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Abstract

We assess the yield impact of asset purchases within the ECB's Securities Markets Programme in five euro area sovereign bond markets during 2010-11. Identification is non-trivial and based on time series panel data regression on predetermined purchases and control covariates. In addition to large and economically significant announcement effects, we find an average impact at the five year maturity per €1 bn of bond purchases of approximately -1 to -2 bps (Italy), -3 bps (Ireland), -4 to -6 bps (Spain), -6 to -9 bps (Portugal), and up to -17 to -21 bps (Greece). The impact depends on market size and a default risk signal, and is approximately -3 basis points at a five-year maturity for purchases of 1/1000 of the respective debt market. Bond yield volatility is lower on intervention days for most SMP countries, due to less extreme movements occurring when the Eurosystem is active as a buyer. A dynamic specification points to both transitory and longer-lived effects from purchases.

Keywords: Central bank asset purchases, European Central Bank, Securities Markets Programme, effectiveness of non-standard monetary policy measures.
JEL classification: C32, G12.

Non-technical summary

The effectiveness of the ECB's Securities Markets Programme (SMP), as well as that of large scale asset purchase programs conducted by other central banks such as the Federal Reserve or Bank of England, are subject to considerable academic and policy debate. The objective of this paper is to assess the effectiveness of asset purchases undertaken within the SMP, in particular in terms of their yield impact. We address the following questions: Have SMP asset purchases affected bond yields in secondary debt markets for the respective countries? If so, by how much? Have purchases affected the volatility of yield changes? Are the effects entirely temporary or are they longer lived? Finally, which transmission channels are the most important for yield impact during a sovereign debt crisis? We report empirical findings that relate to these questions.

We identify the yield impact of SMP purchases through robust time series regression of country-specific yield changes on SMP purchases. We explain how the decision-making processes and coordination within the Eurosystem require the purchase decisions and purchase amounts to be essentially predetermined at a daily frequency. In addition, effective observed and unobserved (latent) control covariates help us control for the fact that purchases are undertaken during a severe sovereign debt crisis.

We show that government bond purchases undertaken within the SMP were effective in affecting yields even despite the context of the severe sovereign debt crisis and the controversy which surrounded it. In addition to large announcement effects, we find that the repeated interventions had an impact ranging from approximately -1 to -2 basis points (Italy) and up to -17 to -21 basis points (Greece) at the five-year maturity per €1 bn of bond purchases. The remaining impact estimates take intermediate values, from approximately -3 bps/bn (Ireland), -4 to -6 bps/bn (Spain), and -6 to -9 bps/bn (Portugal), where the ranges per country refer to different point estimates across different model specifications. The cross-country differences in yield impact can be explained by different sizes of the respective markets and a default risk signal which pertains to market participants' beliefs about the uncertain and time-varying probability of a credit event. Per 1/1000 of the respective debt market, the impact estimates are approximately -3 basis points at a five-year maturity for Italy and Spain.

Finally, we attempt to address the question about how long lasting the effects of intervention are. The persistence of effects of bond purchases is hardly considered in the literature, if at all. Among other reasons, this is due to persistence being extremely challenging to assess based on event study methodologies centred around program announcement dates. Given that the SMP purchases are repeated interventions in the same market, we can estimate a dynamic specification that allows for lagged effects from contemporaneous purchases. We find tentative evidence for both transitory and long-run effects, and estimate that the long run impact is on average approximately three quarters of the immediate impact. Longer-lived effects from purchases may be due to long-lasting reductions in the local supply of bonds. In addition, a signal pertaining to the default risk premium may also have longer-lasting effects.

1 Introduction

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

The effectiveness of the Securities Markets Programme - in line with that of other asset purchase programs - is subject to academic, public, and policy debate. In this paper, we contribute to the literature on impact evaluation of non-standard monetary policy measures by assessing asset purchase interventions within the ECB's Securities Markets Programme (SMP) during 2010-2012 in five euro area countries: Greece, Ireland, Portugal, Spain, and Italy. The SMP targets government debt securities. About €220 billion (bn) of bonds (par value, excluding redemptions) were acquired from 2010 to early 2012.

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

The objective of this paper is to assess the effectiveness of asset purchases undertaken within the SMP, in particular in terms of their yield impact. We seek to address the following questions: have SMP asset purchases affected bond yields in secondary debt markets for the respective countries? If so, by how much? Have purchases affected the volatility of yield changes? Are the effects entirely temporary or are they longer lived? Finally, which transmission channels are the most important for yield impact during a sovereign debt crisis?

We show that government bond purchases undertaken within the SMP were effective in affecting yields even despite the context of the severe sovereign debt crisis and the controversy which surrounded it. In addition to large announcement effects, we find that the repeated interventions had an impact ranging from approximately -1 to -2 basis points (Italy) and up to -17 to -21 basis points (Greece) at the five-year maturity per €1 bn of bond purchases. The remaining impact estimates take intermediate values, from approximately -3 bps/bn (Ireland), -4 to -6 bps/bn (Spain), and -6 to -9 bps/bn (Portugal), where the ranges per country refer to different point estimates across different model specifications. The cross-country differences in yield impact can be explained by different sizes of the respective markets and a default risk signal. Per 1/1000 of the respective debt market, the impact estimates are approximately -3 basis points at a five-year maturity for Italy and Spain. Three quarters of the immediate yield impact appears to be longer lived, possibly because the overall supply effects and signal effects can be longer lived. We furthermore document that bond yield volatility is lower on intervention days for most SMP countries, due to less extreme (tail) movements that occur when the Eurosystem is active in the market.

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

In a first analysis of yield and daily disaggregated purchase data we find that, on average, yield changes and SMP purchases are positively correlated. Bond yields did not fall (compared to their previous close) on average on days during which the Eurosystem purchased bonds in a given debt market. This observation is not surprising, given that the program explicitly targets debt markets that were perceived as dysfunctional - of which excessively high and volatile yields are symptoms. The program implies buying debt securities for which there is limited private sector demand, during intense phases of debt crisis, in market segments that are the most affected by it. This observation suggests that the positive correlation of yield changes and purchase amounts over time is due to the shared exposure to a third factor: the backdrop of an intense sovereign debt crisis. In the case of the SMP, rising and volatile yields are the symptom of the non-standard setting in which this non-standard measure operated. The identification of the impact effect is a central focus of this paper.

