in the timing of wage setting decisions among industrialized countries provide an ideal framework to study the importance of wage rigidity in the transmission of monetary policy. The Japanese *Shunto* presents the most well-known case of bunching in wage setting decisions: From February to May, most firms set wages that remain in place until the following year; wage rigidity, thus, is relatively higher immediately after the *Shunto*. Similarly, in the United States, a large fraction of firms adjust wages in the last quarter of the calendar year. In contrast, wage agreements in Germany are well-spread within the year, implying a relatively uniform degree of rigidity. We exploit variation in the timing of wage-setting decisions within the year in Japan, the United States, Germany, the United Kingdom, and France to investigate

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the effects of monetary policy under different degrees of effective wage rigidity. Our findings lend support to the long-held, though scarcely tested, view that wage-rigidity plays a key role in the transmission of monetary policy.

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1 Introduction

A wide body of empirical evidence suggests that monetary policy has an important effect on the behavior of real variables at business cycle frequencies. Most theoretical models that seek to identify the connection between nominal causes and real effects posit some form of nominal rigidity in wages and (or) prices.[1] Empirical evidence assessing the importance of predetermined nominal wages in the transmission mechanism from monetary policy to real variables is, however, regrettably scarce.[2] This paper attempts to partially fill this empirical void by providing a study that exploits differences in the effective degree of nominal wage rigidity within and across countries. We start by observing that the synchronization of wage setting decisions varies significantly across advanced economies. In Japan, the most well-known example of synchronization of wage setting decisions, the majority of firms set wages during the first and second quarters of the calendar year in what is known as "Shunto," (or spring offensive), and wages remain in place until the following year. In the United States, a large fraction of firms set wages once a year, typically at the end of the calendar year. In contrast, wage bargaining renegotiations in Germany take place throughout the year, and contracts tend to last one to three years. Theories of the transmission of monetary impulses to real variables based on wage

[1] Christiano, Eichenbaum, and Evans (2005) go even further to argue that wage rigidity – unlike price rigidity – is crucial for standard dynamic, stochastic, general-equilibrium (DSGE) models to match key features of the data.

[2] For prices, instead, there is rich information on the frequency of adjustment, though authors differ in their reading of the evidence. (See Bils and Klenow, 2005 and Nakamura and Steinsson, 2007).

rigidity would hence predict that, other things equal, monetary policy innovations in Japan should have a larger effect when the shock takes place in the second half of the year, i.e. after the Shunto has occurred and wages are relatively rigid. In the United States, the effect should be larger when the shock occurs in the first half of the year, as wages tend to be reset at the end of the calendar year. However, in Germany, where there appears to be little bunching in wage setting decisions within the year, the effect should not vary with the quarter in which the shock takes place.

The aim of our study is to test whether these predictions find support in the data. More precisely, we assess whether the response of the economy to monetary policy shocks differs according to the time of year in which the shock takes place and whether this difference can be reconciled with the observed variation in the timing of wage setting decisions. To this end, we introduce quarter dependence in an otherwise standard, recursive VAR model and analyze the empirical impulse responses of aggregate variables to a monetary policy innovation in five large and highly developed countries. The countries we consider are France, Germany, Japan, the United Kingdom, and the United States. Our focus on these countries is related to the extant literature on central banking practices: The wider consensus in the literature on the monetary instruments used by these countries' central banks provides a natural baseline from which we deviate to study the potential for seasonal dependence in monetary policy effects.[3]

Our empirical exercise has a "difference-in-difference" flavor, in that we test for potential differences in the effect of monetary policy across quarters of the calendar year for each of the countries we consider, and then we relate our findings across countries to each country's degree of wage rigidity over the calendar year.[4] We find that for both Japan and the United States, there are, indeed, impor-

[3]Smaller and (or) less developed economies are less suitable for a quarter-dependent VAR representation. These countries are more likely to have changed their economic structure and the conduct of monetary policy over time. This higher propensity to monetary and to real intrinsic instability would require the inclusion of structural-change parameters. Given the extent of data availability, the empirical analysis would be impaired by the lack of sufficient degrees of freedom at the estimation stage.

[4]Note that direct cross-country comparisons are impaired by a large range of country-specific characteristics (in-

tant differences in the response of the economy to monetary policy shocks that depend on the timing of the policy innovation. These differences, in turn, can be related to the differing degree of wage rigidity across the calendar year. Specifically, a monetary policy innovation in Japan that occurs during the first or second quarter – i.e., during the Shunto period in which wages are being reset – has a relatively small effect on output, whereas an innovation in the third quarter – i.e., immediately after the Shunto – has a remarkably large effect. The pattern is reversed in the United States: A monetary policy innovation in the first half of the calendar year has a significantly larger effect on output, whereas an innovation in the second half has a relatively small effect. Again, this pattern conforms well with the degree of wage rigidity in the United States, which is high in the first half of the year and low in the second half. In sharp contrast, in Germany, France, and the United Kingdom, where there is a more uniform degree of wage rigidity within the year as well as a longer duration of contracts, the quarter in which a monetary policy shock takes place appears to be less relevant.

Our findings for the United States essentially replicate those in Olivei and Tenreyro (2007). This paper extends their empirical analysis to test whether the degree of synchronization in wage setting decision also matters for the transmission of monetary impulses in countries other than the United States. Overall, our findings complement and reinforce their conclusion that wage rigidities can play an important role in the transmission of monetary policy.

The remainder of the paper is organized as follows. Section 2 briefly describes various pieces of evidence on wage setting patterns and the policy strategies used by the countries' central banks. Section 3 presents the empirical method and introduces the data. Section 4 describes the dynamic effects of monetary policy on different macroeconomic aggregates. Section 5 discusses the robustness of our findings, and Section 6 provides concluding remarks.

cluding variation in labour market institutions and in the conduct of monetary policy).

2 Wage Setting Practices and Monetary Policy Instruments in Large Developed Economies

In this section, we first describe the wage setting practices in the countries we study, and then discuss the monetary policy instruments that prevailed as well as the objectives pursued by the central banks in each country throughout the estimation period.

2.1 Wage Setting Practices

The Japanese Shunto is the quintessential example of synchronization in wage setting decisions.[5] Since 1955, it has become customary for the main unions in Japan to conduct annual negotiations for wage increases on a national scale; the negotiations with large companies start in February and about half of the wage settlements are made by the end of March, coinciding with the beginning of the fiscal year. Taking the annual wage increase set by the top firms in major industries as the benchmark, smaller companies, government agencies, and non-unionized employees negotiate their wages during April and May (Sako, 1997). All wage settlements last for one year. Thus, in the Japanese economy, the first and second quarters are periods of substantially larger wage flexibility than the last two quarters.[6]

Systematic evidence on the timing of wage setting decisions in the United States is surprisingly scarce. There are, however, various pieces of anecdotal evidence supporting the notion of "lumping" or uneven staggering of wage contracts. For example, evidence from firms in manufacturing, defense, information technology, insurance, and retail in New England surveyed by the Federal Reserve Sys-

[5] See, for example, Grossman and Haraf (1989) and Taylor (1999).

[6] While there are semi-annual bonus payments, the extent of flexibility in actual compensation that they allow for remains an empirical question. As we argue later, the differences in the effect of monetary policy interventions before and after the Shunto observed in the data suggest that the bonus practice does not make up for the wage rigidity intrinsic to the Shunto.

tem in 2003 for the "Beige Book" indicates that most firms make decisions regarding compensation changes (base-pay and health insurance) during the fourth quarter of the calendar year. Changes in compensation then become effective at the very beginning of the next year. Consistent with this evidence, the Radford Surveys of compensation practices in the information technology sector reveals that more than 90 percent of the companies use a focal base-pay administration, with annual pay-change reviews; pay changes usually take place at the beginning of the new fiscal year. According to the same survey, 60 percent of IT companies close their fiscal year in December.[7] More generally, and to the extent that there is a link between pay changes and the end of the fiscal year, it is worth noting that 64 percent of the firms in the Russell 3000 Index end their fiscal year in the fourth quarter, 16 percent in the first, 7 percent in the second, and 13 percent in the third quarter.[8] Finally, reports on collective bargaining activity compiled by the Bureau of Labor Statistics show that the distribution of expiration and wage reopening dates tends to be tilted towards the second semester of the year.[9]

Synchronization in the timing of wage contracts, however, is not the norm in other countries. In France, only public sector bills ("décrets") on civil servants' salaries tend to be bunched in a single quarter (the fourth) every year. The private sector shows significantly less bunching. According to the "*Bilan Annuel de la Negociation Collective 2003*," almost all companies in the private sector (92 percent of the sample) sign one agreement a year, and the actual dates of agreements are well spread throughout the calendar year, with a slightly higher concentration in the second and fourth quarters; new agreements typically become effective in January.[10] It is important to stress that what is relevant from the perspective of models with wage rigidity is the time at which the settlement is agreed upon,

[7] We thank Andy Rosen of Aon Consulting's Radford Surveys for providing us with the information.

[8] This information is for the year 2003 and is available from Standard and Poor's COMPUSTAT. To compute the percentages, we weight each firm in the Russell 3000 index by the firm's number of employees.

[9] See *Current Wage Developments*, various issues, Bureau of Labor Statistics.

[10] The "Bilan Annuel de la Negociation Collective 2003" describes collective agreements in sectors with more than 5000 employees, representing around 50% of the active working population.

rather than the date at which it becomes effective.[11] Thus, synchronization of wage setting decisions in France is less prevalent than in Japan or the United States; to the extent that the public sector has a bearing, some bunching of wage contracts occurs in the fourth quarter.

Bunching of wage setting decisions within the year is even less prevalent in Germany and the United Kingdom. In Germany, according to the *Hans-Böckler Stiftung Tarifdaten*,[12] the dates for new collective agreements tend to be well spread throughout the year and, equally important, collective agreements tend to last from 12 to 36 months. Note that multiple-year contracts alone imply that, in principle, the quarter in which a monetary shock takes place should be less relevant than in a yearly contract. This is so for either of two reasons. Consider first the case in which multiple-year contracts are mostly renegotiated in the same quarter in synchronized years. Renegotiation will then take place in some years and not in others. Hence, a given quarter might display high or low wage rigidity depending on a particular year. Monetary policy innovations in a given quarter of the calendar year will thus have different effects in different years. Consider then the case in which multiple-year contracts are staggered, with only a fraction of contracts being renegotiated in a given year, albeit in the same quarter. This implies that the fraction of contracts being renegotiated in a given quarter-year should be smaller than in the case of yearly contracts. Hence, in Germany, both the more uniform distribution of wage setting decisions within the year and the longer duration of contracts should make the timing of monetary policy innovations within the year less relevant than in Japan or the United States.

In the United Kingdom, wage settlements typically become effective around January and April.

[11] Suppose the agreement takes place at time t_0 and becomes effective from time $t_1 > t_0$ on, and then expires at time $t_2 > t_1$. Insofar as there are no contingent clauses, this means that wages are effectively rigid from time t_0 until t_2, since any new information coming after t_0 is not used in the agreed wage. In this respect, what is relevant is the information set at t_0, when the settlement was agreed upon, while the date at which the agreement becomes effective, t_1, is irrelevant.

[12] The Hans-Böckler Stiftung Tarifdaten is the collective-agreement archive that tracks and analyses developments concerning collective agreements in the country.

However, actual negotiations tend to start one to five months before the date the new wage becomes effective, depending on the sector. For example, the National Council for Local Government Services typically settles wage agreements five months before the effective date, while the Nursing and Other Health Professionals Review Body does so two months before the effective date. Thus, actual decisions on wage adjustments are more spread over the calendar year than in Japan or the United States. Furthermore, as was the case with Germany, a large fraction of collective agreements in the United Kingdom last for more than one year: *Incomes Data Services* reports that in 2005, more than 40 percent of wage contracts had a duration of three years. Thus, as argued for Germany, the response of the economy in the United Kingdom should be less sensitive to the quarter in which a monetary shock takes place.

The differences in the timing of wage setting decisions and in the duration of contracts among the countries we consider provide an ideal framework to study the importance of wage rigidity in the transmission of monetary policy. Models emphasizing nominal wage rigidity predict that the effects of monetary policy innovations should vary substantially more with the quarter of origin of the shock in Japan or the United States than in any of the European countries we study.

2.2 Monetary Policy Instruments and Goals

Evaluating the effects of monetary policy shocks requires identification of a measure of policy and the variables the monetary authority is responding to when setting policy. The policy measure can vary over time, and so can the emphasis on the central bank's objectives, such as short-run stabilization of output or exchange rates, and medium- and long-run inflation targets. As the next section will make clear, we need to adopt a parsimonious specification because our VAR-based empirical analysis is constrained by a degrees-of-freedom problem. For this reason, our benchmark specification assumes a short-term measure of the interest rate (typically an interbank lending rate for overnight loans) as the appropriate indicator of monetary policy. This assumption still allows for the possibility that the central bank is targeting a narrow reserve aggregate, provided that the reserves target is set with the purpose of achieving a specific target for the short-term interest rate (see Clarida, Galí, and Gertler, 1998). The level of the short-term interest rate is chosen by the central bank as a function of the level of output and inflation. We are thus assuming that the central bank's objectives are short-run stabilization of output and a medium to long-term inflation target. As a result, the reduced-form VAR we are considering in our benchmark specification, by including a measure of output, prices, and the short-term interest rate, nests generalizations of the simple interest rate rule proposed by Taylor (1993).

Such a minimal framework for describing the economy and, for the purpose of the present discussion, the conduct of monetary policy in the countries we consider, is an oversimplification. Exchange rate objectives played a prominent role in the Bretton Woods era for all countries included in the analysis, and subsequently, for France and the United Kingdom in the context of the European Monetary System. Still, even under such circumstances, the central banks retained some degree of monetary control, either via capital controls (prevalent in the Bretton Woods era) or exchange rate realignments. In all, for the United States, Germany, and Japan – at least during the post-Bretton Woods period – monetary policy was not particularly affected by external constraints and autonomy in policy management was, thus, greatest for these countries in the analysis. For France and the United Kingdom, instead, external constraints have also operated in the post-Bretton Woods era. Clarida, Galí, and Gertler (1998) document how Germany's monetary policy influenced the conduct of monetary policy in both these countries. It will, thus, be important to evaluate whether the empirical findings in our benchmark specification are robust to the inclusion of the German policy rate as an additional explanatory variable.

Money supply targets have also played a role in the monetary policy strategies of the central banks (see Bernanke and Mishkin, 1992), at least over certain periods of time. The importance of any misspecification resulting from the omission of monetary aggregates is debatable. For Germany, Bernanke and Mihov (1997) argue that while the Bundesbank has operated since 1974 in a framework officially designated as money targeting, inflation goals – rather than money growth targets – have been driving the conduct of monetary policy. Similarly, Clarida, Galí, and Gertler (1998) find little role for monetary policy aggregates in Japan as an additional regressor in an estimated Taylor rule. As concerns the United States, growth in monetary aggregates was an input into policy decisions at certain times of Chairman Burns' and Chairman Volcker's tenures. Still, the empirical results in Friedman and Kuttner (1996), among others, suggest only a limited role for an independent response of the Federal Reserve to deviations of money from the target path.[13] In all, these findings suggest that specifying an inflation target in the policy reaction function may render the inclusion of monetary aggregates redundant.

As concerns the choice of policy instrument, the assumption that a short-term measure of the interest rate is the appropriate policy variable finds support in studies that empirically identify the relevant policy indicator instead of relying on prior information about a central bank's operating procedures. Indeed, while the Federal Reserve operating procedures have varied over the past 40 years, several authors have argued that funds-rate targeting provides a good description of Federal Reserve policy over most of the period (see, for example, Bernanke and Blinder, 1992, and Bernanke and Mihov, 1998). In a similar vein, Bernanke and Mihov (1997) show that the Lombard rate is the relevant policy indicator for the Bundesbank, at least over the period 1975 to 1990. Using an approach similar to Bernanke and Mihov (1997,1998), Nakashima (2006) argues that the call rate should be identified as the most appropriate policy indicator for the Bank of Japan over the period

[13] Sims and Zha (2006), however, argue that the inclusion of monetary policy aggregates in a policy reaction function is important for fitting the Federal Reserve's policy reaction function in the context of a regime-switching framework.

1975 to 1995. As for France and the United Kingdom, given the mentioned influence of Germany's monetary policy, it is plausible that a short-run measure of the interest rate must have played a relevant role in setting policy.

Overall, we think that a description of the economy based on output, prices, and a short-term interest rate represents a meaningful benchmark for the countries we consider. This minimal framework for the economy and the operating rule for the monetary authority, whereby the central bank targets the short-term interest rate to achieve output stability in the short term and a medium to long run inflation outcome, needs to be checked against richer specifications - something that we do in Section 5. But to the extent that the misspecification arising from our benchmark specification is relatively small, this framework has the notable advantage of economizing on degrees of freedom at the estimation stage.

We note here that even if the proposed benchmark were to provide a reasonably accurate summary of a country's macroeconomy, there are lingering issues concerning stability. Over the sample period we consider, a central bank could have changed the way it negotiates the trade-off between output and inflation stability that arises, for example, in the event of adverse supply shocks. Indeed, Clarida, Galí and Gertler (1998, 2000) argue that central banks' behavior pre-1979 differed from the behavior post-1979, in that central banks shifted their focus from stabilizing output to placing more emphasis on targeting inflation. This claim, even in a much-studied country such as the United States, remains controversial. Sims and Zha (2006), for example, argue that the conduct of monetary policy in the United States has been relatively stable over the period 1959 to 2003, while Cochrane (2007) contends that there are identification issues in the Taylor rules as estimated in Clarida, Galí and Gertler (2000). In all, the question of stability in the conduct of monetary policy remains open and it is important to keep in mind that our findings rest on the assumption of stability of the estimated relationships over the sample period we consider. Because of degrees-of-freedom problems, a check of the robustness of our findings over subsamples can only be limited in scope. Still, we will mention the stability of our findings over the post-Bretton Woods period in Section 5.

3 Method

3.1 Empirical Model

Our benchmark empirical analysis for measuring the effect of monetary policy shocks relies on a general model of the macroeconomy represented by the following system of equations:

$$\mathbf{Y}_t = \sum_{s=0}^{k} \mathbf{B}(q_t)_s \mathbf{Y}_{t-s} + \sum_{s=1}^{k} \mathbf{C}(q_t)_s \; p_{t-s} + \mathbf{A}^y(q_t)\mathbf{v}_t^y \tag{1}$$

$$p_t = \sum_{s=0}^{k} \mathbf{D}_s \mathbf{Y}_{t-s} + \sum_{s=1}^{k} \mathbf{g}_s \; p_{t-s} + \mathbf{a}^p v_t^p. \tag{2}$$

Boldface letters indicate vectors or matrices of variables or coefficients. $\mathbf{Y}_t$ is a vector of non-policy macroeconomic variables (e.g., output and prices), and p_t is the variable that summarizes the policy stance. We take the short-term interest rate as our measure of policy, and use innovations in these measures as monetary policy shocks. Equation (1) allows the non-policy variables $\mathbf{Y}_t$ to depend on both current and lagged values of $\mathbf{Y}$, on lagged values of p, and on a vector of uncorrelated disturbances $\mathbf{v}^y$. Equation (2) states that the policy variable p_t depends on both current and lagged values of $\mathbf{Y}$, on lagged values of p, and on the monetary policy shock v^p.[14,15] As such, the system embeds the key assumption for identifying the dynamic effects of exogenous policy shocks on the

[14] Note that the vector of disturbances $\mathbf{v}_t^y$, composed of uncorrelated elements, is pre-multiplied by the matrix $\mathbf{A}^y(q_t)$ to indicate that each element of $\mathbf{v}_t^y$ can enter into any of the non-policy equations. This renders the assumption of uncorrelated disturbances unrestrictive.

