Semiparametric Estimates of Monetary Policy Effects: String Theory Revisited*

Joshua D. Angrist
MIT and NBER

Òscar Jordà
Federal Reserve Bank of San Francisco
and U.C. Davis

Guido M. Kuersteiner
University of Maryland

August 2014

Abstract

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 macro-dynamic setting, without the need for assumptions about the process generating macroeconomic outcomes. The proposed procedure, based on propensity score weighting, easily accommodates asymmetric and nonlinear responses. Application of this estimator shows clear effects of monetary tightening on the yield curve and on economic activity. Monetary accommodation, however, appears to generate a less pronounced change in yields and little change in output. Estimates for recent financial crisis years are similar to those for the earlier, pre-crisis period.

Keywords: policy response, non-linear, semiparametric, policy propensity score, local projections, vector autoregression.

JEL codes: C14, C22, C54, E42, E43, E44, E47, E51

*Thanks go to Jordi Galì, 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 reáecting the views of the Federal Reserve Bank of San Francisco or the Board of Governors of the Federal Reserve System. Early Elias and Daniel Molitor provided expert research assistance. Contact Information: angrist@mit.edu; oscar.jorda@sf.frb.org; kuersteiner@econ.umd.edu

"Our credibility will ultimately be judged by how we do on both of these mandates, not just the price mandate," Mr. (Charles) Evans said Tuesday night. "I think we will be judged very badly" if officials do not act forcefully to reduce unemployment and instead, he said, "worry obsessively" about inflation.

"There's little more that we can do," Mr. (Jeffrey) Lacker said of monetary policy. "There's little more that we can contribute to growth."

- The *New York Times*, April 2, 2013

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 capital 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? Many have recognized that this question is easy to ask but hard to answer. Cochrane (1994) and Romer and Romer (2013) remind 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 likely provides a poor control for another.

Many contemporary investigations of macro policy rely on structural models of economic behavior to solve this fundamental identification problem (see, for example, 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 mimic 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 argument fleshed out in Hoover and Pérez, 1994; see Romer and Romer, 1994 for a rebuttal).

This paper 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 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's 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 buys us 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, Galí and Gertler (2000), Woodford (2001) and Galí and Gertler (2007) for recent contributions. Propensity score methods, introduced by Rosenbaum and Rubin (1983), have proven useful for cross-sectional causal inference (see, e.g., Dehejia and Wahba (1999) and 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 anticipated policy. 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-dimension aggregator of publicly available information. This in turn leads to parsimonious policy models well suited to a data-poor time series setting.

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 interest in 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.

The asymmetric response to policy uncovered here in part reflects the more gradual implementation of accommodative policy. In addition, while our estimates suggest federal funds target rate increases feed strongly into other rates, target rate reductions do not. The combination may explain the failure of rate reductions to stimulate output. The question of whether this lack of a "first stage" reflects policymakers' failure to follow through on target rate declines once initiated, or a structural insensitivity of yields to target rate declines, remains open.

2 Potential outcomes and macro causal effects

2.1 Conceptual framework

The economy is described by the vector, $\chi_t = (x_t', y_t', D_t')'$, where y_t is a vector of outcome variables, D_t is a vector of policy variables that takes on values $d_0, ..., d_J$, and x_t is a vector of other contemporaneous covariates.

Policy is determined by lagged economic conditions, lagged policy choices, and covariates, combined in the vector $z_t = (x_t', \chi_{t-1}')'$. The policy regime is indexed by a parameter, ψ, which takes values in a parameter space Ψ. In addition, policymakers are assumed to react to idiosyncratic information or taste variables, represented by the scalar ε_t, that we don't get to see. The realized policy D_t is determined by both observed and unobserved variables according to $D_t = D(z_t, \psi, \varepsilon_t)$. For identification purposes, we assume that ε_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 χ_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 ψ, 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 $D_t = D(z_t, \psi, \varepsilon_t) = d$, for all possible policy choices d, $d \in \{d_0, ..., d_j, ..., d_J\}$.*

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. The causal effect of a policy change is defined as the difference $y_{t,l}^{\psi}(d_j) - y_{t,l}^{\psi}(d_0)$, where d_j indicates an intervention and d_0 indicates benchmark policy. This notation makes a conceptual distinction between changes to the policy regime (indicated by changes in ψ) 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. Cochrane (1994), for example, quotes Kareken and Solow (1963):"... 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." (emphasis added)

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

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

Average policy effects are given by

$$E\left[Y^{\psi}_{t,L}(d_j) - Y^{\psi}_{t,L}(d_0)\right] \equiv \theta_j, \tag{2}$$

where $\theta_j = \left(\theta'_{1,j}, ..., \theta'_{L,j}\right)'$ describes the response of y_t to policy d_j at horizons 1 to L. The effects of all possible policy changes are summarized by $\theta = \left(\theta'_1, ..., \theta'_J\right)'$, a vector of dimension $k = k_y \times L \times J$, 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, θ describes an average generalized impulse response function for all possible policy choices which in addition can be both asymmetric and nonlinear.

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:*

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

Our conditional independence assumption focuses on variation in policy interventions while holding the policy regime fixed, after conditioning on observables, z_t. Leeper and Zha's (2003) notion of *modest policy interventions* captures the same idea.

Using Equation (1) and Condition 1, we can write the average policy effect conditional on z_t in terms of observable distributions as:

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

Although cast in terms of in-principle 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 function. Angrist and Kuersteiner (2011) call this model the *policy propensity score*.

The policy propensity score is $P(D_t = d_j|z_t) = p^j(z_t, \psi)$, 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

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

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

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

This weighting scheme was first used to estimate population means in non-random 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.[1]

The estimand described by (5) is similar to that approximated using local linear projections in Jordà (2005), though here no approximation is required. The estimand can also be related to the nonlinear impulse response function introduced by Gallant, Rossi and Tauchen (1993). The latter is based on estimation of $E\left[y_{t+l}|x_t\right]$ where $x_t=(y_t,...,y_{t-p})$ and y_t is assumed to be a Markov process. An impulse response function can then be defined as a marginalized version of $E\left[y_{t+l}|x_t^+\right]-E\left[y_{t+l}|x_t\right]$ where x_t^+ perturbs y_t by a constant. Although $E\left[y_{t+l}|x_t\right]$ is in principle nonparametrically identified, extrapolation to counterfactual $E\left[y_{t+l}|x_t^+\right]$ 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 our model for $p^j\left(z_t,\psi\right)$. We need 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 Connection to VARs and a discussion of identification

This section explains the connection between expression (5) and impulse responses calculated from an identified vector autoregression (VAR). Suppose the data are generated by the following, highly stylized, bivariate structural VAR(1) relating the outcome y_t to the policy variable D_t :

$$\begin{pmatrix} y_t \\ D_t \end{pmatrix}=\begin{pmatrix} a_{yy} & a_{yd} \\ a_{dy} & a_{dd} \end{pmatrix}\begin{pmatrix} y_{t-1} \\ D_{t-1} \end{pmatrix}+\begin{pmatrix} 1 & 0 \\ c_{dy} & 1 \end{pmatrix}\begin{pmatrix} u_t \\ \varepsilon_t \end{pmatrix} \tag{6}$$

[1] 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 condition on covariates, they're also independent of treatment conditional on the propensity score.

with $E(u_t) = E(\varepsilon_t) = 0; E(u_t, \varepsilon_t) = 0$; and $0 < \sigma_u^2, \sigma_\varepsilon^2 < \infty$. This model is used for expositional simplicity but it is not hard to see how it would be extended to a multivariate context.

Expression (6) is broadly representative of the manner VARs are specified in monetary economics. Consider the equation for D_t first, often interpreted as the policy reaction function. The systematic response of the policymaker to lagged values of the outcome is captured through a_{dy} whereas a_{dd} captures a policy smoothing factor. In rational expectations models, the systematic response of the policymaker is assumed to be fully anticipated by the public. In addition, we allow policy to respond to contemporaneous information on the outcome through c_{dy} (the normalization of the diagonal terms to 1 is largely inconsequential and used for convenience). This is a common assumption in the literature. It is meant as a parable for the ability of the policymaker to respond to new information quickly relative to how movements in the policy rate affect outcomes. Finally ε_t captures fluctuations in the policy rate that cannot be explained through the systematic (and possibly anticipated) component of policy.

The effect of policy on y_t is captured by a_{yd} in the outcome equation. This coefficient implicitly summarizes how policy and expectations about systematic policy interact. Because of these interactions, rather than interpreting a_{yd} directly, it is often more convenient to focus on the propagation mechanisms of unanticipated/unsystematic policy fluctuation as measured by ε_t.

Specifically, the structural impulse response from (6) can be written in parallel fashion to expression (5) as:

$$R(y^{\psi}_{t,l}, d_j - d_0) \equiv E\left\{E\left[y_{t+l}|z_t; \varepsilon_t = d_j\right] - E\left[y_{t+l}|z_t; \varepsilon_t = d_0\right]\right\} \tag{7}$$

where $z_t = (y_{t-1}, D_{t-1})'$ in this particular example and $\psi = (a_{dy}, a_{dd}, c_{dy})'$. In a VAR usually one chooses $d_0 = 0$ and because of linearity, it is only relevant to know what the experimental increment $d_j - d_0$ is (rather than d_j and d_0 individually) since it does not depend on d_0. On impact, we have

$$\begin{aligned} R(y^{\psi}_{t,0}, d_j - d_0) &= E\left\{E\left[a_{yy}y_{t-1} + a_{yd}D_{t-1} + u_t|z_t; \varepsilon_t = d_j\right] - E\left[a_{yy}y_{t-1} + a_{yd}D_{t-1} + u_t|z_t; \varepsilon_t = d_0\right]\right\} \\ &= E\left\{a_{yy}y_{t-1} + a_{yd}D_{t-1} - a_{yy}y_{t-1} + a_{yd}D_{t-1}\right\} = 0 \end{aligned}$$

since $E(u_t|z_t; \varepsilon_t) = 0$ which reflects the assumption that outcomes do not respond contemporaneously to policy. However, by period 1 notice that, by repeated substitution of (6) in y_{t+1} and taking expectations as in (7) we have

$$R(y^{\psi}_{t,1}, d_j - d_0) = a_{yd}(d_j - d_0)$$

and repeating this forward substitution process for $l = 2, 3, ...$ would simply trace out the impulse response of the structural VAR in (6). Importantly, notice that identification comes from focusing on fluctuations in the *unsystematic* component of policy, ε_t. The maintained assumption of the VAR literature and in our framework is that the systematic component of monetary policy remains constant —specifically, the coefficients a_{dy}, a_{dd} and c_{dy} remain invariant. Furthermore by conditioning on z_t we are effectively controlling for changes in y_{t-1} and D_{t-1} —policy variation due to its predictable components. This feature of VARs parallels that in our own setup except that our assumptions are more general as they allow for possible nonlinearities.

