

# Semiparametric Estimates of Monetary Policy Effects: String Theory Revisited

**Joshua D. ANGRIST**

MIT, Department of Economics, 50 Memorial Drive Building E52, Room 436 Cambridge, MA 02142; and NBER, Department of Economics, E52-436 MIT 77 Massachusetts Avenue, Cambridge, MA 02139 ([angrist@mit.edu](mailto:angrist@mit.edu))

**Òscar JORDÀ**

Federal Reserve Bank of San Francisco, San Francisco; and U. C. Davis ([oscar.jorda@sf.frb.org](mailto:oscar.jorda@sf.frb.org); [ojorda@ucdavis.edu](mailto:ojorda@ucdavis.edu))

**Guido M. KUERSTEINER**

Department of Economics, University of Maryland, College Park, MD 20742 ([kuersteiner@econ.umd.edu](mailto:kuersteiner@econ.umd.edu))

We develop flexible semiparametric time series methods for the estimation of the causal effect of monetary policy on macroeconomic aggregates. Our estimator captures the average causal response to discrete policy interventions in a macrodynamic setting, without the need for assumptions about the process generating macroeconomic outcomes. The proposed estimation strategy, based on propensity score weighting, easily accommodates asymmetric and nonlinear responses. Using this estimator, we show that monetary tightening has clear effects on the yield curve and on economic activity. Monetary accommodation, however, appears to generate less pronounced responses from both. Estimates for recent financial crisis years display a similarly dampened response to monetary accommodation.

**KEY WORDS:** Local projections; Nonlinear impulse response functions; Propensity score; Semiparametric models; Vector autoregression.

Congressman Goldsborough: You mean you cannot push a string. Governor Eccles: That is a good way to put it, one cannot push on a string. We are in the depths of a depression and . . . beyond creating an easy money situation through reduction of discount rates and through the creation of excess reserves, there is very little if anything that the reserve organization [Federal Reserve Board] can do toward bringing about recovery. I believe that in a condition of great business activity that is developing to a point of credit inflation, monetary action can very effectively curb undue expansion.

—*Testimony before the House Committee on Banking and Currency, March 18, 1935.*

## 1. INTRODUCTION

The string metaphor is an enduring feature of the debate over monetary policy: increasing borrowing costs may slow an expansion, but cheap finance need not stimulate economic activity in a downturn. What does the historical record say regarding the possibly different macroeconomic effects of monetary contraction and expansion? Cochrane (1994) and Romer and Romer (2013) reminded us that since the creation of the Federal Reserve, central bankers have struggled to understand the limits of their power. The identification challenge in this context arises from the fact that policy changes are rarely isolated from other economically important developments, including, perhaps, anticipated changes in economic conditions. If these changes are related to the outcome variables of interest, one subset of time series observations provides a poor control for another.

Many contemporary investigations of macro policy rely on structural models to solve this fundamental identification problem (see, e.g., the survey by Christiano, Trabandt, and Walentin

2011). This approach, typically cast in a dynamic structural general equilibrium (DSGE) framework, begins with a model of the macroeconomy that is meant to explain the time series behavior of key macro variables. In addition to theoretical predictions, DSGE models generate a system of linear (or linearized) difference equations that provide the basis for empirical work. These equations can be interpreted as vector autoregressions (VARs) with an associated set of restrictions on coefficients or error covariance matrices (as in Bagliano and Favero 1998; Christiano, Eichenbaum, and Evans 1999; and many others). The heart of the DSGE approach is a model of the entire economy, which is then used to isolate shocks that identify causal policy effects. The validity of the resulting causal inferences therefore turns in part on how accurately economic models describe the macroeconomy.

An alternative strategy, inspired by the landmark Friedman and Schwartz (1963) volume, tries to identify policy shocks through a close reading of the Federal Reserve's Open Market Committee (FOMC) meeting minutes. Romer and Romer (1989) is the first in a series of influential contemporary studies in this mold. A drawback of the narrative approach is the subjective manner in which shocks are identified. Moreover, some of the putatively random policy shifts identified in Romer and Romer (1989) may be correlated with omitted economic variables (an

© 2016 American Statistical Association  
Journal of Business & Economic Statistics  
XXX 2016, Vol. 00, No. 0

DOI: 10.1080/07350015.2016.1204919

Color versions of one or more of the figures in the article can be found online at [www.tandfonline.com/r/jbes](http://www.tandfonline.com/r/jbes).

argument fleshed out in Hoover and Pérez 1994; see Romer and Romer 1994 for a rebuttal).

This article outlines a new route to causal inference for monetary policy effects, sidestepping some of the difficulties encountered in structural and narrative-based efforts. The defining feature of our approach is a laser-like focus on the policy-making process. In contrast with the narrative approach, which also focuses on Fed decision-making, our analysis of Fed behavior is more formal and data-driven. Our solution to the policy evaluation problem starts with the presumption that, conditional on market-derived statistics that embed optimal forecasts of future outcomes and anticipated policy moves, along with a small set of institutional and economic variables, the remaining policy variation can be used to identify causal effects. This assumption allows us to quantify the causal effect of unpredictable policy changes in an environment of stable expectations and goals. At the same time, our empirical strategy easily accommodates nonlinear effects, while distinguishing the effects of monetary easing from those of tightening.

The selection-on-observables framework outlined here is founded on strong identifying assumptions, but also provides a natural starting point for time series causal inference. In the absence of purposefully designed experiments or naturally occurring quasi-experimental shifts, it is hard to see how one can do better than to use the policy variation at hand. Our focused approach limits the task of model specification and robustness checking to the formulation and testing of a model of the policy determination process. The selection-on-observables assumption also generates strong testable restrictions that can be used to assess the plausibility of causal claims. The principal econometric question that arises in our context is how to exploit selection-on-observables identification in a manner that imposes minimal auxiliary assumptions and facilitates specification testing.

Our econometric policy model describes the probability of federal funds rate target changes conditional on market statistics, past policy choices, lagged outcomes, and a few other controls. The resulting set of conditional distributions defines a function we call the *policy propensity score*. Monetary policy rules have long been studied in macroeconomics; see, for example, Clarida, Gali, and Gertler (2000), Woodford (2001), and Gali and Gertler (2007). Propensity score methods, introduced by Rosenbaum and Rubin (1983), have proven useful for cross-sectional causal inference (see, e.g., Dehejia and Wahba 1999; Heckman, Ichimura, and Todd 1998). In a pair of papers related to this one, Angrist and Kuersteiner (2004, 2011), adapt the propensity score framework to the problem of time series causality testing of the sort discussed by Granger (1969) and Sims (1972). We extend this framework here, deriving flexible, easy-to-compute propensity score estimators of the causal effects of a dynamic multinomial treatment. These semiparametric estimators are then used to assess the impact of monetary policy before and since the Great Recession. The main payoff to our approach is the ability to go directly from the policy process to causal effects on outcomes. The resulting causal estimates are valid for all processes generating outcomes, nonlinear and complex as they might be, while allowing distinct assessments of the impact of tight and easy money.

The first task on our empirical agenda is the construction of a credible model for the policy propensity score. To that

end, we build on work by Kuttner (2001), Faust, Swanson, and Wright (2004), Piazzesi and Swanson (2008), and especially Hamilton (2008) in using market-based measures of the public's policy expectations. On the theoretical side, we also rely on Piazzesi's (2005) model linking Federal Reserve policy actions with asset prices. This link justifies a model for target rate changes as a function of the price of federal funds rate futures contracts. Market-based predictions of policy actions provide a low-dimensional aggregator of publicly available information. This in turn leads to parsimonious policy models well suited to a data-poor time series setting.

The propensity score approach also obviates the need to define and separately construct a generic series of monetary policy shocks as in Romer and Romer (2004). In a recent study, Tenreiro and Thwaites (2013) used these shocks and stratified local projections (Jordà 2005) to address heterogeneity in the consequences of monetary policy effects. Propensity score models implicitly define policy shocks to be the component of policy variation that remains after conditioning on the variables supporting a selection-on-observables assumption. The question of what constitutes a policy shock is fundamentally an identification question that is most concretely addressed in the context of the sample to be used to identify causal effects.

Our investigation of monetary policy effects replicates findings from earlier work while uncovering some that are new. Echoing Christiano, Eichenbaum, and Evans (1996, 1999), among others, our results suggest contractionary monetary policy slows real economic activity, reducing employment and, to a lesser extent, inflation. At the same time, in contrast with a number of earlier studies (reviewed in Christiano, Eichenbaum, and Evans 1999), the semiparametric estimation strategy developed here suggests the consequences of Fed efforts to support the real economy have generally been disappointing. Motivated by the Fed's attempts to stimulate the economy during the Great Recession, we compare responses calculated using a sample that ends in mid-2005 with results from a sample running through 2010, including a period when the federal funds target rate hit zero. We also compute estimates for the latter period only. Our conclusions regarding the Fed's limited ability to boost real economic activity stand under both variations.

## 2. POTENTIAL OUTCOMES AND MACRO CAUSAL EFFECTS

### 2.1 Conceptual Framework

The economy is described by the vector  $\chi_t$  which contains macroeconomic aggregates as well as asset prices and information about Fed policy action. In this article, we distinguish three types of variables that are constructed from leads and lags of  $\chi_t$ :  $y_t$  is a vector of outcome variables,  $D_t$  is a policy variable that takes values  $d_0, \dots, d_J$ , and  $z_t$  is a vector of predetermined variables that predict  $D_t$ . In our application, the predictors  $z_t$  include asset prices as well as lags of  $y_t$  and  $D_t$ .

The policy regime is indexed by a parameter,  $\psi$ , which takes values in a parameter space  $\Psi$ . In addition, policymakers are assumed to react to idiosyncratic information or taste variables, represented by the scalar  $\varepsilon_t$ , that we do not get to see. The realized policy  $D_t$  can be thought of as being deter-

mined by both observed and unobserved variables according to  $D_t = D(z_t, \psi, \varepsilon_t)$ . As detailed below, Identification is provided by the assumption that  $\varepsilon_t$  is independent of potential outcomes. This is reminiscent of the recursive ordering proposed by Christiano, Eichenbaum, and Evans (1996, 1999), but our approach requires no description of the structural process connecting  $y_t$  with policy choices or expected future values of  $\chi_t$ .

Our framework for causal inference builds on the notion of potential outcomes. Potential outcomes describe realizations of  $y_t$  that arise in response to a hypothetical change in monetary policy. The potential outcomes concept originated in experimental studies where the investigator has control over the assignment of treatments, but is now widely used in observational studies. Although potential outcomes most commonly appear in studies looking at the causal effects of a binary treatment or policy intervention, the idea is easily extended to ordered discrete or continuous interventions (see, e.g., Angrist and Imbens 1995).

The definition of potential outcomes used here comes from Angrist and Kuersteiner (2011):

*Definition 1.* For fixed  $t$ ,  $l$ , and  $\psi$ , potential outcomes  $\{y_{t,l}^\psi(d); d \in \mathcal{D}\}$  are defined as the set of values the observed outcome variable  $y_{t+l}$  would take on if  $D_t = D(z_t, \psi, \varepsilon_t) = d$ , with  $d \in \mathcal{D} = \{d_0, \dots, d_j, \dots, d_J\}$ .

The vector of potential outcomes includes the observed outcome,  $y_{t+l} = y_{t,l}^\psi(D_t)$ , as well as counterfactual outcomes describing the consequences of policy choices not taken. In the application that follows,  $y_{t+l}$  is the percentage change in  $y$  from time  $t$  to  $t + l$ . The causal effect of policy choice  $d_j$  is defined as the difference  $y_{t,l}^\psi(d_j) - y_{t,l}^\psi(d_0)$ , where  $d_0$  is a benchmark policy. This notation makes a conceptual distinction between changes to the policy regime (indicated by changes in  $\psi$ ) and policy changes within the same regime, though only the latter are identified in our framework. Although the notation introduced here is nonstandard in a macro setting, the notion of macroeconomic causal effects determined by counterfactual states has a long history. For example, Kareken and Solow (1963) argued: "...One cannot deduce conclusions about the effects of monetary policy or about their timing without making some hypothesis, explicit or implicit, about what the course of events *would have been* had the monetary authorities acted differently."<sup>1</sup>

Individual causal effects can never be observed since the real world gives us only one realization. We therefore focus on average causal effects. Let  $Y_{t,L} = (y'_{t+1}, \dots, y'_{t+L})'$  and define the vector of potential outcomes up to horizon  $L$  by  $Y_{t,L}^\psi(d) = (y_{t,L}^{\psi'}(d), \dots, y_{t,L}^{\psi'}(d))'$ . Potential outcomes determine observed outcomes as follows:

$$Y_{t,L} = \sum_{d \in \mathcal{D}} Y_{t,L}^\psi(d) 1\{D_t = d\}. \quad (1)$$

Hence, the average responses to policy  $d_j$  relative to the benchmark policy,  $d_0$ , can be written

$$E \left[ Y_{t,L}^\psi(d_j) - Y_{t,L}^\psi(d_0) \right] \equiv \theta_j. \quad (2)$$

The effects of all possible policy choices relative to the benchmark policy are collected in  $\theta = (\theta'_1, \dots, \theta'_J)'$ , a vector of di-

mension  $k = k_y \times L \times J$ , with  $k_y$  the number of outcome variables,  $L$  the horizon of interest, and  $J + 1$  the number of policy options. In contrast with traditional impulse response analyses in empirical macro,  $\theta$  describes an average generalized impulse response function for all possible policy choices. These responses can be both asymmetric (increases may have different effects than decreases) and nonlinear (e.g., a convex function of the policy variable).

