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Flow and stock effects of large-scale treasury purchases: Evidence on the importance of local supply ☆

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ABSTRACT

The Federal Reserve's 2009 program to purchase $300 billion of US Treasury securities represented an unprecedented intervention in the Treasury market and provides a natural experiment with the potential to shed light on the price elasticities of Treasuries and theories of supply effects in the term structure. Using security-level data on Treasury prices and quantities during the course of this program, we document a 'local supply' effect in the yield curve—yields within a particular maturity sector responded more to changes in the amounts outstanding in that sector than to similar changes in other sectors. We find that this phenomenon was responsible for a persistent downward shift in yields averaging about 30 basis points over the course of the program (the "stock effect"). In addition, except at very long maturities, purchase operations caused an average decline in yields in the sector purchased of 3.5 basis points on the days when those operations occurred (the "flow effect"). The sensitivity of our results to security characteristics generally supports a view of segmentation or imperfect substitution within the Treasury market during this time.

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1. Introduction

Do fluctuations in the supply of government debt affect Treasury yields? This possibility is generally ruled out under the expectations hypothesis and canonical arbitrage-free models of the term structure, but it can arise in models that account for imperfect asset substitutability or preferred-habitat investors. Theories consistent with these notions have existed informally for decades (e.g., Culbertson, 1957; Modigliani and Sutch, 1966), and they have recently received greater attention as researchers have begun to supply them with rigorous foundations, as in the models of Andres, Lopez-Salido, and Nelson (2004) and Vayanos and Vila (2009). Evaluating the significance of these mechanisms has the potential to inform modeling of the determination of bond and other asset prices. It is also important for a variety of policy issues, including the conduct of open market operations by central banks and the structure of debt issuance by governments.

We provide evidence on the response of the US Treasury yield curve to the relative supply of Treasury securities by exploiting the natural experiment of the Federal Reserve's

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first round of Large Scale Asset Purchases (LSAPs) in 2009. Using new identification and estimation procedures based on security-level price and quantity data, we document what might be viewed as a relative-price anomaly in the Treasury market during this period, in the spirit of the evolving literature on market segmentation and supply shocks. In particular, we estimate a significant "local-supply" effect in the Treasury term structure: the yield on a given security fell in response to purchases of that security and securities of similar maturity. This response is conceptually distinct from—and more suggestive of market segmentation than—other mechanisms by which asset purchases might shift the yield curve, such as by changing the expected path of policy rates and inflation or changing the aggregate duration risk that market participants must bear. The $300 billion Treasury LSAP program, announced and implemented in the immediate aftermath of the financial crisis, is an ideal testing ground for a local-supply channel, not just because it represented a large (and largely unexpected) exogenous shock to the available Treasury supply, but also because it took place during a period of heightened risk aversion, which is precisely when the Vayanos-Vila (2009) theory predicts such a channel might be most operative. The security-level data allow us to examine how the scale of the local-supply effect varied across security characteristics such as maturity and liquidity and to gauge the degree of substitution across securities by estimating the cross-elasticities of their prices.

Within this framework, we distinguish two ways in which asset purchases might operate—through stock effects and through flow effects. "Stock effects" are defined as persistent changes in prices that result from movements along Treasury demand curves. To estimate stock effects, we model the cumulative change in each CUSIP's price between March 17, 2009 and October 30, 2009 (i.e., the cross-section of Treasury returns) as a function of the total amount that the Fed purchased of that CUSIP and its potential substitutes.[1] Because, over the life of the program, purchased amounts could have responded endogenously to price changes, we instrument these LSAP amounts with the purchased securities' characteristics prior to the announcement of the program. 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 local supply channel had not been present. Meanwhile, "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 that lead to sluggish price discovery. 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 on those days. This exercise is similar to the study of Brandt and Kavajecz (2004) on the response of yields to order-flow imbalances.

Our results suggest that, through the local-supply channel, the Fed's 2009 Treasury purchases reduced yields by an average of about 30 basis points over the life of the program (the stock effect) and led to a further 3 to 4 basis point decline in purchased sectors on the days when purchases occurred (the flow effect). We find that the stock effects were driven largely by the responses of less liquid securities, such as those that were several issues off the run. 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, which is consistent with the stock effect being mainly driven by this category of assets.[2] 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 the second half of the sample, by which time Treasury market conditions had substantially improved.

Both the stock- and flow-effect results provide support for preferred-habitat theories, as they demonstrate that Treasury rates at a given maturity can be sensitive to the amount of privately held Treasury debt available around that maturity. Our results further indicate that, on the days when a security was eligible to be bought, purchases of securities with similar maturities had 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 imperfect substitutability across the term structure. This set of results is also consistent with a series of papers, including Greenwood (2005), Gabaix, Krishnamurthy, and Vigneron (2007), Garleanu, Pedersen, and Poteshman (2009), Vayanos and Vila (2009), and Greenwood and Vayanos (2010b), where arbitrageurs transmit demand shocks for one asset to other assets, with the effects being the largest for assets that covary the most with the original asset—that is, for close substitutes. In addition, we find that certain types of Treasury securities exhibit greater evidence of segmentation, which is also supportive of preferred-habitat theories. For example, we generally reject equality of the own- and cross-elasticities in far-off-the-run Treasuries, suggesting that limits to arbitrage may play an even greater role among those securities.

Our paper fits within a growing literature studying the relations between Treasury prices and quantities, including Bernanke, Reinhart, and Sack (2004), Engen and Hubbard (2005), Han, Longstaff, and Merrill (2007), Krishnamurthy and Vissing-Jorgensen (2007), Greenwood and Vayanos (2010a, b), and Hamilton and Wu (2012). Much of this literature has relied on time-series studies of

[1] A CUSIP is a unique identifying number for each security issued.

[2] These results are consistent with Brandt and Kavajecz (2004), who show that the relation between yields and order flow is stronger when they condition their analysis on liquidity of the Treasury security. In particular, the effect is larger and permanent when liquidity is low.

constant-maturity yields and aggregate characteristics of Treasury debt. Our panel-data approach offers a number of advantages over these methods. As noted above, the panel data allow us to get a granular picture of how supply effects differ across different types of securities and to estimate cross-elasticities of individual Treasuries with respect to their potential substitutes, procedures that are not generally feasible with aggregate data. In addition, the results of time-series studies may be affected by endogeneity problems typical of any estimated relations between prices and quantities—indeed, these problems likely became more severe during the LSAP period as the Fed may have attempted to purchase securities that it viewed as underpriced. Security-level data allow us to build instrumental variables to address this endogeneity. Finally, analysis based on the aggregate characteristics of Treasury debt outstanding is not equipped to separate *local* supply effects from other mechanisms through which a change in supply may affect yields. By employing the prices of multiple securities and controlling for changes in the overall shape of the yield curve, we are able to isolate the local price responses to changes in the supply within specific, narrow maturity sectors.

The following section of the paper discusses the theory and notation behind our tests and positions our work within the existing theoretical and empirical literature. Section 3 gives an overview of our data. Section 4 develops our general empirical specification and presents our results, with Section 4.2 considering stock effects, and Section 4.3 considering flow effects. Section 5 offers a comparison with subsequent Fed programs. Section 6 concludes.

2. Theory and evidence on the effects of Treasury supply

In this paper, we ask whether, during the first LSAP program, changes in the stock of Treasuries affected the yields on Treasuries in the specific sectors where purchases occurred—a possibility that we term the "local supply" effect. A number of previous studies (most explicitly, Greenwood and Vayanos, 2010b) have argued that Treasury supply may affect the term structure by changing the total quantity of duration risk that arbitrageurs must hold—when debt in public hands increases or shifts toward longer maturities, market participants are more exposed to shifts in interest rates and require higher premiums to bear this extra risk. This result, which we call the "duration effect," is distinct from the local-supply effect. The latter reflects relatively isolated movements within particular sectors of the yield curve, abstracting from any changes in the broader term structure that might have to do with duration exposure.

The local-supply effect falls within the category of relative-price anomalies of closely related assets, which, as pointed out in Gromb and Vayanos (2010), have only been documented sporadically, in part due to the rarity of natural experiments involving asset pairs with closely related payoffs. LSAPs provide precisely this type of natural experiment for the cross-section of Treasury securities.[3] Gromb and Vayanos (2010) also note that these anomalies are difficult to reconcile with standard asset-pricing models. Indeed, most of the arbitrage-free models of the term structure of interest rates that have become common in the finance literature (e.g., Cox, Ingersoll, and Ross, 1985) do not generally allow for effects of bond supply on interest rates, through either a duration channel or a local-supply channel.

On the other hand, the literature on the limits of arbitrage and preferred habitat has recently provided rigorous models that hold promise for explaining these anomalies. In particular, the theory that helps us to motivate the design of our tests is provided by Vayanos and Vila (2009) (V&V), as this is to the best of our knowledge the only formal model providing a mechanism by which demand and supply factors may affect yields locally. In the V&V theory, preferred-habitat investors have exogenously given demand curves for securities of each maturity, and they do not trade across different maturities. Meanwhile, arbitrageurs do trade across different maturities and render the term structure arbitrage-free in equilibrium by buying securities that are in low demand and selling those that are in high demand, but risk aversion prevents them from engaging in this process until expected returns are equated across securities. Thus, exogenous shocks to preferred-habitat demand can have effects on prices.

To be concrete, suppose that there are N distinct Treasury securities outstanding, indexed by $n=1,\ldots, N$. Let $\tau_{n,t}$ be the remaining maturity of security n at time t and $r_{n,t}$ be its yield to maturity. Let the outstanding stock of Treasury debt available to arbitrageurs be characterized by K "demand factors," $\boldsymbol{\beta}_t=(\beta_{1,t} \ldots \beta_{K,t})$, and $\boldsymbol{\theta}_k=(\theta_k(1) \ldots \theta_k(N))$ be the vector of loadings determining how the kth factor maps into the quantity of each security that arbitrageurs must hold.[4] V&V assume that each security's demand-factor loadings depend only on its maturity, i.e., $\theta_k(n)=\theta_k(m)$ whenever $\tau_{n,t}=\tau_{m,t}$.[5] In this case, their model generates a solution of the form

$$r_{n,t}=A_r(\tau_{n,t})r_t^0+\mathbf{A}_{\boldsymbol{\beta}}(\tau_{n,t})\boldsymbol{\beta}_t+C(\tau_{n,t}), \tag{1}$$

where r_t^0 is the short-term interest rate (assumed to follow an exogenous process), $A_r(\tau_{n,t})$ is the sensitivity of maturity-τ yield to short-rate fluctuations, and $\mathbf{A}_{\boldsymbol{\beta}}(\tau_{n,t})=(\mathbf{A}_{\beta(1)}(\tau_{n,t}), \ldots, \mathbf{A}_{\beta(K)}(\tau_{n,t}))$ is the sensitivity of the maturity-τ yield to each of the demand factors.

V&V examine two polar cases of this model. In one extreme, when arbitrageurs are nearly risk neutral, the equilibrium value of each $\mathbf{A}_{\beta(k)}(\tau_{n,t})$ does not depend on

[3] Other examples of such anomalies in the Treasury market include those documented by Amihud and Mendelson (1991), Krishnamurthy (2002), Longstaff (2004), and Musto, Nini, and Schwarz (2011).

[4] Where possible, our notation and terminology follows V&V. In their model, the demand shocks enter by shifting the demand curves of preferred-habitat investors, which are downward sloping in the yield for each security $r_{n,t}$. Here, we focus on shocks to the overall supply that the government leaves in the hands of the public, which can be viewed as a special case. This interpretation is similar to that of Kaminska, Vayanos, and Zinna (2011).

