Evaluating the impact of unconventional monetary policy measures: Empirical evidence from the ECB's Securities Markets Programme

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Abstract

We assess the yield impact of asset purchases within the European Central Bank's (ECB) Securities Markets Programme (SMP) in five euro area sovereign bond markets from 2010–11. In addition to large announcement effects, we find an impact of approximately -3 basis points at the five-year maturity for purchases of 1/1000 of the outstanding debt. Bond yield volatility and tail risk are lower on intervention days for most SMP countries. A dynamic specification points to both transitory and long-run effects. Purchases improved liquidity conditions and reduced default-risk premia, while the signaling of future low interest rates did not play a role.

JEL classification: C32, E58, G12.

Keywords: Central bank asset purchases, European Central Bank, Securities Markets Programme, non-standard monetary policy measures, yield impact.

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1. Introduction

Exceptional times can require exceptional policy measures. Since the onset of the financial crisis in 2007, the major central banks have implemented both standard and non-standard monetary policy measures to contain financial instability and adverse economic outcomes. Since October 2008, non-standard monetary policy measures in the euro area have included fixed rate full allotment tender procedures that provide central bank liquidity to banks at low and predictable interest rates, 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, Carmona Amaro, Iacobelli, and Rubens (2012). Major central banks have undertaken other non-standard monetary policy measures such as the large-scale asset purchases programs (LSAPs) of the Federal Reserve, the quantitative easing (QE) of the Bank of England, and the qualitative and quantitative monetary easing (QQE) of the Bank of Japan.

Similar to other asset purchase programs, the effectiveness of the ECB Securities Markets Programme (SMP) has been 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 between 2010–2011 in five euro area countries: Greece, Ireland, Italy, Portugal, and Spain. Approximately €214 billion (bn) of bonds were acquired between 2010 and early 2012, see ECB (2013).[1] 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, and if so, to what extent? Have purchases affected the volatility and extreme tail behavior of yield changes? Which channels explain the yield impact? Are the effects entirely temporary or longer lived?

The SMP was announced on 10 May 2010 and focused on Greek, Irish, and Portuguese debt securities. The program was extended to include Italian and Spanish bonds on 8 August 2011. The SMP was replaced by the Outright Monetary Transactions (OMTs) program on 6 September 2012. The SMP and the OMTs are related but different programs, see Cœuré (2013).

The SMP had the objective of helping to restore the monetary policy transmission mecha-

[1] At the end of 2012, the ECB held €99.0bn 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, see the ECB (2013) Annual Report. Interestingly, during the press conference on 21 February 2013, the ECB also reported that it earned €555 million (mn) in 2012 on its holdings of Greek sovereign bonds that were bought during the crisis.

nism by addressing the malfunctioning of certain government bond markets; see, for instance, González-Páramo (2011). The SMP consisted of interventions in the form of outright secondary market purchases. Implicit in the concept of malfunctioning markets is the notion that government bond yields can be unjustifiably high and volatile, see Constâncio (2011). Importantly, the SMP was not designed to make the monetary policy stance more accommodative as such. Therefore, the liquidity effect resulting from SMP interventions was sterilized through one-week liquidity-absorbing operations. 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.

Compared to other central bank asset purchase programs, the SMP differs in several dimensions and, in particular, contains features that resemble foreign exchange interventions. An analysis of the SMP thus requires specific methods and yields significant additional insights into the effects of central bank asset purchases in stressed bond markets. First, SMP purchases were made during a severe sovereign debt crisis, when sovereign yields in several euro area countries were at a high, on the rise, and volatile. During this phase, the targeted securities met little private sector demand. The purchases were undertaken during the most intense phases of the debt crisis and in the markets most affected by the crisis. This contrasts with the setting of the Federal Reserve's LSAPs and the Bank of England's QE, where longer-term yields and yield volatilities were relatively low and default risk premia negligible. Second, key features of the SMP were not disclosed while the program was active, for example, the targeted securities, the amounts to be purchased, and the duration of the program. Apart from the initial announcements about the SMP,[2] 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 and within the Eurosystem, i.e., the ECB and all National Central Banks (NCBs). The extent of the controversy within the Eurosystem balance sheet became evident with the resignation of the Bundesbank President in February 2011 and an ECB Executive Board member in September 2011.

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, as some suggested,[3] lead to the impression that the SMP was ineffective. In particular, yields were rising as purchases were taking place. We document that yield changes and SMP purchase amounts at a daily frequency

[2] See the ECB press release of 10 May 2010 "ECB decides on measures to address severe tensions in financial markets" and 7 August 2011 "Statement by the President of the ECB."

[3] See, for instance, Fulcrum Research Notes, January 2013 or Natixis Flash Economics Economic Research, 13 September 2013.

are positively correlated for most SMP countries when interventions took place. As a result, simple regression-based techniques that relate yield changes to purchase amounts can lead to insignificant or even positive impact coefficient estimates. These approaches, however, neglect the presence of common factors, such as the escalating sovereign debt crisis. These common factors partly explain both the rising yields (dependent variable) and the activation of the non-standard monetary policy measure (explanatory variable). This calls for a careful 'impact identification.'

In our framework, identification is based on a panel regression that exploits both the cross-sectional and the time series dimension of the data. Within this framework it does not matter for identification that yields rose over time in many euro area countries during the sovereign debt crisis. However, what matters is whether, after controlling for other factors, yields rose *to a lesser extent* in those markets in which purchases were undertaken, compared to what one would have expected based on yield developments in a larger set of euro area countries. We exploit the fact that coordination within the Eurosystem required the purchase decisions and amounts to be predetermined at a daily frequency. A factor structure allows us to control for cross-sectional dependence in the benchmark bond yields of multiple euro area countries and the fact that purchases were undertaken against the backdrop of a severe and escalating sovereign debt crisis. We compare our results to simpler difference-in-difference (DID) estimates which yield similar results but require more restrictive assumptions.

We report four main empirical findings. First, in addition to large and statistically significant announcement effects, we find that per €1 bn of bond purchases the SMP had an impact at the five-year maturity ranging from -1 to -2 basis points (bps) in Italy to -16 to -21 bps in Greece. The other impact estimates take intermediate values, from approximately -3 bps/bn in Ireland, -4 to -6 bps/bn in Spain, and -7 to -10 bps/bn in Portugal. The different sizes of the respective markets explain part of the cross-country differences in yield impact. A given amount of purchases has a larger impact in a smaller debt market. Per 1/1000 of the respective debt market, the impact estimates are approximately -2.6 basis points at the five-year maturity. The estimates, both in terms of €1 bn spent and in terms of the relative market size, are larger than what is common in the literature for purchases of U.S. Treasuries during 2008–09 within the Federal Reserve's LSAP (see, for example, Gagnon, Raskin, Remache, and Sack, 2011; D'Amico, English, Lopéz-Salido, and Nelson, 2012; 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 stressed euro area countries between 2010–12. We find that yield impact coefficients are higher at the short end than at the long end of the yield curve.

Second, in terms of impact channels, we find that the relatively large effects from SMP purchases can be explained in terms of reduced liquidity risk premia, default risk signaling effects, and possibly local supply effects in segmented markets (see Duffie, Garleanu, and Pedersen, 2005, 2007; and De Pooter, Martin, and Pruitt, 2013). Firstly, the flow of purchases from an investor of last resort reduces liquidity risk premia by making a counterparty easier to find. Information effects may play an additional role (see Pasquariello, Roush, and Vega, 2014). Secondly, purchases of sovereign bonds reduce the local supply of government bonds. Vayanos and Vila (2009) and Greenwood and Vayanos (2010) argue that if demand for such bonds is not perfectly elastic, for example, due to market segmentation, a reduction in supply pushes prices up and lowers yields. Thirdly, the flow of purchases could have implied a signal that the ECB regards country yields as higher than justified based on country fundamentals and that it is willing to implement non-standard measures to address the implied distortion of the monetary policy transmission mechanism. Unduly high yields could, for example, be the result of contagion concerns, high liquidity risk premia, or markets coordinating on a bad equilibrium in the presence of multiple equilibria (Corsetti and Dedola, 2013). Consistent with these channels, we find that bid-ask spreads declined substantially and the credit default swap (CDS)-bond basis became less negative when the ECB intervened, suggesting substantially improved market liquidity conditions. SMP purchases also lowered CDS spreads, but to a lesser extent than the corresponding sovereign bond yields.

The 'signaling channel' of Christensen and Rudebusch (2012) and Bauer and Rudebusch (2014), according to which purchases affect medium- and long-term rates by signaling low future monetary policy rates, is not important in the case of SMP purchases. The ECB emphasized throughout that the monetary policy stance would remain unaffected. A 'separation principle' applied, according to which non-standard monetary policy measures, such as asset purchases within the SMP, are complements to, not substitutes for, interest rate policies (see González-Páramo, 2011). The additional central bank liquidity in circulation owing to the SMP was sterilized, and no forward guidance on policy interest rates was given in the period 2010–12. Following the announcement of the SMP, government bond yields as well as money market overnight index swap rates hardly moved in non-stressed countries, and in particular, much less than in stressed countries. Taken together, this suggests that the signaling of future low monetary policy rates is not necessary for asset purchases to be effective.

Third, we document that bond yield volatility as well as the probability of observing extreme yield changes, as proxied by standard deviation, kurtosis, and Hill (1975) tail index estimates, were lower on intervention days for most SMP countries. Outright purchases mitigate extreme downside price movements on intervention days. The 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 could force institutional investors and market makers to retreat from a given market, particularly if value-at-risk constraints are binding; see, for example, Vayanos and Vila (2009) and Adrian and Shin (2010), and Pelizzon, Subrahmanyam, Tomio, and Uno (2013) for anecdotal evidence that this occurred in the trading of Italian debt securities.

Fourth, we disentangle transitory dynamics from long-run effects. To this end, we estimate a dynamic specification that allows for lagged effects from recurring interventions. We find evidence of transitory dynamics in some cases and estimate that the total long-run impact is approximately half of the immediate impact. Long-run effects from purchases could be the result of longer lasting reductions in the local supply of bonds, ceteris paribus, as bonds were announced to be held on the central bank's balance sheet until their maturity. Combining the long-run effects with the total purchases at the country level allows us to estimate the cumulated counterfactual yield reduction. Cumulated effects for the five-year maturity are -1.9% in Spain, -3.3% in Greece, -0.1% in Ireland, -2.1% in Italy, and -1.7% in Portugal. These are large numbers, but not unreasonable given the high yield levels in some SMP countries at the end of 2011. Caveats to this calculation are discussed in the main text.

We proceed as follows. Section 2 discusses the related literature on the SMP's impact and relates its key features to those of other central bank asset purchase programs. The data and 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. The appendices contain additional empirical results and robustness checks.

2. Asset purchases and yield impact

2.1. Related literature on the SMP impact

The two papers that are most closely related to ours are De Pooter et al. (2013) and Ghysels, Idier, Manganelli, and Vergote (2014). De Pooter et al. (2013) use the published weekly data of aggregate SMP purchases across all countries and seek to proxy actual purchases by country, assuming weekly purchases per country were made in proportion to the total purchases per country, which were also published. The authors contribute to the literature in two ways. First, they provide a theoretical search-based asset pricing framework, which rationalizes short- and long-term 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 bps for purchases 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 do not only consider the impact on liquidity risk premia. Instead we focus on the identification of the overall yield impact. We also assess volatility effects and the reduction of extreme market movements. Furthermore, the confidential data of daily recorded purchases allow our estimates to be more precise.