Potential outcomes for counterfactual policy choices are unobserved, so the expectation in (2) cannot be estimated directly. The variation that identifies causal relationships in our framework is characterized by a conditional independence assumption, also known as *selection on observables*:

*Condition 1.* Selection on observables:

$$y_{t,l}^\psi(d_j) \perp D_t | z_t \text{ for all } l \geq 0 \text{ and for all } d_j, \text{ with } \psi \text{ fixed; } \psi \in \Psi.$$

Our conditional independence assumption focuses on variation in policy interventions while holding the policy regime fixed, after conditioning on observables,  $z_t$ . In contrast with a long empirical tradition in empirical macroeconomics, our Condition 1 is not formulated in terms of an econometrically constructed policy shock. At the same time, when the policy function is given by  $D(z_t, \psi, \varepsilon_t)$ , Condition 1 postulates conditional independence between  $\varepsilon_t$  and potential outcomes, given  $z_t$ . In other words, variation in policy decisions that remains after conditioning on the policy function, is implicitly taken to be a shock that identifies policy effects under the current regime. In the vernacular of empirical macroeconomics, our estimates are effects of "unanticipated monetary policy."

Our identification strategy generalizes widely used recursive identification schemes in that no assumptions are made about functional form, while the conditioning variables need not overlap with those used to estimate a particular VAR. Moreover, while not directly testable, Condition 1 generates testable implications, a feature explored in our empirical work. The selection-on-observables condition implies that conditioning variables related to outcomes, including variables not used to construct the policy propensity score, should be independent of policy conditional on the propensity score. A correctly specified score also makes policy independent of the conditioning variables used to construct the score. We check both of these implications below.

The fact that we specify only the policy equation  $D(z_t, \psi, \varepsilon_t)$  without modeling the process determining  $y_t$  has practical as well as conceptual advantages. Specifically, our application looks at the effects of federal funds rate target rate announcements (our  $D_t$ ), by conditioning on daily financial market data in  $z_t$ . Identification comes from the assumption that information revealed by an announcement, conditional on market rates the day before the announcement, is independent of future potential outcomes at any frequency. The short window between prediction and policy announcement helps make this credible. The propensity score approach therefore allows us to use the identifying power of daily financial and target rate changes to estimate average causal effects on economic outcomes measured monthly.

As a first step on the road from identification to estimation, we use Equation (1) and Condition 1 to write the average policy

<sup>1</sup>Emphasis added; this is also quoted by Cochrane (1994).

effect conditional on  $z_t$  in terms of observable distributions as

$$E \left[ Y_{t,L}^\psi(d_j) - Y_{t,L}^\psi(d_0) | z_t \right] = E \left[ Y_{t,L} | D_t = d_j, z_t \right] - E \left[ Y_{t,L} | D_t = d_0, z_t \right]. \quad (3)$$

Expression (3) is cast in terms of observable conditional means. In applications with a high-dimensional conditioning set involving continuous random variables estimation of these conditional expectations is empirically demanding. The estimation problem is simplified by use of a parametric model for the policy propensity score, that is, for the conditional distribution of  $D_t$  given covariates,  $z_t$ .

The policy propensity score is modeled here as  $P(D_t = d_j | z_t) = p^j(z_t, \psi)$ , where  $p^j(z_t, \psi)$  is a flexible parametric model with parameters determined by the policy regime. Average causal effects can then be estimated using the fact that Condition 1 implies

$$E \left[ Y_{t,L} 1\{D_t = d_j\} | z_t \right] = E \left[ Y_{t,L}^\psi(d_j) | z_t \right] p^j(z_t, \psi). \quad (4)$$

Solving (4) for  $E[Y_{t,L}^\psi(d_j) | z_t]$  and integrating over  $z_t$  allows us to write

$$\begin{aligned} \theta_j &= E \left[ Y_{t,L}^\psi(d_j) - Y_{t,L}^\psi(d_0) \right] \\ &= E \left[ Y_{t,L} \left( \frac{1\{D_t = d_j\}}{p^j(z_t, \psi)} - \frac{1\{D_t = d_0\}}{p^0(z_t, \psi)} \right) \right]. \end{aligned} \quad (5)$$

This weighting scheme was first used to estimate population means in nonrandom samples by Horvitz and Thompson (1952) and adapted for causal inference with cross-sectional Bernoulli treatments by Hirano, Imbens, and Ridder (2003). In cross-sectional studies of causal effects of Bernoulli interventions, (5) is known as an *average treatment effect*. Our setup allows for multinomial or ordered treatments.<sup>2</sup>

The estimand described by (5) is similar to that approximated using Jordà's (2005) local projections, though here no approximation is required. This estimand can also be compared with the nonlinear impulse response function introduced by Gallant, Rossi, and Tauchen (1993). The latter is based on estimation of  $E[y_{t+l} | x_t]$  where  $x_t = (y_t, \dots, y_{t-p})$  and  $y_t$  is assumed to be a Markov process. An impulse response function can then be defined as a marginalized version of  $E[y_{t+l} | x_t^+] - E[y_{t+l} | x_t]$  where  $x_t^+$  perturbs  $x_t$  by a constant. Although  $E[y_{t+l} | x_t]$  is in principle nonparametrically identified, extrapolation to counterfactual  $E[y_{t+l} | x_t^+]$  in the Gallant, Rossi, and Tauchen (1993) framework is likely to require a model for the conditional expectation of outcomes.

Our approach leans on parametric policy models but requires no functional form assumptions for the outcome process,  $y_t$ . We use institutional knowledge and economic reasoning to guide the choice of  $z_t$ , and specification testing to assess the model for  $p^j(z_t, \psi)$ . Again, it is worth highlighting the fact that this approach does not define or estimate structural innovations for the policy process, nor develop, solve, or simulate a model for the joint process governing outcomes  $y_t$ , policy variables  $D_t$ , and

covariates  $z_t$ . The estimator based on (5) is an easily constructed weighted average, for which inference is straightforward.

## 2.2 Estimation and Inference

Inverse probability weighting estimators can be written as weighted averages of the vector of future outcomes  $Y_{t,L} = (y'_{t+1}, \dots, y'_{t+L})'$ , with weights formed from

$$\delta_{t,j}(\psi) = \frac{1\{D_t = d_j\}}{p^j(z_t, \psi)} - \frac{1\{D_t = d_0\}}{p^0(z_t, \psi)}.$$

In a correctly specified model, these weights have mean zero and are uncorrelated with  $z_t$ . In practice, we ensure this is true for macro variables by using weights based on the residuals from a regression of  $\delta_{t,j}(\hat{\psi})$  on  $z_t$  and a constant. Define these residual weights as  $\ddot{\delta}_{t,j} = \delta_{t,j}(\hat{\psi}) - \hat{\delta}_{t,j}$ . This correction preserves consistency while implicitly removing stochastic trends in macro variables since  $z_t$  includes lagged dependent variables. Hence, the average causal response vector at horizon  $l$  due to a policy intervention  $D_t = d_j$  relative to the benchmark  $D_t = d_0$  is a weighted average with typical element

$$\hat{\theta}_{l,j} = \frac{\sum_{t=1}^T y_{t+l} \ddot{\delta}_{t,j}}{T}, \quad (6)$$

which can be arranged in a vector by writing  $\hat{\theta}_j = (\hat{\theta}'_{1,j}, \dots, \hat{\theta}'_{L,j})'$ . In practice, the estimator  $\hat{\theta}_j$  can be computed as follows:

1. Fit the model  $p^j(z_t, \psi) = \Pr(D_t = d_j | z_t, \psi)$ .
2. Compute the predicted probabilities  $\hat{p}_t^j = p^j(z_t, \hat{\psi})$  for all  $j, t$ .
3. Drop observations with very low  $p^j(z_t, \hat{\psi}) < .025$  when  $1\{D_t = d_j\} = 1$ . This removes a few observations that receive extreme weights.
4. Construct the weights

$$\delta_{t,j}(\hat{\psi}) = \frac{1\{D_t = d_j\}}{p^j(z_t, \hat{\psi})} - \frac{1\{D_t = d_0\}}{p^0(z_t, \hat{\psi})}$$

5. Regress  $\delta_{t,j}(\hat{\psi})$  on a constant and  $z_t$  and construct the residual  $\ddot{\delta}_{t,j}$ .
6. Compute the vector of estimated responses to policy choice  $j$  as

$$\hat{\theta}_j = \frac{\sum_{t=1}^T Y_{t,L} \ddot{\delta}_{t,j}}{T}.$$

The general theory of extremum estimators is used in the Supplemental Appendix to derive a limiting distribution for this estimator. An important diagnostic for our purposes asks whether lagged macro aggregates are independent of policy changes conditional on the policy propensity score. In other words, we would like to show that the policy shocks implicitly defined by our score model look to be “as good as randomly assigned.” Angrist and Kuersteiner (2011) developed semiparametric tests that can be used for this purpose. The Supplemental Appendix also gives a detailed description of these tests and justifies their use in this context.

<sup>2</sup>With Bernoulli treatments, the formulation in (5) reflects Rosenbaum and Rubin's (1983) propensity score theorem, which says that if potential outcomes are independent of treatment conditional on covariates, they are also independent of treatment conditional on the propensity score.



### 3. A PROPENSITY SCORE FOR MONETARY POLICY INTERVENTIONS

As in many studies of monetary policy, we measure policy interventions with the federal funds rate. The federal funds market is an interbank loan market intended for the management of reserve requirements; the rate for overnight loans in this market, known as the federal funds rate, has historically provided a benchmark for securities across the risk and maturity spectrum. Monetary policy targeted the level of the federal funds rate until mid-December, 2008, when the fed funds rate was set to trade between 0 and 0.25%. With no room to lower nominal rates further, the Fed turned to other tools, such as large-scale asset purchases. We focus here on the pre-2009 policy era, going back to July 1989. Because Federal Open Market Committee (FOMC) meetings are very nearly a monthly occurrence (meetings take place 8 times a year), we work with monthly data.

#### 3.1 Federal Funds Rate Targeting Since 1989

Central bank staff look at many series. By conditioning on the price of financial derivatives based on the federal funds rate, we rely on the notion that market participants similarly use all available information to price contracts that implicitly bet on future policy. Specifically, our application conditions on predictions derived from federal funds rate futures (FFF) contracts. The market for these contracts started in October 1988 but, as in other studies using futures data, our sample starts in July 1989.

We consider two sample end-points. July 2005 marks the last in a series of increases in the federal funds target. From then on, the target rate remained at 5.25% until September 2007, right before the Great Recession. We use July 2005 as a first sample end-point for estimation of the propensity score and evaluate policy responses up to 24 months ahead on data running through July 2007. The target fell gradually thereafter, until December 2008 when it hovered between zero and 0.25%, marking the end of conventional monetary policy. The second sample end-point for propensity score estimation therefore extends through the end of 2008 with an additional 24 months ending in December 2010 used to estimate policy responses. Finally, we also experiment separately with data from the Great Recession period only.

Meulendyke (1998) dated the FOMC's transition to fed funds rate targeting as following the stock market crash in October 1987, but Hamilton and Jordà (2002) dated this transition a bit later, to 1989, a date coinciding with the start of our sample.<sup>3</sup> Since February 1994, changes in the federal funds rate target have been announced after each FOMC meeting, eight times a year. Changes in the target usually come in 25 basis-point increments in a  $[-0.50\%, 0.50\%]$  interval, though the target

was twice changed by 75 basis points in our sample period. Of the 78 target rate changes in our sample, 23 were outside of an FOMC meeting. Most of these happened before February 1994. On three occasions, there was more than one change in a given month, in which case we sum them.<sup>4</sup> The space of possible policy choices is defined here to be  $\{-0.50\%, -0.25\%, 0\%, 0.25\%, 0.50\%\}$ , where the  $\pm 0.50\%$  events include the few larger changes. The policy propensity score is fit using an ordered probit model for the set of all changes from  $-0.50\%$  to  $+0.50\%$ . Causal effects are reported only for the  $\pm 0.25\%$  changes, which are far and away the most common.

#### 3.2 Policy Propensity Score Specification

Efficient markets price assets using all available information. This motivates students of monetary policy to define policy shocks as deviations from the predictions implicit in asset prices. Cochrane and Piazzesi (2002), for example, use changes in the Eurodollar rate around meeting dates to define target rate surprises, while Thapar (2008) use T-Bill futures in a similar manner. Kuttner (2001), Faust, Swanson, and Wright (2004), Gürkaynak, Sack, and Swanson (2005, 2007), Bernanke and Kuttner (2005), Hamilton (2008), and Wingender (2011) construct monetary policy shocks from financial derivatives that price the federal funds rate directly.<sup>5</sup>

Federal funds rate derivatives include a futures contract on the effective federal funds rate and an options contract on these futures (though the latter started only in 2003). Futures contracts refer to calendar-month averages of the effective federal funds rate published by the New York Fed, with spot, and one- through five-month contracts. We use these derivatives to predict target changes, implicitly defining policy surprises as deviations from market-based forecasts of Federal Reserve behavior.

