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Journal of
MONETARY
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CONTENTS
D. Coen-Pirani
Understanding gross worker flows across U.S. states 769
G. Olivei and S. Tenreyro
Wage-setting patterns and monetary policy: International evidence 785
A.J. Patton and A. Timmermann
Why do forecasters disagree? Lessons from the term structure of cross-sectional dispersion 803
E. Faraglia, A. Marcet and A. Scott
In search of a theory of debt management 821
J.-P. Danthine and A. Kurmann
The business cycle implications of reciprocity in labor relations 837
T. Kurozumi and W. Van Zandweghe
Labor market search, the Taylor principle, and indeterminacy 851
S. Chakraborty, C. Papageorgiou and F. Pérez Sebastián
Diseases, infection dynamics, and development 859
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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 for the transmission of monetary policy. Synchronization in wage-setting decisions is prevalent in Japan and the United States, yielding varying degrees of wage rigidity within the year; instead, in France, Germany, and the United Kingdom decisions are more uniformly spread over time. Exploiting within-year variation in the timing of wage-setting decisions in these economies, we find support for the long-held but scarcely tested view that wage rigidity plays a critical role in the transmission of monetary policy.

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 extent of nominal wage rigidity and its relevance 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. Synchronization of wage-setting decisions varies significantly across advanced economies. In Japan, the best-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, the available evidence suggests that 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 rigidity would hence predict that, other

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[1] Christiano et al. (2005) 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 now rich information on the frequency of adjustment, though authors differ in their reading of the evidence. (See Bils and Klenow, 2004 and Nakamura and Steinsson, 2008).

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things equal, monetary policy innovations in Japan should have a larger effect when the shock takes place in the second half of the year, that is, 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 this study is to test whether these predictions find support in the data. More precisely, this study assesses whether the response of the economy to monetary policy shocks differs according to 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, this study introduces quarter dependence in an otherwise standard, recursive VAR setup and analyzes the empirical impulse responses of aggregate variables to a monetary policy innovation in five large and highly developed countries. The countries considered are France, Germany, Japan, the United Kingdom, and the United States. The 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 it tests for potential differences in the effect of monetary policy across quarters of the calendar year for each of the countries considered, and then relates the findings across countries to each country's degree of wage rigidity over the calendar year.[4] The findings indicate that, for both Japan and the United States, there are, indeed, important 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 – that is, during the Shunto period in which wages are being reset – has a relatively small effect on output, whereas an innovation in the third quarter – that is, 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 the degree of wage rigidity is more uniform within the year and the contracts are of longer duration, 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 decisions also matters for the transmission of monetary impulses in countries other than the United States. Overall, the 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.

2. Wage-setting practices and monetary policy instruments in large developed countries

This Section begins by describing the wage-setting practices in the countries analyzed, and then discuses 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 (see for example, Grossman and Haraf, 1989; Taylor, 1999; Du Caju et al., 2008). Since 1955, it has became 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 contracts are stipulated 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 nonunionized employees negotiate their wages during April and May (Sako, 1997; Taniuchi, 1982). All wage settlements last for one year (Du Caju et al., 2008). Despite a fall in unionization rates since the early 1970s, the Shunto system of yearly wage negotiations has not been altered and the practice of setting wages in the spring has also been followed by the growing nonunionized sector

[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 (including variation in labor market institutions and in the conduct of monetary policy).

(Du Caju et al., 2008).[5] This coordinated and seasonal wage-bargaining process implies that over the sample period considered in our empirical analysis, the first and second quarters of the calendar year in Japan were 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 industries in New England surveyed by the Federal Reserve Bank of Boston in 2003 for the Federal Reserve System's "Beige Book" survey indicates that a large fraction of 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. More concretely, nearly 90 percent of the firms surveyed for the Beige Book in New England reported that they revise compensation once a year, for both salaried and hourly workers; moreover, 46 percent of the firms take decisions regarding changes in base pay and 55 percent take decisions on changes in health insurance or other benefits in the last quarter of the year. The changes typically become effective in the first quarter of the following year.[7] Consistent with this evidence, the Radford Survey of compensation practices in the information technology (IT) sector reveals that among the 856 firms surveyed in 2003, more than 90 percent of the companies use a focal base-pay administration with annual pay-change reviews and that pay changes usually take place at the beginning of the new fiscal year.[8] According to the same survey, 60 percent of IT companies close their fiscal year in December.[9] Conversations with pay and compensation consultants confirm the tendency of most firms to make decisions about compensation near the end of a firm's fiscal year, with the decisions becoming effective at the beginning of the new fiscal year.[10] To the extent that there is a link between pay changes and the end of the fiscal year, it is worth noting that in the universe of U.S. companies included in the CRSP/Compustat database, the percentage of firms ending their fiscal year in the fourth quarter has always been greater than 65 percent over the period 1970 to 2008. This share was at its highest (around 75 percent) in the early part of the sample. Over time, the share of firms ending their fiscal year in the first quarter has increased to approximately 18 percent. The share of firms ending their fiscal year in the second or the third quarter has remained roughly constant over time, amounting to a combined 15 percent on average. To these various pieces of evidence, it should be added that the degree of unionization has been declining steadily over the past 40 years (Farber and Western, 2000). The distribution of expiration and wage reopening dates for collective bargaining activity does not show a pronounced seasonality.[11] Given the decline in unionization rates, this pattern is unlikely to detract much from the apparently common practice of yearly wage adjustments at the end of a firm's fiscal year. In all, the available information for the United States is indicative of a greater degree of wage rigidity in the first half of the calendar year than in the second half.

Large-scale synchronization in the timing of wage contracts, however, is not the norm in other countries. In France, evidence collected in the context of the Wage Dynamic Network (WDN) survey indicates that wages are changed once a year on average, with two separate spikes in the distribution of wage-setting decisions. One is in January and the other in July, with each of the two months accounting for 20–25 percent of wage changes (Montornes and Sauner-Leroy, 2009; see also Druant et al., 2009).[12] This double spike in the distribution of wage-setting decisions may already hint at the fact that there is more staggering in the wage-setting process in France than in Japan, where wage negotiations are concentrated in *consecutive* months. There is, however, one difficulty in interpreting the results from the WDN survey: they refer to the time when the actual change in wages occurs, rather than the time when the decision about the wage change is made. It is important to stress that what is relevant from the perspective of models with wage rigidity is the time when the

[5] Unionization rates fell from roughly 35 percent in 1970 to nearly 25 percent in 1995. Weathers (2008) notes that unions' power weakened significantly in the 1995–2002 period (although unions are currently undergoing a revival) and the contractual emphasis on wages shifted to other aspects of the compensation package. Du Caju et al. (2008), however, argue that the decline of unions has not altered the time-dependent nature of wage setting and in particular the practice of fixing wages in March and April. In any event, for reasons that we later explain, our empirical analysis for Japan focuses on a sample period ending in 1995.

