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Essays on Sovereign Risk and Banking

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Abstract

This thesis consists of three essays on sovereign risk and banking.

In the first essay, we examine the determinants of sovereign risk in the Eurozone focusing on the recent crisis episode and search for a self-fulfilling contagion link by using an exogenous ECB policy announcement for identification. Our principal components analysis reveals that the perceived commonality in default risk among peripheral and core Eurozone countries increased after the announcement. An event study detects significant pre-announcement news transmission from Spain to Italy, Belgium, France and Austria that clearly dissipates post-announcement. Country-specific regressions of CDS spreads on systematic risk factors illustrate frequent days of large adverse shocks affecting simultaneously those same Eurozone countries during the pre-announcement period; but not afterwards. Altogether these findings support the view that market expectations during Eurozone crisis were at least partially self-fulfilling and ECB policy helped to contain such adverse dynamics.

In the second essay, we focus on European banks' sovereign bond exposures. By using a novel bank-level dataset covering the entire timeline of the Eurozone crisis, we first re-confirm that the crisis led to the reallocation of sovereign debt from foreign to domestic banks. This reallocation was only visible for banks as opposed to other domestic private agents and it cannot be explained by the banks' risk-shifting tendency. In contrast to the recent literature focusing only on sovereign debt, we show that banks' private sector exposures were (at least) equally affected by a rise in home bias. Finally, we propose a new debt reallocation channel based on informational frictions and show that crisis-country debt was not only reallocated to domestic banks, but also to the informationally closer foreign banks. Our results imply that informational asymmetries among banks played a key role in the recent fragmentation across Eurozone debt markets.

In the third essay, our investigation shifts towards political economy aspects of the relationship between sovereigns and domestic banks. We use data on the universe of credit extended over a 14-year period in Turkey to document a strong political lending cycle. We find that state-owned banks systematically adjust their provincial lending around local elections relative to the private banks in the same province. There is considerable tactical redistribution: state-owned banks increase loans in politically competitive provinces with a current mayor aligned with the ruling party but reduce it in similar provinces with a current mayor from opposition. This effect only exists in corporate lending as opposed to consumer loans, suggesting that tactical redistribution targets job creation to increase electoral success. Such political lending also seems to influence real outcomes as the credit-constrained opposition areas suffer a drop in economic output as measured by local construction activity.

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"Hayat kısa,
Kuşlar uçuyor."

Cemal Süreya

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Chapter 1

Introduction

In this PhD thesis, I investigate three topics at the intersection of sovereign risk and banking.

The first one (*Chapter 2*) is a joint work with my PhD supervisors (Ana-Maria Fuertes and Elena Kalotychou) and focuses on the contagion links observed among European governments' CDS (and bond) spreads during Eurozone crises. While most of the literature attributes such contagion or transmission of shocks to fundamental channels that operate across countries (such as banking, trade or investment linkages), there is also a recent stream of studies that interpret the clustering of large common shocks as a sign of market panic or self-fulfilling dynamics that do not have much to do with individual countries' macroeconomic fundamentals. However, in the absence of a comparable control group of countries that do not have any fundamental linkages among each other, it is difficult to empirically differentiate between these two different channels of contagion.

We attempt to overcome this obstacle by using an exogenous policy change of the European Central Bank (ECB) in which it expressed its willingness to start acting as a lender of last resort (LOLR) in government debt markets for illiquid but solvent Eurozone states. There are good reasons in the theoretical literature to assume that countries of a common currency would be especially prone to self-fulfilling market dynamics as their central banks would lose the ability to print money and could not step into the market in times of high illiquidity. In such an environment, negative market expectations might validate themselves by increasing the interest rates on government bonds, making it costlier to roll-over maturing debt and eventually incentivising the government to default rather than refinance itself at those high rates. The key to our identification is the theoretical assertion that if such dynamics were at play, then ECB's LOLR position should help to contain them. However, such a policy change would not have an effect if the cross-country transmission of shocks were mainly due to fundamental channels. Therefore, this policy experiment gives us a clear empirical strategy in which we focus on two short (one-year) time periods around the announcement date to minimize the effect of any changes other than the policy itself.

Our findings are: (*i*) principal components analysis reveals that the perceived commonality in default risk among peripheral and core Eurozone countries increased after the announcement. In the meantime, the link between country fundamentals and spreads strengthened implying that there might be non-fundamental factors at play prior to the announcement. (*ii*) An event study detects significant pre-announcement news transmission from Spain to Italy, Belgium, France and Austria that clearly dissipates post-announcement. This is consistent with the view that news in one country could act as a trigger (sunspot) for self-fulfilling market movements against other countries. (*iii*) Country-specific regressions of CDS spreads on systematic risk factors illustrate frequent days of large adverse shocks affecting simultaneously those same Eurozone countries during the pre-announcement period; but not afterwards. Altogether these findings support the view that market expectations during Eurozone crisis were at least partially self-fulfilling and ECB policy helped to contain such adverse dynamics.

In *Chapter 3*, I focus on European banks' sovereign bond exposures and investigate the main drivers of their evolution throughout the Eurozone crisis from early 2010 to mid-2015. Recent literature points to an interesting observation: European banks started increasing the amount of domestic government debt in their balance sheets as their governments were struck by the sovereign debt crisis. Though it may come across as puzzling at first, existing literature already provides various explanations. For instance, it could be the case that these banks were (morally) pressured by their governments who were in need of urgent liquidity to roll-over their debt payments. Moreover, most of those banks located in crisis countries were undercapitalised, which may have led them to bet on their own government bonds to shift the risk onto their creditors in case of default. Also, it is possible that these banks may have anticipated that their governments would be less willing to default on domestic creditors and thus absorbed the bond sales of foreign banks in the secondary markets.

By using a novel bank-level dataset compiled from various stress-tests, transparency and capital exercises of the European Banking Authority (EBA), I reach several interesting results: (*i*) I first re-confirm that the crisis led to the reallocation of sovereign debt from foreign to domestic banks. (*ii*) However, this reallocation was only visible for banks as opposed to other domestic private agents, which does not seem to be consistent with the secondary market or exchange rate channel of the rising home bias. (*iii*) I find weak evidence for risk-shifting tendency of the troubled country banks; nonetheless this does not come close to explaining the full extent of the preference for local government bonds. (*iv*) In contrast to the recent literature focusing only on sovereign debt, I also show that banks' private sector exposures were (at least) equally affected by a rise in home bias, which implies that the specific channel of moral suasion on sovereign debt has limited explanatory power in sample.

Given the insufficiency of the existing explanations, I propose a new debt reallocation channel based on informational frictions and (*v*) show that crisis-country debt was not only reallocated to domestic banks, but also to the informationally closer foreign banks. I further confirm that this effect is independent of the previous channels proposed in the literature, robust to various sample recompositions and exists more generally rather than being specific to the periods of extreme sovereign stress. Hence, these results imply that informational asymmetries among banks played a key role in the recent fragmentation across Eurozone debt markets.

In *Chapter 4*, which is a joint work with Çağatay Bircan, we take a political economy approach to study the relationship between sovereigns and banking sector. Specifically, we explore whether ruling parties in central government use bank lending in order to advance their own private agendas. We use data on the universe of bank loans extended over a 14-year period in Turkey to confirm that this is indeed the case. Differently from previous literature, we exploit a newly-available quarterly database to pin down the exact timing of electoral credit misallocation. Same database also gives us the chance to test whether governments would target corporate or consumer lending for re-election purposes. Finally, in order to check the effect of credit on local economic outcomes, we benefit from a novel database on private construction activity at the province-level.

For identification, we exploit three layers of differences. First, state-owned banks would be more susceptible to government pressure and thus we compare them with private banks. Second, political pressure on banks would be stronger prior to elections and thus we exploit the time dimension in our data and compare near-election periods with non-election periods. Third, government's targeting of bank loans would rather focus on politically attractive regions where the marginal benefit of an additional vote would be higher, which gives us a cross-sectional dimension to compare provinces with high levels of electoral competition with the ones where elections are not heavily contested.

Our findings are two-fold: (*i*) We find that state-owned banks systematically adjust their provincial lending around local elections relative to the private banks in the same province. There is considerable tactical redistribution: state-owned banks increase loans in politically competitive provinces with a current mayor aligned with the ruling party but reduce it in similar provinces with a current mayor from opposition. Besides, rolling estimations in non-election years show some evidence that central government may have resorted to patronage when it did not have election concerns. (*ii*) Political cycle only exists in corporate lending as opposed to consumer loans, suggesting that tactical redistribution targets job creation to increase electoral success. In line with this conjecture, real local outcomes seem to be influenced by the political cycle as the credit-constrained opposition areas suffer a drop in economic output measured by local construction activity.

Chapter 2

ECB Policy and Eurozone Fragility: Was De Grauwe Right?

"You have large parts of the euro area in what we call a 'bad equilibrium', namely an equilibrium where you may have self-fulfilling expectations that feed upon themselves and generate very adverse scenarios."

(ECB President Mario Draghi; 6th September 2012)

2.1 Introduction

Since 2009, when the debt problems of Greece came to light, the suddenness and magnitude of changes in Eurozone bond yield spreads have sparked a debate among economists regarding the likely causes. The fundamentalist viewpoint is that the surge in Eurozone yield spreads is purely a reflection of deteriorating macroeconomic fundamentals. The multiple-equilibria view contends that markets may not always function optimally and thus, without any major change in fiscal fundamentals, the decisions of panic-driven investors may lead a country to a self-fulfilling liquidity crisis that otherwise would not have occurred.

The idea that sovereign members of a currency union are more vulnerable to investor sentiment is encapsulated in the "Eurozone fragility hypothesis" (De Grauwe, 2012; 2013). The hypothesis states that, by issuing debt in a currency that they cannot control, member states are susceptible to a self-fulfilling dynamics fuelled by market sentiment. Panic and fear about a sovereign's defaulting on its debt obligations is likely to trigger sudden stops in capital inflows and hence, higher interest rates. The latter make it harder for the sovereign to roll over its short-term debt, creating a perverse feedback effect between market sentiment and interest rates that could trigger a liquidity crisis and ultimately, the feared default.

This self-fulfilling dynamics is unlikely to happen in debt markets of sovereigns that retain control of their currencies because investors recognize the presence of a central bank that, acting as lender-of-last-resort (LOLR), will inject the necessary liquidity in crisis. The fragility hypothesis thus sums the idea that such a self-fulfilling dynamics would not be present in Eurozone debt markets if the European Central Bank (ECB) takes the LOLR role. This paper provides empirical tests of the Eurozone fragility hypothesis from different angles.

Mario Draghi announced on July 26, 2012 that the ECB was prepared within its mandate to do "whatever it takes" to preserve the euro. Draghi's announcement gained more meaning a few days later on August 2, 2012 when the ECB Governing council declared its intention to undertake outright open market operations in secondary government bond markets.[1] On September 6, 2012, the Outright Monetary Transactions (OMT) program was formally launched, under which the ECB would act as LOLR for countries backed by the European Stability Mechanism (ESM). Through this program, the ECB can make purchases or outright transactions in the secondary sovereign bond markets of member countries to ease liquidity pressures. Conditionality (strict fiscal supervision) is attached to avoid moral hazard and bond purchases are fully sterilized to prevent inflationary pressures. Furthermore, the OMT program is only activated for a country if, by unanimity among ESM members, its debt is deemed sustainable (Wolff, 2014).

[1] See http://www.ecb.europa.eu/press/pressconf/2012/html/is120802.en.html

The main goal of this paper is to investigate whether the important change in the ECB's policy stance (signalled by the OMT program) has effectively curbed the self-fulfilling dynamics in Eurozone debt markets. Our line of argument builds upon the fact that the OMT is neither a form of fiscal distribution among Eurozone members nor a bailout plan. Even if markets had initially misinterpreted the speech as a 'promise' for solvency support, the conditionality attached as part of the requirements of the OMT program rules this out. We examine the sovereign credit default swaps (CDS) of 14 countries in Europe as representative of the credit risk of 'periphery' versus 'core' Eurozone countries, as well as European countries that have not adopted the euro. This can shed light on the way markets have discriminated in terms of sovereign risk pricing between these three groups of countries. We provide convincing evidence that the Eurozone debt markets began to anticipate the LOLR role of the central bank (that materialized in the OMT program) following Draghi's "whatever-it-takes" pledge on July 26, 2012; hence, we focus on this implicit OMT announcement date for most of our analysis and conduct sensitivity analysis later on.

An eclectic methodology is deployed to test De Grauwe's Eurozone fragility hypothesis. A principal component analysis of daily Eurozone CDS spreads reveals a 'structural break' in Eurozone sovereign risk perceptions on the (implicit OMT) announcement date. The first and second principal components suggest that the announcement increases the commonality in sovereign risks of periphery and core Eurozone countries, and marks a change in the way markets discriminate among Eurozone members towards a more fundamental-based approach. Both a news transmission analysis and a herding contagion analysis conducted at the daily frequency produce evidence suggesting that pessimistic self-fulfilling dynamics has been at play in Eurozone debt markets; the implicit OMT announcement significantly lessens this contagion channel. The news transmission study suggests that the Eurozone contagion triggered by news from Spain to Italy, Belgium, France and Austria is only present prior to the announcement. The herding contagion analysis reveals frequent occurrences of simultaneous adverse shocks to the CDS spreads precisely of those same countries that were identified as exhibiting significant news contagion effects but again only pre-announcement.

The findings support De Grauwe's fragility hypothesis and the underlying multiple-equilibria theory of the crisis. The latter does not overlook the importance of fundamentals but adds that, in times of massive economic adjustment, panic amplifies exogenous shocks and can push an otherwise solvent country toward default. Our findings suggest that there is more to the recent Eurozone debt crisis than a strong link between credit spreads and fundamentals.

The rest of the paper is organized as follows. Next section outlines the relevant background literature. Section 3 describes the data and methodology. The empirical results are presented in section 4. Section 5 discusses various policy implications and concludes the paper.

2.2 Literature Review

Our paper is motivated by theoretical multiple-equilibria models that predict that fundamentals matter to the extent that countries with sound and weak fundamentals would incur a single outcome, default or no default, respectively, while multiple equilibria can happen for countries with similar fundamentals that lie in between these two poles (Calvo, 1988; De Grauwe, 2012; Gros, 2012; Corsetti & Dedola, 2016). Thus, the multiple-equilibria theory does not preclude increased sensitivity to fundamentals such as fiscal space and public debt ratios in the run-up to the Eurozone debt crisis but rather it contends that they are not the whole story (Aizenman, Hutchison, & Jinjarak, 2013; De Grauwe & Ji, 2013). It is well known that peripheral Eurozone countries suffered dramatic rises in their CDS spreads (during the pre-OMT period) compared with non-euro European countries with similar fundamentals; clearly, this leaves a role for self-fulfilling dynamics in the absence of a lender-of-last-resort.

Our paper relates to a burgeoning literature on the recent European sovereign debt crisis that provides evidence on 'contagion' broadly construed (with many nuances) as cross-country linkages driven by market expectations that are somewhat divorced from fundamentals. Beirne & Fratzscher (2013) document herding contagion or cross-country clustering of adverse shocks that cannot be traced to fundamentals. Likewise, De Grauwe & Ji (2013) show that a substantial part of the simultaneous rise in credit spreads of Eurozone countries was driven by market sentiment of panic and fear. Aizenman et al. (2013) show that Eurozone periphery default risk was overpriced in 2010 relative to that of non-euro 'matched' countries (i.e., similar countries in terms of fiscal space) and provide as one of two alternative explanations that the mispricing was due to a wave of contagious pessimism or a "bad" self-fulfilling expectational equilibrium. Alter & Beyer (2014) provide evidence of cross-country links in CDS spreads after controlling for exogenous common factors, while De Santis (2014) find contagion from Greece to other Eurozone member countries.

A parallel literature argues that investors became more sensitive to fundamentals during the crisis, a phenomenon that has been often formalized as "wake up calls" or fundamental contagion (Caceres, Segoviano Basurto, & Guzzo, 2010; Arghyrou & Kontonikas, 2012; Beirne & Fratzscher, 2013; Manasse & Zavalloni, 2013; Mink & De Haan, 2013). In a similar vein, an alternative more fundamentals-based explanation provided in Aizenman et al. (2013) for their findings is that the CDS market prices the risk of default not only on current but also future fundamentals which were expected to worsen for Eurozone periphery countries due to the adjustment challenges faced given their exchange rate and monetary constraints. Our paper adds to these studies by comparing the dynamics of Eurozone debt markets over two short length (12-month) windows that differ in an incontestable fact while members of the common currency faced similar fiscal problems, exchange rate and monetary constraints in both windows, only in the post-OMT announcement window the ECB assumed the LOLR role. This important change in the ECB policy stance ought to restrain the dramatic rises in credit spreads in the region only if these had a significant self-fulfilling dynamics component (De Grauwe, 2013).

Finally, our research is related to a stream of the literature that tests the effects of unconventional ECB policies on the market-priced risks of sovereign debt. For instance, De Pooter, Martin, & Pruitt (2013) find significant stock and flow effects on sovereign bonds' liquidity risk resulting from ECB bond purchases under the Security Markets Program (SMP). Similarly, Eser & Schwaab (2013) show that the SMP program had a long-lasting impact on the sovereign bond yields of periphery Eurozone countries. Likewise, Ghysels, Idier, Manganelli, & Vergote (2014) conclude that the SMP was effective in reducing both the level and volatility of bond yield spreads. Some recent studies found similarly depressing effects in the context of the OMT programme (Lucas, Schwaab, & Zhang, 2014; Falagiarda & Reitz, 2015; Altavilla, Giannone,

& Lenza, 2016a).

2.3 Data Description and Preliminary Analysis

2.3.1 Sovereign CDS spreads

The analysis is based on daily midpoint closing spread quotes on 5-year sovereign credit default swap (CDS) contracts from January 1, 2008 to July 25, 2013 from *Datastream*. The CDS contracts pertain to four Eurozone 'periphery' countries (Ireland, Italy, Portugal and Spain), six Eurozone 'core' countries (Austria, Belgium, Finland, France, Germany and the Netherlands), and four European countries that are not members of the euro currency union (Denmark, Norway, Sweden and UK). Following the literature, recent Eurozone members (such as Latvia) and other economically small countries (such as Estonia, Slovakia, Malta and Slovenia) are excluded from the periphery (see, for instance, Arghyrou & Kontonikas, 2012; Beirne & Fratzscher, 2013; Beetsma, Giuliodori, De Jong, & Widijanto, 2013; De Grauwe & Ji, 2013; 2014; De Santis, 2014). We conjecture that the exclusion of Greece, due to lack of CDS data from March 2012, is immaterial given that the systemic importance of Greece lessens notably in the most recent years (Alter & Beyer, 2014; González-Hermosillo & Johnson, 2017). This conjecture is later substantiated through a sensitivity analysis using bond yield data.

CDS prices are arguably more informative than bond yields for various reasons. Firstly, sovereign CDS data are more liquid and allow more accurate and timely estimates of credit risks (see, e.g., Ang & Longstaff, 2013 and Aizenman et al., 2013). In addition, CDS spreads are recognized as a more direct measure of default risk than bond yield spreads as they are not affected by differences in contractual arrangements, contract-specific liquidity effects, inflation expectations and demand/supply for credit conditions; see, e.g., Longstaff, Pan, Pedersen, & Singleton (2011) and Aizenman et al. (2013). Nevertheless, as noted above, we employ bond yield spreads as another proxy for sovereign risk in various robustness checks.

The evolution of daily CDS spreads in Figure 2.1 shows that an upward trend of pessimism in peripheral Eurozone credit risk erupts around March 2010 when Greece was first rescued. A marked downward trend in CDS spreads is observed after July 26, 2012 when Mario Draghi stated that the ECB was prepared to do "whatever it takes" to preserve the euro. This is the main event in our study (referred to as the implicit OMT announcement) because although the actual announcement of the OMT program took place barely a month later, it has been argued that Draghi's pledge effectively signalled the new stance of the ECB as LOLR in Eurozone sovereign debt markets (Pisani-Ferry, 2013).

Table 2.1 summarises the distribution of CDS premiums in basis points over the 12-month window preceding the (implicit OMT) announcement, Panel A, and the 12-month post-announcement window, Panel B; the last column shows the change in CDS spreads. The level and volatility of CDS spreads notably rise during the pre-announcement period; Ireland and Portugal are plausible exceptions since they both received bailout packages earlier on.[2] The CDS spreads of all countries exhibit an overall decrease in the post-announcement window.

As part of our preliminary data analysis, we regress the CDS spreads on macroeconomic fundamentals using quarterly data pooled across the 10 Eurozone countries. Our focus is on four key ratios often used in studies of the Eurozone debt crisis: Debt/GDP which measures the country's government debt relative to GDP (Caceres et al., 2010; Arghyrou & Kontonikas, 2012; Beirne & Fratzscher, 2013; De Grauwe & Ji, 2013; 2014), Budget/GDP or the relative government budget balance (Caceres et al., 2010; Arghyrou & Kontonikas, 2012; Beirne & Fratzscher, 2013; De Santis, 2014), Debt/Tax or 'fiscal space' defined as government debt relative to tax base averaged over the previous five years to account for business cycle fluctuations (Aizenman et al., 2013; De Grauwe & Ji, 2013), and Current Account/GDP or current account balance cumulated from 2009:Q4 divided by the GDP level (Beirne & Fratzscher, 2013; De Grauwe & Ji, 2013; 2014). Figure 2.2 shows scatter-plots and corresponding regression lines pre- and post-announcement which clearly reveal various outliers defined as the country-quarter CDS spreads that were most misaligned with fundamentals.