Thus, the propensity score weighting in expression (5) can be seen as meeting two objectives. At a statistical level and under the more realistic scenario in which the data generating process is unknown, it allows one to calculate $E(y^{\psi}_{t,l}(d_j)|z_t)$ flexibly yet conveniently. At a more intuitive level, the reweighting procedure gives more weight to observations that are harder to sort into one policy bin or another. In other words, it emphasizes those observations for which the random element of policy allocation (the ε_t in our example) is the greatest.

2.3 Estimation

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

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

In a correctly specified model, these weights have mean zero and are uncorrelated with z_t. To ensure this is true, for outcomes other than interest rates which do not exhibit a trend, we weight with the residuals from a regression of $\delta_{t,j}\left(\hat{\psi}\right)$ on z_t and a constant. Define $\hat{h}_t = Y_{t,L}\left(\delta_{t,j}\left(\hat{\psi}\right) - \hat{\delta}_{t,j}\right)$, where $\hat{\delta}_{t,j}$ is the relevant fitted value from this auxiliary regression. The average causal response vector is then estimated as the sample average

$$\hat{\theta} = T^{-1}\sum_{t=1}^{T}\hat{h}_t. \tag{8}$$

The estimator $\hat{\theta}$ solves

$$\hat{\theta} = \arg\min_{\theta}\left(T^{-1}\sum_{t=1}^{T}\hat{h}_t - \theta\right)'\Omega^{-1}\left(T^{-1}\sum_{t=1}^{T}\hat{h}_t - \theta\right), \tag{9}$$

a minimum distance objective function. This objective function has an equal number of parameters and moment conditions. Therefore, the choice of Ω does not affect the solution to (9). Sometimes, however, it is interesting to restrict θ. For example, to facilitate comparisons with VAR's, symmetric responses can be estimated by assuming that the effect of d_j and $-d_j$ are of the same magnitude but opposite in sign. In order to accommodate restrictions $\theta = \theta\left(\alpha\right)$, where α is a reduced set of free parameters, we rewrite (9) as follows:

$$\hat{\alpha} = \arg\min_{\alpha}\left(T^{-1}\sum_{t=1}^{T}\hat{h}_t - \theta\left(\alpha\right)\right)'\Omega^{-1}\left(T^{-1}\sum_{t=1}^{T}\hat{h}_t - \theta\left(\alpha\right)\right). \tag{10}$$

The dimension of α is smaller than the number of moment conditions. In that case, the optimal Ω for this over-identified scenario is the spectral density matrix of $\hat{h}_t$ at zero frequency, which can be estimated as detailed in Newey and West (1994). Asymptotic approximations to the sampling distribution of $\hat{\theta}$ or $\hat{\alpha}$ account for the fact that ψ is estimated in a first stage. The relevant limiting distributions for estimators and test statistics are derived in the supplemental appendix.

3 A propensity score for monetary policy interventions

Following a long tradition, we measure monetary 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, provides 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 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 FOMC meetings are very nearly a monthly occurrence, we work with monthly data.

3.1 Federal funds rate targeting since 1989

The ideal propensity score would model how the federal funds rate is determined by the policymaker. A vast literature in monetary economics often characterizes the central bank's policy choices using deviations of inflation from a target (say, two percent), some measure of economic activity in deviation from a "natural" level (be it output from potential or the unemployment rate from the NAIRU), and some policy smoothing component. In summary, some version of a Taylor (1993) rule.

In practice the central bank uses all available information and we too set the bar higher. Using financial derivatives based on the federal funds rate, we rely on the notion that market participants similarly use all information available to properly price such derivatives. In our application, we rely on federal funds rate futures (FFF) contracts. The market for these contracts started in October 1988, but we follow the literature and rely only on data that start in July 1989 to avoid distortions associated with the launch of the market. This is therefore the start of our sample.

Next 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 began. 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 to 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. This is a small sample and it imposes some limits to the experiments we can conduct that we will discuss below.

According to Meulendyke (1998), the Federal Open Market Committee's (FOMC) transition to fed funds rate targeting was complete following the stock market crash in October 1987 although Hamilton and Jordà (2002) date this transition a bit later, to 1989 and thus coinciding with the start of our sample.[2] We note that 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.[3] The space of possible policy choices is defined here to be {-0.50%, -0.25%, 0%, 0.25%, 0.50% }, where the ±0.50% events include the larger changes.

3.2 Policy propensity score specification

Efficient markets price futures contracts using all available information. This motivates students of monetary policy to define policy shocks as deviations from the optimal 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) uses 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.[4]

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 ζ_t. Piazzesi shows that bond yields and related derivatives likewise depend on ζ_t. Under the additional assumption that pricing functions are invertible, we can replace ζ_t, which may be only partially observable, with a vector of observed asset prices z_t. This theoretical argument is fleshed out in the appendix.

As a practical matter, our analysis distinguishes between months with scheduled FOMC meetings ("meeting months") and months without. In non-meeting 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. Meeting months on the other hand, require special adjustments to reflect the microstructure of the federal funds market and the reserve maintenance period[5] in particular. We reserve

[2] 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.

[3] The relevant months are December 1990, December 1991, and January 2001.

[4] Krueger and Kuttner (1996), Kuttner (2001), Piazzesi and Swanson (2008), Gürkaynak et al. (2005, 2007) and Hamilton (2009) discuss the efficiency of the market for federal funds futures.

[5] Reserves are averaged over a two week period to determine whether the reserve requirement is met. This two-week

a more detailed discussion for the appendix. In broad terms, we construct s_t^1, a predictor based on the scaled difference (to adjust when within the month the FOMC meeting takes place) 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. In addition, we factor in 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.

With a few exceptions, our predictor for meeting months is s_t^1, while in non-meeting months, s_t^0 is used. 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 FFF_t, equal to either s_t^0 or s_t^1, as described above.

Because target changes are naturally ordered in 0.25% increments over the range ±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, such as might appear in a conventional 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. Moreover, our "Taylor Rule" specification can be motivated by results of Blanchard and Galí (2010) and Galí (2011) who 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 a number of 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 time. Finally, the model includes

maintenance period starts on a Thursday and ends on *settlement* Wednesday.

dummies for the Y2K event and the September 11, 2001 attacks.[6]

3.3 Policy propensity score estimates

Table 1 reports average marginal effects of the effects of 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, we note that the end dates for the outcome samples are July 2007 and December 2010, respectively.

Estimates of a benchmark Taylor-type 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} 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) (labeled OP_{T1}) use same-month measures of inflation and unemployment only, while the estimates reported in columns (2) and (6) (labeled OP_{T2}) are from models that add inflation and unemployment lags, the size of the last target change, and seasonal and scheduling dummies.[7]

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_t terms that differ in the pre-crisis and full samples. The results indicate that market-based factors are better predictors of target rate changes than the combination of inflation and unemployment and their lags in a Taylor specification. The pseudo-R^2s in columns (3) and (7) (OP_{F1}), based on estimates of the policy propensity score using these factors but excluding inflation and unemployment terms are virtually identical to the pseudo-R^2s from the specifications augmented with these Taylor model terms and reported in columns (4) and (8) (OP_{F2}). Neither inflation nor unemployment marginal effects are significantly different from zero when estimated in the more elaborate models.

Fitted values from the full policy score model (8) 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_{F2C} estimates from column (8) of Table 1. The figure also shows the time series of Industrial Production (IP) growth to mark cyclical fluctuations. We overlay IP growth in this figure to illustrate that fluctuations in the propensity score are not associated with fluctuations in the business cycle.

An important diagnostic for our purposes looks at whether lagged macro aggregates are independent

[6] The scale factor is defined as $\kappa/(\kappa - t)$ where κ is the number of days in a given month and t is the day of the month when the FOMC meeting is scheduled.

[7] Detailed variable definitions and sources appear in Appendix B.

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) develop semiparametric tests that can be used for this purpose. Panel A of Table 2 reports test results for the null hypothesis of orthogonality between the policy innovation, $\{D_t = d_j\} - p^j(z_t, \psi)$, and conditioning variables in the model. Panel B looks at the correlation between innovations and lagged outcomes. Both panels show p-values for joint (3 *df*) tests of the null hypothesis that the score model indicated in the column headings is an adequate specification of the conditional probability that the target rate is unchanged, rises by a quarter point, or falls by a quarter point (these are the shocks analyzed below). The table also shows p-values for joint orthogonality of an aggregate market based factor FFF_t. We consider linear and quadratic terms of FFF_t ($2 \times 3 = 6$ *df*).

Test results for the simple Taylor model (reported in columns labelled OP_{T1}) show substantial correlation with economic variables, 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 labelled OP_{T2}) still show some evidence of correlation between FFF_t and the average federal funds rate with policy innovations, especially in the longer sample. Test results for the model with FFF_t alone (reported in columns labelled OP_{F1}) mostly pass, contrary to those for the expanded Taylor model, OP_{T2}. Similarly, test results for the model that includes both FFF_t and inflation and unemployment terms (reported in columns labeled OP_{F2}) offer little evidence against the hypothesis of random policy innovations. In what follows, we proceed using the p-score model based on OP_{F2}.

4 Dynamic 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 the 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 25 basis point changes, up and down, out to a horizon of 24 months. Figures 2, 3 and 4 plot these estimated responses, constructed using propensity score model $OP_{F2/C}$ (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 (pps) after about a year, then falling to under half a percentage point after two years. This pattern is similar to that found in VAR-type estimates (e.g., Figure 3 in Christiano et al. 1999, when cumulated). By contrast, a 25 basis points (bps) reduction lowers the federal funds rate by less than -0.4 pp one year out, although this decline largely endures for two years. Table 6 shows that the federal funds rate response to a 25bps decrease in the target at the 6, 12, and 18-month marks is about half the responses to a 25bps 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.

Estimated effects on macroeconomic aggregates are reported in Figure 4 and Table 4. A 25bps 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's worth noting that inflation is low in our sample period, with only modest deviations around the two percent level the Fed sees as desirable. Interestingly, we see no evidence of a "price puzzle," that is, the common finding in VAR-based estimates of the effects of monetary shocks that inflation *increases* in the short-run with an increase in the target (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 percent, equivalent to about a 0.75 percent 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 six 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.