[6] While there is a practice of extending semi-annual bonus payments, the extent of flexibility in actual compensation that these bonuses 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] Specifically, 54 percent of all surveyed firms make the wage changes effective in the first quarter of the calendar year, and 63 percent do so for health and other benefits. The survey questions clearly distinguish between the time when the decision about the wage change is made, and the time when the change becomes effective. The former is, as we later argue, the relevant date for assessing the importance of wage rigidity in the transmission of monetary policy.

[8] Of the remaining 10 percent, some firms revise wages based on the date the employee was hired, and others follow an "informal" system. Hence, for the modal firm, a more recent hire's pay review occurs at the same time of the year as the review of employees with longer tenure in the firm.

[9] The survey covers firms in the following sectors: semiconductors, computers, software products and services, internet and e-commerce, telecommunication products and services, capital equipment, and others. We thank Andy Rosen, formerly of Aon Consulting's Radford Survey, for providing us with the information.

[10] For example, William Brown, vice president of ORC Worldwide noted that: "From experience on the client side and from my consulting work, companies conduct their analyses, competitive positioning, and their financial situation/state of the business in Q4 of their fiscal year (most companies' fiscal year is calendar year), and they make a decision in November/December with implementation in the new year. This review and decision information is anecdotal, although supported with extensive contact and information from clients." (March 2010).

[11] Data on the timing of collective bargaining agreements can be found in the Bureau of Labor Statistics' *Current Wage Developments*, various issues.

[12] The third month in terms of proportion of firms changing wages is April, with roughly 10 percent of the changes.

settlement is agreed upon, rather than the date at which it becomes effective.[13] Heckel et al. (2008) highlight the relevance of this issue for France. They present evidence suggestive of significant lags between the time when wage changes are decided and the time when they are implemented. Additional survey evidence referring to collective bargaining agreements is presented by Du Caju et al. (2008). Collective bargaining coverage is high,[14] and the periods for collective wage negotiations are the fourth and first quarters of the calendar year and the end of the second quarter, when the minimum wage is set. New agreements typically become effective in January and July. However, delays in negotiations are frequent and as a result the actual dates of agreements often tend to be well spread throughout the calendar year. For example, according to the Bilan Annuel de la Negociation Collective, in both 2002 and 2003 agreement dates are almost uniformly staggered, with only a slightly higher concentration of wage agreements in the second and fourth quarters.[15] In all, the bimodal distribution of wage changes, together with the lags between decision and implementation, suggests that systematic seasonal wage synchronization is less prevalent in France than in Japan or the United States, where wage negotiations are conducted in a few consecutive months every year.

Synchronization of wage-setting decisions within the year is even less prevalent in Germany. (Unfortunately, results from the WDN survey were not released for Germany). As with France, the collective bargaining coverage is high.[16] According to the *Hans–Böckler Stiftung Tarifdaten*,[17] 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. This is consistent with survey evidence provided by Du Caju et al. (2008), who report an average duration of wage contracts of about two years, with no particular seasonality in the timing of the contracts. Delays in negotiations are also frequent. With multiple-year contracts, the relevance of the quarter in which a monetary shock takes place diminishes compared to its relevance with one-year contracts. 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 the specific year. Monetary policy innovations in a given quarter of the calendar year will thus have different effects in different years. Consider now 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 of the year should be smaller than in the case of yearly contracts. Overall, 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 in Germany than in Japan or the United States.

In the United Kingdom, wage settlements typically last for one year. Settlements occur predominantly at the firm level, and tend to bunch in January and April, possibly reflecting traditional financial reporting years. There is, however, also a smaller spike in settlements in the month of July. Despite this conventional element in timing, the share of settlements in the second half of the calendar year can be non-negligible. For example, according to the CBI Pay Databank, which surveys the manufacturing sector, in the years 1979–80 42 percent of the settlements had an implementation date in the period July–December. According to the same survey, at the end of the 1980s the proportion of settlements in the second half of the year was about 35 percent (Gregory et al., 1985; Ingram, 1991). In more recent years, while January and April continue to be the months with the largest number of settlements, July and October settlements have become more common.[18] It should be noted that, depending on the sector, actual negotiations tend to start one to five months before the date the new wage becomes effective. 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. Overall, this evidence suggests that while some bunching of wage settlements is present, actual decisions on wage adjustments are spread more evenly over the calendar year than in Japan or the United States.

For all the countries studied, an important issue when analyzing the role of wage rigidity in the transmission of monetary policy is whether wages of new hires are more responsive to current economic conditions than the wages of workers in ongoing employment relationships. More flexible wages for new hires will, to some degree, offset the rigidity of existing wage contracts. In Japan, the wage of new graduate hires is also agreed upon once every year in the context of the Shunto. Thus, it preserves the seasonality aspect and it varies with economic conditions in the same way as the wage of existing workers.[19] For the United States, survey evidence in Bewley (1999) indicates that wages of new hires follow the

[13] 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.

[14] Coverage is high because (i) employers voluntarily apply to nonunion members the terms of an agreement, and (ii) there are legal extension procedures that make a collective agreement binding for all employees and employers, even if some employers or trade unions did not directly sign the agreement.

[15] See Table 9.1.8 on page 416 in the Bilan Annuel de la Negociation Collective 2003.

[16] A study by the Institut für Arbeitsmarkt und Berufsforschung, in Industrielle Beziehungen, 4(4) 1997, pp. 317–334, shows that in 1995 collective bargaining covered 83.1% of west German employees. Our empirical analysis focuses on a sample ending in 1994.

[17] The Hans-Böckler Stiftung Tarifdaten is the collective-agreement archive that tracks and analyzes developments concerning collective agreements in Germany.

[18] See IRS Employment Review, issue 882, 19 October 2007.

[19] See Japan Institute for Labor Policy and Training's website at http://www.jil.go.jp/jil/laborinfo-e/docs/hrm_jp3.pdf, and Weathers (2008).

internal pay structure of a firm. However, micro data evidence shows that wages of new hires can be more responsive to economic conditions than wages of those in continuing jobs. A summary of the accumulated micro evidence is that the procyclicality of wages is especially pronounced among job changers, but even among workers staying with the same employer earnings appear to be substantially procyclical (Shin and Solon, 2006). In the United Kingdom, Devereux and Hart (2006) show that the absolute wage procyclicality of both stayers and movers is high in British micro data. As a result, job stayers account for about 95 percent of overall cyclicality. Finally, evidence from the WDN survey indicates that the wage of newly hired workers follows the internal pay structure of the firm, though there is significant cross-country variation. Results from the WDN survey for France indicate that wages of new hires are usually set according to the firm's pay scale (Montornes and Sauner-Leroy, 2009). Overall, this evidence does suggest that the wage of new hires is linked to the wage of existing hires. As a result, seasonality in wage settlements for existing hires should, at least to some degree, also apply to workers newly hired over the course of a calendar year.

