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ABSTRACT

Using a panel of daily CUSIP-level data, we study the effects of the Federal Reserve's program to purchase $300 billion of U.S. Treasury coupon securities announced and implemented during 2009. This program represented an unprecedented intervention in the Treasury market and thus allows us to shed light on the price elasticities and substitutability of Treasuries, preferred-habitat theories of the term structure, and the ability of large-scale asset purchases to reduce overall yields and improve market functioning. We find that each purchase operation, on average, caused a decline in yields in the sector purchased of 3.5 basis points on the days when these purchases occurred (the "flow effect" of the program). In addition, the program as a whole resulted in a persistent downward shift in the yield curve of as much as 50 basis points (the "stock effect"), with the largest impact in the 10- to 15-year sector. The coefficient patterns generally support a view of segmentation or imperfect substitution within the Treasury market.

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1. Introduction

During the crisis of 2008, policymakers took a number of extraordinary steps to improve the functioning of financial markets and stimulate the economy. Among the most important of these measures, in terms of both scale and prominence, were the Federal Reserve's purchases of large quantities of government-backed securities in the secondary market, conventionally known as the Large Scale Asset Purchase—or "LSAP"—programs. The LSAPs included debt obligations of the government-sponsored housing agencies, mortgage-backed securities (MBS) issued by those agencies, and coupon securities issued by the U.S. Treasury, and they collectively amounted to $1.7 trillion over a period of about 15 months—the single largest government intervention in financial-market history. Given the unprecedented size and nature of these programs and the speed with which they were proposed and implemented, policymakers could have had, at best, only a very rough *ex ante* sense of their potential impact. The minutes of the December 2008 Federal Open Market Committee meeting summarized the prospects thus: "The available evidence indicated that [LSAP] purchases would reduce yields on those instruments, and lower yields on those securities would tend to reduce borrowing costs for a range of private borrowers, although participants were uncertain as to the likely size of such effects."

There was particular cause for skepticism regarding the program to purchase Treasury securities. The market for U.S. government debt is among the largest and most liquid in the world, and it was not obvious that even such a sizeable intervention—the $300 billion purchased by the Fed constituted about 8 percent of the market at the time—would have significant effects, given the array of other securities that serve as potential substitutes for Treasuries. Indeed, while the MBS and agency-debt LSAP programs were quickly judged successful, with conventional mortgage rates dropping about 1 percentage point after the announcement of these programs and remaining in the range of 5 percent for the duration of the crisis, the effects of the Treasury program were far from obvious. Treasury yields fell notably when the program was announced on March 18, 2009, but they retraced those declines in subsequent weeks. Some observers even speculated that the program could perversely serve to *increase* yields if the accompanying rise in reserve balances were seen as inflationary or if the Fed were viewed as accommodating fiscal expansion by "monetizing the debt."[1]

The FOMC's conviction that purchases of longer-term Treasury securities would reduce longer-term yields appears to have rested on a premise that the relative prices of financial assets are influenced in

[1] For example, former Fed Governor Gramley told Reuters in June, 2009, "I don't think they can afford to go out and aggressively buy longer-term Treasuries or even step-up aggressively their purchases of mortgage debt… There is this fear going around in financial markets that the Fed is going to monetize the debt, and we're going to have big inflation. I don't believe that for a minute, but the perception is a reality that the Fed is going to have to deal with." (Bull, 2009).

part by the quantity available to the public. Though perhaps intuitive, this view runs counter to the treatment of interest rates implied by the expectations hypothesis and canonical models of the term structure. Instead, such an idea would seem to require grounding in a theory of "imperfect asset substitutability," "portfolio balance," or "preferred habitat." Such theories have existed informally for decades (e.g., Modigliani and Sutch, 1966), but they have recently received greater attention as researchers have begun to supply them with rigorous foundations, as in the models of Andres *et al*. (2004) and Vayanos and Vila (2009). Still, empirical work documenting these effects—that is, the extent to which changes in the relative supply of and demand for Treasury debt affect its pricing—is limited. The evidence that does exist typically relies on time-series or event-study methods examining aggregate measures of yields and outstanding debt (as in Bernanke *et al*., 2004; Engen and Hubbard, 2005; Han *et al*., 2007; Krishnamurthy and Vissing-Jorgensen, 2007; and Hamilton and Wu, 2010). While potentially informative, the nature of the data in such studies complicates identification and limits what can be learned about differences in impact across securities.

In this paper, we use a panel of daily CUSIP-level data on LSAP purchases and returns to study the effects of changes in the supply of publicly available Treasury debt on yields. The differences in returns between the securities that were purchased in different amounts during the program identify the impacts of the purchases—that is, the Treasury price elasticities. The CUSIP-level data allow us to parse these reactions more finely than has been possible in previous studies. For example, we are able to examine differential effects of purchases across security characteristics such as maturity and liquidity. In addition, we estimate substitution effects across securities by constructing for each CUSIP buckets of Treasuries with similar remaining maturities and estimating the cross-elasticities of their prices. These cross-elasticities are crucial for determining how the aggregate level and term structure of interest rates were affected by the LSAP program and can themselves shed light on the relevance of preferred-habitat theories (which suggest imperfect substitution across securities).

An additional innovation of our approach is to distinguish between two types of impact that the LSAP program might have had—flow effects and stock effects. "Flow effects" are defined as the response of prices to the ongoing purchase operations and could reflect, on top of portfolio rebalancing activity due to the outcome of the purchases, impairments in liquidity and functioning of the Treasury market. Such market imperfections might allow even pre-announced withdraws of supply to have effects on prices when they occur. To estimate flow effects, we model the percentage change in each CUSIP's price on each day that purchase operations occurred as a function of the amount of that CUSIP and the amounts of substitute securities purchased. Meanwhile, "stock effects" are defined as persistent changes in price that result from movements along the Treasury demand curve and include the market reaction due to changes in expectations about future withdraws of supply. To estimate stock effects, we model the cumulative change in each CUSIP's price between March 17, 2009 and October 29, 2009 (i.e., the cross section of Treasury returns) as a function of total own and substitute Treasury purchases. Because, over the life of the program, purchased amounts could have responded endogenously to price changes, we instrument LSAP purchases with the purchased securities' characteristics prior to the announcement of the program when estimating stock effects. By removing our estimated stock effects from the actual cross section of Treasury prices as of the end of the LSAP program, we are able to construct a counterfactual yield curve that represents what interest rates might have looked like if the program had never existed.

The results suggest that, on average, Treasury purchases reduced yields by about 30 basis points across the yield curve over the life of the program (the stock effect) and led to a further 4 basis point decline in purchased sectors on the days when purchases occurred (the flow effect). The stock effects were most pronounced among securities with 10 to 15 years of remaining maturity—which we estimate would have had yields as much as 50 basis points higher in the absence of the program—but less pronounced among the relatively liquid note securities. The flow effects were concentrated in securities with remaining maturities of less than 15 years that were eligible for purchase on a given day. Within this set, coefficients across various types of security characteristics and subperiods are quite robust, although we find that the flow effects were more persistent for off-the-run bonds. The sample of securities that were ineligible for purchase exhibits some instabilities in its flow effects, but those results are consistent with the results for eligible securities over at least part of the sample.

We view these results as economically important. A decline in longer-term Treasury yields on the order of 30 to 50 basis points is large by historical standards. Moreover, if this decline had indeed been passed through to private credit markets, it would have represented a substantial reduction in the cost of borrowing for businesses and households. Although we do not test whether this pass-through actually occurred, the observation that most credit spreads declined during the life of the program suggests that at least some of it may have. It thus appears that the Treasury LSAP program was probably successful in its stated goal of broadly reducing interest rates, at least relative to what they would otherwise have been.

Both the stock- and flow-effect results provide strong support for portfolio-balance theories, as they demonstrate that Treasury rates are sensitive to the amount of Treasury debt available to the public. This is consistent with the widely held view that Treasury securities play a special role in the global economy and thus are not perfect substitutes for other types of debt.[2] Our results further indicate that, on the days when a security was eligible to be bought, purchases of securities with similar maturities had

[2] Some possible reasons for this specialness include the exceptional liquidity and safety that are associated with the Treasury market, Treasuries' heavy use as collateral in repurchase agreements, and their preferential treatment as assets in assigning regulatory and credit ratings for financial institutions.

almost as large effects on its yield as did purchases of the security itself—that is, the cross- and own elasticities for flow effects were nearly identical—while purchases of maturities further away had smaller effects. This supports the view that Treasuries of similar maturities are close substitutes but that substitutability diminishes as maturities get farther apart, consistent with a role for preferred habitat in the term structure. In addition, we find evidence that certain portions of the Treasury market are more highly segmented. For example, we generally reject equality of the own- and cross-elasticities in far-off-the-run bonds, suggesting that preferred habitat may play an even greater role among those securities.

The following section of the paper provides an overview of the Treasury LSAP program. Section 3 reviews some of the theory that could motivate the LSAP programs and some previous evidence on the link between Treasury supply and interest rates. Section 4 presents our empirical work, with sub-section 4.1 discussing some general issues, sub-section 4.2 considering flow effects, and sub-section 4.3 considering stock effects. Section 5 offers concluding thoughts.

2. Details of the Treasury LSAP Program

Although smaller than the MBS LSAP program in dollar volume and smaller than the agency-debt program as a fraction of its respective market, the Treasury program merits special study for several reasons. First, while the agency-debt and MBS programs were primarily focused on reducing mortgage rates and stimulating the housing market, the Treasury program was seen as having the ability to contribute to reductions in the cost of credit across a range of markets. Indeed, there were those on the FOMC who argued for a greater role for the Treasury program, rather than the other two, on precisely these grounds.[3] Second, understanding the effects of changes in the supply of Treasury debt available to the public is likely to have broader policy importance. Under normal circumstances, the Fed conducts most of its open-market operations in Treasuries, the Treasury Department may worry about the interest-rate effects of introducing new supply, and foreign central banks conduct sizeable interventions in this market to maintain their reserves. The issue also received renewed attention in mid-2010, when the FOMC announced that it would begin reinvesting the principal payments on its MBS and agency debt portfolios into Treasury securities, potentially taking substantial additional quantities out of the market.

The program to purchase up to $300 billion of Treasury coupon securities was announced in the FOMC statement on March 18, 2009. Although policymakers had floated the possibility of Treasury purchases prior to this announcement, market participants did not appear to place high odds on the decision being taken, and the announcement generally came as a surprise.[4] The first operation under the

[3] See, for example, the minutes of the January 2009 FOMC meeting (Federal Reserve, 2009).
[4] For instance, the morning of the FOMC announcement, Bloomberg reported that Goldman Sachs and several other banks believed that policymakers would not introduce such a program and that the March FOMC statement would be largely similar to the January statement:

Treasury LSAP program was conducted on March 25. Purchases continued at a pace of about $10 billion per week over the subsequent five months. On August 12, 2009 the Committee announced that it would purchase the full $300 billion (eliminating the ambiguity of the "up to" language) and that it would wind down the program in October. After this announcement, the pace of purchases gradually slowed to minimize any potential disruption that might have resulted from a sudden closing of the program.