[5] In the V&V model securities are indexed only by their maturity (τ), whereas we index by specific security (n) to allow different securities with the same maturity to have different prices, anticipating the structure of our data as we will show in the next section.

any of the particular values of the factor loadings $\theta_k(n)$, and it is mainly determined by a term that can be interpreted as the average duration of the arbitrageurs' portfolio.[6] Consequently, demand shocks have effects on the entire term structure, including maturities that are distant from the particular sectors hit by those shocks. In the other extreme, when risk aversion is close to infinity and K is large, V&V suggest that the effects of a demand shock on the yields at maturity τ may depend on how the shock affects quantities at that maturity—that is, $A_{\beta(k)}(\tau_{n,t})$ is a function only of $\theta_k(n)$. Since each $\theta_k(n)$ itself is completely unrestricted (for example, it may be discontinuous across $\tau_{n,t}$), this means that demand shocks would have effects on yields that are local to particular maturities and would not, apart from these effects, change the term structure more broadly. Between these two extremes, the solution for $\mathbf{A}_{\boldsymbol{\beta}}(\tau_{n,t})$ is complicated and intractable. However, as suggested by V&V's numerical investigations, we conjecture that $\mathbf{A}_{\boldsymbol{\beta}}(\tau_{n,t})$ can be represented as a convex combination of the risk-neutral and infinitely risk-averse outcomes. Therefore, we approximate $\mathbf{A}_{\boldsymbol{\beta}}(\tau_{n,t})$ as the combination of two distinct effects:

$$A_{\beta,k}(\tau_{n,t}) \approx D_k(\tau_{n,t}) + L_n(\boldsymbol{\theta}_k). \tag{2}$$

D_k is the *duration effect*—the impact that the kth demand shock has on yields by changing the total duration risk that arbitrageurs are forced to bear—which depends only on a security's maturity. L_n is the *local-supply* effect that we are interested in. Although its shape and magnitude are determined by the model parameters, it generally depends on a subset of (or potentially all of) the securities' exposures to the kth demand factor. This is because, for a given demand shock, these exposures determine the changes in quantities outstanding of each security, and possible substitution effects can cause the change in quantity of one security to affect the price of another—i.e., as long as some arbitrage takes place, a demand shock at maturity τ will generally have a nonzero local-supply effect at a different maturity.

The distinction between duration effects and local-supply effects is important because yields can depend on the duration of investors' portfolios even when asset prices are determined solely by the behavior of arbitrageurs. For example, V&V also present a one-factor model, in which preferred-habitat demand is constant and, consequently, $r_{n,t}$ is an affine function only of the short rate. Yet, even in this case, $r_{n,t}$ still depends on the quantity of securities that are left in the hands of the arbitrageurs—the more supply the arbitrageurs are required to bear, in equilibrium, the greater the expected return they demand. Thus, finding a nonzero response of yields to supply shocks is not necessarily evidence of preferred habitat per se. A more stringent test of the preferred-habitat hypothesis is whether local effects L_n are nonzero. Our tests below isolate local-supply effects, using LSAP purchases to identify exogenous shifts in the supply of each security available to investors.

While no study to date has specifically estimated the V&V model using observed data on Treasury supply fluctuations, Greenwood and Vayanos (2010b) have tested a number of qualitative hypotheses generated by V&V, and Kaminska, Vayanos, and Zinna (2011) have recently produced an estimated version of that model using unobserved factors and Treasury Inflation-Protected Securities (TIPS) prices. In both cases, the model fares well in explaining the aggregate pattern of Treasury yields. Yet neither of these studies examines the effects during the most recent financial crisis or any other period of heightened risk aversion. Indeed, Greenwood and Vayanos (2010b) formulate all of their hypotheses in terms of the *low*-risk-aversion case—thus, they are essentially testing only for the presence of D_k. Kaminska, Vayanos, and Zinna (2011), who include data through 2009 in their sample, note that their model generates large fitting errors during the crisis period and suggest specifically that this may be due to their assumption of constant risk aversion.

In addition, a number of studies (Bernanke, Reinhart, and Sack, 2004; Greenwood and Vayanos, 2010b; Hamilton and Wu, 2012) find that various characteristics of the aggregate supply of Treasury debt are indeed correlated with the time-series behavior of the yield curve. In most of these specifications, quantity shocks affect the term structure through essentially one supply factor proxied by some aggregate characteristic of the outstanding Treasury debt, such as average maturity. These studies have also typically restricted attention to rates at just one or a few points on the yield curve. Despite the econometric challenges with regard to the possible endogeneity and persistence of the supply variables noted in the introduction, collectively—and together with anecdotal evidence on particular episodes—these studies suggest that supply plays a role. Still, it is difficult to distinguish the two components of $\mathbf{A}_{\boldsymbol{\beta}}(\tau_{n,t})$ using aggregate data alone. Thus, such studies cannot isolate local-supply effects.

Another type of evidence comes from event studies of particular episodes that have involved relatively large or rapid changes in Treasury supply (see, for example, Gagnon, Raskin, Remache, and Sack, 2010). Our analysis of the first LSAP program fits within this tradition of exploiting "natural experiments," although with a different econometric methodology. One such episode 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. Although contemporaneous studies (Modigliani and Sutch, 1966; Ross, 1966; Holland, 1969) suggested that this program had little overall impact, a more recent reexamination by Swanson (2011) finds statistically significant effects. Another interesting case is the Treasury Department's initiative to repurchase its long-term debt securities in 2000, which was similar to the Treasury LSAP program in mechanics, if not in scale. Bernanke, Reinhart, and Sack (2004), Longstaff (2004), and Greenwood and Vayanos (2010a) have argued that this program lowered long-term yields.[7] While the results of these case studies are suggestive

[6] For more detailed intuition about this point, see Cochrane (2008).

[7] Under this program the government bought back \$67.5 billion of bonds, 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. Importantly, however, in contrast to

of local-supply effects' ability to rotate the yield curve, this type of finding could also be due to the effects of removing duration from the market.

In summary, although the existing evidence seems consistent with the possibility that changes in Treasury supply affect yields, it is difficult to get a precise idea of the magnitude of these effects, whether they reflect preferred habitat (in the sense of having local effects), or of how they vary with security characteristics because most previous analysis has been 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), endogeneity, substitutability across the term structure, and specific security characteristics.

Table 1
Characteristics of nominal Treasury LSAP purchases vs. universe of Treasury coupons.

The table shows sample averages for securities that were bought by the Federal Reserve under the LSAP program between March 18 and October 31, 2009, relative to the universe of outstanding Treasury coupon securities. All figures are dollar-weighted averages over the period March 18–October 29, 2009.

	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%

3. A first look at the data

Our empirical work attempts to test for local-supply effects—that is, to characterize the functions L_n that determine how shocks to quantities translate into changes in yields on the securities that are directly affected by these shocks and in yields on nearby securities—using as a natural experiment the program to purchase up to \$300 billion of Treasury coupon securities that the Federal Open Market Committee (FOMC) announced on March 18, 2009.[8] Our data consist of daily observations on the universe of outstanding nominal Treasury coupon securities from March 17 through October 30, 2009 (the day after the purchases were concluded). To simplify the analysis, we exclude TIPS and securities with remaining maturities of less than 90 days, leaving us with an unbalanced panel of 204 CUSIPs. Our primary variables of interest are the percentage price changes in each of these securities, measured at end-of-day, and the face values of the security-level quantities outstanding and amounts purchased under the LSAP program.[9]

This section details some characteristics of the overall data. A variety of cursory observations here further motivate our more rigorous search for local-supply effects in the following section. First, on the announcement of the program on March 18, 2009, yields on securities that were subsequently bought fell by more than those that were not, regardless of their exposure to duration risk. Second, analysis of the time series of yields shows that the usual level, slope, and curvature factors do not adequately explain the behavior of yields over this period. Finally, we also discuss indicators of Treasury market functioning around this time, which show that, following the Fed purchases, the liquidity in the cash market improved and the repurchase market activity was not adversely impacted. These features of the Treasury markets are instrumental in better understanding some of the results that will be presented in the next sections.

3.1. Purchased securities

The characteristics of the purchased securities are summarized in Table 1 and Fig. 1. Overall, purchases of nominal securities under the program included 160 unique CUSIPs, spanning remaining maturities of about two to 30 years. \$300 billion represented about 3% of the total stock of outstanding Treasury debt and about 8% of the outstanding coupon securities as of the time of the announcement. Most purchases were concentrated in the two- to seven-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 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%) of purchases were on-the-run issues, which generally have lower yields. This suggests that, consistent with contemporary commentary, the Fed deliberately purchased securities that were underpriced, a claim that we will illustrate more formally below.

3.2. The announcement effect by CUSIP

While a number of studies have previously examined the effects of LSAP announcements on constant maturity Treasury yields and other asset prices, none has employed data at CUSIP level. The bottom panel of Fig. 2 shows the changes in yields on March 18, 2009 for all the available CUSIPs ordered by remaining maturity (shown on the horizontal axis). After the initial announcement, medium- to long-dated yields fell dramatically, with the largest reactions in the three- to 15-year maturity range, where the yields declined more than 40 basis points, with yields in the

(*footnote continued*)
the LSAP program, the Treasury specifically attempted to *minimize* the effects of operations on market prices, since increases in prices would have driven up the cost of the purchases.

[8] We examine only the initial round of Treasury purchases announced and conducted in 2009, not the second round (the so-called QE2) that commenced in November 2010, or the Maturity Extension Program announced in September 2011. Meaning and Zhu (2011) have recently applied our methodology to other Fed LSAPs. See Section 5 for a further discussion.

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

Table 2
Indicators of liquidity in the nominal Treasury market.
The table shows sample averages of various Treasury-market liquidity statistics (averages of daily values) over the first and second half of the period during which the Treasury LSAP program took place. 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 ten-year note and ten-year yield from a Svensson curve fit to off-the-run securities.

	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	39 bp	$73 bil	6.4 bp
July 7–October 29 2009	$120 bil	29 bp	$15 bil	3.3 bp

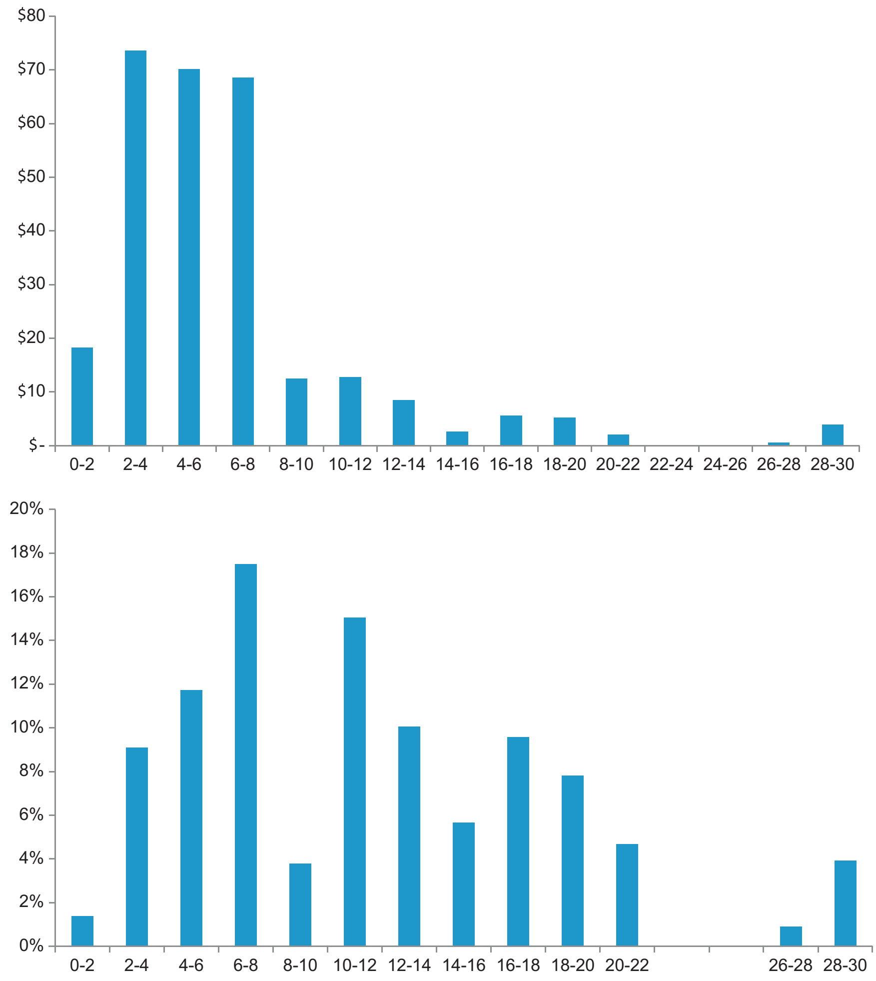

Fig. 1. Maturity distribution of nominal Treasury LSAP purchases. In the top panel the vertical axis is the amount purchased in billions of dollars and the horizontal axis shows the maturity sectors. In the bottom panel the vertical axis shows the percentage of the maturity sector purchased, which is measured by 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.

seven-year sector declining twice as much as those in the 30-year sector.[10] This indicates that the impact of the announcement was not entirely driven by the exposure to duration risk, but was strongly affected by expectations about changes in local supply. This can be seen by comparing the yield changes on March 18 to the maturity distribution of the Fed's subsequent purchases (as percentage of the outstandings), which are shown in the top portion of the panel. The sectors in which yields fell the most on the announcement day match those in which the Fed later purchased the most, regardless of duration.