Ghysels, Idier, Manganelli, and Vergote (2014) analyze 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 a daily frequency. By estimating regression models based on data sampled at 15-minute intervals, they seek to minimize the bias that unobserved factors introduced. This approach leads to local high-frequency impact estimates on conditional moments. The authors find that an intervention of €100 million had an immediate impact on bond yields of between -0.1 and -25 bps, depending on the debt market and timing. Based on volatility time series models, their study also suggests that SMP purchases contributed to reducing the volatility of targeted government bond yields. In contrast to their study, we go beyond *local* high-frequency impact estimates and volatility effects. Furthermore, our econometric approach explicitly controls for unobserved third-factor effects, such as the upward pressure on yields during the crisis, which was particularly pronounced in the periods when most purchases were made. We see such controls as crucial, as using higher-frequency data does not remove the upwards bias, although it may attenuate the bias to some extent.

Other research studies also investigate the impact of the SMP, but are less related. Trebesch and Zettelmeyer (2014) 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, which compare bonds that were bought to bonds that were not bought. Purchased bonds show a much larger drop in yields following the start of the SMP. The authors document that purchases of €1 bn resulted in a drop of yields by up to -204 bps during the first eight weeks of the program. Finally, Beetsma, de Jong, Giuliodori, 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 realized volatility and correlation measures from intraday data. The authors find statistically and economically large effects and conclude that the SMP announcement and purchases weakened the observed positive co-movement of yields among distressed countries to some extent, also reducing flight-to-safety capital flows from stressed countries to non-stressed countries. Krishnamurthy, Nagel, and Vissing-Jørgensen (2014) evaluate three ECB non-standard monetary policy measures, including the SMP, based on an event study around program announcement dates.

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

Before the ECB introduced the SMP in May 2010, both the Federal Reserve with its LSAPs and the Bank of England with its QE had also embarked on outright purchases of government bonds. This section explains in detail how the SMP differs from these programs with respect to the overall objective, market conditions, and implementation strategy.

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

Second, concerning *market conditions*, the SMP was active in government bond markets whose depth and liquidity were impaired. This lack of depth and liquidity, in turn, was related to concerns about the sustainability of public finances and associated default risk premia. This stands in contrast to the conditions surrounding the LSAPs and QE, as 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, see D'Amico and King (2013) and Joyce, Lasaosa, Stevens and Tong (2011).

Third, in terms of the *implementation strategy*, both for the LSAP and QE programs the Federal Reserve and the Bank of England announced total amounts of purchases over specific time horizons. The actual purchases within these programs are usually undertaken in the form of auctions at relatively constant intervals. By contrast, 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 — the ECB announced that it would undertake bond purchases and what their objective would be. However, the ECB neither disclosed the total amounts that would be spent, nor a time frame over which the program would be active, nor the set of securities that would be targeted. In the case of the SMP, almost no details apart from the fact that interventions would be undertaken were disclosed on announcement days. These marked differences in communication also imply that event study methodologies around announcement days are less appropriate to evaluate the SMP.

Finally, bond purchases under the SMP sent a signal. This signaling, however, is different from the "signaling channel" in Christensen and Rudebusch (2012) and Bauer and Rudebusch (2014), where purchases signal future low short-term interest rates. Such signaling of future low monetary policy rates was neither intended by the SMP, nor did it result from it, as the monetary policy stance remained unaffected and the impact on central bank liquidity in circulation was sterilized. In the case of the SMP, however, purchases are likely to have signaled something else, namely, that the ECB regarded country yields as higher than justified based on country fundamentals. Purchases could have also communicated that the ECB was willing to consider and implement unprecedented non-conventional approaches to combat the crisis. As non-standard monetary policy measures are costly, they can increase the impact of central bank communication in a strategic setting, as in 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). Finally, as no specific duration was announced for the SMP, the flow of purchases communicated that the SMP was still being actively implemented.

3. Data and regression setup

3.1. Data

We use data from three sources for this study. First, we consider SMP bond purchase data by country at a daily frequency. Bond purchases are entered at par values. Assets were purchased in over-the-counter dealer markets via non-anonymous trades. On intervention days, market participants quickly learned that SMP-related trades were taking place. Fig. 1 plots weekly total purchases across countries as well as their accumulated book value over time. Noticeably, the weekly purchase data are unevenly spread 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 was open but inactive. From the end of March 2011 until the beginning of August 2011, the SMP was inactive for 19 weeks. This is in stark contrast to the regular auctions undertaken, for example, by the Federal Reserve within the LSAPs. The SMP's daily cross-country breakdown of the purchase data is confidential at the time of writing. However, the ECB released its total cross-country SMP portfolio holdings at the end of 2012 in its 2013 Annual Report. We use the confidential daily and country-specific data for this study.

[Insert Fig. 1 near here]

As a second data panel, we consider government bond yields for ten euro area countries: Austria (AT), Belgium (BE), Germany (DE), Spain (ES), France (FR), Greece (GR), Ireland (IE), Italy (IT), the Netherlands (NL), and Portugal (PT). SMP interventions mostly targeted the two- to ten-year maturity bracket, with the five-year maturity approximately in the middle of that spectrum. As a result, we mostly focus on the impact at the five-year 'midpoint' of the yield curve. The yield data are from Bloomberg and computed as yields to maturity from dealer prices. The 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 repricing of euro area 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 (see Acharya and Steffen, 2015), which we want to keep separate from the impact of the SMP.

Fig. 2 plots the development of yields since 1 January 2008 for the five SMP countries (top panel) and five euro area non-SMP countries (bottom panel). The two shaded areas indicate when the SMP was most active (compare Fig. 1). Euro area sovereign yields are generally highly correlated both over time and in the cross section, suggesting a strong role of common factors. During the debt crisis, some yields exhibit occasional large and sudden moves of up to 200 bps at a daily frequency. Strong announcement effects are clearly visible in the yield data: five-year yields dropped by -772.9 bps in Greece, - 138.6 bps in Ireland, and -226.4 bps in Portugal on 10 May 2010; and by -97.0 bps in Spain and -93.0 bps in Italy on 8 August 2011. The bottom panel of Fig. 2 suggests that the benchmark yields of non-SMP countries, that of Germany in particular, remained approximately unchanged on 10 May 2010 and 8 August 2011. This strongly suggests that the observed yield changes in SMP countries were not brought about by a signaling of expected future low monetary policy rates (see also Fig. 3).

[Insert Fig. 2 near here]

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 credit quality spreads 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 can also affect global liquidity flows because it impacts financial intermediary and market maker value-at-risk constraints and leverage (see Adrian and Shin, 2010, 2014).

3.2. Panel regression model

We start by considering the panel regression model

$$\Delta y_{it} = c_{it} + \delta_{it} z_{it} + \beta_i' W_t + \lambda_i' f_t + \gamma_i g_{it}, \tag{1}$$

where Δy_{it} is the observed change in yield of a benchmark bond of country $i = 1, \ldots, N$, where $N = 10$, at the daily frequency $t = 1, \ldots, T$, c_{it} is a country-specific intercept term, z_{it} are country-level bond purchases at par value, W_t are observed covariates, f_t are common latent factors to be estimated from the data, and g_{it} are country-specific residual factors. Inference on the coefficients δ_{it} is our main focus of interest. We compare our results from the analysis of (1) to data summary statistics and difference-in-difference (DID) estimates in Section 4.1.

We consider yield data Δy_{it} in first differences since the yield data are non-stationary, see Fig. 2. Unless noted otherwise, yields-to-maturity refer to five-year benchmark bonds. The five-year maturity lies approximately in the middle of the two- to ten-year maturity spectrum that was targeted by the SMP. We include bond yields from ten euro area countries in our panel: Austria, Belgium, Germany, Spain, France, Greece, Ireland, Italy, the Netherlands, and Portugal, see Section 3.1. Considering data from a larger cross section of euro area countries simultaneously, allows us to control for shocks that are common to all countries in the euro area. Controlling for common shocks is important because the euro area countries are strongly related in the cross section (for example, through the common exchange rate and the single monetary policy) and because the SMP tended to intervene only during the most severe periods during the sovereign debt crisis. We control for these data features through common factors. These common factors are statistically highly significant. Appendix A investigates how sensitive our estimation results are to the specification of the factor structure. If common shocks are neglected, our impact estimates become insignificant or even turn positive. This would misleadingly suggest that interventions effectively *raised* yields.

We consider four different specifications of the intercept term c_{it} in (1). The specification of the intercept is potentially important, as the yield impact is modeled as a shift in the conditional mean. The intercept is country-specific and either (i) constant over the entire estimation sample, (ii) piecewise constant over certain time periods, or (iii) based on a rolling window specification (RW). As a result, specifications (ii) and (iii) combine country fixed effects and some time (dummy) effects within c_{it}. In each case, the intercept terms are estimated in a first step as averages over non-intervention yields. Specification (iv) is similar to (i) but treats c_{it} as free parameters to be estimated simultaneously with all other model coefficients.

The coefficients δ_{it} in (1) are our main parameters of interest and measure the impact (in bps) per €1 bn of purchases $z_{it} \geq 0$,

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

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 SMP announcement on 10 May 2010 and its reactivation and extension on 8 August 2011. The additive specification (2) distinguishes announcement effects and the direct effects from outright purchases. Attributing the entire yield change on announcement days only to the program announcement would overestimate the announcement effect because a substantial amount of purchases were made on the announcement days. 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 announcement effects from the impact of the purchases.

Observed covariates are given by

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

where Δ denotes first differences. The factors W_t are common to all the data, and are standardized to have zero mean and unit variance.

The unobserved factors f_t and autocorrelated residual terms g_t capture the remaining systematic and idiosyncratic variation in the panel. They evolve according to a first-order vector autoregression as

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

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

where w_t and ξ_t are vectors of innovation terms, and the autoregressive matrices Φ and Γ are diagonal for both common factors f_t and the residual terms g_{it}. Both f_t and g_t are initialized at their stationary distribution. Coefficients β_i, $\bar{\delta}_i$, $\bar{\delta}_{10\text{May}2010}$, $\bar{\delta}_{8\text{Aug}2011}$, λ_i, γ_i, as well as the diagonal entries in Φ and Γ are model parameters to be estimated.

If the innovation terms in (4) and (5) are Gaussian, then model (1) – (5) is a standard linear Gaussian model in state space form, for which the log-likelihood is easily obtained by a single run of the Kalman Filter (Hamilton, 1994). In that case estimation and inference on model parameters is straightforward. However, data plots and summary statistics (see Fig. 2 and Table 1 below) indicate that there are occasional extreme observations, or fat tails, in our data, especially during the sovereign debt crisis. In practice, we observe that convergence to the global maximum of the log-likelihood function is unreliable if the innovation terms are assumed to be Gaussian. In addition, if the innovation terms are Gaussian, inference is sensitive to a few extreme observations. Both the convergence and the inference problems are solved if we take w_t and ξ_t to be t-distributed error terms. We treat the degrees of freedom parameter ν as a common robustness parameter to be estimated from the data (see Franses and Lucas, 1998). We refer to the textbook treatment by Durbin and Koopman (2001, p. 208f) for details on parameter and risk factor estimation by maximum likelihood for non-Gaussian models that contain unobserved factors.[4] 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 (2002) and Creal, Schwaab, Koopman, and Lucas (2014). The 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 the loading coefficients λ_i and γ_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. Comments on 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 provides further details on why these assumptions hold for the SMP.

[4] In this context, see also Koopman, Lucas, and Schwaab (2011, 2012), Schwaab, Koopman, and Lucas (2014), and Mesters and Koopman (2014) for similar non-Gaussian frameworks. Estimation codes and the results from a small-scale simulation study are available from the authors upon request. The appendix of the ECB working paper version of this article contains further technical results.