The outliers thus identified pertain to the Eurozone periphery (Ireland, Italy, Portugal and

[2] Ireland and Portugal were bailed out on November 22, 2010 and May 16, 2011, respectively.

Spain) and are only observed in the pre-announcement period. In the lower panel of Figure 2.2, excluding Portugal and Ireland (under the premise that their bailouts may have diluted the link of their CDS spreads with fundamentals), the outliers pertain to Spain and Italy. The pooled OLS regression results in Table 2.2 further confirm that fundamentals played a significant role as drivers of CDS spreads in both periods but their explanatory power is stronger post-announcement. These findings constitute evidence that the implicit OMT announcement may have helped the Eurozone debt markets to coordinate on a more fundamental-based equilibrium. We further investigate this conjecture in the next sections.

2.3.2 Commonality in credit risks of Eurozone sovereigns

We conduct a principal component analysis of daily CDS spreads over the two-year sample period around the implicit OMT announcement. Following Longstaff et al. (2011) and Arghyrou & Kontonikas (2012), we interpret the first two principal components (hereafter, PC1 and PC2) as common risk factors. PC1 represents a Eurozone sovereign risk factor broadly defined as an equal-weighted average of country CDS spreads; thus, the loadings capture the systemic contribution of each sovereign. PC2 represents the divergence among core and periphery countries; a negative (positive) loading indicates a core (periphery) country. This divergence amounts to the risk differential from investing in periphery versus core bonds which Arghyrou & Kontonikas (2012) link with the notion of 'contagion' through a default domino effect and the increased probability of aggregating fiscal risks.

As Table 2.3 shows, in the pre-announcement window the two components PC1 and PC2 explain together about 75% of the total variation in Eurozone CDS spreads, and the explanatory power of PC2 is 20%. Post-announcement, the two factors capture 96% of the total variation, and the contribution of PC2 falls to 5%. However, this increased commonality across Eurozone spreads cannot be attributed to a convergence trend among these countries' fundamentals in the post-announcement window since such a trend did not occur (see Figure 2.A1). A better interpretation is that there were additional unobserved risk factors (i.e., self-fulfilling dynamics) in the region which the announcement served to contain.

The contribution of Ireland and Portugal to Eurozone credit risk in the pre-announcement period is small as suggested by respective PC1 loadings of 0.097 and 0.020. This result aligns well with our previous finding (Table 2.1) that Ireland and Portugal are the only two Eurozone countries that experienced an overall pre-announcement decline in CDS spreads as a result of earlier EU/IMF bailout programs which altered investors' risk perceptions. A similar conclusion is reached by Alter & Beyer (2014) albeit from a different methodology. The loadings of PC2 also provide interesting reading. Before the announcement, markets clearly discriminated against countries such as Spain and Italy but perceived Portugal and Ireland more favourably. Post-OMT announcement, the positive loadings of Spain, Italy, Portugal and Ireland indicate that investors classify them together again alongside Belgium and France.

The dynamics of the two principal components in Figure 2.3 informally suggests a 'break' at the implicit OMT announcement date on July 26, 2012 which is confirmed by formal tests; thus, investors begin by then to anticipate the LOLR stance symbolized by the OMT program soon after.[3] PC1 and PC2 exhibit distinct behaviour pre- and post-announcement. PC1 swings wildly around a high plateau pre-announcement and stabilizes at a much lower level post-announcement. This further suggests that the announcement serves to restrain overreaction and mispricing of Eurozone credit risks. PC2 exhibits a steep upward trend in the first half of 2012, echoing investors' perception of growing divergence ('periphery' versus 'core') in Eurozone credit risk. The OMT announcement marks the beginning of a reversal.

2.4 Empirical Results

2.4.1 Spain-news transmission

In this section we conduct an event study to assess the impact of news specific to a "troubled" Eurozone country on other Eurozone member countries and on non-euro (or stand-alone)

[3]More formally, the Chow breakpoint test reveals a significant change in the conditional mean (level) of PC1 and PC2 on the day of the implicit OMT announcement (July 26, 2012); however, the evidence of a break on September 6, 2012 is relatively weaker as the test only suggests a break in the mean level of PC1 but not of PC2. Detailed results are tabulated in Table 2.A1.

European countries; a similar approach has been adopted in extant studies of contagion such as Mink & De Haan (2013). The first task then is to choose a periphery country as the main contagion source over the entire sample period under study from July 26, 2011 to July 25, 2013. Greece, Portugal and Ireland are ruled out since they received rescue packages at earlier stages and hence, lost their capacity to generate contagion later on (Alter & Beyer, 2014; González-Hermosillo & Johnson, 2017). Spain is a good candidate since it experiences the peak of its debt problems during the sample period (its CDS spreads peaked 2 days before the implicit OMT announcement) and the Eurozone core versus periphery contagion factor (PC2) loadings reveal that Spain is perceived as the riskiest country before the announcement.

In order to construct a Spain-specific news variable ($News_{t,Spain}$), a key input in the event-study analysis, we identify the days of most salient events through the OLS regression:

$$\Delta CDS_{Spain,t} - r_{f,t} = \alpha + \beta(\Delta European_t - r_{f,t}) + u_{Spain,t} \tag{2.1}$$

where $\Delta CDS_{Spain,t}$ denotes the daily change in the Spanish CDS spread, $\Delta European_t$ is the daily change in a European sovereign risk index constructed as an average of the CDS spreads of the remaining 9 Eurozone countries and 4 non-euro European countries in the sample, and $r_{f,t}$ is the ECB's daily Euro Over Night Index Average (EONIA) rate from *Datastream*. We estimate the model, which can be broadly perceived as a CAPM benchmark, separately over the pre- and post- announcement windows using the OLS method and examine the residuals.[4]

We identify the 10 days in each window (pre- and post-OMT announcement) that show the largest (absolute) residuals $|u_{Spain,t}|$ and relate those days to news from *Reuters* and *Bloomberg Businessweek* that may have caused the unexpected CDS change. This residual approach mitigates the possibility of 'event contamination' by market-wide (i.e., Eurozone) shocks since it identifies the Spain-specific event dates as days when the actual change in the Spanish CDS premium deviates substantially from the expected (CAPM-based) change.

Table 2.4 shows the 10 most salient Spain-specific events thus identified in each window and

[4]Innovations to Spanish CDS spreads might influence other countries' spreads but it is unlikely that they can drive the entire European index; thus, endogeneity does not represent a serious concern here.

associated news; the symbols R (*Reuters*) and B (*Bloomberg*) indicate the news source. Building upon the semi-strong form of the efficient markets hypothesis, we assume that a large residual on any day reflects news arriving on that day; that is, the CDS premium quickly incorporates all public information. Of course, the *Reuters* or *Bloomberg Businessweek* news may not always represent the actual underlying causes of significant market movements. Yet it provides a good approximation of what the average or representative investor might think about the important events of each day and about their potential effects on debt markets (Mink & De Haan, 2013). The discrete Spanish-news variable is constructed as $News_{Spain,t} = d_t * \widehat{u}_{Spain,t}$ where d_t is equal to 1 on the salient news dates and 0 elsewhere.

Next we estimate by OLS the following CAPM type model to measure the news contagion:

$$\Delta CDS_{i,t} - r_{f,t} = \beta(\Delta European_t - r_{f,t}) + \alpha_0 + \alpha_1 News_{Spain,t} + \varepsilon_{i,t} \tag{2.2}$$

where $\Delta CDS_{i,t}$ is the daily change in the Eurozone country ith CDS spread, and $\varepsilon_{i,t}$ is an innovation; $\Delta European_t$ and $r_{f,t}$ are as defined after Equation 2.1. The parameter of interest, α_1, captures the responsiveness of CDS spread changes in country i to news specific to the Spanish economy (contagion from Spain); $\alpha_t = \alpha_0 + \alpha_1 News_t$ is a time-varying abnormal return that captures the model's mispricing; the European risk factor loading, β, measures the sensitivity of the ith country CDS premium to the European CDS premium. Table 2.5 shows the estimation results over the pre- and post-OMT announcement windows.[5]

In the pre-OMT announcement window, the Spanish-news impact is positive for Austria, Belgium, France, Italy and the Netherlands although insignificant so for the latter. The strongest Spanish-news impact is found for Italy, in line with extant evidence of co-movements of Spanish and Italian debt spreads (e.g., González-Hermosillo & Johnson, 2017). These results suggest that pre-announcement, investors' perception of the creditworthiness of other Eurozone countries is tainted by Spanish news. This evidence is consistent with our early findings from the

[5]Inspired by Arghyrou & Kontonikas (2012), we estimate the country-specific CAPM Equation 2.2 without the Spain-news variable but expanded with PC2 as a proxy for contagion within the Eurozone. For none of the countries the PC2 coefficient is significant post-OMT. The only contagion effect pre-OMT is revealed for Spain (a significantly positive coefficient at the 1% level) which confirms the role played by this country as contagion-source; see Table 2.A9.

principal component analysis. In contrast, the Spanish-news coefficient for Portugal and Ireland is negative which confirms that the epicentre of the crisis had moved away from them, namely, investors' perception had shifted favourably towards countries that had applied strict austerity measures relative to the new 'strugglers' that were resisting those actions. Post-announcement, no significantly positive Spanish-news coefficient is obtained and thus, there is no news transmission from the troubled Spanish sovereign bond market to any other Eurozone bond market.[6] How do we explain the significant lessening in the Spanish news contagion effects before and after the implicit OMT announcement?

Under the premise that the ECB "whatever-it-takes" pledge was most credibly interpreted by investors as a hint of the bank's intention to act as LOLR (not as some form of fiscal redistribution or a bailout plan), our explanation is that there was self-fulfilling dynamics in the region. If the news transmission had been purely a wake-up call (i.e., news about Spain prompting investors to closely pay attention to other countries' fundamentals) then one would not expect the OMT to have such calming down effect on the news transmission.

CDS spreads of Eurozone member countries respond to (generally) adverse Spain-specific news but not necessarily because of the information content about their own current or future fundamentals; if this was the reason then the effect ought to have been present both pre- and post-announcement. The news transmission occurred most likely because, in the absence of a LOLR, investors' panic and fear channelled the markets toward a 'bad' equilibrium. Moreover, the finding of insignificant Spanish-news transmission for stand-alone countries (e.g., Denmark and UK, as shown in Table 2.6) further strengthens the evidence in favour of the Eurozone fragility hypothesis as it proves that the phenomenon was specific to members of the currency union. In the next section, we test the fragility hypothesis from a different angle.

[6] We assess the significance of the news impact differential ($H_0 : {\alpha_1}^{pre} = {\alpha_1}^{post}$ vs $H_A : {\alpha_1}^{pre} > {\alpha_1}^{post}$) with an F test statistic. To do so, we estimate the CAPM benchmark Equation 2.2 including the two dummy variables $News^{pre}_{Spain,t}$ and $News^{post}_{Spain,t}$ (pre- and post-announcement Spanish news, respectively) as regressors. The null is strongly rejected at the 1% significance level, as shown in Table 2.A2, for Austria, Belgium, France and Italy.

2.4.2 Herding effects

Following Beirne & Fratzscher (2013) and De Grauwe & Ji (2013) among others, our analysis of herding effects builds upon the notion that a simultaneous rise in sovereign CDS premiums that cannot be explained by common risk factors represents a "debt run" against the particular group of countries. Such phenomenon is commonly interpreted as reflecting contagion through unobservables such as herding due to investor sentiments of panic and fear.

We analyse the clustering of large unexplained changes in the pricing of sovereign risk (i.e., herding effect) through OLS estimation of the following CAPM type equation:

$$\Delta CDS_{i,t} - r_{f,t} = \alpha + \beta(\Delta European_t - r_{f,t}) + \varepsilon_{i,t} \tag{2.3}$$

using daily data over the two-year sample period around the implicit OMT announcement; the estimation is carried out per country i=1,...,N (N=14) which produces N distributions of daily residuals, $\varepsilon_{i,t}$, t=1,...,T (T=523 residuals). Our focus is on the right-tail of the distribution, that is, the most extreme positive residuals (i.e., unexpected CDS spread changes above those explained by the European risk factor) defined using the 20th or 10th percentile rule. The herding contagion index is conservatively defined on each day of the sample period as the proportion of countries with extreme positive residuals if this proportion exceeds 80% (i.e. clustering of extreme bad news) and zero otherwise.

Based on our findings from the principal components decomposition of CDS spreads and the Spain-news transmission analysis, the sample countries are allocated to three groups; a 'contagion' set comprising Austria, Belgium, France, Italy and Spain; a 'non-contagion' set comprising Finland, Germany, Ireland, the Netherlands and Portugal; and a non-euro set (or control group) with Denmark, Norway, Sweden and the UK. According to De Grauwe's Eurozone fragility hypothesis, the market perception of creditworthiness of these stand-alone countries cannot be tainted by self-fulfilling dynamics precisely because they retain sovereign control over their own currencies; namely, they have a superior force of last resort (their central bank) that prevents investors from precipitating a liquidity crisis (De Grauwe, 2013). Figure 2.4 shows the daily herding contagion indices for all three groups of countries.

As for the 'contagion' group, we identify a large number of days with simultaneous large unexpected changes in the pricing of sovereign risk preceding the implicit OMT announcement. However, even with the lenient 20th percentile rule, only three such clusters are identified post-announcement, and two of them occur on the days immediately before the formal OMT announcement on September 6, 2012 so they would not have qualified as post-announcement herding days had the formal OMT announcement been adopted as threshold to define the windows. The strict 10th percentile rule produces similar findings. In contrast, for the 'non-contagion' set and the non-euro set, we identify less than a handful of days with herding in both the pre- and post-announcement windows.

A formal Chow type test suggests that the mean level of the daily herding index for the 'contagion' Eurozone set is significantly higher (at the 1% level) in the pre-announcement window than post-announcement; detailed results are shown in Table 2.A3. Thus, herding (or fear-driven) contagion afflicted several Eurozone debt markets before, but not after, the implicit OMT announcement; these findings, in conjunction with our previous analysis, endorse De Grauwe's fragility hypothesis.[7]

2.4.3 Robustness checks

This section discusses additional estimations and tests. We begin by adopting a pricing equation which is more in the spirit of the arbitrage pricing theory (Ross, 1976; APT). This is the route taken also by empirical studies that employ market indices as proxies for unobserved sources of commonality among sovereigns (e.g., Manasse & Zavalloni, 2013; Bekaert, Ehrmann, Fratzscher,

[7]Our focus is on the unexpected movements in sovereign CDS spreads that are driven by 'bad' news (the right-tail of the residual distribution) because De Grauwe's fragility hypothesis in the context of the recent Eurozone sovereign debt crisis goes hand-in-hand with market sentiments of panic and fear (De Grauwe, 2012; 2013). However, more generally, self-fulfilling dynamics applies in both directions; the multiple-equilibria theory rationalizes both contagious pessimism and contagious optimism. In a robustness check we construct herding contagion indices pertaining to good news (left-tail of residuals) as shown in Figure 2.A2; the results confirm our main findings.

& Mehl, 2014). Accordingly, we estimate the following pricing equation with daily data:

$$\Delta CDS_{Spain,t} - r_{f,t} = \alpha + \beta_1(\Delta European_t - r_{f,t}) + \beta_2(\Delta Financial_t - r_{f,t}) + \beta_3(\Delta Global_t - r_{f,t}) + u_{Spain,t} \quad (2.4)$$

where the betas $(\beta_1, \beta_2, \beta_3)$ measure the sensitivity of the country's spreads to three common risk factors: one for European sovereigns, a second for financial intermediaries, and a third one for global sovereigns. $European_t$ is as defined after Equation 2.1, $Financial_t$ is the Markit iTraxx Senior Financials index based on the 25 most liquid CDS reference entities for senior debt issued by European financial firms, and $Global_t$ is an equally-weighted average of CDS spreads of the same 26 (non-European) sovereigns as in Longstaff et al. (2011) that proxies global sovereign credit risk. The data are sourced from *Datastream*.

As in the preceding analysis (reported in Section 2.4.1) we estimate the pricing model, separately, over the one-year windows before and after the implicit OMT announcement, in order to identify salient Spain-specific news dates. Once those event dates are identified we re-construct the discrete Spain news variable $News_{Spain,t}$ and estimate the contagion model:

$$\Delta CDS_{i,t} - r_{f,t} = \beta_1(\Delta European_t - r_{f,t}) + \beta_2(\Delta Financial_t - r_{f,t}) + \beta_3(\Delta Global_t - r_{f,t}) + \alpha_0 + \alpha_1 News_{Spain,t} + \varepsilon_{i,t} \quad (2.5)$$

where the relevant coefficient that captures contagion from Spain is α_1. The estimation results in Table 2.5 show a significantly positive news coefficient for Italy, Belgium, as with the CAPM, but also for France and Austria providing somewhat stronger evidence of news transmission from Spain to these two countries. The difference in the pre- and post-announcement news coefficients of these countries ($H_0 : {\alpha_1}^{pre} - {\alpha_1}^{post} = 0$) is strongly significant according to an F test statistic; detailed results are provided in Table 2.A2. Likewise, the herding contagion indexes derived from the APT benchmark with three common risk factors, instead of Equation 2.3, did not challenge our previous findings; the results are reported in Figure 2.A3.

In another robustness check we compare shorter (6-month as opposed to 12-month) windows around the implicit OMT announcement. The pre-announcement period is from January 26, 2012 to July 25, 2012, and the post-announcement period from July 27, 2012 to January 25, 2013. These shorter windows allow less room for changes in fundamental channels of cross-country credit risk transmission such as trade or financial links and hence, permit us to assess in a more 'sterilized' manner the impact of the implicit OMT announcement. We focus now on the 5 most salient event dates (instead of 10) in each period owing to the shorter periods. The results summarized in Table 2.7 confirm that the contagion from Spain news to the CDS premiums of other Eurozone countries (e.g., Italy and Belgium) unambiguously loses significance after the implicit OMT announcement.

Next we re-deploy the different approaches (principal components, Spain-news transmission and herding effects) by adopting as "announcement date" the day of the formal OMT announcement on September 6, 2012. The results summarized in Figures 2.A4 and 2.A5, and Table 2.A4 do not change our main findings.

Furthermore, we generalize the Spain-news transmission and herding contagion analysis to a setting that allows for time variation in the common risk factor loadings (the beta coefficients in the benchmark). Thus, we estimate by OLS the CAPM and APT pricing equations sequentially over rolling estimation windows. The event indicator $News_{Spain,t}$ is obtained as follows; the residual for day t is obtained as the difference between the actual CDS change on that day and the expected CDS change for that day according to the pricing model estimated over the corresponding rolling window (spanning a one-year period of 261 days). The window is then rolled forward one day to obtain the residual for day $t+1$ and so forth. The results from this analysis make no material difference to the news identification; in fact, about 90% of the dates thus detected are listed in Table 2.4. It is also reassuring to see that the evidence of herding contagion does not materially change when we sequentially estimate the CAPM benchmark over rolling windows. Detailed results are provided in Table 2.A5 (Spain-news identification) and Figure 2.A6 (herding effects).

In a final robustness check we analyse bond yield spreads defined, as it is usual, with reference to Germany. Detailed results are provided in Appendix figures and tables; Figure 2.A7 and Tables 2.A6-2.A7 summarize the principal components analysis. Figure 2.A8 and Table 2.A8 show, respectively, the herding contagion indices and F tests for the significance of the change in herding pre- and post-announcement. These analyses include Greece as periphery Eurozone country. The findings are robust to using different measures of sovereign credit risk and confirm that the earlier exclusion of Greece from the sample period due to lack of CDS spreads data is immaterial.

2.5 Concluding Remarks

The turmoil in Eurozone debt markets that erupted more than five years ago revived an old debate. Fundamental theorists blame periphery countries' deteriorating fundamentals. However, without denying the role of fundamentals, multiple-equilibria theorists argue that a self-fulfilling dynamics fuelled by market sentiments of fear and panic has been at play in the region pushing countries towards a worse equilibrium than is justified by fundamentals alone.

In the spirit of the multiple-equilibria discourse, De Grauwe (2013) articulates the Eurozone fragility hypothesis which states that countries that have adopted the euro are prone to sudden reversals in capital flows triggered by market sentiment of fear which can ultimately trigger the feared default. This self-fulfilling dynamics is unlikely to occur in the US, UK or Japan because the financial markets know that these countries have a central bank acting as lender-of-last resort (LOLR). Absent the latter, the Eurozone member countries are in essence like "emerging countries" issuing debt in a foreign currency; thus, their credit risk spreads can be subject to self-fulfilling dynamics that misaligns them with fundamentals.

