Empirical Analysis on Unconventional Monetary Policies and Issues around Policy Conduct

by

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Submitted in Partial Fulfillment of the Requirements
for the Degree of Doctor of Philosophy in Economics

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2021

Abstract

Central banks conduct monetary policy amid of uncertainty about the state of the economy. Information available in real time is imperfect, and the recent financial crisis forces policymakers to decide policy actions in a highly uncertain economic environment with the presence of the effective lower bound.

Concerning the issue on imperfect information, major macroeconomic data available in real time are subject to revision. Since upcoming data revisions are hardly predictable, economic agents cannot have perfect information on the current and past state of the economy. Accordingly, both policymakers and private agents have to make decisions based on less accurate information. The data uncertainty about the state of the economy may affect decision-making of policymakers by influencing the private sector's optimization. While the problem of data uncertainty is important in the practice of monetary policy, the impacts of data uncertainty on macroeconomic models are far less explored.

The recent financial crisis has heightened uncertainty about future monetary policy in a very low interest rate environment. After the crisis erupted, central banks in advanced economies have adopted unconventional measures of monetary policy to stabilize the financial system and spur economic recovery. Several economies including euro area and Japan have embarked on negative interest rate policy, and major advanced economies have been utilizing large asset purchase programs.

Negative interest rate policy marks a dramatic shift from conventional policies with positive policy rates. However, it is unclear whether the move from zero interest rate policy to negative interest rate policy reduces the binding constraint on monetary policy. A vast literature studies when and how well the asset purchase programs by cen-

tral banks work. Recent studies argue possibility of state-dependence of asset purchase programs, though there is yet no consensus on this issue.

The aim of my dissertation is twofold. First, I intend to uncover how data uncertainty about current and past state of the economy influences business cycles. I shed light on the effects of data uncertainty about labor productivity, which is obviously a key variable in monetary policy decisions. Second, I aim to explore the effectiveness of unconventional monetary policies in a changing environment after the financial crisis. I focus on two popular measures of unconventional monetary policy, namely, negative interest rate policies and asset purchase programs.

My dissertation is organized as follows. Chapter 1 summarizes the literature and presents motivation and research questions of my dissertation. Chapter 2 investigates influence of data uncertainty on business cycles, which plays an important role in monetary policy conduct. Chapter 3 studies negative interest rate policies conducted in several advanced economies. Chapter 4 examines changes in the effectiveness of unconventional monetary policies in the United States since the recent financial crisis. Chapter 5 concludes my dissertation and suggests interesting avenues for future research.

Acknowledgements

First of all, my sincere gratitude goes to my two primary supervisors, Professors Etsuro Shioji and Yohei Yamamoto for their patient instructions and constructive advice on my dissertation. I would like to thank Professors Takashi Kano, Junko Koeda, and Toshiaki Watanabe for their valuable comments and suggestions.

Chapter 3 is based on Fatum, Rasmus, Naoko Hara, and Yohei Yamamoto (2019): "Negative Interest Rate Policy and the Influence of Macroeconomic News on Yields," IMES Discussion Paper 2019-E-2, Bank of Japan. Chapter 4 is based on Hara, Naoko, Tatsuyoshi Okimoto, and Ryuzo Miyao (2020): "The Effects of Asset Purchases and Normalization of U.S. Monetary Policy," *Economic Inquiry* 58(3), 1279–1296, © 2020 Western Economic Association International, Wiley. I am very grateful to my co-authors for fruitful discussions and suggestions.

The views expressed in this dissertation are those of mine and do not necessarily reflect those of any institutions with which I am affiliated.

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Chapter 1

Overview of the Dissertation

1.1 Introduction

Central banks conduct monetary policy amid of uncertainty about the state of the economy. Information available in real time is imperfect, and the recent financial crisis forces policymakers to decide policy actions in a highly uncertain economic environment with the presence of the effective lower bound.

Concerning the issue on imperfect information, major macroeconomic data available in real time are subject to revision. Since upcoming data revisions are hardly predictable, economic agents cannot have perfect information on the current and past state of the economy. Accordingly, both policymakers and private agents have to make decisions based on less accurate information. Data uncertainty about the state of the economy may affect decision-making of policymakers by influencing the private sector's optimization. While the problem of data uncertainty is important in the practice of monetary policy, the impacts of data uncertainty on macroeconomic models are far less explored.

The recent financial crisis has heightened uncertainty about future monetary policy in a very low interest rate environment. After the crisis erupted, central banks in advanced economies have adopted unconventional measures of monetary policy to stabilize the financial system and spur economic recovery. Several economies including euro area and Japan have embarked on negative interest rate policy, and major advanced economies have been utilizing large asset purchase programs.

Negative interest rate policy marks a dramatic shift from conventional interest rate policies with positive policy rates. These policies are expected to increase potential margins to the extent interest rates further fall. However, it is unclear whether the move from zero interest rate policy to negative interest rate policy reduces the binding constraint on monetary policy. A vast literature studies when and how well the asset purchase programs by central banks work. Recent studies argue possibility of state-dependence of asset purchase programs, though there is yet no consensus on this issue.

The aim of my dissertation is twofold. First, I intend to uncover how data uncertainty about the current and past state influences business cycles. I shed light on the effects of data uncertainty about labor productivity, which is obviously a key variable in monetary policy decisions. Second, I aim to explore the effectiveness of unconventional monetary policies in a changing environment after the financial crisis. In this dissertation, I focus on two popular measures of unconventional monetary policy, namely, negative interest rate policies and asset purchase programs.

1.2 Literature Review

1.2.1 Data uncertainty as a challenge against central banks

A growing number of studies attempt to uncover roles of noisy signals on current and future productivity in business cycles. Lorenzoni (2009) shows that noise contained in current productivity induces substantial and persistent effects on economic activity in the short run. Using a DSGE model with dispersed information, he describes how public noisy signals on current productivity generate excess optimism and pessimism about the economy. Blanchard et al. (2013) and Forni et al. (2017) propose identification methodologies for noisy signals on future productivity. They show that the noise around future productivity substantially contributes to business cycles.

Imperfect information including data uncertainty plays an important role in macroeconomic dynamics. The literature on imperfect information explores roles of rigidities regarding processing information in agents' expectations and decision-making. Mankiw and Reis (2002) propose a model of sticky information, in which agents infrequently update their information sets even though they continuously receive accurate signals. Woodford (2003) constructs a noisy information model, in which agents only respond to part of available information because they receive noisy signals on the true state of the economy. Rational inattention model proposed by Sims (2003) assumes that agents do not react to part of information due to their limited capacity in processing information. In his model, information friction causes an imperfect information problem.

There are few papers measuring impacts of noisy signals on the past state of the economy and to what extent economic dynamics observed in real time is over- or under-estimated. Among the papers, Amir-Ahmadi et al. (2017) show that effects of monetary policy tend to be underestimated when researchers rest on the preliminary data for macroeconomic indicators. Masolo and Paccagnini (2019) estimate a noise shock using a real-time output growth series to the model by Blanchard and Quah (1989), confirming the findings by Lorenzoni (2009).

There is a large body of research investigating statistical characteristics of revisions to macroeconomic data, which is extensively surveyed by Croushore (2011). Croushore (2011) shows that revision patterns for major macroeconomic data are different between first few revisions and the rest of the revisions. Aruoba (2008) points out that real-time data are noisy and biased estimates of true data, and data revision can be a combination of noise reduction and updating new information. Jacobs and van Norden (2016) confirm validity of the findings of Aruoba (2008) for U.S. productivity data.

A vast majority of the literature focus on relatively short-term revisions made within a couple of years after the first release. The role of late revisions occurring long after the first release is far less explored in the literature. However, late revisions tend to be large because majority of them are annual or comprehensive revisions to the source data. Among few papers on this issue, Siklos (2008) emphasizes usefulness of information contained in comprehensive revisions for inflation forecasts.

1.2.2 Effectiveness of negative interest rate policy

In recent years, several advanced economies including the euro area and Japan have deployed negative interest rate policies. Negative interest rate policy is designed to relaxing the constraint of the effective lower bound on policy rates by allowing the policy rates being in negative territory. Grisse et al. (2017) empirically show that market participants revise down their perception of the effective lower bound in response to the announcement about adoptions of negative interest rate policies. Koeda (2019) examines the effects of a change in the effective lower bound on the Japanese economy using structural vector autoregression models. She provides evidence suggesting that increasing the effective lower bound from negative territory can be expansionary. Dell'Ariccia et al. (2017) summarize details on the economies adopting negative interest rate policies and some early assessments of the successfulness of the policies.

However, it is an open question whether monetary policy becomes less constrained after introducing the negative interest rate policies. While policy rates can go negative, deposit rates and other market rates can be still bounded to zero. Moreover, the effective lower bound on policy rates is unobservable in the economies with negative interest rate policies.

To measure the degree of the effective lower bound constraint on monetary policy, sensitivity of government bond yields to macroeconomic surprises is widely used in the literature. Gürkaynak et al. (2005) show that macroeconomic surprises significantly influence U.S. long-term interest rates. Moessner and Nelson (2008) find an increase in the sensitivity of U.S. interest rate futures to macroeconomic surprises despite of the FOMC's guidance about future monetary policy around the middle of 2000s. Swanson and Williams (2014a) and Swanson and Williams (2014b) find that the influence of surprises in macroeconomic announcements on bond yields varies with the monetary policy regimes. Altavilla et al. (2017) present that macroeconomic surprises can have persistent impacts on bond yields over a few months.

The literature also presents that sensitivity of long-term bond yields to macroeconomic surprises in major advanced economies has fallen after the recent financial crisis.

From the theoretical side, Swanson and Williams (2014a) show that bond yields in an economy facing the zero lower bound are less responsive to macroeconomic news than those in an economy not constrained by the bound. They suggest that the zero lower bound substantially contributed to a decline in the sensitivity of yields for U.S. in the early 2010s. Swanson and Williams (2014b) find the decline in the sensitivity for Germany and UK during the early 2010s. Moessner et al. (2016) report a decrease in the sensitivity of shorter-term interest rates in Sweden. More recent studies focus on effects of forward guidance on the sensitivity of bond yields. Ehrmann et al. (2019) study the effectiveness of forward guidance policies using a panel data containing macroeconomic surprises for several advanced economies. Moessner and Rungcharoenkitkul (2019) show that forward guidance in the U.S. reduced the sensitivity of shorter maturity bonds even after the policy liftoff from the zero lower bound.

1.2.3 Effectiveness of Large-Scale Asset Purchase Program

There is a vast literature on the Large-Scale Asset Purchase Programs (LSAPs) since the recent financial crisis. Various papers propose theoretical frameworks to explain the effectiveness of asset purchases by central banks. They suggest that asset purchases are particularly effective when financial markets are disrupted. Cúrdia and Woodford (2011) construct a New Keynesian model with imperfect financial intermediation and lending to the private sector by the central bank and find that asset purchases targeted at specific types of assets can be stimulative, particularly during a period of financial market turmoil. Gertler and Karadi (2013) extend a New Keynesian framework to introduce a central bank that purchases government bonds and private securities and compare the effectiveness of different QE programs. They find that a purchase of private securities is more stimulative than that of government bonds. Moreover, they find that the LSAP is more effective the longer the time expected at the effective lower bound. Bauer and Rudebusch (2014) find evidence of signaling effects from asset purchases that effectively lower expectations on future short-term interest rates.

In contrast, there seems to be no conclusive empirical evidence of changes in the effectiveness of the LSAPs on the macroeconomy. Many studies including D'Amico and King (2013), Krishnamurthy and Vissing-Jorgensen (2011) and Krishnamurthy and Vissing-Jorgensen (2013) find a decline in the effect of monetary policy on financial markets during the zero lower bound period, although Ihrig et al. (2018) and Swanson (2018) show that the announcement of an LSAP has significant effects on the financial markets. As for the macroeconomic effects, Haldane et al. (2016) find that an increase in asset purchases can be more stimulative when financial markets are disrupted. In a similar vein, Hesse et al. (2018) report that the stimulative effects of the LSAPs have been declining in the recent post-crisis period. They also argue that anticipated asset purchases can have substantial stimulative effects even in the later stages of an LSAP.

Since the LSAPs and the zero interest rate policy were conducted simultaneously, a growing number of studies devote much attention to evaluating the effectiveness of multiple monetary policy measures in a unified way. Against this background, shadow-rate term structure models have been developed to deal with the zero lower bound by a number of studies. Bullard (2012), Krippner (2013), and Wu and Xia (2016) claim that the shadow rate can be used as a single measure of both conventional and unconventional monetary policies. Wu and Xia (2016) find an expansionary monetary policy shock is highly stimulative during the zero lower bound period. Furthermore, Bauer and Rudebusch (2016) suggest that the shadow rate can capture monetary policy expectations.

A growing body of research focuses on the state-dependence of monetary policy effectiveness. Lo and Piger (2005) find that policy shocks are more stimulative in recessions and that the asymmetry may not be caused by either the direction or size of the policy shock. In contrast, Tenreyro and Thwaites (2016) provide empirical evidence that a change in the Federal Funds rate is less effective in recessions, particularly for durable goods consumption and business investment.

1.3 Structure of the Dissertation

The rest of my dissertation is organized as follows.

Chapter 2 uncovers the role of measurement errors in past productivity data in business cycles. To this end, I use a structural VAR with different vintages of productivity data to identify noise components by revision round. The main findings are twofold. First, noise contained in real-time U.S. labor productivity data significantly affects the economy in the short run. Noise has qualitatively the same impacts as demand shocks. This is particularly evident for long-lived noise which even remains in revised data available a few years after the first release. Second, responses of real-time labor productivity data to fundamental shocks on impact are much smaller than those of the final data. Data revisions adjust the underestimation of the immediate responses only gradually. These findings suggest that measurement errors in productivity play an important role in business cycles for a long period of time.

Chapter 3 studies sensitivity of bond yields to macroeconomic news for selected advanced economies adopting negative interest rate policies. Specifically, I estimate the influence of surprises in domestic and U.S. macroeconomic announcements on daily bond yields since the late 1990s for Germany, Japan, Sweden, and Switzerland. I find that for all four countries under study the influence of macroeconomic surprises during the negative interest rate policy period is non-existent or noticeably weaker than during the preceding zero interest rate policy period. The results are consistent with the suggestion that negative interest rate policy is characterized by a lower bound that is no less constraining than the zero lower bound that characterizes zero interest rate policy.

Chapter 4 examines changes in the effectiveness of unconventional monetary policies in the United States since the global financial crisis. To this end, I estimate a Markov-switching VAR model with absorbing regimes to capture possible structural changes. My results detect regime changes around the beginning of 2011 and the middle of 2013. Before 2011, the U.S. large-scale asset purchases had relatively large impacts on the real economy and prices, but became weaker and less persistent after the middle of 2013. In addition, after the middle of 2013, which includes the monetary policy normalization period, the asset purchase shocks had slightly weaker effects than during the early stage of the LSAPs, but stronger effects than during the late stage of the LSAPs. By contrast, interest rate shocks had insignificant effects on the real economy and prices. Finally, the results suggest that the positive responses of durables and capital goods expenditures to interest rate shocks weakened the negative impacts of interest rate hikes after the middle of 2013 including the period of monetary policy normalization.

Chapter 5 concludes my dissertation and suggests interesting avenues for future research.

Chapter 2

Noisy Past and Business Cycles

2.1 Introduction

Major macroeconomic data available in real time are subject to revision. Since upcoming data revisions are hardly predictable, economic agents cannot have perfect information on the current and past state of the economy. Accordingly, the agents have to make decisions based on less accurate information. Imperfect information about the state of the economy should affects the agents' optimization behavior. Recent studies on monetary policy, Aoki (2003) and Lorenzoni (2010) for example, point out that it substantially affects optimal monetary policy. Cimadomo (2016) empirically shows influence of measurement errors in macroeconomic data on estimation of fiscal rules.

Recent studies show that initial announcements of macroeconomic data, often called "real-time" data, are signals on true data with measurement errors. The measurement errors contained in real-time data gradually diminish through data revisions. Data revisions incorporate different information across revision rounds. Early revisions reflect marginal improvements in information, and later revisions contain shifts in economic structures as well as more comprehensive information. Hence, measurement errors adjusted shortly after the first release may have different impacts on business cycles from those remaining in heavily-revised data. Though, this point has been far less explored in the literature on real-time data.

Against the background, this study investigates macroeconomic impacts of measurement errors about the past state of the economy. In this study, I assume that real-time data is a noisy signal on fundamentals. I also consider possibilities that the signal systematically over- or under-estimates true dynamics of fundamentals. The variable of interest in this study is labor productivity, because productivity plays a key role in business cycles and the data have generally experienced sizable revisions over decades.

I use a structural vector autoregression (SVAR) model to jointly model dynamics of preliminary and true productivity data. As for noise, I consider noise components of real-time (initially-released) labor productivity data by revision round. In the model, noise contained in the real-time data consists of two components. These are the noise remaining in revised data, called "long-lived" noise, and the noise disappearing from the revised data, called "short-lived" noise. The long-lived noise contemporaneously affects both of the preliminary productivity data, while the short-lived noise only affects the real-time data on impact. To estimate the two noise components, I construct an SVAR model based on Galí (1999) with two vintages of labor productivity data.[1] By using short-run restrictions on long- and short-lived noise shocks, I uncover how agents respond to different noise shocks, and to what extent their responses observed in real time data are over- or under-estimated compared with true responses. To confirm if the identified noise shocks actually affect expectations, I estimate impacts of the identified noise shocks on the U.S. Survey of Professional Forecasters (SPF).

The main findings are twofold. First, noise contained in real-time U.S. labor productivity data significantly affects underlying fundamentals in the short run. Noise has qualitatively the same impacts as demand shocks. This is particularly evident for long-lived noise which even remains in revised data available a few years after the first release. I also find that the long-lived noise affects current and near-term forecasts on output and unemployment. Second, responses of real-time labor productivity data to fundamental shocks on impact are much smaller than those of the final data. Data revisions gradually adjust the underestimation of the immediate responses over a decade. These findings suggest that noise plays an important role in business cycles for a long

[1] In the literature on real-time data, the term "vintage" is often used to indicate timing at which the data are released. For example, "first vintage" means first release of the data, and "January 2000 vintage" is the latest available data as of January 2000.

period of time.

This study is related to a vast literature regarding noisy signals on current and future productivity. The literature provides evidence suggesting that noise in signals on current or future productivity is important to describe business cycles. As for the signals, Beaudry and Portier (2006) propose a scheme to identify signals to future productivity (news shocks) in vector error correction models. Following Beaudry and Portier (2006), a large number of studies attempt to refine the identification scheme of news shocks.[2] Meanwhile, a growing body of research also sheds light on noise around news shocks. Lorenzoni (2009) shows that noise contained in current productivity induces substantial and persistent effects on economic activity in the short run. Using a DSGE model with dispersed information, he describes how public noisy signals of current productivity generate excess optimism and pessimism about the economy. Blanchard et al. (2013) and Forni et al. (2017) propose identification methodologies for noisy signals on future productivity. They show that the noise around future productivity substantially contributes to business cycles. While the literature assumes perfect information about the past state of the economy, my study considers imperfect information about the past productivity. In the presence of continuous revisions to major official statistics, perfect information on the past seems to be a too strong assumption. My empirical results confirm that imperfect information about the past productivity is important to explain business cycles.

Data uncertainty about the past state is an example of imperfect information. The literature on imperfect information explores roles of rigidities regarding processing information in agents' expectations and decision-making. Mankiw and Reis (2002) propose a model of sticky information, in which agents infrequently update their information sets even though they continuously receive accurate signals. Woodford (2003) constructs a noisy information model, in which agents only respond to part of available information because they receive noisy signals on the true state of the economy.

[2] Kurmann and Mertens (2014) critically argue the relevancy of Beaudry and Portier (2006) in terms of long-run restrictions and common trends in the data.

Rational inattention model proposed by Sims (2003) assumes that agents do not react to part of information due to their limited capacity in processing information. In his model, information friction causes an imperfect information problem. My focus is more firmly related to the literature on noisy information. In my model, agents make decisions based on noisy signals produced by statistics agencies. In this chapter, I provide empirical evidence about the roles of noisy information about the past productivity in business cycles.

There are few papers measuring impacts of noisy signals on the past state of the economy and to what extent economic dynamics observed in real time is mismeasured. Among them, Amir-Ahmadi et al. (2017) show that impacts of monetary policy tend to be underestimated when researchers rest on the preliminary data for macroeconomic indicators. Masolo and Paccagnini (2019) estimate a noise shock using a real-time output growth series to the model by Blanchard and Quah (1989), confirming the findings by Lorenzoni (2009). I extend the scheme of Masolo and Paccagnini (2019) to the model with both revised and real-time data of productivity. Using the model, I analyze noise impacts by revision round. I also examine if the finding of Amir-Ahmadi et al. (2017) hold true for other fundamental shocks and explore what is behind the results.

This study is also related to a large literature on data revisions, which is extensively surveyed by Croushore (2011). Croushore (2011) shows that revision patterns for major macroeconomic data are different between first few revisions and the rest of the revisions. Aruoba (2008) points out that real-time data tend to be noisy and biased estimates of true data, and data revision can be a combination of noise reduction and updating new information. Jacobs and van Norden (2016) confirm validity of the findings of Aruoba (2008) for U.S. productivity data. Considering their findings, my model describes data revision process as a combination of noise reduction and adjustment of over- or under-estimation about dynamics of fundamentals.

A vast majority of the literature focuses on relatively short-term revisions made within a couple of years after the first release. The role of late revisions occurring long after the first release is far less explored in the literature. However, late revisions tend to be large because majority of them are annual or comprehensive revisions to the source data. Among few papers on this issue, Siklos (2008) emphasizes usefulness of information contained in comprehensive revisions for inflation forecasts. My study highlights importance of such late revisions in the real side of the economy.

The remainder of this chapter is organized as follows. Section 2.2 presents properties of revisions to U.S. labor productivity data. Section 2.3 introduces a framework to study economic impacts of noisy signals about the past state of fundamentals. Section 2.4 presents an SVAR model with noise shocks, and describes my identification methodology. Section 2.5 explains the dataset. Section 2.6 presents my results and robustness check. Section 2.7 concludes.

2.2 Properties of Revisions to U.S. Labor Productivity

The productivity measure in this study is quarterly real output per hour of all persons of nonfarm business sector for the United States, a principal U.S. productivity measure among policymakers and economists. It is released by the Bureau of Labor Statistics eight times per year, in second and third months of every quarter. Hours worked of nonfarm business sector hours are released together with the labor productivity data. The real output used to compute productivity is based on the gross domestic product (GDP) of the National Income and Product Accounts (NIPA) constructed by the Bureau of Economic Analysis. The hours worked is based on the Current Employment Statistics (CES) by the Bureau of Labor Statistics. Hence, the labor productivity data are going to be updated when these source data are revised.

Table 2.1 summarizes types of revision to the U.S. labor productivity data. The first and second revisions for a quarter occur one month and three months after the first release, respectively. These revisions include more complete information. Two annual revisions to labor productivity data subsequently take place in every March and August. March releases incorporate annual or comprehensive revisions of the CES, and August releases reflect those of the NIPA. The annual revisions of the source data include comprehensive data, annual surveys for instance, and reflect improvements in methodology. Comprehensive revisions of the NIPA and CES, occurring every five years, include the most comprehensive available data such as census data, and reflect substantial methodological changes including new international statistical standards.

Figure 2.1 illustrates chronological changes in the labor productivity growth rate for 2008Q1. The figure clearly shows that the data revision proceeds is not monotonic. In May 2008, when the data was first released, labor productivity in 2008Q1 grew by 3.2% from a year ago. The figure was revised up to 3.5% by March 2009. However, a comprehensive revision of the NIPA in July 2009 caused a large downward revision, and the resulting figure recorded 2.5% in August 2009. By subsequent annual revisions of the NIPA, the figure was further revised down to 1.9% in August 2011. In August 2013, the productivity growth rate for 2008Q1 was revised down again to 1.4%, largely due to the NIPA comprehensive revision in July 2013. The growth rate has been only slightly revised since then.

To better understand how much revisions improve the data, I compute summary statistics of revisions to labor productivity data by revision round. I consider total revision from the first release to the latest releases, and separate total revision into two rounds referring to the revision schedule of labor productivity statistics. The first round is up to 24 revisions, which occur roughly three years after the first release. It covers early revisions up to first two annual revisions to the labor productivity data. The second round covers revisions from the 25th to the latest releases. Revisions in this round reflect third and further annual revisions, part of which reflects comprehensive revisions of the NIPA and CES. I set the break at 24 revisions, because major studies on real-time data focus on revisions during the first couple of years after the initial release.[3] The sample period is from 1968Q1 to 2008Q1. The starting date is set to be the end of May 1968 vintage of output per hour, which is its first vintage data. My sample ends in 2008Q1 to obtain the preliminary data experiencing sufficient numbers

[3] For example, Aruoba (2008) defines final data as the data 3 years after the first release. Faust et al. (2005) use the term "short-term" revisions for revisions made during two years after the first release.

of annual and further revisions.

Table 2.2 provides summary statistics for revisions to labor productivity growth rates. Both total revision and its components are broadly sizable and very volatile. The mean values range from -0.16 to 0.47 percentage points, and majority of them are statistically significant. Specifically, the mean values of 25th and later revisions are much larger than the earlier revisions. The noise-to-signal ratios, the ratio of the standard deviation of revisions to that of the final data, range from 0.50 to 0.87. This means that variance of revisions is quantitatively much close to the variation of the final data. The last column reports negative correlation between revisions and the first estimates of labor productivity. If revisions convey only new fundamental information, one would observe no correlation between revisions and preliminary estimates. The results provide informal evidence of the presence of noise in preliminary estimates of labor productivity.

To summarize the results, labor productivity data are subject to large revisions over a long time period. Preliminary estimates of labor productivity may contain noise which gradually disappears from the data through a long process of data revision. The revisions to productivity data show different statistical characteristics by revision round, suggesting importance of analyzing noise by revision round.

2.3 A Simple Model with Noisy Signals on the Past

My primary interest is to describe how noise contained in real-time data about the past economy affects business cycles, and to what extent economic dynamics observed in real time is over- or under-estimated. Section 2.2 shows that revisions to labor productivity have different characteristics by revision round. This suggests that roles of noise remaining in heavily-revised data can differ from those of noise disappearing shortly. In this section, I present a framework to study how the two types of noise in signals about past fundamentals influence expectations.

2.3.1 Setup

I rely on three assumptions to identify noise shocks. The first assumption is that economic agents learn the true state of the economy from public noisy signals. The idea comes from dispersed information models. In a dispersed information setup, each agent receives a noisy signal on the current state of the economy, which is slightly different among agents. To better understand the economy, each agent also relies on public noisy signals on the state of the economy. This assumption may be reasonable in practice, considering the fact that financial markets respond to new releases of GDP and other macroeconomic data.

The second assumption is that public signals for a period become available with a certain lag. In reality, major macroeconomic indicators are first released after the end of reference periods. For example, first estimates of labor productivity for a quarter become available a few weeks after the quarter ends. In this study, I assume that public noisy signals on the period t are released at the beginning of the period $t+1$. In learning the state of the economy, agents respond to noise contained in signals because they cannot perfectly disentangle the noise and fundamental fluctuations in the signals.

The third assumption is that public information for a period is subject to revision, and a revision eliminates only part of the noise contained in the information. I call the noise contained both in the revised and initially available information "long-lived noise," and the noise disappearing from the revised information "short-lived noise." This assumption is based on facts shown in Section 2.2, which suggests difference between noise remaining in heavily-revised data and noise disappearing shortly.

The first two assumptions are employed in Masolo and Paccagnini (2019). They identify a single noise shock by introducing a real-time output growth data to the VAR model of Blanchard and Quah (1989). I extend their scheme by introducing the third assumption to disentangle noise shocks using multiple preliminary data.

2.3.2 Influence of noise shocks on expectations

I start with a small model with two noisy signals on aggregate long-run productivity to describe how noise in the signals affects the economy. In the model, agents, indexed by $i \in [0,1]$, cannot observe the state of aggregate economy in real time. Each agent learns the current and past fundamental from signals receiving at every period.

At the beginning of the period t, the agent i receives a signal $a_{i,t}$ which is on the current aggregate long-run productivity a_t:

$$a_{i,t} = a_t + \xi_{i,t} \tag{2.1}$$

$$a_t = a_{t-1} + \varepsilon_t, \tag{2.2}$$

where $\xi_{i,t}$~iid $N(0,\sigma_\xi^2)$ is an idiosyncratic shock, and ε_t~iid $N(0,\sigma_\varepsilon^2)$ is a technology shock. I assume that each agent cannot separately observe a_t and $\xi_{i,t}$, and that the average of the idiosyncratic shock across agents is zero ($\int_0^1 \xi_{i,t} di = 0$).

At the beginning of every period since $t+1$, he/she receives a public noisy signal on the long-run productivity a_t.[4] Let $s_{t+j,t}$ denote a public noisy signal on a_t released at the beginning of the period $t+j$ for $j \geq 1$. I assume that part of noise contained in the first signal, $s_{t+1,t}$, disappears in the second signal, $s_{t+2,t}$. I further assume that the third and later signals on a_t are equal to its true values. The relationship among the signals $s_{t+1,t}$, $s_{t+2,t}$, and $s_{t+3,t}$ is described as:

$$s_{t+1,t} = a_t + \nu_{t+1,t} + \omega_{t+1,t}, \tag{2.3}$$

$$s_{t+2,t} = a_t + \nu_{t+1,t}, \tag{2.4}$$

$$s_{t+3,t} = a_t, \tag{2.5}$$

where $\nu_{t+j,t}$~iid $N(0,\sigma_\nu^2)$ and $\omega_{t+j,t}$~iid $N(0,\sigma_\omega^2)$. $\nu_{t+j,t}$ and $\omega_{t+j,t}$ are supposed to be uncorrelated with a_t. Hereafter I call $\nu_{t+1,t}$ "long-lived" noise, and call $\omega_{t+1,t}$ "short-lived" noise. The long-lived noise exists in both the first and second noisy signals on

[4] This assumption is based on the presence of publication lags of major macroeconomic statistics.

a_t, while the short-lived noise appears only in the first signal. Due to the presence of noise in the signals, the agent i does not have perfect information on a_t until the release of $s_{t+3,t}$. Accordingly, he/she has to learn a_t through these signals to make decisions.

I construct a model to describe how the two noise shocks influence the economy. At the beginning of the period t, the agent i receives an agent-specific signal $a_{i,t}$ defined by Equation (2.1) and the following three signals:

$$s_{t,t-1} = a_{t-1} + \nu_{t,t-1} + \omega_{t,t-1}, \tag{2.6}$$

$$s_{t,t-2} = a_{t-2} + \nu_{t-1,t-2}, \tag{2.7}$$

$$s_{t,t-3} = a_{t-3}. \tag{2.8}$$

Suppose he/she then updates expectations on a latent variable a_t by a Kalman filter:[5]

$$\begin{aligned} E_{i,t}a_t &= \alpha E_{i,t-1}a_{t-1} + \beta_0(a_{i,t} - E_{i,t-1}a_{i,t}) \\ &+ \beta_1(s_{t,t-1} - E_{i,t-1}s_{t,t-1}) \\ &+ \beta_2(s_{t,t-2} - E_{i,t-1}s_{t,t-2}) \\ &+ \beta_3(s_{t,t-3} - E_{i,t-1}s_{t,t-3}). \end{aligned} \tag{2.9}$$

$E_{i,t}a_t$ reflects its lagged value and realized prediction errors regarding the four signals ($a_{i,t}$, $s_{t,t-1}$, $s_{t,t-2}$, and $s_{t,t-3}$).