We identify the yield impact of SMP purchases through robust time series regression of country-specific yield changes on SMP purchases. We explain how coordination within the Eurosystem requires the purchase decisions and purchase amounts to be essentially *predetermined* at a daily frequency. Substantial coordination of purchases is required since it is not a single institution (the ECB) but the Eurosystem (the ECB and the 17 national central banks) that undertakes the purchases. Since 18 institutions are involved, a strategy was usually fixed before markets open. In addition, which markets are perceived as dysfunctional (along with other guidance) is determined during the meetings of the ECB's Governing Council in which the six Executive Board members and the 17 Governors of the national central banks meet. The scope of the SMP was always subject to the decisions of the Governing Council. The guidance and constraints set by the Governing Council remained in place when the purchases are being implemented. Both these institutional factors mean, from the point of view of an econometrician, that intervention day yield changes and purchase amounts are not simultaneously determined. We treat purchase amounts as predetermined covariates in time series panel regression. If there were simultaneity (systematic 'leaning against the wind', for example), our estimates would be lower bounds in terms of absolute magnitude.

Observed and unobserved (latent) control covariates help us control for the fact that purchases are undertaken against the backdrop of a severe sovereign debt crisis. Some control covariates are easily observed and readily available. For example, we include the U.S. VIX volatility index as a global liquidity proxy, and a euro area risk aversion corporate yield spread. Both covariates help explain yield changes across countries and over time. However, a large share of systematic co-movement across euro area sovereign yields during the crisis remains unaccounted for after conditioning on observed covariates. We therefore allow for latent common correlated effects that capture the remaining common movement. In our panel time series regression framework, common observed and latent factors serve as powerful control covariates, while at the same time also providing insurance against dynamic and cross-sectional model mis-specification. That a pronounced factor structure underlies bond yields in a monetary union is intuitive and a recurring finding in the sovereign risk literature, see, for example, Pan and Singleton (2008), Ang and Longstaff (2011) and Longstaff, Pan, Pedersen, and Singleton (2011).

We present four main empirical findings. These results are robust to alternative specifications. First, the results from our baseline model suggest that yields are lowered on average, per €1 bn of asset purchases, by approximately -1 to -2 basis points (Italy) and up to -17 to -21 basis points (Greece). These estimates, both in terms of €1 bn and in terms of relative market size, are considerably larger than what is found in the literature for purchases of U.S. Treasuries during 2008-09 within the Federal Reserve's Large-Scale Asset Purchase (LSAP) program, see for example Gagnon, Raskin, Remache, and Sack (2011), Krishnamurthy and Vissing-Jørgensen (2011), D'Amico and King (2013), and Cahill, D'Amico, Li, and Sears (2013). This is intuitive given the differences in market sizes, liquidity, and default risk premia. Announcement effects are statistically significant and economically large. We estimate that for every €1 bn of purchases on announcement days, yield impacts are substantially higher by an additional 86 basis points on 10 May 2010 and 7 basis points on 8

August 2011. Possible concerns regarding the senior creditor status of the official purchases do not appear to have outweighed the combined yield-reducing effects for the purchases analyzed in this study.

Second, our yield impact estimates are inversely related to the size and liquidity of the debt market in question. That is, a given amount of purchases has a larger impact in a smaller and thus less liquid debt market. Controlling for market size, the yield impact tends to be higher the higher the interest rate (default risk premium) on intervention days. We rationalize the relatively large impact effects from SMP purchases in terms of reduced liquidity risk, local supply (portfolio balance) effects and signalling effects. This interpretation is in line with most of the empirical literature and theoretical work on government bond pricing in over-the-counter markets, see, for example, Duffie, Garleanu and Pedersen (2005, 2007). In such models, the flow of purchases reduces liquidity risk premia by making a counterparty easier to find. Second, purchases of sovereign bonds reduce the (local) supply of government bonds. Assuming that demand is not perfectly elastic, a reduction in effective supply should raise prices and lower yields, see also Vayanos and Vila (2009). Finally, the flow of purchases may have been interpreted by market participants to mean that the Governing Council regards country yields as higher than justified based on country fundamentals (for example due to contagion concerns in a monetary union and high liquidity risk premia), and that it is willing to consider and implement non-conventional approaches to combat the crisis.

Third, we document an effect of purchases on bond yield volatility. The standard deviation of yield changes on intervention days is lower on average than that on non-intervention days during the debt crisis in most SMP countries. We argue that this is the case because purchases prevent extreme downside price movements on intervention days. Kurtosis and tail index estimates suggest that the lower volatility on intervention days compared to non-intervention days is due to fewer extreme movements. This is relevant since high bond yield volatility alone may force institutional investors and capital constrained market makers to leave a given market, in particular if there are binding value-at-risk constraints, see for example Vayanos and Vila (2009) and Adrian and Shin (2010).

Finally, we attempt to address the question about how long lasting the effects of intervention are. Doing so is challenging, and we merely provide tentative evidence. With the exception of Wright (2012), the persistence of effects of bond purchases is not considered in the literature. Among other reasons, this is because persistence is extremely challenging to assess based on event study methodologies centred around program announcement dates. Given that the SMP purchases are repeated interventions in the same market, we can estimate a dynamic specification that allows for lagged effects from contemporaneous purchases. We find both transitory and long-run effects, and estimate that the long run impact is approximately three quarter of the immediate impact. We conjecture that longer-lived effects from purchases are due to longer-lasting reductions in the local supply of bonds; in addition a signal pertaining to the default risk premium may also have longer-lasting effects.

The papers that are most related to ours are De Pooter, Martin, and Pruitt (2012) and Ghysels, Idier, Manganelli, and Vergote (2012). De Pooter et al. (2012) investigate the effects of purchases within the SMP on yields within a search-based asset pricing model that allows for sovereign default. Purchases can have a permanent effect in their theoretical framework by taking supply out of the market. Their empirical estimates suggest that the purchases led to significant temporary and lasting decreases in liquidity premia. Compared to De Pooter et al. (2012), we focus on the identification of the overall yield impact, instead of focusing on liquidity risk. Furthermore, we assess volatility effects and extreme market movements, as well as dynamic effects from purchases. Finally, we have the actual data of recorded purchases available and do not rely on a rule of proportionality to infer them from the weekly disclosed information. Ghysels et al. (2012) provide a high frequency assessment of purchases within the SMP. Based on data sampled at 15 minute intervals, the authors seek to isolate the immediate effects from purchases from other shocks that move the market. In contrast to their study, we rely on control covariates at the daily frequency to account for the fact that purchases occur against the background of an extreme sovereign debt crisis. This allows us to go beyond *local* impact and volatility effects. Despite marked differences in terms of modeling setup and econometric techniques, their impact estimates are comparable to ours in terms of overall magnitude. In contrast to both studies, we consider all SMP countries and relate the cross-country variation to observed market characteristics such as debt market size.

The remainder of the paper is structured as follows. Section 2 introduces the SMP by comparing it to other central bank asset purchase programs and discusses channels of effectiveness. Data and the modeling strategy are discussed in Section 3. Section 4 presents our main empirical findings. Section 5 considers changes in volatility and the risk of extreme market movements. Section 6 concludes.

