

# Assessing asset purchases within the ECB's Securities Markets Programme \*

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## Abstract

We assess the yield impact of asset purchases within the ECB's Securities Markets Programme in five euro area sovereign bond markets during 2010-11. Identification is based on panel regression on predetermined purchases and control covariates. In addition to large and economically significant announcement effects, we find an average impact of approximately -3 basis points at a five-year maturity for purchases of 1/1000 of the respective outstanding debt. Bond yield volatility is lower on intervention days for most SMP countries, due to less extreme movements occurring when the Eurosystem is active as a buyer. A dynamic specification points to both transitory and longer-lived effects from purchases. Our result suggest that the signalling of future low money market rates is not necessary for an LSAP to 'work'.

**Keywords:** Central bank asset purchases, European Central Bank, Securities Markets Programme, effectiveness of non-standard monetary policy measures.

**JEL classification:** C32, G12.

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# 1 Introduction

Exceptional times may require exceptional policy measures. Since the onset of the financial crisis in 2007, central banks around the globe have implemented both standard and non-standard monetary policy measures in an attempt to contain financial instability and to avoid economic contraction. Since October 2008, non-standard monetary policy measures in the euro area have included fixed rate full allotment tender procedures that provide central bank funding to the financial sector at a low and predictable interest rate, expansions of the set of eligible collateral, foreign exchange swap lines, longer-term refinancing operations with maturities of up to three years, and purchases of covered bonds and government bonds within asset purchase programs, see Lenza, Pill, and Reichlin (2010) and Eser et al. (2012). Other non-standard monetary policy measures have been undertaken by other major central banks such as, for instance, the Federal Reserve System, the Bank of England, and the Bank of Japan.

The effectiveness of the ECB's Securities Markets Programme (SMP) - in line with that of other asset purchase programs - is subject to intense academic, public, and policy debate. The objective of this paper is to quantify the financial market impact of asset purchases undertaken within the SMP, in particular on yield levels and yield volatility. We study SMP interventions in government debt securities markets during 2010-2012 in five euro area countries: Greece, Ireland, Portugal, Spain, and Italy. Approximately €214 billion (bn) of such bonds were acquired from 2010 to early 2012, see Eser et al. (2012) and ECB (2013).<sup>1</sup> We address the following questions: How is the SMP different from other asset purchase programs? Have SMP asset purchases affected bond yields in secondary debt markets for the respective countries? If so, how much? Have purchases affected the volatility and extreme tail behavior of yield changes? Are the effects entirely temporary or longer lived? While a decomposition of yield impact into different potentially active transmission channels is outside the scope of this paper, we argue that the yield impact was not achieved by signaling future low monetary policy rates, i.e., through a classical 'signalling' channel.

Compared to other central bank asset purchase programs, the SMP differs in several

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<sup>1</sup>At the end of 2012 the ECB held €99bn in Italian sovereign bonds, €30.8bn in Greek debt, €43.7bn in Spanish debt, €21.6bn in Portuguese debt and €13.6bn in Irish bonds, according to the ECB (2013) Annual Report. Interestingly, during the press conference on 21 February 2013 the ECB also reported that it earned €555mm in 2012 on its holdings of Greek sovereign bonds that were bought during the crisis.

dimensions, and in particular contains features that resemble foreign exchange intervention. First, purchases within the SMP occurred during a severe sovereign debt crisis, when sovereign yields in several euro area countries were high, rising, and volatile. During this phase, the targeted securities met little private sector demand. The purchases were undertaken during intense phases of the debt crisis and in the markets most affected by the crisis. This is in contrast to the setting of the Federal Reserves large-scale asset purchases (LSAPs) and the Bank of Englands quantitative easing (QE) setting, for which longer term yields and yield volatilities are relatively low and default risk premia are negligible. Second, key features of the program - such as total amounts, the duration of the program, as well as the targeted securities - were not disclosed while the program was active. Apart from the initial announcement about the SMP, market participants learned about the program as purchases were implemented in a non-anonymous dealer market. Finally, the introduction of the SMP was subject to significant controversy, both outside but also within the Eurosystem, as demonstrated by the public resignation of the Bundesbank president at that time and an ECB executive board member in late 2011.

The SMP was announced on 10 May 2010, with the objective of helping to restore the monetary policy transmission mechanism by addressing the mal-functioning of certain government bond markets, see for instance González-Páramo (2011). The SMP consists of interventions in the form of outright secondary market purchases. Implicit in the concept of impaired markets is the notion that government bond yields can be unjustifiably high and volatile, see Constâncio (2011). Importantly, the SMP is not designed to make the monetary policy stance more accommodative as such. Therefore, the liquidity effect resulting from SMP interventions is sterilized through a term deposit facility. While the overall objective of the SMP was to restore monetary policy transmission, we assess the SMP by investigating the yield impact per euro spent. We focus on the impact of the actual bond purchases, and treat announcement effects as important additional effects. The SMP was replaced by the Outright Monetary Transactions, or OMT program, on 6 September 2012. The SMP and OMT are related but different programs, see Coeuré (2013).

In general, the identification of the yield impact of bond market interventions is a challenging task. A cursory look at bond yields and bond purchases within the SMP can indeed,

and as suggested by some<sup>2</sup>, lead to the impression that the SMP was overall ineffective. In particular, yields rose over time as purchases were being implemented. We document that yield changes and SMP purchase amounts at a daily frequency are positively correlated for the five countries where (and when) the SMP intervened. As a result, simple regression-based techniques that relate yield changes to purchase amounts lead to positive impact coefficients. These approaches, however, neglect the presence of a common factor - the escalating sovereign debt crisis - which explains in part both the rising yields (left-hand-side variable) and the activation of the non-standard monetary policy measure (right-hand-side variable). The issue of ‘impact identification’ is thus of crucial importance.

In our framework, identification is based on a panel regression and essentially comes from the cross-sectional dimension of the data. As a result, it does not matter for identification that yields rose over time in many euro area countries during the sovereign debt crisis. Instead, what matters is whether, after controlling for other factors, yields rose *relatively less* in those markets in which more purchases were undertaken on a given trading day. Observed and unobserved (latent) factors allow us to control for the fact that purchases were undertaken against the backdrop of a severe and escalating sovereign debt crisis. Importantly, we also explain how coordination within the Eurosystem requires the purchase decisions and purchase amounts to be predetermined at a daily frequency.

In this paper we show that government bond purchases undertaken within the SMP were effective in lowering yields even despite the context of the severe sovereign debt crisis and the controversy which surrounded it. We report three main empirical findings.

First, in addition to large and statistically significant announcement effects, we find that the recurring interventions had an impact ranging from approximately -1 to -2 basis points (Italy) and up to -17 to -21 basis points (Greece) at the five-year maturity per €1 bn of bond purchases. The remaining impact estimates take intermediate values, from approximately -3 bps/bn (Ireland), -4 to -6 bps/bn (Spain), and -6 to -9 bps/bn (Portugal).<sup>3</sup> The cross-country differences in yield impact can be explained by different sizes of the respective markets and possibly a default risk signal. These estimates, both in terms of €1 bn and in terms of relative market size, are considerably larger than what is found in the literature

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<sup>2</sup>See, for instance, Fulcrum Research Notes, January 2013 or Natixis Flash Economics Economic Research, 13 September 2013.

<sup>3</sup>Ranges per country refer to different point estimates across model specifications in Section 4.1.

for purchases of U.S. Treasuries during 2008-09 within the Federal Reserve's Large-Scale Asset Purchase (LSAP) program, see for example Gagnon, Raskin, Remache, and Sack (2011), Krishnamurthy and Vissing-Jørgensen (2011), D'Amico and King (2013), and Cahill, D'Amico, Li, and Sears (2013). This is intuitive given the stark differences in terms of market stress, market illiquidity, and default risk premia for some euro area countries in financial and economic difficulties during 2010-2012.

