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Synchronization in wage setting and the effects of monetary policy

Working paper

Synchronization in Wage Setting and the Effects of Monetary Policy*

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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*, for example, presents a clear 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*. In contrast, wage agreements in Germany are well-spread within the calendar year, implying a relatively uniform degree of rigidity. We exploit the variation in timing of wage agreements within the year in Japan vis-à-vis the three largest European countries (Germany, the UK, and France) to investigate the effects of monetary policy under different degrees of effective wage rigidity. Our

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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 frequency. 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 relation between monetary policy and macroeconomic fluctuations 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, for example, most 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 Germany, instead, wage bargaining renegotiations 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 rigidity would hence predict that monetary policy innovations in Japan should have a larger effect when the shock takes place in the second half of the year (after

[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). The relevance of price rigidity is nevertheless challenged by Christiano et al. (2005), who argue that what matters most for monetary policy is wage rigidity.

the Shunto), when wages are relatively rigid, whereas in Germany the effect of monetary policy innovations should not vary with the quarter in which the shock takes place. The aim of our study is to test whether this reasoning is supported by the data, or, put differently, to asses whether the response of the economy to monetary policy shocks differs with the time of the 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 four developed countries: Japan, Germany, France, and the United Kingdom.

The exercise thus exploits the potentially different degrees of synchronization in wage-setting decision within a country during the calendar year, and compares the differences in the effect of monetary policy across quarters of the calendar year in a given country vis-à-vis the corresponding differences in other countries.[3] We find that for Japan there are, indeed, important differences in the response of the economy to monetary policy shocks that depend on the timing of the policy innovation. In particular, a monetary policy innovation 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 (immediately after the Shunto) has a remarkably large effect. In 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 complement and reinforce those in Olivei and Tenreyro (2007): In the United States, monetary policy innovations that take place in the first half of the calendar year have strong effects on

[3] Note that direct cross-country comparisons are impaired by a large range of country-specific characteristics (including variation in labour market institutions and in the conduct of monetary policy).

output, while innovations in the second half have negligible effects, a pattern consistent with evidence that U.S. firms tend to adjust wages once a year, typically in the fourth quarter.

We should perhaps clarify at the outset that our focus on big, developed economies is largely determined by the extant literature on central banks: 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.[4]

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.

2 Wage Setting Practices and Monetary Policy Instruments in Japan and Europe

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

[4]Smaller, less developed countries are less fit to admit a quarter-dependent VAR representation: Their higher propensity to monetary or real intrinsic instability would require the inclusion of structural-change parameters that exceed the degrees of freedom allowed by the data.

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 fist and second quarters are periods of substantially larger wage flexibility than the last two quarters.[6]

Synchronization in the timing of wage contracts is, however, not a widespread phenomenon. 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 during the calendar year, with a slightly higher concentration in the second and fourth quarters; new agreements typically become effective in January.[7] 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, rather than the date at which it becomes effective.[8] Thus, in France synchronization of wage setting decisions is

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[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.

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

[8] In other words, what is relevant is the information set at the time the settlement was agreed. For example, if the agreement takes place at time t_0 and becomes effective from time $t_1 > t_0$ until time $t_2 > t_1$, insofar as there are no continget clauses, this means that wages are effectively rigid from time t_0 till t_2, since any new information coming after

less prevalent than in Japan; to the extent that the public sector has a bearing, some bunching of wage contracts occurs in the fourth quarter.

Synchronization of wage setting decisions within the year is even less prevalent in Germany and the UK. In Germany, according to the *Hans-Böckler Stiftung Tarifdaten*, the dates for new collective agreements tend to be well spread throughout the year and, equally important, collective agreements tend to last for one to three years (not necessarily multiples of a year).[9] 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. First, if multiple-year contracts are mostly re-negotiated in the same quarter in synchronized years, renegotiation will take place in some years and not in others. Hence, a given quarter might display high or low wage rigidity depending on the year. Monetary policy innovations in a given season will thus have different effects in different years. Second, if multiple-year contracts are staggered, with only a fraction being renegotiated in a given year (though in the same quarter), this implies that a relatively small fraction of contracts are renegotiated in a given quarter-year (that is, smaller than in the case of yearly contracts). Hence, in Germany, both the more uniform distribution of wage setting decisions across years and the longer duration of contracts (more than a year) should make the timing of monetary policy innovations within the year less relevant than in Japan and France.

In the UK, wage settlements typically become effective around January and April, however the actual negotiations tend to start from 1 through 5 months before the date in which the new wage becomes effective, depending on the sector; for example, the National Council for Local Government Services typically agrees on settlements 5 months before the date in which they become effective, while the Nursing and Other Health Professionals Review Body does so 2 months before the effective

t_0 is not used in the agreed wage. In that sense, the date at which the aggreement becomes effective, t_1, is irrelevant.

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

date. Thus, actual decisions on wage adjustments are more spread during the calendar year than in Japan. Furthermore, as was the case for Germany, a large fraction of collective agreements in the UK lasts for more than one year (for example, *Income Data Service* reports that in 2005 more than 40 percent of the wage contracts had a duration of three years). Thus, as argued for Germany, in the UK the response of the economy 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 these countries 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 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 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-run 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-run interest rate (see Clarida, Galí, and Gertler, 1998). The level of the short-run 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 three-variable 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 four 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 Germany and Japan – at least for the post-Bretton Woods period – monetary policy was not particularly affected by external constraints and autonomy in policy management was, thus, greatest among the countries included in the analysis. For France and the United Kingdom, instead, external constraints have operated also in the post-Bretton Woods era. Clarida, Galí, and Gertler (1998) document how German's monetary policy influenced the conduct of monetary policy in both 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). It's not clear, however, that omitting monetary aggregates results in an important misspecification. Bernanke and Mihov (1997) argue that while the Bundesbank since 1974 operated 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. In all, these findings suggest that specifying an inflation target in the policy reaction function may render the inclusion of monetary aggregates redundant, not only from a statistical but also from an economic standpoint.

As concerns the choice of policy instrument, the assumption that a short-run measure of the interest rate is the appropriate policy variable finds support in empirical studies that investigate central banks' operating procedures to identify the relevant policy indicator rather than picking an indicator on purely a priori grounds. Bernanke and Mihov (1997) show that the Lombard rate is the relevant indicator 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 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, Gali 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 Gali and Gertler. 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.[10,11] As such, the system embeds the key assumption for identifying the dynamic effects of exogenous policy shocks on the 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.[12]

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

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

[11] 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).

[12] See, among others, Bernanke and Blinder (1992), Rotemberg and Woodford (1997), Bernanke and Mihov (1998), Christiano, Eichenbaum and Evans (1999), and Boivin and Giannoni (2003).

indexed by q_t through their impact on the non-policy variables, $\mathbf{Y}_t$.[13]

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:

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

where $\mathbf{X}_t = [\mathbf{Y}_t, p_t]'$, $\mathbf{U}_t$ is the corresponding vector of reduced-form residuals, and $\mathbf{A}(L, q)$ 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 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 effects of monetary policy shocks can differ depending on which quarter the shock takes place. Quarter dependence in (3) 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

[13] 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.

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:

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

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 $\mathbf{A}(L,q)$ and $\mathbf{B}(L,q)$ 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 sometime 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 were occurring 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 help to solve the empirical finding of prices temporarily rising after a monetary policy tightening (the so-called price puzzle). As for foreign interest rates, over the sample period we consider, monetary management in France and the United Kingdom has 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-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, x_t the response in the standard non-time-dependent model.[14] We construct an empirical distribution of D by bootstrapping

[14] We compute the supremum of the difference in impulse-response functions over 20 quarters following a monetary

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 the standard VAR. 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 bootstrap p-value, which is the percentage of simulated D^s exceeding the observed D.

3.3 Data and Estimation

Our empirical analysis is based on seasonally adjusted quarterly data.[15] In the benchmark specification (3) the vector of non-policy variables $\mathbf{Y}_t$ 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 and sample periods for each of the countries we consider.

For Japan, we use data from 1964: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 (West) Germany, we use data from 1964:Q1 to 1994:Q4. Unification complicates the use of 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

policy shock.

[15] 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. Alternatively, non-seasonally adjusted data could be used, provided that seasonal dummies are used as controls. (See Olivei and Tenreyro, 2007).

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 1964: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 1964: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 $\mathbf{Z}_t$.

For all four countries we consider, 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. All variables enter the VAR analysis in log levels except for the policy variable p_t, which is expressed in levels. 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. We will note in Section 5 that our findings are not driven by this constrained estimation. Unconstrained ordinary least squares produce essentially the same findings, but the constrained estimation saves on degrees of freedom.

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). This reduced-form quarter-dependent VAR consists of three endogenous variables: a measure of real activity, a measure of the price level, and the policy variable. In section 5 we discuss the robustness of our findings to a specification that includes additional endogenous (and possibly also exogenous) variables, as represented by the VAR in (4).

4.1 Japan

The impulse-responses to a monetary policy shock in Japan are depicted in Figures 1 through 5, with the shades denoting the 80 percent confidence bands around the estimated responses.[16] We consider a monetary policy shock that corresponds to a 25 basis points decline in the policy rate on impact. For ease of comparison, the response of the variables to the shock are graphed on the same scale

[16] Much applied work uses 95 percent confidence intervals. Sims and Zha (1999) note that the use of high-probability intervals camouflages the occurence 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.

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 the price response 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 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.[17] 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 in the fourth quarter.[18] It is also apparent from the figures that the differences in the response of activity to a policy shock across quarters are significant from an economic standpoint.

4.2 Germany

The impulse-responses to a monetary policy shock in West Germany are depicted in Figures 6 through 10. We consider a monetary policy shock that lowers the Lombard rate by 25 basis points on impact. Figure 6 illustrates the impulse-responses corresponding to the VAR without quarter-dependence.

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

[18] The D-statistic in Table 1 also indicates that the price responses to a policy sock 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 quarter 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.

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 7 through 10, are remarkably similar across quarters, except perhaps for a slightly weaker response of activity 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 2 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 is 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 not synchronized 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.

4.3 France

The impulse-responses to a monetary policy shock in France are displayed in Figures 11 through to 15. 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 12 through 15, 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 3, show that none of the quarter-dependent responses is 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.

4.4 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.[19] 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 17 through 20 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.[20] 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.

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

[20] 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 so the wage responses) become very similar across different 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 is statistically different from the corresponding non-quarter-dependent responses at better than the asymptotic 5 percent level.

4.5 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 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 5 for the four 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 deserve some attention, given that this is the country where we find significant differences in the response of activity to a policy shock across quarters. Specifically, the hypothesis that policy shocks occurring in the first quarter have the same distribution as policy shocks occurring in the other quarters cannot be rejected with reasonable confidence.

Another issue of concern is whether the different impulse-responses for activity across quarters that we document for Japan 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. Our use of seasonally adjusted data should in principle control for the seasonal component of output. And even if such a control were imperfect, the pattern of the seasonal cycle documented by Beaulieu and Miron for Japan's industrial production[21] – our measure of activity – does not conform to the pattern of our quarterly responses to the policy shock. Industrial production 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.

5 Robustness

Here, we summarize results pertaining to the robustness of our baseline specification along several dimensions.[22] 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 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 vari-

[21] See Table 3 in their paper.

[22] 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.

able 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 17 and 18.