The differences in the timing of wage-setting decisions and in the duration of contracts among the countries considered 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 analyzed.

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 makes clear, a parsimonious specification is adopted because the VAR-based empirical analysis is constrained by limited degrees of freedom. 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 et al., 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. It is thus assumed 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 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 considered, 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, on the other hand, external constraints have also operated in the post-Bretton Woods era. Clarida et al. (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 et al. (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. Empirical studies often suggest only a limited role for an independent response of the Federal Reserve to deviations of money from the target path.[20] 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; Bernanke and Mihov, 1998).

[20] See Friedman and Kuttner (1996), among others. 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.

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 1975–1990. 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 1975–1995. As for France and the United Kingdom, given the mentioned influence of Germany's monetary policy, it is plausible to assume that a short-run measure of the interest rate played a relevant role in setting policy.

3. Method

This Section discusses the empirical model and describes the testing strategies. It then introduces the data and estimation approach.

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 (for example, output and prices), and p_t is the variable that summarizes the policy stance. The policy variable is given by a short-term interest rate, with innovations in this measure representing monetary policy shocks. Eq. (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 $\mathbf{v}^y$. Eq. (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.[21,22] Thus, 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.[23]

The system represented by Eqs. (1) and (2) replicates the specification of Bernanke and Blinder (1992), with the crucial difference that it allows 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 jth 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, $\mathbf{Y}_t$.[24]

Given the identifying assumption that policy shocks do not affect macro variables within the current period, the system can be re-written 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 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.

[21] 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.

[22] 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).

[23] See, among others, Bernanke and Blinder (1992), Rotemberg and Woodford (1997), Bernanke and Mihov (1998), Christiano et al. (1999), and Boivin and Giannoni (2006).

[24] 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 Eqs. (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.

One implication of quarter dependence is that the immediate effects of monetary policy shocks can differ, depending on the quarter in which 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 in (3) describe our benchmark specification. The robustness section discusses results based on more general specifications, which can be written in reduced form as

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

where now $\tilde{\mathbf{X}}_t = [\mathbf{Y}_t, p_t, \mathbf{Y}_{2,t}]$, $\mathbf{Z}_t$ is a vector of exogenous variables, $\tilde{\mathbf{U}}_t$ a vector of reduced-form residuals, and $\tilde{\mathbf{A}}(L,q_t)$ and $\mathbf{B}(L,q_t)$ are lag polynomials that allow coefficients at each lag to depend on the particular quarter q indexing the dependent 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 $\tilde{\mathbf{X}}$ 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 considered. The additional identifying assumption in the context of the reduced-form VAR in (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, the paper also discusses findings based on a different identification strategy. The vector of exogenous variables $\mathbf{Z}_t$ comprises variables such as commodity prices and foreign interest rates. The inclusion of 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). As for foreign interest rates, over the sample period considered 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 there is no feedback from $\tilde{\mathbf{X}}$ 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 in (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, it is important to assess the significance of quarter dependence on the impulse-response functions more 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:

$$D = \sup_t |x_t^q - x_t|, \tag{5}$$

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.[25] We also consider the absolute value of the cumulated difference between the impulse responses of variable x in the quarter-dependent VAR and in the standard non-time-dependent VAR.[26] The difference is cumulated over 20 periods after the shock:[27]

$$CD = \left| \sum_{t=1}^{20} (x_t^q - x_t) \right|. \tag{6}$$

While the D statistic captures the maximum size of the difference in impulse responses, the CD statistic captures the size and persistence of the difference in responses. For each of these two statistics, an empirical distribution is constructed by bootstrapping the residuals of the reduced-form non-time-dependent VAR. At each draw, a new data set is generated and new impulse responses are estimated from both the quarter-dependent and standard VARs. This yields a new value for D^s

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

[26] We thank the editor and an anonymous referee for suggesting the computation of the *CD* statistic.

[27] The structure of the VAR in (3) implies that in the period of the monetary policy shock (t=0), the quarter-dependent and non-time dependent responses are equal. For this reason, we consider the difference in responses starting in the period immediately following the shock.

(CD^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 (CD^s) exceeding the observed D (CD).

3.3. Data and estimation

Our empirical analysis is based on seasonally adjusted quarterly data.[28] 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 considered.

For Japan, the data runs from 1964:Q1 through 1995:Q2. 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, the data covers the period 1966:Q1 through 2006:Q4. 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 includes commodity prices 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, the sample covers the period 1964:Q1 to 1994:Q4. Unification complicates the use of German data, and to obtain a consistent measure of output, the analysis uses 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, the data runs 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. The analysis also considers 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, the data covers the period 1964:Q1 to 1997:Q1. The sample ends prior to the independence of the Bank of England 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 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 in the case of 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. The analysis also includes the German Lombard rate as an exogenous variable in $\mathbf{Z}_t$.

Alternative specifications in which oil prices (expressed in U.S. dollars per barrel) are included in $\mathbf{Z}_t$ as an exogenous variable were also considered for all countries with the exception of the United States, where commodity prices are already included as an additional variable in $\mathbf{Y}_t$.[29] In all of the specifications, the variables enter the VAR analysis in log levels except for interest rates, which are expressed in levels.[30] 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. Each of the equations includes quarter-dependent intercepts. The estimated VAR includes four lags of the endogenous variables and each equation in the VAR is estimated separately. As in Olivei and Tenreyro (2007), we use unconstrained ordinary least squares (OLS) for the United States. For the other countries, given the large number of coefficients that need to be estimated in a quarter-dependent VAR and the relatively short available sample periods, the coefficients on the four lags of each of the endogenous variables within a given equation are estimated by means of a second-order polynomial distributed lag. Section 5 notes that the empirical findings are not driven by this constrained estimation. Unconstrained OLS produce the same qualitative 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 in (3). The monetary policy shock corresponds to a 25-basis-point decline in the policy rate on impact. In the interest of space, the figures plot the impulse responses to a policy shock that takes place in the first and third quarter, together with the response to a policy shock from a standard VAR (when we do not allow for quarter dependence). The complete set of responses to shocks originating in the

[28] 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.

[29] For further discussion, see footnote 18.

[30] We also studied specifications in which monetary aggregates entered in terms of growth rates, rather than log-levels.

other quarters, as well as the confidence-band intervals and the responses of the policy variable for all countries, are displayed in Figures A1 through A25 in the online Supplemental Appendix. The *p*-values for the *D* and *CD* statistics are also reported in the online Appendix, in Tables A1 through A5. Section 5 discusses the robustness of the empirical findings to specifications that include additional endogenous (and possibly also exogenous) variables, as represented by the VAR in (4).