A given amount of purchases naturally has a larger impact in a relatively smaller debt market. Per 1/1000 of the respective debt market, the impact estimates are approximately -3 basis points at a five-year maturity on average. We rationalize the relatively large impact effects from SMP purchases in terms of reduced liquidity risk, local supply effects in segmented markets, and default risk signalling effects, see Duffie, Garleanu and Pedersen (2005, 2007) and Vayanos and Vila (2009). First, the flow of purchases from an investor of last resort trivially reduces liquidity risk premia by making a counterparty easier to find. Second, purchases of sovereign bonds reduce the local supply of government bonds. If demand for such bonds is not perfectly elastic, for example due to severe market segmentation, a reduction in supply raises prices and lower yields. Finally, the flow of purchases may have been perceived as a signal that the ECB's Governing Council regards country yields as higher than justified based on country fundamentals, for example due to contagion concerns, high liquidity risk premia, or markets coordinating on a bad equilibrium, and that it is willing to consider and implement non-conventional approaches to combat the crisis.

Interestingly, the 'signalling channel' of Bauer and Rudebusch (2011) and Christensen and Rudebusch (2012), according to which purchases affect medium and long term rates because they signal low future monetary policy rates, is unlikely to be active for SMP purchases. The ECB's Governing Council stressed throughout that the monetary policy stance remains unaffected, and that a 'separation principle' applies, according to which non-standard monetary policy measures are complements, not substitutes, to interest rate policies. The additional central bank liquidity in circulation due to the SMP was sterilized, and no forward guidance on yields was offered. Government bond yields in non-stressed countries, as well as money market forward swap rates, moved little upon the announcement of the program. Collectively then, our empirical estimates suggest that the signaling of future low monetary policy rates is not necessary for an LSAP to 'work'.

Second, we document that bond yield volatility as well as the probability of observing extreme yield changes, as proxied by standard deviation, kurtosis, and Hill tail index estimates, respectively, are lower on intervention days for most SMP countries, as purchases mitigate extreme downside price movements on intervention days. The kurtosis and tail index estimates suggest that the lower standard deviation on intervention days compared to non-intervention days is due to fewer extreme movements. This is relevant since high uncertainty about future bond yields alone may force institutional investors and capital constrained market makers to leave a given market, in particular if there are binding value-at-risk constraints, see for example Vayanos and Vila (2009) and Adrian and Shin (2010), and Pelizzon et al. (2013) for anecdotal evidence for Italy.

Finally, we address the question about how long lasting the effects of intervention are. Given that the SMP purchases are repeated interventions in the same market, we estimate a dynamic specification that allows for lagged effects from contemporaneous purchases. We find both transitory and long-run effects, and estimate that the total long run impact is approximately three quarter of the immediate impact. We conjecture that longer-lived effects from purchases are due to longer-lasting reductions in the local supply of bonds (*ceteris paribus*), as bonds are known to be kept on the central bank's balance sheet until their maturity. In addition, a signal that pertains to the default risk premium may also have longer-lasting effects.

The remainder of the paper is structured as follows. Section 2 discusses the related literature on SMP impact and relates its key features to those of other central bank asset purchase programs. Data and the modeling strategy are presented in Section 3. Section 4 summarizes our main empirical findings. Section 5 considers changes in volatility and changes in the risk of extreme market movements during intervention and non-intervention days. Section 6 concludes.

## **2 Asset purchases and yield impact**

### **2.1 Related literature on SMP impact**

The two papers that are most related to ours are De Pooter, Martin, and Pruitt (2013) and Ghysels, Idier, Manganelli, and Vergote (2013). De Pooter et al. (2013) contribute to the

literature in two ways. First, they provide a theoretical search-based asset pricing framework that rationalizes short term and long run price effects from recurring bond market interventions. Second, they test empirically whether the SMP had an impact on sovereign bond liquidity premia. The authors find an average impact of -2.3 basis points on average for purchases within the SMP of 1/1000 of the respective outstanding debt, and document both transitory as well as long-run effects from purchases. Compared to De Pooter et al. (2013), we focus on the identification of the overall yield impact instead of focusing on liquidity risk premia only. Furthermore, we assess volatility effects and the impact on extreme market movements. Most importantly, we use the actual confidential data of daily recorded purchases and do not rely on a rule of proportionality to infer them from the weekly disclosed information.

Ghysels et al. (2013) analyse the yield impact of SMP asset purchases by considering the high-frequency dynamics of bond yields and asset purchases, rather than relying on data sampled at the daily frequency. By estimating regression models based on data sampled at 15-minute intervals, they minimize the bias that is introduced by unobserved third factor effects. This approach leads to reliable local impact estimates on conditional moments. On average over time, the authors find that a EUR 100 million intervention has an immediate impact on bond yields of between -0.1 and -25 basis points, depending on the debt market and timing. Based on volatility time series models, their study also suggests that SMP purchases have contributed to reducing the volatility of targeted government bond yields. In contrast to their study, we go beyond *local* high frequency impact and volatility effects. In contrast to both studies, we consider purchases for all five SMP countries and relate the cross-country variation to observed market characteristics such as debt market size.

Other research studies also investigate the impact of the SMP, but are less closely related. Trebesch and Zettelmeyer (2013) focus on the yield impact of SMP purchases of Greek government bonds in May and June 2010. Their identification strategy is based on cross sectional regressions at the bond level which compares bonds that were bought to bonds that were not bought. Purchased bonds show a much larger drop in yields after the start of the SMP. The authors document that purchases of 1 bn Euro resulted in a drop of yields by up to -204 basis points during the first eight weeks of the programme. These authors also note that, in the case of Greece, most purchased bonds have been benchmark bonds at some point during

their maturity. Finally, Beetsma, de Jong, Giuliadori, and Widijanto (2014) investigate the impact of the SMP on the volatility and the co-movement of sovereign bond yields in the euro area, as captured by realised volatility and correlation measures from intraday data. The authors find statistically and economically large effects, and conclude that the SMP announcement and purchases contributed to weaken the observed positive co-movement of yields among distressed countries during the euro area crisis, and reduced flight-to-safety capital flows from distressed countries to non-distressed countries.

## 2.2 How is the SMP different from other purchase programs?

Before the Eurosystem started its government bond purchases in May 2010, both the Federal Reserve within its Large Scale Asset Purchase (LSAP) programs and the Bank of England within its Quantitative Easing (QE) programs also embarked on outright purchases of government bonds. This section explains how the SMP differs from these programs with respect to the overall objective, market conditions, implementation strategy, and likely channels of effectiveness.

The SMP has a *different objective* compared to LSAP and QE. LSAP and QE can be seen as purchase programs that make the monetary policy stance more accommodative once the main policy interest rate has reached its lower bound. In contrast, the aim of the SMP is to address a perceived mal-functioning of the monetary policy transmission mechanism. The transmission of the monetary policy stance for countries with mal-functioning bond markets is to be aligned with that of the rest of the euro area. The SMP is not designed to make the monetary policy stance more accommodative as such. Therefore, the liquidity effect resulting from SMP interventions is sterilized. In this sense, the SMP is a complement, rather than a substitute, for standard interest rate policy.

Second, concerning *market conditions*, the SMP is active in government bond markets whose depth and liquidity is impaired. This lack of depth and liquidity, in turn, is related to concerns about the sustainability of public finances and the associated default risk premia. This stands in contrast to LSAP and QE, see D'Amico and King (2013) and Joyce, Lasaoa, Stevens and Tong (2011), respectively. Both the U.S. and U.K. bond markets are large in size, liquid, and generally perceived as safe havens with low default risk premia.<sup>4</sup>

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<sup>4</sup>It is not only the market conditions per se that may matter for the yield impact, but also the market

Third, in terms of *implementation strategy*, both LSAP and QE programs announced total amounts of purchases over certain time horizons. The actual purchases are usually undertaken in the form of auctions at relatively constant intervals. By contrast, whilst on the two key announcement dates for the SMP - the initial announcement on 10 May 2010 and that of the reactivation of the program on 7 August 2011<sup>5</sup> - the ECB announced government bond purchases and their objective, the ECB did not disclose the total amounts that would be spent, a time frame over which the program would be active, or a set of securities that would be targeted. These marked differences in communication also imply that event study methodologies around announcement days are less appropriate for our data. In the case of the SMP, almost no details apart from the fact that interventions would be undertaken were disclosed on announcement days. Similarly, no public meetings and policy announcements accompany the purchases.