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 had set monetary targets. We thus augment the baseline specification by introducing money as an additional endogenous variable. Money is ordered last in the VAR. In terms of the notation in (4), this is a variable belonging to $\mathbf{Y}_2$. 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 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.

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 and 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.[23] Another way of modelling the external constraint for France and the United Kingdom is to introduce Germany's policy rate as an explanatory variable.[24] 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 exogenous variable measuring oil prices or commodity prices helps to mitigate the price puzzle. With the exception of the United Kingdom,[25] this control variable provides little improvement in the price

[23] 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 alleviates concerns about identification somewhat.

[24] 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.

[25] As mentioned in footnote 19, figures 16 to 20 for the United Kingdom already show impulse-responses that are estimated from a VAR with oil prices as an exogenous control variable.

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 1970 – 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.

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 to estimated impulse-responses that are similar to the ones obtained from estimating the reduced-form VAR with polynomial distributed lags.

6 Concluding Remarks

Our main conclusions have been amply foreshadowed. We found that the degree of synchronization 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. One critical implication of this synchronized annual wage setting is that if preset wages are important in accounting for the connection between monetary policy and real activity at business cycle frequency, 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 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. 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. We contrast the empirical findings for Japan 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 relative 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.[26] Yet, the new generation of Keynesian models has glossed over this finding and assumed uniform staggering as both a convenient modeling tool and an essential element in the transmission mechanism of monetary policy shocks. This paper notes that while uniform staggering 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.

[26] 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.

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Figure 1. Japan, No Quarter−Dependence