Noise shocks are contained in the prediction errors regarding public signals. The prediction error about $s_{t,t-1}$ in Equation (2.9) can be rewritten as:

$$\begin{aligned} s_{t,t-1} - E_{i,t-1}s_{t,t-1} &= a_{t-1} + \nu_{t,t-1} + \omega_{t,t-1} - E_{i,t-1}[a_{t-1} + \nu_{t,t-1} + \omega_{t,t-1}] \\ &= a_{t-1} - E_{i,t-1}a_{t-1} + \nu_{t,t-1} + \omega_{t,t-1}. \end{aligned} \tag{2.10}$$

Two noise shocks arriving at the beginning of the period t ($\nu_{t,t-1}$ and $\omega_{t,t-1}$) positively

[5] This formula of expectations follows the literature on imperfect information including Woodford (2003) and Lorenzoni (2009). Woodford (2003) discusses that the Kalman filter can describe an agent's optimal prediction of the current state.

influence the agent's expectations. As for the prediction error term about $s_{t,t-2}$, taking Equations (2.6) and (2.7) and $E_{i,t-1}s_{t,t-j} = s_{t-1,t-j}$ into this term gives:

$$
\begin{aligned}
s_{t,t-2} - E_{i,t-1}s_{t,t-2} &= s_{t,t-2} - s_{t-1,t-2} \\
&= (a_{t-2} + \nu_{t-1,t-2}) - (a_{t-2} + \nu_{t-1,t-2} + \omega_{t-1,t-2}) \\
&= -\omega_{t-1,t-2}. \qquad (2.11)
\end{aligned}
$$

This shows that $\omega_{t-1,t-2}$, which is the short-lived noise contained in $s_{t-1,t-2}$, disappears when $s_{t,t-2}$ is released. Similarly, the prediction error about $s_{t,t-3}$ can be rewritten as:

$$
\begin{aligned}
s_{t,t-3} - E_{i,t-1}s_{t,t-3} &= s_{t,t-3} - s_{t-1,t-3} \\
&= a_{t-3} - (a_{t-3} + \nu_{t-2,t-3}) \\
&= -\nu_{t-2,t-3}. \qquad (2.12)
\end{aligned}
$$

This captures deduction of $\nu_{t-2,t-3}$, the long-lived noise contained in $s_{t-1,t-3}$ (and $s_{t-2,t-3}$), by the release of $s_{t,t-3}$ which is equal to a_{t-3}. Equations (2.11) and (2.12) show that noise reduction has a negative impact on expectations. Taking Equations (2.1), (2.10), (2.11) and (2.12) into Equation (2.9) gives:

$$
\begin{aligned}
E_{i,t}a_t = {} & \alpha E_{i,t-1}a_{t-1} + \beta_0(a_{t-1} - E_{i,t-1}a_{t-1}) + \beta_0\varepsilon_t + \beta_0\xi_{i,t} \\
& +\beta_1(a_{t-1} - E_{i,t-1}a_{t-1}) + \beta_1\nu_{t,t-1} + \beta_1\omega_{t,t-1} \\
& -\beta_2\omega_{t-1,t-2} - \beta_3\nu_{t-2,t-3}.
\end{aligned}
$$

Rearranging this, I obtain:

$$
\begin{aligned}
E_{i,t}a_t = {} & (\alpha - \beta_0 - \beta_1)E_{i,t-1}a_{t-1} + (\beta_0 + \beta_1)a_{t-1} \\
& +\beta_0\varepsilon_t + \beta_1\nu_{t,t-1} + \beta_1\omega_{t,t-1} - \beta_2\omega_{t-1,t-2} - \beta_3\nu_{t-2,t-3} \\
& +\beta_0\xi_{i,t}. \qquad (2.13)
\end{aligned}
$$

Equation (2.13) describes $E_{i,t}a_t$ as a function of its lagged value, lagged long-run productivity, three structural shocks in current and past periods, and a current idiosyncratic shock. Current noise shocks influence the expectations on current aggregate productivity. Their impacts may remain in the next period because the current expectations depend on the past expectations. As noted earlier, noise reduction has a negative impact on the current expectations $(-\beta_2\omega_{t-1,t-2} - \beta_3\nu_{t-2,t-3})$.

Equation (2.13) suggests that $E_{i,t}a_t$ can be written as a function of current and past structural shocks:

$$E_{i,t}a_t = \sum_{j=0}^{t-1} \alpha_j \varepsilon_{t-j} + \sum_{j=0}^{t-1} \gamma_j \nu_{t-j,t-j-1} + \sum_{j=0}^{t-1} \theta_j \omega_{t-j,t-j-1} + \sum_{j=0}^{t-1} \psi_j \xi_{i,t-j}, \tag{2.14}$$

where α_j, γ_j, θ_j, and ψ_j are coefficients capturing responsiveness of expectations to corresponding shocks.

After receiving the signals, the agent i determines current level of labor input based on $E_{i,t}a_t$. Labor input by the agent i, denoted by $h_{i,t}$, can be written as:

$$h_{i,t} = \sum_{j=0}^{t-1} \alpha_j^h \varepsilon_{t-j} + \sum_{j=0}^{t-1} \gamma_j^h \nu_{t-j,t-j-1} + \sum_{j=0}^{t-1} \theta_j^h \omega_{t-j,t-j-1} + \sum_{j=0}^{t-1} \psi_j^h \xi_{i,t-j}, \tag{2.15}$$

where α_j^h, γ_j^h, θ_j^h, and ψ_j^h are coefficients. The agent also chooses current output $q_{i,t}$ based on $E_{i,t}a_t$:

$$q_{i,t} = \sum_{j=0}^{t-1} \alpha_j^* \varepsilon_{t-j} + \sum_{j=0}^{t-1} \gamma_j^* \nu_{t-j,t-j-1} + \sum_{j=0}^{t-1} \theta_j^* \omega_{t-j,t-j-1} + \sum_{j=0}^{t-1} \psi_j^* \xi_{i,t-j}. \tag{2.16}$$

Consequently, noise shocks influence current labor input and output of the agent i by changing expectations on a fundamental variable a_t. Figure 2.2 summarizes the timeline of shock arrivals and the agent i's decisions.

I focus on a symmetric equilibrium where expectations and decisions are symmetric across all agents. From Equation (2.14), aggregated expectations on a_t is given by:

$$E_t a_t = \int_0^1 E_{i,t}a_t di = \sum_{j=0}^{t-1} \alpha_j \varepsilon_{t-j} + \sum_{j=0}^{t-1} \gamma_j \nu_{t-j,t-j-1} + \sum_{j=0}^{t-1} \theta_j \omega_{t-j,t-j-1}. \tag{2.17}$$

To see impacts of noise shocks on the economy, I consider current aggregate labor input h_t and aggregate output q_t, where $h_t = \int_0^1 h_{i,t} di$ and $q_t = \int_0^1 q_{i,t} di$. Since agents make decisions based on their expectations, the resulting h_t and q_t can be expressed as:

$$h_t = \sum_{j=0}^{t-1} \alpha_j^h \varepsilon_{t-j} + \sum_{j=0}^{t-1} \gamma_j^h \nu_{t-j,t-j-1} + \sum_{j=0}^{t-1} \theta_j^h \omega_{t-j,t-j-1}, \tag{2.18}$$

$$q_t = \sum_{j=0}^{t-1} \alpha_j^* \varepsilon_{t-j} + \sum_{j=0}^{t-1} \gamma_j^* \nu_{t-j,t-j-1} + \sum_{j=0}^{t-1} \theta_j^* \omega_{t-j,t-j-1}. \tag{2.19}$$

Accordingly, aggregate labor output and output respond to noise shocks as well as a technology shock.

To sum, noise shocks can influence the economy through expectations because agents face imperfect information on the economy. By contrast, econometricians have perfect information on the economy (a_t in the example presented above) and its noisy signals in real time. Thus, they can distinguish noise shocks from the signals by recursively solving the system of Equations (2.3) to (2.5). Having identified these shocks, they can measure influence of noise shocks on the economy.

2.4 An SVAR Model with Two Noise Shocks

To study how noise shocks influence the economy, I extend a bivariate structural VAR model of Galí (1999) by including two public noisy signals on the economy. While Section 2.3.2 considers that econometricians can access true data for long-run productivity, in practice the data is not observable. Due to data availability, my model considers public signals on aggregate labor productivity instead of those on long-run productivity. In this section, I am going to describe how to estimate impacts of noise shocks on the aggregate economy using my SVAR model.

In my model, agents receive public signals on past aggregate labor productivity at the beginning of every period. Let $s_{t+j,t}$ denote a public noisy signal on aggregate productivity a_t released at the beginning of the period $t+j$. I assume that the first noisy signal $s_{t+1,t}$ contains both short- and long-lived noise components, and that upcoming two revisions to official productivity statistics remove the noise components. Specifically, the short-lived noise $\omega_{t+1,t}$ disappears in the first revision at the period $t+k$, and the long-lived noise $\nu_{t+1,t}$ vanishes in the second (final) revision at the period $t+K$. The relationship between true productivity and two noisy signals is given by:

$$
\begin{aligned}
s_{t+1,t} &= a_t + \nu_{t+1,t} + \omega_{t+1,t}, && (2.20)\\
s_{t+k,t} &= a_t + \nu_{t+1,t}, && (2.21)\\
s_{t+K,t} &= a_t. && (2.22)
\end{aligned}
$$

I introduce a non-technology shock $\eta_t \sim \text{iid } N(0, \sigma_\eta^2)$ as in Galí (1999) to capture fundamental shocks influencing the level of aggregate productivity on impact in the short run. Similar to the long-run productivity, the agent i receives $\eta_{i,t} = \eta_t + \tau_{i,t}$ at the beginning of the period, where $\tau_{i,t} \sim \text{iid } N(0, \sigma_\tau^2)$ is an idiosyncratic shock. I note that the non-technology shock should also have effects on expectations via public signals $s_{t+k,t}$. Accordingly, aggregate labor input can be described as:

$$
h_t = \sum_{j=0}^{t-1} \alpha_j^h \varepsilon_{t-j} + \sum_{j=0}^{t-1} \kappa_j^h \eta_{t-j} + \sum_{j=0}^{t-1} \gamma_j^h \nu_{t-j,t-j-1} + \sum_{j=0}^{t-1} \theta_j^h \omega_{t-j,t-j-1}. \tag{2.23}
$$

Likewise, aggregate output q_t is supposed to be expressed as a function of the structural shocks. The resulting aggregate labor productivity $a_t = q_t - h_t$ is given by:

$$
a_t = \sum_{j=0}^{t-1} \alpha_j^a \varepsilon_{t-j} + \sum_{j=0}^{t-1} \kappa_j^a \eta_{t-j} + \sum_{j=0}^{t-1} \gamma_j^a \nu_{t-j,t-j-1} + \sum_{j=0}^{t-1} \theta_j^a \omega_{t-j,t-j-1}. \tag{2.24}
$$

From Equations (2.20), (2.21) and (2.24), two noisy signals on a_t are expressed as:

$$
\begin{aligned}
s_{t+1,t} &= \sum_{j=0}^{t-1} \alpha_j^a \varepsilon_{t-j} + \sum_{j=0}^{t-1} \kappa_j^a \eta_{t-j} + \sum_{j=0}^{t-1} \gamma_j^a \nu_{t-j,t-j-1} + \sum_{j=0}^{t-1} \theta_j^a \omega_{t-j,t-j-1} + \nu_{t+1,t} + \omega_{t+1,t}, && (2.25)\\
s_{t+k,t} &= \sum_{j=0}^{t-1} \alpha_j^a \varepsilon_{t-j} + \sum_{j=0}^{t-1} \kappa_j^a \eta_{t-j} + \sum_{j=0}^{t-1} \gamma_j^a \nu_{t-j,t-j-1} + \sum_{j=0}^{t-1} \theta_j^a \omega_{t-j,t-j-1} + \nu_{t+1,t}. && (2.26)
\end{aligned}
$$

Equations (2.23), (2.24), (2.25) and (2.26) can be summarized as a structural moving average representation. Its corresponding structural VAR(p) model can be written as:

$$B_0 y_t = B_1 y_{t-1} + B_2 y_{t-2} + \cdots + B_p y_{t-p} + \zeta_t, \tag{2.27}$$

where $y_t = (a_t, h_t, s_{t+k,t}, s_{t+1,t})'$ is a vector of dependent variables in the model, $\zeta_t = (\varepsilon_t, \eta_t, \nu_{t+1,t}, \omega_{t+1,t})'$ is a vector of structural shocks, and B_j is a 4×4 coefficient matrix. The model contains labor productivity and labor input as in the SVAR model constructed by Galí (1999). The reduced-form representation of Equation (2.27) is given by:

$$y_t = A_1 y_{t-1} + A_2 y_{t-2} + \cdots + A_p y_{t-p} + u_t, \tag{2.28}$$

where $A_j = B_0^{-1} B_j$ is a 4×4 coefficient matrix and $u_t = B_0^{-1}$ is a vector of error terms having zero means and a covariance matrix Σ. In estimating Equation (2.28), I use the first difference of logarithm of labor productivity as in Galí (1999), hence $y_t = (\Delta a_t, h_t, \Delta s_{t+k,t}, \Delta s_{t+1,t})'$, where $\Delta a_t = a_t - a_{t-1}$ and $\Delta s_{t+j,t} = s_{t+j,t} - s_{t+j,t-1}$.

I restrict the matrix B_0^{-1} as:

$$B_0^{-1}\zeta_t = \begin{bmatrix} b_{11} & b_{12} & 0 & 0 \\ b_{21} & b_{22} & 0 & 0 \\ b_{31} & b_{32} & b_{33} & 0 \\ b_{41} & b_{42} & b_{43} & b_{44} \end{bmatrix} \begin{bmatrix} \varepsilon_t \\ \eta_t \\ \nu_{t+1,t} \\ \omega_{t+1,t} \end{bmatrix}. \tag{2.29}$$

The second and third assumptions restrict five entries in the matrix to be zero. The second assumption of information arrival lags imposes restrictions on b_{13}, b_{14}, b_{23}, and b_{24} to be zero. Since labor productivity data for the period t becomes first available at $t+1$, agents at the period t do not react to noise contained in the first release of labor productivity for the period t. As a result, the long- and short-lived noise shocks do not affect true fluctuations of productivity and labor input on impact. The third assumption sets b_{34} to be zero. This assumption is necessary to identify the two noise shocks. The short-lived noise shock for the period t does not affect the revised data on impact, since it has disappeared from the data. The long-lived noise shock has immediate impacts on both the real-time and revised data, since the long-lived noise component for the period t still remains in the revised data for the period t.

I note that b_{11}, b_{12}, b_{21}, and b_{22} take nonzero values. Since agents observe these shocks in real time, aggregate fundamental shocks influence the economy on impact. Following the identification method used by Galí (1999), I impose a long-run restriction to identify technology and non-technology shocks. That is, both of the two fundamental shocks influence all of the variables on impact, while only the technology shock influences the level of labor productivity in the long run.

While I have so far assumed invertibility of the structural moving average representation, it should be empirically tested. If the moving average representation is not invertible, the representation is called "non-fundamental." This means that the moving average representation fails to recover true structural shocks. Recent studies propose empirical tests for non-fundamentalness in SVAR models.[6] To confirm whether my model can recover structural shocks, I compute a diagnostic for non-fundamentalness proposed by Beaudry et al. (2019). I will present the results in Section 2.6.

It is noteworthy that data revision is not necessarily equal to noise contained in economic data. It is possible that public noisy signals also systematically over- or underestimate true dynamics of fundamentals. Consider total revision to labor productivity data for the period t denoted by $rev_t \equiv a_t - s_{t+1,t}$. If the first signal $s_{t+1,t}$ systematically underestimates its true value a_t, the signal can be defined as:

$$s_{t+1,t} = \phi a_t + \nu_{t+1,t} + \omega_{t+1,t}, \tag{2.30}$$

where the parameter ϕ reflects the signal's tendency of underestimation and $\phi < 1$. I rewrite a structural moving average representation of $s_{t+1,1}$ as:

$$s_{t+1,t} = \sum_{j=0}^{t-1} \tilde{\alpha}_j^a \varepsilon_{t-j} + \sum_{j=0}^{t-1} \tilde{\kappa}_j^a \eta_{t-j} + \sum_{j=0}^{t-1} \tilde{\gamma}_j^a \nu_{t-j,t-j-1} + \sum_{j=0}^{t-1} \tilde{\theta}_j^a \omega_{t-j,t-j-1} + \nu_{t+1,t} + \omega_{t+1,t}. \tag{2.31}$$

[6] Majority of non-fundamental tests are based on theoretical conditions of invertibility provided by Fernández-Villaverde et al. (2007).

Coefficients in Equation (2.31) are expected to be somewhat smaller from those in Equation (2.24). Subtracting Equation (2.31) from Equation (2.24) gives:

$$\begin{aligned} rev_t &= \sum_{j=0}^{t-1}(\alpha_j^a - \tilde{\alpha}_j^a)\varepsilon_{t-j} + \sum_{j=0}^{t-1}(\kappa_j^a - \tilde{\kappa}_j^a)\eta_{t-j} \\ &+ \sum_{j=0}^{t-1}(\gamma_j^a - \tilde{\gamma}_j^a)\nu_{t-j,t-j-1} + \sum_{j=0}^{t-1}(\theta_j^a - \tilde{\theta}_j^a)\omega_{t-j,t-j-1} \\ &- \nu_{t+1,t} - \omega_{t+1,t}. \end{aligned} \tag{2.32}$$

The total revision rev_t comprises (i) noise reduction $(-\nu_{t+1,t} - \omega_{t+1,t})$ and (ii) adjustment of over- or under-estimation about dynamics of fundamentals reflected in coefficients on the structural shocks.

Having identified fundamental shocks, I compare impulse responses to fundamental and noise shocks, and study how data revisions affect responses to the fundamental shocks.

2.5 Data

I use the ALFRED of the Federal Reserve Bank of St. Louis to construct a real-time dataset for nonfarm business sector output per hour of all persons. The first vintage (the date at which the data are released) of output per hour is May 1968, and the latest vintage in this study is December 2019. The dataset contains the first, the 25th, and the latest releases of the labor productivity for each quarter in the sample period. I choose $k = 25$ in my baseline case, taking account of facts presented in Section 2.2. Hereafter I call the first, 25th, and latest releases "real-time," "heavily-revised," and "final" data, respectively. For labor input, I use hours worked per capita obtained by dividing nonfarm business sector hours worked by population aged 16 and over. The two series are the latest data taken from the FRED of the Federal Reserve Bank of St. Louis.[7] All of the data are quarterly and seasonally adjusted. Labor productivity data

[7] Population data are generally not subject to revision. I do not use preliminary data for hours worked due to insufficient availability of the data. The real-time data for hours worked are only available

are expressed in log difference and multiplied by 100. The hours worked per capita series is in log level and detrended by the Hamilton (2018) filter.[8] The sample period is from 1968Q1 to 2008Q1. The starting date is the date when the first vintage (the date at which the data are released) of labor productivity data ends. My sample ends in 2008Q1 to have a sufficiently long period after the end of the sample for data revisions.

2.6 Empirical Evidence

2.6.1 Estimation results of baseline VAR model

First, I present estimation results of the baseline model.[9] I select the lag length $p = 4$ as in Galí (1999). Figure 2.3 plots the estimated responses of the final labor productivity data, hours worked, and output to the fundamental and noise shocks. The variables in the figure is expressed in log levels. Solid lines in the figure give the point estimate response with its 90 percent confidence bands as dotted lines and its 68 percent confidence bands as dashed lines. The confidence bands are made by wild bootstrap using 10,000 bootstrap replications.

The panels in the top two rows in Figure 2.3 show responses to technology shock and non-technology shock. The results follow those presented in Galí (1999). The technology shock persistently increases productivity and output, and decreases hours worked in the short run. The non-technology shock increase both productivity, hours worked, and output in the short run. The bottom two panels present the responses to the two noise shocks. While I do not impose any sign restrictions on the model, the long-lived noise shock increases both productivity, hours worked, and output. The responses to the long-lived noise shock are qualitatively the same as those to a positive non-technology shock. This result is in line with the finding by Lorenzoni (2009)

from the vintage of August 1999. Although the data for hours worked are subject to revision, the revisions are much smaller than those to productivity.

[8] I compute cyclical component based on 2-year-ahead forecast error of a random walk model.

[9] The estimated model includes constant terms so that biases in data revisions are controlled in estimation.

which documents an expansionary effect of over-estimation about the current productivity. The short-lived noise shock causes similar responses of the endogenous variables, though the responses are not statistically significant.

To check if my model sufficiently recovers structural shocks, I use a diagnostic for non-fundamentalness proposed by Beaudry et al. (2019). The diagnostic is R^2 from a regression of an identified shock on lagged factors representing macroeconomic fluctuations. Beaudry et al. (2019) report that non-fundamentalness does not matter in the model if R^2 is lower than 0.25. I compute the diagnostic for each of four identified shocks. To this end, I estimate three principal components using 125 quarterly series of U.S. macroeconomic indicators in the FRED-QD dataset provided by the Federal Reserve Bank of St. Louis.[10,11] Then, I regress each shock on one and four lags of up to three principal components over the period from 1968Q1 to 2008Q1. Table 2.3 displays the diagnostics for non-fundamentalness. All of the reported values of R^2 are below the threshold of 0.25, suggesting that my model sufficiently recovers structural shocks.

I also assess whether the identified noise shocks satisfy the assumptions of normality and no serial-autocorrelation. Applying the Jarque-Bera test and Ljung-Box Q-test to the shocks, I confirm that these assumptions are satisfied.[12]

I turn to comparison of shock responses of real-time, heavily-revised, and final labor productivity data. Figure 2.4 plot responses of the final, heavily-revised, and real-time data for labor productivity growth to technology and non-technology shocks. While the real-time and heavily-revised data show quantitatively the same responses, their responses are smaller than the final data. The heavily-revised data are more responsive than the real-time data, though they are still less responsive than the final data. It implies that data revisions adjust the underestimation about responsiveness of

[10] See McCracken and Ng (2020) for details on the FRED-QD. The series in my dataset are used in Stock and Watson (2012), and the data vintage is December 2019. Current and past vintages of the FRED-QD can be downloaded at: https://research.stlouisfed.org/econ/mccracken/fred-databases/

[11] The number of factors is selected by the criteria proposed by Bai and Ng (2002). The factors in first difference are used in regressions, as in Beaudry et al. (2019).

[12] I include up to eight lags in computing the Ljung-Box Q-test statistics.

productivity only gradually. This result echoes findings by Amir-Ahmadi et al. (2017) that differences between responses of real-time and final data are persistent over time. My result suggests that differences between responses of real-time and final data are also persistent over different data releases.

To investigate the gradual adjustment by data revisions, I measure changes in responses of real-time and final data to fundamental shocks over different data releases. Specifically, I change the value of k in $\Delta s_{t+k,t}$ from 1 to 80, and estimate Equation (2.28) for each value of k. I note that the data $s_{t+80,t}$ becomes available roughly 10 years after the first release. Then, I compute impulse responses of $s_{t+k,t}$ in levels to technology and non-technology shocks at horizon h. I call $r(k, h)$ the response of $s_{t+k,t}$ to a technology shock at horizon h, and $s(k, h)$ the response to a non-technology shock at horizon h. $r(h)$ and $s(h)$ are the response of the final productivity data to technology and non-technology shocks at horizon h, respectively.

First, I focus on the difference between immediate responses of each release of productivity and those of the final data ($r(k, 1) - r(1)$ and $s(k, 1) - s(1)$). The difference takes a negative value if revised labor productivity data responds less to a shock than the final data. Figure 2.5 presents the point estimates (bold) with 68 (dashed) and 95 (dotted) percentile confidence bands for the difference. I consider the cases of a positive technology shock and a non-technology shock. The size of each shock is its one standard deviation. Both panels show negative differences between the responses of preliminary and final data. The difference is largest when the first signal ($k = 1$) is used. It gradually diminishes as data revision proceeds (k increases), and almost vanishes after 80 releases. This means that the adjustment of the underestimation takes about 10 years (80 releases).

To check whether the finding holds the same for longer horizons, I compute the difference between responses of each release of productivity and those of the final data at $h = 4$, that is, $r(k, 4) - r(4)$ and $s(k, 4) - s(4)$. Figure 2.6 plots the results. Both panels show that the point estimates of the responses for the preliminary data are smaller than those for the final data. However, the difference between their responses is much smaller than the case of $h = 1$. The 68% confidence bands cover zero, suggesting the underestimation of impulse responses may be far less severe for longer horizons.

To summarize, the results show that impulse responses of real-time data to fundamental shocks are smaller than those of final data. The underestimation about the responses on impact is substantial, and data revisions adjust it only gradually. The results shown in Figure 2.5 are a complement to the finding by Amir-Ahmadi et al. (2017) for a monetary policy shock. They report that difference between responses of real-time and final data is persistent over time. My results provide evidence that the difference in their immediate responses is persistent among different vintages.

I turn to impacts of noise on variance of the final data. Table 2.4 reports forecast error variance decompositions computed from the estimated baseline model. The first two rows show variance decompositions about the final labor productivity and hours worked data. The noise shocks explain 6.3% of the variance of labor productivity, and 22.7% of the variance of hours worked. The long-lived noise shock shows larger contributions than the short-lived noise shock. It explains 4.6% of the variance of labor productivity and 18.8% of the variance of hours worked. The next two rows show the results about heavily-revised and real-time data for labor productivity growth. The noise shocks explain roughly half of the variances of both the heavily-revised and real-time data.

The last three rows in Table 2.4 present variance decompositions of the revisions to productivity data. The noise shocks explain roughly half of the variance of total revisions ($\Delta a_t - \Delta s_{t+1,t}$). This indicates that noise reduction and updating information on fundamentals are equally important in describing the roles of total revisions to productivity data. Looking at components of total revisions, more than 70% of variance of earlier revisions ($\Delta s_{t+25,t} - \Delta s_{t+1,t}$) and that of later revisions ($\Delta a_t - \Delta s_{t+25,t}$) are attributed to the noise shocks. The technology shock explains larger (smaller) portion of later (earlier) revisions than the non-technology shock. This implies that later revisions reflect richer information on long-term productivity than earlier revisions.

To sum up, a positive noise shock has an expansionary effect, and the long-lived noise which remains in heavily-revised data has a larger impact than the short-lived noise disappearing during earlier revisions. Responses of productivity to fundamental shocks are underestimated in real time, and data revisions adjust the underestimation only gradually. Noise reduction and updating information on fundamentals are equally important in understanding the roles of total revisions to productivity data.

2.6.2 Impacts of noise shocks on professional forecasts

I turn to measuring impacts of the noise shocks on expectations to confirm whether the findings of the SVAR analyses are supported by actual data for expectations. To this end, I estimate the following equation:

$$z_{t+h|t} = \rho_0 + \rho_1 z_{t+h|t-1} + \phi_1 \hat{\varepsilon}_t + \phi_2 \hat{\eta}_t + \phi_3 \hat{\varepsilon}_{t-1} + \phi_4 \hat{\eta}_{t-1} + \phi_5 \hat{\nu}_{t,t-1} + \phi_6 \hat{\omega}_{t,t-1} + e_t, \quad (2.33)$$

where $z_{t+h|t}$ is expectations on the variable z made at the period t for horizon h. $\hat{\varepsilon}_t$ and $\hat{\eta}_t$ are identified technology and non-technology shocks at the period t, respectively. $\hat{\nu}_{t,t-1}$ and $\hat{\omega}_{t,t-1}$ are long-lived and short-lived noise shocks about the productivity for the period $t-1$ that forecasters receive at the period t. Agents update their expectations for the period $t+h$ in response to structural shocks arriving at the period t. If agents precisely filter out noise, they do not respond to noise shocks, thus $\phi_5 = 0$ and $\phi_6 = 0$.

I use the Survey of Professional Forecasters (SPF) as a proxy for agents' expectations. The SPF is a quarterly macroeconomic survey currently conducted by the Federal Reserve Bank of Philadelphia.[13, 14] The forecasts considered in this analysis are median forecasts for real GDP, private consumption, business fixed investment, indus-

[13] The respondents of the SPF, mainly forecasters in private firms, submit their forecasts by every Thursday of the second week of February, May, August, and November, which are generally just after first estimates of labor productivity are released. The SPF had been conducted by the NBER/ASA prior to 1990Q2. Stark (2010) reports that their schedule was basically the same as the current SPF schedule. The SPF collected real GNP instead of real GDP prior to February 1992.

[14] I use $\hat{\varepsilon}_t$ and $\hat{\eta}_t$ as regressors in Equation (2.33), considering that the SPF respondents may have had some information on the economy for the first month of quarter t before they post their forecasts.

trial production, and unemployment rate. All of the forecasts except for those for unemployment rate are annualized percent change from a quarter ago. The forecast horizons are from current to three quarters ahead.[15] Due to data availability, the starting period is 1981Q4 for real private consumption and business fixed investment, 1975Q1 for the rest of the variables. The sample ends in 2008Q1 for all of the regressions.

I focus on signs of ϕ_5 and ϕ_6, that is, directions of noise-induced forecast revisions. If positive noise shocks induce upward forecast revisions to GDP growth and downward forecast revisions to unemployment rate, the noise shocks play a role like non-technology shocks. Conversely, if forecasts of these three variables are revised upward in response to positive noise shocks, the shocks have qualitatively the same impacts as technology shocks.

Table 2.5 summarizes the regression results. A positive long-lived noise shock has qualitatively the same short-run effects as a positive non-technology shock. It significantly increases near-term growth forecasts for real GDP, business fixed investment, and industrial production and decreases forecasts for the unemployment rate. The impacts of a long-lived noise shock on forecasts for unemployment are statistically significant for all the horizons considered. A positive short-lived noise shock has similar impacts to a positive long-lived noise shock, though the magnitude is smaller and insignificant in majority of the results. The results confirm the finding from the SVAR analyses, that is, the noise contained in preliminary productivity data has qualitatively the same impact as a non-technology shock.

2.6.3 Robustness check

In this subsection, I examine whether my findings are robust to several alternative specifications. The specifications considered here are (i) using an alternative lag length, (ii) using an alternative definition of heavily-revised data, and (iii) using first difference

[15] My dataset does not contain forecasts of productivity measures due to lack of data availability.

of hours worked data in log level.[16]

First, I change the lag length from four to two. The specification of VAR(2) is selected by the Bayesian information criterion. Figure 2.7 presents responses of the final labor productivity data and hours worked to two noise shocks using the VAR(2) model. Although the estimated responses are smaller and peaked a bit earlier possibly due to the shorter lag length, they are qualitatively the same to those obtained from the baseline specification.

Second, I change the definition of heavily-revised data from 25th release to ninth release, which becomes available roughly one year after the first release. In this case, revisions from initial to heavily-revised data for productivity only reflect early revisions to productivity data occurring before its first annual revision. Figure 2.8 presents the estimated responses. Although the responses are qualitatively the same as the baseline results, those to the short-lived noise shock become much smaller. This suggests that noise disappearing within one year after the first release does not have significant impacts on business cycles.

Third, I convert the hours worked series to first difference of log levels, which is employed by Galí (1999). Figure 2.9 presents the estimated responses. Again, the responses are qualitatively the same as the baseline results. The responses of hours worked and output are more persistent than the baseline.

2.7 Conclusion

This study investigates the role of measurement errors contained in the data about past productivity in business cycles. I find that noise contained in real-time labor productivity data significantly affects underlying fundamentals and expectations in the short run. Responses to noise shocks are qualitatively the same as those to a non-technology shock. This is particularly evident for the long-lived noise remaining even in heavily-

[16] In the Appendix, I show results using an alternative filtering method to hours worked, and those using an alternative measure of labor productivity. The results confirm that my findings are robust to the use of these alternative measures.

revised labor productivity data. I also find that responses of real-time labor productivity data to fundamental shocks on impact are much smaller than those of the final data. Data revisions adjust the underestimation only gradually.

