# Shadow Rate Models and Monetary Policy

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**Abstract**

We examine the channels and efficacy of monetary policy at the zero lower bound (ZLB) through the lens of various shadow rate models. Our key methodological contribution is to extend the discretization filter to incorporate missing observations. This allows us to estimate shadow rate models that both incorporate survey forecasts and allow for departures from rational expectations. The models disagree about the level of the shadow rate and the duration of the ZLB in real time, but they are remarkably consistent in terms of their implications for the effects and channels of policy across a number of structural and reduced-form exercises. Particularly, they attribute most of the effects of major Federal Reserve policy announcements to changes in term premia, and imply that large scale asset purchases affected term premia both by changes in average duration and changes in local supply of substitute assets.
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# 1 Introduction

Between 2008 and 2015, the Federal Reserve lowered its policy interest rate to the zero lower bound (ZLB) and employed new tools – large scale asset purchases (LSAPs) and forward guidance – to influence long-term interest rates. The consensus among policymakers is that these new tools were effective (Caldara et al. (2020)). However, the channels through which they operated are still not well understood.

The nonlinearity resulting from the ZLB makes it difficult to separately identify interest-rate expectations from term premia in standard term-structure affine models. Despite the challenges of identifying *how* these new policies worked, the Federal Reserve employed similar strategies in response to the COVID-19 recession. Hence, understanding the effects and channels of monetary policy at the ZLB remains an important question.

Shadow rate models are a common tool for analyzing the effects of unconventional monetary policy. The shadow rate is the counterfactual one-period interest rate that would have obtained absent the ZLB (Black (1995)). Shadow rate term structure models combine a time series process for the stochastic discount factor of financial market participants alongside an effective lower bound on short-term interest rates and the absence of arbitrage to estimate the parameters governing the physical and risk-neutral dynamics of realized yields. The path of the shadow rate reflects market expectations of the length of time that short-term interest rates would remain beneath its lower bound. These models have been used to summarize the stance of monetary policy and to evaluate its real effects (Wu and Xia (2016); Bauer and Rudebusch (2014)).

In this paper, we estimate different models using the discretization filter (Farmer (2021)): with two and three latent factors, with "rational" survey forecasts, and with "subjective" survey forecasts. Our set of models, while not exhaustive, encompasses a broad range of specifications commonly used in the shadow rate and affine term structure literature. We find that models estimated with the discretization filter tend to be competitive in forecasting forward rates relative to a yields-only model estimated with the extended Kalman filter as in Wu and Xia (2016), and outperform it during and after the COVID-19 recession. We also find that three-factor models have superior ability to match the data (relative to two-factor models) particularly after 2007. Given that our interest is understanding policy during the ZLB and afterwards, we subsequently primarily focus on the policy implications of our three-factor specifications.

Using our estimates, we re-evaluate a number of questions concerning the effects of monetary policy on financial markets and the macroeconomy during and after the Great Recession. First, we examine the model-implied real-time beliefs about the duration of the zero lower bound across our models. The level of the shadow rate differs markedly *across* models. However, all of our estimates qualitatively agree that markets persistently expected a much shorter duration of the ZLB in the early stage of the Great Recession than occurred ex-post. Our results suggest that the introduction of calendar-based forward guidance in 2011 led to an upward reassessment of ZLB duration. Models allowing for distorted forecasts imply market-expected duration longer than forward guidance suggested, while the other specifications usually align with the Fed's Summary of Economics Projections (SEP). These findings are consistent with prior work on the expected liftoff from the ZLB (Swanson and Williams (2014)) and suggest that calendar-based forward guidance was at least somewhat effective at shaping the beliefs (and behavior) of market participants.

After examining the level of the shadow rate, we focus on shadow rate model estimates for policy applications. The overall message is a remarkable consistency in the estimated effects and channels of policy across different choices of shadow rate estimate. We first focus on the use of shadow rates as a "plug in" for the Federal Funds rate in VARs during the ZLB period. We estimate an identical Factor-Augmented VAR (FAVAR) as in Wu and Xia, replacing their shadow rate estimates with ours. We find mixed evidence for parameter instability in the FAVAR, depending on the specification, but relatively consistently estimated effects of policy shocks across all models. While our results are qualitatively similar to those of Wu and Xia, the confidence intervals are often quite wide. Results for the "post-crisis" sample are generally attenuated relative to the results reported in Wu and Xia, (although their median result is usually in the 90% confidence band).

We then turn to less structural applications of the model estimates for the assessment of policy. We use the estimated models to decompose the yield curve into the expectations hypothesis (EH) component of yields and term premia. We find that the first round of LSAPs largely affected long-term yields by reducing term premia, a result that is consistent with earlier findings (Gagnon et al. (2011)). The magnitude of the fall in term premia differs across models, but it explains at least two thirds of the decline in yields. Finally, we study the predicted effects of LSAPs on available Treasury supply and composition following D'Amico et al. (2012). We find effects of LSAPs on long-term bond yields largely attributable to changes in duration and changes in "local scarcity" of long-term Treasuries.

Relative to existing work on shadow rates and the macroeconomy, we innovate, and synthesize, along a number of dimensions. Methodologically, we incorporate survey forecasts of average short-term interest rates into the data used in estimation. Survey forecasts of interest rates are commonly used to estimate affine term structure models to improve the precision of estimates of the short-rate process (Kim and Orphanides (2012)), which in turn materially impacts the properties of estimated term premia (Li et al. (2017)). In the context of shadow rate models, having precise estimates of the short rate process is especially important because the inferred dynamics of shadow rates depend on the estimated degree of mean reversion in short rates. Priebsch (2013, 2017) and Kim and Priebsch (2020) also incorporate surveys into shadow rates estimation as rational expectations forecasts of short rates. We go beyond this restriction by exploring an alternative model of forecast formation following Piazzesi et al. (2015). They estimate a term structure model which does not impose that survey forecasts are the same as the forecast of the marginal bond trader. We extend their work to incorporate the effective lower bound. We find in general that incorporating survey data improves the ability of the model to forecast yields, with Piazzesi et al. (2015) models having an advantage at the short end and rational survey forecasts having superior performance for bonds maturing in a year or more. Relative to the set of papers that incorporates "rational" forecasts (Priebsch (2013, 2017); Kim and Priebsch (2020)) we also explore the implications of short rates and term premia estimated using these models. Since much of the literature following Wu and Xia (2016) has focused on using their shadow rate series in other applications, it is particularly important to understand whether our assessment of monetary policy holds up to alternative data sets and structural assumptions.

We obtain our shadow rate estimates using the discretization filter (Farmer (2017, 2021)). Farmer (2017) applied the discretization filter to the estimation of a three-factor yields-only shadow rate model and compared his estimated shadow rate series to Wu and Xia (2016). Relative to Farmer (2017), we consider a larger range of models and data (both after the liftoff from the ZLB and using forecasts) and explore the implications of our range of models to quantify the impact of monetary policy at the zero lower bound during the Great Recession and the COVID-19 period. Additionally, we extend the filter to incorporate missing data, in order to account for the different horizons of forecasts available in different periods.

The most widely used estimate of the shadow rate for applied work, Wu and Xia (2016), applies the extended Kalman filter (EKF). The EKF uses local linearization of the observation equation to evaluate the likelihood (Durbin and Koopman (2012).) Others (e.g. Bauer and Rudebusch (2016)) estimate parameters on pre-ZLB data alone. The discretization filter, by contrast, does not rely on linearization and its computational efficiency allows for comparison of a wide selection of models. We find that shadow rates estimated using this method tend to have competitive forecasting performance relative to those using the EKF. Models estimated using the discretization filter and including forecasts also perform better during the COVID-19 recession and beyond.

Lastly, we examine whether the number of factors matters for our conclusions. Since at least Litterman (1991), the consensus has been that at least three factors are necessary to capture the statistical properties of the Treasury yield curve. Krippner (2015) argues that two-factor specifications are more suited for monitoring and summarizing the stance of monetary policy at the effective lower bound. Two-factor estimates are often used as an alternative benchmark in applied work (e.g., Carriero et al. (2021), Corrado et al. (2021), De Rezende and Ristiniemi (2023), King (2019), Johannsen and Mertens (2021), Nyholm (2021)). Three-factor models outperform their two-factor analogs in out-of-sample exercises and when fitting the *average* forward rate curve from 2007 onward. Given their superior performance in the main time period of interest, we focus mainly on the implications of three-factor models. Importantly, many of the qualitative and quantitative implications for policy are insensitive to the choice of the number of factors used to estimate the models.

The paper proceeds as follows. The next section reviews the relevant literature. Section 3 describes the shadow rate model, our information assumptions, and the estimation. Section 4 discusses the estimation results and the implied paths of the shadow rate; section 5 examines the macroeconomic and policy applications of our estimates. We subsequently discuss results over the COVID-19 recession, followed by the conclusion.

## 2 Literature Review

Our paper contributes to the growing literature on shadow interest rates in arbitrage-free models, originated by Black (1995). The most closely related papers are Wu and Xia (2016), Priebsch (2013, 2017), Kim and Priebsch (2020), and Farmer (2017). We adopt the Wu and Xia (2016) approximation for short rates at the zero lower bound, and like them we investigate the properties of estimated shadow rates in a FAVAR. Farmer (2017) illustrated that the estimated parameters of a Wu and Xia (2016)-style three-factor model differ when obtained using the discretization filter. We extend the discretization filter to incorporate forecasts, estimate a wider range of models, and explore their policy implications. We also examine the out-of-sample performance of the different shadow rate models. Priebsch (2013, 2017) and

Kim and Priebsch (2020) incorporate forecasts, but mainly focus on the implications for the yield curve. They also assume that forecasts are formed via rational expectations, an assumption that we relax in this paper. Our results complement theirs by showing that including forecasts can improve the out-of-sample forecasting performance of these models in a pseudo-real-time environment. We also contribute to prior work by investigating the implications of our estimates for the assessment of LSAPs (D'Amico et al. (2013)).

Our paper is related to two other papers that obtain shadow rate estimates using term structure data. Bauer and Rudebusch (2016) estimate a term structure model with macroeconomic and (latent) financial factors using data from *prior* to the ZLB period associated with the Great Recession. They then use simulations over the ZLB period to find the modal forecast of the shadow rate and time to liftoff. Their paper emphasizes the sensitivity of shadow rate level estimates to model specifications, a theme we explore in other dimensions. Gust et al. (2017) estimate a shadow rate in the context of a dynamic stochastic general equilibrium model. We differ from these papers by jointly explaining forecasts and forward rates, by generalizing the forecast formation process, and using a fully nonlinear estimation method.

Several papers in the affine term structure literature have also incorporated forecasts into estimation – for example, Kim and Wright (2005), Wright (2011), and Kim and Orphanides (2012). Piazzesi et al. (2015) show that risk premia constructed using survey forecasts have different time series properties than those typically calculated from market data alone. We detail the relationship of our paper to Piazzesi et al. (2015) in more detail in section 3. Relative to these papers, we jointly account for survey forecasts and the ZLB in our framework.

A related literature connects forecast expectations to financial variables explicitly. Colacito et al. (2016) develop an equity pricing model that includes variance and skewness of professional forecasts, which they treat as exogenous. Barillas and Nimark (2018) and Struby (2018) estimate affine term structure models with dispersed information and survey forecasts. These papers do not incorporate the ZLB.

We contribute to a large literature attempting to measure the effects of Federal Reserve policy at the zero lower bound, especially forward guidance and LSAPs. Many of these papers, such as Krishnamurthy and Vissing-Jorgensen (2011) and Nakamura and Steinsson (2018b) use event studies (in part) to measure the impact of policy announcements.[1] Gagnon et al. (2011) use an event study and a reduced-form model of the term premium to measure the effects of LSAPs. Wright (2012) estimates a VAR, identifying monetary policy shocks using heteroskedasticity, as well as an event study approach, and finds that monetary stimulus at the ZLB has a short-lived effect on longer-term Treasury yields and corporate yields. Hanson and Stein (2015) find large effects of FOMC announcements over a sample that includes the ZLB period. Bauer and Rudebusch (2014) estimate a suite of affine term

[1] Martin and Milas (2012) and Swanson (2018) survey this literature.

structure models at a daily frequency to distinguish between the effect on term premia versus changes in the path of short rates (what they label the *forward guidance* effect). Their interest is on characterizing how model and parameter uncertainty affects the assessment of the two channels.

We differ from most of these papers by jointly estimating the dynamics of forecasts and forward rates in a structural model. The advantage of our approach is that it allows us to not just measure the raw effect of LSAPs, but understand the changes in expectations of short rates and risk premia, assess the perceived duration of the ZLB as it evolved over time, and examine the robustness of our results to different structural assumptions. D'Amico and King (2012) and Li and Wei (2013) examine the effects of changes in supply from LSAPs on term premia estimated using affine term structure models. We examine whether their interpretation of supply effects is robust to term premia estimated using shadow rate models.

Lastly, we contribute to a literature examining whether unconventional monetary policy stimulated real economic activity during the ZLB. Wu and Xia (2016) estimate a FAVAR using their shadow rate as the policy rate during the ZLB period associated with the Great Recession; they find monetary policy was effective at lowering the unemployment rate during this period. Corrado et al. (2021) find a similar result using the Wu and Xia (2016) and Krippner (2015) shadow rate estimates in the context of a Markov-switching FAVAR. We revisit these results using our suite of estimates based on alternative model specifications.

# 3 The shadow rate model and discretization filter

This section contains the details of the shadow rate model and estimation procedure. First, we outline the shadow rate model, following Wu and Xia (2016). Following that, we explain the alternative information assumptions we use to map the forecast data into the shadow rate model. Finally, we discuss estimation.

## 3.1 The Wu-Xia Shadow Rate Model

Following Wu and Xia (2016), the nominal short rate $r_t$ is given by

$$r_t = \max(\underline{\mathrm{r}}, s_t) \tag{1}$$

The shadow rate $s_t$ is affine in the state vector $X_t$:

$$s_t = \delta_0 + \delta_1 X_t \tag{2}$$

The stochastic discount factor $M_{t+1}$ is exponentially affine, and is related to the prices of risk $\lambda_t$ and innovations to the state $\varepsilon_{t+1}$:

$$\ln M_{t+1} \equiv m_{t+1} = -r_t - \frac{1}{2}\lambda_t' \lambda_t - \lambda_t' \varepsilon_{t+1} \tag{3}$$

The prices of risk are themselves a linear function of the state:

$$\lambda_t = \lambda_0 + \lambda_1 X_t \tag{4}$$

Using the superscript $\mathbb{Q}$ to indicate the risk-neutral probability measure, the law of motion for fundamental factors under the risk neutral measure is

$$X_{t+1} = \mu^{\mathbb{Q}} + \rho^{\mathbb{Q}} X_t + \Sigma\varepsilon^{\mathbb{Q}}_{t+1} \quad \varepsilon^{\mathbb{Q}}_{t+1} \sim^{\mathbb{Q}} N(0, I) \tag{5}$$

Under the physical measure, the law of motion is:

$$X_{t+1} = \mu + \rho X_t + \Sigma\varepsilon_{t+1} \quad \varepsilon_{t+1} \sim N(0, I) \tag{6}$$

The change of measure is related to the prices of risk (the $\lambda$ terms) and the sizes of risks that bond traders face ($\Sigma$) in the following way:

$$\mu - \mu^{\mathbb{Q}} = \Sigma\lambda_0 \tag{7}$$

$$\rho - \rho^{\mathbb{Q}} = \Sigma\lambda_1 \tag{8}$$

Finally, we denote the forward rate from $t + n$ to $t + n + 1$ as

$$f_{n,n+1,t} = (n + 1)y_{n+1,t} - ny_{n,t} \tag{9}$$

where $y_{n,t}$ is the log yield on a zero coupon bond bond that pays a dollar at time $t + n$. Given this setup,

$$f^{SR}_{n,n+1,t} \approx \underline{\mathrm{r}} + \sigma^{\mathbb{Q}}_n g\left(\frac{a_n + b_n X_t - \underline{\mathrm{r}}}{\sigma^{\mathbb{Q}}_n}\right) \tag{10}$$

where

$$g(z) = z\Phi(z) + \phi(z) \tag{11}$$

$\Phi(\cdot)$ and $\phi(\cdot)$ are a standard normal CDF and PDF, respectively (see Wu and Xia (2016) for details). $a_n$ and $b_n$ are nonlinear expressions of the prices of risk and parameters governing the state, and are defined explicitly in Appendix A.

## 3.2 Incorporating forecast survey data

We deviate from Wu and Xia (2016) and other earlier shadow rate estimates by incorporating forecasts data in the estimation. We utilize the Blue Chip Financial Forecasts survey, which is a monthly publication that collects macroeconomic and financial forecasts of market participants' beliefs over subsequent quarters.[2] We use the average forecasts of the 3-month constant-maturity Treasury bill yield to construct paths of average expected short-term rates over different horizons. We identify forecasts with the average short rate implied by the physical measure over the relevant horizon. Using analogous steps in the derivation of forward rates under the risk-neutral measure, one can show that expected short rates under the physical measure

[2] In Appendix B we show that the average forecast from the survey is consistent with short-term yields.

are

$$E_t\left[r_{t+n}\right] \approx \underline{\mathrm{r}} + \sigma_n^{\mathbb{P}} g\left(\frac{a_n^{\mathbb{P}} + b_n^{\mathbb{P}} X_t - \underline{\mathrm{r}}}{\sigma_n^{\mathbb{P}}}\right) \tag{12}$$

where

$$a_n^{\mathbb{P}} \equiv \delta_0 + \delta_1 \left[\sum_{j=0}^{n-1} (\rho)^j\right] \mu \tag{13}$$

$$b_n^{\mathbb{P}} \equiv \delta_1 (\rho)^n \tag{14}$$

$$(\sigma_t^{\mathbb{P}}) \equiv \mathrm{Var}_t^{\mathbb{P}} = \sum_{j=0}^{n-1} \delta_1 \left[(\rho)^j \Sigma\Sigma'(\rho')^j\right] \delta_1' \tag{15}$$

Because the Blue Chip survey asks questions about *quarterly* averages, the horizon over which respondents are actually forecasting varies depending on the survey month. We account for this by extracting the purely forward-looking component from current-quarter forecasts (see Appendix C). Forecasts at horizons beyond the current quarter are raw average forecasts provided by the survey. Depending on the survey month, the horizon over which the survey reflects a *forecast* is changing, which we adjust for in our estimation method as detailed in section 3.3.3.

The addition of surveys to the data set requires making an assumption about their data generating process. In an affine context, Kim and Orphanides (2012) treat average forecasts as generated under the physical measure and full information rational expectations (FIRE), and observed with *i.i.d.* measurement error. We refer to estimates made using these assumptions as "KO" estimates for brevity.

A large literature has documented that there are aspects of forecast surveys that are inconsistent with FIRE. In Appendix B, we show that forecast errors are predictable in economically and statistically significant ways. Because we are interested in whether our results are sensitive to the choice of *how* to add forecasts into the model, we also estimate models using a different strategy following Piazzesi et al. (2015). They use short rate forecasts to construct "subjective" interest rate expectations and risk premia using quarterly data. They estimate a statistical model of yields and expected inflation, and then estimate parameters governing the risk-neutral measure and a subjective "distorted" measure in a second step by minimizing mean square error between the model-implied yields and forecasts. We modify their approach along several dimensions. In addition to estimating a model with the ZLB, we focus on forecasts of three-month Treasuries at horizons of 1-18 months ahead. We also estimate the dynamics of the physical, risk neutral, and distorted measure jointly in a single (quasi-) maximum likelihood step. Like Piazzesi et al. (2015), we assume that forecasts are formed under a "distorted" physical measure as in equation (12), but with $\rho$ replaced by

$$\rho - \Sigma k$$

where $k$ is a conformable matrix of parameters that govern the degree of distortion. We refer to this set of estimates as the "PSS" model. In both the KO and PSS cases, we assume the *i.i.d.* measurement error has a constant variance across forecast horizons. Finally, we also estimate models without including forecasts, which we refer to as yields-only models ("YO"). The three-factor YO model (YO3) is most analogous to Wu and Xia (2016)'s estimates, with the main difference being the underlying yields data and the use of the discretization filter rather than the EKF. In some figures, we compare results based on our estimates to those of Wu and Xia (2016). We label their results as "WX."

## 3.3 Estimation details

This section contains details about the factor structure, mapping of the model into a state-space representation, and the estimation details.

### 3.3.1 Factor normalization and structure

We estimate the parameters governing the physical dynamics of our latent risk factors (equations (5) and (6)) and the prices of risk parameters $\lambda_0$ and $\lambda_1$. For three-factor models, we impose a similar normalization to Joslin et al. (2011) and Wu and Xia (2016) for the risk neutral factors:

$$\delta_1' = \begin{bmatrix} 1 \\ 1 \\ 0 \end{bmatrix} \tag{16}$$

$$\mu^{\mathbb{Q}} = \mathbf{0} \tag{17}$$

We also assume $\rho^{\mathbb{Q}}$ is in real Jordan form with eigenvalues in descending order and $\Sigma$ is lower triangular. Joslin et al. (2011) show that this is the maximally flexible specification for latent dynamics that is econometrically identified. Unlike Wu and Xia (2016), we do not impose a repeated eigenvalue in estimation. We also estimate two-factor versions of each model with analogous restrictions following Krippner (2015).

#### 3.3.2 The nonlinear state-space representation

Throughout, we assume that the state equation is a VAR(1). The physical dynamics of the fundamental states are as in equation (6). The observed forward rate is the same as in equation (10) augmented with measurement error:

$$f_{n,n+1,t} = \underline{\mathrm{r}} + \sigma_n^{\mathbb{Q}} g\left(\frac{a_n + b_n' X_t - \underline{\mathrm{r}}}{\sigma_n^{\mathbb{Q}}}\right) + \omega e_{nt} \tag{18}$$

where $\omega$ is a measurement error parameter common across horizons and $e_{nt} \sim \mathbb{N}(0,1)$. We allow this measurement error to have a different variance than the measurement error of forecasts. Generically, we collect observables by stacking them in an observation equation $G_t(X_t)$.

### 3.3.3 Details of the estimation procedure

The data runs from 1987-2019 at a monthly frequency. Forward rates are constructed using the Liu and Wu (2021) yield curve estimates and averaged over the month (to align the data with the Blue Chip question wording).

We estimate the nonlinear state space model using the discretization filter proposed by Farmer (2021). Farmer's discretization filter approximates the state distribution on a discrete grid. We use the method outlined in Gospodinov and Lkhagvasuren (2014) to approximate the state distribution, choosing grid points to approximate the first two moments of the underlying Gaussian VAR. We then use $G_t(X_t)$ to calculate predicted values of the observable forward rates and forecasts at each point on the grid. Treating each point as a regime for the data, the likelihood is estimated in a method similar to the Hamilton (1989) filter. Standard errors on parameter estimates are QMLE standard errors as in Hamilton (1989). We estimate smoothed states via an appropriately modified version of Kim's smoother (Kim (1994)).