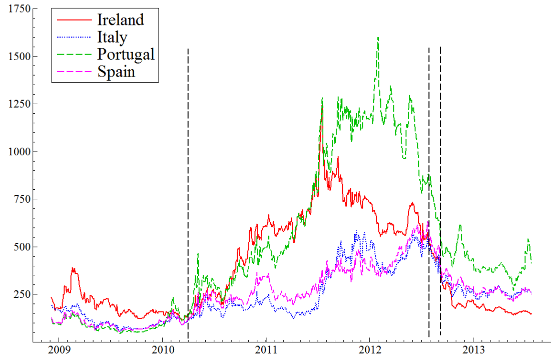
ECB President Mario Draghi announces on July 26, 2012 that the ECB is prepared within its mandate to do "whatever it takes" to preserve the euro; a month later, the ECB introduces the Outright Monetary Transactions (OMT) program that represents the lender-of-last resort stance. In response to German Eurosceptics' protests against the legality of the OMT program, the German Constitutional Court on February 2014 passes the case to the European Court of

Justice. Our paper contributes to making an informed judgement on this matter.

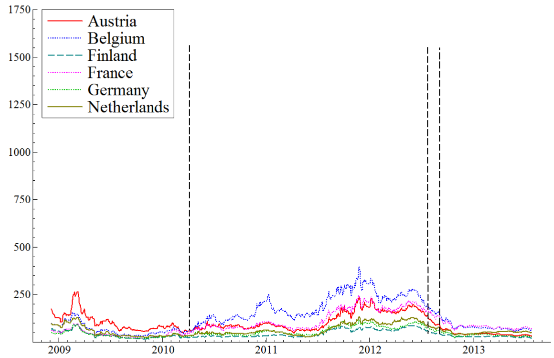
It provides empirical evidence that supports De Grauwe's Eurozone fragility hypothesis by showing, through an eclectic methodology subject to various robustness checks, that the sovereign debt crisis afflicting many Eurozone countries should be ascribed to more than fundamentals. A principal component analysis of Eurozone CDS spreads suggests that their commonality increases post-announcement. The link between Eurozone fundamentals and CDS spreads is found to increase post-announcement. The transmission of news about Spain and herding contagion significantly lessen after the announcement.

These findings suggest that a self-fulfilling dynamics was present in Eurozone debt markets and Draghi's implicit OMT announcement served to contain it. This policy stance of the ECB has helped not only 'periphery' members (such as Italy and Spain) but also 'core' members (such as Belgium and France) that are struggling to restore their economies to their pre-crisis state, even as their southern neighbours face the risk of deflation and stagnation.

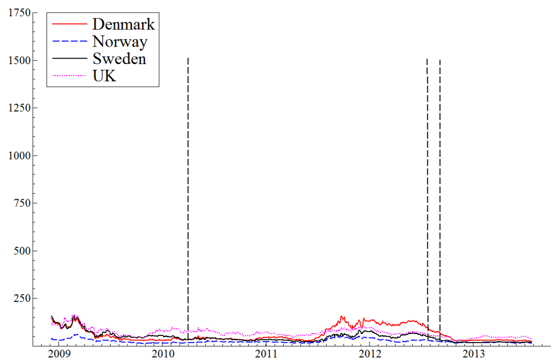
In suggesting that fundamentals are not the whole story, our findings challenge the 'you-deserve-what-you-get' attitude of advocates of strict austerity programs. Our findings stress the institutional role that the ECB plays in preventing debt runs in the region. A positive (albeit not sufficient) step in addressing the Eurozone structural fragility is the unanimous political backing by its members of the ECB's role as a lender of last resort. However, further structural reforms at supranational level such as a fiscal union possibly with centralized taxation and redistribution power are also crucial to fully overcome such fragility.



(a) Peripheral Eurozone countries



(b) Core Eurozone countries



(c) Stand-alone European countries

Figure 2.1: **Daily sovereign CDS spreads from December 5, 2008 to July 25, 2013.** The graphs show the daily evolution of the CDS spreads (in basis points) of 10 Eurozone countries distinguished as 'core' and 'peripheral', and four non-euro European countries. The first vertical line on March 31, 2010 marks the date of the first rescue package for Greece. The second vertical line on July 26, 2012 marks the date of ECB President Mario Draghi's statement that the Bank was prepared to do "whatever it takes to preserve the euro"; this paper refers to this date as the implicit Outright Monetary Transactions program (OMT) announcement. The third vertical line marks the formal OMT announcement on September 6, 2012.

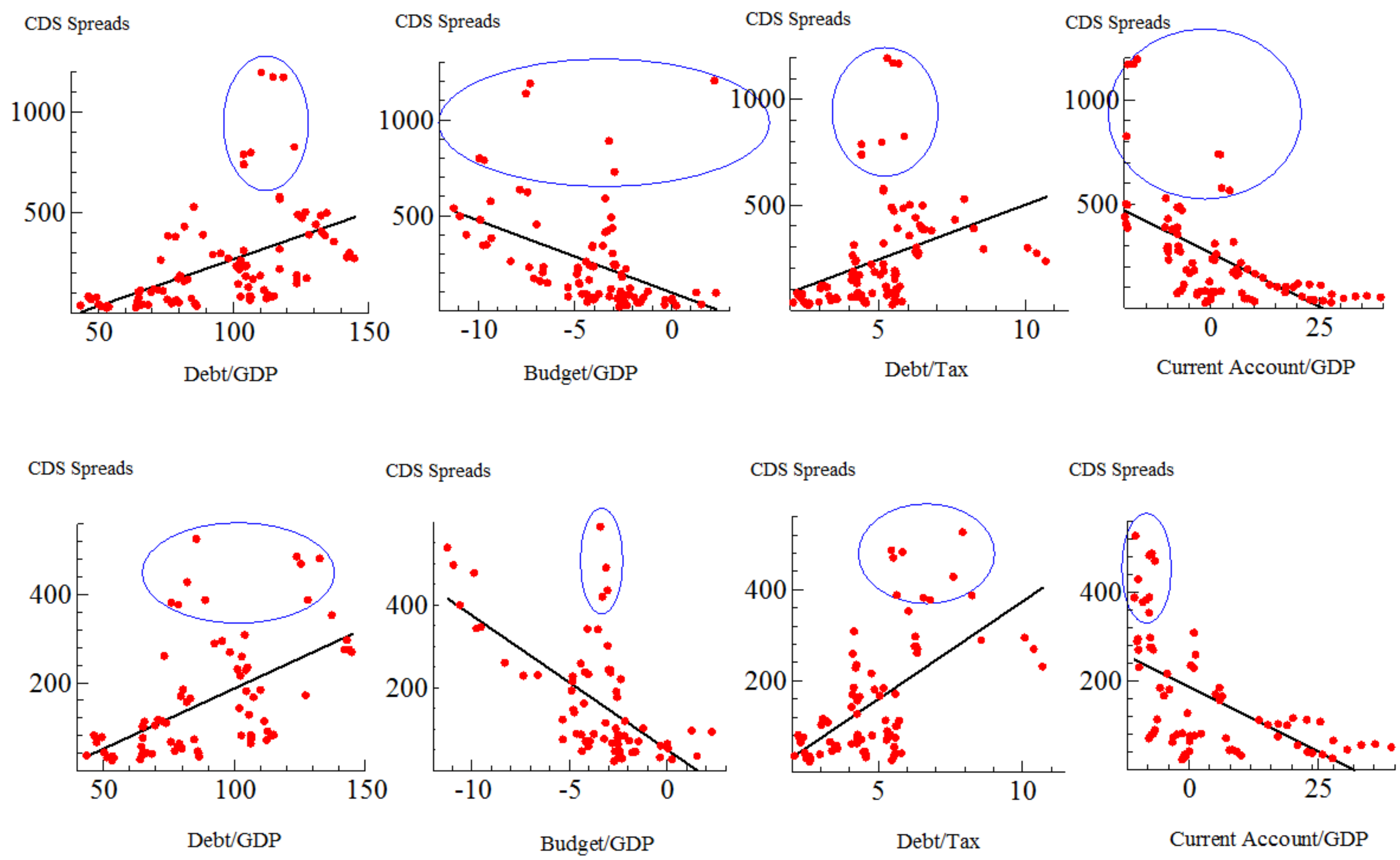


Figure 2.2: **Scatter-plots and OLS regression line for Eurozone CDS spreads versus macro fundamentals from mid-2011 to mid-2013.** The four graphs in the upper panel are based on pooled quarterly observations for 10 Eurozone countries (Austria, Belgium, Finland, France, Germany, Ireland, Italy, the Netherlands, Portugal and Spain) and the ones in the lower panel are for the same set of countries excluding Ireland and Portugal. The Y-axis represents the quarterly CDS spreads and the X-axis represents the corresponding macroeconomic indicators of these countries over the two-year period from mid-2011 to mid-2013.

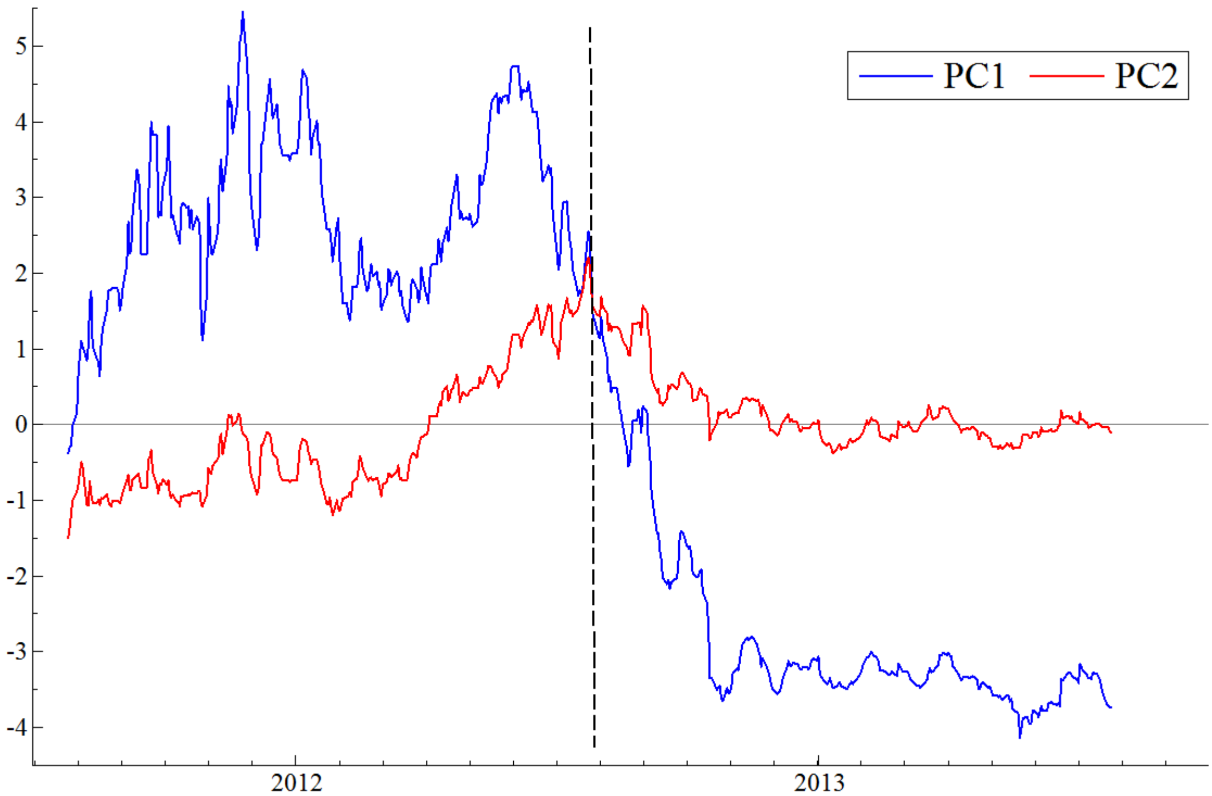


Figure 2.3: **First and second principal components of daily Eurozone CDS spreads.** The first and second principal components plotted are extracted from the correlation matrix of daily CDS spreads over the two-year sample period around the implicit OMT announcement (on July 26, 2012; vertical dashed line) that commences on July 26, 2011 and ends on July 25, 2013. The CDS spreads pertain to 10 Eurozone countries, of which 4 are 'peripheral' countries (Ireland, Italy, Portugal and Spain) and 6 are 'core' countries (Austria, Belgium, Finland, France, Germany and the Netherlands) according to the standard classification.

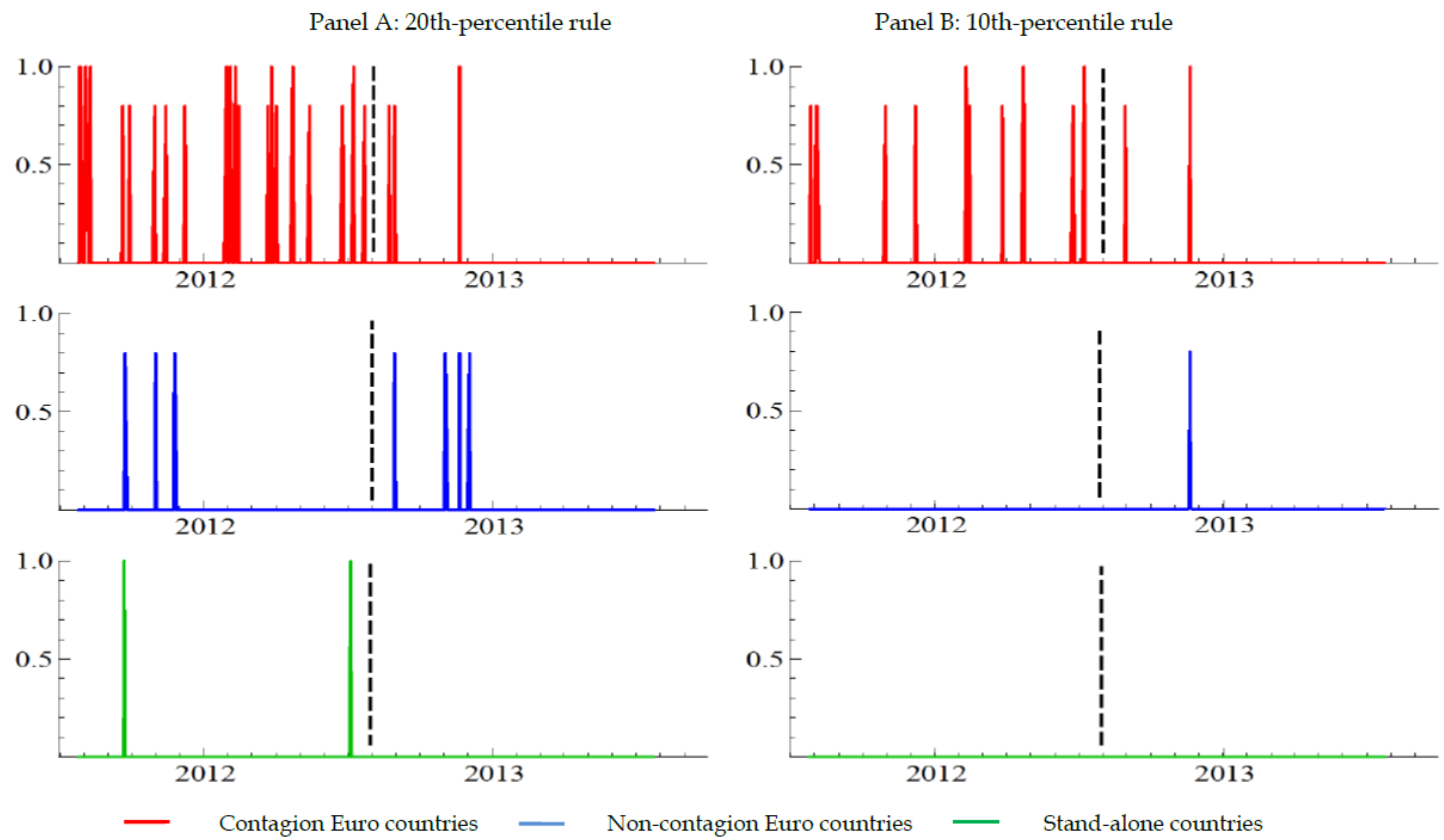


Figure 2.4: **Herding contagion index.** The graph plots on each day of the two-year sample period around the implicit OMT announcement (July 26, 2012; vertical dotted line) the herding contagion index. The index is defined as the proportion of countries that experienced unexpected extreme increases in their CDS spreads according to the CAPM benchmark, Equation 2.3, if this exceeds 80%, and zero otherwise; extreme is defined according to the 20th percentile or 10th percentile criteria applied to the residual distribution. The residuals are obtained through OLS estimation. The 'contagion' Eurozone countries are Austria, Belgium, France, Italy and Spain; the 'non-contagion' Eurozone countries are Finland, Germany, Ireland, the Netherlands and Portugal. The stand-alone (non-euro) countries are Denmark, Norway, Sweden and the UK.

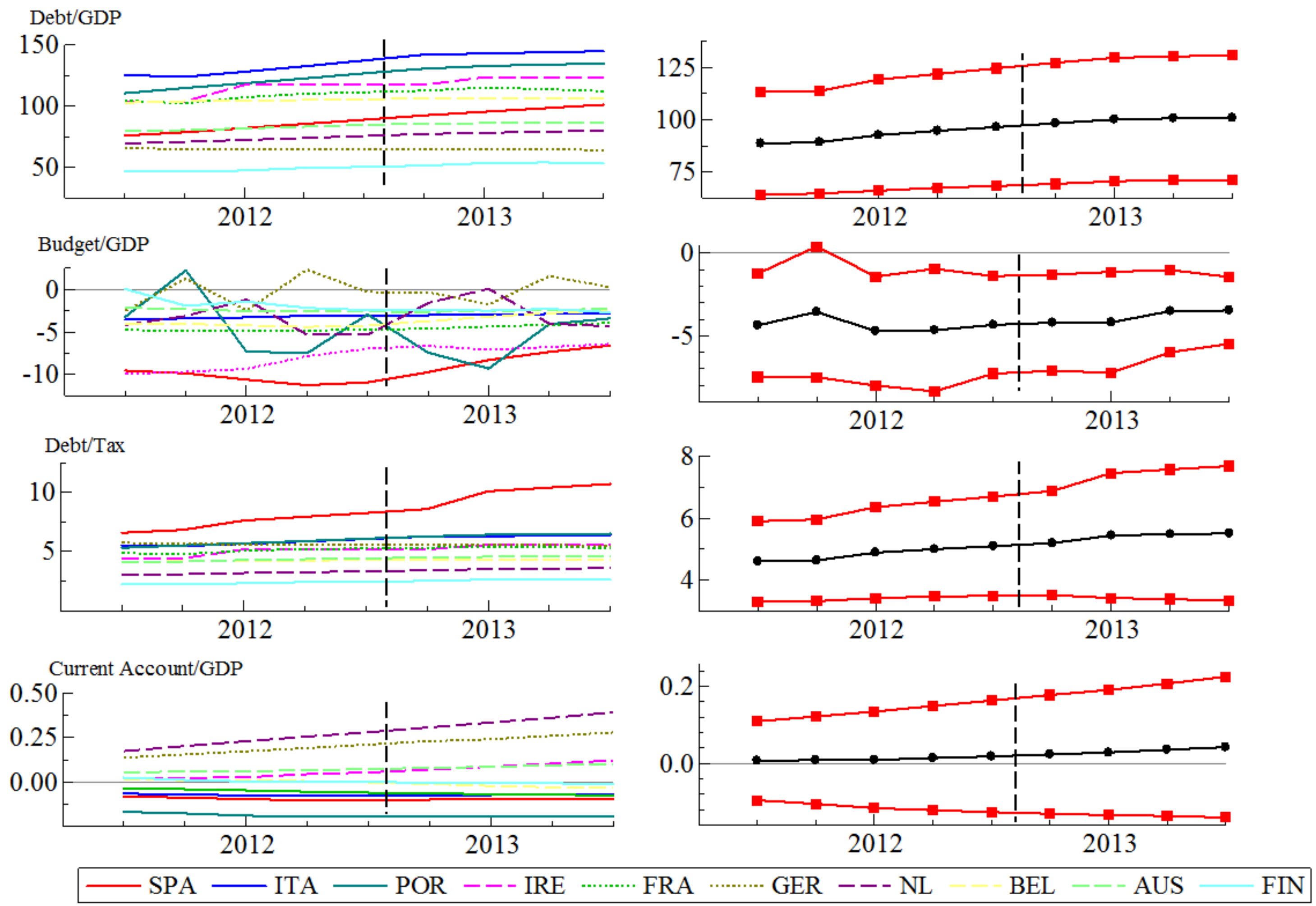


Figure 2.A1: **Evolution of macroeconomic fundamentals over the two-year observation window around the implicit OMT announcement.** Panel A plots from mid 2011 to mid 2013 the quarterly time-series of four key economic indicators widely used in the credit risk literature, Debt/GDP, Budget/GDP, Debt/Tax and Current Account/GDP, of which detailed definitions are given in the paper. Panel B plots the cross-sectional mean of each ratio and the one-standard-deviation upper and lower bands around the mean. The vertical dotted line denotes the implicit OMT announcement on July 26, 2012.