The logistics of the purchase operations were as follows. Every-other Wednesday, the Desk announced the broad maturity sectors in which it would be buying over the subsequent two weeks and the days on which it would be conducting these operations. Auctions took place from Monday of the first week through Friday of the second week and typically settled on the following day.[5] At 10:15 on the morning of each auction, the Desk published a list of CUSIPs that were eligible for purchase, which generally included nearly all securities in the targeted sector,[6] and began accepting propositions. At 11:00 AM, the auction closed. The Desk then determined which securities to buy from among the submitted bids based on a confidential algorithm, and it published the auction results within a few minutes of the auction close. Market participants were not aware in advance of the total amount to be purchased or of the distribution of purchases across CUSIPs.

Overall, purchases of nominal securities under the Treasury LSAP program included 160 unique CUSIPs, spanning remaining maturities of about two to thirty years. $300 billion represented about 3 percent of the total stock of outstanding Treasury debt and about 8 percent of the outstanding coupon securities as of the time of the announcement. Table 1 and Figure 1 provide some additional statistics summarizing the characteristics of purchased securities. Most purchases were concentrated in the 2- to 7-year sectors, although, as a percentage of total outstanding Treasuries within each sector, purchases across maturities were less concentrated. Coupon rates and vintages of securities purchased were roughly similar to the averages of all outstanding Treasuries. The average maturity of securities bought was a bit longer than average, but the yields on purchased securities were notably higher than average—seemingly by too great a margin to be accounted for solely by their slightly longer maturities, especially given that a relatively high fraction (approximately 30 percent) of purchases were on-the-run issues, which generally have lower yields. This suggests that, consistent with contemporary commentary, the Desk deliberately purchased securities that were underpriced, a claim that we will illustrate more formally below.

Figure 2 shows the behavior of Treasury yields over the period of the program. After the initial announcement, medium- to long-dated yields fell by as much as 50 basis points, with yields in the 5- to

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http://www.bloomberg.com/apps/news?pid=20601087&sid=aIt7yEi9XAhc&refer=home

[5] In practice, the Desk avoided conducting Treasury operations on Fridays, preferring to reserve these days for agency purchases.

[6] The securities that were excluded were the cheapest-to-deliver in futures markets, those with high scarcity value in the repo market, and those for which 35 percent of the issue has already been purchased under the LSAP program.

10-year sector declining the most. However, the decline was short-lived—by early May most yields had returned to their pre-announcement levels, and they shot up further in June as the economic outlook improved, policy uncertainty increased, and investors shed duration following a rise in mortgage rates. Although some of these increases reversed by October, most yields were still 20 to 40 basis points higher at the end of the program than they were before it started, and indeed they increased by the greatest amount in precisely the 5- to 10-year portion of the term structure where purchases were concentrated. These increases led some observers (e.g., Thornton, 2009) to conclude that the LSAPs had been ineffective in reducing interest rates. Of course, as we demonstrate below, such reasoning ignores other factors that may have been influencing yields over this period, including the possibility that the distribution of purchases itself was responding endogenously to relative changes in Treasury prices.

Another notable pattern over this period—and one that may itself have been due in part to the Treasury LSAP program—is the improvement in liquidity in the Treasury market. Traders had pointed to reduced liquidity as an important factor putting upward pressure on some yields in the weeks leading up to the introduction of the program. As Table 2 illustrates, almost every measure of liquidity improved between the first and second halves of the program's life. Average trading volumes increased by 20 percent, the yield premium paid for on-the-run (i.e., the most recently issued) 10-year note over off-the-run securities with comparable remaining maturities fell by a quarter, and failures to deliver securities into repurchase agreements on Treasuries declined by 80 percent. The final column of the table shows the average residuals that result from fitting a smooth curve, using the functional form proposed by Svensson (1994), to the cross-section of yields on each day. These yield curve "fitting errors," which can be interpreted as a measure of unexploited price discrepancies, declined by about half between the two sub-periods.

3. Theory and Evidence on the Effects of Treasury Supply

3.1 Theoretical Motivation

Federal Reserve officials put forth a variety of objectives for the Treasury purchases and for the LSAP programs more generally, including the provision of liquidity and the improvement of market functioning. By far the most frequently cited objective was to increase the flow of credit to businesses and households by reducing the general level of interest rates.[7] However, given the unprecedented nature of the policy experiment, the weak theoretical guidance available, and the fairly inconclusive evidence on the possible effects of purchases on asset prices, policymakers repeatedly expressed uncertainty about the likely size of such effects.

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[7] See, for example, Chairman Bernanke's April 3 speech (Bernanke, 2009).

In order for large-scale purchases to result in a meaningful reduction in private interest rates, two conditions must hold: (1) Removing Treasury securities from the supply available to the public must have a negative effect on Treasury yields; and (2) interest rates on private credit must depend upon Treasury yields. Although there is some empirical support for both of these hypotheses, it is not obvious from a theoretical standpoint that either of them should be true—indeed, they could be viewed as somewhat contradictory. The notion that changing the publicly available supply of Treasuries by a finite amount could have notable effects on their yields (Condition 1) would seem to rest upon an assumption that the stock of close substitutes for Treasuries is limited. And if rates on other types of credit also respond to Treasury purchases (Condition 2), it must be the case that these types of credit are part of this stock of close substitutes. (If they were not substitutes, their prices, by definition, would not change in response to quantity variations in the Treasury market.) But this would mean that the pool from which the Treasury LSAP program was draining included not just Treasuries themselves but at least some portion of the vast markets for mortgages, consumer credit, and corporate debt—to say nothing of foreign securities—and by this standard \$300 billion would appear to be an almost trivial amount.

Having noted this tension, we leave Condition 2 aside in this paper and focus on the question of whether changes in the stock of Treasuries during the LSAP program affected the yields on Treasuries themselves.[8] Perhaps surprisingly, the arbitrage-free models of the term structure of interest rates that have become standard in the finance literature do not generally account for this possibility. To see why, consider the simple example of a frictionless market with risk-neutral traders, where the short-term (instantaneous) risk-free rate evolves according to some exogenous stochastic process. Because of risk neutrality, a strict version the expectations hypothesis, in which there are no term premiums, holds, and the rate of maturity *n* at time *t* is given by

$$r_t^n = \frac{1}{n}\int_0^n E_t r_{t+s}^0 ds \qquad (1)$$

This condition must be true, because, for example, if r_t^n were greater than the integral on the right, an arbitrageur could profit by selling short-term debt and buying long-term debt; this arbitrage would drive r_t^n down until the equality was restored. Note that this condition involves no role for fluctuations in the outstanding supply of longer-term debt, provided that such fluctuations do not influence the instantaneous risk-free rate. Indeed, if we further suppose that the short-term rate follows the mean-reverting process

$$dr_t^0 = \phi\left(k - r_t^0\right)dt + \sigma dB_t \qquad (2)$$

[8] In our empirical work below, we will implicitly assume that the Treasury market as a whole is segmented enough from other markets that the stock of outstanding Treasuries in a given maturity range serves as a good proxy for the total stock of relevant substitutes for any Treasury security within that range.

where B_t is Brownian motion, then the integral in (1) reduces to an affine function of the time-t short-term rate:

$$r_t^n = a(n) + b(n) r_t^0 \tag{3}$$

where $b(n) = (1\text{-}\exp(\text{-}\phi n))/\phi n$, and $a(n) = k(1\text{-}b(n))$. Under the expectations hypothesis, asset purchases should have no effect on yields because they do not appear anywhere in this equation.

Relaxing the assumption of risk neutrality allows for departures from the expectations hypothesis, but does not, by itself, create room for the supply of debt to matter. For example, now suppose that market participants choose the maturities of (zero-coupon) bonds to purchase in order to maximize a risk-return objective function over an instantaneous investment horizon:

$$\max_n V_t(n)$$

for

$$V_t(n) = E_t\left[\frac{dp_t^n}{p_t^n}\right] - \alpha\,\mathrm{var}_t\left[\frac{dp_t^n}{p_t^n}\right] \tag{4}$$

where $p_t^n = \exp[-nr_t^n]$ is the price of the zero-coupon bond with maturity n and $\alpha \geq 0$ is a risk-aversion parameter. Maintaining the process (2) for the short-term rate, it can be shown (e.g., Duffie and Kan, 1996) that, in the absence of arbitrage (i.e., when the value of V_t is identical at all maturities), the maximization still leads to a solution of the form (3), but the functions $a(n)$ and $b(n)$ are now determined by the model parameters σ and α, in addition to k and ϕ. This model generates departures from the expectations hypothesis and risk-sensitive term premia, but there is still no role for changes in the quantity of debt outstanding to influence securities prices.

Also note that simply introducing motives other than risk and return for holding particular securities is not sufficient for quantity variations to matter. For example, one could incorporate a preference for specific maturities by amending the objective function:

$$V_t(n) = E_t\left[\frac{dp_t^n}{p_t^n}\right] - \alpha\,\mathrm{var}_t\left[\frac{dp_t^n}{p_t^n}\right] - \beta\left(n - n^*\right)^2 \tag{5}$$

where $\beta \geq 0$ is a parameter that penalizes participants for investing far away from their preferred maturity n^*.[9] While such a formulation can result in adjustments to $a(n)$ and $b(n)$ that allow for persistent peaks and troughs in the term structure, altering the relative supply at those maturities would not affect equilibrium interest rates in this model.

[9] The classic example of such a preference is an insurance company or pension fund that wants to match a particular set of long-term liabilities with a portfolio of assets that have similar cash flows or a bond mutual fund that is restricted to hold longer-term maturities. However, it is also possible to imagine investors with preferences for short- or medium-term securities, securities with especially high liquidity (such as on-the-run issues), or securities with particular coupons.

Allowing time variation in σ and α or allowing for more sophisticated processes for r_t^0 and V_t generally changes the functional form of $a(n)$ and $b(n)$ and introduces additional linear terms in equation (3), giving rise to the class of so-called affine term-structure models, but these extensions still do not generally create an explicit role for quantities to matter. While *estimations* of such models could of course capture the effects of changing supply through the use of latent factors, that possibility provides little scope for testing whether supply matters or for shedding light on exactly how it might matter, given that those factors rarely have clean interpretations. Indeed, the usual way that unobserved factors enter in affine models—by affecting short-term rates, risk, or the price of risk—does not appear to capture fully the types of mechanisms that economists seem to have in mind when they consider supply effects.