The intuitive notion that futures prices provide an optimal policy forecast can be made rigorous using Piazzesi's (2005) term structure model. Denote the information available to policymakers at time  $t$  by  $\zeta_t$ . Piazzesi shows that bond yields and related derivatives likewise depend on  $\zeta_t$ . Under the additional assumption that pricing functions are invertible, we can replace  $\zeta_t$ , which may be only partially observable, with a vector of observed asset prices. This theoretical argument is fleshed out in Appendix A.

As a practical matter, our analysis distinguishes between months with scheduled FOMC meetings ("meeting months") and months without. For nonmeeting months, we construct  $s_t^0$ , the difference between the price of a one-month-ahead contract in that month and the target rate in effect on the last day of the previous month. This measure approximates market expectations of the change in the target rate and is expected to be a good predictor of the dependent variable in the policy model. Meeting months, on the other hand, require special adjustments to reflect the microstructure of the federal funds market and an institutional feature known as the reserve maintenance period.

<sup>3</sup>The FOMC comprises the Fed Chairman and six other Board Governors, the president of the Federal Reserve Bank of New York and a rotating pool of four presidents from the remaining eleven regional Federal Reserve Banks. It is the governing body of the Federal Reserve System in charge of determining monetary policy.

<sup>4</sup>The relevant months are December 1990, December 1991, and January 2001.

<sup>5</sup>Krueger and Kuttner (1996), Kuttner (2001), Piazzesi and Swanson (2008), Gürkaynak, Sack, and Swanson (2005, 2007), and Hamilton (2009) discuss the efficiency of the market for federal funds futures in greater detail.

These details appear in a supplemental data Appendix. Briefly, the meeting-month series,  $s_t^1$ , is a scaled difference (adjusting for the exact day of a meeting) between the price of the futures contract expiring during the same month as the meeting and the current target rate, both observed at market close on the day before the meeting. This series also allows for maintenance period dynamics so as to account for anomalies caused by weekend effects and volatility in the federal funds rate usually observed during the last few days of the maintenance period.<sup>6</sup>

Because target changes are naturally ordered in 0.25% increments over the range  $\pm 0.50\%$ , we model the policy propensity score with an ordered probit specification. Hamilton and Jordà (2002) and Scotti (2011) likewise use ordered probit to model federal funds rate target changes. The dependent variable is the change in the target rate during the month, irrespective of whether an FOMC meeting was announced or whether the target was changed. For the few occasions where more than one target change occurred in the same month, recall that we use the accumulated monthly change.

In addition to controlling for market expectations through FFF contracts, we include inflation (measured by the personal consumption expenditures price index) and the unemployment rate, variables that typically appear in a Taylor-type monetary policy rule. The unemployment rate is available at a monthly frequency and is a natural substitute for output gap measures commonly used with quarterly data. Our “Taylor Rule” specification can also be motivated by Blanchard and Galí (2010) and Galí (2011), which show that the optimal monetary policy rule in a New Keynesian model with real-wage rigidities depends both on inflation and unemployment. Finally, the specification of the propensity score includes terms designed to capture a variety of idiosyncratic factors. These include a dummy variable  $FOMC_t$  indicating months with a scheduled FOMC meeting, the target change in the previous month, the target rate change in the previous month interacted with  $FOMC_t$ , a scale factor that accounts for when within the month the FOMC meeting is scheduled, and a set of monthly seasonal dummies. We also include the variable  $CRISIS_t$ , a dummy that takes the value of 1 starting August 2006 to capture a window that includes the financial crisis with about a one-year lead. Finally, the model includes dummies for the Y2K event and the September 11, 2001 attacks.<sup>7</sup>

<sup>6</sup>In constructing  $s_t^1$  for months in the pre-1994 era, before target rate changes were announced, our coded announcement date is delayed by one day relative to the later period. Before 1994, the market became aware of a target change only through a reading of the open market operations implemented by the New York Fed’s Trading Desk. These operations take place at the beginning of the trading day and hence are observed the day after a meeting, which generally concludes after the close of the market. This nuance affects the construction of  $s_t^1$  only. In a few instances, target rate changes in meeting months preceded meetings, with an additional change or no change at the meeting, as in February, March, and August 1991. In such situations, the predictor is taken to be  $s_t^0$  rather than  $s_t^1$ . The active futures’ variable for any given month is denoted by  $FFF_t$ , equal to either  $s_t^0$  or  $s_t^1$ , as described above.

<sup>7</sup>The scale factor is defined as  $\kappa/(\kappa - t)$ , where  $\kappa$  is the number of days in a given month and  $t$  is the day of the month when the FOMC meeting is scheduled.

### 3.3 Policy Propensity Score Estimates

Table 1 reports average marginal effects of policy predictors on the likelihood of a 0.25% increase in the target. These estimates use the sample through July 2005 for the pre-crisis sample (July 1989 to July 2005) and through December 2008 for the full sample (July 1989 to December 2008). Since responses are calculated by shifting the outcome variable up to two years forward, the end dates for the outcome samples are July 2007 and December 2010, respectively.

Estimates of a benchmark specification that predicts target rate changes with inflation and unemployment alone are reported in Columns (1) and (2). These are labeled  $OP_{T1}$  and  $OP_{T2}$  (the  $T$  subscript is a mnemonic for Taylor-rule) and use the pre-crisis sample. Columns (5) and (6) report estimates for  $OP_{T1}$  and  $OP_{T2}$  using the full sample. Broadly speaking, the estimates show that both variables affect policy largely as expected, though the negative unemployment effect is stronger than the very small positive and imprecisely estimated inflation effect. The Taylor-type model estimates shown in Columns (1) and (5) use same-month measures of inflation and unemployment only, while the estimates reported in Columns (2) and (6) are from models that add inflation and unemployment lags, the size of the last target change, and seasonal and scheduling dummies.<sup>8</sup>

Columns (3) and (4) for the short sample and Columns (7) and (8) for the long sample, labeled  $OP_{F1}$  and  $OP_{F2}$  report estimates from specifications that include FFF<sub>*t*</sub> terms that differ in the pre-crisis and full samples. The results here indicate that market-based factors are better predictors of target rate changes than the combination of inflation and unemployment and their lags alone. The likelihoods in columns (3) and (7) show dramatically improved fit as a result of the inclusion of FFF, even though the models here have fewer parameters than the simple Taylor models reported in columns (2) and (6). Moreover, as can be seen in columns (4) and (8), adding Taylor variables to the models with FFF does little to raise the log likelihood further.

Fitted values from the full policy score model seem to track realized shifts well over the course of the business cycle. This can be seen in Figure 1, which plots actual and predicted target changes (i.e., the expected target change conditional on regressors in the policy propensity score). Predictions were computed using the  $OP_{F2}$  estimates from Column (8) of Table 1. The figure also shows the time series of Industrial Production (IP) growth to mark cyclical fluctuations. The figure overlays IP growth to show that fluctuations in the propensity score are not associated with fluctuations in the business cycle.

### 3.4 Specification Testing

An important diagnostic for the estimated propensity score looks at whether economic variables related to outcomes are independent of policy changes conditional on the score. In other words, we would like to show that the policy shocks implicitly defined by our score model look to be as good as randomly assigned. Angrist and Kuersteiner (2011) developed semiparametric tests that can be used for this purpose.<sup>9</sup> Table 2 checks

<sup>8</sup>Detailed variable definitions and sources appear in Appendix A.

<sup>9</sup>Implementation details appear in the supplemental appendix.

Table 1. Ordered probit specifications for the expected change in the target rate

	July 1989 to July 2005				July 1989 to Dec. 2008			
	OP <sub>T1</sub>	OP <sub>T2</sub>	OP <sub>F1</sub>	OP <sub>F2</sub>	OP <sub>T1</sub>	OP <sub>T2</sub>	OP <sub>F1</sub>	OP <sub>F2</sub>
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
FFF <sub>t</sub> Pre-Crisis			0.550*** (0.110)	0.519*** (0.105)			0.612*** (0.098)	0.583*** (0.095)
FFF <sub>t</sub> Post-Crisis							0.575** (0.211)	0.556** (0.205)
Inflation, Lag 1	0.081 (0.074)	0.049 (0.072)		0.080 (0.067)	0.075 (0.062)	0.019 (0.060)		−0.000 (0.056)
Inflation, Lag 2		0.104 (0.074)		0.023 (0.066)		0.168* (0.069)		0.057 (0.058)
Unem. Rate, Lag 1	−0.312** (0.096)	−0.185* (0.088)		−0.148 (0.080)	−0.300** (0.094)	−0.164 (0.087)		−0.143 (0.078)
Unem. Rate, Lag 2		−0.257** (0.092)		−0.127 (0.078)		−0.296** (0.091)		−0.125 (0.077)
Target Rate		−0.014** (0.005)	−0.016** (0.005)	−0.016** (0.005)		−0.016** (0.006)	−0.017** (0.005)	−0.017*** (0.005)
Last Target Change		0.101 (0.082)	0.136 (0.072)	0.078 (0.072)		0.102 (0.086)	0.148* (0.071)	0.088 (0.074)
LTC×FOMC		0.207* (0.101)	0.092 (0.082)	0.123 (0.084)		0.295** (0.107)	0.178* (0.082)	0.220* (0.086)
FOMC		−0.025 (0.031)	−0.004 (0.029)	−0.006 (0.029)		−0.030 (0.031)	−0.018 (0.027)	−0.014 (0.028)
CRISIS					−0.072 (0.038)	−0.060 (0.034)	−0.029 (0.036)	−0.017 (0.037)
Log Likelihood	−187.04	−149.85	−121.04	−117.60	−231.93	−185.36	−141.42	−138.53
Observations	192	192	192	192	233	233	233	233

NOTE: This table reports selected marginal effects on the probability of a 25 bps increase in the fed funds target rate. Standard errors are shown in parentheses. \*\*\*/\*\*/\* indicates significance at the 99/95/90% confidence level. LTC=Last Target Change. For a definition of FFF<sub>t</sub>, see the text. “FFF<sub>t</sub> Pre-Crisis” is the marginal effect of FFF<sub>t</sub> interacted with a pre-crisis dummy, “FFF<sub>t</sub> Post-Crisis” is the marginal effect of FFF<sub>t</sub> interacted with the crisis dummy. For other variable definitions see text.

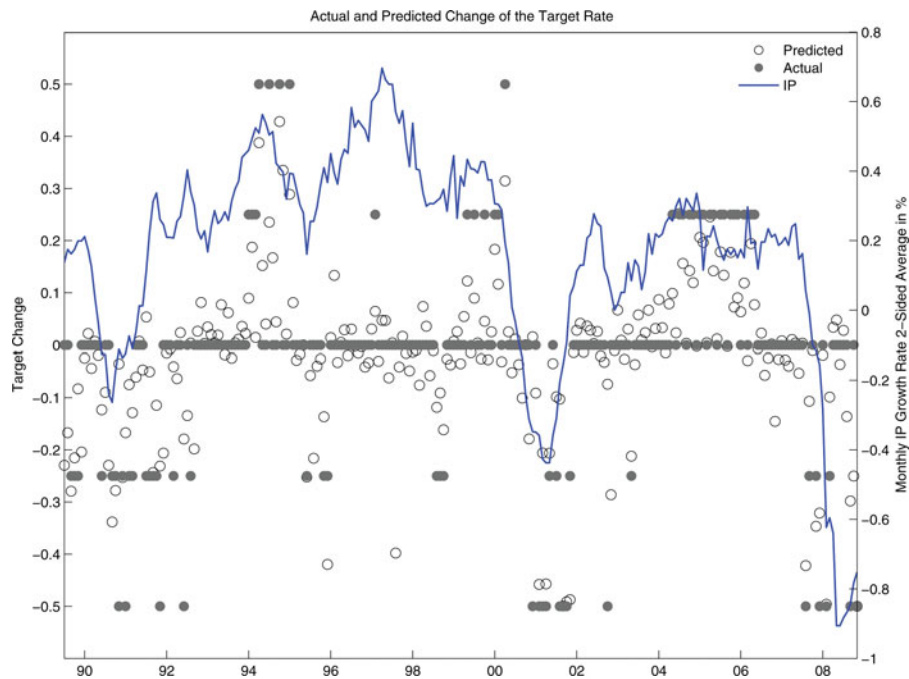


Figure 1. Actual changes (circles) and predicted changes (dots) in the target rate. Predictions are from the propensity score model labeled OP<sub>F2</sub> in Table 1. The figure also shows IP growth over the same period.