2 Large scale asset purchases and yield impact

2.1 A comparison of LSAP, QE, and SMP

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

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

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

Third, in terms of *implementation strategy*, both LSAP and QE programs announced total amounts of purchases over certain time horizons. The actual purchases are usually undertaken in the form of auctions at relatively constant intervals. By contrast, whilst on the two key announcement dates for the SMP - the initial announcement on 10 May 2010 and that of the reactivation of the program on 7 August 2011[1] - the ECB announced government bond purchases and their objective, the ECB did not disclose the total amounts that would be spent, a time frame over which the program would be active, or a set of securities that would be targeted. These marked differences in communication also imply that event study methodologies around announcement days are less appropriate for our data. In the case of the SMP, almost no details apart from the fact that interventions would be undertaken were disclosed on announcement days. Similarly, no public meetings and policy announcements accompany the purchases.

2.2 Yield impact studies and transmission channels

The stark differences in objectives and practical implementation across different large scale asset purchase programs suggest that their respective impact on bond yields may operate through different transmission channels. In the case of LSAP and QE, almost all relevant information is revealed on the announcement day. Flow effects from announced purchases are likely to be relatively low due to the depth and liquidity of the respective debt markets. In the case of LSAP and QE, signalling is more likely to take place regarding future monetary policy rates rather than the default risk premium.

For the U.S., Gagnon et al. (2011) find a cumulative decline of 10-year yields by 91 basis points following eight key announcements regarding the LSAPs. D'Amico, English, Lopéz-Salido, and Nelson (2011) identify a reduction of longer-term yields of 44 basis points due to LSAP1, which had a size of \$300 bn, and a reduction of 55 basis points due to LSAP2, which amounted to \$600 bn. In addition, D'Amico and King (2013) identify temporary flow effects of purchases of around 3.5 basis points for a \$5 bn operation. This translates into temporary effects of 0.7 basis points per \$1 bn of purchases. These flow effects are defined as differences in the yield of securities that were bought relative to comparable securities that were not bought. Using an event study methodology, Krishnamurthy and Vissing-Jørgensen (2012) find that for both LSAP1 and LSAP2, a signalling effect, a stock or supply effect, and an inflation channel are at work, while there is little evidence for a (duration)

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

risk channel. For the U.K., Joyce, Lasaosa, Stevens, and Tong (2011) identify a cumulative reduction of around 100 basis points following six key announcements regarding QE. For maturities beyond 10 years, the authors find effects of up to 50 basis points. Christensen and Rudebusch (2011) argue that much of the initial yield impact came from the signal that lower long-term rates are warranted.

Regarding transmission channels, local supply, liquidity, and signalling effects are also likely to be present for SMP purchases. As total amounts, purchase schedules, and targeted securities are not announced, it is unlikely that these effects are fully captured by a single large reduction in yields on the announcement day. Asset purchases reduce the supply of assets available to the private sector. As a result, assuming that demand schedules are downward sloping and remain fairly stable, the purchases should tend to increase prices and lower yields. Local supply effects may be particularly strong in segmented markets. Government bond markets were considerably fragmented during the sovereign debt crisis, see e.g. ECB (2012a). Liquidity effects from purchases are likely to be present due to considerably worse market conditions.

Finally, bond purchases under the SMP also send a signal. First, purchases may be interpreted to mean that the Eurosystem regards country yields as higher than justified based on country fundamentals. This may be due to high liquidity risk and contagion concerns. Second, purchases may also be understood as a signal that the Eurosystem is willing to consider and implement non-conventional approaches to combat the crisis. In either case, market participants can learn from the central bank's actions. Since purchases are costly, they can increase the impact of central bank communication in a strategic setting, see Hoerova, Monnet, and Temzelidesc (2012). In related settings, a central bank can help coordinate market expectations in a setting of multiple equilibria, see Corsetti and Dedola (2013) and references therein. Finally, as no specific duration was announced, the flow of purchases provides information that the SMP is still active.

3 Data, identification, and regression setup

3.1 Data

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

Figure 1 plots the development of yields since 1 January 2008 for five SMP countries (top panel) and five non-stressed countries (bottom panel). Two shaded areas indicate when the SMP program was most active (compare Figure 2). Strong announcement effects are clearly visible in the data. In addition, sovereign yields are highly correlated over time and in the cross section, suggesting an unmistakable role for common factors. In addition, yields during the debt crisis also exhibit occasional larger moves.

As a second panel, we consider SMP bond purchases by country at a daily frequency. Bond purchases are entered at par values. Assets are purchased in over-the-counter dealer markets via non-anonymous trades. On intervention days, market participants learn relatively quickly that SMP-related trades are taking place. Figure 2 plots weekly total purchases across countries as well as their accumulated book value over time. Noticeably, purchases

Figure 1: Sovereign bond yield levels for euro area countries

The top and bottom panels plot yield data from five SMP countries and five non-stressed euro area countries, respectively. The yields shown are yields-to-maturity of five year benchmark bonds in percentage points. The shaded areas indicate two periods when the SMP was the most active (see also Figure 2).