We adjust the Farmer (2021) procedure slightly to account for the fact that we do not observe each forecast horizon every month. Our treatment of missing values is analogous to the treatment of missing data in other state space models (e.g. Durbin and Koopman (2012)). $G_t(X_t)$ has a time-varying dimension to capture the fact that forecasts of particular horizons are periodically unobservable. Because which forecasts are missing is a deterministic function of time and unrelated to the (hypothetical) values of those forecasts, the missing observations are "ignorable" in the sense of Rubin (1976). Hence, they do not affect inference of the parameters governing the hidden Markov model (see Speekenbrink and Visser (2021)). The process for which observations are missing can be factored out of the likelihood function, the resulting likelihood is proportional to the likelihood function without missing data, and the convergence proofs in Douc et al. (2004), Douc et al. (2011) and Farmer (2021) are unaffected.

We adopt the discretization filter, rather than using the extended Kalman filter (EKF) as in Wu and Xia (2016). Both involve approximation: the discretization filter approximates the state space on a grid, while the EKF linearizes the observation and state equations locally. Farmer (2021) shows in the context of a different model that the discretization filter can achieve lower root mean square error and bias than the EKF. We present results from estimating a three-factor, yields-only version of the model with the EKF in Appendix G. For that model, the within-sample fits for both the EKF and the discretization filter are similar. Out of sample, the discretization filter-based models are competitive in forecasting relative to those estimated with the EKF. In practice, we also found that the EKF was much more computationally expensive in models that incorporate forecasts.[3]

[3]The EKF is much quicker than the discretization filter for yields-only models. However, at each time $t$, the EKF requires linearization of the observation equation $G_t$ to evaluate the likelihood, which adds a considerable computational burden when the dimension of $G_t$ varies over time, as in the KO and PSS models.

# 4 Estimation results

In this section we discuss parameter estimates and model fit, and present the estimated shadow rates and implied ZLB duration.

## 4.1 Parameter estimates and model fit

We report parameter estimates for all models in Appendix D. In the estimation, we constrain the lower bound parameter to fall below 25 basis points, which is the value assumed by Wu and Xia (2016). For the three-factor models, the estimates fall between 9 and 21 basis points. For reference, our lowest estimate is similar to that used by Federal Reserve Board staff estimates in 2012 (around 10 basis points).

Model-implied shadow rate expectations depend on the set of parameters governing the physical dynamics of the factors. Our three-factor estimates mainly differ in the third eigenvalue. The first and second eigenvalues are in the neighborhood of .98 and .97 for each system. The KO and YO models have a somewhat larger third eigenvalue (on the order of 0.71) than the PSS estimates (about 0.67). Long-run implied short rates range from 3.1 (PSS) to 3.6 (KO).

The PSS models also allow for differences in the physical dynamics and the time-series process perceived by forecasters. The differences in estimated eigenvalues are small, but noticeable. Impulse responses to $s_t$ under each measure (shown in figure 1) are qualitatively different, depending on the factor specification.

For the two-factor model, shocks to the first factor peak earlier but have smaller effects at longer horizons; for the second, shocks are less persistent at every horizon. A similar dynamic (albeit with smaller absolute differences) is reflected in shocks to the second and third factors of the PSS model. Although the differences may appear quantitatively slight, they are large enough to imply differences in perceived duration of the ZLB across the two measures, as we will see below.

**In- and out-of-sample fit** Having estimated the complete suite of models, we now investigate how well they can explain the data.

In terms of comparisons across models, different criteria point to different models as providing the best fit. Since the PSS three-factor models nest the other specifications, achieves the highest likelihood. Arguably, we should focus on the ability of the model to capture the data in the post-2007 period – i.e., how well do shadow rate models capture the data at the zero lower bound and in the post-ZLB regime? Appendix figure D.1 shows the average yield curve predicted during the ZLB period versus the data. Each model appears to capture the shifts and changes in the shape of the yield curve during the ZLB period and the nonlinearity at the short end. Panel A of Table 1 summarizes in-sample fit (mean absolute error and root mean square error), on average, for each model. The three-factor YO model appears to have the best performance matching yields in-sample (although it does not have to simultaneously explain forecasts and yields).

We also examine out-of-sample fit in a pseudo-real-time forecasting exercise for the ZLB period. Starting in 2007, we estimate the model parameters using only the data available as of December of that year. We then forecast forward rates at monthly horizons 1- to 12-months ahead, and we re-estimate adding the subsequent 12 months of data. Although our main sample ends in 2019, we conduct the pseudo-forecasting exercise all the way through the end of 2023. Hence, for forward rates of each maturity, we have 216 (18 estimates of 12 horizons) sets of forecasts.[4] Panel B of Table 1 displays the RMSE and MAE from this exercise, and panels C and D break the out-of-sample forecasting results between the pre-COVID (2007-2019) and post-COVID period (2020-2023). Overall, panel B suggests that the three-factor estimates generally outperform their two factor counterpacts, with the best model depending on the horizon of interest. For the subperiod between 2020 and 2023, two factor models tend to have the lowest estimated forecast error, although the advantage is slight. Taken together, these results seem to point towards the superiority of three-factor models relative to their two-factor counterparts, particularly for the analysis of policy prior to 2019. In our subsequent discussion, we focus mainly on the implications of the three-factor models. The results are less definitive on which specification – YO, KO, or PSS – is most useful for applied work. The YO model as the best overall fit of the actual yields data during the period of interest. The PSS model and KO models

[4]Because of the computational burden of estimating the model, re-estimating for each additional month of data is infeasible.

trade off relative ability to fit within sample, depending on the horizon, and the PSS model appears to have the smallest pseudo-forecast error at the long end of the yield curve. Hence, we generally focus on areas of consensus and disagreement across the three specifications.

## 4.2 Shadow rate estimates and the forecasted length of the ZLB period

Figure 2 shows our shadow rate estimates alongside the Wu and Xia (2016) estimates during the ZLB period for the three-factor models. We also indicate a selection of policy event dates during this period. The *level* of our estimates is generally lower than that estimated by Wu and Xia (2016), at least through the taper announcement. Our estimated rates are somewhat smoother than the EKF estimates, and tighten following the taper announcement.

The level of the shadow rate reflects the speed with which short-term interest rates are projected to revert to their mean. Arguably, *when* short-term interest rates rise above their lower bound is more informative. We translate the level of the shadow rate to the implied belief about how long short rates will remain at the ZLB. This duration is shown in Figure 3. All of our models suggest that market participants initially under-predicted the (ex-post) duration of the zero lower bound.

In August 2011, the Federal Open Market Committee (FOMC) introduced specific calendar-based forward guidance in their Summary of Economic Projections (SEP). In Figure 3, the black dashed lines indicate the range of dates for target rate liftoff implied by the SEP. These dates are shown as a range because they are only reported using end-of-quarter or end-of-year estimates for the federal funds rate. The introduction of calendar-based forward guidance corresponded with an upward reassessment of the expected ZLB duration in the KO and PSS models, but not the YO model. We display this effect in Figure 4, which focuses on the period surrounding the introduction of the guidance. The point estimates suggest that calendar-based forward guidance extended the market's estimate of the ZLB duration by between 5 months in the KO model and 15 in the PSS model. Although it does not experience a change at the forward guidance announcement, the YO3 model's implied liftoff date drifts upwards in the months prior to the announcement. This could reflect market reaction to the same information that contributed to the FOMC's new policy.

The models do not agree on the precise month of liftoff even after the introduction of calendar-based forward guidance, although all of the models agree that liftoff would occur sometime after mid-2013. The YO and KO models suggests market participants' beliefs were consistent with the SEP's guidance of "exceptionally low levels for the federal funds rate at least through mid-2013." although the diverge somewhat towards the end of the ZLB period (less so for KO). The beliefs implied by the physical and distorted dynamics of the PSS model diverge, with the physical dynamics implying a longer duration of the ZLB relative to other estimates (and the SEP's guidance) after the introduction of calendar-based forward guidance. The distorted dynamics, by contrast, imply beliefs that are more-or-less consistent with the SEP's forward guidance. The difference in expected duration across models – particularly from 2014-onward – is informative about the interpretation of each. The PSS model essentially relaxes a requirement imposed by the KO model that the market forecast and the Blue Chip forecast must have the same data generating process. The results imply that Blue Chip forecasters' beliefs were essentially consistent with the forward guidance provided by the FOMC. By contrast, the "market" forecast (implied by the physical measure) implied more persistence in the underlying factors that drive short rates in order to match the actual behavior of forward rates. In other words, the estimated physical dynamics implied that once rates were low, they would stay low for longer, perhaps reflected in the median YO and KO durations being at or above the upper bound implied by the SEP starting around 2014. The KO model, in attempting to reconcile a possible change in the beliefs of participants with what it sees in yields, appears to split the difference relative to the YO and PSS measures.[5]

In short, our results confirm that markets initially under-estimated how long interest rates would remain at their lower bound in 2008 and 2009. There is evidence that calendar-based forward guidance shifted the market's perceived duration of the ZLB in the desired direction, although the exact degree to which that occurred differs across models, as does whether the

[5] Interestingly, relatively more persistence in physical than subjective short rates is the opposite of the estimated result in Piazzesi et al. (2015).

models imply beliefs consistent with the SEP's calendar.

## 5 The effects of monetary policy during and after the Great Recession

In the previous section, we showed how beliefs about the effective liftoff of monetary policy evolved over the course of the ZLB period. In some ways, the features of these beliefs reflect very different beliefs about monetary policy. In this section, we explore *how* novel monetary policy tools affected asset prices and the macroeconomy during this period, viewed through the lens of our estimates. The punchline is a relative *insensitivity* of policy assessment despite these differences.

We conduct three exercises. First, we estimate a structural FAVAR as in Wu and Xia (2016). We test whether there was a structural break during this period; we find mixed evidence for a break. This raises some caution in using the shadow rate to "plug in" for the effective federal funds rate. When we estimate impulse responses using the original sample of Wu and Xia, we find somewhat smaller effects of monetary policy shocks. However, using alternative sample (1987-2019), we find larger (albeit imprecisely estimated) effects. The results are remarkably consistent across all of the shadow rate estimates, although they are sensitive to choices like lag length in the FAVAR.

Second, we examine the effects of policy announcements on term premia. All of our models suggest there were substantial reductions in yields and term premia around the first two rounds of LSAPs and the introduction of calendar-based forward guidance, but not for the third round of LSAPs.

Third, we relate our term premia estimates to measures of Treasury supply and duration in the pre-ZLB period and decompose the first round of LSAPs into different supply channels. Consistent with prior studies, we find that term premia fell as aggregate duration was removed from the market and that there is evidence of "local scarcity" effects on term premia.

## 5.1 Estimation of FAVAR and the effects of monetary policy shocks

Following Wu and Xia (2016), we estimate a Factor-Augmented Vector Autoregression (FAVAR) as in Bernanke et al. (2005), where we substitute the shadow rate for the effective federal funds rate when policy rates are constrained. To isolate the difference driven by shadow rate estimates alone, we initially use the same data specification and code as Wu and Xia (2016) to estimate the FAVAR. The unrestricted model is:

$$\begin{aligned}
\begin{bmatrix} F_t \\ s_t \end{bmatrix} =& \mathbf{1}_{t<\text{Dec 2007}} \mathbf{B_1}(\mathbf{L}) \begin{bmatrix} F_{t-1} \\ s_{t-1} \end{bmatrix} \\
&+ \mathbf{1}_{t>\text{Dec 2007 \& } < \text{ July 2009}} \mathbf{B_2}(\mathbf{L}) \begin{bmatrix} F_{t-1} \\ s_{t-1} \end{bmatrix} \\
&+ \mathbf{1}_{t\geq \text{ July 2009}} \mathbf{B_3}(\mathbf{L}) \begin{bmatrix} F_{t-1} \\ s_{t-1} \end{bmatrix} + v_t
\end{aligned}$$

By construction the macroeconomic factors ($F_t$) have been purged of effects from the policy/shadow rate, and the variance-covariance matrix of the structural shocks $v_t$ is lower triangular.

The null hypothesis of the structural break test is that $\mathbf{B_1}(\mathbf{L})$ and $\mathbf{B_3}(\mathbf{L})$ are equal. Like in Wu and Xia (2016), we examine whether this is the case using a likelihood ratio test adjusting for small-sample bias (Sims (1980)). The first column of table 2, panel (a) reports the p-value for the coefficients of lagged shadow rates vis-a-vis the macroeconomic factors. Focusing on the three-factor specifications, we fail to reject the null for the YO and PSS models, but do reject it for the KO model at the 10% level. The second column reports the p-value for the coefficients of lagged macroeconomic factors on shadow rates. We reject the null for the yields-only model, but not the PSS model.[6]

Because our shadow rate estimates are based on smoothed factors, they cover the full sample of yields. Since the contribution of Wu and Xia (2016), we have an additional ZLB period that may be informative about the parameters governing the shadow rate. As an experiment, we substitute shadow rates based on our yield and parameter estimates from 1987-2019 to those using data from 1987-2023 (e.g., the COVID-19 pandemic period and its aftermath). We similarly estimate the FAVAR using the same data and spec-

[6]While we focus mainly on the three-factor specifications, we note that we *do* reject the null for each of the two-factor specifications in at least one of the tests. As mentioned in the introduction, a number of authors continue to use shadow rates estimated with two factors in applied work, at least as a robustness check; our results suggest they should be very cautions about the presence of a structural break.

ification as Wu and Xia, covering the period 1960-2013, so any differences are driven completely by new information about the shadow rate alone and subsequent macroeconomic developments. The results are shown in panel (b) of Table 2. We fail to reject either null for the 3-factor YO and PSS estimates, but find significant evidence of a structural break for the KO estimates (and for at least one hypothesis for all of the two-factor estimates). While not definitive, these result cautions against assuming continuity in macro-monetary policy relationships before and after the Great Recession.[7]

**Is it the shadow rate or the macroeconomic data?** Up to this point, we have focused on whether swapping our shadow rate estimates in for the Wu and Xia (2016) estimate materially impacts the FAVAR structural break test. One way of seeing if the *substantive* conclusions differ across our rates is to focus on the "post-crisis" period. Figure 5 compares the impulse response for the unemployment rate to 25 basis point decreases in the policy rate for an estimated FAVAR(1) using the same macroeconomic data as Wu and Xia, but swapping our shadow rate estimates. From the figure, we see that the median impulse responses across models are relatively similar. Although the peak magnitude of the effects are somewhat smaller, the confidence intervals generally include the Wu and Xia estimates. However, they also generally

[7]This result is in contrast to Wu and Xia (2016)'s suggestion that "the continuity of our shadow rate allows researchers to update their favorite VAR during and post the ZLB period." While it is true that models estimated with their shadow rate to not appear to have a structural break at the Great Recession, this is not necessarily true of any given shadow rate estimate. We note that estimating using the Liu and Wu (2021) yields and our extended sample, but using the EKF as in Wu and Xia (2016), we do not reject the null of no structural break before and after the Great Recession.

include zero, reflecting a great deal of uncertainty in the estimated effects of shocks.

Subsequently, we estimate the FAVAR on a different macroeconomic sample that partially overlaps with the that used in Wu and Xia. We match as many of the data series used in Wu and Xia (2016) as possible to their analogs in the FRED-MD data set (McCracken and Ng (2015)), from 1987 to 2019.[8] Other than changing the data set, we maintain the same FAVAR(13) specification for the complete sample and FAVAR(1) for the "post-crisis" (July 2009-onward) sample as Wu and Xia (2016). The resulting impulse responses are shown in Figures 6 and 7, respectively. The first column of each figure, labeled WX (2016), replicates the existing estimates from Wu and Xia for reference. The second uses the December 2024 vintage of Wu and Xia shadow rate estimates provided by the Federal Reserve Bank of Atlanta, combined with our alternative macroeconomic sample. Subsequent columns report impulse responses using our shadow rate estimates. We generally find quantitatively larger effects for the median impulse response, although the confidence intervals are generally sizable. However, for the post-crisis sample (see Figure 7), the macroeconomic effects are basically null (and the median impulse response has the wrong sign)). Since this is also true for the updated WX estimates (column 2) and the EKF (column 6), this not due to the choice

[8]Because we did not have access to the Global Insight data used by Wu and Xia (2016), we can not match all of the series. The main difference is that FRED-MD does not contain the Institute for Supply Management survey data. We use the FRED-MD vintage as of November 2024. We follow Wu and Xia (2016)'s transformations of the underlying data. The series from FRED-MD used in our estimates are listed in Online Appendix J.

of shadow rate. It seems most likely that the limited effects are driven by misspecification of the FAVAR and the choice of macroeconomic sample.

It is worth emphasizing that the presence of a structural break, and the differences in impulse responses, do not necessarily imply the shadow rates are not a useful replacement for the federal funds rate in monetary VARs. Our argument is that care is needed; the choice of sample and lag length appears to be more material than the method of estimating the shadow rate in this particular application.

## 5.2 Decomposition of yields around policy events

Changes to the stance of monetary policy can simultaneously affect both expected future short-term rates and expected future risk premia.[9] We use our estimates of shadow rate paths to decompose changes in the monthly 10-year Treasury yield following select Federal Reserve unconventional policy event dates during the ZLB period. Term premia are calculated by using the estimated parameters to calculate the expected path of nominal *actual* short rates out to ten years – that is, the expectations hypothesis component reflects the effective lower bound on interest rates.

The effect of unconventional policy on term premia is debated in the literature. For example, Gagnon et al. (2011) argue that early asset pur-

[9] Hanson and Stein (2015) find significant effects of changes in the two-year U.S. Treasury yield on long-term real rates in a two-day window of FOMC announcements. In contrast, Nakamura and Steinsson (2018a) provide high-frequency evidence that term premia are virtually unaffected by monetary policy shocks. Kuttner (2018) surveys the evidence on the effectiveness of unconventional policy.

chases were consistent with a portfolio rebalancing channel through which the reduction in supply of long-duration assets reduced term premia and hence long-term yields.[10] Swanson (2018) finds that LSAPs affected long term yields, while forward guidance affected short-term yields. But Krishnamurthy and Vissing-Jorgensen (2011) attribute some change in yields to changes in the expectations hypothesis effects and Bauer and Rudebusch (2014) attribute roughly 40-50% of the reduction in 10-year yields around LSAP events to changes in policy rate expectations.

Using our collection of models, we decompose rates on 10-year Treasuries into their expectations hypothesis and term premium components, in order to understand whether our results suggest these events primarily operated through term premia or not.[11] Figure 8 displays the 10-year U.S. Treasury yield in black along with the EH component of yields calculated from each model. The decomposition shows that the 10-year yield experienced steep declines in the months of major early Federal Reserve policy announcements. For example, between November and December 2008 (LSAP1), the 10-year yield fell by about 112 basis points.

All of our models attribute a small share of this decline to the EH component of yields - ranging from 5 basis points for the three-factor YO model to 40 basis points for the PSS model. This implies the share of the decrease

[10] The June 2020 assessment of the Fed's monetary-policy framework (Caldara et al. (2020)) cited effects on the term premium from LSAP1 event dates of Gagnon et al. (2011).

[11] The decomposition of the yields across maturities for the entire sample is available upon request.

in yields from the LSAP announcement attributable to changes in the EH component is between 4% and 36%. Notably, the three-factor PSS EH component displayed in Figure 8 tracks those of the other two three-factor specifications until roughly the second round of LSAPs and the divergence is more prominent beginning with the introduction of forward guidance. This is consistent with the larger jump in expected duration of the zero lower bound displayed in Figures 3 and 4 for the PSS model. Following the taper announcement and at the end of the ZLB period, the EH component of all three models climbs slowly, and the PSS model starts to align more with the ten year yields towards the end of the sample.

Narrowing in on three major event dates, Figure 9 plots the cumulative change in the 10-year yield (black line) and EH components across models from the month before LSAPs 1 and 2 and the introduction of calendar-based forward guidance (FG). The bulk of the change in the 10-year yield following LSAP1 and the forward guidance announcement were due to changes in term premia. In contrast, the three-factor models that include forecasts attribute much of the change around LSAP2 to the EH component.

In summary, the interpretation of how major policy announcements affected yields appears to be somewhat sensitive to a number of features of the model used to estimate those premia. Given this sensitivity, it is unsurprising that previous efforts have found mixed evidence on the precise effects of policy announcements. However, the results here imply that the range of disagreement appears to be about whether term premia explain about two thirds or nearly all of the cumulative change in yields.[12]

### 5.3 Supply effects and term premia

Previous studies have found that the composition of medium- to long-term Treasury securities in the Fed's portfolio can have sizable effects on yields.[13] To the extent that LSAPs change that composition, the effects could operate either through changes in the EH component of yields or through the term premium. The former channel would suggest that LSAPs signal changes in expectations of the path of future short-term rates (Krishnamurthy and Vissing-Jorgensen (2011)). Alternatively, the Fed's purchases could coincide with lower interest-rate risk through the removal of aggregate *duration* of Treasury securities (Gagnon et al. (2011)) or changes in the *scarcity* of assets with similar maturities (D'Amico et al. (2012)). The results in the previous section suggest that the majority of the change in yields during the first round of LSAPs was due to changes in term premia. Hence, in this section, we examine the channels for those changes in term premia and whether conclusions about those channels differs based on the method of estimating term premia.

To construct measures of Treasury supply, we first merge the CUSIP identifiers of all outstanding U.S. Treasury securities from the Center for Research in Securities Prices with the Fed's weekly System Open Market Account

[12]This particular result is quantitatively sensitive to the choice of underlying yield curve data, a point we discuss more in appendix I.

[13]D'Amico et al. (2012) and Huther et al. (2017) provide thorough historical descriptions of the Fed's balance sheet policies.