One way of getting changes in bond supply to affect pricing in a theoretical model is to introduce a friction that limits arbitrage across different types of financial assets. Greenwood and Vayanos (2008) and Vayanos and Vila (2009) develop models formalizing this idea.[10] In their theory, risk-averse arbitrageurs do not completely eliminate price differentials because they are concerned about possible adverse price movements in the specific positions they would need to take to execute this arbitrage. If some investor classes have preferences over security maturities or other characteristics, the resulting demand can thus translate into persistent price differences that would appear to violate no-arbitrage conditions in standard models. A simple version of the Vayanos-Vila model generates a solution of the form

$$r_t^n = a(n) + b(n) r_t^0 + c(n) \delta_t \qquad (6)$$

where δ_t is a "demand factor" that, for any given configuration of the yield curve, maps into specific quantities purchased by preferred-habitat investors. Preferred-habitat investors are those investors who have motives other than risk and return for holding particular maturities of Treasuries, and thus LSAP purchases could be modeled as shifts in δ_t. This is the type of mechanism we have in mind.[11] If a channel like this one exists, withdrawing Treasury supply through programs like the LSAP would be able to affect the level and slope of the yield curve (implying a change in term premiums).

Though not emphasized in previous work, another consequence of a market-segmentation assumption is that changes in supply could have effects when they occur, even if those changes are expected in advance. Just as risk-averse (or otherwise constrained) arbitrageurs cannot perfectly

[10] Andres *et al*. (2005) provide an alternative model that also allows for preferred habitat. However, it is difficult to square their framework with empirical work on Treasury prices because it abstracts from many features of the Treasury market, including the possibility of secondary-market trading, which is obviously crucial when studying daily movements in the yield curve.

[11] While we believe that this channel likely captures the first-order effect of LSAPs, it does not rule out the possibility that LSAP purchases could also change expectations of short-term interest rates through a signaling channel.

eliminate the effects of demand factors across the yield curve at any point in time, they also cannot perfectly eliminate the effects of demand factors at a point on the yield curve over time. If preferred-habitat investors respond only to spot prices and not to expectations of possible returns, spot prices will change when supply is withdrawn, even if that withdraw is perfectly anticipated. This could be one mechanism generating "flow effects" of LSAP purchases.

3.2 Previous Evidence

A number of previous studies have examined empirically the extent to which changes in outstanding Treasury supply affect the nominal yield curve by examining various circumstances in which supply was added or withdrawn by the government. Tarhan (1995) examined market prices around Federal Reserve purchases of longer-term securities for the SOMA portfolio in the usual course of its open-market operations. Engen and Hubbard (2005) and Krishnamurthy and Vissing-Jorgenson (2008) both used time series analysis to examine the extent to which various yields track levels and forecasts of government debt/GDP ratios.[12] Kuttner (2006) used "excess returns" regressions in the style of Cochrane and Piazessi (2005) to test whether the Fed's SOMA purchases had significant effects on term premia. Finally, Greenwood and Vayanos (2008) used the aggregate term structure of outstanding Treasury debt to explain the time-series patterns in longer-term yields. Results from these studies generally suggest negative effects of supply on yields, but there is substantial variance across the magnitudes and statistical significance of the estimates. All of them use a fairly limited amount of the data that are potentially relevant (e.g., by looking only at a handful of yields and some aggregated measure of outstanding supply), which limits what they can say about variation across different classes of securities. It is also often unclear how the results in these studies might be affected by expectations of supply versus the amount of supply that is actually outstanding—that is, possible flow and stock effects are often conflated. By using CUSIP-level data, we hope to be able to address these issues.

A recent paper by Hamilton and Wu (2010) uses a variant of the Vayanos-Vila model to explicitly relate estimates of an affine term-structure model to measures of outstanding Treasury supply. One of their results (based on pre-LSAP data) is that substituting $400 billion in long-term Treasury debt with an equal amount of short-term debt would reduce longer-term rates by about 17 basis points, suggesting an elasticity of about 0.4 basis points per billion dollars purchased, at least for longer-term securities. While we view this work is complementary to ours, we note that the Hamilton-Wu approach may understate the yield response for at least two reasons. First, their model assumes that it is only the

[12] Engen and Hubbard (2005) also review related literature in macroeconomics and public finance that studies whether government deficits are related to the level of interest rates (i.e., tests of Ricardian equivalence). They argue that this literature is generally inconclusive.

relative amounts of Treasury securities at different maturities that matters—proportional changes in the total amount of outstanding debt are assumed to have no effect. Second, they do not account for *expectations* of changing supply, instead assuming that only currently outstanding debt has effects. Moreover, although the Hamilton-Wu specification allows changes in supply to have separate effects on the level, slope, and curvature of the yield curve, it still focuses on a rather limited set of (constant-maturity) yields, ignoring potentially interesting variation across securities.

Another type of evidence comes from the case studies of particular episodes that have involved relatively large or rapid changes in Treasury supply. One such episode that is well known is the Federal Reserve's attempt to decrease long-term yields relative to short-term yields in the early 1960s—termed "Operation Twist"—which involved the sale of short-term Treasury debt and the purchase of long-term debt. This program was analyzed in several contemporaneous studies[13] and more recently by Hakim and Rashidian (2000). These studies have generally not found large effects of the program.

Bernanke *et al*. (2004) studied the responses of the yield curve during several more-recent cases of government intervention in Treasury market and concluded that such interventions could have significant effects on yields, and they cited this evidence as providing possibilities for monetary policy when short-term interest rates are constrained by the zero lower bound. One of these cases in particular is worth noting, because it came close to the LSAP program in mechanics, if not in scale. Namely, under Treasury Department's repurchase of long-term debt during the early 2000's, the government bought back $67.5 billion of bonds (about 1.2 percent of outstanding Treasury debt), entirely in off-the-run issues with original maturities of 30 years. In many details, the operations were similar to LSAP purchases—for example, the broad sector for each operation was announced in advance, but Treasury could choose which securities to purchase from among submitted bids within that sector.[14] Bernanke *et al*. (2004) argued that the buyback program had significant effects (although they did not provide precise estimates of the magnitudes). Longstaff (2004) found also that the Treasury buyback program had significant effects on spreads between Treasuries and other government-guaranteed debt (RefCo bonds), and Greenwood and Vayanos (2010) argue that the purchases served to decrease longer-term Treasury yields relative to short-term. On the other hand, Han et al. (2005) found small and statistically insignificant differences in yield changes between bonds that were purchased and those that were not.[15]

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[13] Modigliani and Sutch (1966), Ross (1966), and Holland (1969).

[14] Importantly however, in contrast to the LSAP program, the Treasury buyback program specifically attempted to *minimize* the effects of operations on market prices, since increases in prices would have driven up the cost of the purchases.

[15] Bernanke et al. (2004) also examine two other episodes that may pertain to the effects of changing Treasury supply on yields—the initiation of large purchases of Treasuries by Asian central banks in 2002 and the market perception that the Fed might undertake an LSAP-type program in 2003. Kuttner (2006) also provides a narrative overview of several instances of large interventions in Treasury markets and suggests that most of these

Overall, the existing evidence seems consistent with the hypothesis that changes in Treasury supply affect yields, perhaps through preferred-habitat type channels. However, it is difficult to get a precise idea of the magnitude of these effects or of how they vary with security characteristics because most previous analysis is done at an aggregated level. No previous study that we know of exploits variation in quantities and prices for individual securities to account for timing issues (flow versus stock effects), substitutability across the term structure, and specific security characteristics, as we do below.

4. Empirical Work

To organize the discussion of our results, we note four qualitative hypotheses that are generated by the preferred-habitat view, on which we will offer evidence:

- H1. Government purchases of Treasury securities have significant effects on Treasury yields.
- H2. Those effects are largest for the particular securities that are purchased, somewhat smaller for securities that have similar maturities to the purchased securities, and minimal for securities that have much different maturities than the purchased securities.
- H3. These differences in responses are more pronounced when purchases take place in portions of the market that are more likely to be segmented—that is, among securities that are not widely traded or for which many good substitutes do not exist.
- H4. Among such securities, even anticipated purchases might not be fully priced until they actually occur, resulting in persistent price changes on the day of operations.

As noted, we will analyze Treasury price changes both using panel data on the days when purchases occurred (flow effects) and cross-sectionally over the period of the program as a whole (stock effects). Both sets of tests share the same data sources and variable definitions, described below.

4.1 Data and Specification Issues

Our data consist of daily observations on the universe of outstanding nominal Treasury coupon securities from March 25 through October 30, 2009. To simplify the analysis, we exclude TIPS and securities with

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interventions have had economically significant effects. Finally, Greenwood and Vayanos (2010) argue that U.K. pension reforms in 2004 had significant effects on the term structure of gilt yields. A few recent papers (Doh, 2010; Gagnon et al., 2010; Neely, 2010) have examined the effects of the LSAP programs using aggregate time-series data and found large effects. However, these studies generally do not distinguish the effects of the Treasury program from those of the other MBS and agency-debt programs, and their methodology does not allow them to consider stock and flow effects as we define them. There is also some disagreement within this literature—Stroebel and Taylor (2009), for example, use similar methods to conclude that the effects of the MBS purchase program were small.

remaining maturities of less than 90 days, leaving us with an unbalanced panel of 204 CUSIPs (including 44 securities that were never purchased under the LSAP program). Our dependent variables are percentage price changes in each of these securities (measured at end-of-day). Our independent variables are constructed from the security-level amounts purchased and total outstanding amounts.[16]

A central element in our approach is the possibility that the price of a given security may also move in response to purchases of other securities for which it is a substitute. Toward this end, we define buckets of substitutes for each security. Although in principle we could choose the size and number of the substitute buckets in a variety of ways, a division into three buckets based on remaining-maturity ranges seemed to provide a good combination of parsimony and flexibility. In particular, for each security *i*, we define our most narrow bucket to include all securities having remaining maturities within two years of security *i*'s maturity. We refer to these securities as "near substitutes" for security *i*. The second bucket, which we call "mid-substitutes" for *i*, includes all securities having remaining maturities that are between two and six years different from security *i*'s. The third bucket ("far substitutes") includes all securities having remaining maturities between six and fourteen years different from security *i*'s. We denote the amount of each bucket purchased by the Desk on day *t* by s_{ijt}, where $j = \{1, 2, 3\}$ indexes the degree of substitutability. In addition, we let q_{it} denote the amount of security *i* purchased. We normalize q_{it} and s_{ijt} by the total amount of securities outstanding that have remaining maturities within two years of security *i* (that is, the sum of a security's own amount outstanding and the amount of its near substitutes). This normalization generates coefficients that all take the same units, allowing us to compare the effects of a given dollar amount of purchases across different sectors.[17]

Because coupon rates and maturities vary considerably across the universe of Treasury securities that we work with, we conduct our regressions in price space, rather than in yield space.[18] Converting equation (6) to prices and applying Ito's lemma gives an equation for the bond return dp_t^n/p_t^n that is linear in the levels of and shocks to the short-term interest rate and demand factor. This motivates the following general form for both our flow- and stock-effect regressions:[19]

$$\frac{\Delta p_{it}}{p_{it-1}} = \beta q_{it} + \sum_{j=1}^{3} \gamma_j s_{ijt} + \boldsymbol{\varphi} \mathbf{x}_{ijt} + \varepsilon_{it} \qquad (7)$$

[16] Purchased amounts by CUSIP are publicly available on the Federal Reserve Bank of New York's website, and amounts outstanding are based on information from the Treasury Department. Daily pricing data come from NPQS.