[10] On March 18, following the FOMC announcement, the Open Market Desk at the New York Fed announced that it "will concentrate purchases in the 2- to 10-year sector of the nominal Treasury curve, although purchases will occur across the nominal Treasury and TIPS yield curves," but it did not provide any other specific information on which securities would be purchased.

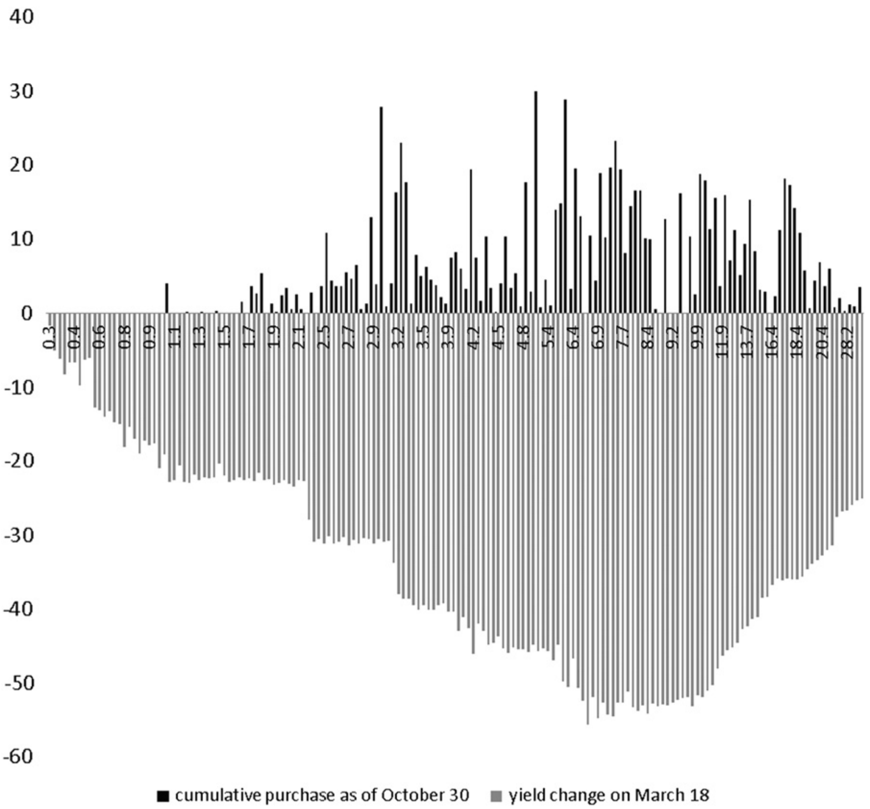

Fig. 2. The announcement effect on yields versus subsequent Treasury LSAP purchases. The top panel shows the cumulative amount of each security purchased under the Treasury LSAP program between March 18 and October 29, 2009, as a percentage of the total amount of that security oustanding. The bottom panel shows the change in the yield on the corresponding security on March 18 (the day the program was announced). Securities in the chart are indexed by their remaining maturity at the time of the announcement, shown on the horizontal axis.

Table 3
March 18 price changes as a function of pre-LSAP security characteristics.

The table shows regression results accounting for changes in individual Treasury securities prices on the day the LSAP program was announced. The sample consists of all Treasury coupon securities that were outstanding with a remaining maturity of more than 90 days during the entire course of the LSAP program in 2009. The dependent variable is the percentage change in each security's price on March 18, 2009. All independent variables are as of March 17, 2009. Standard errors in parentheses. Asterisks indicate statistical significance at the 5% (**) and 1% (***) levels.

Intercept	−1.04***
	(0.105)
Remaining maturity	0.75***
	(0.019)
Remaining maturity squared	−0.02***
	(0.001)
Svensson fitting error	0.64***
	(0.26)
Percentage of issue held by Fed	1.59***
	(0.58)
On-the-run dummy	0.14
	(0.16)
< Two-years dummy	0.19*
	(0.09)
# Obs	148
Adjusted R^2	0.97

These figures suggest that market participants were quite successful in forecasting the distribution of those purchases based on the information available at the time of the announcement. For example, Table 3 indicates that using the CUSIP's characteristics as of the day before the announcement, it is possible to account for 97% of the decline in yields that occurred on the day of the announcement. In particular, we run a CUSIP-level regression of the March 18 price changes on a set of variables summarizing the security's characteristics: maturity, maturity squared, yield curve fitting errors,[11] the percentage of each security held by the Fed in the System Open Market Account (SOMA) portfolio, a dummy variable taking the value of one if the security was an on-the-run issue, and a dummy variable taking the value of one if the remaining maturity of the security was less than two years; all these explanatory variables, with the exception of the on-the-run dummy, are statistically significant and with the expected sign. For example, the increase in price was larger for securities with longer maturity and positive fitting errors (i.e., cheap relative to the curve), as well as for securities that were already in the Fed's portfolio and with remaining maturity

[11] Throughout the paper, "fitting errors" refer to the 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 residuals can be interpreted as a measure of unexploited price discrepancies in the Treasury market. Hu, Pan, and Jiang (2010) use an index of similarly constructed fitting errors to gauge the evolution of Treasury-market liquidity.

Table 4
Factor structures in the intervention (2009–2010) and non-intervention (2000–2008) periods.
Results for the factor decomposition of the time series of eight constant-maturity yields shown over the two periods. In each panel, the top rows show the loadings of each yield on each factor. The bottom two rows show the percentage of the total variation in the yields explained by each factor.

Yield loading	1st Factor	2nd	3rd	4th	5th	6th	7th	8th
Intervention period								
6 Months	0.181	0.555	0.571	−0.373	−0.297	−0.220	0.224	0.086
1 Year	−0.180	0.595	0.177	0.400	0.149	0.393	−0.456	−0.196
2 Years	−0.385	0.288	−0.180	0.497	0.021	−0.207	0.581	0.330
5 Years	−0.428	0.083	−0.220	−0.074	−0.364	−0.471	−0.131	−0.620
7 Years	−0.430	0.055	−0.174	−0.312	−0.265	0.015	−0.449	0.642
10 Years	−0.430	0.044	−0.065	−0.483	0.119	0.584	0.415	−0.218
20 Years	−0.400	−0.176	0.461	−0.114	0.658	−0.373	−0.108	0.022
30 Years	−0.273	−0.461	0.562	0.324	−0.489	0.227	0.048	−0.007
% Explained	0.658	0.284	0.044	0.010	0.004	0.000	0.000	0.000
Cumulative		0.942	0.986	0.996	1.000			
Non-intervention period								
6 Months	0.336	−0.412	0.363	0.489	0.288	0.442	−0.257	−0.046
1 Year	0.351	−0.372	0.243	0.117	−0.132	−0.469	0.634	0.164
2 Years	0.371	−0.284	0.074	−0.367	−0.414	−0.289	−0.568	−0.254
5 Years	0.396	−0.070	−0.207	−0.480	−0.037	0.478	0.106	0.569
7 Years	0.396	0.049	−0.366	−0.147	0.264	0.152	0.325	−0.698
10 Years	0.381	0.204	−0.500	0.359	0.295	−0.415	−0.289	0.303
20 Years	0.305	0.504	0.052	0.391	−0.657	0.235	0.087	−0.058
30 Years	0.273	0.556	0.612	−0.281	0.373	−0.148	−0.044	0.020
% Explained	0.783	0.202	0.011	0.003	0.001	0.000	0.000	0.000
Cumulative		0.985	0.996	0.999	1.000			

of less than two years (although we suspect that for this variable the result was driven by the downward revision to the expected monetary policy path on that day). This, in turn, might also suggest that market participants already had in mind a good approximation of the variables that could have been relevant for the Fed in determining the issues and the quantities to purchase. (An approximation of the rule for quantities actually purchased by the Fed is examined more closely in Section 4.2.)

3.3. *The factor structure of Treasury yields*

One way of examining whether the LSAP program had effects on Treasury yields is to see whether the time-series behavior of yields was different during this period than in other times. To do this, we perform standard factor decompositions and investigate whether the multivariate structure of the data across maturities underwent major changes during the time span of the LSAP program. In particular, we use principal-component analysis to extract the orthogonal factors and factor loadings from the matrix of eight daily constant maturity yields, during two sample periods: December 1999–December 2008 (the "non-intervention" period) and January 2009–December 2010 (the "intervention" period).

Table 4 presents the factor loadings and the percentage explained by the extracted factors over the two sample periods. Consistent with the previous term-structure literature on nominal US Treasury securities, during the non-intervention period, shown in the bottom panel, two factors explain more than 98% of the variation in yields. The first factor behaves like the so-called "level" factor as it loads very similarly on all maturities, with a slightly higher weight on the two- to ten-year range. The second factor loads positively on long and negatively on short maturities and hence behaves like a "slope" factor; and the third factor behaves like a "curvature" factor, as it loads positively on long and short maturities and negatively on medium maturities.

However, during the intervention period, shown in the top panel, the factors' structure and loadings are quite different. First, to explain the same percentage of variation in yields, an additional factor is required—to explain 98.5% of variation, three rather than two factors are required and to explain 99.6% of variation, four rather than three factors are required. Second, the first factor's loadings are not similar across maturities but exhibit an upward-sloping structure that steepens sharply around the two-year maturity, then remains flat up to the 20-year maturity, and finally declines at the 30-year. Interestingly, this factor seems to be important for the maturity sectors where the Fed purchases were concentrated, suggesting that it might be correlated with available supply. The second factor also exhibits loadings that are a bit different from what is typical of normal periods in the Treasury market. In contrast, the third factor mostly preserves the behavior of the curvature factor.[12] Finally, the fourth factor, which is the higher-order factor that emerges from this analysis, does not present a clear pattern in its loadings. In summary, the principal-component analysis reveals that the simple level, slope, and curvature interpretation of the factors does not seem to hold during the exceptional intervention period, and that different and additional factors play a nontrivial role.

[12] Factor analysis using changes in yields, rather than levels, is extremely similar.

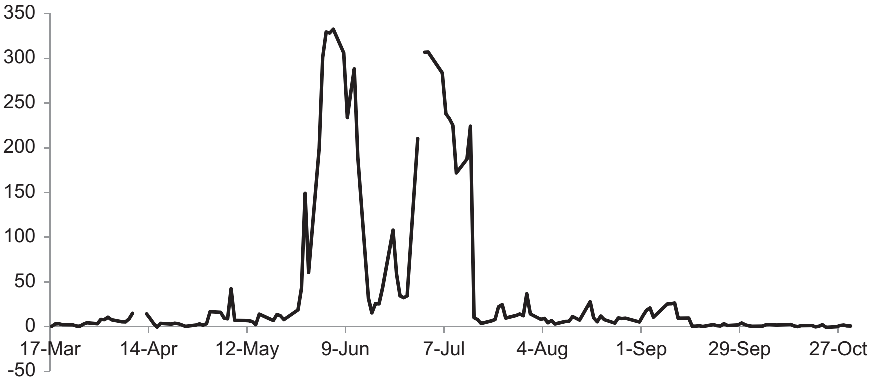

Fig. 3. Repo specialness spread for the on-the-run ten-year US Treasury notes. The plotted time series is the difference between the overnight general collateral repo rate and the corresponding specific collateral repo rate for the on-the-run ten-year note (in basis points). The dates of the 10-year Treasury auctions reopening were June 10 and July 8, 2009.