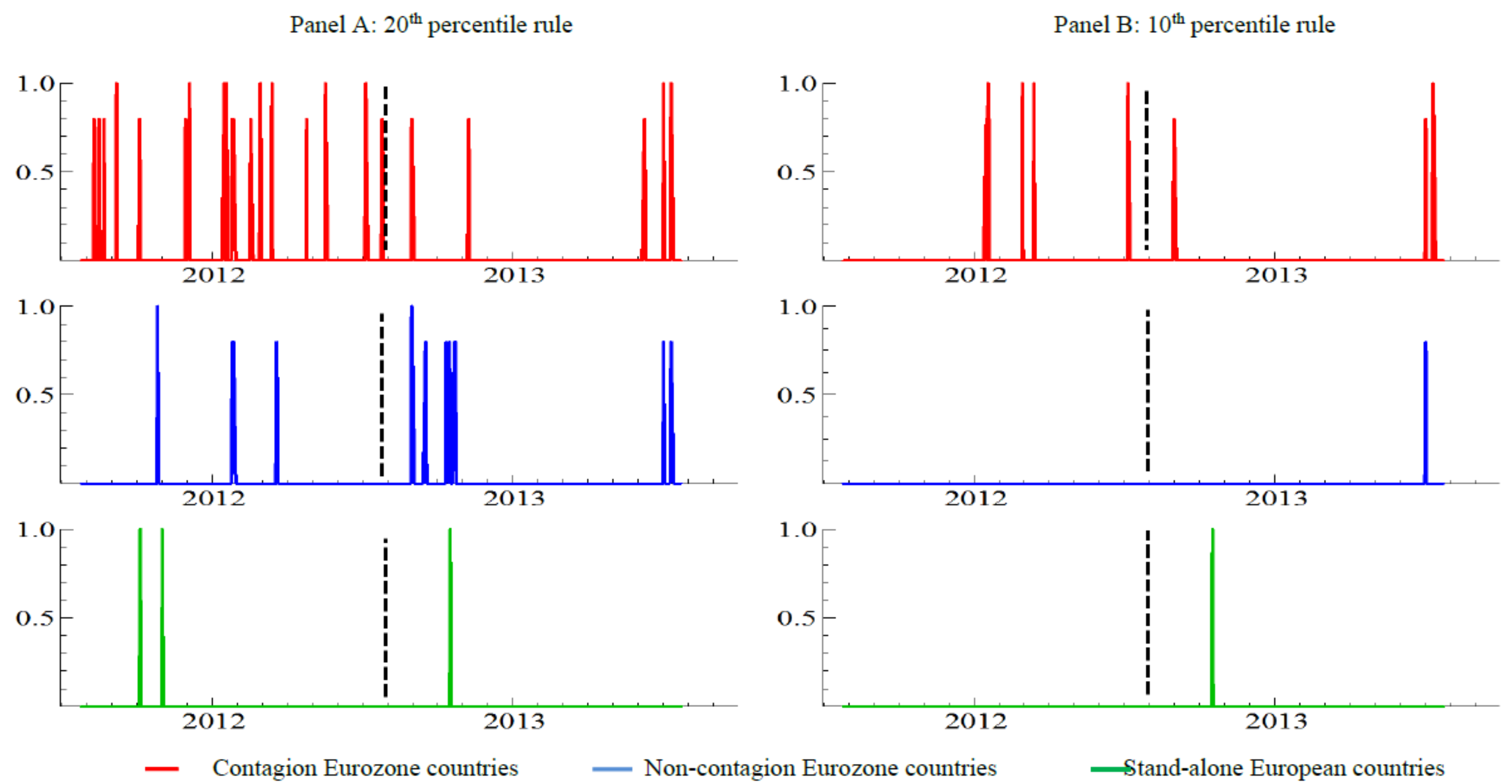


Figure 2.A2: **CAPM-based herding indices for 'good news' around the implicit OMT announcement (daily CDS spreads).** The figure plots herding contagion indices obtained through the CAPM benchmark, Equation 2.3, which is fitted to daily CDS spreads observed in the two-year period around the implicit OMT announcement on July 26, 2012 (vertical dotted line). The corresponding left-tail of the residual distribution is used to identify the 'good news' days. The graph plots on each day of the two-year sample period the herding index which is conservatively defined as the proportion of countries that experience simultaneously extreme favourable shocks (negative residuals defined according to the 20th or 10th percentile criteria), if this exceeds 80%, and zero otherwise. The 'contagion' euro countries are Austria, Belgium, France, Italy, and Spain; the 'non-contagion' euro countries are Finland, Greece, Ireland, the Netherlands and Portugal; the stand-alone countries are Denmark, Norway, Sweden and the UK.

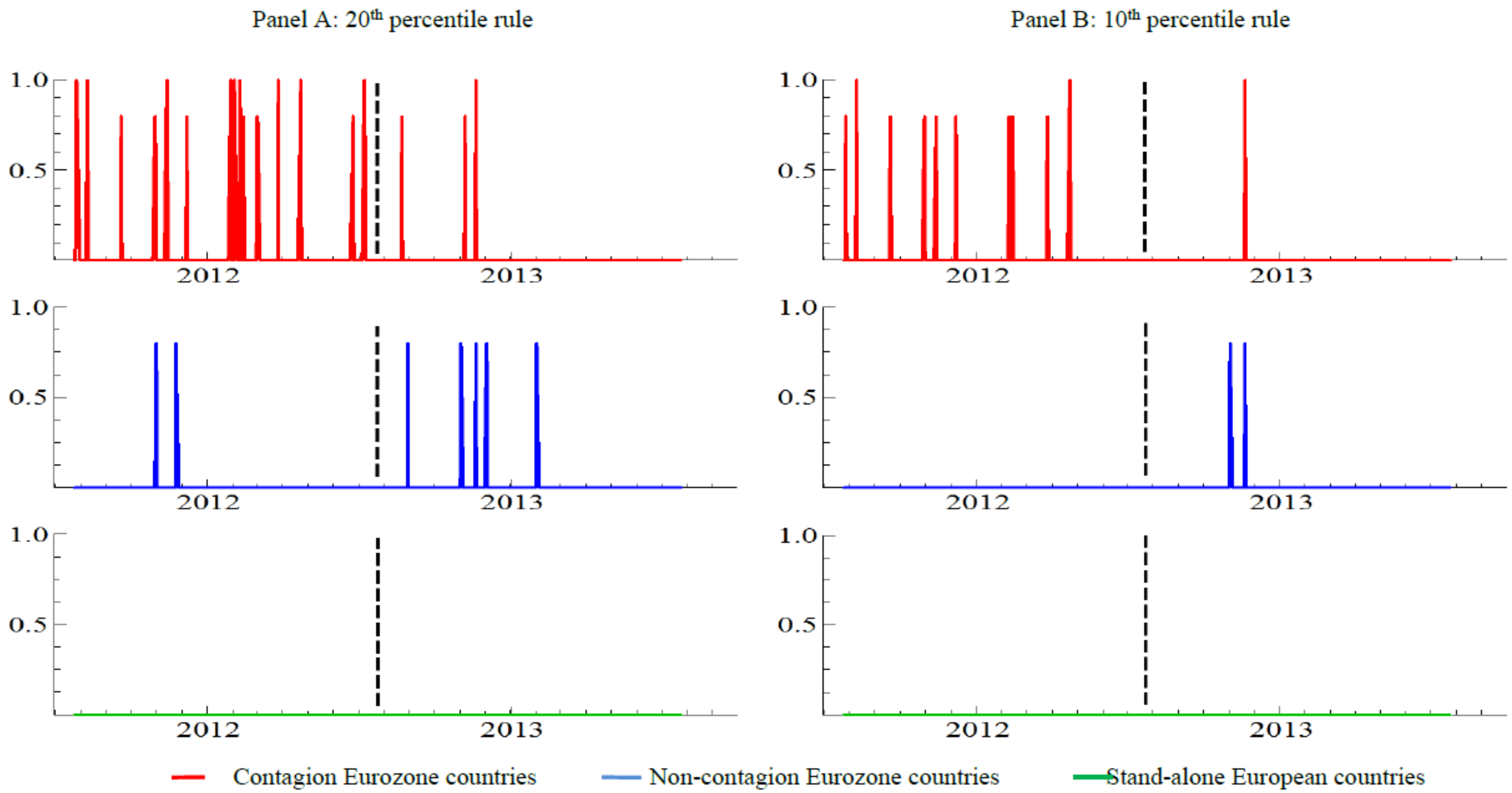


Figure 2.A3: **APT-based herding indices for 'bad news' around the implicit OMT announcement (daily CDS spreads).** The figure plots herding contagion indices obtained by fitting the APT benchmark, Equation 2.4, to daily CDS spread data in the two-year window around the implicit OMT announcement on July 26, 2012 (vertical dotted line). The right-tail of the residual distribution is used to identify the 'bad news' days. The graph plots on each day of the two-year sample period the herding index which is conservatively defined as the proportion of countries that experience simultaneously extreme adverse shocks (positive residuals defined according to the 20th or 10th percentile criteria), if this exceeds 80%, and zero otherwise. The 'contagion' euro countries are Austria, Belgium, France, Italy, and Spain; the 'non-contagion' euro countries are Finland, Greece, Ireland, the Netherlands and Portugal; the stand-alone countries are Denmark, Norway, Sweden and the UK.

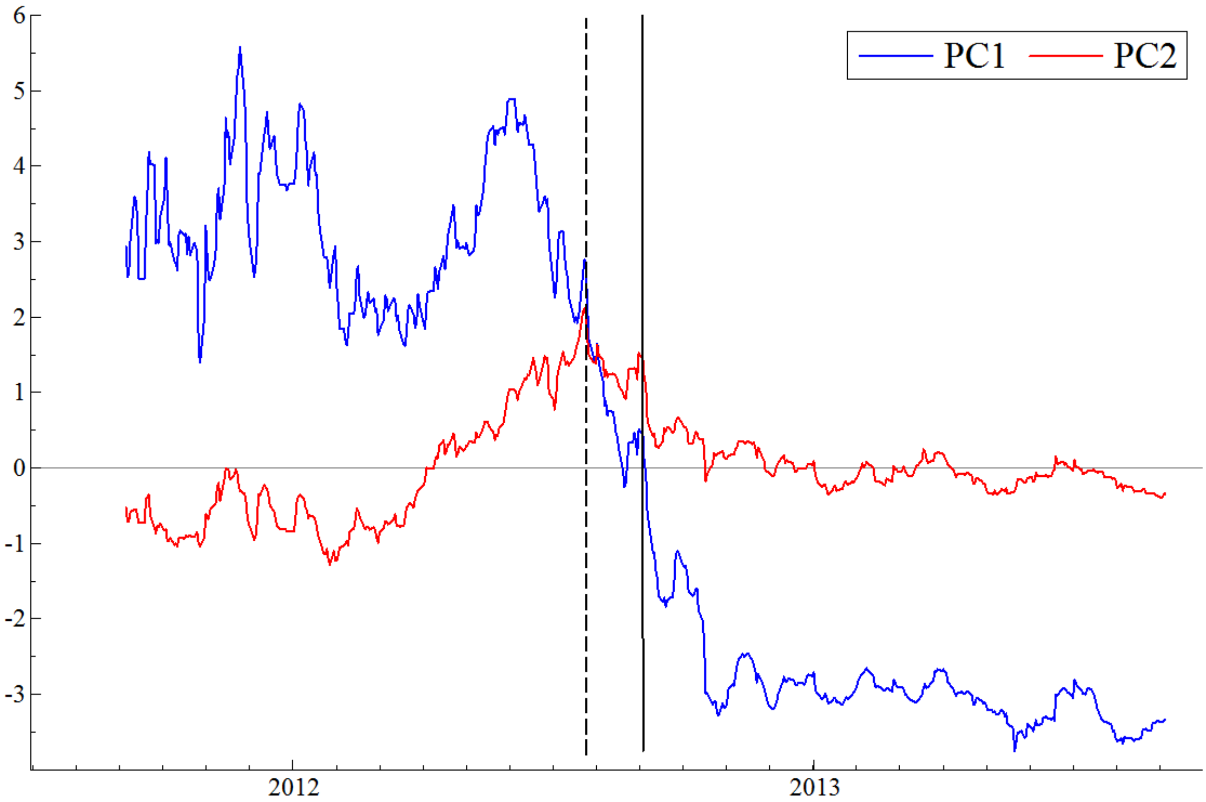


Figure 2.A4: **First and second principal components of daily Eurozone CDS spreads over the two-year observation window around the formal OMT announcement.** The figure plots the first and second principal components of daily CDS spreads for 10 Eurozone countries. The vertical continuous line marks the formal OMT announcement date on September 6, 2012. The vertical dotted line marks the implicit announcement date on July 26, 2012. The principal components are extracted from the correlation matrix of daily Eurozone CDS spreads for 4 peripheral countries (Ireland, Italy, Portugal and Spain) and 6 core countries (Austria, Belgium, Finland, France, Germany and the Netherlands).

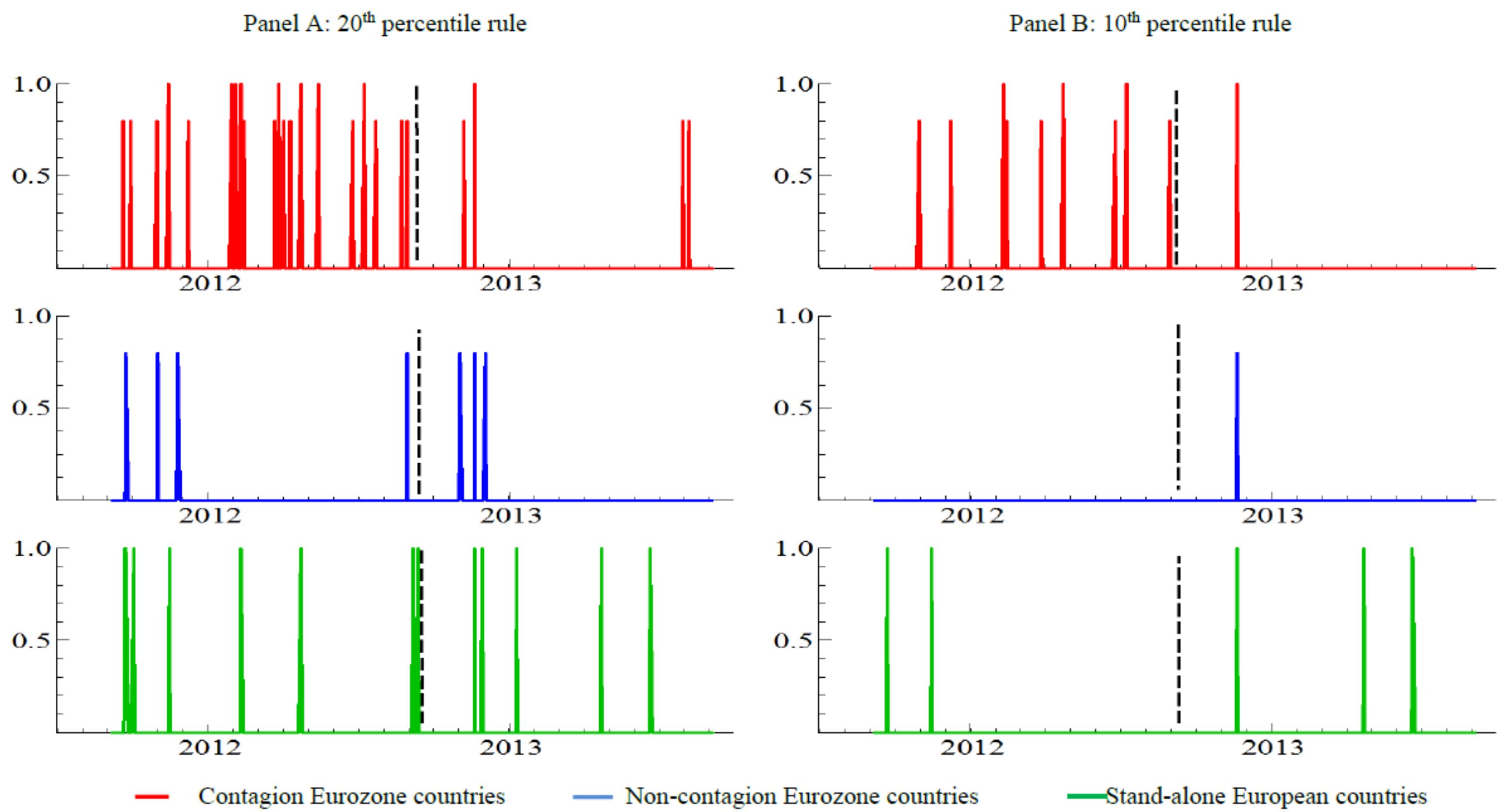


Figure 2.A5: **CAPM-based herding indices for 'bad news' around the formal OMT announcement (daily CDS spreads).** The figure plots herding contagion indices obtained by fitting the CAPM benchmark, Equation 2.3, to daily CDS spreads observed in the two-year period around the formal OMT announcement on September 6, 2012 (vertical dotted line). The corresponding right-tail of the residual distribution is used to identify the 'bad news' days. The graph plots on each day of the two-year sample period the herding index which is conservatively defined as the proportion of countries that experience simultaneously extreme adverse shocks (positive residuals defined according to the 20th or 10th percentile criteria), if this exceeds 80%, and zero otherwise. The 'contagion' euro countries are Austria, Belgium, France, Italy, and Spain; the 'non-contagion' euro countries are Finland, Greece, Ireland, the Netherlands and Portugal; the stand-alone countries are Denmark, Norway, Sweden and the UK.

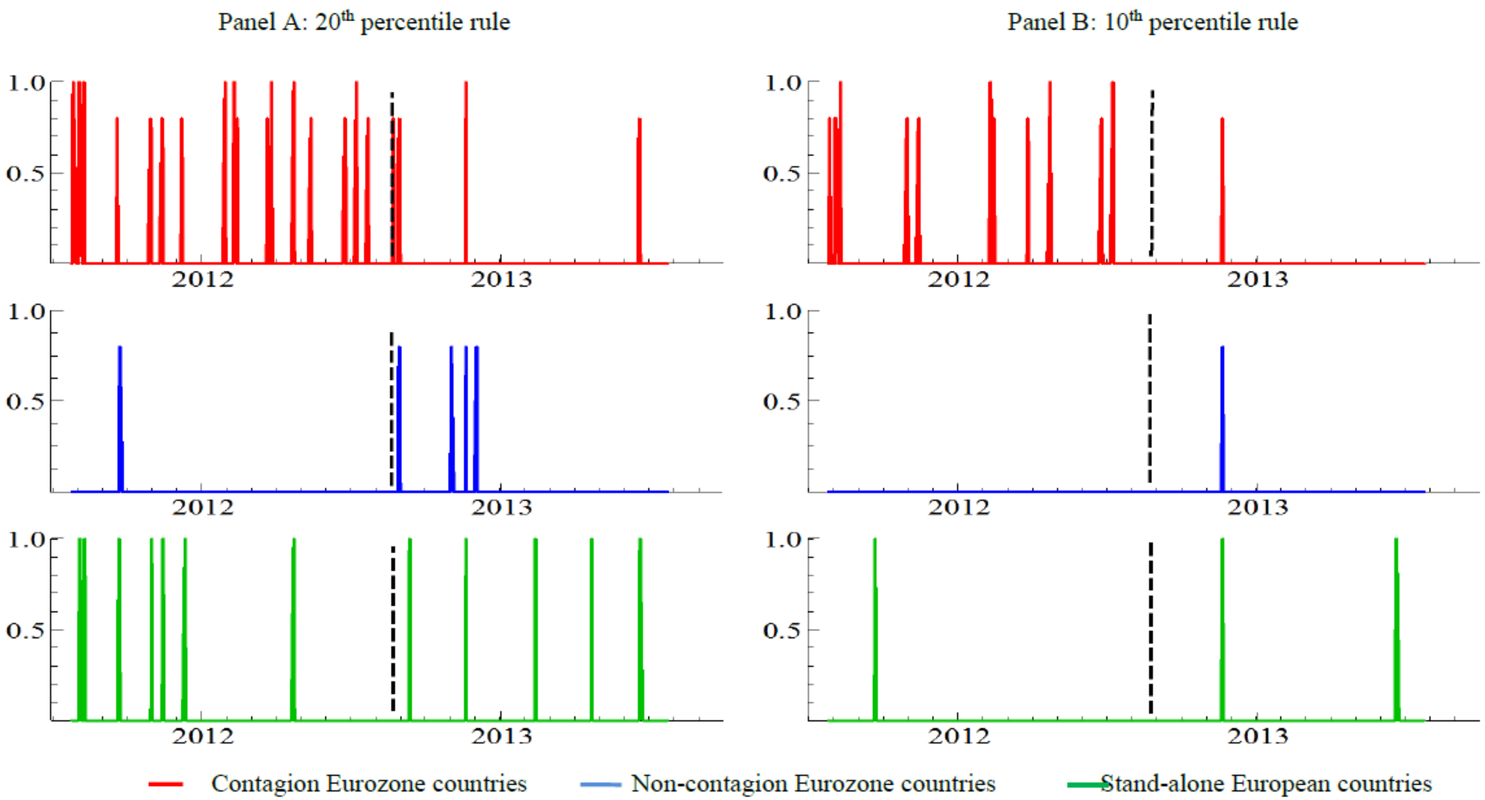


Figure 2.A6: **CAPM-based herding indices for 'bad news' around the implicit OMT announcement using rolling windows (daily CDS spreads).** The figure plots herding contagion indices obtained by fitting the CAPM benchmark, Equation 2.3, to daily CDS spreads observed in the two-year period around the implicit OMT announcement on July 26, 2012 (vertical dotted line). The corresponding right-tail of the residual distribution is used to identify the 'bad news' days. The estimation is carried out sequentially using rolling windows of fixed 261-days length. The graph plots on each day of the two-year sample period the herding index which is conservatively defined as the proportion of countries that experience simultaneously extreme adverse shocks (positive residuals defined according to the 20th or 10th percentile criteria), if this exceeds 80%, and zero otherwise. The 'contagion' euro countries are Austria, Belgium, France, Italy, and Spain; the 'non-contagion' euro countries are Finland, Greece, Ireland, the Netherlands and Portugal; the stand-alone countries are Denmark, Norway, Sweden and the UK.