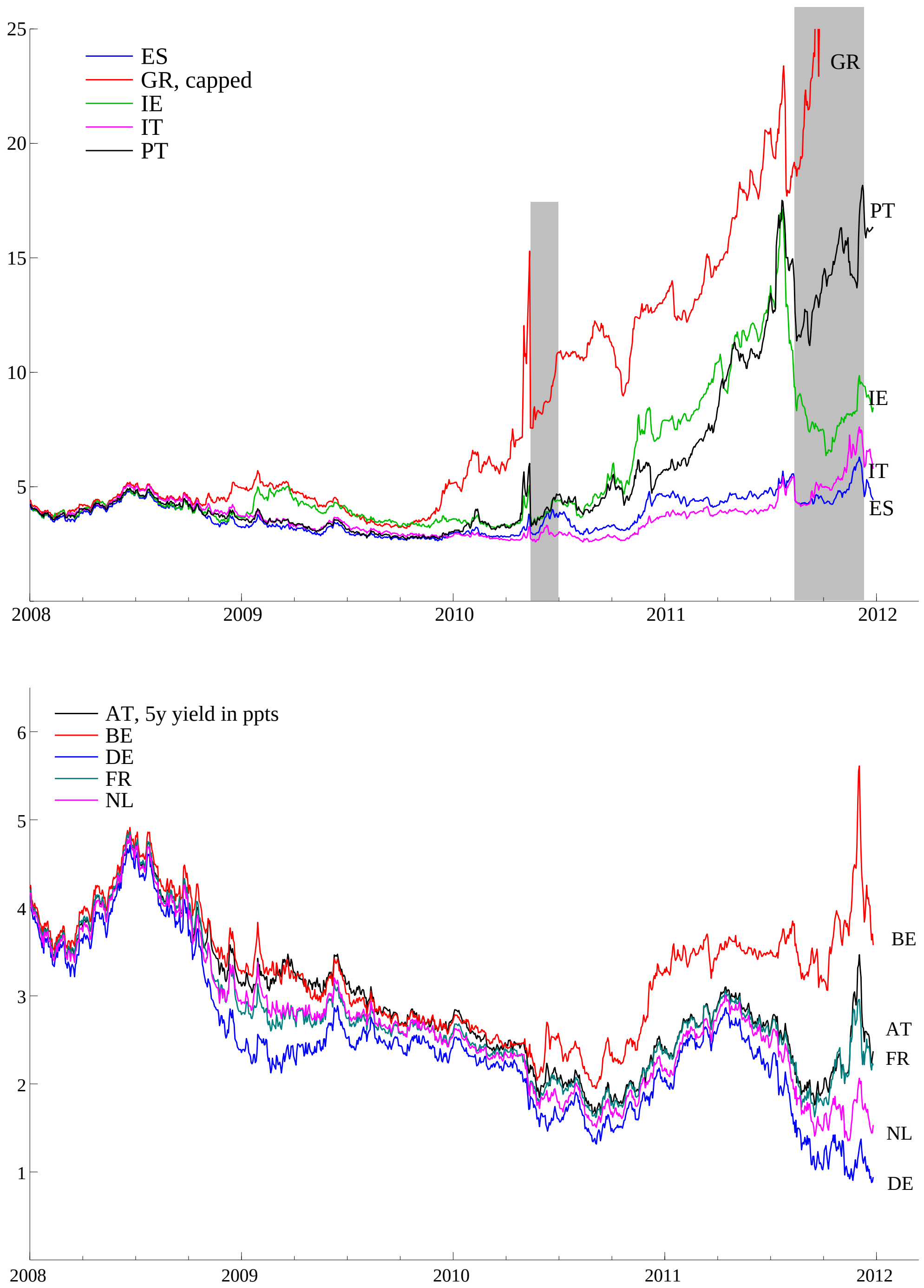
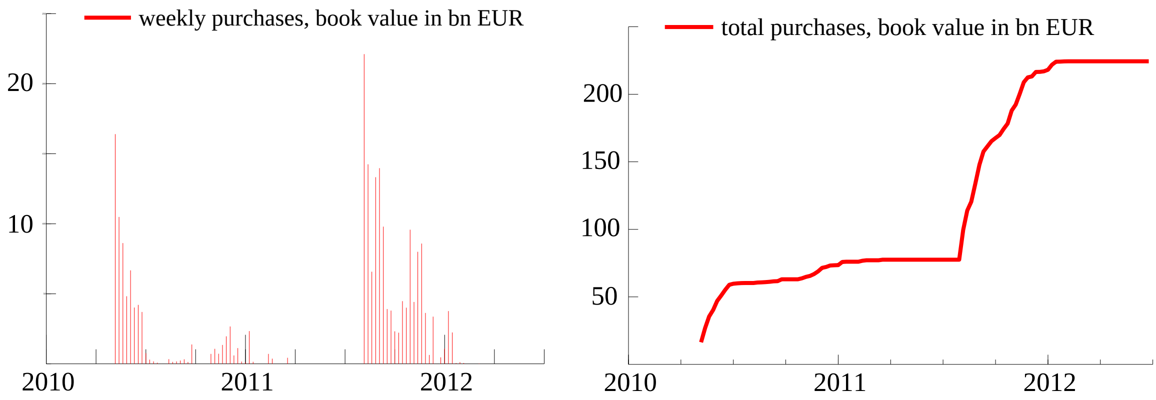


Figure 2: Weekly and total SMP purchase amounts
The figure plots the book value of settled SMP purchases as of the Friday of a given week. We report weekly purchases across countries (left panel) as well as the respective cumulative amounts (right panel). Maturing amounts are excluded.



are not spread out evenly over time. The largest purchases occurred after the introduction of the SMP on 10 May 2010 and after its reactivation on 8 August 2011. The chart also suggests that there are long periods during which the SMP is open but inactive. From the week ending in 25 March 2011 until 8 August 2011 the SMP is inactive for 19 weeks. This is in stark contrast to the regular auctions undertaken, for example, by the Federal Reserve during the LSAP.

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 U.S. VIX volatility index, and daily changes in the yield spread between BBB and AAA rated corporate bonds in the euro area. These two covariates serve as a proxy for global risk aversion. The VIX volatility index may also affect global liquidity flows, and financial intermediaries' and market makers' value at risk constraints.

3.2 Identification

We identify the yield impact of SMP purchases through robust time series regression of country-specific yield changes on SMP purchases. We assume that purchase amounts are *predetermined* as well as only observed against the backdrop of a severe sovereign debt crisis.

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

While purchases were fixed before markets open, purchases overall are still determined against the backdrop of an extremely severe sovereign debt crisis. As a result, purchases are only observed during times of intense crisis, of which high, rising, and volatile yields are symptoms. Since the program *explicitly targeted* debt markets that were perceived as dysfunctional, the program effectively entails buying debt securities for which there is little private sector demand at that time, during phases of intense debt crisis, in the market segments that are the most affected by it. Altogether, this means that purchases are not strictly exogenous but predetermined, and that control covariates will be important to disentangle the direct (negative) effect of purchases on yields from the (positive) correlation between yield changes and purchases that are due to common factor effects.

Some candidate control covariates are easily observed and readily available. Such covariates help explain yield changes both across countries and over time. However, we argue below that observed common factors are not sufficient as controls. We find that much systematic co-movement across euro area sovereign yields during the crisis remains unaccounted for after conditioning on relevant observed controls. We therefore allow for unobserved common correlated effects that can capture the leftover common movements. These factors can also be seen as providing insurance against dynamic and cross-sectional model mis-specification. (If omitted effects were not important, then we would estimate zero loading coefficients that pre-multiply the respective factors). In our panel time series regression framework, common and idiosyncratic factors constitute powerful control covariates. We verify below that a pronounced factor structure underlies bond yield changes in the euro area, see also Pan and Singleton (2008), Ang and Longstaff (2011) and Longstaff, Pan, Pedersen, and Singleton (2011).

3.3 Panel time series regression

We consider the panel time series regression model

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

where y_{it} is the observed change in yield of a benchmark bond of country $i = 1, \ldots, N$ at a daily frequency $t = 1, \ldots, T$. We consider first differences since the yield data is highly non-stationary, see Figure 1. Inference in principal-components type factor models, however, commonly relies on covariance stationary data processes, see Stock and Watson (2002a, 2002b). Yields-to-maturity refer to five year benchmark bonds. The five year maturity is approximately in the middle of the two year to ten year maturity spectrum that is targeted by the SMP. We assume that the purchase of any bond in that two year to ten year maturity bracket also affects the yields of other bonds issued by the respective government in that maturity bracket as well, as these bonds are close substitutes. We take the impact at the five year mid-point of the term structure as indicative of a shift in the overall yield curve in that respective country.