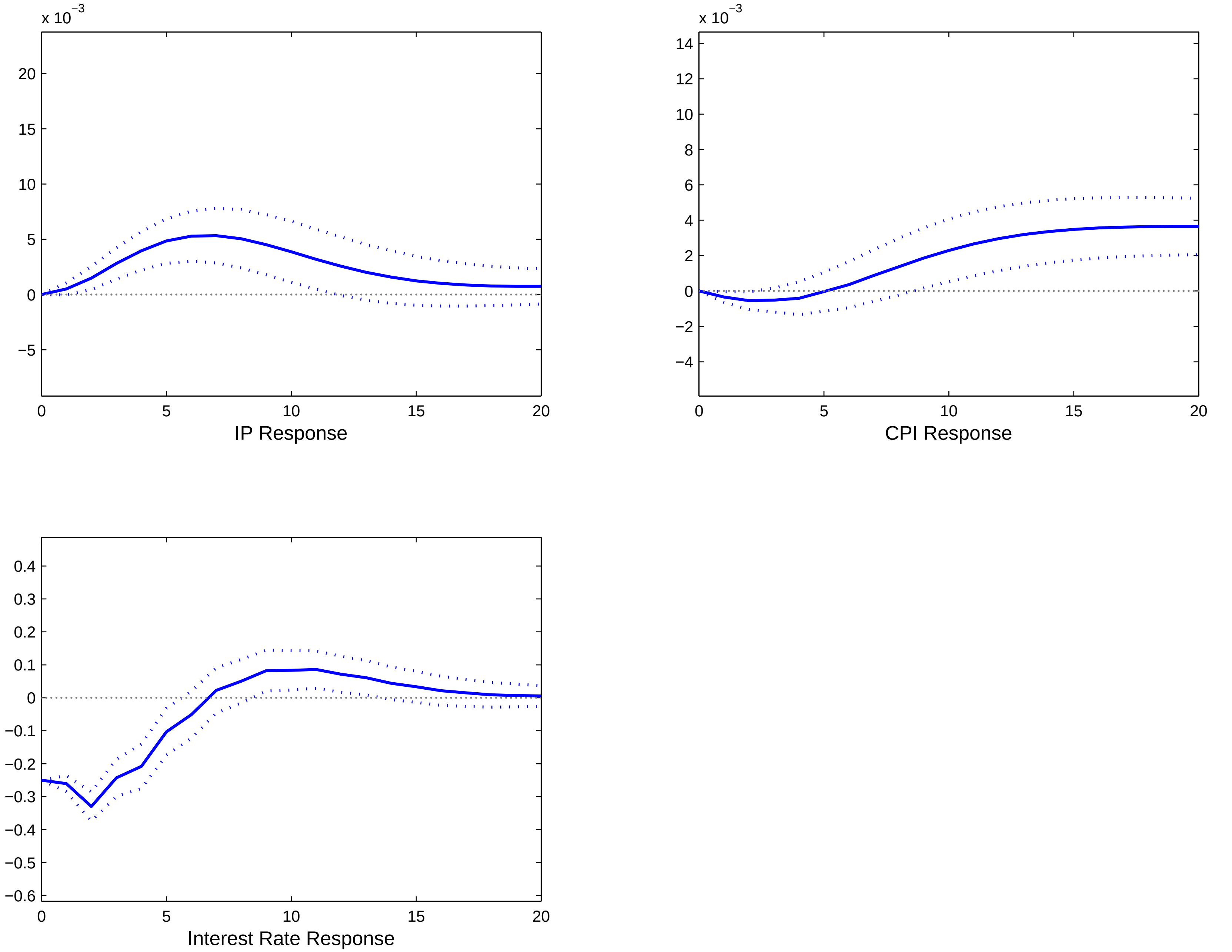

Figure 2. Japan, Quarter−Dependence Q1 Shock

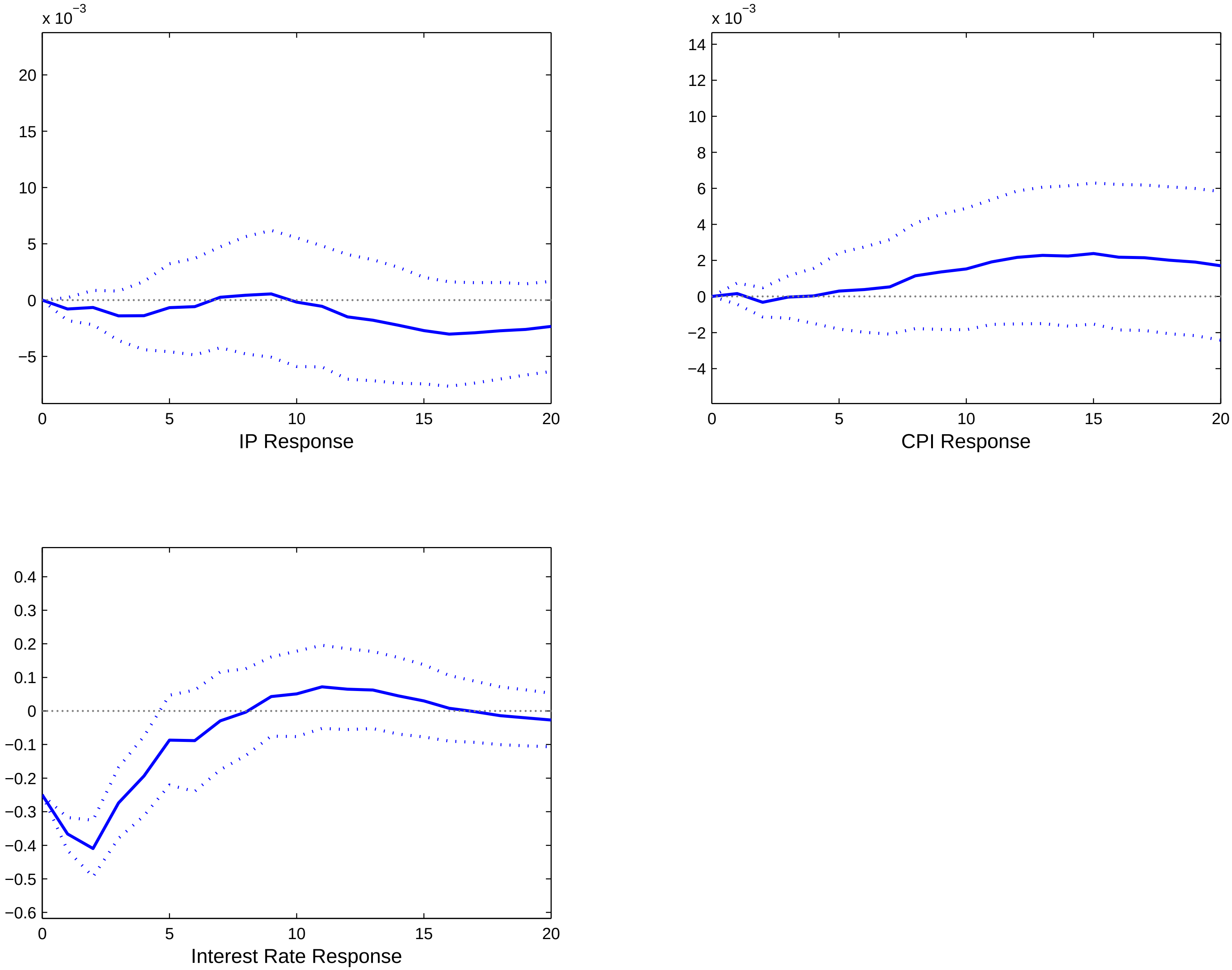

Figure 3. Japan, Quarter−Dependence Q2 Shock

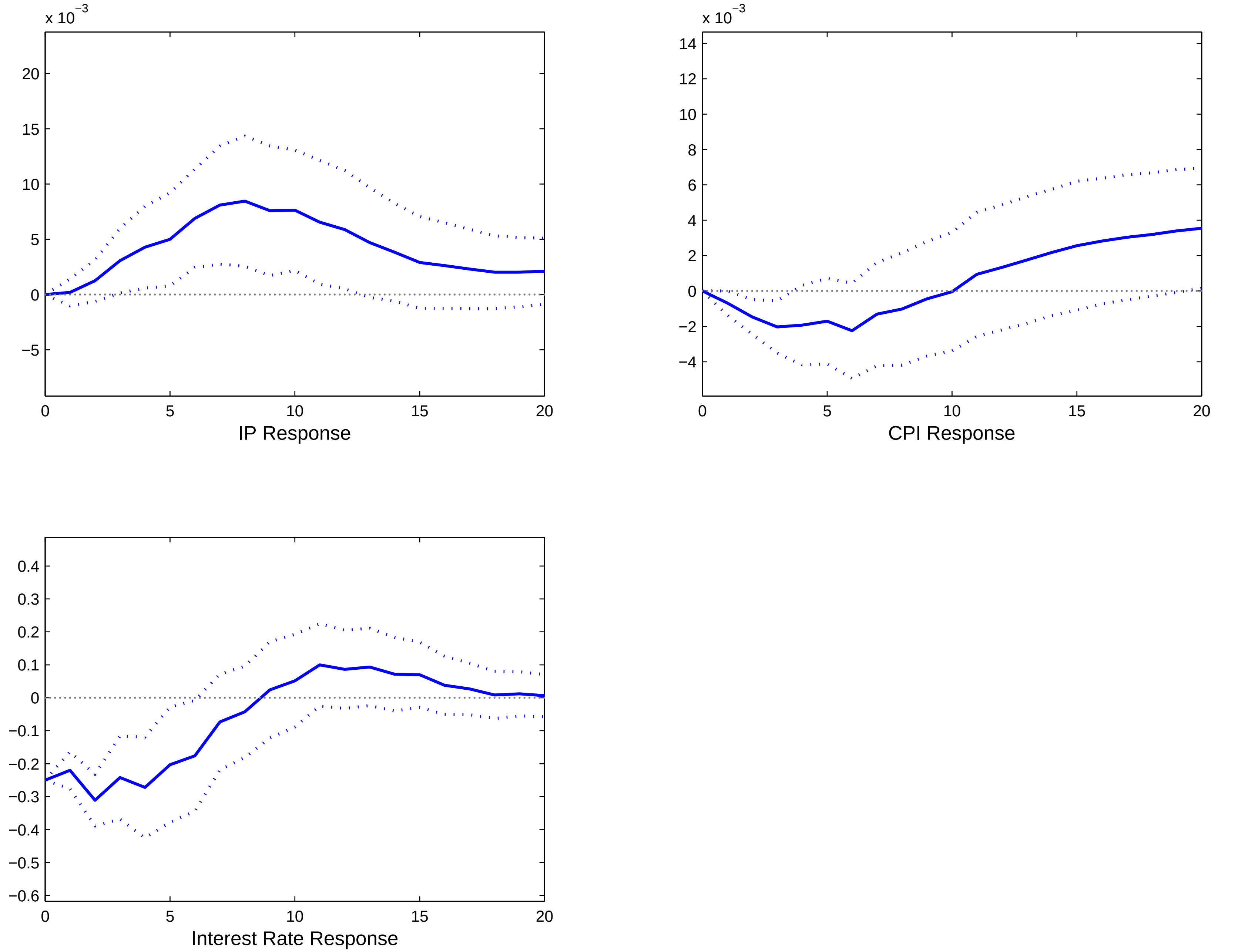

Figure 4. Japan, Quarter−Dependence Q3 Shock

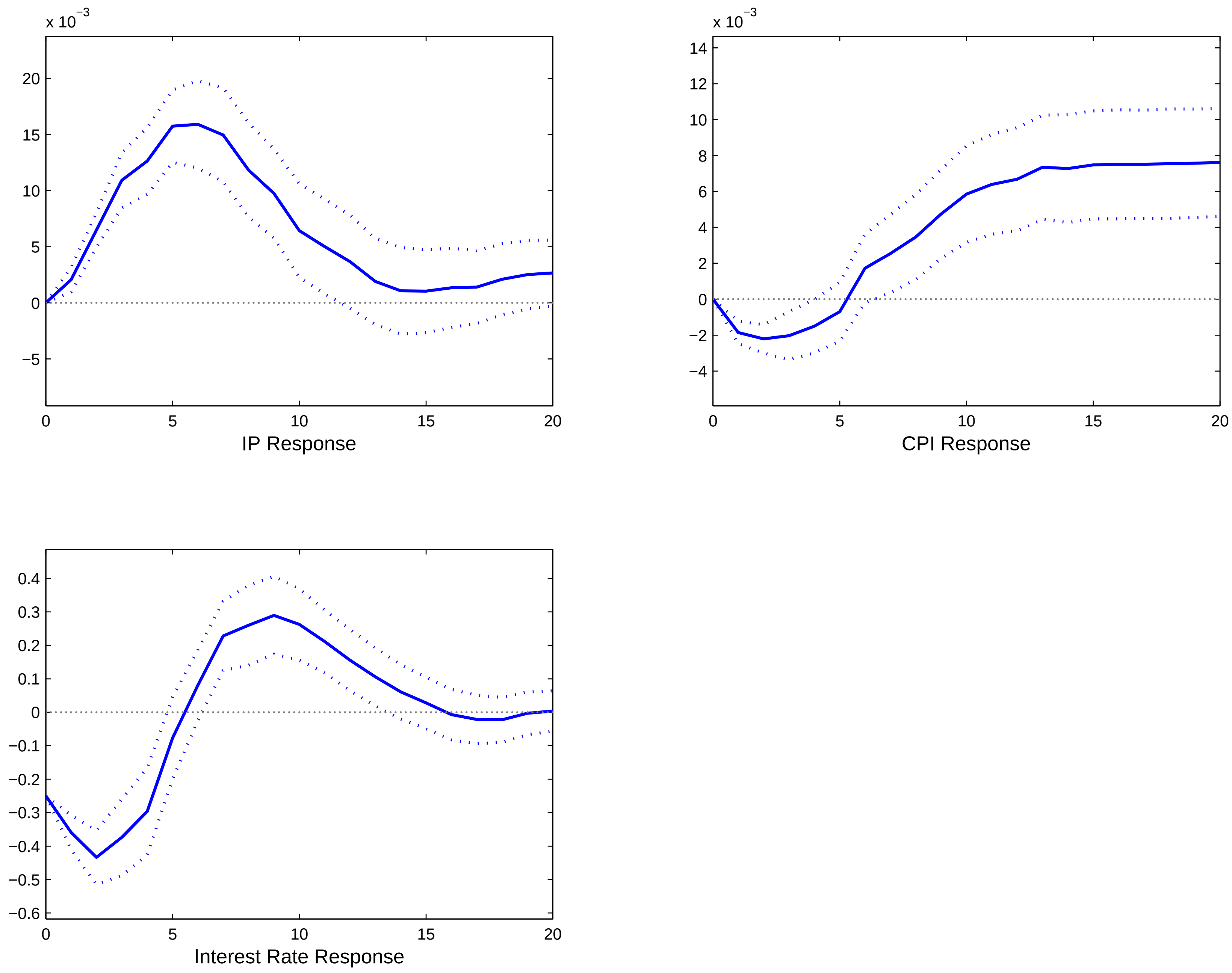

Figure 5. Japan, Quarter−Dependence Q4 Shock

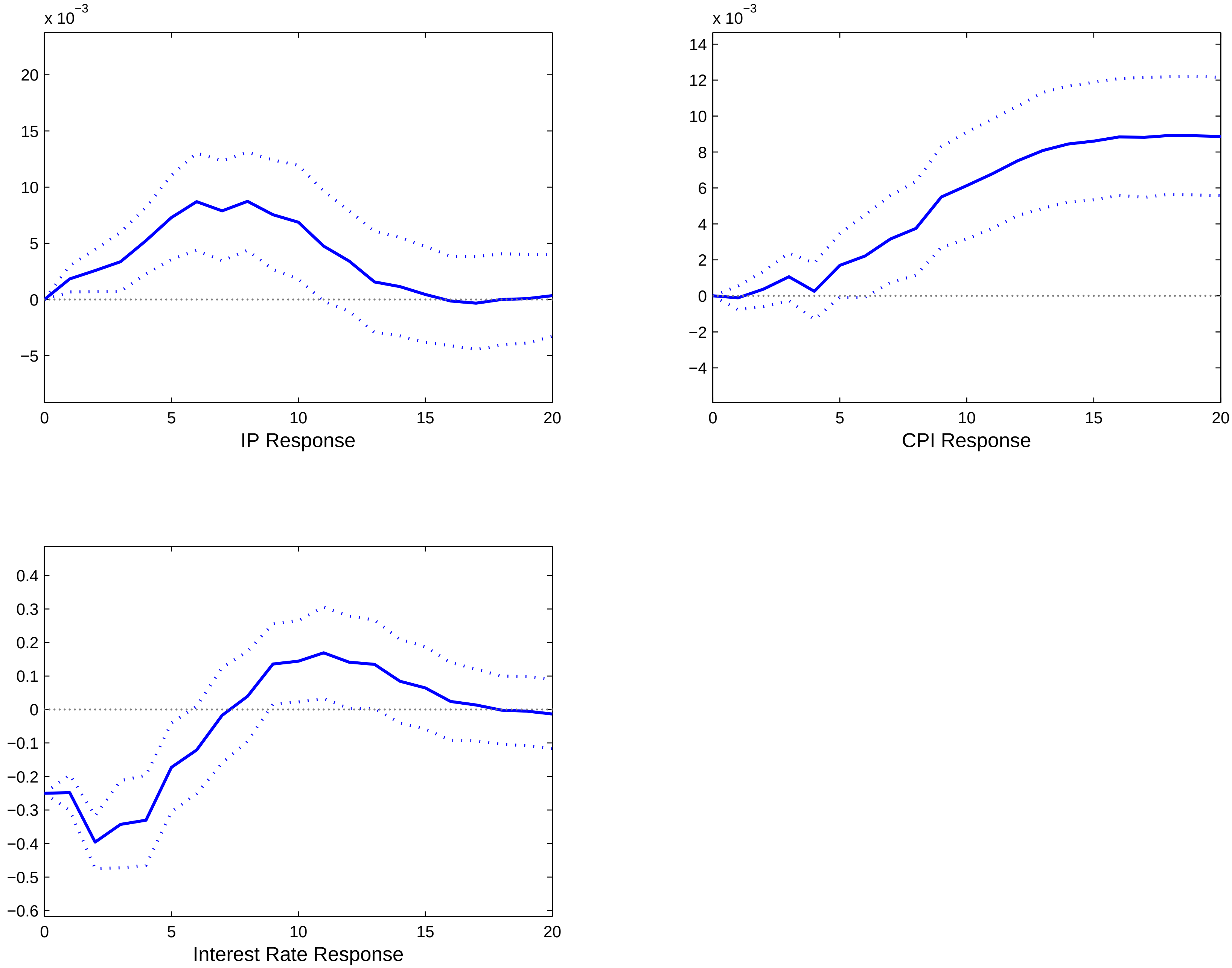