Target rate reductions generate a markedly different pattern of responses, far from mirroring that seen in our estimates of the effects of rate increases. This pattern is documented in Figure 3, 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 percent in response to an increase of 25bps in the target, but remain unchanged to a 25bps decrease. Industrial production declines by a significant (economically and statistically) -1.7 percent in response to a target increase, but is essentially unchanged (at an insignificant -0.2 percent) when the target is reduced. The unemployment rate increases by 0.3 percentage point when the target is increased but is unresponsive when the target is reduced instead. The asymmetry in yield curve and macro aggregate responses to US 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. This weakness in turn may reflect either the Fed's failure to follow through in its efforts to push yields down or a structural insensitivity in the yield response to target rate reductions.

4.1 Other Comparisons and Context

In an influential study of the effects of monetary policy shocks on the yield curve and macro variables, Cochrane and Piazzesi (2002) report estimates of policy effects on the yield curve similar to ours. On the other had, 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) use policy-induced changes in federal funds futures price 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, Tenreyro and Thwaites (2013) identify 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, monetary 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 does. Using international data, Karras (1996) finds 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).

5 Crisis Intervention

Did the Great Recession change the effects of monetary policy? This section addresses this question 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 we limit the horizon to 12 months for the crisis sample).

Figure 5 and Table 6 show the estimated response of the federal funds rate in the longer sample (the first of our two experiments). We report results only for the case where the Fed lowered the target rate by 25bps 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 the shorter sample.

Estimated term rate responses to a decrease in the target of 25bps are plotted in Figure 6 and Table 5. There is very little difference (certainly none based on statistical criteria) between the responses obtained using each sample. 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 response of the longer yields is estimated quite imprecisely with point estimates being mildly positive, which could be a reflection of inflation expectations being priced into the yield curve.

Finally, macro aggregate responses in the longer sample, reported in Figure 7, are somewhat more symmetric. They show that a target rate reduction affects inflation positively, with an average increase of 0.2 percent in the price level occurring mostly in the first six months. The response of IP is largely statistically insignificant with an increase of just 0.5 percent after two years. The unemployment rate response is somewhat more visible and at the two year mark drops by about 0.3 pp although estimated imprecisely. Overall the responses of the macro variables to rate reductions based on the larger sample have the expected signs, but the overall effects are small and not statistically significant, just as we found in the shorter sample.

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 regular binary 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). Notice that the sample only contains 28 observations, which limits considerably the specifications we can consider and the statistical precision of the estimates.

The resulting 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 basic message is that 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 readily see by comparing the likelihood values across columns (1) and (2). The specification in column (3) contains all three regressors and allows one to formally test the null that the coefficients on inflation and unemployment are jointly significant. The F-statistic of this null suggests that the term FFF is doing the heavy lifting.

Next, we report the transmission of monetary policy to: (1) the federal funds rate path (we can do this even though the federal funds rate is constrained by the zero lower bound after 2008); (2) the term structure; and (3) macro aggregates. In all cases, the estimated responses are imprecisely estimated due to the short sample available. However, in broad terms, we are unable to detect meaningful differences with respect to the estimates based on the larger sample and the ordered probit specification. Notice that due to sample limitations, we are only able to report estimates up to 1-year ahead.

Figure 8 shows the estimated response of the federal funds rate to target rate changes using only the crisis period. Just as in the larger sample, the response of the federal funds rate is relatively muted. Figure 9 plots term rate responses to policy interventions in the crisis sample. Again and consistent with the muted response of the federal funds rate, the responses do not indicate that rate reductions are passed through to the yield curve. This finding is also in line with the findings for the earlier sample periods. Finally, Figure 10 reports the responses of the macro variables. Although inconclusive, these responses offer little support on the effectiveness of monetary stimulus over the crisis period.

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.

We use our propensity score weighting estimator for ordered time series treatments 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 string metaphor. 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, 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 the question of why target rate reductions have little effect on the yield curve. 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. We plan to explore this and other hypotheses for asymmetric policy effects in future work, focusing on the effects of the Fed's recent large scale asset purchases. These interventions were designed to reduce the long end of the yield curve in the face of exceptionally low short-term rates.

A Asset Price Based Policy Predictions

Our formulation of the propensity score is based on Piazzesi's (2005) term structure model. Piazzesi (2005) provides 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 ζ_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 ζ_t as the policy function $p_\zeta^j(\zeta_t, \psi)$. Under the additional assumption that the pricing functions are invertible, we can recover ζ_t with a vector of asset prices. This is particularly appealing because some of the elements in ζ_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 ζ_t. Let the state price density M_t be such that the price at time t of a random payoff V at time τ is $E\left[M_\tau V|\zeta_t\right]/M_t = E_\tau^Q\left[V\right]$ where $E_\tau^Q\left[V\right]$ is the expectation operator with respect to the risk neural measure. Harrison and Kreps (1979) show 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

$$P_{V_\tau}(t, \tau|\zeta_t) = E_\tau^Q\left[V|\zeta_t\right]. \tag{11}$$

We assume that the relationship between the state vector ζ_t and (11) is invertible, an assumption that is satisfied for example in affine models. Let $z_t = \left(P_{V_0}(t, \tau_0|\zeta_t), ..., P_{V_q}(t, \tau_q|\zeta_t)\right)$ be a vector of observed price data with maturities $\tau_0, .., \tau_q$ and assume that the pricing function has an inverse g such that

$$\zeta_t = g\left(z_t, t, \tau_0, ..., \tau_q\right). \tag{12}$$

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

$$p^j(z_t, \psi) := p^j_\zeta\left(g\left(z_t, t, \tau_0, ..., \tau_q\right), \psi\right). \tag{13}$$

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^0_t. 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 is 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 κ days to economize on notation, we denote $f^1_{t,\kappa}$ 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.[8] The payoff for a buyer of a fed funds futures contract is the difference between the futures rate $f^1_{t,\kappa}$ and the average fed funds rate over month $t+1$,

$$r^a_{t+1} = \frac{1}{\kappa}\sum_{k=1}^{\kappa} r_{t+1,k} \tag{14}$$

with the payoff cash settled the day after expiration of the futures contract (see Piazzesi and Swanson, 2008, p. 679). Pricing equation (11) and (14) imply that the spread between a funds future $f^1_{t,\kappa}$ and the prevailing target rate $\bar{r}_{t,\kappa}$ at the last day κ of month t is

$$s^0_t = f^1_{t,\kappa} - \bar{r}_{t,\kappa} = \frac{E^Q\left[r^a_{t+1} - \bar{r}_{t,\kappa}|\zeta_{t,\kappa}\right]}{P_1\left((t,\kappa),(t+1,\kappa)|\zeta_{t,\kappa}\right)} \tag{15}$$

where $(t+1,\kappa)$ denotes the last day of month $t+1$ and $P_1\left((t,\kappa),(t+1,\kappa)|\zeta_{t,\kappa}\right)$ is the month t, day κ, (t,κ), price of a zero coupon bond maturing at $(t+1,\kappa)$. Note that $f^1_{t,\kappa}$ 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 (15) shows that the futures-target rate spread is the best risk adjusted predictor of a target rate change during the coming month.[9] Whether (15) can

[8] 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.

[9] In other words, it minimizes the squared prediction error amongst all predictors based on $\zeta_{t,\kappa}$ of r^a_{t+1} and under the risk neutral discounted measure.

be inverted to recover $\zeta_{t,\kappa}$ as in (12) depends on the dimension of ζ_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 $f^1_{t,\kappa} - \bar{r}_{t,\kappa}$ in the propensity score is sufficient.

B Market based expectations in meeting months

This section discusses the construction of s^1_t, the indicator of the risk-adjusted expected change in the federal funds rate target based on futures prices for months in which there was an FOMC meeting scheduled. Notice that this variable will become a key ingredient of the vector z_t when we construct the ordered propensity score model $p^j(z_t, \psi)$. The discussion in this section borrows many elements from Hamilton (2008). Using the same notation as in the previous section, the effective federal funds rate observed on day k of month t can be described as

$$r_{t,k} = \overline{r}_{t,k} + u_{t,k}$$

where $u_{t,k}$ is a deviation from target. Where possible, we omit referencing t to any particular month and we use κ to denote the total number of days in the month generically to simplify the exposition. Historically, the Fed has been able to keep the effective federal funds rate trading within a few basis points of the target. However, on occasion there can be considerable fluctuations that are related to the *seasonality* of the maintenance period.

Denote by $f^0_{t,k}$ the spot federal funds rate contract. The contract settlement r^a_t is the average of $r_{t,k}$ over the month. Assuming the month has κ days

$$r^a_t = \frac{1}{\kappa}\sum_{k=1}^{\kappa} r_{t,k}$$

and using the risk neutral measure to price the spot federal funds rate contract

$$f^0_{t,k} = E^Q_{t,k}(r^a_t).$$

Suppose there is a change Δ in the target, effective on day k^* of that month. The target rate will be $\overline{r}_{t,k} = \overline{r}$ for $k = 1, ..., (k^* - 1)$ and $\overline{r}_{t,k} = \overline{r} + \Delta$ for $k = k^*, ..., \kappa$ (assuming only one target change to simplify the exposition). Accordingly, the futures rate observed on day k^* within the month is

$$\begin{aligned} f^0_{t,k^*} &= E^Q_{t,k^*}\left[\frac{1}{\kappa}\sum_{k=1}^{\kappa} r_{t,k}\right] = E^Q_{t,k^*}\left[\frac{1}{\kappa}\sum_{k=1}^{\kappa}(\overline{r}_{t,k} + u_{t,k})\right] = \\ & \frac{k^*-1}{\kappa}\overline{r} + \frac{\kappa - (k^*-1)}{\kappa} E^Q_{t,k^*}\left[\overline{r} + \Delta\right] + \frac{1}{\kappa}\sum_{k=1}^{k^*} u_{t,k} + \frac{1}{\kappa}\sum_{k=k^*+1}^{\kappa} E^Q_{t,k^*}\left[u_{t,k}\right]. \end{aligned}$$

Notice that $u_{t,k} = r_{t,k} - \bar{r}$ is observed in the data on a daily basis.