[15] Policy shocks are assumed to be uncorrelated with the elements of $\mathbf{v}^y$. Independence from contemporaneous economic conditions is considered part of the definition of an exogenous policy shock. The standard interpretation of v^p is a combination of various random factors that might affect policy decisions, including data errors and revisions, preferences of participants at the FOMC meetings, politics, etc. (See Bernanke and Mihov 1998).

various macro variables $\mathbf{Y}$: Policy shocks do not affect macro variables within the current period. Although debatable, this identifying assumption is standard in many recent VAR analyses.[16]

The system represented by equations (1) and (2) replicates the specification of Bernanke and Blinder (1992), with the crucial difference that we allow for time dependence in the coefficients for the equations in the non-policy block (1) of the system. Specifically, $\mathbf{B}(q_t)_s$ and $\mathbf{C}(q_t)_s$ are coefficient matrices whose elements, the coefficients at each lag, are allowed to depend on the quarter q_t that indexes the dependent variable, where $q_t = j$ if t corresponds to the j^{th} quarter of the year. In the policy block (2) of the system, the coefficients $\mathbf{D}_s$ and $\mathbf{g}_s$ are constant across seasons, as there is no evidence suggesting that policy responses to given outcomes vary by season. Still, the systematic response of policy takes the time dependence feature of the non-policy variables into account: Substituting (1) into (2) shows that the coefficients in the policy equation are indirectly indexed by q_t through their impact on the non-policy variables, $\mathbf{Y}_t$.[17]

Given the identifying assumption that policy shocks do not affect macro variables within the current period, we can rewrite the system in a standard VAR reduced form, with only lagged variables on the right-hand side:

$$\mathbf{X}_t = \mathbf{A}(L, q_t)\mathbf{X}_{t-1} + \mathbf{U}_t, \tag{3}$$

where $\mathbf{X}_t = [\mathbf{Y}_t, p_t]'$, $\mathbf{U}_t$ is the corresponding vector of reduced-form residuals, and $\mathbf{A}(L, q_t)$ is a four-quarter lag polynomial that allows for the coefficients at each lag to depend on the particular quarter q indexing the dependent variable. The system can then be estimated equation-by-equation using ordinary least squares. The effect of policy innovations on the non-policy variables is identified with the impulse-response function of $\mathbf{Y}$ to past changes in v^p in the unrestricted VAR (3), with the

[16] See, among others, Bernanke and Blinder (1992), Rotemberg and Woodford (1997), Bernanke and Mihov (1998), Christiano, Eichenbaum and Evans (1999), and Boivin and Giannoni (2006).

[17] Note that allowing for quarterly dependence in the coefficients of the policy equation will lead to the same reduced-form VAR as the one implied from equations (1) and (2). Without loss of generality, we prefer to write the policy equation as in (2) because there is no evidence that policy makers appear to follow seasonally dependent policy rules.

monetary policy variable placed last in the ordering. An estimated series for the policy shock can be obtained via a Choleski decomposition of the covariance matrix of the reduced-form residuals.

One implication of quarter dependence is that the immediate effects of monetary policy shocks can differ depending on which quarter the shock takes place. Quarter dependence in (3) also allows the reduced-form dynamics of the non-policy variables to vary across quarters. As a result, the timing of the policy shocks matters in tracing the variables' response to a policy shock. For example, when a monetary shock occurs in the first quarter, the response of the non-policy variables in the next quarter will be governed by the reduced-form dynamics of the non-policy variables in the second quarter. The response two quarters after the initial shock will be governed by the reduced-form dynamics of the non-policy variables in the third quarter, and so on.

The system (1) and (2) and the corresponding unrestricted VAR (3) describe our benchmark specification. In the robustness section we will discuss, among other things, results based on more general specifications which we can write in reduced form as:

$$\widetilde{\mathbf{X}}_t = \widetilde{\mathbf{A}}(L, q_t)\widetilde{\mathbf{X}}_{t-1} + \mathbf{B}(L, q_t)\mathbf{Z}_t + \widetilde{\mathbf{U}}_t, \tag{4}$$

where now $\widetilde{\mathbf{X}}_t = [\mathbf{Y}_t, p_t, \mathbf{Y}_{2,t}]$, $\mathbf{Z}_t$ is a vector of exogenous variables, $\widetilde{\mathbf{U}}_t$ a vector of reduced-form residuals, and $\mathbf{A}(L, q_t)$ and $\mathbf{B}(L, q_t)$ are four-quarter lag polynomials that allow coefficients at each lag to depend on the particular quarter q indexing the independent variable. The reduced-form VAR in (4) allows for an additional block of endogenous variables, denoted by $\mathbf{Y}_2$. The ordering of the variables in $\widetilde{\mathbf{X}}$ still embodies the identifying assumption that monetary policy shocks do not have a contemporaneous impact on $\mathbf{Y}$, but monetary policy shocks can now affect the variables in $\mathbf{Y}_2$ immediately. One variable included in $\mathbf{Y}_2$ is a broad monetary aggregate, because money developments have sometimes played a role in the monetary policy strategies of some of the countries we consider. The additional identifying assumption in the context of the reduced-from VAR (4) is that the policy variable p can respond to contemporaneous movements in $\mathbf{Y}$, but only to lagged movements in $\mathbf{Y}_2$. However, when $\mathbf{Y}_2$ includes an exchange rate measure among the variables, such an identifying assumption is not entirely appropriate. In France and the United Kingdom in particular, there have been instances when the policy variable p moved so as to respond to changes in the exchange rate that occurred within the same quarter. For this reason, we will also discuss findings based on a different identification strategy. The vector of exogenous variables $\mathbf{Z}_t$ comprises variables such as world commodity prices and foreign interest rates. The inclusion of world commodity prices can, in principle, help to solve the empirical finding of prices temporarily rising after a monetary policy tightening (the so-called price puzzle).[18] As for foreign interest rates, over the sample period we consider monetary management in France and the United Kingdom to have been influenced by interest rate developments in Germany. Treating these variables as exogenous means that we are assuming no feedback from $\widetilde{\mathbf{X}}$ to $\mathbf{Z}$.

3.2 Testing

The quarter-dependent VAR in (3) generates four different sets of impulse responses to a monetary policy shock, depending on the quarter in which the shock occurs. It is then important to assess whether the quarter-dependent impulse-response functions are statistically different from the impulse responses of the nested standard VAR with no time dependence. A first natural test for the empirical relevance of quarterly effects consists of simply comparing the estimates obtained from the quarter-

[18] For the United States, commodity prices are included directly as an endogenous variable in $\mathbf{Y}_t$. This is to keep with the specification in Olivei and Tenreyro (2007), and with the fact that, over the chosen sample period, fluctuations in U.S. economic activity affected commodity prices. Essentially, we are treating the United States as a large country whose economic developments can impact world commodity prices. Instead, we treat the other countries we consider in our empirical exercise as small, in that movements in world commodity prices are not affected by economic developments in these countries.

dependent VAR (3) with those obtained from the restricted standard VAR using an F-test, equation by equation. However, even if F-tests reject the null hypothesis of no time dependence, this does not ensure that the impulse responses generated by the quarter-dependent VAR are statistically different from the responses generated by the standard VAR. Impulse-response functions are nonlinear combinations of the estimated coefficients in the VAR, and as a result, F-tests on the linear reduced-form VAR do not map one-for-one into a test on the impulse responses.

For this reason, we assess the significance of quarter dependence on the impulse-response functions directly. Specifically, we consider the maximum difference, in absolute value, between the impulse responses of variable x in the quarter-dependent VAR and in the standard non-time-dependent VAR:

$$D = \sup_t |x_t^q - x_t|$$

where x_t^q denotes the period t response in the quarter-dependent model and x_t the response in the standard non-time-dependent model.[19] We construct an empirical distribution of D by bootstrapping the residuals of the reduced-form non-time-dependent VAR. At each draw, we generate a new data set and estimate new impulse responses from both the quarter-dependent and standard VARs. This yields a new value for D^s, where the superscript s denotes a simulated value. The procedure is repeated 2,000 times to obtain a bootstrapped p-value, which is the percentage of simulated D^s exceeding the observed D.

[19] We compute the supremum of the difference in impulse-response functions over 20 quarters following a monetary policy shock.

3.3 Data and Estimation

Our empirical analysis is based on seasonally adjusted quarterly data.[20] In the benchmark specification (3) the vector of non-policy variables $\mathbf{Y}_t$ generally consists of a measure of activity and a price index. The policy variable p_t is given by a short-term interest rate. The following is a description of the data and sample periods for each of the countries we consider.

For Japan, we use data from 1963:Q1 through 1995:Q2. After 1995:Q2, the call rate starts to be at the same level or below the discount rate. The measure for activity is given by industrial production, while the price level is given by the overall consumer price index. The policy variable p_t is the call money rate. In alternative specifications, the set of non-policy variables $\mathbf{Y}_t$ is augmented to include hourly compensation in the manufacturing sector, while the set of non-policy variables $\mathbf{Y}_{2,t}$ influenced contemporaneously by p_t consists of money (M2+CD).

For the United States, we use data over the period 1966:Q1 through 2002:Q4. The beginning of the estimation period is dictated by the behavior of monetary policy. Only after 1965 did the federal funds rate, the policy variable p_t in our study, exceed the discount rate and hence began to act as the primary instrument of monetary policy. The activity measure is real GDP, and the price measure is the GDP deflator. In keeping with Olivei and Tenreyro (2007), the baseline specification for the United States also includes a commodity price index as an additional variable in $\mathbf{Y}_t$. In alternative specifications, the set of non-policy variables $\mathbf{Y}_t$ is augmented to include hourly compensation in the manufacturing sector, while the set of non-policy variables $\mathbf{Y}_{2,t}$ influenced contemporaneously by p_t consists of money (M2 divisia).

For (West) Germany, we use data from 1963:Q1 to 1994:Q4. Unification complicates the use of

[20] The use of seasonally adjusted data allows us to directly identify the interaction between the effect of the innovation in monetary policy and the season in which the innovation takes place. In other words, it allows us to control for the independent effect of the season on macroeconomic variables. Alternatively, non-seasonally adjusted data could be used, provided that seasonal dummies are used as controls. (See Olivei and Tenreyro, 2007).

German data, and to obtain a consistent measure of output, we use real GDP for West Germany - a series that is available through 1994. The price measure is given by the GDP deflator, and the policy variable p_t is the Lombard rate. In alternative specifications, the set of non-policy variables $\mathbf{Y}_t$ is augmented to include hourly compensation in the manufacturing sector, while the set of non-policy variables $\mathbf{Y}_{2,t}$ influenced contemporaneously by p_t consists of money (M2).

For France, we use data from 1963:Q1 through 1998:Q4. The sample stops with the inception of the single European currency. The activity measure for France is real GDP, and the price measure is the overall consumer price index. The policy variable p_t is given by the call rate. In alternative specifications, the set of non-policy variables $\mathbf{Y}_t$ is augmented to include hourly compensation in the manufacturing sector. The set of non-policy variables $\mathbf{Y}_{2,t}$ influenced contemporaneously by p_t consists of the nominal exchange rate vis-à-vis the Deutsche Mark. We also consider the German Lombard rate as an exogenous variable in $\mathbf{Z}_t$. This formalizes the notion that, while the country retained some leverage over domestic monetary policy, German monetary policy was also exerting a constraint.

For the United Kingdom, we use data from 1963:Q1 to 1997:Q1. The sample ends prior to the Bank of England independence in May 1997. The activity variable is real GDP and the price measure is the overall consumer price index. The policy variable p_t is given by the the three-month Treasury bill rate. In alternative specifications, the set of non-policy variables $\mathbf{Y}_t$ is augmented to include hourly compensation in the manufacturing sector. As it is for France, the set of non-policy variables $\mathbf{Y}_{2,t}$ influenced contemporaneously by p_t consists of the nominal exchange rate vis-à-vis the Deutsche Mark. We also consider the German Lombard rate as an exogenous variable in $\mathbf{Z}_t$.

We also examine specifications in which the price of oil (expressed in U.S. dollars per barrel) is included in $\mathbf{Z}_t$ as an exogenous variable for all countries with the exception of the United States, where oil prices are included as an additional variable in $\mathbf{Y}_t$.[21] In all of the specifications, the variables enter the VAR analysis in log levels except for interest rates, which are expressed in levels.[22] We formalize trends in the non-policy variables as deterministic and allow for a linear trend in each of the equations of the reduced-form VAR. We allow for four lags of the endogenous variables at the estimation stage. Each equation in the VAR is estimated separately. Given the large number of coefficients that need to be estimated in a quarter-dependent VAR and the relatively short available sample periods, for each quarter, the coefficients on the four lags of each of the endogenous variables within a given equation are estimated by means of a third-order polynomial distributed lag. For the United States, this is not needed, since the sample period is sufficiently long and we hence use the unconstrained ordinary least square procedure.[23] We will note in Section 5 that our findings for Japan, Germany, France and the United Kingdom are not driven by the constrained estimation. Unconstrained ordinary least squares produce essentially the same findings, but the constrained estimation saves on degrees of freedom and hence achieves higher precision.

4 The Dynamic Effects of Monetary Policy Shocks

This section reports the estimated dynamic effects of monetary policy on macroeconomic variables for each of the countries in the analysis. The estimates are based on the benchmark specification (3). In section 5 we discuss the robustness of our findings to specifications that include additional endogenous (and possibly also exogenous) variables, as represented by the VAR in (4).

[21] For further discussion, see footnote 18.
[22] We also studied specifications in which monetary aggregates entered in terms of growth rates, rather than log-levels.
[23] The constrained estimation leads to virtually identical results.

4.1 Japan

The impulse responses to a monetary policy shock in Japan are depicted in Figures 1 through 5, with shaded areas denoting, correspondingly, the 80 and 95 percent confidence intervals around the estimated responses.[24] We consider a monetary policy shock that corresponds to a 25-basis-point decline in the policy rate on impact. For ease of comparison, the responses of each variable to shocks occurring in different quarters are graphed on the same scale across figures.

Figure 1 displays impulse responses to the policy shock when we do not allow for quarter dependence in the reduced-form VAR, as is customary in the literature. The top-left panel shows the response of industrial production to the policy shock, which is persistent and peaks about 7 quarters after the shock. The top-right panel shows that the response of prices to the monetary policy easing displays a price puzzle with prices declining marginally for a few quarters before starting to increase.

Figures 2 to 5 display the impulse responses corresponding to the quarter-dependent VAR (3). The responses to a monetary policy shock occurring in the first quarter of the year are shown in Figure 2. The response of activity is essentially nil. The price response does not exhibit as much of a price puzzle as that for the non-quarter-dependent VAR, though the response is estimated rather imprecisely. Figure 3 displays impulse responses to a shock that takes place in the second quarter. Activity now shows a significant positive response to the monetary policy easing. The increase in prices following the policy shock is more delayed than in the case in which the shock occurs in the first quarter. Again, the price response is estimated imprecisely. The responses to a monetary policy shock in the third quarter are depicted in Figure 4. Activity responds very strongly to the policy easing, while the price response in the first few periods following the shock now displays a more

[24] Much applied work uses 95 percent confidence intervals. Sims and Zha (1999) note that the use of high-probability intervals camouflages the occurrence of large errors of over-coverage and advocate the use of smaller intervals, such as intervals with 68 percent coverage (one standard error in the Gaussian case). An interval with 80 percent probability corresponds to about 1.3 standard errors in the Gaussian case. We report both 80 and 90 percent confidence intervals for the sake of completeness.

pronounced (and statistically significant) price puzzle. Figure 5 shows the responses to a monetary policy shock occurring in the fourth quarter. The increase in output is significant, but not as large as when the shock occurs in the third quarter. The increase in prices after the shock is less delayed than in the case in which the shock occurs in either the second or the third quarter.

The findings illustrate that the response of economic activity to a monetary policy shock differs sharply according to the quarter in which the policy shock takes place. The pattern in the quarterly responses of economic activity to a policy shock is also consistent with Japan's non-uniform distribution of wage contracts over the calendar year. Activity responds insignificantly in the first quarter. As we have mentioned, this is a period of great wage flexibility, with most wage contracts being renegotiated in March and taking effect at the very beginning of the second quarter. In the second quarter, a smaller fraction of wage contracts are still being renegotiated, particularly at small firms. The response of activity in the second quarter is now significant. Still, the response is not as large as the response of activity following a shock in the third quarter. In this quarter, wage rigidity is high, and the transmission of monetary shocks to the real economy is amplified by the fact that the fourth quarter is also a quarter with high wage rigidity. The estimated response of activity to a shock in the third quarter is, at its peak, twice as large as the response to a shock occurring in the second quarter. When the shock occurs in the fourth quarter, the response of activity is again significant. The response, however, is similar in magnitude to the response occurring in the second quarter (though it is estimated more precisely), and thus much smaller than the response to a shock occurring in the third quarter. The fourth quarter is a period of high wage rigidity, but it is followed by two quarters (especially the first quarter of the calendar year) in which wages become very flexible. This, to some extent, impairs the transmission of the monetary policy impulse to the real economy.

Overall, the empirical findings provide evidence that wage rigidity matters for the transmission of monetary policy shocks to the real economy. The impact on the real economy is larger when the shock occurs right after the Shunto, and weaker when the shock occurs right before the Shunto. As concerns the response of prices, there is less that we can say with high confidence. The responses tend to be estimated with wider standard errors; still, the point estimates show a slower increase in prices during the first few periods following a shock in the third quarter, when wage rigidity is high, than in the first quarter, when wage rigidity is low.

The difference in impulse responses documented in Figures 2 through 5 is corroborated by the two statistical tests on the importance of quarter dependence described in Section 3.2. Specifically, an F-test on the relevance of quarter dependence for the real activity equation in the reduced-form VAR(3) yields a p-value of 0.018.[25] While indicative of the existence of quarter dependence, this finding does not necessarily translate into statistically different impulse responses. For this purpose, we evaluate the D-statistic, which assesses whether the maximum difference between the impulse response of a given variable in the quarter-dependent VAR and the corresponding response of that variable in the standard non-time-dependent VAR is statistically different. Table 1 reports the bootstrapped p-values for the D-statistic in each quarter for activity, prices, and the policy rate. The table shows that according to this test, the response of activity to a policy shock in the first and in the third quarter are statistically different from the non-quarter-dependent impulse response at better than the asymptotic 5 percent level. The null hypothesis of a response of real activity equal to the non-time-dependent response cannot be rejected when the shock takes place in the second or fourth quarter.[26]

[25] The p-values for the price level and the policy variable equations, instead, are 0.17 and 0.087, respectively.

[26] The D-statistic in Table 1 also indicates that the price responses to a policy shock in the third and fourth quarters are significantly different from the non-quarter-dependent price response. The quarterly impulse responses for the price level show prices reaching a higher level in the third and fourth quarters than in the non-quarter-dependent case. This is not inconsistent with an explanation that relies on wages being more flexible in the first half of the calendar year. What is important is for wages to be rigid at the time (and for some period immediately after) the shock occurs. This generates an immediate expansion in output which, in the presence of real rigidities such as habit formation in consumption and adjustment costs in investment, will persist over time (see, for example, Christiano, Eichenbaum, and Evans, 2005). The persistence of output above its natural level can ultimately yield a higher price level than in the case in which wages are flexible at the time the shock occurs. This will depend, among other things, on how strongly the monetary authority responds to inflationary pressures.

It is also apparent from the figures that the differences in the response of activity to policy shocks occurring in different quarters are significant from an economic standpoint.

4.2 United States

The impulse responses to a monetary policy shock in the United States that corresponds to a 25-basis-point decline in the Federal Funds rate on impact are depicted in Figures 6 through 10. The figures essentially replicate the findings in Olivei and Tenreyro (2007). Figure 6 shows impulse responses to the monetary policy shock without allowing for quarter dependence in the system, as it is standard in the literature. The output response to the policy shock is persistent, peaking 5 quarters after the shock and slowly decaying thereafter. Prices start to rise reliably a year after the shock, although it takes about two years for the increase to become significant.