(SOMA) holdings and Treasury buyback operations. Following D'Amico et al. (2012), we proxy for local scarcity using privately held nominal Treasuries (PHNT), the share of Treasury securities held by the private sector - outside the Federal Reserve and U.S. government. We focus on the holdings of securities with maturities ranging from 2 to 10 years as a share of total Treasury debt outstanding, due to the Fed's concentration in purchases of these assets in 2008. To proxy for duration risk, we calculate the duration gap (DG), the difference between aggregate duration risk in the 2-10 year maturity bucket and the duration of the on-the-run 10-year Treasury bond. Aggregate duration risk is the sum of modified duration weighted by PHNT for each CUSIP. In addition, we control for the slope of the term structure, proxied by the difference between the 10-year and 2-year nominal Treasury yields. The regression equation is:

$$\text{TP(10yr)}_t = \beta_0 + \beta_1 PHNT(m : 2 - 10)_t + \beta_2 DG_t + \beta_3 \text{Slope(10-2yr)}_{t-1} + \epsilon_t \tag{19}$$

Table 3 displays results from these regressions. In the first column, we regress 10-year U.S. Treasury yields (rather than term premia) against our local scarcity, duration, and slope proxies. The adjusted $R^2$ for this regression is about 56%. We then use the model-implied monthly 10-year term premium as the dependent variable in regression (19) to examine whether the impacts of policy differ across models. We find robust evidence that local scarcity and the duration gap both significantly explain term premia;

the slope is also generally significant. D'Amico et al. (2012), using weekly data over the same sample period and a term premium estimated using an affine term structure model, found point estimates of 4.34 and 123.47 for local scarcity and duration, respectively. In both cases, these variables were robustly significant in explaining 10-year term premia. Our estimated point elasticities on local scarcity are 20-30% smaller, while the elasticity of term premia with respect to the duration gap is anywhere between 5% smaller to 30% larger. As emphasized by Wu and Xia (2016) and Bauer and Rudebusch (2016), the behavior of short-term rates (and hence expected rates and term premia) is quite different for affine versus nonlinear models which likely affects the results. But our suite of models suggests that the local scarcity channel's statistical and economic significance is not especially sensitive to the term structure model used to estimate term premia.

We use the estimated point elasticities from the pre-2008 sample to predict the effects of changes in supply on yields and term premia. D'Amico et al. (2013) document that the first (second) round of LSAPs decreased privately held nominal treasuries by about 4.69% (6.98%) and decreased the average duration gap by about 0.12 (0.10) years. Their estimates imply that the first LSAP program decreased term premia by about 42 basis points overall. Using our estimated results from Table 3, we calculate the predicted change in 10-year Treasury yields and term premia, with results reported in Table 4. Based on these estimates, we would have predicted yields to decrease by about 39.6 basis points overall as a result of LSAP1 and 43.7 basis points as a result of LSAP2. We interpret these numbers as the predicted change in yields attributable to the supply factors in the reduced-form model. Term premia are predicted to fall between 29 and 34 basis points for LSAP1 and 34 to 44 basis points for LSAP2, depending on the model. Given that the (predicted) change in yields must be attributed to either term premia or expectations, we interpret the residual as the variation in short-rate expectations induced by the supply changes from the LSAP programs. These effects are statistically insignificant and relatively small.

We are cautious to not draw a causal interpretation from these regressions. Moreover, these results in isolation do not imply that there were no signaling effects of the LSAP programs, since they are isolated to the effects predicted by duration and local scarcity effects. The robust finding across our specifications is that the change in duration and local scarcity were associated with economically and statistically significant changes in term premia for 10-year Treasuries. However, the precise magnitudes and *relative* importance of each channel varies somewhat across the specifications used to estimated term premia. Moreover, our estimated term premium changes are about 15-30% smaller than the elasticities reported in D'Amico et al. (2012), and we generally estimate relatively smaller contributions from scarcity and larger from duration relative to that paper. However, the differences *across* shadow rate estimates are not particularly notable.

# 6 What do we learn from the COVID-19 recession?

Our focus in this paper has been on the ZLB period following the 2008 financial crisis and the effects of monetary policy at that time. Accordingly, we ended our main sample in 2019, in order to avoid influencing our estimates with the dramatic, but short-lived, recession associated with the COVID-19 pandemic. The Federal Reserve lowered rates to their lower bound during 2020 and maintained that policy for two years, so it is worth examining how our models handle this period. We plot the yields from this period in Figure 11. The decline in yields was quite sharp at the beginning of both crises, but the length of time at the lower bound was notably shorter for the pandemic recession, and the increase in rates at the short end of the yield curve was much more rapid once the Federal Reserve began tightening.

We have already noted in section 5 that yields data from this period inform the estimates of shadow rates for earlier periods. This affects conclusions about applications back to the Great Recession period. We report parameter estimates for the extended sample in online appendix H, as well as a measure of average fit. The models seem to be able to capture the general contours of the yield curve during this period.

We also examine the implied duration of the ZLB during this period, compared to the guidance from the Fed's SEP (Figure 12). The three-factor PSS models are most consistent with the SEP's guidance during this period, while the YO and KO models tend to fall under the duration implied by the SEP's guidance.

Taking these results alongside those from section 5, it seems that three-factor shadow rate models that incorporate forecast information are capable of bridging both recent ZLB episodes, although (like for the pre-COVID period), the implied level of the shadow rate and duration of the ZLB may differ depending on different specifications.

# 7 Conclusion

Using data on forecasts and financial prices, we estimate shadow rates, interest rate expectations, and term premia for US Treasury markets during the zero lower bound period associated with the Great Recession. We extend on previous work by fully estimating a nonlinear state space model, incorporating interest rate forecasts alongside forward rates, allowing for deviations from full information rational expectations in those forecasts, and incorporating new yield curve estimates that incorporate more information from the short end of the maturity spectrum.

Our goal in this paper has been to identify robust effects of monetary policy at the zero lower bound and, additionally, to provide some guidance for applied researchers who are interested in producing or using estimated shadow rates. We find that the Farmer (2021) discretization filter appears to be an accurate and computationally competitive alternative to the extended Kalman filter for estimating shadow rate models, and our results generally support using three-factor, rather than two-factor specifications (and incorporating interest rate forecasts).

We find that despite differences in features of the estimated shadow rates, the conclusions about the effects of policy are remarkably consistent across different choices about data and factor structures. In particular, while we obtain different estimated impulse responses estimates than Wu and Xia (2016), the difference appears to be driven by choices like the sample horizon and macroeconomic data, not the way the shadow rate is estimated. Other policy assessments, like the response of risk premia to announcements or the assessment of supply-based channels of LSAPs, are similarly insensitive to choice of shadow rate estimation method, although there is some quantitative variation.

We have not closely investigated whether innovations to the shadow rate were driven by particular factor innovations that can be linked to macroeconomic or financial developments. But our estimated factors are correlated with macroeconomic indicators such as labor market variables; like interest rate forecasts (Caldara et al. (2020)), forecasts of labor market variables were also subject to substantial over-optimism and revision during the recovery from the Great Recession. Accounting for variation in the yield curve using a macro-factor structure and respecting the zero lower bound would be a natural extension.

# 8 Tables

Table 1: Table reports model fits. Columns 1-6 report estimates for the mean absolute error (MAE) and Columns 7-12 report estimates for the root-mean-square error (RMSE) across models. Panel A contains estimates for the in-sample fit that uses all observations (396 months). Panel B contains estimates for the out-of-sample fit, which estimates the model each December from 2007-2023, and calculates forecasts for 1- to 12-months ahead. Panels C and D report subcomponents of the out-of-sample forecasts splitting the sample before and after the COVID-19 pandemic. MAE and RMSE are reported across all horizons (10 sets of forecasts at 12 horizons each).

| Statistic | MAE | | | | | | RMSE | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Model | YO | | KO | | PS | | YO | | KO | | PS | |
| Factors | 2 | 3 | 2 | 3 | 2 | 3 | 2 | 3 | 2 | 3 | 2 | 3 |
| Panel A: In-Sample Fit (N=384) | | | | | | | | | | | | |
| 3mo | 0.18 | **0.18** | 0.21 | 0.22 | 0.19 | 0.26 | 0.24 | **0.24** | 0.26 | 0.29 | 0.24 | 0.34 |
| 6mo | 0.11 | **0.10** | 0.22 | 0.27 | 0.16 | 0.19 | **0.14** | 0.15 | 0.32 | 0.37 | 0.22 | 0.25 |
| 12mo | 0.18 | **0.16** | 0.31 | 0.35 | 0.23 | 0.22 | 0.23 | **0.20** | 0.42 | 0.47 | 0.29 | 0.26 |
| 24mo | 0.24 | **0.20** | 0.31 | 0.33 | 0.25 | 0.24 | 0.31 | **0.27** | 0.40 | 0.43 | 0.33 | 0.30 |
| 60mo | 0.26 | **0.24** | 0.26 | 0.26 | 0.25 | 0.25 | 0.34 | **0.32** | 0.34 | 0.34 | 0.34 | 0.32 |
| 84mo | 0.20 | **0.19** | 0.24 | 0.22 | 0.26 | 0.24 | 0.28 | **0.25** | 0.32 | 0.29 | 0.34 | 0.30 |
| 120mo | 0.33 | **0.24** | 0.34 | 0.28 | 0.36 | 0.28 | 0.47 | **0.32** | 0.48 | 0.37 | 0.52 | 0.39 |
| Panel B: Out-of-Sample Fit: 1-12 month-ahead forecasts (N=180) | | | | | | | | | | | | |
| 3mo | 0.45 | **0.36** | 0.46 | 0.40 | 0.43 | 0.41 | 0.76 | **0.63** | 0.81 | 0.69 | 0.82 | 0.80 |
| 6mo | 0.45 | **0.36** | 0.52 | 0.40 | 0.46 | 0.47 | 0.78 | **0.65** | 0.85 | 0.70 | 0.83 | 0.83 |
| 12mo | 0.50 | **0.39** | 0.57 | 0.44 | 0.49 | 0.53 | 0.76 | **0.63** | 0.82 | 0.68 | 0.76 | 0.80 |
| 24mo | 0.51 | **0.42** | 0.55 | 0.53 | 0.51 | 0.51 | 0.69 | **0.59** | 0.75 | 0.69 | 0.70 | 0.68 |
| 60mo | 0.60 | **0.50** | 0.55 | 0.56 | 0.69 | 0.57 | 0.74 | **0.65** | 0.71 | 0.72 | 0.91 | 0.73 |
| 84mo | 0.71 | 0.61 | 0.72 | 0.71 | 1.12 | **0.59** | 0.87 | 0.78 | 0.91 | 0.92 | 1.29 | **0.78** |
| 120mo | 0.87 | 0.72 | 0.91 | 0.84 | 1.26 | **0.70** | 1.10 | 0.91 | 1.12 | 1.09 | 1.49 | **0.88** |
| Panel B: Out-of-Sample Fit, 2007-2019, 1-12 month-ahead forecasts (N=132) | | | | | | | | | | | | |
| 3mo | 0.33 | **0.22** | 0.29 | 0.31 | 0.30 | 0.28 | 0.46 | **0.28** | 0.39 | 0.48 | 0.42 | 0.39 |
| 6mo | 0.33 | **0.23** | 0.38 | 0.31 | 0.32 | 0.34 | 0.46 | **0.31** | 0.49 | 0.47 | 0.46 | 0.46 |
| 12mo | 0.39 | **0.28** | 0.47 | 0.37 | 0.37 | 0.41 | 0.51 | **0.38** | 0.59 | 0.52 | 0.45 | 0.51 |
| 24mo | 0.45 | **0.36** | 0.53 | 0.51 | 0.39 | 0.46 | 0.61 | **0.53** | 0.73 | 0.69 | 0.55 | 0.60 |
| 60mo | 0.62 | **0.52** | 0.56 | 0.62 | 0.72 | 0.57 | 0.75 | **0.68** | 0.73 | 0.79 | 0.90 | 0.74 |
| 84mo | 0.83 | 0.69 | 0.85 | 0.86 | 1.06 | **0.67** | 0.99 | 0.87 | 1.01 | 1.05 | 1.26 | **0.86** |
| 120mo | 1.05 | 0.78 | 1.03 | 1.03 | 1.22 | **0.76** | 1.25 | 0.97 | 1.24 | 1.24 | 1.47 | **0.95** |
| Panel B: Out-of-Sample Fit, 2020-2023,-1-12 month-ahead forecasts (N=48) | | | | | | | | | | | | |
| 3mo | 0.31 | 0.24 | 0.31 | 0.22 | 0.13 | **0.11** | 0.41 | 0.26 | 0.43 | 0.25 | 0.15 | **0.14** |
| 6mo | 0.36 | 0.17 | 0.42 | 0.26 | **0.06** | 0.14 | 0.47 | 0.21 | 0.55 | 0.33 | **0.07** | 0.22 |
| 12mo | 0.46 | 0.23 | 0.59 | 0.41 | **0.17** | 0.30 | 0.58 | 0.35 | 0.76 | 0.52 | **0.20** | 0.42 |
| 24mo | 0.59 | 0.55 | 0.90 | 0.69 | **0.37** | 0.54 | 0.78 | 0.73 | 1.10 | 0.86 | **0.52** | 0.79 |
| 60mo | 0.71 | **0.62** | 0.73 | 0.68 | 0.66 | 0.65 | 0.84 | 0.84 | 0.96 | 0.90 | **0.80** | 0.88 |
| 84mo | 0.62 | **0.62** | 0.67 | 0.64 | 0.65 | 0.66 | **0.76** | 0.85 | 0.94 | 0.91 | 0.82 | 0.91 |
| 120mo | 0.70 | **0.63** | 0.70 | 0.77 | 0.70 | 0.74 | **0.86** | 0.90 | 0.96 | 1.01 | 0.90 | 0.99 |

(a) Shadow rate estimated from 1987-2019

| | | |
|---|---|---|
| Wu and Xia 2016 | 0.289 | 1.000 |
| YO | 0.997 | 0.021 |
| YO 2 factor | 1.000 | 0.038 |
| KO | 0.053 | 0.978 |
| KO 2 factor | 0.011 | 1.000 |
| PSS | 0.305 | 0.983 |
| PSS 2 factor | 0.004 | 1.000 |
| YO3 factor using EKF, LW data | 0.999 | 0.538 |

(b) Discretization-filter based shadow rates estimated from 1987-2023

| | | |
|---|---|---|
| YO | 1.000 | 0.183 |
| YO 2 factor | 1.000 | 0.006 |
| KO | 0.016 | 1.000 |
| KO 2 factor | 0.011 | 1.000 |
| PSS | 0.216 | 1.000 |
| PSS 2 factor | 0.098 | 0.996 |

Table 2: p-values for tests of a structural break in the FAVAR estimated in Wu and Xia (2016), substituting different estimates of the shadow rate. In each panel, the first column contains the p-values for the test of structural break in effect of lagged shadow rate on macroeconomic factors, and the second column contains p-values for test of structural break in effect of lagged macroeconomic factors on shadow rate. The test is a likelihood ratio test adjusting for small-sample bias as in Sims (1980), with a null is that there are no structural breaks. In panel (a), the shadow rates estimated using the discretization filter are estimated using yields data from 2019, while in panel (b) the data goes to 2023. In both cases, the FAVAR itself is estimated from 1960-2013 using identical data as Wu and Xia (2016) other than the shadow rate during the ZLB period.

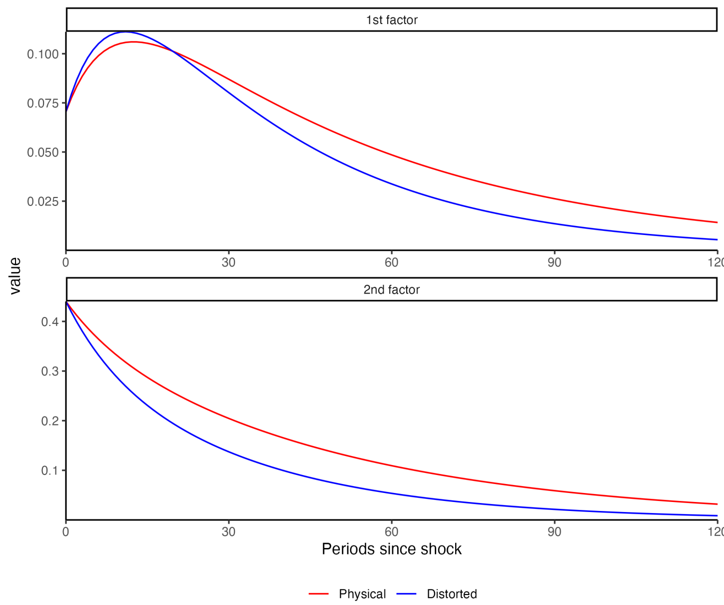
| | Sample: December 2002 - October 2008 | | | | | | |
|---|---|---|---|---|---|---|---|
| | 10yr | Term Premia on 10-year Bond | | | | | |
| | Yield | YO | YO | KO | KO | PSS | PSS |
| | | 2 fac | 3 fac | 2 fac | 3 fac | 2 fac | 3 fac |
| PHNT(m:2-10) | 3.48** | 2.98*** | 3.17*** | 3.06*** | 3.46*** | 3.37*** | 3.47*** |
| | (1.44) | (1.00) | (1.03) | (1.10) | (1.01) | (1.01) | (1.07) |
| Duration Gap | 194.41*** | 155.64*** | 161.48*** | 149.59*** | 161.79*** | 118.17*** | 125.61*** |
| | (27.22) | (19.07) | (19.33) | (22.66) | (21.00) | (16.86) | (17.19) |
| Slope (1mo lag) | -0.08* | 0.25*** | 0.24*** | 0.05 | 0.09** | 0.51*** | 0.55*** |
| | (0.05) | (0.04) | (0.04) | (0.04) | (0.04) | (0.04) | (0.04) |
| Adjusted $R^2$ | 0.56 | 0.62 | 0.58 | 0.33 | 0.35 | 0.85 | 0.87 |
| N | 71 | 71 | 71 | 71 | 71 | 71 | 71 |

Table 3: Supply regressions: First column: coefficients from regression of 10-year U.S. Treasury zero-coupon yield on supply factors and yield curve slope. Columns two through six: coefficients from regression of term premia on supply factors and yield curve slope. ***,**,* indicate significance at the 1, 5, and 10 percent levels, respectively calculated using the Newey-West correction for standard errors.

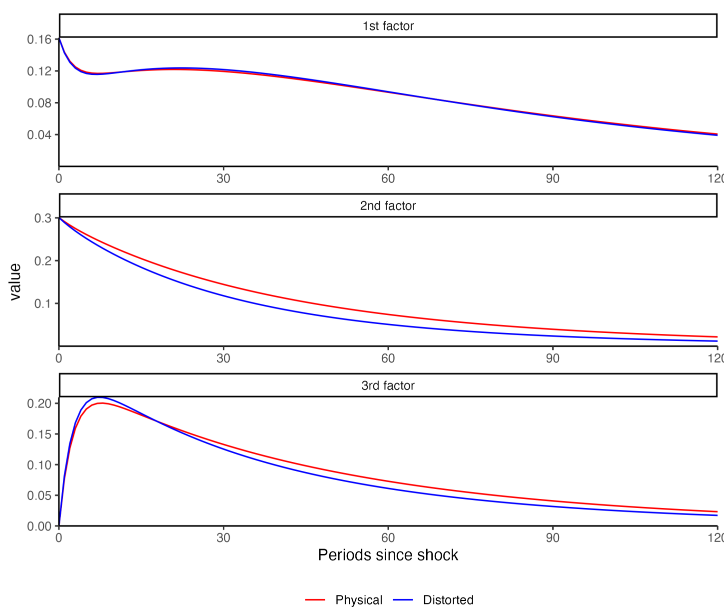
| | Sample: December 2002 - October 2008 | | | | | | |
|---|---|---|---|---|---|---|---|
| | 10yr | Term Premia on 10-year Bond | | | | | |
| | Yield | YO | YO | KO | KO | PSS | PSS |
| | | 2 fac | 3 fac | 2 fac | 3 fac | 2 fac | 3 fac |
| LSAP 1 | | | | | | | |
| Predicted effect from scarcity | -16.29** | -13.98*** | -14.86*** | -14.34*** | -16.24*** | -15.78*** | -16.28*** |
| Predicted effect from duration | -23.33*** | -18.68*** | -19.38*** | -17.95*** | -19.41*** | -14.18*** | -15.07*** |
| Total | -39.62*** | -32.66*** | -34.23*** | -32.29*** | -35.65*** | -29.96*** | -31.36*** |
| Residual (expectations effects) | | -6.96 | -5.39 | -7.33 | -3.97 | -9.66 | -8.26 |
| LSAP 2 | | | | | | | |
| Predicted effect from scarcity | -24.24** | -20.81*** | -22.11*** | -21.34*** | -24.17*** | -23.49*** | -24.24*** |
| Predicted effect from duration | -19.44*** | -15.56*** | -16.15*** | -14.96*** | -16.18*** | -11.82*** | -12.56*** |
| Total | -43.69*** | -36.37*** | -38.26*** | -36.30*** | -40.35*** | -35.31*** | -36.80*** |
| Residual (expectations effects) | | -7.31 | -5.43 | -7.39 | -3.34 | -8.38 | -6.89 |

Table 4: Predicted effects from supply regressions: First column: predicted change (in basis points) of 10-year U.S. Treasury zero-coupon yield due to duration and scarcity effects. Columns two through six: predicted change (in basis points) of term premia due to duration and scarcity effects. ***,**,* indicate signfiicance at the 1, 5, and 10 percent levels, respectively calculated using the Newey-West correction for standard errors.

# 9 Figures




(a) 2 factor IRF




(b) 3 factor IRF

Figure 1: Estimated impulse response of the shadow rate $s_t$ to a 1-standard deviation shock to each latent factor under physical and subjective (distorted) dynamics for the estimated PSS models. The red line shows the evolution of the shadow rate using the physical estimated physical dynamics, while the blue line shows the evolution using the subjective dynamics. Panel (a) shows the results for the two-factor PSS model; Panel (b) shows the three-factor PSS model.

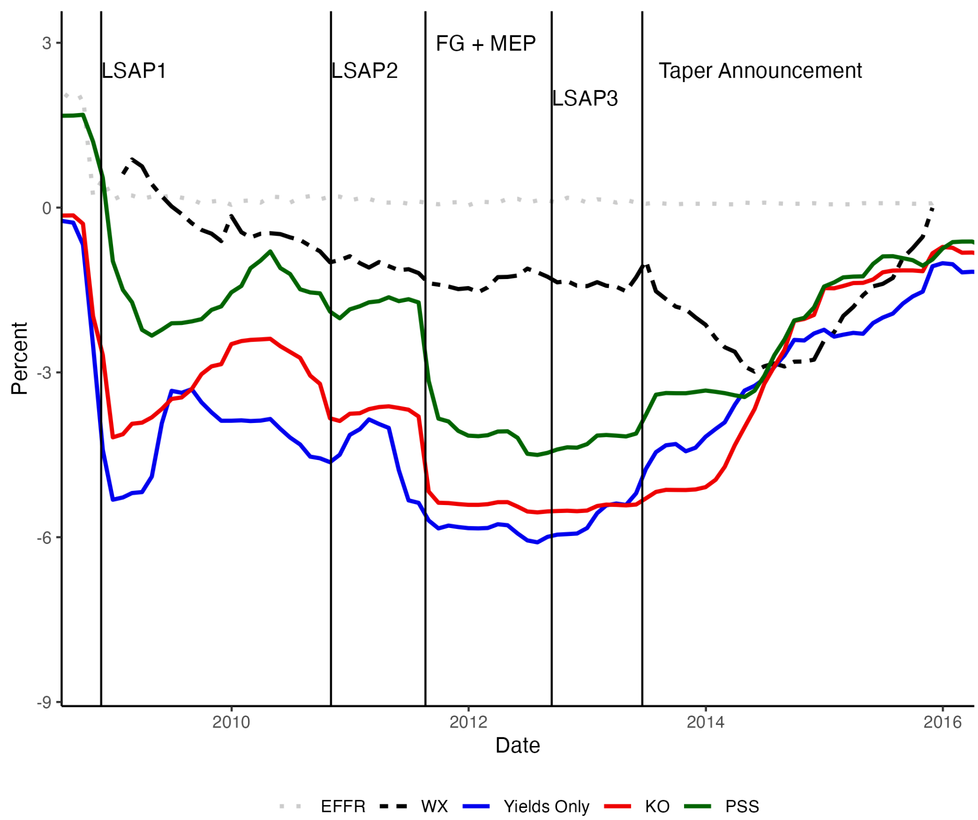



Figure 2: Smoothed estimates of shadow rate during/post Great Recession, with event dates (three rounds of Large Scale Asset Purchases (LSAPs), the introduction of calendar-based forward guidance and the Maturity Extension Program (FG+MEP), Taper Announcement). FG and MEP were introduced in August and September 2011, respectively, but are shown in August 2011.