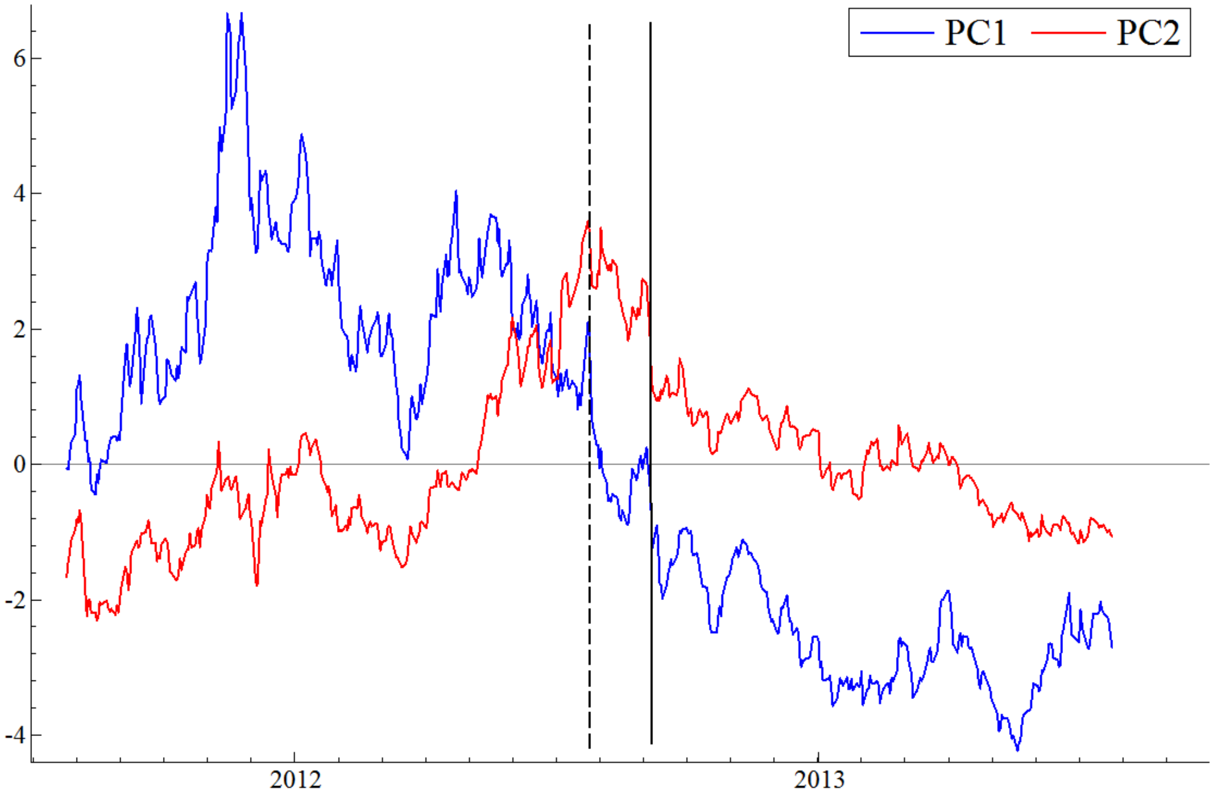


Figure 2.A7: **First and second principal components of daily Eurozone bond yield spreads over the two-year observation window around the implicit OMT announcement.** The figure plots the first and second principal components of daily bond yield spreads for 10 Eurozone countries defined with reference to Germany. The vertical dotted line marks the implicit OMT announcement date on July 26, 2012. The vertical continuous line marks the formal announcement date on September 6, 2012. The principal components are extracted from the correlation matrix of daily bond yield spreads for 5 Eurozone peripheral countries (Greece, Ireland, Italy, Portugal and Spain) and 5 core countries (Austria, Belgium, Finland, France and the Netherlands) countries.

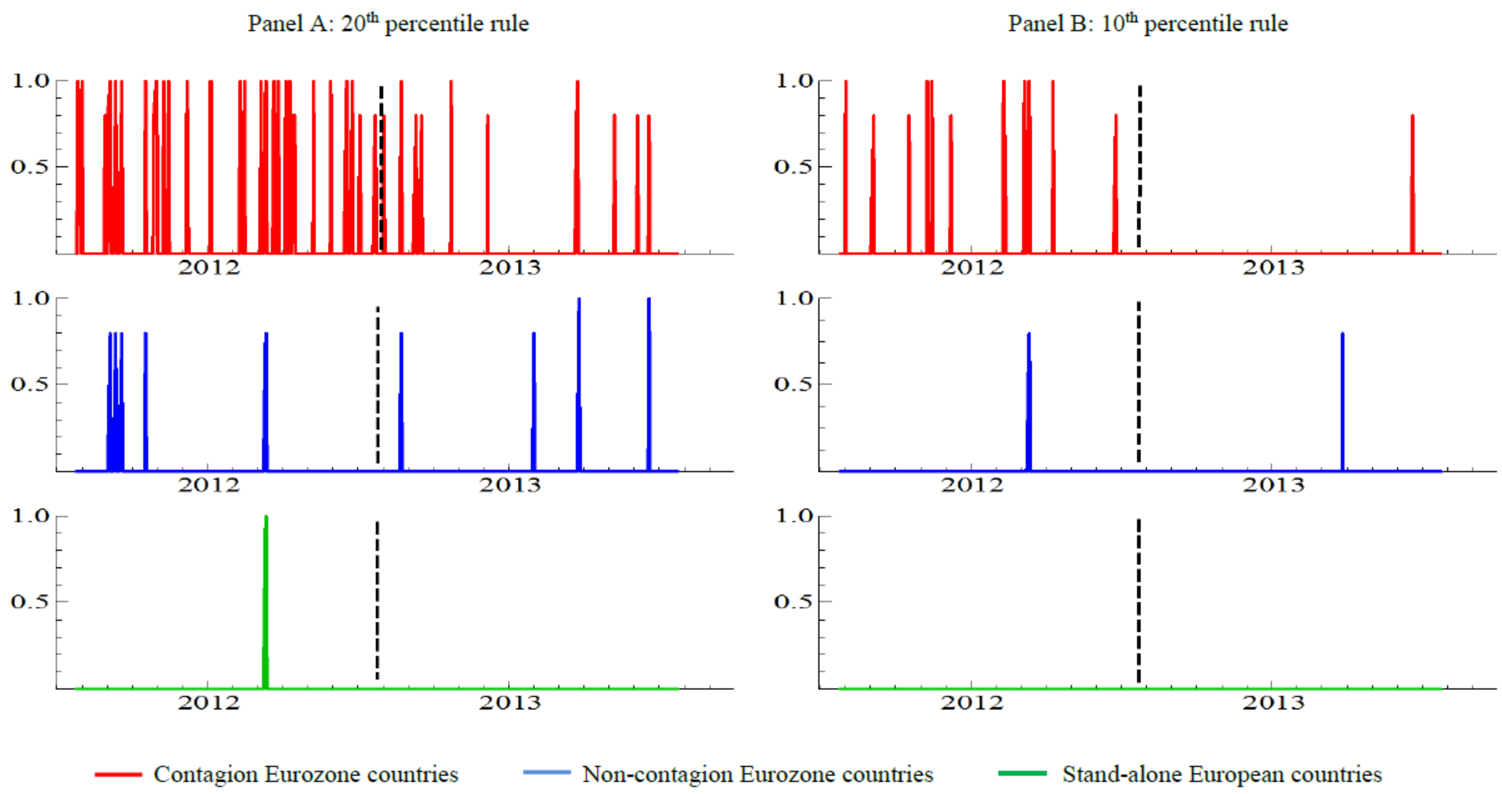


Figure 2.A8: **CAPM-based herding indices for bad news around the implicit OMT announcement (daily bond yield spreads).** The figure plots herding contagion indices obtained by fitting the CAPM benchmark, Equation 2.3, to daily bond yield spread data in the two-year window around the implicit OMT announcement on July 26, 2012 (vertical dotted line). For each country, the regressor $\Delta European_t$ is the daily change in a European sovereign risk index constructed as an average of the bond yield spreads of the remaining 9 Eurozone countries and 4 non-euro European countries in the sample. The right-tail of the residual distribution is used to identify the 'bad news' days. The graph plots on each day of the two-year sample period the herding index which is conservatively defined as the proportion of countries that experience simultaneously extreme adverse shocks (positive residuals defined according to the 20th or 10th percentile criteria), if this exceeds 80%, and zero otherwise. The 'contagion' euro countries are Austria, Belgium, France, Italy, and Spain; the 'non-contagion' euro countries are Finland, Greece, Ireland, the Netherlands and Portugal; the stand-alone countries are Denmark, Norway, Sweden and the UK.

	Panel A: Pre- announcement (July 26, 2011 to July 25, 2012)					Panel B: Post- announcement (July 26, 2012 to July 25, 2013)				
	Average	StDev.	Min.	Max.	ΔCDS	Average	StDev.	Min.	Max.	ΔCDS
Periphery Eurozone countries										
Ireland	696.76	110.06	522.25	973.43	- 370.31	222.95	97.65	142.39	550.71	- 401.40
Italy	452.92	68.16	271.88	586.70	276.63	288.96	63.31	221.96	515.71	- 257.80
Portugal	1133.30	145.71	781.71	1601.00	- 125.39	453.77	120.28	269.42	881.56	- 465.33
Spain	435.54	78.32	314.91	634.35	294.89	306.69	80.52	213.72	583.56	- 332.86
Core Eurozone countries										
Austria	164.72	28.42	86.91	236.13	50.47	50.92	21.12	31.60	127.89	- 94.11
Belgium	260.92	40.15	175.76	398.78	26.14	88.32	31.10	54.66	191.44	- 128.39
Finland	70.63	8.95	44.34	87.24	15.54	31.85	7.13	22.02	57.57	- 35.55
France	184.25	24.23	112.23	245.27	66.34	88.18	22.54	57.11	168.47	- 97.27
Germany	88.00	11.97	59.97	118.38	20.43	39.45	10.88	22.85	75.82	- 47.36
Netherlands	103.13	16.58	52.19	133.84	41.82	54.61	10.24	37.98	89.43	- 37.97
Non-Eurozone countries										
Denmark	117.54	15.23	65.31	157.46	32.04	38.27	16.46	24.50	93.44	- 68.94
Norway	36.15	8.47	20.84	52.11	4.15	19.76	3.10	15.28	30.96	- 15.40
Sweden	58.71	10.29	32.69	84.23	19.77	23.61	7.27	17.04	50.99	- 30.63
United Kingdom	78.25	11.53	58.66	101.64	- 10.24	44.85	7.38	27.60	58.41	- 20.34

Table 2.1: **Descriptive statistics for daily sovereign CDS spreads of European countries.** The table summarizes the distribution of daily sovereign CDS spreads (in basis points) of 14 European countries over the two yearly windows surrounding the implicit OMT announcement on July 26, 2012. ΔCDS denotes the change in the CDS spread from the initial day to the last day of the corresponding window.

	Pre-announcement (mid 2011 to mid 2012)		Post- announcement: (mid 2012 to mid 2013)	
	t-statistic	Adj.R^2	*t*-statistic	Adj.R^2
Debt/GDP	3.48**	0.35	5.59**	0.49
Budget/GDP	-2.39*	0.13	-4.75**	0.37
Debt/Tax	1.91	0.05	3.10**	0.28
Current Account/GDP	-3.43**	0.46	-4.79**	0.51

Table 2.2: **Pooled OLS regressions of quarterly CDS spreads on fundamentals.** The table reports the OLS slope coefficient estimates of regressions of CDS spreads on four macro indicators in the one-year windows before and after the implicit OMT announcement on July 26, 2012. The estimation is based on pooled quarterly data for 10 Eurozone countries. Auto-correlation and heteroskedasticity robust Newey-West t-statistics are reported. ** and * denote significance at the 1% and 5% levels, respectively.

	Eigenvalues	Total variation explained (%)	Country loadings (eigenvectors)	PC1	PC2
Panel A: Pre-announcement (July 26, 2011 to July 25, 2012)					
PC1	5.646	56.46	Austria	0.385	-0.025
PC2	1.877	75.22	Belgium	0.320	-0.398
PC3	1.253	87.76	Finland	0.346	0.040
PC4	0.470	92.46	France	0.405	-0.037
PC5	0.334	95.80	Germany	0.368	-0.109
PC6	0.229	98.09	Ireland	0.020	-0.533
PC7	0.072	98.81	Italy	0.368	0.095
PC8	0.057	99.38	Netherlands	0.377	0.162
PC9	0.034	99.72	Portugal	0.097	-0.449
PC10	0.028	100.00	Spain	0.208	0.552
Panel B: Post-announcement (July 26, 2012 to July 25, 2013)					
PC1	9.148	91.48	Austria	0.328	-0.025
PC2	0.450	95.98	Belgium	0.328	0.039
PC3	0.223	98.21	Finland	0.322	-0.135
PC4	0.070	98.91	France	0.325	0.004
PC5	0.044	99.35	Germany	0.310	-0.344
PC6	0.028	99.63	Ireland	0.327	0.056
PC7	0.014	99.77	Italy	0.320	0.106
PC8	0.011	99.89	Netherlands	0.282	-0.628
PC9	0.007	99.96	Portugal	0.294	0.613
PC10	0.004	100.00	Spain	0.323	0.276

Table 2.3: **Principal component decomposition of daily Eurozone CDS spreads.** The table reports eigenvalues $\lambda_j, j = 1, ..., 10$ and the proportion of the total variation in CDS spreads of 10 Eurozone countries that is explained by each principal component given by $\lambda_j / \sum \lambda_j$. The last two columns report the eigenvectors or country loadings to construct the first and second principal components (denoted PC1 and PC2, respectively). The principal components are extracted from the correlation matrix of daily sovereign CDS spreads over the two-year sample period around the implicit OMT announcement (on July 26, 2012). The principal components are summarized, separately, over the two yearly windows surrounding the implicit OMT announcement. The 10 Eurozone countries are 4 'peripheral' (Ireland, Italy, Portugal and Spain) and 6 'core' (Austria, Belgium, Finland, France, Germany and the Netherlands) according to the standard classification.

Date	News Description	Residual (%)	ΔCDS (%)
Panel A: Pre-announcement (July 26, 2011 to July 25, 2012)			
10.08.2011	Spain’s Banca Civica BCIV.MC said on Wednesday its non-performing loan ratio at the end of the first half was 5.43 percent compared to 4.70 percent at the end of 2010 (R).	5.77	11.34
23.08.2011	An agreement between Spain's ruling Socialists and other political parties over controlling public spending is possible, Spanish Prime Minister Jose Luis Rodriguez Zapatero said on Tuesday (R).	-5.01	0.64
23.09.2011	Spain approved the sale of a stake in state-owned lottery operator Loterias y Apuestas del Estado on Friday, leaving what will be the country's biggest initial public offering on track despite tough markets and political opposition. While revenue from privatisation sales cannot be used to reduce a European country's annual public deficit under EU rules, the proceeds will mean Spain has to issue less debt (R).	-4.33	7.14
14.11.2011	Spain's borrowing costs risk hitting euro-era highs at auction this week, fuelling fears it is getting dragged back into the heart of the euro zone debt crisis as markets await evidence of a new government's commitment to economic reform (R).	6.07	9.07
03.01.2012	Registered unemployment in Spain, where almost half of young people are out of work, rose for a fifth month in December as the euro area’s fourth-largest economy contracted. The number of people registering for unemployment benefits rose 1,897 to 4.42 million, the Labor Ministry in Madrid said in an e-mailed statement today (B).	5.85	6.45
04.01.2012	The heavily indebted Spanish region of Valencia delayed a 123 million euro repayment to Deutsche Bank by a week, its deputy chief minister said, but did not call on the country's government to guarantee the funds. Ratings agency Fitch said in December it believed the government would step in to help Valencia if it faced problems (R).	4.81	7.17
02.03.2012	Spain set itself a softer deficit target for 2012 than originally agreed under the euro zone's austerity drive, putting a question mark over the credibility of the European Union's new fiscal pact (R).	4.65	4.86
27.03.2012	Spain’s economy is suffering its second recession since 2009, the Bank of Spain said today, a development that obstructs the government’s efforts to reorder public finances as it prepares the budget for this year (B).	5.42	- 0.01
18.06.2012	Spanish bond yields hit a new euro-era high above 7 percent on Monday as initial relief after a pro-bailout vote in Greek elections gave way to pessimism about the problems surrounding the bigger Spanish economy (R).	4.29	3.76
09.07.2012	European ministers were set to grant Spain an extra year to reach its deficit targets in exchange for further budget savings but remained far from pinning down details of bank rescues and emergency bond-buying that are of greater concern to markets. Spain faces budget risks despite the looser target (R).	5.59	2.38
	(Cont.)		

Date	News Description	Residual (%)	ΔCDS (%)
Panel B: Post-announcement (July 26, 2012 to July 26, 2013)			
30.08.2012	Spanish consumer prices surged in August driven by higher fuel costs and a value-added tax hike in September could drive another jump, complicating Spain's efforts to get out of recession and generate the growth needed to reduce its debts (R).	6.87	3.03
17.09.2012	Ten-year Spanish government bond yields extended their rise on Monday, driven by pressure ahead of this week's auctions and lingering doubts over when, or if, Spain will seek financial aid (R).	3.40	1.87
18.09.2012	Spain will consider seeking a bailout if the conditions imposed are acceptable, Deputy Prime Minister Soraya Saenz de Santamaria said as loan defaults at Spanish banks climbed and lending dropped (B).	5.10	5.52
17.10.2012	Spanish government bond yields fell to their lowest since early April on Wednesday after Moody's kept Spain's investment grade rating, removing an immediate threat to the euro zone's fourth largest economy (R).	- 5.11	- 16.81
18.10.2012	Spain's banks face more loan losses as the pace of an economic slump risks turning a worst-case scenario dismissed in stress tests into reality, according to data published by the Bank of Spain on its website today (R).	4.09	2.63
22.10.2012	Spanish bonds fell for a second day on speculation Prime Minister Mariano Rajoy's regional election victory gives him more room to delay seeking a bailout that would allow Europe's central bank to buy the nation's debt (B).	6.28	3.70
23.10.2012	The Bank of Spain said on Tuesday that Spain was at risk of missing its 2012 budget deficit target of 6.3 percent of GDP, including regions and social security, as a prolonged recession slashes revenues (R).	3.98	5.68
04.02.2013	Ten-year Spanish government bond yields rose on Monday as the country's opposition party called for the resignation of Prime Minister Mariano Rajoy over a corruption scandal (R).	3.94	6.07
26.02.2013	Spain is no closer to seeking bond-buying help from the European Central Bank than it was before Italy's election, which has triggered renewed market turmoil, Economy Minister Luis de Guindos said on Tuesday (R).	3.76	7.92
22.07.2013	Spain's Prime Minister on Monday said he would soon appear in Parliament to face questions over a corruption scandal that has dented his ruling People's Party's credibility and upset Spaniards as they go through deep cuts in social welfare (R).	3.55	- 2.16

Table 2.4: **Spain-specific daily news.** The news source is *Reuters* (R), or *Bloomberg Businessweek* (B). The residuals shown in the penultimate column, obtained by OLS estimation of the CAPM Equation 2.1, are a proxy for the salience of the Spain-specific news on the corresponding days listed in the first column. The last column reports the actual daily change in the Spanish CDS spread. The spreads (and residuals) are expressed in percentage points.