4.1. Japan

Impulse responses for Japan are depicted in Fig. 1. The response of activity to the policy shock (Panel *a*) when we do not allow for quarter dependence is persistent and peaks at about 8 quarters after the shock. The response to a policy shock occurring in the first quarter of the year is essentially nil. In contrast, activity responds very strongly to the policy shock when the shock takes place in the third quarter, with a peak at about 6 quarters after the shock. The responses of activity to the policy shock in the second and in the fourth quarter display similar patterns: they are significant and stronger than in the first quarter, but considerably weaker than in the third quarter. The response of prices to the policy easing (Panel b) in the non-quarterly dependent VAR displays a price puzzle. When the shock takes place in the first quarter, the price response is generally positive on impact, although it is estimated rather imprecisely. When the shock takes place in the

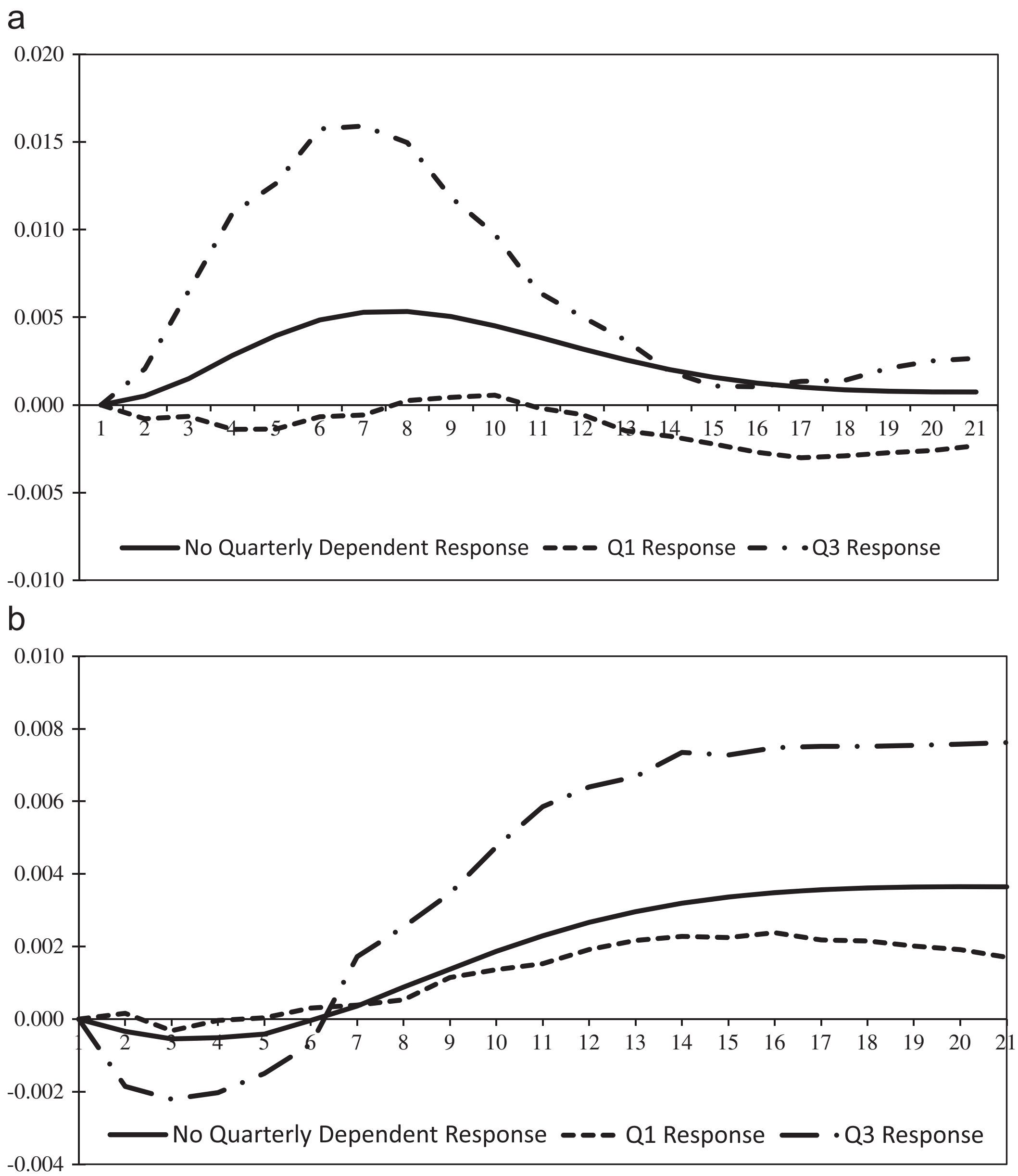

Fig. 1. Japan, 25-basis-point decline in call rate. 1963:Q1–1995:Q2. (a) Response of industrial production. (b) Response of consumer price index. Note: The figure reports impulse responses of industrial production and prices to a monetary policy shock in a standard non-quarter-dependent VAR (solid line) and in a quarter-dependent VAR. For this latter specification, the figure reports impulses responses following a shock in the first and in the third quarters of the calendar year.

third quarter, the price response in the first few periods following the shock displays a more pronounced price puzzle, becoming positive 7 quarters after the shock.

The findings illustrate that the response of economic activity to a monetary policy shock differs noticeably according to the quarter in which the policy shock takes place. The pattern is consistent with Japan's non-uniform distribution of wage contracts over the calendar year. Activity responds insignificantly in the first quarter, a period of great wage flexibility, with many wage contracts being renegotiated in March and taking effect at the very beginning of the second quarter. Activity responds most in the third quarter. This is a period of high wage rigidity, as it occurs right after all Shunto-related wage negotiations have ended.

The difference in impulse responses is corroborated by the three 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. According to the tests based on the *D* -statistic and the *CD*-statistic, 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.[31] It is also apparent from the plotted impulse responses that the difference in the response of activity to a policy shock in the first or in the third quarter is significant from an economic standpoint.

4.2. United States

Fig. 2 depicts impulse responses to a policy shock for the United States. The figure essentially updates the findings in Olivei and Tenreyro (2007). The output response without allowing for quarter dependence is persistent, peaking seven quarters after the shock and slowly decaying thereafter. When the policy shock occurs in the first quarter of the year activity displays a fairly rapid response, which then persists for some time. The peak response is now almost twice as large as in the case with no quarter dependence. When the shock takes place in the second quarter, the response of output is even faster and more sizable.[32] When the shock occurs in the third quarter, the response of output is instead small and short-lived. A similar pattern is also evident in the responses to a policy shock occurring in the fourth quarter. The responses of prices show the opposite pattern. In the system without quarter dependence, prices start to rise reliably a year after the shock, although it takes about two years for the increase to become significant. When the shock takes place in the first quarter, the price response is slow and despite controlling for oil prices, there is a "price puzzle," although the decline in prices is not statistically significant. When the shock takes place in the third quarter, prices increase immediately.