Table 2. This table shows  $p$ -values for tests that policy changes are independent of the covariates listed, conditional on the propensity score

	July 1989 to July 2005				July 1989 to Dec. 2008			
	OP <sub>T1</sub> (1)	OP <sub>T2</sub> (2)	OP <sub>F1</sub> (3)	OP <sub>F2</sub> (4)	OP <sub>T1</sub> (5)	OP <sub>T2</sub> (6)	OP <sub>F1</sub> (7)	OP <sub>F2</sub> (8)
<i>A. Propensity Score Covariates</i>								
FFF <sub><i>t</i></sub>	0.000	0.000	.	.	0.000	0.000	0.890	0.842
FFF <sub><i>t</i></sub> squared	0.006	0.074	0.738	0.767	0.002	0.075	0.721	0.723
Target Rate	0.001	.	.	.	0.007	.	.	.
Last Target Change	0.000	.	.	.	0.000	.	.	.
FOMC Meetings	0.013	.	.	.	0.002	.	.	.
Inflation, Lag 1	.	.	0.319	.	.	.	0.883	.
Unem. Rate Lag 1	.	.	0.336	.	.	.	0.534	.
Inflation, Lag 2	0.879	.	0.576	.	0.799	.	0.787	.
Unem. Rate Lag 2	0.003	.	0.279	.	0.007	.	0.492	.
<i>B. Lagged Outcome Variables</i>								
All Outcomes	0.000	0.002	0.060	0.163	0.000	0.000	0.056	0.131
Combined Yields	0.000	0.002	0.181	0.255	0.000	0.001	0.140	0.174
Funds Rate (Monthly Average)	0.000	0.055	0.952	0.854	0.000	0.002	0.958	0.493
IP	0.008	0.020	0.045	0.141	0.020	0.100	0.116	0.184
3 Month T-Bill	0.000	0.022	0.715	0.826	0.000	0.058	0.421	0.371
2 Year T-Bond	0.000	0.001	0.763	0.816	0.000	0.001	0.490	0.459
5 Year T-Bond	0.001	0.002	0.852	0.900	0.000	0.001	0.654	0.633
10 Year T-Bond	0.041	0.052	0.927	0.964	0.009	0.021	0.707	0.746
Funds Rate (End of Month)	0.061	0.517	0.040	0.079	0.034	0.464	0.082	0.129

NOTE: Panel A reports test results for variables that appear in propensity score models other than the one being tested. Panel B reports test results for variables that don't appear in any score model. Test statistics labeled Combined Yields are joint for the four yield curve variables 3 Month T-Bills through 10 Year T-Bonds. All Outcomes is a joint test using all yield curve variables, IP, the Federal Funds Rate (Monthly Average), and the effective Federal Funds Rate at the end of the month prior to target rate changes. Entries marked "." refer to variables that are orthogonal to  $\delta_t$  by construction.

two sorts of variables, those included in at least one score model (in Panel A) and a set of lagged outcome variables omitted from all models (in Panel B). The table reports joint tests ( $df = 2$ ) for quarter-point increases and decreases, the focus of our analysis. The table also shows  $p$ -values for tests that look jointly at the four yield curve variables ( $2 \times 4 = 8$   $df$ ), namely 3-month and 2-, 5-, and 10-year T-notes, and joint tests for all lagged excluded outcome variables ( $2 \times 7 = 14$   $df$ ). Missing entries in each column indicate the variables included in the model being tested in that column; these are orthogonal to score residuals by construction.

Test results for the simple Taylor model (reported in columns labeled OP<sub>T1</sub>) show substantial correlation with the economic variables included as controls in other more elaborate score models, including the lagged federal funds rate. The addition of controls for inflation and unemployment lags, the size of the last target change, and seasonal and scheduling dummies generates better results, although these  $p$ -values (reported in columns labeled OP<sub>T2</sub>) still show some evidence of correlation between FFF<sub>*t*</sub> and the average federal funds rate with  $\delta_{t,j}$ . By contrast, test results for the model with FFF<sub>*t*</sub> alone (reported in columns labeled OP<sub>F1</sub>) mostly pass, accept the null of orthogonality. Similarly, test results for the model that includes both FFF<sub>*t*</sub> and inflation and unemployment terms (reported in columns labeled OP<sub>F2</sub>) offer little evidence against the hypothesis of random policy innovations. In particular, a joint test of all yields and a joint test of all lagged excluded outcome variables fail to reject. We therefore use OP<sub>F2</sub> for the estimation of policy effects below.

#### 4. DYNAMIC MONETARY POLICY EFFECTS

It is commonly assumed that the longer-end of the yield curve is the proximate channel through which target rate changes eventually affect inflation and the real economy. We therefore begin with an analysis of policy effects on the yield curve, specifically, the federal funds rate, and the 3-month T-Bill and 2- and 10-year T-Bond rates. In addition to the yield curve, we look at effects on inflation measured by the change in (100 times) the log of the Personal Consumption Expenditures Price Index (PCEPI), effects on output as measured by the change in (100 times) the log of the Industrial Production (IP) index, and effects on changes in the unemployment rate. Policy responses refer to the percent or percentage point (for unemployment) change in the outcome variable measured from the month of the policy intervention to the relevant horizon.

Our analysis shows the impact of increasing/decreasing the target funds rate by 25 basis points, out to a horizon of 24 months. All responses are measured as cumulative percent changes. Figures 2–4 plot these estimated responses, constructed using propensity score model OP<sub>F2</sub> (corresponding to score estimates reported in Columns (4) and (8) of Table 1). The figures also show 90% confidence bands. Estimates and associated standard errors for effects at select horizons appear in Tables 3–6 as well.

The federal funds rate responds more sharply to increases in the target rate than to decreases, as can be seen in Figure 2. A 25 basis point (bps) increase in the target appears to spark a sequence of further changes that induces a peak increase in the federal funds rate close to 0.8 percentage points after about a



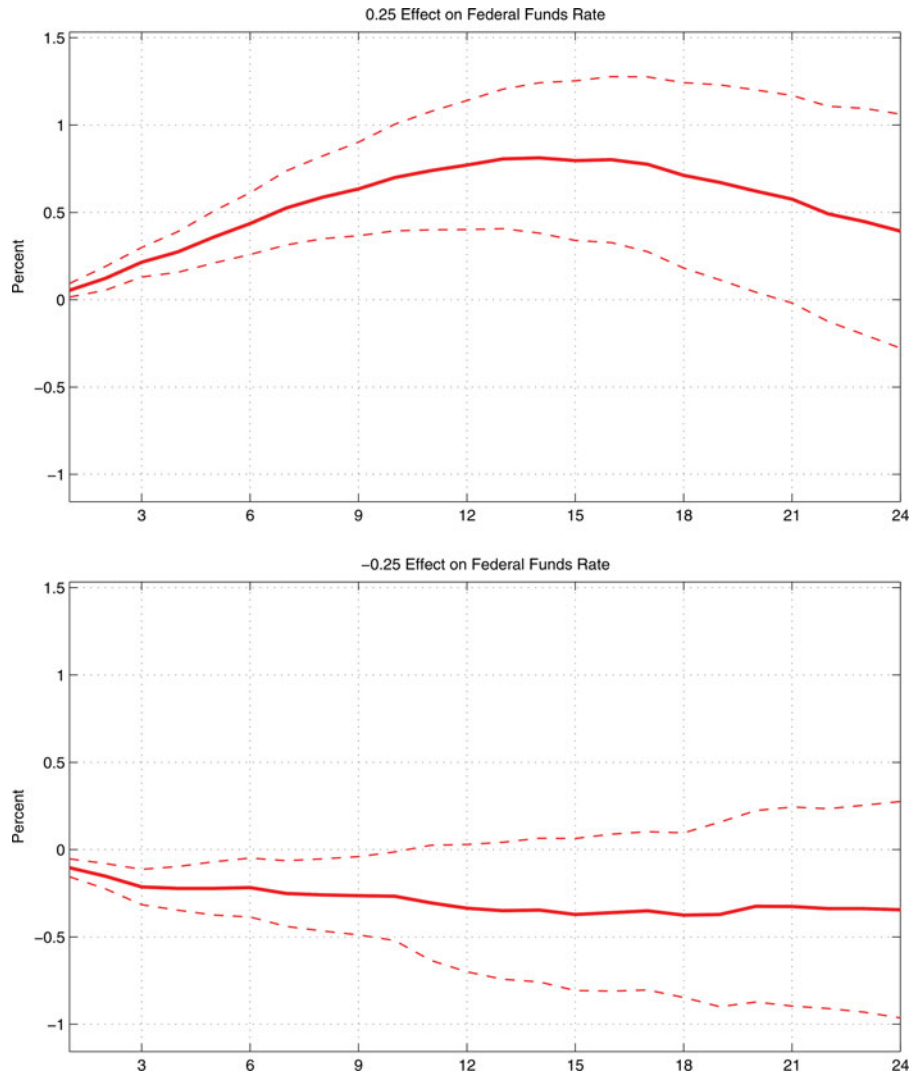


Figure 2. Estimated effects of target rate changes on the Federal Funds Rate. These estimates use data from August 1989 through July 2007, and the propensity score model labeled  $OP_{F2}$  in Table 1. Dashed lines indicate 90% confidence bands.

year, falling to under half a percentage point after two years. This pattern is similar to that found in VAR-type estimates (e.g., those in Figure 3 in Christiano et al. 1999, when cumulated). By contrast, a 25 bps reduction lowers the federal funds rate by less than  $-0.4$  either percentage points one year out, although this decline largely endures for two years. Table 6 shows that the federal funds rate response to a 25 bps decrease in the target at the 6, 12, and 18-month marks is about half the responses to a 25 bps increase. At the 24-month mark, increases and decreases have effects of about the same size.

Estimated causal effects on bond yields appear in Figure 3 and Tables 3 and 4. As we might expect, rate increases move through the yield curve with diminished intensity as maturities lengthen. For example, the estimates of effects at the 12-month mark in Table 3 show effects falling from 0.7 to 0.5 to 0.4 to 0.3 as maturities move from 3 months to 10 years. A similar pattern appears in estimates reported by Cochrane and Piazzesi (2002). The estimated yield curve response to a rate decrease goes the other way, but is considerably more muted, as shown in the right-hand column of the figure and in Table 3. Estimated responses to rate decreases are not significantly different from zero at the

one- and two-year marks. The 3-month T-bill decline is also only about half of the corresponding effect on the federal funds rate. Effects on 2, 5, and 10-year T-Bond rates are similarly reduced in magnitude. Thus, we see a consistent picture of weak effects of target rate reductions on rates across the maturity spectrum. Some of this weakness reflects differences in the average path of the federal funds rate observed in response to a decline in the target of 25 bps. However, even if this average target path were strictly symmetric, our estimates suggest that term rates are not as sensitive to policy accommodation as they are to tightening.

Estimated effects on macroeconomic aggregates are reported in Figure 4 and Table 4. A 25 bps target rate increase reduces the price level from the relevant counterfactual, but with a long lag. Two years out, an initial quarter point increase in the target is estimated to have reduced prices by just under a quarter percent, equivalent to a reduction of about a tenth of a percent in the annual inflation rate. To put this in perspective, it is worth noting that inflation is low in our sample period, with only modest deviations around two percent level. Interestingly, we see no evidence of a “price puzzle,” that is, the common

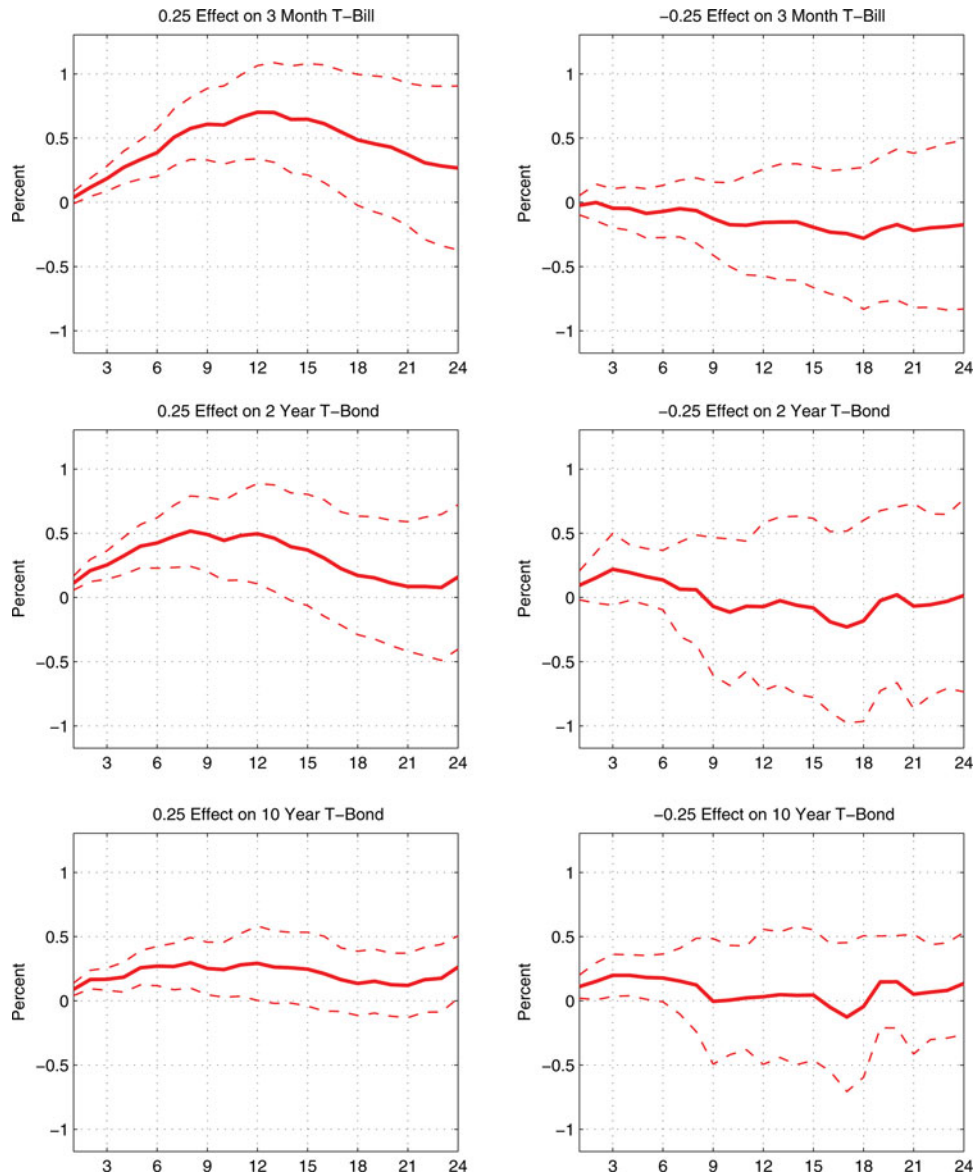


Figure 3. Estimated effects of target rate changes on Bond Yields. These estimates use data from August 1989 through July 2007, and the propensity score model labeled  $OP_{F2}$  in Table 1. Dashed lines indicate 90% confidence bands.

finding in VAR-based estimates showing short-run increases in inflation with an increase in the target rate (see, e.g., Sims 1992).