[17] In economic terms, the rationale for this denominator is that it proxies for the relevant measure of supply within a given sector. We tried various alternative normalization schemes and generally found results consistent with those reported below.

[18] However, the central results also hold if we use yields as the dependent variable.

[19] Of course, this motivation for the functional form is only heuristic—the securities in our dataset are not zero-coupon, and we do not impose the restrictions on the coefficients that would be required by a formal affine term-structure model.

where Δp_{it} is the change in the price of Treasury security i in period t and $\mathbf{x}$ is a vector of control variables. In our flow-effect regressions we use daily data, while the stock-effect regressions are cross-sectional, so that t represents the entire sample period in that case. The constituents of $\mathbf{x}$ also differ between the flow- and stock-effect models.

The parameter β reflects the *own*-price elasticity of Treasury securities, while the parameters γ_j primarily reflect the *cross*-elasticities of Treasury security prices with respect to other Treasury securities. These latter elasticities depend on the degree of substitutability between different Treasury issues, which in turn depends upon the liquidity of the Treasury market and the ability of participants to arbitrage away price inefficiencies in this market. The own-price response β is of some interest, as its magnitude is indicative of the purchases' effects on the amounts by which an individual security's yield could deviate from those of similar securities (i.e., yield-curve fitting errors). The cross-responses γ_j, however, are likely to be much more important in terms of the aggregate level and term structure of interest rates. This is because the purchase of a particular security affects that security's yield alone through the βq_{it} term, but it affects *every* security's yield through the applicable $\gamma_j s_{ijt}$ terms.

4.2 Flow Effects

In this section, we test whether Treasury LSAP purchases had effects on Treasury prices around the times when purchases occurred. Again, we define such responses as the "flow effects" of the program. Because the sectors of purchase operations were announced in advance and both the list of CUSIPs and sizes of these operations were fairly predictable, one might expect that examining yield changes as function of contemporaneous purchases would reveal no statistically significant responses. While this may well have been the case at the aggregate level, however, it need not be the case at the CUSIP level. Because the particular CUSIPs that were purchased and the amounts of these purchases were not known in advance to the market, yield differentials should have emerged on the days of purchases between those that are purchased and those that are not (assuming the demand for Treasury securities is sufficiently elastic). In addition, market illiquidity and other technical factors could cause yields to move in response to purchases, even if those purchases were perfectly predictable. Our measures of flow effects will include both of these phenomena, as well as the possible dynamic described in hypothesis H4.

<u>Specification</u>

Our flow-effect regressions are the following special case of equation (7):

$$\frac{\Delta p_{it}}{p_{it-1}} = \alpha_i + \delta_t + \beta q_{it} + \sum_{j=1}^{3} \gamma_j s_{ijt} + \varepsilon_{it} \qquad (8)$$

where Δp_{it} is the change in the price of Treasury security *i* on day *t*, α_i is a security-specific fixed effect, δ_t is a time dummy, and ε_{it} is an error term. The panel structure of our data, by allowing for both CUSIP-level fixed effects and daily time dummies, enables us to control for unobservable variables that might otherwise cloud the analysis, such as the level of and changes in short-term interest rates and other factors, like Treasury auctions, that could shift relative demand and supply in the nominal coupon market.

Because the broad maturity sectors within which securities were purchased on any given day were announced in advance, we may expect that securities within those sectors might have reacted differently to the purchase operations than securities that were outside the purchased sectors. To examine this possibility, we split the sample into (1) observations of securities on days when those securities were within the announced purchase sectors (defined as "eligible" securities) and (2) observations of securities on days when purchases took place in different sectors (defined as "ineligible"). These subsets are mutually exclusive and exhaustive within the set of days on which purchases took place.

Finally, it is possible that, because of settlement lags or other microstructure issues, the effects of purchases are not fully realized until the day after they occur. To check for this possibility, we also look at returns on the days after purchase operations by running regressions of the form

$$\frac{\Delta p_{it+1}}{p_{it}} = \alpha_i + \delta_t + \beta q_{it} + \sum_{j=1}^{3} \gamma_j s_{ijt} + \varepsilon_{it} \tag{9}$$

Baseline Results

In this sub-section, we report the results for the full sample and for some illustrative sub-samples. Beyond the distinctions we note here, the results are generally robust to alternative ways of constructing the sub-samples, as well as to adjustments for possible correlation in the error terms. Those robustness checks are reported in the following sub-section.

In Table 3 we report the baseline results. Initial tests suggested that the coefficients were not stable for securities with very long remaining maturities, so we report a sample split at the midpoint maturity of 15 years. (As seen in Figure 1, the vast majority of purchases were in the less-than-15-year sector.) Focusing on the first column of the table, which pertains to eligible securities with remaining maturities of less than 15 years, the coefficient of 0.276 on own purchases implies that, on average, purchasing $1 billion of Treasuries increased the price on the securities purchased by about 0.02 percent; this translates into a yield decrease of about 0.7 basis points per billion dollars purchased, on average for this subsample.[20] The strong statistical significance of this result supports hypothesis H1—purchases

[20] Specifically, the average price change is given by (0.2763/1203) =0.00023, where $1,203 is the average amount (in billions) of near substitutes outstanding for each security in the subsample. The average yield change is given by -0.29*0.00023 = -0.000067, where 0.29 is the average of the inverse of the modified duration of the subsample.

have effects on yields. In addition, on the days when a security was eligible to be bought, purchases of its "near substitutes" had almost as large effects on its yield as did purchases of the security itself, pointing to a very high degree of substitutability among these securities.[21] The coefficient gets smaller as the degree of substitutability decreases, supporting hypothesis H2. Applying the aggregate coefficients to averages of the dependent variables, we find that the typical effect of each operation was on the order of -3.5 basis points for the sector being purchased, consistent with the elasticity reported above and the average operation size of around $5 billion. The second column of Table 3 shows that these results did not generally hold for longer-maturity issues. The two right-hand columns suggest that a security was also insensitive to the purchases of its substitutes on days when it was not eligible to be purchased itself, although we will show below that this is not true in all subsamples.

In the remainder of this section, we focus only on securities with less than 15 years to maturity, given that that is where most purchases occurred and where most of the statistical significance seems to be. Within this sub-sample, Table 4 splits the data into purchases that occurred during the first half of the LSAP program (March 25 – July 6) and those that occurred during the second half (July 7 – October 29). As noted earlier, liquidity in the Treasury market was substantially better during the second half of the sample. Thus, if the price responses to LSAP purchases were due to impediments to market clearing and price discovery resulting from poor market functioning, we would expect the results to be substantially weaker in the second sub-sample. The first two columns of the table show that there is no evidence of this, at least among securities that were eligible for purchase—the coefficients are nearly identical for the two sub-periods and are very close to the pooled results reported in the first column of Table 3. However, the story is more complicated for the securities that were ineligible for purchase. For those securities, the coefficients on all of the substitute purchases are negative in the first half of the sample and positive in the second half. (Recall that they were insignificant when the two sub-samples were pooled.) During the second half of the sample, the coefficients on near- and mid-substitute purchases are close to those for the eligible sample, as we would expect given that there was generally little qualitative difference between eligible and not-eligible securities. Thus, the first half of the sample for the not-eligible securities is the puzzling piece of the data. A possible explanation is that dealers anticipated being able to sell more to the Fed than they actually were able to sell and thus sold securities (including securities that had not been eligible) after LSAP operations in order to maintain a portfolio target. Such an effect would likely have dissipated by the second half of the sample, as participants learned the pattern of the Desk's operations.

Table 5 re-runs the results in Table 4 but splitting the sample by security type: the top panel presents the results for Treasury notes, and the bottom panel presents the results for Treasury bonds. This

[21] Because it was rare to purchase securities with maturities more than six years apart in the same operation, the far-substitute variable is omitted from this regression.

distinction is potentially interesting largely as a proxy for other security features. The bonds all had original maturities of 30 years and, in this sample, have remaining maturities of less than 15 years. Thus, they are all very old issues, and they tend to have much smaller trading volumes and lower liquidity than notes, as well as higher coupons.[22] However, the results for the bond and note subsamples are generally similar to each other and to the results presented in Table 4 in terms of sign and significance. (The one possible exception is for eligible bonds in the second half of the sample, where there are too few observations to achieve statistical significance.) The second-half coefficients for not-eligible bonds are somewhat smaller than in the pooled sample, but, in general, this breakdown does not illuminate the puzzling results noted above for not-eligible securities.

Table 6 turns to the question of what happened on the days after LSAP operations took place. The sample breakdown and independent variables are the same as those used in Table 5, but now the dependent variable is the security return on day t+1. Consider first the sample of eligible securities, presented in the left-hand sets of columns. For eligible note securities, prices almost uniformly reversed the increases they experienced on the days of purchases—the coefficients are of roughly similar magnitudes to those reported in the top panel of Table 5, but they are all negative (although they are not individually significant in the second half of the sample). This suggests that flow effects among notes were short-lived. For eligible bonds, on the other hand, prices actually increased further on the days after purchases. Indeed, tests using price changes on subsequent days (not shown) suggest that the effects of LSAP purchases on these bonds may have never fully reversed.[23]

These results are broadly consistent with hypothesis H4—that even anticipated purchases could have significant effects on prices. A brief spike and retreat in prices, such as occurred among notes, can be explained by settlement, clearing, and rebalancing frictions that do not necessarily constitute material departures from market efficiency. But a *persistent* increase in prices following a purchase that was announced in advance would seem to call for a more substantial explanation, such as preferred habitat. That this pattern is evident among the less-traded securities further supports the idea that such a mechanism may be at work.

Finally, turning to the day-after results for not-eligible securities, the coefficients for notes are similar to those in the eligible sample, again suggesting good substitutability (as we would expect) across these groups. The sign and significance of the coefficients on not-eligible bonds do not show a clear

[22] That the market perceived important differences between the two types of securities was obvious immediately prior to the announcement of the LSAP program, when the yields on 5- to 10-year maturity bonds were as much as 50 basis points higher than those on comparable-maturity notes. See Gurkaynak and Wright (2010).

[23] Our finding that purchases had significant effects on prices the days after they occurred raises the possibility that our baseline regressions are misspecified (since they do not control for these effects). However, when we reestimated those equations with lagged purchases and lagged price changes as dependent variables, the coefficient estimates were essentially unchanged.

pattern, but the coefficient magnitudes are small compared to most of the other samples we have reported. In general (also taking into account Table 5), it does not appear that the prices of not-eligible bonds increase with their eligible counterparts following purchases. This is somewhat puzzling but could again be consistent with the relatively weak liquidity for these securities.