We consider four different specifications of the intercept term $\bar{c}_{it}$. This is potentially important, as the yield impact is a shift in the conditional mean of (1). The intercept is estimated as either (i) constant over the entire estimation sample, (ii) piecewise constant over certain subsamples, or (iii) based on a 65-day rolling window specification (RW). The intercept terms can be estimated as averages over non-intervention yields in a first step, or estimated simultaneously with the other parameters.

Purchase amounts $z_{it} \geq 0$ are in terms of nominal value. This facilitates the comparison to the existing LSAP and QE literature, see for example D'Amico and King (2013). Observed control covariates W_t and unobserved factors f_t load on yields with coefficient vectors β_i' and λ_i', respectively. The remaining time series variation is captured by the idiosyncratic (residual) factor g_{it}.

The coefficients δ_{it} are our main parameters of interest and measure the impact (in bps) of a unit increase in purchases. We distinguish between announcement effects and direct effects from outright purchases,

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

where $\bar{\delta}_i$ are country-specific effects corresponding to purchases z_{it}. Coefficients $\bar{\delta}_{10\text{May}2010}$ and $\bar{\delta}_{8\text{Aug}2011}$ are time-specific fixed effects that correspond to the initial announcement day and reactivation of the SMP of 10 May 2010 and 8 August 2011, respectively.

The additive specification (2) decomposes the yield movement on the two announcement days into the direct effect due to purchases $\bar{\delta}_i z_{i\tau}$, and an additional effect due to the announcement $\bar{\delta}_\tau z_{i\tau}$ on 10 May 2010 and 8 August 2011. Disentangling the two effects in such a way takes into account that substantial purchases occurred on these two days. The overall yield movement is therefore not only due to the policy announcements. We take the announcement effect as proportional to the amount of purchases undertaken on the announcement day. This specification captures that purchases are undertaken only for a subset of countries at each time, and parsimoniously summarizes the additional effects in a single coefficient.

Observed covariates are given by

$$W_t = (\Delta \text{ U.S. VIX}_t, \quad \Delta \text{ E.A. risk aversion spread}_t)', \tag{3}$$

where Δ denotes first differences. Factors W_t are common to all data, and are standardized to have zero mean and unit variance. Unobserved dynamic effects f_t and g_t capture the remaining systematic and idiosyncratic variation in the panel, respectively, and evolve according to a first order vector autoregression as

$$f_{t+1} = \Phi^f f_t + w_t, \quad w_t \sim \mathbf{t}(0, H_w, \nu), \tag{4}$$

$$g_{t+1} = \Phi^g g_t + \xi_t, \quad \xi_t \sim \mathbf{t}(0, H_\xi, \nu), \tag{5}$$

where w_t and ξ_t are vectors of t-distributed innovation terms with low degrees of freedom ν. Autoregressive matrices Φ^f and Φ^g are diagonal for both common factors f_t as well as the residual factors g_{it}.

We impose a scaling restriction to identify the factor loading coefficients, see for example Stock and Watson (2002a) and Creal et al. (2013). Covariance matrices are $H_w = I - \Phi^f \Phi^{f\prime}$ and $H_\xi = I - \Phi^g \Phi^{g\prime}$, which implies that $\mathrm{Var}[f_t] = I$ and $\mathrm{Var}[g_t] = I$, where I is the appropriate identity matrix. Latent factors are unconditionally orthogonal (orthonormal) as a result. The scaling restriction identifies the elements of loading coefficients λ_i and γ_i as standard deviation (volatility) parameters. The sign of the latent factors is identified by restricting appropriate factor loadings to be positive. Latent factors are initialized at their stationary distribution. Time series plots and data summary statistics indicate fat tails for our data sample of euro area yield changes, see Figure 1 and the discussion of volatility and extreme market movements in Section 5. We capture fat tails of y_{it} through explanatory covariates as well as t-distributed error terms. We treat the degrees of freedom parameter ν as a common robustness parameter to be estimated from the data. Allowing for t-distributed errors means that parameter estimation and statistical inference is less sensitive to a small number of large yield changes (outliers).

Parameter and risk factor estimation is based on maximum likelihood and fairly standard, see for example Meesters and Koopman (2012) and Koopman, Lucas, and Schwaab (2012). Estimation details are provided in the Appendix.

4 Major empirical findings

4.1 Yield impact

This section discusses our main empirical findings. Table 1 reports estimates of yield-impact per bond purchases (notional value) for five SMP countries. We consider the panel time

Table 1: Estimation results: yield impact in bps per 100 mn

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

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

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

series regression 1 of yield changes y_{it} on a constant c_{it}, purchase amounts $z_{it} \geq 0$, two observed covariates W_t, common factors f_t, and possibly autocorrelated residual terms g_{it}, for $i = 1, \ldots, N$.

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 $\bar{c}_{it}$. The intercept is either constant over the entire estimation sample from 1 October 2008 to 20 December 2011 (model m1); piece-wise constant over three periods: 1 October 2008 to 9 May 2010 (pre-SMP), 10 May 2010 to 7 August 2011 (initial purchases), and 8 August 2011 to 20 December 2011 (purchases after re-announcement and until the allotment of the first three year LTRO) (model m2); a time varying intercept based on a 65-day rolling window averages over non-intervention days (model m3), or estimated along with the other parameters by numerically maximizing the log-likelihood function (model m4). We find that over time, on average €1 bn of bond purchases lowered yields from approximately -1 to -2 basis points (Italy) and up to more than -20 basis points (Greece). The remaining impacts take intermediate values, from approximately -3 bps/bn (Ireland), -4 to -6 bps/bn (Spain), and -6 to -9 bps/bn (Portugal). These ranges are point estimates across different model specifications. Impact coefficients are statistically significant according to their t-values for most, but not all SMP countries. Statistical power may be low also due to relatively few intervention days in the estimation sample. We therefore suggest focusing on economic rather than statistical significance when discussing the yield impact. The log-likelihood is highest when intercepts are estimated along with the other model parameters.

In addition, Table 1 reports substantial announcement effects for the initial announcement on 10 May 2010 and the reactivation of the program on 8 August 2011. The impact coefficients increase in absolute value by an additional 86 bps per €1 bn on 10 May 2010 and 7 bps per €1 bn on 8 August 2011. Both announcement effects are statistically significant and economically large. The bottom panel of Table 1 reports the estimation results from four alternative specifications. These alternatives explore the robustness of our estimates to changes in model specification.