Finally, bond purchases under the SMP also send a signal. This signalling, however, is probably different from the “signalling channel” in Bauer and Rudebusch (2011) and Christensen and Rudebusch (2012), where purchases signal future low short term interest rates. Such signalling of future low monetary policy rates is unlikely with the SMP, as the monetary policy stance remains unaffected and the impact on central bank liquidity in circulation is sterilized. In case of the SMP, however, purchases may have been perceived to signal something else. For example, purchases may signal that the Eurosystem regards country yields as higher than justified based on country fundamentals. This may be due to high liquidity risk and cross border contagion concerns. Purchases may also have meant that the Eurosystem is willing to consider and implement unprecedented non-conventional approaches to combat the crisis. As non-standard monetary policy measures are potentially costly, they can increase the impact of central bank communication and signalling in a strategic setting, see for example Hoerova, Monnet, and Temzelidesc (2012). In related settings, a central bank can help coordinate market expectations in a setting of multiple equilibria, see Corsetti and Dedola (2013) and references therein. Finally, as no specific duration was announced, the flow of purchases provides the (trivial) information that the SMP is still

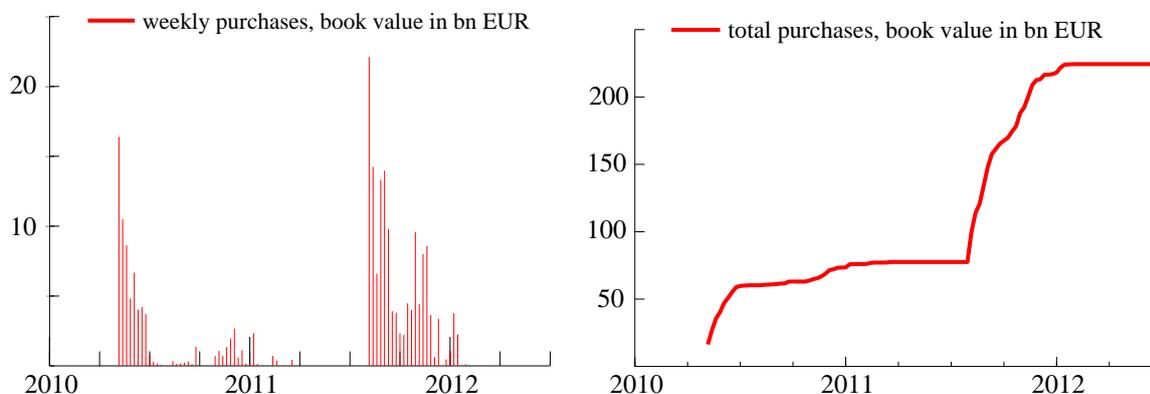
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conditions relative to some norm. While it is true that U.S. markets were in much better shape than European markets, they may have been in poor shape relative to their norms during 2008-10.

<sup>5</sup>See the press releases “ECB decides on measures to address severe tensions in financial markets” from 10 May 2010, and “Statement by the President of the ECB” from 7 August 2011.

**Figure 1: Weekly and total SMP purchase amounts**

The figure plots the book value of settled SMP purchases as of the Friday of a given week. We report weekly purchases across countries (left panel) as well as the respective cumulative amounts (right panel). Maturing amounts are excluded.



active.

### 3 Data and regression setup

#### 3.1 Data

We use data from three sources for this study. First, we consider SMP bond purchases by country at a daily frequency. Bond purchases are entered at par values. Assets are purchased in over-the-counter dealer markets via non-anonymous trades. On intervention days, market participants learn relatively quickly that SMP-related trades are taking place. Figure 1 plots weekly total purchases across countries as well as their accumulated book value over time. Noticeably, the weekly purchase data is not spread out evenly over time. The largest purchases occurred after the introduction of the SMP on 10 May 2010 and after its reactivation on 8 August 2011. There are long periods during which the SMP is open but inactive. From the week ending in 25 March 2011 until 8 August 2011 the SMP is inactive for 19 weeks. This is in stark contrast to the regular auctions undertaken, for example, by the Federal Reserve during the LSAP. The daily cross country breakdown of the purchase data is confidential at the time of writing. However, the ECB released its cross-country holdings at the end of 2012 in its 2013 Annual Report, see footnote (1) for the amounts. We

use the confidential daily and country-specific data for this study.<sup>6</sup>

As a second panel, we consider government bond yields at a five year maturity for ten euro area countries: Austria (AT), Belgium (BE), Germany (DE), Spain (ES), France (FR), Greece (GR), Ireland (IE), Italy (IT), Netherlands (NL) and Portugal (PT). The five SMP countries are a subset of these countries. SMP interventions focused on the two to ten year maturity bracket, with the five year maturity in the middle of that spectrum. As a result, we focus on the impact at the five year ‘midpoint’ of the yield curve, and consider five year benchmark bonds. The yield data are from Bloomberg and computed from dealer prices. Yield data are at a daily frequency from 1 October 2008 to 20 December 2011. Thus, the estimation sample starts shortly after the bankruptcy of Lehman Brothers on 15 September 2008 and the Irish government guarantee for six large Irish banks on 30 September 2008, which together mark the beginning of a substantial re-pricing of European sovereign debt by international investors. The sample ends before the allotment of the first three-year ECB longer-term refinancing operation (LTRO) on 21 December 2011. The LTRO had a considerable impact on the dynamics and levels of sovereign bond yields (Acharya and Steffen (2013)), which we do not want to confound with the impact of the SMP.

Table 1 reports simple summary statistics of yield changes in SMP countries. We distinguish a pre-debt crisis (1 Oct 2008 to 31 Mar 2010) and debt crisis sample (1 Apr 2010 to 20 Dec 2011), and further distinguish intervention days from non-intervention days. Intervention days are country-specific, i.e., days on which purchases took place in the debt market of a particular country. Pre-debt crisis yield changes exhibit a lower mean and lower volatility than on non-intervention days during the crisis in all five countries. This is intuitive, as increasing and volatile yields are the symptom of a debt crisis that is worsening over time. The sample skewness and kurtosis measures confirm that almost all yield change distributions are characterized by occasional extreme market movements (‘fat tails’). Excluding the announcement days, the mean yield change on intervention days are larger than the mean yield change for pre-crisis non-intervention days in all five countries. This is again intuitive because the interventions happen as the debt crisis escalates.

Figure 2 plots the development of yields since 1 January 2008 for five SMP countries (top

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<sup>6</sup>Approximate weekly data has been assembled by Barclays (2012). This approximate data is used in e.g. De Pooter, Martin, and Pruitt (2013).

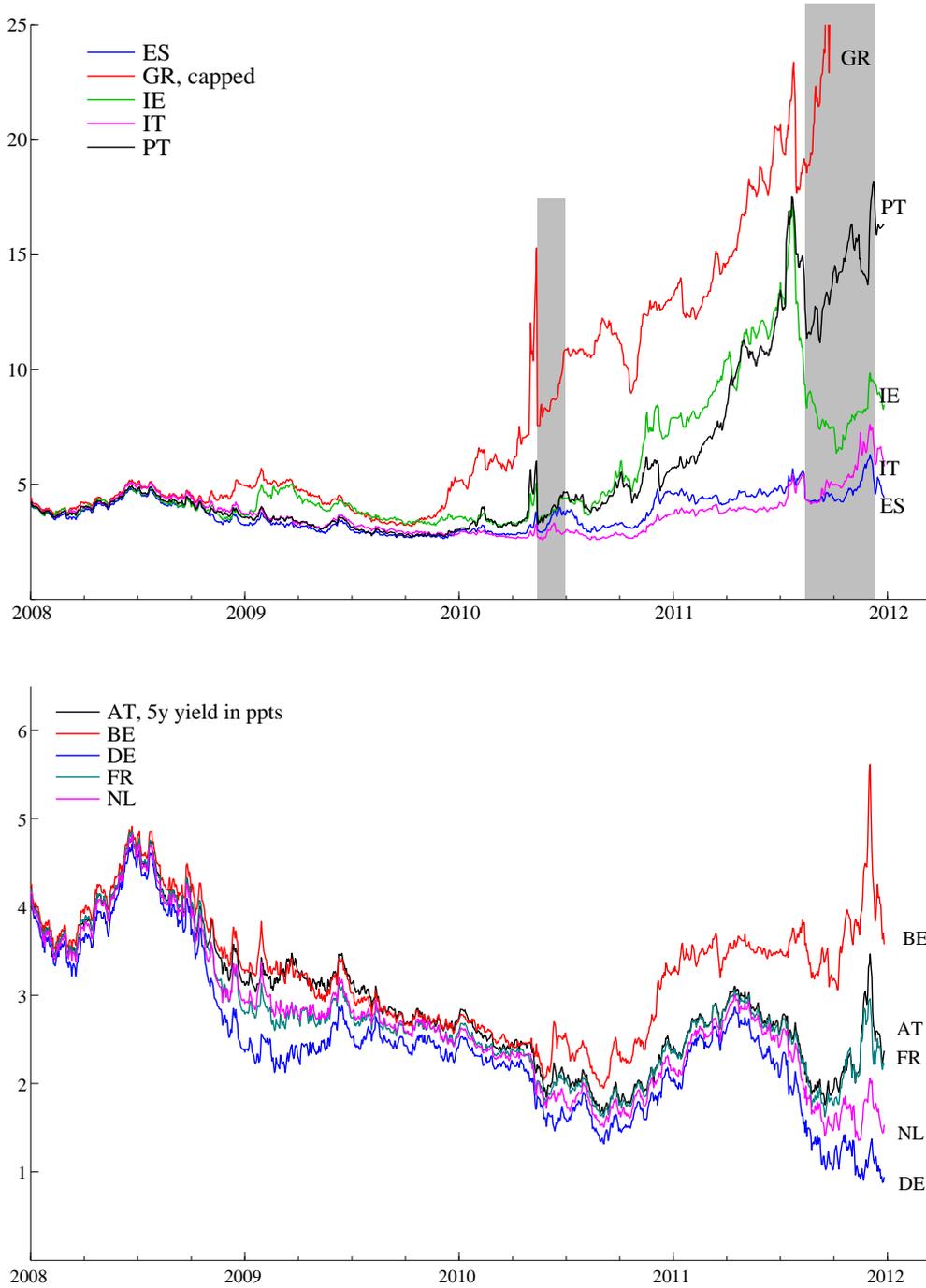
**Table 1: Summary statistics for yield changes**