Let $\mu = (k^* - 1)/\kappa$, that is, the proportion of the month before the target changes. Let

$$v_{t,k^*} = \frac{1}{\kappa}\sum_{k=1}^{k^*} u_{t,k} + \frac{1}{\kappa}\sum_{k=k^*+1}^{\kappa} E_{t,k^*}^{Q}\left[u_{t,k}\right]$$

then it is easy to see that by solving for the optimal predictor[10] of the target change, $E_{t,k^*}^{Q}\left[\Delta\right]$, we obtain

$$s_t^1 \equiv E_{t,k^*}^{Q}\left[\Delta\right] = \frac{1}{1-\mu}(f_{t,k^*}^0 - \overline{r}) - \frac{1}{1-\mu}v_{t,k^*}.$$

The first term is simply the difference between the federal funds rate futures price minus the target before the change, scaled to reflect where within the month the target change took place. The second term is usually close to zero. For any day of the month before the target change, $u_{t,k}$ is directly observable from $u_{t,k} = r_{t,k} - \overline{r}$. But for the remaining days in the month, we need to construct a model for $E_{t,k^*}^{Q}\left[u_{t,k}\right]$. Following Hamilton (2008), we assume that $u_{t,k}$ follows an AR(1) process (we implicitly assume that professional investors are risk-neutral as is customary and therefore do not add a correction for unknown risk aversion) and include a rich set of dummies, one for each day of the maintenance period, so as to capture weekend effects (reserves held on Fridays count for Saturdays and Sundays therefore inducing more volatility on the federal funds rate on Fridays), as well as end-of-the-maintenance-period effects (when banks may be willing to pay extra to make up for any reserve shortfalls before the maintenance period is over). Following Hamilton (2008), we also include a dummy for the last day of the month and for the last day of the calendar year.

C Data

a) 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.

b) 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 1-month constant maturity, quoted on investment basis at 3 month and 2, 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'

c) 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),

[10] Optimality is in terms of a mean square criterion under the risk neutral measure.

monthly average, Source: Federal Reserve Statistical Release H.15; (iv) Civilian Unemployment Rate, seasonally adjusted (UNRATE), Source: Bureau of Labor Statistics.

d) 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[11] 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 percent 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.

References

Angrist, J. D., and G. W. Imbens (1995): "Two-Stage Least Squares Estimates of Average Causal Response in Models with Variable Treatment Intensity," *Journal of the American Statistical Association*, 90(430), 431–442.

Angrist, J. D., and G. M. Kuersteiner (2004): "Semiparametric Causality Tests Using the Policy Propensity Score," *NBER Working Paper 10975.*

Angrist, J. D., and G. M. Kuersteiner (2011): "Causal effects of monetary shocks: Semiparametric conditional independence tests with a multinomial propensity score," *Review of Economics and Statistics.*

Bagliano, F. C., and C. A. Favero (1998): "Measuring Monetary Policy with VAR Models: An Evaluation," *European Economic Review*, 42(6), 1069–1112.

Bernanke, B. S., and K. N. Kuttner (2005): "What Explains the Stock Market's Reaction to Federal Reserve Policy?," *Journal of Finance*, 60(3), 1221–1257.

[11] Retrieved from *http://www.federalreserve.gov/monetarypolicy/fomchistoricalYYYY.htm* where *YYYY* indicates the year in 1988-2008.

BLANCHARD, O., AND J. GALÍ (2010): "Labor Markets and Monetary Policy: A New Keynesian Model with Unemployment," *American Economic Journal: Macroeoconomics*, 2, 1–30.

CHRISTIANO, L. J., M. EICHENBAUM, AND C. EVANS (1996): "The Effects of Monetary Policy Shocks: Evidence from the Flow of Funds," *The Review of Economics and Statistics*, 78, 16–34.

CHRISTIANO, L. J., M. EICHENBAUM, AND C. L. EVANS (1999): "Monetary Policy Shocks: What Have We Learned And To What End?," in *Handbook of Macroeconomics, Volume 1*, ed. by J. B. Taylor, and M. Woodford, chap. 2, pp. 65–148. Elsevier Science B.V.

CHRISTIANO, L. J., M. TRABANDT, AND K. WALENTIN (2011): "DSGE Models for Monetary Policy Analysis," in *Handbook of Monetary Economics, Volume 3A*, ed. by B. M. Friedman, and M. Woodford, pp. 285–367. North Holland, Netherlands.

CLARIDA, R., J. GALÍ, AND M. GERTLER (2000): "Monetary Policy Rules and Macroeconomic Stability: Evidence and Some Theory," *The Quarterly Journal of Economics*, pp. 147–180.

COCHRANE, J. H. (1994): "Comment on: What Ends Recessions? by Christina D. Romer and David H. Romer," in *NBER Macroeconomics Annual*, ed. by S. Fischer, and J. J. Rotemberg, vol. 9. MIT Press: Cambridge.

COCHRANE, J. H., AND M. PIAZZESI (2002): "The Fed and Interest Rates - A High Frequency Identification," *The American Economic Review*, 92, 90–95.

COVER, J. P. (1992): "Asymmetric Effects of Positive and Negative Money-Supply Shocks," *The Quarterly Journal of Economics*, 107(4), 1261–1282.

DEHEJIA, R. H., AND S. WAHBA (1999): "Causal Effects in Nonexperimental Studies: Reevaluating the Evaluation of Training Programs," *Journal of the American Statistical Association*, 94, 1053–1062.

DELONG, J. B., AND L. H. SUMMERS (1988): "How Does Macroeconomic Policy Affect Output?," *Brookings Papers on Economic Activity*, 2, 433–94.

FAUST, J., E. T. SWANSON, AND J. H. WRIGHT (2004): "Identifying VARS based on high frequency data," *Journal of Monetary Economics*, 51, 1107–1131.

FRIEDMAN, M., AND A. J. SCHWARTZ (1963): *A Monetary Historory of the United States: 1867-1960*. National Bureau of Economic Research. Princeton University Press: Princeton, NJ.

GALÍ, J. (2011): "Monetary Policy and Unemployment," in *Handbook of Monetary Economics, Volume 3A*, chap. 10, pp. 487–546. Elsevier Science B.V.

GALÍ, J., AND M. GERTLER (2007): "Macroeconomic Modeling for Monetary Policy Evaluation," *Journal of Economic Perspectives*, 21, 25–45.

Gallant, A. R., P. E. Rossi, and G. Tauchen (1993): "Nonlinear Dynamic Structures," *Econometrica*, 61(4), 871–907.

Granger, C. W. J. (1969): "Investigating Causal Relations by Econometric Models and Cross Spectral Methods," *Econometrica*, 37(3), 424–438.

Gurkaynak, R. S., B. P. Sack, and E. Swanson (2005): "The Sensitivity of Long-Term Interest Rates to Economic News: Evidence and Implications for Macroeconomic Models," *American Economic Review*, 95(1), 425–436.

Gurkaynak, R. S., B. P. Sack, and E. T. Swanson (2007): "Market-Based Measures of Monetary Policy Expectations," *Journal of Business and Economic Statistics*, 25(2), 201–212.

Hamilton, J. D. (2008): "Assessing Monetary Policy Effects Using Daily Federal Funds Futures Contracts," *Federal Reserve Bank of St. Louis Review*, 90(4), 377–393.

——— (2009): "Daily Changes in Fed Funds Futures Prices," *Journal of Money, Credit and Banking*, 41(4), 567–582.

Hamilton, J. D., and Òscar Jordà (2002): "A Model of the Federal Funds Rate Target," *Journal of Political Economy*, 110, 1135–1167.

Harrison, H. M., and D. M. Kreps (1979): "Martingales and Arbitrage in Multiperiod Securities Markets," *Journal of Economic Theory*, 20, 381–408.

Heckman, J. J., H. Ichimura, and P. E. Todd (1998): "Matching as an Econometric Evaluation Estimator," *Review of Economic Studies*, 65, 261–294.

Hirano, K., G. Imbens, and G. Ridder (2003): "Efficient Estimation of Average Treatment Effects using the Estimated Propensity Score," *Econometrica*, 71, 1161–1189.

Hoover, K. D., and S. J. Perez (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.

Horvitz, D., and D. Thompson (1952): "A Generalization of Sampling Without Replacement from a finite Population," *Journal of the American Statistical Association*, 47(260), 663–685.

Jordà, O. (2005): "Estimation and Inference of Impulse Responses by Local Projections," *American Economic Review*, 95(1), 161–182.

Kareken, J., and R. M. Solow (1963): "Lags in Monetary Policy," in *Comission on money and credit, stabilization policies.*, pp. 14–96. Englewood Cliffs, NJ: Prentice-Hall.

KARRAS, G. (1996): "Are the Output Effects of Monetary Policy Asymmetric?," *Oxford Bulletin of Economics and Statistics*, 58(2), 267–78.

KEYNES, J. M. (1936): *The General Theory of Employment, Interest and Money.* McMillan.

KRUEGER, J. T., AND K. N. KUTTNER (1996): "The Fed Funds Futures Rate as a Predictor of Federal Reserve Policy," *The Journal of Futures Markets*, 16(8), 865–879.

KUTTNER, K. N. (2001): "Monetary Policy Surprises and Interest Rates: Evidence from the Fed Funds Futures Market," *Journal of Monetary Economics*, 47(3), 523–544.

LEEPER, E. M., AND T. ZHA (2003): "Modest Policy Intervention," *Journal of Monetary Economics*, 50, 1673–1700.

MEULENDYKE, A.-M. (1998): *U.S. Monetary Policy and Financial Markets.* Federal Reserve Bank of New York.

NEWEY, W. K., AND K. D. WEST (1994): "Automatic Lag Selection in Covariance Matrix Estimation," *Review of Economic Studies*, 61, 631–654.

PIAZZESI, M. (2005): "Bond Yields and the Federal Reserve," *Journal of Political Economy*, 113(2), 311–344.

PIAZZESI, M., AND E. T. SWANSON (2008): "Futures Prics as Risk-Adjusted Forecasts of Monetary Policy," *Journal of Monetary Economics*, 55(4), 677–691.

RAVN, M. O., AND M. SOLA (2004): "Aysmmetric Effects of Monetary Policy in the United States," *Federal Reserve Bank of St. Louis Review*, 86(5), 41–60.

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.

——— (1994): "Monetary Policy Matters," *Journal of Monetary Economics*, 34(1), 75–88.

——— (2004): "A New Measure of Monetary Shocks: Derivation and Implications," *The American Economic Review*, 94, 1055–1084.

——— (2013): "The Most Dangerous Idea in Federal Reserve History: Monetary Policy Doesn't Matter," *American Economic Review: Papers and Proceedings*, 103(3), 55–60.

ROSENBAUM, P. R., AND D. B. RUBIN (1983): "The Central Role of the Propensity Score in Observational Studies for Causal Effects," *Biometrika*, 70(1), 41–55.

SCOTTI, C. (2011): "A Bivariate Model of Federal Reserve and ECB Main Policy Rates," *International Journal of Central Banking*, 7(3), 37–78.

Sims, C. A. (1972): "Money, Income and Causality," *American Economic Review*, 62(4), 540–562.

——— (1992): "Interpreting the macroeconomic time series facts: The effects of monetary policy," *European Economic Review*, 36(5), 975–1000.

Singleton, K. J. (2001): "Estimation of affine asset pricing models using the empirical characteristic function," *Journal of Econometrics*, 102(1), 111–141.