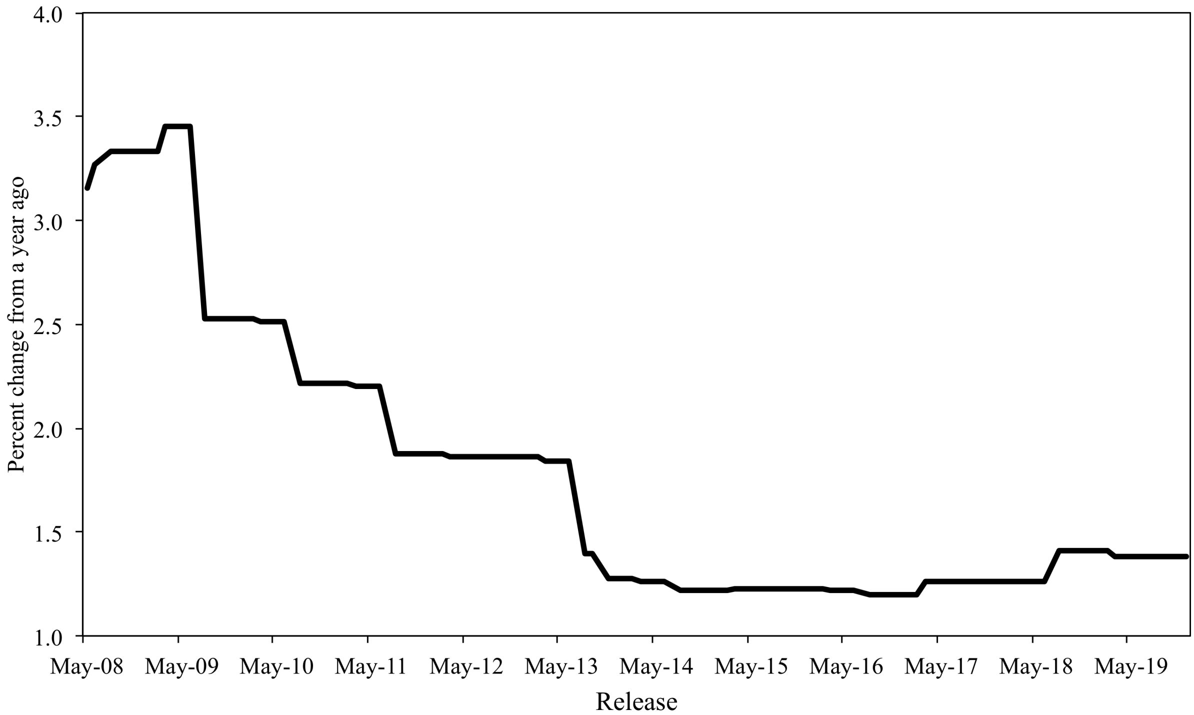
My model shows that agents can recognize a positive noise in a public signal as a positive surprise, and that long-lived noise can have more persistent impacts than short-lived noise. This may explain why a positive long-lived noise shock particularly has an expansionary effect by improving expectations on the economy.

The presence of data revisions also indicates possibility of an imperfect information problem of statistics agencies. If statistics agencies construct public data by filtering noisy signals on economic variables, the data should contain noise and underestimate true economic dynamics. If this is indeed the case, imperfect information of statistics agencies contributes to imperfect knowledge of agents on the past economy. According to my results, public signals respond less to fundamental shocks compared with true data. This finding suggests noise-filtering behavior by statistics agencies.

I note that the literature on noisy signals on future productivity assumes perfect information about the current and past state of the economy. However, in reality, accurate information on the state of the economy becomes available with a very long delay, and only noisy signals on the past state are observable in real time. In this environment, a noisy signal on future productivity is not revised to its true value even when the corresponding period comes. Thus, noise in signals on future productivity is likely to affect the economy longer than considered in the current literature.

The presence of long-lived noise and underestimation of economic dynamics has potentially important implications for policymakers. A large literature on monetary policy has emphasized that noise contained in public information substantially affects optimal monetary policy. In addition, measurement errors in labor productivity can amplify uncertainty about measured natural rate of interest, which is a key variable for decision-making of policymakers. My findings suggest that noise and underestimation of economic dynamics in real-time data can have sustained effects on monetary policy conduct, because they remain in the statistics data for a long period of time.

Figure 2.1: Growth rate of real output per hour for 2008Q1



Note. This figure plots the growth rate of U.S. nonfarm business sector output per hour of all persons for 2008Q1. The unit is percent change from four quarters ago.

Figure 2.2: Timeline

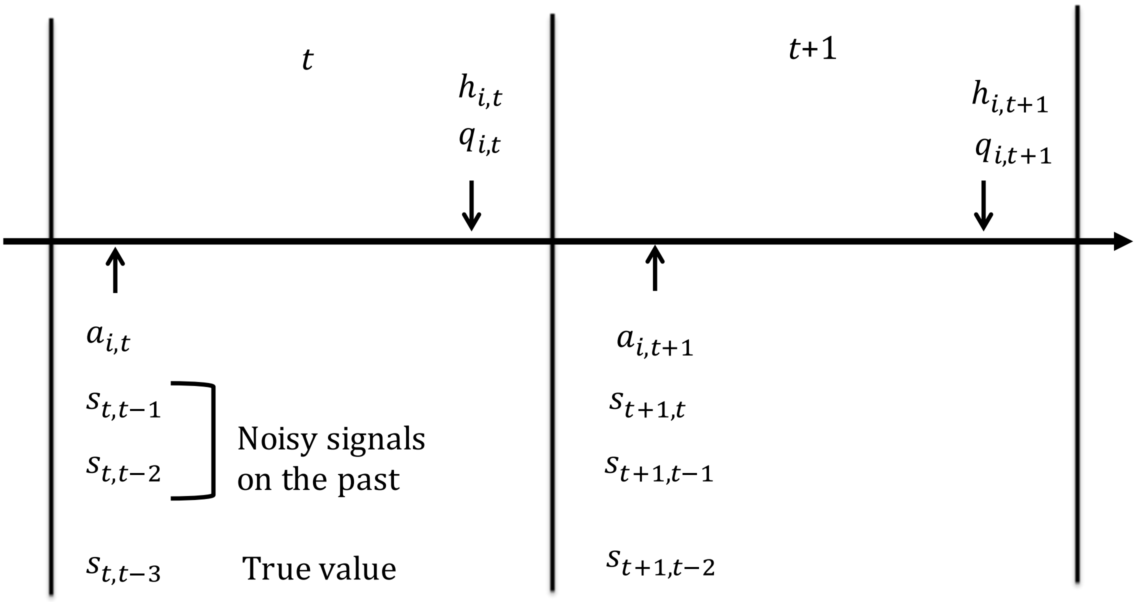
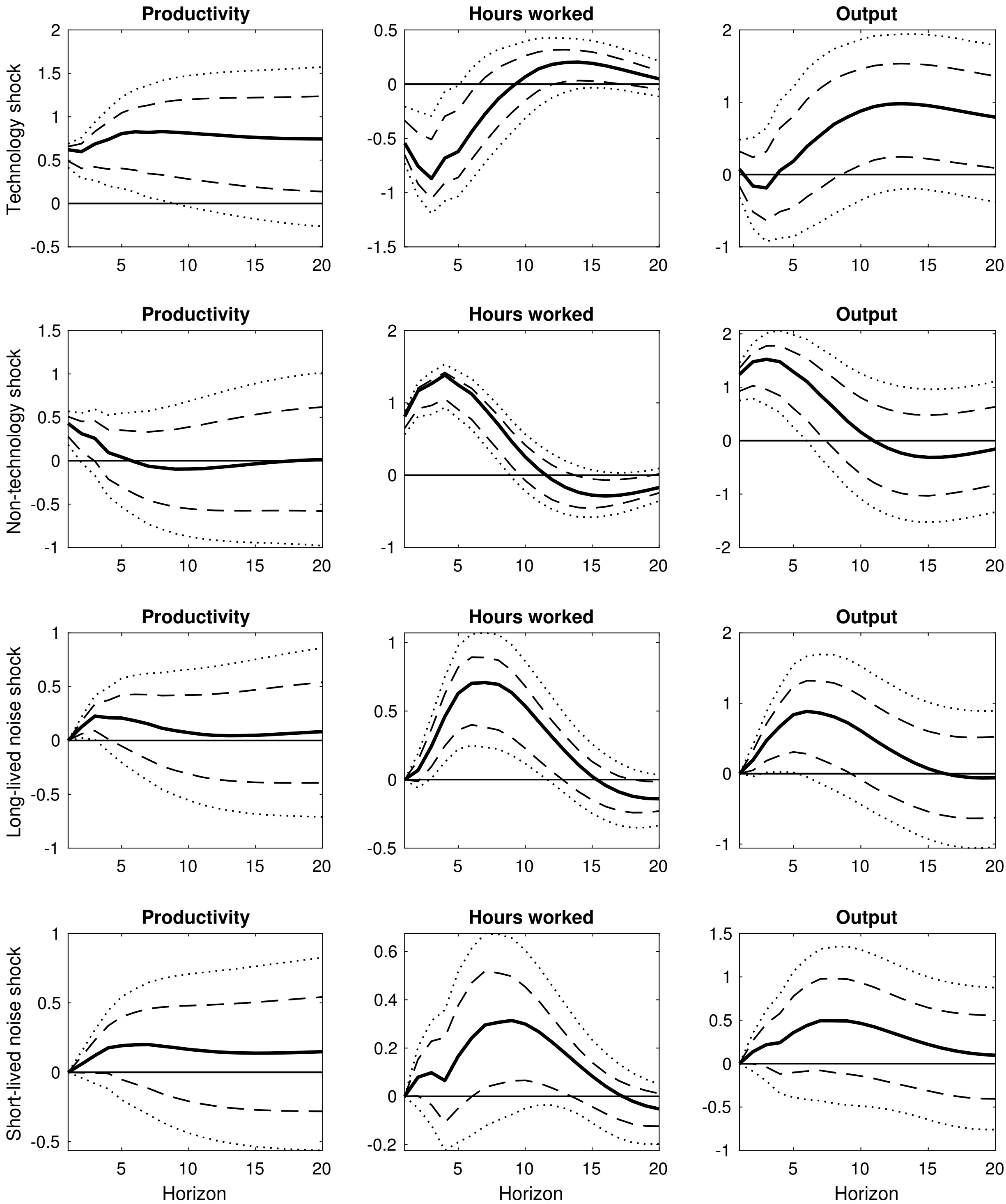
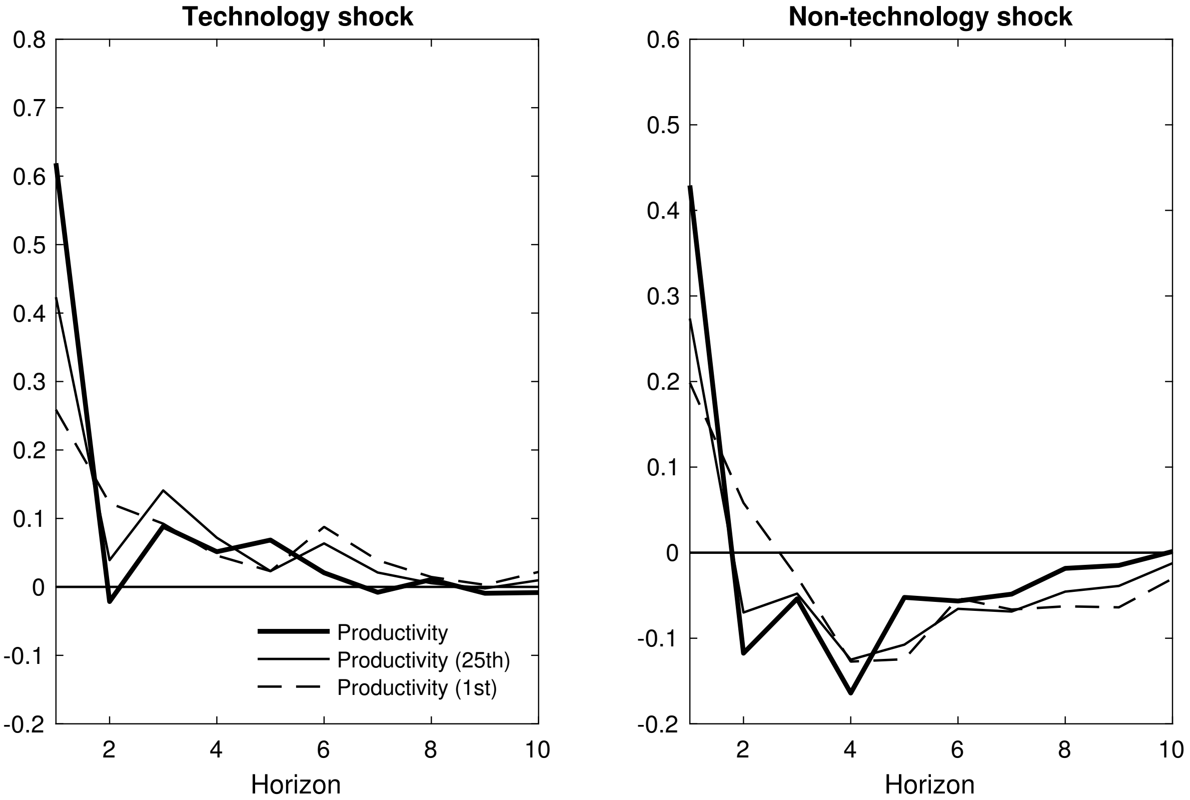


Figure 2.3: Impulse response functions of the baseline VAR(4) model



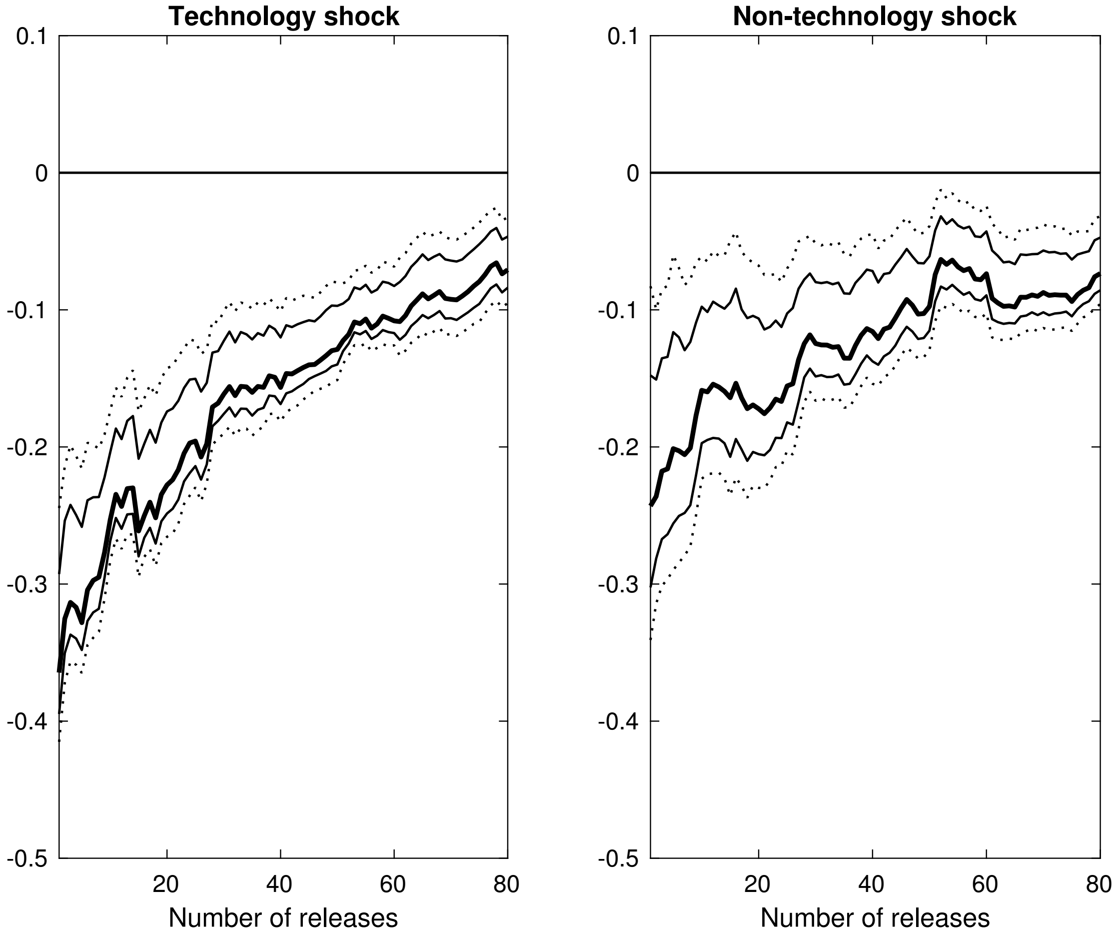
Note. Responses of the final, heavily-revised and real-time labor productivity data and hours worked data to four structural shocks. All of the responses displayed are to one standard deviation shocks. The 25th release is used in the figure as heavily-revised data. All of the variables in this figure are in log levels. The hours worked is in per capita and detrended by the Hamilton filter. The solid lines give the point estimate response with its 68 (90) percentile confidence bands as dashed (dotted) lines.

Figure 2.4: Impulse responses of three productivity data from the baseline estimation



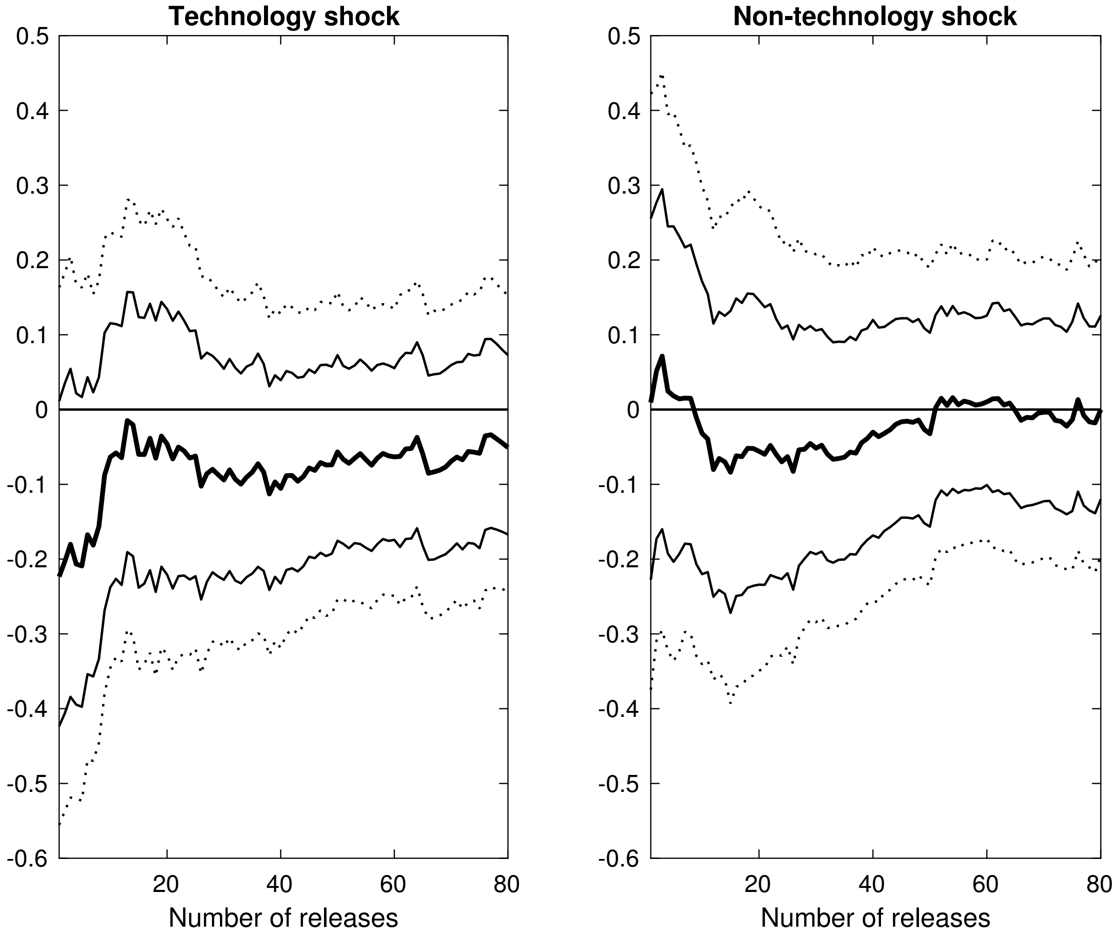
Note. Responses of the final (bold solid), heavily-revised (solid), and real-time data (dashed) of labor productivity to technology and non-technology shocks. All of the responses displayed are to one standard deviation shocks. The 25th release is used in the figure as heavily-revised data. All of the variables in this figure are in log differences (%).

Figure 2.5: Immediate responses of labor productivity by release



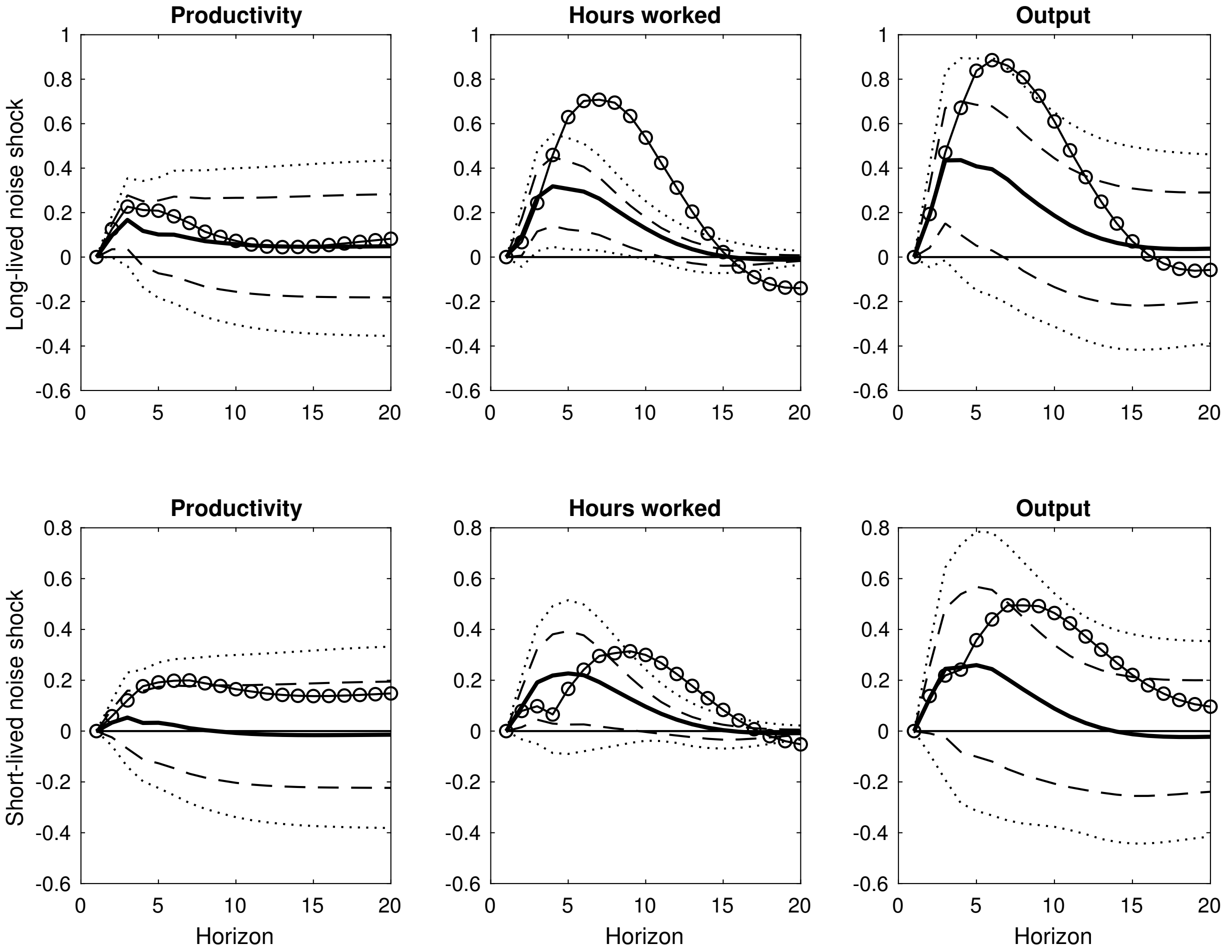
Note. Responses of the level of labor productivity to technology and non-technology shocks on impact ($h = 1$) using the baseline VAR(4) model. All of the responses displayed are to one standard deviation shocks. A solid bold line in each panel gives the point estimate response of real-time data with its 68 percentile confidence bands as dashed lines. A solid line with circle markers gives the point estimate response of final data with its 68 percentile confidence bands as dotted lines.

Figure 2.6: Responses of labor productivity at the fourth quarter by release



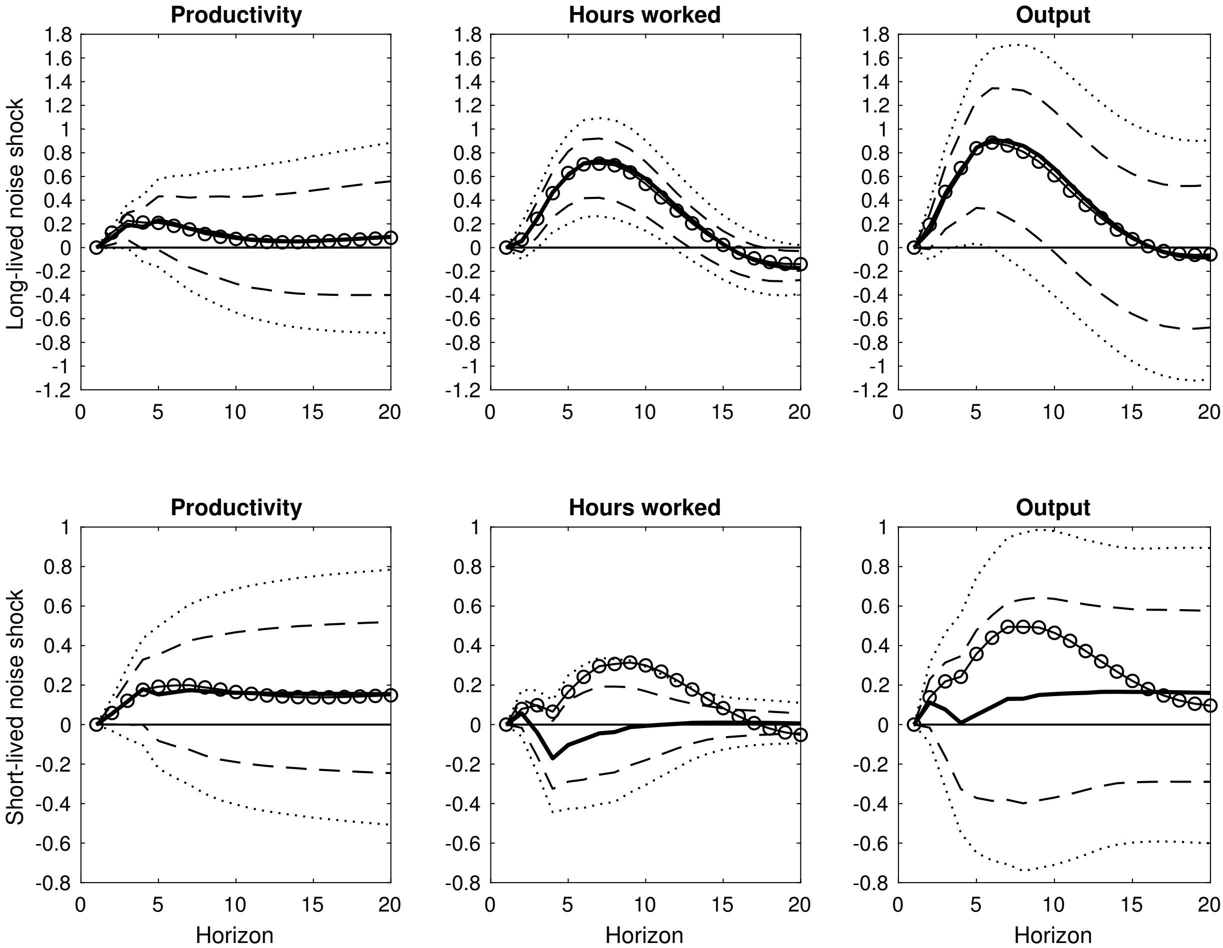
Note. Responses of the level of labor productivity to technology and non-technology shocks at $h = 4$ using the baseline VAR(4) model. All of the responses displayed are to one standard deviation shocks. A solid bold line in each panel gives the point estimate response of real-time data with its 68 percentile confidence bands as dashed lines. A solid line with circle markers gives the point estimate response of final data with its 68 percentile confidence bands as dotted lines.

Figure 2.7: Impulse response functions of VAR(2) model



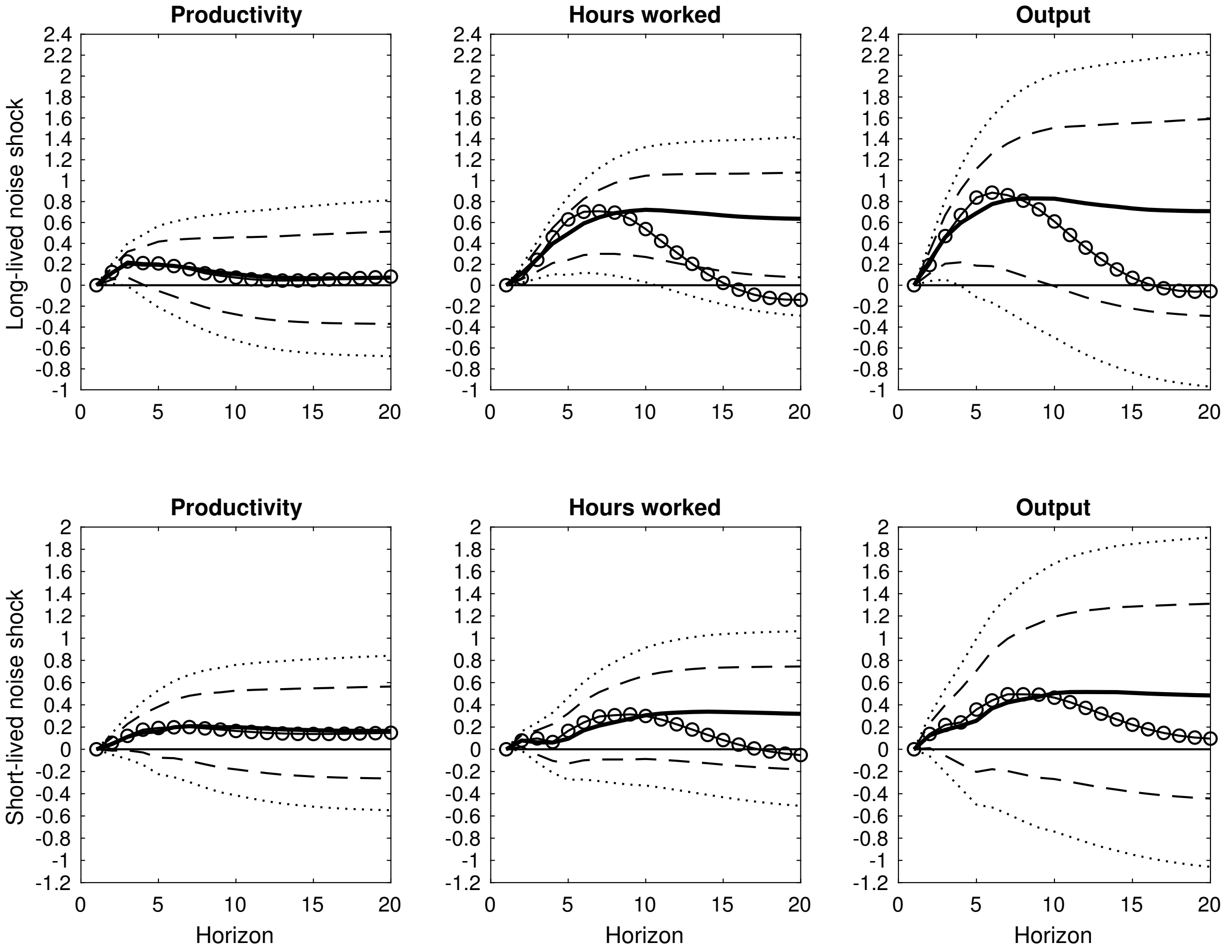
Note. Responses of the final, heavily-revised and real-time labor productivity data and hours worked data to two noise shocks. All of the responses displayed are to one standard deviation shocks. The 25th release is used in the figure as heavily-revised data. All of the variables in this figure are in log levels. The hours worked is in per capita and detrended by the Hamilton filter. A solid line in each panel gives the point estimate response with its 68 (90) percentile confidence bands as dashed (dotted) lines. A solid line with circle markers gives the point estimate response of the baseline VAR(4) model presented in Figure 2.3.