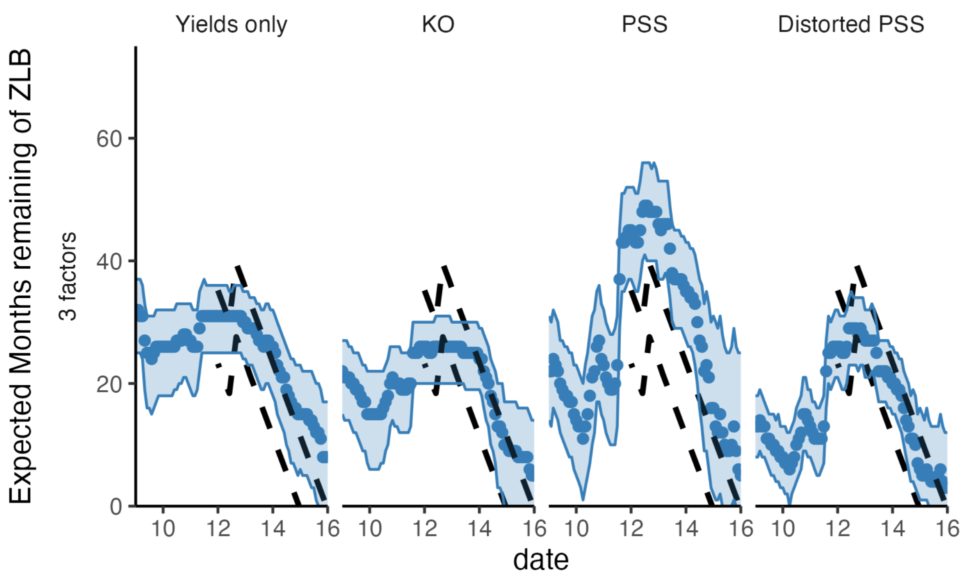



Figure 3: Real-time implied mean duration of ZLB period. Bands indicate 99th percentile of liftoff dates.

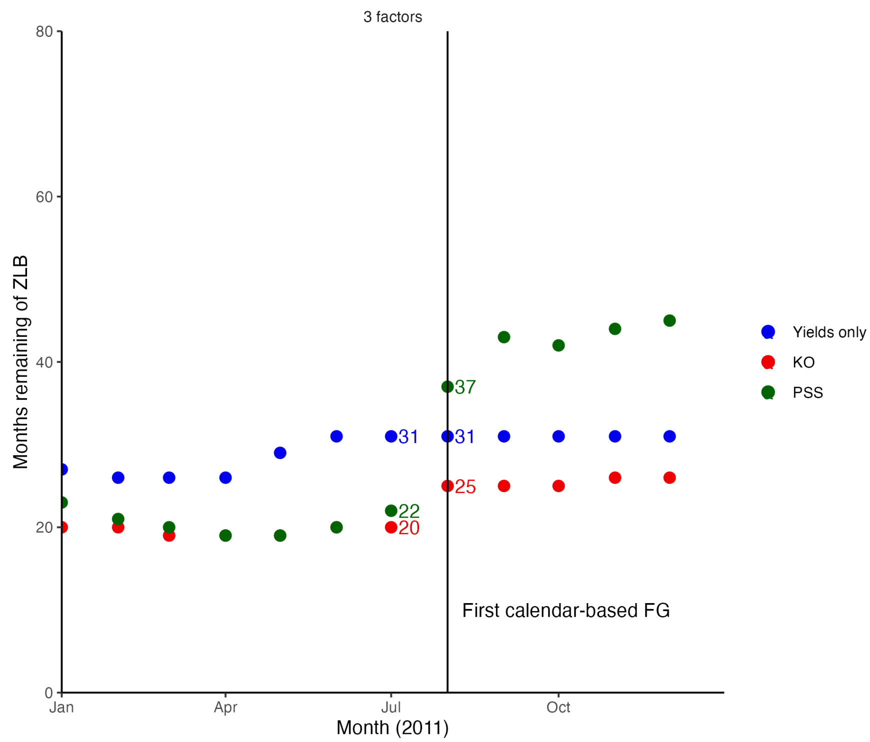



Figure 4: Real-time implied mean duration of 2011 using the same estimates as those in Figure 3.

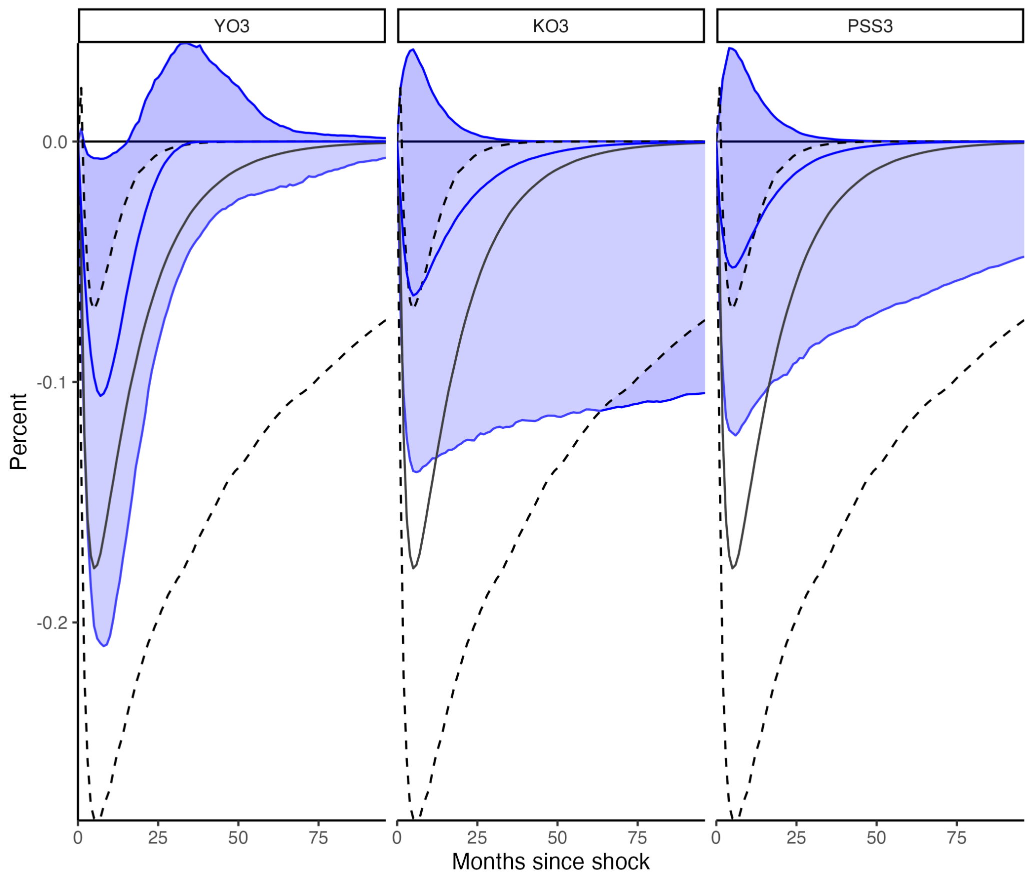



Figure 5: Estimated impulse response of unemployment to a 25 basis point decrease in the policy rate (Federal funds rate/shadow rate). Solid lines indicate median response and bands indicate 90% confidence intervals. Black is the impulse response reported in Wu and Xia (2016) and blue indicates estimates with shadow rates estimated with the discretization filter. The model is estimated as a FAVAR(1) using the "post-crisis" specification and data in Wu and Xia.

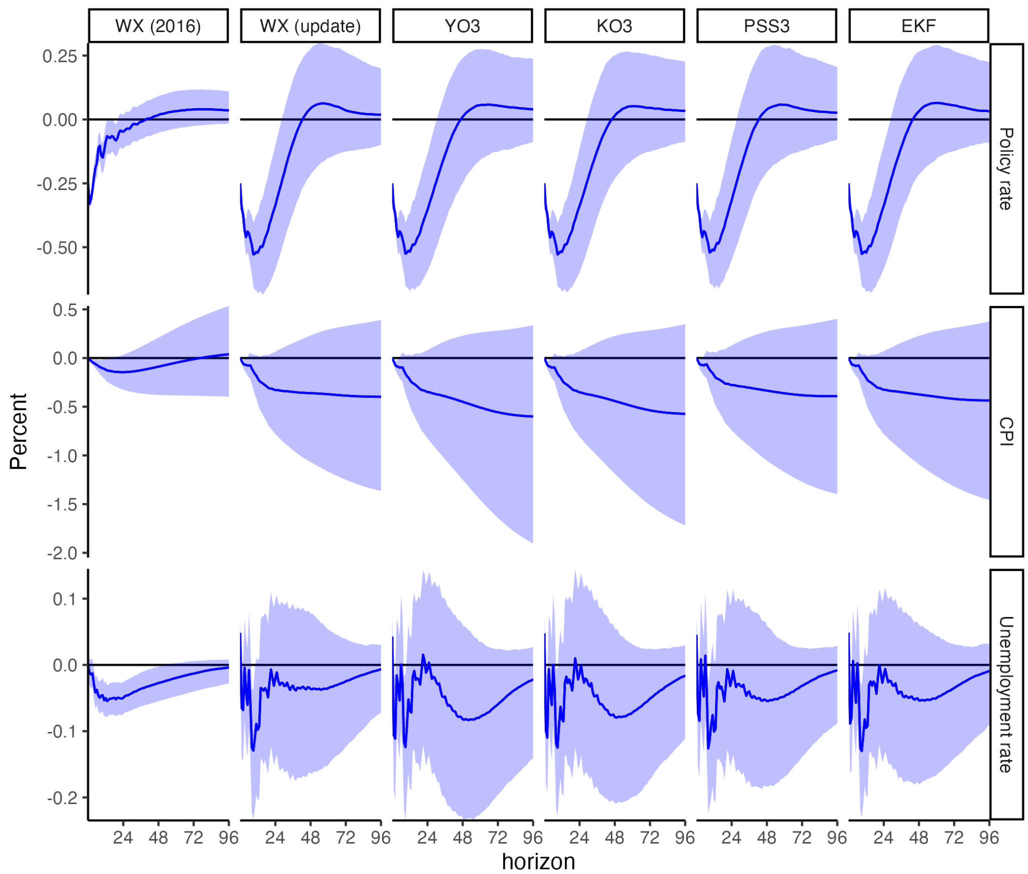



Figure 6: Estimated impulse response of unemployment and inflation to a 25 basis point decrease in the policy rate (Federal funds rate/shadow rate). Solid lines indicate median response and bands indicate 90% confidence intervals. Impulse responses are estimated using a FAVAR(13) estimated using data from 1987-2019. The difference across columns is the underlying estimate of the shadow rate. "WX (2016)" indicates the original impulse response reported in Wu and Xia (2016); "WX (updated)" uses the most recent vintage of data as of December 2024. "EKF" is the YO3 factor model estimated using Liu and Wu (2021) yields and the extended Kalman filter.

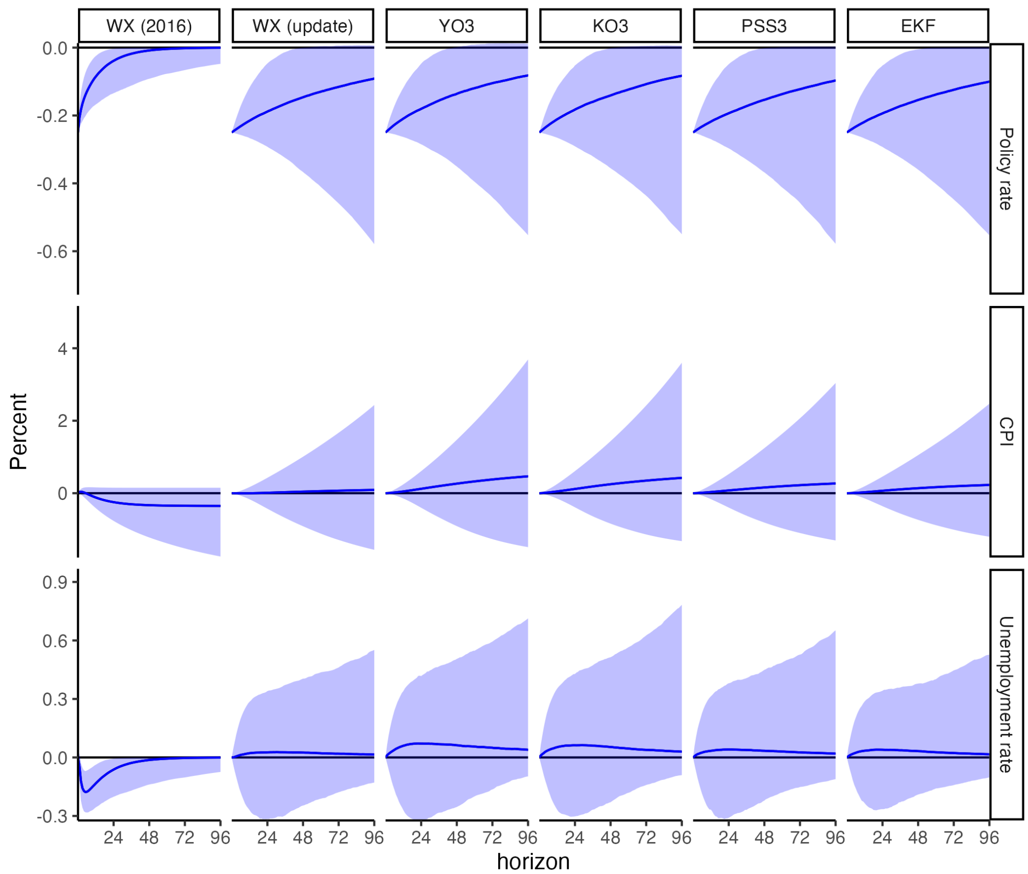



Figure 7: Estimated impulse response of unemployment and inflation to a 25 basis point decrease in the policy rate (Federal funds rate/shadow rate). Solid lines indicate median response and bands indicate 90% confidence intervals. Impulse responses are estimated using a FAVAR(1) estimated using data from July 2009-December 2019. The difference across columns is the underlying estimate of the shadow rate. "WX (2016)" indicates the original impulse response reported in Wu and Xia (2016); "WX (updated)" uses the most recent vintage of data as of December 2024. "EKF" is the YO3 factor model estimated using Liu and Wu (2021) yields and the extended Kalman filter.

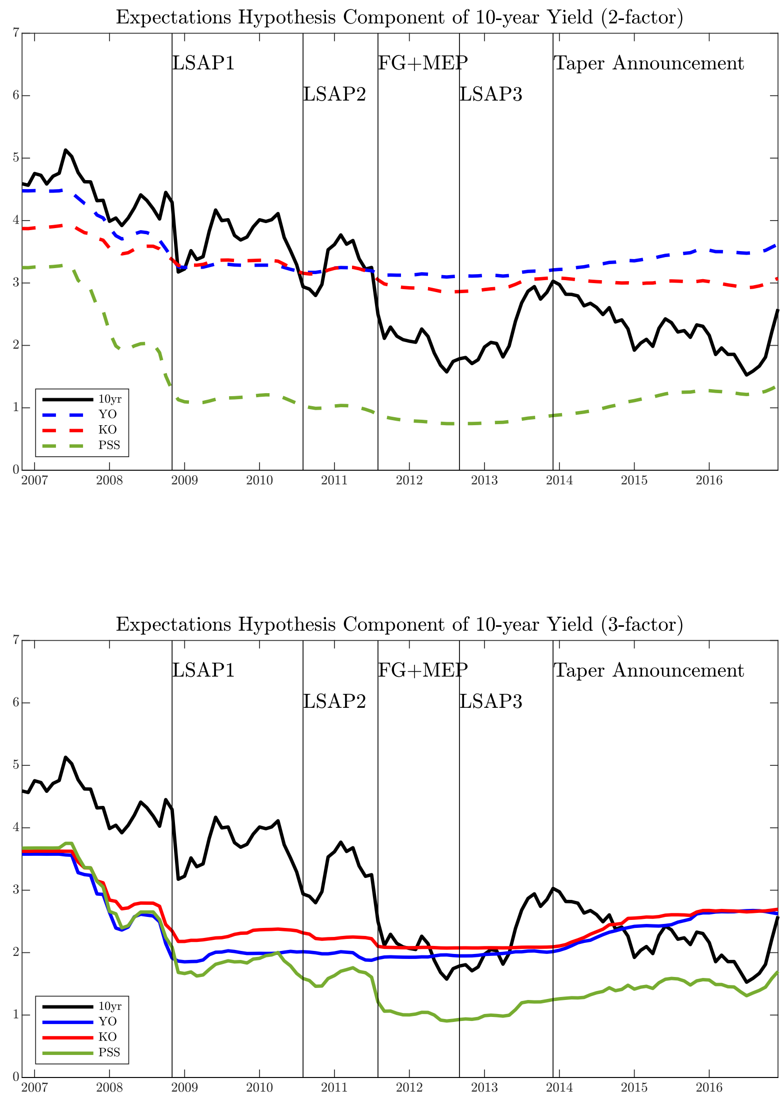
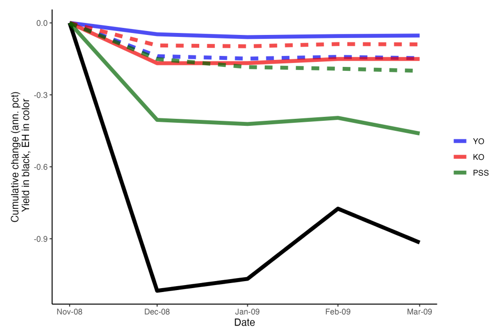
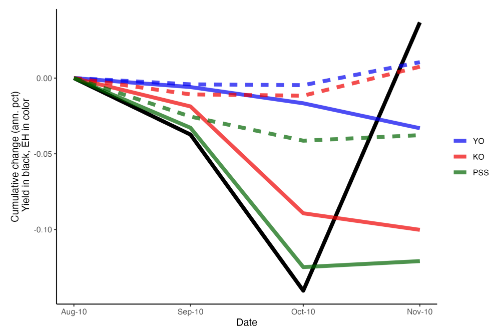


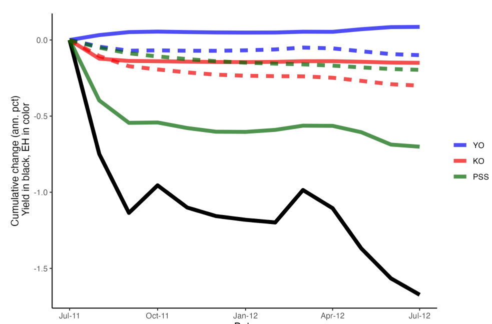

Figure 8: Decomposition of the 10-year U.S. Treasury yield (black line) during the ZLB. The Expectations Hypothesis component of the 10-year yield is shown for the YO (blue line), KO (red line), and PSS (green line) models. Results for the two-(three-)factor model are in the upper (lower) panel.




(a) LSAP1




(b) LSAP2




(c) FG

Figure 9: Decomposition of the change in the 10-year U.S. Treasury yield (black line) during specific ZLB dates. All measures are shown related to the month preceding the following events: LSAP1 (November 2008), LSAP2 (August 2010), the introduction of calendar-based forward guidance (FG) (July 2011). The change in the Expectations Hypothesis component of the 10-year yield is shown for the three-factor YO (blue line), KO (red line), and PSS (green line) models. Results for the two-(three-)factor model are given by dashed (solid) lines.

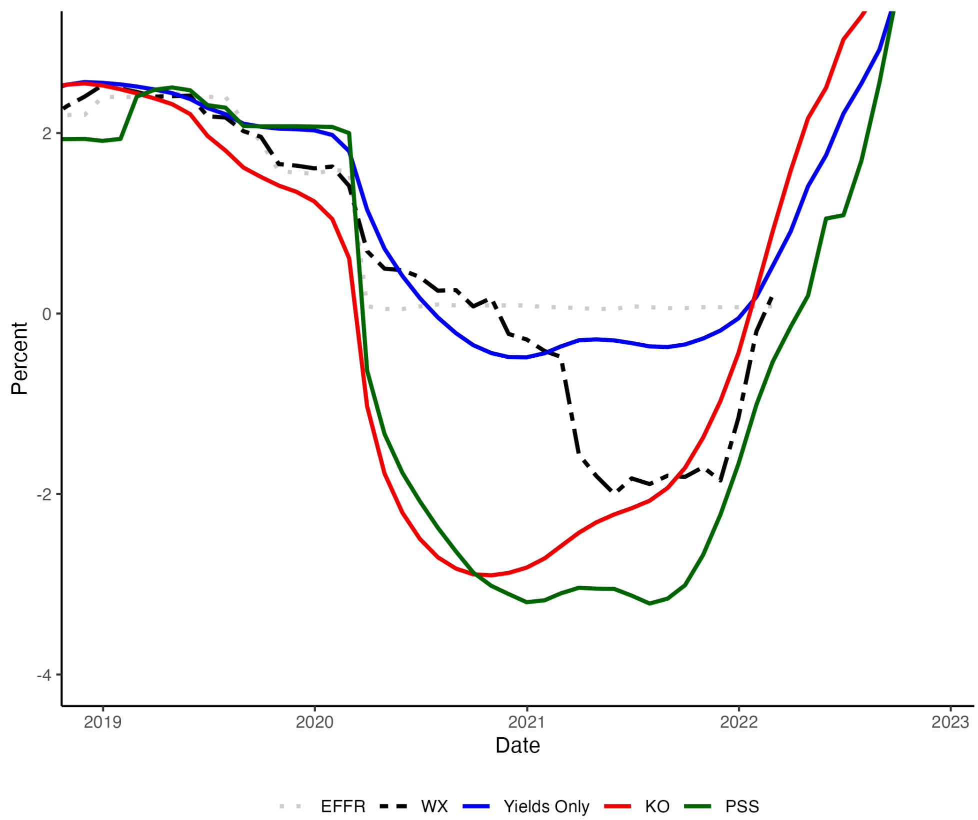



Figure 10: Smoothed estimates of effective federal funds rate and shadow rate estimates during and after the COVID-19 recession.