Target rate increases initially change IP little, with a gradual decline emerging after about a year. At the two year mark, a target rate increase is estimated to have pushed IP down by about 1.7%, equivalent to about a 0.75% decrease in annual growth rates. As with inflation, individual coefficient estimates for each horizon are mostly imprecise, but IP effects are significantly different from zero in the last twelve months of our 24-month window. The unemployment rate response to a rate increase mirrors the pattern of IP responses, with a total increase of about a third of a percentage point after two years. This is somewhat less than the two-to-one ratio that a contemporary Okun's law would predict for the economy as a whole. This may reflect the fact that IP is a diminishing and increasingly volatile proportion of total output.

Target rate reductions generate a markedly different pattern of real-economy responses, far from mirroring that seen in our esti-

mates of the effects of rate increases. This pattern is documented in the right-hand column of Figure 4, which offers little evidence that target rate reductions have consequences beyond changes in the federal funds rate itself. Table 4 quantifies these effects. At a 24-month horizon, for example, prices decline by  $-0.2\%$  in response to an increase of 25 bps in the target, but remain unchanged in response to a 25 bps decrease. Industrial production declines by a significant (economically and statistically)  $-1.7\%$  in response to a target increase, but is essentially unchanged (an insignificant  $-0.2\%$ ) when the target is reduced. The unemployment rate increases by 0.3 percentage points when the target is increased but is unresponsive when the target is reduced. This asymmetry in yield curve and macro aggregate responses to U.S. monetary policy shifts echoes the findings in Hamilton and Jordà (2002) and Angrist and Kuersteiner (2011), but does not feature in most VAR-based estimates. A natural explanation for the absence of output effects of target rate decreases is the weak effect of decreases on bond yields documented in the right-hand column of Figure 3.

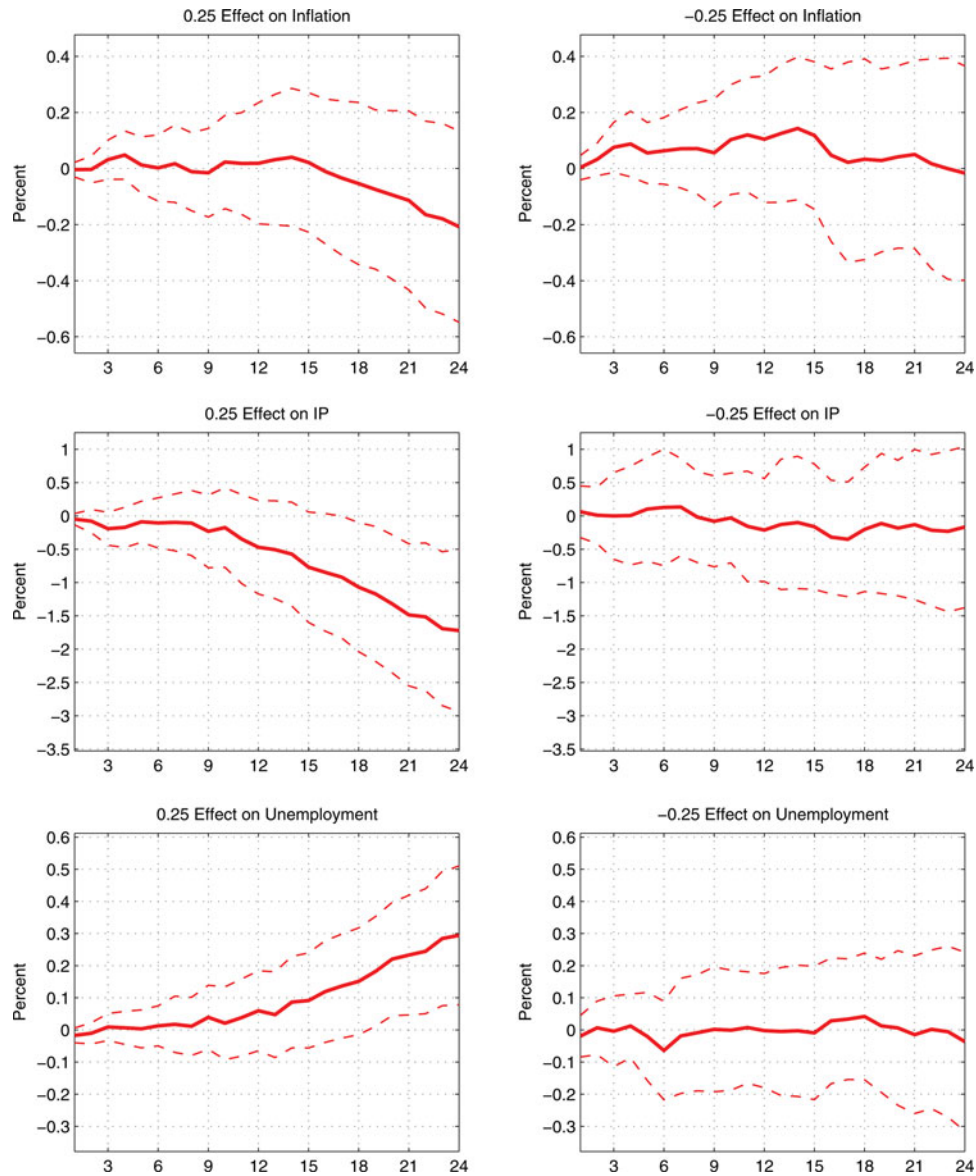


Figure 4. Estimated effects of target rate changes on macrovariables. These estimates use data from August 1989 through July 2007, and the propensity score mode labeled  $OP_{F2}$  in Table 1. Dashed lines indicate 90% confidence bands.

#### 4.1 Other Empirical Comparisons

In an influential study of the effects of monetary policy shocks on the yield curve and macro variables, Cochrane and Piazzesi (2002) reported estimates of policy effects on the yield curve similar to ours. On the other hand, their results show little effect of policy changes on prices, while suggesting employment increases after a rate increase. The yield curve effects reported here are stronger than the VAR-based responses reported in Christiano, Eichenbaum, and Evans (1996, 1999).

Faust, Swanson, and Wright (2004) used policy-induced changes in federal funds futures prices to quantify policy shocks. Their VAR-based estimates of the effect of a positive 25 basis point surprise show price decreases similar to those reported here. The corresponding estimated effects on output line up less well, however, with a mixture of positive and negative effects. In contemporaneous work related to ours, Tenreiro and Thwaites

(2013) identified monetary policy effects using the events isolated by Romer and Romer (2004), highlighting differences in policy effectiveness in expansions and recessions. They find that Romer shocks appear to be more effective in the former than the latter.

As a theoretical matter, macro models with nominal rigidities, information asymmetries, menu costs, or lending constraints typically imply asymmetric responses to monetary policy interventions. For example, Cover (1992) and DeLong and Summers (1988) argue that contractionary monetary policy affects real variables more than expansionary policy. Using international data, Karras (1996) find strong evidence of asymmetry in the effects of monetary policy on output using European data. These papers are consistent with Keynes' (1936) observations on the role of sticky wages in business cycles (see Ravn and Sola 2004 for a recent review of the relevant history of thought in this context).

Table 3. Estimates of cumulated impulse responses at horizons 6, 12, 18, and 24 months based on data from August 1989 through July 2007

	3-month T-Bill		2-year T-Bond		5-year T-Bond		10-year T-Bond	
	+0.25	−0.25	+0.25	−0.25	+0.25	−0.25	+0.25	−0.25
6	0.387*** (0.113)	−0.070 (0.124)	0.424*** (0.120)	0.136 (0.141)	0.351*** (0.106)	0.197 (0.138)	0.271*** (0.094)	0.178 (0.113)
12	0.702*** (0.221)	−0.157 (0.253)	0.496** (0.237)	−0.072 (0.398)	0.373* (0.207)	−0.018 (0.392)	0.293* (0.177)	0.031 (0.321)
18	0.486 (0.311)	−0.280 (0.337)	0.172 (0.282)	−0.182 (0.477)	0.131 (0.201)	−0.110 (0.437)	0.136 (0.152)	−0.045 (0.336)
24	0.268 (0.389)	−0.174 (0.400)	0.159 (0.343)	0.016 (0.457)	0.230 (0.225)	0.092 (0.348)	0.263* (0.148)	0.135 (0.243)

NOTE: Reported values are cumulated changes measured as fractions of 100 basis points. Standard errors in brackets. \*\*\*/\*\*/\* indicates significance at the 99/95/90% confidence level.

Table 4. Estimates of cumulated impulse responses at horizons 6, 12, 18, and 24 months based on data from August 1989 through July 2007

	Funds Rate		Inflation		Indust. Prod.		Unemp. Rate	
	+0.25	−0.25	+0.25	−0.25	+0.25	−0.25	+0.25	−0.25
6	0.436*** (0.108)	−0.217** (0.103)	0.002 (0.072)	0.063 (0.073)	−0.105 (0.229)	0.129 (0.534)	0.013 (0.038)	−0.064 (0.094)
12	0.771*** (0.225)	−0.335 (0.222)	0.019 (0.132)	0.104 (0.137)	−0.471 (0.428)	−0.212 (0.471)	0.060 (0.076)	−0.002 (0.108)
18	0.712** (0.324)	−0.375 (0.287)	−0.054 (0.176)	0.033 (0.218)	−1.070* (0.590)	−0.202 (0.569)	0.151 (0.101)	0.042 (0.120)
24	0.393 (0.409)	−0.344 (0.378)	−0.208 (0.208)	−0.016 (0.233)	−1.720** (0.743)	−0.167 (0.737)	0.294** (0.132)	−0.037 (0.170)

NOTE: Reported values are cumulated changes measured in percent of the level of Inflation and IP and as point changes in the rates for the federal funds rate and the unemployment rate. Standard errors in brackets. \*\*\*/\*\*/\* indicates significance at the 99/95/90% confidence level.

## 5. CRISIS INTERVENTION

Did the Great Recession change the effects of monetary policy? This question is addressed by first extending the sample to cover target rate changes through the end of 2008, with outcomes measured through the end of 2010 and then by analyzing policy changes for the early crisis period only (October 2006 to December 2008), with outcomes measured through December 2009 (one year earlier than the full-sample analysis as the horizon is limited to 12 months for the crisis sample).

Table 5. Estimates of cumulated impulse responses at horizons 6, 12, 18, and 24 months in response to a reduction of the Federal Funds Target by 0.25%

	3-month T-Bill	2-year T-Bond	5-year T-Bond	10-year T-Bond
6	−0.026 (0.124)	0.290* (0.160)	0.324* (0.167)	0.273* (0.151)
12	−0.003 (0.247)	0.190 (0.436)	0.169 (0.419)	0.150 (0.365)
18	0.038 (0.345)	0.197 (0.511)	0.164 (0.440)	0.125 (0.336)
24	0.251 (0.440)	0.496 (0.511)	0.452 (0.382)	0.376 (0.262)

NOTE: These estimates use data from August 1989 through December 2010. Reported values are cumulated changes measured as fractions of 100 basis points. Standard errors in brackets. \*\*\*/\*\*/\* indicates significance at the 99/95/90% confidence level.

Figure 5 and Table 6 show the estimated response of the federal funds rate in the longer sample. We report results only for target rate reductions since there were no rate increases between mid-2005 and the end of 2008. These estimates show that the response to a target decrease is less persistent than in the shorter sample, ending the two-year horizon with no decline in the federal funds rate, while the response up to 12 months is comparable to that seen in the shorter sample.