Figure 2.8: Impulse response functions of VAR(4) using 9th release as heavily-revised data



Note. Responses of the final, heavily-revised and real-time labor productivity data and hours worked data to two noise shocks. All of the responses displayed are to one standard deviation shocks. The 9th release is used in the figure as heavily-revised data. All of the variables in this figure are in log levels. The hours worked is in per capita and detrended by the Hamilton filter. A solid line in each panel gives the point estimate response with its 68 (90) percentile confidence bands as dashed (dotted) lines. A solid line with circle markers gives the point estimate response of the baseline VAR(4) model presented in Figure 2.3.

Figure 2.9: Impulse response functions of VAR(4) using log first difference of hours worked data



Note. Responses of the final, heavily-revised and real-time labor productivity data and hours worked data to two noise shocks. All of the responses displayed are to one standard deviation shocks. The 9th release is used in the figure as heavily-revised data. All of the variables in this figure are in log levels. The hours worked is in per capita and converted to first difference of log levels. A solid line in each panel gives the point estimate response with its 68 (90) percentile confidence bands as dashed (dotted) lines. A solid line with circle markers gives the point estimate response of the baseline VAR(4) model presented in Figure 2.3.

Table 2.1: Schedule and main reasons for revisions to labor productivity data

Revision	Timing of revision	Reason of revision
First	1 month after the first release	More complete information
Second	3 month after the first release	More complete information
Annual	Every August	Annual/comprehensive revision of NIPA
	Every March	Annual/comprehensive revision of CES

Table 2.2: Summary statistics of revisions to U.S. labor productivity

Revision round	Mean	Mean absolute revision	Noise-to-signal ratio	Corr. with first release
	Percent change from four quarters ago			
Total revision	0.280***	0.945	0.714	-0.446
Up to 24th	-0.158*	0.836	0.634	-0.336
25th and later	0.438***	0.710	0.503	-0.207
	Percent change from a quarter ago (annualized)			
Total revision	0.398*	2.135	0.867	-0.279
Up to 24th	-0.072	1.664	0.669	-0.210
25th and later	0.470***	1.786	0.717	-0.141

Note. The measure of labor productivity used in the table is nonfarm business sector output per hour of all persons. The data used to compute percent changes from a quarter ago are seasonally adjusted. Entries marked with superscripts * and *** are significantly different from zero at the 10% and 1% levels respectively, using HAC standard errors. The sample period is from 1968Q1 to 2008Q1, and its corresponding vintages are from May 1968 to May 2008.

Table 2.3: Beaudry et al. (2019) diagnostics for non-fundamentalness

	Tech. shock			Non-tech. shock			Long-lived noise			Short-lived noise		
r	1	2	3	1	2	3	1	2	3	1	2	3
1 lag	0.00	0.03	0.04	0.01	0.01	0.02	0.06	0.07	0.07	0.01	0.01	0.02
4 lags	0.01	0.09	0.10	0.02	0.07	0.16	0.06	0.07	0.10	0.02	0.03	0.06

Note. This table reports R^2 from regressions of each of four identified structural shocks on lagged principal components extracted from the FRED-QD of the December 2019 vintage. r denotes the number of factors used in the regression. "Tech." is an abbreviation for "technology". The sample period is from 1968Q1 to 2008Q1.

Table 2.4: Forecast error variance decompositions

Variable	Fundamental shock		Noise shock	
	Technology	Non-technology	Long-lived	Short-lived
Productivity				
Final	58.8	35.0	4.6	1.7
Heavily-revised	35.5	20.7	41.2	2.6
Real-time	23.5	21.3	14.8	40.4
Hours worked	17.3	60.0	18.8	3.9
Revision				
Total revision	31.7	19.7	14.4	34.2
25th and later	14.4	9.6	72.9	3.0
Up to 24th	12.4	8.1	23.9	55.6

Note. Forecast error variance decompositions at infinite horizon. "Heavily-revised" denotes variance decompositions for labor productivity data experiencing the first 24 revisions. "Real-time" denotes variance decompositions for the first release of labor productivity.

Table 2.5: Regression of forecasts on identified noise shocks

	Forecast horizon			
	$h = 0$	$h = 1$	$h = 2$	$h = 3$
Real GDP				
Long-lived noise	0.350***	0.119	0.088	-0.019
	(0.109)	(0.078)	(0.075)	(0.064)
Short-lived noise	0.280***	0.048	0.044	-0.018
	(0.098)	(0.101)	(0.063)	(0.056)
Real private consumption				
Long-lived noise	0.099	0.029	0.070	0.026
	(0.114)	(0.063)	(0.044)	(0.036)
Short-lived noise	-0.012	0.051	0.038	-0.008
	(0.111)	(0.063)	(0.042)	(0.036)
Real business fixed investment				
Long-lived noise	0.724**	0.705***	0.338**	-0.269
	(0.281)	(0.203)	(0.160)	(0.212)
Short-lived noise	0.205	0.419*	0.389**	0.245
	(0.297)	(0.227)	(0.161)	(0.184)
Industrial production				
Long-lived noise	0.709***	0.087	0.117	-0.096
	(0.254)	(0.172)	(0.151)	(0.139)
Short-lived noise	0.629**	0.107	-0.068	-0.216
	(0.292)	(0.227)	(0.137)	(0.148)
Unemployment rate				
Long-lived noise	-0.090***	-0.106***	-0.100***	-0.109***
	(0.023)	(0.024)	(0.025)	(0.026)
Short-lived noise	-0.009	-0.033	-0.038	-0.036
	(0.022)	(0.025)	(0.025)	(0.025)

Note. The variables used in regressions (except for unemployment rate) are annualized quarterly growth rates. The series of business fixed investment used in regressions is non-residential. Entries marked with superscripts *, **, and ***, are significantly different from zero at the 10%, 5%, and 1% levels respectively, using HAC standard errors. The starting period is 1981Q4 for real private consumption and business fixed investment, and 1975Q1 for the other variables. The sample ends in 2008Q1 for all of the regressions.

Appendix: Extra results using alternative measures

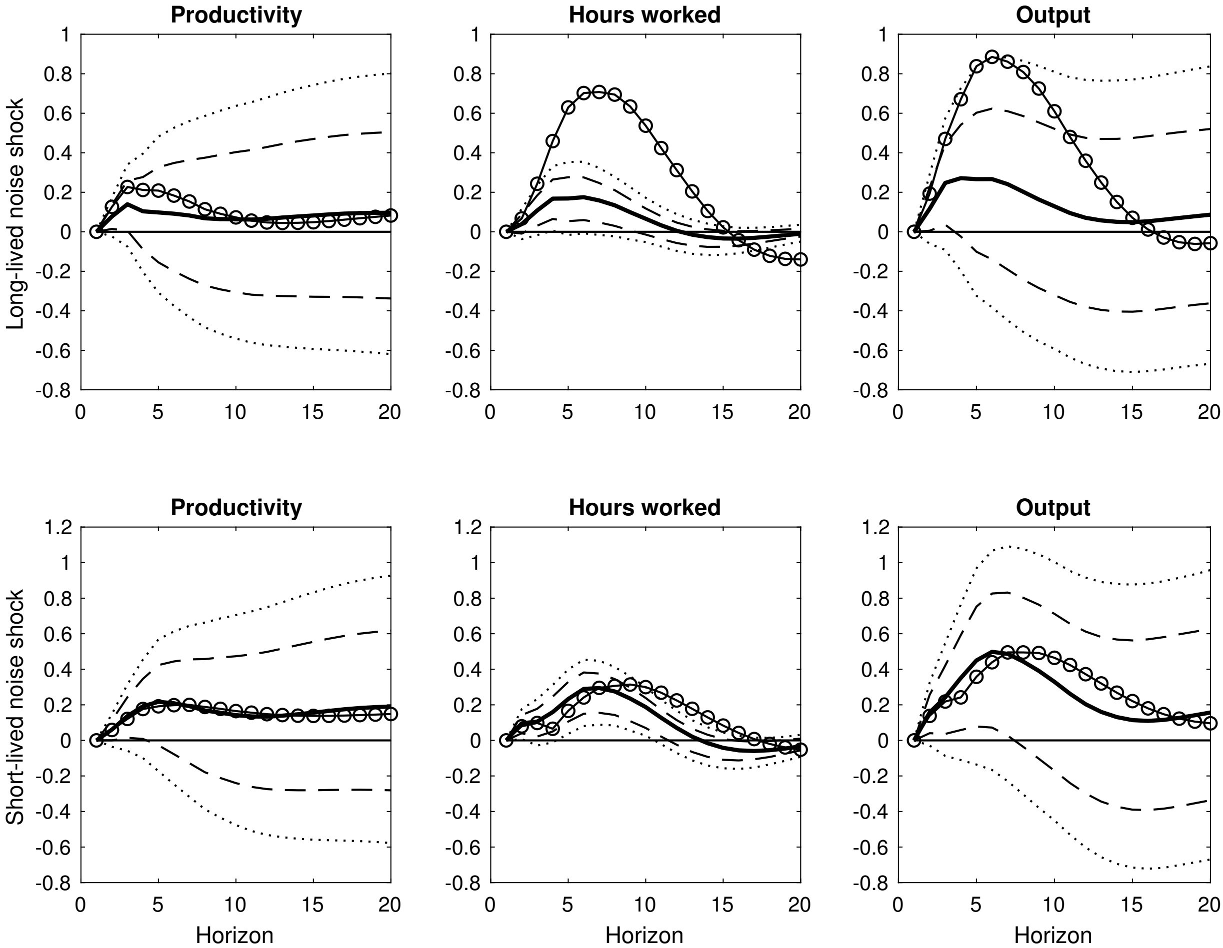
This appendix examines whether my findings are robust to alternative measures of hours worked and labor productivity. I consider the following two cases: (i) using an alternative filtering method to hours worked and (ii) using output per employment as a measure of labor productivity.

Regarding detrending methods, Fernald (2007) points out that a response of hours to a technology shock can be considerably different among detrending methods applied to the hours series. He points out that testing multiple detrending methods is necessary for researchers to confirm that detrending methods do not affect obtained results. In this study, I apply the Christiano-Fitzgerald filter to the hours worked per capita series.[17] Figure A1 shows responses of the VAR(4) model with the Christiano-Fitzgerald filter. The responses to the technology shock are smaller, though all the variables show qualitatively the same responses as in the baseline model.

As for an alternative labor productivity measure, I use output per employment, specifically the ratio of real GDP to nonfarm payroll employment. In this case, the employment series is detrended by the Hamilton filter as in the baseline estimation. My dataset contains final, heavily-revised and real-time real GDP data and final nonfarm payroll employment data from the Real-Time Data Set for Macroeconomists provided by the Federal Reserve Bank of Philadelphia. All of the data are seasonally adjusted and converted to log levels. I use data in log differences (%) for growth rates. The heavily-revised data are the data which become available three years after the first release of real GDP. Figure A2 shows that the alternative productivity measure produces smaller but qualitatively the same responses as in the baseline model.

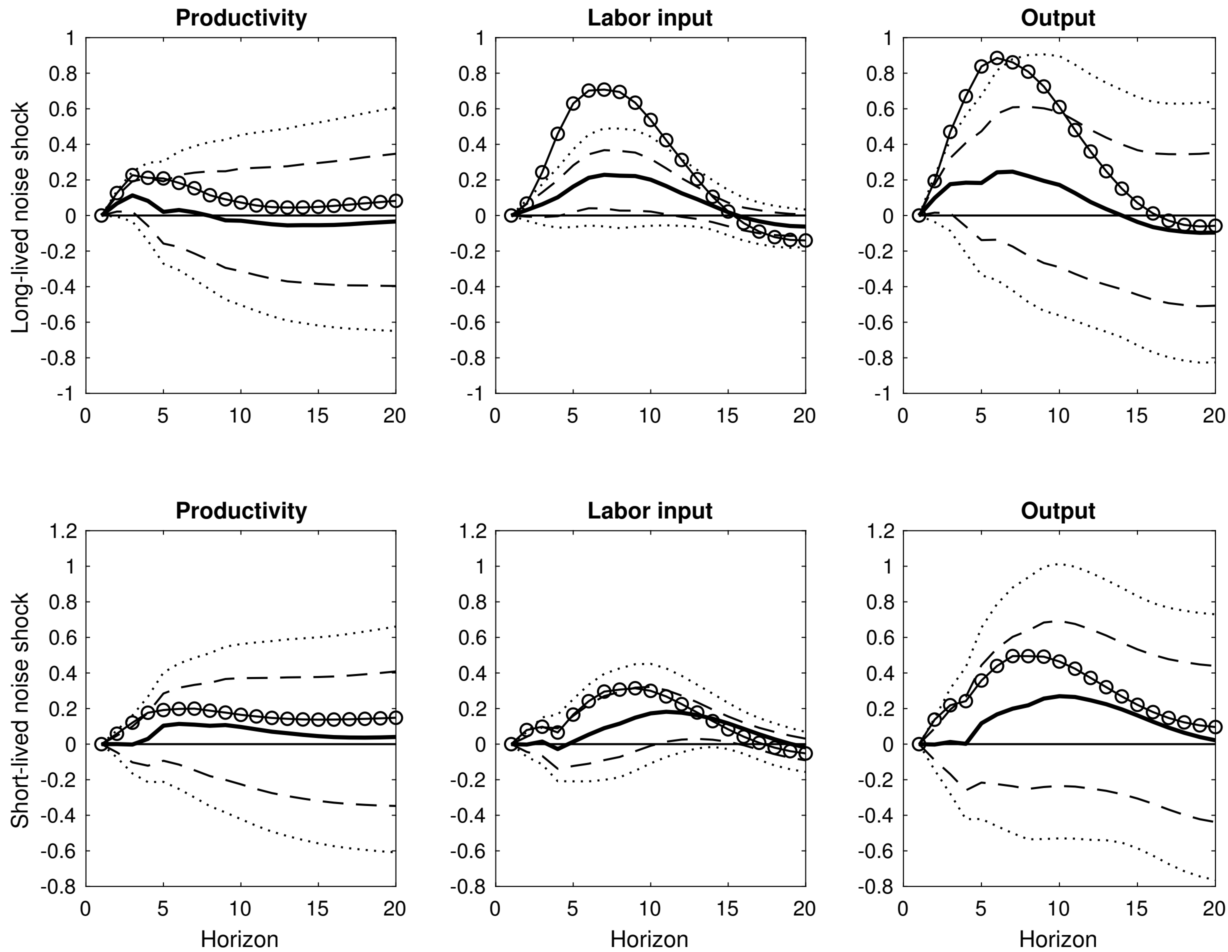
[17] The Christiano-Fitzgerald filter has frequency band 2–32 quarters to extract components of high- and business cycle frequencies.

Figure A1: Impulse response functions of VAR(4) model with labor input detrended by the Christiano-Fitzgerald filter



Note. Responses of the final, heavily-revised and real-time labor productivity data and hours worked data to two noise shocks. The 25th release is used in the figure as heavily-revised data. All of the variables in this figure are in log levels. The hours worked is in per capita and detrended by the Christiano-Fitzgerald filter with frequency band of 2–32 quarters. A solid line in each panel gives the point estimate response with its 68 (90) percentile confidence bands as dashed (dotted) lines. A solid line with circle markers gives the point estimate response of the baseline VAR(4) model presented in Figure 2.3.

Figure A2: Impulse response functions of VAR(4) using real GDP per employment



Note. Responses of the final, heavily-revised and real-time labor productivity data and employment data to two noise shocks. Labor productivity in this figure is defined as real GDP per employment. The data available three years after the first release of real GDP is used in the figure as heavily-revised data. All of the variables in this figure are in log levels. The employment series is in per capita and detrended by the Hamilton filter. A solid line in each panel gives the point estimate response with its 68 (90) percentile confidence bands as dashed (dotted) lines. A solid line with circle markers gives the point estimate response of the baseline VAR(4) model presented in Figure 2.3.

Chapter 3

Negative Interest Rate Policy and the Influence of Macroeconomic News on Yields

3.1 Introduction

In recent years, several advanced economies including the euro area and Japan have deployed negative interest rate policies (NIRPs). NIRP is designed to relaxing the constraint of the effective lower bound on policy rates by allowing the policy rates being in negative territory. However, it is an open question whether monetary policy becomes less constrained after introducing the NIRP. While policy rates can go negative, deposit rates and other market rates can be still bounded to zero. Moreover, the effective lower bound on policy rates in NIRP economies is unobservable in contrast to non-NIRP economies in which the lower bound is virtually zero. Accordingly, it is more difficult for NIRP economies to measure to what extent monetary policy is constrained.

Sensitivity of government bond yields to macroeconomic surprises is a widely-used measure capturing the degree of the effective lower bound constraint on monetary policy. Swanson and Williams (2014a) theoretically show that bond yields in an economy facing the zero lower bound are less responsive to macroeconomic news than those in an economy not constrained by the bound. If the sensitivity of bond yields in a NIRP economy increases since adopting the NIRP, it indicates that the effective lower bound becomes less binding.

This chapter examines whether NIRPs have relaxed constraints of the effective lower bound. To this end, I attempt to shed light on whether the influence of surprises in macroeconomic announcements is different during the NIRP period compared to during the preceding zero interest rate policy (ZIRP) period. I estimate the effects of surprises in domestic and U.S. macroeconomic announcements on government bond yields over the January 1999 to January 2018 period for four advanced NIRP economies – Germany, Japan, Sweden, and Switzerland. I focus on the possibility of time-variation in the influence of macroeconomic surprises coinciding with changes in the country-specific monetary policy regimes.

To provide additional insights on the NIRPs, I conduct the following analyses after obtaining the baseline results. First, I consider whether forward guidance (FG) dominates changes in the sensitivity of government bond yields during the NIRP period. Forward guidance, often implemented together with the NIRP, may contribute to reducing the sensitivity of bond yields to economic shocks via commitments on monetary policy. The second analysis is about impacts of macroeconomic uncertainty on the sensitivity of bond yields. Macroeconomic uncertainty is likely to influence the responsiveness of bond yields. I examine possible influence of changes in uncertainty on the sensitivity of bond yields, confirming whether it dominates recent changes in the sensitivity of bond yields. The third analysis is on the possibility of asymmetric effects of positive versus negative news. If the NIRP substantially shifts down the effective lower bound, it creates a margin for nominal interest rates to move downward. In this case, bond yields will respond more to a recessionary shock than during the pre-NIRP period, while their response to an expansionary shock will be relatively unchanged. If such asymmetry is observed in the data, it indicates that monetary policy is less constrained after the adoption of the NIRP.

I find that the influence of surprises in macroeconomic announcements is for all of the four NIRP countries either noticeably weaker or non-existent during the NIRP period than during the preceding ZIRP period. The results suggest that NIRP is associated with a lower bound that is no less constraining than the ZIRP lower bound. The finding is robust to the presence of macroeconomic uncertainty. I also find that the sensitivity of bond yields to macroeconomic surprises is symmetric between positive and negative surprises. These findings suggest that the effective lower bound has been binding in the NIRP economies during the NIRP period.

This study is firmly related to a vast literature on effects of macroeconomic news on asset prices. Gürkaynak et al. (2005) provide empirical evidence suggesting that macroeconomic surprises significantly influence long-term interest rates. Moessner and Nelson (2008) find an increase in the sensitivity of U.S. interest rate futures to macroeconomic surprises despite of the FOMC's guidance about future monetary policy around the middle of 2000s. Swanson and Williams (2014a) and Swanson and Williams (2014b) find that the influence of surprises in macroeconomic announcements on bond yields varies with the monetary policy regimes. Altavilla et al. (2017) find that macroeconomic surprises explain one third of quarterly variation in bond yields, suggesting that macroeconomic surprises can have persistent impacts on bond yields. My empirical analysis follows the procedure described in Swanson and Williams (2014a) and Swanson and Williams (2014b).

Recent studies show that sensitivity of long-term bond yields to macroeconomic surprises in major advanced economies has fallen after the recent financial crisis. Swanson and Williams (2014b) report the decline in the sensitivity for Germany, U.K., and U.S. during the early 2010s. Moessner et al. (2016) find that in Sweden the sensitivity of shorter-term interest rates declined around 2010, though the sensitivity of longer-term rates had been high and stable until the middle of 2015. This chapter is a complement to these studies, extending the sample period to include the NIRP period.

The recent decrease in the sensitivity of bond yields can also reflect effects of forward guidance policy. If the forward guidance effectively controls expectations, bond yields may respond less to macroeconomic surprises. Based on this idea, Ehrmann et al. (2019) study the effectiveness of forward guidance policies using a panel data containing macroeconomic surprises for several advanced economies. Moessner and Rungcharoenkitkul (2019) show that forward guidance in the U.S. reduced the sensitivity of shorter maturity bonds even after the policy liftoff from the zero lower bound.

A growing body of research sheds light on bank lending channel to uncover the role of the effective lower bound in NIRP economies. Brunnermeier and Koby (2019) propose a theoretical framework of "reversal interest rate" which determines the effective lower bound. A policy rate cut below the reversal interest rate is recessionary because banks reduce lending to keep profitability. Eggertsson et al. (2019) present evidence suggesting that pass-through of policy rates to deposit rates was muted in Sweden when the policy rates went below zero. They critically argue that monetary policy in NIRP economies should be constrained because of zero lower bound on deposit rates. By contrast, Altavilla et al. (2020) report that the pass-through of policy rates to deposit rates in the euro area has increased since the NIRP started. If the sensitivity of bond yields to macroeconomic surprises has increased since the adoption of NIRP, it suggests that the pass-through of policy rates has also revived.

The rest of the chapter is organized as follows. Section 3.2 describes institutional aspects. Section 3.3 presents characteristics of NIRP. Sections 3.4 and 3.5 describe details on my empirical methodology and data, respectively. Section 3.6 presents my results. Section 3.7 concludes.

3.2 Institutional Aspects

The unprecedented era of major central banks pursuing negative interest rate policies began a few years after the recent financial crisis.[1] Denmark was the first advanced economy to enter the NIRP regime as Denmark lowered its certificate of deposit rate to -0.20 percent as early as July 5, 2012. On June 11, 2014, the European Central Bank (ECB) deposit rate was lowered to -0.10 percent. Subsequently, on February 16, 2016, the second major central bank, the Bank of Japan, lowered its deposit rate to -0.10 percent.[2] Between the introduction of NIRP by the ECB and the Bank of Japan, Switzer-

[1] Dell'Ariccia et al. (2017) summarize details on NIRP economies, and some early assessments of the successfulness of NIRP.

[2] Prior to the introduction of the Japanese NIRP, the ECB deposit rate had been further lowered and was at the time of Japan entering NIRP held at -0.30 percent. Shortly after the Bank of Japan announcement

land and Sweden also went from ultra-low to negative interest rates, and both did so around the same time. Switzerland lowered its deposit rate (the so-called "sight deposit rate") to -0.75 percent on January 15, 2015, while Sweden lowered its policy rate, the repo rate, to -0.10 percent on February 12, 2015.[3] The salient and common policy objective of NIRP for all four countries under study is to counter deflationary pressures and raise inflation. For Switzerland, the stated objective of NIRP is dual in that the policy also aims to reduce or prevent domestic currency appreciation pressures in order to avoid a stifling of economic growth.

In my analysis, I consider four NIRP economies, namely, Germany, Japan, Sweden, and Switzerland. I do not include Denmark in my sample because the context and circumstance of the Danish NIRP are very different from those of the countries under study. The objective of the Danish NIRP pertains to maintenance of the DKK vis-á-vis the EUR within the Exchange Rate Mechanism (ERM) II framework.[4]

3.3 Characteristics of Negative Interest Rate Policy

The move from a zero or positive policy rate to negative policy rate marks at least a nominally dramatic shift in monetary policy. However, two essential aspects associated with the ZIRP regime also characterize the NIRP regime.

First, an effective lower bound on nominal interest rates nevertheless remains in NIRP economies. Brunnermeier and Koby (2019) propose a framework of the reversal interest rate which determines the effective lower bound in a negative interest rate environment. Eggertsson et al. (2019) argue that a policy rate cut in a NIRP economy is rather contractionary because the policy change reduces bank lending due to imperfect pass-through to deposit rates. These studies suggest the presence of the unobservable

of NIRP the ECB on March 16, 2016, reduced its deposit rate to -0.40 percent. See Wu and Xia (2020) for details on the ECB's rate cuts and a careful analysis of their impact on the yield curve.

3 The Swedish deposit rate entered negative territory, at -0.50 percent, on June 7, 2014.

4 While Bulgaria and Hungary have introduced negative policy rates, these countries are not considered in my analysis primarily due to the limited data availability of macroeconomic surprises.

effective lower bound in a negative interest rate environment.[5]

Second, even if monetary policy is constrained at the lower bound, monetary policy can be effective by influencing expectations about the path of future monetary policy. Reifschneider and Williams (2000) show that central banks facing the constraint can influence the economy with credible commitments about monetary policy conduct in the future when the lower bound becomes no longer binding. While Reifschneider and Williams (2000) do not directly consider negative interest rate environments, the basic argument is the same during NIRP periods.

These two essential characteristics of NIRP can be formalized using an illustrative New Keynesian model presented by Swanson and Williams (2014a). In their model, the central bank follows a Taylor-type rule with a latent lower bound. The policy rate i_t is described as follows:

$$i_t = \max[\tilde{i}_t, \bar{i}], \tag{3.1}$$

where i_t denotes the policy rate set by a central bank who faces a given effective lower bound $\bar{i}$.[6] The term $\tilde{i}_t$ is the notional shadow rate determined by a Taylor-type rule such that:

$$\tilde{i}_t = \pi_t + r_t^* + a(\pi_t - \bar{\pi}) + b\hat{y}_t, \tag{3.2}$$

where π_t is the inflation rate, r_t^* is the natural rate of interest, $\bar{\pi}$ is the central bank inflation target, $\hat{y}_t$ is the output gap. The coefficients a and b are non-zero constants that sum to one.

[5] Moreover, negative interest rates are only meaningful when these are above or equate the cost of holding money. Dong and Wen (2017) note that how far in the negative interest rates can go depends on the cost to the private sector of holding money.

[6] Very recent studies including Brunnermeier and Koby (2019) discuss endogeneity of the latent effective lower bound in a negative interest rate environment. However, there is yet no consensus on what factors crucially and quantitatively affect the level of the effective lower bound in a NIRP economy. Hence, I assume a given effective lower bound in this chapter.

An m-period zero-coupon government bond yield is described as:

$$i_t^m = E_t \frac{1}{m} \sum_{j=0}^{m-1} i_{t+j} + \phi^m, \tag{3.3}$$

where i_t^m denotes the annual bond yield with maturity m, E_t is the expectations operator at time t, and ϕ^m is the term premium. Equation (3.3) shows that the dynamics of bond yields reflects current and expected future short-term interest rates. The term premium in the model is constant over time and is different across maturities as assumed in Swanson and Williams (2014a) and Swanson and Williams (2014b).[7]

A change in the m-period bond yield at time t is written as:

$$\Delta i_t^m = \frac{1}{m} \sum_{j=0}^{m-1} (E_t i_{t+j} - E_{t-1} i_{t+j}) + \frac{1}{m} \sum_{j=0}^{m-1} (E_{t-1} i_{t+j} - E_{t-1} i_{t+j-1}). \tag{3.4}$$

When a macroeconomic news arrives, it influences the first term in the right-hand side, which captures responses of expectations to the news. In Section 3.6, I will study whether the sensitivity of bond yields to macroeconomic news has changed during the NIRP period as well as the ZIRP period.

3.4 Empirical Methodology

My approach is an event study setup, following Swanson and Williams (2014a) and Swanson and Williams (2014b). My primary interest is in the sensitivity of bond yields to surprises in major macroeconomic announcements. If the effective lower bound is binding in a NIRP economy, bond yields respond less to surprises than in the economy not constrained by the lower bound.

I examine how the sensitivity of bond yields to macroeconomic surprises in each of the ZIRP and NIRP periods differs from the average sensitivity over the pre-ZIRP

[7] Swanson and Williams (2014a) point out that a change in term premia can have positive or negative effects on the sensitivity of yields to macroeconomic news. The direction of the effects depends on several factors including the degree of risk aversion and the dynamics of demand for safe assets.

period. To this end, I estimated the following model by nonlinear least squares:

$$\Delta y_{m,t} = \alpha_m^P + x_t' \beta_m \delta_m^P + \varepsilon_{m,t}, \tag{3.5}$$

where $\Delta y_{m,t}$ denotes daily changes at the day t in the bond yield with m years to maturity. x_t is an $S \times 1$ vector of macroeconomic surprises. $\varepsilon_{m,t}$ is an error term reflecting the other news on the day t. α_m^P is a constant term for the monetary policy period P. An $S \times 1$ coefficient vector β_m collects the average sensitivity of the yield to individual macroeconomic surprises over the pre-ZIRP period. I choose the pre-ZIRP period as my normalization sub-sample, because monetary policy is considered to be less constrained in the period compared to during the ZIRP and NIRP periods.

A key coefficient is δ_m^P, which captures the sensitivity of the yield $y_{m,t}$ to macroeconomic surprises during the period P relative to the average sensitivity during the pre-ZIRP period. It takes one for the pre-ZIRP period by construction, δ^Z for the ZIRP period, and δ^N for the NIRP period. If δ^Z is less than one, the sensitivity of bond yields to macroeconomic news during the ZIRP period is lower than the sensitivity during the pre-ZIRP period. If δ^Z is zero, it means that bond yields were not responsive to macroeconomic news during the ZIRP period.

While Equation (3.5) focuses on immediate responses of bond yields to macroeconomic news, the macroeconomic surprises are also likely to have persistent impacts on bond yields. Altavilla et al. (2017) find evidence that the explanatory power of news surprises with respect to the variation in bond yields increases when they estimate the model at a lower frequency. To address the persistence of influences of macroeconomic news, I implement this procedure at monthly frequency. The monthly regression model to be estimated is:

$$\Delta y_{m,\tau} = \alpha_m^P + x_\tau' \beta_m \delta_m^P + \varepsilon_\tau, \tag{3.6}$$

where $\Delta y_{m,\tau}$ denotes changes in the bond yield with m years to maturity during the month τ, and x_τ contains the monthly average of macroeconomic surprises. Again, δ_m^P

is the coefficient of interest capturing the relative sensitivity of the yield to macroeconomic surprises for a month during the period P.

3.5 Data

My news data consists of a comprehensive set of date-stamped Germany, Japan, Sweden, Switzerland, and U.S. macroeconomic announcements and preceding professional forecasts. My full sample spans the 1 January 1999 to 31 January 2018 period. Table 3.1 shows the country-specific monetary policy regime change dates.[8] Table 3.2 displays the list of my news data. The dataset covers five domestic and six U.S. macroeconomic announcements for each country. As for domestic news, CPI, business survey, and real GDP are included in the dataset for all of the four countries. The selection of the other two news depends on data availability. The two series are retail sales and unemployment series for Germany and Sweden, PPI and unemployment for Switzerland, and PPI and machinery orders for Japan. The U.S. news are initial jobless claims, CPI, ISM, capacity utilization, retail sales, and advance estimates of real GDP. The U.S. news series are included in all of my baseline estimations. For each NIRP country, I consider the effects of the domestic news pertaining to the country in question alongside the effects of the U.S. news.