S oderlind, P., and L. Svensson (1997): "New techniques to extract market expectations from financial instruments," *Journal of Monetary Economics*, 40, 383–429.

Tenreyro, S., and G. Thwaites (2013): "Pushing on a String: US Monetary Policy is Less Powerful in Recessions," *CEP Discussion Paper No 1218*.

Thapar, A. (2008): "Using Private Information to Estimate the Effects of Monetary Policy," *Journal of Monetary Economics*, 55, 806–824.

Wingender, A. M. (2011): "Monetary Policy Shocks and Risk Premia in the Interbank Market," *The B.E. Journal of Macroeconomics*, 11(1), 1–19.

Woodford, M. (2001): "The Taylor Rule and Optimal Monetary Policy," *American Economic Review*, 91(2), 232–237.

	July 1989 to July 2005				July 1989 to Dec 2008			
	OP_{T1}	OP_{T2}	OP_{F1}	OP_{F2}	OP_{T1}	OP_{T2}	OP_{F1}	OP_{F2}
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre-Crisis			0.55***	0.52***			0.61***	0.58***
			(0.11)	(0.11)			(0.10)	(0.09)
Post-Crisis							0.58**	0.56**
							(0.21)	(0.21)
Inflation, Lag 1	0.08	0.05		0.08	0.08	0.02		-0.00
	(0.07)	(0.07)		(0.07)	(0.06)	(0.06)		(0.06)
Inflation, Lag 2		0.10		0.02		0.17*		0.06
		(0.07)		(0.07)		(0.07)		(0.06)
Unem. Rate, Lag 1	-0.31**	-0.18*		-0.15	-0.30**	-0.16		-0.14
	(0.10)	(0.09)		(0.08)	(0.09)	(0.09)		(0.08)
Unem. Rate, Lag 2		-0.26**		-0.13		-0.30**		-0.12
		(0.09)		(0.08)		(0.09)		(0.08)
Target Rate		-0.01**	-0.02**	-0.02**		-0.02**	-0.02**	-0.02***
		(0.01)	(0.01)	(0.01)		(0.01)	(0.01)	(0.01)
Last Target Change		0.10	0.14	0.08		0.10	0.15*	0.09
		(0.08)	(0.07)	(0.07)		(0.09)	(0.07)	(0.07)
LTC×FOMC		0.21*	0.09	0.12		0.30**	0.18*	0.22*
		(0.10)	(0.08)	(0.08)		(0.11)	(0.08)	(0.09)
FOMC		-0.02	-0.00	-0.01		-0.03	-0.02	-0.01
		(0.03)	(0.03)	(0.03)		(0.03)	(0.03)	(0.03)
CRISIS					-0.07	-0.06	-0.03	-0.02
					(0.04)	(0.03)	(0.04)	(0.04)
Log Likelihood	-187.04	-149.85	-121.04	-117.60	-231.93	-185.36	-141.42	-138.53
Observations	192				233			

Table 1. Ordered Probit Specifications for the expected change of the Target Rate. 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 other variable definitions see text.

	July 1989 to July 2005				July 1989 to Dec 2008			
	OP_{T1}	OP_{T2}	OP_{F1}	OP_{F2}	OP_{T1}	OP_{T2}	OP_{F1}	OP_{F2}
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A.Propensity Score Covariates								
FFF_t Combined	0.000***	0.002***	0.152	0.158	0.000***	0.000***	0.193	0.202
FFF_t	0.006***	0.019**	0.068*	0.058*	0.000***	0.009***	0.081*	0.079*
FFF_t squared	0.001***	0.003***	0.466	0.598	0.000***	0.002***	0.530	0.679
Effective Funds Rate	0.032**	0.128	0.026**	0.111	0.021**	0.072*	0.026**	0.069*
Target Rate	0.014**	0.113	0.016**	0.079*	0.009***	0.068*	0.021**	0.059*
Last Target Change	0.000***	0.330	0.470	0.457	0.000***	0.071*	0.174	0.182
Inflation, Lag 1	0.971	0.962	0.982	0.926	0.635	0.688	0.629	0.642
Unem. Rate Lag 1	0.221	0.782	0.377	0.366	0.706	0.990	0.541	0.564
Inflation, Lag 2	0.703	0.200	0.091*	0.063*	0.821	0.275	0.119	0.112
Unem. Rate Lag 2	0.003***	0.710	0.125	0.277	0.006***	0.932	0.423	0.876
B. Lagged Outcome Variables								
Federal Funds Rate	0.000***	0.121	0.188	0.209	0.000***	0.027**	0.139	0.198
IP	0.251	0.561	0.289	0.323	0.216	0.432	0.200	0.265
3 Month T-Bill	0.258	0.869	0.955	0.992	0.048**	0.500	0.726	0.830
2 Year T-Bond	0.176	0.889	0.930	0.880	0.146	0.789	0.823	0.722
10 Year T-Bond	0.432	0.966	0.800	0.738	0.690	0.851	0.660	0.577

Table 2. Specification Tests. See Table 1 for model definitions. This table shows p-values for joint tests of the null hypothesis that the ordered probit model correctly specifies the conditional probability of $D_t = \{-.25, 0, .25\}$. Moment conditions are formed for each candidate predictor variable individually. Test statistics labeled FFF (Combined) are joint for FFF and FFF^2.

	3-month T-Bill		2-year T-Bond		5-year T-Bond		10-year T-Bond	
	+.25	-.25	+.25	-.25	+.25	-.25	+.25	-.25
6	0.387**	-0.070	0.424**	0.136	0.351**	0.197	0.271**	0.178
	(0.162)	(0.109)	(0.179)	(0.134)	(0.139)	(0.132)	(0.114)	(0.110)
12	0.702**	-0.157	0.496**	-0.072	0.373**	-0.018	0.293*	0.031
	(0.306)	(0.232)	(0.241)	(0.414)	(0.189)	(0.430)	(0.160)	(0.348)
18	0.486	-0.280	0.172	-0.182	0.131	-0.110	0.136	-0.045
	(0.317)	(0.318)	(0.258)	(0.499)	(0.203)	(0.492)	(0.174)	(0.383)
24	0.268	-0.174	0.159	0.016	0.230	0.092	0.263*	0.135
	(0.383)	(0.383)	(0.324)	(0.466)	(0.217)	(0.370)	(0.157)	(0.260)

Table 3. Estimates of cumulated impulse responses at Horizons 6,12,18 and 24 months. 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

	Funds Rate		Inflation		Indust. Prod.		Unem. Rate	
	+.25	-.25	+.25	-.25	+.25	-.25	+.25	-.25
6	0.436**	-0.217**	0.002	0.063	-0.105	0.129	0.013	-0.064
	(0.183)	(0.099)	(0.069)	(0.071)	(0.249)	(0.517)	(0.047)	(0.107)
12	0.771**	-0.335	0.019	0.104	-0.471	-0.212	0.060	-0.002
	(0.350)	(0.210)	(0.130)	(0.133)	(0.416)	(0.383)	(0.074)	(0.110)
18	0.712*	-0.375	-0.054	0.033	-1.070	-0.202	0.151	0.042
	(0.364)	(0.271)	(0.176)	(0.216)	(0.696)	(0.473)	(0.100)	(0.116)
24	0.393	-0.344	-0.208	-0.016	-1.720*	-0.167	0.294*	-0.037
	(0.406)	(0.365)	(0.208)	(0.223)	(1.007)	(0.637)	(0.160)	(0.176)

Table 4. Estimates of cumulated impulse responses at Horizons 6,12,18 and 24 months. 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

	3-month T-Bill	2-year T-Bond	5-year T-Bond	10-year T-Bond
6	-0.026	0.290*	0.324**	0.273*
	(0.114)	(0.155)	(0.164)	(0.149)
12	-0.003	0.190	0.169	0.150
	(0.237)	(0.424)	(0.462)	(0.394)
18	0.038	0.197	0.164	0.125
	(0.334)	(0.499)	(0.501)	(0.389)
24	0.251	0.496	0.452	0.376
	(0.421)	(0.502)	(0.398)	(0.276)

Table 5. Estimates of cumulated impulse responses at Horizons 6,12,18 and 24 months to a reduction of the Federal Funds Target by .25%. 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

	Funds rate	Inflation	Indust. Prod.	Unemp. Rate
6	-0.233**	0.190*	0.543	-0.194
	(0.111)	(0.103)	(0.647)	(0.130)
12	-0.265	0.221	0.243	-0.199
	(0.230)	(0.169)	(0.748)	(0.169)
18	-0.136	0.163	0.254	-0.162
	(0.300)	(0.214)	(0.929)	(0.208)
24	0.047	0.174	0.501	-0.293
	(0.403)	(0.226)	(1.200)	(0.312)

Table 6. Estimates of cumulated impulse responses at Horizons 6,12,18 and 24 months to a reduction of the Federal Funds Target by .25%. 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

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

Table 7. Probit Models for Target Rate Reductions during the 2006-2008 period. The table shows marginal effects. ***/**/* indicates significance at the 99/95/90% confidence level

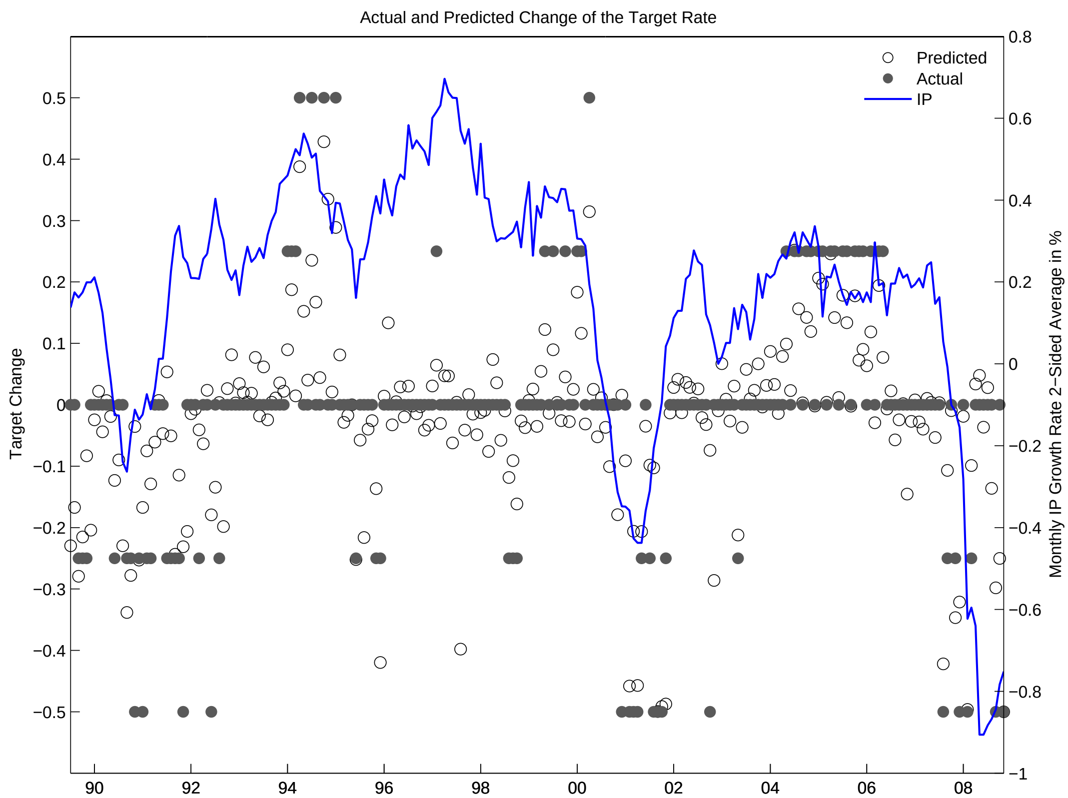

Figure 1. Actual Changes (circles) and Predicted Changes (dots) in the Target Rate. Predictions are from the propensity score model labeled OP_{F2} in Table 1. The figure also shows IP growth over the same period.