Figure 6. Germany, No Quarter−Dependence

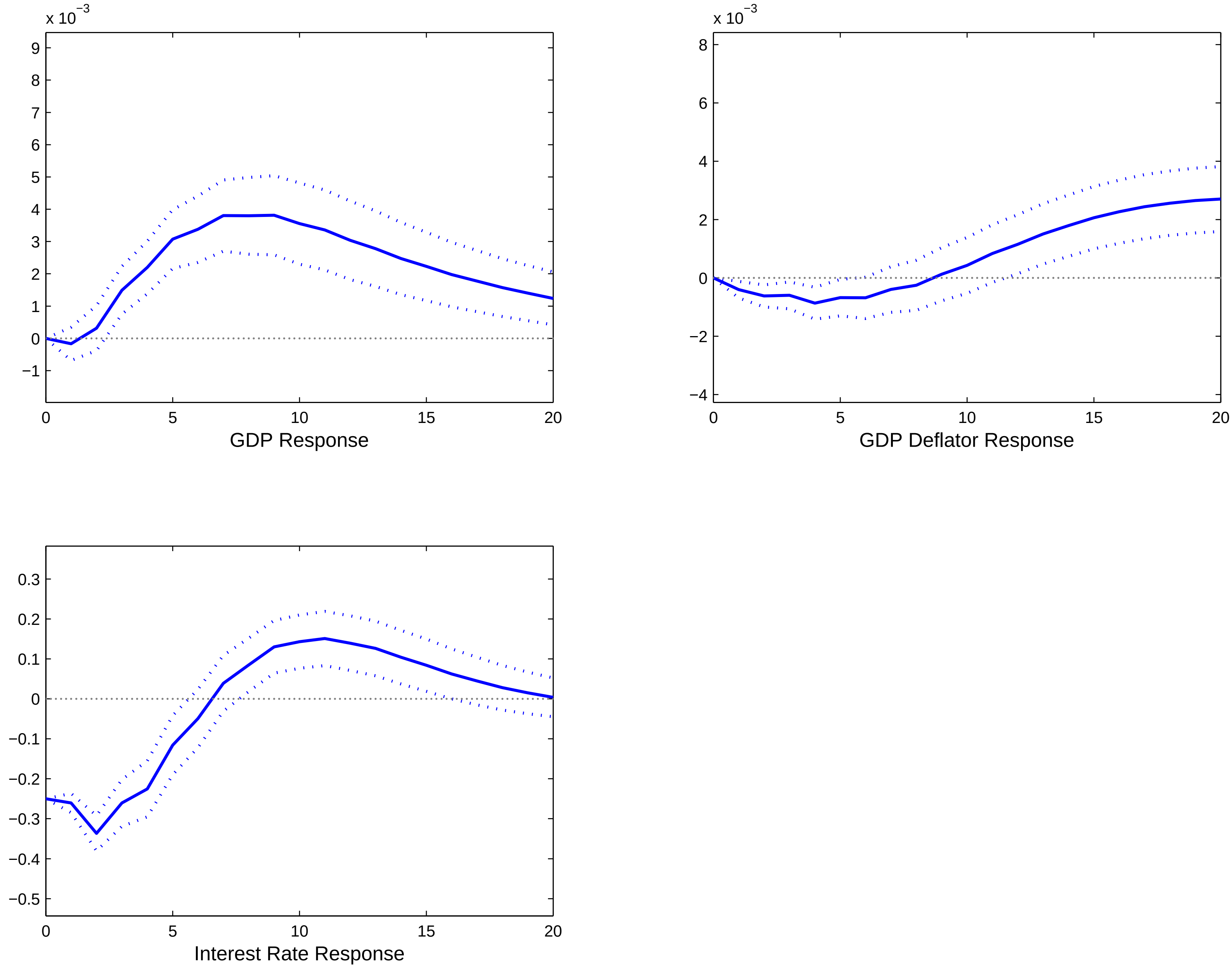

Figure 7. Germany, Quarter−Dependence Q1 Shock

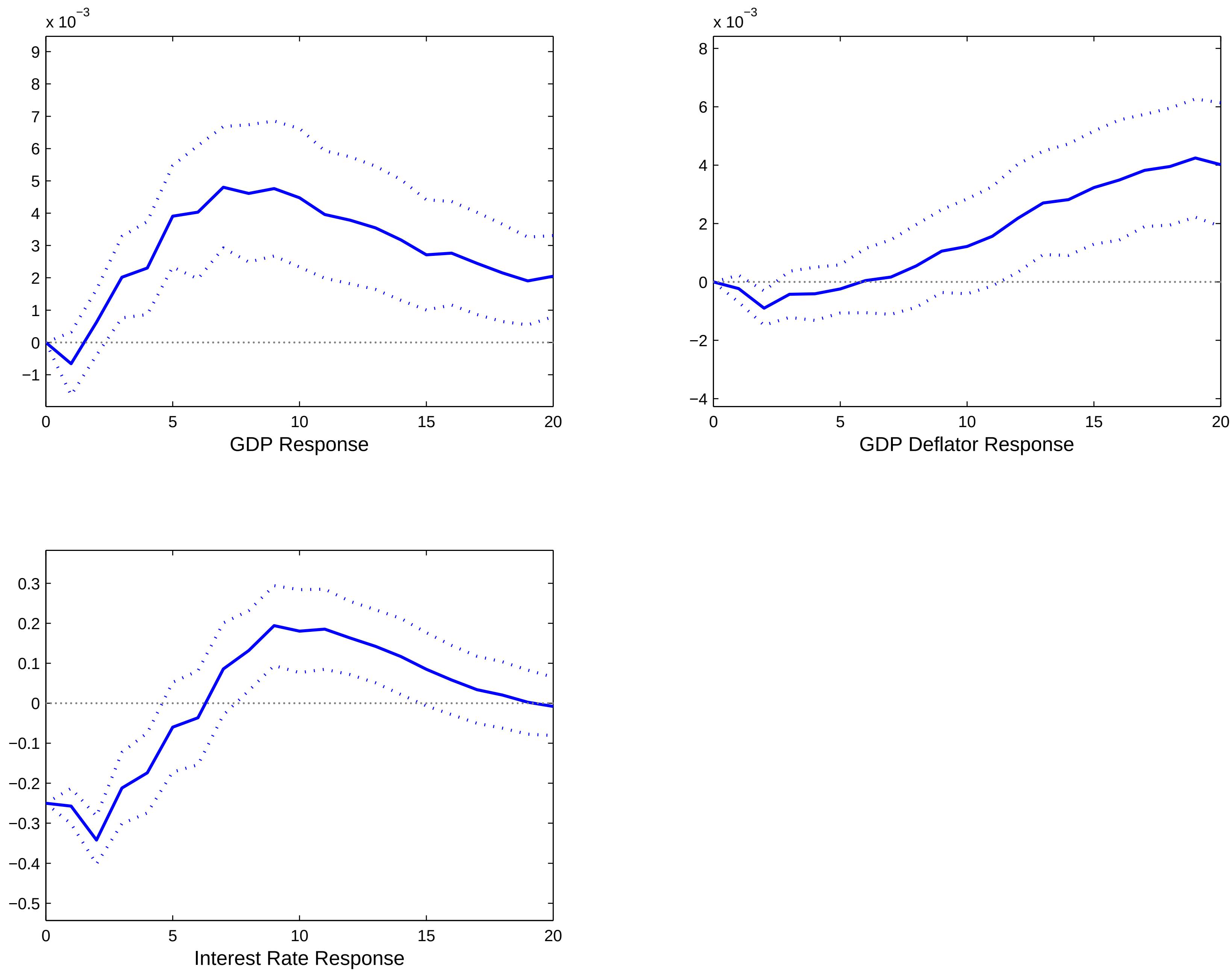

Figure 8. Germany, Quarter−Dependence Q2 Shock

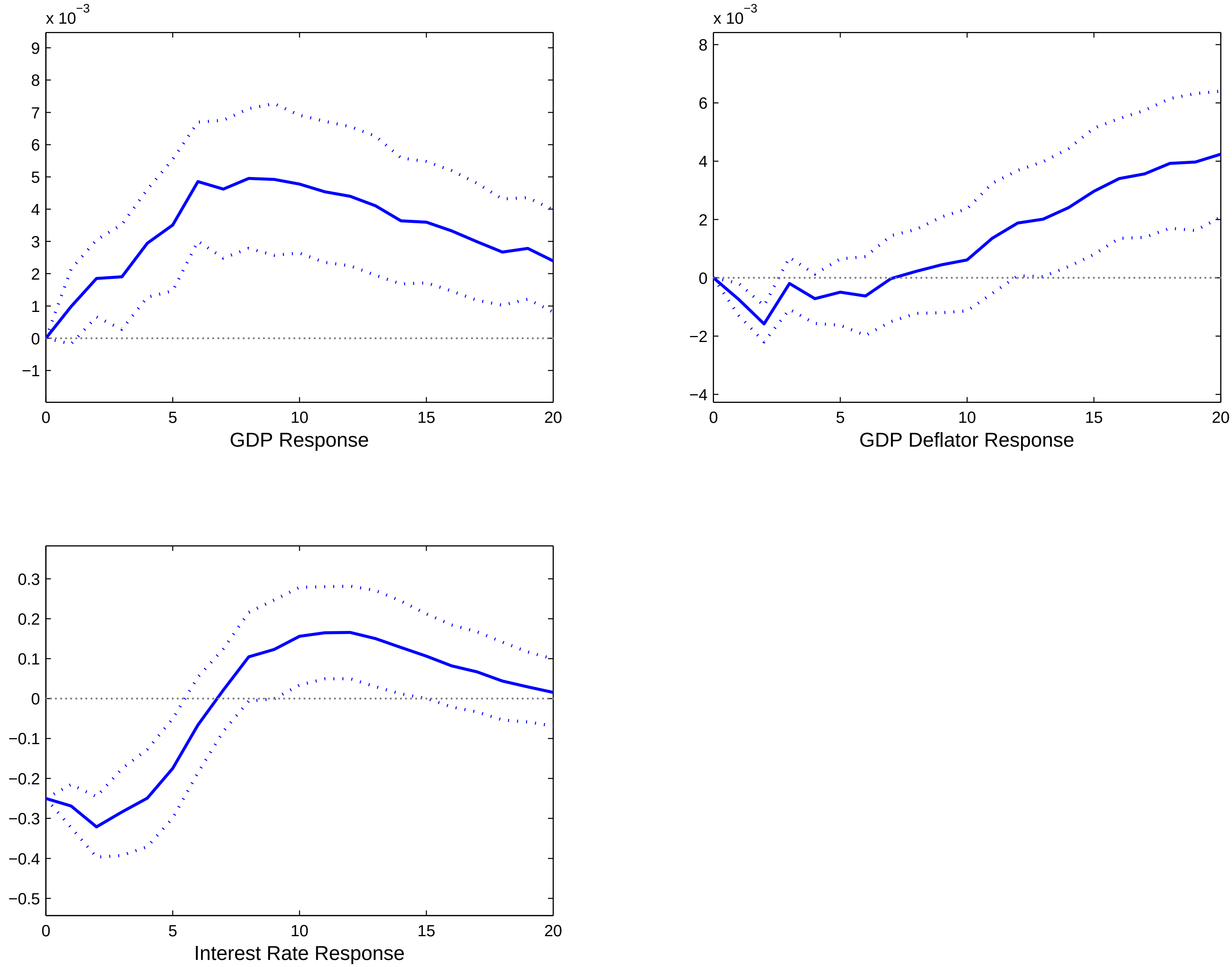

Figure 9. Germany, Quarter−Dependence Q3 Shock

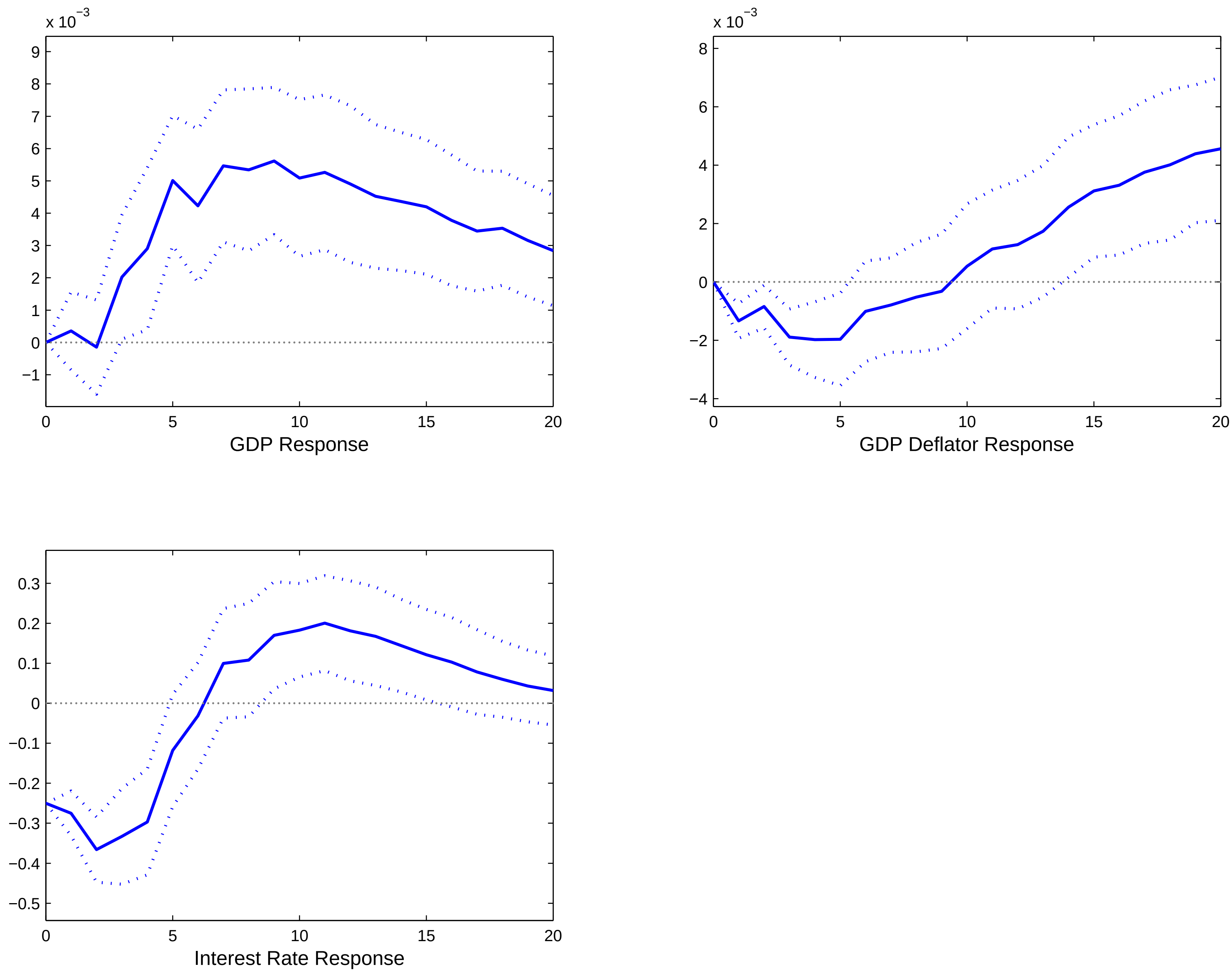

Figure 10. Germany, Quarter−Dependence Q4 Shock