The data on professional forecasts is obtained from Money Market Services (MMS) provided by Haver Analytics and from Bloomberg News Service. Following the literature (e.g. Andersen et al. (2003), Andersen et al. (2007)), I construct for each news variable the standardized news surprise as the unexpected component of the announcement divided by the associated sample standard deviation.[9] When constructing the

[8] For Japan's case, the beginning of the first adoption of ZIRP coincides with the beginning of my full sample period. To focus on the sensitivity of yields during the post-crisis period, I set the start date of the ZIRP period at the day when the Bank of Japan cut the policy rate to virtually zero in response to the recent financial crisis.

[9] Let $A_{i,t}$ denote the value of an announcement of the variable i on day t. Let $E_{i,t}$ refer to the median value of the most recent market expectations, and let $\hat{\sigma}_i$ denote the standard deviation of the

news data set for each country I account for the difference in timing across U.S. and domestic macroeconomic announcements. The dataset used in regression analysis covers non-zero announcement days only, though the results are qualitatively the same if non-announcement days are also included in the observations.

The interest rates in my dataset are daily zero-coupon government bond yields with 1, 2, and 10 years of maturities. Figure 3.1 shows the evolution of the yields over the full sample. German, Japanese, and Swiss yields are available from Deutsche Bundesbank, the Japanese Ministry of Finance, and the Swiss National Bank, respectively. Swedish yields are provided by Sveriges Riksbank.

3.6 Results

3.6.1 Baseline results

Table 3.3 presents the baseline results for β, δ^Z, and δ^N in country-by-country regression of Equation (3.5) for 1-, 2-, and 10-year bond yields using daily data. The first eleven rows of the table present the estimated coefficients on individual macroeconomic surprises β. The estimated coefficients on the surprises have signs consistent with a Taylor-type rule of monetary policy when they are significant. The results show that macroeconomic surprises influence bond yields during the pre-ZIRP period for all of the four countries.

Concerning the ZIRP period, the estimates of δ^Z are significantly different from zero but lower than one for Germany, Sweden, and Switzerland, except for the sensitivity of Swedish 10-year bond yield which is larger than one. Table 3.3 also shows a larger decline in the sensitivity of bond yields with shorter maturities for all the countries. The results indicate that these countries were partially constrained in the zero lower bound. By contrast, δ^Z is not significant for Japan, suggesting that monetary policy in Japan was constrained during the ZIRP period.

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surprises regarding the variable i over the entire sample period. The standardized surprise of the macroeconomic fundamental announced on day t is then defined as $(A_{i,t} - E_{i,t})/\hat{\sigma}_i$.

For all of the four NIRP countries, the sensitivity of the yields has further decreased during the NIRP period. The estimates of δ^N for the yields with up to two years to maturity are not significant for Germany, Switzerland, and Japan, and those for Swiss and Japanese 10-year bond yields are not significant at the 5% level. The results suggest that monetary policy is constrained in all the four countries, and that the degree of constraint has increased in Japan and Switzerland since the adoption of the NIRP.

Turning to the regression results at monthly frequency, Table 3.4 shows the results of the monthly regressions with Equation (3.6). The most of all the estimates of the coefficients on individual surprises have signs consistent with a Taylor-type rule of monetary policy when they are significant. The values of R^2 in Table 3.4 are much larger than the corresponding R^2 in Table 3.3, as Altavilla et al. (2017) find in U.S. data. The results suggest that a macroeconomic surprise influences bond yields not only within its announcement day but also over the corresponding month. Concerning the δ^N, the estimate is much lower than δ^Z in the most cases, suggesting that the effects of macroeconomic news has become less persistent during the NIRP period.

To summarize, the baseline results suggest that monetary policy in the NIRP countries has been further constrained during the NIRP period. However, I note that one should not jump to the conclusion now. Sensitivity of asset prices can also fall in response to decreasing uncertainty about the future policy conducts through forward guidance and the other unconventional monetary policy measures. In the next subsection, I will examine whether changes in uncertainty can substantially explain the fall in the sensitivity of bond yields.

3.6.2 Influence of macroeconomic policy uncertainty

In theory, when agents face higher uncertainty on future policy conducts, they more rely on public information to learn about future states of the economy. Accordingly, the sensitivity of asset prices to macroeconomic announcements rises in response to increasing uncertainty. Forward guidance and other unconventional monetary policy measures, aiming at stabilizing expectations on future policy conducts, can substantially contribute to reducing the sensitivity of asset prices to macroeconomic surprise.[10] In this sense, the observed decline in the sensitivity of bond yields can be rather mainly attributed to the effects of these policy measures. To study the influence of changes in policy uncertainty, I conduct the following two analyses.

The first analysis is to examine the effects of forward guidance on the sensitivity of bond yields. I estimate Equation (3.5) with splitting the ZIRP period (for Germany and Sweden) and the NIRP period (for Japan) into pre-FG and FG periods. Switzerland is not considered in this exercise because the country does not adopt explicit forward guidance.

The period of forward guidance is from July 4, 2013 for Germany and is from December 20, 2011 for Sweden. While Japan has a long history of forward guidance, I focus on the effect of the Bank of Japan's inflation-overshooting commitment implemented on September 21, 2016.[11]

Table 3.5 presents the results. The coefficients $\delta^{Z-PreFG}$ and δ^{Z-FG} are the relative sensitivity of bond yields to macroeconomic news for the pre-FG and FG periods within the ZIRP period, respectively. Likewise, $\delta^{N-PreFG}$ and δ^{N-FG} are the relative sensitivity for the pre-FG and FG periods within the NIRP period. The estimates of $\delta^{Z-PreFG}$ and δ^{Z-FG} are very close for all the maturities of German bonds. The estimated $\delta^{N-PreFG}$ and δ^{N-FG} for Japanese bond yields are not significant at the 5% level.[12] The sensitivity of Swedish bond yields rather increases between the pre-FG and FG periods.[13] These results suggest that forward guidance is not the main reason for the decline in the sensitivity of bond yields for these three NIRP countries.

[10] Ehrmann et al. (2019) propose a theoretical model to explain the roles of forward guidance, presenting how the effects of forward guidance differ across its types.

[11] The announcement days of forward guidance are excluded from the observations in regression to eliminate on-impact effects of the implementation of forward guidance.

[12] The Japan's result is likely to overestimate the effects of forward guidance because the inflation-overshooting commitment was adopted together with yield curve control, which aims to stabilize long-term bond yields.

[13] Ehrmann et al. (2019) find that time-contingent forward guidance over a short horizon, which Riksbank uses, rather increases the sensitivity of bond yields to macroeconomic surprises.

The second analysis is to examine possible influence of a broad range of policy uncertainty on the sensitivity of bond yields to macroeconomic surprises. To get direct evidence on this, I include the monthly Economic Policy Uncertainty (EPU) Index, which is proposed by Baker et al. (2016), in my regressors as a country-level macroeconomic uncertainty measure.[14] This index, available at monthly frequency, incorporates a variety of information on policy-related economic uncertainty including newspaper articles and disagreement among economic forecasters. Figure 3.2 plots the EPU Index for Germany, Sweden, and Japan. Since the EPU Index for Switzerland is not available, my analysis about uncertainty focuses on the three countries.

To measure the influence of policy uncertainty on the sensitivity, I estimate the following model at monthly frequency:

$$\Delta y_{m,\tau} = \alpha_m + \gamma_m \hat{x}_{m,\tau} + \lambda_m u_\tau + \theta_m (u_\tau \times \hat{x}_{m,\tau}) + \varepsilon_{m,\tau}, \tag{3.7}$$

where λ_m and θ_m are scalar, and both of them capture the effects of policy uncertainty u_τ on the sensitivity of the bond yield $y_{m,\tau}$. If θ_m is positive and statistically significant, it indicates that decreasing uncertainty after the financial crisis explains the observed decline in the sensitivity of bond yields from the ZIRP to NIRP periods. To focus on the influence of policy uncertainty on the sensitivity, I assume that uncertainty influences the aggregate sensitivity of bond yields to macroeconomic surprises. This means that a rise in uncertainty increases the sensitivity of the yield to all of the individual surprises by θ. I compile the estimated responses of bond yields to macroeconomic surprises into a single news index $\hat{x}_{m,\tau} \equiv x'_\tau \hat{\beta}_m$. I call it the "news" index. The values of $\hat{\beta}_m$ are obtained from the baseline monthly regressions presented in Table 3.4.

Table 3.6 reports the results of the regression in which I use the news index and the cross term of the EPU Index and the news index as the regressors. The estimated

[14] The index is normalized to a mean of 100 from 1993 to 2010 for Germany, from 1985 to 2009 for Sweden, and from 1987 to 2015 for Japan. The value above 100 indicates that the economy faces above-average uncertainty. All of the data used in this chapter are available at the Economic Policy Uncertainty Index website: https://www.policyuncertainty.com/

coefficient value on the news index is positive and significant with no exceptions. By contrast, the coefficient on the cross term with the EPU Index is not statistically significant for all the yields considered, suggesting that policy uncertainty may not dominate the observed changes in the sensitivity of bond yields to macroeconomic surprises.

3.6.3 Asymmetry in the sensitivity of bond yields

If the NIRP substantially shifts down the effective lower bound, bond yields will respond more to a recessionary shock than during the pre-NIRP period. By contrast, their response to an expansionary shock will be relatively unchanged. I examine whether the sensitivity to a recessionary shock has increased since the adoption of the NIRP.[15]

Table 3.7 shows the regression results from Equation (3.5) with the data only on the days of negative surprises.[16] The estimated relative sensitivity of bond yields is fairly close to the baseline results in Table 3.3. Focusing on the cases of negative surprises, the estimates of δ^N are smaller than δ^Z for all the cases except for Swedish 10-year bond yields. Thus, the sensitivity of bond yields to macroeconomic surprises may be symmetric, suggesting that the effective lower bound has been also binding in the NIRP countries during the NIRP period. In other words, very low and stable policy rates are expected in the NIRP countries.

3.7 Conclusion

In this chapter, I have studied the influence of surprises in domestic and U.S. macroeconomic announcements on government bond yields for four NIRP countries, namely, Germany, Japan, Sweden, and Switzerland. I focus on the possibility of time-variation

[15] Since an observed surprise may reflect multiple fundamental shocks, structural identifications about the surprises should facilitate more accurate understanding on the asymmetry in the sensitivity of bond yields. However, in practice, the number of non-zero surprises in prices is much smaller than that in real indicators. Due to the data availability, I focus on signs of the surprises rather than fundamental sources of the surprises.

[16] I collect positive surprises for unemployment indicators.

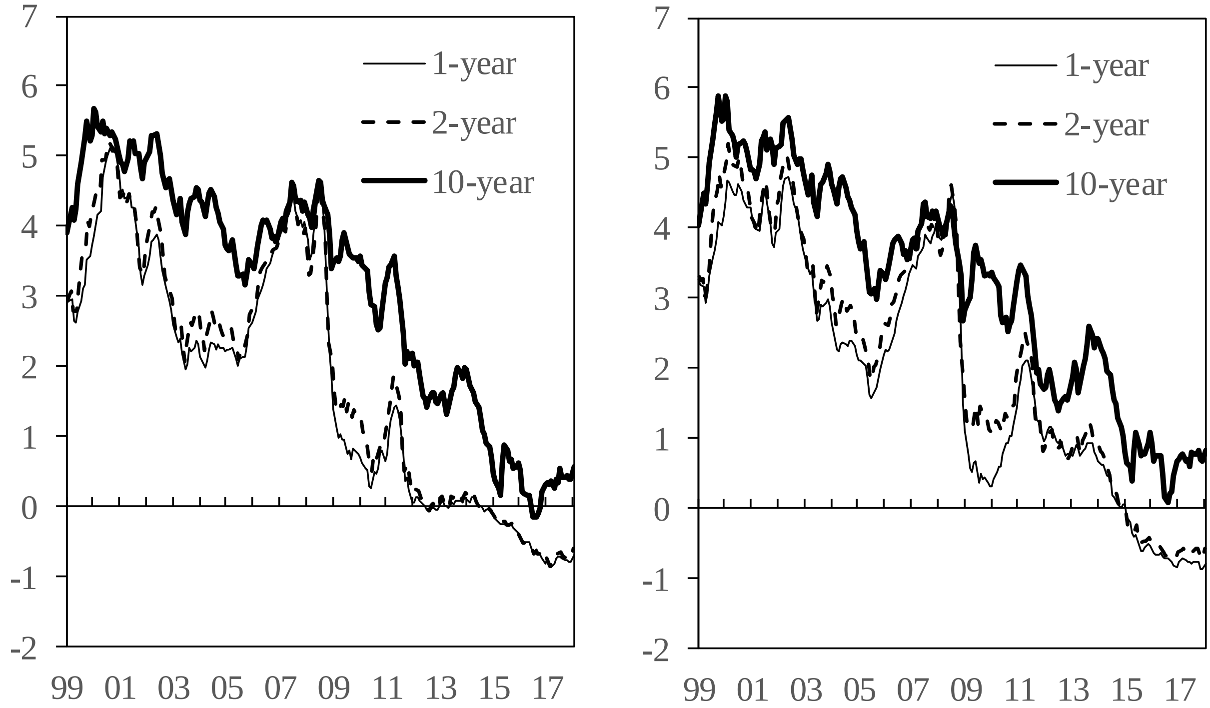
in the influence of macroeconomic surprises coinciding with changes in the domestic monetary policy regimes. I find that the sensitivity of bond yields in these NIRP countries is either noticeably weaker or non-existent during the NIRP period when compared to the preceding ZIRP period. My empirical results show that decreasing economic policy uncertainty does not have significant impacts on the sensitivity during the NIRP period. These findings suggest that the effective lower bound associated with the NIRP is no less constraining than the ZIRP lower bound.

While macroeconomic surprises can contribute to the dynamics of bond yields, the explanatory power of the surprises are small even at monthly frequency. A useful direction for future research would be to explore what is behind the low power of macroeconomic news to bond yields. The sensitivity analysis using intra-day data is also a promising avenue for future research. The use of high-frequency data, which is less likely to contaminated other shocks, will help better exploit surprises in financial data to a macroeconomic announcement.

In this chapter, monetary policy regimes are supposed to be firmly linked to structural changes. However, structural changes and shifts in monetary policy regimes do not necessarily occur in tandem. Time-varying parameter estimation can provide more precise evidence about the effective lower bound with the NIRP, and I leave this for future research.

Figure 3.1: Yields for NIRP countries

(a) Germany (left) and Sweden (right)



(b) Switzerland (left) and Japan (right)

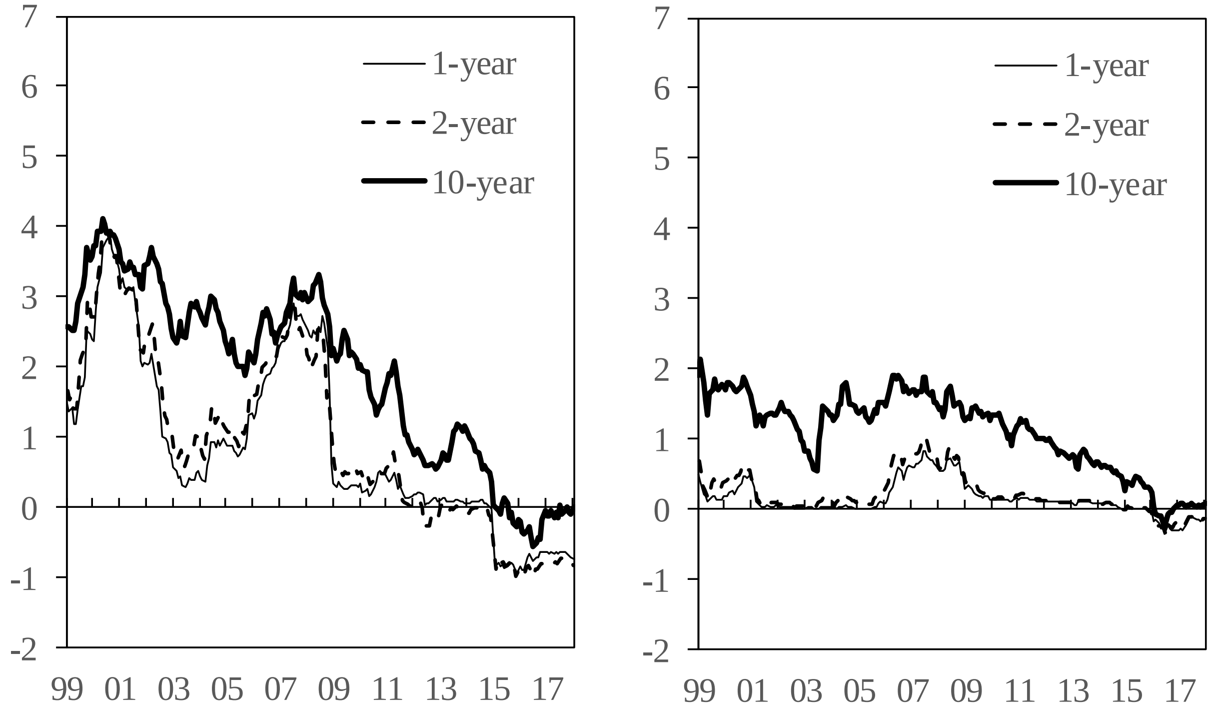
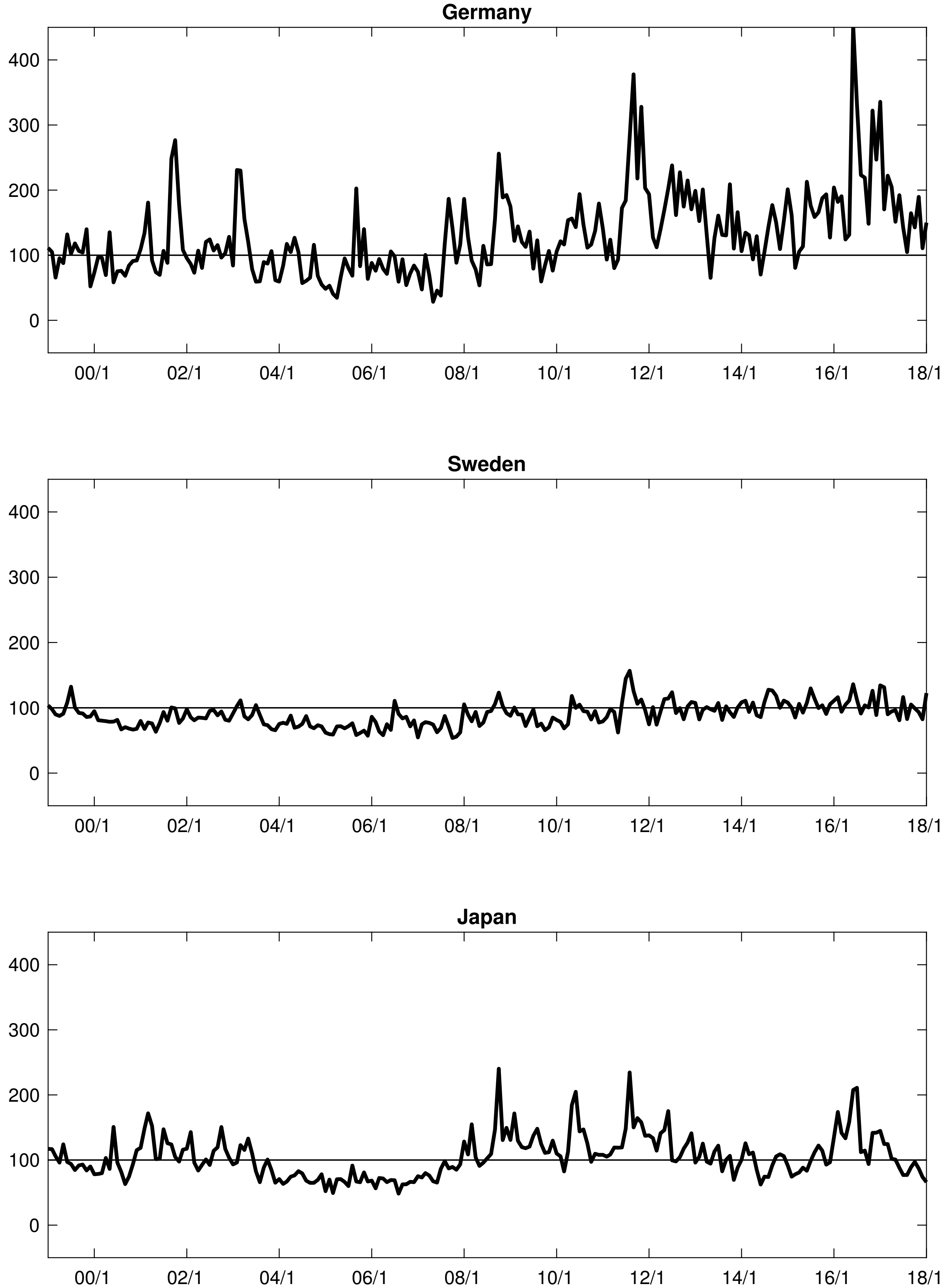


Figure 3.2: Economic Policy Uncertainty Index



Note. The index shows the level of uncertainty. It is normalized to a mean of 100 from 1993 to 2010 for Germany, from 1985 to 2009 for Sweden, and from 1987 to 2015 for Japan. The last observation is January 2018. The source is the Economic Policy Uncertainty Index website (https://www.policyuncertainty.com/), accessed on September 12, 2020.

Table 3.1: Monetary policy regime change dates

	Germany	Sweden	Switzerland	Japan
ZIRP	10/30/2008	7/13/2009	12/11/2008	12/19/2008
NIRP	6/11/2014	2/18/2015	1/22/2015	1/29/2016

Table 3.2: Macroeconomic announcements

(a) Germany

	Non-zero announcement surprises				Starting date	Frequency
	Full	Pre-ZIRP	ZIRP	NIRP		
Domestic announcements						
CPI	152	75	47	30	1/10/2000	Monthly
IFO	222	114	65	43	1/21/1999	Monthly
Retail sales	218	113	62	43	1/13/1999	Monthly
Unemployment	222	115	66	41	1/8/1999	Monthly
Real GDP	61	30	21	10	2/19/1999	Quarterly
U.S. announcements						
Initial jobless claims	972	497	288	187	1/7/1999	Weekly
CPI	138	69	40	29	1/14/1999	Monthly
ISM manufacturing	224	115	67	42	1/4/1999	Monthly
Capacity utilization	209	109	61	39	1/15/1999	Monthly
Retail sales	179	82	58	39	6/13/2001	Monthly
Real GDP	74	39	20	15	1/29/1999	Quarterly

(b) Sweden

	Non-zero announcement surprises				Starting date	Frequency
	Full	Pre-ZIRP	ZIRP	NIRP		
Domestic announcements						
CPI	187	104	54	29	1/19/1999	Monthly
PMI	147	50	65	32	12/1/2004	Monthly
Retail sales	224	123	66	35	1/20/1999	Monthly
Unemployment rate	199	114	59	26	1/20/1999	Monthly
Real GDP	72	41	20	11	3/10/1999	Quarterly
U.S. announcements						
Initial jobless claims	972	532	289	151	1/7/1999	Weekly
CPI	138	76	39	23	1/14/1999	Monthly
ISM manufacturing	224	124	66	34	1/4/1999	Monthly
Capacity utilization	209	117	60	32	1/15/1999	Monthly
Retail sales	179	90	58	31	6/13/2001	Monthly
Real GDP	74	42	20	12	1/29/1999	Quarterly

Table 3.2: Macroeconomic announcements (cont'd.)

(c) Switzerland

	Non-zero announcement surprises				Starting date	Frequency
	Full	Pre-ZIRP	ZIRP	NIRP		
Domestic announcements						
CPI	169	88	57	24	2/4/1999	Monthly
PPI	185	97	59	29	2/15/1999	Monthly
PMI	170	64	72	34	4/1/2003	Monthly
Unemployment rate	79	48	24	7	1/7/1999	Monthly
Real GDP	61	28	22	11	6/8/2000	Quarterly
U.S. announcements						
Initial jobless claims	972	503	314	155	1/7/1999	Weekly
CPI	138	70	45	23	1/14/1999	Monthly
ISM manufacturing	224	117	72	35	1/4/1999	Monthly
Capacity utilization	209	110	67	32	1/15/1999	Monthly
Retail sales	179	83	64	32	6/13/2001	Monthly
Real GDP	74	40	21	13	1/29/1999	Quarterly

(d) Japan

	Non-zero announcement surprises				Starting date	Frequency
	Full	Pre-ZIRP	ZIRP	NIRP		
Domestic announcements						
CPI	122	51	53	18	9/28/2001	Monthly
PPI	131	46	68	17	10/14/2003	Monthly
Tankan	57	30	22	5	4/5/1999	Monthly
Machinery orders	215	107	84	24	2/10/2000	Monthly
Real GDP	48	15	26	7	2/16/2005	Quarterly
U.S. announcements						
Initial jobless claims	972	504	364	104	1/7/1999	Weekly
CPI	138	71	51	16	1/14/1999	Monthly
ISM manufacturing	224	117	84	23	1/4/1999	Monthly
Capacity utilization	209	111	76	22	1/15/1999	Monthly
Retail sales	179	84	74	21	6/13/2001	Monthly
Real GDP	74	40	25	9	1/29/1999	Quarterly

Note. Starting dates are in local time.

Table 3.3: Baseline results: daily frequency

(a) Germany

	1-year		2-year		10-year	
CPI	0.518	(0.362)	0.546	(0.454)	0.914^{***}	(0.340)
IFO	1.341^{***}	(0.311)	2.285^{***}	(0.398)	1.461^{***}	(0.335)
Real retail sales	0.467	(0.348)	0.461	(0.418)	0.367	(0.387)
Unemployment change	-0.137	(0.243)	-0.440	(0.285)	0.037	(0.275)
Real GDP	0.966^{*}	(0.520)	1.476	(1.002)	1.377^{**}	(0.559)
U.S. capacity utilization	0.532^{*}	(0.290)	1.254^{***}	(0.383)	0.988^{**}	(0.389)
U.S. CPI	0.745^{*}	(0.411)	0.954^{*}	(0.529)	0.917^{**}	(0.425)
U.S. real GDP	1.341^{**}	(0.668)	1.955^{**}	(0.891)	0.644	(0.673)
U.S. initial jobless claims	-0.785^{***}	(0.210)	-1.165^{***}	(0.234)	-0.969^{***}	(0.183)
U.S. ISM manufacturing	1.944^{***}	(0.430)	1.971^{***}	(0.444)	1.539^{***}	(0.352)
U.S. retail sales	1.220^{***}	(0.437)	1.716^{***}	(0.553)	1.279^{***}	(0.494)
δ^Z	0.324^{**}	(0.133)	0.365^{***}	(0.123)	0.717^{***}	(0.193)
δ^N	0.019	(0.087)	0.030	(0.077)	0.494^{**}	(0.230)
R^2	0.052		0.070		0.048	

(b) Sweden

	1-year		2-year		10-year	
CPI	2.564^{***}	(0.351)	3.078^{***}	(0.425)	0.850^{***}	(0.297)
PMI	1.453^{***}	(0.503)	1.118^{***}	(0.393)	0.186	(0.311)
Retail sales	0.564^{**}	(0.223)	0.651^{**}	(0.262)	0.093	(0.212)
Unemployment rate	-0.032	(0.383)	-0.395	(0.443)	-0.590^{*}	(0.307)
Real GDP	0.715	(0.826)	1.705	(1.086)	0.900	(0.653)
U.S. capacity utilization	1.138^{**}	(0.457)	0.965^{**}	(0.423)	-0.009	(0.295)
U.S. CPI	-0.136	(0.250)	0.070	(0.288)	0.215	(0.265)
U.S. real GDP	0.533	(0.423)	0.231	(0.421)	-0.155	(0.458)
U.S. initial jobless claims	-0.446^{***}	(0.146)	-0.627^{***}	(0.166)	-0.583^{***}	(0.155)
U.S. ISM manufacturing	1.216^{***}	(0.253)	1.414^{***}	(0.324)	1.450^{***}	(0.327)
U.S. retail sales	1.064^{***}	(0.289)	1.144^{***}	(0.333)	1.044^{***}	(0.375)
δ^Z	0.577^{***}	(0.128)	0.743^{***}	(0.152)	1.271^{***}	(0.315)
δ^N	0.369^{***}	(0.111)	0.583^{***}	(0.109)	1.261^{***}	(0.437)
R^2	0.094		0.101		0.041	

Table 3.3: Baseline results: daily frequency (cont'd)

(c) Switzerland

	1-year		2-year		10-year	
CPI	0.570*	(0.315)	1.011***	(0.357)	0.171	(0.267)
PPI	-0.205	(0.400)	-0.120	(0.444)	0.306	(0.407)
PMI	0.178	(0.435)	0.061	(0.627)	0.454	(0.323)
Unemployment rate	0.062	(0.476)	-0.162	(0.378)	-0.138	(0.338)
Real GDP	0.567	(0.428)	0.338	(0.428)	0.251	(0.195)
U.S. capacity utilization	0.224	(0.306)	-0.063	(0.329)	0.250	(0.298)
U.S. CPI	0.005	(0.460)	0.919	(0.635)	0.596	(0.379)
U.S. real GDP	0.726	(0.483)	0.378	(0.619)	0.353	(0.551)
U.S. initial jobless claims	-0.658*	(0.375)	-0.889***	(0.263)	-0.749***	(0.154)
U.S. ISM manufacturing	1.864***	(0.446)	1.851***	(0.473)	0.880***	(0.312)
U.S. retail sales	0.906**	(0.448)	1.061**	(0.521)	1.109***	(0.383)
δ^Z	0.300*	(0.154)	0.268**	(0.126)	0.551**	(0.234)
δ^N	0.000	(0.243)	-0.024	(0.242)	0.219	(0.265)
R^2	0.030		0.044		0.032	

(d) Japan

	1-year		2-year		10-year	
CPI	0.464**	(0.232)	0.774**	(0.309)	1.007***	(0.335)
PPI	0.373**	(0.180)	0.531**	(0.258)	0.572*	(0.344)
Tankan	-0.043	(0.112)	-0.214	(0.224)	-0.064	(0.598)
Machinery orders	0.033	(0.116)	0.232	(0.170)	0.408	(0.291)
Real GDP	0.949*	(0.501)	1.592***	(0.614)	1.373*	(0.776)
U.S. capacity utilization	0.127	(0.088)	0.083	(0.140)	0.539**	(0.241)
U.S. CPI	0.051	(0.086)	-0.018	(0.113)	0.062	(0.292)
U.S. real GDP	0.056	(0.099)	0.185	(0.183)	0.549	(0.335)
U.S. initial jobless claims	-0.066*	(0.040)	-0.167**	(0.071)	-0.215*	(0.127)
U.S. ISM manufacturing	0.119	(0.075)	0.169	(0.130)	0.756*	(0.399)
U.S. retail sales	0.116	(0.079)	0.219**	(0.109)	0.662***	(0.256)
δ^Z	-0.011	(0.049)	0.019	(0.035)	0.198	(0.120)
δ^N	-0.502	(0.305)	-0.168	(0.229)	-0.387*	(0.201)
R^2	0.035		0.041		0.026	

Note. Heteroscedasticity-corrected standard errors are reported in parenthesis. Constant terms are included in all estimations but associated coefficient estimates and standard errors are not reported for reasons of brevity. ***, **, and * represent statistical significance at 1%, 5%, and 10% levels, respectively.