Robstness Checks

Tables 7 through 11 present evidence supporting the claim above that our central flow effect results are robust to different cuts of the data and possible correlation of the errors. Table 7 splits the sample with less than 15 years remaining maturity into notes and bonds. We saw above that there was very little difference on the day of purchase between these two classes of securities when the sample was further split into early and late periods. The results in this table confirm that that is also true when the data are pooled over the entire program.

Table 8 performs a similar exercise, instead splitting the sample by security "vintage." In particular, we divide the sample (again, using just securities with less than 15 years remaining maturity) into "near-on-the-run" and "far-off-the-run" subsamples. (Security *i* is said to be "*x* issues off-the-run" if there have been *x* other issues of securities with the same initial maturity as security *i* in the time since security *i* was issued.) This distinction is potentially important because off-the-run securities typically trade less frequently and are considered less liquid. We include securities more than 5 issues off the run in the far-off-the-run category and all more-recently issued securities in the near-on-the-run category.[24] This sample split does not reveal any notable differences from our baseline results.

Table 9 drills down even further, splitting the sample of far-off-the-run securities into notes and bonds. (Because we are restricting attention to the less-than-15-year maturity sector, all of the near-on-the-run securities are notes.) Again, the essential results of the baseline continue to hold, although there is some loss of statistical significance with the relatively small samples here.

Tables 10 and 11 present results for the baseline samples and key subsamples using clustered standard errors. Because it seems plausible that the regression errors are correlated across maturity, we allow for clustering within one-year maturity buckets for each security. This adjustment does not alter any of our results, either for the baseline breakdown or for the subsamples. We also clustered by security type (not shown) and did not observe any notable differences with the results reported above.

[24] Although this definition stretches the notion of "near on the run" a bit, a much more restrictive cutoff would leave too small a sample to draw meaningful conclusions.

4.3 Stock Effects

Specification

By "stock effects" we mean the impact that the LSAP program had on yields by permanently reducing the total amount of Treasury securities available for purchase by the public. Of course, expectations of such effects should have been impounded into Treasury prices as soon as the market became aware of the program, before any purchases took place—presumably, this mechanism accounted for much of the 25 to 50 basis point drop in Treasury yields on the day the program was announced. Thus, it is crucial to account for expectations when measuring stock effects.

Our identification relies on two ideas. First, preferred-habitat or portfolio-balance effects are large enough such that reducing the available supply of a particular security should have greater effects on that security (and its near substitutes) than on securities that are of much different maturities. Second, we note that the expectation effects just mentioned only matter prior to the conclusion of the program. In other words, while there may be temporary price fluctuations reflecting changing expectations of future purchases, these expectations become irrelevant once the total actual amounts and distribution of purchases is revealed. Thus, all else equal, the difference in price changes across two securities between the time the program was announced and the time it was concluded should depend only on the relative amount of each security that was *actually* purchased over the life of the program. With this in mind, our regressions for the stock effects use the cross section of total price changes for all nominal Treasury coupon securities between March 17 and October 30, 2009. Some previous studies of LSAPs, such as Gagnon *et al*. (2010), have tried to identify their effects by looking at the reaction of prices within specific event windows around important announcements. The difficulty with this approach (apart from the danger of mis-specifying the appropriate windows) is that it relies solely on changes in expectations of purchases that occur within the windows—if market participants had some expectation of purchases prior to the windows, or if they changed their expectations any time outside the windows, or if they waited until purchases actually occurred to fully impound their effects, the event study will not capture the true effects of the program. Instead, our approach relies solely on cross-sectional variation for identification and is therefore less susceptible to this sort of timing critique.

However, there is an obvious danger of endogeneity in our exercise—if the Fed was deliberately targeting securities that were underpriced, purchases may have been higher among issues whose yields rose the most during the life of the program. To control for this possibility, we use two-stage least squares. In the first stage, we instrument the LSAP purchase amounts of each security using information available before the program was announced. Specifically, our instruments are: remaining maturity and remaining maturity squared; the residuals from a yield curve estimated on March 17 using the Svensson functional form; the average of these Svensson fitting errors for each security's near substitutes; the price of each security on March 17; a dummy variable for whether the security was on the run; and a dummy variable indicating whether the security was an "old bond," meaning an originally 30-year security with less than 15 years remaining until maturity.[25] In the second-stage regression, we use instrumented purchases from the first stage as independent variables and the cumulative changes in Treasury prices as the dependent variable. We also include remaining maturity and remaining maturity squared as regressors to account for secular changes in the slope and curvature of the yield curve that were unrelated to the LSAP program, such as could have resulted from macroeconomic conditions and new Treasury issuance.

Our procedure departs from standard two-stage least-squares in two ways that deserve mention. First, as in the flow-effect regressions, we want to account for the possible effects of substitute purchases. This variable is subject to the same endogeneity concerns as own purchases, but rather than instrumenting it separately we construct substitutes by adding up the instrumented values of own purchases from the first stage. This ensures consistency across our regressors.[26] Second, we allow for different second-stage coefficients depending on security characteristics. In particular, we again divide the sample into bonds with less than 15 years of remaining maturity, bonds with more than 15 years of remaining maturity, notes more than five issues off the run, and notes zero to five issues off the run. The small number of observations makes running separate regressions on these sub-samples problematic, and, moreover, there is no particular reason to think that the remaining-maturity coefficients should vary across them. Therefore, we run a single regression but use interactive dummies to allow the coefficients on own and substitute purchases to differ across the subsamples.[27]

In summary, our two-stage system takes the form

$$Q_i = \boldsymbol{\theta}\mathbf{x}_i + u_i \tag{10}$$

$$\frac{\Delta P_i}{p_i^{Mar17}} = \alpha + \beta_k \hat{Q}_i + \gamma_k \hat{S}_i + \phi_1 m_i + \phi_2 m_i^2 + \varepsilon_i \tag{11}$$

Where p_i^{Mar17} is security i's price on the day before the LSAP program was announced, $\mathbf{x}_i$ is the vector of instruments, hats indicate instrumented values from the first stage, capital letters indicate cumulative values over the sample period of the variables we introduced in the previous section, $k = \{1, \ldots, 4\}$ indexes the security type, and m_i is the remaining maturity as of March 17. Because we are using a cross

[25] The old-bond control is potentially important because, as noted earlier, the yields on these securities differed significantly from more recently issued notes with similar remaining maturities around the time the program was announced.
[26] We do not use the mid- or far-substitute categories in the cross section because of the high degree of collinearity, especially given our inclusion of the remaining maturity variable.
[27] Note that neither of these departures from standard 2SLS changes the consistency of the usual standard-error computation, which we use below.

section, we exclude securities that matured or were issued while the program was in progress, leaving us with 148 observations.

Results

The results of the first-stage regression are shown in Table 12. The coefficients on the maturity variables suggest that purchases depended strongly on remaining maturity and, controlling for other factors, peaked around the fifteen-year sector (in percentage-of-issue terms). The yield-curve fitting errors are also highly significant, and they have a positive sign, confirming the conjecture that the Desk tended to purchase securities that were underpriced (i.e., had higher yields) than other securities with similar remaining maturities. The coefficient on the fitting errors of near substitutes is also marginally significant, suggesting that this tendency may have extended beyond individual securities to whole sectors of the term structure. Finally, as was suggested in Table 1, the Desk was more likely to purchase on-the-run than off-the-run issues. The remaining variables in our first stage regression were not individually significant, although they are jointly significant, arguing for their inclusion.[28]

The results of the second-stage regressions, with security price changes as the dependent variable, are presented in the top panel of Table 13. Own-purchase effects for both categories of bonds are large, positive, significant, and of similar magnitudes. The coefficients on near substitutes for these sub-samples, though all positive, are statistically insignificant. This suggests that, while stock effects were important among these classes of Treasuries, illiquidity or security-specific features prevented purchases from having price effects beyond the specific securities that were bought. In contrast, the results for notes indicate that the effects of near-substitute purchases were highly significant and fairly large, while the own-purchase coefficients were only marginally significant. In particular, the cross-elasticity coefficient of 0.18 for near-on-the-run notes implies that, on average, $1 billion of purchases decreased the yield on every near-substitute security by more than half a basis point. Statistically, we can reject the equality of the own and near-substitute coefficients for the bond subsamples, but not for notes.[29] These results generally support hypotheses H1 – H3.

Because the data in this exercise are cross-sectional, it is impossible to do the sorts of sub-sample robustness checks we reported for the flow-effect regressions. However, one interesting modification is to exclude the day of the announcement of the program, when yields dropped the most. Although this

[28] One interpretation of these results is that savvy market participants could have predicted with a fair degree of accuracy which securities would be purchased the most. Indeed, it appears that this occurred to some extent. For example, on the day the program was announced, yields on securities that were ultimately purchased fell by an average of 39 basis points, while yields on securities that did not end up being purchased fell by only 27 basis points, even though the announcement gave no specific indication of which securities would be purchased.

[29] Qualitatively similar, though not statistically significant, differences can be seen between the coefficients in the bond versus note and on- versus off-the-run comparisons presented for flow effects above.

event clearly represented a large stock effect from the market's point of view, our method should still be able to identify the parameters of the model without this information, assuming the specification is correct. Table 14 illustrates that, indeed, when we exclude price changes that occurred on the announcement day, our coefficient estimates are unchanged, although the standard errors are larger. In other words, while variation stemming from the announcement effect appears to have contributed important identification power, behavior during the remainder of the sample period was consistent with the price movements on that day.

Finally, Table 15 illustrates the importance of our two-stage procedure by showing what would happen if we failed to account for the endogeneity of purchases and simply regressed price changes on purchased amounts using OLS. In this case, virtually nothing is significant, and the explanatory power of the regression is much lower. Given the form of the endogeneity that we hypothesized—that the Desk bought more of the securities for which prices were lower—we would have expected that ignoring this endogeneity would indeed have pushed the coefficients toward insignificance (or even turned them negative).

To summarize the stock effects of the LSAP program, we construct a counterfactual yield curve using the results just presented. In particular, by using the actual value of purchases of each security and its near substitutes, together with the coefficients for the appropriate sub-sample, we compute the estimated amount by which the price of that security changed as a result of LSAP purchases. Subtracting this value from the actual price at the end of the program gives the counterfactual price of each security that would have obtained if the LSAPs had not occurred. These prices are then smoothed using a Svensson yield curve. The 5 percent and 95 percent confidence bands around the counterfactual prices are treated in the same way.

The results are shown, together with the actual yield curve on October 30, 2009, in Figure 3. The difference between the solid red and solid blue lines represents the stock effects of the LSAP program on the yield curve. From this picture, we can see that the effects were statistically significant over nearly the entire term structure and were largest—with a point estimate of about 50 basis points—in the 10- to 15-year sector. The reason that the biggest effects occur in this region is a combination of the relatively large coefficients on old bonds and the relatively large amount of purchases (as a fraction of outstandings) within this sector.