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 limited impact on output in the second half of the year. In essence, a policy shock then is "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 Fig. 2 are also supported by formal tests on the importance of quarter dependence. The *F*-test in the reduced-form VAR in (3) is highly indicative of the presence of quarter dependence for the price equation, while significance is more marginal for the output equation. The *CD*-statistic corroborates the quantitative importance and the persistence of the difference between the response of output to a first quarter shock and the response from the non-quarter-dependent system. It also highlights that the response of output to shocks originating in the third quarter is significantly different from the non-quarter-dependent response. The difference in the response of activity to a policy shock in the first or in the third quarter is also substantial from an economic standpoint.[33]

4.3. Germany

The impulse responses for West Germany are depicted in Fig. 3. The impulse response of activity corresponding to the VAR without quarter dependence peaks about 8 quarters after the shock and slowly decays thereafter. The impulse

[31] The *D*- and *CD*-statistics in Table A1 also indicate that the price responses to a policy shock in the third and fourth quarters are significantly different from the non-quarter-dependent response. The quarterly dependent responses show prices reaching a higher level in the third and fourth quarters than in the non-quarter-dependent case. This is not inconsistent with higher wage flexibility in the first half of the 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. 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 of the shock. This will depend, among other things, on how strongly the monetary authority responds to inflationary pressures.

[32] In the second quarter, output reaches its peak three 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.

[33] Interestingly, the *CD*-statistic indicates that the strong, fast, but short-lived response of output to a second-quarter shock is not statistically different from the non-quarter-dependent response. Put differently, over a 20-quarter period, the more persistent non-quarter-dependent response reaches cumulative levels that are not statistically different from the response to a second-quarter shock. This illustrates that the *D*- and *CD*-statistics capture different features of the difference in the response. From a policy point of view, the difference in these two statistics is interesting because it speaks to differences in the shape (or timing) of the response. In situations in which the near-term impact is important, even though the cumulative responses are similar over a 5-year period, the faster response to a second-quarter shock (captured in this case by the *D*-statistic) might be critical.

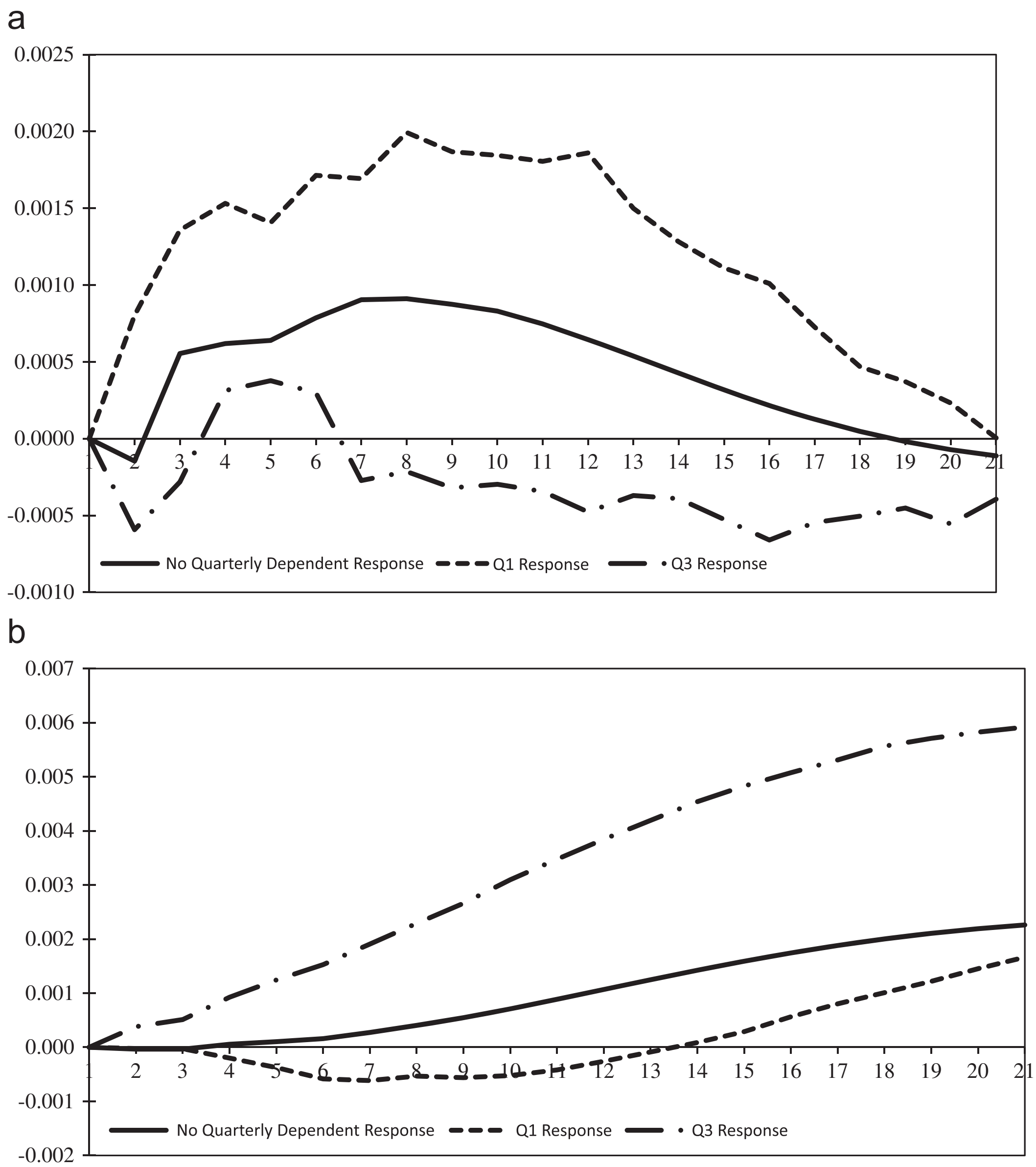

Fig. 2. United States, 25-basis-point decline in federal funds rate. 1966:Q1–2006:Q4. (a) Response of GDP. (b) Response of GDP deflator. Note: The figure reports impulse responses of real GDP and prices to a monetary policy shock in a standard non-quarter-dependent VAR (solid line) and in a quarter-dependent VAR. For this latter specification, the figure reports impulses responses following a shock in the first and in the third quarters of the calendar year.

responses corresponding to the quarter-dependent VAR in (3) are remarkably similar across quarters, except perhaps for a slightly weaker response of activity following a shock in the fourth quarter, though the differences are not statistically significant. The price response displays a fairly protracted price puzzle, but again there is little difference across quarters.