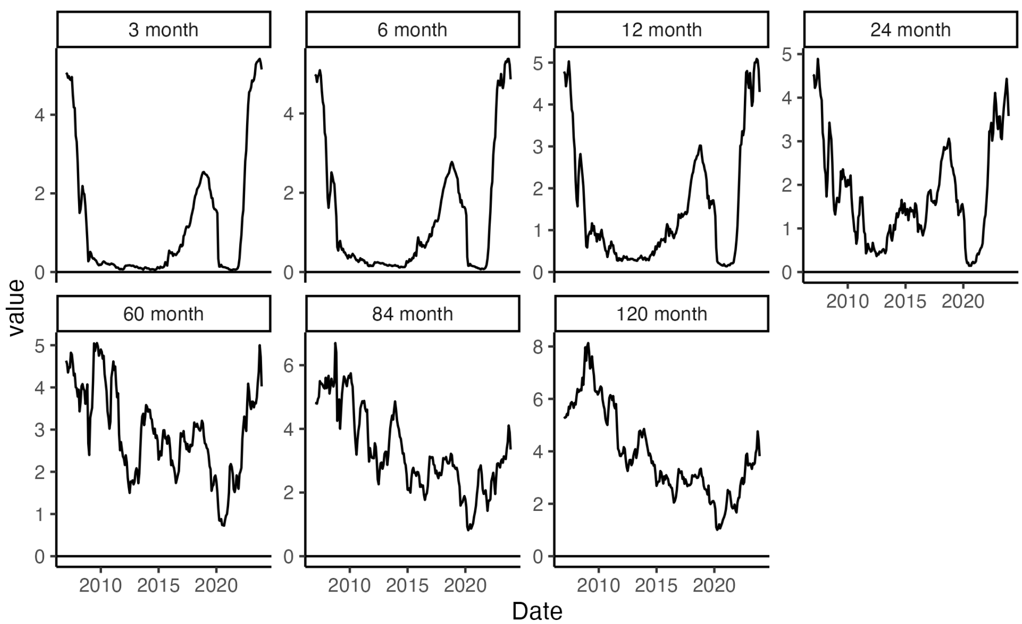



Figure 11: Liu and Wu (2021) zero coupon yield curve estimates, 2006-2023.

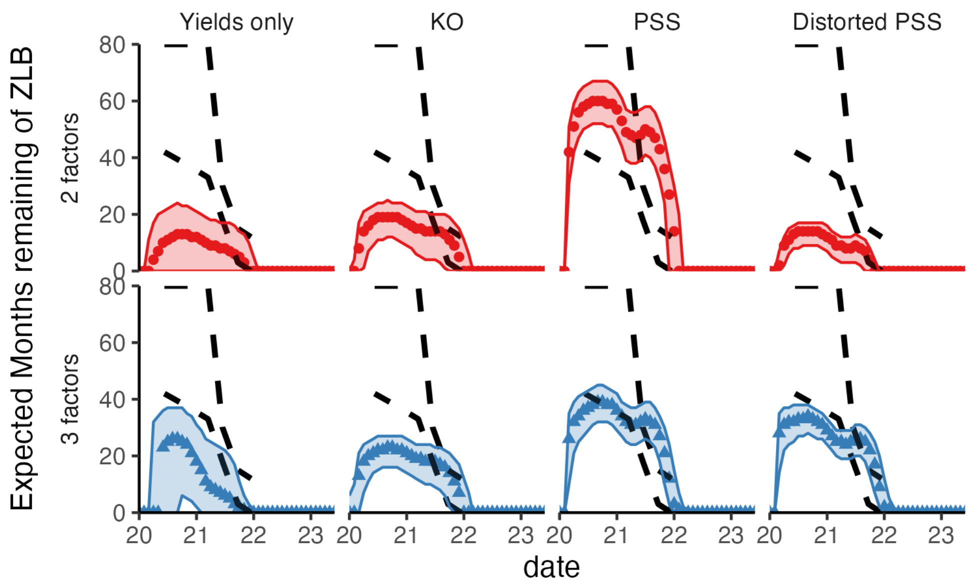



Figure 12: Real-time implied mean duration of ZLB period during and following the COVID-19 pandemic. Bands indicate 99th percentile of liftoff dates. Dashed lines indicate forward guidance implied by median respondent to FOMC Statement of Economic Projections.

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# A Explicit expressions from the Wu-Xia shadow rate model

We include the complete expression for the recursions in Wu and Xia (2016). Interested readers should refer to their paper for a complete derivation.

$$\bar{a_n} = \delta_0 + \delta_1 \sum_{k=0}^{n-1} (\rho^{\mathbb{Q}})^k \mu^{\mathbb{Q}} \tag{20}$$

$$a_n = \bar{a_n} - \frac{1}{2} \sum_{j=0}^{n-1} \delta_1 \left[ (\rho^{\mathbb{Q}})^j \Sigma \Sigma' ((\rho^{\mathbb{Q}})')^j \right] \delta_1' \tag{21}$$

$$b_n = \delta_1 (\rho^{\mathbb{Q}})^n \tag{22}$$

And

$$E_t(s_{t+n}) = \bar{a_n} + b_n X_t$$

# B Examining beliefs during the ZLB period and the usefulness of forecast data

Hamilton (2018) argues that event-study estimates of the impact of monetary policy actions make it difficult to separately identify the pure effects of LSAPs from informational effects. For example, figure B.1 shows the yield curve on US Treasuries at the end of day on March 17, 2009 and March 19, 2009. On March 18, 2009, the FOMC announced it would be maintaining a target for the Federal Funds rate at 0-25 basis points for an "extended period" and expanded the scale of LSAPs.[14] The shift in the long end of the yield curve conflates this news about short term interest rates and economic conditions which affect risk premia.

In principle, shadow rate models allow for the separation of these forces by identifying the pure EH component of yields separately from risk premia, even when short term interest rates are stuck at or near the zero lower bound. Forecasts are potentially an additional source of information about expectations. Because the decision to use forecast data is not innocuous (see Li et al. (2017)), it is worth briefly rationalizing our approach.

First, the Blue Chip panelists are primarily private sector forecasters, and policymakers frequently make use of the Blue Chip surveys as an indicator of market expectations that are free of effects from priced risk premia, both in public speeches (see, for example, Clarida (2019)) and internally as a benchmark (D'Amico et al. (2013), Cieslak (2018)). This is consistent with

[14]The March 18 2009 FOMC statement included the following language: "The Committee will maintain the target range for the federal funds rate at 0 to 1/4 percent and anticipates that economic conditions are likely to warrant exceptionally low levels of the federal funds rate for an extended period. To provide greater support to mortgage lending and housing markets, the Committee decided today to increase the size of the Federal Reserve's balance sheet further by purchasing up to an additional $750 billion of agency mortgage-backed securities, bringing its total purchases of these securities to up to $1.25 trillion this year, and to increase its purchases of agency debt this year by up to $100 billion to a total of up to $200 billion. Moreover, to help improve conditions in private credit markets, the Committee decided to purchase up to $300 billion of longer-term Treasury securities over the next six months."

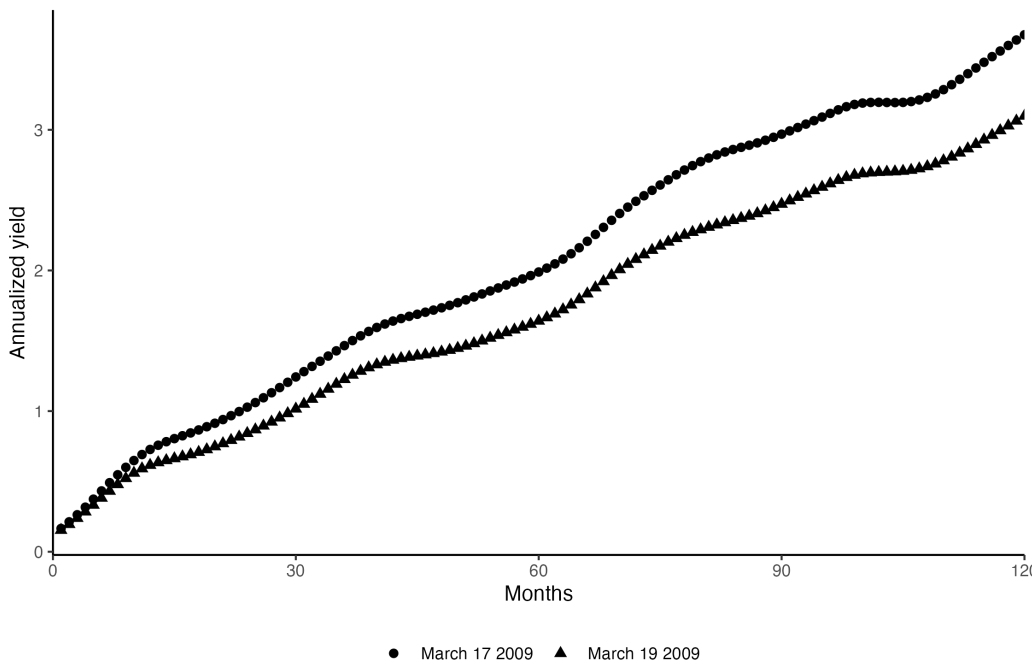


Figure B.1: US Treasury yield curve on March 17 (dashed) and March 19 (circles), 2009. Data from Liu and Wu (2021).

their use in other shadow rate studies; for example, Bauer and Rudebusch (2016) verify their model-based forecasts are sensible by comparing them to surveys.

Second, graphical evidence suggests that forecasts for short-term bond yields – which one might expect have relatively small, if any, risk premia – are reasonably close to what would be implied by prices. For instance, figure B.2 compares the yield on a 12-month zero coupon Treasury bond to the average expected short-term interest rate over the next 12 months. In general, the forecasts are consistent with prevailing prices.

Third, and most importantly, our approach in this paper is neither to

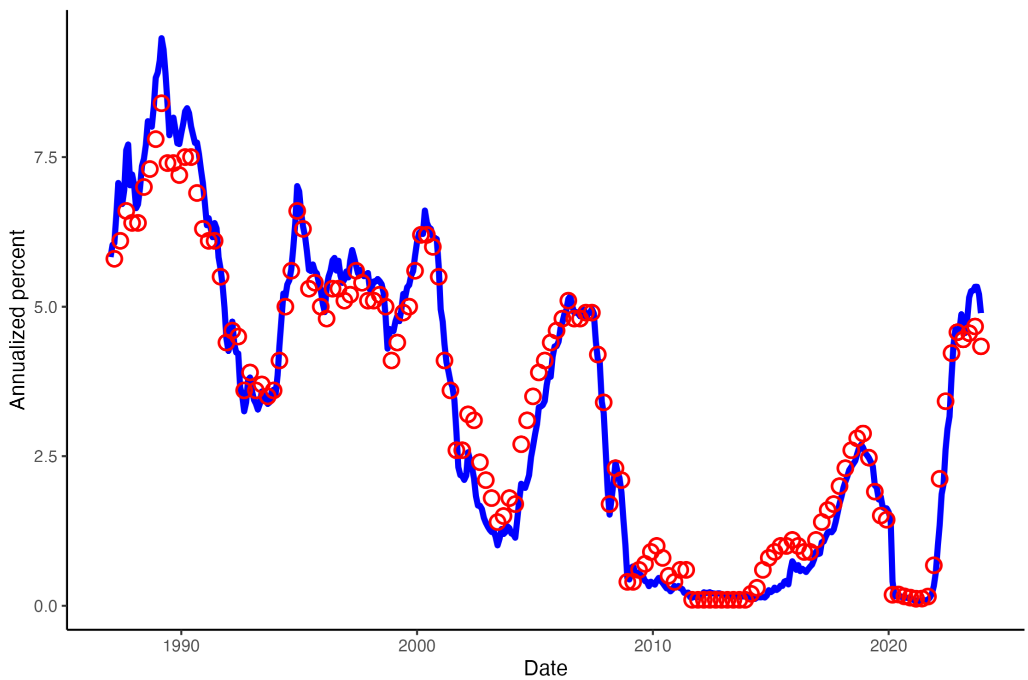


Figure B.2: Yields on 12 month Treasuries (solid line) and average expected short rates of 12 month Treasuries from the Blue Chip Financial Survey (circles). 12 month Treasury yields from Liu and Wu (2021).

ignore forecasts or assume they are the same as market expectations. We estimate several models that allow for surveys to be identified with traders directly (a-la Kim and Orphanides (2012)), as well as allowing for interest rate forecasts from surveys to be related to those implied by yields but possibly distorted (as in Piazzesi et al. (2015)). Allowing for distortion may be important given a large literature (e.g. Coibion and Gorodnichenko (2015)) which has demonstrated professional forecasts often significantly deviate from the FIRE benchmark.

To test for the existence of distorted beliefs in Blue Chip short-rate forecasts, we regress future forecast errors on revisions of the same forecast (as in Coibion and Gorodnichenko (2015)) in each month. Define $\mathrm{E}_t[\bar{r}_{t+n-2,t+n}]$ to be the consensus forecast made in month $t$ of the average level of short rates between months $t+n-2$ and $t+n$.[15] Because Additionally, call $FE_t(\bar{r}_{t+n}) = \bar{r}_{t+n} - \mathrm{E}_t[\bar{r}_{t+n-2,t+n}]$ the forecast *error* from month $t$ to month $t+n$ and $FR_t(\bar{r}_{t+n}) = \mathrm{E}_t[\bar{r}_{t+n-2,t+n}] - \mathrm{E}_{t-1}[\bar{r}_{t+n-2,t+n}]$ the forecast revision between months $t-1$ and $t$.

We regress forecast errors across horizons $n$ on forecast revisions:

$$FE_t(\bar{r}_{t+n}) = \alpha(n) + \beta(n) FR_t(\bar{r}_{t+n}) + \epsilon_{t+n} \tag{23}$$

Under the null hypothesis of FIRE, rational expectations errors would be unpredictable ($\beta(n) = 0$) as would be efficiently incorporating all information available at $t$. However, as the results in figure B.3 suggest, such errors can be predicted using revisions to forecasts from time $t-1$ to $t$. This effect is nearly always significant at the 95% level. The results imply that knowing forecasts for the next quarter had been revised upward by 25 basis points between the first and second month of the current quarter implies a likely *underestimate* of the actual average 3-month rate by around 25 basis points (despite the upward revision). This economically and statistically significant result is inconsistent with FIRE.

While we believe surveys are a source of information about the beliefs of traders, we are cognizant that there is a possible tension in (1) treating them

[15] Further details of the construction of Blue Chip forecasts are provided in appendix C.

as FIRE and (2) identifying them with traders' beliefs. Since the literature has not reached a consensus, we examine whether our results are robust to assuming forecasts are FIRE or perhaps generated by a distorted belief about the underlying state.

# C Incorporating the Blue Chip Financial Forecasts Survey into the structural estimates

The Blue Chip Financial Forecasts survey has been conducted at a monthly frequency since 1982. Survey participants are asked for their quarterly average forecasts of a range of financial-market variables at horizons of 1- to 5-quarters ahead (6-quarters ahead beginning in 1997).[16] The analysis in this paper utilizes forecasts of 3-month Treasury bill constant-maturity yields, which proxies for the risk-free short-term interest rate.

The Blue Chip survey is generally published on the first day of each month. However, forecasters complete the survey over a two-day period in the prior week. We follow Cieslak (2018) and choose the "survey date" to be the earliest business day in the range of the 23rd-27th of the month for January through November and the 17th-20th for December. Yields used in estimation are selected on those dates to correspond with the forecasters' true information set.

Current-quarter forecasts published in the second and third months of

[16]The Blue Chip also publishes long-horizon forecasts semi-annually, which we do not utilize due to the sparse time series.

a quarter already contain past realizations of yields. To address this issue, we adjust forecasts for prior yields within a given quarter.[17] Consider the case of Q1 forecasts published in February. These forecasts reflect interest rates that already occurred in January. We calculate a forward-looking forecast by subtracting the average of 3-month interest rates (taken from the Fed's H.15 release) over the first three weeks of January. The *two*-month ahead forecast then equals $\mathrm{E}_t[\bar{r}_{t+1,t+2}] = (3 \times \mathrm{E}_t[\bar{r}_{t,t+2}] - \bar{r}_t)/2$. Now consider the case of Q1 forecasts published in March, which are made in February. These forecasts reflect interest rates that already occurred in January and the first three weeks of February. The *one*-month ahead forecast subtracts the monthly average of yields in January and the average of the first three weeks of February: $\mathrm{E}_t[\bar{r}_{t+1}] = 3 \times \mathrm{E}_t[\bar{r}_{t-1,t+1}] - \bar{r}_t - \bar{r}_{t-1}$. In both cases, the average of the first three weeks of the month is assumed to be approximately equals to the monthly average.

[17]This procedure is identical to Xu (2019), except that we use a slightly different forecast horizon convention.

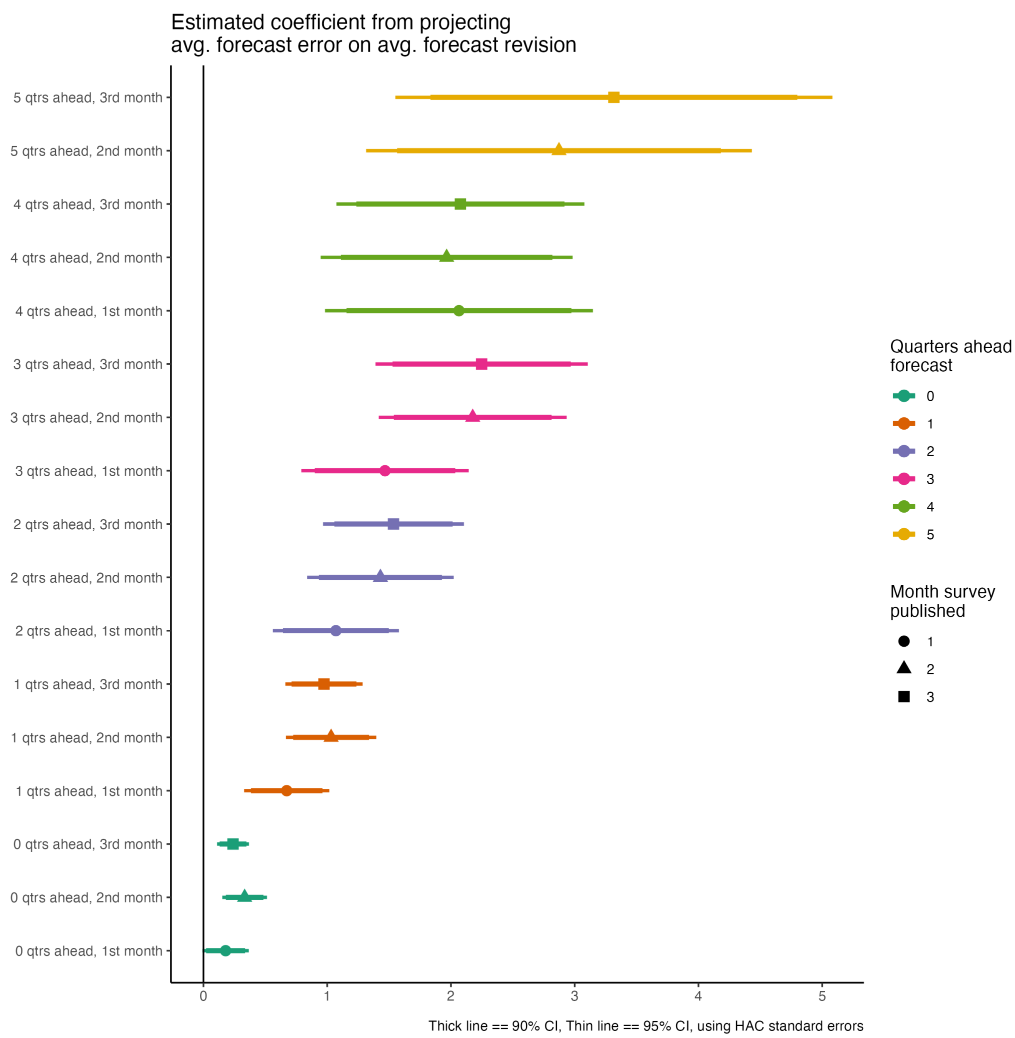


Figure B.3: Each line represents confidence intervals for coefficient estimates of forecast error on forecast revision as in equation (23), by forecast horizon and month within the quarter.