Table 3.4: Baseline results: monthly frequency

(a) Germany

	1-year		2-year		10-year	
CPI	-0.082	(0.100)	-0.089*	(0.119)	-0.023	(0.098)
IFO	0.260**	(0.102)	0.304	(0.114)	0.159**	(0.076)
Real retail sales	-0.107	(0.071)	-0.109	(0.096)	-0.066	(0.097)
Unemployment change	0.048	(0.089)	0.092	(0.108)	0.032	(0.090)
Real GDP	0.329**	(0.138)	0.337*	(0.178)	-0.308*	(0.170)
U.S. capacity utilization	0.085	(0.102)	0.073	(0.124)	-0.064	(0.081)
U.S. CPI	0.043	(0.075)	0.040	(0.098)	-0.035	(0.080)
U.S. real GDP	0.047	(0.145)	0.165	(0.188)	0.140	(0.159)
U.S. initial jobless claims	-0.078*	(0.043)	-0.147**	(0.057)	-0.089**	(0.037)
U.S. ISM manufacturing	0.168*	(0.101)	0.274**	(0.129)	0.252***	(0.091)
U.S. retail sales	0.278***	(0.106)	0.369***	(0.123)	0.233**	(0.102)
δ^Z	1.139**	(0.469)	0.940**	(0.416)	1.409***	(0.496)
δ^N	-0.019	(0.166)	0.010	(0.136)	0.601	(0.374)
R^2	0.165		0.177		0.136	

(b) Sweden

	1-year		2-year		10-year	
CPI	0.138	(0.128)	0.194	(0.129)	0.213*	(0.121)
PMI	0.845**	(0.362)	0.750***	(0.264)	0.400*	(0.238)
Retail sales	0.081	(0.125)	0.139	(0.130)	0.110	(0.117)
Unemployment rate	0.037	(0.117)	0.026	(0.125)	0.018	(0.108)
Real GDP	0.114	(0.176)	-0.045	(0.215)	-0.227	(0.202)
U.S. capacity utilization	0.431**	(0.168)	0.363**	(0.152)	-0.067	(0.107)
U.S. CPI	-0.004	(0.116)	-0.020	(0.123)	0.075	(0.114)
U.S. real GDP	0.080	(0.166)	0.189	(0.199)	0.295	(0.217)
U.S. initial jobless claims	-0.149***	(0.056)	-0.203***	(0.061)	-0.139***	(0.047)
U.S. ISM manufacturing	0.182	(0.121)	0.217*	(0.121)	0.219**	(0.090)
U.S. retail sales	0.323**	(0.130)	0.369**	(0.128)	0.170	(0.109)
δ^Z	0.136	(0.092)	0.211*	(0.115)	0.499	(0.336)
δ^N	0.069	(0.052)	0.036	(0.053)	0.183	(0.261)
R^2	0.257		0.234		0.131	

Table 3.4: Baseline results: monthly frequency (cont'd)

(c) Switzerland

	1-year		2-year		10-year	
CPI	0.081	(0.115)	0.152	(0.115)	0.113	(0.088)
PPI	0.310***	(0.106)	0.423***	(0.118)	0.256	(0.100)
PMI	-0.099	(0.111)	0.018	(0.118)	-0.135*	(0.167)
Unemployment rate	-0.077	(0.107)	-0.060	(0.097)	-0.075	(0.075)
Real GDP	-0.073	(0.107)	-0.172	(0.108)	-0.188	(0.073)
U.S. capacity utilization	0.087	(0.103)	0.156	(0.122)	0.073	(0.078)
U.S. CPI	0.115	(0.090)	0.057	(0.090)	0.061	(0.087)
U.S. real GDP	-0.121	(0.140)	-0.178	(0.145)	-0.040	(0.116)
U.S. initial jobless claims	-0.093***	(0.056)	-0.088***	(0.050)	-0.099**	(0.044)
U.S. ISM manufacturing	0.307	(0.115)	0.342	(0.118)	0.195**	(0.084)
U.S. retail sales	0.385**	(0.140)	0.321	(0.108)	0.249**	(0.096)
δ^Z	0.444*	(0.202)	0.502**	(0.183)	0.247	(0.326)
δ^N	-0.162	(0.072)	-0.196	(0.101)	-0.445**	(0.199)
R^2	0.198		0.237		0.141	

(d) Japan

	1-year		2-year		10-year	
CPI	-0.002	(0.005)	-0.002	(0.012)	0.079	(0.061)
PPI	0.010	(0.011)	0.006	(0.012)	-0.074	(0.069)
Tankan	0.012	(0.013)	0.040	(0.024)	0.169	(0.152)
Machinery orders	0.007	(0.008)	0.014	(0.012)	0.090	(0.068)
Real GDP	-0.008	(0.009)	0.000	(0.021)	0.182	(0.160)
U.S. capacity utilization	-0.007	(0.008)	-0.016	(0.013)	-0.061*	(0.062)
U.S. CPI	-0.014	(0.016)	-0.037	(0.023)	-0.031	(0.079)
U.S. real GDP	-0.037	(0.039)	-0.103*	(0.054)	0.175	(0.097)
U.S. initial jobless claims	0.002	(0.004)	0.013	(0.012)	-0.008	(0.025)
U.S. ISM manufacturing	0.001	(0.004)	0.002	(0.013)	0.134*	(0.077)
U.S. retail sales	0.010	(0.012)	0.035	(0.024)	0.169***	(0.056)
δ^Z	0.087	(0.763)	0.421	(0.434)	0.430*	(0.219)
δ^N	18.50	(19.62)	7.440*	(3.913)	-0.475	(0.440)
R^2	0.102		0.093		0.098	

Note. For details on the statistical test used in the table, see notes to Table 3.3.

Table 3.5: Effects of forward guidance on the sensitivity of bond yields

(a) Germany

	1-year		2-year		10-year	
$\delta^{Z-PreFG}$	0.325**	(0.150)	0.374***	(0.137)	0.715***	(0.211)
δ^{Z-FG}	0.309**	(0.130)	0.290*	(0.169)	0.728*	(0.375)
δ^{N}	0.019	(0.087)	0.030	(0.077)	0.494**	(0.230)
R^2	0.052		0.070		0.048	

(b) Sweden

	1-year		2-year		10-year	
$\delta^{Z-PreFG}$	0.372***	(0.144)	0.541**	(0.216)	1.062***	(0.360)
δ^{Z-FG}	0.757***	(0.180)	0.923***	(0.175)	1.532***	(0.429)
δ^{N}	0.363***	(0.111)	0.576***	(0.109)	1.207***	(0.431)
R^2	0.097		0.102		0.042	

(c) Japan

	1-year		2-year		10-year	
δ^{Z}	-0.014	(0.050)	0.019	(0.035)	0.205*	(0.122)
$\delta^{N-PreFG}$	-1.238*	(0.731)	-0.720	(0.554)	-0.876	(0.533)
δ^{N-FG}	-0.211	(0.307)	0.033	(0.250)	-0.186	(0.182)
R^2	0.037		0.043		0.026	

Note. The coefficient δ captures relative sensitivity of bond yields to news during a specific period. If δ is less than one, bond yields during the period respond less to macroeconomic news than during pre-ZIRP period. In the panels (a) and (b), $\delta^{Z-PreFG}$ and δ^{Z-FG} are for the ZIRP period before and after the introduction of forward guidance, respectively, and δ^{N} is for the NIRP period. In the panel (c), δ^{Z} is for the ZIRP period, and $\delta^{N-PreFG}$ and δ^{N-FG} are for the NIRP period before and after the introduction of the Bank of Japan's inflation-overshooting commitment, respectively. For details on the statistical test used in the table, see notes to Table 3.3.

Table 3.6: Influence of macroeconomic uncertainty

(a) Germany

	1-year		2-year		10-year	
News index	1.082***	(0.209)	0.988***	(0.165)	1.154***	(0.181)
EPU	-0.056***	(0.013)	-0.051***	(0.011)	-0.032***	(0.011)
News index × EPU	0.541	(0.380)	0.232	(0.211)	-0.002	(0.200)
R^2	0.285		0.248		0.153	

(b) Sweden

	1-year		2-year		10-year	
News index	0.448***	(0.13)	0.539***	(0.117)	0.854***	(0.168)
EPU	-0.036***	(0.009)	-0.035***	(0.011)	-0.023**	(0.012)
News index × EPU	0.265	(0.173)	0.196	(0.149)	0.179	(0.183)
R^2	0.205		0.188		0.122	

(c) Japan

	1-year		2-year		10-year	
News index	0.316	(0.217)	0.581***	(0.156)	0.660***	(0.170)
EPU	-0.012***	(0.004)	-0.015***	(0.003)	-0.019***	(0.005)
News index × EPU	0.106	(0.232)	0.178	(0.183)	-0.097	(0.126)
R^2	0.081		0.116		0.100	

Note. The news index is a sum of the responses of bond yields to individual surprises. The index is computed using the estimated average sensitivity of bond yields to the surprises during the pre-ZIRP period. The EPU is the Economic Policy Uncertainty (EPU) Index proposed by Baker et al. (2016) which is used in this table as the country-level macroeconomic uncertainty measures. For details on the statistical test used in the table, see notes to Table 3.3.

Table 3.7: The sensitivity of bond yields to negative surprises

(a) Germany

	1-year		2-year		10-year	
δ^Z	0.394**	(0.189)	0.393**	(0.157)	0.931***	(0.307)
δ^N	0.013	(0.104)	0.003	(0.088)	0.725*	(0.375)
R^2	0.030		0.043		0.024	

(b) Sweden

	1-year		2-year		10-year	
δ^Z	0.343***	(0.123)	0.482***	(0.145)	1.094***	(0.400)
δ^N	0.312*	(0.166)	0.452***	(0.138)	2.107***	(0.762)
R^2	0.061		0.067		0.025	

(c) Switzerland

	1-year		2-year		10-year	
δ^Z	0.343	(0.210)	0.305**	(0.138)	0.419*	(0.222)
δ^N	-0.197	(0.224)	-0.208	(0.201)	-0.224	(0.274)
R^2	0.027		0.045		0.029	

(d) Japan

	1-year		2-year		10-year	
δ^Z	-0.047	(0.045)	0.001	(0.028)	0.020	(0.058)
δ^N	-0.866	(0.534)	-0.403	(0.249)	-0.343***	(0.125)
R^2	0.017		0.023		0.024	

Note. For details on the statistical test used in the table, see notes to Table 3.3.

Chapter 4

The Effects of Asset Purchases and Normalization of U.S. Monetary Policy

4.1 Introduction

After the global financial crisis (GFC) erupted and short-term interest rates fell close to zero, central banks in advanced economies, most notably in the United States, adopted large-scale asset purchases (LSAPs) as an unconventional policy tool to stabilize the financial system and spur economic recovery. Such asset purchase programs are typically called "quantitative easing (QE)," and the Bank of England, the Bank of Japan, and the European Central Bank all followed a similar approach in providing ample monetary accommodation. A policy of adjusting the size and composition of their balance sheets appears to have been added to the toolkit of major central banks.

But how well the balance sheet policy works, particularly in normal economic conditions, remains an open question despite its heavy use during the crisis and recession. Critics argue that these measures have significant effects only when financial markets are under severe stress and that their effectiveness may diminish when economic and financial conditions move from crisis to normal conditions (e.g., Borio and Zabai (2018); Goodhart and Ashworth (2012)). The policy transmission mechanism could also be weaker in an economic recovery when interest rates are persistently low, partly due to post-crisis headwinds such as substantial deleveraging and heightened uncertainty (e.g., Borio and Hofmann (2017); Hesse et al. (2018)). Meanwhile, policymakers tend to offer a more positive assessment of the efficacy of LSAPs as a policy tool to respond to future economic downturns (e.g., Bernanke (2017); Yellen (2016)). In addition, based on simulations, Kiley (2018) suggests that active QE improves economic performance given that equilibrium real interest rates become lower and that interest rates are near their effective lower bound. Now that the post-crisis headwinds have dissipated and normalization of monetary policy is well underway, it is an opportune time to reinvestigate the macroeconomic effects of LSAPs and possible shifts in their effects in the United States.

Against this background, this chapter empirically examines changes in the effects of unconventional monetary policies (UMPs) in the United States. To this end, I build on a benchmark vector autoregression (VAR) analysis with a combination of zero and sign restrictions, based on Weale and Wieladek (2016) and Hesse et al. (2018), and estimate a Markov-switching VAR (MSVAR) model with absorbing states to capture possible structural changes.[1] This is relevant because the Federal Reserve (Fed) undertook the LSAPs starting in December 2008 and expanded or modified the program several times thereafter. Partly because of these LSAPs, the U.S. economy experienced not only a quick recovery but also solid growth, forcing the Fed to consider tapering the LSAPs. In speeches given in May and June 2013, former Fed Chair Ben Bernanke hinted at reducing the size of the third-round of the LSAP, causing the taper tantrum, and this may have altered market expectations of aggressive monetary accommodation in the future (i.e., a possible beginning of monetary policy normalization). Indeed, the Fed ended the LSAPs in December 2014 and started raising the federal funds (FF) rates in December 2015. Given these evolutions of the U.S. unconventional monetary policies, the MSVAR model with absorbing regimes is reasonable and attractive, since it allows me to examine the existence and timing of possible permanent regime changes in the effects of U.S. unconventional monetary policies.[2] Thus, my regime-shift analysis in-

[1] This type of models is called change-point models in the literature of Bayesian time series models. For example, Chib (1998) proposes a Bayesian estimation method for models with multiple change points. The name MSVAR is widely used in the macroeconomics literature including Hara et al. (2020).

[2] An alternative, popular VAR model to assess changes in the effects of structural shocks is a time-varying parameter VAR (TPVAR) model. However, the coefficients of the TPVAR model are typically

corporates the developments of the U.S. unconventional monetary policies in assessing their efficacy.

One of the challenges of estimating an MSVAR model is that there may be no single monetary policy measure that can capture the full range of U.S. unconventional monetary policies over the last decade. I employ the shadow rate of Wu and Xia (2016) as a single monetary policy measure to deal with this issue. Since the shadow rate compiles information on expectations on future policy actions from short- and long-term interest rates, the use of the shadow rate matches what unconventional monetary policy aims at, that is, to influence the expectations at the zero lower bound. Moreover, the shadow rate is free from the zero lower bound.[3] Having these advantages, the shadow rate can arguably capture the easing of monetary policy during QE, decreasing significantly between 2009 and 2014, even when the FF rate remained at the effective lower bound. In addition, the shadow rate was mostly increasing after early 2014, indicating a movement towards the normalization of U.S. monetary policy. Therefore, it is not unreasonable to assume that the shadow rate can describe the monetary policy stance of the Fed over the last decade.

My model has at least three notable features. First, the analysis allows me to detect the timing of breaks in the policy effects formally. Either two or three regimes are assumed for the period of January 2009–September 2018 to accommodate multiple regime changes. Second, two primary regimes–corresponding to before and after the middle of 2013–emerge from the Markov-switching model and I interpret them as an "LSAP regime" and a "monetary policy normalization regime" respectively. For

modeled as a random walk, making it hard to capture the sudden changes along with the monetary policy regime changes. I might also be able to extend my model by modeling the transition probabilities based on financial conditions or the business cycle to explicitly model endogenous monetary policy regime changes, depending on the economic conditions. However, given the relatively small sample size, it is not easy to do so. Therefore, the use of the MSVAR with exogenous switching is best suited for my purpose.

[3] These advantages, not explicitly addressed in Hara et al. (2020), are pointed out by the literature on shadow rates including Wu and Xia (2016).

each regime, the macroeconomic effects are further investigated with relevant policy measures. Third, I examine components of GDP (nondurable, service, and durable consumption, as well as capital investment) as alternative indicators of real economic activity to discuss possible factors that generate different policy effects for each regime.

The main findings of this chapter can be summarized as follows. The MSVAR analysis detects regime changes around the beginning of 2011 and the middle of 2013. Before 2011, the LSAPs had relatively large impacts on the real economy and prices, but after the middle of 2013, their effects were weaker and less-persistent. In addition, during the monetary policy normalization regime after the middle of 2013, the asset purchase or balance sheet shocks had slightly weaker effects than during the early stage of the LSAPs but stronger effects than during the late stage of the LSAPs. This suggests that asset purchases can be used at least as a secondary tool to respond to future downturns. On the other hand, interest rate shocks had insignificant impacts on the real economy and prices. Finally, my results using the components of GDP indicate that a positive response of durable and capital goods expenditures to interest rate shocks weakened the negative impacts of an interest rate hike during the period of monetary policy normalization.

The remainder of this chapter is organized as follows. Section 4.2 briefly reviews the U.S. unconventional monetary policies and the related literature, while Section 4.3 introduces my empirical methodology. Section 4.4 summarizes the empirical results based on the MSVAR model using the shadow rate as a policy measure. Section 4.5 analyzes each regime in detail using more appropriate policy measures for each regime. Finally, Section 4.6 concludes the paper.

4.2 Related Literature

There is a vast literature on the LSAPs since the GFC. Various papers propose theoretical frameworks to explain the effectiveness of asset purchases by the central bank. They suggest that asset purchases are particularly effective when financial markets are disrupted. In other words, their effectiveness may become weaker as financial markets return to normal. For example, Cúrdia and Woodford (2011) construct a New Keynesian model with imperfect financial intermediation and lending to the private sector by the central bank and find that asset purchases targeted at specific types of assets can be stimulative, particularly during a period of financial market turmoil. Gertler and Karadi (2013) extend a New Keynesian framework to introduce a central bank that purchases government bonds as well as private securities and compare the effectiveness of different QE programs. They find that a purchase of private securities is more stimulative than that of government bonds. Moreover, they find that the LSAP is more effective the longer the time expected at the effective lower bound. Bauer and Rudebusch (2014) find evidence of signaling effects from asset purchases that effectively lower expectations on future short-term interest rates.

In contrast, there seems to be no conclusive empirical evidence of changes in the effectiveness of the LSAPs on the macroeconomy. Many studies find a decline in the effect of monetary policy on financial markets during the zero lower bound (ZLB) period (e.g., D'Amico and King (2013); Krishnamurthy and Vissing-Jorgensen (2011); Krishnamurthy and Vissing-Jorgensen (2013)), although some studies also find that the announcement of an LSAP has significant effects on the financial markets (Ihrig et al. (2018); Swanson (2018)). As for the macroeconomic effects, Haldane et al. (2016) find that an increase in asset purchases can be more stimulative when financial markets are disrupted. In a similar vein, Hesse et al. (2018) report that the stimulative effects of the LSAPs have been declining in the post-GFC period. They also argue that anticipated asset purchases can have substantial stimulative effects even in the later stages of an LSAP.

Since the LSAPs and the zero interest rate policy were conducted simultaneously, a growing number of studies devote much attention to evaluating the effectiveness of multiple monetary policy measures in a unified way. Against this background, shadow rate term structure models have been developed to deal with the ZLB by a number of studies, including Ichiue and Ueno (2013), Krippner (2013), Bauer and

Rudebusch (2016), and Wu and Xia (2016). More specifically, Bullard (2012), Krippner (2013), and Wu and Xia (2016) claim that the shadow rate can be used as a single measure of both conventional and unconventional monetary policy stances. For example, Wu and Xia (2016) estimate the effectiveness of monetary policy from 1990 to 2013 using their shadow rate and find an expansionary monetary policy shock is highly stimulative during the ZLB period. Furthermore, Bauer and Rudebusch (2016) suggest that the shadow rate can capture monetary policy expectations. In this study, I use the shadow rate data computed according to the method of Wu and Xia (2016), and extend the existing empirical studies in a few ways.[4] First, by expanding the sample period up to September 2018, I analyze the effectiveness of the U.S. unconventional monetary policies over both LSAP and normalization periods. Second, I consider regime changes in the effects of the U.S. unconventional monetary policies, allowing for the possibility of a difference in the effects of unconventional monetary policies during the LSAP and normalization regimes. Third, I use the estimated SVAR model to compare the effectiveness of the policy rate and LSAPs during both the LSAP and normalization regimes, as detected by the model.

Other studies related to this study include those on the state-dependence of monetary policy effectiveness. For example, Lo and Piger (2005) find that policy shocks are more stimulative in recessions and that the asymmetry may not be caused by either the direction or size of the policy shock. In contrast, Tenreyro and Thwaites (2016) provide empirical evidence that a change in the FF rate is less effective in recessions, particularly for durable goods consumption and business investment. Berger and Vavra (2015) extend a standard incomplete market model to incorporate fixed costs into households' durable goods consumption adjustment. They find that durable expenditures are less responsive to economic shocks during recessions in the presence of adjustment costs to households' durable purchases. In other words, durable expenditures can be more sensitive to shocks in an economic recovery. Suzuki (2016) provides empirical evidence of this by using the U.S. Consumer Expenditure Survey. He

[4] The data are available at https://sites.google.com/view/jingcynthiawu/shadow-rates

reports that the fixed costs of adjustment on households' durable goods consumption are larger than those on nondurable goods consumption. In this study, I investigate the state-dependence of policy effectiveness potentially arising from households' durables consumption adjustment. Specifically, I introduce durable and nondurable goods as well as services into my VAR model and analyze changes in the policy effects on these types of goods before and after the start of U.S. monetary policy normalization.

4.3 Methodology

This study employs an MSVAR model with absorbing regimes to examine possible permanent regime changes in the effects of U.S. unconventional monetary policies. This is relevant because the U.S. unconventional monetary policies have evolved significantly over the last decade, as I briefly discussed in the introduction. In this section, I introduce my baseline model, followed by the MSVAR model and its estimation.

4.3.1 Baseline model

The baseline model is taken from Weale and Wieladek (2016) and Hesse et al. (2018). Both studies employ the following VAR model estimated on monthly data:

$$\mathbf{Y}_t = \boldsymbol{\alpha} + \sum_{k=1}^{L} \mathbf{A}_k \mathbf{Y}_{t-k} + \varepsilon_t, \varepsilon_t \sim \text{iid } N(\mathbf{0}, \boldsymbol{\Sigma}) \tag{4.1}$$

where $\mathbf{Y}_t$ is a vector of endogenous variables, $\boldsymbol{\alpha}$ is a vector of constants, $\mathbf{A}_k$ is the array of coefficients associated with the corresponding vector of variables for lag k. The endogenous variables comprise the logarithm of monthly (seasonally adjusted) real gross domestic product (GDP), the logarithm of the (seasonally adjusted) consumer price index (CPI), a measure of the policy instrument which is discussed in detail below, the yield on the 10-year government bond and the logarithm of the real stock price index (deflated by the CPI). For my empirical analysis, L is set to two, following Weale and Wieladek (2016) and Hesse et al. (2018).

One of the difficulties with assessing the effects of the U.S. unconventional monetary policies over the last decade is that there may be no single variable which can capture the U.S. unconventional monetary policies during this period. During the GFC the Fed lowered the FF rate effectively to zero and introduced the LSAP program in December 2008. Since then, the Fed has been using asset purchases as one of its policy instruments. Because of this, Weale and Wieladek (2016) and Hesse et al. (2018) use the announcement of asset purchases as a policy measure. In addition, the Fed started raising the FF rate in December 2015, making it an active policy tool during the normalization of U.S. monetary policy. The dynamics of these two policy measures are displayed in Figure 4.1. As can be seen, the cumulative amount of asset purchases announced has been constant since 2014, while the FF rate was essentially at its lower bound with little fluctuation before December 2015. As a consequence, econometrically it might not be appropriate to use either of them as a policy measure throughout the entire sample period.

To overcome this problem, I adopt the shadow rate of Wu and Xia (2016) as a single monetary policy measure over the last decade. Figure 4.1 also plots the shadow rate along with the two other policy measures. As can be seen, even though the FF rate was almost constant at the effective lower bound until the end of 2015, the shadow rate decreased significantly between 2009 and 2014, reflecting the increase in the cumulative amount of asset purchases announced. On the other hand, the shadow rate was mostly increasing after early 2014, indicating a movement towards the normalization of U.S. monetary policy. Thus, it seems not unreasonable to assume that the shadow rate can describe the monetary policy stance of the Fed over the last decade as discussed by Krippner (2013) and Wu and Xia (2016). Therefore, I will use the shadow rate as a single policy instrument for my analysis in the next section.

Another issue for the VAR analysis is how to identify monetary policy shocks. In this study, following Hesse et al. (2018), I use a combination of zero and sign restrictions to identify monetary policy shocks. More specifically, I assume that outputs and prices do not respond contemporaneously to any shocks, including monetary policy shocks, other than aggregate demand and supply shocks. This is a classical assumption used in the block-recursive identification of Christiano et al. (1999) and not unreasonable given the sticky nature of output and prices. In addition, I assume that a contractionary monetary policy shock increases the shadow rate and long-term bond yield and reduces real stock prices.[5] My identification assumptions are essentially the same as those of Hesse et al. (2018) and similar to one of the identification schemes of Weale and Wieladek (2016). All sign restrictions are imposed upon impact and 1 month thereafter.

4.3.2 MSVAR model

The U.S. unconventional monetary policies have evolved greatly over the last decade, including the introduction of the LSAP and the start of monetary policy normalization. Therefore, it is important to consider possible regime changes to assess the effects of the unconventional monetary policies. To this end, I incorporate Markov-switching into the baseline model, following Sims and Zha (2006), among others.

My baseline VAR model (4.1) is extended as:

$$\mathbf{Y}_t = \boldsymbol{\alpha}(s_t) + \sum_{k=1}^{L} \mathbf{A}_k(s_t)\mathbf{Y}_{t-k} + \boldsymbol{\varepsilon}_t, \boldsymbol{\varepsilon}_t \sim \text{iid } N(\mathbf{0}, \boldsymbol{\Sigma}(s_t)) \tag{4.2}$$

where s_t is a latent variable that takes a value from 1, 2, $\ldots$, K, with K being the number of regimes. In other words, this model allows me to specify different VAR models for different regimes.

The Markov chain is a simple model that describes the dynamics of a discrete random variable. Hamilton (1989) proposes modeling the stochastic process of s_t using a Markov chain. The law of regime evolution is governed by the transition probability

[5] While I follow the approach used in Hara et al. (2020), I note that the identified LSAP shock might also reflect a forward guidance shock which may have qualitatively the same effects. It is hard to separately identify the LSAP and forward guidance shocks because they tend to be simultaneously implemented. This approach implicitly assumes that a forward guidance shock is contained in a shock to the long-term interest rate.

matrix $\mathbf{P}$, where the (i, j) element of $\mathbf{P}$, p_{ij}, indicates $Pr[s_t = i|s_{t-1} = j]$. The expected duration of each regime and inferences about s_t can be calculated based on this matrix. Although the Markov chain is a simple model, it can describe various patterns of regime transitions. Specifically, I can capture permanent structural changes by imposing zero restrictions on the elements of matrix $\mathbf{P}$. Below is an example of a transition matrix of a three-regime model with absorbing regimes:

$$\mathbf{P} = \begin{bmatrix} p_{11} & 0 & 0 \\ 1 - p_{11} & p_{22} & 0 \\ 0 & 1 - p_{22} & 1 \end{bmatrix}. \tag{4.3}$$

With this transition probability matrix, the regime dynamics are assumed to start from Regime 1. The regime can shift from Regime 1 to Regime 2 but not to Regime 3, due to the restriction that $p_{31} = 0$. Once the regime moves from Regime 1 to Regime 2, it can shift to Regime 3 but it will never return to Regime 1 because $p_{12} = 0$. Finally, once the model reaches Regime 3, it will stay in Regime 3 for the remainder of the sample period, since the zero restrictions on p_{13} and p_{23} prevent a regime change from Regime 3 to Regime 1 or Regime 2. Therefore, by imposing restrictions on $\mathbf{P}$ in this manner, I can model two permanent structural changes within the sample period.

I will use the transition matrix (4.3) for the three-regime model in Section 4.4. The restrictions are reasonable for my purpose of studying the changes in the effects of the U.S. unconventional monetary policies, corresponding to their evolution over the last decade. In other words, I assume that there are permanent regime shifts along with changes in the U.S. unconventional monetary policies employed.[6]

As will be discussed in the next section, Regimes 1 and 2 are identified with the periods corresponding to the early and late stages of the LSAPs and Regime 3 with the

[6] Uncertainty measures seem to support this assumption originally employed by Hara et al. (2020). For example, the VIX had been stable until shortly before the onset of the global financial crisis in the late 2000s. This suggests that agents might recognize risk of a financial crisis only shortly before the crisis actually occurs. Since agents seem to not presume a future crisis in making decisions, my model does not assume the economy going back to the previous crisis state in the future.

period corresponding to the normalization of monetary policy. Note that this does not necessarily mean that the policy effects have to be very different in each regime, since I do not impose any restrictions on the VAR parameters and they could be similar across regimes. I will examine the differences in the policy effects systematically based on the impulse response functions in the following two sections.

MSVAR models are most commonly estimated by the Bayesian Markov chain Monte Carlo (MCMC) approach, or more specifically a Gibbs sampler, which is what I employ in this study. In Bayesian statistics, parameters are random variables and data are fixed, while parameters are fixed and data are random variables in frequentist statistics. In a Bayesian setup, we set a prior distribution for parameters, and estimate them using the posterior distribution. Applying the MCMC to MSVAR models enables us to sample regime change points and impulse response functions as well as the parameters from the posterior distribution. The samples of regime change points and impulse response functions can be used for the calculation of posterior probabilities of regime shifts and credible intervals of impulse response functions taking account of parameter uncertainty.[7] Appendix of this chapter provides details on the estimation procedure including the prior and full conditional posterior distributions of the VAR parameters and the multi-move sampler for sampling the latent variables $(s_1, \ldots, s_T)$ jointly from the full conditional posterior distribution.

4.4 Results with the Shadow Rate as a Policy Measure

In this and the following section, I discuss my empirical results. In this section, I use the entire sample to observe possible changes in the effects of monetary policy over the last decade. For this purpose, I use the shadow rate as a single monetary policy measure. Based on the results of this section, I will analyze each regime more carefully using more direct monetary policy measures in the following section.

[7] As a reason for employing the MCMC, Hara et al. (2020) address practical concerns about the use of maximum likelihood estimation for a model with many parameters.

4.4.1 Data

My empirical analysis is based on monthly data of real GDP, the CPI, the shadow rate, the U.S. 10-year government bond yield and real stock prices, with the sample period lasting from January 2009 to September 2018. Monthly real GDP data were obtained from Macroeconomic Advisors, while the shadow rate was taken from Wu's website. I also obtained 12-month FF futures data from Bloomberg. Other data were downloaded from the Federal Reserve Economic Data database.

The cumulative asset purchases announced series was constructed in a similar manner to Weale and Wieladek (2016) and Hesse et al. (2018). Specifically, I calculated the cumulative sum of asset purchase announcements by adding up the Fed's announced purchases of Treasuries, mortgage-backed securities and agency debt under the three LSAPs (LSAP1, LSAP2, and LSAP3). In addition, I included asset purchases associated with the maturity extension program. Finally, I divided the cumulative sum of asset purchase announced by the nominal GDP of the previous quarter to mitigate the possible endogeneity problem.[8]

4.4.2 Results of the two-regime model

I start by estimating the two-regime MSVAR model, assuming one permanent structural change. I use 30,000 Gibbs iterations discarding the first 20,000 as burn-in. To examine whether introducing the regime switching improves the model or not, I compare the values of the Bayesian Information Criterion (BIC) between the VAR and two-regime MSVAR models. The BIC of each model is 3.83 and 3.54, meaning that the two-regime MSVAR model is a better model.

Figure 4.2 plots the posterior probabilities of Regime 2.[9] As can be seen, the two-regime model detects a structural change around July 2013, immediately after speeches by former Fed Chair Ben Bernanke about a possible end to QE in the United States on

[8] For this calculation, I used monthly nominal GDP obtained by linearly interpolating quarterly nominal GDP.

[9] See the Appendix for details on computation of the posterior probabilities.

May 22 and June 19, 2013. At that time, financial markets appeared not to be prepared for the tapering of QE. As a consequence, the U.S. Treasury yields surged and more than two trillion dollars was lost on international stock markets within 4 weeks, an event which is sometimes called the taper tantrum. In other words, my results suggest that after the taper tantrum, the economic regime might have shifted to a new regime as the market became aware of the possible termination of massive monetary easing.[10]

In order to see the change in the effects of monetary policy that occurred along with this regime change, Figure 4.3 plots the impulse responses of each variable to a contractionary monetary policy shock, defined as a one-standard-deviation increase in the shadow rate along with 68% Bayesian credible intervals for each regime.[11] The results of Regime 1, shown in the first column of the figure, indicate fairly standard responses of each variable to a contractionary monetary policy shock. Specifically, a contractionary monetary policy shock significantly reduces real output and prices and its effects are relatively persistent. On the other hand, as can be seen from the second column of Figure 4.3, the impacts of the same shock on the real economy and prices during Regime 2 are somewhat weaker and less persistent. In other words, my results suggest that the normalization of U.S. monetary policy has had only marginal effects on the real economy and inflation.