Panel A: Pre-announcement (July 26, 2011 to July 25, 2012)

	Austria		Belgium		Finland		France		Germany	
	CAPM	APT	CAPM	APT	CAPM	APT	CAPM	APT	CAPM	APT
European	**1.137	**0.751	**1.141	**0.938	**0.822	**0.628	**1.107	**0.850	**0.973	**0.616
	(0.085)	(0.067)	(0.069)	(0.072)	(0.081)	(0.092)	(0.086)	(0.125)	(0.085)	(0.090)
Financial	-	*0.107	-	0.036	-	**0.110	-	**0.107	-	0.077
		(0.045)		(0.044)		(0.039)		(0.038)		(0.048)
Global	-	**1.485	-	**1.239	-	**0.965	-	**1.289	-	**1.312
		(0.097)		(0.076)		(0.077)		(0.083)		(0.092)
α_0	0.002	0.001	0.000	-0.001	0.001	0.000	0.002	0.001	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
News (α_1)	**0.219**	****0.432**	****0.404**	****0.506**	**-0.086**	**0.210**	**0.207**	***0.281**	**-0.148**	**0.011**
	(0.184)	**(0.099)**	**(0.097)**	**(0.118)**	**(0.178)**	**(0.138)**	**(0.134)**	**(0.120)**	**(0.165)**	**(0.101)**
Adj. R^2	0.57	0.68	0.68	0.72	0.45	0.49	0.59	0.64	0.52	0.63

	Ireland		Italy		Netherlands		Portugal	
	CAPM	APT	CAPM	APT	CAPM	APT	CAPM	APT
European	**0.612	**0.636	**1.236	**1.190	**0.951	**0.676	**0.681	**0.736
	(0.050)	(0.063)	(0.054)	(0.097)	(0.072)	(0.080)	(0.064)	(0.078)
Financial	-	0.039	-	**0.089	-	0.104	-	-0.043
		(0.026)		(0.030)		(0.055)		(0.041)
Global	-	**0.544	-	**1.098	-	**1.186	-	**0.607
		(0.075)		(0.081)		(0.090)		(0.098)
α_0	-0.002	-0.002	0.003	0.001	0.002	0.002	-0.000	0.000
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
News (α_1)	***-0.547**	***-0.544**	****0.566**	****0.562**	**0.086**	**0.225**	***-0.410**	****-0.561**
	(0.229)	**(0.240)**	**(0.137)**	**(0.122)**	**(0.163)**	**(0.135)**	**(0.175)**	**(0.171)**
Adj. R^2	0.47	0.48	0.72	0.73	0.50	0.57	0.40	0.41

(*Cont.*)

Panel B: Post-announcement (July 26, 2012 to July 26, 2013)

	Austria		Belgium		Finland		France		Germany	
	CAPM	APT	CAPM	APT	CAPM	APT	CAPM	APT	CAPM	APT
European	**0.735	**0.643	**0.870	**0.734	**0.502	**0.449	**0.845	**0.794	**0.726	**0.624
	(0.078)	(0.091)	(0.106)	(0.117)	(0.091)	(0.108)	(0.107)	(0.123)	(0.127)	(0.138)
Financial	-	-0.006	-	-0.033	-	-0.034	-	-0.037	-	-0.015
		(0.046)		(0.039)		(0.050)		(0.037)		(0.042)
Global	-	**0.720	-	**0.895	-	**0.472	-	**0.718	-	**0.730
		(0.095)		(0.079)		(0.086)		(0.092)		(0.146)
α_0	*-0.003	**-0.004	-0.001	**-0.003	-0.002	**-0.003	0.000	-0.002	-0.001	-0.003
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
News (α_1)	**-0.058**	**-0.070**	**-0.107**	**-0.113**	***-0.236**	**-0.236**	**-0.158**	**-0.137**	**-0.273**	**-0.281**
	(0.117)	**(0.126)**	**(0.281)**	**(0.293)**	**(0.118)**	**(0.135)**	**(0.281)**	**(0.316)**	**(0.380)**	**(0.411)**
Adj. R^2	0.41	0.43	0.49	0.52	0.24	0.25	0.46	0.46	0.26	0.27

	Ireland		Italy		Netherlands		Portugal	
	CAPM	APT	CAPM	APT	CAPM	APT	CAPM	APT
European	**0.700	**0.665	**1.096	**1.089	**0.627	**0.588	**0.846	**0.771
	(0.099)	(0.113)	(0.088)	(0.107)	(0.098)	(0.117)	(0.138)	(0.143)
Financial	-	-0.006	-	0.065	-	-0.059	-	**0.171
		(0.045)		(0.048)		(0.031)		(0.053)
Global	-	**0.593	-	**0.811	-	**0.538	-	**0.779
		(0.105)		(0.067)		(0.073)		(0.108)
α_0	*-0.003	**-0.005	0.002	-0.002	0.000	-0.002	0.000	-0.001
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
News (α_1)	**0.054**	**0.075**	**-0.115**	**-0.144**	**-0.142**	**-0.105**	**-0.288**	**-0.423**
	(0.177)	**(0.206)**	**(0.683)**	**(0.722)**	**(0.241)**	**(0.265)**	**(0.283)**	**(0.284)**
Adj. R^2	0.47	0.47	0.45	0.44	0.40	0.40	0.33	0.36

Table 2.5: **Spain-specific news effects on Eurozone sovereign CDS spreads.** The table reports the OLS estimation of the CAPM benchmark, Equation 2.2, which controls for European sovereign risk, and the APT benchmark, Equation 2.5, which additionally controls for global sovereign risk and European financial risk. The main parameter of interest is α_1 that measures the impact of Spanish-specific (predominantly bad) news. Autocorrelation and heteroskedasticity robust Newey-West standard errors are shown in parentheses. ** and * denote significance at the 1% and 5% levels.

Pre-announcement: 26.07.2011 – 25.07.2012				
	Denmark	Norway	United Kingdom	Sweden
	CAPM	CAPM	CAPM	CAPM
European	**0.988	**0.691	**0.758	**0.935
	(0.071)	(0.095)	(0.058)	(0.091)
α_0	0.002	0.000	-0.001	0.002
	(0.002)	(0.002)	(0.001)	(0.002)
News (α_1)	**-0.136**	**0.245**	**-0.076**	***-0.239**
	(0.117)	**(0.268)**	**(0.102)**	**(0.096)**
Adj. R^2	0.48	0.25	0.50	0.33
Post-announcement: 26.07.2012 – 25.07.2013				
	Denmark	Norway	United Kingdom	Sweden
	CAPM	CAPM	CAPM	CAPM
European	**0.470	**0.554	**0.421	**0.491
	(0.097)	(0.107)	(0.075)	(0.085)
α_0	**-0.003	-0.001	0.000	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
News (α_1)	**-0.157**	**-0.128**	**-0.182**	**0.068**
	(0.250)	**(0.135)**	**(0.256)**	**(0.217)**
Adj. R^2	0.21	0.24	0.20	0.16

Table 2.6: **Spain-specific news effects on non-Eurozone sovereign CDS spreads.** The table reports the OLS estimation of the CAPM benchmark, Equation 2.2, which controls for European sovereign risk, for 4 stand-alone European countries: Denmark, Norway, Sweden and the UK. The parameter of interest is α_1 that measures the impact of Spanish-specific (predominantly bad) news. Autocorrelation and heteroskedasticity robust Newey-West standard errors are shown in parentheses. ** and * denote significance at the 1% and 5% levels, respectively.

	Pre-announcement 26.01.2011 – 25.07.2012		Post-announcement 26.07.2012 – 25.01.2013	
	CAPM	APT	CAPM	APT
Austria	**0.354	**0.299	0.023	-0.023
	(0.124)	(0.109)	(0.133)	(0.134)
Belgium	**0.472	**0.447	0.029	0.063
	(0.106)	(0.112)	(0.375)	(0.376)
Finland	0.521	0.445	-0.121	-0.054
	(0.324)	(0.379)	(0.142)	(0.142)
France	**0.563	*0.526	0.143	0.250
	(0.214)	(0.220)	(0.227)	(0.271)
Germany	0.224	0.140	-0.106	-0.025
	(0.207)	(0.170)	(0.414)	(0.428)
Ireland	*-1.031	-0.939	0.062	0.155
	(0.481)	(0.494)	(0.202)	(0.240)
Italy	**0.536	**0.459	-0.733	-0.820
	(0.130)	(0.076)	(0.905)	(1.010)
Netherlands	**0.630	**0.560	-0.084	0.008
	(0.214)	(0.197)	(0.309)	(0.321)
Portugal	*-0.462	-0.448	-0.044	-0.305
	(0.231)	(0.249)	(0.220)	(0.229)

Table 2.7: **Spanish-specific news effects over 6-month windows.** The table repeats the analysis of Spain-specific news based on news identification over shorter 6-month windows around the implicit OMT announcement. In each window only the five days of most salient news are considered in the construction of $News_{t,Spain}$. The table reports the OLS estimate of the coefficient of the Spanish-news variable (α_1) in the CAPM Equation 2.2 which controls only for European sovereign risk, and the APT Equation 2.5 that additionally controls for global sovereign risk and European financial risk. Autocorrelation and heteroskedasticity robust Newey-West standard errors are reported in parentheses. ** and * denote significant coefficients at the 1% and 5% level, respectively.

Panel A: Chow breakpoint test			
	Mean level		**F statistic**
	Pre-announcement	**Post-announcement**	***p*-value**
Specified break date: implicit OMT announcement (July 26, 2012)			
PC1	**2.75	**-2.76	****0.00**
PC2	*-0.17	**0.18	****0.00**
Specified break date: formal OMT announcement (September 6, 2012)			
PC1	**2.51	**-3.17	****0.00**
PC2	-0.02	0.03	0.43

Panel B: Bai-Perron multiple breakpoint test				
Number of breaks (null vs alternative)	**Scaled F stat**	**Critical values 1%**	**5%**	**Identified break date**
PC1				
0 vs 1	**25.26****	12.29	8.58	25/07/2012
1 vs 2	3.75	13. 89	10.13	
PC2				
0 vs 1	**35.46****	12.29	8.58	25/07/2012
1 vs 2	3.55	13. 89	10.13	

Table 2.A1: **Breakpoint test for changes in the level of the first two principal components derived from daily Eurozone CDS spreads over the two-year observation window around the implicit OMT announcement.** The table reports the Chow test (Chow, G., 1960; *Econometrica*, 28, 591-501) and Bai and Perron test (Bai, J. & P. Perron, 2003; *Econometrics Journal*, 16, 1-22) applied to the first two principal components, PC1 and PC2, of the daily CDS spreads of 10 Eurozone countries (Austria, Belgium, Finland, France, Germany, Ireland, Italy, the Netherlands, Portugal and Spain) from July 26, 2011 to July 25, 2013. The null hypothesis of the Chow test is no change and the alternative is that a change occurred at a specific date (either the implicit OMT announcement date on July 26, 2012 or the formal OMT announcement date on September 6, 2012). The Bai-Perron test allows for multiple unknown breakpoints. ** and * denote significance at the 1% and 5% levels, respectively.

	News coefficient Pre-announcement α_1^{pre}	Post-announcement α_1^{post}	F statistic *p*-value $(H_0: \alpha_1^{pre} = \alpha_1^{post})$
Panel A. CAPM benchmark			
Austria	0.253	-0.159	0.0595
	(0.178)	(0.147)	
Belgium	****0.415**	-0.163	****0.0026**
	(0.093)	(0.288)	
Finland	-0.062	****-0.312**	0.2281
	(0.158)	**(0.110)**	
France	0.224	-0.218	***0.0361**
	(0.121)	(0.270)	
Germany	-0.130	-0.331	0.4051
	(0.146)	(0.373)	
Ireland	***-0.550**	0.066	****0.0002**
	(0.223)	(0.175)	
Italy	****0.570**	-0.146	****0.0010**
	(0.134)	(0.668)	
Netherlands	0.105	-0.211	0.1209
	(0.143)	(0.230)	
Portugal	****-0.432**	-0.235	0.3954
	(0.166)	(0.244)	
Panel B. APT benchmark			
Austria	***0.347**	-0.236	****0.0044**
	(0.148)	(0.139)	
Belgium	****0.469**	-0.202	****0.0003**
	(0.110)	(0.282)	
Finland	-0.008	****-0.357**	0.0879
	(0.127)	**(0.116)**	
France	***0.286**	-0.269	****0.0069**
	(0.115)	(0.277)	
Germany	-0.044	-0.394	0.1305
	(0.102)	(0.380)	
Ireland	***-0.544**	0.058	****0.0004**
	(0.229)	(0.177)	
Italy	****0.597**	-0.182	****0.0004**
	(0.127)	(0.663)	
Netherlands	0.167	-0.260	***0.0312**
	(0.127)	(0.238)	
Portugal	***-0.406**	-0.255	0.5170
	(0.171)	(0.239)	

Table 2.A2: **Test for significance of differences in news transmission before and after the implicit OMT announcement (daily CDS spreads).** The table reports estimation results for the CAPM benchmark Equation 2.2 in Panel A and the APT benchmark Equation 2.5 in Panel B using as news variables $News_{t,Spain}^{pre} = d_t \cdot \widehat{u}_{t,Spain}^{pre}$ and $News_{t,Spain}^{post} = d_t \cdot \widehat{u}_{t,Spain}^{post}$ where d_t is equal to 1 on the salient news dates and 0 elsewhere. The news impact coefficients are denoted α_1^{pre} and α_1^{post}, respectively. The equations are estimated by OLS using data over the two-year period from July 26, 2011 to July 25, 2013. Autocorrelation and heteroskedasticity robust Newey-West standard errors are in parentheses. ** and * denote significance at the 1% and 5% level, respectively.

	Herding contagion (mean level)		Chow breakpoint test
Country groups	**Pre-announcement**	**Post-announcement**	**F statistic *p*-value**
Panel A. CAPM (breakpoint: implicit OMT announcement on July 26, 2012)			
		20th percentile rule	
Contagion Eurozone set	**0.074	0.010	****0.0001**
Non-contagion Eurozone set	0.009	*0.012	**0.7004**
Stand-alone set	0.008	0.000	**0.1579**
		10th percentile rule	
Contagion Eurozone set	**0.036	0.007	***0.0140**
Non-contagion Eurozone set	0.000	0.003	**0.3168**
Stand-alone set	0.000	0.000	**N/A**
Panel B. APT (breakpoint: implicit OMT announcement on July 26, 2012)			
		20th percentile rule	
Contagion Eurozone set	**0.056	0.010	****0.0021**
Non-contagion Eurozone set	0.006	*0.015	**0.2524**
Stand-alone set	0.000	0.000	**N/A**
		10th percentile rule	
Contagion Eurozone set	**0.032	0.004	****0.0088**
Non-contagion Eurozone set	0.000	*0.006	**0.1563**
Stand-alone set	0.000	0.000	**N/A**
Panel C: Rolling CAPM (breakpoint: implicit OMT announcement on July 26, 2012)			
		20th percentile rule	
Contagion Eurozone set	**0.064	0.013	****0.0012**
Non-contagion Eurozone set	0.003	*0.012	**0.1769**
Stand-alone set	**0.027	*0.019	**0.5645**
		10th percentile rule	
Contagion Eurozone set	**0.040	0.003	****0.0019**
Non-contagion Eurozone set	0.000	0.003	**0.3168**
Stand-alone set	0.004	0.008	**0.5612**
Panel D: CAPM (breakpoint: formal OMT announcement on September 6, 2012)			
		20th percentile rule	
Contagion Eurozone set	**0.067	0.013	****0.0008**
Non-contagion Eurozone set	*0.012	0.009	**0.7080**
Stand-alone set	*0.023	**0.027	**0.7740**
		10th percentile rule	
Contagion Eurozone set	**0.030	0.004	***0.0144**
Non-contagion Eurozone set	0.000	0.003	**0.3168**
Stand-alone set	0.008	0.012	**0.6508**

Table 2.A3: **Test for significance of structural break in mean level of herding index (daily CDS spreads).** The table reports the mean level of the daily herding contagion index and the Chow breakpoint test. The null hypothesis of the Chow test states that there is no change in the level of herding contagion and the alternative states that there is a change on a specified date. Panel A pertains to the CAPM benchmark, Equation 2.3. Panel B pertains to the APT benchmark, Equation 2.4. Panel C pertains to the CAPM benchmark estimated sequentially over 261-day rolling windows. All three Panels A, B and C corresponds to the 2-year period around the implicit OMT announcement on July 26, 2012 (specified break date for Chow test). Panel D corresponds to the 2-year period around the formal OMT announcement on September 6, 2012 (break date for Chow test). N/A indicates that the herding index is zero on all days of the 2-year sample period. ** and * denote significance at the 1% and 5% level, respectively.

	News coefficient (α_1)	
	Pre-OMT **Sept 6, 2011 – Sept 5, 2012**	**Post-OMT** **Sept 6, 2012 – Sept 5, 2013**
Austria	0.199	-0.202
	(0.245)	(0.107)
Belgium	**0.317	-0.385
	(0.093)	(0.203)
Finland	-0.109	-0.243
	(0.218)	(0.155)
France	0.176	-0.212
	(0.170)	(0.321)
Germany	-0.158	-0.510
	(0.205)	(0.360)
Ireland	*-0.642	0.020
	(0.282)	(0.214)
Italy	**0.457	**0.693
	(0.134)	(0.178)
Netherlands	0.068	-0.220
	(0.200)	(0.274)
Portugal	-0.264	-0.489
	(0.164)	(0.338)

Table 2.A4: **Spain news transmission to daily Eurozone CDS spreads before and after the formal OMT announcement.** The table reports the news transmission coefficient obtained through the CAPM benchmark, Equation 2.2, estimated by OLS separately over two 1-year windows before and after the formal OMT announcement on September 6, 2012. Autocorrelation and heteroskedasticity robust Newey-West standard errors are in reported parentheses. ** and * denote significance at the 1% and 5% levels, respectively.

CAPM	APT	Rolling CAPM	Rolling APT
Panel A. Pre-announcement (July 26, 2011 to July 25, 2012)			
10/08/2011	*29/07/2011*	10/08/2011	10/08/2011
23/08/2011	10/08/2011	23/08/2011	23/08/2011
23/09/2011	23/08/2011	*20/09/2011*	*20/09/2011*
14/11/2011	14/11/2011	14/11/2011	23/09/2011
03/01/2012	03/01/2012	03/01/2012	14/11/2011
04/01/2012	04/01/2012	04/01/2012	03/01/2012
02/03/2012	02/03/2012	02/03/2012	04/01/2012
27/03/2012	27/03/2012	27/03/2012	02/03/2012
18/06/2012	18/06/2012	18/06/2012	27/03/2012
09/07/2012	09/07/2012	09/07/2012	09/07/2012
Panel B: Post-announcement (July 26, 2012 to July 25, 2013)			
30/08/2012	30/08/2012	30/08/2012	30/08/2012
17/09/2012	17/09/2012	17/09/2012	17/09/2012
18/09/2012	18/09/2012	18/09/2012	18/09/2012
17/10/2012	17/10/2012	17/10/2012	17/10/2012
18/10/2012	18/10/2012	18/10/2012	18/10/2012
22/10/2012	22/10/2012	22/10/2012	22/10/2012
23/10/2012	23/10/2012	23/10/2012	23/10/2012
04/02/2013	04/02/2013	*29/10/2012*	*29/10/2012*
26/02/2013	26/02/2013	04/02/2013	04/02/2013
22/07/2013	22/07/2013	26/02/2013	26/02/2013

Table 2.A5: **Spain news identification based on daily CDS spreads using alternative benchmarks (CAPM versus APT) and estimation methods (full sample versus sequential rolling-window estimation).** The table reports salient news dates identified through the OLS residuals of the CAPM Equation 2.1, first column; APT Equation 2.4, second column; and via rolling window estimations of both Equation 2.1 and Equation 2.4 in the third and fourth columns, respectively. Spain-specific news dates that differ from those reported in Table 4 of the paper are denoted in italics. Panel A and Panel B pertain to the OLS estimation conducted over two 1-year windows before and after the implicit OMT announcement on July 26, 2012.

	Eigenvalues	Total variation explained (%)		Country loadings (eigenvector: PC1	PC2
Panel A. Pre-announcement (July 26, 2011 to July 25, 2012)					
PC1	3.936	39.36	Austria	0.477	-0.116
PC2	2.225	61.60	Belgium	0.386	-0.287
PC3	2.038	81.99	Finland	0.302	-0.401
PC4	0.989	91.88	France	0.446	0.229
PC5	0.326	95.14	Greece	0.324	0.001
PC6	0.206	97.20	Ireland	-0.003	-0.404
PC7	0.137	98.57	Italy	0.377	0.290
PC8	0.065	99.22	Netherlands	0.112	0.282
PC9	0.052	99.74	Portugal	0.242	-0.133
PC10	0.026	100.00	Spain	0.127	0.589
Panel B: Post-announcement (July 26, 2012 to July 25, 2013)					
PC1	6.526	65.26	Austria	0.277	-0.411
PC2	1.403	79.29	Belgium	0.354	-0.221
PC3	1.144	90.72	Finland	-0.098	-0.669
PC4	0.378	94.51	France	0.295	-0.236
PC5	0.332	97.83	Greece	0.377	0.128
PC6	0.079	98.62	Ireland	0.372	0.156
PC7	0.059	99.21	Italy	0.372	0.109
PC8	0.043	99.64	Netherlands	0.038	-0.443
PC9	0.023	99.87	Portugal	0.379	0.003
PC10	0.013	100.00	Spain	0.371	0.175

Table 2.A6: **Principal component decomposition of daily Eurozone bond yield spreads.** The table reports eigenvalues $\lambda_j, j = 1, ..., 10$ and the proportion of the total variation in the 10 Eurozone bond spreads explained by each principal component given by $\lambda_j / \sum \lambda_j$. The last two columns report the eigenvectors or country loadings for the first and second principal components (denoted PC1 and PC2). Bond yield spreads are defined with reference to Germany.