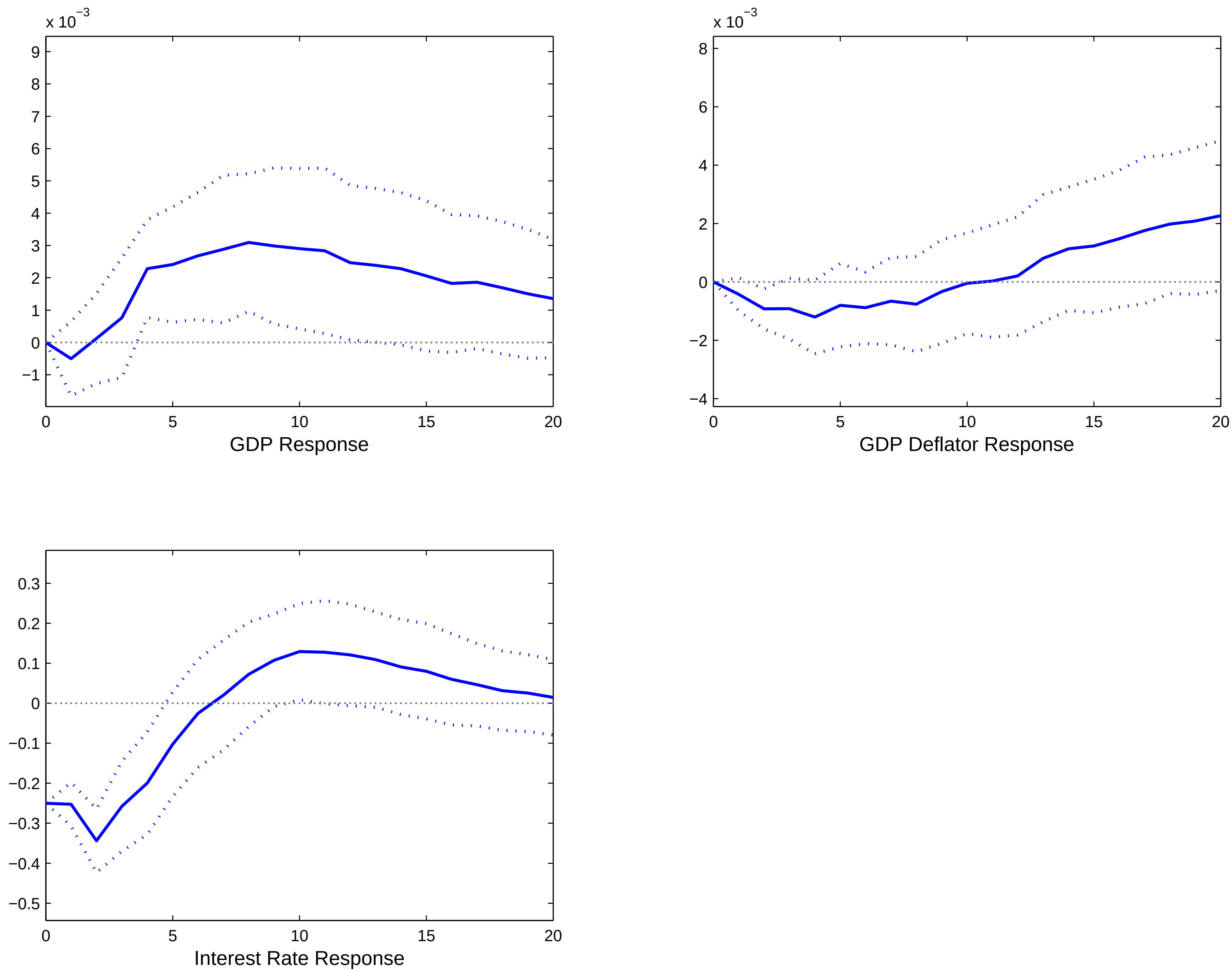

Figure 11. France, No Quarter−Dependence

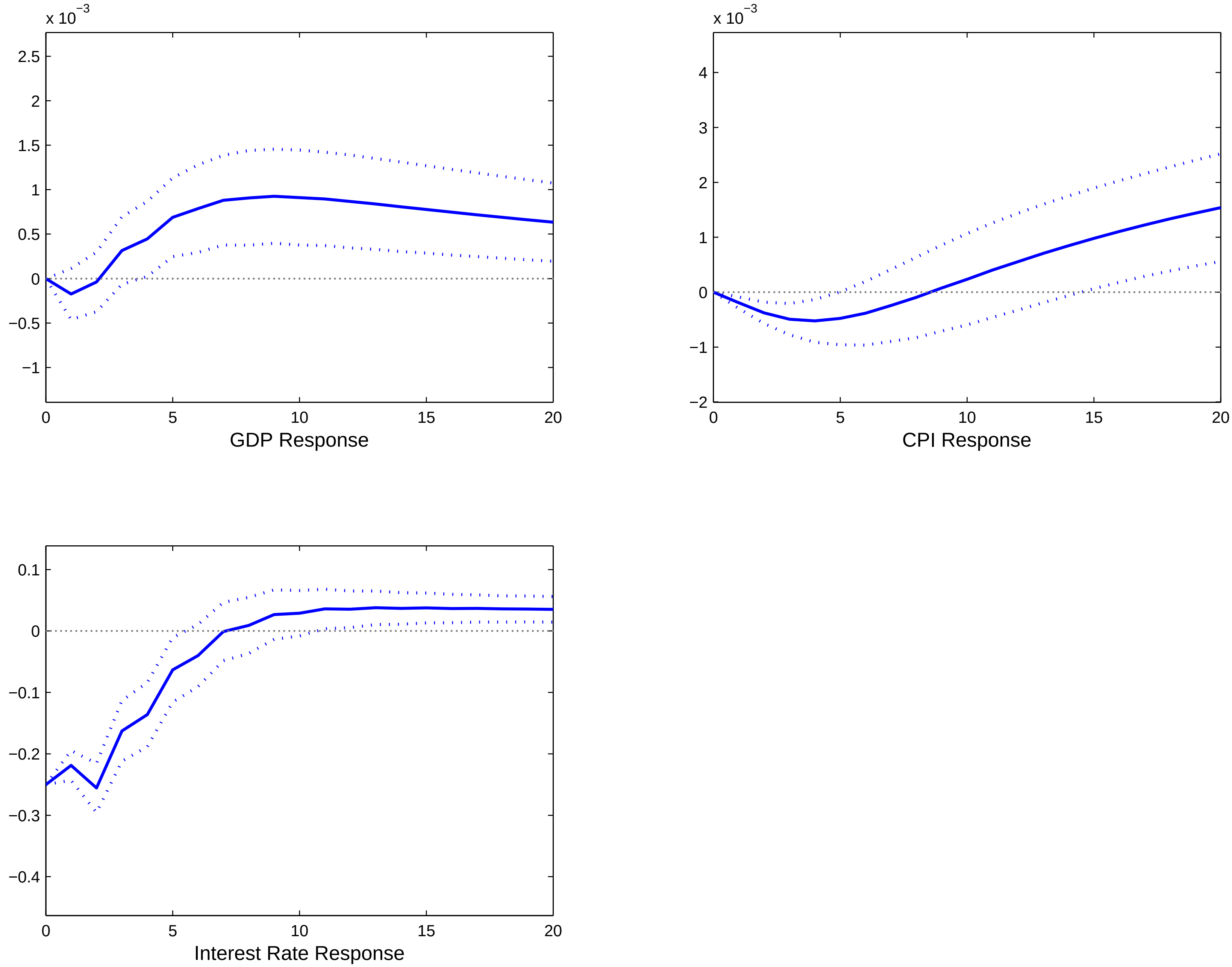

Figure 12. France, Quarter−Dependence Q1 Shock

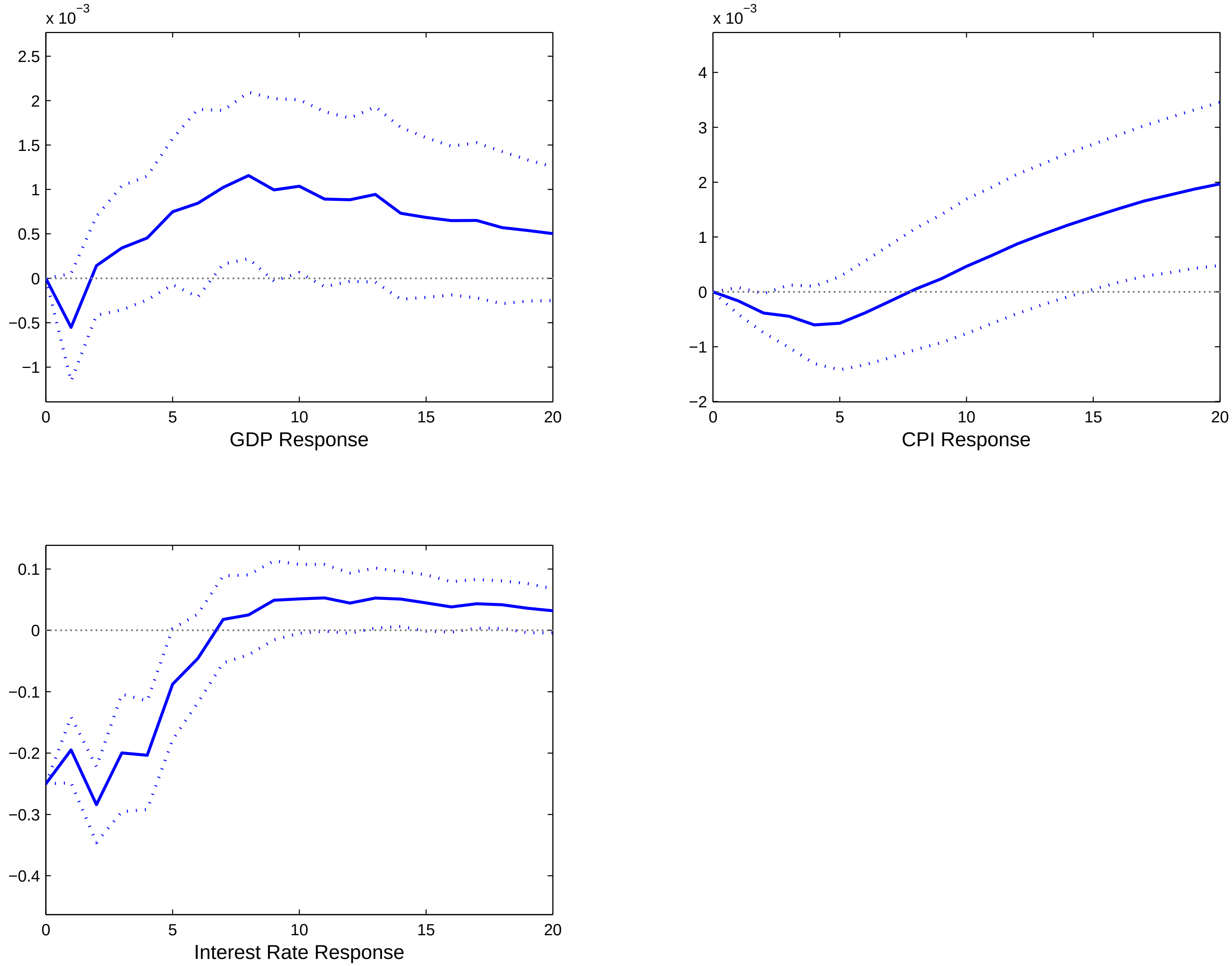

Figure 13. France, Quarter−Dependence Q2 Shock

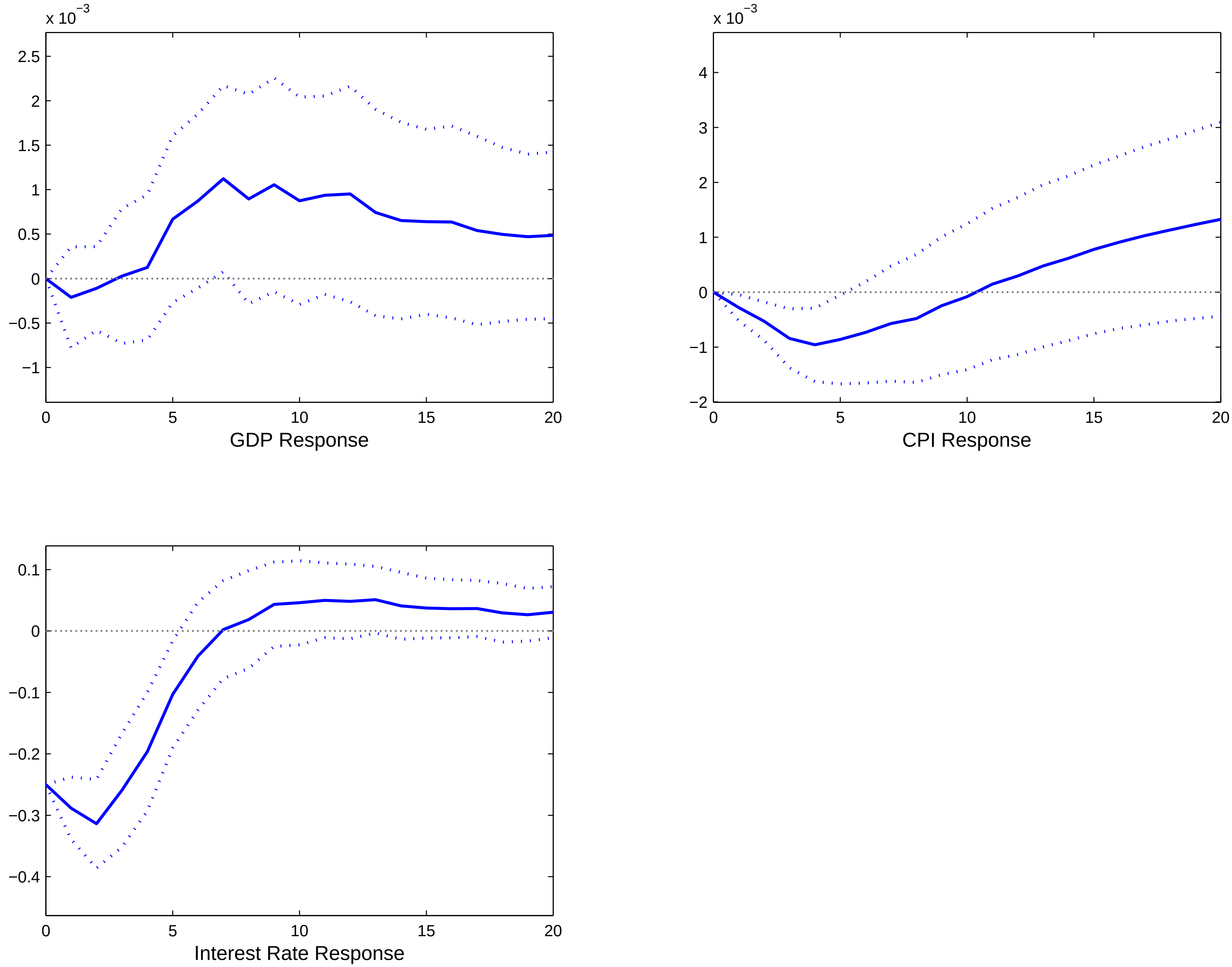

Figure 14. France, Quarter−Dependence Q3 Shock

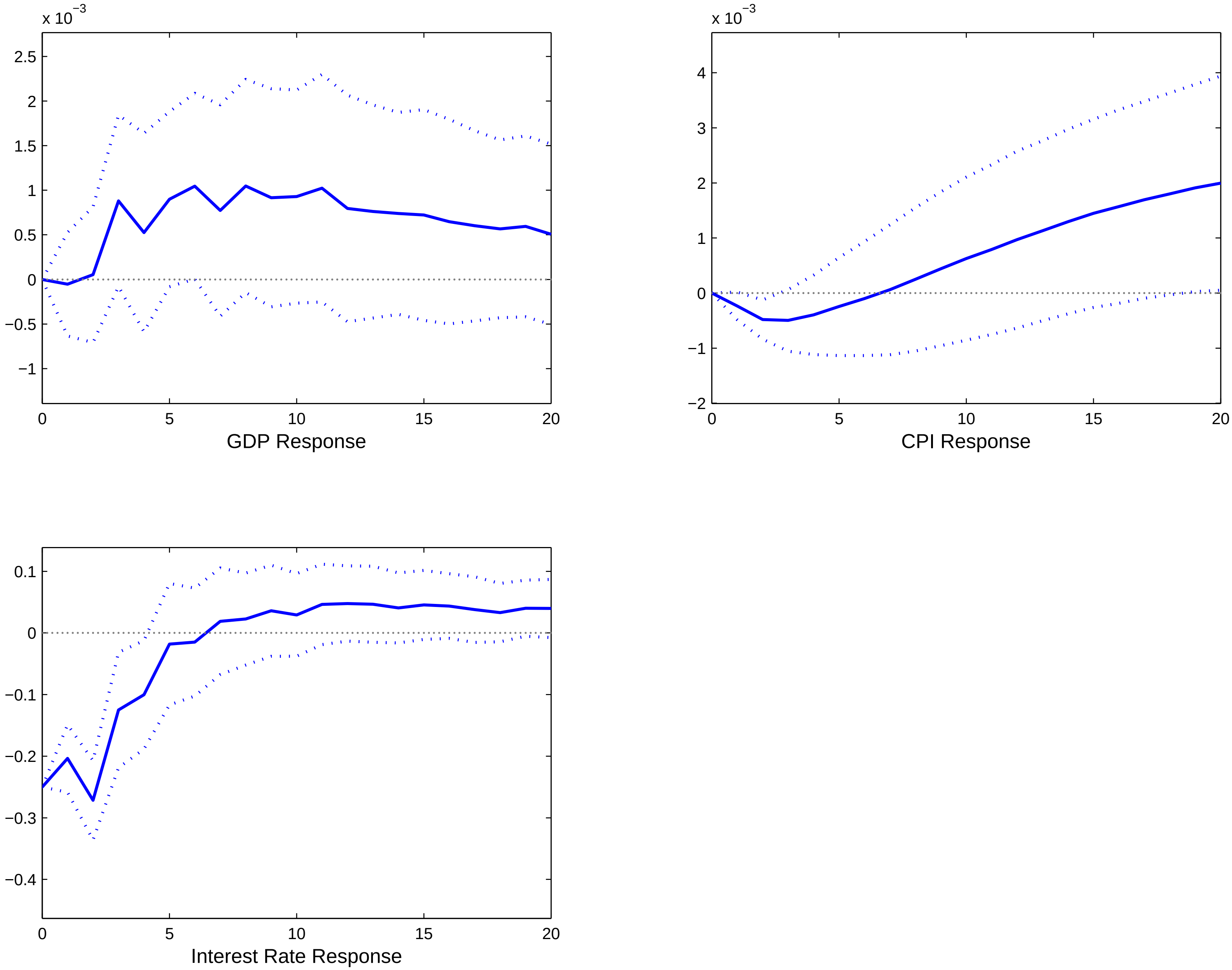

Figure 15. France, Quarter−Dependence Q4 Shock