Extended-sample estimated term rate responses to a decrease in the target of 25 bps are plotted in Figure 6 and Table 5. These are similar to the estimates using the sample that omits the crisis

Table 6. Estimates of cumulated impulse responses at horizons 6, 12, 18, and 24 months in response to a reduction of the Federal Funds Target by 0.25%

	Funds rate	Inflation	Indust. Prod.	Unemp. Rate
6	−0.233* (0.119)	0.190* (0.106)	0.543 (0.568)	−0.194 (0.132)
12	−0.265 (0.235)	0.221 (0.171)	0.243 (0.729)	−0.199 (0.174)
18	−0.136 (0.316)	0.163 (0.228)	0.254 (0.943)	−0.162 (0.213)
24	0.047 (0.419)	0.174 (0.244)	0.501 (1.252)	−0.293 (0.320)

NOTE: These estimates use data from August 1989 through December 2010. Reported values are cumulated changes measured in percent of the level of Inflation and IP and as point changes in the rates for the federal funds rate and the unemployment rate. Standard errors in brackets. \*\*\*/\*\*/\* indicates significance at the 99/95/90% confidence level.



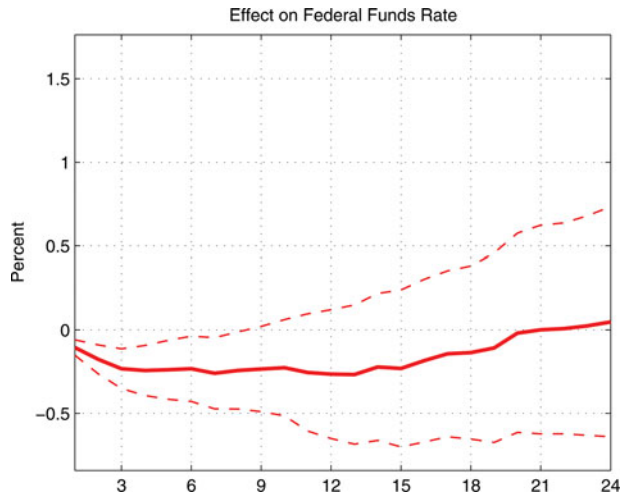


Figure 5. Estimated effects of target rate decreases on the Federal Funds Rate through 2010. These estimates use data from August 1989 through December 2010, and the propensity score model labeled  $OP_{F2}$  in Table 1. Dashed lines indicate 90% confidence bands.

period. The estimated effects in Figure 6 are remarkably flat for the 3-month T-Bill, consistent with the flatter response of the federal funds rate to target rate declines plotted in Figure 5, but again markedly weaker than the response of the fed funds rate itself. The estimated response of longer yields are positive but imprecise.

Finally, macro aggregate responses in the longer sample, reported in Figure 7, suggest target rate reductions boosted inflation, with an average increase of 0.2% in the price level occurring mostly in the first 6 months. The response of IP is statistically insignificant with an increase of just 0.5% after two-years. The unemployment rate response is somewhat more visible and at the 2-year mark drops by an imprecisely estimated .3 points. Here too, the effects are broadly similar to those estimated for the pre-crisis period.

Our short “crisis sample” saw no target rate increases, so we model policy changes as any rate decrease of 25 bps or more using a standard probit model for the policy propensity score. The specification of the policy propensity score combines the  $FFF_t$  variable, along with inflation and unemployment (the Taylor variables used for the policy model estimated in the longer sample). The crisis sample contains only 28 observations, naturally limiting the precision of estimates for this period. The residual correction is therefore omitted and estimation horizon is limited to 12 months.

Crisis-period policy-change marginal effects, reported in Table 7, are normalized to show the impact on the probability that the target rate is left unchanged, so the signs align with the ordered estimates reported earlier. The model with inflation and unemployment alone does not predict target rate decreases very well, specially when compared to the model with only  $FFF_t$ . This can be seen by comparing the likelihood values across Columns (1) and (2). The specification in Column (3) contains all three regressors; the coefficients on inflation and unemployment are not jointly significant in this model.

Figure 8 shows the estimated response of the federal funds rate to crisis-period target rate reductions. Just as in the larger

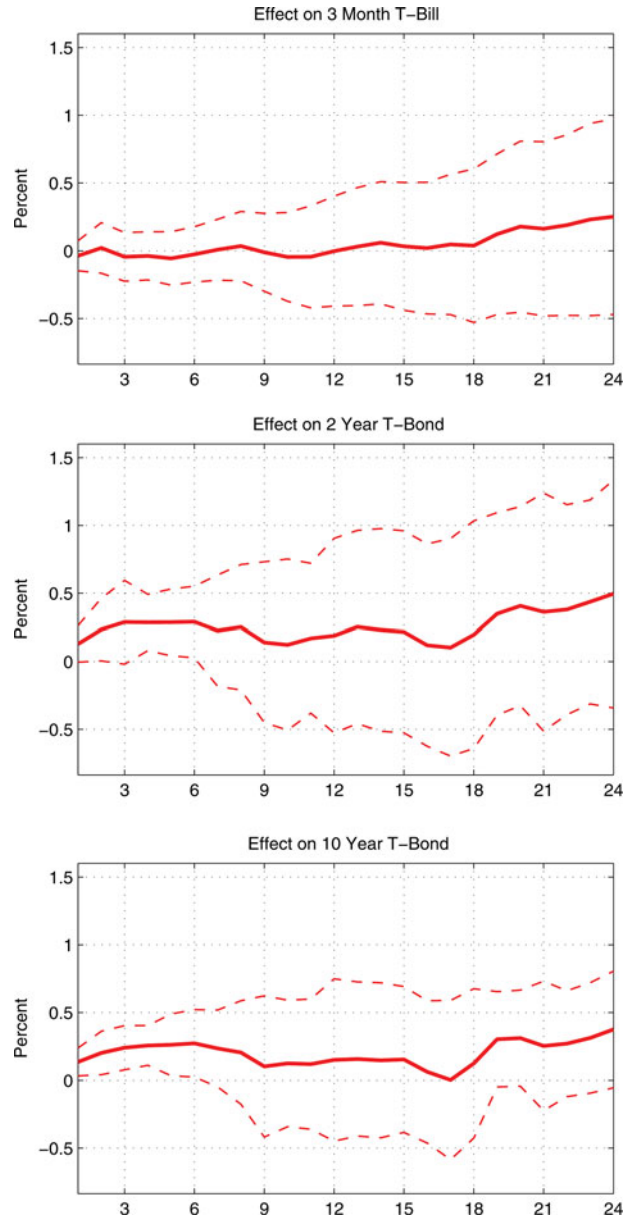


Figure 6. Estimated effects of target rate decreases on Bond Yields through 2010. These estimates use data from August 1989 through December 2010, and the propensity score model (8) labeled  $OP_{F2}$  in Table 1. Dashed lines indicate 90% confidence bands.

sample, the response of the federal funds rate is relatively muted. Figure 9 plots term rate responses to policy interventions in the crisis sample. Consistent with the muted response of the federal funds rate, there is little evidence that crisis-period rate reductions were passed through to the yield curve. This finding is also in line with the findings for the earlier sample periods. Finally, Figure 10 offers little support for the effectiveness of monetary stimulus over the crisis period. In fact the estimates here go the wrong way, suggesting rate reductions reduced output and employment. This may signal a failure of our identifying conditional independence assumption during the crisis period. It should also be noted, however, that the estimated confidence bands for this small sample are especially wide.

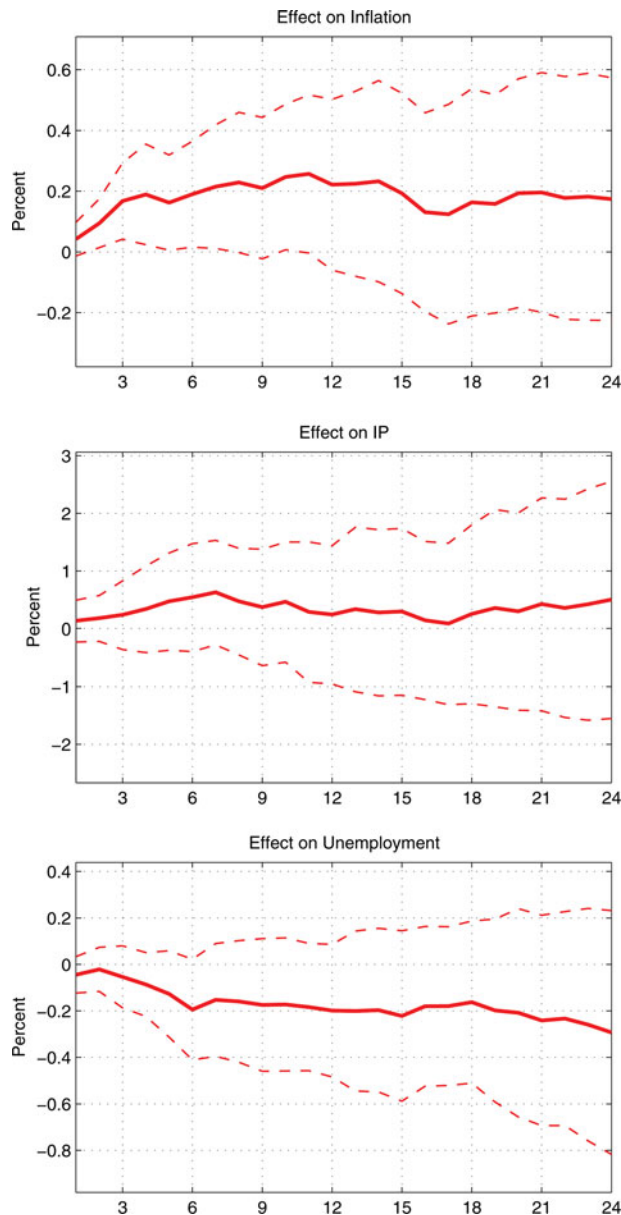


Figure 7. Estimated effects of target rate reductions on macrovariables through 2010. These estimates use data from August 1989 through December 2010, and the propensity score model (8) labeled  $OP_{F2}$  in Table 1. Dashed lines indicate 90% confidence bands.

Table 7. Probit models for target rate reductions during the 2006–2008 period.

	$P_T$ (1)	$P_{F1}$ (2)	$P_{F2}$ (3)
$FFF_t$		1.21* (0.35)	1.93* (0.45)
Inflation	0.08 (0.24)		-0.69* (0.26)
Unemployment Rate	0.24 (0.58)		-0.72 (0.51)
Fed. Funds Rate	0.07 (0.06)	0.01 (0.04)	0.00 (0.05)
Log Likelihood	-16.04	-10.36	-7.45
Sample Size	28	28	28

NOTE: The table shows marginal effects on the probability of no change. \*\*\*/\*\*/\* indicates significance at the 99/95/90% confidence level.

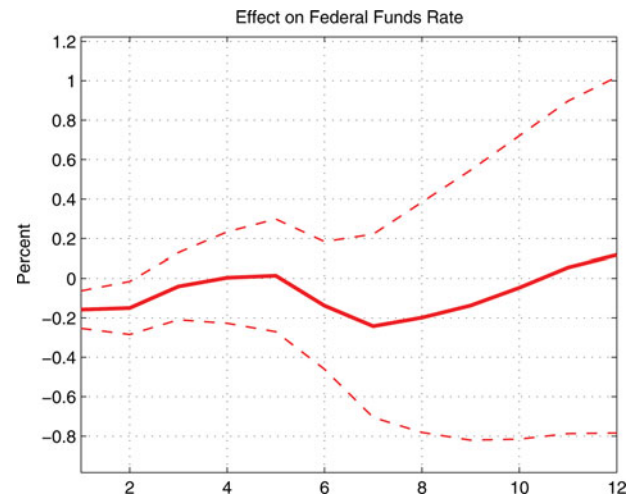


Figure 8. Estimated effects of target rate reductions on the Federal Funds Rate. These estimates use data from October 2006 through December 2009, and the propensity score model labeled  $P_{F2}$  in Table 3. Dashed lines indicate 90% confidence bands.