Yield changes are in basis points and refer to a five year benchmark bonds. For non-intervention days, we distinguish a ‘pre-crisis’ subsample from 1 Oct 2008 to 31 March 2010, and a ‘debt crisis’ subsample from 1 Apr 2010 to 20 Dec 2011. The reported kurtosis is raw (not excess) kurtosis. The announcement days 10 May 2010 and 8 August 2011 are excluded from the intervention day column.

Country	Statistic	Non-intervention days			Intervention days
		all	pre-crisis	debt crisis	
ES	Mean	0.0	-0.4	0.3	2.3
	Median	0.2	-0.2	0.7	2.2
	Std. Dev.	8.4	5.4	10.6	10.5
	Skewness	-1.2	0.2	-1.3	0.0
	Kurtosis	17.5	4.0	13.8	3.2
GR	Mean	9.1	0.4	18.8	5.9
	Median	0.8	0.1	4.0	3.6
	Std. Dev.	47.5	9.8	67.1	20.0
	Skewness	1.3	-0.6	0.5	0.4
	Kurtosis	21.0	11.1	10.8	7.4
IE	Mean	0.4	-0.2	1.1	2.7
	Median	-0.1	-0.2	0.4	4.1
	Std. Dev.	17.8	7.0	25.1	23.8
	Skewness	0.0	0.5	-0.1	-0.8
	Kurtosis	26.1	6.3	14.5	8.1
IT	Mean	-0.1	-0.5	0.3	4.7
	Median	0.0	-0.4	0.7	1.3
	Std. Dev.	8.3	5.3	10.5	18.9
	Skewness	-1.0	0.0	-1.1	0.7
	Kurtosis	22.7	4.4	17.8	5.4
PT	Mean	1.7	-0.3	4.4	2.5
	Median	0.2	-0.4	3.2	4.8
	Std. Dev.	20.3	5.3	30.0	25.2
	Skewness	4.1	0.0	2.7	-0.8
	Kurtosis	65.0	4.4	31.0	6.4

### Figure 2: Sovereign bond yield levels for euro area countries

The top and bottom panels plot yield data from five SMP countries and five non-stressed euro area countries, respectively. The yields shown are yields-to-maturity of five year benchmark bonds in percentage points. The shaded areas indicate two periods when the SMP was the most active (see also Figure 1).



panel) and five non-stressed countries (bottom panel). Two shaded areas indicate when the SMP program was most active (compare Figure 1). Strong announcement effects are clearly visible in the data. Sovereign yields are highly correlated over time and in the cross section, suggesting an unmistakable role for common factors. In addition, yields during the debt crisis also exhibit occasional larger moves of up to 200 basis points at a daily frequency. The bottom panel of Figure 1 suggests that benchmark yields of non-SMP countries, in particular that of Germany, remained approximately unchanged during the 10 May 2010 and the weeks thereafter. This strongly suggests that the observed yield changes in SMP countries were not brought about by a signalling of expected future low monetary policy rates.<sup>7</sup>

Finally, we consider a panel of observed control covariates. Two variables capture an important share of the cross sectional and time series dependence in bond yields across euro area countries: daily changes in the yield spread between BBB and AAA rated corporate bonds in the euro area, and daily changes in the U.S. VIX volatility index. The euro area quality spread serve as a proxy for risk appetite regarding euro area debt in general. The VIX volatility index is a gauge of market fear and global risk aversion, but may also affect global liquidity flows because it impacts financial intermediaries' and market makers' value at risk constraints and leverage, see Adrian and Shin (2010, 2014).

### 3.2 Panel regression

We consider the panel time series regression model

$$y_{it} = c_{it} + \delta_{it}z_{it} + \beta'_i W_t + \lambda'_i f_t + \gamma_i g_{it}, \quad (1)$$

where  $y_{it}$  is the observed change in yield of a benchmark bond of country  $i = 1, \dots, N$  at a daily frequency  $t = 1, \dots, T$ . We consider first differences since the yield data is non-stationary, see Figure 2 for obvious visual evidence. Yields-to-maturity refer to five year benchmark bonds. The five year maturity is approximately in the middle of the two year to ten year maturity spectrum that is targeted by the SMP. We assume that the purchase of any bond in that two year to ten year maturity bracket also affects the yields of other bonds issued by the respective government in that maturity bracket as well, as these bonds

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<sup>7</sup>An inspection of money market forward swap rates around the 10 May 2010 confirms this assessment.

are close substitutes. We take the impact at the five year mid-point of the term structure as indicative of a shift in the overall yield curve in that respective country.

We consider four different specifications of the intercept term  $c_{it}$ . This is potentially important, as the yield impact is a shift in the conditional mean of (1). The intercept is estimated as either (i) constant over the entire estimation sample, (ii) piecewise constant over certain subsamples, or (iii) based on a 65-day rolling window specification (RW). The intercept terms are estimated as averages over non-intervention yields in a first step, or (iv) estimated simultaneously with all other parameters. Purchase amounts  $z_{it} \geq 0$  are in terms of nominal value. This facilitates the comparison to the existing LSAP and QE literature, see for example D’Amico and King (2013). Observed control covariates  $W_t$  and unobserved factors  $f_t$  load on yields with coefficient vectors  $\beta'_i$  and  $\lambda'_i$ , respectively. The remaining time series variation is captured by the idiosyncratic (residual) factor  $g_{it}$ .

The coefficients  $\delta_{it}$  are our main parameters of interest and measure the impact (in bps) of a unit increase in purchases,

$$\delta_{it} \equiv \bar{\delta}_i + \bar{\delta}_{10\text{May}2010} + \bar{\delta}_{8\text{Aug}2011}, \quad (2)$$

where  $\bar{\delta}_i$  are country-specific effects corresponding to purchases  $z_{it}$ , and coefficients  $\bar{\delta}_{10\text{May}2010}$  and  $\bar{\delta}_{8\text{Aug}2011}$  are time-specific fixed effects that correspond to the initial program announcement on 10 May 2010 and the reactivation and extension of the SMP on 8 August 2011, respectively. The additive specification (2) distinguishes between announcement effects and the direct effects from outright purchases. Attributing the entire yield change on announcement days only to the program announcement would overestimate its effect because a substantial amount of purchases occurred on these days as well. We take the pure announcement effect as proportional to the amount of purchases undertaken on the respective day. This specification is parsimonious while still allowing us to disentangle the effects.