To confirm convergence of the Markov chain, I use the Geweke (1992) convergence diagnostic.[12] This diagnostic for a model parameter tests equality of means of the first

[10] While it is not explicitly addressed in Hara et al. (2020), I note that the timing of the detected change is much earlier than that of the actual liftoff in the late 2015. The shadow rate can reflect a substantial shift in expectations on future monetary policy actions triggered by the Bernanke's speech, which may not be incorporated in the actual FF rate.

[11] See the Appendix for details on computation of the impulse responses and credible intervals.

[12] The convergence diagnostics and basic statistics for selected parameters in this chapter are supplemental analyses for Hara et al. (2020).

n_A and last n_B draws from the corresponding posterior distribution.[13,14] If the sequence of the MCMC sampling is stationary, this statistic has an asymptotically standard normal distribution. I use $n_A = 0.1N$ and $n_B = 0.5N$ as suggested by Geweke (1992), where $N = 10,000$.[15] To consider autocorrelation across draws, I compute the standard errors of $\bar{\theta}_A$ and $\bar{\theta}_B$ using a Parzen window with the bandwidth of $0.1n_A$ and $0.1n_B$ respectively, following Nakajima et al. (2011). Panel (a) of Table 4.1 presents the estimates for posterior means, standard deviations, 95% Bayesian credible intervals, and the Geweke's convergence diagnostics for selected parameters in the model.[16] The convergence diagnostic for the transition probability p_{11} is 1.356, suggesting that the null hypothesis of convergence is not rejected, and the statistics for the other parameters are not significant at the 1% significance level. These results confirm convergence of the Markov chain in my estimation with the burn-in period.

4.4.3 Results of the three-regime model

There is controversy about whether the effects of LSAPs have declined or not. For example, while Weale and Wieladek (2016) confirm that including LSAP1 or not does not change the effectiveness of LSAPs, Hesse et al. (2018) suggest that the effects of the later LSAP seem to be weaker. Therefore, it might be instructive to consider multiple regime shifts to examine a possible change in the effects of the LSAPs. In addition, the actual FF rate hikes started in December 2015, which might have shifted the economy to a new regime, giving us another motivation to accommodate multiple regime

[13] Let $\theta^{(n)}$ denote the n-th draw of a parameter to be tested. The means of the first n_A and last n_B draws are expressed as $\bar{\theta}_A = \frac{1}{n_A}\sum_{n=1}^{n_A}\theta^{(n)}$ and $\bar{\theta}_B = \frac{1}{n_B}\sum_{n=N-n_B+1}^{N}\theta^{(n)}$, where N is the number of total draws discarding the burn-in samples. The Geweke's statistic for θ is defined as $(\bar{\theta}_A - \bar{\theta}_B)/\sqrt{[SE(\bar{\theta}_A)]^2 + [SE(\bar{\theta}_B)]^2}$, where $SE(\bar{\theta}_A)$ and $SE(\bar{\theta}_B)$ denote the standard errors of $\bar{\theta}_A$ and $\bar{\theta}_B$, respectively.

[14] I note that an insignificant Geweke's diagnostic is a necessary but not a sufficient condition for convergence of the Markov chain.

[15] I place an interval of $0.4N$ between the two groups to assure independence between them.

[16] To compute a 95% credible interval for a parameter, I sort draws of the parameter after the burn-in period in ascending order and take the 2.5 and 97.5 percentiles.

changes. To see when an additional regime shift can be observed and examine the possible changes in the effects of unconventional monetary policies along with regime changes, I report the results of the three-regime MSVAR model in this subsection.

At first, I compare the BIC to see an additional regime shift can give a better description of the data. The BIC of the three regime model turns out to be 2.62, indicating the three-regime model is the best model among three models with the smallest BIC value.

Figure 4.4 plots the posterior probabilities of Regimes 2 (solid line) and 3 (broken line). The results indicate an additional regime shift around the beginning of 2011 in addition to a break in July 2013. Thus, the three-regime model detects some changes during the LSAPs rather than during the normalization period after the middle of 2013. I note that the beginning of the second regime is fairly close to the start of the second subsample of Hesse et al. (2018). To assess convergence of the Markov chain in the three-regime model, Panel (b) of Table 4.1 presents the estimates for the convergence diagnostics and several key statistics for selected parameters in the model. The statistics for the most parameters are not significant at the 1% significance level, suggesting that the burn-in period is sufficiently long for convergence of the Markov chain.[17]

To visualize the variations in the effects of unconventional monetary policies that occur along with these regime changes, Figure 4.5 summarizes the impulse responses of each variable to a contractionary monetary policy shock for each regime. The impulse responses of Regime 1, shown in the first column of the figure, are standard, persistently depressing real output and prices. On the other hand, as shown in the second column, the same shock in Regime 2 seems to have a larger impact on output and a similar impact on prices in the short-run, but the effects disappear quickly, within 6 months. Thus, my findings are consistent with those of Weale and Wieladek (2016) in the short-run and similar to those of Hesse et al. (2018) in the long run. Finally, and not surprisingly, the responses of Regime 3, shown in the last column, are essentially the

[17] Only an exception is the variance of residuals of the equation for output in Regime 1 labelled $\sigma_{y,1}$ with the statistic of -3.646.

same as those of Regime 2 of the two-regime model.

4.5 Analysis of Each Regime with More Relevant Policy Measures

The results of the previous section provide clear evidence of a regime shift around the middle of 2013. In this section, I conduct further analyses of each regime. More specifically, given the results of the previous section, I divide the sample into two subsamples: January 2009 to June 2013 (LSAP regime) and September 2013 to September 2018 (normalization regime).[18] One advantage in considering subsamples separately is that I can use more relevant measures of monetary policy for each regime instead of the shadow rate, enabling a more precise assessment of the effects of the U.S. unconventional monetary policies based on similar VAR models, but with more appropriate policy measures.

4.5.1 Results of LSAP regime

I start by estimating the two-regime MSVAR model using the first subsample, since the results of the three-regime MSVAR model detected a regime shift around the beginning of 2011. For this period, the asset purchases can be considered as the unique active monetary policy instrument, since the FF rate was essentially at the lower bound with little fluctuation during the entire period. More specifically, following Weale and Wieladek (2016) and Hesse et al. (2018), I use the cumulative amount of asset purchases announced divided by the nominal GDP of the previous quarter as the policy instrument.

Figure 4.6 plots the posterior probabilities of Regime 2, showing a similar regime shift around the beginning of 2011 to the one found in the previous section. Thus, there may have been some change in the propagation mechanism of monetary policy

[18] The second subsample starts from September 2013, since I use the first 2 months of data for the lags of the VAR model.

shocks around this period.[19] To investigate this point more closely, Figure 4.7 depicts the impulse responses along with 68% credible intervals of each regime. As can be seen, the impulse responses of Regime 1 shown in the first column exhibit the proper responses to monetary policy shocks, while those of Regime 2 in the second column suggest that the effects of monetary policy shocks became weaker and less persistent. These findings are partially consistent with those of Weale and Wieladek (2016) in the short-run and similar to those of Hesse et al. (2018) in the long-run.

These findings might reflect the state-dependence of the effectiveness of monetary policy. Berger and Vavra (2015) prove that expenditures on durable goods are less responsive to economic shocks during recessions in the presence of adjustment costs on durable goods consumption. This implies that durable expenditures respond to shocks more in an economic recovery. Against that background, I examine the state-dependence of policy effectiveness potentially arising from households' durable consumption adjustments. To this end, I replace GDP with nondurable consumption, service consumption, durable consumption and capital goods new orders, and estimate the same two-regime MSVAR model.[20]

The impulse response of each variable for the first and second regimes is illustrated in the first and second columns of Figure 4.8, respectively. As can be seen from the first column, each GDP component responds significantly and persistently negatively to a monetary policy shock in Regime 1. In contrast, a monetary policy shock in Regime 2 has smaller and less persistent effects on each GDP component, with the exception of the effect on durable consumption, which is larger but still less persistent in Regime 2 than in Regime 1. In other words, my results indicate that the monetary policy effects in Regime 2 seem to be smaller for a wide range of consumption types. The larger but less persistent responsiveness of durable consumption might imply the state-dependence of monetary policy effectiveness arising from adjustment costs on durable goods con-

[19] I confirm that the Geweke's convergence diagnostics for the most parameters in the model are not significant at the 1% significance level.

[20] More specifically, I use the manufacturers' new orders for non-defense capital goods excluding aircraft.

sumption.

4.5.2 Results of normalization regime

In this subsection, I document the results of the second subsample from September 2013 to September 2018. In this regime, there are arguably two active policy instruments: the Fed's asset purchases, or balance sheet, and the FF rate. Therefore, I extend the benchmark 5-variate VAR model to 6-variate VAR model to incorporate both monetary policy measures. For the balance sheet, I use the Fed's total assets divided by the nominal GDP of the previous quarter. This policy instrument is slightly different from the cumulative amount of asset purchases announced used in the previous subsection, as there were not many announcements of changes in the Fed's balance sheet during this regime. For the FF rate, I use the implied FF rate based on 12-month FF futures price data, since the actual FF rate exhibited little fluctuation before the Fed started raising the FF rate in December 2015. To identify a monetary shock for each measure, I use the same sign restrictions as above. More specifically, I assume that a contractionary balance sheet policy shock reduces the Fed's total assets and real stock prices and increases long-term bond yields, while the interest rate policy shock increases the FF rates and long-term bond yields and reduces real stock prices.

Figure 4.9 summarizes the impulse responses of each variable to each contractionary monetary policy shock based on the 6-variate VAR model.[21] Specifically, the first column plots the impulse responses of each variable to an unexpected decrease in the size of the Fed's balance sheet. The results indicate that real output responds significantly negatively to quantitative tightening. Although the effects are slightly smaller than those of Regime 1 of the LSAP regime, as can be seen in Figure 4.7, they are still larger and more persistent than those of Regime 2 of the LSAP regime. In contrast, the second column of Figure 4.9 suggests that neither real output nor the price level shows a strong response to an unexpected increase in the FF rate. In other words, my results

[21] The Geweke's statistics for all of the selected parameters are not significant at the 1% significance level, suggesting convergence of the Markov chain.

indicate that during the normalization regime FF rate hikes had only marginal effects.

As for the size of the balance sheet, Bullard (2019) points out that unanticipated announcements of a balance sheet reduction during 2017 seem to have had a smaller economic impact than earlier balance sheet expansions. Based on this fact, he concludes that the signaling effects from the asset purchases suggested by Bauer and Rudebusch (2014) can induce asymmetric effects between an expansion and a reduction of the central bank's balance sheet. My findings suggest that a decline in the signaling effect might be less obvious if I look at the whole normalization period after the taper tantrum triggered by Bernanke's speeches in 2013. Regarding the policy rates, the results are consistent with Lo and Piger (2005), who find that policy shocks are more stimulative in recessions.

To obtain some insight into the possible reasons for the differences in the effects of monetary tightening between the two instruments, I conduct a similar exercise to that performed earlier and replace GDP with nondurable consumption, service consumption, durable consumption and capital goods new orders, and estimate the same 6-variate VAR model with two monetary policy instruments. Figure 4.10 plots the impulse responses of each variable for each instrument. As can be seen, regardless of the policy measure, a monetary policy tightening shock significantly reduces nondurable and service consumption in the long-run, although the effects are not statistically significant. In addition, both measures significantly dampen capital goods new orders in the short-run. On the other hand, durable consumption responds significantly positively to an FF rate shock in the short-run, raising durable consumption even in the long-run, although the positive responses are insignificant in the long-run. One possible explanation for these unusual responses is that in the normalization regime the tightening of monetary policy produces an expectation of further monetary policy tightening with future interest rate hikes, inducing consumers to buy durables before the interest rate rise occurs. In fact, in January 2012 the Fed publicly started to report a "dot plot," which shows the Federal Open Market Committee (FOMC) member's projections for future interest rates in subsequent years and in the longer run, making it easier to predict the future interest rate hikes. For example, as of March 2014, FOMC participants largely anticipated several interest rates hikes both in 2015 and 2016, leading the median target federal funds rate to 2.25% by the end of 2016. Since the dot plots usually show diversified projections, particularly in the monetary normalization regime, an interest rate hike could reduce the uncertainty about the future path of the FF rates, yielding stronger anticipation of future interest rate hikes. As a consequence, in the case of FF rate shocks, the positive effects on durables offset the negative effects on other goods, rendering the responses on GDP insignificant.

What do the findings of this study imply for the conduct of monetary policy going forward? The empirical results indicate that unexpected expansions or reductions in the size of the Fed's balance sheet had relatively clear macroeconomic effects not just in the early stage of the LSAP but also in the period from September 2013 to September 2018, including the period of monetary policy normalization. Given that the present economic structure remains essentially unchanged, this suggests that the balance sheet policy is likely to remain in the policy toolkit for the Fed to use in response to future economic downturns, as Yellen (2016) anticipates. My results also support an argument by Kiley (2018) that QE can play a useful role in offsetting the adverse effects of the effective lower bound when the equilibrium real interest rate is low. In sum, the findings of this study generally support the case for an active balance sheet policy, at least as a secondary tool in ordinary times.[22]

4.6 Conclusion

Over the last decade, U.S. monetary policy has evolved significantly. In response to the GFC, the Fed lowered the policy rate to the effective lower bound, introduced the LSAP in December 2008 and expanded the LSAP on two occasions. With the help of these policy initiatives, the U.S. economy recovered and grew steadily, causing former

[22] This view seems consistent with a recent announcement by the Fed. In January 2019 Fed Chair Jerome Powell announced publicly at the American Economic Association Meetings that the Fed would not hesitate to make changes to the balance sheet reduction plan if necessary.

Fed Chair Ben Bernanke to suggest a possible tapering of the LSAPs and the beginning of monetary policy normalization in May and June 2013, which induced the taper tantrum. The Fed eventually ended the LSAPs in December 2014 and started raising the FF rate in December 2015.

Against this background, this study empirically assessed the effects of the U.S. unconventional monetary policies over the last decade. Given the evolution of the U.S. unconventional monetary policies, it is crucial to consider possible regime shifts. To this end, I estimated a MSVAR model by incorporating Markov-switching into the benchmark VAR model based on Weale and Wieladek (2016) and Hesse et al. (2018). To overcome the problem that there may be no single monetary policy measure during this period, I adopted the shadow rate of Wu and Xia (2016) as an appropriate monetary policy indicator over the last decade.

My estimation of the MSVAR model detected a regime shift around the middle of 2013, immediately after the taper tantrum triggered by Bernanke's speeches. In other words, my results demonstrate that there were two distinct regimes over the last decade of U.S. monetary policy: the LSAP regime before the middle of 2013 and the monetary normalization regime after the middle of 2013. In addition, the three-regime MSVAR model detected an additional regime change around the beginning of 2011, suggesting a possible change in the effects of the LSAPs.

I further investigated the details of each regime with relevant policy measures based on subsamples before and after the middle of 2013. My analysis indicated that in the early stage of LSAPs, that is, before 2011, the U.S. LSAPs had relatively large impacts on the real economy and prices, but their effects during the late stage of the LSAPs were weaker and less persistent. My results also suggest that the effects of the asset purchase or balance sheet shocks were slightly weaker during the monetary normalization regime after the middle of 2013 than during the early stage of the LSAPs, but stronger than during the late stage of the LSAPs. In contrast, the real economy and prices showed no significant responses to interest rate shocks. Additional analysis using GDP components demonstrated that negative responses of nondurable and service consumption and capital goods expenditure to interest rate shocks were somewhat offset by the positive impacts of the interest rate hike on durable consumptions after the middle of 2013. These findings seem to support the view that LSAPs will retain a useful role for central banks to use in responding to future economic downturns, at least as a secondary tool.

Figure 4.1: Cumulative Sum of Asset Purchases Announced, Federal Funds Rate, and the Wu-Xia Shadow Rate from January 2009 to September 2018

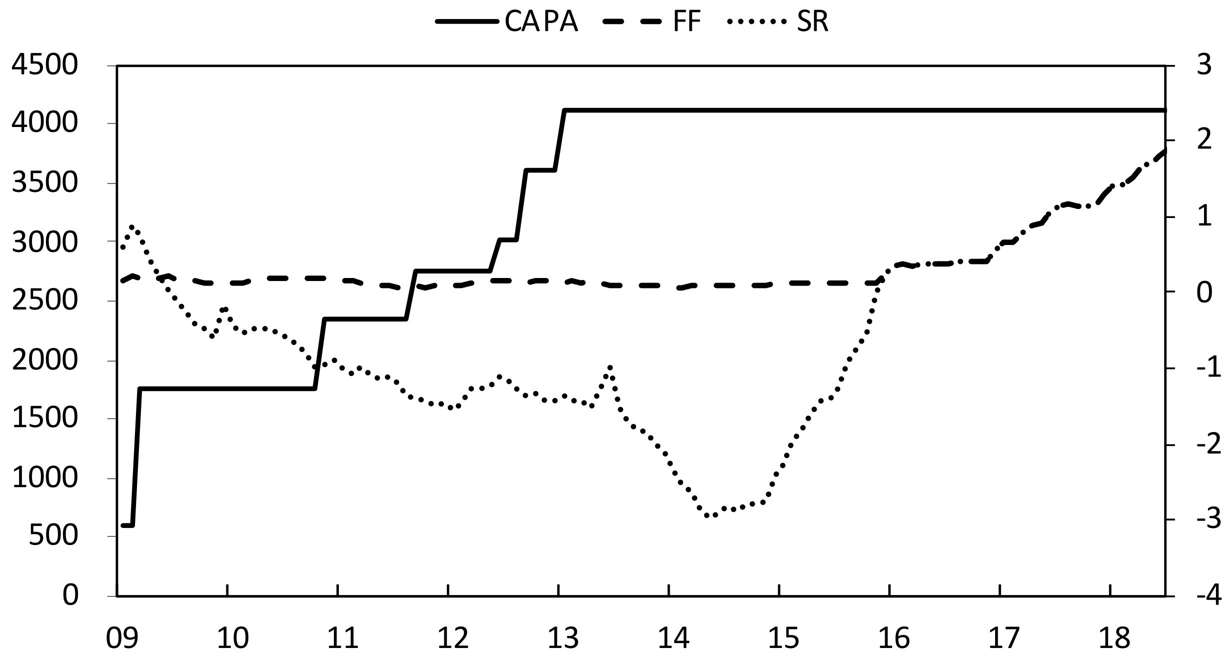
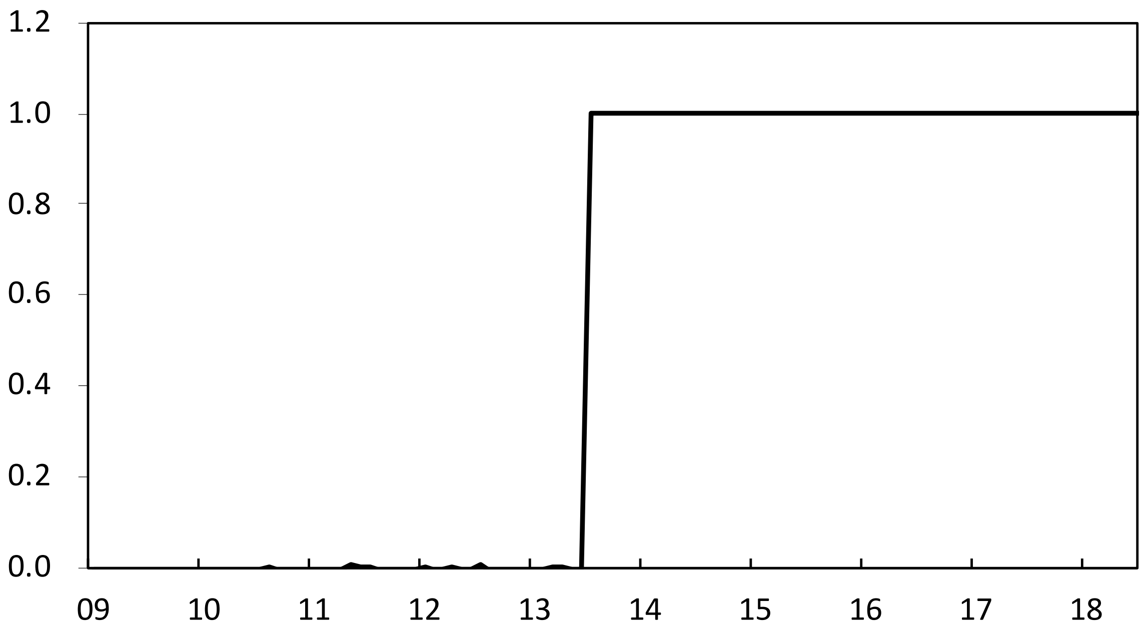
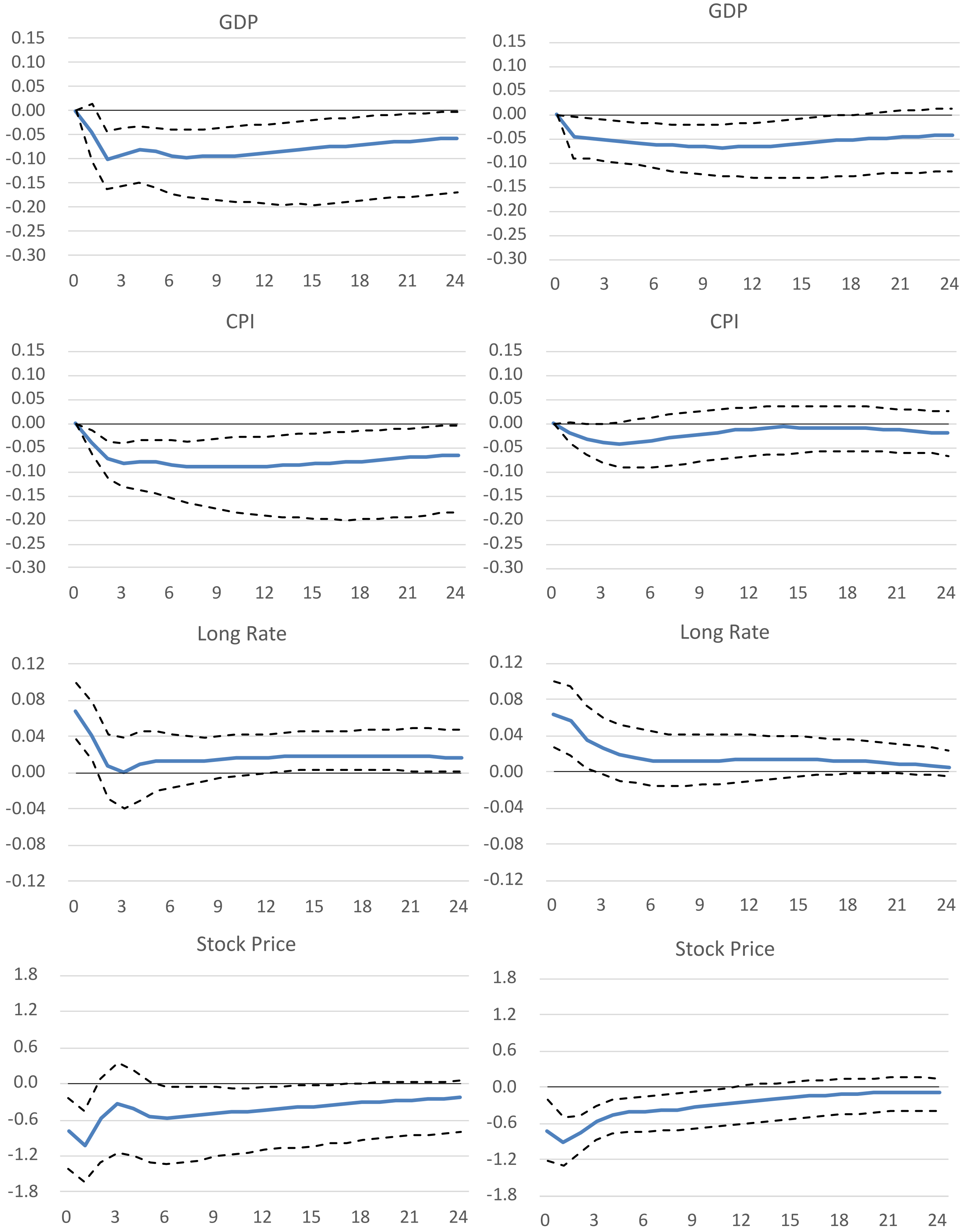


Figure 4.2: Posterior Probabilities of Regime 2 from Equation (4.2) with $K = 2$



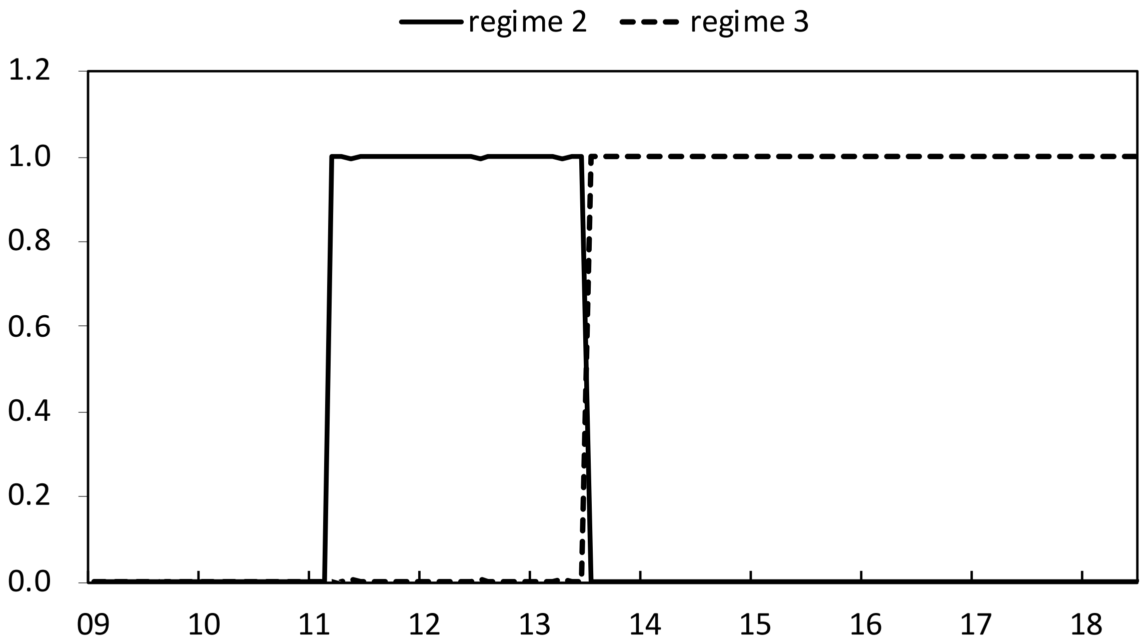
Note: The sample period is January 2009 to September 2018.

Figure 4.3: Impulse Responses of GDP, the CPI, the Long-Term Interest Rate and Stock Prices to a Contractionary Monetary Policy Shock under Regimes 1 (Left) and 2 (Right)



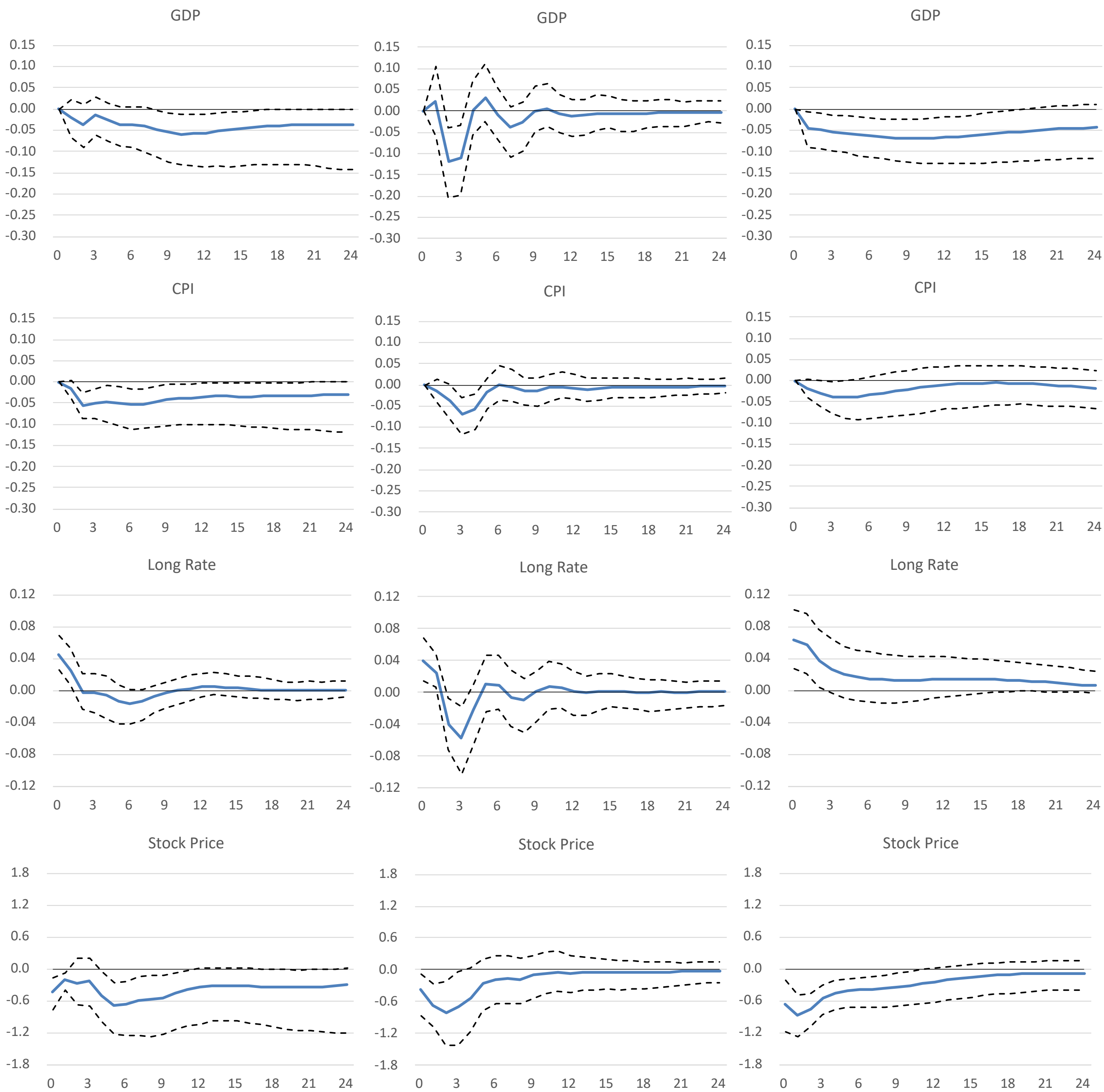
Note: The results are obtained from estimating Equation (4.2) with $K = 2$. The sample period is January 2009 to September 2018. All of the responses displayed are to a one standard deviation shock. Dashed lines represent the 68% credible intervals of the impulse responses.