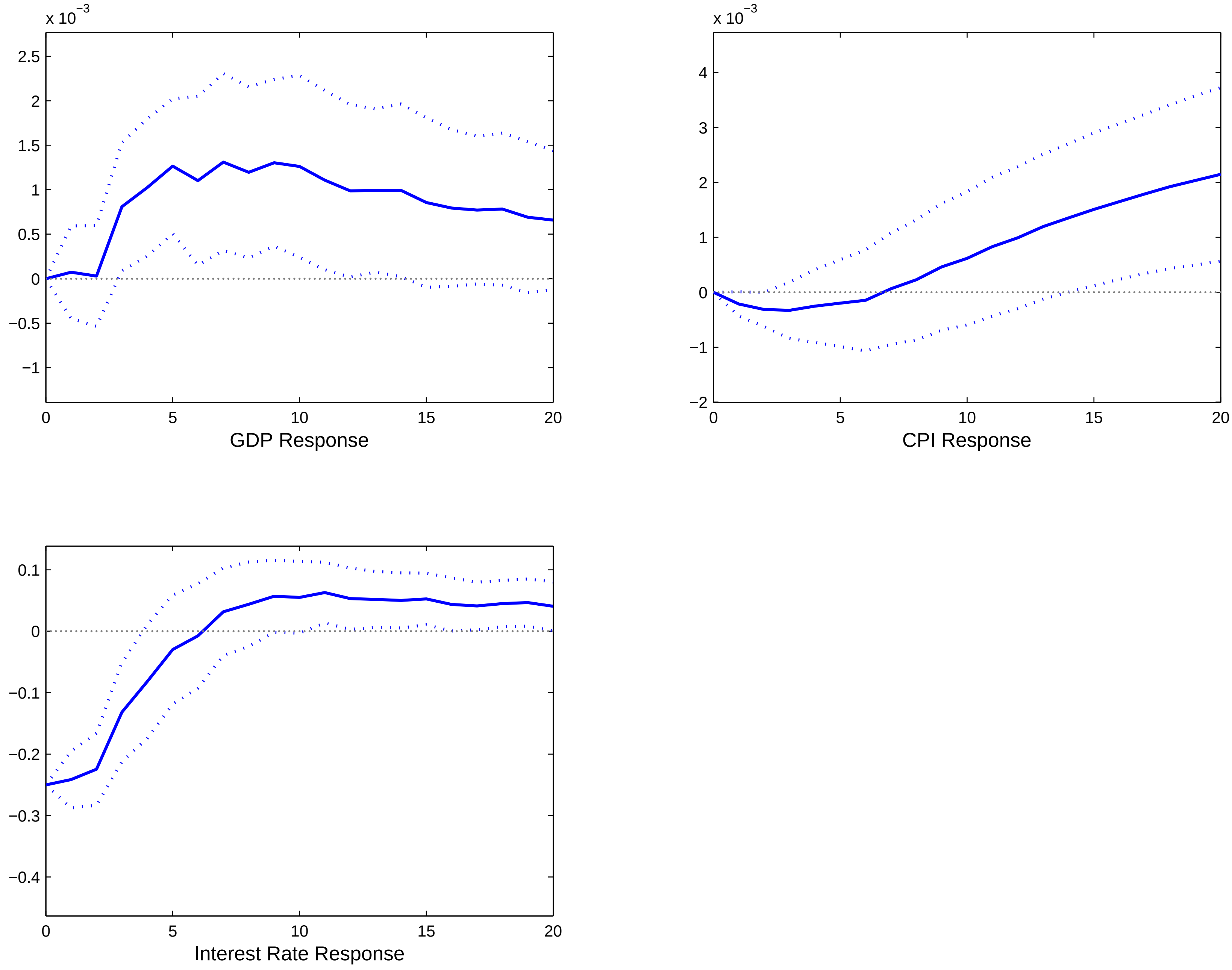

Figure 16. UK, No Quarter−Dependence

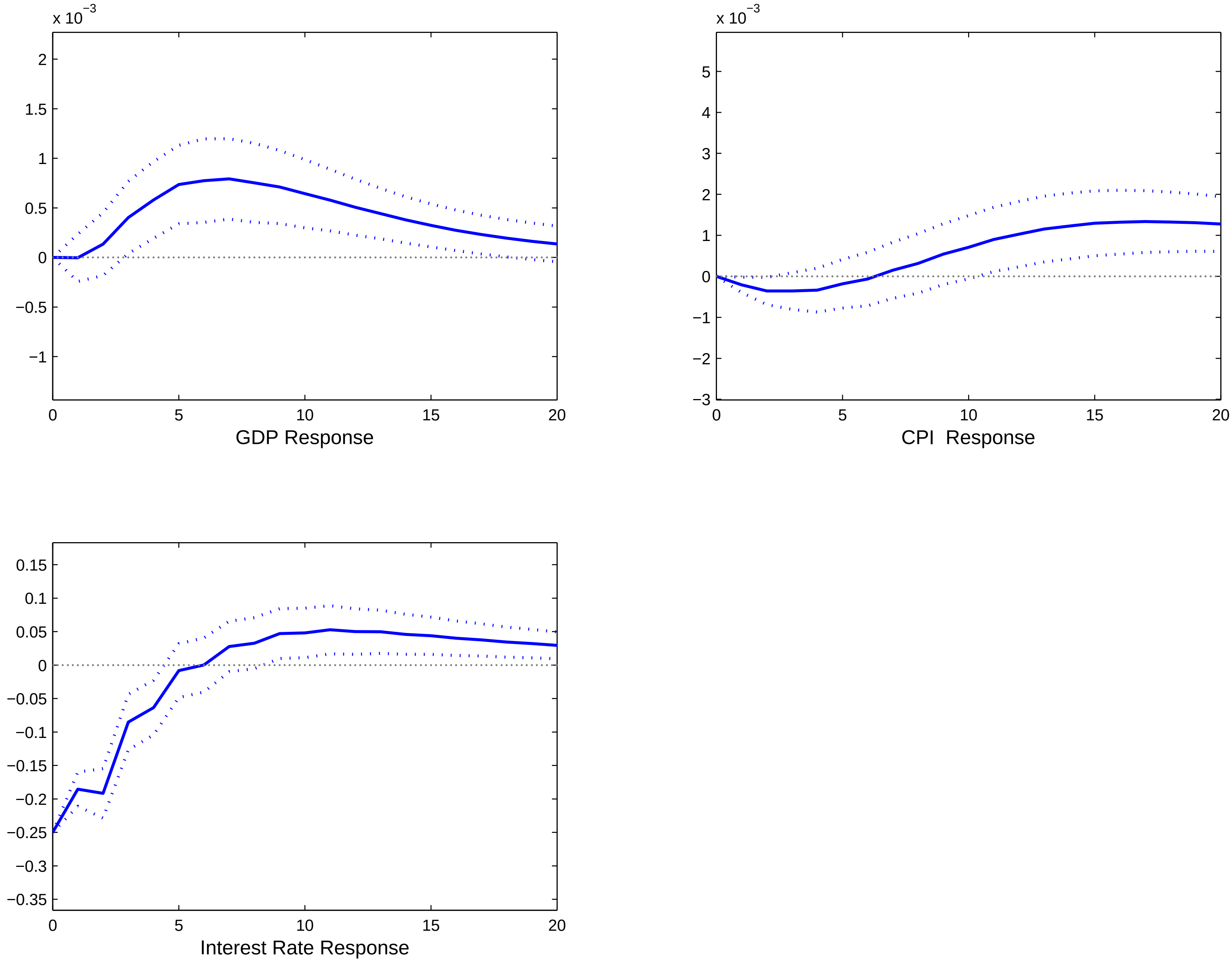

Figure 17. UK, Quarter−Dependence Q1 Shock

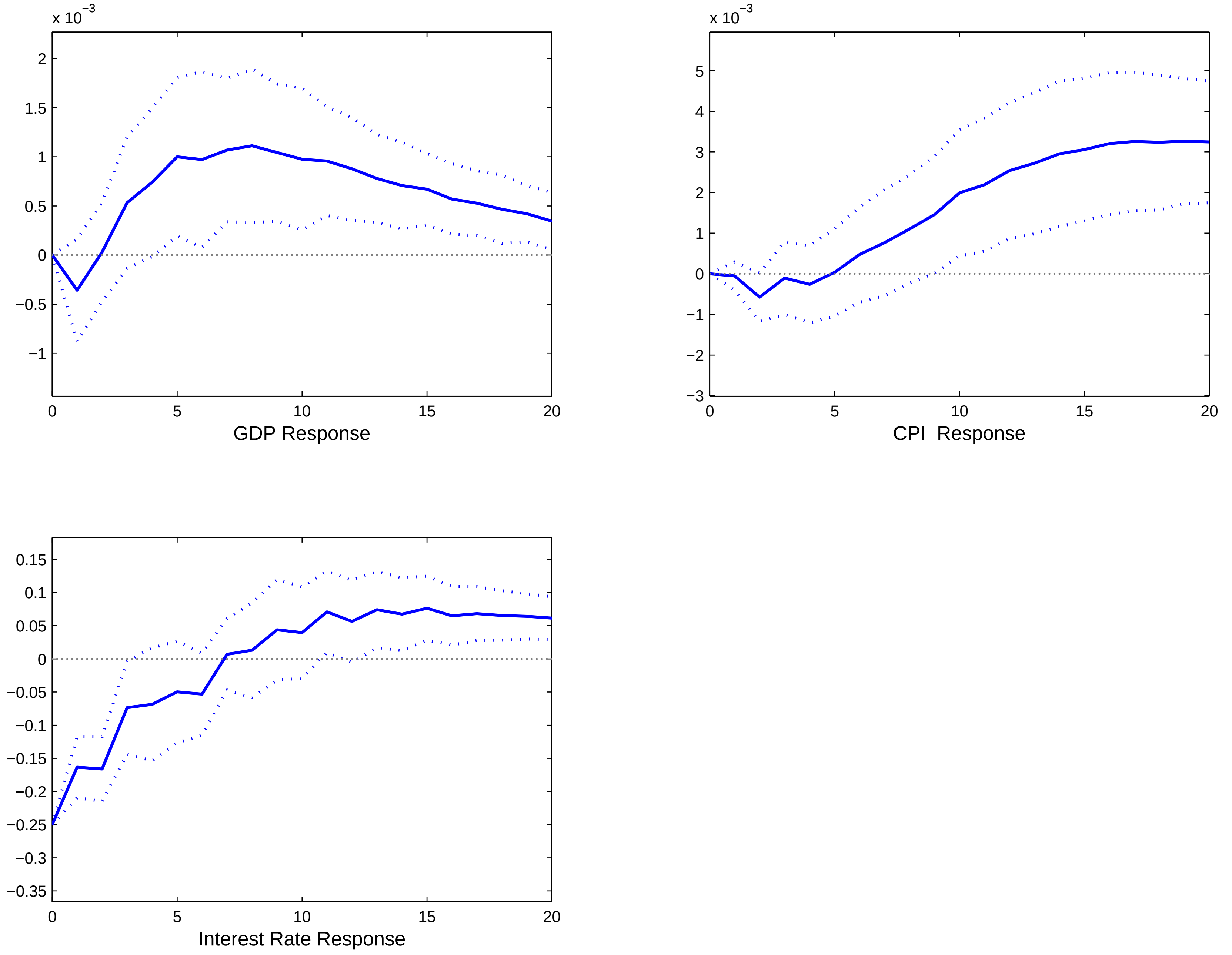

Figure 18. UK, Quarter−Dependence Q2 Shock

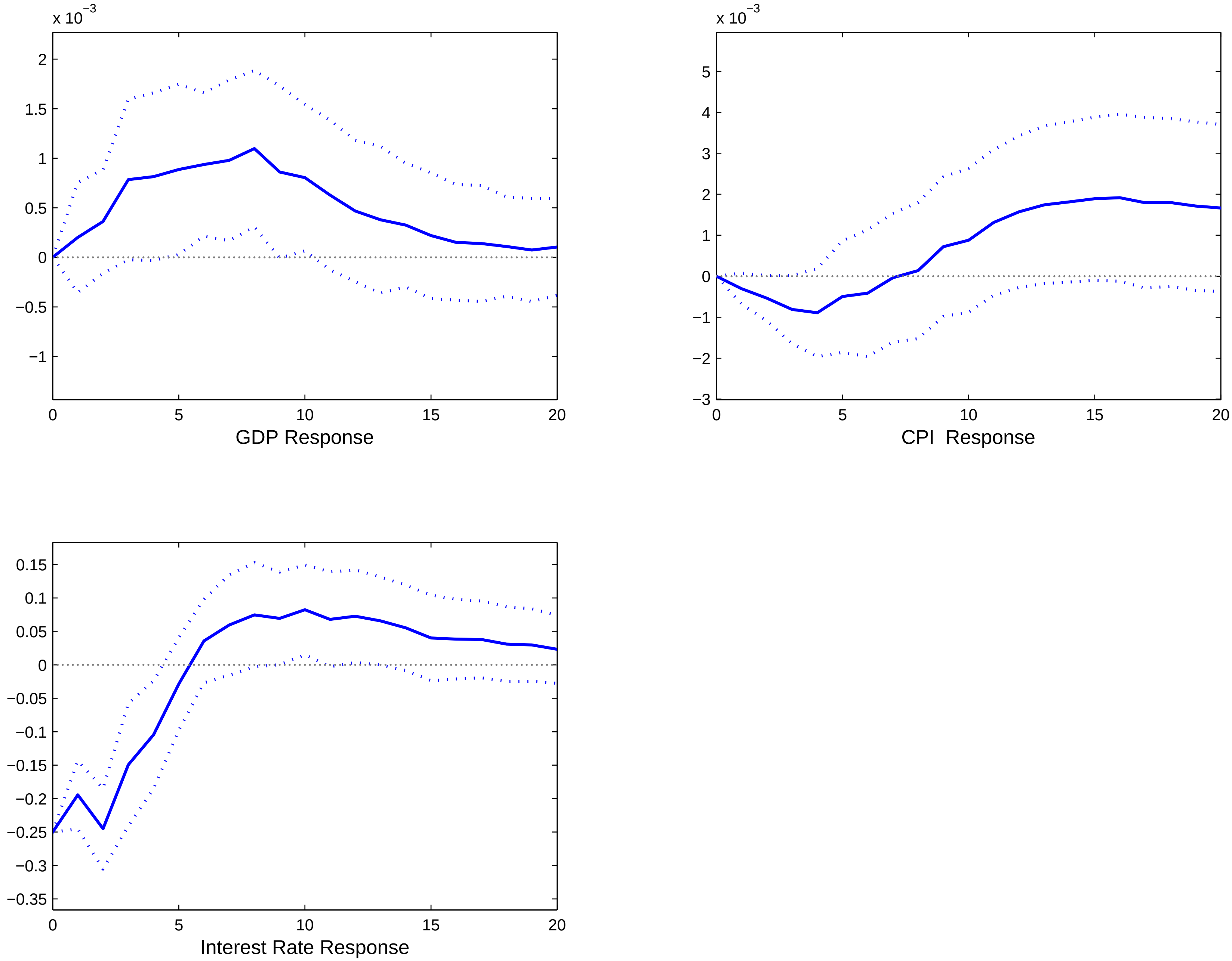

Figure 19. UK, Quarter−Dependence Q3 Shock

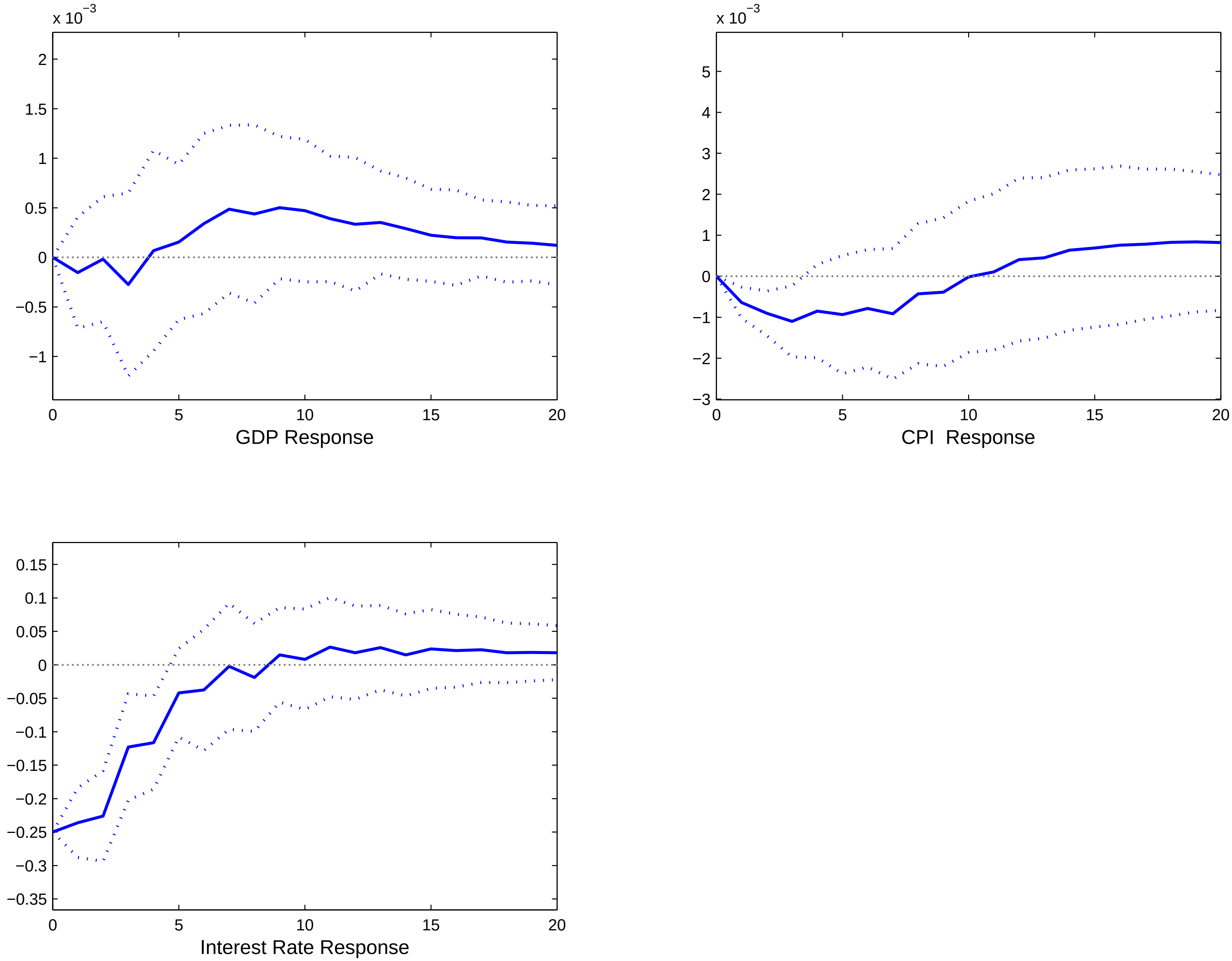