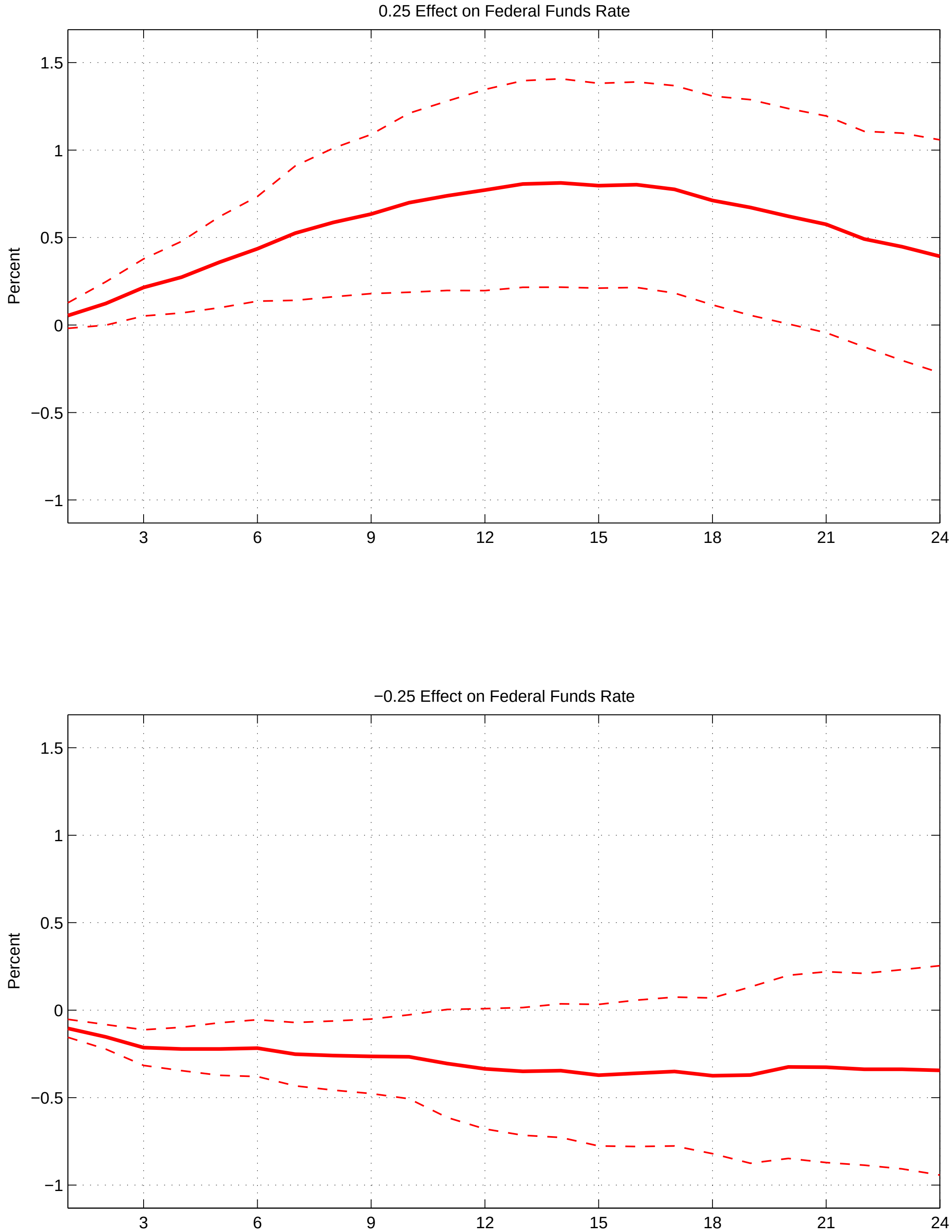

Figure 2. Estimated Effects of Target Rate Changes on the Federal Funds Rate. These estimates use data from August 1989 through July 2007, and the propensity score mode labeled OP_{F2} in Table 1. Dashed lines indicate 90% confidence bands.

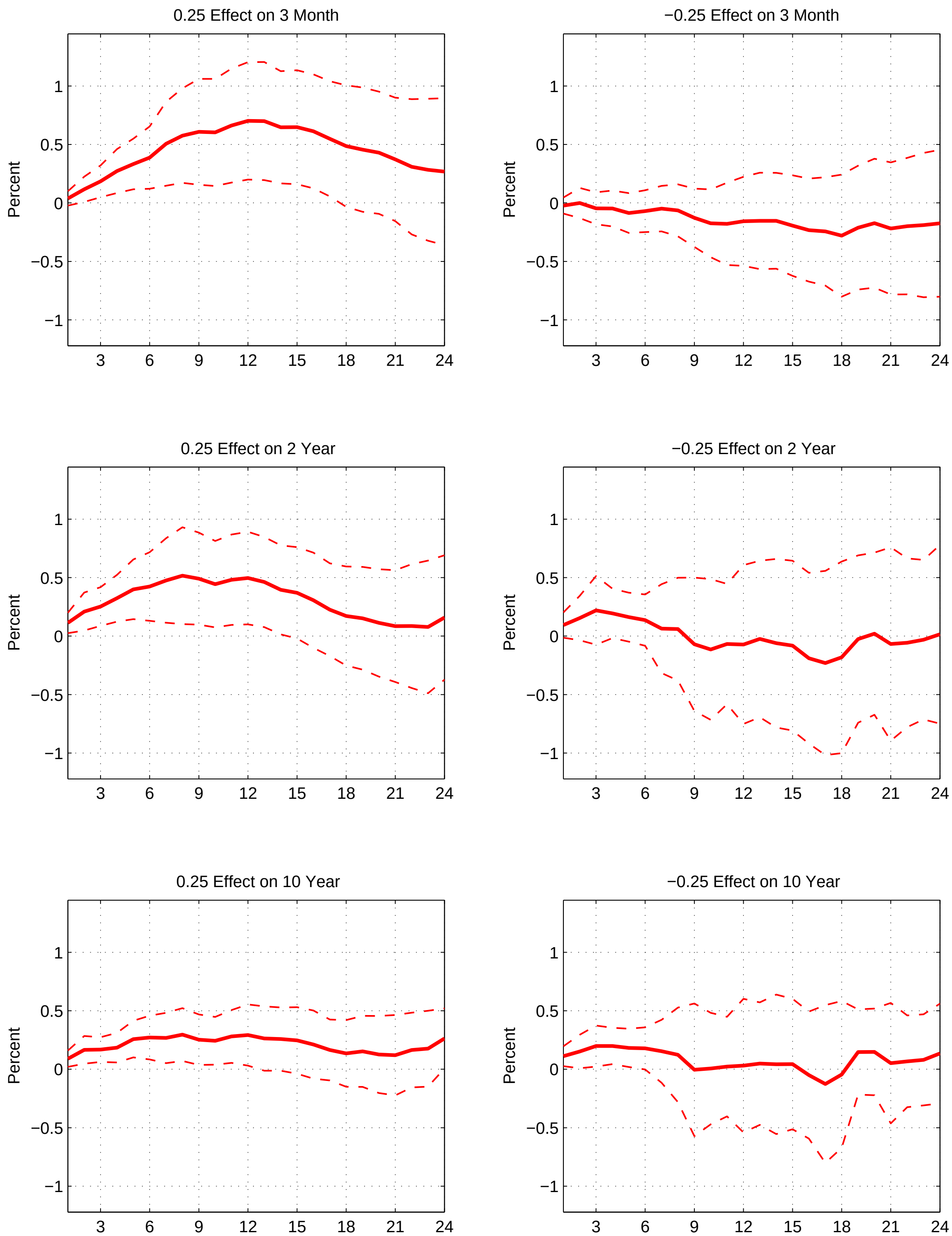

Figure 3. Estimated Effects of Target Rate Changes on Bond Yields. These estimates use data from August 1989 through July 2007, and the propensity score mode labeled OP_{F2} in Table 1. Dashed lines indicate 90% confidence bands.