Panel A: Chow breakpoint test			
	Mean level		**F statistic**
	Pre-announcement	**Post-announcement**	***p*-value**
Specified break date: implicit OMT announcement (July 26, 2012)			
PC1	**2.29	**-2.30	****0.00**
PC2	*-0.30	**0.30	****0.00**
Specified break date: formal OMT announcement (September 6, 2012)			
PC1	**2.03	**-2.57	****0.00**
PC2	-0.00	0.00	0.95

Panel B: Bai-Perron multiple breakpoint test				
Number of breaks (null vs alternative)	**Scaled F statistic**	**Critical values 1%**	**5%**	**Identified break date**
PC1				
0 vs 1	**17.87****	12.29	8.58	25/07/2012
1 vs 2	3.13	13. 89	10.13	
PC2				
0 vs 1	2.40	12.29	8.58	N/A

Table 2.A7: **Breakpoint test for changes in the level of principal components derived from daily Eurozone bond yield spreads over the two-year observation window around the implicit OMT announcement.** The table reports the Chow test (Chow, G., 1960; *Econometrica*, 28, pp. 591-501) and Bai and Perron test (Bai, J. & P. Perron, 2003; *Econometrics Journal*, 16, pp. 1-22) applied to the first two principal components, PC1 and PC2, of the daily bond yield spreads of 10 Eurozone country members (Austria, Belgium, Finland, France, Greece, Ireland, Italy, the Netherlands, Portugal and Spain) from July 26, 2011 to July 25, 2013. Bond yield spreads are defined with reference to Germany. The null hypothesis of the Chow test is no change and the alternative is that a change occurred at a specific date (either at the implicit OMT announcement date on July 26, 2012 or at the formal OMT announcement date on September 6, 2012). The Bai-Perron test allows for multiple unknown breakpoints. ** and * denote significance at the 1% and 5% levels, respectively.

Country groups	Herding contagion (mean level) Pre-announcement	Post-announcement	Chow breakpoint test F statistic *p*-value
	20^{th} *percentile rule*		
Contagion Eurozone set	**0.122	*0.033	****0.0001**
Non-contagion Eurozone set	*0.015	*0.017	0.8752
Stand-alone set	0.004	0.000	0.3187
	10^{th} *percentile rule*		
Contagion Eurozone set	**0.039	0.003	****0.0029**
Non-contagion Eurozone set	0.003	0.003	0.9978
Stand-alone set	0.000	0.000	N/A

Table 2.A8: **Test for significance of differences in mean level of herding index pre- and post-implicit OMT announcement (daily bond yield spreads).** The table reports the mean level of the daily herding contagion index derived from the CAPM benchmark, Equation 2.3, estimated with bond yield spread data over the 2-year sample period around the implicit OMT announcement on July 26, 2012. Bond yield spreads are defined with reference to Germany. For each country, the European sovereign risk index ($\Delta European_t$) is an average of bond yield spreads for the remaining 9 Eurozone countries and 4 stand-alone European countries. The 'contagion' euro countries are Austria, Belgium, France, Italy, and Spain; the 'non-contagion' euro countries are Finland, Greece, Ireland, the Netherlands and Portugal; the stand-alone countries are Denmark, Norway, Sweden and the UK. The null hypothesis of the Chow test says that there is no change in the level of herding contagion and the alternative that there is a change on July 26, 2012. N/A indicates that the herding index is zero on all days of the sample period. ** and * denote significance at the 1% and 5% level, respectively.

	Pre-announcement July 26, 2011 to July 25, 2012	Post- announcement July 26, 2012 to July 25, 2013
Austria	-0.002 (-1.59)	0.002 (1.00)
Belgium	-0.000 (-0.63)	0.000 (0.19)
Finland	-0.001 (-0.77)	0.001 (0.62)
France	0.000 (0.24)	-0.002 (-0.95)
Germany	-0.000 (-0.20)	-0.001 (-0.34)
Ireland	0.001 (0.93)	-0.001 (-0.83)
Italy	0.002 (1.95)	0.000 (0.09)
Netherlands	-0.001 (-1.01)	0.003 (1.73)
Portugal	-0.002 (-2.39)*	-0.003 (-1.23)
Spain	0.003 (2.90)**	0.001 (0.67)

Table 2.A9: **Test for significance of within-Eurozone contagion proxied by the second principal component of daily Eurozone CDS spreads.** The table reports the OLS estimates and Newey-West h.a.c. t-statistics for the second principal component of daily Eurozone CDS spreads (denoted PC2 in the paper) as an additional regressor in the CAPM Equation 2.2 without the Spain-news variable; see Arghyrou & Kontonikas (2012). The principal components are extracted from the correlation matrix of daily CDS spreads of 10 Eurozone countries (Austria, Belgium, Finland, France, Germany, Ireland, Italy, the Netherlands, Portugal and Spain) over the two-year observation period around the informal OMT announcement from July 26, 2011 to July 25, 2013. ** and * indicate significance at the 1% and 5% levels, respectively.

Chapter 3

Domestic banks as lightning rods? Home bias during Eurozone crisis

"The same personal and professional ties that may allow sovereigns to apply moral suasion on domestic banks might also give domestic bankers better information about the likelihood of sovereign default or repayment."
Ethan Ilzetzki, in Economic Policy Discussion Panel (2014)

3.1 Introduction

Can domestic banks act as lightning rods for government bonds in the midst of a financial storm? On the contrary, by now, the deathly loop between sovereign and bank credit risks has been very well documented. Increasing risk pressures in the banking sector may put unnecessary burden on public finances due to potential future bailout costs and negative spillovers to the lending in real economy. In turn, a spike in the sovereign credit risk might trigger a deterioration in the banking sector through losses on banks' government bond holdings and the loss of credibility for future government support (Acharya, Drechsler, & Schnabl, 2014). However, despite this adverse feedback mechanism, the link between governments and their domestic banks may have a silver lining: local banks might have soft information advantages regarding their clients thanks to their "daily exposure to local news stories, first-hand knowledge of the local economy, and personal relationships with key people at the issuing body" (Butler, 2008). During market downturns, such informational advantage might lead them to act as buyers of last resort absorbing the local assets while (potentially uninformed) foreign banks shed their exposures in panic. This is especially possible when markets generally move in a self-fulfillingly pessimistic way ignoring fundamental information regarding the solvency of individual countries, as recently illustrated in the context of the Eurozone crisis (De Grauwe & Ji, 2013; Saka, Fuertes, & Kalotychou, 2015).

In this paper, I present evidence for the latter view. I show that when European banks retreated from the sovereign debt markets of the crisis countries in the Eurozone, they did less so for the countries to which they were informationally closer. To put it another way, *ceteris paribus*, a bank whose home country had better linkages with a target country (measured in terms of geographical distance or cross-border banking activities) increased its relative exposure when that target country was struck by a sovereign debt crisis. This result holds even among the foreign banks and does not depend on the alternative mechanisms such as the risk-shifting tendency of the individual bank, the political strength of its home country or the exchange rate/redenomination risk. Furthermore, the relationship between information and sovereign risk is much stronger in general terms rather than being specific to the episodes of extreme sovereign stress. Hence, I interpret these findings as supportive of the view that informational asymmetries among banks played a key role in the recent fragmentation across Eurozone debt markets.

Figure 3.1 clearly illustrates the puzzling phenomenon that this paper aims to address. Since early 2010, Eurozone banks have lifted up their home bias for sovereign debt, especially in crisis countries. That is, at the peak of the government debt problems, banks started accumulating domestic government bonds. The initial rise and the gradual reversal of this trend -along with the respective bond spreads- is visible only in periphery part of the Eurozone. In contrast, the corresponding bias in core Euro countries seems to have been more or less stable throughout the Eurozone crisis. Intriguingly, the observation still stands in Figure 3.2 even after correcting for how much of the domestic debt the banks should hold in a standard Capital Asset Pricing Model (CAPM).[1]

With the dismal interaction between sovereign and banking crisis in the background, most of the recent literature attributed this observation to the argument of financial repression/moral suasion (Becker & Ivashina, 2014; De Marco & Macchiavelli, 2015; Ongena, Popov, & Van Horen, 2016). In other words, in order to gain relief from crisis and to be able to roll-over their debts, governments may have (implicitly) forced the banks in their jurisdiction to increase domestic sovereign exposures. Pointing to the highly positive correlations between government-relatedness[2] and public bond holdings of the banks, these papers argue that there has been a clear tendency of troubled governments to impose moral suasion on the banks that they can control. From this perspective, the resulting home bias has been mostly involuntary for domestic banks and created an unnecessary burden on the financial health of the banking sectors in crisis countries.

Another competing argument for the repatriation of public debt from non-crisis to crisis countries is based on the assumption that governments would be less willing to default if their debt was held by the domestic agents rather than foreign ones due to the costs such a default would inflict on the domestic economy (Broner, Martin, & Ventura, 2010; Gennaioli, Martin, & Rossi, 2014b). Hence, in the existence of well-functioning secondary markets, sovereign debt should naturally be reallocated back to host countries as domestic agents will attach a higher value to these securities than their foreign counterparts. According to this view, the resulting home bias has been a dark side-effect of secondary bond markets and might have even benefited the creditors if it eventually decreased governments' willingness to default. With respect to this argument, Figure 3.3 illustrates the evolution of the home bias for different types of creditors in the Eurozone periphery and core countries. Though it is clear from Panel A that resident

[1] As discussed later in the Data section, a simple asset pricing model would predict that banks must hold sovereign debt in proportion to the relative weight of their sovereign portfolio in the universe of total sovereign bond holdings.

[2] Either through direct government ownership of the bank or political links in the board of directors.

banks in the periphery accumulated a big portion of domestic sovereign debt, this is hardly true for other non-bank residents in the same countries, which goes against the intuition of Broner et al. (2010) and asks for a further link between resident banks and government debt.

This paper proposes an alternative channel and shows that European banks' increasing sovereign home bias in crisis countries is not so surprising if one takes into account one of the most conventional (albeit lately-forgotten) theories of the home bias in asset markets: informational frictions (Brennan & Cao, 1997; Van Nieuwerburgh & Veldkamp, 2009; Dziuda & Mondria, 2012). As true for risky asset classes (e.g. equity), home bias usually exists when there is an informational advantage in favour of domestic agents. In tranquil periods and well-integrated markets such as in Europe, one would not expect to observe a high level of home bias in risk-free sovereign debt.[3] Nonetheless, in crisis episodes during which government debt gets risky, it becomes crucial to have soft information regarding the true repayment intentions of the government and thus market behaviour might deviate from publicly observed hard information such as debt/gdp ratios or growth rates of individual countries. In that case, uninformed foreign banks may naturally rush to exit these markets in panic, selling most of their exposures to domestic banks at fire-sale prices. Such market trajectory is indeed compatible with the evidence in De Grauwe & Ji (2013) and Saka et al. (2015) who detect the apparent disconnection between bond spreads and the publicly observable hard information (i.e., country fundamentals) during the Eurozone crisis.

By taking a global portfolio approach and using a novel bank-level dataset compiled from various stress-tests, transparency and capital exercises of the European Banking Authority (EBA), I first re-confirm that European banks' home bias increased and sovereign debt was indeed reallocated from foreign to domestic banks at the peak of the crisis. Consistent with Acharya & Steffen (2015) and Crosignani (2015), I also find evidence of risk-shifting behaviour for banks located in crisis countries; however it is also shown that home bias goes much beyond this behaviour. Interestingly, and in contrast with the secondary market theory of Broner et al. (2010), this reallocation does not seem to be visible at all for the domestic agents other than

[3]This can be seen in Figure 3.3 as the average home bias for resident banks in both core and periphery countries is around 15 percent in early 2009 before it doubles in the periphery at the peak of the crisis.

banks. Additionally, I illustrate that, in response to crisis, private forms of debt (retail and corporate) in bank balance sheets have experienced an equally large (if not larger) increase in home bias. This finding is not easy to reconcile with the moral suasion story unless one assumes that, in a sovereign debt turmoil, governments would prioritise pressuring local banks to buy private sector debt more than that of their own. On the other hand, this finding is what one would expect from informationally more sensitive assets (e.g. private debt) if crisis episodes were associated with informational frictions. Finally, I present a direct information channel and demonstrate that European banks headquartered in informationally-closer territories have increased their relative exposures to troubled countries. This effect is robust to controlling for various alternative channels and changing sample compositions.

Sovereign debt crises in a well-integrated monetary union constitutes an ideal setting to isolate the effect of information asymmetry on bank behaviour. Avoiding the cross-country differences in exchange rates, liquidity provision or collateral requirements, this paper presents evidence that information (or the lack thereof) played a key role in recent fragmentation across Eurozone debt markets. Thus, revisiting the initial question, it is possible that domestic banks may have acted as lightning rods collecting the sovereign debt while governments suffered from informational frictions as foreign banks left the market in panic, triggering a financial storm. Despite the so-called doom loop between the two, the relationship between governments and domestic banks may have an underexplored silver lining.

The rest of the paper is organized as follows. Next section briefly outlines the relevant background literature. Section 3 describes the data. The empirical methodology and results are presented in section 4. Final section concludes the paper.

3.2 The Related Literature

3.2.1 Recent home bias in the Eurozone

The main motivation of the paper comes from the recently-aroused interest in academic and policy circles on the causes of rising fragmentation -home bias- across Eurozone sovereign debt markets. One of the earlier contributions by Becker & Ivashina (2014) illustrates the positive association between country-level government ownership in the banking sector and domestic government bond holdings of the banks. They further extend this finding by showing that crisis-country banks with a higher number of government-affiliated board members hold more government bonds in their balance sheets. De Marco & Macchiavelli (2015) follow a similar path to point out that, upon receiving liquidity injections, only politically-related European banks increased their exposure to domestic sovereign debt. Using a proprietary bank-level dataset from European Central Bank (ECB), Ongena et al. (2016) demonstrate that, compared to foreign ones, domestic banks were more inclined to increase their exposures when governments had to roll-over large chunks of outstanding public debt. Many other recent papers confirm these observations (Horváth, Huizinga, & Ioannidou, 2015; Altavilla, Pagano, & Simonelli, 2016b) and conclude that a moral suasion channel was in operation during Eurozone crisis.[4] Nonetheless, none of these studies take into account the possible information channel that might have been active between governments and related banks. By constructing an identification strategy based on the heterogeneity across foreign banks and thus minimising the moral suasion concerns, I contribute to this literature and illustrate that information was a key determinant in recent sovereign debt reallocation across European banks.

Another strand of home bias literature specific to sovereign debt underlines the assumption that it is harder for governments to default on their promises when most of the debt is held domestically. In such a scenario, government would rather choose not to default since the benefits could be offset by its harm on the domestic economy. Hence, in expectation of this by

[4]These findings are not always consistent though. For example, using the same source of data as in Ongena et al. (2016), Altavilla et al. (2016b) find evidence for moral suasion also in core Eurozone countries, which ex-post is hard to reconcile with the observation that these countries did not have any difficulty in rolling over their debts at the time.

local agents, government debt will flow back to the host country during times of rising sovereign risk (Broner et al., 2010). Analysing a vast database covering 191 countries, Gennaioli, Martin, & Rossi (2014a) show empirical patterns consistent with this prediction although they cannot differentiate between domestic and foreign bonds at the bank-level. In a recent paper, Brutti & Sauré (2016) present confirming evidence in the context of Eurozone crisis by demonstrating that reallocation was more intense for sovereign debt than the private one. Furthermore, debt of the crisis governments tended toward those banks whose countries were politically more powerful in the Euro area, implying that debt reallocation was mainly driven to discourage the troubled governments from declaring bankruptcy. By using a dataset covering the entire Eurozone crisis episode for 30 European countries at the bank-level, I complement and challenge these findings: I find that reallocation of sovereign debt indeed occurred in the Eurozone crisis; however this only holds for domestic banks as opposed to other domestic agents, which goes against the earlier prediction of Broner et al. (2010). Furthermore, compared to government debt, retail and corporate debt in bank balance-sheets suffered equally (if not more) from an increase in home bias in response to crisis, which is hard to reconcile with the earlier finding of Brutti & Sauré (2016) who only focus on the first part of the Eurozone crisis in their sample period with a limited coverage of European countries.[5] Finally, I find weak evidence for the argument that political strength of the banks' home countries mattered in debt reallocation and show that my estimations are robust to the inclusion of such variables.

A related literature focuses on the risk-shifting tendency of the undercapitalized banks. According to this argument, banks with low capital ratios prefer high-risk instruments such as the government bonds of crisis countries so that the shareholders would benefit from a resurrection of the country while their losses would be limited in case of a default. (Acharya & Steffen, 2015; Horváth et al., 2015). However, this argument does not necessarily explain why weak banks would especially risk-shift by accumulating domestic government bonds rather than the bonds of other governments struck by crisis. In line with Crosignani (2015), I find evidence that (potentially weak) banks located in crisis countries shift their sovereign portfolios more favourably towards other countries in crisis; but this behaviour is found to be much more prominent when

[5] Their sample period goes from 2007 to late-2011 and is mainly restricted to Eurozone countries with also some non-European countries such as Brazil and Mexico.

it is the domestic government who is in crisis, indicating the need for a further investigation of the link between banks and domestic sovereign bond holdings.

3.2.2 Home bias in other markets

There are many studies exploring the home bias in portfolio holdings of different asset classes. Most of this literature focuses on equity holdings (French & Poterba, 1991) whereas others look at the regional biases in international bond portfolios of various country groups (Lane, 2005). Previous studies mainly revolve around three broad categorical explanations for home bias: exchange rate risk, transaction costs and informational frictions (Coeurdacier & Rey, 2013). In the specific context of Europe, with the increasing financial integration and exchange rate stability over the years, it is reasonable to argue that the most realistic culprit for the recently sky-rocketing home bias would be the informational asymmetries.

Brennan & Cao (1997), for example, model the sensitivity to asset-related news when there is a difference between informational endowments of domestic and foreign agents. They illustrate that, in such a scenario, home bias would be positively associated with the negative news as foreign investors would try to infer the local information from past asset prices and react more to such news.[6] On a similar path, Van Nieuwerburgh & Veldkamp (2009) show that, in the existence of (initially small) informational differences, costly information acquisition process may boost the agents' home bias. Lastly, Dziuda & Mondria (2012) demonstrate that, even in the portfolios of sophisticated institutions such as investment funds, home bias may arise due to the fact that fund investors would be better at judging the performance of fund managers when they invest in local assets rather than foreign ones. Therefore, one might observe home bias even in the portfolios of highly sophisticated institutions such as banks or mutual funds.

Following the intuition that informational frictions might lie behind the widely-observed home bias for various asset classes,[7] many researchers have empirically studied the effects of several

[6] Inspired by Brennan & Cao (1997), there is a stream of studies in the asset-pricing literature that detect the foreign investors' trend-following behaviour. See Choe, Kho and Stulz (1999; 2005); Grinblatt & Keloharju (2000); Froot, Oconnell, & Seasholes (2001); Griffin, Nardari, & Stulz (2004); Richards (2005).

[7] For further evidence on the informational advantage that domestic investors may hold vis-à-vis foreign investors, see Kang & Stulz (1997); Kim & Wei (2002) and Kaufmann, Mehrez, & Schmukler (2005).

forms of informational-distance on portfolio holdings. For instance, Coval and Moskowitz (1999, 2001) find that geographical proximity is crucial for US investors' portfolio composition and the risk-adjusted returns, even within the same country. Grinblatt & Keloharju (2001) discover that investors might be biased towards firms that are close to them in terms of physical location, culture and language of communication. Hau (2001) exemplifies a case in which professional traders located in Germany or in German-speaking cities make more profit in German stocks. Finally, Portes & Rey (2005) conclude that geographical distance matters for cross-border capital flows; however it mostly proxies the effects of other informational variables such as bank branches across countries or telephone call traffic. I borrow the informational distance proxies (such as geographical distance and bank branches) from this literature and contribute to it by extending their evidence to the scope of Eurozone crisis.