An *F*-test on the relevance of quarter dependence for the real activity equation in the reduced-form VAR in (3) yields a *p*-value of 0.52. According to the *D*-statistic, 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 *CD*-statistic identifies a statistically significant difference between the output response to a shock in the third quarter and the non-quarter dependent response. Most of the difference between the third quarter and the non-quarter dependent response, however, appears to cumulate two years after the shock.

Overall, 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

Fig. 4 depicts impulse responses to a policy shock for France. 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

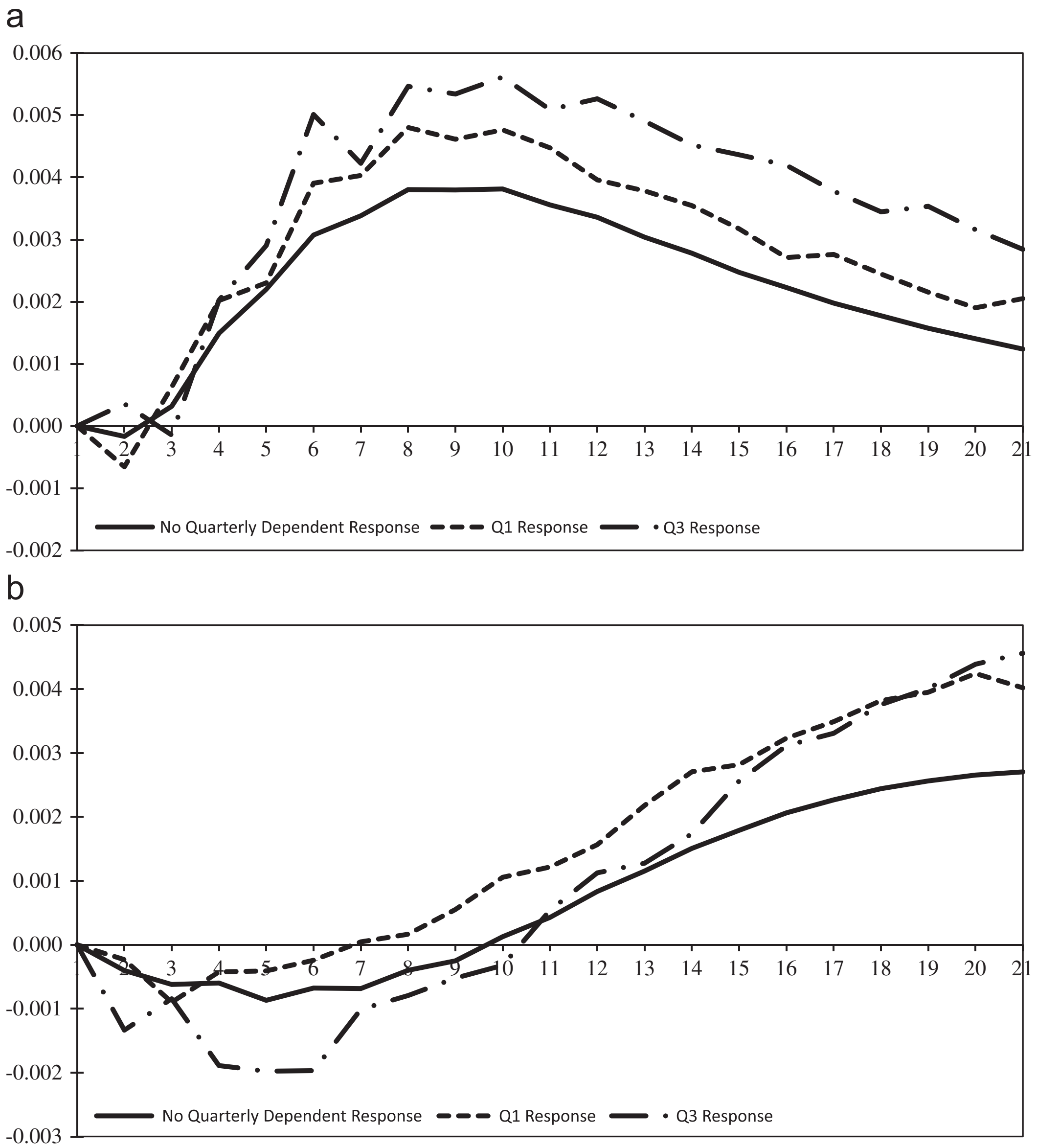

Fig. 3. Germany, 25-basis-point decline in Lombard rate. 1963:Q1–1994:Q4. (a) Response of GDP. (b) Response of GDP deflator. Note: The figure reports impulse responses of real GDP and prices to a monetary policy shock in a standard non-quarter-dependent VAR (solid line) and in a quarter-dependent VAR. For this latter specification, the figure reports impulses responses following a shock in the first and in the third quarters of the calendar year.

VAR in (3) are fairly similar across quarters, both for activity and prices. 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* and *CD* statistics in each quarter for activity and prices show that none of the quarter-dependent responses are statistically different from the corresponding non-quarter-dependent responses.

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, is not enough to generate significantly different output and price responses across quarters for the economy as a whole.

4.5. United Kingdom

The impulse responses to a policy shock in the United Kingdom are displayed in Fig. 5.[34] The pattern of the responses without allowing for quarter dependence is similar to the one documented for the other countries, with a persistent response of output that outlasts the shock to the policy rate. As for France, the quarterly responses are estimated rather imprecisely.

[34] 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. See also Figs. A21–A25 in the online supplemental appendix.