3.4. Indicators of Treasury liquidity and activity

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. 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%, the yield premium paid for on-the-run 10-year notes 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%. The final column of the table shows that the average yield curve fitting errors declined by about half between the two subperiods, suggesting improved arbitrage activity.

As a further indicator of market functioning, we briefly examine the behavior of the repo market during the period using, as one metric of liquidity, the repo "specialness" spread for each CUSIP. This spread, interpreted by Duffie (1996) as a "scarcity premium," is computed as the difference between the general collateral (GC) repo rate as of 9 AM and the corresponding rate for each particular security, averaged over 7 AM to 10 AM (when most of the trades take place).[13] As previously shown in the literature (see, for example, Duffie, 1996, and Jordan and Jordan, 1997), this spread is strongly affected by the demand for short positions as well as the Treasury auction cycle, and we provide evidence that this was also the case also during the LSAP period. First, in analyzing the time series of the repo specialness spread for the on-the-run securities at each maturity, we find that the largest peaks for almost all of them occurred ahead of the auction dates and disappeared soon after. In particular, as shown in Fig. 3, for the ten-year Treasury note, the largest peaks were around the auction reopening of June 10 and July 8. This shows that most of the variance of the specialness spread was not connected to the Fed purchases but rather to Treasury auctions. Some market participants reportedly claimed that the quantities purchased by the Fed should not necessarily affect the repo specialness spread because once the purchased securities entered in the SOMA portfolio, then they became available through the securities lending program (at a borrowing cost similar to the repo market).[14] Fig. 4 further illustrates this point by showing the security-level repo specialness spread relative to the Fed's cumulative purchases (as a fraction of the issue) during the LSAP program. It can be seen that the repo specialness spread is not positively related to the amount purchased by the Fed. This is not surprising since, on March 24, 2009, the Open Market Desk at the New York Fed ("the Desk") indicated that it "will refrain from purchasing those securities trading with heightened scarcity value in the repo market for specific collateral and those securities which are cheapest to deliver into the front-month Treasury futures contracts. Specific issues that will be excluded from consideration will be announced at the start of each operation."

4. Empirical tests for local-supply effects

This section presents our main tests for the presence of local-supply effects using the security-level purchase and price data during the LSAP program. We test for both stock effects (the cumulative shift in yields) and flow effects (the changes in yields at the times when purchases took place). We first discuss some specification issues that are common to both sets of tests.

4.1. General specification issues

For each of the sample securities n, we partition the universe of outstanding securities into $I+1$ segments, $S_i(n)$, that define buckets of "substitutes" for security n, where $i=0, \ldots, I$. Note that we reserve $i=0$ for the

[13] We are grateful to the New York Fed for providing the data. The specific collateral rate is obtained from BrokerTec Interdealer Market Data that includes rates quoted across different platforms and the GC rate is obtained surveying the Primary Dealers every morning at 9 a.m.

[14] By the end of 2008, in response to the extraordinary settlement fails, the Fed eased the terms of its securities lending program: the minimum loan fee was reduced from 50 to 1 basis point and the limit on total borrowings by a single dealer was raised from \$3 to \$5 billion.

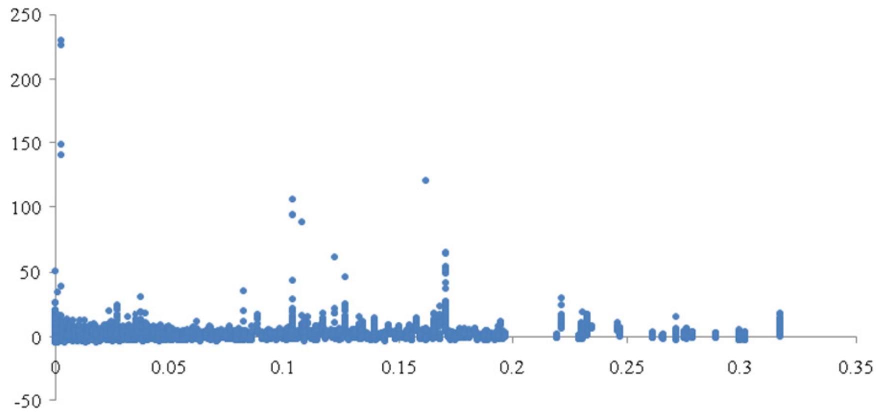

Fig. 4. Repo specialness spread relative to Fed purchases. The figure plots the spread (in basis points) between the overnight general collateral repo rate and the corresponding specific collateral repo rate on each security in the sample against the Fed's cumulative purchases of that security (as a fraction of the amount outstanding) during the entire LSAP program. The sample excludes on-the-run securities.

partition consisting only of security n itself. This will allow for the possibility that shocks that affect the quantity of security n have effects on the yield of n that differ from the effects on all other securities (an extreme form of the local-supply channel similar to the infinite-risk-aversion case in V&V). The dollar amount of substitutes for each security n purchased under the LSAP program in the ith bucket is denoted by $Q_{n,i} \equiv \sum_{m \in Si(n)} Q_m$. Note that $Q_{n,0}$ is the amount of security n itself purchased under the LSAP program. (We will refer to this as "own purchases.") We assume that the potential influence of dollar quantities purchased in a given sector may depend inversely on the dollar amounts outstanding, say $O_{n,i}$, in that sector or in other sectors. Thus, we consider a normalized quantity variable $q_{n,i}$, where the normalization is a function of $O_{n,0}, \ldots, O_{n,I}$.

Because coupon rates and maturities vary considerably across the universe of Treasury securities, we conduct our regressions in price space, rather than in yield space. Specifically, both our flow- and stock-effect regressions take the following general form:

$$R_n = \gamma_0 q_{n,0} + \sum_{i=1}^{I} \gamma_i q_{n,i} + \phi(\tau_n) + \varepsilon_n, \tag{3}$$

where R_n is the gross return (change in price plus accrued coupon payments) on Treasury security n, τ_n is its remaining maturity, and $\gamma_0, \ldots, \gamma_I$ are scalar coefficients.[15] We assume that $\phi(\tau_n)$ is a smooth function of τ_n, consistent with most term-structure models.

This empirical specification is consistent with the theory sketched in Section 2, once we allow for quantity fluctuations at the individual-security level rather than at maturity level—i.e., we relax the V&V assumption that $\tau_n = \tau_m$ implies $\theta_k(n) = \theta_k(m)$. This specification allows for a higher level of disaggregation to exploit the richness of our security-level data. If we then specify

$$q_{n,0} = \sum_{k=1}^{K} \theta_k(n)\Delta\beta_k \tag{4}$$

$$q_{n,i} = \sum\nolimits_{m \in S(n)} \sum_{k=1}^{K} \theta_k(m)\Delta\beta_k,$$

Eq. (3) is consistent with Eq. (1), once we substitute in Eq. (2) and further define

$$\phi(\tau_n) = \sum_{k=1}^{K} D_k(\tau_n)\Delta\beta_k + \Delta\big(A_r(\tau_n)r^0 + C(\tau_n)\big). \tag{5}$$

and the local-supply effect

$$L_n(\boldsymbol{\theta}_k) = \gamma_0 \theta_k(n) + \sum_{i=1}^{I} \left[\gamma_i \sum_{m \in S(n)} \theta_k(m) \right], \tag{6}$$

where the changes (Δ) are computed over the observation frequency in our sample.[16] In words, this specification assumes (a) that LSAP quantities can be thought of as linear combinations of unobserved demand factors, (b) that movements in prices other than local-supply effects can be captured by smooth function of maturity, and (c) that the local-supply effects on returns are piecewise linear across the substitute buckets as we have defined them. Less formally, Eq. (3) simply conjectures that each security's price responds linearly to purchases of itself and its substitutes, after controlling for changes in the general configuration of the yield curve. Note that this specification does not require us to separately identify $\boldsymbol{\theta}$ and $\boldsymbol{\beta}$ or, for that matter, to specify the number of factors K.

The parameter γ_0 reflects the local own-price elasticity of Treasury securities, while the parameters $\gamma_1, \ldots, \gamma_I$ reflect the cross-elasticities of Treasury security prices with respect to other Treasury securities. These latter elasticities depend on the degree of substitutability

[15] The central results reported below also hold if we use yields as the dependent variable. Heuristically, converting Eq. (1) to prices and applying Ito's lemma gives an equation for the bond return that is linear in the short-term interest rate and the demand factors, although, of course, the securities in our data set 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. The time subscripts are not essential here and so are dropped for the exposition in this section.

[16] The frequency is daily in our flow-effect regressions and the entire seven-month program period in our stock-effect regressions.

between different Treasuries, which in turn depends upon the ability of arbitrageurs to substitute across maturities and correct price discrepancies given their level of risk aversion, capital constraints, and market liquidity, among other things.[17] The own-price response γ_0 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, 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 γ_0 term, but it affects *every* security's yield through the applicable γ_i terms.

It remains to specify the partitions determining the substitute buckets $S_i(n)$. Although in principle we could choose the size, number, and composition of these buckets in a variety of ways, a division into $I=3$ buckets based on remaining-maturity ranges seemed to provide a good combination of parsimony and flexibility and allows us to test whether the degree of substitutability between securities depends systematically on the difference between their maturities. In particular, for each security n, we define our most narrow substitute bucket (arbitrarily, $i=1$) to include all securities having remaining maturities within two years of security n's maturity—that is, $S_1(n)=\{m: |\tau_n - \tau_m| < 2; m \neq n\}$. We refer to these securities as "near substitutes" for security n. The second bucket ($i=2$), which we call "mid-substitutes" for n, includes all securities having remaining maturities that are between two and six years different from security n's. The third bucket ($i=3$, or "far substitutes") includes all securities having remaining maturities between six and 14 years different from security n's. To compute $q_{n,i}$, we normalize by the total amount of securities outstanding that have remaining maturities within two years of security n—that is, $q_{n,i}=Q_{n,i}/(O_{n,0}+O_{n,1})$.[18]

4.2. Stock effects

By "stock effects" we mean the impact that the LSAP program had on prices by permanently reducing the total amount of Treasury securities available for purchase by the public within a given sector. 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.[19] Thus, it is crucial to account for expectations when measuring stock effects. However, we note that changes in investor expectations matter only 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.

Some previous event studies of LSAPs, such as Gagnon, Raskin, Remache, and Sack (2010), have tried to identify their effects by looking at the reaction of prices within a specific time-window around important announcements. The difficulty with this approach (apart from specifying the appropriate window length) 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.

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 (the day before the first LSAP announcement and the day after the last purchase). We seek estimates of the coefficients γ in Eq. (3). We assume that the maturity-dependent yield-curve movements $\phi(\tau_n)$ are sufficiently smooth that they can be well approximated by a second-order polynomial. In addition to the duration effects of LSAP purchases, these terms account for possible secular changes in the slope and curvature of the yield curve during our period that could have resulted from macroeconomic conditions and new Treasury issuance. In the cross-section analysis, we do not use the mid- or far-substitute categories because of the high degree of collinearity, especially given our inclusion of the remaining maturity variables. Thus, Eq. (3) reduces to a linear regression of returns on own- and near-substitute purchases, with linear and quadratic terms for maturity as control variables.

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

[17] These are among the most important forms of limits to arbitrage that have received significant attention in the literature; see Gromb and Vayanos (2010) for a survey.

[18] 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. 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.

[19] Although policymakers had floated the possibility of Treasury purchases prior to March 18, market participants did not appear to place high odds on the decision being made, and the announcement generally came as a surprise. 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:http://www.bloomberg.com/apps/news?pid=20601087&sid=aIt7yEi9XAhc&refer=home.

Table 5
LSAP purchases as a function of pre-LSAP information.

Regression results accounting for the amount of each Treasury security issue that was purchased under the LSAP program. The sample consists of all Treasury coupon securities that were outstanding with a remaining maturity of more than 90 days during the entire course of the LSAP program in 2009. The dependent variable is LSAP purchase amounts as a percentage of the par value of each security outstanding; the independent variables are various CUSIP characteristics as of March 17, 2009, the day before the LSAP announcement. Asymptotic standard errors in parentheses. Asterisks indicate statistical significance at the 1% level.