Observed covariates are given by

$$W_t = (\Delta \text{ U.S. VIX}_t, \quad \Delta \text{ euro area corporate yield spread}_t)' , \quad (3)$$

where  $\Delta$  denotes first differences. Factors  $W_t$  are common to all data, and are standardized to have zero mean and unit variance. Unobserved dynamic effects  $f_t$  and  $g_t$  capture the remaining systematic and idiosyncratic variation in the panel, respectively, and evolve

according to a first order vector autoregression as

$$f_{t+1} = \Phi f_t + w_t, \quad w_t \sim F_w(0, H_w, \cdot), \quad (4)$$

$$g_{t+1} = \Gamma g_t + \xi_t, \quad \xi_t \sim F_\xi(0, H_\xi, \cdot), \quad (5)$$

where  $w_t$  and  $\xi_t$  are vectors of innovation terms. Autoregressive matrices  $\Phi$  and  $\Gamma$  are diagonal for both common factors  $f_t$  as well as the residual factors  $g_{it}$ . All latent factors are initialized at their stationary distribution.

If the innovation terms in (4) and (5) are Gaussian, then the model (1) - (5) is a standard linear Gaussian model in state space form, for which the log-likelihood can be easily obtained by a single run of the Kalman Filter (Hamilton (1994)). Inference on model coefficients is straightforward as a result. However, summary statistics and data plots (see Table 1 and Figure 2) indicate that there are occasional extreme observations, or fat tails, in our daily yield change data during the euro area sovereign debt crisis. In practise, we observe that convergence to the global maximum of the log-likelihood is unreliable in the Gaussian case. In addition, inference may be sensitive to a few extreme observations. Both the convergence and the inference problems are solved if we take  $w_t$  and  $\xi_t$  to be t-distributed error terms. We treat the degrees of freedom parameter  $\nu$  as a common robustness parameter to be estimated from the data, see Franses and Lucas (1998). Parameter and risk factor estimation by maximum likelihood for a non-Gaussian model in state space form remains fairly standard, see for example the textbook treatment by Durbin and Koopman (2001, p. 208); see also Koopman, Lucas, and Schwaab (2011, 2012) and Mesters and Koopman (2012) for related applied frameworks. The relevant asymptotics do not depend on  $N$  or  $T$  going to infinity at certain relative rates.

Finally, we impose scaling restrictions to identify the factor loading coefficients, as in for example Stock and Watson (2002a) and Creal et al. (2014). Covariance matrices are  $H_w = I - \Phi\Phi'$  and  $H_\xi = I - \Gamma\Gamma'$ , which implies that  $\text{Var}[f_t] = I$  and  $\text{Var}[g_t] = I$ , where  $I$  is an identity matrix of appropriate dimension. Latent factors are unconditionally orthogonal (orthonormal) as a result. This scaling restriction essentially identifies the elements of loading coefficients  $\lambda_i$  and  $\gamma_i$  as standard deviation (volatility) parameters. The sign of the latent factors is identified by restricting one of the factor loadings to be positive for each factor.

### 3.3 Identification

The modeling framework presented in Section 3.2 implicitly assumes that purchase amounts are *predetermined*, i.e., that prices and quantities are not simultaneously determined. It also assumes that the effects of the severe and escalating sovereign debt crisis on yields can be controlled for in a parsimonious and efficient way by allowing for unobserved factors. This section gives further details why these assumptions are likely to hold in case of the SMP.

First, substantial coordination within the Eurosystem requires the intervention decisions and purchase amounts to be essentially predetermined at the daily frequency. Such coordination is required since it is not a single institution (the ECB), but the Eurosystem (the ECB and 17 national central banks), which jointly undertakes the purchases. Since a large number of institutions are involved, a strategy is generally discussed and fixed before markets open. The strategies were generally not systematically conditional on yield developments during the upcoming trading day. In addition, which markets are perceived as dysfunctional (along with other guidance) is determined during the monthly meetings of the ECB's Governing Council. Decisions from the Governing Council guide and constrain the implementation of the purchases. Both institutional factors mean that intervention day yield changes and purchase amounts are not simultaneously determined. We therefore treat purchase amounts as predetermined covariates. Predetermination is a substantially weaker requirement than strict exogeneity in time series regression, but sufficient to ensure consistency as well as asymptotic normality of the maximum likelihood estimator in our setting, see Davidson and MacKinnon (1993, Chapter 18) and Durbin and Koopman (2001, Chapter 12). If purchases were not predetermined in reality, and simultaneity of the 'leaning against increasing yields'-type were present, then our regression estimates constitute a *lower bound* in terms of absolute value of impact.<sup>8</sup> That is, purchases would be at least as effective as indicated by the regression estimates.

Second, while purchases are fixed before markets open, purchases overall are still determined against the backdrop of an escalating sovereign debt crisis. As a result, purchases are only observed during times of intense crisis, of which high, rising, and volatile yields are

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<sup>8</sup>This is based on an implicit assumption about the likely sign of the bias in case the identifying assumption is violated. Systematic leaning against increasing yields means that regression methods would tend to understate the effect from interventions, see Neely (2005) for a discussion in the context of central bank FX interventions.

symptoms. Since the program targeted debt markets that were perceived as dysfunctional, the program entails buying debt securities in the market segments that are the most affected by the crisis. Control covariates are important to disentangle the direct (negative) effect of purchases on yields from the (positive) correlation between yield changes and purchases that are due to the crisis. For this reason we allow for unobserved common factors in our specification.<sup>9</sup> Some candidate control covariates are readily available and easily included. We find, however, that a significant amount of systematic co-movement across sovereign yields during the crisis remains unaccounted for after conditioning on relevant observed controls. As a result, observed factors are not sufficient.<sup>10</sup> That a pronounced factor structure underlies bond yield changes in a monetary union is intuitive and a common specification choice in the sovereign risk literature, see Pan and Singleton (2008), Ang and Longstaff (2011) and Longstaff, Pan, Pedersen, and Singleton (2011).

## 4 Major empirical findings

### 4.1 Yield impact

This section discusses our main empirical findings. Table 2 reports estimates of yield-impact per bond purchases (notional value) for five SMP countries. We consider the panel time series regression 1 of yield changes  $y_{it}$  on a constant  $c_{it}$ , purchase amounts  $z_{it} \geq 0$ , two observed covariates  $W_t$ , common factors  $f_t$ , and possibly autocorrelated residual terms  $g_{it}$ , for  $i = 1, \dots, N$ .

Our favorite model specification includes two common factors  $f_t$ . This selection is based

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<sup>9</sup>Latent factors also provide insurance against dynamic and cross-sectional model mis-specification. If the unobserved factors were not important, one would tend to estimate insignificant loadings that pre-multiply a white noise factor, see Durbin and Koopman (2001) and Koopman, Lucas, and Schwaab (2011). Unobserved factors could alternatively be regarded as serially correlated time fixed effects in panel regression.

<sup>10</sup>Importantly, the latent factors also help us to avoid ‘over-controlling’ in our setting, see Duflo (2002, p. 8) and Angrist and Pischke (2009, p. 61). For example, we could control for general increases in euro area sovereign risk during the crisis by using the first principal component of the 5 year CDS spreads for the countries in our sample on the right hand side of (1). This observed measure could easily be included as a right hand side regressor. However, the SMP purchases caused the 5 year CDS spreads to move to a similar extent as the 5 year benchmark bond yields (Lucas, Schwaab, and Zhang (2014)), possibly reflecting an arbitrage relationship between the two (Duffie (1999)). Regressing yield changes on purchases and the CDS spreads would incorrectly attribute some of the effect of the purchases to the CDS. This would bias the impact coefficient of the purchases upwards towards zero. Finally, our common factor estimates are less sensitive to outliers due to t-distributed error terms; outliers are known to be a problem for the method of principal components.