# D Parameter estimates and model fit

This appendix first presents tables of the parameter estimates for each model. Following that, figure D.1 plots the average fit for the yield curve during the zero lower bound period. The top figure shows results for two-factor models, while the bottom shows results for three-figure models. The average yield curve implied by the model is shown by the individual markers, while the actual yield curve is plotted as a solid line.

| | | | |
|---|---|---|---|
| $1200\mu$ | -0.1518 | -0.2174 | 0.5000 |
| | (0.1062) | (0.0509) | (0.1300) |
| $\rho$ | 0.9760 | 0.0086 | 0.0122 |
| | (0.0120) | (0.0004) | (0.0097) |
| | -0.0183 | 0.9634 | 0.0660 |
| | (0.0173) | (0.0051) | (0.0700) |
| | 0.0207 | -0.0069 | 0.7135 |
| | (0.0037) | (0.0065) | (0.0014) |
| $\text{diag}(\rho^{\mathbb{Q}})$ | 0.9972 | 0.9816 | 0.0138 |
| | (0.0001) | (0.0003) | (0.0182) |
| $1200\Sigma$ | 0.4234 | | |
| | (0.0904) | | |
| | -0.3556 | 0.6696 | |
| | (0.0715) | (0.0438) | |
| | -0.0044 | -0.2180 | 0.1327 |
| | (0.0024) | (0.0525) | (0.0148) |
| 1200 $\underline{\text{r}}$ | 0.2145 | | |
| | (0.0081) | | |
| 1200 $\delta_0$ | 10.0130 | | |
| | (0.6989) | | |
| 1200 (yield meas. err) | 0.2768 | | |
| | (0.0003) | | |
| Log Likelihood | | 18762.5379 | |

Table D.1: Estimated parameters for 3 factor model without forecasts (YO model). QMLE standard errors in parentheses

| | | |
|---|---|---|
| $1200\mu$ | -0.2303 | -0.0010 |
| | (0.2567) | (0.0031) |
| $\rho$ | 0.9812 | 0.0645 |
| | (0.3118) | (1.0425) |
| | -0.0210 | 0.8950 |
| | (0.3925) | (1.3064) |
| $\text{diag}(\rho^{\mathbb{Q}})$ | 0.9932 | 0.9793 |
| | (0.0052) | (0.0340) |
| $1200\Sigma$ | 0.7864 | 0.0000 |
| | (1.9687) | (0.0000) |
| | -0.6440 | 0.4503 |
| | (2.4428) | (2.9878) |
| 1200 $\underline{\mathrm{r}}$ | 0.1090 | |
| | (0.8160) | |
| 1200 $\delta_0$ | 10.1522 | |
| | (7.8751) | |
| 1200 (yield meas. err) | 0.5330 | |
| | (1.2144) | |
| Log Likelihood | | 17912.6297 |

Table D.2: Estimated parameters for 2 factor model without forecasts (YO model). QMLE standard errors in parentheses

| | | | |
|---|---|---|---|
| $1200\mu$ | -0.1161 | -0.2454 | 0.5000 |
| | (0.0166) | (0.0213) | (0.1240) |
| $\rho$ | 0.9773 | 0.0039 | 0.0106 |
| | (0.0049) | (0.0012) | (0.0094) |
| | -0.0202 | 0.9606 | 0.0639 |
| | (0.0061) | (0.0210) | (0.0163) |
| | 0.0269 | 0.0040 | 0.7148 |
| | (0.0089) | (0.0095) | (0.0234) |
| $\text{diag}(\rho^{\mathbb{Q}})$ | 0.9978 | 0.9818 | 0.0009 |
| | (0.0011) | (0.0012) | (0.0007) |
| $1200\Sigma$ | 0.3708 | | |
| | (0.1083) | | |
| | -0.3133 | 0.6543 | |
| | (0.1204) | (0.1547) | |
| | -0.0090 | -0.1669 | 0.1814 |
| | (0.0058) | (0.0418) | (0.0135) |
| $1200\ \underline{\mathrm{r}}$ | 0.0897 | | |
| | (0.0052) | | |
| $1200\ \delta_0$ | 10.1449 | | |
| | (2.1732) | | |
| 1200 (yield meas. err) | 0.3845 | | |
| | (0.0225) | | |
| 1200 (fcst meas. err) | 0.1995 | | |
| | (0.0211) | | |
| Log Likelihood | | 34226.6986 | |

Table D.3: Estimated parameters for 3 factor model including forecasts (KO model). QMLE standard errors in parentheses

| | | |
|---|---|---|
| $1200\mu$ | -0.2735 | 0.0550 |
| | (0.2074) | (0.2625) |
| $\rho$ | 0.9534 | 0.0034 |
| | (0.0286) | (0.0032) |
| | 0.0188 | 0.9293 |
| | (0.0422) | (0.0622) |
| $\text{diag}(\rho^{\mathbb{Q}})$ | 0.9966 | 0.9748 |
| | (0.0025) | (0.0104) |
| $1200\Sigma$ | 0.4845 | |
| | (0.1682) | |
| | -0.4481 | 0.4177 |
| | (0.0437) | (0.4897) |
| 1200 $\underline{\text{r}}$ | 0.0285 | |
| | (0.0094) | |
| 1200 $\delta_0$ | 10.6584 | |
| | (1.3225) | |
| 1200 (yield meas. err) | 0.8194 | |
| | (1.9095) | |
| 1200 (fcst meas. err) | 0.6011 | |
| | (1.2587) | |
| Log Likelihood | | 31814.9829 |

Table D.4: Estimated parameters for 2 factor model including forecasts (KO model). QMLE standard errors in parentheses

| | | | |
|---|---|---|---|
| $1200\mu$ | -0.1999 | -0.1731 | 0.5000 |
| | (0.2200) | (1.6069) | (2.1606) |
| $\rho$ | 0.9707 | 0.0022 | 0.0367 |
| | (0.0535) | (0.0238) | (0.1410) |
| | -0.0180 | 0.9652 | 0.1377 |
| | (0.2492) | (0.0723) | (0.7448) |
| | 0.0614 | 0.0153 | 0.6845 |
| | (0.2900) | (0.2562) | (0.9483) |
| $\mathrm{diag}(\rho^{\mathbb{Q}})$ | 0.9966 | 0.9767 | 0.0005 |
| | (0.0007) | (0.1288) | (0.0715) |
| $1200\Sigma$ | 0.3804 | | |
| | (1.2905) | | |
| | -0.2179 | 0.3006 | |
| | (1.6237) | (0.7542) | |
| | -0.0407 | 0.0018 | 0.4445 |
| | (0.5030) | (0.1646) | (0.3235) |
| 1200 $\underline{\mathrm{r}}$ | 0.1181 | | |
| | (0.0365) | | |
| 1200 $\delta_0$ | 10.0228 | | |
| | (2.4612) | | |
| $k$ | -4.8124 | 4.9304 | -25.0594 |
| | (0.7966) | (0.5480) | (0.0265) |
| | 21.4678 | 24.4602 | -32.2431 |
| | (0.1469) | (0.1714) | (0.1120) |
| | 5.4719 | 4.2973 | -32.2431 |
| | (1.2736) | (0.0876) | (0.1120) |
| 1200 (yield meas. err) | 0.3350 | | |
| | (0.0367) | | |
| 1200 (fcast meas. err) | 0.1236 | | |
| | (0.0257) | | |
| Log Likelihood | | 35460.0538 | |

Table D.5: Estimated parameters for 3 factor model with distorted forecaster dynamics (PSS model). QMLE standard errors in parentheses

| | |
|---|---|
| $\mathrm{eig}(\rho - \Sigma k)$ | 0.6796 |
| | 0.9823 |
| | 0.9660 |

Table D.6: Subjective physical dynamics, 3 factor PSS model

| | | |
|---|---|---|
| $1200\mu$ | -0.0319 | -0.1410 |
| | (0.1248) | (0.0283) |
| $\rho$ | 0.9571 | 0.0521 |
| | (0.3512) | (0.4676) |
| | 0.0282 | 0.9145 |
| | (0.4339) | (0.4498) |
| $\mathrm{diag}(\rho^{\mathbb{Q}})$ | 0.9961 | 0.9681 |
| | (0.0382) | (0.0997) |
| $1200\Sigma$ | 0.5040 | |
| | (1.3983) | |
| | -0.4335 | 0.4403 |
| | (1.5932) | (0.1694) |
| 1200 $\underline{\mathrm{r}}$ | 0.2051 | |
| | (0.4181) | |
| 1200 $\delta_0$ | 10.3037 | |
| | (20.1519) | |
| $k$ | 3.3769 | 17.6891 |
| | (189.7350) | (174.5853) |
| | 25.3979 | 38.9036 |
| | (105.1044) | (204.9911) |
| 1200 (yield meas. err) | 0.9272 | |
| | (1.0115) | |
| 1200 (fcast meas. err) | 0.8635 | |
| | (0.8668) | |
| Log Likelihood | | 30938.4136 |

Table D.7: Estimated parameters for 2 factor model with distorted forecaster dynamics (PSS model). QMLE standard errors in parentheses

| | |
|---|---|
| $\mathrm{eig}(\rho - \Sigma k)$ | 0.9699 |
| | 0.8924 |

Table D.8: Subjective physical dynamics, 2 factor PSS model

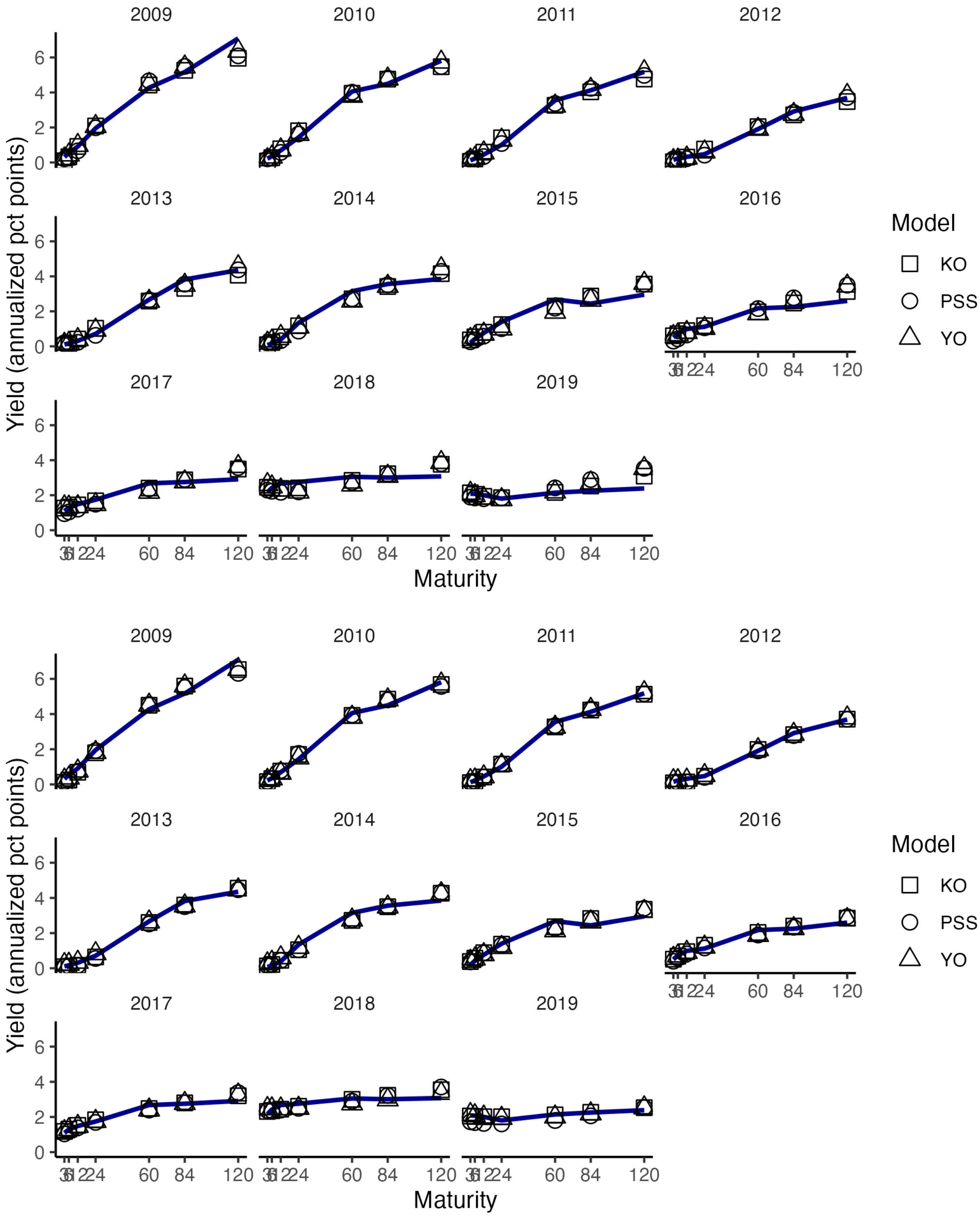




Figure D.1: Average fit of 2-figure (top) and 3-figure (bottom) model across years, using smoothed state estimates. Line indicates average yield curve in the indicated year.

# E Variance decomposition of yields

In this appendix, we report the variance decomposition of yields across models and horizons. All of the models imply that the variation in short-maturity yields is due to expectations of future short-term rates. However, the YO and two-factor KO models attribute relatively more of the variation in medium- and long-term yields to the term premium component than the other models. The PSS models and three-factor KO model cannot distinguish between these two components for long-maturity bonds. The difference in results across models emphasizes that these decompositions are sensitive to the underlying structural model and the presence of survey forecasts in the estimation.

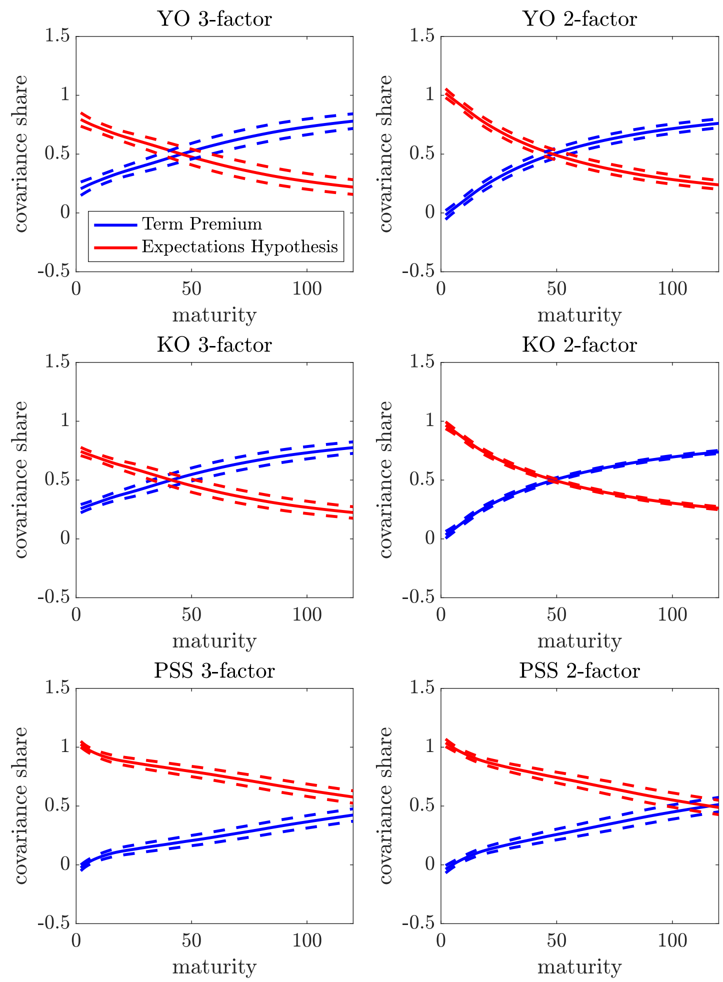


Figure E.1: Decomposition of unconditional variance of the level of yields into Term Premium and Expectations Hypothesis components for yields only, KO, and PSS models for the full sample (1987-2019). Newey-West 99% confidence bands are shown for point estimates. Maturity is reported in months.

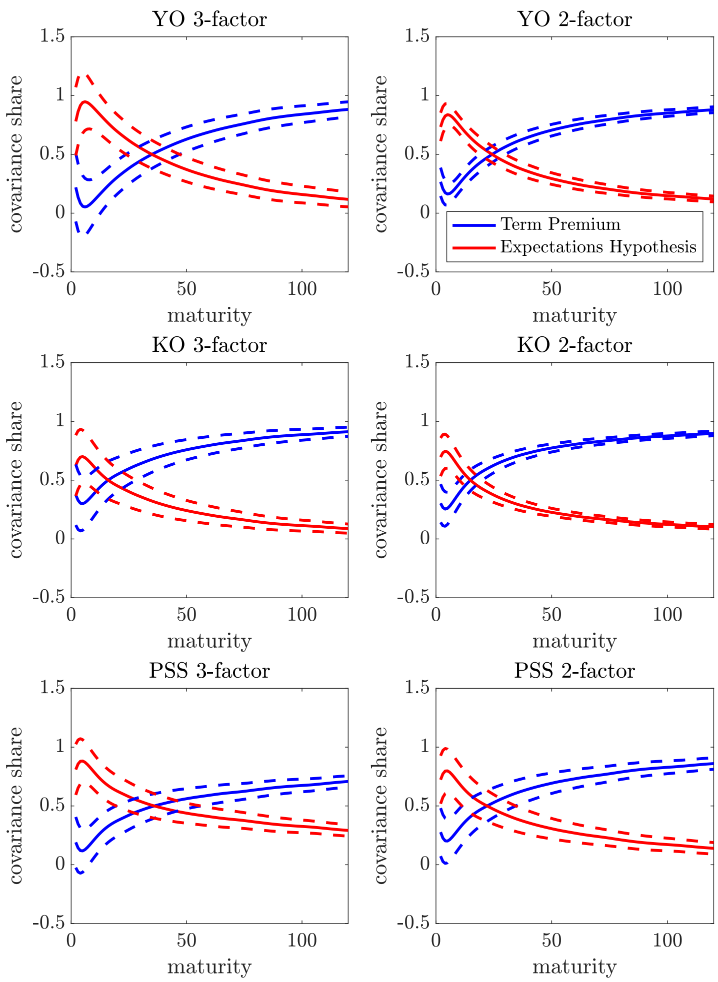


Figure E.2: Decomposition of unconditional variance of the change in yields into Risk Premium and Expectations Hypothesis components for yields only, KO, and PSS models for the ZLB sample (1987-2018). Newey-West 99% confidence bands are shown for point estimates. Maturity is reported in months.

# F Two and three factor results

In this appendix, we report analogous results for both two- and three-factor models for policy applications.

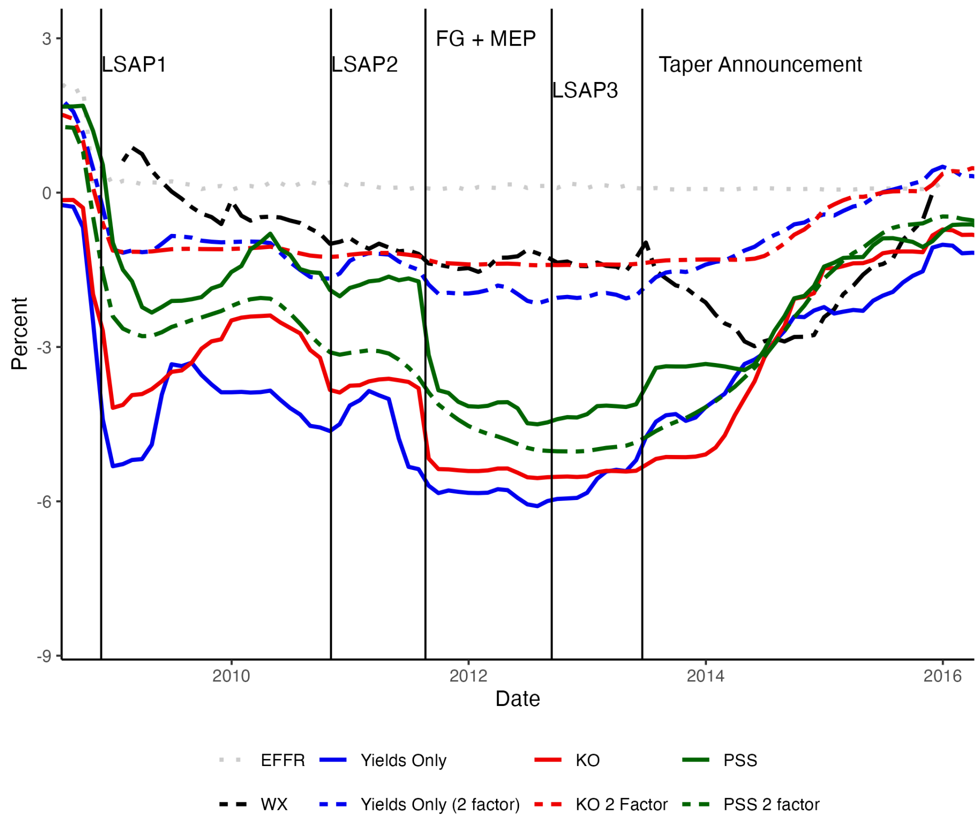


Figure F.1: Smoothed estimates of shadow rates for both two- and three-factor models during/post Great Recession, with event dates (three rounds of Large Scale Asset Purchases (LSAPs), the introduction of calendar-based forward guidance and the Maturity Extension Program (FG+MEP), Taper Announcement). FG and MEP were introduced in August and September 2011, respectively, but are shown in August 2011.

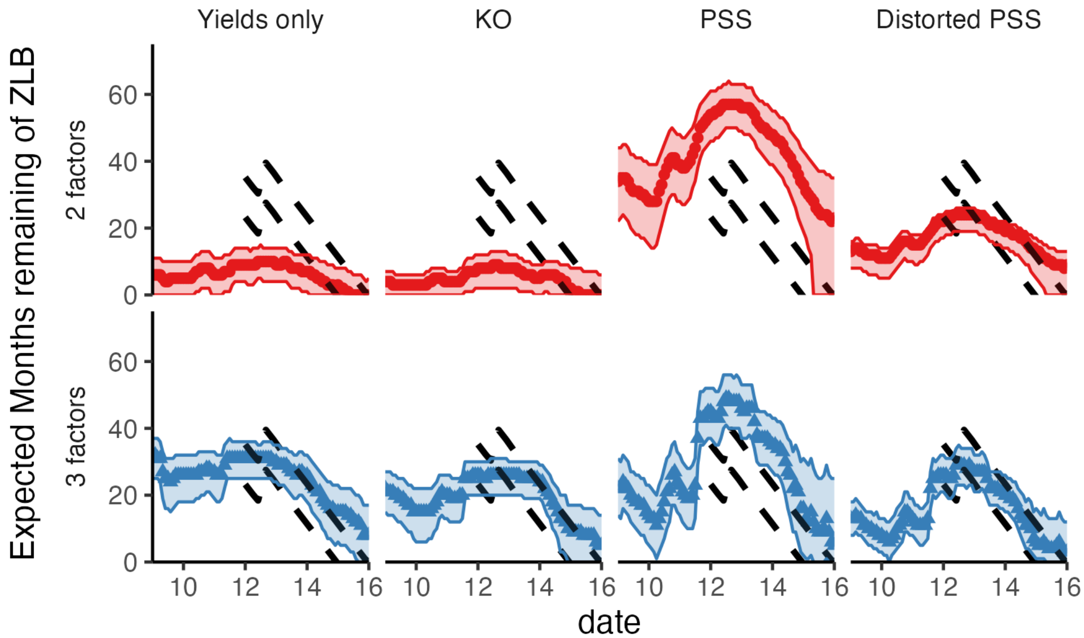


Figure F.2: Real-time implied mean duration of ZLB period. Bands indicate 99th percentile of liftoff dates.