Figures 7 to 10 display impulse responses based on the quarter-dependent reduced-form VAR (3). The responses to a monetary policy shock occurring in the first quarter of the year are depicted in Figure 7. Output rises on impact and reaches a level close to its peak response 5 quarters after the shock. The peak response is now almost twice as large as in the case with no quarter dependence. Despite controlling for oil prices, there is still a "price puzzle," although the decline in prices is not statistically significant. Figure 8 displays impulse responses to a shock that takes place in the second quarter. The response of output is fast and sizable. Output reaches its peak 3 quarters after the shock, and the peak response is more than two and a half times larger than the peak response in the case with no quarter dependence. Note that the large output response occurs despite the fact that the policy shock exhibits little persistence. Figure 9 shows the impulse responses to a shock that occurs in the third quarter. The response of output is now small and short-lived. A similar pattern is also evident in the responses to a policy shock occurring in the fourth quarter, which are depicted in Figure 10. The response of output is estimated rather imprecisely, but shows a muted effect of the policy impulse when compared to the response to a shock occurring in the first half of the calendar year.

The VAR results and the anecdotal evidence on uneven wage staggering in the United States documented in Section 2.1 are consistent with a role for wage rigidity in the transmission of monetary policy. Monetary policy shocks have a large impact on output in the first half of the year, right after wages have been set. In contrast, monetary policy shocks appear to have little impact on output in the second half of the year. In essence, the policy shock is then "undone" by the new wage contracts put in place at the turn of the year. As a result, the effect on output is smaller on average.

The differences documented in Figures 7 to 10 are corroborated by formal tests on the importance of quarter dependence. Equation-by-equation F-tests in the reduced-form VAR (3) yield p-values of 0.008 for the output equation, 0.057 for the price equation, and 0.0002 for the federal funds rate equation. Table 2 reports the bootstrapped p-values for the D-statistic in each quarter for activity, prices, and the policy rate. The table shows that according to this test, the response of activity to a policy shock in either the first or second quarter is statistically different from the non-quarter-dependent impulse response at the asymptotic 5 percent level or better. As with Japan, it is also apparent from the figures that the differences in the response of activity to policy shocks occurring in different quarters are significant from an economic standpoint.

4.3 Germany

The impulse responses to a monetary policy shock in West Germany are depicted in Figures 11 through 15. We consider a monetary policy shock that lowers the Lombard rate by 25 basis points on impact. Figure 11 illustrates the impulse responses corresponding to the VAR without quarter dependence. The response of real activity peaks about 7 quarters after the shock and slowly decays thereafter. The price response displays a fairly protracted price puzzle. The impulse responses corresponding to the quarter-dependent VAR (3), depicted in Figures 12 through 15, are remarkably similar across quarters, except perhaps for a slightly weaker response of activity following a shock in the fourth quarter. The differences, however, are not statistically significant. An F-test on the relevance of quarter dependence for the real activity equation in the reduced-form VAR(3) yields a p-value of 0.52. Table 3 reports the bootstrapped p-values for the D-statistic in each quarter for activity, prices, and the policy rate. None of the quarter-dependent responses are statistically different from the corresponding non-quarter-dependent responses at better than the asymptotic 5 percent level.

The findings are consistent with wage contracts in Germany being more staggered and of longer duration than in Japan. This implies that, to the extent that wage rigidity is important for the transmission of monetary policy shocks to the real economy, the effects of monetary policy should vary little with the timing of the shocks.

4.4 France

The impulse-responses to a monetary policy shock in France are displayed in Figures 16 through 20. We consider a 25-basis point shock in the call money rate. Figure 11 illustrates the impulse responses corresponding to the VAR without quarter dependence. The response of activity is highly persistent, and the extent of the price puzzle is not dissimilar from that of Germany. The impulse responses corresponding to the quarter-dependent VAR (3), depicted in Figures 17 through 20, are estimated with considerable uncertainty. This is particularly true for the response of real economic activity. The estimated responses, however, are fairly similar across quarters. The hypothesis that quarter dependence is not relevant for describing the reduced-form dynamics of the economy is not rejected at standard confidence levels. The bootstrapped p-values for the D-statistic in each quarter for activity, prices, and the policy rate (reported in Table 4) show that none of the quarter-dependent responses are statistically different from the corresponding non-quarter-dependent responses at better than the asymptotic 5 percent level.

As with Germany, the results are consistent with the lack of synchronization in wage-setting decisions documented in Section 2.1. Only the public sector tends to settle agreements in a single season (the fourth quarter). This bunching of public wage contracts, however, does not translate into different output and price responses across quarters for the economy as a whole.

4.5 United Kingdom

The impulse responses to a 25-basis-point decline in the Treasury Bill rate in the United Kingdom are displayed in Figures 16 through 20.[27] Figure 16 shows the impulse responses to the policy shock when we do not allow for quarter dependence in the reduced-form VAR. The pattern of the responses is similar to the one we have documented for the other countries, with a persistent response of output that outlasts the shock to the policy rate. Figures 22 through 25 display impulse responses when we estimate the quarter-dependent reduced-form VAR (3). As with France, the quarterly responses are estimated rather imprecisely. The output response is larger when the shock occurs in the first half of the calendar year. The first half of the calendar year is also when the policy shock has the strongest impact on prices.[28] In all, these findings are hard to reconcile with an explanation that relies on a non-uniform distribution of wages over the calendar year. As we discussed in Section 2.1, wage contracts in the United Kingdom tend to last more than a year, with no significant bunching of contract renegotiations at a particular time of the year.

However, the hypothesis that quarter dependence is not relevant for describing the reduced-form dynamics of the economy is not rejected at standard confidence levels. The bootstrapped p-values

[27] The specification for the United Kingdom includes three lags of oil prices as exogenous variables. Without controlling for oil prices, the price impulse responses show an implausibly large and persistent price puzzle.

[28] This finding does not appear to be robust. We will mention in Section 5 that in a VAR with wages as an additional endogenous variable, the estimated price responses (and also the wage responses) become very similar across different quarters.

for the D-statistic in each quarter for activity, prices, and the policy rate, reported in Table 5, show that none of the quarter-dependent responses are statistically different from the corresponding non-quarter-dependent responses at better than the asymptotic 5 percent level.

4.6 The Distribution of Monetary Policy Shocks and the Seasonal Cycle

An important issue to consider is whether the potentially different impulse responses obtained across quarters are the result of different types of monetary policy shocks. In principle, differences in the intensity and direction (expansionary versus contractionary) of shocks could result in different impulse responses. To explore this hypothesis, we test for the equality of the distributions of the shocks across quarters by means of a Kolmogorov-Smirnov test. The test consists of a pairwise comparison of the distributions of shocks between any 2 quarters with the null hypothesis of identical distributions. The p-values for these tests are displayed in Table 6 for the five countries we consider. As the table shows, in no instance can we reject the null hypothesis of identical distributions across quarters. The results for Japan and the United States deserve some attention, given that these are the countries where we find significant differences in the response of activity to a policy shock across quarters. Specifically, for both these countries, we cannot reject the null hypothesis of equal distribution of policy shocks in any two quarters.

Another issue of concern is whether the different impulse responses for activity across quarters that we document for Japan or the United States are driven by the seasonal cycle. Beaulieu and Miron (1992) trace a parallel between seasonal and business cycles and note that seasonally unadjusted data show a cycle during the calendar year.[29] Our use of seasonally adjusted data should, in principle,

[29] See also Barsky and Miron (1989).

control for the seasonal component of output. And even if such a control were imperfect, the pattern of the seasonal cycle in activity documented by Beaulieu and Miron for either Japan or the United States[30] does not conform to the pattern of our quarterly responses to the policy shock. In Japan, industrial production—our measure of activity—declines sizably in January, but resumes sharply in both February and March. As a result, the first quarter for Japan's industrial production is not a recession quarter from the perspective of the seasonal cycle. The months of April and May show a seasonal slowdown in activity, followed by some recovery in June. Overall, this is a mildly recessionary quarter from a seasonal standpoint. The third quarter is essentially neutral, because the seasonal decline in August is offset by a similar seasonal recovery of September industrial production. The fourth quarter, instead, is mildly expansionary. In contrast, our empirical findings show a weak response of activity to a policy shock in the first quarter, when the seasonal cycle is neutral if not expansionary. Moreover, the response of industrial production is particularly strong in the third quarter, a quarter which does not display a seasonal expansion. The response of output does not seem to be driven by the seasonal cycle in the United States, either. The output response is in fact large when the policy shock occurs in the first (recession) and second (expansion) quarters, and weak when the shock occurs in the third (recession) and fourth (expansion) quarters.

5 Robustness

Here, we summarize results pertaining to the robustness of our baseline specification along several dimensions.[31] As already mentioned, our benchmark reduced-form VAR (3) uses only three endogenous variables to preserve degrees of freedom at the estimation stage. It is still useful, however, to

[30] See Table 3 in their paper.

[31] In the interest of space, we provide, in this section, a discussion of our findings but we do not report the impulse responses associated with each of the robustness checks we are performing. All of the results (together with the programs and data used to generate the results), however, are available from the authors upon request.

check whether the results change significantly with the introduction of additional variables in the specification. To reduce the potential impact of small-sample bias, we add only one endogenous variable at a time. We first consider the introduction of wages as an additional variable. According to our interpretation of the baseline findings, wages play a crucial role in the transmission mechanism of monetary policy shocks to the real economy. It is then interesting to check whether the findings change when we explicitly introduce wage dynamics into the system. It turns out that the results are largely unaffected by this modification to the reduced-form VAR (3). The wage response mimics the price response, and having wages as an additional variable does not alter the pattern of the responses of real activity to the policy shock. For the United Kingdom, the introduction of wages in the VAR has the benefit of making the price responses to a shock occurring in the second half of the calendar year very similar to the price responses to a shock occurring in the first half as documented in Figures 22 and 23.

We also checked the robustness of our findings to the extended specification described by the reduced-form VAR (4). As mentioned in Section 2.2, for parts of the sample period we consider, central banks in Japan, the United States, and Europe had set monetary targets. We thus augment the baseline specification by introducing money as an additional endogenous variable, belonging to $\mathbf{Y}_2$, such that it is ordered last in the VAR.[32] The additional identification assumption we make in this case is that an interest rate shock can affect money on impact, but not vice-versa. Because of data availability, we perform this exercise only for Japan, the United States, and Germany. The quarterly responses remain very similar to the ones estimated with the baseline specification. An interesting byproduct of this exercise for the case of Japan is that prices now increase immediately following a policy shock in the first quarter. The response of prices following a shock in the third quarter, instead, is very sluggish.

[32] Using the log-level of monetary aggregates or their growth rates was of no consequence for the results.

A different robustness check concerns the importance of external constraints for a country's conduct of monetary policy. In the post-Bretton Woods era, France and the United Kingdom had, to different extents and over different periods, some form of exchange rate management. In contrast, exchange rate management was not a predominant concern for the conduct of monetary policy in Japan, the United States, or Germany. Therefore, we checked whether the baseline results for France and the United Kingdom change when we introduce the country's exchange rate vis-à-vis the Deutsche Mark as an additional variable. We order the exchange rate last in the VAR, with the identifying assumption that an interest rate shock can affect the exchange rate on impact, but not vice-versa. Our baseline findings are unaffected by the inclusion of the exchange rate in the VAR. This finding is not very surprising. It is hard to firmly tie changes in the exchange rate to future changes in activity and prices, especially after controlling for changes in the policy rate. As a result, the reduced-form dynamics for activity and prices is little influenced by the introduction of the exchange rate as an additional variable. Changing the identification scheme to have the exchange rate ordered next-to-last and the policy rate last yields to similar results.[33] Another way of modelling the external constraint for France and the United Kingdom is to introduce Germany's policy rate as an explanatory variable.[34] Since macroeconomic events in France and the United Kingdom are unlikely to have affected policy decisions in Germany, the German Lombard rate can be introduced in the VAR specification (4) as an exogenous variable belonging to $\mathbf{Z}$. Again, the results are not materially affected by having this control.

Finally, we have noted when illustrating the findings from our baseline specification that several price impulse-responses exhibit a noticeable price puzzle. We checked whether the inclusion of an

[33] This ordering embeds the identifying assumption that exchange rate shocks affect the policy rate on impact, but not vice versa. Both proposed identification schemes represent limiting (and unrealistic) cases. However, the insensitivity of the findings to the alternative ordering somewhat alleviates concerns about identification.

[34] Of course, the two approaches are not mutually exclusive and, indeed, the best strategy would consist in having both the exchange rate and the foreign policy interest rate as additional variables in the reduced-form VAR. As mentioned in the text, we introduce just one variable at a time to preserve degrees of freedom at the estimation stage.

exogenous variable measuring oil prices or commodity prices helps to mitigate the price puzzle. With the exception of the United States and the United Kingdom,[35] these control variables provides little improvement in the price responses, while the responses of activity are not affected.

Overall, our baseline findings appear robust to the introduction of additional variables in the specification. As concerns robustness to the sample period, we checked that starting the sample in 1973 – and thus eliminating most of the Bretton Woods years – does not affect the results. In this case, the impulse responses are often estimated more imprecisely and the price puzzle becomes more pronounced in the case of Japan.

The chosen estimation method is also of little consequence to our findings. Estimation of the reduced-form VAR with unconstrained OLS on four lags yields estimated impulse responses that are similar to the ones obtained from estimating the reduced-form VAR with polynomial distributed lags; the only difference is that the responses with the unconstrained OLS procedure are estimated with less precision.

6 Concluding Remarks

Our main conclusions have been amply foreshadowed. We found that the degree of bunching of wage setting decisions matters for the transmission of monetary policy to the real economy. In Japan, wage setting has conformed to a synchronized pattern in the form of the annual Shunto and the associated process of collective bargaining. In the United States, various sources of anecdotal evidence point to a significant fraction of firms adjusting wages in the last quarter of the calendar year. One critical implication of this synchronized annual wage setting is that if preset wages are

[35] As already mentioned, the impulse responses reported for the United States are estimated from a VAR that includes commodity prices as an additional endogenous variable in $\mathbf{Y}$. Responses for the United Kingdom are estimated from a VAR that includes oil prices as an exogenous variable in $\mathbf{Z}$.

important in accounting for the connection between monetary policy and real activity at business cycle frequencies, then the transmission of a monetary impulse to the real economy should differ according to when the impulse occurs within a calendar year. Specifically, a shock in Japan that occurs in the first part of the year, i.e. when the Shunto is taking place, should have a small impact on output, since this is a period of relative wage flexibility. In contrast, a shock occurring later in the calendar year should have a larger impact on real activity, because at this time of the year wages are relatively rigid. In the United States, the timing should be reversed: A shock in the first half of the calendar year should have a relatively large impact on real activity, whereas a shock in the second half should have a smaller impact. An empirical analysis of the transmission of monetary policy shocks to the real economy based on a quarter-dependent VAR supports this claim for Japan and the United States. We contrast the empirical findings for Japan and the United States with those for Germany, France, and the United Kingdom. In these countries, synchronization in wage setting has been low, with wage bargaining almost uniformly distributed across the calendar year and wage contracts often lasting for longer than a year. Correspondingly, the response of activity to a monetary policy shock has been relatively uniform across quarters.

In this paper we make no claim as to whether synchronization of wage changes is preferable to uniform staggering. This is a problem that has been studied in the past, and the general finding of this literature is that synchronization is the equilibrium timing in many simple Keynesian models of the business cycle.[36] Yet, the new generation of Keynesian models has glossed over this finding and assumed uniform staggering as both a convenient modelling tool and an essential element in the transmission mechanism of monetary policy shocks. This paper notes that while uniform staggering

[36] Ball and Cecchetti (1988) show that staggering can be the equilibrium outcome in some settings with imperfect information, but even then, such a result is not necessarily pervasive, since it depends on the structure of the market in which firms compete and on firms setting prices for a very short period of time. In other settings, staggering can be the optimal outcome for wage negotiations if the number of firms is very small (see Fethke and Policano 1986). The incentive for firms to stagger wage negotiation dates, however, diminishes the larger the number of firms in an economy.

may be a realistic assumption for some countries, it is not for others. For these other countries, the empirical implications of non-uniform wage staggering can be important and should be taken seriously from a modelling standpoint.

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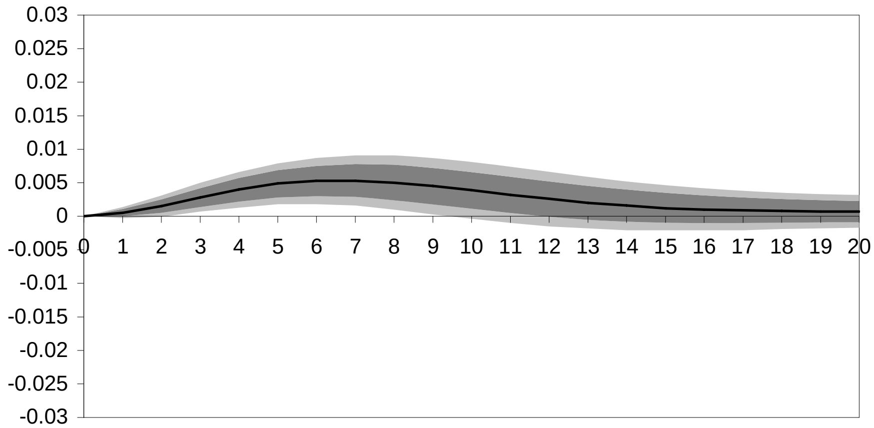
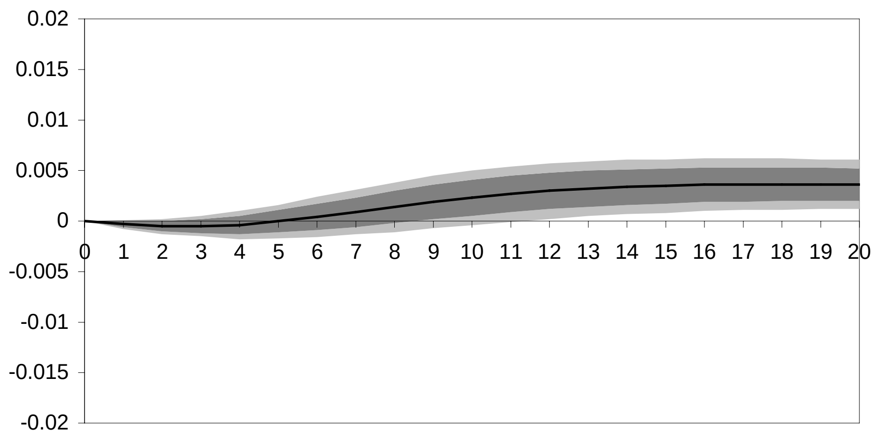
FIGURE 1

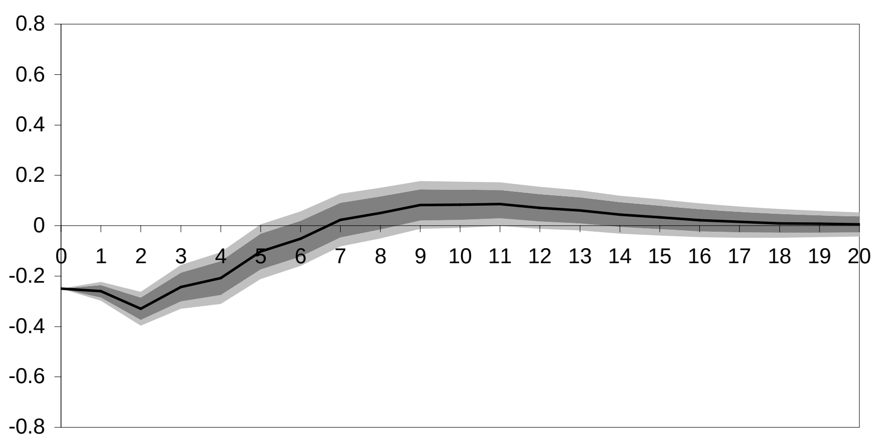
Japan
25-Basis-Point Decline in Call Rate
No Quarterly Dependence. 1963:Q1 to 1995:Q2