**Table 2: Estimation results: yield impact in bps per 100 mn**

We report estimation results for four different models. Impact coefficients refer to a purchase of €100 mn. Regression specifications in the top panel differ only regarding the intercept term  $c_{it}$ . The intercept is either constant (CO) over the entire estimation sample from 1 October 2008 to 20 December 2011; piecewise constant (PC) over three periods: 1 October 2008 to 09 May 2010 (pre-SMP), 10 May 2010 to 7 August 2011 (initial purchases), and 8 August 2011 to 20 December 2011 (purchases after re-announcement and until the allotment of the first three year LTRO); time varying based on a 65-day rolling window average over non-intervention days (RW), or estimated along with the other parameters by maximum likelihood (ML). The bottom panel contains parameter estimates for four alternative specifications that allow for time-variation in factor innovation volatility (tvv), lagged observed control covariates ( $W_{t-1}$ ), and an additional unobserved factor (F3).

Model	m1, CO		m2, PC		m3, RW		m4, ML	
	par	(t-val)	par	(t-val)	par	(t-val)	par	(t-val)
ES	-0.61	(6.8)	-0.40	(4.5)	-0.62	(7.0)	-0.66	(7.5)
GR	-2.02	(3.6)	-2.14	(4.1)	-1.71	(3.0)	-1.81	(3.3)
IE	-0.31	(0.5)	-0.29	(0.5)	-0.25	(0.4)	-0.34	(0.6)
IT	-0.15	(2.6)	-0.02	(0.4)	-0.16	(2.9)	-0.18	(3.1)
PT	-0.90	(1.5)	-0.96	(1.7)	-0.70	(1.2)	-0.61	(1.0)
10May10	-8.57	(14.6)	-8.51	(14.5)	-8.68	(14.8)	-8.51	(14.6)
8Aug11	-0.66	(7.8)	-0.76	(9.0)	-0.65	(7.7)	-0.65	(7.7)
loglik	10268.8		10279.8		10258.4		10319.8	

Model	m5: tvv, CO		m6: tvv, PC		m7:F2, $W_{t-1}$ ,CO		m8:F3, $W_t$ ,CO	
	par	(t-val)	par	(t-val)	par	(t-val)	par	(t-val)
ES	-0.59	(5.6)	-0.40	(3.7)	-0.61	(6.8)	-0.57	(7.5)
GR	-2.79	(3.6)	-2.92	(3.9)	-2.08	(3.7)	-1.95	(3.3)
IE	-0.02	(0.0)	0.02	(0.0)	-0.21	(0.3)	-0.31	(0.6)
IT	-0.10	(1.5)	-0.18	(0.3)	-0.15	(2.6)	-0.14	(3.1)
PT	-1.50	(2.5)	-1.57	(2.7)	-0.77	(1.3)	-0.25	(1.0)
10May10	-7.74	(11.7)	-7.68	(11.6)	-8.68	(14.8)	-8.96	(14.6)
8Aug11	-0.66	(6.7)	-0.76	(7.6)	-0.65	(7.6)	-0.65	(7.7)
loglik	11264.8		11279.2		10211.7		10391.5	

on minimal information criteria as suggested in Bai and Ng (2002). The top panel considers this specification and only varies the intercept term  $c_{it}$ . The intercept is either constant over the entire estimation sample from 1 October 2008 to 20 December 2011 (model m1); piecewise constant over three periods: 1 October 2008 to 9 May 2010 (pre-SMP), 10 May 2010 to 7 August 2011 (initial purchases), and 8 August 2011 to 20 December 2011 (purchases after re-announcement and until the allotment of the first three year LTRO) (model m2); a time varying intercept based on a 65-day rolling window averages over non-intervention days (model m3). Alternatively, the intercept terms are estimated along with the other parameters by numerically maximizing the log-likelihood function (model m4). Reading across the point estimates in Table 2, we find that over time, on average €1 bn of bond purchases lowered yields from approximately -1 to -2 basis points (Italy) and up to more than -20 basis points (Greece). The remaining impacts take intermediate values, from approximately -3 bps/bn (Ireland), -4 to -6 bps/bn (Spain), and -6 to -9 bps/bn (Portugal). Impact coefficients are statistically significant according to their t-values for most, but not all SMP countries. Statistical power is low also due to relatively few intervention days in the estimation sample for some countries. The log-likelihood is highest when intercepts are estimated along with the other model parameters.

Table 2 reports substantial announcement effects for the initial announcement on 10 May 2010 and the reactivation of the program on 8 August 2011. The impact coefficients increase in absolute value by an additional 86 bps per €1 bn on 10 May 2010 and 7 bps per €1 bn on 8 August 2011. Both announcement effects are statistically significant and economically large. The announcement effect from 10 May 2010 is substantially larger. This could be due to a combination of two effects. First, purchases on the 8 August 2011 mainly focused on the Italian and Spanish debt markets, which are relatively larger and deeper, and possibly in better overall shape, compared to the Greek, Irish, and Portuguese markets in May 2010 (see Figure 2). Second, market participants may have been somewhat disenchanted by the ‘temporary’ and ‘limited’ nature of the program at that point in time (see Blackstone (2013) for a discussion in the financial press in context of this paper).

The bottom panel of Table 2 reports the estimation results from four alternative specifications. These alternatives explore the robustness of our estimates to changes in model specification. Models m5 and m6 both allow for volatility clustering in observed yield data.

Roughly speaking, accounting for volatility clustering means that one learns more about the intervention impact from days that are calmer; such days receive a relatively larger weight in likelihood estimation. In Models m5 and m6, variance matrices  $H_w$  and  $H_\xi$  are thus time varying. The intercept  $c_{it}$  is constant in Model m5 (as in m1) and piecewise constant in m6 (as in m2). Time-varying volatilities are estimated based on an exponentially weighted moving average specification for squared observations, see for example Engle (2002); factor variances are then taken as proportional to these volatility estimates. The bottom panel in Table 2 suggests that allowing for time-varying volatility in the latent factor innovation terms increases the data likelihood but also leaves the impact coefficients approximately unchanged. We prefer our baseline specifications (m1-m4) because these are simpler and produce equivalent results.

We further explore robustness of our empirical results by replacing  $W_t$  (correlated with  $z_{it}$ ) with  $W_{t-1}$  (predetermined). Since  $W_t$  and  $f_t$  are control covariates and are not used as instrumental variables in a counterfactual experiment, contemporaneous correlation of  $W_t$  and  $f_t$  with purchases  $z_{it}$  is not a problem for identification (instead, it is what makes these factors useful as control covariates; the main identification assumption in our regression setup is that  $z_{it}$  and  $y_{it}$  are not simultaneously determined, see Section 3.3). Table 2, for Model m7, clarifies that the replacement has almost no effect on the estimated yield impact coefficients. The latent effects adjust to reflect the diminished role of the observed control covariates. The respective loading coefficients on  $W_{t-1}$ , however, are less intuitive. This is another reason why we prefer our benchmark models (models m1-m4).

Finally, Model m8 investigates the robustness of our yield estimates with respect to the number of latent components in the model. The estimates for yield impact are robust for most countries, with the exception of Portugal. A few extreme market moves (see Figures 2 and 3) and a relatively large role for the idiosyncratic component contribute to the variability of the parameter estimate.

## 4.2 Comparing yield impact and debt market size

This section relates the impact estimates from Table 2 to the size of the respective debt markets. A purchase of any given amount constitutes a significantly smaller purchase in terms of the share of the overall outstanding debt in a large and liquid market (e.g., Italy)

**Table 3: Impact and debt market size**

The table relates the estimated yield impact on benchmark bonds at the five year maturity from Model m1 (see Table 2) to the respective stock of public debt. Public debt data are from Bloomberg. Yield impact is in bps per 1/1000 of the debt market.

Impact	per 100mn Model m1		total debt in bn EUR		impact (bps) per 1/1000 of debt market size	
	val	(t-val)	2010	2011	2010	2011
ES	-0.61	(6.8)	549	649	-3.3	-3.9
GR	-2.02	(3.6)	340	346	-6.9	-7.0
IE	-0.31	(0.5)	93	114	-0.3	-0.4
IT	-0.15	(2.6)	1688	1589	-2.5	-2.4
PT	-0.90	(1.5)	152	165	-1.4	-1.5

compared to a smaller market (e.g., Ireland).

The impact estimates reported in Table 2 are larger than what is commonly found in the LSAP and QE literature. The debt markets under consideration in this study are typically smaller and less liquid than the market for U.S. Treasuries or U.K. gilts. In particular, this was the case during the sovereign debt crisis, when private demand for stressed government debt is low and required default risk premia are substantial. As a result, impact estimates from the LSAP and QE literature seem to serve as a lower bound for the magnitude of yield impact estimates for European debt purchases.