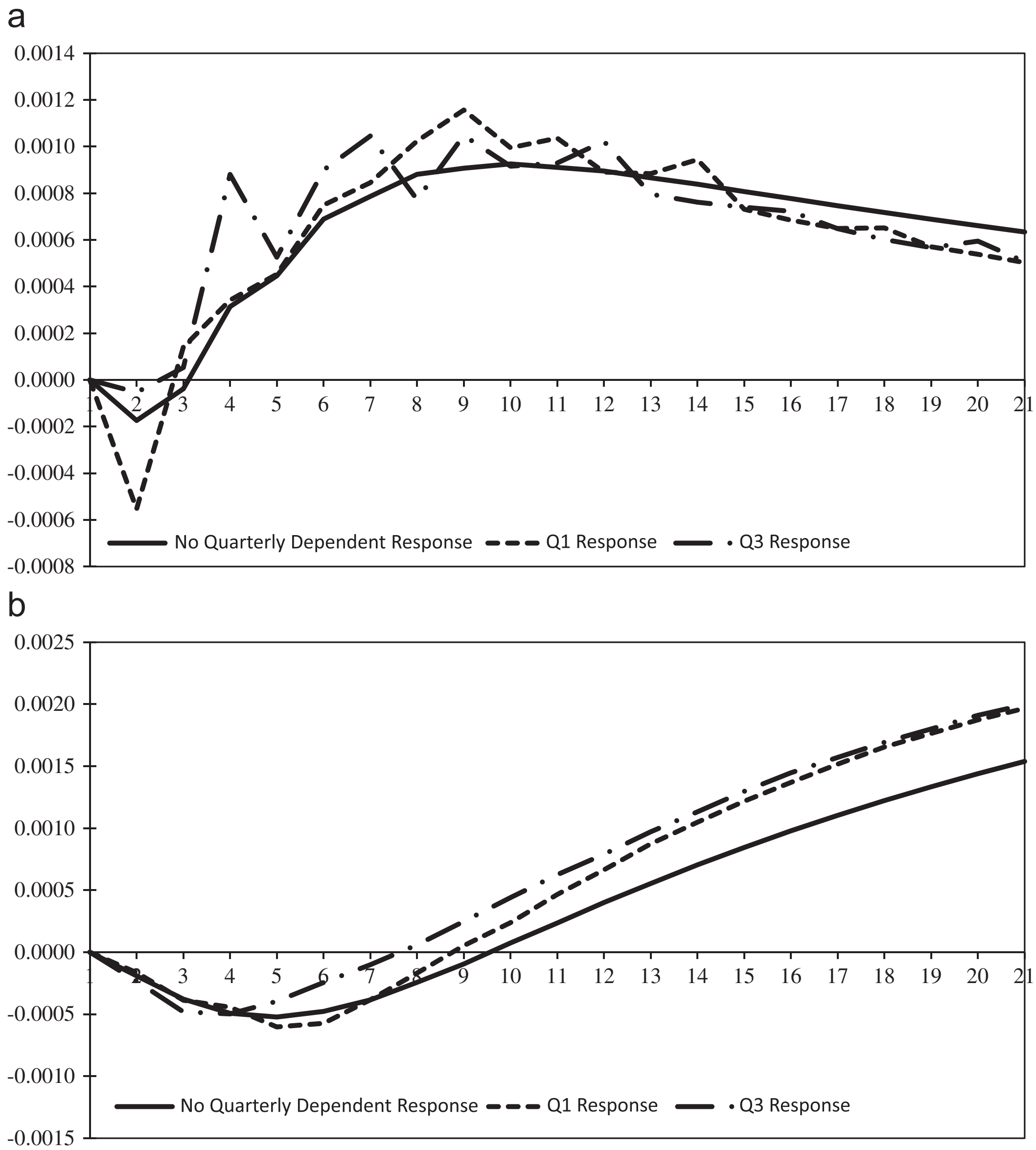

Fig. 4. France, 25-basis-point decline in call rate. 1963:Q1–1998:Q4. (a) Response of GDP. (b) Response of consumer price index. Note: The figure reports impulse responses of real GDP and prices to a monetary policy shock in a standard non-quarter-dependent VAR (solid line) and in a quarter-dependent VAR. For this latter specification, the figure reports impulses responses following a shock in the first and in the third quarters of the calendar year.

The output response is larger when the shock occurs in the first half of the calendar year. However, there is little to suggest that prices are more rigid in the first half of the calendar year.[35] The response of prices, as the figure shows, is faster in the first quarter than in the third quarter. While the stronger response of activity in the first half of the year is consistent with the observation that January and April are important pay settlement months, the price response makes the empirical findings on the whole difficult to reconcile with an explanation that relies on a non-uniform distribution of wages over the calendar year.

The hypothesis that quarter dependence is not statistically relevant for describing the reduced-form dynamics of the economy is not rejected at standard confidence levels. An F-test on the relevance of quarter dependence for the real activity equation in the reduced-form VAR in (3) yields a p-value above 0.9. The bootstrapped p-values for the D and CD statistics show that none of the quarter-dependent GDP responses are statistically different from the corresponding non-quarter-dependent response 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

[35] 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 more similar across different quarters.