Models m5 and m6 both allow for volatility clustering in observed yield data. Roughly speaking, accounting for volatility clustering means that one learns more about the intervention impact from days that are calmer; such days receive a relatively larger weight in likelihood estimation. In Models m5 and m6, variance matrices H_w and H_ξ are thus time varying. The intercept $\bar{c}_{it}$ is constant in Model m5 (as in m1) and piecewise constant in m6 (as in m2). Time-varying volatilities are estimated based on an exponentially weighted moving average specification for squared observations, see for example Engle (2002); factor variances are then taken as proportional to these volatility estimates. The bottom panel in Table 1 suggests that allowing for time-varying volatility in the latent factor innovation terms increases the data likelihood but also leaves the impact coefficients approximately unchanged. We prefer our baseline specifications (m1-m4) because these are simpler and produce equivalent results.

We further explore robustness of our empirical results by replacing W_t (correlated with z_{it}) with W_{t-1} (predetermined). Since W_t and f_t are control covariates and are not used as instrumental variables in a counterfactual experiment, contemporaneous correlation of W_t and f_t with purchases z_{it} is not a problem for identification (instead, it is what makes these factors useful as control covariates; the main identification assumption in our regression setup is that z_{it} and y_{it} are not simultaneously determined, see Section 3.2). Table 1, for Model m7, clarifies that the replacement has almost no effect on the estimated yield impact coefficients. The latent effects adjust to reflect the diminished role of the observed control covariates. The respective loading coefficients on W_{t-1}, however, are less intuitive. This is another reason why we prefer our benchmark models (models m1-m4).

Finally, Model m8 investigates the robustness of our yield estimates with respect to the number of latent components in the model. The estimates for yield impact are robust for most countries, with the exception of Portugal. In the latter case, the point estimates range more widely from -2 bps per €1 bn to up to -15 bps per €1 bn across model specifications. We conjecture that a few extreme market moves (see Figures 1 and 3) and a relatively large role for the idiosyncratic component in Portugal contribute to the variability of this particular parameter estimate.

Table 2: Impact and debt market size

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

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

4.2 Comparing yield impact and debt market size

This section relates the impact estimates from Table 1 to the size of the respective debt markets. A purchase of any given amount constitutes a significantly smaller purchase in terms of the share of the overall outstanding debt in a large and liquid market (e.g., Italy) compared to a smaller market (e.g., Ireland).

The impact estimates reported in Table 1 are larger than what is commonly found in the LSAP and QE literature. The debt markets under consideration in this study are typically smaller and less liquid than the market for U.S. Treasuries or U.K. gilts. In particular, this was the case during the sovereign debt crisis, when private demand for stressed government debt is low and required default risk premia are substantial. As a result, impact estimates from the LSAP and QE literature seem to serve as a lower bound for the magnitude of yield impact estimates for European debt purchases.

Table 2 suggests an average yield impact of approximately -3 bps for an intervention of the size equivalent to 1/1000 of a country's outstanding debt stock (measured at the end of 2010). Deviations are considerable for Greece (approximately 7 bps per 0.1 percent of the debt market) and Ireland (where the overall debt stock is relatively small). The wide range of cross-sectional variation, after taking into account overall debt market size, suggests that other channels contribute to determine the yield impact.

Notably, the ordering of the re-scaled yield impact estimates in Table 2 is positively re-

Table 3: Yield impact from a dynamic specification

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

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

lated to average yields over our sample period. We refer to Figure 1 which documents a relative stable ordering of relative yield and implied risk. This comparison suggests that purchases send a signal regarding the default risk premium. First, purchases may be interpreted to mean that the Eurosystem regards country yields as higher than justified based on country fundamentals. This may also be due to high liquidity risk and contagion in a monetary union. Second, purchases may also be understood as a signal that the Eurosystem is willing to consider and implement non-conventional policies to combat the crisis. Finally, as no specific duration was announced, the flow of purchases always provides the information that the program is still active.

4.3 Dynamic effects

This section extends our baseline model to allow for the possibility of lagged effects on yields from contemporaneous purchases. This extension is possible since SMP interventions can be considered as repeated and unannounced interventions in the same markets. To our knowledge, the persistence of dynamic effects of bond purchases is not considered in the literature, with the exception of Wright (2012). Among other reasons, this is because persistence is extremely hard to assess based on event study methodologies centred around program announcement dates.

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

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

$$y_{it} = \bar{c}_{it} + \delta_{it} z_{it} + \omega_i \sum_{k=1}^{K} \left[(\kappa)^{k-1} z_{i,t-k} \right] + \beta_i' W_t + \lambda_i' f_t + \gamma_i g_{it}, \tag{6}$$

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

Table 3 reports the impact estimates from the dynamic specification (6), along with the estimated long-run effects. A comparison suggests that the long run impacts are approximately three quarters of the immediate impact in most countries. Positive estimates of ω_i suggest that bond yields tend to 'bounce back' to some extent on the day following an intervention. Hence, some of the initial impact appears to be temporary, and may be related to dealer inventory market microstructure effects. However, in some, although not all, countries the contemporaneous yield impact coefficients $\bar{\delta}_i$ are higher in (6) than in our baseline model (1) in those countries for which ω_i is positive. Overall, the long-run effects from the dynamic model are fairly similar in magnitude to the contemporaneous impact effect from our baseline specification without lagged effects. The small increase in likelihood (of approximately three points) suggests only a marginal improvement in explanatory power from moving to the dynamic specification.

4.4 Additional discussion

This section provides additional discussion of our main empirical results. First, purchases could in principle lead to rising yields if the amount of purchases falls short of market expectations, as argued by Cahill et al (2013). Indeed, such reasoning may contribute in part towards explaining the rapid rise in yields of SMP countries in the last two quarters of 2010 and first two quarters of 2011, see the top panel in Figure 1. Cahill et al. (2013) distinguish between expectation and surprise components in the market reaction to several program announcements based on a confidential Fed survey which allows them to disentangle the two effects. We cannot control for market expectations in such a way. We recall, however, that the Eurosystem did neither disclose the total amounts that would be spent within the SMP, nor a time frame over which the program would be active, nor a set of securities that would be targeted. This suggests that our yield impact estimates for outright purchases, after controlling for announcement effects, contain a substantial surprise component. This, in turn, is consistent with the notion that new information is being revealed to market participants with each purchase.