Response of Industrial Production



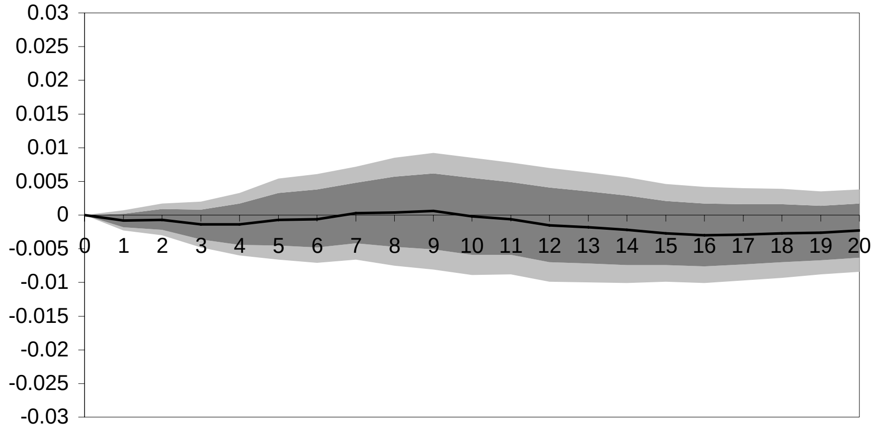
Response of Consumer Price Index



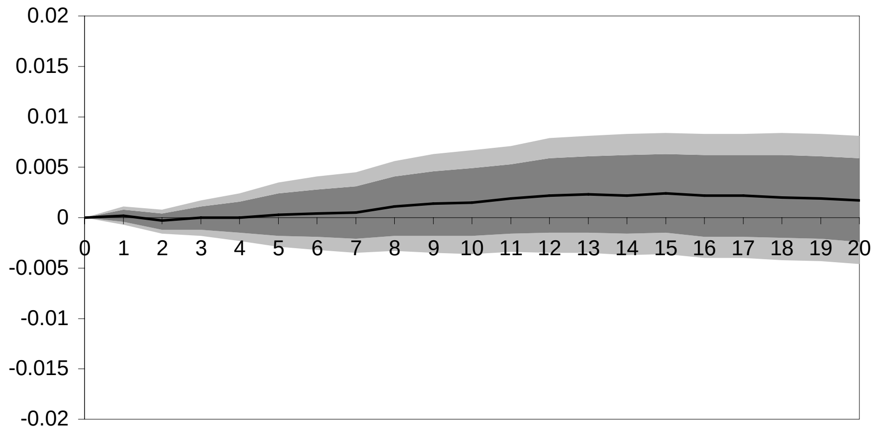
Response of Interest Rate

FIGURE 2

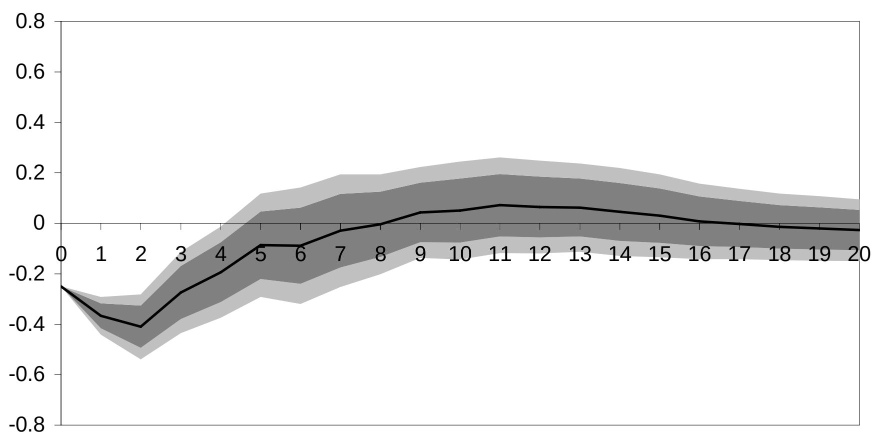
Japan
25-Basis-Point Decline in Call Rate in Q1
Quarterly Dependence. 1963:Q1 to 1995:Q2



Response of Industrial Production



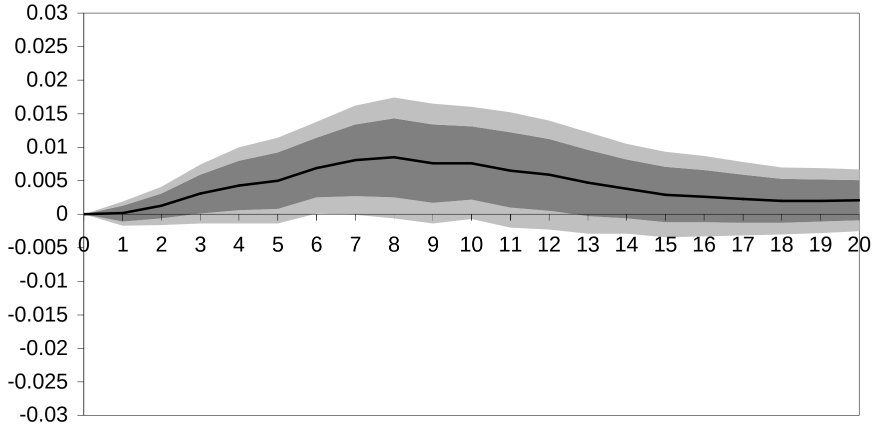
Response of Consumer Price Index



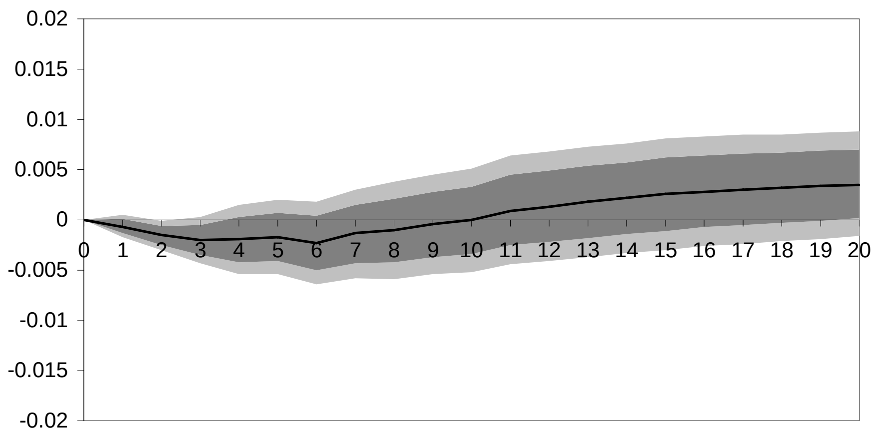
Response of Interest Rate

FIGURE 3

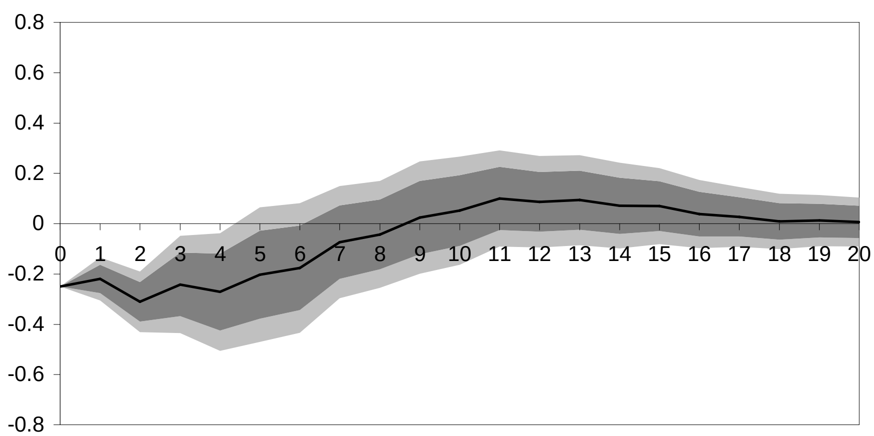
Japan
25-Basis-Point Decline in Call Rate in Q2
Quarterly Dependence. 1963:Q1 to 1995:Q2



Response of Industrial Production



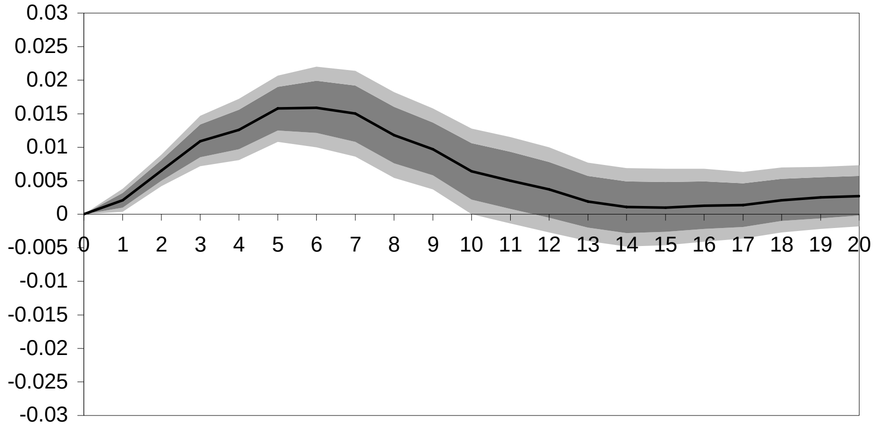
Response of Consumer Price Index



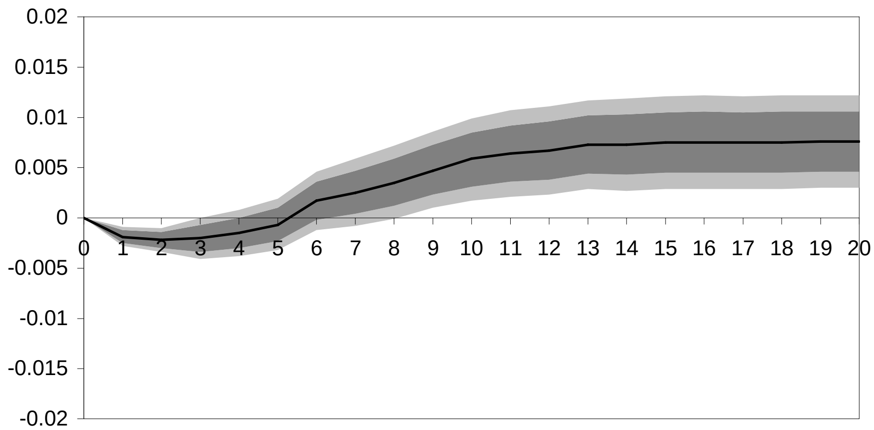
Response of Interest Rate

FIGURE 4

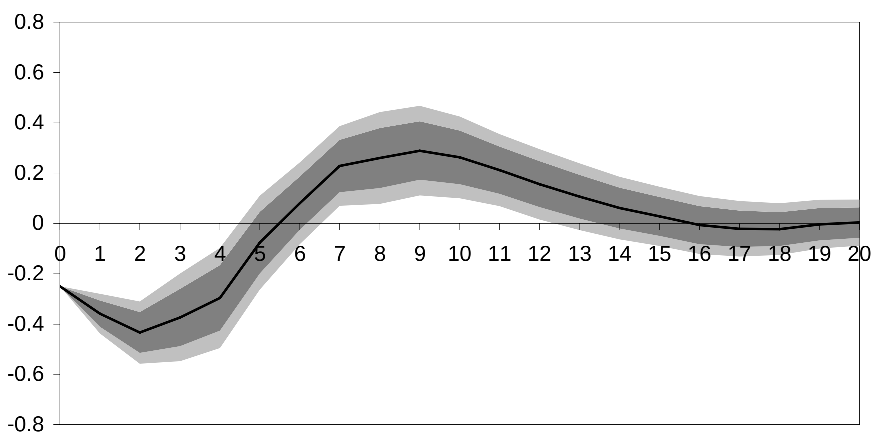
Japan
25-Basis-Point Decline in Call Rate in Q3
Quarterly Dependence. 1963:Q1 to 1995:Q2



Response of Industrial Production



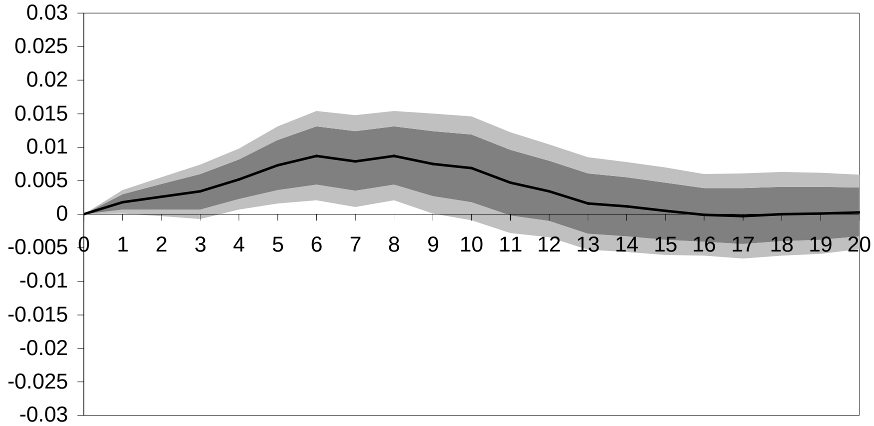
Response of Consumer Price Index



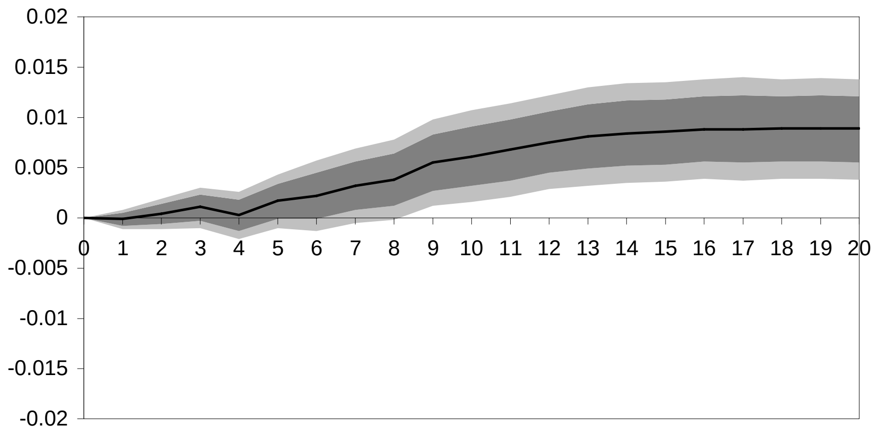
Response of Interest Rate

FIGURE 5

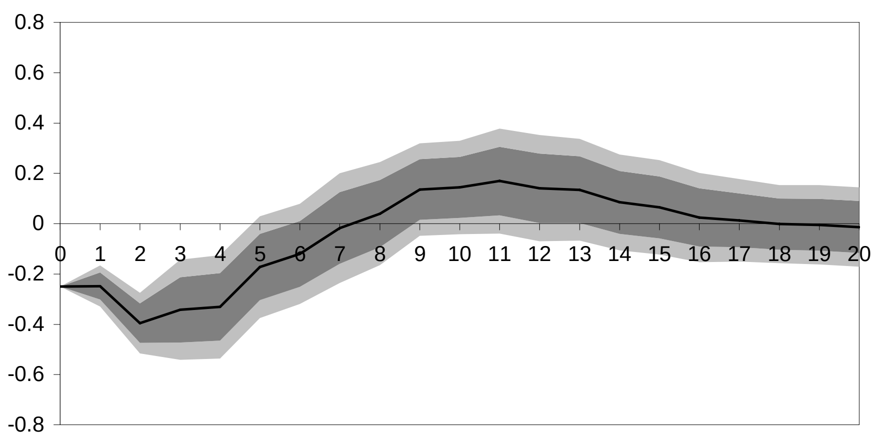
Japan
25-Basis-Point Decline in Call Rate in Q4
Quarterly Dependence. 1963:Q1 to 1995:Q2



Response of Industrial Production



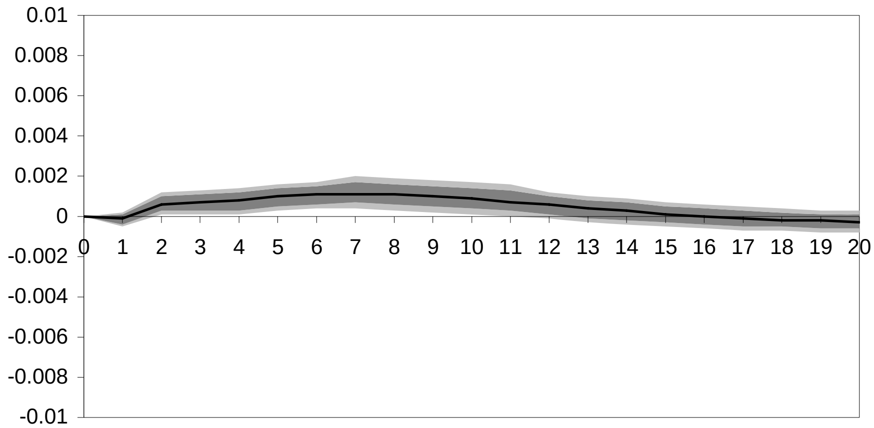
Response of Consumer Price Index



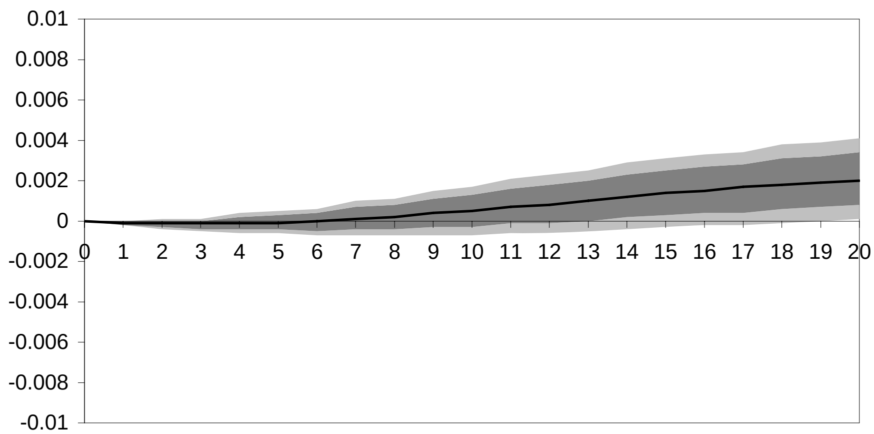
Response of Interest Rate

FIGURE 6

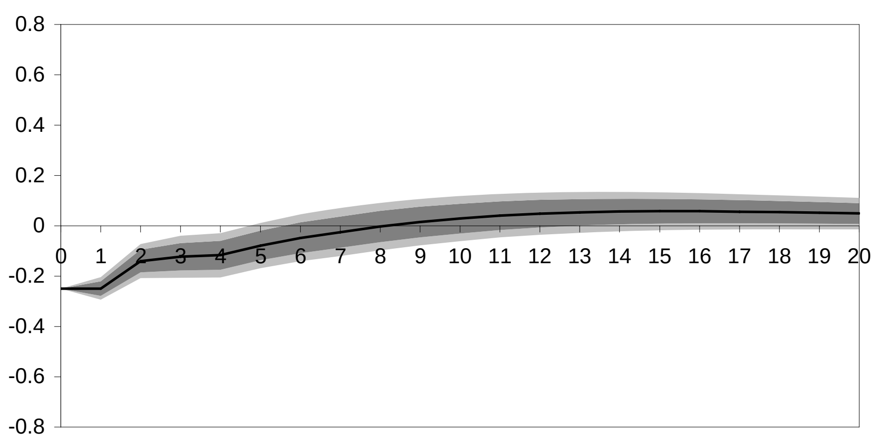
United States
25-Basis-Point Decline in Federal Funds Rate
No Quarterly Dependence. 1966:Q1 to 2002:Q4