Table 3 suggests an average yield impact of approximately -3 bps for an intervention of the size equivalent to 1/1000 of a country's outstanding debt stock (measured at the end of 2010). Deviations are considerable for Greece (approximately 7 bps per 0.1 percent of the debt market) and Ireland (where the overall debt stock is relatively small). The wide range of cross-sectional variation, after taking into account overall debt market size, suggests that other channels contribute to determine the yield impact.

Notably, the ordering of the re-scaled yield impact estimates in Table 3 is positively related to average yields over our sample period. We refer to Figure 2 which documents a relative stable ordering of countries in terms of yield levels and implied risk. This comparison suggests that purchases do send a signal that is related to the default risk premium. This

**Table 4: Yield impact from a dynamic specification**

Parameter estimates refer to specification (6) and refer to yield impact per €100 mn. C1 to C5 denote SMP countries. Specification (6) implies a long-run effect of approximately  $\bar{\delta}_i + \omega_i/(1 - \kappa)$  for large  $K$  and a common parameter  $0 < \kappa < 1$ . We report a long run effect for  $K = 20$ . The estimation sample is 1 Oct 2008 until 20 Dec 2011.

	$\delta_i$	$\omega_i$	long run
ES	-0.85	0.14	-0.62
(t-stat)	(8.38)	(2.88)	
GR	-2.81	0.48	-2.03
(t-stat)	(4.36)	(1.77)	
IE	-0.46	0.03	-0.41
(t-stat)	(0.69)	(0.10)	
IT	-0.31	0.08	-0.19
(t-stat)	(4.20)	(2.95)	
PT	-0.77	-0.14	-1.00
(t-stat)	(1.24)	(0.53)	
10 May 2010	-8.71		
(t-stat)	(14.1)		
8 August 2011	-0.50		
(t-stat)	(5.19)		
$\kappa$		0.38	
(t-stat)		(2.19)	
loglik	9498.16		

is most obvious for the Greek purchases, for which yields moved much in relative terms, but also from a the highest yield level in the sample. Such a default risk signal can take several forms. For example, purchases may have been interpreted to mean that the Eurosystem regards country yields as higher than justified based on country fundamentals, also due to contagion concerns in a monetary union. Second, purchases could have been perceived as a ‘commitment device’ (Hoerova et al. (2012)), and that the Eurosystem is determined to continue to support stressed banks in the euro area. Third, and more generally, purchases signalled that the Eurosystem was willing to consider and implement unprecedented and non-conventional policies to combat the crisis.

### 4.3 Long run effects

This section extends our baseline model to allow for the possibility of lagged effects on yields from contemporaneous purchases. This extension is possible since SMP interventions are

recurring and partially announced interventions in the same markets. To our knowledge, the persistence of dynamic effects of bond purchases is not considered in the literature, with the exception of Wright (2012). Among other reasons, this is because persistence is hard to assess based on event study methodologies centred around program announcement dates.

Lagged effects can occur in particularly illiquid markets due to market microstructure and dealer inventory effects, see for example Roll (1984) and Vayanos and Vila (2009). If yields fully bounced back after an intervention, then the yield impact identified in Section 4.1 would be entirely transitory. Longer-lived effects are also possible, however, for example when the effects on the local supply of bonds are longer lived, or when a signal is perceived by market participants that the program is active for a longer time.

To investigate the persistence of the impact, we extend (1) to allow for lagged effects from purchases according to

$$y_{it} = c_{it} + \delta_{it}z_{it} + \omega_i \sum_{k=1}^K [(\kappa)^{k-1}z_{i,t-k}] + \beta'_i W_t + \lambda'_i f_t + \gamma_i g_{it}, \quad (6)$$

where parameters  $c_{it}, \delta_{it}, \beta_i, \lambda_i, \gamma_i$  and factors  $W_t, f_t, g_{it}$  are as before. New parameters  $\omega_i$  capture a lagged impact of a previous intervention. The coefficient  $0 < \kappa < 1$  determines how quickly a lagged impact decays over time. If  $K = 1$ , or  $K > 1$  but  $\kappa \approx 0$ , then all dynamic adjustment takes place on the first day following an intervention. For  $K$  large, the long-run effect from a given intervention is approximately  $\bar{\delta}_i + \omega_i/(1 - \kappa)$ . We estimate the long run effect for  $K = 20$ . We focus on a decay parameter  $\kappa$  that is common across countries. Pooling is required since only relatively few interventions are observed for some countries. A common parameter refers to an average effect across countries.

Table 4 reports the impact estimates from the dynamic specification (6), along with the estimated long-run effects. A comparison suggests that the long run impacts are approximately three quarters of the immediate impact in most countries. Positive estimates of  $\omega_i$  suggest that bond yields tend to ‘bounce back’ to some extent on the day following an intervention. Hence, some of the initial impact appears to be temporary, and may be related to dealer inventory market microstructure effects. However, in some, although not all, countries the contemporaneous yield impact coefficients  $\bar{\delta}_i$  are higher in (6) than in our baseline model (1) in those countries for which  $\omega_i$  is positive. Overall, the long-run effects from the dynamic model are fairly similar in magnitude to the contemporaneous impact

effect from our baseline specification without lagged effects. The small increase in likelihood (of approximately three points) suggests only a marginal improvement in explanatory power from moving to the dynamic specification.

#### 4.4 Additional discussion and robustness checks

This section provides additional discussion of our main empirical results. First, purchases could in principle lead to rising yields if the amount of purchases falls short of market expectations, as argued by Cahill et al (2013). Indeed, such reasoning may contribute in part towards explaining the rapid rise in yields of SMP countries in the last two quarters of 2010 and first two quarters of 2011, see the top panel in Figure 2. Cahill et al. (2013) distinguish between expectation and surprise components in the market reaction to several program announcements based on a confidential Fed survey which allows them to disentangle the two effects. We cannot control for market expectations in such a way. We recall, however, that the Eurosystem did neither disclose the total amounts that would be spent within the SMP, nor a time frame over which the program would be active, nor a set of securities that would be targeted. This suggests that our yield impact estimates for outright purchases, after controlling for announcement effects, contain a substantial surprise component. This, in turn, is consistent with the notion that new information is being revealed to market participants with each intervention.

Second, we have implicitly ruled out systematic cross-country effects from purchases in our empirical setup. If purchases in one country had a significant systematic effect on all other yields in our panel data, i.e., other SMP countries as well as non-stressed countries, then the conditional (on factors  $f_t$ ) mean in panel regression (1) could be lower on intervention days due to an effect that works through inference on common factors  $f_t$  and factor loadings that are of the same sign. This would introduce a bias towards zero of the negative yield impact estimates, and purchases would then be at least as effective as indicated by the time series regression estimates. Two observations suggest that cross-country effects from purchases are small, however. First, government bond markets were considerably fragmented during the sovereign debt crisis, see ECB (2012) and references therein. This means that government bonds from stressed and non-stressed countries would not be close substitutes during that time. This, in turn, suggests that local supply effects as well as signaling

effects are mainly country-specific. Second, the purchase announcements from 10 May 2010 and 8 August 2011 moved yields substantially in stressed countries, but left yields in core countries approximately unchanged (see Figure 2). This again points towards significant market fragmentation across countries.

We now explore the sensitivity of our empirical estimates to alternative regression specifications, and verify that our main results are robust to a few plausible variations in the modeling setup.

First, extending our data to include yield data from September 2008 — thereby including the failure of Lehman Brothers on 15 September 2008 and the announcement of the Irish guarantee of six banks' assets and liabilities on 30 September 2008 — has a negligible impact on the yield impact estimates. Second, extending the sample to data until 28 February 2012 increases the magnitude of the impact coefficients minimally. This is likely due to additional effects on sovereign yields that are due to two 36 month longer-term refinancing operations conducted at that time. Finally, the dynamic specification in Section 4.3 is somewhat restrictive in terms of decay dynamics due to data characteristics. We investigate the robustness of the long run effects by fixing different decay parameters a-priori to alternative values such as  $\kappa = 0.2$  and  $\kappa = 0.6$ , and then estimating only the remaining model parameters. In this case the impact coefficient estimates adjust, and the reported long-run effects remain similar.