Intercept	0.051***
	(0.015)
Remaining maturity	0.015***
	(0.003)
Remaining maturity squared	−0.0006***
	(0.00009)
Svensson fitting error	0.208***
	(0.038)
Percentage of issue held by Fed	−0.271***
	(0.083)
On-the-run dummy	0.091***
	(0.024)
< Two-years dummy	−0.044***
	(0.014)
# Obs	148
Adjusted R^2	0.431

instrument the LSAP purchase amounts of each security using information about the security's characteristics available before the program was announced. Specifically, our instruments are the security-level variables previously used in the regression reported in Table 3.

Table 5 reports the results of a regression of actual LSAP purchases (as a percentage of the par value of each security outstanding) on these instruments. All of the coefficients are statistically significant at the 1% level. The coefficients on the maturity variables suggest that purchases depended strongly on remaining maturity and, controlling for other factors, peaked around the 12-year sector (in percentage-of-issue terms). Moreover, the Fed purchased very few securities with maturity of less than two years. This much may have been obvious simply from observing the announcements of sectors in which purchases took place.[20] However, the other variables give some insights into the factors that the Desk used to determine purchases *within* these sectors—a decision that it based on a confidential algorithm at the time of each operation. The yield-curve fitting errors 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. However, controlling for this effect, the Desk was more likely to purchase on-the-run than off-the-run issues. Finally, the Desk was less likely to purchase securities that it already owned, presumably reflecting its self-imposed ownership limit of 35% of each issue. Interestingly, the sign on this coefficient is the opposite of what we found when examining the market reaction to the program announcement in Table 3, suggesting that market participants may have initially misjudged this aspect of the Fed's purchase rule.

Two further transformations are required for our instruments. First, the specification presented in Table 5 is probably the right one—the Fed likely determined how much of each CUSIP to buy as a fraction of the amount outstanding of that CUSIP. However, in our second-stage regressions, we want to use the quantity normalized by the amount outstanding in the relevant *sector*—$Q_{n,0}/(O_{n,0}+O_{n,1})$, not $Q_{n,0}/O_{n,0}$. We thus use $q_{n,0}$ as the dependent variable in the first stage but, to maintain consistency, we weight each of our four security-level instruments by $O_{n,0}/(O_{n,0}+O_{n,1})$. Second, purchases of substitutes are subject to the same endogeneity concerns as own purchases, so we also instrument for $q_{n,1}$. As instruments, we simply average each of the four instrumental variables listed above over the bucket of near substitutes for each security, weighting by amounts outstanding. We include both the security-specific and sector-average instruments in both of our first-stage equations. 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.

In summary, our baseline two-stage system takes the form

$$\begin{pmatrix} q_{n,0} \\ q_{n,1} \end{pmatrix} = \boldsymbol{\delta}\mathbf{z}_n + \mathbf{u}_n \tag{7}$$

$$R_n = \gamma_0 \hat{q}_{n,0} + \gamma_1 \hat{q}_{n,1} + \phi_0 + \phi_1 \tau_n + \phi_2 \tau_n^2 + \varepsilon_n, \tag{8}$$

where R_n is security n's gross return, $\mathbf{z}_i$ is the vector of instruments, hats indicate instrumented values from the first stage, and τ_n is the remaining maturity as of March 17. Because we are using a cross-section, we exclude securities that matured or were issued while the program was in progress, leaving us with 148 observations.

We do not explicitly control for the Federal Reserve's purchases of other assets—agency debt and mortgage-backed securities (MBS)—during the LSAP period. In this choice, we follow the previous literature in this area, which almost exclusively confines attention to the supply of Treasuries when studying the effects of debt supply on yields. (As a more practical matter, the data necessary to control for MBS purchases in our framework are not publicly available.) Note that in order for the omission of these other asset purchases to bias our results, they would have to be cross-sectionally correlated with the pattern of Treasury purchases and yield changes, and there is no particular reason to suspect this.[21] Similarly,

[20] Every-other Wednesday, the Desk announced the broad maturity sectors in which it would be buying over the subsequent two weeks.

[21] The most plausible way in which cross-sectional correlation between MBS and Treasury purchases could have occurred is if the MBS purchases had been concentrated in the same maturity sectors as Treasury purchases. As noted in Gagnon, Raskin, Remache, and Sack (2010), the vast majority of the MBS purchased by the Fed were low coupon (near-on-the-run) issues, and thus had remaining maturities close to 30 years, while, as shown in Fig. 1, there were very few Treasury purchases with maturities this high. Therefore, if MBS purchases had a large local effect on Treasury yields, omitting them would bias our

Table 6
Stock effects (IV)–pooled.

The second-stage of the 2SLS regressions, where the dependent variable is the cumulative percentage holding return from March 18 to October 30, 2009 and the amounts purchased are instrumented using variables as of March 17, as shown in Table 5. The sample consists of all Treasury coupon securities that were outstanding with a remaining maturity of more than 90 days during the entire course of the LSAP program in 2009. All purchase variables are normalized by the total quantity of near substitutes outstanding. Asymptotic standard errors in parentheses. Asterisks indicate statistical significance at the 10% (*), 5% (**), and 1% (***) levels.

	Baseline	Controlling for initial prices
Own purchases (IV)	2.17***	0.61***
	(0.43)	(0.21)
Purchases of near substitutes (IV)	0.13**	0.07**
(maturity w/in 2 years of own)	(0.07)	(0.03)
Remaining maturity	−0.003**	−0.0005
	(0.001)	(0.0007)
Remaining maturity squared	0.00003	−0.00004
	(0.00004)	(0.00002)
Log initial price	–	0.019***
		(0.007)
Initial % fitting error	–	−0.58***
		(0.07)
Intercept	0.011***	−0.08**
	(0.002)	(0.03)
# Obs	148	148
Adj. R^2	0.695	0.857

an identifying assumption for our regressions is that LSAP purchases were cross-sectionally uncorrelated with other changes in Treasury supply over the period of the program (after controlling for maturity).

Finally, in order to examine how our results vary with liquidity and other security characteristics, we want to allow the second-stage coefficients to differ across security groups. In particular, we will divide the sample by security type, maturity, and vintage. The small number of observations makes running separate regressions on each of these groups problematic, and, moreover, there is no particular reason to think that the first-stage equation or the second-stage coefficients on τ_n should differ across them. Therefore, we continue to run all of our regressions on the full sample but, in the second stage, we allow the coefficients on own and substitute purchases to differ across the groups:

$$R_n = \gamma_0^g \hat{q}_{n,0} + \gamma_1^g \hat{q}_{n,1} + \phi_0 + \phi_1 \tau_n + \phi_2 \tau_n^2 + \varepsilon_n, \tag{9}$$

where g indexes the groups. We perform this estimation by interacting $\hat{q}_{n,0}$ and $\hat{q}_{n,1}$ with dummy variables that divide the sample into mutually exclusive subsamples—notes vs. bonds, short vs. long maturities, and near-on-the-run vs. far-off-the-run securities.

4.2.1. Results

The results of the second-stage regressions, with gross returns as the dependent variable, are presented in the first column of Table 6. Both own purchases and near-substitute purchases have positive and statistically significant effects on returns, although the effects of own purchases appear to be considerably larger. However, a likely source of misspecification in these results is that, if individual yield curve fitting errors are not persistent, the prices of securities with positive fitting errors would tend to fall relative to other securities and those with negative fitting errors would tend to rise, even in the absence of LSAP purchases—in other words, initial fitting errors might be correlated with the second-stage error term. In addition, there may be other information embedded in the initial prices of securities that reflects expectations of future returns. To control for these possibilities, we run an alternative specification in which we include the initial percentage price fitting error and the initial log price as exogenous regressors. (We also include both variables in the first-stage regression.) These results are shown in the second column. Both of the new regressors are highly significant, arguing for their inclusion. In this specification, the estimated values of both the own and near-substitute coefficients fall notably, but they retain their basic qualitative pattern and statistical significance.

The coefficient of 0.07 on substitute purchases suggests that buying one percent of a security's near substitutes (about \$10 billion for the average security in our sample) increased the price of that security by 0.07%. For a typical ten-year Treasury, with a modified duration of seven years, this translates into a yield change of −1 basis point. The coefficient on own purchases implies that if the same dollar amount had been used entirely to purchase a single security, the price of that security would have risen by 0.61%; again taking the ten-year Treasury as representative, its yield would have fallen by about 9 basis points. The strong statistical and economic significance of these results supports the existence of local-supply effects. In addition, the smaller magnitude of the substitute coefficient than the own coefficient suggests imperfect substitution across securities within the same sector.

We next break our data into subsamples, using the interactive-dummy technique described above. In particular, we consider possible differences between notes and bonds, securities with relatively long maturities versus those with shorter maturities, and recently issued securities versus older issues. The distinction between notes and bonds is potentially interesting largely as a proxy for other security features. The bonds all had original maturities of 30 years and most of them are old issues that tend to have much smaller trading volumes and lower liquidity than notes, as well as higher coupons.[22] To distinguish longer and shorter maturities, we split the sample at the middle of the yield curve, 15 years. (About 90% of the securities in our sample were in the less-than-15-year sector.) To distinguish

(*footnote continued*)
coefficients upward at the long end of the yield curve. Yet, as shown in the following section, we already find very small effects at high maturities.

[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 five- to ten-year maturity bonds were as much as 50 basis points higher than those on comparable-maturity notes; see Gurkaynak and Wright (2012). Musto, Nini, and Schwarz (2011) document the difference in liquidity between notes and bonds during this period.

Table 7
Stock effects (IV)-subsamples.
The second-stage of the 2SLS regressions, where the dependent variable is the cumulative percentage holding return from March 18 to October 30, 2009 and the amounts purchased are instrumented using variables as of March 17, as shown in Table 5. The sample consists of all Treasury coupon securities that were outstanding with a remaining maturity of more than 90 days during the entire course of the LSAP program in 2009. All purchase variables are normalized by the total quantity of near substitutes outstanding. The table shows three ways of splitting the sample, which is accomplished using interactive dummy variables on the amounts purchased, as shown in the top two rows. Asymptotic standard errors in parentheses. Asterisks indicate statistical significance at the 10% (*), 5% (**), and 1% (***) levels.

	Notes	Bonds	> 15 Years	< 15 Years	Near on-the-run	Far off-the-run
Own purchases (IV)	0.35	0.68***	0.38	0.66***	−0.03	1.23***
	(0.32)	(0.24)	(0.31)	(0.21)	(0.34)	(0.34)
Purchases of near substitutes (IV)	0.08**	0.09*	0.04	0.04	0.17***	0.11***
(maturity w/in 2 years of own)	(0.03)	(0.05)	(0.07)	(0.03)	(0.06)	(0.04)
Remaining maturity	−0.0006		−0.00007		−0.0022**	
	(0.0007)		(0.0008)		(0.0011)	
Remaining maturity squared	−0.00003		−0.00005*		0.00002	
	(0.00002)		(0.00003)		(0.00004)	
Log initial price	0.015		0.018***		0.025**	
	(0.0098)		(0.007)		(0.0099)	
Initial % fitting error	−0.56***		−0.57***		−0.52***	
	(0.07)		(0.06)		(0.08)	
Intercept	−0.06		−0.07**		−0.11**	
	(0.04)		(0.03)		(0.05)	
# Obs	148		148		148	
Adj. R^2	0.903		0.904		0.914	

securities of recent versus older vintage, we split the sample into securities that are more than five issues off-the-run and those that are less than six. In all of these regressions, we include controls for initial prices and fitting errors, given the better fit of that model in the pooled regression above, although the qualitative results are robust to these controls.

Table 7 presents the results for these various subsamples. The magnitude and significance of substitute purchases vary across the subsamples, but the differences are not economically large and we cannot reject that any of these coefficients are equal to the value of 0.07 presented in the previous table. The effects of own purchases are only statistically significant among the subsamples of bonds, far-off-the-run securities, and shorter-maturity securities. We note that the results split by maturity support the claim that we have in fact identified local-supply effects rather than duration effects, since the latter should generally be increasing in maturity.

However, we cannot reject that the own coefficients are the same for notes and bonds and for short and long maturities. The one instance in which we do find significant differences in the own coefficients is when we split according to security vintage—purchases of far-off-the-run securities have large and highly significant effects, while purchases of near-on-the-run securities have none at all. Consistent with the significance of this difference, this specification also fits the data better in terms of adjusted R^2 than any of the others we tried. Proximity to on-the-run status is a proxy for liquidity, suggesting that a great deal of the local-supply stock effect of the LSAP program was due to the relative illiquidity of the securities purchased.