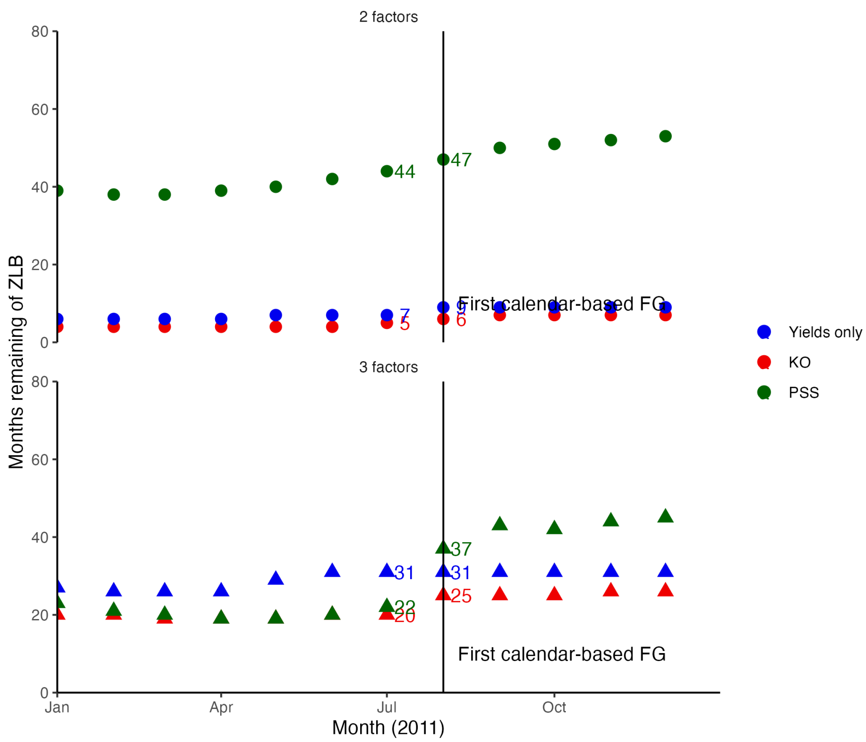


Figure F.3: Real-time implied mean duration of 2011 using the same estimates as those in Figure 3.

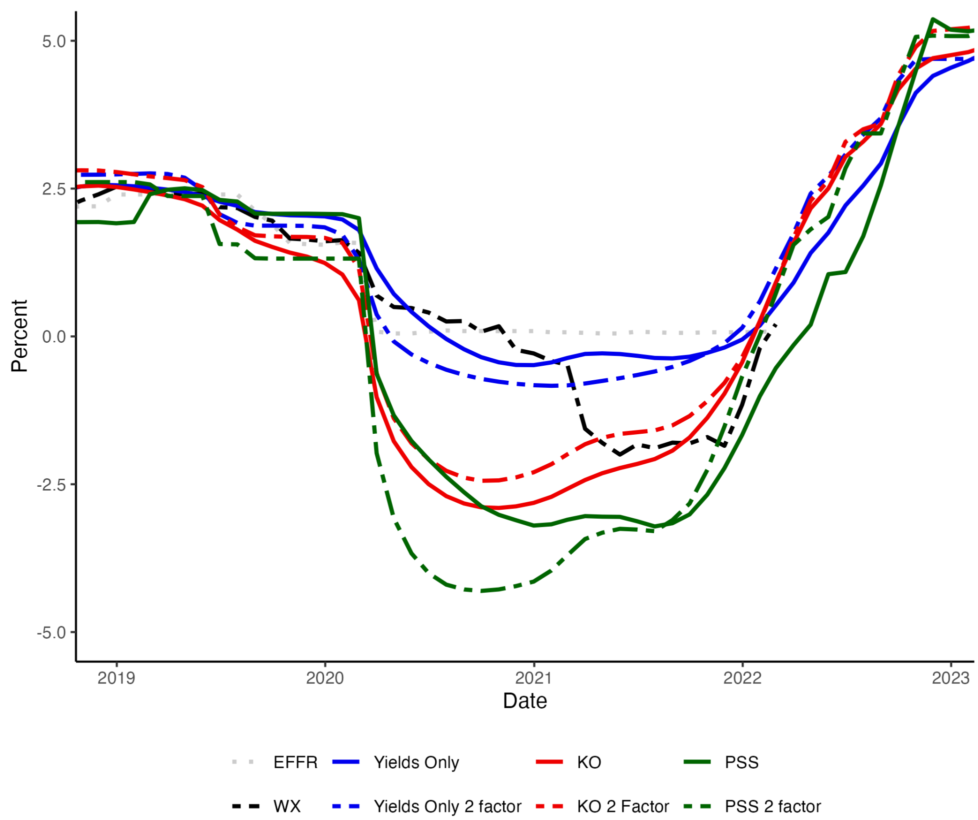


Figure F.4: Smoothed estimates of effective federal funds rate and shadow rate estimates during and after the COVID-19 recession.

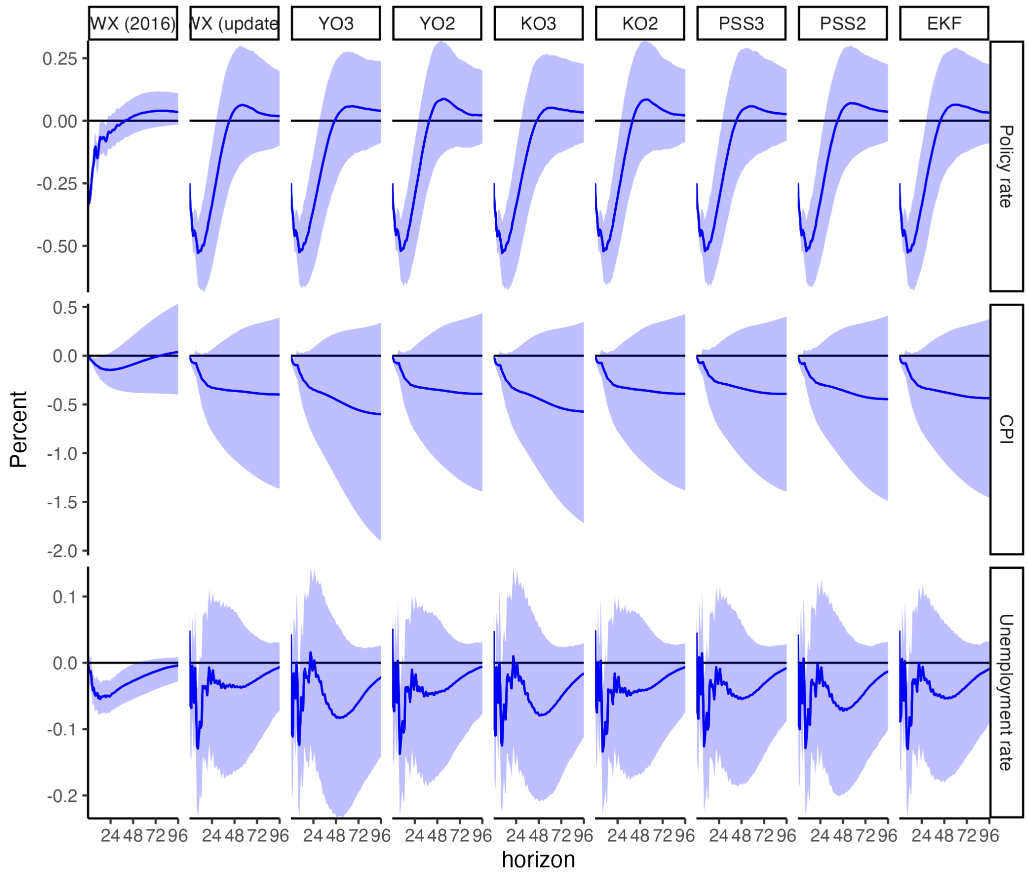


Figure F.5: Estimated impulse response of unemployment to a 25 basis point decrease in the policy rate (Federal funds rate/shadow rate). Solid lines indicate median response and bands indicate 90% confidence intervals. The difference across columns is the underlying estimate of the shadow rate. "WX (2016)" indicates the original impulse response reported in Wu and Xia (2016); "WX (updated)" uses the most recent vintage of data as of December 2024. "EKF" is the YO3 factor model estimated using Li and Wei (2013) yields and the extended Kalman filter.

# G EKF estimates

In this section, we include comparisons between our results and ones estimated with the extended Kalman filter. We focus on a three-factor, yields-only model. Unlike the main results in the paper, but like Wu and Xia (2016), we use annualized yields data for the EKF exercises (as opposed to estimating on un-annualized data and then annualizing after the fact).

We compare four different sets of results. First, we employ the same procedure as is used for the discretization filter – global minimization without constraining the lower bound of the short rate $\underline{r}$. We then restrict $\underline{r} = 0.25$ as in Wu and Xia (2016), but use global search and the complete dataset as in the previous case. We then use the same local search and the same data as Wu and Xia (2016), but using smoothed state estimates. Finally, we include the results obtained from Wu and Xia's code. Average fits for each model during the ZLB period (within the sample used to estimate) are shown in figure G.1. Within the period 2009-2012 (when all four sets of models are on equal footing), no model is clearly superior.

The estimated shadow rates (along with the Wu and Xia (2016) shadow rate) are shown in figure G.2. Here, global search with an unrestricted ZLB yields nonsensical estimates for the shadow rate (that are positive when the rate was constrained). Calibrating the lower bound as in Wu and Xia (2016) gives an estimated path more similar to theirs. The differences between the green and black shadow rates are attributable to differences between

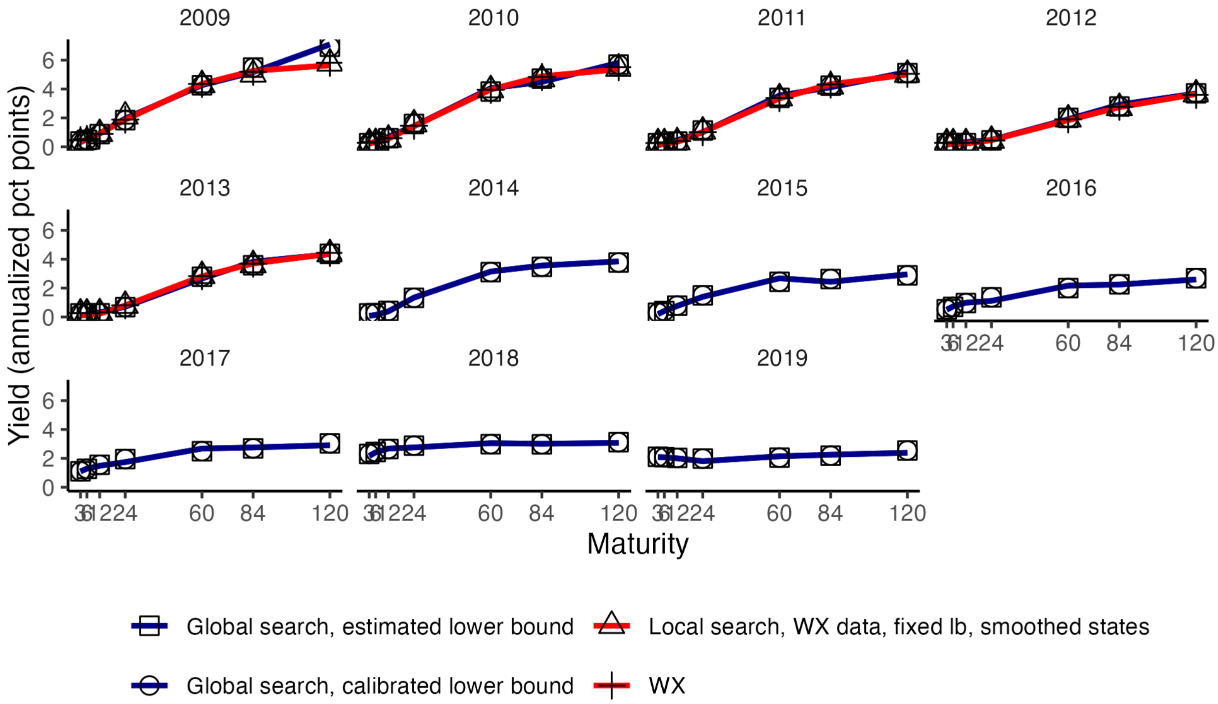


Figure G.1: Predicted and actual forward curves for 3 factor YO model estimated with extended Kalman filter. Blue line: Gürkaynak et al. (2007) forward rate curves. Square: estimated with global search and without fixing $\underline{r}$. Triangle: Global search with $\underline{r} = 0.25$. Circle: local search with $\underline{r} = 0.25$. Plus sign: Results from Wu and Xia (2016).

smoothed and filtered estimates and the computing environment. Turning to the duration plot (figure G.3), we see that both the sets of global search results give extremely long implied horizons for the ZLB, while the local search is slightly more consistent with Wu and Xia (2016). Notably, neither set of 'local' results is completely consistent with the calendar-based forward guidance provided by the FOMC, although the WX results are closer.

For the remainder of the comparison, we focus on comparing our main

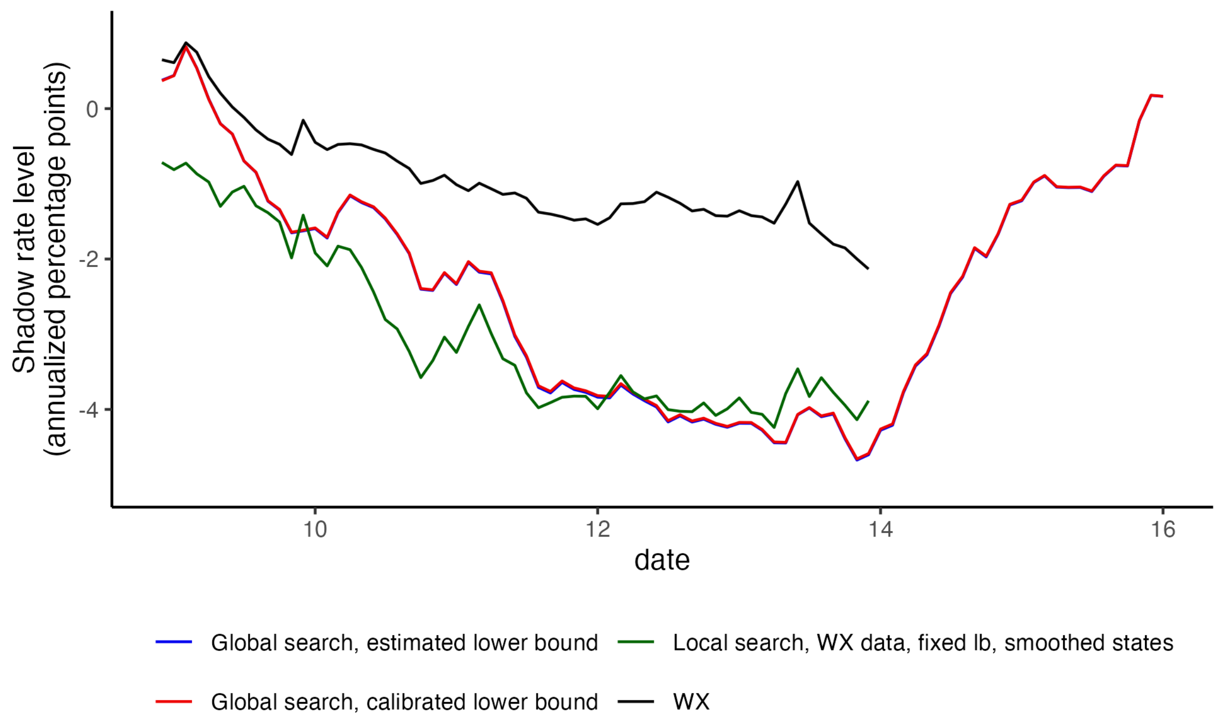


Figure G.2: Estimated shadow rates from 3 factor YO model estimated with extended Kalman filter. Blue: estimated with global search and without fixing $\underline{r}$. Red: Global search with $\underline{r} = 0.25$. Green: local search with $\underline{r} = 0.25$. Black: Results from Wu and Xia (2016).

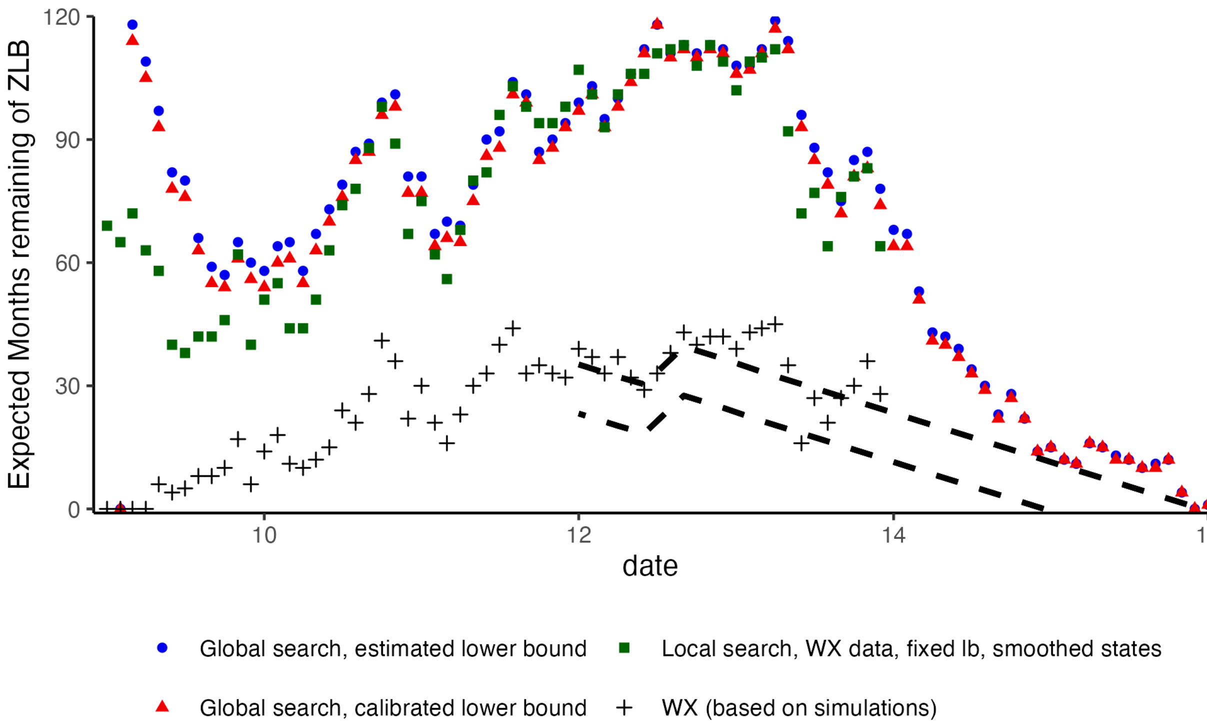


Figure G.3: Implied duration of ZLB from 3 factor YO model estimated with extended Kalman filter. Blue circle: estimated with global search and without fixing $\underline{r}$. Red triangle: Global search with $\underline{r} = 0.25$. Green square: local search with $\underline{r} = 0.25$. Black plus sign: Results from Wu and Xia (2016) based on simulation. The black dashed corridor is the implied range of liftoff dates based on the FOMC SEP as described in the main text.

.

results with EKF results estimated using global methods and a fixed lower bound at $\underline{r} = .25$. We report the in-sample fitting error in panel A of table G.1. Compared to the results in panel A of table 1, the fitting errors are Smaller, with the difference being largest at the long end of the yield curve (22 bp). On the other hand, we achieve reasonably similar average fits despite the grid approximation method of the discretization filter, particularly at the short end. This suggests that the in-sample fit of the DF does not suffer much due to approximating on a grid.—

Next we turn to the pseudo-out-of-sample fit. Here, the EKF model outperforms the YO3 model on average, although the gains are marginal at the short end. During the COVID period, the YO three-factor model does better for short horizons and similarly at long horizons. The performance of the models with forecasts at the short end is superior for both the sub-periods of the forecasting exercise.

Table G.1: Table reports model fits for models estimated using the Extended Kalman Filter as described in the text. The first column reports mean absolute error (MAE) while the second column reports RMSE. Panel A contains estimates for the in-sample fit that uses all observations (396 months). Panel B contains estimates for the out-of-sample fit, which estimates the model each December from 2007-2023, and calculates forecasts for 1- to 12-months ahead. Panels C and D report subcomponents of the out-of-sample forecasts splitting the sample before and after the COVID-19 pandemic. MAE and RMSE are reported across all horizons (10 sets of forecasts at 12 horizons each).

| Statistic | MAE | RMSE |
|---|---|---|
| Panel A: In-Sample Fit (N=396) | | |
| 3mo | 0.12 | 0.14 |
| 6mo | 0.06 | 0.09 |
| 12mo | 0.11 | 0.15 |
| 24mo | 0.12 | 0.16 |
| 60mo | 0.15 | 0.19 |
| 84mo | 0.15 | 0.21 |
| 120mo | 0.10 | 0.14 |
| Panel B: Out-of-Sample Fit: 1-12 month-ahead forecasts (N=180) | | |
| 3mo | 1.15 | 1.97 |
| 6mo | 1.19 | 1.99 |
| 12mo | 1.28 | 1.93 |
| 24mo | 1.38 | 1.76 |
| 60mo | 1.89 | 2.23 |
| 84mo | 1.85 | 2.11 |
| 120mo | 2.04 | 2.35 |
| Panel B: Out-of-Sample Fit, 2007-2019, 1-12 month-ahead forecasts (N=132) | | |
| 3mo | 0.68 | 1.02 |
| 6mo | 0.73 | 1.13 |
| 12mo | 0.89 | 1.25 |
| 24mo | 1.13 | 1.37 |
| 60mo | 1.67 | 1.98 |
| 84mo | 1.71 | 1.99 |
| 120mo | 1.85 | 2.17 |
| Panel B: Out-of-Sample Fit, 2020-2023,-1-12 month-ahead forecasts (N=48) | | |
| 3mo | 1.15 | 1.97 |
| 6mo | 1.19 | 1.99 |
| 12mo | 1.28 | 1.93 |
| 24mo | 1.38 | 1.76 |
| 60mo | 1.89 | 2.23 |
| 84mo | 1.85 | 2.11 |
| 120mo | 2.04 | 2.35 |

# H Additional figures from COVID-19 period

This appendix contains parameter estimates and measures of model fit for each model when we extend the estimation period to the end of 2023.

| | | | |
|---|---|---|---|
| $1200\mu$ | -0.3035 | -0.2309 | 0.0262 |
| | (1.7316) | (1.1599) | (0.0787) |
| $\rho$ | 0.9640 | -0.0038 | 0.3813 |
| | (0.1946) | (0.0755) | (4.2327) |
| | -0.0226 | 0.9208 | 0.9612 |
| | (0.0908) | (0.0748) | (1.6770) |
| | 0.0033 | 0.0028 | 0.8862 |
| | (0.0068) | (0.0167) | (0.1500) |
| $\text{diag}(\rho^{\mathbb{Q}})$ | 0.9980 | 0.9756 | 0.8646 |
| | (0.0017) | (0.0464) | (0.5394) |
| $1200\Sigma$ | 0.4340 | | |
| | (0.4464) | | |
| | -0.3251 | 0.2112 | |
| | (0.8159) | (1.1742) | |
| | -0.0093 | -0.0017 | 0.0448 |
| | (0.1058) | (0.0658) | (0.0177) |
| 1200 $\underline{\text{r}}$ | 0.2105 | | |
| | (0.4251) | | |
| 1200 $\delta_0$ | 13.3745 | | |
| | (6.9440) | | |
| 1200 (yield meas. err) | 0.7655 | | |
| | (1.0346) | | |
| Log Likelihood | | 19428.9339 | |