Second, we have implicitly ruled out *systematic* cross-country effects from purchases in our empirical setup. If purchases in one country had a significant systematic effect on all other yields in our panel data, i.e., other SMP countries as well as non-stressed countries, then the conditional (on factors f_t) mean in panel regression (1) could be lower on intervention days due to an effect that works through inference on common factors f_t and factor loadings that are of the same sign. This would introduce a bias towards zero of the negative yield impact estimates: purchases would be at least as effective as indicated by the time series regression estimates. Two simple observations suggest that cross-country effects from purchases are small, however. First, government bond markets were considerably fragmented during the sovereign debt crisis, see ECB (2012a) and references therein. This means that government bonds from stressed and non-stressed countries were not close substitutes during that time. This, in turn, suggests that local supply effects as well as signaling effects are mainly country-specific. Second, Figure 1 reveals that the purchase announcements from 10 May 2010 and 8 August 2011 moved yields substantially in stressed countries, but left yields in core countries approximately unchanged. This again points towards significant market fragmentation, and suggests that government bonds from stressed and non-stressed countries are not close substitutes during the debt crisis.

4.5 Further robustness checks

This section further explores the sensitivity of our empirical estimates to alternative regression specifications. 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 of the yield impact estimates. Extending the sample to data until 28 February 2012 increases the magnitude of the impact coefficients minimally. This is likely due to additional effects on sovereign yields that are due to two 36 month longer-term refinancing operations conducted at that time.

To investigate whether the effectiveness of the program decreased over time we include an extra dummy effect for the initial purchases from 11 May 2010 to 10 June 2010. The respective coefficient is not significant. As a result, we find no evidence that the purchases in the first month after the announcement of the program were relatively more effective after controlling for the announcement effect from 10 May 2010 and common factor effects.

Finally, our dynamic specification in Section 4.3 is somewhat restrictive in terms of decay dynamics due to data characteristics. We investigate the robustness of the long run effects by fixing different decay parameters a-priori to alternative values such as $\kappa = 0.2$ and $\kappa = 0.6$, and then estimating only the remaining model parameters. In this case the impact coefficient estimates adjust, and the reported long-run effects remain similar.

5 Market volatility and extreme market movements

This section considers bond yield volatility and the probability of extreme market movements during intervention days versus non-intervention days. We find that bond yield volatility is lower on intervention days for most SMP countries, due to less extreme (tail) movements occurring when the Eurosystem is active in the market. This matters, since highly volatile bond yields alone may force institutional investors and capital constrained market makers to leave a given market due to value-at-risk constraints, see for example Vayanos and Vila (2009) and Adrian and Shin (2010). Indeed, dealers occasionally ceased to provide quotes for government bond transactions during particularly volatile periods during the debt crisis, see Pelizzon, Subrahmanyam, Tomio, and Uno (2013). For interesting volatility-based identification of LSAP effects in a vector autoregressive setup, we refer to Wright (2012).

Table 4 reports simple summary statistics of yield changes in SMP countries. We distinguish a pre-debt crisis (1 Oct 2008 to 31 Mar 2010) and debt crisis sample (1 Apr 2010 to 20 Dec 2011), and further distinguish intervention days from non-intervention days. Intervention days are country-specific, i.e., days on which purchases took place in the debt market of a particular country.

Pre-debt crisis yield changes exhibit a lower mean and lower volatility than on non-intervention days during the crisis in all five countries. This is intuitive, as increasing and volatile yields are the symptom of a debt crisis that is worsening over time. The sample skewness and kurtosis measures confirm that almost all yield change distributions are characterized by occasional extreme market movements (‘fat tails’). Excluding the announcement days, the mean yield change on intervention days are larger than the mean yield change for pre-crisis non-intervention days in all five countries. This is again intuitive because the interventions happen as the debt crisis escalates.

Importantly, Table 4 suggests a clear effect of purchases on bond yield volatility and the

Table 4: Summary statistics for yield changes

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

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

Figure 3: Kernel density estimates of SMP country yield changes

Non-parametric density estimates of yield changes in five year government bonds (based on a Gaussian kernel). The densities distinguish between yield changes on intervention days from yield changes on non-intervention days during the debt crisis (1 Apr 2010 to 20 Dec 2011). The density estimates refer to Spain (ES), Greece (GR), Ireland (IE), Italy (IT), and Portugal (PT). The vertical axes are re-scaled so that the tails are visible.