Response of GDP



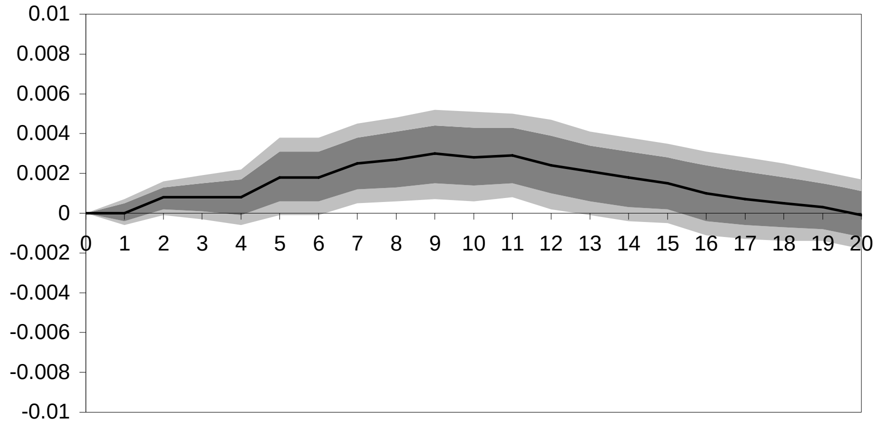
Response of GDP Deflator



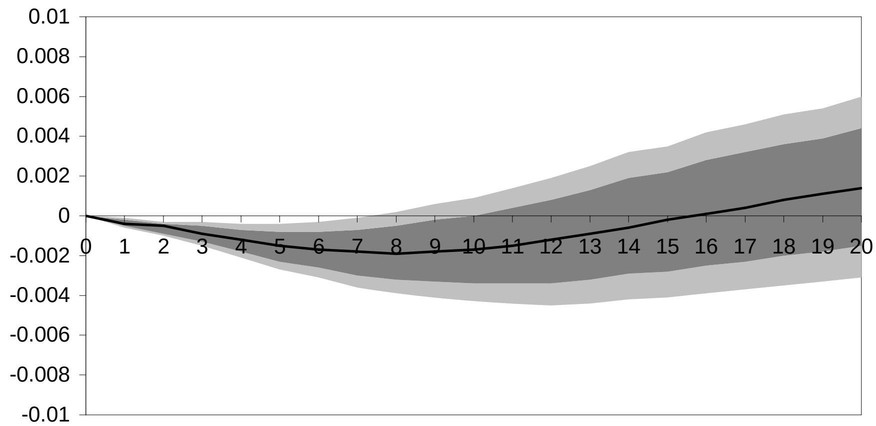
Response of Interest Rate

FIGURE 7

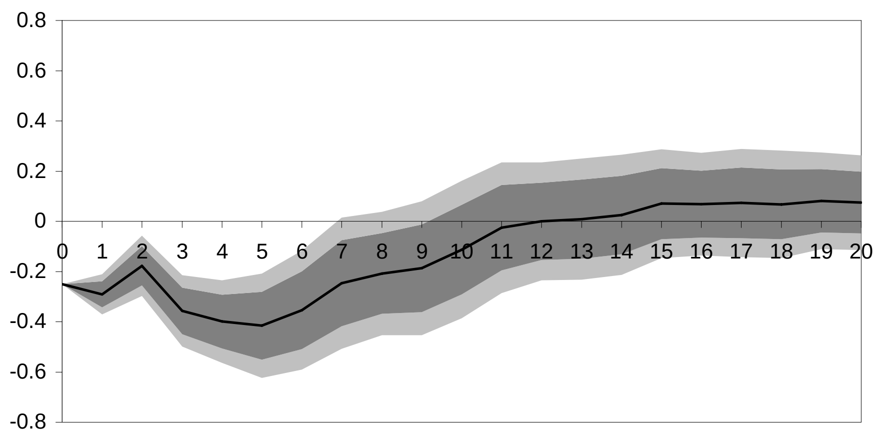
United States
25-Basis-Point Decline in Federal Funds Rate in Q1
Quarterly Dependence. 1966:Q1 to 2002:Q4



Response of GDP



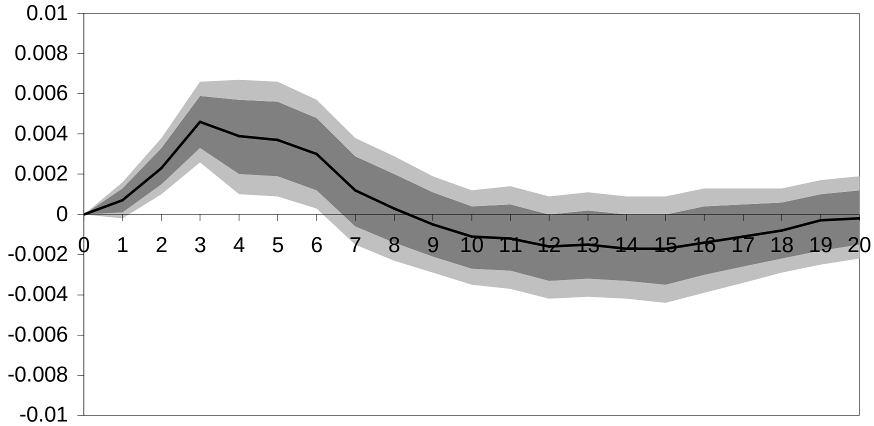
Response of GDP Deflator



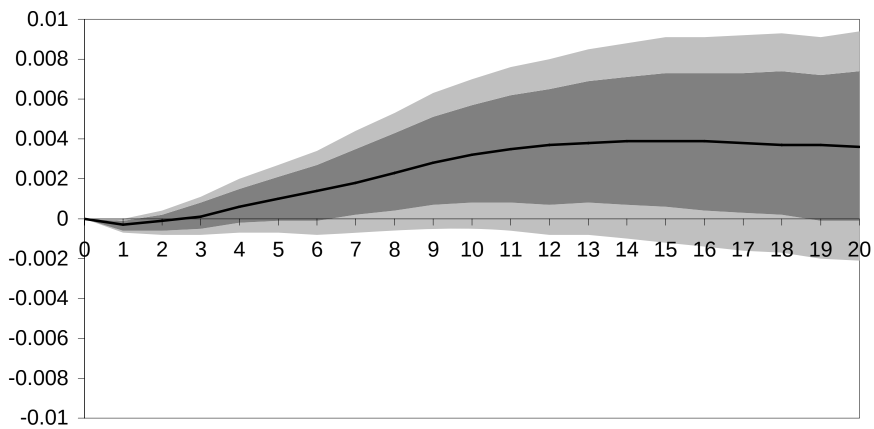
Response of Interest Rate

FIGURE 8

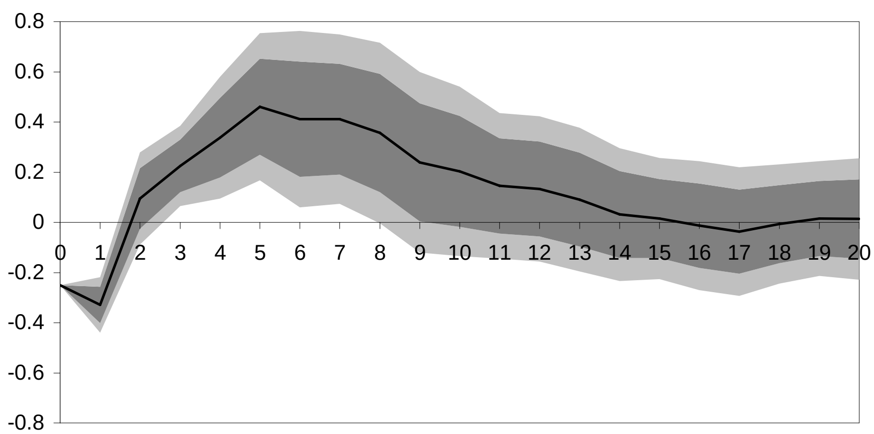
United States
25-Basis-Point Decline in Federal Funds Rate in Q2
Quarterly Dependence. 1966:Q1 to 2002:Q4



Response of GDP



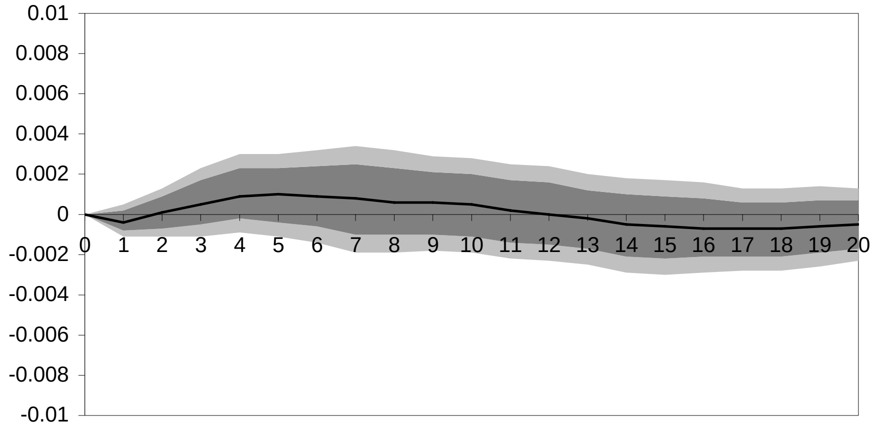
Response of GDP Deflator



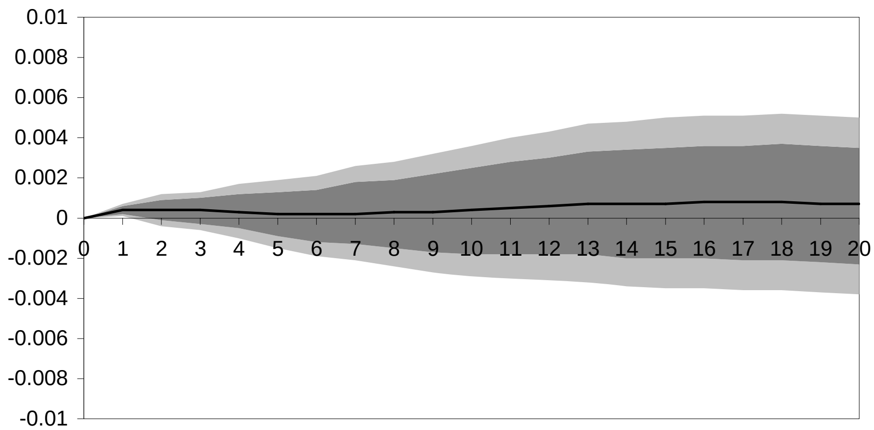
Response of Interest Rate

FIGURE 9

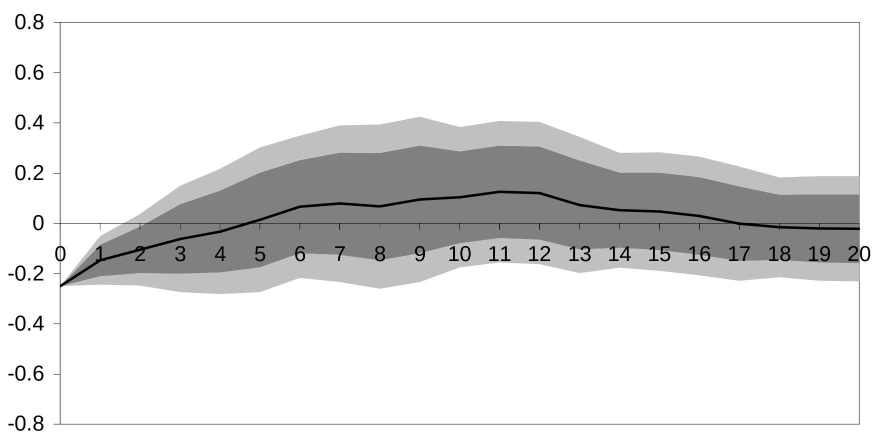
United States
25-Basis-Point Decline in Federal Funds Rate in Q3
Quarterly Dependence. 1966:Q1 to 2002:Q4



Response of GDP



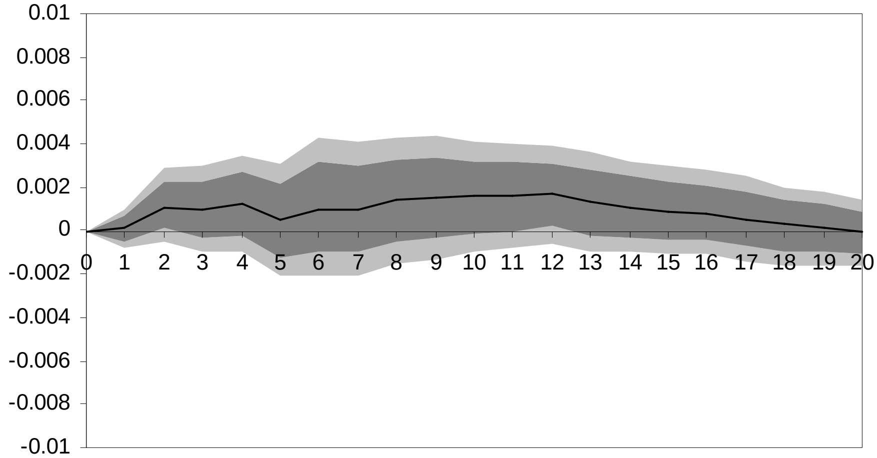
Response of GDP Deflator



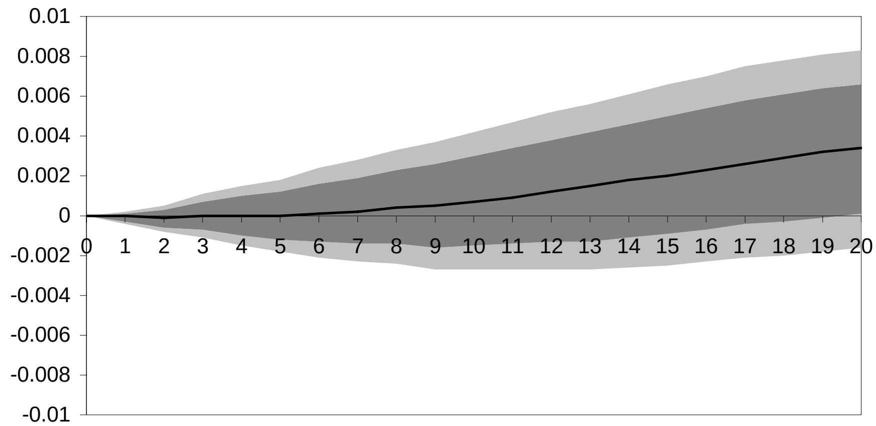
Response of Interest Rate

FIGURE 10

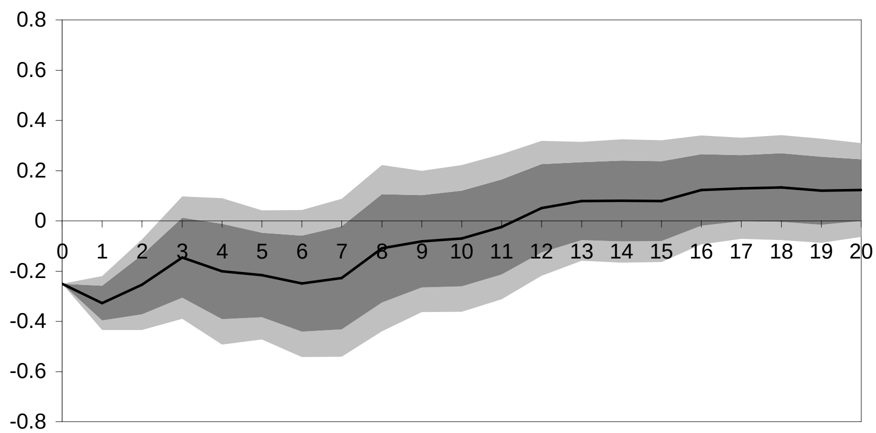
United States
25-Basis-Point Decline in Federal Funds Rate in Q4
Quarterly Dependence. 1966:Q1 to 2002:Q4



Response of GDP



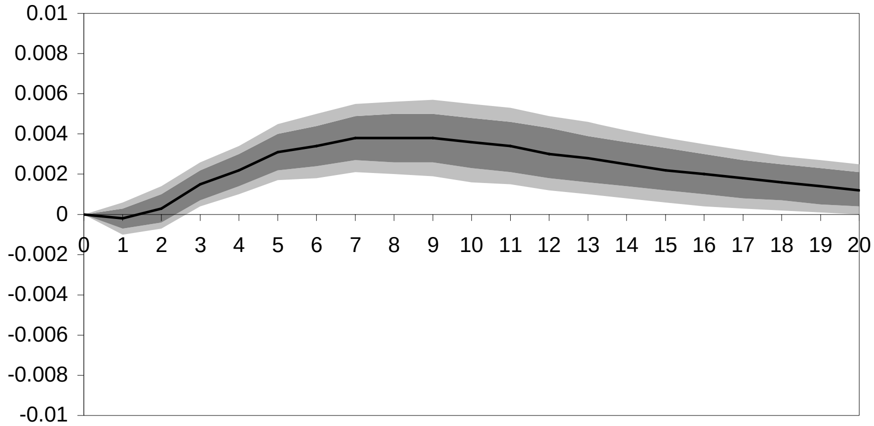
Response of GDP Deflator



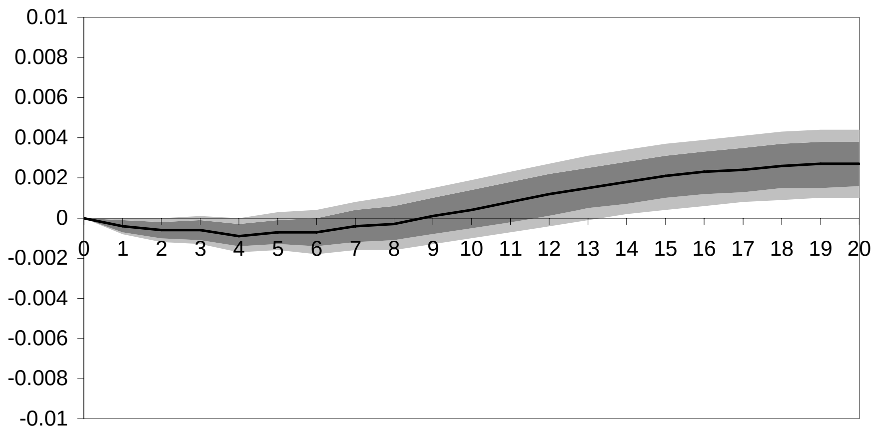
Response of Interest Rate

FIGURE 11

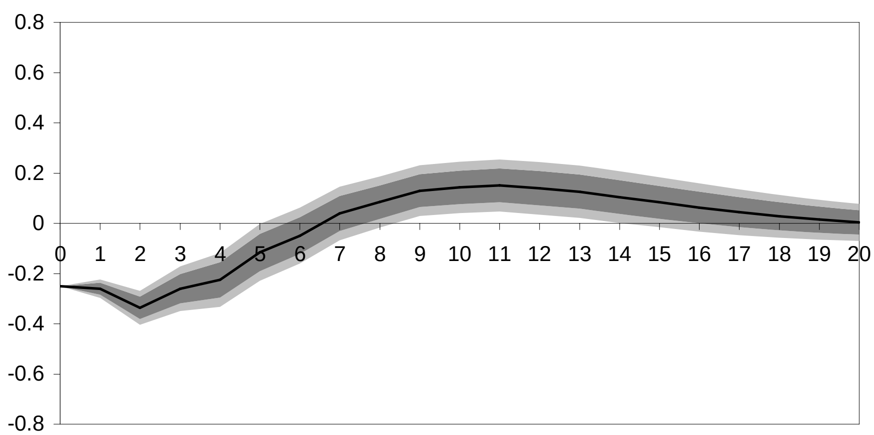
Germany
25-Basis-Point Decline in Lombard Rate
No Quarterly Dependence. 1963:Q1 to 1994:Q4