4.2.2. Counterfactual yield curve

To summarize the local-supply stock effects of the LSAP program, we construct a counterfactual yield curve using the best-fitting results presented in Table 7. In particular, by using the actual value of purchases of each security and its near substitutes, together with the coefficients for the appropriate subsample, 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 (holding other effects on the slope and curvature of the yield curve constant). The corresponding yields are shown as the hollow circles in Fig. 5, with dashes indicating the 95% and 5% confidence bounds for each security. (These are calculated by finding the confidence bounds for the fitted value of each security's price and then transforming those bounds into yields.) The solid diamonds in the figure show the actual yields on October 30, 2009. The difference between the red and blue marks represents the local-supply stock effects of the LSAP program on yields to maturity. The peaks in the red squares around the seven-, 13-, and 17-year maturity sectors are clear illustrations of the local effects.

For almost all securities, the counterfactual yields lie significantly above the actual yields. The largest effect, on average, appears to be at the very short end of the curve, but these responses are estimated with some noise and are driven by the very short duration of securities in that region. (The estimated effect on *prices* at the short end is very small.) Beyond maturities of about a year, the largest effects

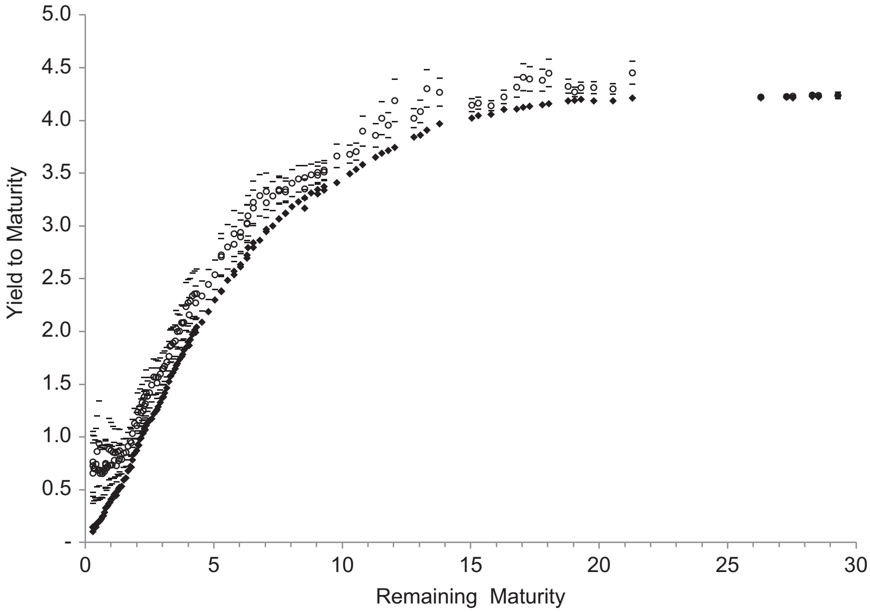

Fig. 5. Stock effect of the Treasury LSAP program on yields to maturity. The solid diamonds are nominal yields on each of the 148 securities outstanding in our cross section as of October 30, 2009. The hollow circles are the counterfactual yield point estimates on that day, using the same set of Treasury prices but with the estimated local-supply effects of LSAP purchases removed according to the coefficients in the last column of Table 7. The dashes are the 95% and 5% confidence bands around each of the counterfactual yields.

are around horizons of about six to eight years and 11 to 14 years, consistent with the relatively high proportion of securities that were purchased in these sectors and the relatively high coefficients on off-the-run securities, which dominate those sectors. Again, we see very little response at the long end of the yield curve, supporting our interpretation of these estimates as local-supply effects. Averaging over all securities, we estimate that the local effects of LSAP purchases shifted the level of yields by about 30 basis points, for an average elasticity of about one basis point for every $10 billion bought.

4.3. *Flow effects*

In this section, employing a panel, we test whether Treasury LSAP purchases had effects on Treasury prices around the times when purchases occurred in the sectors where they occurred. Again, we define such responses as the "flow effects" of the program. Some additional detail on the mechanics of the individual purchase operations may be useful in interpreting the flow-effect results. The first operation under the Treasury LSAP program was conducted on March 25, 2009. Purchases continued at a pace of about $10 billion per week over the subsequent five months but gradually slowed beginning in August to minimize any potential disruption that might have resulted from a sudden closing of the program. In total, there were 57 purchase operations of nominal Treasury securities. The logistics of these 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. These maturity sectors included securities spanning ranges of between one year (at the short end of the yield curve) and 13 years (at the long end), with an average range of about four years. Auctions took place from Monday of the first week through Friday of the second week and typically settled on the following day.[23] 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, and began accepting propositions from primary dealers.[24] Propositions included the amount of each CUSIP that the dealer was willing to sell to the Desk and the price at which it was willing to sell. 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 published the auction results within a few minutes. Market participants were not aware in advance of the total amount to be purchased or of the distribution of purchases across CUSIPs. Notably, (consistent with standard market practice) settlement of the winning bids did not occur until the following day, so that dealers could, in principle, have submitted propositions for securities they did not own and, if they won, purchased these securities to settle the next day with the Desk.

[23] In practice, the Desk avoided conducting Treasury operations on Fridays, preferring to reserve these days for agency purchases. They also avoided conducting purchases in a given sector on days when Treasury auctions were occurring in that sector.

[24] 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% of the issue was already owned by SOMA.

Because the sectors of purchase operations were announced in advance and both the list of CUSIPs and the total size of each operation were fairly predictable, one might expect that examining yield changes as functions of contemporaneous purchases would reveal no statistically significant responses. However, within the list of eligible securities, the particular CUSIPs that were purchased and the amounts of these purchases were not known in advance to the market, so yield differentials could have emerged on the days of purchases between securities that were purchased and those that were not. In addition, under limits to arbitrage, even perfectly anticipated changes in supply could have effects on prices when they occur, as shown by Lou, Yan, and Zhang (2010) in the case of Treasury auctions. The tests below will include both of these phenomena. Market-microstructure, settlement, and other technical factors could also cause yields to move in response to supply fluctuations, even if those fluctuations are predictable. However, in this case, we would expect the effects to reverse quickly. We therefore also examine price movements on the day after purchase operations to test whether the effects are persistent, which would favor more fundamental explanations such as preferred habitat.

4.3.1. Specification details

Having panel data leads us to make four notable adjustments to the specification we used in the cross-sectional stock-effect estimation above. First, to control for duration effects and other shifts to the overall term structure (i.e., $\phi(\tau_n)$) at a daily frequency, we use time-dummies. This is less restrictive than it may at first appear because individual purchase operations were conducted within fairly small maturity ranges (about four years, on average). Within these ranges, daily slope and curvature shifts are generally quite small. Second, we include security-level fixed effects to control for the possibility, suggested in the stock-effect results, that some securities may have experienced higher returns over the period, independent of local-supply and duration effects (perhaps as a result of decaying fitting errors). Third, we do not use instrumental-variables estimation when measuring flow effects because, at a daily frequency, the Fed is unlikely to have responded meaningfully to price changes in specific securities or sectors. Finally, we are able to consider our mid- and far-substitute buckets in the flow-effect regressions because the greater sample size and observation heterogeneity reduces the multicollinearity problem.

Our flow-effect regressions are thus the following analog of Eq. (8):

$$R_{n,t} = \gamma_0 q_{n,0,t} + \sum_{i=1}^{3} \gamma_i q_{n,i,t} + \delta_t + \alpha_n + \varepsilon_{n,t}, \tag{10}$$

where α_n is a security-specific fixed effect, δ_t is a time dummy, $\varepsilon_{n,t}$ is an error term, and we have added time subscripts to the return and quantity variables to indicate price changes and quantities purchased on specific days. Note that, in addition to accounting for the issues mentioned above, the fixed effects and daily time dummies enable us to control for a variety of occurrences, such Treasury auctions, that could shift relative demand and supply in a small portion of the nominal coupon market.

Because the 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, though the same CUSIP can appear in both groups on different days.

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. Moreover, we are interested in the persistence of the flow effects and would like to test for possible reversion in prices in the days following the operations. To check for these possibilities, we also look at returns on the days after purchase operations by running regressions of the form

$$R_{n,t+1} = \gamma_0 q_{n,0,t} + \sum_{i=1}^{3} \gamma_i q_{n,i,t} + \delta_t + \alpha_n + \varepsilon_{n,t}, \tag{11}$$

where $R_{n,t+1}$ is the return the day after the operation.

4.3.2. Results for eligible securities

We begin by analyzing the results for the eligible securities. In Table 8 we report the baseline results. Initial tests suggested that the coefficients differed for securities with very long remaining maturities, so we report a sample split at the midpoint maturity of 15 years. Focusing on the first column of the table, which pertains to

Table 8

Flow effects on day of purchase-all eligible securities.

Regression results, where the dependent variable is the daily percentage price change in each outstanding CUSIP. The sample consists of Treasury coupon securities that were outstanding with a remaining maturity of more than 90 days and eligible to be bought in each purchase operation under the LSAP program, March 18–October 31, 2009. Only days when LSAP purchases occurred are included. Security-level fixed effects and daily time dummies not shown. Standard errors in parentheses. Asterisks indicate statistical significance at the 10% (*), 5% (**), and 1% (***) levels.

	< 15 years to maturity	> 15 years to maturity
Own purchases	0.276***	−0.106
	(0.053)	(0.098)
Purchases of		
Near substitutes	0.240***	−0.124***
(maturity w/in 2 years of own)	(0.048)	(0.044)
Mid-substitutes	0.170***	−0.050*
(maturity 2 to 6 years away)	(0.045)	(0.026)
# Obs.	923	145
# CUSIPS	146	23
Adj. R^2	0.976	0.985

Table 9
Flow effects on day of purchase-subsamples of eligible securities.
The dependent variable is the daily percentage price change in each outstanding CUSIP. The sample consists of Treasury coupon securities that were outstanding with a remaining maturity of between 90 days and 15 years and eligible to be bought in each purchase operation under the LSAP program, March 18–October 31, 2009. Only days when LSAP purchases occurred are included. Security-level fixed effects and daily time dummies not shown. Standard errors in parentheses. Asterisks indicate statistical significance at the 10% (*), 5% (**), and 1% (***) levels.

	Mar 25–Jul 6	Jul 7–Oct 29	Notes	Bonds	Near on-the-run	Far off-the-run
Own purchases	0.344***	0.297***	0.267***	0.249***	0.232**	0.248***
	(0.094)	(0.089)	(0.068)	(0.090)	(0.107)	(0.065)
Purchases of						
Near substitutes	0.286***	0.304***	0.250***	0.169**	0.243**	0.158***
(maturity w/in 2 years of own)	(0.086)	(0.083)	(0.062)	(0.083)	(0.105)	(0.057)
Mid-substitutes	0.199***	0.204**	0.208**	0.093	0.250***	0.074
(maturity 2 to 6 years away)	(0.082)	(0.073)	(0.055)	(0.080)	(0.092)	(0.055)
# Obs.	563	360	769	154	249	674
# CUSIPS	131	121	123	23	53	114
Adj. R^2	0.974	0.975	0.976	0.986	0.986	0.977

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%; for a representative ten-year security with a duration of seven years, this translates into a yield decrease of about 0.3 basis points per billion dollars purchased. 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.[25] However, the coefficients are somewhat smaller for mid-substitutes, consistent with our hypothesis that local effects should die off as maturity distance increases. Applying the aggregate coefficients to averages of the dependent variables, and multiplying by the average inverse modified duration, we find that the typical effect of each operation was on the order of -3.5 basis points for the sector being purchased. (Operations averaged about $5 billion in size.) The second column of Table 8 shows that these results did not generally hold for issues with maturities greater than 15 years.