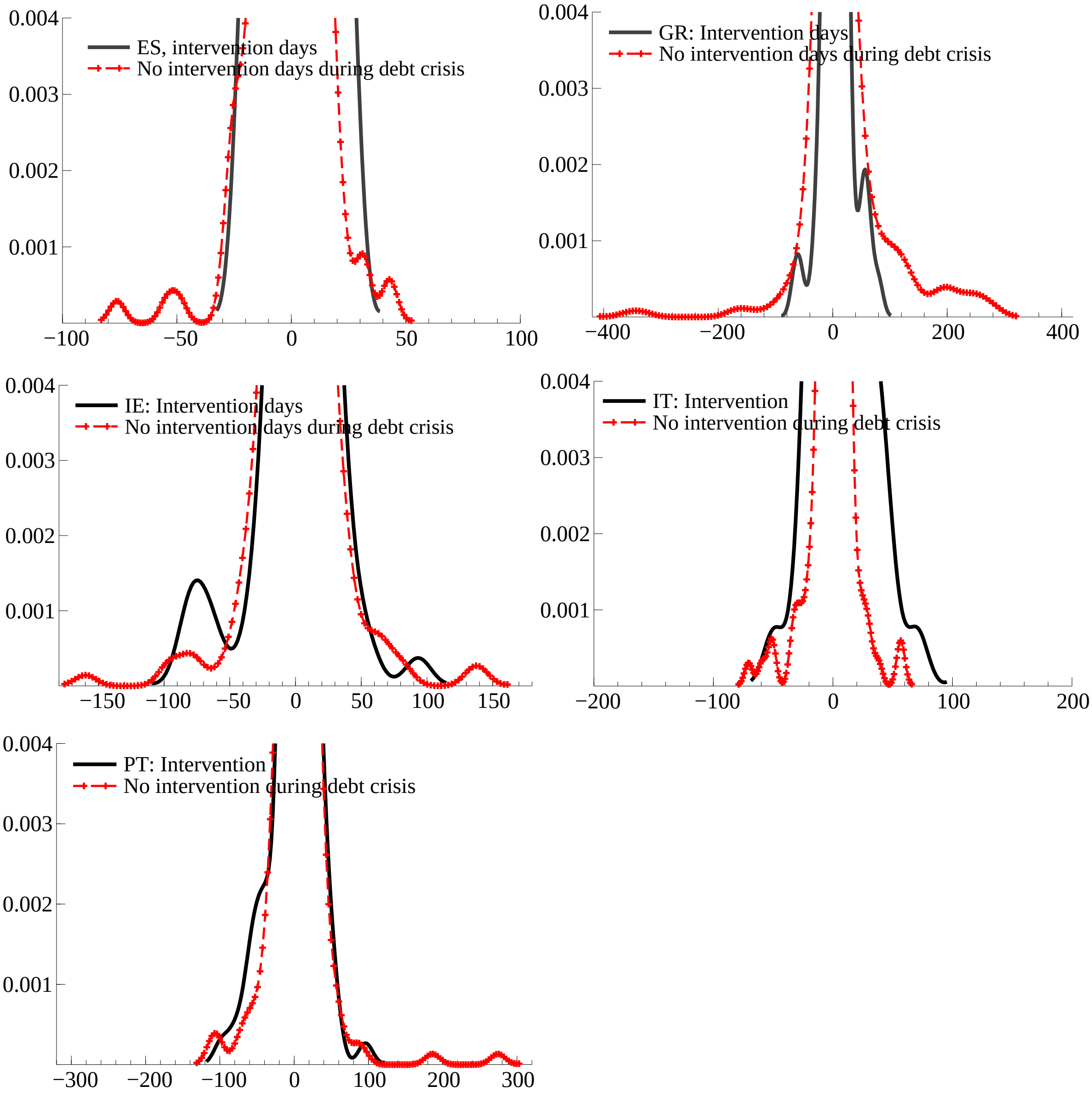


Table 5: Estimates of the Hill tail index

The estimates of the tail index are obtained following the approach of Huisman, Koedijk, Kool, and Palm (2001). 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 $\gamma(k) = \beta_0 + \beta_1\kappa + \epsilon(\kappa)$. Given the small sample size for intervention days in particular, 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 as β_0^{-1}. Non-intervention days are decomposed into a pre-debt crisis (1 Oct 2008 to 31 Mar 2010) and debt crisis sample (1 Apr 2010 to 20 Dec 2011). The smaller the tail index the more probable are extreme market movements.

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

probability of extreme market movements. 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 indicates that there is a reduced (tail) risk of extreme movements on intervention days.

Table 5 presents estimates of the tail index for intervention days and non-intervention days. We again distinguish pre-crisis and crisis times (before and after 1 April 2010). We refer to Hill (1975) and Huisman, Koedijk, Kool, and Palm (2001) for the methodology. We use the latter approach to mitigate small sample bias for the intervention day sample. Yield changes on intervention days tend to display thinner tails and therefore also higher tail index estimates. As a result, there are fewer extreme market movements on intervention days, compared to non-intervention days during the crisis. Figure 3 graphs density (kernel) estimates that pertain to yield changes of five year benchmark bonds. The plots distinguish yield changes that occurred on intervention days from yield changes that occurred on non-intervention days during the debt crisis. Overall, the non-parametric density plots confirm the impression from Tables 4 and 5 that the SMP prevented or substantially limited extremely adverse yield movements. The visual evidence is strongest for Greece, Ireland, and Portugal, and less strong for Spain and Italy. The difference may reflect a difference in terms of timing of the purchases during our estimation sample from 2010-11.

6 Conclusion

This paper contributes to the literature on the effectiveness of central bank asset purchase programs by considering the bond market interventions within the ECB's Securities Markets Programme during 2010-2011. We assess the yield impact of asset purchases in five euro area sovereign bond markets: Greece, Spain, Ireland, Italy, and Portugal. We identify yield impact based on time series panel data regression on purchases and control covariates. In addition to a large and economically significant announcement effects, we find that the ECB's repeated interventions had an impact of approximately -1 to -2 basis points (Italy) and up to -17 to -21 basis points (Greece) at a five-year maturity per €1 bn of purchases across euro area countries. The yield impact depends on size and market conditions, and a default risk signal. Bond yield volatility is lower on intervention days for most countries, due to less extreme (tail) movements occurring when the Eurosystem is active in the market. Finally, a dynamic specification points to both transitory dynamics as well as longer-lived effects from purchases.

Appendix: parameter estimation and signal extraction

The introduction of latent factors into a model for non-Gaussian panel data implies that parameter and factor estimation is slightly nonstandard. We use the approach outlined in Shephard (1994) and Durbin and Koopman (2001, Ch. 10), which expresses Student-t distributed factor innovations in terms of innovations that are normally distributed, conditional on Chi-squared scaling variables. This is convenient, since the likelihood is easily obtained from the Kalman Filter when innovation terms are Gaussian. If a factor innovation $\eta_{r,t} \sim t_\nu\left(0, \sigma^2_{\eta rt}\right)$, where $r = 1, \ldots, R$, then it has a mixture representation

$$\eta_{r,t} = (\nu - 2)^{\frac{1}{2}} \sigma_{\eta rt} c_{rt}^{-\frac{1}{2}} \eta^*_{r,t},$$

where $\eta^*_{r,t} \sim \mathrm{N}(0,1)$, $c_{rt} \sim$ i.i.d.$\chi^2(\nu)$, with degrees of freedom $\nu > 2$. As a result,

$$\eta_{r,t}|c_{rt} = \mathrm{N}\left(0, (\nu - 2)\sigma^2_{\eta rt} c^{-1}_{rt}\right).$$

To obtain the data likelihood for our model with Student-t distributed innovation terms, we integrate out the unobserved scaling variables $c = (c_{11}, \ldots, c_{RT})'$ from their joint density with the observations at each evaluation of the likelihood. The likelihood $p(y) = \int p(y|c)p(c)\ dc$ can be estimated as

$$\hat{p}(y) = S^{-1} \sum_{i=1}^{S} p\left(y|c^{(i)}\right),$$

where S are the number of simulations, and $c_{rt} \sim$ i.i.d.$\chi^2(\nu)$, $\nu > 2$ as before. $p\left(y|c^{(i)}\right)$ is a Gaussian likelihood and obtained from the Kalman Filter. A central limit theorem guarantees that the parameter estimates obtained this way are consistent and asymptotically normal, see, for example, Durbin and Koopman (2001).

The location of the factors f_t can be estimated as

$$\hat{f}_t = \hat{\mathrm{E}}[f_t|y] = \frac{\sum_{i=1}^{S} f^{(i)} p(y|c^{(i)})}{\sum_{i=1}^{S} p(y|c^{(i)})}, \tag{A.1}$$

if required. All model parameters (such as the yield impact of purchases $\bar{\delta}_i$) and unobserved factors f_t and $g_t = (g_{1t}, \ldots, g_{Nt})'$ are estimated simultaneously. The common factor estimates $\hat{f}_t$ are different from principal component estimates, as they take the parametric model structure and other right-hand-side variables (such as W_t and z_{it}) into account.

We refer to Durbin and Koopman (2001) for a textbook treatment, and to Koopman, Lucas, and Schwaab (2011, 2012) and Meesters and Koopman (2012) for related non-Gaussian panel data frameworks.

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