First, substantial coordination within the Eurosystem required the intervention decisions and purchase amounts to be essentially predetermined at the daily frequency. Such coordination was required since it was in practice not the ECB as a single institution, but the whole Eurosystem, consisting of the ECB and the (then) 17 NCBs, which jointly implemented the SMP and undertook the purchases. As a large number of institutions were involved, a strategy for the day had been, as a rule, discussed and fixed before markets open. The strategies were generally not conditional on yield developments during the upcoming trading day. Second, during its monthly meetings the ECB's Governing Council determined which markets were perceived as dysfunctional and provided guidance on the implementation of the SMP. The Governing Council decisions set the scope and overall strategy for the SMP implementation and guided and constrained the purchases. Both institutional factors, i.e., the decision-making role of the Governing Council and the joint implementation by the ECB and the 17 NCBs, mean that intervention-day yield changes and purchase amounts were 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 is 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, and simultaneity of the 'leaning against increasing yields'-type were present intraday, then our regression estimates constitute a *lower bound* in terms of absolute value of the yield impact.[5] That is, purchases would be at least as effective as indicated by our estimates.

Second, while purchases had been fixed before markets opened, purchases were still determined against the backdrop of an escalating sovereign debt crisis. As a result, purchases were only observed during an intense crisis, whose symptoms include high, rising, and volatile yields. Since the SMP targeted debt markets that were perceived as dysfunctional, the program entailed buying debt securities in the market segments that were 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.[6] Some candidate control covariates are readily available and easily included. We find, however, that a significant amount of systematic co-

[5] 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; Neely (2005) discusses this effect in the context of central bank foreign exchange interventions.

[6] Latent factors also provide insurance against dynamic and cross-sectional model misspecification. 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).

movement across sovereign yields during the crisis remains unaccounted for after conditioning on the relevant observed control variables. As a result, observed factors are likely to be insufficient.[7] That a pronounced factor structure underlies bond yield changes in a monetary union is intuitive and more generally a common specification choice in the sovereign risk literature (see Pan and Singleton, 2008; Longstaff, Pan, Pedersen, and Singleton, 2011; and Ang and Longstaff, 2013).

4. SMP impact on bond yields

4.1. Preliminary data analysis

This section provides a preliminary data analysis based on simple summary statistics and simple DID estimates. These serve as a point of comparison and robustness check for our main empirical analysis below.

Table 1 reports summary statistics of yield changes for five-year benchmark bonds across euro area countries. The table distinguishes between a pre-debt crisis (1 Oct 2008 to 31 Mar 2010) and a debt crisis sample (1 Apr 2010 to 20 Dec 2011), and also differentiates between intervention days and non-intervention days. For SMP countries, intervention days are country-specific, i.e., days on which purchases took place in the respective debt markets. For non-SMP countries, intervention days are taken to be days on which at least one purchase occurred in any market. In all five SMP countries pre-debt crisis yield changes exhibit a lower mean and lower volatility than yield changes on non-intervention days during the crisis. This is intuitive, as rising and volatile yields are the symptom of a worsening debt crisis. The sample kurtosis statistics confirm that our yield change data are characterized by occasional extreme market movements ('fat tails').

[Insert Table 1 near here]

Excluding the announcement days, the mean yield change on intervention days (column 6) is *higher* than the mean yield change for non-intervention days during the debt crisis (column 5) in three out of five SMP countries. Taken at face value, this would suggest that purchases *raised* yields in three out of five SMP countries. More likely, however, this observation reflects the circumstance

[7] Importantly, the latent factors also help us to avoid 'over-controlling' in our setting (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 five-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 five-year CDS spreads to move as well (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.

that interventions were made only as the debt crisis escalated, and only in the markets that were most affected by the crisis at the time. This, in turn, suggests that additional factors need to be taken into account for valid inference on country-specific impact coefficients.

Announcement day effects can be read off the penultimate column in Table 1. As already discussed in Section 3.1, five-year yields dropped by -772.9 bps in Greece, -138.6 bps in Ireland, and -226.4 bps in Portugal on 10 May 2010, and by -97.0 bps in Spain and -93.0 bps in Italy on 8 August 2011. Yield changes on the respective announcement days are thus large for all SMP countries and are approximately an order of magnitude higher than the respective standard deviations on non-intervention days. When interpreting these announcement effects we need to recall, however, that few details regarding the SMP were actually announced on those dates.[8]

Simple differential effects for the SMP announcement can be calculated from column 5 and columns 7 and 8 in Table 1. We obtain these effects as the difference in yield changes on the respective announcement day (columns 7 and 8) and non-intervention days during the debt crisis (column 5), minus the same difference for average yield changes for five non-SMP countries in the euro area (bottom rows of Table 1, referring to Austria, Belgium, Germany, France, and the Netherlands). The differential effects are -84.2 bps in Spain, -797.6 bps in Greece, -145.6 bps in Ireland, -80.2 bps in Italy, and -236.7 bps in Portugal.

Similar differential effects for continuing purchases excluding the announcement days can also be calculated from columns 5 and 6 of Table 1. These effects are the difference in mean from intervention days (column 6) and non-intervention days during the debt crisis (column 5), minus the same difference for five non-SMP countries in the euro area (bottom rows of Table 1). Such differential effects are in bps per intervention day, excluding announcement days, and are 1.5 bps in Spain, -13.5 in Greece, 0.8 in Ireland, 3.9 in Italy, and -3.3 in Portugal. Three countries show positive entries for continuing purchases. Again, rather than suggesting that purchases raised yields, this observation suggests that additional controls are required. Greece and Portugal exhibit negative entries, giving a first indication that these countries, in particular, benefited from the program, with their yields rising less on intervention days than they otherwise would have. We estimate more involved difference-in-difference regressions that control for a variety of other factors in Appendix B.[9] These DID results serve as a simple point of comparison and robustness check for

[8] The creation of the European Financial Stability Facility, a bailout fund with a total lending capacity of up to €750 bn was also announced on Sunday 09 May 2010 by euro area heads of state. As a result, the large yield reduction in Greece, Ireland, and Portugal on 10 May 2010 is not solely due to the SMP announcement.

[9] The mean and median rows of Table 1 indicate that yields rose more before and during the sovereign debt crisis in some stressed countries than in other less or non-stressed countries. The observed heterogeneity across countries before the SMP, but also other concerns such as the SMP reacting to prior increases in yields, cast some doubt on the appropriateness of the parallel trends assumption that underlies the DID regressions. We discuss DID estimates

our main empirical analysis below.

4.2. *Country-specific impact estimates*

This section discusses our main empirical findings based on parameter estimates for model (1) – (5). Table 2 reports the estimates of the yield impact per €1 bn of bond purchases at notional value for five SMP countries. The panel regression (1) relates yield changes Δy_{it} to a constant c_{it}, purchase amounts $z_{it} \geq 0$, two observed covariates W_t, common unobserved factors f_t, and autocorrelated residuals g_{it}, for $i = 1, \ldots, N$.

[Insert Table 2 near here]

Our favorite model specification includes two common factors f_t. This selection is based 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 average 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 bps in Italy up to more than -20 bps in Greece. The remaining impact estimates take intermediate values, from approximately -3 bps per bn in Ireland, -4 to -6 bps per bn in Spain, and -7 to -10 bps per bn in Portugal. The impact coefficients are statistically significant according to their t-values for most, but not all SMP countries. The statistical power is also low as there were relatively few intervention days for some countries. The log-likelihood is highest when intercepts are piecewise constant (model m2), although the difference to model m1 is (borderline) not statistically significant. We thus report estimates for both m1 and m2.

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 87 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 impact of the initial

in Appendix B with this important caveat in mind.

announcement from 10 May 2010 is substantially larger than the impact of the re-announcement on 8 August 2011. This could be due to a combination of two effects. First, purchases on 8 August 2011 mainly focused on the Italian and Spanish debt markets, which are relatively larger and deeper. In addition, in Italy and Spain yield levels before the announcement in August 2011 were lower than the Greek, Irish, and Portuguese yields before the announcement in May 2010, see Fig. 2. Second, based on the fact that the SMP interventions that began in May 2010 were toned down after a while, it is possible that some market participants expected a similar development after the reactivation of the SMP.

The bottom panel of Table 2 reports the estimation results for four alternative specifications. Models m5 and m6 both allow for volatility clustering in observed yield data. Generally speaking, accounting for volatility clustering means that one learns more about the intervention impact from quieter days. Quieter days thus receive a relatively larger weight in likelihood estimation. In models m5 and m6, the variance matrices H_w and H_ξ are therefore 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). The factor variances are then taken as proportional to these volatility estimates. The estimates for m5 and m6 suggest 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 similar results.

We further explore the robustness of our empirical results by replacing W_t (which is correlated with z_{it}) with W_{t-1} (which is predetermined). Since W_t and f_t are control covariates and are not used as instrumental variables in a counterfactual experiment, the contemporaneous correlation of W_t and f_t with purchases z_{it} is not a problem for identification (instead, it makes these factors useful as control covariates; the main identification assumption in our regression setup is that z_{it} and Δy_{it} are not simultaneously determined, see Section 3.3). For model m7, Table 2 shows that the replacement of W_t with W_{t-1} 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 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 the yield impact are robust for most countries, with the exception of Portugal. A few extreme market moves (see Fig. 2 and Fig. 5) and a relatively large role for the idiosyncratic component contribute to the variability of the parameter estimate.

Table 3 relates the five-year 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 market (e.g., Italy) compared to a smaller market (e.g., Ireland). 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 when measured at the end of 2010. Deviations are considerable for Greece (approximately -6 bps per 1/1000 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.

[Insert Table 3 near here]

Appendix B reports impact estimates based on four difference-in-difference regression for five-year bonds. Our favorite DID specification controls for announcement day effects and also includes the first two principal components from the cross-country yield change data as country-specific controls. The least squares estimates suggest an impact coefficient of -3.0 bps per purchases of 1/1000 of the outstanding debt. These represent approximately similar results.

The impact estimates discussed in this section are substantially larger than what is commonly found in the LSAP and QE impact literature (see, for instance, Krishnamurthy and Vissing-Jørgensen, 2011; and D'Amico and King, 2013). This is intuitive, as the debt markets under consideration in this study are typically (much) more stressed, smaller in size, and less liquid than the market for U.S. Treasuries or U.K. gilts. This was the case, in particular, during the sovereign debt crisis, when private demand for stressed government debt was low and the required default risk premia were substantial.

4.3. Yield impact at different maturities

This section reports additional country-specific and pooled impact estimates of SMP purchases for two-year, five-year, and ten-year benchmark bonds. Almost all SMP purchases occurred between the two-year and ten-year maturity.

Table 4 reports country-specific impact estimates at these maturities. We focus on three main findings. Firstly, the yield impact estimates tend to be higher at the short end of the term structure. In particular, the two-year impact estimates are almost always more negative than those for ten-year bonds. Put differently, the bond yield elasticity with respect to purchases appears to decline with bond maturity.

Secondly, we estimate an average, or 'level factor', yield impact across maturities. Table 4 reports the impact estimate that refers to an unweighed average over two-year, five-year, seven-year, and ten-year benchmark bond yields.[10] The midpoint is effectively at the $2+5+7+10 = 24/4 = 6$ year maturity. The estimated average impacts are similar in magnitude to that for five-year bonds as reported in Section 4.2. The pooled (across countries) impact estimate is -2.5 bps per purchases of 1/1000 of the respective total debt (t-value of 5.1).

[**Insert Table 4 near here**]

Thirdly, to further explore the intuition that yield impact tends to decline with bond maturity, we estimate a pooled specification of our model (1)–(5). This specification restricts country-specific impact coefficients $\bar{\delta}_i$ to be the same across countries, after standardizing purchase amounts to fractions of the respective country's overall government debt. The pooled impact estimates are -3.2 bps for purchases of 1/1000 of the respective gross government debt at the two-year maturity (t-value of 4.8), -2.1 bps per purchases of 1/1000 of total debt at the seven-year maturity (t-value of 5.3), and -1.3 bps per purchases of 1/1000 of total debt at the ten-year maturity (t-value of 3.5). For comparison, and as reported in Section 4.2, the impact estimate at the five-year maturity is -2.6 bps per 1/1000 of total debt (t-value of 5.2).