In the remainder of this section, we focus only on securities with less than 15 years to maturity, because that is where the great majority of purchases occurred. Within this subsample, Table 9 further splits the data into various groups to examine the consistency of the coefficients. First, we split the sample 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 among securities that were eligible for purchase—the coefficients are nearly identical for the two subperiods and are very close to the pooled results reported in the first column of Table 8. The split-sample results would also seem to rule out the possibility that the significant coefficients in the pooled sample were due only to the initial operations, in which market participants may have been surprised as they learned about the Desk's procedures. (We also tried splitting the sample at earlier dates, to further examine the latter possibility, and found that, if anything, the coefficient estimates were larger when the first few operations were excluded.)

Table 10
Flow effects on day of purchase-ineligible securities.
The dependent variable is the daily percentage price change in each outstanding CUSIP. The sample consists of Treasury coupon securities that were outstanding with a remaining maturity of more than 90 days and that were not eligible to be purchased on days when operations occurred. The sample period is March 18 – October 31, 2009. Fixed effects and daily time dummies not shown. Standard errors in parentheses. Asterisks indicate statistical significance at the 10% (*), 5% (**), and 1% (***) levels.

	< 15 years to maturity	> 15 years to maturity
Purchases of		
Near substitutes	0.0665***	−0.0268
(maturity w/in 2 years of own)	(0.018)	(0.053)
Mid-substitutes	0.0047	−0.007
(maturity 2 to 6 years away)	(0.0099)	(0.021)
Far substitutes	−0.0238**	0.0021
(maturity 6 to 14 years away)	(0.008)	(0.003)
# Obs.	8008	1104
# CUSIPS	181	23
Adj. R^2	0.52	0.96

[25] 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.

The middle columns of Table 9 split the sample into notes and bonds. Again, the results for the subsamples are generally similar to each other and to the results presented in Table 8 in terms of sign, magnitude, and significance. Similarly, the last two columns, which split the sample into securities more than five and less than six issues off-the-run, show no major differences. The modest exception is that the samples of bonds and far-off-the-run securities show somewhat smaller effects of substitute purchases. This is consistent with weaker substitutability among these relatively illiquid securities.

Table 11
Flow effects on day of purchase-ineligible securities by subperiod.

The dependent variable is the daily percentage price change in each outstanding CUSIP. The sample consists of Treasury coupon securities that were outstanding with a remaining maturity of between 90 days and 15 years that were not eligible to be purchased on days when operations occurred. The sample period is March 18–October 31, 2009. Fixed effects and daily time dummies not shown. Standard errors in parentheses. Asterisks indicate statistical significance at the 1% level.

	Mar 25–Jul 6	Jul 7–Oct 29
Purchases of		
Near substitutes	−0.127***	0.384***
(maturity w/in 2 years of own)	(0.025)	(0.031)
Mid-substitutes	−0.143***	0.202***
(maturity 2 to 6 years away)	(0.015)	(0.017)
Far substitutes	−0.153***	0.093***
(maturity 6 to 14 years away)	(0.014)	(0.011)
# Obs.	4529	3479
# CUSIPS	167	172
Adj. R^2	0.51	0.57

4.3.3. Results for ineligible securities

Table 10 displays results for securities that were ineligible for purchase, comparably to Table 8. In the aggregate, these responses display little economic or statistical significance. However, a further split of this sample reveals a more interesting pattern. Namely, as shown in Table 11, the coefficients on all of the substitute purchases are negative in the first half of the sample and positive in the second half. (As above, we focus here on the less-than-15-year sector of the yield curve, where most purchases took place.) 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

Table 12
Flow effects on day of purchase-ineligible securities by subperiod.

The dependent variable is the daily percentage price change in each outstanding CUSIP. The sample consists of Treasury coupon securities that were outstanding with a remaining maturity of between 90 days and 15 years and eligible to be bought in each purchase operation under the LSAP program, March 18–October 31, 2009. Only days when LSAP purchases occurred are included. Fixed effects and daily time dummies not shown. Standard errors in parentheses. Asterisks indicate statistical significance at the 10% (*), 5% (**), and 1% (***) levels.

Notes

	Eligible securities		Ineligible securities	
	Mar 25–Jul 6	Jul 7–Oct 29	Mar 25–Jul 6	Jul 7–Oct 29
Own purchases	0.445***	0.245**	–	–
	(0.147)	(0.122)		
Purchases of				
Near substitutes	0.396***	0.314***	−0.213***	0.486***
(maturity w/in 2 years of own)	(0.148)	(0.107)	(0.041)	(0.049)
Mid-substitutes	0.346***	0.260***	−0.244***	0.266***
(maturity 2 to 6 years away)	(0.144)	(0.086)	(0.037)	(0.038)
Far substitutes	–	–	−0.061***	0.063***
(maturity 6 to 14 years away)			(0.040)	(0.039)
# Obs.	442	327	3891	2960
# CUSIPS	108	111	144	149
Adj. R^2	0.979	0.972	0.477	0.548

Bonds

	Eligible securities		Ineligible securities	
	Mar 25–Jul 6	Jul 7–Oct 29	Mar 25–Jul 6	Jul 7–Oct 29
Own purchases	0.381***	0.108	–	–
	(0.159)	(0.129)		
Purchases of				
Near substitutes	0.270***	0.011	−0.208***	0.084*
(maturity w/in 2 years of own)	(0.109)	(0.134)	(0.049)	(0.046)
Mid-substitutes	0.178*	−0.161	−0.077***	0.074***
(maturity 2 to 6 years away)	(0.104)	(0.134)	(0.019)	(0.020)
Far substitutes	–	–	−0.077***	0.041***
(maturity 6 to 14 years away)			(0.014)	(0.012)
# Obs.	121	33	638	519
# CUSIPS	23	10	23	23
Adj. R^2	0.978	0.994	0.946	0.939

Table 13
Flow effects on day after purchase.
The dependent variable is the percentage price change in each outstanding CUSIP on the days after LSAP operations. The sample consists of Treasury coupon securities that were outstanding with a remaining maturity of between 90 days and 15 years and eligible to be bought in each purchase operation under the LSAP program, March 18 – October 31, 2009. Only days when LSAP purchases occurred are included. Fixed effects and daily time dummies not shown. Standard errors in parentheses. Asterisks indicate statistical significance at the 10% (*), 5% (**), and 1% (***) levels.

Notes

	Eligible securities		Ineligible securities	
	Mar 25–Jul 6	Jul 7–Oct 29	Mar 25–Jul 6	Jul 7–Oct 29
Own purchases	−0.379***	−0.145	–	–
	(0.143)	(0.116)		
Purchases of				
Near substitutes	−0.478***	−0.152	−0.464***	−0.135***
(maturity w/in 2 years of own)	(0.145)	(0.108)	(0.039)	(0.049)
Mid-substitutes	−0.620***	−0.106	−0.436***	−0.009
(maturity 2 to 6 years away)	(0.139)	(0.087)	(0.035)	(0.039)
Far substitutes	–	–	−0.308***	0.134***
(maturity 6 to 14 years away)			(0.039)	(0.038)
# Obs.	442	327	3886	2957
# CUSIPS	108	111	144	149
Adj. R^2	0.986	0.974	0.573	0.507

Bonds

	Eligible securities		Ineligible securities	
	Mar 25–Jul 6	Jul 7–Oct 29	Mar 25–Jul 6	Jul 7–Oct 29
Own purchases	0.377***	0.58***	–	–
	(0.145)	(0.117)		
Purchases of				
Near substitutes	0.157	0.556***	−0.047	0.086*
(maturity w/in 2 years of own)	(0.098)	(0.119)	(0.046)	(0.047)
Mid-substitutes	0.111	0.472***	−0.057***	−0.046***
(maturity 2 to 6 years away)	(0.082)	(0.125)	(0.018)	(0.020)
Far substitutes	–	–	−0.049***	0.015***
(maturity 6 to 14 years away)			(0.014)	(0.013)
# Obs.	121	33	638	519
# CUSIPS	23	10	23	23
Adj. R^2	0.99	0.99	0.94	0.93

little qualitative difference between eligible and ineligible securities.[26] Thus, the first half of the sample for the ineligible securities is the puzzling piece of the data. It seems possible that the differences between eligible and ineligible securities during this time may have had to do with the liquidity impairments in the market noted above, although the exact mechanism that would result in this pattern is unclear. Another possible explanation is that dealers initially anticipated being able to sell more to the Fed than they actually were able to sell and thus unloaded portions of their inventories (including securities that had not been eligible) in the wake of LSAP operations in order to maintain portfolio targets. 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 12 shows that the basic patterns described above for the eligible and ineligible securities in the first and second halves of the sample do not depend on the liquidity characteristics of the securities considered, as proxied by the split into notes and bonds. (Again the bonds are largely far-off-the-run issues that trade infrequently.) The coefficients on purchases of eligible securities are almost always positive and significant, with fairly consistent magnitudes,[27] while the negative coefficients for ineligible securities in the first half of the sample appear irrespective of security type.

4.3.4. The dynamic effects of purchases

Table 13 turns to the question of what happened on the days after LSAP operations took place. For comparison, the sample breakdown and independent variables are the same as those used in Table 12, 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

[26] Han, Longstaff, and Merrill (2007) found little difference in price responses between securities that were eligible and those that were ineligible for the 2000 Treasury buyback program.

[27] The sample of eligible purchases of bonds in the second half of the period contains only 33 observations and consequently the coefficients are not statistically significant. However, we also cannot reject that they are equal to their full-sample counterparts.

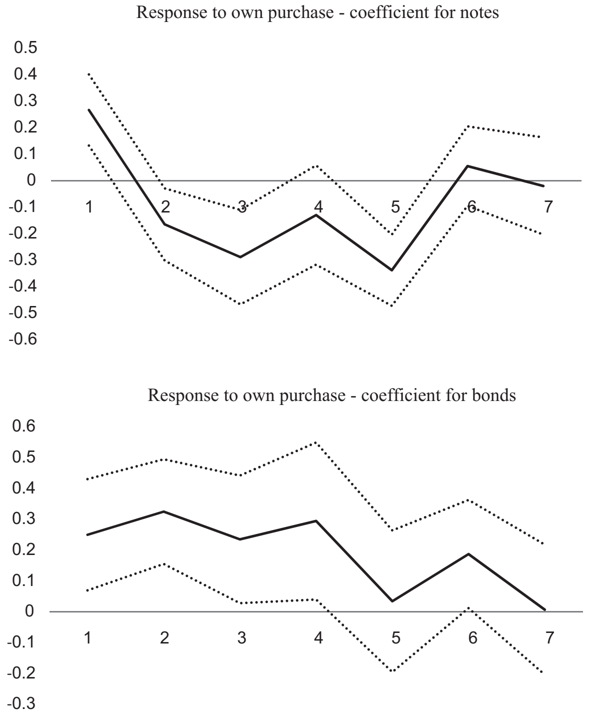

Fig. 6. Evolution over time of the flow effect. The figures plot the coefficients on purchases of a specific security, as a percentage of its near substitutes outstanding, in panel regressions where the dependent variables are cumulative percentage price changes for that security over horizons of one to seven days. The regressions control for fixed and time effects, purchases of substitutes, and lagged purchases. Dotted lines show two-standard-error bands.

on the days of purchases—the coefficients are of roughly similar magnitudes to those reported in the top panel of Table 12, but they are all negative (although they are not individually significant in the second half of the sample). This suggests that the local-supply flow effects among notes were short-lived.

To investigate and illustrate the dynamic effect further, the top panel of Fig. 6 shows for eligible notes the cumulative response of the prices to own purchases in the six-day period following the purchases (i.e., period 1 in the figure) estimated using the entire sample. It can be seen that by the second day after the purchase the flow-effect is fully reversed and that by the fifth day the coefficient stabilizes around zero.[28] On the other hand, for eligible bonds (which, again, tend to be less liquid), prices actually increased further on the day after purchases. Indeed, analysis of the cumulative price changes on subsequent days, summarized by the impulse response for the coefficient on own purchases (shown in the bottom panel of Fig. 6) suggests that the effects of LSAP purchases on these bonds took longer to reverse as the coefficient converged toward zero after six days from the day of purchase.[29]

These results suggest that even anticipated purchases can have significant local effects on prices in some circumstances. A brief spike and retreat in prices, such as occurred among notes, can be explained by settlement, clearing, and rebalancing frictions. But a more 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, as in less-liquid portions of the market, preferred-habitat investors' demand might dominate arbitrage activity.