5. Conclusion

In this paper, we have used CUSIP-level data to estimate the flow and stock effects of the Federal Reserve's 2009 program to purchase nearly $300 billion of nominal Treasury coupon securities. We find that both types of effect were statistically and economically significant. Specifically, we estimate that the average purchase operation temporarily reduced yields by about 3.5 basis points and that the program as a whole shifted the yield curve down by up to 50 basis points. It thus seems likely that the Treasury LSAP program met the Federal Reserve's objectives of improving Treasury market liquidity and contributing to a reduction in the cost of credit.

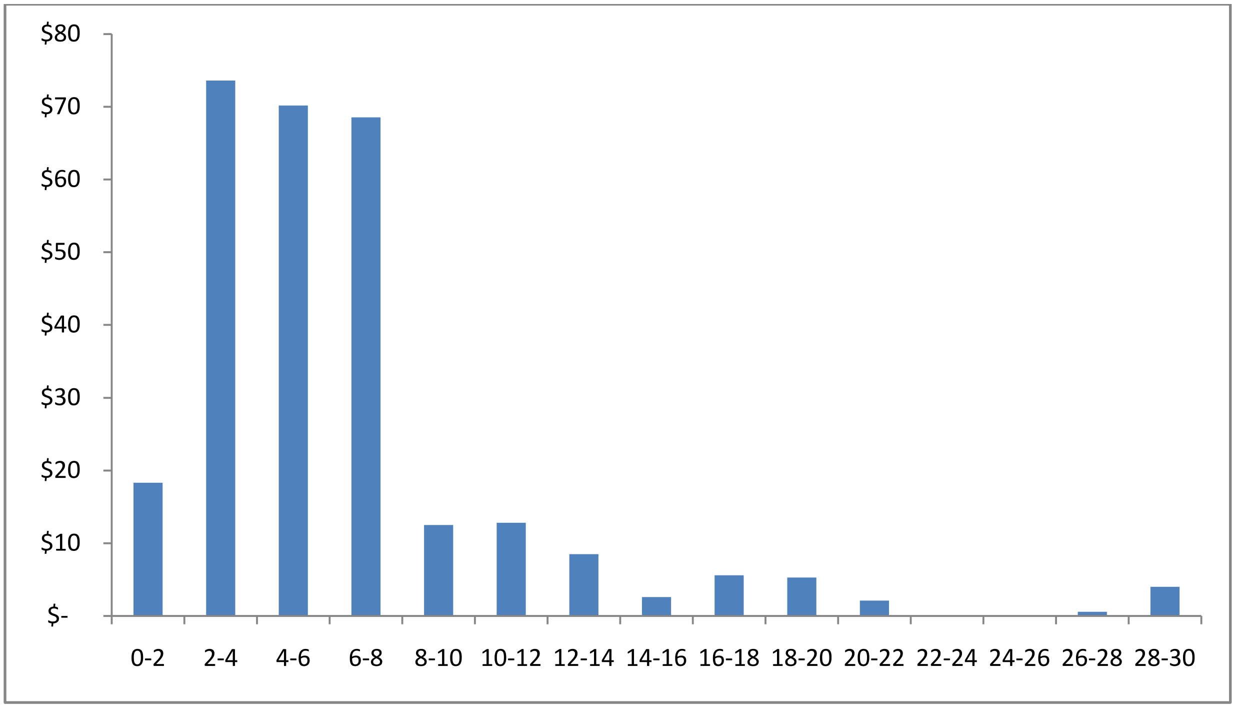
More broadly, our results provide support for preferred-habitat and portfolio-balance theories of the term structure. Consistent with such theories, we find that (1) withdraws of Treasury supply decrease yields by an economically meaningful amount; (2) these decreases are generally biggest for the specific securities being bought and for securities of similar maturities but smaller for securities with much different maturities; (3) particularly for stock effects, the discrepancies between own purchases and substitute purchases are larger in less-liquid segments of the market (off-the-run bonds); and (4) also among off-the-run bonds, the flow effects are persistent, suggesting that they are not just due to short-run rebalancing or microstructure-related distortions. None of these findings is consistent with a model (such as most standard affine term-structure models) in which all segments of the Treasury market are perfectly integrated and supply fluctuations do not affect prices.

Our study is the first to specifically treat the effects of substitute cross-elasticities, but the overall magnitudes of our stock-effect estimates (combining own and cross effects) are roughly comparable to what would be implied by Treasury price elasticities found in some previous studies, such as Kuttner (2006). As far as we are aware, no other study has estimated flow effects as we have defined them. It is perhaps surprising that these effects should be so large in most subsamples, given that most details of the purchases were announced in advance. There is certainly room for additional work to understand whether similar effects hold in other markets and in other periods and, if so, exactly what mechanisms are behind them.

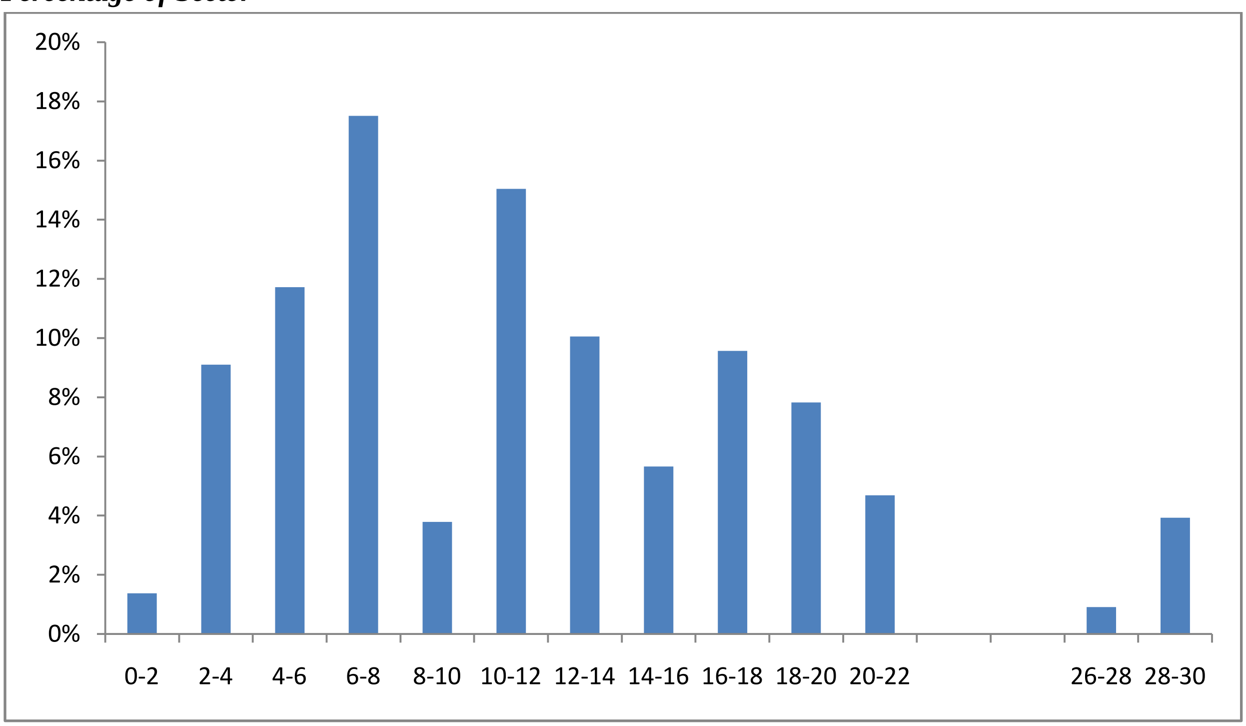
Finally, we caution that the environment that produced our data involved exceptional conditions in the Treasury market that could make extrapolation of these results to other situations problematic. We provided some evidence that our main flow-effect results did not depend greatly on the liquidity environment over the course of 2009. But we are not able to conduct a similar test for stock effects, and we still cannot rule out that other special circumstances during that period might have helped to drive the results—for example, Gurkaynak and Wright (2010) conjecture that increased risk aversion during the financial crisis could have reduced arbitrage activity and led to greater market segmentation. Testing how stock and flow effects differ across risk and liquidity regimes is an interesting area for future research and could further illuminate the factors that influence pricing and functioning in the Treasury market.