The observed monotonic decline of yield impact with bond maturity is striking. Generally, further out in the term structure, yields were less sensitive to SMP purchases. Two effects may contribute to this result. First, market stress tended to raise short-term yields in particular, also relative to the long end and relative to their respective historical averages. This, in turn, suggests that liquidity risk and default risk premia were particularly elevated at the short end, which allowed purchase to be relatively more effective there. Second, the SMP may have been seen by market participants as a temporary and somewhat 'limited' program. After its announcement, market participants may not have expected the program to be active for many years, possibly reducing its effect on liquidity risk premia at the longer end of the term structure. We refer to Mesters, Koopman, and Schwaab (2014) for a more detailed analysis of the level, slope, and curvature effects from both standard and non-standard monetary policy measures in the euro area based on a robust dynamic term-structure model at the country level.

[10] The Irish seven-year yield is replaced by the respective ten-year bond yield since no seven-year benchmark bond traded in 2010 when the initial purchases were made.

4.4. Channels: signaling, default risk, or liquidity effects?

This section more closely examines which channels contribute towards explaining the price impact. Above we mentioned four channels through which the ECB's SMP could in principle have affected bond yields: (i) the 'classical' signaling channel, which signals future low monetary policy rates (Christensen and Rudebusch, 2012; Bauer and Rudebusch, 2014), (ii) signaling that affects a country's default risk premium (Hoerova et al., 2012; Corsetti and Dedola, 2013), (iii) reducing required liquidity risk premia (Duffie, Garleanu, and Pedersen, 2007; De Pooter et al., 2013), and (iv) local supply effects in weakly segmented markets (Vayanos and Vila, 2009; Duffie et al., 2007). We examine the different channels by studying the impact of bond purchases on other markets, such as that for overnight index swap (OIS) contracts and credit default swaps (CDS), as well as on bond market bid-ask spreads and the CDS-bond basis. To investigate the role of local supply effects, this section also briefly considers the yield impact from purchases in adjacent maturity brackets. We conclude that (i), the signaling of future low monetary policy rates was not important, but channels (ii), reduced default-risk premia, (iii), lower liquidity risk premia, and (iv), local supply effects, could have played a role. Of all three channels, reduced liquidity risk premia appear to be the most important.

Starting with the possibility of signaling future low monetary policy rates, Fig. 3 reports the OIS rates at different maturities on the business days before and after the announcement of the SMP on 10 May 2010 (left panel) and before and after the re-announcement and extension of the program to Italy and Spain (right panel) on 8 August 2011. Expected (average) future monetary policy rates can be inferred from such OIS rates at different maturities. Following the first announcement of the SMP in May 2010, when by far the largest impact on yields was observed, the OIS curve hardly moved. If anything, OIS swap rates went up at the long end of the curve. The OIS curve shifted slightly downward around the re-announcement of the SMP in August 2011, although the effect is minuscule at the, say, five-year maturity. If anything, the small shift is more likely due to the ECB's announcement of additional monetary accommodation in the form of an additional six-month LTRO on 4 August 2011. That OIS rates moved little if at all around announcement dates is intuitive, as we recall that the objective of the SMP was not to change the monetary policy stance regarding the future path of short-term interest rates. This suggests that the 'classical' interest rate signaling channel is not required to move yields at longer maturities.[11]

[11] OIS swap rates at longer maturities also contain a term premium. If SMP purchases had lowered bond yields via a reduction in common term premia, this should be visible as a downward shift in OIS rates as well (Cahill et al., 2013; Krishnamurthy et al., 2014). Fig. 3 demonstrates that this is not the case.

[Insert Fig. 3 near here]

Regarding the SMP's impact on default risk premia, a default-risk-related signal could in principle take several forms. For example, purchases could have been interpreted to mean that the ECB considers country yields as higher than justified based on country fundamentals, also due to contagion concerns in a monetary union. In addition, purchases could have been perceived as a costly commitment device in a strategic setting (Hoerova et al., 2012). Furthermore, purchases could signal that the ECB is willing to consider and implement unprecedented non-standard measures to continue to support the weakened banks in stressed countries.

We present two pieces of evidence that the SMP affected the default risk perceptions of market participants. First, the ordering of the re-scaled yield impact estimates in Table 3 is positively related to the average yields over our sample period (see Fig. 2). This comparison suggests that purchases send a signal whose importance increases with the respective default risk premium. This is most obvious for the Greek purchases, for which yields fall the most after controlling for debt market size and also from the highest yield level in the sample. Second, Table 5 reports estimates for the impact of €1 bn of SMP purchases on five-year CDS premia in the different countries (top left panels). We obtain these estimates by substituting changes in bond yields with changes in CDS on the left-hand side of (1).[12] The main finding is that SMP purchases have an impact on CDS spreads, and that the impact is lower than that for the corresponding bond yields. This holds for all SMP countries except Portugal, and suggests an important role for reductions in liquidity risk premia. Interestingly, the impact on CDS is positive and significant for Italy for both models m1 and m2. A positive impact of purchases on CDS but not on the bond yield could be an indication of market participants that worried about moral hazard but welcomed the reduced liquidity risk premia on the bonds.

[Insert Table 5 near here]

Regarding liquidity risk premia, next we consider bid-ask spreads as a direct measure of market liquidity. The top two panels of Fig. 4 plot bid-ask spreads between 2010–12 for Greek, Irish, and Portuguese bonds (left panels) and for Italian and Spanish bonds (right panels). The plots are striking in that they strongly suggest substantial improvements in liquidity conditions (declining bid-ask spreads) while the SMP was active in each bond market. When purchases were scaled

[12] Changes in CDS data are exceptionally fat tailed and volatile during the euro area sovereign debt crisis. We overcome the respective estimation challenges by fixing $\nu = 4$ and allowing for time-varying volatility in the factor innovation terms.

back, bid-ask spreads tended to widen again. This suggests that the impact on liquidity premia is temporary.

[Insert Fig. 4 near here]

In the context of large-scale asset purchases, bid-ask spreads are imperfect measures of asset-specific liquidity in that they apply only to a small fraction of a potentially much larger order. The CDS-bond basis can serve as an additional useful indicator of asset-specific liquidity and the well-functioning of bond markets. The CDS-bond basis represents the difference between the CDS premium and the yield spread of a corresponding bond over a risk-free bond.[13] A small (or even negative) CDS-bond basis means that the yield required on a risky bond is large relative to the premium payable on the corresponding CDS contract (Oehmke and Zawadowski, 2014). The bottom panels of Fig. 4 plot the CDS-bond basis at the five-year maturity for Greek, Irish, and Portuguese bonds (left panels) and for Italian and Spanish bonds (right panels). Again, the plots are in line with much improved market liquidity conditions. The top right panels in Table 5 report the impact estimates for SMP purchases on the CDS-bond basis. Interventions increased the CDS-bond basis in all five SMP countries.

Finally, we shed light on the question whether local supply effects might play a role in determining the relative impact of purchases along the term structure. To this end we briefly consider the impact of bond purchases in different maturity brackets. We distinguish two such brackets. The first bracket comprises SMP purchases of bonds with a maturity of less than five years. The second bracket comprises purchases of bonds with maturities of at least five years, almost all of which are between five and ten years. SMP purchases in these two brackets are almost multicollinear. In terms of timing, there are virtually no purchases in one bracket when there are not purchases in the other bracket. In addition, the relative amounts are approximately constant over time. Without reporting the respective parameter estimates for brevity, we find that purchases in the first bracket have a larger effect on short-term (two-year) yields than purchases from the second bracket.[14] This finding holds uniformly for all five SMP countries. Results for long-term bonds (seven and ten years) are mixed. We need to remain tentative when interpreting these results, as both maturity

[13] In theory, arbitrage ensures that the CDS-bond basis should be close to zero (Bai and Collin-Dufresne, 2013). In practice, arbitrage is risky and requires capital whose availability depends also on market conditions (Brunnermeier and Pedersen, 2009). The part of the bond spread which is not explained by default risk (the CDS) can be explained in particular by bond-specific liquidity. For example, Bhanot and Guo (2012) show that in the 2008–09 financial crisis, negative deviations of the CDS-bond basis are explained primarily by funding liquidity and asset-specific market liquidity. DePooter, Martin, and Pruitt (2013) show that the CDS-bond basis corresponds closely to the measure of liquidity premia they derive from a search-based asset pricing model. We calculate the CDS-bond basis taking the German benchmark bond as the risk-free asset.

[14] The estimation results are available from the authors upon request.

effects and local supply effects play a role. Nevertheless, it seems to matter where in the yield curve purchases are undertaken, at least for some segments of the term structure. Purchases of short-term bonds reduce short-term yields by more than the same amount of purchases of longer-term bonds.

4.5. Transitory dynamics and long-run effects

This section extends our baseline model (1) to allow for the possibility of contemporaneous purchases that have lagged effects on yields. The persistence of dynamic effects of bond purchases is rarely considered in the literature, with the exception of Wright (2012) and Rogers, Scotti, and Wright (2014). Among other reasons, this is due to persistence being hard to assess based on event study methodologies that focus on variation around program announcement dates. Long-run effects can materialize, for example, when the bonds are known to be held on a central bank's balance sheet until they mature, or when a signal is received that leads market participants to revise their expectations about the future.

Transitory dynamics, such as lagged effects from interventions, can naturally occur in over-the-counter markets due to dealer inventory effects (Duffie, Garleanu, and Pedersen, 2005; Vayanos and Vila, 2009). Inventory effects result from dealers quoting higher yields (lower prices) after having sold off a large position, at which point they are exposed to price risks from an unbalanced inventory position. Such market microstructure effects are expected to be particularly pronounced in stressed and illiquid markets. In a dynamic model it is therefore possible that the immediate yield impact 'overshoots' the long-run impact to some extent (Duffie, Garleanu, and Pedersen, 2007), before prices converge to their new efficient levels. If, however, following an intervention, bond prices quickly returned to the level that they would have been at without the intervention, then the yield impact identified in Section 4.2 would be entirely transitory.

To disentangle transitory dynamics from the long-run impact, we extend (1) to allow for lagged effects from purchases according to

$$\Delta y_{it} = c_i + \delta_{it} z_{it} + \sum_{k=1}^{K} \omega_{i,k} z_{i,t-k} + \beta_i' W_t + \lambda_i' f_t + \gamma_i g_{it}, \tag{6}$$

$$\text{where } \omega_{i,1} = \bar{\omega}_{i,1}, \tag{7}$$

$$\text{and } \omega_{i,k} = \bar{\omega}_{i,2} \left(\bar{\kappa}_i\right)^{k-2}, \quad \text{for } k = 2, \ldots, K, \tag{8}$$

where the parameters $c_i, \delta_{it}, \beta_i, \lambda_i, \gamma_i$ and the factors W_t, f_t, g_{it} are as before, $i = 1, \ldots, 10$. The additional scalar parameters $\bar{\omega}_{i,1}, \bar{\omega}_{i,2}$, and $0 < \bar{\kappa}_i < 1$ pertain to the five SMP countries only and capture the lagged impact of a previous intervention. The coefficient $0 < \bar{\kappa}_i < 1$ determines how quickly a lagged impact decays over time. If $\bar{\kappa}_i \approx 0$, then all dynamic adjustments take place on the first two days following an intervention. For K large, the long-run effect from a given intervention is approximately $\bar{\delta}_i + \bar{\omega}_{i,1} + \omega_{i,2}/(1 - \bar{\kappa}_i)$. We estimate the long-run effect for $K = 20$, as four weeks should be sufficient to restore a dealer's target inventory.