Response of GDP



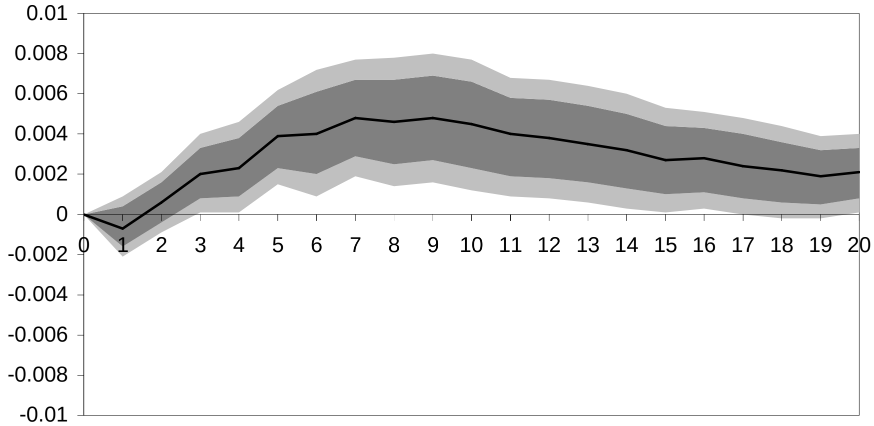
Response of GDP Deflator



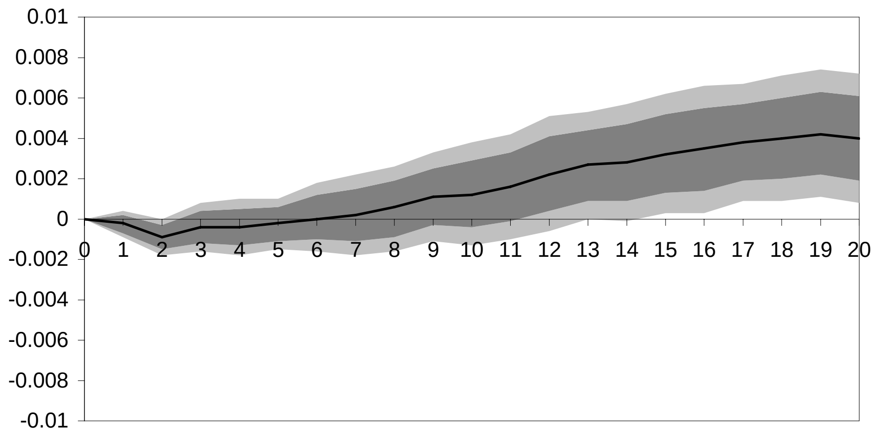
Response of Interest Rate

FIGURE 12

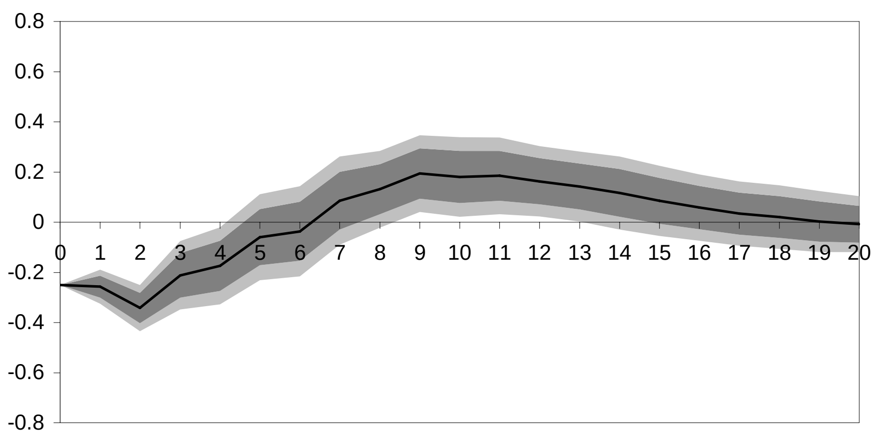
Germany
25-Basis-Point Decline in Lombard Rate in Q1
Quarterly Dependence. 1963:Q1 to 1994:Q4



Response of GDP



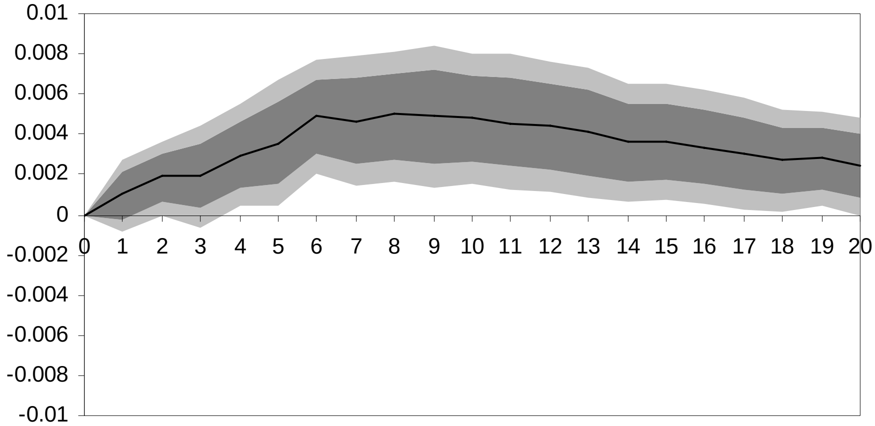
Response of GDP Deflator



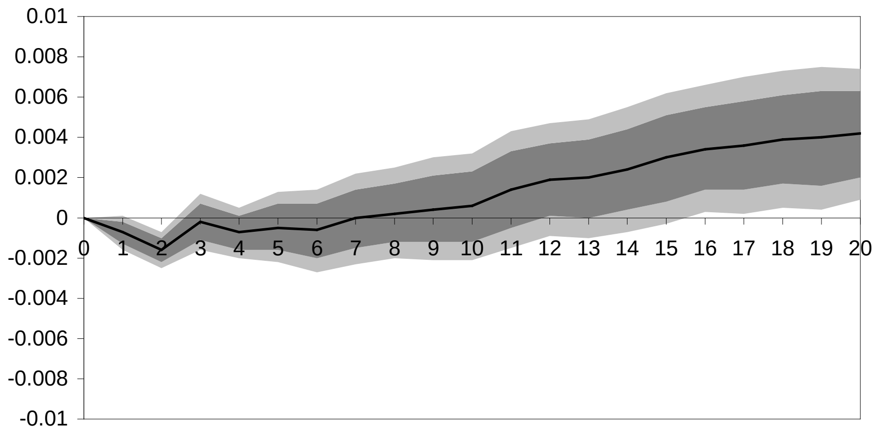
Response of Interest Rate

FIGURE 13

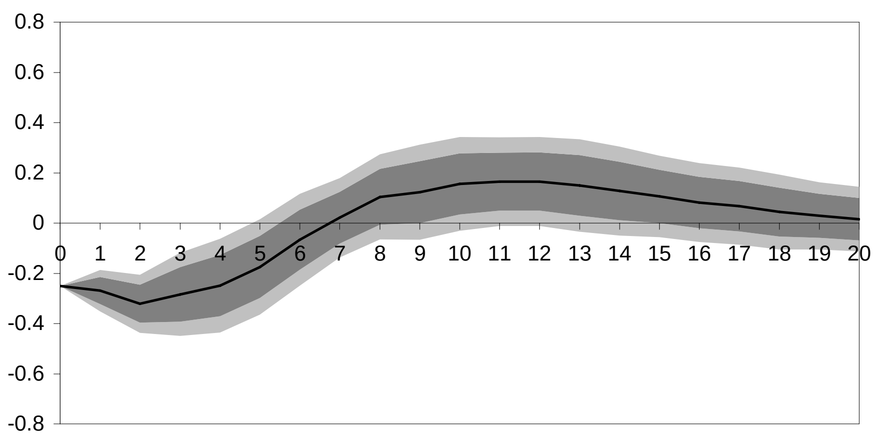
Germany
25-Basis-Point Decline in Lombard Rate in Q2
Quarterly Dependence. 1963:Q1 to 1994:Q4



Response of GDP



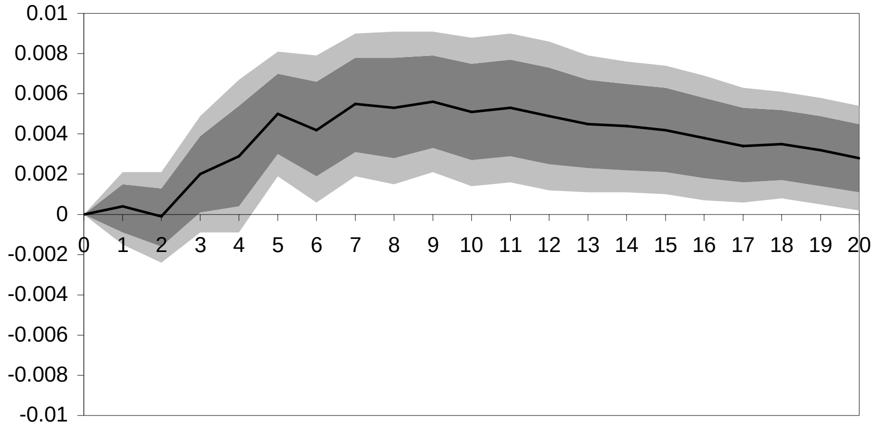
Response of GDP Deflator



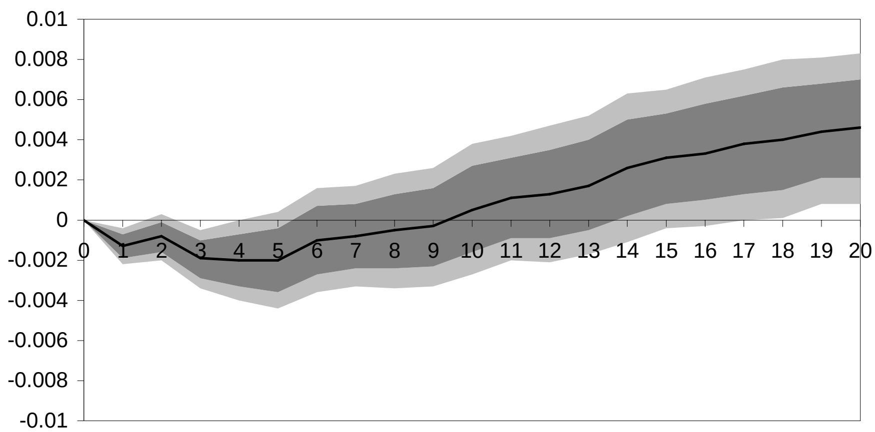
Response of Interest Rate

FIGURE 14

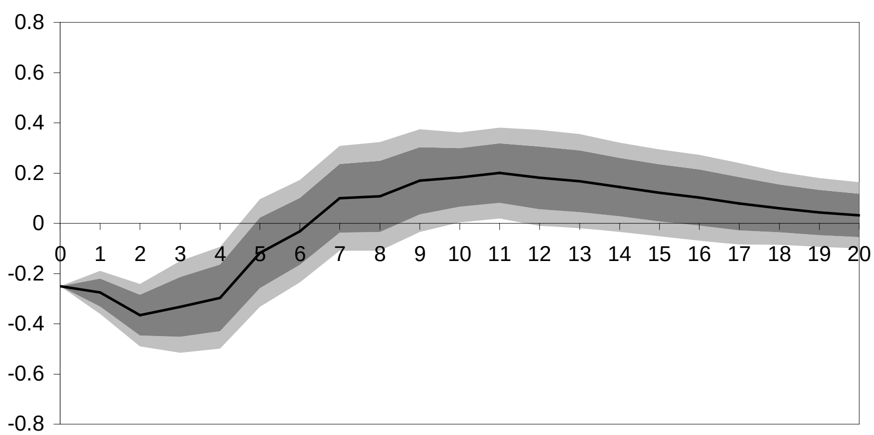
Germany
25-Basis-Point Decline in Lombard Rate in Q3
Quarterly Dependence. 1963:Q1 to 1994:Q4



Response of GDP



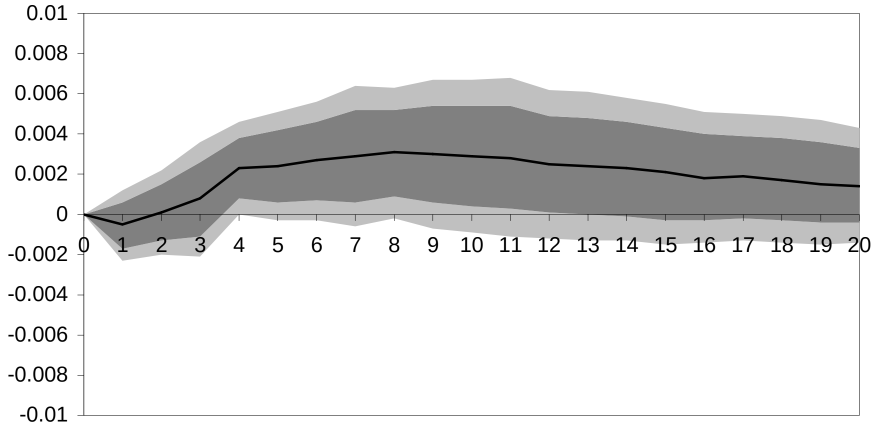
Response of GDP Deflator



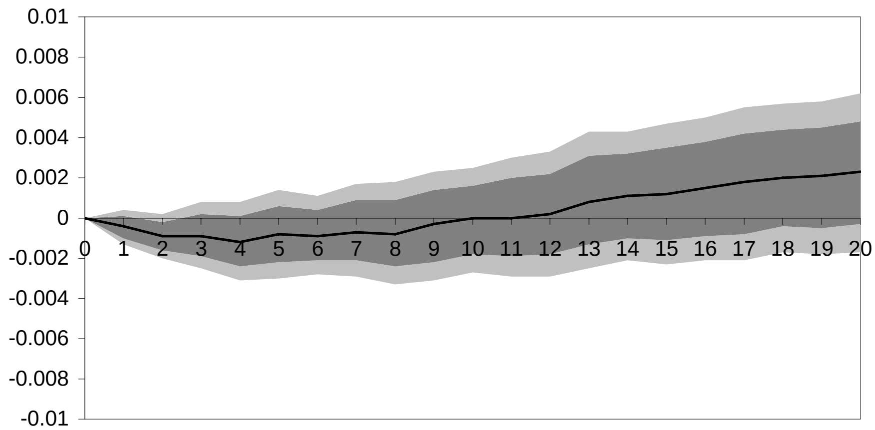
Response of Interest Rate

FIGURE 15

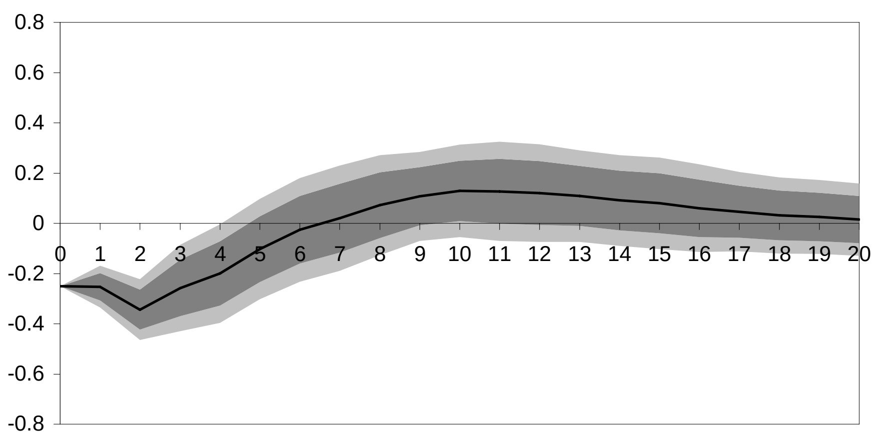
Germany
25-Basis-Point Decline in Lombard Rate in Q4
Quarterly Dependence. 1963:Q1 to 1994:Q4



Response of GDP



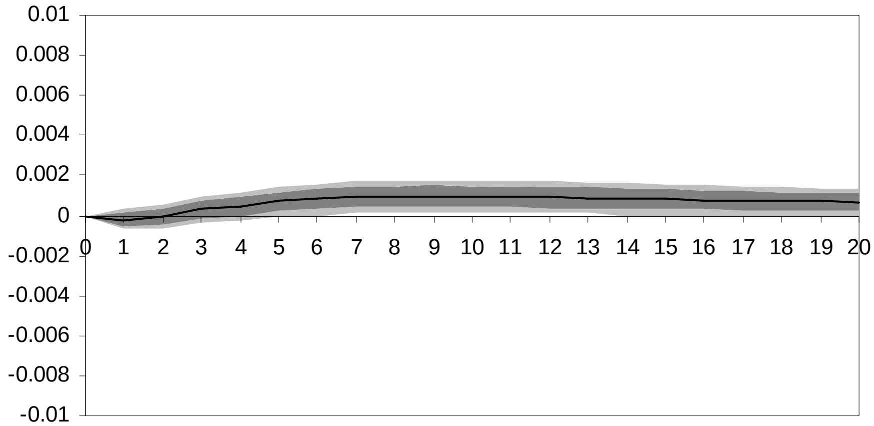
Response of GDP Deflator



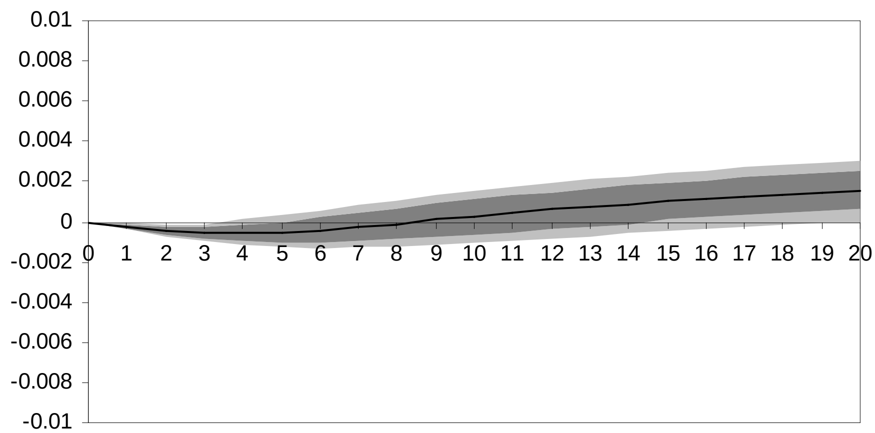
Response of Interest Rate

FIGURE 16

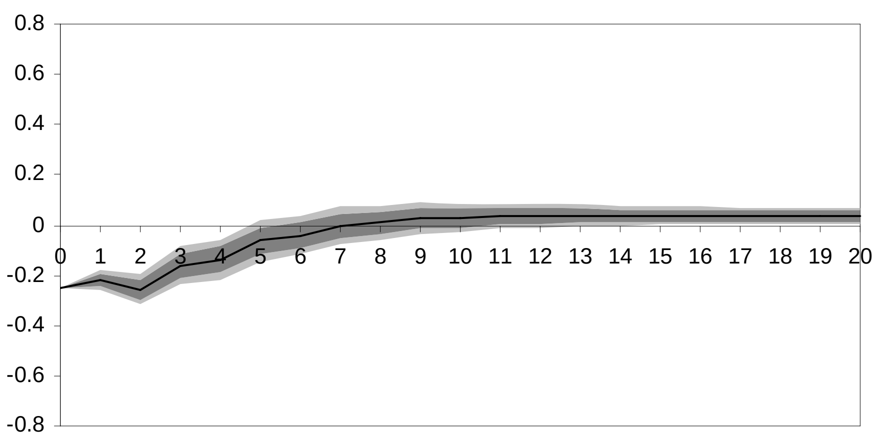
France
25-Basis-Point Decline in Call Rate
No Quarterly Dependence. 1963:Q1 to 1998:Q4