Finally, turning to the day-after results for ineligible 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 ineligible bonds do not show a clear pattern, but the coefficient magnitudes are small compared to most of the other samples we have reported. In general (also taking into account Table 12), it does not appear that the prices of ineligible 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.

4.3.5. *Robustness to specification issues*

Tables 14 and 15 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—indeed, in some cases the clustered standard errors are smaller than in the baseline case (suggesting negative correlation within clusters). We also clustered by security type (not shown) and did not observe any notable differences with the results reported above.

Another concern is that the Fed's purchases may have been correlated with other supply-and-demand factors that could have affected yields on particular securities. To address this possibility, Table 16 shows the estimates of the flow effects for eligible securities controlling for changes in the repo specialness spread at the CUSIP level. As discussed in Section 3, since this spread represents the premium that market participants pay to borrow particular securities, it can proxy for the scarcity of an issue; indeed, Duffie (1996) shows that repo specialness can have effects on the pricing of on-the-run Treasury issues. As the spread is computed employing data as of the morning before the release of the Fed auction's results, it should not be simultaneously affected by the purchase

[28] The impulse responses for the coefficients on substitute purchases for both notes and bonds preserve the same shape of that one on own purchases and as such are not shown.

[29] 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 controlling for lagged purchases and lagged price changes, the coefficient estimates were essentially unchanged.

Table 14
Flow effects on day of purchase-all securities, clustered standard errors.

The dependent variable is the daily percentage price change in each outstanding CUSIP. The sample consists of Treasury coupon securities that were outstanding with a remaining maturity of between 90 days and 15 years and eligible to be bought in each purchase operation under the LSAP program, March 18 – October 31, 2009. Only days when LSAP purchases occurred are included. Fixed effects and daily time dummies not shown. Standard errors, computed by clustering observations into one-year maturity buckets, are shown in parentheses. Asterisks indicate statistical significance at the 10% (*), 5% (**), and 1% (***) levels.

	Eligible securities		Ineligible securities	
	< 15 Years	> 15 Years	< 15 Years	> 15 Years
Own purchases	0.2763***	−0.1063	–	–
	(0.044)	(0.089)		
Purchases of				
Near substitutes	0.2403***	−0.1238***	0.0665***	−0.0268
(maturity w/in 2 years of own)	(0.038)	(0.029)	(0.029)	(0.017)
Mid-substitutes	0.1700***	−0.0501**	0.0047	−0.007
(maturity 2 to 6 years away)	(0.053)	(0.014)	(0.016)	(0.037)
Far substitutes	–	–	−0.0238**	0.0021
(maturity 6 to 14 years away)			(0.009)	(0.003)
# Obs.	923	145	8008	1104
# CUSIPS	146	23	181	23
Adj. R^2	0.976	0.985	0.519	0.968

Table 15
Flow effects on day of purchase-subsamples, clustered standard errors.

The dependent variable is the daily percentage price change in each outstanding CUSIP. The sample consists of Treasury coupon securities that were outstanding with a remaining maturity of between 90 days and 15 years and eligible to be bought in each purchase operation under the LSAP program, March 18–October 31, 2009. Only days when LSAP purchases occurred are included. Fixed effects and daily time dummies not shown. Standard errors, computed by clustering observations into one-year maturity buckets, are shown in parentheses. Asterisks indicate statistical significance at the 10% (*), 5% (**), and 1% (***) levels.

	Bonds	Notes	Near on-the-run	Far off-the-run
Own purchases	0.2498**	0.2669***	0.2318**	0.2488***
	(0.06)	(0.058)	(0.098)	(0.051)
Purchases of				
Near substitutes	0.1694**	0.2503***	0.2435**	0.1584***
(maturity w/in 2 years of own)	(0.03)	(0.051)	(0.073)	(0.029)
Mid-substitutes	0.0929	0.2088**	0.2501***	0.0744
(maturity 2 to 6 years away)	(0.035)	(0.059)	(0.059)	(0.026)
# Obs.	154	769	249	674
# CUSIPS	23	123	53	114
Adj. R^2	0.986	0.976	0.986	0.977

operations, which should mitigate endogeneity concerns in our regressions. For the entire sample of eligible securities, the second column shows that the coefficient for the own and substitute purchases are extremely similar to the baseline estimates shown in the second column of Table 8. This is in line with the preliminary analysis of the repo market presented in Section 3.4. In addition, also the results across the different cuts of the data, shown in the last four columns of Table 16, confirm the robustness of the estimates to the inclusion of the repo specialness spread, as these are very similar to those shown in Table 9, with the exception of the coefficients for bonds that are slightly smaller in magnitude and not significant for the substitutes.

5. Implications for subsequent LSAPs

The previous section demonstrates that, during the period of the Fed's first LSAP program, Treasury yields responded significantly to purchases in the specific sectors where those purchases occurred. Collectively, these local-supply effects resulted in a downward shift of Treasury yields at most maturities on the order of 30 basis points. If this decline was passed through to private credit markets—as the FOMC's announcement made clear was the intent of the program—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 LSAP program suggests that much of it may have. It thus appears that the Treasury LSAP program that we analyze was probably successful in its stated goal of broadly reducing interest rates, at least relative to what they would otherwise have been.

Taken at face value, these results suggest that, properly structured and under certain conditions, asset-purchase programs can be powerful tools for reducing longer-term

Table 16
Flow effects on day of purchase-subsamples for eligible securities, controlling for repo spread.
The dependent variable is the daily percentage price change in each outstanding CUSIP. The sample consists of Treasury coupon securities that were outstanding with a remaining maturity of between 90 days and 15 years and eligible to be bought in each purchase operation under the LSAP program, March 18–October 31, 2009. Only days when LSAP purchases occurred are included. Fixed effects and daily time dummies not shown. Standard errors in parentheses. Asterisks indicate statistical significance at the 10% (*), 5% (**), and 1% (***) levels.

	All < 15 Years	Notes	Bonds	Near on-the-run	Far off-the-run
Own purchases	0.2712***	0.2536***	0.2244**	0.2349**	0.2361***
	(0.058)	(0.070)	(0.109)	(0.114)	(0.076)
Purchases of					
Near substitutes	0.237***	0.2304***	0.1315	0.2408**	0.1358**
(maturity w/in 2 years of own)	(0.055)	(0.065)	(0.106)	(0.111)	(0.070)
Mid-substitutes	0.1955***	0.2449**	0.068	0.2565***	0.0801
(maturity 2 to 6 years away)	(0.051)	(0.057)	(0.100)	(0.097)	(0.066)
Change in repo specialness	−0.0005	−0.001	−0.0043	−0.0009	−0.0007
spread	(0.0011)	(0.001)	(0.0051)	(0.0014)	(0.002)
# Obs.	817	717	100	235	582
# CUSIPS	143	122	21	53	110
Adj. R^2	0.978	0.977	0.988	0.986	0.979

interest rates and thus, presumably, for stimulating economic activity. Our findings imply that such programs may achieve their maximum effects when directed at the least-liquid segments of the market—as, for example, in the seven- to 15-year maturity range, where most of the off-the-run bonds were concentrated in our sample. Of course, the desirability of concentrating purchases in particular sectors in future purchase programs must also depend on other factors, including the conditions prevailing in the market at the time and the particular set of yields that policymakers see as appropriate targets.

Importantly, as we have emphasized, our estimated effects are only those related to local supply—we sweep out the effects that the program may have had by changing the duration risk held by the market. Nonetheless, the magnitudes of the effects that we estimate are large relative to other estimates in the literature of the effects of LSAPs on yields.[30] It thus appears to be the case that the local-supply effects were the primary contributors to the yield responses during the first LSAP program. If so, local-supply effects may represent an important (and understudied) channel for central bank balance-sheet actions to affect financial conditions. Recent theory in this area suggests that such supply effects in the Treasury market are more likely to arise in environments of heightened risk aversion and limits to arbitrage. Indeed, these arguments suggest that LSAP-type programs may be particularly effective at reducing interest rates during periods of market stress. The flip side of this argument, however, is that one should be cautious about extrapolating our results to other periods. The singular market environment observed in the wake of the financial crisis may have led to outsized risk aversion and illiquidity that caused the effects of LSAPs to be larger than they otherwise would have been.

Possible support for this conjecture can be found by comparing our results to those of Meaning and Zhu (2011), who use our methodology to estimate the stock- and flow-effects of the Fed's second round of Treasury purchases (LSAP2), which involved $600 billion of transactions conducted between November 2010 and June 2011. They find that LSAP2 lowered the yield curve, on average, by 21 basis points through the stock effect.[31] Thus, although the total amount of purchases under LSAP2 was twice as large as LSAP1, it had a smaller overall effect on yields through the local-supply channel. (Interestingly, Meaning and Zhu estimate the largest impact for securities with remaining maturity of about 20 years, while we find that the impact in the more-than-15-year sector is negligible and not statistically significant for LSAP1.) For the flow effects, they find that a typical LSAP2 operation—which was about twice as large as a typical LSAP1 operation—reduced yields by 4.7 basis points on the day it occurred, indicating that the actual purchases also had a smaller impact during the second round than we find. Again, a likely reason for these findings is the improved risk sentiment and better market functioning that prevailed during the second LSAP program.

If local-supply effects account for most of the response of yields during the first LSAP program, it must be the case that the duration effects of that program were relatively

[30] For example, event studies like Gagnon, Raskin, Remache, and Sack (2010) find that the Treasury, agency debt, and agency MBS purchase programs announced in March 2009 together reduced the ten-year yield by a total of about 40 to 50 basis points. Our estimates suggest that roughly 30 basis points of this response was due to the local-supply effects of the Treasury program.

[31] In interpreting the Meaning and Zhu (2011) results, it is important to bear in mind certain differences between LSAP1 and LSAP2 that cloud the comparison to our paper. Namely, compared to the first round of Treasury purchases, investor expectations for LSAP2 are harder to pin down—investors started pricing in the likelihood of additional Treasury purchases well before its announcement, in part due to the introduction of the Fed's Treasury reinvestment program on August 10, 2010. In addition, there were certain implementation differences. For example, the Fed relaxed its self-imposed SOMA portfolio limits during LSAP2, and the Desk announced the full maturity distribution of purchases prior to commencing the program.

modest. There is no obvious reason to expect that duration effects should have been abnormally low during this period—indeed, theory suggests that those effects should also be greater during periods of high risk aversion. This raises the question of how effective asset-purchase programs can be during periods of relatively low market stress, when local-supply effects are likely to be small and LSAPs must work primarily through the duration effects on term premiums. It also implies that asset sales from the Fed's portfolio, which are presumably more likely to be conducted in an environment of improved functioning, may have smaller effects than the purchases of those same securities did.

6. Conclusion

In this paper, we have used CUSIP-level data to estimate the local 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 the flow and stock effects were statistically and economically significant. Specifically, we estimate that the average purchase operation temporarily reduced yields by about 3.5 basis points in the sector of the purchase and that the local-supply effects of the program as a whole shifted the yield curve down by up to 30 basis points, with both effects concentrated at medium maturities. These are distinct from any effects the program may have had through other channels, such as lowering the expected path of short-term rates, removing duration from the market, or improving overall market liquidity. The results suggest that asset-purchase programs can be effective in reducing longer-term interest rates, at least during periods of market stress, particularly when purchases occur in the less-liquid segments of the market.

Our findings provide support for preferred-habitat and portfolio-balance theories of the term structure. Consistent with such theories, we find that: withdraws of Treasury supply decreased yields by an economically meaningful amount; these decreases were generally biggest for the specific securities being bought and for securities of similar maturities, but smaller for securities with much different maturities; particularly for stock effects, the discrepancies between own purchases and substitute purchases were larger among less-liquid securities, namely off-the-run bonds; also among off-the-run bonds, the flow effects were more persistent, suggesting that they were not just due to microstructure-related distortions.

Our study is the first to specifically consider local-supply effects on the yield curve, but the overall magnitudes of our stock-effect estimates are roughly comparable to what would be implied by Treasury price elasticities found in some other research, such as Kuttner (2006). As far as we are aware, no previous study has estimated flow effects controlling for different degrees of substitution (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.

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