Table 6 reports impact estimates from the dynamic specification (6) (second column), along with the respective implied long-run effects (rightmost column). A comparison suggests that the long-run impacts are approximately half of the immediate impact in most countries. Positive estimates of $\bar{\omega}_{i,2}$ for all countries suggest that bond yields do 'bounce back' to some extent following an intervention. Mixed signs for $\bar{\omega}_{i,2}$ suggest that this reversion does not necessarily occur on the first day after an intervention. These findings are consistent with temporary dealer inventory effects. Interestingly, we observe the most pronounced transitory dynamics (i.e., the largest difference between the instantaneous and the long-run effect) for Greek debt, which is also by far the most stressed market in terms of yield levels in our sample (see Fig. 2). The lagged effects, however, are not always statistically significant. After accounting for transitory dynamics, the long-run effects from the dynamic model are slightly smaller in magnitude compared to the impact estimates from the baseline model specification in Section 4.2.

[Insert Table 6 near here]

Combining the long-run effects from Table 6 with the total purchases at the country level allows us to obtain rough estimates of a cumulated counterfactual yield reduction in a simple back-of-the-envelope calculation. For example, the -10.69 bps / 1 bn long-run impact for Greek purchases, for €30.8 bn of total purchases together represent a combined total impact of -3.3%. This is a large number, but not unreasonable given yields of more than 20% at the end of 2011 (see Fig. 2). The other cumulated effects are -1.91% in Spain, -0.03% in Ireland, -2.09% in Italy, and -1.66% in Portugal. Two main qualifications apply to this simple calculation. First, if the long-run effects — in the sense of accounting for transitory dynamics of up to four weeks — which are reported in Table 6, eventually revert to zero, then the total effects are upper bounds to the counterfactual total yield impact. Second, the impact estimates are necessarily subject to both estimation uncertainty and model risk. Small changes in the impact point estimates, when multiplied with a large number (purchases), map into large changes in the total yield reduction. We acknowledge that the long-run impact of asset purchases is more uncertain than their contemporaneous impact.

4.6. Additional discussion and robustness checks

This section provides an additional discussion of our main empirical results. First, we have implicitly ruled out systematic cross-country spillover effects from purchases in our empirical setup. If purchases in one country had a significant 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 the panel regression (1) would be lower on intervention days as a result of 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 of the negative yield impact estimates towards zero. 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, however, small. 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 have been close substitutes during that time. This, in turn, suggests that local supply effects and 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 approximately unchanged in core countries (Fig. 2). This again points towards limited cross-country effects from interventions.

Second, 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). While it is, of course, not the purchases themselves that raise yields, but only the failure of purchase amounts to match expectations that increases yields, such reasoning could contribute to 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 Fig. 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 this way. We recall, however, that the ECB 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 revealed to market participants with each intervention.

We now further explore the sensitivity of our empirical estimates to alternative panel regression specifications, and verify that our main results are robust to 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. Extending the sample to data until 28 February 2012 hardly increases the magnitude of the impact coefficients. This is likely owing to additional effects on sovereign yields that are due to two three-year LTROs conducted at that time. Third, we checked that our empirical results are not inadvertently driven by other monetary policy announcements by the ECB or other major central banks such as the Federal Reserve during our estimation sample. We do this by setting all data entries to missing values that pertain to announcement dates which Rogers et al. (2014) classify as relevant for asset purchases for the Fed and the ECB. The SMP was active on five of these Fed and ECB event dates; changes in parameter estimates are marginal as a result. Finally, to some degree the cross-sectional dimension of our panel matters. In principle, a larger cross section means more reliable inference on common factors (in particular, given fat tailed and volatile data during times of crisis) and smaller parameter standard errors. Considering a few non-stressed countries is important because interventions in stressed debt markets usually occurred at the same time. For example, from 2010 to mid-2011, interventions were usually made in Greece, Portugal, and Ireland on roughly the same dates. Taking $N = 10$, this leaves seven other countries that were not subject to intervention. That said, results are broadly unchanged when we only retain France and Germany as non-stressed countries ($N = 7$). Impact estimates become less precise and somewhat smaller in magnitude when only stressed countries are considered ($N = 5$).

5. SMP impact on yield volatility and tail risk

This section considers the impact of SMP purchases on bond yield volatility and on the probability of observing extreme yield changes on intervention days versus non-intervention days. We argue that yield volatility is lower on intervention days for most SMP countries, and that this is due to less extreme (tail) movements occurring when the Eurosystem is active in the market. Volatility and market tail risk matter, since a high level of uncertainty alone can force institutional investors and market makers to leave a given market, for example, due to binding value-at-risk constraints (see Vayanos and Vila, 2009; and Adrian and Shin, 2010). Indeed, dealers ceased to provide quotes for government bond transactions in particularly volatile periods during the debt crisis (Pelizzon et al., 2013).

The summary statistics in Table 1 in Section 3.1 already suggest a pronounced 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 demonstrates that there is a reduced risk from extreme movements on intervention days.

Table 7 presents estimates of the tail index for intervention days and non-intervention days. We again distinguish pre-crisis and debt crisis times (before and after 1 April 2010 in our sample). 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.[15] The table confirms that yield changes on intervention days tend to display thinner tails and therefore higher tail index estimates. As a result, there are fewer extreme market movements on intervention days compared to non-intervention days.

[Insert Table 7 near here]

Fig. 5 reports density (kernel) estimates that pertain to yield changes of five-year benchmark bonds. Each panel distinguishes yield changes for five-year benchmark bonds that occurred on intervention days from yield changes that occurred on non-intervention days during the debt crisis. The density plots confirm the findings from Tables 1 and 7, i.e., the SMP prevented or substantially limited extremely adverse yield movements. The visual evidence is the strongest for Greece, Ireland, and Portugal, and somewhat less strong for Spain and Italy. This difference across sets of countries likely reflects the different timing of purchases during 2010–11.

[Insert Fig. 5 near here]

6. Conclusion

This paper contributes to the literature on the effectiveness of central bank non-standard monetary policy measures by investigating the yield impact of bond market interventions within the ECB's Securities Markets Programme. We assess the yield impact of asset purchases in five euro

[15] This is a relevant issue for our data at hand. Defining the $\imath^{th}$-order statistic so that $X_\imath \geq X_{\imath-1}$ for all $\imath = 2, ..., n$, where n is the sample size, and including k observations from the right tail of the sample, the estimator of the reciprocal of the tail index is $\gamma(k) = \frac{1}{k}\sum_{j=1}^{k} ln(X_{n-j+1}) - ln(X_{n-k})$. An unbiased estimate of $\gamma(k)$ in small samples is obtained as β_0 in the regression $\gamma(k) = \beta_0 + \beta_1\kappa + \epsilon(\kappa)$. We choose $5 \leq \kappa \leq 50$ for non-intervention days and $5 \leq \kappa \leq 15$ for intervention days. We report the tail index $\alpha = \beta_0^{-1}$ in the table.

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 large announcement effects, we find that the ECB's repeated interventions had an average impact of approximately -3 bps at the five-year maturity for purchases of 1/1000 of the respective outstanding debt. We find the relatively large effect of SMP purchases can be explained in terms of reduced liquidity risk premia, default risk signaling effects, and possibly local supply effects in segmented markets. A dynamic specification points to both transitory dynamics and medium-run effects from purchases. In addition, we show that 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, notwithstanding these large effects, it cannot be argued that the introduction of the SMP was sufficient to end the euro area sovereign debt crisis. Despite the SMP, some European bond yields continued to rise shortly after the initial announcement of the SMP in May 2010 and after the extension of the program to Italy and Spain in 2011. Following the discontinuation of the SMP, the ECB decided to implement other unconventional measures to combat the crisis such as two three-year longer-term refinancing operations in December 2011 and February 2012, the OMT announcements in August and September 2012, and its commitment to keep interest rates low for an extended period in July 2013.

A. Alternative specifications

This appendix explores how sensitive our main empirical results are to the precise specification of the factor structure. It also sheds light on the extent to which the cross-sectional and time series dimension of our panel data contribute to inference on the impact coefficients.

Table A1 reports the estimation outcomes from alternative model specifications. Each is obtained from successively setting to zero certain features of our preferred model specification. Model r1 contains an intercept c_i, purchases z_{it}, three unobserved common factors f_t, two observed controls W_t, as well as one autocorrelated residual term g_{it} for each of the $N = 10$ countries in the panel. Model r2 is as model r1 except that it does not contain the unobserved common factors f_t. Model r3 does not contain any common factors. Model r4 contains only the constant, the purchases, and identically and independently distributed (iid) error terms as right-hand side variables. We observe that almost all identification comes from the cross-sectional dimension of our panel. Removing the common factors introduces a significant upward bias. (The same effect happens when running ten univariate time series regressions equation by equation.) Without common controls, the negative impact estimates move towards zero, and in part turn positive. This is intuitive, as asset purchases occurred during the most intense phases of the euro area sovereign debt crisis, and in the debt markets most affected by it. Common factors capture this effect and as a result serve as effective and parsimonious control covariates in our context.

[Insert Table A1 near here]

B. Difference-in-differences results

This appendix considers difference-in-difference regressions with observed control covariates. In particular, we estimate by least squares

$$\Delta y_{it} = c + \delta_S \cdot D_s + \delta_T \cdot D_T + \delta_{DD} \cdot I_{it} + \beta_i' W_t + \epsilon_{it}, \tag{B1}$$

where Δy_{it} is the observed change in yield of a five-year benchmark bond of country $i = 1, \ldots, N$, $N = 10$, at the daily frequency $t = 1, \ldots, T$, c is a constant, D_s is one if country i belongs to the set of SMP countries and zero otherwise, D_T is a time dummy that is one after the SMP program has been activated and zero otherwise, I_{it} is an interaction term, W_t contains observed controls, and ϵ_{it} is the error term. We include all ten euro area countries in the panel: Austria, Belgium, Germany, France, and the Netherlands (five non-SMP countries), as well as Greece, Ireland, Italy, Spain, and Portugal (five SMP countries).

[Insert Table B1 near here]

Table B1 reports the estimation results. The main parameter of interest is δ_{DD} and corresponds to our yield impact parameter δ_{it} in our model (1) in the main text. We consider four different interaction terms I_{it}. Regression d1 takes $I_{it} = D_T \cdot D\{z_{it} > 0\}$, where $D\{z_{it} > 0\}$ is a dummy variable that indicates purchases in a given market. The coefficient estimate of δ_{DD} suggests that when the SMP was active in a market on a given day, irrespective of the purchase amount, yield changes were approximately 2.8 bps lower than they would otherwise have been. This effect is not statistically significant, however.

Regression d2 uses the actual purchases rather than a dummy variable. The estimation results suggest that when the SMP was active in a debt market, yield changes were approximately -12.7 bps / bn lower than they would otherwise have been on that day. This estimate is statistically significant and approximately in line with the average effect reported in Section 4.1.

Regression d3 is as before but contains standardized purchase amounts in terms of 1% of the respective gross government debt at the end of 2010 from Eurostat. The regression results suggest that purchases of 1% of the outstanding government debt lowered yields by a total of 81 bps. This number needs to be interpreted with care, as it pools over the effects from the program announcements and the effect from continuing purchases. In addition, the low R-squared for regression d3 suggests that our control covariates W_t are only partially successful in explaining the potentially large amount of common variation across euro area yield changes.

Regression d4 controls for announcement day effects and also includes the first two principal components from the cross-country yield change data as country-specific control covariates. The principal components are extracted from our panel of five-year benchmark bond yield changes, following the standard method of Stock and Watson (2002). Importantly, we replaced observations from intervention days with the most recently available observation from a non-intervention day to avoid overcontrolling for the yield impact. Including the principal components renders the time dummy coefficient insignificant, suggesting that time fixed effects are parsimoniously and effectively controlled for. The DID impact estimate for regression estimate d4 suggests a yield impact of ongoing purchases of -30 bps per purchases of 1% of the outstanding debt. This is comparable to the panel estimate of -26 bps per 1% of debt as discussed in Section 4. Announcement effects are in line with those reported in Table 1. Without principal components as additional controls, the

DID impact estimate reduces to -11 bps per 1% of total debt.