Response of GDP



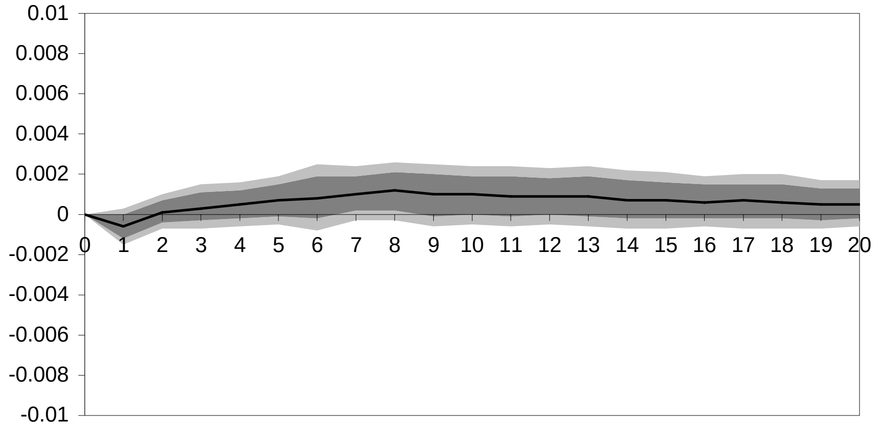
Response of Consumer Price Index



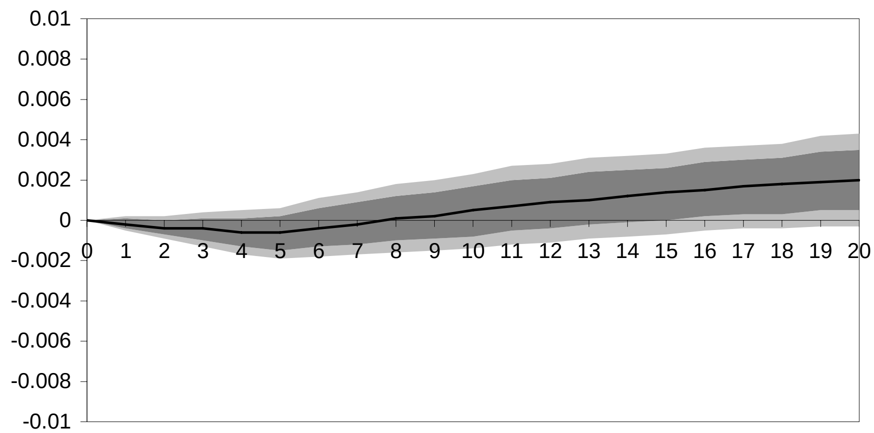
Response of Interest Rate

FIGURE 17

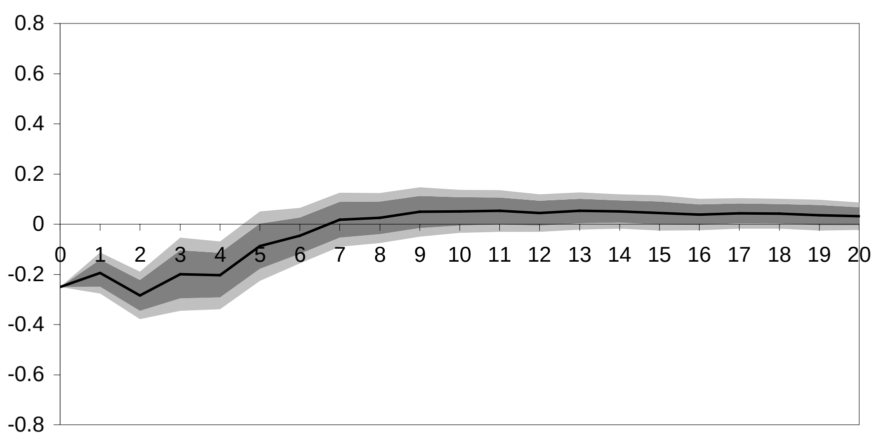
France
25-Basis-Point Decline in Call Rate in Q1
Quarterly Dependence. 1963:Q1 to 1998:Q4



Response of GDP



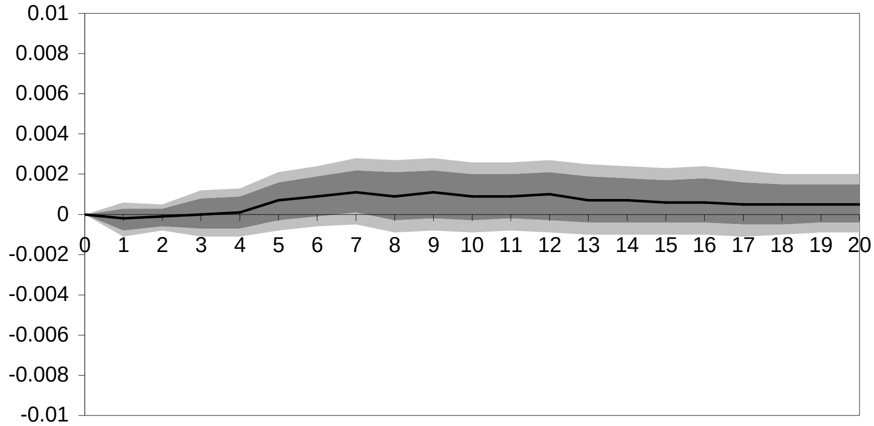
Response of Consumer Price Index



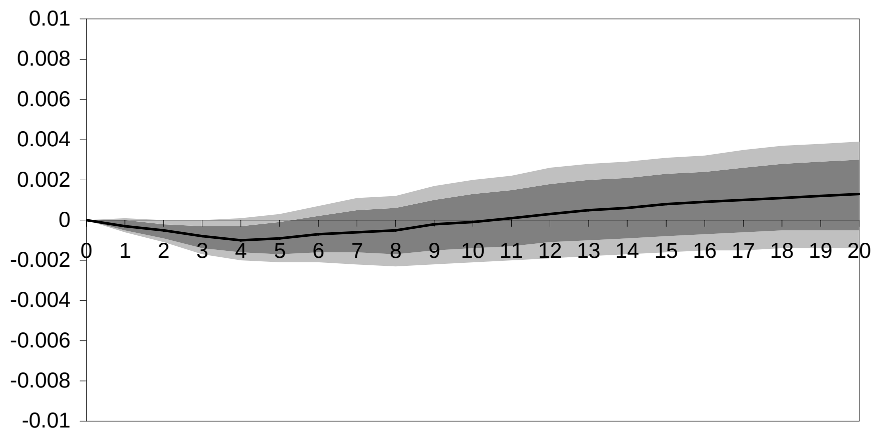
Response of Interest Rate

FIGURE 18

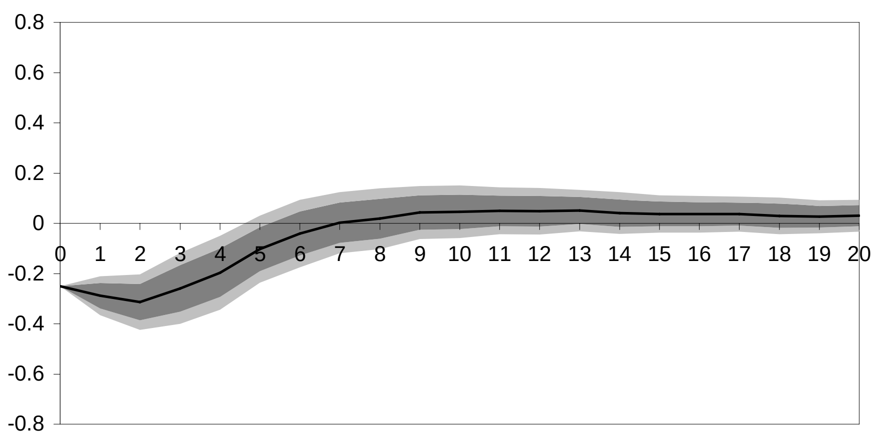
France
25-Basis-Point Decline in Call Rate in Q2
Quarterly Dependence. 1963:Q1 to 1998:Q4



Response of GDP



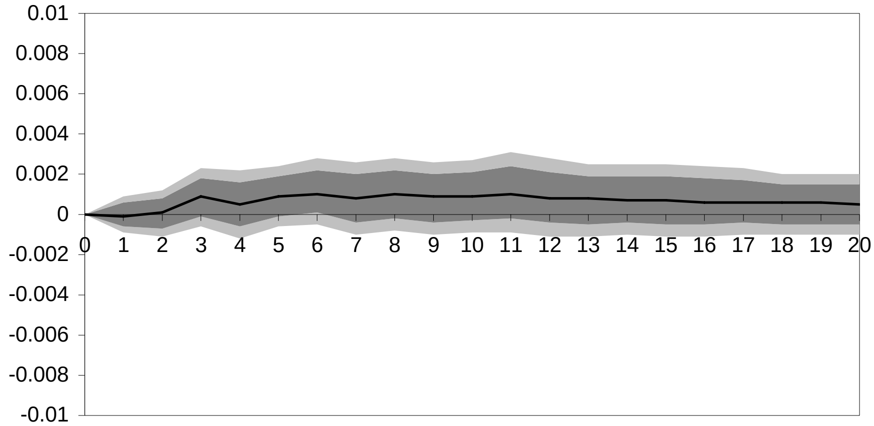
Response of Consumer Price Index



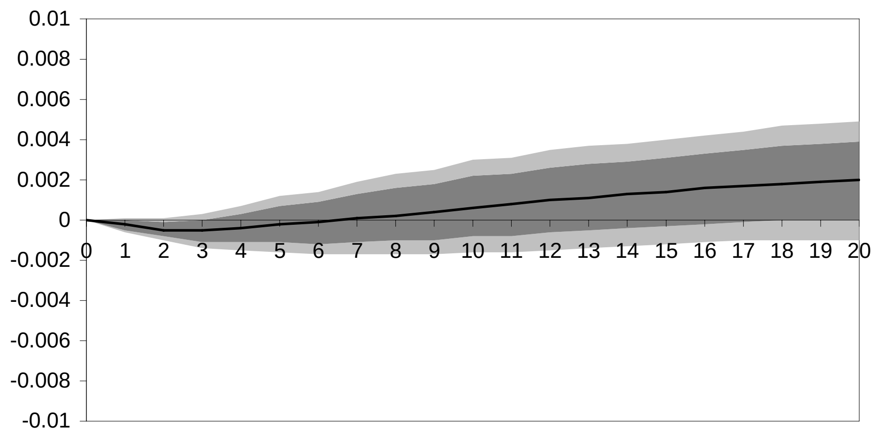
Response of Interest Rate

FIGURE 19

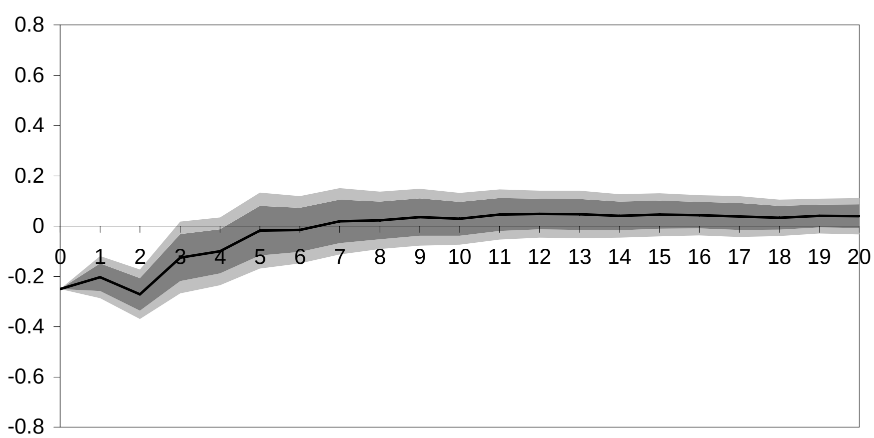
France
25-Basis-Point Decline in Call Rate in Q3
Quarterly Dependence. 1963:Q1 to 1998:Q4



Response of GDP



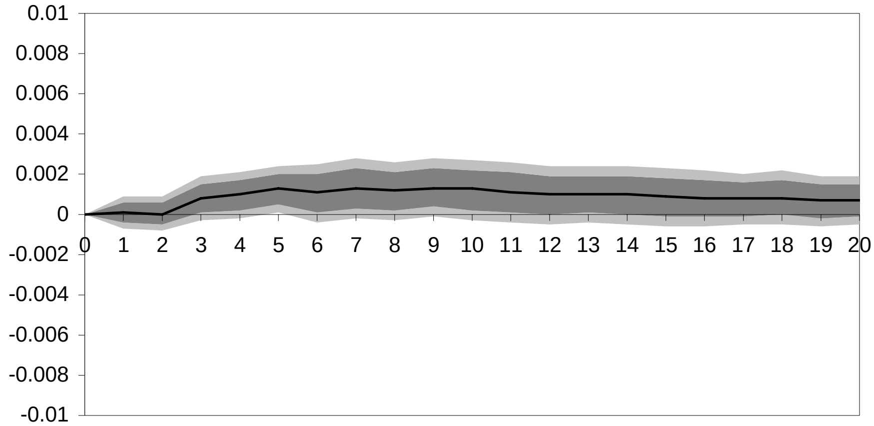
Response of Consumer Price Index



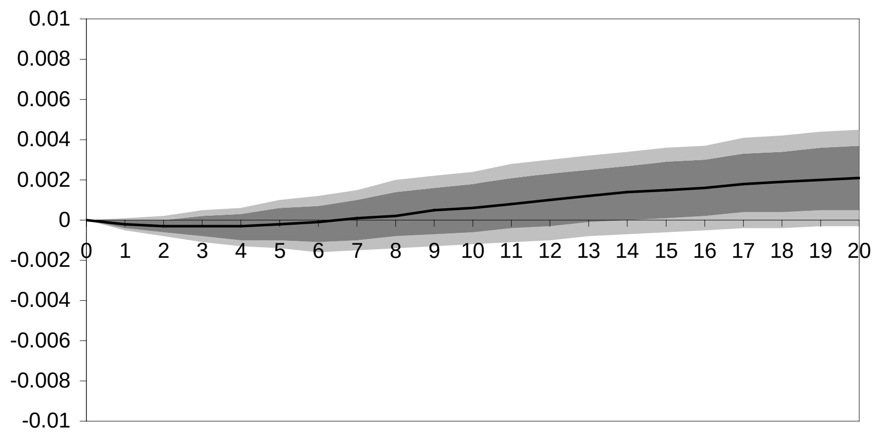
Response of Interest Rate

FIGURE 20

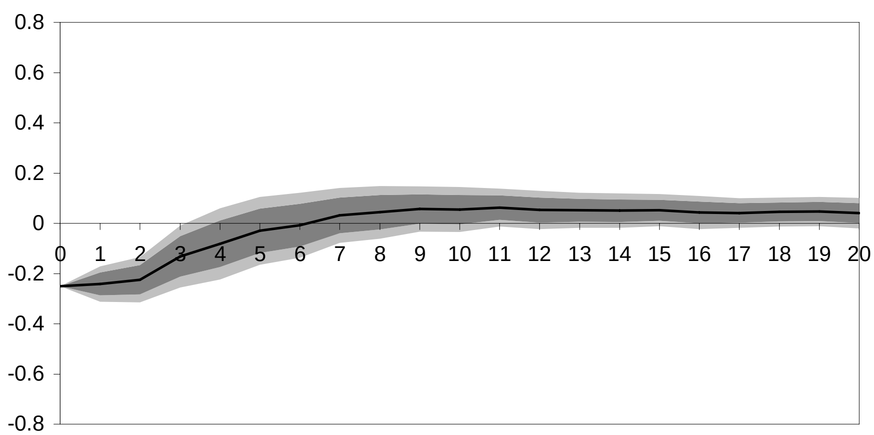
France
25-Basis-Point Decline in Call Rate in Q4
Quarterly Dependence. 1963:Q1 to 1998:Q4



Response of GDP



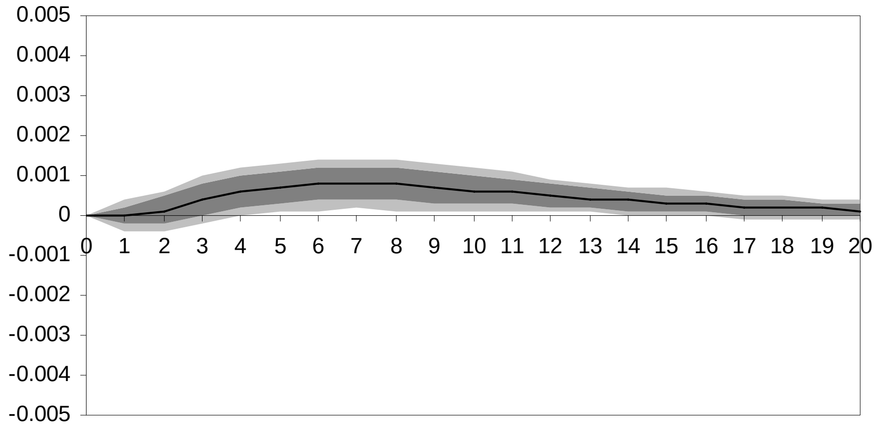
Response of Consumer Price Index



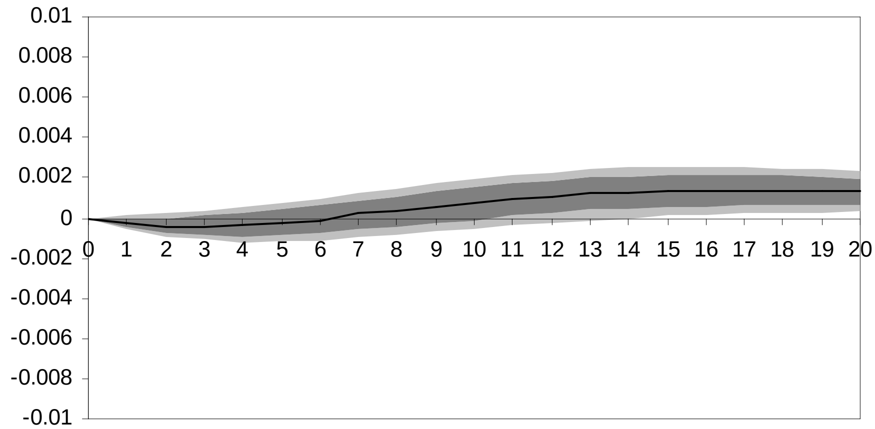
Response of Interest Rate

FIGURE 21

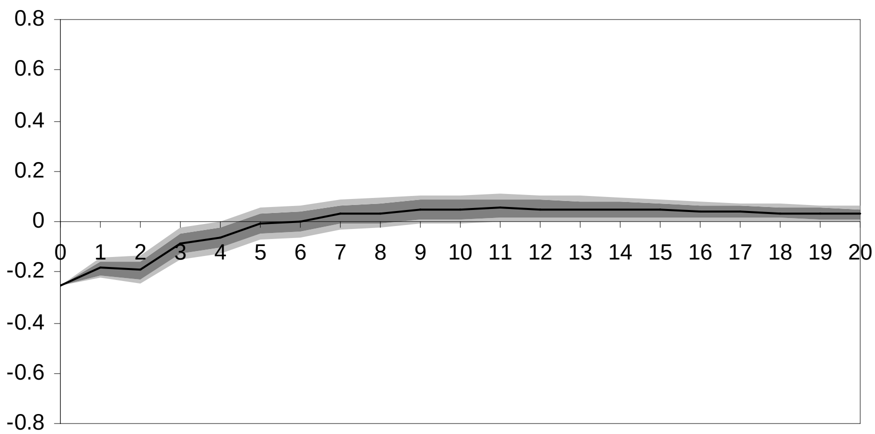
United Kingdom
25-Basis-Point Decline in Three-Month Treasury Bill Rate
No Quarterly Dependence. 1963:Q1 to 1997:Q1