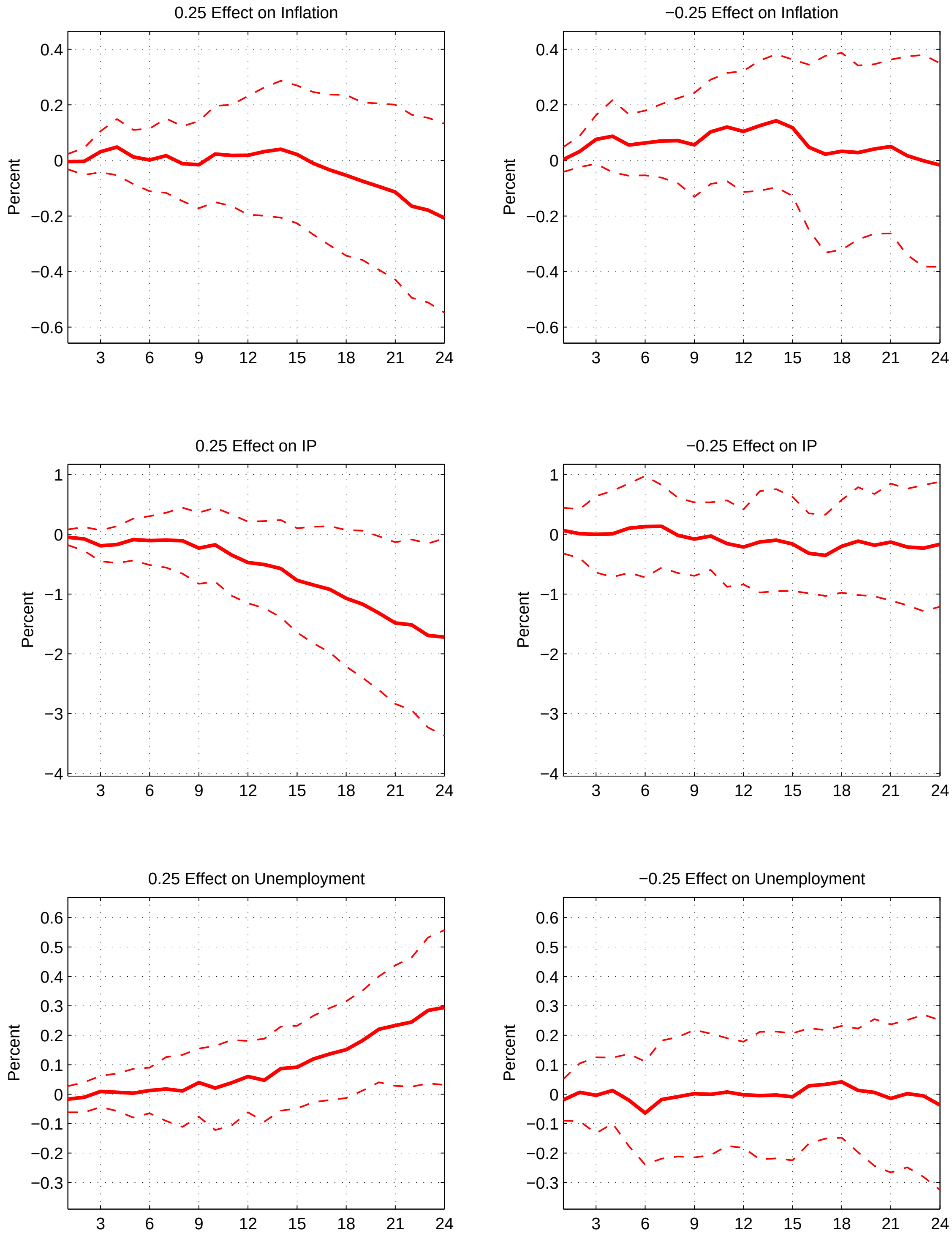

Figure 4. Estimated Effects of Target Rate Changes on Macro Variables. These estimates use data from August 1989 through July 2007, and the propensity score mode labeled OP_{F2} in Table 1. Dashed lines indicate 90% confidence bands.

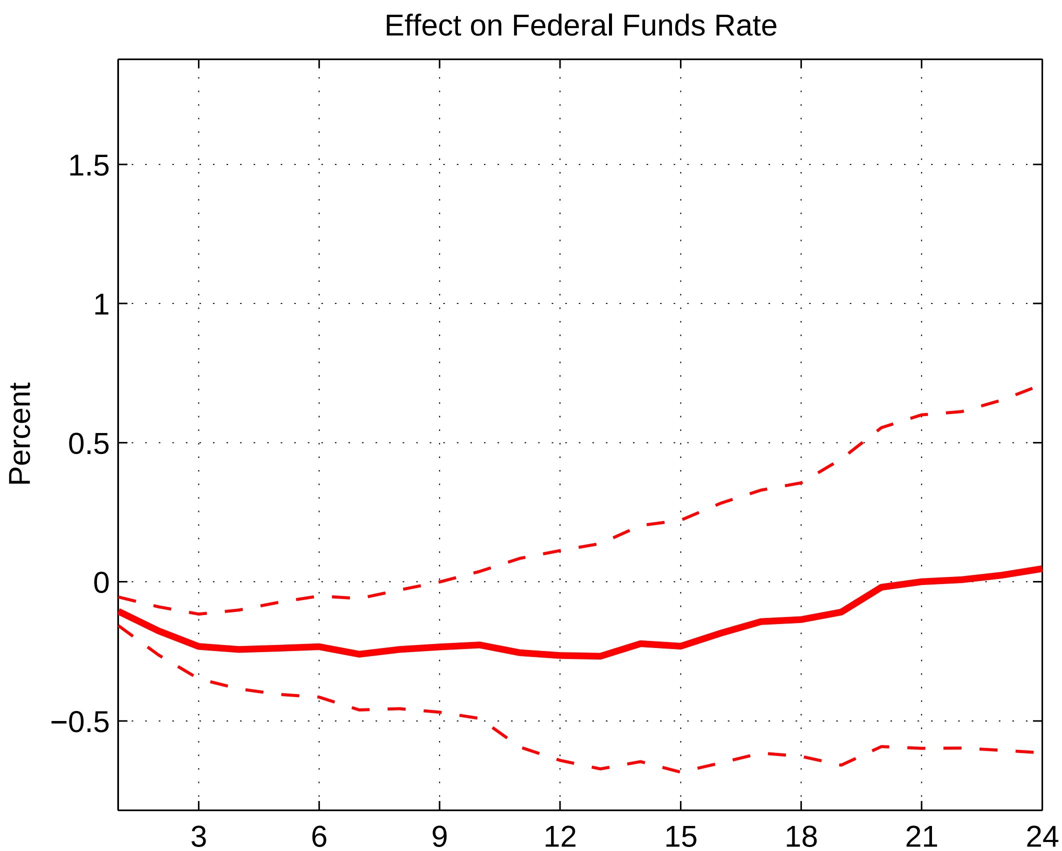

Figure 5. Estimated Effects of Target Rate Changes on the Federal Funds Rate Through 2010. These estimates use data from August 1989 through December 2010, and the propensity score model (8) labeled OP_{F2} in Table 1. Dashed lines indicate 90% confidence bands.

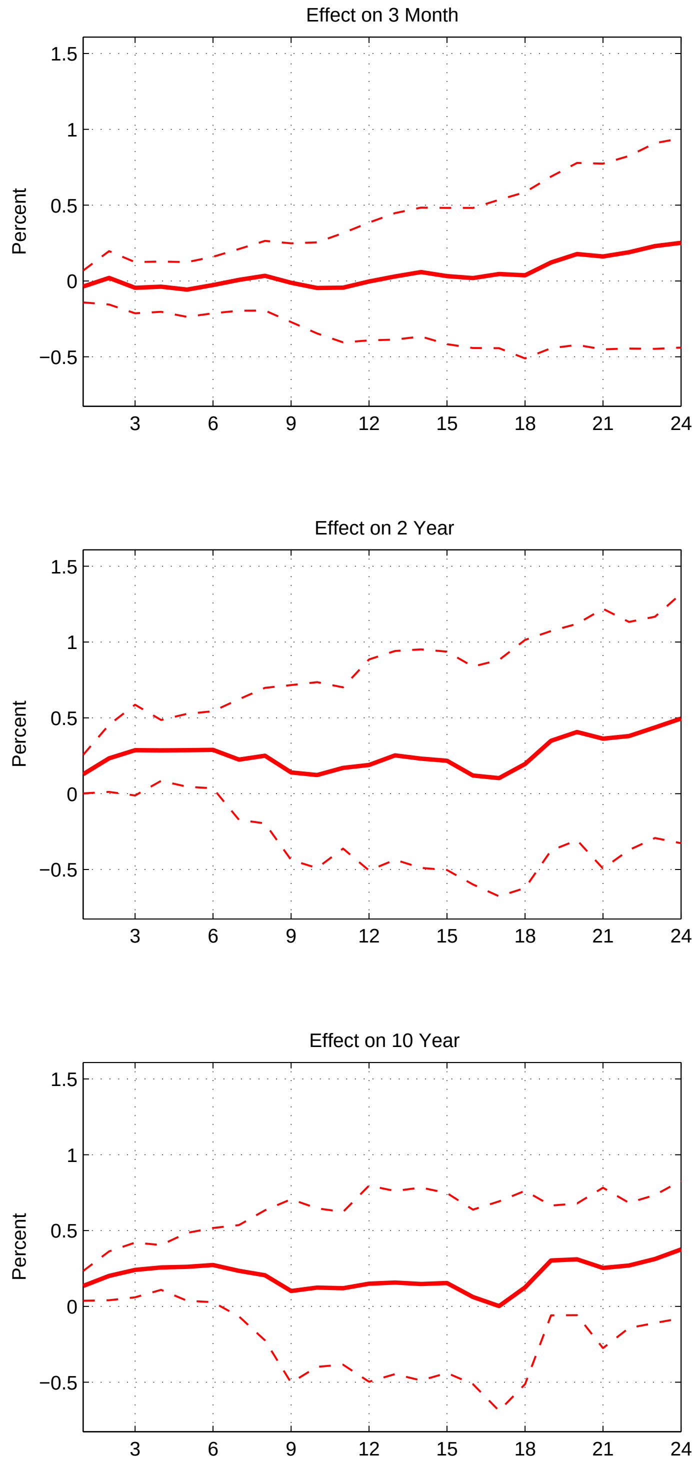

Figure 6. Estimated Effects of Target Rate Changes on Bond Yields Through 2010. These estimates use data from August 1989 through December 2010, and the propensity score model (8) labeled OP_{F2} in Table 1. Dashed lines indicate 90% confidence bands.