Figure 1. Maturity Distribution of Nominal Treasury LSAP Purchases

Billions of Dollars

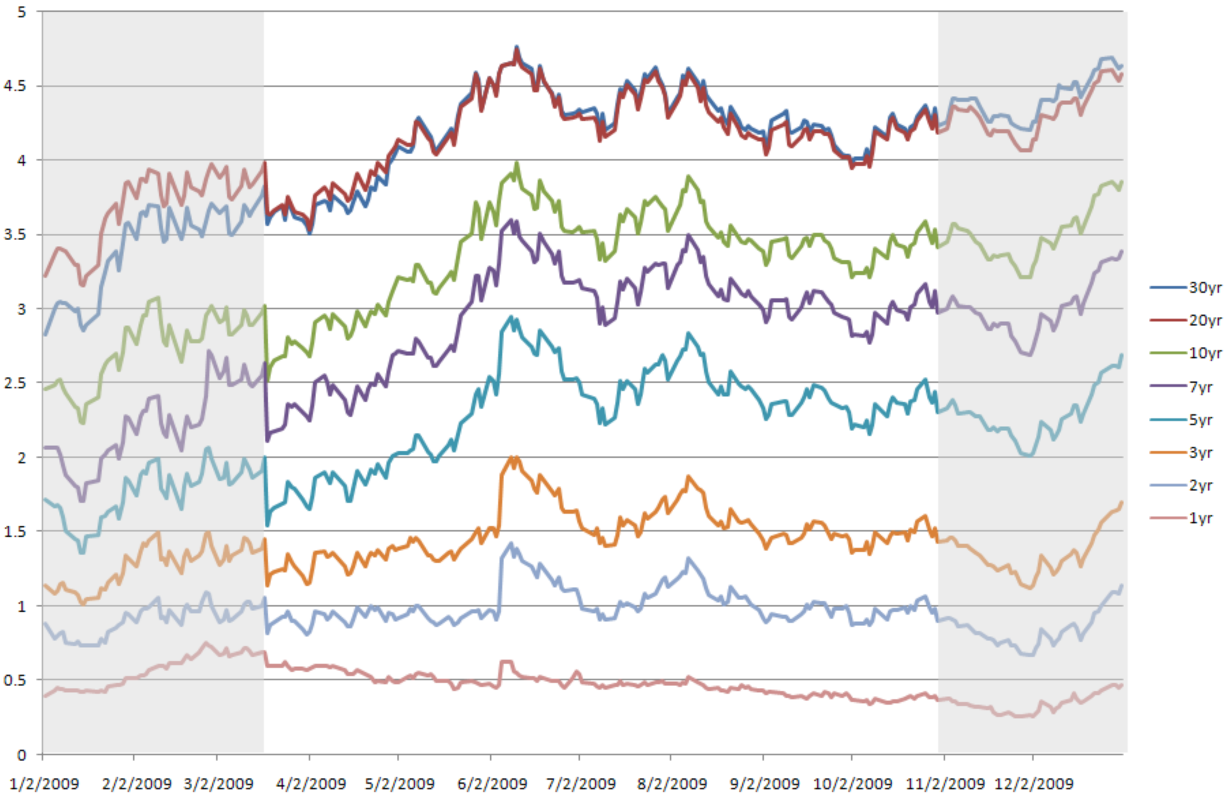


Percentage of Sector



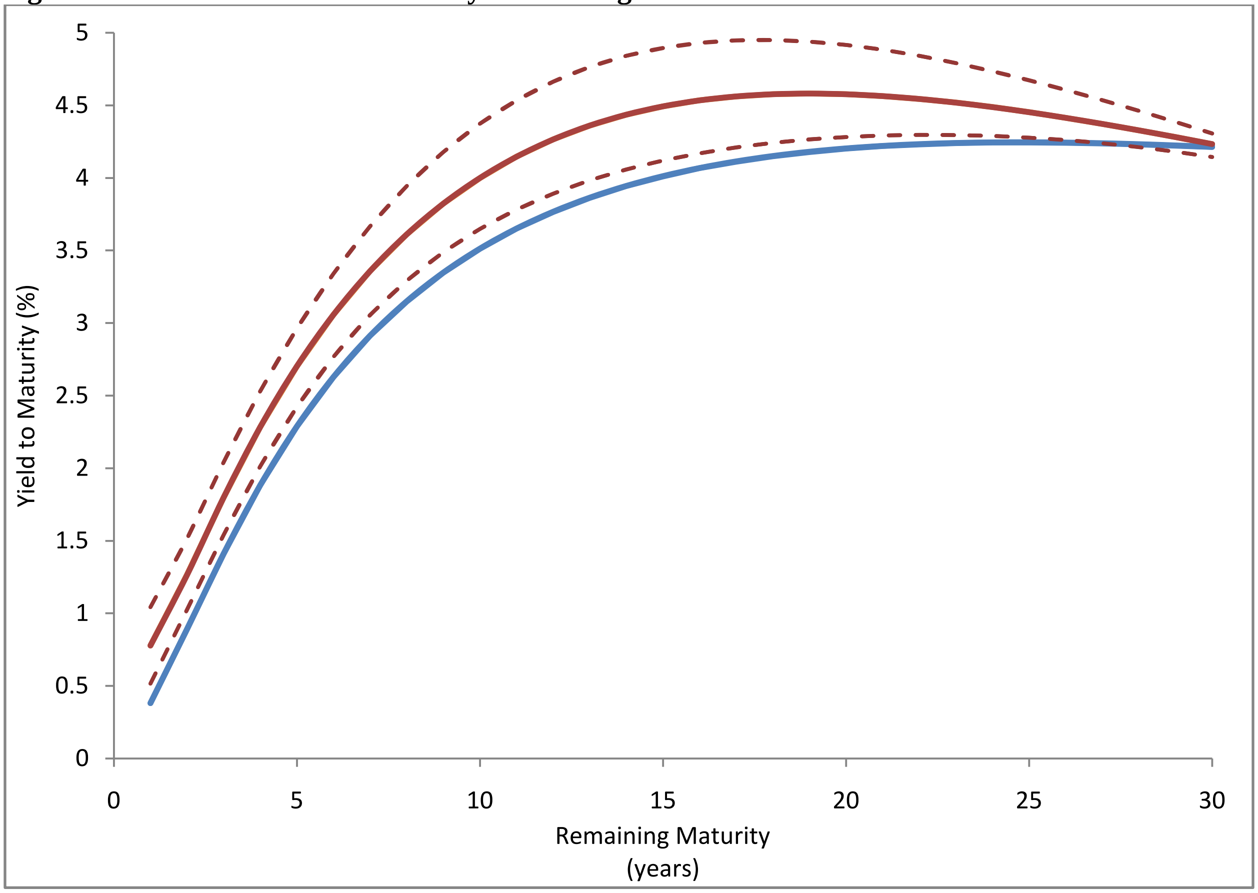
Note: Denominator is par value of outstanding securities in each sector as of July 13, 2009. No securities were outstanding with remaining maturities between 22 and 26 years.

Figure 2. Selected Treasury Yields during the Treasury LSAP Program



Notes: Yields are constant-maturity. Highlighted region shows the period (March 18 – October 30, 2009) during which the Treasury LSAP program was in effect.

Figure 3. Stock Effect of the Treasury LSAP Program on the Nominal Yield Curve



Notes: The blue line is the nominal yield curve as of October 30, 2009, based on the 148 securities in our cross section and constructed as in Svensson (1994). The solid red line is the counterfactual yield curve on that day, using the same set of Treasury prices but with the estimated effects of LSAP purchases removed according to the coefficients in Table 13. Dotted lines show Svensson curves based on 90% confidence intervals around the counterfactual prices.

Table 1. Characteristics of Nominal Treasury LSAP Purchases

	Average of LSAP purchases	Average of all outstanding coupon securities
Remaining maturity	6.5 years	5.7 years
Coupon	3.7%	3.8%
Yield	2.4%	1.9%
Time since issued	4.0 years	3.9 years
% On-the-run	29.0%	4.9%
% Notes	79.5%	82.8%

Note: All figures are dollar-weighted.

Table 2. Indicators of Liquidity in the Nominal Treasury Market

	Daily market volume	10-year on-the-run premium	Fails to deliver	Average absolute fitting errors of Svensson curve
March 25-July 6 2009	$100 bil	38 bp	$73 bil	6.4 bp
July 7-October 29 2009	$120 bil	28 bp	$15 bil	3.3 bp

Notes: Volume data come from Bloomberg, and fails-to-deliver data come from the FR 2004 reports. The on-the-run premium is the difference between the yield on the on-the-run 10-year note and 10-year value from a Svensson curve fit to off-the-run securities.

Table 3. Flow Effects on Day of Purchase—All Securities

	Eligible		Ineligible	
	<15y to maturity	>15y to maturity	<15y to maturity	>15y to maturity
Own Response (*β*)	0.2763*** (0.053)	-0.1063 (0.098)	---	---
Cross Responses (γ_j):				
Near substitutes (within 2 years)	0.2403*** (0.048)	-0.1238*** (0.044)	0.0665*** (0.018)	-0.0268 (0.053)
Mid-substitutes (2 to 6 years away)	0.1700*** (0.045)	0.0501* (0.026)	0.0047 (0.0099)	-0.007 (0.021)
Far substitutes (6 to 14 years away)	---	---	-0.0238** (0.008)	0.0021 (0.003)
# Obs.	923	145	8008	1104
# CUSIPS	146	23	181	23
Adj. R^2	0.64	0.76	0.51	0.96

Notes: The dependent variable is the daily percentage price change in each outstanding CUSIP. Only days when LSAP purchases occurred are included. Standard errors in parentheses. Asterisks indicate statistical significance at the 10 percent (*), 5 percent (**), and 1 percent (***) levels.

Table 4. Flow Effects on Day of Purchase, by Sub-Period (remaining maturity < 15 years)

	Eligible		Ineligible	
	Mar 25 – Jul 6	Jul 7 – Oct. 29	Mar 25 – Jul 6	Jul 7 – Oct. 29
Own Response (*β*)	0.3442*** (0.094)	0.2975*** (0.089)	---	---
Cross Responses (γ_j):				
Near substitutes (within 2 years)	0.2863*** (0.086)	0.3038*** (0.083)	-0.127*** (0.025)	0.3844*** (0.031)
Mid-substitutes (2 to 6 years away)	0.1989*** (0.082)	0.2037** (0.073)	-0.1426*** (0.015)	0.2021*** (0.017)
Far substitutes (6 to 14 years away)	---	---	-0.153*** (0.014)	0.0925*** (0.011)
# Obs.	563	360	4529	3479
# CUSIPS	131	121	167	172
Adj. R^2	0.42	0.27	0.38	0.53

Notes: The dependent variable is the daily percentage price change in each outstanding CUSIP. Only days when LSAP purchases occurred are included. Standard errors in parentheses. Asterisks indicate statistical significance at the 10 percent (*), 5 percent (**), and 1 percent (***) levels.

Table 5. Flow Effects on Day of Purchase, by Security Type and Sub-Period (remaining maturity < 15 years)

Notes

	Eligible		Ineligible	
	Mar 25 – Jul 6	Jul 7 – Oct. 29	Mar 25 – Jul 6	Jul 7 – Oct. 29
Own Response (*β*)	0.445*** (0.147)	0.245** (0.122)	---	---
Cross Responses (γ_j):				
Near substitutes (within 2 years)	0.396*** (0.148)	0.314*** (0.107)	-0.213*** (0.041)	0.486*** (0.049)
Mid-substitutes (2 to 6 years away)	0.346*** (0.144)	0.269*** (0.086)	-0.244*** (0.037)	0.266*** (0.038)
Far substitutes (6 to 14 years away)	---	---	-0.061*** (0.040)	0.063*** (0.039)
# Obs.	442	327	3891	2960
# CUSIPS	108	111	144	149
Adj. R^2	0.73	0.26	0.46	0.53

Bonds

	Eligible		Ineligible	
	Mar 25 – Jul 6	Jul 7 – Oct. 29	Mar 25 – Jul 6	Jul 7 – Oct. 29
Own Response (*β*)	0.381*** (0.159)	0.108 (0.129)	---	---
Cross Responses (γ_j):				
Near substitutes (within 2 years)	0.270*** (0.109)	0.011 (0.134)	-0.208*** (0.049)	0.084* (0.046)
Mid-substitutes (2 to 6 years away)	0.178* (0.104)	-0.161 (0.134)	-0.077*** (0.019)	0.074*** (0.020)
Far substitutes (6 to 14 years away)	---	----	-0.077*** (0.014)	0.041*** (0.012)
# Obs.	121	33	638	519
# CUSIPS	23	10	23	23
Adj. R^2	0.0075	0.89	0.90	0.93

Notes: The dependent variable is the daily percentage price change in each outstanding CUSIP. Only days when LSAP purchases occurred are included. Standard errors in parentheses. Asterisks indicate statistical significance at the 10 percent (*), 5 percent (**), and 1 percent (***) levels.