Figure 4.4: Posterior Probabilities of Regimes 2 and 3 from Equation (4.2) with $K = 3$



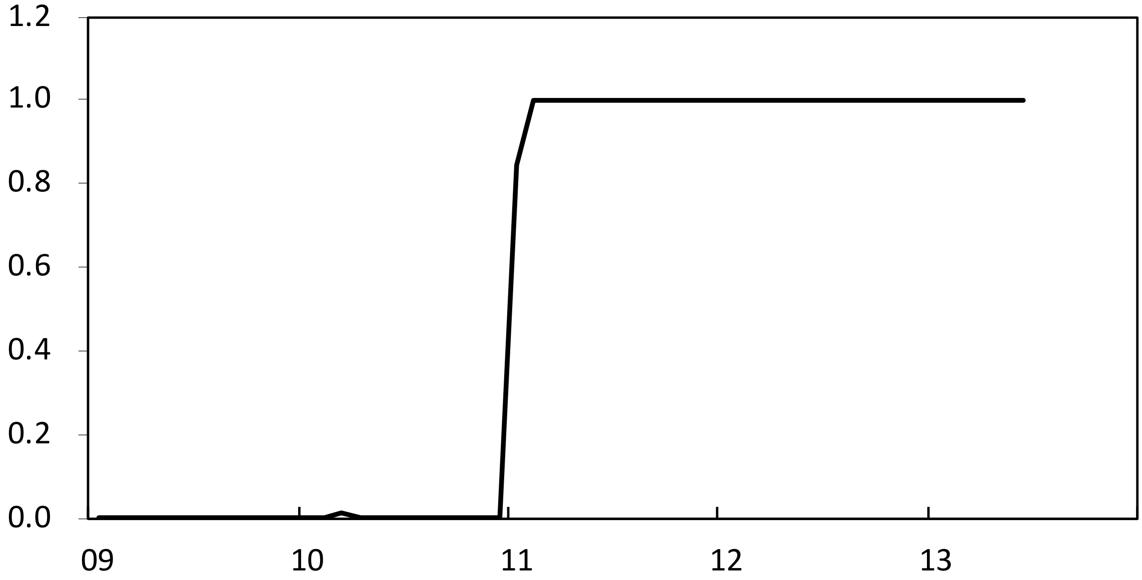
Note: The sample period is January 2009 to September 2018.

Figure 4.5: Impulse Responses of GDP, the CPI, the Long-Term Interest Rate and Stock Prices to a Contractionary Monetary Policy Shock under Regimes 1 (Left), 2 (Center), and 3 (Right)



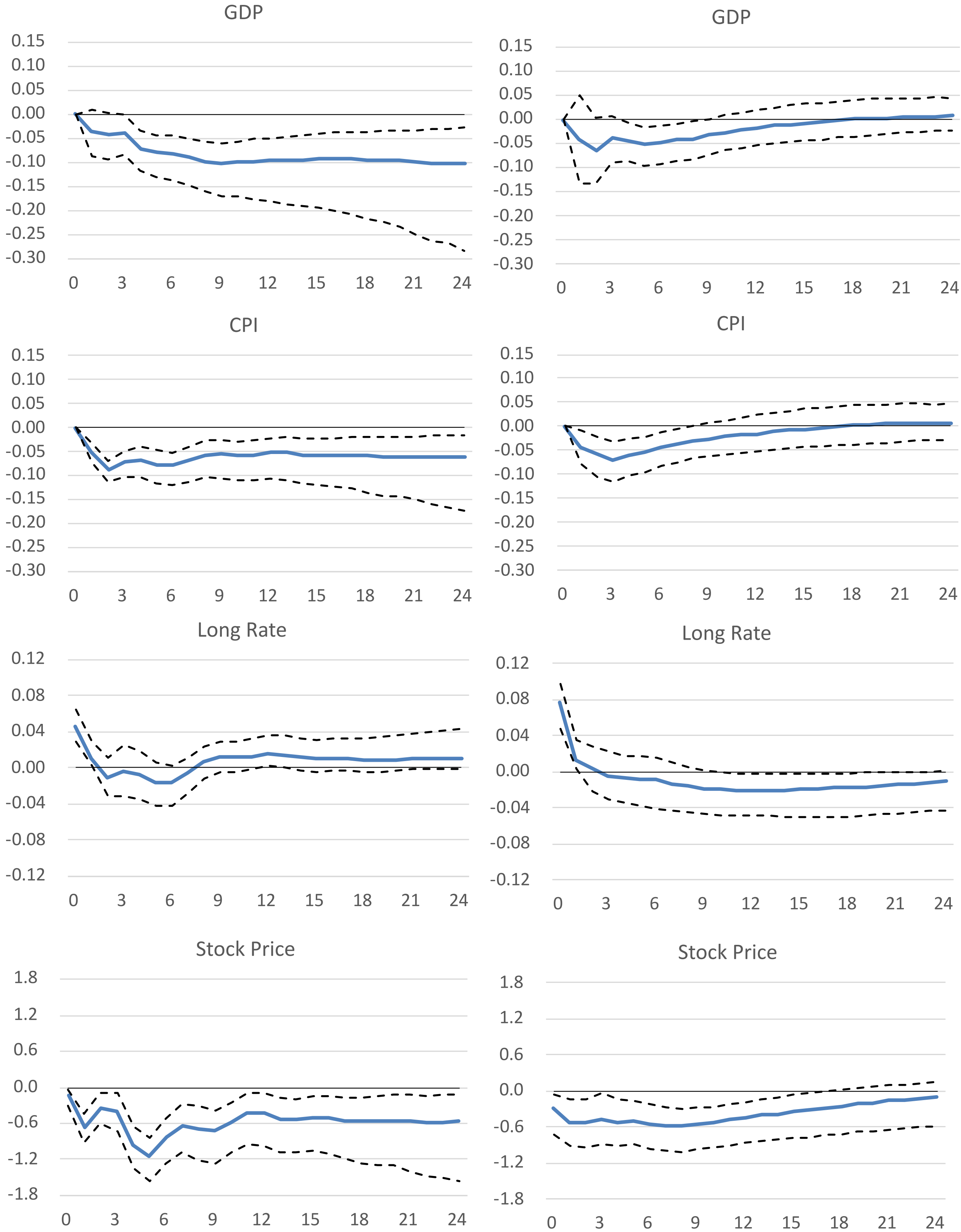
Note: The results are obtained from estimating Equation (4.2) with $K = 3$. The sample period is January 2009 to September 2018. All of the responses displayed are to a one standard deviation shock. Dashed lines represent the 68% credible intervals of the impulse responses.

Figure 4.6: Posterior Probabilities of Regime 2 from Equation (4.2) with $K = 2$



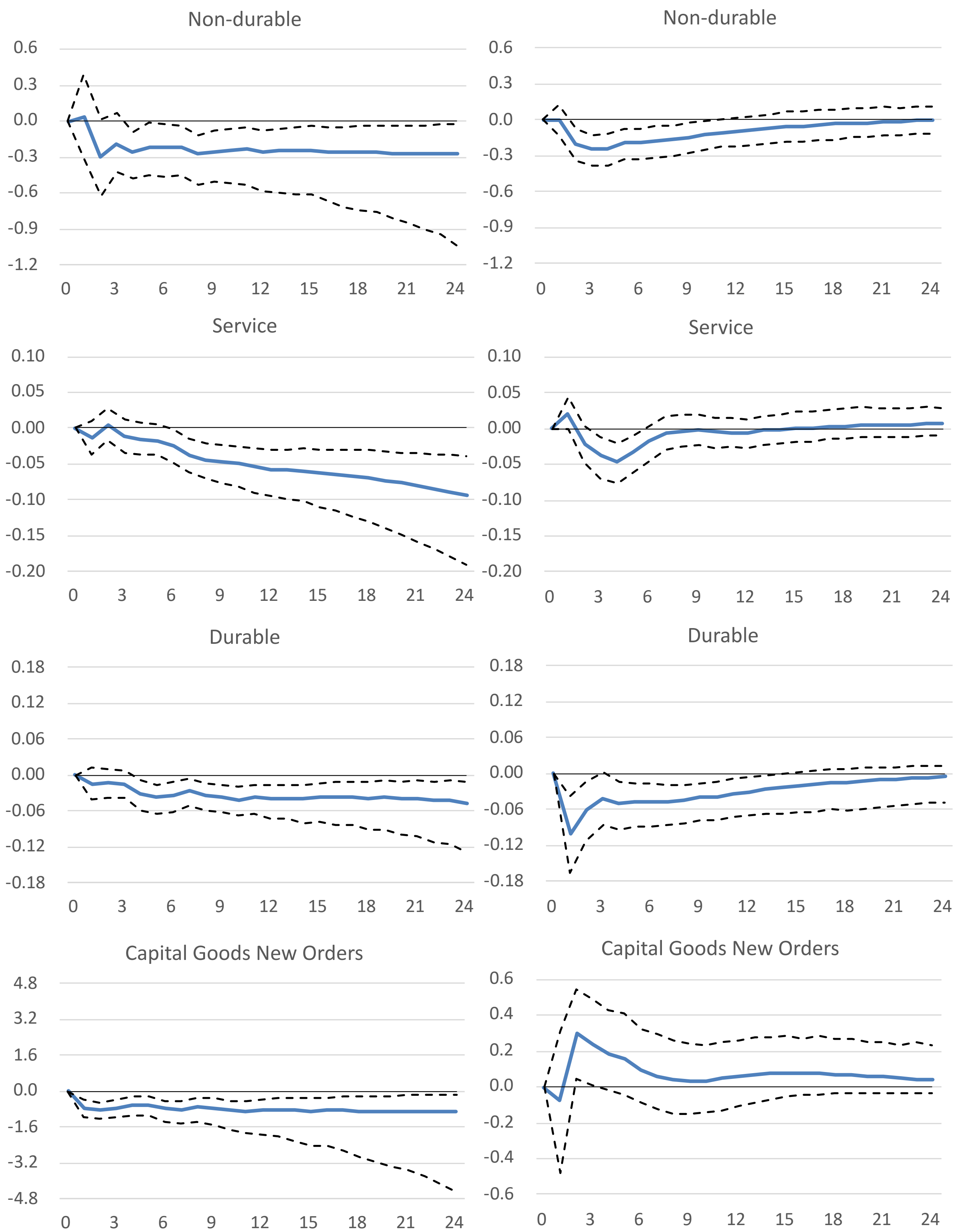
Note: The sample period is January 2009 to June 2013.

Figure 4.7: Impulse Responses of GDP, the CPI, the Long-Term Interest Rate and Stock Prices to a Contractionary Monetary Policy Shock under Regimes 1 (Left) and 2 (Right)



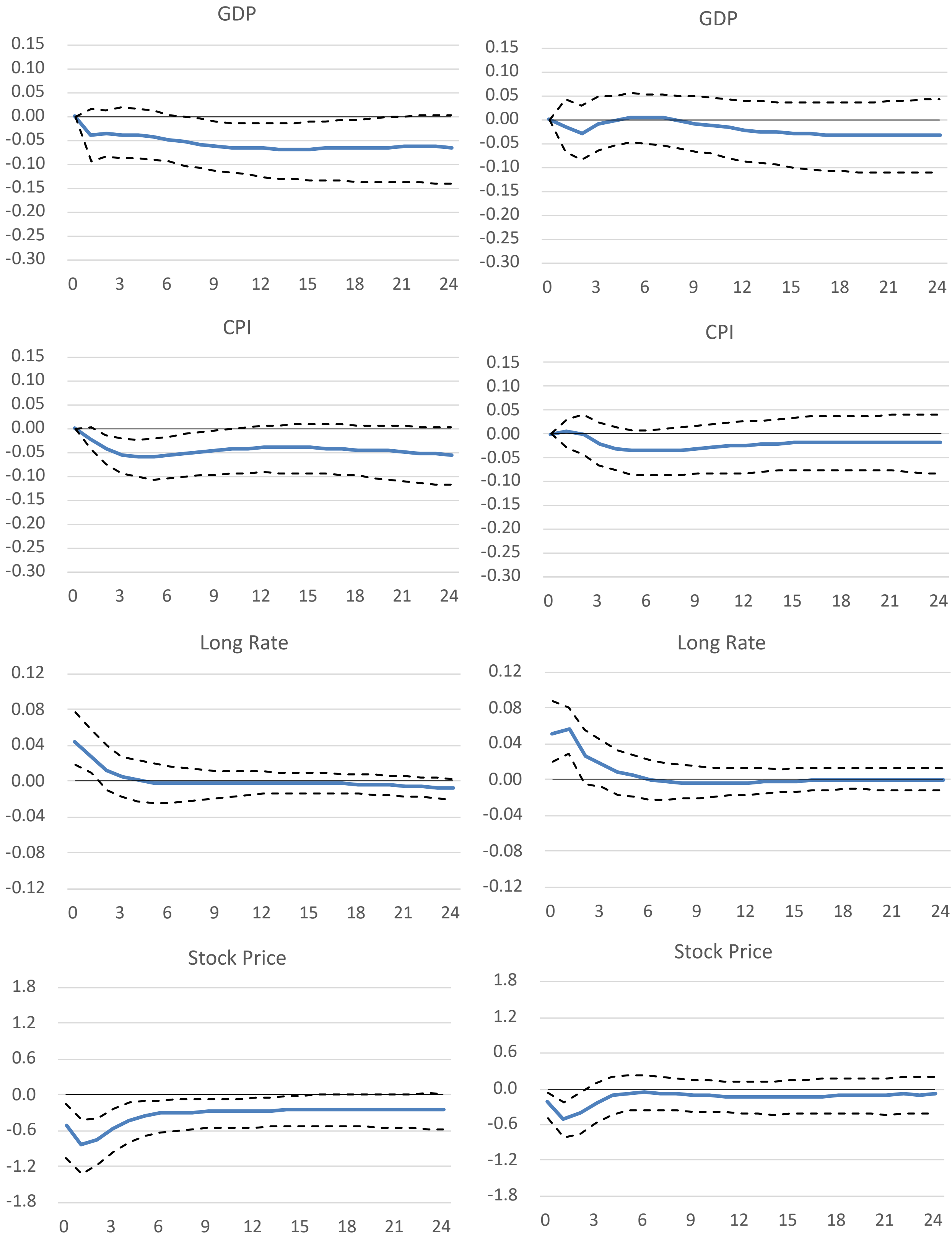
Note: The results are obtained from estimating Equation (4.2) with $K = 2$. The sample period is January 2009 to June 2013. All of the responses displayed are to a one standard deviation shock. Dashed lines represent the 68% credible intervals of the impulse responses.

Figure 4.8: Impulse Responses of Nondurable Consumption, Service Consumption, Durable Consumption and Capital Goods New Orders to a Contractionary Monetary Policy Shock under Regimes 1 (Left) and 2 (Right)



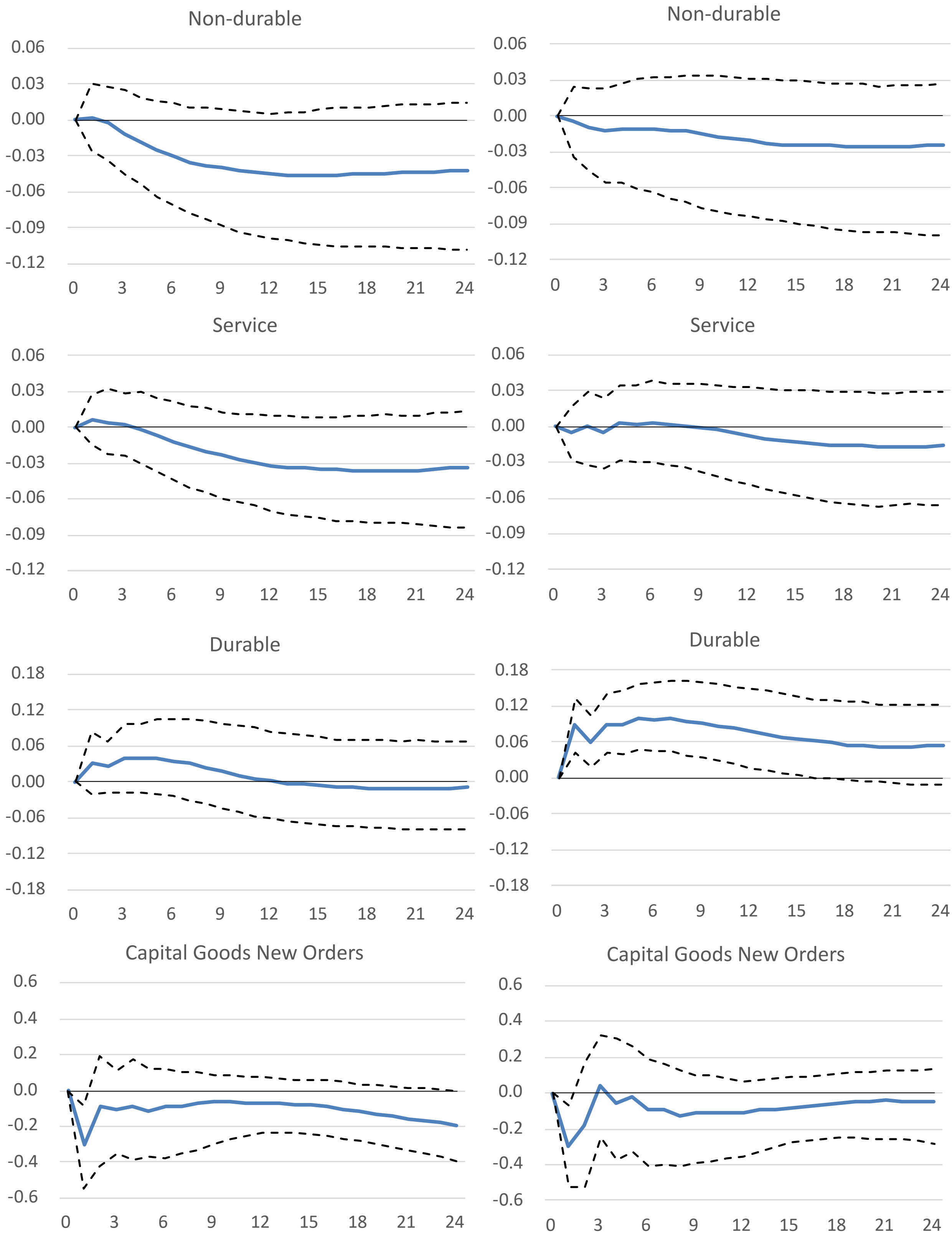
Note: The results are obtained from estimating Equation (4.2) with $K = 2$. The sample period is January 2009 to June 2013. All of the responses displayed are to a one standard deviation shock. Dashed lines represent the 68% credible intervals of the impulse responses.

Figure 4.9: Impulse Responses of GDP, the CPI, the Long-Term Interest Rate and Stock Prices to an Unexpected Decrease in the Fed's Total Assets (Left) and those to an Unexpected Increase in the FF Rate (Right)



Note: The results are obtained from estimating Equation (4.2) with $K = 1$. The sample period is September 2013 to September 2018. All of the responses displayed are to a one standard deviation shock. Dashed lines represent the 68% credible intervals of the impulse responses.

Figure 4.10: Impulse Responses of Nondurable Consumption, Service Consumption, Durable Consumption and Capital Goods New Orders to an Unexpected Decrease in the Fed's Total Assets (Left) and those to an Unexpected Increase in the FF Rate (Right)



Note: The results are obtained from estimating Equation (4.2) with $K = 1$. The sample period is September 2013 to September 2018. All of the responses displayed are to a one standard deviation shock. Dashed lines represent the 68% credible intervals of the impulse responses.

Table 4.1: Estimation results for selected parameters in the MSVAR models

Parameter	Mean	St. dev.	95 % interval	CD
(a) Two-regime model				
p_{11}	0.965	0.024	[0.906, 0.996]	1.356
$a_{y,1}$	0.327	0.141	[0.050, 0.605]	-1.061
$a_{y,2}$	0.433	0.120	[0.196, 0.673]	-0.858
$a_{p,1}$	-0.456	0.347	[-1.140, 0.218]	0.058
$a_{p,2}$	-0.373	0.263	[-0.887, 0.142]	2.087
$\sigma_{y,1}$	0.232	0.048	[0.156, 0.344]	-0.809
$\sigma_{y,2}$	0.105	0.020	[0.073, 0.150]	-0.533
$\sigma_{p,1}$	0.041	0.009	[0.028, 0.061]	-1.268
$\sigma_{p,2}$	0.026	0.005	[0.018, 0.037]	-0.202
(b) Three-regime model				
p_{11}	0.931	0.047	[0.817, 0.991]	0.707
p_{22}	0.936	0.043	[0.828, 0.992]	-1.423
$a_{y,1}$	0.695	0.197	[0.307, 1.092]	-0.810
$a_{y,2}$	-0.003	0.192	[-0.386, 0.374]	-0.494
$a_{y,3}$	0.436	0.118	[0.203, 0.663]	-1.388
$a_{p,1}$	-0.377	0.681	[-1.729, 0.964]	2.132
$a_{p,2}$	-0.187	0.547	[-1.275, 0.901]	-0.453
$a_{p,3}$	-0.377	0.261	[-0.883, 0.135]	-1.587
$\sigma_{y,1}$	0.172	0.058	[0.092, 0.315]	-3.646
$\sigma_{y,2}$	0.225	0.071	[0.126, 0.397]	-0.119
$\sigma_{y,3}$	0.105	0.020	[0.073, 0.151]	-1.020
$\sigma_{p,1}$	0.023	0.007	[0.012, 0.041]	-0.128
$\sigma_{p,2}$	0.029	0.010	[0.016, 0.053]	-0.941
$\sigma_{p,3}$	0.026	0.005	[0.018, 0.038]	1.048

Note. "CD" denotes the Geweke's convergence diagnostic. $a_{y,j}$ and $a_{p,j}$ denote coefficients on one-period lagged output and price in Regime j, respectively. $\sigma_{y,j}$ is the variance of residuals of the equation for output in Regime j, and $\sigma_{p,j}$ is the variance of residuals of the equation for price in Regime j.

Appendix: Gibbs sampling procedure for two-regime MSVAR models

This appendix describes how the Gibbs sampling procedure is implemented to my two-regime MSVAR model with absorbing regimes.[23]

I start by rewriting the VAR(L) model of Equation (4.2) as:

$$\mathbf{Y}_t = \mathbf{X}_t \boldsymbol{\beta}(s_t) + \boldsymbol{\varepsilon}_t, \boldsymbol{\varepsilon}_t \sim \text{iid } N(\mathbf{0}, \boldsymbol{\Sigma}(s_t)), \tag{A1}$$

where $\mathbf{Y}_t = (y_{1,t}, \ldots, y_{n,t})'$ and $\mathbf{X}_t = \mathbf{I}_n \otimes [1, \mathbf{Y}'_{t-1}, \ldots, \mathbf{Y}'_{t-L}]$. The VAR coefficients are expressed as $\boldsymbol{\beta}(s_t) = \text{vec}([\boldsymbol{\alpha}(s_t), \mathbf{A}_1(s_t), \ldots, \mathbf{A}_L(s_t)]')$. For convenience, I define $\boldsymbol{\beta} = [\boldsymbol{\beta}(1)', \boldsymbol{\beta}(2)']$, $\boldsymbol{\Sigma} = [\text{vech}(\boldsymbol{\Sigma}(1))', \text{vech}(\boldsymbol{\Sigma}(2))']'$, $\widetilde{\boldsymbol{Y}}_T = [\mathbf{Y}_1, \ldots, \mathbf{Y}_T]'$, and $\tilde{s}_T = [s_1, \ldots, s_T]'$, where T is the number of total observations.

Suppose I have run the Gibbs sampler r times. The Gibbs sampling for the $(r+1)$-th iteration takes the following steps:

1. Generate $\tilde{s}_T^{(r+1)}$ conditional on $p_{11}^{(r)}$, $\boldsymbol{\Sigma}^{(r)}$, $\boldsymbol{\beta}^{(r)}$, and $\widetilde{\boldsymbol{Y}}_T$

   I use a multi-move sampler proposed by Kim and Nelson (1998).[24] The full conditional posterior distribution is given by:

$$g(\tilde{s}_T | p_{11}, \boldsymbol{\Sigma}, \boldsymbol{\beta}, \widetilde{\boldsymbol{Y}}_T) = g(s_T | p_{11}, \boldsymbol{\Sigma}, \boldsymbol{\beta}, \widetilde{\boldsymbol{Y}}_T) \prod_{t=1}^{T-1} g(s_t | s_{t+1}, p_{11}, \boldsymbol{\Sigma}, \boldsymbol{\beta}, \widetilde{\boldsymbol{Y}}_t), \tag{A2}$$

   where g denotes a probability distribution function, and $g(s_t | s_{t+1}, p_{11}, \boldsymbol{\Sigma}, \boldsymbol{\beta}, \widetilde{\boldsymbol{Y}}_t)$ is defined as:

$$g(s_t = s | s_{t+1}, p_{11}, \boldsymbol{\Sigma}, \boldsymbol{\beta}, \widetilde{\boldsymbol{Y}}_{t+1}) = \frac{g(s_t = s | p_{11}, \boldsymbol{\Sigma}, \boldsymbol{\beta}, \widetilde{\boldsymbol{Y}}_t) g(s_{t+1} | s_t = s)}{\sum_{i=1}^{2} g(s_t = i | p_{11}, \boldsymbol{\Sigma}, \boldsymbol{\beta}, \widetilde{\boldsymbol{Y}}_t) g(s_{t+1} | s_t = i)}, \tag{A3}$$

   for $s = 1, 2$. I note that $g(s_{t+1}|s_t)$ is the transition probability with a transition

[23] This appendix is an extended version of the appendix in Hara et al. (2020).

[24] Chib (1996) provides detailed descriptions for the procedure.

matrix of:

$$\begin{bmatrix} p_{11} & 0 \\ 1-p_{11} & 1 \end{bmatrix}.$$

To sample $\tilde{s}_T$ from its posterior distribution, I start with $g(s_1 = 1|p_{11}, \mathbf{\Sigma}, \boldsymbol{\beta}, \widetilde{\mathbf{Y}}_1) = 1$ and compute $g(s_t|p_{11}, \mathbf{\Sigma}, \boldsymbol{\beta}, \widetilde{\mathbf{Y}}_t)$ for $t = 2, \ldots, T-1$ by running the Hamilton (1989) filter. Next, starting with $s_T = 2$, I recursively draw s_t from $g(s_t|s_{t+1}, p_{11}, \mathbf{\Sigma}, \boldsymbol{\beta}, \widetilde{\mathbf{Y}}_t)$ for $t = T-1, \ldots, 2$, and set $s_1 = 1$.

2. Generate $p_{11}^{(r+1)}$ conditional on $\tilde{s}_T^{(r+1)}$

   I give a non-informative Beta prior for the transition probability, $p_{11}\sim$beta(u_{11}, u_{12}) where $u_{11} = u_{12} = 1$, which is a uniform distribution. The posterior distribution from which I draw $p_{11}^{(r+1)}$ is given by $p_{11}\sim$beta$(u_{11}+n_{11}, u_{12}+n_{12})$, where n_{ij} is the number of observations that satisfy $s_t = i$ and $s_{t+1} = j$ which is given by $\tilde{s}_T^{(r+1)}$.

3. Generate $\mathbf{\Sigma}^{(r+1)}$ conditional on $\tilde{s}_T^{(r+1)}$, $p_{11}^{(r+1)}$, $\boldsymbol{\beta}^{(r)}$, and $\widetilde{\mathbf{Y}}_T$, and generate $\boldsymbol{\beta}^{(r+1)}$ conditional on $\tilde{s}_T^{(r+1)}$, $p_{11}^{(r+1)}$, $\mathbf{\Sigma}^{(r+1)}$, and $\widetilde{\mathbf{Y}}_T$

   I assume a Normal Wishart prior for the VAR coefficients and the error covariance. The prior is described by $\boldsymbol{\beta}(s)\sim N(\bar{\beta}(s), \mathbf{\Sigma}(s)\otimes\bar{V}(s))$ and $\mathbf{\Sigma}(s)\sim IW(\bar{S}(s), \tau(s))$ for $s = 1, 2$, where $\bar{\beta}(s)$, $\bar{V}(s)$, $\bar{S}(s)$, and $\tau(s)$ are hyperparameters. Uhlig (2005) shows that, given a weak prior such that $\bar{V}_0(s) = 0$ and $\tau_0(s) = 0$ with arbitrary values for $\bar{\beta}(s)$ and $\bar{S}(s)$, the full conditional posterior distribution is given by $\boldsymbol{\beta}(s)\sim N(\hat{\boldsymbol{\beta}}(s), \hat{\mathbf{\Sigma}}(s)\otimes(\mathbf{X}_t'\mathbf{X}_t)^{-1})$ and $\mathbf{\Sigma}(s)\sim IW(\hat{S}(s), T(s))$, where $\hat{S}(s) = U(s)'U(s)$, $U(s)$ is the residual matrix and $T(s)$ is the number of observations for the regime s. $\hat{\boldsymbol{\beta}}(s)$, $\hat{\mathbf{\Sigma}}(s)$, and $U(s)$ are obtained by estimating Equation (A1) with given $\tilde{s}_T$. I sample $\mathbf{\Sigma}^{(r+1)}$ from its posterior distribution first, then generate $\boldsymbol{\beta}^{(r+1)}$ from its posterior distribution using $\mathbf{\Sigma}^{(r+1)}$ together with $\tilde{s}_T^{(r+1)}$, $p_{11}^{(r+1)}$, and $\widetilde{\mathbf{Y}}_T$.

I repeat the steps 30,000 times, discarding the first 20,000 as burn-in. Figures showing posterior probabilities of $s_t = i$ plot the ratios of the number of draws of $s_t = i$ after the burn-in period to the total number of draws after the burn-in period, that is 10,000.[25]

[25] The term "smoothed probabilities" is used in Hara et al. (2020) instead of "posterior probabilities".

Figures presenting impulse responses of a model plot the median of the impulse responses across the 10,000 draws after the burn-in period. The 68% Bayesian credible intervals in the figures are computed by sorting the sampled impulse responses in ascending order and taking the 16 and 84 percentiles.

A three-regime MSVAR model with absorbing states has another transition probability p_{22} as shown in Equation (4.3). In this case, I add p_{22} to the information set in the steps 1, 3, and 4, and give a non-informative Beta prior for p_{22}. I sample $p_{22}^{(r+1)}$ from the posterior distribution p_{22}∼beta$(u_{22} + n_{22}, u_{23} + n_{23})$, where $u_{22} = u_{23} = 1$.

Chapter 5

Concluding Remarks

My dissertation uncovers how data uncertainty about current and past state of the economy influences business cycles. I shed light on the effects of data uncertainty about labor productivity, which is obviously a key variable in monetary policy decisions. Then I investigate changes in effectiveness of unconventional monetary policies in recent years. I focus on two popular measures of unconventional monetary policy, namely, negative interest rate policies and asset purchase programs.

This dissertation provides several important findings on economic policy conducts. A key finding from my real-time data analysis is that noise contained in real-time data significantly affects underlying fundamentals and expectations. Another important finding is that responsiveness of productivity to fundamental shocks observed in real-time data tends to be underestimated, and data revisions adjust the underestimation only gradually. My findings suggest that the noise and underestimation contained in the realized data can have sustained effects on monetary policy conduct, because they remain in the statistics data for a long period of time.

As for unconventional monetary policies, I find that NIRP is associated with a lower bound that is no less constraining than the ZIRP lower bound. The finding is robust even if macroeconomic policy uncertainty is considered. Bond yields symmetrically respond to positive and negative macroeconomic surprises, suggesting that very low and stable policy rates are expected in the NIRP countries. Concerning monetary policy normalization, I find that the LSAPs by the Fed had slightly weaker effects than during the early stage of the LSAPs but stronger effects than during the late stage of the LSAPs. On the other hand, interest rate shocks had insignificant impacts on the real economy and prices. Hence, asset purchase programs like LSAPs will retain a useful role for central banks to use in responding to future economic downturns, at least as a secondary tool.

Several avenues for future research are worth considering. Noisy signals on the past state should play important roles in monetary and fiscal policies. Applying the identification method proposed in Chapter 2 to New Keynesian models will provide insights on optimal policy conducts. While macroeconomic surprises can contribute to the dynamics of bond yields, the explanatory power of the surprises are small even at monthly frequency. A useful direction for future research would be to explore what is behind the low power of macroeconomic news to bond yields. The sensitivity analysis using intra-day data is also a promising avenue for future research. There seems yet no consensus on the macroeconomic effects of monetary policy normalization. An optimal combination of policy measures during a liftoff may be different from that when monetary policy is strongly constrained. Studying this issue will lead to deeper understanding on optimal monetary policy in a low interest rate environment.

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