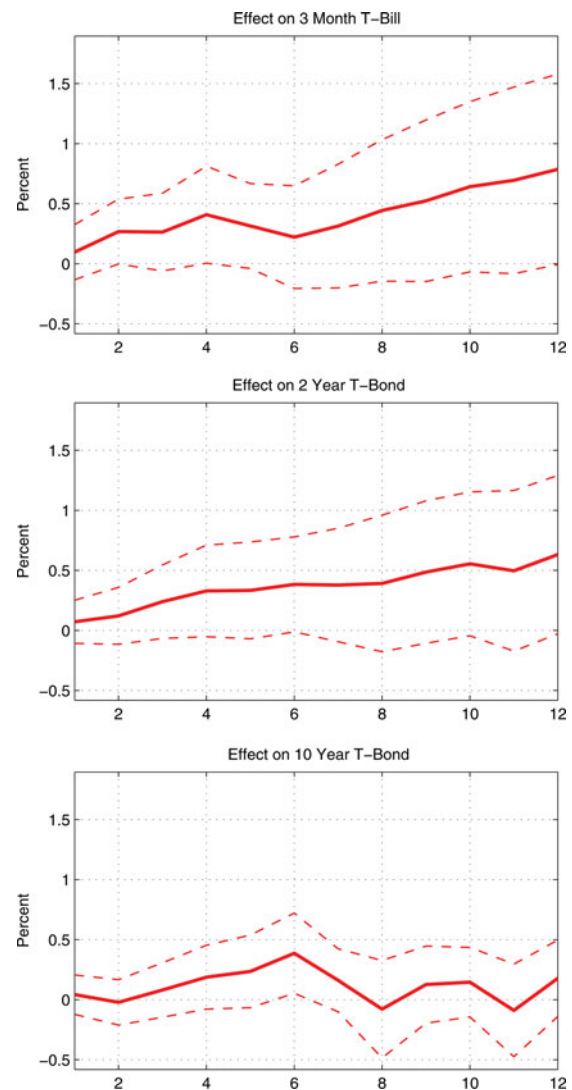


Figure 9. Estimated effects of target rate reductions on bond yields in the Crisis Period. These estimates use data from October 2006 through December 2009, and the propensity score model labeled  $P_{F2}$  in Table 3. Dashed lines indicate 90% confidence bands.

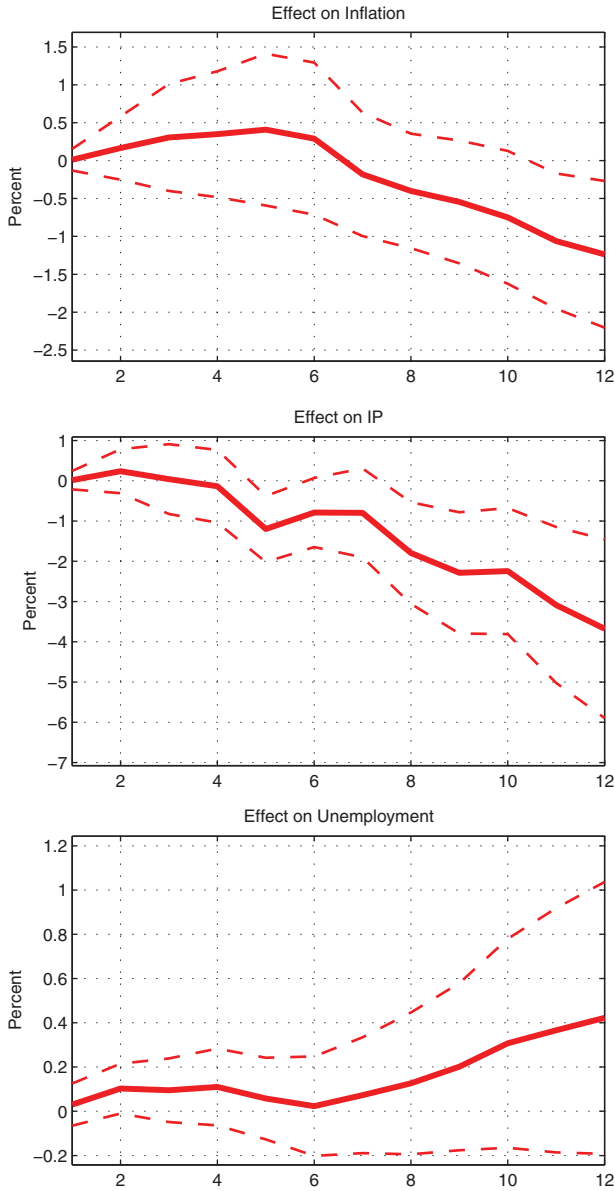


Figure 10. Estimated effects of target rate reductions on macrovariables in the Crisis Period. These estimates use data from October 2006 through December 2009, and the propensity score model labeled  $P_{F2}$  in Table 3. Dashed lines indicate 90% confidence bands.

## 6. SUMMARY AND CONCLUSIONS

We identify causal effects by presuming that policy changes are independent of potential outcomes conditional on observed market-based forecasts of these changes plus a small set of economic predictors. Selection-on-observables is a strong assumption, but a natural starting point for macroeconomic empirical work. We then consider how best to make use of the selection-on-observables identification condition in a potential outcomes framework. The resulting propensity score weighting estimator captures possibly nonlinear and asymmetric causal responses to an ordered dynamic treatment through a simple reweighting procedure. Our framework focuses on the process that determines policy decision; the model for outcomes is left unspecified.

Our propensity score weighting estimator for ordered time series treatments is used to evaluate the effect of monetary policy interventions on macroeconomic outcomes before and during the Great Recession. Results for the pre-recession period suggest an asymmetric response to changes in the federal funds rate target, much as implied by the classic string metaphor for monetary policy. Our findings suggest that target rate increases reduce employment and industrial output, and somewhat less successfully, inflation. At the same time, target decreases appear to have little stimulative effect on output or inflation. Perhaps surprisingly, although the results here are more suggestive than conclusive, an extension of our analysis to cover the “zero lower bound years” since 2008 leaves these findings essentially unchanged.

What explains the asymmetric response of macro aggregates to monetary policy interventions? An important finding emerging from our analysis is the relatively weak effect of target rate reductions on medium and long term bond rates. Because changes in these rates provide an important—perhaps the primary—channel through which policy affects outcomes, the relative unresponsiveness of these bond rates to policy may account for much of the weak impact of target rate reductions on macro aggregates.

This leaves several questions open for macroeconomists to explore. Why do target rate reductions affect the yield curve so moderately? A natural candidate explanation is a failure of policy to aggressively follow through, in which the Fed is not so much pushing on a string as pushing and then laying off a more solid lever. And why is the economy less sensitive to declines in interest rates than to increases?

On the methodological front, applications of our approach in long panels on the one hand and using high-frequency data on the other hand are promising areas for future work. Larger time-series datasets open the door to a less parametric implementation of the methods developed here.

## APPENDIX A

### A.1 Asset Price Based Policy Predictions

Our formulation of the policy propensity score draws on Piazzesi’s (2005) term structure model. Piazzesi (2005) provided an explicit parametric framework that links Fed-policy actions to the yield curve. Her model consists of a monetary policy rule  $p_{\zeta}^j(\zeta_t, \psi)$ , the probability of choosing  $D_t = d_j$ , conditional on information  $\zeta_t$ . Monetary policy then affects the state price density  $M_t$  and consequently, through no-arbitrage arguments, the yield curve. The key insight of Piazzesi’s model that is relevant here is the fact that asset prices, in particular bond yields and related derivatives, depend on the same state vector  $\zeta_t$  as the policy function  $p_{\zeta}^j(\zeta_t, \psi)$ . Under the additional assumption that the pricing functions are invertible, we can recover  $\zeta_t$  with a vector of asset prices. This is particularly appealing because some of the elements in  $\zeta_t$  may not be directly observable.

We rely on a no-arbitrage pricing relationship between the price of zero coupon bonds as well as a federal funds futures contract and the state vector  $\zeta_t$ . Let the state price density  $M_t$  be such that the price at time  $t$  of a random payoff  $V_{\tau}$  at time  $\tau$  is  $E[M_{\tau} V_{\tau} | \zeta_t] / M_t = E_t^Q[V_{\tau}]$ , where  $E_t^Q[V_{\tau}]$  is the expectation operator with respect to the risk neutral measure. Harrison and Kreps (1979) showed that the existence of a state price density is essentially equivalent to the existence of an equilibrium price system, something we impose as an assumption. Using the risk



neutral measure, random payoffs at various maturities are priced as

$$P_t(V_\tau) = E_t^Q[V_\tau]. \quad (\text{A.1})$$

We assume that the relationship between the state vector  $\zeta_t$  and (A.1) is invertible, an assumption that is satisfied for example in affine models. Let  $z_t = (P_t(V_{\tau_0}), \dots, P_t(V_{\tau_q}))$  be a vector of observed price data with maturities  $\tau_0, \dots, \tau_q$  and assume that the pricing function has an inverse  $g$  such that

$$\zeta_t = g(z_t, t, \tau_0, \dots, \tau_q). \quad (\text{A.2})$$

The technique of inverting the yield curve to elicit unobservable state variables is well established in the financial econometrics literature—see Söderlind and Svensson (1997), Singleton (2001), or Piazzesi (2005) for examples. Our empirical model for the propensity score is related to the policy function by

$$p^j(z_t, \psi) := p_\zeta^j(g(z_t, t, \tau_0, \dots, \tau_q), \psi). \quad (\text{A.3})$$

Federal funds futures maturing shortly after FOMC announcements are probably good candidates for  $z_t$ . The reason is that there is a direct link between their expected future cash flow and changes in the federal funds target rate. We focus on the case where no FOMC meeting is announced first, that is,  $s_t^0$ . Notice that in months where an FOMC meeting is scheduled but the change in the target precedes the FOMC meeting, we assume that the change was as if it had happened in a month where the FOMC was not scheduled.

Because macroeconomic data are released at different days throughout the month and because we are interested in good predictors of expected Fed policy for the entire month  $t + 1$ , we concentrate our attention on the futures price on the last day of the prior month  $t$ . Assume for convenience that each month  $t$  has  $\kappa$  days to economize on notation, we denote  $f_{t,\kappa}^1$  the price of a one-month ahead contract traded at the last day of month  $t$ . On any given day  $k$  in month  $t + 1$ , let  $r_{t+1,k}$  be the effective federal funds rate at the close of the market.<sup>10</sup> The payoff for a buyer of a fed funds futures contract is the difference between the futures rate  $f_{t,\kappa}^1$  and the average fed funds rate over month  $t + 1$ ,

$$r_{t+1}^a = \frac{1}{\kappa} \sum_{k=1}^{\kappa} r_{t+1,k} \quad (\text{A.4})$$

with the payoff cash settled the day after expiration of the futures contract (see Piazzesi and Swanson 2008, p. 679). Pricing Equations (A.1) and (A.4) imply that the spread between a funds future  $f_{t,\kappa}^1$  and the prevailing target rate  $\bar{r}_{t,\kappa}$  at the last day  $\kappa$  of month  $t$  is

$$s_t^0 = f_{t,\kappa}^1 - \bar{r}_{t,\kappa} = \frac{E_{t,\kappa}^Q[r_{t+1}^a - \bar{r}_{t,\kappa}]}{P_{t,\kappa}(V_{t+1,\kappa}^0)} \quad (\text{A.5})$$

where  $(t + 1, \kappa)$  denotes the last day of month  $t + 1$  and  $P_{t,\kappa}(V_{t+1,\kappa}^0)$  is the month  $t$ , day  $\kappa$ , price of a zero coupon bond  $V_{t+1,\kappa}^0$  maturing at  $(t + 1, \kappa)$ . Note that  $f_{t,\kappa}^1$  reflects both uncertainty about whether and when a target rate change will occur in month  $t + 1$  and more general uncertainty about the economy captured by the pricing kernel  $M_t$ . Equation (A.5) shows that the futures-target rate spread is the best risk adjusted predictor of a target rate change during the coming month.<sup>11</sup> Whether (A.5) can be inverted to recover  $\zeta_{t,\kappa}$  as in (A.2) depends on the dimension of  $\zeta_t$  as well as the exact functional form of the conditional expectations. In the absence of an explicit pricing model, which would require a more parametric framework than we are willing to entertain, it is ultimately an empirical question whether controlling for  $f_{t,\kappa}^1 - \bar{r}_{t,\kappa}$  in the propensity score is sufficient.

<sup>10</sup>The effective federal funds rate is published by the Federal Reserve Bank of New York. It is the volume weighted daily average of trades arranged by major brokers (source: <http://www.newyorkfed.org/markets/omo/dmm/fedfundsdata.cfm>). Using the model implied rate at the end of the day thus is a slight simplification.

<sup>11</sup>In other words, it minimizes the squared prediction error among all predictors based on  $\zeta_{t,\kappa}$  of  $r_{t+1}^a$  and under the risk neutral discounted measure.