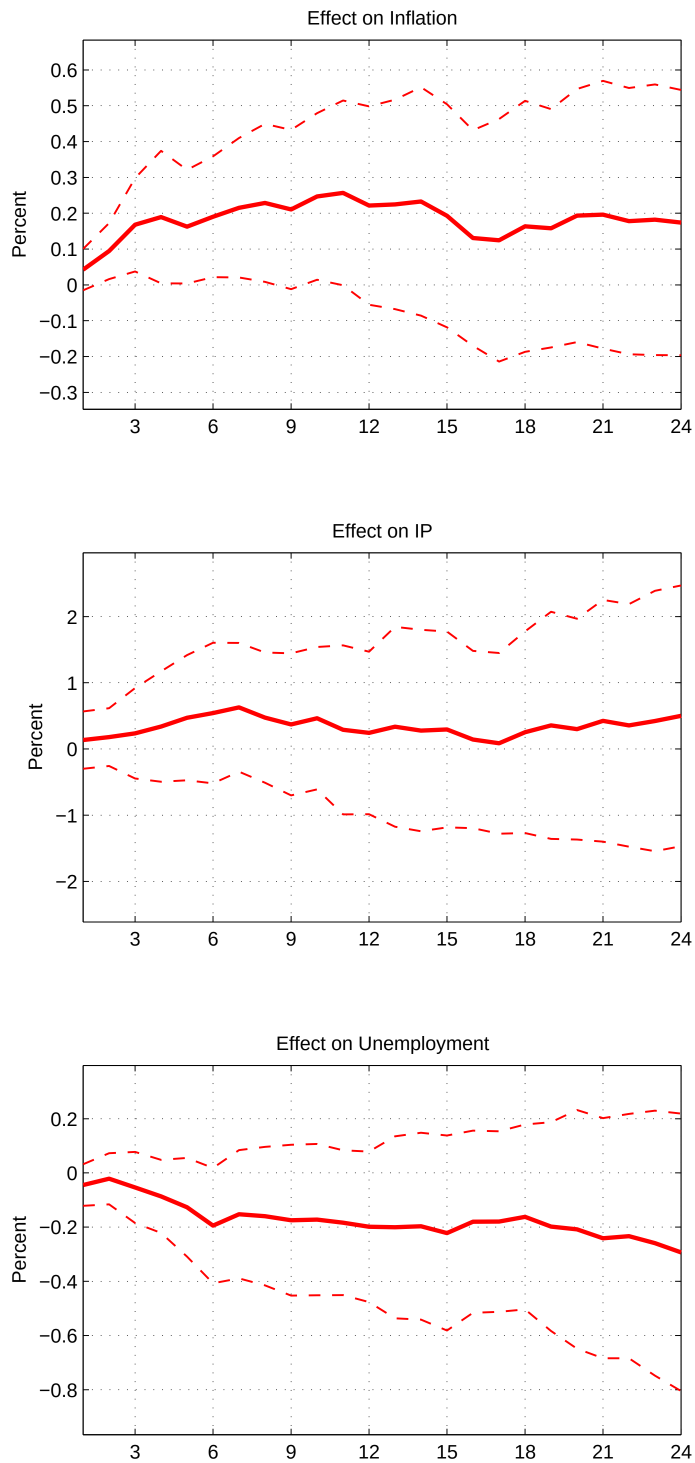

Figure 7. Estimated Effects of Target Rate Changes on Macro Variables Through 2010. These estimates use data from August 1989 through December 2010, and the propensity score model (8) labeled OP_{F2} in Table 1. Dashed lines indicate 90% confidence bands.

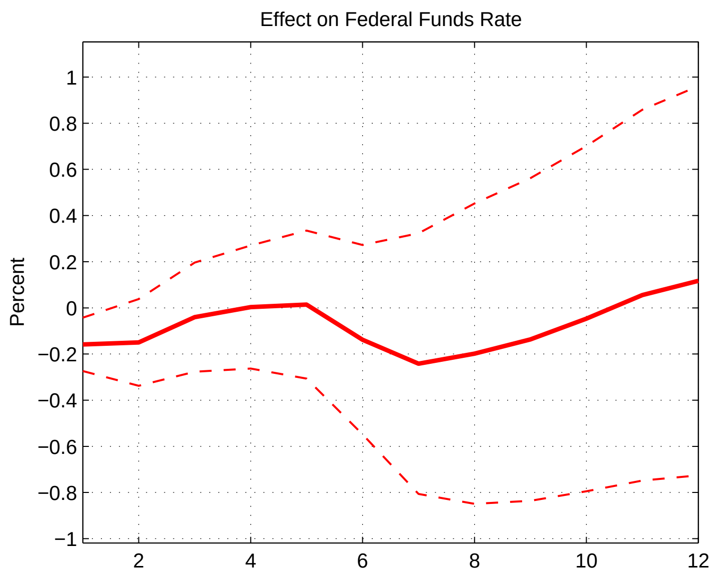

Figure 8. Estimated Effects of Target Rate Drops on the Federal Funds Rate. These estimates use data from October 2006 through December 2009, and the propensity score model labeled PF2 in Table 3. Dashed lines indicate 90% confidence bands.

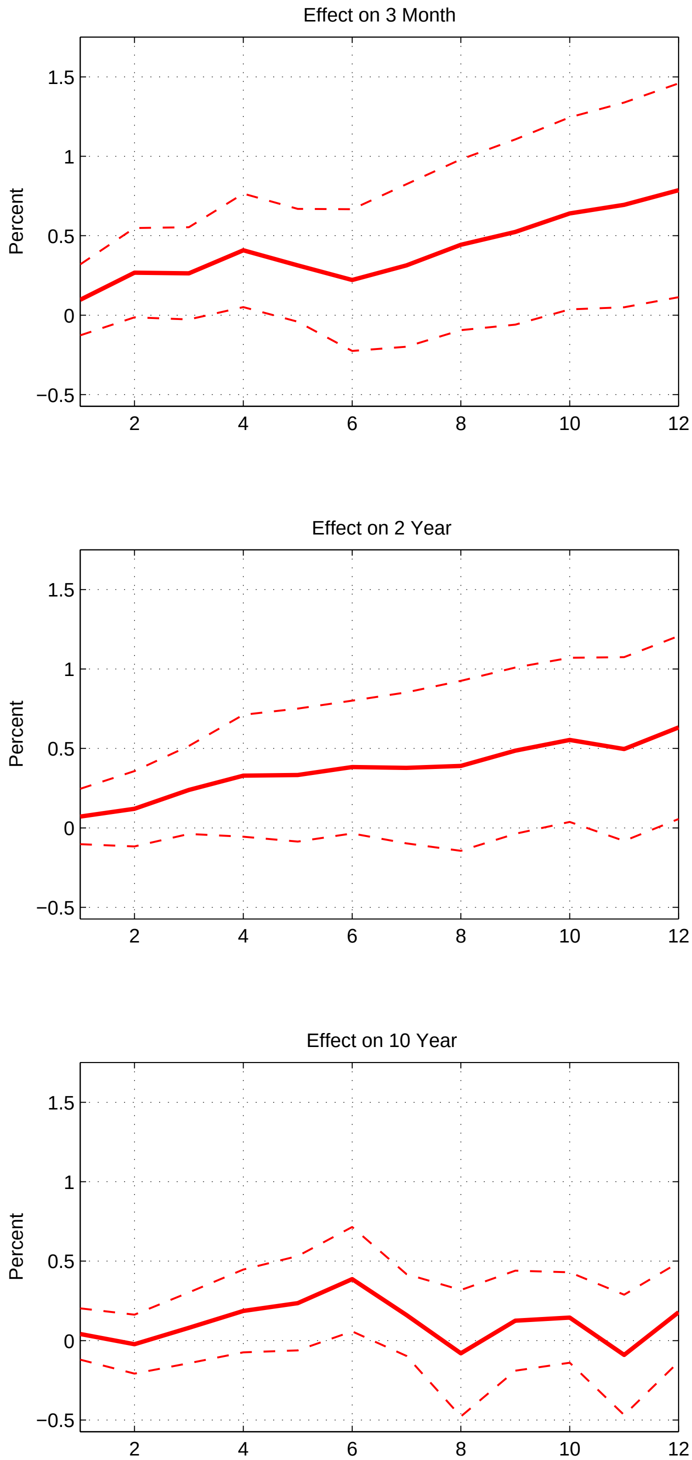

Figure 9. Estimated Effects of Target Rate Drops on Bond Yields in the Crisis Period. These estimates use data from October 2006 through December 2009, and the propensity score model labeled PF2 in Table 3. Dashed lines indicate 90% confidence bands.

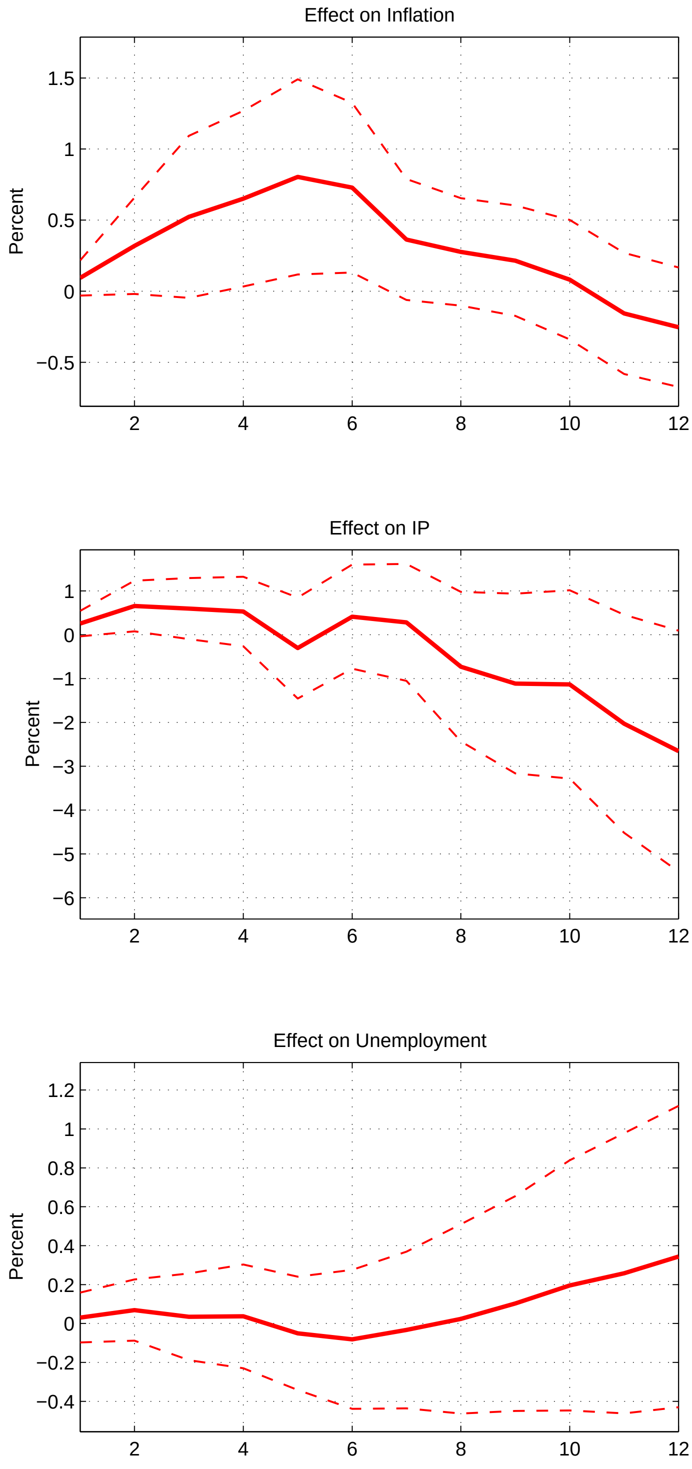

Figure 10. Estimated Effects of Target Rate Drops on Macro Variables in the Crisis Period. These estimates use data from October 2006 through December 2009, and the propensity score model labeled P_{F2} in Table 3. Dashed lines indicate 90% confidence bands.