Figure 20. UK, Quarter−Dependence Q4 Shock

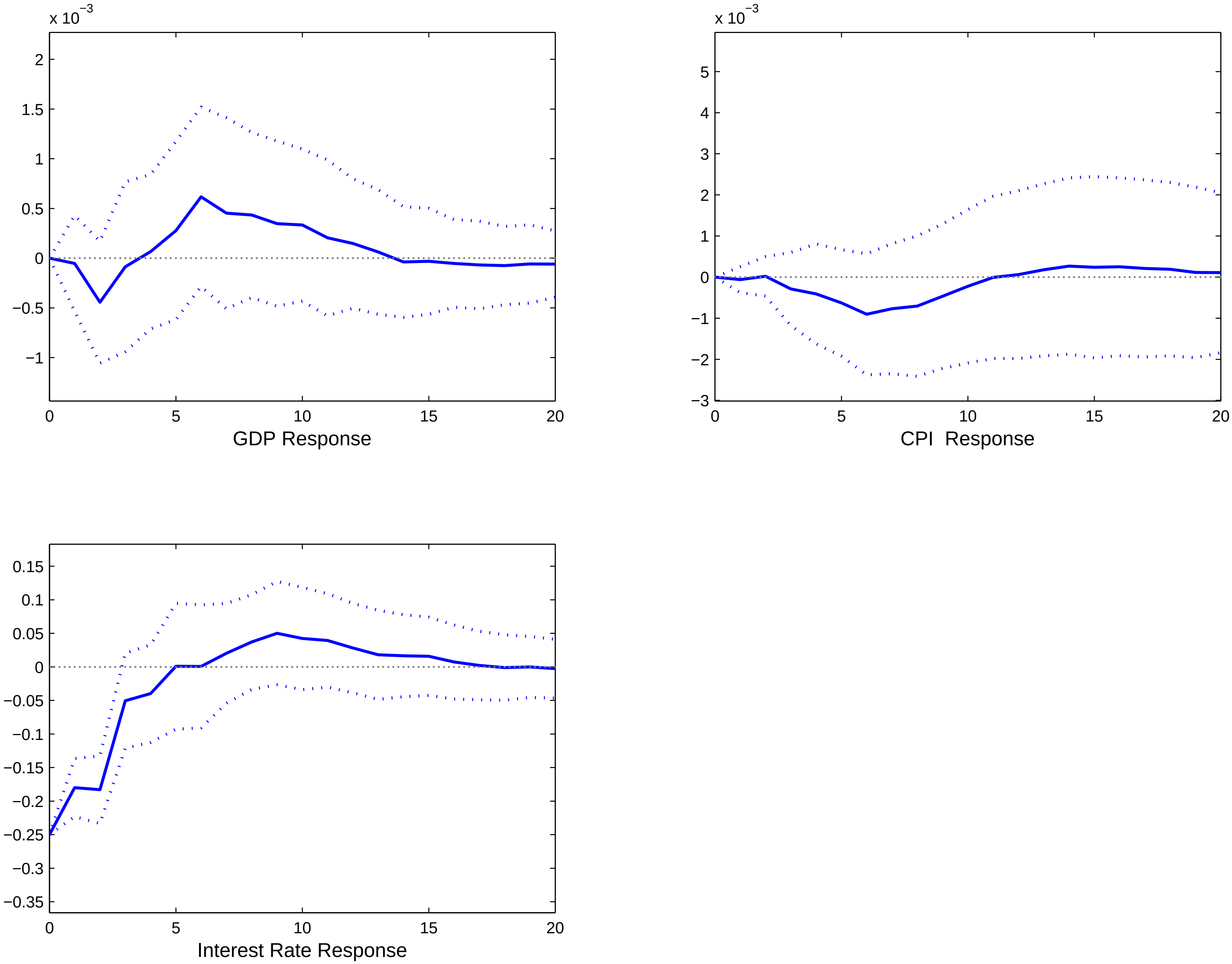

TABLE 1 – DIFFERENCES IN IMPULSE RESPONSES ACROSS QUARTERS (JAPAN)
(*p-values for D-statistic*)

	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 (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 3 – 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 4 – 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 5 – KOLMOGOROV-SMIRNOV TESTS OF IDENTICAL DISTRIBUTIONS OF MONETARY POLICY SHOCKS IN DIFFERENT QUARTERS
(*p-values for KS-test*)

Japan			
Quarters	Q1	Q2	Q3
Q2	0.38		
Q3	0.42	0.15	
Q4	0.64	0.16	0.94
Germany			
Quarters	Q1	Q2	Q3
Q2	0.44		
Q3	0.28	0.17	
Q4	0.97	0.64	0.64
France			
Quarters	Q1	Q2	Q3
Q2	0.94		
Q3	0.94	0.78	
Q4	0.56	0.56	0.36
United Kingdom			
Quarters	Q1	Q2	Q3
Q2	0.99		
Q3	0.99	0.96	
Q4	0.99	1.00	0.96