## A.2 Data

- Federal funds futures: CBOT prices of current month and next month federal funds futures contract at market close, cry out market Monday–Friday. Source: Bloomberg.
- Treasury Bond yields: Daily data from the Federal Reserve Board of Governors. Federal Reserve Statistical Release H.15. Daily observations for Market yields on U.S. Treasury securities at one-month constant maturity, quoted on investment basis at 3-month and 2-year, and 10-year maturities. Respective data-codes are “RIFLGFCM03,” “RIFLGFCY02,” “RIFLGFCY10.” Federal funds rate: daily data based on weighted average of brokered trades. Data-code “RIFSPFF\_N.B”
- Macro Data, all monthly frequency. Source: Federal Reserve Bank of St. Louis, Fred. (i) Personal Consumption Expenditures Price Index (PCEPI) seasonally adjusted, Source: Bureau of Economic Analysis; (ii) Industrial Production Index seasonally adjusted (IP), Source: Board of Governors of the Federal Reserve System; (iii) Federal Funds effective Rate in percent per annum (FFED), monthly average, Source: Federal Reserve Statistical Release H.15; (iv) Civilian Unemployment Rate, seasonally adjusted (UNRATE), Source: Bureau of Labor Statistics.
- Data used to construct  $s_t^1$  come from two sources, the Board of Governors of the Federal Reserve System and Bloomberg L.P. The daily futures contract series were obtained using the Bloomberg Terminal software and correspond to equity codes “FF1 Comdty” and “FF2 Comdty.” Data from the Board of Governors include the dates of FOMC meetings<sup>12</sup> as well as the daily series of published Federal Funds rate targets and effective Federal Funds rates. The daily series were obtained via the FRED Excel Add-In from the Federal Reserve Bank of St. Louis, which compiles data from the Board (among other sources). Beginning December 16, 2008, the FOMC moved from a single target rate to a target range, including an upper and lower limit. For these days (of which there are 12 in our sample), we compute the midpoint between the upper and lower limits (0.125% in each of the 12 cases) and use this value in lieu of a single target rate. We collect four series from the Board via FRED, using codes “DFF,” “DFEDTAR,” “DFEDTARL,” and “DFEDTARU,” which respectively correspond to the effective Federal Funds rate, the Federal Funds target rate (series ends December 15, 2008), and the upper and lower limits of Federal Funds target range (beginning December 16, 2008). The sample consists of an observation for each weekday between December 6, 1988 and December 31, 2008. We replace missing values (due to holidays) using the value from the previous day.

## SUPPLEMENTARY MATERIALS

This supplemental appendix provides additional information on the construction of the futures-market-based policy predictor, develops asymptotic theory for our estimators, and describes our specification test in detail.

## ACKNOWLEDGMENTS

Thanks go to Jordi Galí, Emi Nakamura, Jon Steinsson, Ivan Werning, and seminar participants at the Bank of England, the European Central Bank, the Federal Reserve Bank of San Francisco, the NBER Summer Institute 2013, and Yale University for helpful suggestions and comments. The views expressed here are solely the responsibility of the authors and should not be interpreted as representing the views of the Federal Reserve Bank of San Francisco or the Board of Governors of the Federal Reserve System. Early Elias, Helen Irvin, and Daniel Molitor provided expert research assistance.

<sup>12</sup>Retrieved from <http://www.federalreserve.gov/monetarypolicy/fomchistorical/YYYY.htm> where YYYY indicates the year in 1988–2008.



[Received February 2016. Revised June 2016.]

## REFERENCES

- Angrist, J. D., and Imbens, G. W. (1995), "Two-Stage Least Squares Estimates of Average Causal Response in Models With Variable Treatment Intensity," *Journal of the American Statistical Association*, 90, 431–442. [3]
- Angrist, J. D., and Kuersteiner, G. M. (2004), "Semiparametric Causality Tests Using the Policy Propensity Score," *NBER Working Paper 10975*. [2]
- Angrist, J. D., and Kuersteiner, G. M. (2011), "Causal Effects of Monetary Shocks: Semiparametric Conditional Independence Tests With a Multinomial Propensity Score," *Review of Economics and Statistics*, 93, 725–747. [2,3,4,6,10]
- Baglioni, F. C., and Favero, C. A. (1998), "Measuring Monetary Policy With VAR Models: An Evaluation," *European Economic Review*, 42, 1069–1112. [1]
- Bernanke, B. S., and Kuttner, K. N. (2005), "What Explains the Stock Market's Reaction to Federal Reserve Policy?" *Journal of Finance*, 60, 1221–1257. [5]
- Blanchard, O., and Galí, J. (2010), "Labor Markets and Monetary Policy: A New Keynesian Model With Unemployment," *American Economic Journal: Macroeconomics*, 2, 1–30. [6]
- Christiano, L. J., Eichenbaum, M., and Evans, C. L. (1996), "The Effects of Monetary Policy Shocks: Evidence from the Flow of Funds," *The Review of Economics and Statistics*, 78, 16–34. [2,11]
- Christiano, L. J., Eichenbaum, M., and Evans, C. L. (1999), "Monetary Policy Shocks: What Have We Learned and to What End?," in *Handbook of Macroeconomics* (vol. 1), ed. J. B. Taylor, and M. Woodford, Amsterdam, The Netherlands: Elsevier Science B.V., chap. 2, pp. 65–148. [1,2,9,11]
- Christiano, L. J., Trabandt, M., and Walentin, K. (2011), "DSGE Models for Monetary Policy Analysis," in *Handbook of Monetary Economics* (vol. 3A), eds. B. M. Friedman, and M. Woodford, The Netherlands: North Holland, pp. 285–367. [1]
- Clarida, R., Galí, J., and Gertler, M. (2000), "Monetary Policy Rules and Macroeconomic Stability: Evidence and Some Theory," *The Quarterly Journal of Economics*, pp. 147–180. [2]
- Cochrane, J. H. (1994), "Comment on: What Ends Recessions? by Christina D. Romer and David H. Romer," in *NBER Macroeconomics Annual* (vol. 9), eds. S. Fischer, and J. J. Rotemberg, Cambridge, MA: MIT Press. [1,3]
- Cochrane, J. H., and Piazzesi, M. (2002), "The Fed and Interest Rates: A High Frequency Identification," *The American Economic Review*, 92, 90–95. [5,9,11]
- Cover, J. P. (1992), "Asymmetric Effects of Positive and Negative Money-Supply Shocks," *The Quarterly Journal of Economics*, 107, 1261–1282. [11]
- Dehejia, R. H., and Wahba, S. (1999), "Causal Effects in Nonexperimental Studies: Reevaluating the Evaluation of Training Programs," *Journal of the American Statistical Association*, 94, 1053–1062. [2]
- Delong, J. B., and Summers, L. H. (1988), "How Does Macroeconomic Policy Affect Output?" *Brookings Papers on Economic Activity*, 2, 433–494. [11]
- Faust, J., Swanson, E. T., and Wright, J. H. (2004), "Identifying VARs Based on High Frequency Data," *Journal of Monetary Economics*, 51, 1107–1131. [2,5,11]
- Friedman, M., and Schwartz, A. J. (1963), *A Monetary History of the United States: 1867–1960*, Princeton, NJ: National Bureau of Economic Research, Princeton University Press. [1]
- Galí, J. (2011), "Monetary Policy and Unemployment," in *Handbook of Monetary Economics* (vol. 3A), B. M. Friedman and M. Woodford, eds., North-Holland: Amsterdam, The Netherlands: Elsevier Science B.V., chap. 10, pp. 487–546. [6]
- Galí, J., and Gertler, M. (2007), "Macroeconomic Modeling for Monetary Policy Evaluation," *Journal of Economic Perspectives*, 21, 25–45. [2]
- Gallant, A. R., Rossi, P. E., and Tauchen, G. (1993), "Nonlinear Dynamic Structures," *Econometrica*, 61, 871–907. [4]
- Granger, C. W. J. (1969), "Investigating Causal Relations by Econometric Models and Cross Spectral Methods," *Econometrica*, 37, 424–438. [2]
- Gürkaynak, R. S., Sack, B. P., and Swanson, E. (2005), "The Sensitivity of Long-Term Interest Rates to Economic News: Evidence and Implications for Macroeconomic Models," *American Economic Review*, 95, 425–436. [5]
- Gürkaynak, R. S., Sack, B. P., and Swanson, E. T. (2007), "Market-Based Measures of Monetary Policy Expectations," *Journal of Business and Economic Statistics*, 25, 201–212. [5]
- Hamilton, J. D. (2008), "Assessing Monetary Policy Effects Using Daily Federal Funds Futures Contracts," *Federal Reserve Bank of St. Louis Review*, 90, 377–393. [2,5]
- (2009), "Daily Changes in Fed Funds Futures Prices," *Journal of Money, Credit and Banking*, 41, 567–582. [5]
- Hamilton, J. D., and Jordà, O. (2002), "A Model of the Federal Funds Rate Target," *Journal of Political Economy*, 110, 1135–1167. [5,6,10]
- Hansen, L. P. (2008), "Generalized Methods of Moments Estimation," in *The New Palgrave Dictionary of Economics* (2nd ed.), eds. S. N. Durlauf, and L. E. Blume, London: Macmillan. [5]
- Harrison, H. M., and Kreps, D. M. (1979), "Martingales and Arbitrage in Multiperiod Securities Markets," *Journal of Economic Theory*, 20, 381–408. [15]
- Heckman, J. J., Ichimura, H., and Todd, P. E. (1998), "Matching as an Econometric Evaluation Estimator," *Review of Economic Studies*, 65, 261–294. [2]
- Hirano, K., Imbens, G., and Ridder, G. (2003), "Efficient Estimation of Average Treatment Effects using the Estimated Propensity Score," *Econometrica*, 71, 1161–1189. [4]
- Hoover, K. D., and Pérez, S. J. (1994), "Post hoc Ergo Propter hoc Once More: An Evaluation of 'Does Monetary Policy Matter?' in the Spirit of James Tobin," *Journal of Monetary Economics*, 34, 47–74. [2]
- Horvitz, D., and Thompson, D. (1952), "A Generalization of Sampling Without Replacement From a Finite Population," *Journal of the American Statistical Association*, 47, 663–685. [4]
- Jordà, O. (2005), "Estimation and Inference of Impulse Responses by Local Projections," *American Economic Review*, 95, 161–182. [2]
- Kareken, J., and Solow, R. M. (1963), "Lags in Monetary Policy," in *Commission on Money and Credit, Stabilization Policies*, Englewood Cliffs, NJ: Prentice-Hall, pp. 14–96. [3]
- Karras, G. (1996), "Are the Output Effects of Monetary Policy Asymmetric?" *Oxford Bulletin of Economics and Statistics*, 58, 267–78. [11]
- Keynes, J. M. (1936), *The General Theory of Employment, Interest and Money*, London: Macmillan. [11]
- Krueger, J. T., and Kuttner, K. N. (1996), "The Fed Funds Futures Rate as a Predictor of Federal Reserve Policy," *The Journal of Futures Markets*, 16, 865–879. [5]
- Kuttner, K. N. (2001), "Monetary Policy Surprises and Interest Rates: Evidence From the Fed Funds Futures Market," *Journal of Monetary Economics*, 47, 523–544. [2,5]
- Meulendyke, A.-M. (1998), *U.S. Monetary Policy and Financial Markets*, New York: Federal Reserve Bank of New York. [5]
- Piazzesi, M. (2005), "Bond Yields and the Federal Reserve," *Journal of Political Economy*, 113, 311–344. [15,16]
- Piazzesi, M., and Swanson, E. T. (2008), "Futures Prices as Risk-Adjusted Forecasts of Monetary Policy," *Journal of Monetary Economics*, 55, 677–691. [2,5,16]
- Ravn, M. O., and Sola, M. (2004), "Asymmetric Effects of Monetary Policy in the United States," *Federal Reserve Bank of St. Louis Review*, 86, 41–60. [11]
- Romer, C. D., and D. H. Romer, (1989), "Does Monetary Policy Matter? A New Test in the Spirit of Friedman and Schwartz," *NBER Macroeconomics Annual*, 4, 121–170. [1]
- (1994), "Monetary Policy Matters," *Journal of Monetary Economics*, 34, 75–88. [2]
- (2004), "A New Measure of Monetary Shocks: Derivation and Implications," *The American Economic Review*, 94, 1055–1084. [2,11]
- (2013), "The Most Dangerous Idea in Federal Reserve History: Monetary Policy Doesn't Matter," *American Economic Review: Papers and Proceedings*, 103, 55–60. [1]
- Rosenbaum, P. R., and Rubin, D. B. (1983), "The Central Role of the Propensity Score in Observational Studies for Causal Effects," *Biometrika*, 70, 41–55. [2]
- Scotti, C. (2011), "A Bivariate Model of Federal Reserve and ECB Main Policy Rates," *International Journal of Central Banking*, 7, 37–78. [6]
- Sims, C. A. (1972), "Money, Income and Causality," *American Economic Review*, 62, 540–562. [2]
- (1992), "Interpreting the Macroeconomic Time Series Facts: The Effects of Monetary Policy," *European Economic Review*, 36, 975–1000. [10]
- Singleton, K. J. (2001), "Estimation of Affine Asset Pricing Models Using the Empirical Characteristic Function," *Journal of Econometrics*, 102, 111–141. [16]
- Söderlind, P., and Svensson, L. (1997), "New Techniques to Extract Market Expectations From Financial Instruments," *Journal of Monetary Economics*, 40, 383–429. [16]
- Tenreiro, S., and Thwaites, G. (2013), "Pushing on a String: US Monetary Policy is Less Powerful in Recessions," *CEP Discussion Paper No. 1218*. [2,11]

- Thapar, A. (2008), "Using Private Information to Estimate the Effects of Monetary Policy," *Journal of Monetary Economics*, 55, 806–824. [5]
- Wingender, A. M. (2011), "Monetary Policy Shocks and Risk Premia in the Interbank Market," *The B.E. Journal of Macroeconomics*, 11, 1–19. [5]
- Woodford, M. (2001), "The Taylor Rule and Optimal Monetary Policy," *American Economic Review*, 91, 232–237. [2]
- Wooldridge, J. M. (2010), *Econometric Analysis of Cross Section and Panel Data* (2nd ed.), Cambridge, MA: MIT Press. [xxxx]