Table H.1: Estimated parameters for 3 factor model without forecasts (YO model). QMLE standard errors in parentheses

| | | |
|---|---|---|
| $1200\mu$ | -0.0647 | -0.0719 |
| | (0.1869) | (0.0585) |
| $\rho$ | 0.9999 | 0.0373 |
| | (0.0226) | (0.0269) |
| | -0.0182 | 0.9310 |
| | (0.0002) | (0.0102) |
| $\text{diag}(\rho^{\mathbb{Q}})$ | 0.9979 | 0.9809 |
| | (0.0008) | (0.0005) |
| $1200\Sigma$ | 0.4185 | 0.0000 |
| | (0.2161) | (0.0000) |
| | -0.4412 | 0.3563 |
| | (0.2559) | (0.1815) |
| $1200\ \underline{\mathrm{r}}$ | 0.1444 | |
| | (0.7177) | |
| $1200\ \delta_0$ | 12.3772 | |
| | (1.5839) | |
| 1200 (yield meas. err) | 0.8067 | |
| | (1.3388) | |
| Log Likelihood | 19276.3415 | |

Table H.2: Estimated parameters for 2 factor model without forecasts (YO model). QMLE standard errors in parentheses

| | | | |
|---|---|---|---|
| $1200\mu$ | -0.1242 | -0.2266 | -0.0284 |
| | (0.1603) | (0.0905) | (0.0460) |
| $\rho$ | 0.9695 | -0.0084 | 0.0034 |
| | (0.0144) | (0.0246) | (0.0078) |
| | -0.0105 | 0.9596 | 0.0077 |
| | (0.0284) | (0.0142) | (0.0401) |
| | 0.0059 | 0.0526 | 0.4183 |
| | (0.0081) | (0.1654) | (0.4131) |
| $\text{diag}(\rho^{\mathbb{Q}})$ | 0.9977 | 0.9820 | 0.0077 |
| | (0.0021) | (0.0058) | (0.0327) |
| $1200\Sigma$ | 0.5535 | | |
| | (0.0345) | | |
| | -0.5401 | 0.4967 | |
| | (0.0652) | (0.0655) | |
| | -0.0587 | -0.0341 | 0.4285 |
| | (0.0814) | (0.3061) | (0.6256) |
| $1200\ \underline{\mathrm{r}}$ | 0.1223 | | |
| | (0.0655) | | |
| $1200\ \delta_0$ | 10.3113 | | |
| | (8.0803) | | |
| 1200 (yield meas. err) | 0.7497 | | |
| | (1.4549) | | |
| 1200 (fcst meas. err) | 0.8512 | | |
| | (1.1561) | | |
| Log Likelihood | | 35246.3723 | |

Table H.3: Estimated parameters for 3 factor model including forecasts (KO model). QMLE standard errors in parentheses

| | | |
|---|---|---|
| $1200\mu$ | -0.1241 | -0.2265 |
| | (0.0022) | (0.3180) |
| $\rho$ | 0.9691 | -0.0067 |
| | (0.0170) | (0.0022) |
| | -0.0105 | 0.9588 |
| | (0.0402) | (0.0514) |
| $\text{diag}(\rho^{\mathbb{Q}})$ | 0.9977 | 0.9859 |
| | (0.0023) | (0.0032) |
| $1200\Sigma$ | 0.5526 | |
| | (0.0403) | |
| | -0.5005 | 0.4948 |
| | (0.0979) | (0.3937) |
| $1200\ \underline{\mathrm{r}}$ | 0.0857 | |
| | (0.1559) | |
| $1200\ \delta_0$ | 10.2930 | |
| | (0.3939) | |
| 1200 (yield meas. err) | 0.8241 | |
| | (1.4520) | |
| 1200 (fcst meas. err) | 0.9285 | |
| | (1.2499) | |
| Log Likelihood | | 34884.1731 |

Table H.4: Estimated parameters for 2 factor model including forecasts (KO model). QMLE standard errors in parentheses

| | | | |
|---|---|---|---|
| $1200\mu$ | -0.2138 | -0.2508 | 0.5000 |
| | (0.3783) | (0.1873) | (0.7196) |
| $\rho$ | 0.9742 | -0.0059 | 0.0173 |
| | (0.0251) | (0.0681) | (0.1704) |
| | -0.0262 | 0.9639 | 0.1847 |
| | (0.0101) | (0.0455) | (0.0389) |
| | 0.0566 | 0.0213 | 0.6811 |
| | (0.0691) | (0.0191) | (0.1899) |
| $\text{diag}(\rho^{\mathbb{Q}})$ | 0.9983 | 0.9773 | 0.0005 |
| | (0.0046) | (0.0049) | (0.0236) |
| $1200\Sigma$ | 0.3143 | | |
| | (0.2633) | | |
| | -0.2245 | 0.2533 | |
| | (0.2816) | (0.1497) | |
| | 0.0014 | 0.0171 | 0.4394 |
| | (0.0156) | (0.0075) | (0.1801) |
| 1200 $\underline{\text{r}}$ | 0.1140 | | |
| | (0.0064) | | |
| 1200 $\delta_0$ | 12.2223 | | |
| | (8.4616) | | |
| $k$ | 14.2010 | 99.9579 | -99.8220 |
| | (0.7918) | (0.3284) | (0.1168) |
| | -1.5000 | -49.9176 | -99.9838 |
| | (15.2680) | (0.0369) | (0.2224) |
| | 7.3229 | 26.5445 | -99.9838 |
| | (2.0723) | (0.1745) | (0.2224) |
| 1200 (yield meas. err) | 0.3374 | | |
| | (0.0054) | | |
| 1200 (fcast meas. err) | 0.1141 | | |
| | (0.0141) | | |
| Log Likelihood | | 40048.3650 | |

Table H.5: Estimated parameters for 3 factor model with distorted forecaster dynamics (PSS model). QMLE standard errors in parentheses

| | |
|---|---|
| eig($\rho - \Sigma k$) | 0.6985 |
| | 0.9922+0.0165i |
| | 0.9922-0.0165i |

Table H.6: Subjective physical dynamics, 3 factor PSS model

| | | |
|---|---|---|
| $1200\mu$ | -0.0835 | -0.0298 |
| | (0.6627) | (0.7211) |
| $\rho$ | 0.9607 | 0.0454 |
| | (0.1070) | (0.4949) |
| | 0.0342 | 0.9192 |
| | (0.1370) | (0.5995) |
| diag($\rho^{\mathbb{Q}}$) | 0.9986 | 0.9768 |
| | (0.0015) | (0.0401) |
| $1200\Sigma$ | 0.3309 | |
| | (1.3601) | |
| | -0.2220 | 0.3342 |
| | (0.9426) | (0.8290) |
| 1200 $\underline{\mathrm{r}}$ | 0.1111 | |
| | (0.0272) | |
| 1200 $\delta_0$ | 10.4337 | |
| | (2.5331) | |
| $k$ | 3.3987 | 39.0801 |
| | (23.6433) | (0.0025) |
| | 33.7299 | -9.3040 |
| | (1.7715) | (1.7449) |
| 1200 (yield meas. err) | 0.3481 | |
| | (0.2113) | |
| 1200 (fcast meas. err) | 0.2034 | |
| | (0.3221) | |
| Log Likelihood | | 38888.7161 |

Table H.7: Estimated parameters for 2 factor model with distorted forecaster dynamics (PSS model). QMLE standard errors in parentheses

| | |
|---|---|
| eig($\rho - \Sigma k$) | 0.9778 |
| | 0.9110 |

Table H.8: Subjective physical dynamics, 2 factor PSS model

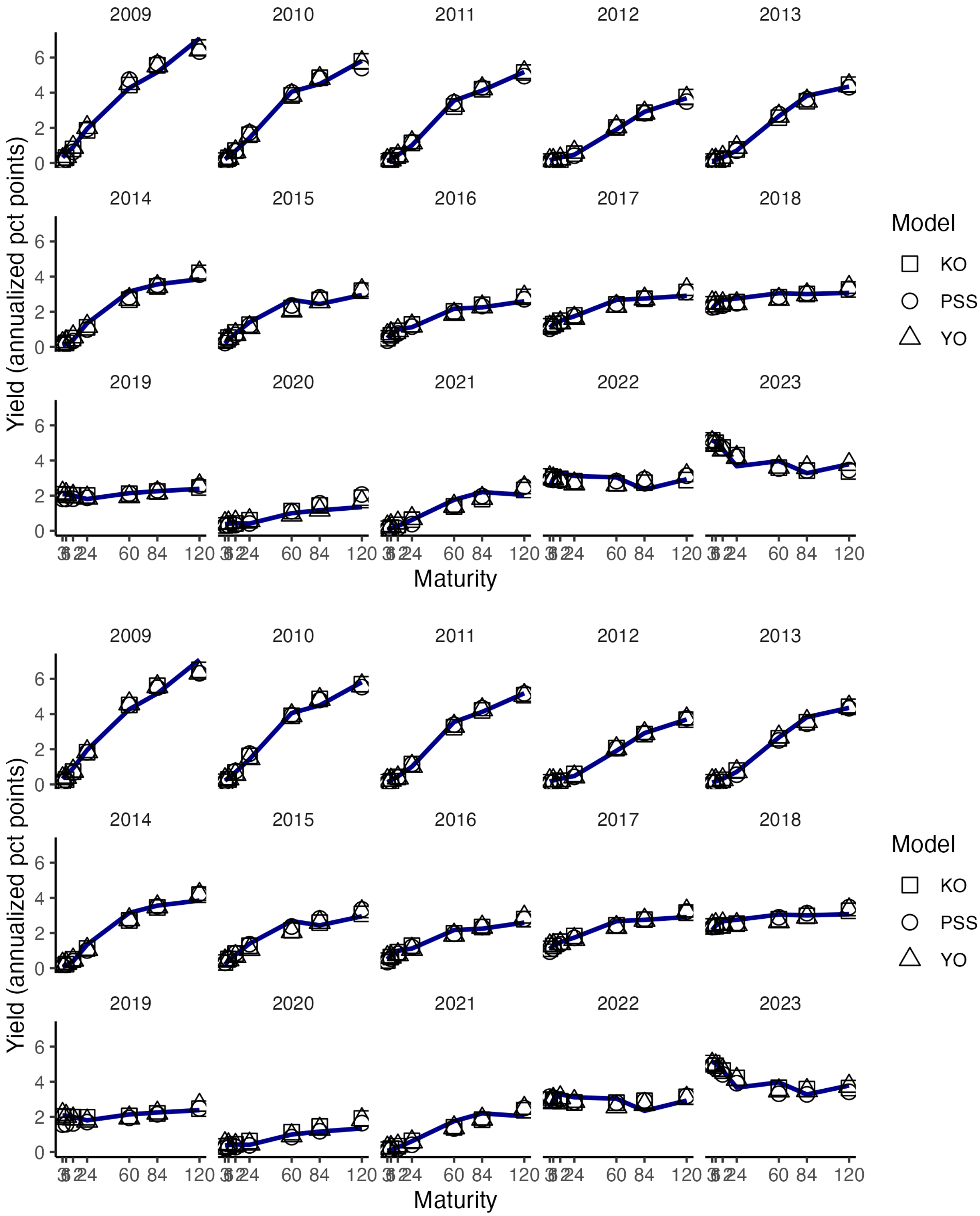




Figure H.1: Average fit of 2-figure (top) and 3-figure (bottom) model across years for extended sample, using smoothed state estimates. Line indicates average yield curve in the indicated year.

# I Sensitivity to yield curve data

In an earlier working paper version of this paper, we used the Gürkaynak et al. (2007) (GSW) zero-coupon yield curve estimates, following the choice made by Wu and Xia (2016). Subsequent to our first completed draft, Liu and Wu (2021) (LW) published new estimates of the zero-coupon yield curve, which we have used in this iteration.[18] The Liu and Wu (2021) yield curve estimation methodology differs from Gürkaynak et al. (2007) in two important respects: Liu and Wu's estimates include data on Treasury bills and securities with less than three months to maturity, and they use nonparametric methods to estimate the constant-maturity zero coupon curve. Liu and Wu (2021) emphasize that their estimates capture the local variation in maturities and have smaller pricing errors, especially at the short and long end of the yield curve. We plot the annualized difference in forward rate estimates in figure I.2. They are often substantial, especially around the financial crisis period.

Some of the qualitative and quantitative conclusions of our estimation differ between the two datasets. This is unsurprising, as the underlying yield curve data are quite different, and this difference is somewhat magnified for forward rates, particularly around the start of the Great Recession. Liu and Wu find a similar result in the context of Cochrane and Piazzesi (2005) regressions.

[18]We thank an anonymous referee for encouraging us to use the Liu and Wu data.

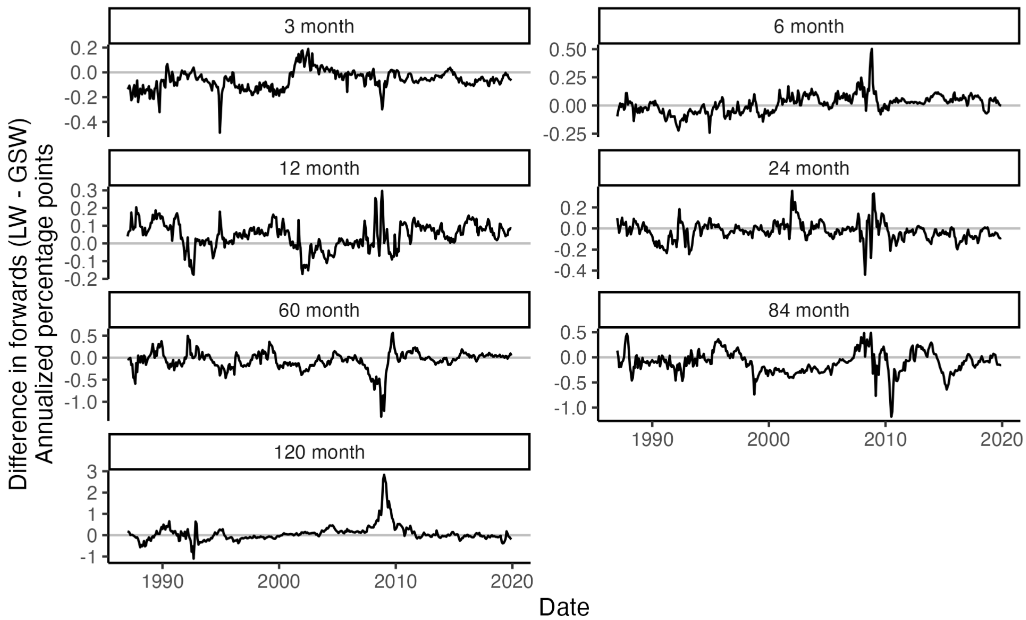


Figure I.2: Difference in forward rates (annualized percentage points) for Liu and Wu (2021) zero coupon yield curve estimates relative to Gürkaynak et al. (2007) estimates.

Rather than double the reported estimates, we summarize which conclusions are sensitive to the choice of zero-coupon yield curve data in this section.

- **Model fit and forecasting performance**: Unsurprisingly, the non-parametrically estimated yield curve is harder to match than the relatively smooth GSW estimates. Using the GSW data, we found that the EKF did a marginally better job fitting in-sample, but had worse forecasting performance. The picture is more nuanced with the LW data, which can be observed by comparing table 1 to appendix table G.1 . Using the LW data set, the performance for the EKF is similar to the best discretization filter estimates except at the 7- and 10-year horizon, where the RMSE is 7 and 22 bp lower for the EKF (panel A in the respective tables). Similarly, the average out-of-sample forecasting performance is somewhat better for the discretization filter estimates at the short end but not for the long end (panel B in each table). The benefit of the short-end forecasts is especially apparent during the 2020-2024 period (panel D). Some of the degradation of fit and forecasting performance is likely due to the greater variability of the yield curve. On average, the 3 factor models estimated with the discretization filter appear to perform well up through 2019 regardless of the data set.

- **Structural break tests and monetary policy VARs**: Using GSW data, we rejected the null of no structural break for most, but not all, of our models (particularly, we failed to reject the null at for the PSS 2-factor model, and rejected it marginally for the KO 2-factor model). Using the LW data, we get a similarly mixed pattern. We also find that the shadow rates estimated with the EKF on the LW data do not reject the null of no structural break.

- **Event analysis**: In general, the estimates using LW attribute a greater fraction of variation around the announcement of LSAPs and forward guidance to risk premia, relative to those using GSW. In particular, the change in expected duration of the ZLB around the forward guidance announcement is greatly attenuated using the LW estimates. Figure 4, for example, implies that that the largest jump in anticipated duration of the ZLB around the forward guidance announcement was about 4 months; using the GSW data, the largest duration was about 9 months. Similarly, we found a greater explanatory power for the expectations hypothesis component of yields in the analysis of LSAPs using the GSW data. The PSS 3-factor model implied more than 40% of the drop in yields was due to changes in short rates, while using the LW data, that fraction is less than 20%. However, it is worth remembering that the GSW short end is a parametric extrapolation, while the LW estimates use near-to-maturity securities. This seems to make LW preferable for the event analysis

- **Mean duration of ZLB** Using the GSW yield curve estimates, we found that real-time mean duration of the ZLB was similar to that implied by the forward guidance from the SEP, except for one model (KO3), whose duration estimates were generally longer. Using the LW data, we get qualitatively different results. The three factor KO model has wide confidence bands, but the median path diverges from the SEP's guidance. The PSS model over-estimates yields, but the implied path of the "distorted" forecast path (which draws mainly on the forecasts) is consistent with the SEP.

- **Effects of Treasury supply changes:** Using GSW's yield curve estimates, we find qualitatively and quantitatively different evidence on the channels of asset purchases. Our estimates based on GSW imply that local scarcity is only significant in half of the term premium regressions. By contrast, the results in tables 3 and 4, which calculate term premia and the yield curve's slope using the Liu and Wu dataset, suggest changes in local scarcity and duration are both statistically associated with changes in the term premium observed during the first two LSAP rounds, in a way that is economically significant.

# J Updated FAVAR data

| FREDMD mn | Transform: | Slow: | WX number |
|---|---|---|---|
| IPFPNSS | 5 | 1 | 1 |
| IPFINAL | 5 | 1 | 2 |
| IPCONGD | 5 | 1 | 3 |
| IPDCONGD | 5 | 1 | 4 |
| IPNCONGD | 5 | 1 | 5 |
| IPBUSEQ | 5 | 1 | 6 |
| IPMAT | 5 | 1 | 7 |
| IPDMAT | 5 | 1 | 8 |
| IPNMAT | 5 | 1 | 9 |
| IPMANSICS | 5 | 1 | 10 |
| IPB51222S | 5 | 1 | 12 |
| INDPRO | 5 | 1 | 13 |
| CUMFNS | 1 | 1 | 14 |
| RPI | 5 | 1 | 17 |
| W875RX1 | 5 | 1 | 18 |
| CLF16OV | 5 | 1 | 19 |
| CE16OV | 5 | 1 | 19 |
| UNRATE | 1 | 1 | 21 |
| UEMPMEAN | 1 | 1 | 22 |
| UEMPLT5 | 1 | 1 | 23 |
| UEMP5TO14 | 1 | 1 | 24 |
| UEMP15OV | 1 | 1 | 25 |
| UEMP15T26 | 1 | 1 | 26 |
| PAYEMS | 5 | 1 | 27 |
| USGOOD | 5 | 1 | 29 |
| CES1021000 | 5 | 1 | 30 |
| USCONS | 5 | 1 | 31 |
| MANEMP | 5 | 1 | 32 |
| DMANEMP | 5 | 1 | 33 |
| NDMANEMP | 5 | 1 | 34 |
| SRVPRD | 5 | 1 | 35 |
| USTPU | 5 | 1 | 36 |
| USWTRADE | 5 | 1 | 37 |
| USTRADE | 5 | 1 | 38 |
| USGOVT | 5 | 1 | 39 |
| CES0600000 | 1 | 1 | 26 |
| AWOTMAN | 1 | 1 | 40 |
| DPCERA3M0 | 5 | 1 | 41 |
| CMRMTSPLx | 5 | 1 | 43 |
| RETAILx | 5 | 1 | |
| HOUST | 4 | 0 | 47 |
| HOUSTNE | 4 | 0 | 48 |
| HOUSTMW | 4 | 0 | 49 |
| HOUSTS | 4 | 0 | 50 |

Transform codes:

| | |
|---|---|
| 1 | No transformation |
| 4 | Natural log |
| 5 | First difference of natural log |

| | | | |
|---|---|---|---|
| HOUSTW | 4 | 0 | 51 |
| PERMIT | 4 | 0 | 52 |
| ACOGNO | 5 | 0 | 57 |
| ANDENOx | 5 | 0 | 58 |
| S&P 500 | 5 | 0 | 59 |
| EXUSUKx | 5 | 0 | 61 |
| EXCAUSx | 5 | 0 | 62 |
| FEDFUNDS | 1 | 0 | 63 |
| TB3MS | 1 | 0 | 64 |
| TB6MS | 1 | 0 | 65 |
| GS1 | 1 | 0 | 66 |
| GS5 | 1 | 0 | 67 |
| GS10 | 1 | 0 | 68 |
| TB3SMFFM | 1 | 0 | 69 |
| TB6SMFFM | 1 | 0 | 70 |
| T1YFFM | 1 | 0 | 71 |
| T5YFFM | 1 | 0 | 72 |
| T10YFFM | 1 | 0 | 73 |
| BUSLOANS | 5 | 0 | 74 |
| NONREVSL | 5 | 0 | 75 |
| M1SL | 5 | 0 | 76 |
| M2SL | 5 | 0 | 77 |
| BOGMBASE | 5 | 0 | 78 |
| WPSFD4920 | 5 | 1 | 81 |
| WPSFD4950 | 5 | 1 | 82 |
| WPSID61 | 5 | 1 | 83 |
| WPSID62 | 5 | 1 | 84 |
| CPIAUCSL | 5 | 1 | 85 |
| CPIAPPSL | 5 | 1 | 83 |
| CPITRNSL | 5 | 1 | 87 |
| CPIMEDSL | 5 | 1 | 88 |
| CUSR0000SA | 5 | 1 | 89 |
| CUSR0000SA | 5 | 1 | 90 |
| CUSR0000SA | 5 | 1 | 91 |
| CPIULFSL | 5 | 1 | 92 |
| CUSR0000SA | 5 | 1 | 93 |
| CUSR0000SA | 5 | 1 | 94 |
| CES2000000 | 5 | 1 | 95 |
| CES3000000 | 5 | 1 | 96 |
| UMCSENTx | 1 | 0 | 97 |

Appendix – for online publication

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