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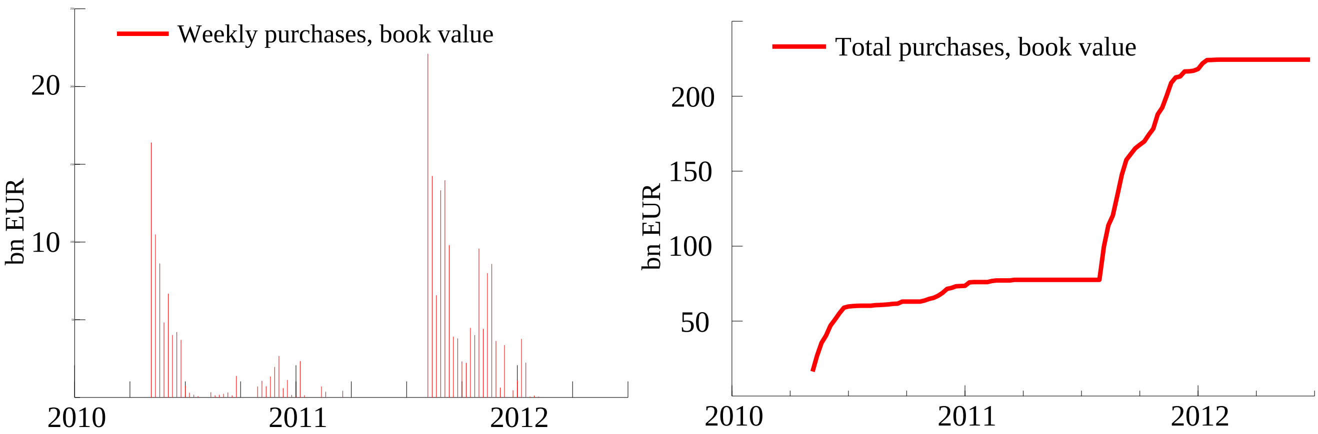

Fig. 1. Weekly and total SMP purchase amounts. The figure plots the book value of settled SMP purchases as of the end of a given week. We report weekly purchases across countries (left panel) as well as the cumulative amounts (right panel). Maturing amounts are excluded.

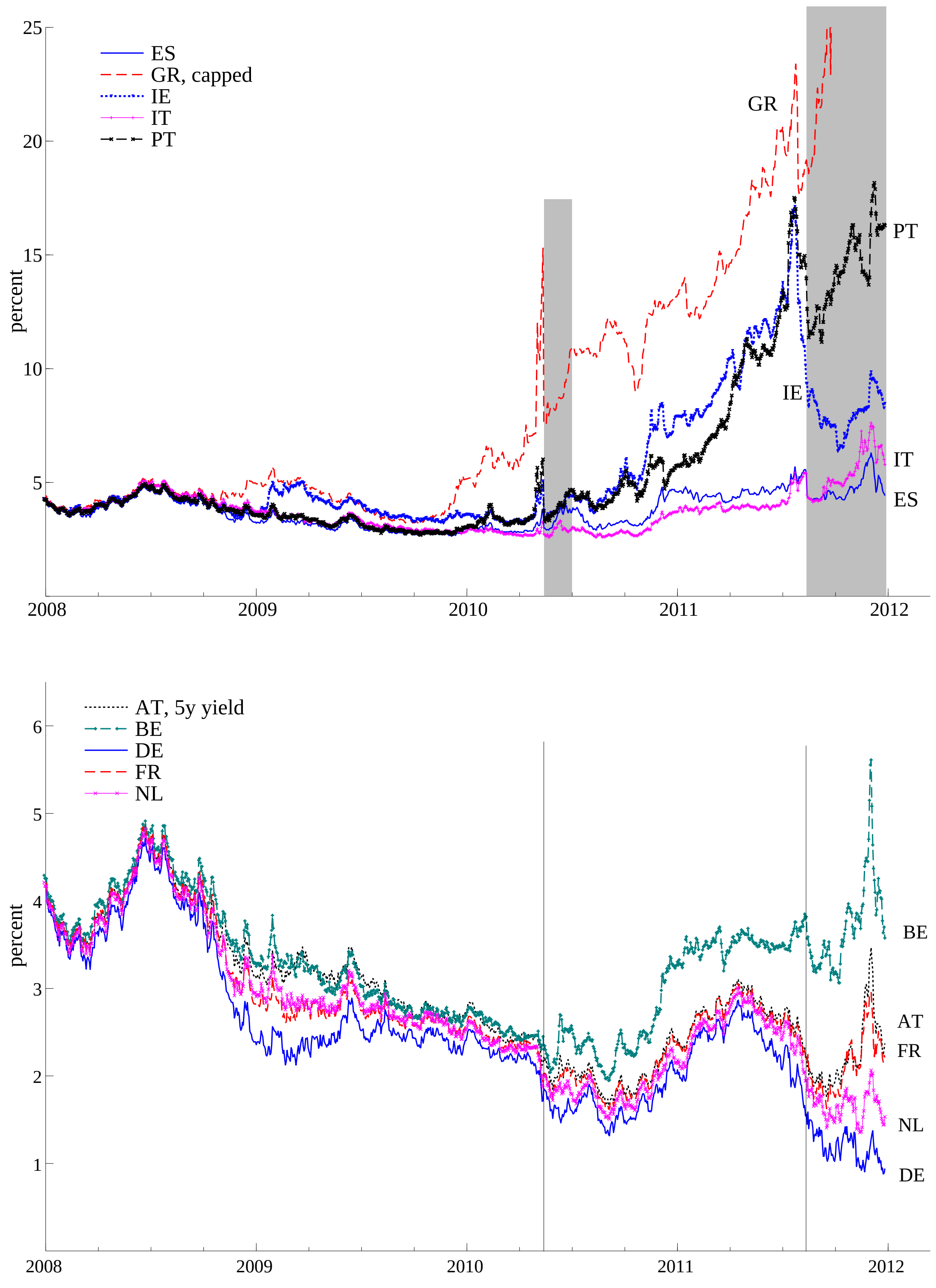

Fig. 2. Sovereign bond yield levels for euro area countries. The top and bottom panels plot yield data from the five SMP countries and five non-stressed euro area countries. The yields shown are yields to maturity of five-year benchmark bonds in percent. The shaded areas in the top panel indicate the two periods during which the SMP was the most active, cf. Fig. 1. The vertical lines in the bottom panel refer to 10 May 2010 and 8 August 2011 announcement dates.

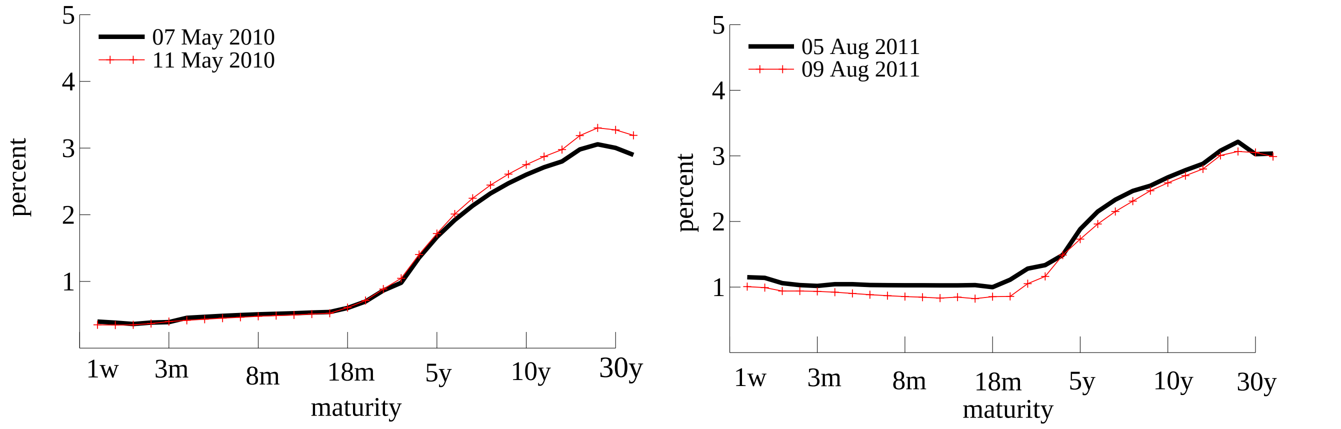

Fig. 3. OIS rates before and after SMP announcements. Overnight Index Swap (OIS) curves are shown before and after the SMP announcements on 10 May 2010 (left panel) and 08 August 2011 (right panel). The OIS data are from Bloomberg.

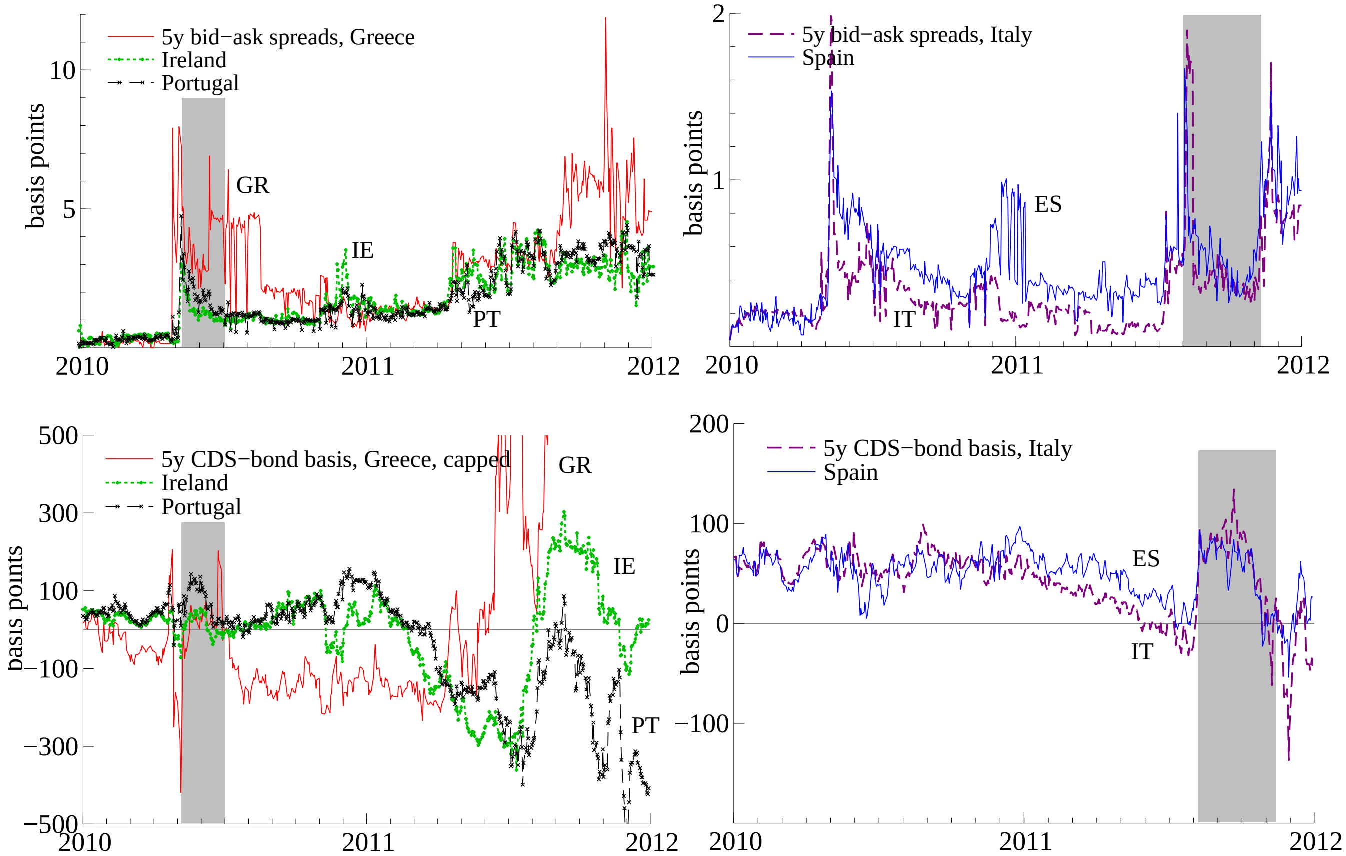

Fig. 4. Impact on the bid-ask spreads and CDS-bond basis. The bid-ask spreads (top panels) and the CDS-bond basis (bottom panels) are at the five-year maturity for Greek, Irish, and Portuguese bonds (left panels) and for Italian and Spanish bonds (right panels) from 2010–12. The shaded areas mark frequent purchases in these periods, see Fig. 1–2. Bid-ask spreads are from Thomson Reuters for five-year benchmark bonds. CDS data are from CMA via Thomson Reuters.