Table 6. Flow Effects on Day after Purchase, by Security Type and Sub-Period (remaining maturity < 15 years)

Notes

	Eligible		Ineligible	
	Mar 25 – Jul 6	Jul 7 – Oct. 29	Mar 25 – Jul 6	Jul 7 – Oct. 29
Own Response (*β*)	-0.379*** (0.143)	-0.145 (0.116)	---	---
Cross Responses (γ_j):				
Near substitutes (within 2 years)	-0.478*** (0.145)	-0.152 (0.108)	-0.464*** (0.039)	-0.135*** (0.049)
Mid-substitutes (2 to 6 years away)	-0.620*** (0.139)	-0.106 (0.087)	-0.436*** (0.035)	-0.009 (0.039)
Far substitutes (6 to 14 years away)	---	---	-0.308*** (0.039)	0.134*** (0.038)
# Obs.	442	327	3891	2960
# CUSIPS	108	111	144	149
Adj. R^2	0.59	0.04	0.46	0.51

Bonds

	Eligible		Ineligible	
	Mar 25 – Jul 6	Jul 7 – Oct. 29	Mar 25 – Jul 6	Jul 7 – Oct. 29
Own Response (*β*)	0.377*** (0.145)	0.58*** (0.117)	---	---
Cross Responses (γ_j):				
Near substitutes (within 2 years)	0.157 (0.098)	0.556*** (0.119)	-0.047 (0.046)	0.086* (0.047)
Mid-substitutes (2 to 6 years away)	0.111 (0.082)	0.472*** (0.125)	-0.057*** (0.018)	-0.046*** (0.020)
Far substitutes (6 to 14 years away)	---	---	-0.049*** (0.014)	0.015*** (0.013)
# Obs.	121	33	638	519
# CUSIPS	23	10	23	23
Adj. R^2	0.17	0.86	0.94	0.93

Notes: The dependent variable is the daily percentage price change in each outstanding CUSIP. Only days when LSAP purchases occurred are included. Standard errors in parentheses. Asterisks indicate statistical significance at the 10 percent (*), 5 percent (**), and 1 percent (***) levels.

Table 7. Robustness Check: Flow Effects on Day of Purchase, by Security Type (remaining maturity < 15 years)

	Eligible		Ineligible	
	Notes	Bonds	Notes	Bonds
Own Response (β)	0.2669*** (0.068)	0.2498*** (0.090)	---	---
Cross Responses (γ_j):				
Near substitutes (within 2 years)	0.2503*** (0.062)	0.1694** (0.083)	-0.0157 (0.027)	-0.044 (0.033)
Mid-substitutes (2 to 6 years away)	0.2088** (0.055)	0.0929 (0.080)	-0.0928*** (0.023)	0.0078 (0.013)
Far substitutes (6 to 14 years away)	---	---	-0.0249 (0.025)	-0.0074 (0.008)
# Obs.	769	154	6851	1157
# CUSIPS	123	23	158	23
Adj. R^2	0.75	0.83	0.49	0.94

Notes: The dependent variable is the daily percentage price change in each outstanding CUSIP. Only days when LSAP purchases occurred are included. The sample is limited to securities with less than 15 years of remaining maturity. Standard errors in parentheses. Asterisks indicate statistical significance at the 10 percent (*), 5 percent (**), and 1 percent (***) levels.

Table 8. Robustness Check: Flow Effects on Day of Purchase, by Security Vintage (remaining maturity < 15 years)

	Eligible		Ineligible	
	Near on-the-run	Far off-the-run	Near on-the-run	Far off-the-run
Own Response (β)	0.2318** (0.107)	0.2488*** (0.065)	---	---
Cross Responses (γ_j):				
Near substitutes (within 2 years)	0.2435** (0.105)	0.1584*** (0.057)	-0.0578 (0.058)	0.0811*** (0.020)
Mid-substitutes (2 to 6 years away)	0.2501*** (0.092)	0.0744 (0.055)	-0.0939** (0.045)	0.0141 (0.010)
Far substitutes (6 to 14 years away)	---	---	-0.0403 (0.046)	-0.0238*** (0.008)
# Obs.	249	674	1463	6545
# CUSIPS	53	114	58	151
Adj. R^2	0.69	0.47	0.70	0.47

Notes: The dependent variable is the daily percentage price change in each outstanding CUSIP. Only days when LSAP purchases occurred are included. The sample is limited to securities with less than 15 years of remaining maturity. Standard errors in parentheses. Asterisks indicate statistical significance at the 10 percent (*), 5 percent (**), and 1 percent (***) levels.

Table 9. Robustness Check: Flow Effects on Day of Purchase, by Security Type (far-off-the-run securities with remaining maturity < 15 years)

	Eligible		Ineligible	
	Notes	Bonds	Notes	Bonds
Own Response (*β*)	0.3041** (0.137)	0.2498** (0.090)	---	---
Cross Responses (γ_j):				
Near substitutes (within 2 years)	0.2131** (0.104)	0.1694** (0.083)	-0.0207 (0.032)	-0.0441 (0.033)
Mid-substitutes (2 to 6 years away)	-0.0228 (0.098)	0.0929 (0.080)	-0.0891** (0.029)	0.0078 (0.013)
Far substitutes (6 to 14 years away)	---	---	-0.0273 (0.038)	-0.0075 (0.009)
# Obs.	520	154	5388	1157
# CUSIPS	91	23	128	23
Adj. R^2	0.43	0.39	0.46	0.94

Notes: The dependent variable is the daily percentage price change in each outstanding CUSIP. Only days when LSAP purchases occurred are included. The sample is limited to securities with less than 15 years of remaining maturity and more than 5 issues off the run. Standard errors in parentheses. Asterisks indicate statistical significance at the 10 percent (*), 5 percent (**), and 1 percent (***) levels.

Table 10. Robustness Check: Flow Effects on Day of Purchase, All Securities (Clustered Standard Errors)

	Eligible		Ineligible	
	<15y to maturity	>15y to maturity	<15y to maturity	>15y to maturity
Own Response (*β*)	0.2763*** (0.044)	-0.1063 (0.089)	---	---
Cross Responses (γ_j):				
Near substitutes (within 2 years)	0.2403*** (0.038)	-0.1238*** (0.029)	0.0665*** (0.029)	-0.0268 (0.017)
Mid-substitutes (2 to 6 years away)	0.1700*** (0.053)	0.0501** (0.014)	0.0047 (0.016)	-0.007 (0.037)
Far substitutes (6 to 14 years away)	---	---	-0.0238** (0.009)	0.0021 (0.003)
# Obs.	923	145	8008	1104
# CUSIPS	146	23	181	23
Adj. R^2	0.71	0.76	0.51	0.96

Notes: The dependent variable is the daily percentage price change in each outstanding CUSIP. Only days when LSAP purchases occurred are included. Standard errors in parentheses. Asterisks indicate statistical significance at the 10 percent (*), 5 percent (**), and 1 percent (***) levels.

Table 11. Robustness Check: Flow Effects on Day of Purchase, by Security Type and Vintage (Clustered Standard Errors; eligible securities with remaining maturity < 15 years)

	Bonds	Notes	Near on-the-run	Far off-the-run
Own Response (*β*)	0.2498** (0.06)	0.2669*** (0.058)	0.2318** (0.098)	0.2488*** (0.051)
Cross Responses (γ_j):				
Near substitutes (within 2 years)	0.1694** (0.03)	0.2503*** (0.051)	0.2435** (0.073)	0.1584*** (0.029)
Mid-substitutes (2 to 6 years away)	0.0929 (0.035)	0.2088** (0.059)	0.2501*** (0.059)	0.0744 (0.026)
Far substitutes (6 to 14 years away)	---	---	---	---
# Obs.	154	769	249	674
# CUSIPS	23	123	53	114
Adj. R^2	0.39	0.75	0.69	0.68

Table 12. First Stage IV Regression for LSAP Purchases

Intercept	0.0020 (0.0052)
Remaining maturity	0.0022*** (0.0003)
Remaining maturity squared	-0.00007*** (0.00001)
Svensson fitting error	0.012*** (0.0041)
Average near-substitute fitting error	0.015* (0.0078)
Price	-0.00007 (0.00005)
On-the-run dummy	0.0038** (0.0016)
"Old bond" dummy	0.0018 (0.0016)
# Obs	148
Adjusted R^2	0.496

Notes: The dependent variable is the total amount of each security purchased under the LSAP program, normalized by the total amount of Treasuries outstanding with remaining maturities within two years. All independent variables are as of March 17, 2009. Standard errors in parentheses. Asterisks indicate statistical significance at the 10 percent (*), 5 percent (**), and 1 percent (***) levels.

Table 13. Stock Effects—Instrumental Variables

	Bonds < 15 years	Bonds > 15 years	Notes Far off-the-run	Notes Near-on the-run
Own Response	4.72*** (1.16)	4.16** (1.98)	3.51* (2.01)	2.17 (1.51)
Cross Response Near substitutes (within 2 years)	0.05 (0.09)	0.04 (0.23)	0.10** (0.05)	0.18*** (0.07)
Remaining maturity	-0.006*** (0.002)			
Remaining maturity squared	0.0001*** (0.00005)			
Intercept	-0.0025* (0.0013)			
# Obs	148			
Adj. R^2	0.816			

Notes: The dependent variable is the cumulative percentage price change from March 17 to October 30, 2009. Regression is 2SLS with March 17-dated variables used as instruments. All purchase variables are normalized by the total quantity of near substitutes outstanding. Asterisks indicate statistical significance at the 10 percent (*), 5 percent (**), and 1 percent (***) levels.

Table 14. Stock Effects, Excluding Announcement Date

	Bonds < 15 years	Bonds > 15 years	Notes Far off-the-run	Notes Near-on the-run
Own Response	4.97** (2.15)	4.00 (4.22)	4.43 (4.52)	1.03 (2.76)
Cross Responses Near substitutes (within 2 years)	0.05 (0.17)	0.23 (0.48)	0.03 (0.13)	0.17 (0.16)
Remaining maturity	-0.012*** (0.003)			
Remaining maturity squared	0.00027*** (0.00009)			
Intercept	0.0058*** (0.0019)			
# Obs	148			
Adj. R^2	0.93			

Notes: The dependent variable is the cumulative percentage price change from March 18 to October 30, 2009. Regression is 2SLS with March 17-dated variables used as instruments. All purchase variables are normalized by the total quantity of near substitutes outstanding. Standard errors in parentheses. Asterisks indicate statistical significance at the 10 percent (*), 5 percent (**), and 1 percent (***) levels.

Table 15. Stock Effects without Accounting for Endogeneity—Ordinary Least Squares

	Bonds < 15 years	Bonds > 15 years	Notes Far off-the-run	Notes Near-on the-run
Own Response	0.77** (0.30)	0.28 (0.29)	-0.50 (0.59)	0.94 (0.57)
Cross Responses Near substitutes (within 2 years)	0.0004 (0.006)	-0.009 (0.086)	0.0009 (0.051)	0.009 (0.063)
Remaining maturity	-0.0009 (0.0009)			
Remaining maturity squared	-0.00002 (0.00003)			
Intercept	-0.007*** (0.002)			
# Obs	148			
Adj. R^2	0.62			

Notes: The dependent variable is the cumulative percentage price change from March 17 to October 30, 2009. Regressio is OLS. All purchase variables are normalized by the total quantity of near substitutes outstanding. Standard errors in parentheses. Asterisks indicate statistical significance at the 10 percent (*), 5 percent (**), and 1 percent (***) levels.

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