## 5 Market volatility and extreme market movements

This section considers the impact of purchases on bond yield volatility and on the probability of extreme market movements during intervention days versus non-intervention days. We find that bond yield volatility is lower on intervention days for most SMP countries, due to less extreme (tail) movements occurring when the Eurosystem is active in the market. This matters, since highly volatile bond yields alone may force institutional investors and capital constrained market makers to leave a given market due to value-at-risk constraints, see for example Vayanos and Vila (2009) and Adrian and Shin (2010). Indeed, dealers occasionally ceased to provide quotes for government bond transactions during particularly volatile periods during the debt crisis, see Pelizzon, Subrahmanyam, Tomio, and Uno (2013). For

**Table 5: Interventions lower the probability of extreme tail movements**

The table reports estimates of the Hill tail index. The smaller the tail index the more probable are extreme market movements. The estimates are obtained following the method of Huisman, Koedijk, Kool, and Palm (2001), which explicitly addresses small sample bias (which the Hill (1975) estimator does not).<sup>11</sup> We report the tail index  $\beta_0^{-1}$ . Non-intervention days are decomposed into a pre-debt crisis (1 Oct 2008 to 31 Mar 2010) and debt crisis sample (1 Apr 2010 to 20 Dec 2011).

	Non-intervention days		Intervention days
	pre-crisis	crisis	
tail index for ES	8.2	2.9	7.2
tail index for GR	4.6	13.6	5.0
tail index for IE	2.7	2.6	3.4
tail index for IT	10.4	2.2	23.5
tail index for PT	6.2	1.8	6.1

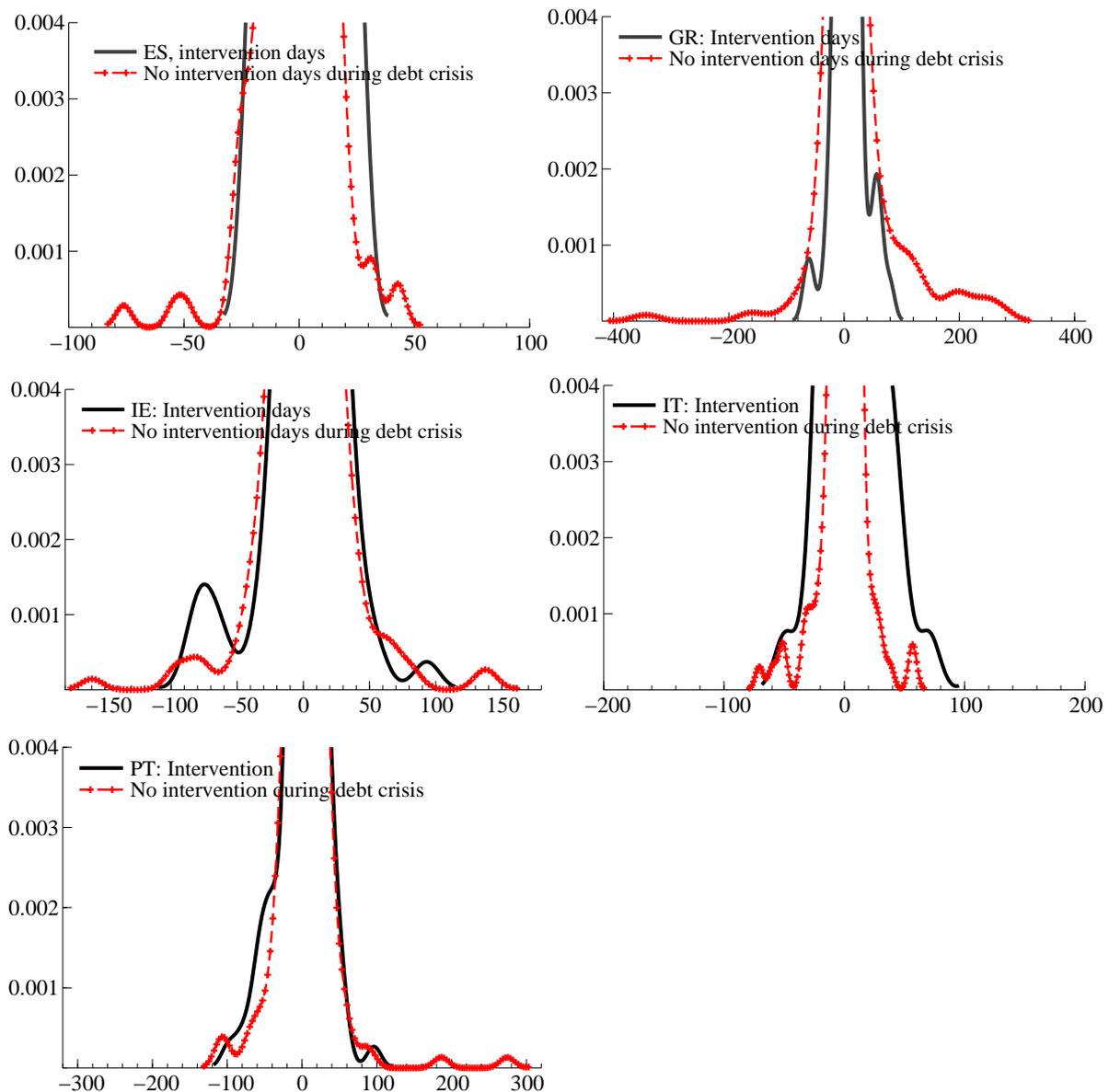
interesting volatility-based identification of LSAP effects in a vector autoregressive setup, we refer to Wright (2012).

The summary statistics in Table 1 in Section 3.1 already suggest a clear effect of purchases on bond yield volatility as well as on the probability of extreme market movements (kurtosis). The observed standard deviation of yield changes is lower on intervention days than on non-intervention days during the debt crisis for most SMP countries (all countries except Italy). The kurtosis statistics are considerably lower during intervention days than on non-intervention days for all five countries. This already strongly indicates that there is a reduced (tail) risk of extreme movements on intervention days.

Table 5 presents estimates of the tail index for intervention days and non-intervention days. We again distinguish pre-crisis and crisis times (before and after 1 April 2010). We refer to Hill (1975) and Huisman, Koedijk, Kool, and Palm (2001) for the methodology. We use the latter approach to mitigate small sample bias for the intervention day sample. Yield changes on intervention days tend to display thinner tails and therefore also higher tail index estimates. As a result, there are fewer extreme market movements on intervention days, compared to non-intervention days during the crisis. Figure 3 graphs density (kernel) estimates that pertain to yield changes of five year benchmark bonds. The plots distinguish yield changes that occurred on intervention days from yield changes that occurred on non-intervention days during the debt crisis. Overall, the non-parametric density plots

### Figure 3: Kernel density estimates of SMP country yield changes

We report non-parametric Kernel density estimates of yield changes in five year government bonds. The densities distinguish yield changes on intervention days from yield changes on non-intervention days during 1 Apr 2010 to 20 Dec 2011. The density estimates refer to Spain (ES), Greece (GR), Ireland (IE), Italy (IT), and Portugal (PT). The vertical axes are re-scaled so that the tails of the densities are visible. For the corresponding summary statistics see Table 1.



confirm the impression from Tables 1 and 5 that the SMP prevented or substantially limited extremely adverse yield movements. The visual evidence is strongest for Greece, Ireland, and Portugal, and less strong for Spain and Italy. The difference may reflect a difference in terms of timing of the purchases during our estimation sample from 2010-11.

## 6 Conclusion

This paper contributes to the literature on the effectiveness of central bank non-standard monetary policy measures by investigating the financial market impact of bond market interventions within the ECB's Securities Markets Programme. We assess the yield impact of asset purchases in five euro area sovereign bond markets: Greece, Spain, Ireland, Italy, and Portugal based on time series panel data regression on purchases and control covariates. In addition to a large and economically significant announcement effects, we find that the ECB's repeated interventions had an impact of approximately -1 to -2 basis points (Italy) and up to -17 to -21 basis points (Greece) at a five-year maturity per €1 bn of purchases across euro area countries. The yield impact depends on size and market conditions, and a default risk signal. Bond yield volatility is lower on intervention days for most countries, due to less extreme (tail) movements occurring when the Eurosystem is active in the market. Finally, a dynamic specification points to both transitory dynamics as well as longer-lived effects from purchases.

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