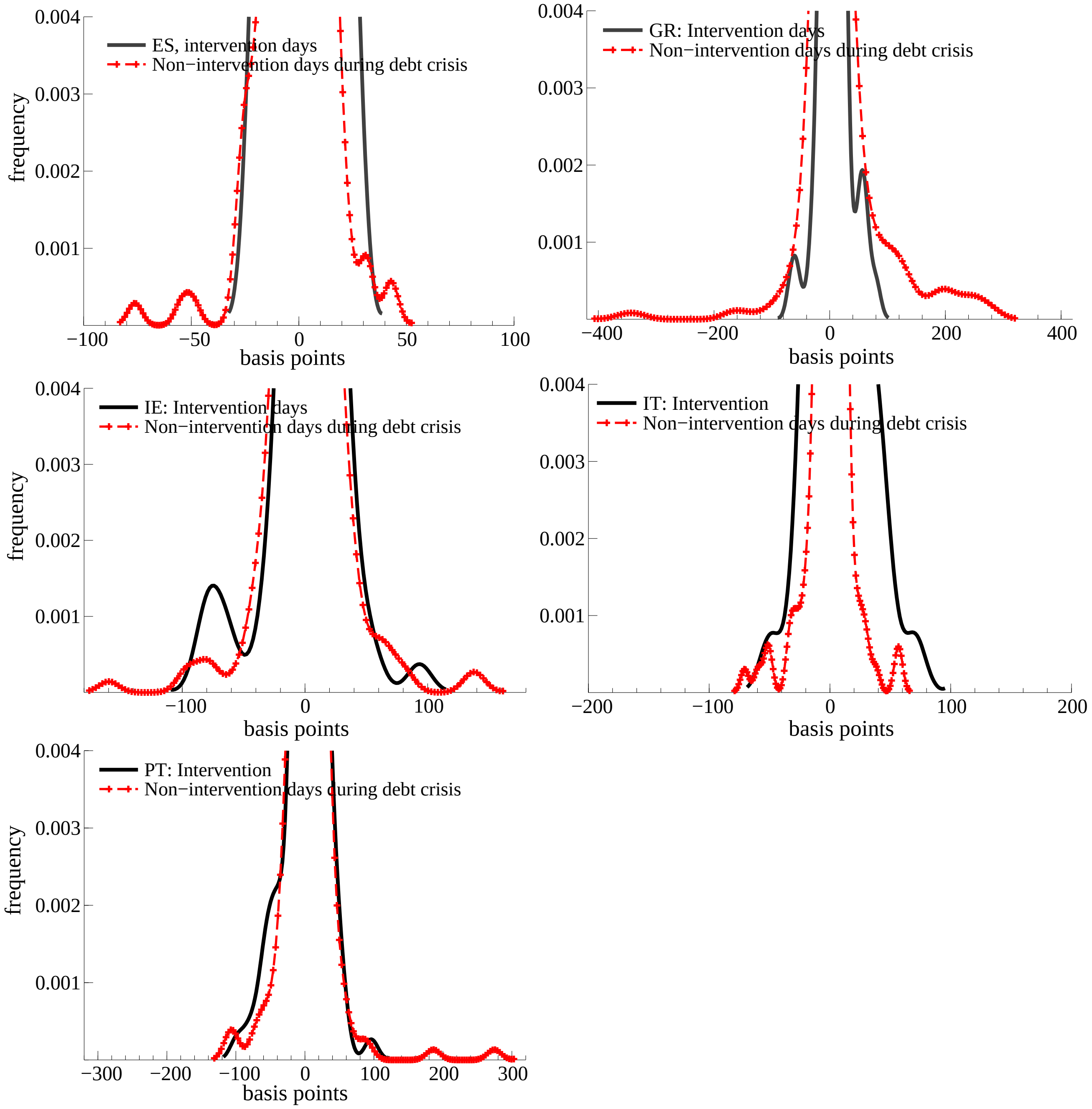

Fig. 5. Density estimates of SMP country yield changes. We report nonparametric density estimates of yield changes in five-year government bonds (based on an Epanechnikov kernel). The densities distinguish yield changes on intervention days from yield changes on non-intervention days during 1 Apr 2010 to 20 Dec 2011. The density estimates refer to benchmark bonds from 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.

Table 1: Summary statistics and announcement effects.
The summary statistics refer to yield changes of five-year benchmark bonds and are in basis points. Countries are listed in the first column. For non-intervention days we report summary statistics for the complete sample from 1 October 2008 to 20 December 2011, a 'pre-crisis' subsample from 1 October 2008 to 31 March 2010, and a 'debt crisis' subsample from 1 April 2010 to 20 December 2011. The summary statistics for intervention days are specific to each country and exclude the yield change on announcement days (even if purchases took place on those days). The final two columns report yield changes on two announcement days, 10 May 2010 (for Greece, Ireland, and Portugal) and 8 August 2011 (for Spain and Italy). The bottom four rows refer to the change in average yield across five non-SMP countries in the euro area: Austria, Belgium, France, Germany, and the Netherlands. Intervention days for non-SMP countries are days on which at least one purchase occurred.

Country	Statistic	Non-intervention days			Intervention days	Announcement days	
		All	Pre-crisis	Debt crisis		10 May 2010	8 Aug 2011
	Mean	0.0	-0.3	0.3	2.3		-97.0
ES	Median	0.2	-0.2	0.7	2.2		
	St. dev.	8.4	5.4	10.6	10.5		
	Kurtosis	17.5	4.0	13.8	3.2		
	Mean	9.1	0.4	18.8	5.8	-772.9	
GR	Median	0.8	0.1	4.0	3.5		
	St. dev.	47.5	9.8	67.0	20.0		
	Kurtosis	21.0	11.1	10.8	7.4		
	Mean	0.4	-0.2	1.1	2.5	-138.6	
IE	Median	-0.1	-0.2	0.5	4.0		
	St. dev.	17.8	7.0	25.0	23.8		
	Kurtosis	26.1	6.3	14.6	8.0		
	Mean	-0.1	-0.4	0.3	4.7		-93.0
IT	Median	0.0	-0.4	0.7	1.3		
	St. dev.	8.3	5.3	10.5	18.9		
	Kurtosis	22.8	4.4	17.8	5.4		
	Mean	1.7	-0.2	4.4	1.6	-226.4	
PT	Median	0.2	-0.5	3.2	4.4		
	St. dev.	20.3	6.0	30.0	27.4		
	Kurtosis	64.9	5.7	31.0	8.6		
	Mean	-0.4	-0.4	-0.3	0.2		
[AT+BE+	Median	-0.1	-0.3	0.1	0.1	5.6	-13.4
DE+FR+	St. dev.	5.0	5.1	4.9	5.7		
NL]/5	Kurtosis	4.4	4.9	3.7	6.6		

Table 2: Yield impact estimates.
The impact coefficients $\bar{\delta}_i$ in (2) refer to five-year benchmark bonds and are in bps per €1 bn. We report estimation results for nine different model specifications. The first four model specifications in the top panel differ only regarding the intercept term c_{it}. The intercept is either [m1] constant (CO) over the entire estimation sample from 1 Oct 2008 to 20 Dec 2011; [m2] piecewise constant (PC) over three periods: 1 Oct 2008 to 9 May 2010 (pre-SMP), 10 May 2010 to 7 Aug 2011 (initial purchases), and 8 Aug 2011 to 20 Dec 2011 (purchases after re-announcement and program extension and until the allotment of the first three-year LTRO); [m3] time-varying based on a 65-day rolling window average over non-intervention days (RW), or [m4] estimated along with the other parameters by maximum-likelihood (ML). Models [m5] and [m6] allow for time variation in factor innovation volatility (tvv), with a constant and piecewise intercept term, respectively. Model [m7] lags the observed control covariates W_{t-1} by one day. Model [m8] contains an additional unobserved factor, F3. Time effects $\bar{\delta}_t$ in the bottom rows refer to the initial announcement on 10 May 2010 and program extension on 8 Aug 2011. We refer to Table 1 for sample characteristics.

Model	m1: CO		m2: PC		m3: RW		m4: ML	
	Par	(t-val)	Par	(t-val)	Par	(t-val)	Par	(t-val)
$\bar{\delta}_{ES}$	-5.81	(6.5)	-3.83	(4.2)	-6.01	(6.7)	-6.07	(6.8)
$\bar{\delta}_{GR}$	-19.34	(3.6)	-20.62	(4.0)	-16.29	(2.9)	-19.18	(3.6)
$\bar{\delta}_{IE}$	-3.34	(0.5)	-3.21	(0.5)	-2.73	(0.5)	-3.34	(0.6)
$\bar{\delta}_{IT}$	-1.40	(2.4)	-0.16	(0.3)	-1.54	(2.7)	-1.57	(2.7)
$\bar{\delta}_{PT}$	-9.30	(1.6)	-10.04	(1.7)	-7.33	(1.3)	-8.97	(1.6)
$\bar{\delta}_{10May10}$	-87.45	(14.9)	-86.59	(14.8)	-88.54	(15.1)	-87.64	(14.9)
$\bar{\delta}_{8Aug11}$	-6.56	(7.8)	-7.52	(8.8)	-6.44	(7.6)	-6.42	(7.6)
Loglik	9712.72		9724.05		9703.21		9714.32	

Model	m5: tvv, CO		m6: tvv, PC		m7: W_{t-1}, CO		m8: F3, CO	
	Par	(t-val)	Par	(t-val)	Par	(t-val)	Par	(t-val)
$\bar{\delta}_{ES}$	-5.68	(5.3)	-3.85	(3.6)	-5.84	(6.5)	-5.71	(6.5)
$\bar{\delta}_{GR}$	-26.34	(3.4)	-27.84	(3.7)	-19.90	(3.7)	-18.87	(3.4)
$\bar{\delta}_{IE}$	0.16	(0.0)	0.26	(0.0)	-2.32	(0.4)	2.50	(0.4)
$\bar{\delta}_{IT}$	-1.04	(1.5)	0.11	(0.2)	-1.39	(2.4)	-1.44	(2.6)
$\bar{\delta}_{PT}$	-15.50	(2.6)	-16.29	(2.7)	-8.35	(1.4)	-4.21	(0.8)
$\bar{\delta}_{10May10}$	-75.80	(11.5)	-74.93	(11.4)	-88.46	(15.1)	-89.51	(13.1)
$\bar{\delta}_{8Aug11}$	-6.25	(6.4)	-7.18	(7.3)	-6.42	(7.5)	-6.70	(8.1)
Loglik	11475.68		11490.32		9656.36		9906.27	

Table 3: Yield impact and debt market size.
The table relates the yield impact estimates for five-year benchmark bonds (model m1 from Table 2) to the respective country's stock of general government gross debt. The final two columns report the yield impacts in bps per 1/1000 of public debt at the end of years 2010 and 2011. The debt data are from Eurostat.