Response of GDP



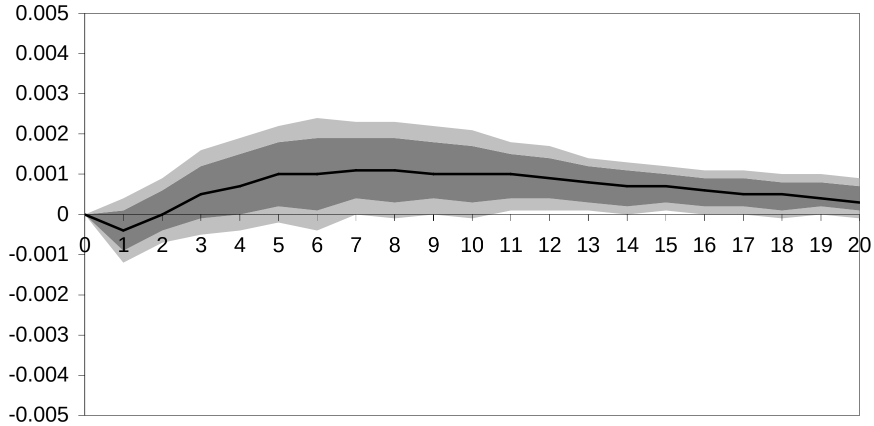
Response of Consumer Price Index



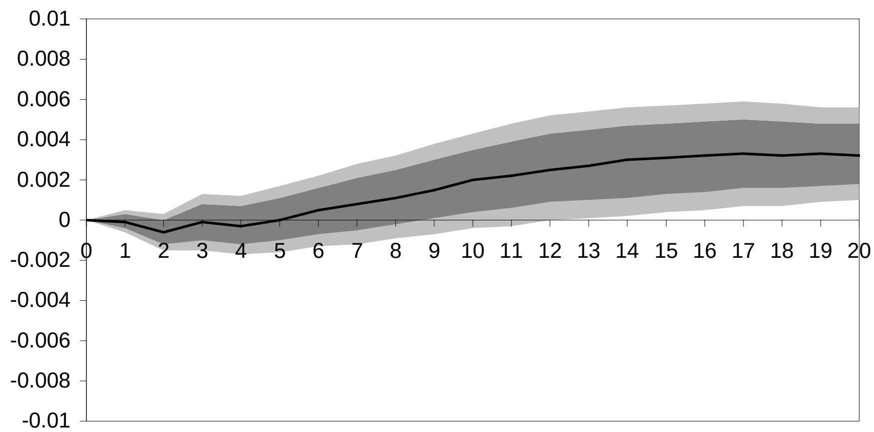
Response of Interest Rate

FIGURE 22

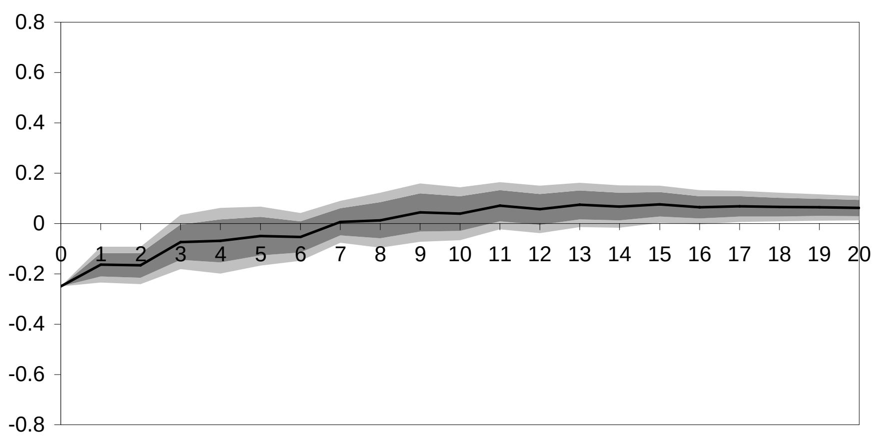
United Kingdom
25-Basis-Point Decline in Three-Month Treasury Bill Rate in Q1
Quarterly Dependence. 1963:Q1 to 1997:Q1



Response of GDP



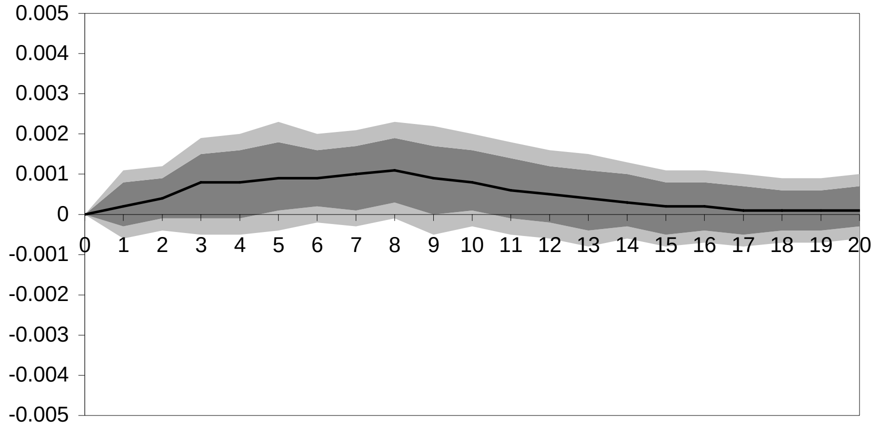
Response of Consumer Price Index



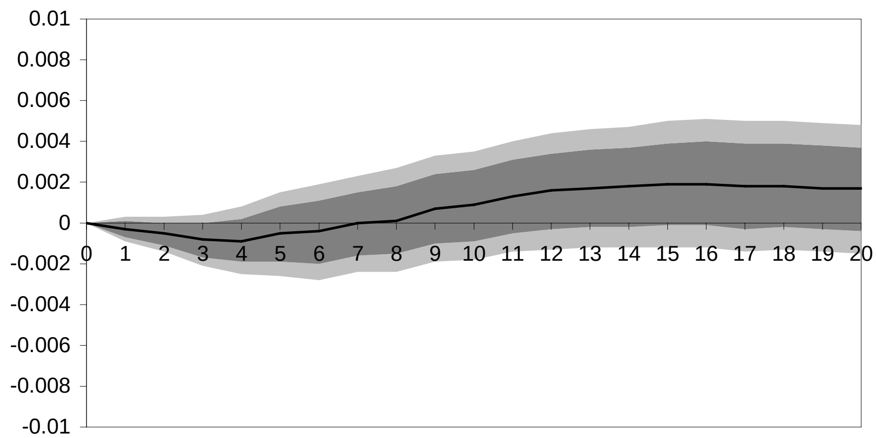
Response of Interest Rate

FIGURE 23

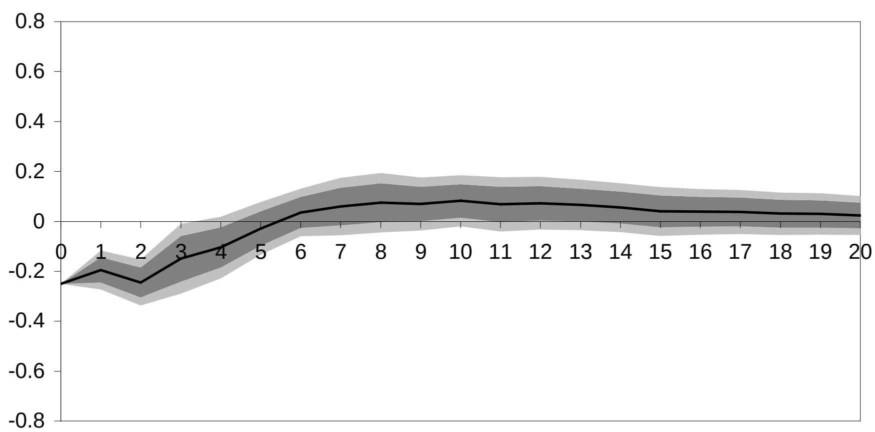
United Kingdom
25-Basis-Point Decline in Three-Month Treasury Bill Rate in Q2
Quarterly Dependence. 1963:Q1 to 1997:Q1



Response of GDP



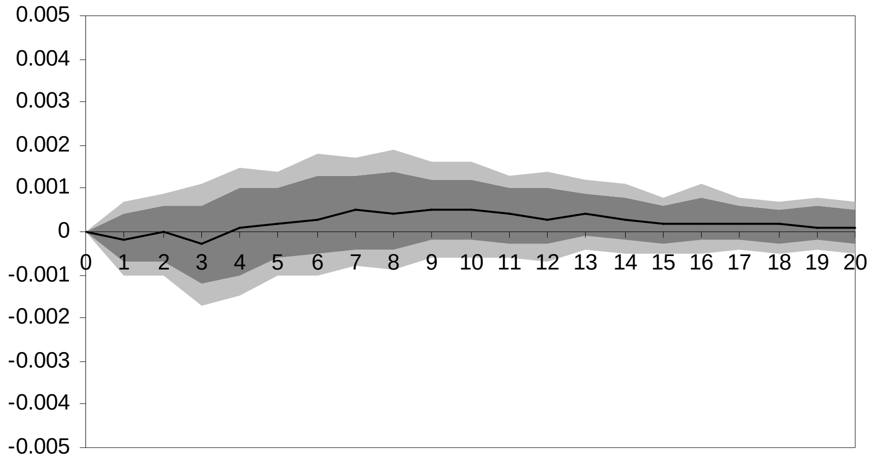
Response of Consumer Price Index



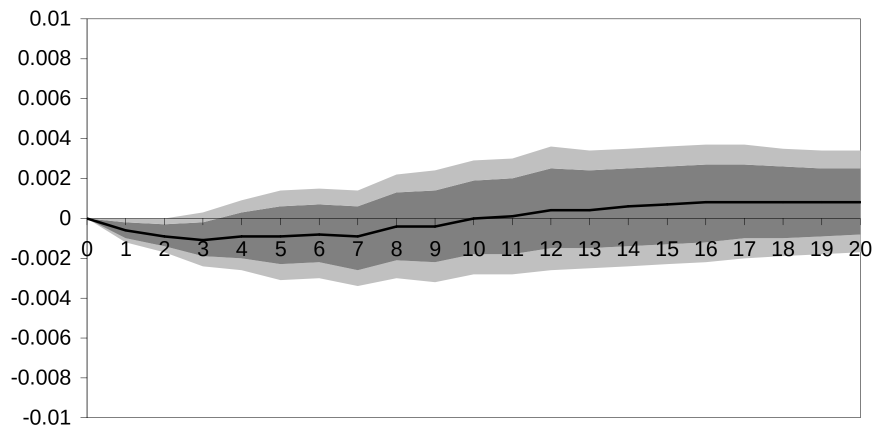
Response of Interest Rate

FIGURE 24

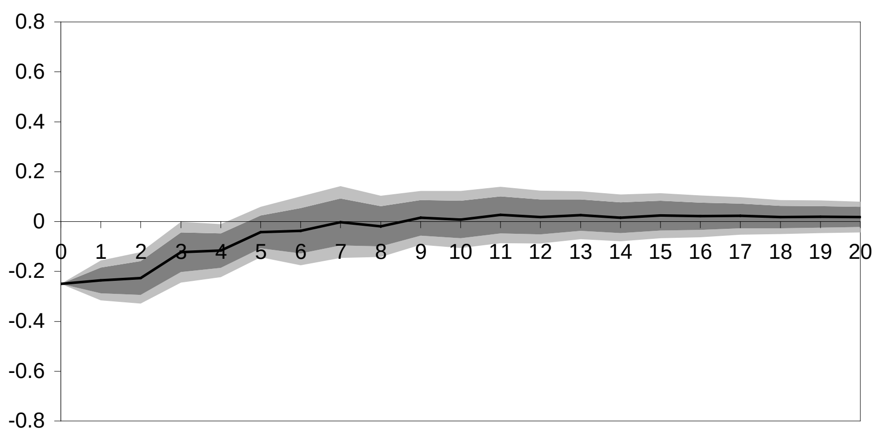
United Kingdom
25-Basis-Point Decline in Three-Month Treasury Bill Rate in Q3
Quarterly Dependence. 1963:Q1 to 1997:Q1



Response of GDP



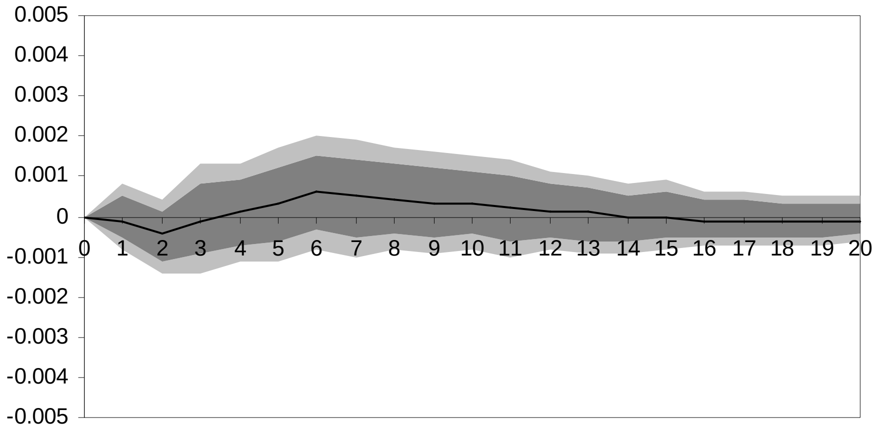
Response of Consumer Price Index



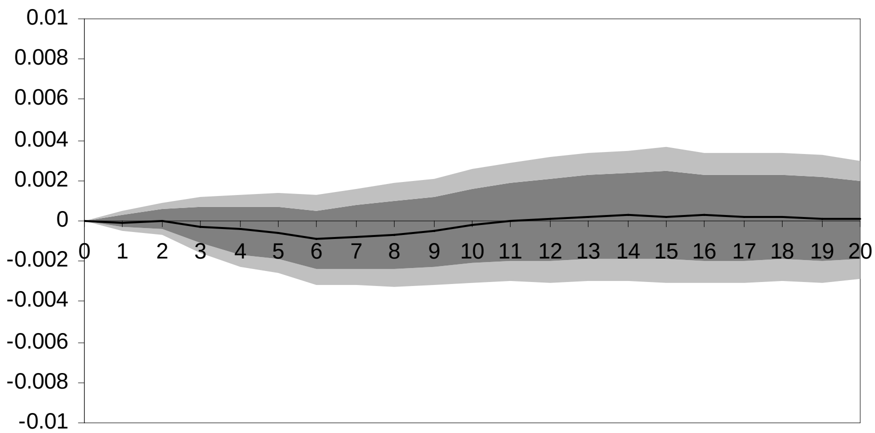
Response of Interest Rate

FIGURE 25

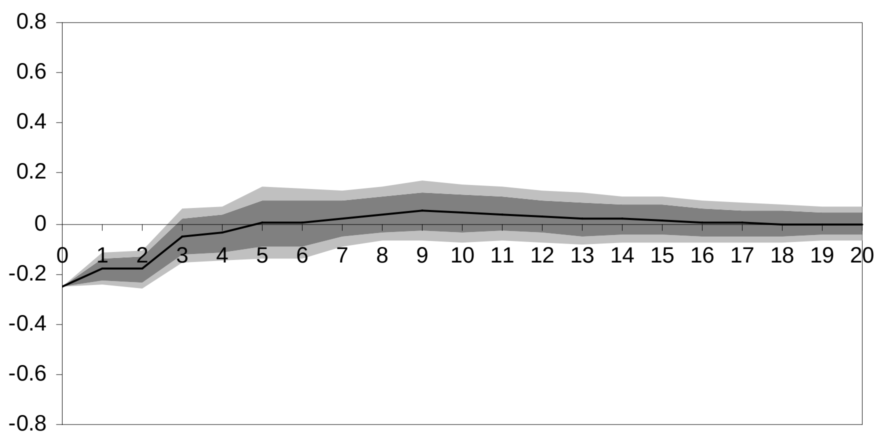
United Kingdom
25-Basis-Point Decline in Three-Month Treasury Bill Rate in Q4
Quarterly Dependence. 1963:Q1 to 1997:Q1



Response of GDP



Response of Consumer Price Index



Response of Interest Rate

TABLE 1 – DIFFERENCES IN IMPULSE RESPONSES ACROSS QUARTERS (JAPAN)
(*p-values for D-statistic*)

	Quarter			
Variable	First	Second	Third	Fourth
IP	0.04	0.21	0.00	0.20
CPI	0.34	0.16	0.03	0.01
Call rate	0.27	0.19	0.02	0.19

TABLE 2 – DIFFERENCES IN IMPULSE RESPONSES ACROSS QUARTERS (UNITED STATES)
(*p-values for D-statistic*)

	Quarter			
Variable	First	Second	Third	Fourth
GDP	0.05	0.00	0.66	0.46
GDP deflator	0.21	0.10	0.44	0.43
Fed funds rate	0.03	0.00	0.56	0.17

TABLE 3 – DIFFERENCES IN IMPULSE RESPONSES ACROSS QUARTERS (GERMANY)
(*p-values for D-statistic*)

	Quarter			
Variable	First	Second	Third	Fourth
GDP	0.80	0.42	0.21	0.85
GDP deflator	0.16	0.18	0.10	0.58
Lombard rate	0.66	0.74	0.58	0.99

TABLE 4 – DIFFERENCES IN IMPULSE RESPONSES ACROSS QUARTERS (FRANCE)
(*p-values for D-statistic*)

	Quarter			
Variable	First	Second	Third	Fourth
GDP	0.85	0.92	0.58	0.58
CPI	0.77	0.77	0.74	0.60
Call rate	0.48	0.19	0.76	0.62

TABLE 5 – DIFFERENCES IN IMPULSE RESPONSES ACROSS QUARTERS (UNITED KINGDOM)
(*p-values for D-statistic*)

	Quarter			
Variable	First	Second	Third	Fourth
GDP	0.81	0.80	0.38	0.50
CPI	0.07	0.75	0.38	0.31
Short term rate	0.71	0.54	0.72	0.92

TABLE 6 – KOLMOGOROV-SMIRNOV TESTS OF IDENTICAL DISTRIBUTIONS OF MONETARY POLICY SHOCKS IN DIFFERENT QUARTERS

(*p-values for KS-test*)

Japan			
Quarters	Q1	Q2	Q3
Q2	0.58		
Q3	0.25	0.25	
Q4	0.88	0.65	0.78
United States			
Quarters	Q1	Q2	Q3
Q2	0.66		
Q3	0.66	0.66	
Q4	0.30	0.18	0. 30
Germany			
Quarters	Q1	Q2	Q3
Q2	0.44		
Q3	0.64	0.28	
Q4	0.97	0.44	0.84
France			
Quarters	Q1	Q2	Q3
Q2	1.00		
Q3	0.78	0.36	
Q4	0.36	0.56	0.06
United Kingdom			
Quarters	Q1	Q2	Q3
Q2	0.97		
Q3	1.00	0.96	
Q4	0.99	0.81	1.00