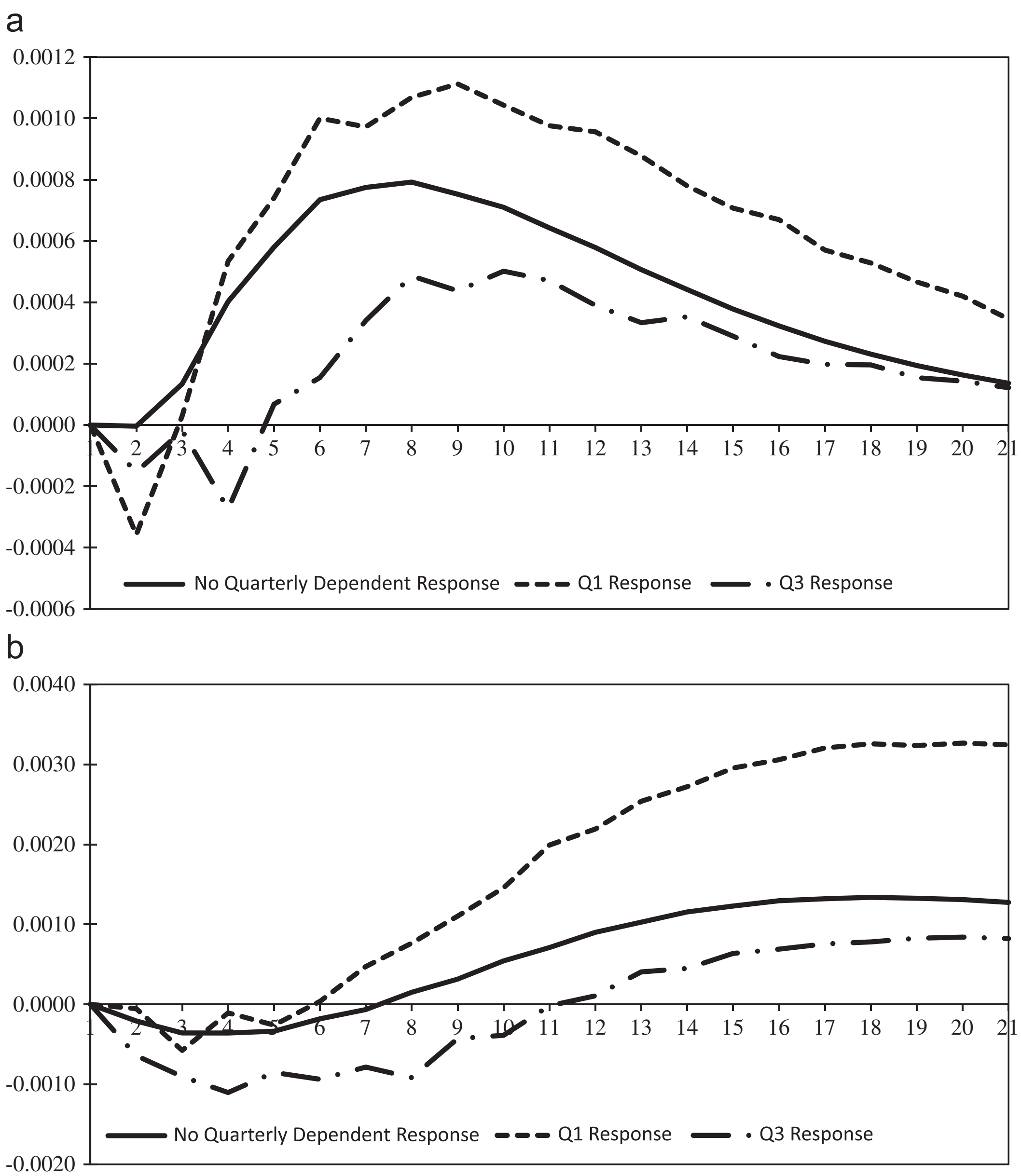

Fig. 5. United Kingdom, 25-basis-point decline in three-month treasury bill rate. 1963:Q1–1997:Q1. (a) Response of GDP. (b) Response of consumer price index. Note: The figure reports impulse responses of real GDP and prices to a monetary policy shock in a standard non-quarter-dependent VAR (solid line) and in a quarter-dependent VAR. For this latter specification, the figure reports impulses responses following a shock in the first and in the third quarters of the calendar year.

versus contractionary) of shocks could result in different impulse responses. To explore this hypothesis, a Kolmogorov–Smirnov test is used to test for the equality of the distributions of the shocks across quarters. The test consists of a pairwise comparison of the distributions of shocks between any two quarters with the null hypothesis of identical distributions. The p-values for these tests are displayed in Table 1 for the five countries considered. As the table shows, in no instance the null hypothesis of identical distributions across quarters can be rejected. The results for Japan and the United States deserve some attention, given that these are the countries with significant differences in the response of activity to a policy shock across quarters. Specifically, for both these countries, the null hypothesis of equal distribution of policy shocks in any two quarters cannot be rejected.

Another issue of concern is whether the different impulse responses for activity across quarters documented 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.[36] 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 in activity documented by Beaulieu and Miron for either Japan or the United States does

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

Table 1
Kolmogorov–Smirnov tests of identical distributions of monetary policy shocks in different quarters (*p-values for test*).

Quarters	Q1	Q2	Q3
Japan			
Q2	0.58		
Q3	0.25	0.25	
Q4	0.88	0.65	0.78
United States			
Q2	0.89		
Q3	0.98	0.72	
Q4	0.53	0.36	0.36
Germany			
Q2	1.00		
Q3	0.78	0.36	
Q4	0.36	0.56	0.06
France			
Q2	0.44		
Q3	0.64	0.28	
Q4	0.97	0.44	0.84
United Kingdom			
Q2	0.97		
Q3	1.00	0.96	
Q4	0.99	0.81	1.00

Note: The table reports $p-$values for pairwise comparisons of the distributions of monetary policy shocks between any two quarters for each country considered, with the null hypothesis of identical distributions.

not conform to the pattern of our quarterly responses to the policy shock.[37] 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 that does not display a seasonal expansion (or recession). 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

This Section summarizes results pertaining to the robustness of our baseline specification along several dimensions.[38] As already mentioned, the benchmark reduced-form VAR in (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, only one endogenous variable is added at a time. The first exercise consists of introducing 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. So, it is interesting to check whether the findings change when the wage dynamics is explicitly introduced into the system. It turns out that the results are largely unaffected by this modification to our benchmark

[37] See Table 3 in their paper.

[38] In the interest of space, we provide in this section a discussion of our findings but 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.

specification. 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, introducing wages in the VAR has the benefit of making the price responses to a shock occurring in the second half of the calendar year more similar to the price responses to a shock occurring in the first half of the year.

The robustness of our findings was also checked using the extended specification described by the reduced-form VAR in (4). As mentioned in Section 2.2, for parts of the sample period studied, central banks in Japan, the United States, and Europe had set monetary targets. The baseline specification is thus augmented by introducing money as an additional endogenous variable belonging to $\mathbf{Y}_2$, such that it is ordered last in the VAR.[39] The additional identification assumption made in this case is that an interest rate shock can affect money on impact, but not vice-versa. Because of limited data availability, this exercise is performed only for Japan, the United States, and Germany. The quarterly responses remain very similar to the ones estimated with the baseline specification. An interesting by-product 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, on the other hand, 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, the United States, or Germany. Therefore, it is important to check whether the baseline results for France and the United Kingdom change when introducing the country's exchange rate vis-à-vis the deutsche mark as an additional variable. The exchange rate is ordered last in the VAR, with the identifying assumption that an interest rate shock can affect the exchange rate on impact, but not vice-versa. The 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 are little influenced by introducing 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 similar results.[40] Another way of modeling the external constraint for France and the United Kingdom is to introduce Germany's policy rate as an explanatory variable.[41] 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 (4) as an exogenous variable belonging to $\mathbf{Z}$. Again, the results are not materially affected by incorporating this control.

Finally, because in the baseline specification several impulse responses exhibit a noticeable price puzzle, it is interesting to check 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 States and the United Kingdom,[42] this control variable provides little improvement in the price responses, while the responses of activity are not affected.

Overall, the baseline findings appear robust to the introduction of additional variables in the specification. As concerns robustness to the sample period, 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. Estimating 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.

6. Concluding remarks

Our main conclusions have been amply foreshadowed. The empirical analysis indicates 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 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 monetary policy shock in Japan that occurs in the first part of the year, that is, when the Shunto is taking place, should have a small impact on output, since this is a period of relative wage flexibility. In contrast,

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

[40] 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.

[41] 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.

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

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 monetary policy 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 latter countries, synchronization in wage setting has been lower, with wage bargaining more uniformly distributed across the calendar year and wage contracts lasting for longer than a year in some instances. Correspondingly, the response of activity to a monetary policy shock has been more uniform across quarters.

This paper makes 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.[43] 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 into consideration from a modeling standpoint. Finally, it is interesting to note that there appears to be some synchronization in the timing of wage and price changes. In this respect, January is the month when the frequency of adjustment for wages and prices is relatively high in some countries.[44] The extent to which this relationship is causal, with the seasonality in wage changes imparting seasonality to price changes, is a topic that deserves more research.

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Appendix A. Supplementary data

Supplementary data associated with this article can be found in the online version at doi:10.1016/j.jmoneco.2010.08.003.

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[43] 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.

[44] Nakamura and Steinsson (2008) report evidence of seasonality in the frequency of price changes in the United States. Specifically, they note that the frequency of price changes peaks in January, and suggest that this pattern may be related to the seasonality of wage changes. Within the countries surveyed by the WDN, Druant et al. (2009) show seasonal dependence in the frequency of wage and price changes, with January being the peak month.

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