Country	Impact per €1 bn model m1		Total debt in €bn		Impact (bps) per 1/1000 of debt market size	
	Val	(t-val)	2010	2011	2010	2011
$\bar{\delta}_{ES}$	-5.81	(6.5)	645	737	-3.7	-4.3
$\bar{\delta}_{GR}$	-19.34	(3.6)	330	355	-6.4	-6.9
$\bar{\delta}_{IE}$	-3.34	(0.5)	144	169	-0.5	-0.6
$\bar{\delta}_{IT}$	-1.40	(2.4)	1851	1908	-2.6	-2.7
$\bar{\delta}_{PT}$	-9.30	(1.6)	162	185	-1.5	-1.7

Table 4: Yield impact at different maturities.

The yield impact estimates $\bar{\delta}_i$ are in bps per €1 bn and refer to two-, seven-, and ten-year benchmark bonds. The midpoint estimate, or level factor impact, is the yield impact from an unweighted average over two-, five-, seven-, and ten-year yields. Estimation results are presented for two model specifications which differ regarding the intercept term. The intercept c_{it} is either [m1] constant (CO) over the entire estimation sample from 1 Oct 2008 to 20 Dec 2011, or [m2] piecewise constant (PC) over three periods: 1 Oct 2008 to 9 May 2010 (pre-SMP), 10 May 2010 to 7 Aug 2011 (initial purchases), and 8 Aug 2011 to 20 Dec 2011 (purchases after re-announcement and program extension and until the allotment of the first three-year LTRO). (*) The Irish seven-year impact estimate is missing since no such benchmark bond existed when most purchases occurred. The seven-year series was replaced by the ten-year series to estimate the Irish level factor impact. Time effects $\bar{\delta}_t$ in the bottom rows refer to the initial announcement on 10 May 2010 and program extension on 8 Aug 2011.

	2-Year impact				Midpoint/level (6y) impact			
	m1, CO		m2, PC		m1, CO		m2, PC	
	Par	(t-val)	Par	(t-val)	Par	(t-val)	Par	(t-val)
$\bar{\delta}_{ES}$	-9.57	(8.9)	-7.12	(6.6)	-6.80	(8.3)	-4.97	(6.0)
$\bar{\delta}_{GR}$	-32.24	(3.2)	-35.13	(3.6)	-18.49	(3.4)	-19.73	(3.8)
$\bar{\delta}_{IE}$	-9.30	(1.0)	-9.50	(1.0)	-4.83	(0.9)	-4.68	(0.8)
$\bar{\delta}_{IT}$	-1.50	(2.2)	0.32	(0.5)	-1.55	(3.0)	-0.39	(0.7)
$\bar{\delta}_{PT}$	-6.74	(0.8)	-6.60	(0.8)	-7.32	(1.4)	-7.56	(1.4)
$\bar{\delta}_{10May10}$	-105.40	(11.8)	-104.73	(11.7)	-82.23	(15.5)	-81.83	(15.4)
$\bar{\delta}_{08Aug11}$	-8.14	(8.1)	-9.55	(9.4)	-6.85	(9.2)	-7.76	(10.4)
Loglik	8231.63		8232.14		10801.63		10804.68	

	7-Year impact				10-Year impact			
	m1, CO		m2, PC		m1, CO		m2, PC	
	Par	(t-val)	Par	(t-val)	Par	(t-val)	Par	(t-val)
$\bar{\delta}_{ES}$	-6.13	(7.3)	-4.14	(4.9)	-4.12	(5.6)	-2.85	(3.9)
$\bar{\delta}_{GR}$	-11.64	(3.3)	-12.11	(3.5)	-8.51	(2.1)	-9.35	(2.4)
$\bar{\delta}_{IE}$	-*	-	-*	-	-2.84	(0.7)	-2.53	(0.6)
$\bar{\delta}_{IT}$	-1.77	(3.3)	-0.68	(1.3)	-0.47	(1.1)	0.19	(0.4)
$\bar{\delta}_{PT}$	-8.51	(1.8)	-9.00	(1.9)	-5.53	(1.4)	-5.70	(1.4)
$\bar{\delta}_{10May10}$	-68.25	(14.9)	-68.51	(15.0)	-65.99	(15.7)	-65.77	(15.7)
$\bar{\delta}_{08Aug11}$	-6.70	(8.5)	-7.73	(9.9)	-6.99	(11.0)	-7.54	(11.8)
Loglik	10485.53		10495.68		11067.79		11079.88	

Table 5: Impact on CDS and CDS-bond-basis.
The impact coefficients $\bar{\delta}_i$ in the top panel refer to five-year CDS (left columns) and to the CDS-bond basis (right columns). The impacts are in bps per €1 bn. We report estimation results for two different model specifications with (i) the intercept term c_{it} constant, CO, over the estimation sample, and (ii) piecewise constant, PC.

	5y CDS				CDS-bond basis			
	m1, CO		m2, PC		m1, CO		m2, PC	
	Par	(t-val)	Par	(t-val)	Par	(t-val)	Par	(t-val)
δ_{ES}	-3.42	(3.4)	-0.63	(0.6)	1.52	(1.9)	2.02	(2.6)
$\bar{\delta}_{GR}$	-10.51	(0.7)	-14.72	(1.0)	21.45	(0.7)	20.38	(0.7)
$\bar{\delta}_{IE}$	1.18	(0.3)	-1.66	(0.4)	11.91	(2.5)	12.17	(2.6)
$\bar{\delta}_{IT}$	1.29	(2.4)	3.80	(5.6)	1.58	(3.4)	2.12	(4.3)
$\bar{\delta}_{PT}$	-13.59	(3.1)	-12.77	(2.9)	9.54	(2.0)	11.49	(2.4)
$\bar{\delta}_{10May10}$	-25.18	(4.3)	- 25.68	(4.4)	26.58	(3.8)	25.42	(3.6)
$\bar{\delta}_{08Aug11}$	-7.59	(9.6)	-8.95	(11.3)	1.84	(2.4)	1.36	(1.8)
Loglik	11596.05		2596.31		8493.15		8498.39	

Table 6: Yield impact from a dynamic specification.
We report selected parameter estimates for the dynamic model specification (6). The yield impact coefficients $\bar{\delta}_i$ are in bps per €1 bn, and refer to five-year benchmark bonds. Specification (6) implies a long-run effect of approximately $\bar{\delta}_i + \bar{\omega}_{i,1} + \omega_{i,2}/(1 - \bar{\kappa}_i)$ for large K and $0 < \kappa_i < 1$. We report the long-run effect for $K = 20$. The estimation sample is 1 Oct 2008 until 20 Dec 2011.

	$\bar{\delta}_i$	$\bar{\omega}_{i,1}$	$\bar{\omega}_{i,2}$	$\bar{\kappa}_i$	Long run
ES	-7.83	0.23	2.88	0.11	-4.37
(t-val)	(7.7)	(0.3)	(3.3)	(1.3)	
GR	-27.62	15.80	0.85	0.25	-10.69
(t-val)	(4.2)	(2.3)	(0.3)	(0.4)	
IE	0.51	-6.76	3.35	0.00	-0.21
(t-val)	(0.1)	(1.0)	(0.6)	(0.0)	
IT	-3.08	-0.95	1.52	0.21	-2.11
(t-val)	(4.2)	(1.7)	(3.8)	(3.4)	
PT	-1.60	-10.40	4.30	0.00	-7.70
(t-val)	(0.3)	(1.9)	(0.8)	(0.0)	
$\bar{\delta}_{10May10}$	-90.55				
(t-val)	(12.4)				
$\bar{\delta}_{08Aug11}$	-5.24				
(t-val)	(5.4)				
Loglik	9920.34				

Table 7: Interventions lower the probability of extreme tail movements.
The table reports estimates of the Hill (1975) tail index. We recall that the smaller the tail index the more probable are extreme market moves. The estimates are obtained following the method of Huisman, Koedijk, Kool, and Palm (2001) which corrects for a small-sample bias in the Hill (1975) estimator. 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).

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

Table A1: Alternative specifications.
We present four alternative specifications and denote (r^f, r^w, r^g) as the number of unobserved common factors f_t, common controls W_t, and autocorrelated country-specific residuals g_{it}, respectively. We present results for model r1, (3,2,1) with three unobserved common factors, two observed control covariates, and one autocorrelated residual each; model r2, (0,2,1) which omits the common unobserved factors; model r3, (0,0,1), with only autocorrelated residual terms; and model r4, (0,0,0) with only iid error terms.

Model	r1, (3,2,1)		r2, (0,2,1)		r3, (0,0,1)		r4, (0,0,0)	
	Par	(t-val)	Par	(t-val)	Par	(t-val)	Par	(t-val)
$\bar{\delta}_{ES}$	-5.71	(6.5)	-0.59	(0.5)	-0.47	(0.4)	-0.10	(0.1)
$\bar{\delta}_{GR}$	-18.87	(3.4)	-17.14	(3.0)	-18.04	(3.1)	-14.90	(3.0)
$\bar{\delta}_{IE}$	2.50	(0.4)	-1.84	(0.3)	-1.76	(0.3)	-1.58	(0.3)
$\bar{\delta}_{IT}$	-1.44	(2.6)	2.23	(2.9)	2.31	(3.0)	2.52	(3.7)
$\bar{\delta}_{PT}$	-4.21	(0.8)	-4.38	(0.7)	-3.43	(0.5)	-5.37	(0.9)
$\bar{\delta}_{10May10}$	-89.51	(13.1)	-112.48	(18.3)	-111.98	(18.2)	-110.59	(15.74)
$\bar{\delta}_{8Aug11}$	-6.70	(8.1)	-13.56	(12.8)	-12.80	(12.0)	-13.81	(12.81)
Loglik	9906.27		6834.11		6662.89		6423.23	

Table B1: Difference-in-differences estimation results.

We report estimation results for DID regressions $\Delta y_{it} = c + \delta_S \cdot D_s + \delta_T \cdot D_T + \delta_{DD} \cdot I_{it} + \beta_i' W_t + \epsilon_{it}$, where the dependent variables are yield changes in basis points. We distinguish four models d1–d4 that differ with regard to their interaction terms and control covariates. Regression d1 takes $I_i t = D_T \cdot D\{z_{it} > 0\}$, where $D\{z_{it} > 0\}$ is a dummy variable that indicates purchases in a given market. Regression d2 takes $I_i t = D_T \cdot z_{it}^{bn}$, where z_{it}^{bn} are purchases in €bn. Regression d3 is as d2 but considers purchases that are standardized in terms of market size (gross government debt). Regression d4 is as d3 but in addition contains announcement effect dummy variables (D_τ) and the first two principal components from yield data (PC_t) as additional control covariates. The t-values are based on Newey-West (1994) heteroskedasticity and autocorrelation-consistent standard errors.

Model	d1		d2		d3		d4	
I_{it}	$D_T \cdot D\{z_{it} > 0\}$		$D_T \cdot z_{it}^{bn}$		$D_T \cdot z_{it}^{\%MS}$		$D_T \cdot z_{it}^{\%MS}$	
Controls	W_t		W_t		W_t		W_t, D_τ, PC_t	
	Par	(t-val)	Par	(t-val)	Par	(t-val)	Par	(t-val)
c	-1.22	(3.3)	-1.40	(4.2)	-1.57	(4.6)	-0.63	(1.7)
δ_s	2.58	(3.5)	2.95	(4.2)	3.29	(4.5)	2.83	(4.1)
δ_T	1.95	(2.7)	2.21	(3.6)	2.65	(4.0)	0.82	(1.1)
δ_{DD}	-2.79	(1.2)	-12.71	(2.1)	-81.00	(2.8)	-30.29	(4.1)
R^2	0.02		0.04		0.06		0.42	