3.3 Data Description

The main body of data that I use in the paper comes from various stress-tests, transparency and recapitalization exercises that are undertaken by the European Banking Authority (EBA) over the course of 5 years for a large set of European banks covering 30 members of the European Economic Area (EEA). The first of these disclosures was undertaken by the Committee of European Banking Supervisors (CEBS), which was comprised of senior representatives of bank supervisory authorities and central banks of the European Union and later succeeded by the EBA. Its results were made public by national regulators at the time; however EBA does not provide the related data. Hence, this dataset was obtained from the Peterson Institute for International Economics while all other datasets were acquired from EBA.

Table 3.1 lists these exercises and the disclosure dates for each of them together with how many banks and which information dates were covered. 10 data time-points start from the first quarter of 2010 and goes all the way to the second quarter of 2015, thus covering the start, rise and fall of the Eurozone crisis. Sovereign bond holdings are reported for each data time-point while private credit exposures (corporate, retail, etc.) can be found for 6 of these. In each disclosure, the full country-breakdown of each bank's debt portfolio for up to 200 countries can be found.[8] However, to focus on the debt reallocation across Europe, only exposures to 30 EEA countries are included in the sample.

The main banks involved in the exercises mostly stay the same even though some smaller banks are added and subtracted from one exercise to another. All exposures are consolidated at the parent bank level and each exercise involves banks with at least 65% of the total banking assets in Europe and 50% of the banking sector of each EEA member. Compared to other studies using proprietary datasets from European Central Bank (Ongena et al., 2016; Altavilla et al., 2016b), EBA data cover banks from a wider range of countries (including non-Eurozone) and documents finer granularity in terms of full country-breakdowns of sovereign exposures at bank-level.

I am mainly interested in what portion of a sovereign's total debt is held by a specific bank. Thus the main variable of interest ($SovereignPortion_{b,c,t}$) measures each bank's (b) nominal exposure to a certain country (c) at a certain time-point (t) divided by the total nominal exposure of all the banks for that country at that time. That is;

$$SovereignPortion_{b,c,t} = \frac{NominalExposure_{b,c,t}}{\sum_{b} NominalExposure_{b,c,t}}$$

It is important to note that this measure is independent of the valuation technique used for the bank-level sovereign exposures as long as all the banks apply the same methodology at a given point in time, which is the case in my sample as all disclosures are centrally directed and homogenized by the EBA. This helps me better quantify the relative distribution of sovereign debt across banks. Furthermore, by construction, $SovereignPortion_{b,c,t}$ does not depend on the price changes as these are automatically reflected in all banks' nominal exposures and thus does not change the particular portion that a specific bank holds out of the total debt. Therefore, it constitutes an ideal measure to understand the reallocation of sovereign debt over time.[9]

[8]Except the first disclosure undertaken by CEBS in which only exposures to 30 European countries can be found.

[9]As an alternative dependent variable, I later use sovereign exposures directly in log form [$log(1 +$

In line with the mainstream literature on home bias (Ahearne, Griever, & Warnock, 2004; Coeurdacier & Rey, 2013), I also create an alternative variable that takes into account an optimal portion of sovereign debt that should be held by a bank according to a standard Capital Asset Pricing Model (CAPM). This variable ($SovereignPortionBias_{b,c,t}$) takes the difference between our main variable of interest ($SovereignPortion_{b,c,t}$) and the portion that is suggested by the CAPM model ($SovereignPortionCAPM_{b,t}$).[10] As conventional in the literature, this difference is standardized by the share of other banks' portfolios in the global portfolio ($1 - SovereignPortionCAPM_{b,t}$).[11] That is;

$$SovereignPortionBias_{b,c,t} = \frac{SovereignPortion_{b,c,t} - SovereignPortionCAPM_{b,t}}{1 - SovereignPortionCAPM_{b,t}}$$

where

$$SovereignPortionCAPM_{b,t} = \frac{\sum_{c} NominalExposure_{b,c,t}}{\sum_{b,c} NominalExposure_{b,c,t}}$$

If bias variable $SovereignPortionBias_{b,c,t}$ takes the value of 1, it means all of the country's debt is held by the specific bank, thus perfect home bias. If it is zero, that means the bank holds exactly the portion of the debt suggested by the CAPM model, thus no home bias. For the later section of the study, I create the corresponding variable for retail exposures ($RetailPortion_{b,c,t}$) exactly in the same way as described above and then merge it with the sovereign exposure variable under a single variable name ($DebtPortion_{d,b,c,t}$) where (d) denotes the type of debt in consideration.

To construct the dummy variable $Crisis_{c,t}$, the daily yields of 10-year maturity bonds of 30 European countries are obtained from Datastream.[12] In the next step, I follow a similar approach

$NominalExposure_{b,c,t}$)] and confirm that my findings are unchanged. Results are available upon request.

[10] Notice that CAPM concludes the optimal portion that a bank would hold in an equilibrium setting should depend only on the size of the bank's sovereign portfolio and the size of the global sovereign portfolio. Hence, it does not depend on the specific country of exposure (c).

[11] In unreported estimations (available upon request), I check my results without this standardization and show that none of my findings depend on it.

[12] Bond yields for two countries (Estonia and Liechtenstein) are not available on Datastream; so these obser-

to Brutti & Sauré (2016) and categorize a country as "in crisis" ($Crisis_{c,t}$) if a country is a Euro member and its average daily bond spreads (with respect to Germany) for the previous three months was above 400 basis points.[13]

To be able to differentiate between different types of creditors, a measure of sovereign holdings for non-bank agents is needed. Unfortunately, EBA datasets only contain information about banks. Hence, I resort to a country-level dataset compiled from various national sources by Merler & Pisani-Ferry (2012), which lists the portion of a country's total debt held by its resident banks and non-bank residents.[14] Observations cover 11 European countries[15] at quarterly intervals, starting from 1990s. For consistency, I choose the same period covered by the EBA dataset, from 2010-Q1 to 2015-Q2. For the panel estimations, I create a dependent variable called $DomesticPortion_{c,k,t}$, which measures the portion of a country's (c) debt held by a certain domestic creditor (*k: ResidentsBanks* or *OtherResidents*) at a certain time-point (t).

Finally, to proxy the informational linkages across countries, I construct 3 different variables in line with the previous home bias literature (Portes, Rey, & Oh, 2001; Portes & Rey, 2005). First one, $CrossCountryDistance_{l,c}$, measures the geographical distance (in thousand kilometres) between the capital city of the bank's home country (l) and the capital city of the exposure country (c). Second one, $CrossCountryBranches_{l,c}$, represents the total number of bank branches (in thousands) in the exposure country of the bank which ultimately belong to a bank from its home country.[16] Finally, $CrossCountryMergers_{l,c}$ is the total number of bank mergers (in hundreds) that occurred between the home country and the exposure country in the years starting from 1985 all the way up to pre-crisis period (2008) in Europe. Geographical distance information is derived via MapQuest. The snapshot of banks' branch networks as of February,

vations are dropped from the sample.

[13]Various robustness checks are conducted later by using different crisis definitions (See Section 3.4.6).

[14]Importantly for our purposes, 'other residents' category does not include the public agencies or central banks, so we can assume that these are private non-bank parties/institutions.

[15]These are Belgium, Finland, France, Germany, Greece, Ireland, Italy, Netherlands, Portugal, Spain and United Kingdom. Data for Belgium and Finland can only be found annually; so I linearly interpolated the data to get quarterly values for these two countries.

[16]This variable is created by taking all of the ultimate-parent banks located in 30 EEA countries available in SNL database, independent of whether the bank is included in EBA dataset or not. The purpose here is to capture the non-time-varying banking linkages across countries. Hence, it is important to consider the full sample available rather than only the restricted EBA sample (though results do not depend on this). This data covers 137,284 bank branches in total which is 92% of all bank branches (149,242) in these countries, estimated using World Bank data for 2014 (see http://data.worldbank.org/indicator/FB.CBK.BRCH.P5).

2016, is acquired from SNL Financial[17] while the merger data come from SDC Platinum.

Table 3.2 gives summary statistics for these variables. It is important to note that for *Sovereign-Portion* variable, more than half of the observations contain zero values. However, these are meaningful zeros, implying that the bank does not have any exposure to that sovereign at that certain point in time. When the mean levels across general and domestic samples are compared, one can clearly see the inclination of the banks to hold a higher fraction of the government debt of their own countries. The same can also be said for retail debt (*RetailPortion*). When we compare different debt categories for domestic bank samples, we see that a bank on average holds a higher fraction of its country's retail debt (0.164) than it holds its country's sovereign debt (0.126). This observation is consistent with the information asymmetry view of home bias, predicting that -in general- informationally more sensitive assets (private debt) should suffer more from home bias than other more standardized assets (public debt) would do.

3.4 Methodology & Results

3.4.1 Sovereign home bias during crisis

The first step in my analysis is to capture the effect of crisis on the sovereign home bias of the European banks. For this purpose, I employ a simple *difference-in-differences (DD)* methodology, which assumes that banks' home bias should share a parallel trend in the absence of crisis. A simple look at Panel A of Figure 3.3 confirms the fact that banks' home bias in core and periphery countries moved in tandem with each other prior to the Eurozone crisis. Hence,

[17]Unfortunately, the branch information is not available historically and SNL Financial only provides the most recent data available. However, to the extent that the current data is representative of the non-time-varying cross-country banking linkages, it is reasonable to assume that estimates would not be biased in any particular direction. Additionally, $CrossCountryMergers_{l,c}$ variable overcomes this timing problem by providing pre-crisis picture of cross-country information linkages.

I go on to estimate the following model:

$$SovereignPortion_{l,b,c,t} = \beta_1(Crisis_{c,t} \times Domestic_{l,c}) + \beta_0 Domestic_{l,c} + \theta_{b,t} + \gamma_{c,t} + \varepsilon_{l,b,c,t} \tag{3.1}$$

where (l) denotes the home country of the bank, (b) identifies the specific bank, (c) is for the country of exposure and (t) specifies the time dimension. All variables are constructed as previously explained in the Data Description section. Controls include a broad set of fixed-effects at the levels of *Bank*Time* $(\theta_{b,t})$ and *ExposureCountry*Time* $(\gamma_{c,t})$. Thus, the model controls for the overall effects of the crisis both at the home country (since banks never change their home country) and exposure country levels and *Crisis* dummy can only enter the regression in an interaction term. Additionally, $Domestic_{l,c}$ is a dummy variable which is equal to 1 if the bank's headquarters are located in the country of exposure (i.e., l=c). In this model, β_0 should give us an idea about the general level and significance of the sovereign home bias in European banks and β_1 measures the additional effect of the crisis on this home bias. Same model is also estimated for the alternative dependent variable with CAPM adjustment $(SovereignPortionBias_{l,b,c,t})$.

Results are presented in Table 3.3. Columns I-II and V-VI confirm the previous literature that banks do have home bias in their sovereign debt holdings. It is economically meaningful as well at a level around 0.126. Given that average sovereign holding in our sample is around 0.01, this finding clearly illustrates that a bank holds a much bigger portion of a country's debt when it comes to its own country. Columns III-IV and VII-VIII of the same table ratifies another observation that is compatible with the previous literature: the sovereign home bias of domestic banks increases during times of crisis (Gennaioli et al., 2014a; Brutti & Sauré, 2016). The effect is economically huge: the portion of a country's debt held by a representative domestic bank almost doubles in response to crisis.[18] Hence, the link between a sovereign debt crisis and the absorption of government bonds by the domestic banks is arguably established at this stage.

[18] This result is also compatible with the recent bank lending literature showing that, during a financial crisis, international banks demonstrate a stronger home bias in terms of syndicated loan issuance (Giannetti & Laeven, 2012) or cut credit less in markets that are geographically close (De Haas & Van Horen, 2013).

However, with this simple observation, it is not yet possible to differentiate among alternative channels that may lead to that rising home bias.

3.4.2 Risk-shifting in crisis-country banks

Findings in Table 3.3 are compatible with information asymmetry, secondary markets or moral suasion stories of the home bias. One may also argue that banks in crisis countries are especially weakly-capitalised, which drives them to invest more in their home country bonds to benefit from shifting the risk onto their creditors (Crosignani, 2015). However, if this is the case, one would expect these banks to also invest in other high-risk countries.

To check for the risk-shifting tendency of banks located in troubled countries, I estimate the following model and separate the home bias phenomenon from the risk-shifting story:

$$\begin{aligned} SovereignPortion_{l,b,c,t} &= \beta_2(Domestic_{l,c} \times Crisis_{c,t} \times StressedBank_{l,t}) \\ &+ \beta_1(Crisis_{c,t} \times StressedBank_{l,t}) + \beta_0 Domestic_{l,c} + \theta_{b,t} + \gamma_{c,t} + \varepsilon_{l,b,c,t} \end{aligned} \tag{3.2}$$

where $StressedBank_{l,t}$ is a dummy variable representing those observations in which the home country of the bank (l) is considered to be in crisis at a certain time (t). All other variables are constructed as previously explained. Due to time-varying fixed effects at the bank and exposure country levels, *Crisis* and *StressedBank* dummies can only enter the regression in interaction with other variables.[19]

Model 3.2 checks for risk-shifting behaviour of (potentially weak) banks located in crisis countries, in line with Crosignani (2015). If the rising home bias in crisis countries is mainly due to risk-shifting, one should observe a similar tendency of crisis-country banks to shift their portfolios towards all crisis countries no matter if it is domestic or foreign. This is captured by β_1. On the other hand, β_2 measures the additional effect of crisis on domestic exposures that

[19]For conciseness, additional two-way interactions of *Domestic*Crisis* and *Domestic*StressedBank* are dropped from the estimation since coefficients are both insignificant and their inclusion does not change the results in any meaningful way.

cannot be explained by the general level of risk-shifting in these crisis-country banks.

Columns I and III in Table 3.4 confirm the earlier predictions by showing that crisis-country banks actually expand their relative exposure to all other crisis countries, potentially risk-shifting. However, as illustrated in columns II and IV, this behaviour is much heavier for the home exposures of these banks, thus indicating that risk-shifting may contribute to the rising home bias in crisis countries but is not even nearly a sufficient explanation. The magnitude of response to a crisis in home country is more than tenfold higher than that to a crisis in a foreign country (0.104 vs 0.008). Indeed, banks located in troubled countries have a special preference for their own government bonds which goes much beyond their risk-shifting incentives.

3.4.3 Bank vs. non-bank domestic creditors

As discussed previously, secondary markets hypothesis states that the increase in banks' sovereign home bias might be related to the presumption that government bonds would be more valuable (due to governments being less willing to default) when they are held domestically. Thus, in the existence of well-functioning secondary markets, debt would naturally flow from foreign to domestic agents. In addition, if redenomination (Eurozone break-up) risk was particularly high for crisis countries, this may have pushed up the selling pressure especially for the foreign investors since they may risk ending up with a currency mismatch between their assets and liabilities in case of a crisis country declaring its exit from the Eurosystem (Battistini, Pagano, & Simonelli, 2014).

However, neither of these channels is specific to banks and, if they were prominent, one could expect to see a rising home bias not only for domestic banks but also for other types of agents in crisis countries. Hence, I differentiate the effect of the crisis on the home bias of different domestic agents operating in the same economy. For this purpose, I use the Bruegel dataset at country-level and estimate the following model:

$$DomesticPortion_{c,k,t} = \beta_1(ResidentBanks_k \times Crisis_{c,t}) + \lambda_{k,t} + \gamma_{c,t} + \varepsilon_{c,k,t} \quad (3.3)$$

where (c) is for the country, (k) is for the creditor type and (t) is for different quarters of the year. $ResidentBanks_k$ is a dummy variable that is equal to 1 if the creditor (k) of the country is its resident banks and zero if it is other private non-bank residents. All other variables are constructed as previously explained. Controls include *Creditor*Time* ($\lambda_{k,t}$) and *Country*Time* ($\gamma_{c,t}$) fixed effects, which should absorb all the time-varying country and creditor characteristics.[20] The coefficient of interest is β_1, which signals whether or not domestic banks behaved somewhat differently compared to other domestic agents.

Table 3.5 compares the responses of two types of domestic agents during crisis. Although statistically insignificant, Column I indicates that the crisis leads domestic agents to decrease their home bias on average, which is counter-intuitive with respect to our earlier finding. However, when I separate the differential response of bank creditors, column II confirms that resident banks in crisis countries are more likely to increase their home bias whereas other non-bank residents seem to have moved in the opposite direction. This finding holds even when time-varying shocks for each creditor are accounted for (columns III-IV) together with national shocks that may impact both creditors at the same time (column IV). Hence, this finding goes against the secondary-markets hypothesis arguing that, during crisis times, government debt should flow back to the home country irrespective of the resident type since government would then prefer keeping its promise not to harm the domestic economy. Although it could be argued that governments "care" more about the banking sector and hence it should be more reasonable that sovereign debt flows to resident banks, one would still expect to see a somewhat positive response for other non-bank residents as well, which does not seem to be visible at all in our findings.

Furthermore, even though the Eurozone could be said to have come to the verge of a break-up in the midst of the crisis, it is not easy to conclude that redenomination risk was instrumental in banks' sovereign exposure behaviour since it does not seem to have affected other types of investors resident in the same troubled countries. On the other hand, it is noteworthy that, since different investors may tend towards different kinds of domestic assets to hedge for the

[20]Notice that with full saturation of fixed effects, *ResidentBanks* and *Crisis* dummies can only enter the regression in interaction form.

currency risk, the ideal setting to test for the redenomination risk would be the case in which we could see the creditor decomposition (bank vs non-bank) of all asset classes rather than only that of sovereign debt. However, in the absence of a more comprehensive dataset and a legitimate argument for why non-bank residents should especially avoid hedging via government bonds, it is safe to say that redenomination risk was not substantial.[21]

3.4.4 Sovereign vs. private debt home bias

Most of the recent literature has focused on the European banks' sovereign home bias although this behaviour might have been just a sub-observation of a more general phenomenon. Thus, I would also like to compare the effect of the crisis on home bias across various assets classes held by the European banks. For this purpose, I use a more generalized model as in the following and differentiate the home bias of two debt types in both normal and crisis times:

$$\begin{aligned} DebtPortion_{d,l,b,c,t} = {} & \beta_3(Sovereign_d \times Crisis_{c,t} \times Domestic_{l,c}) + \beta_2(Crisis_{c,t} \times Domestic_{l,c}) \\ & + \beta_1(Sovereign_d \times Domestic_{l,c}) + \beta_0(Retail_d \times Domestic_{l,c}) \\ & + \zeta_d + \theta_{b,t} + \gamma_{c,t} + \varepsilon_{d,l,b,c,t} \end{aligned} \tag{3.4}$$

where $Sovereign_d$ and $Retail_d$ are dummy variables indicating the respective asset classes. All other variables are constructed as previously explained.[22] The coefficients β_1 and β_0 should give us an idea about the home bias in these different asset classes in general. β_2 reflects the overall effect of the crisis on the home bias for both asset classes and β_3 should tell us if the increase in home bias was stronger for sovereign debt, as would be suggested by the other competing theories of home bias (moral suasion and secondary market theory).

To get a better sense of whether sovereign debt was the only asset that has suffered from home bias during crisis, Table 3.6 draws the following comparison. Columns I and V confirm that

[21] Also see the extra analysis undertaken in Section 3.4.6 to control for redenomination risk.

[22] To focus on the main coefficients of interest, the two-way interaction of *Sovereign*Crisis* is dropped from the estimation since the coefficient is statistically insignificant and its inclusion does not change the results in any meaningful way.

there is a significant home bias across both assets classes together. When I separate the home bias for different assets, columns II and VI show that the magnitude of general home bias for retail debt (0.167) is more than 30 percent higher than the one for sovereign debt (0.126) and the difference between these two coefficients is statistically significant, which is perfectly in line with the information asymmetry theory of home bias. Compared to standard products such as government securities, informationally more sensitive assets such as retail debt should be held more by the domestic agents who have an advantage in reaching the relevant information for such assets (Portes et al., 2001; Portes & Rey, 2005).

The remaining columns in Table 3.6 provide even more interesting results. Columns III and VII show that crisis has a positively significant effect on home bias for both asset classes. Columns IV and VIII shed light on the additional response of the sovereign debt to crisis, but there seems to be none. At best, this additional effect is negative (-0.026, though not statistically significant), meaning that it is the retail debt that may suffer more intensely from home bias in times of crisis. Obviously, this finding is again consistent with the expectation that, during crisis episodes that are usually associated with rising informational frictions, informationally sensitive assets should experience a much larger reallocation from foreign to domestic agents. For robustness, the same analysis is repeated with the corporate debt in Table 3.A1. Not surprisingly, results are very much in line: in general, European banks have a higher home bias in their corporate exposures and, compared to sovereign debt, this bias rises at least equally in response to a crisis in a country.[23] Overall, it seems that the recent sovereign debt reallocation in Europe could be a part of a more general phenomenon (such as informational frictions) that may have influenced all asset classes simultaneously.

3.4.5 Effect of informational distance on banks' sovereign exposures

It is already well established in the literature that the proximity to the borrower matters for the banks' lending behaviour and it usually determines the amount of soft information that the

[23] In another unreported robustness check, I repeat the analysis by only including EBA disclosure dates in which both types of debt exposures were disclosed (6 dates; see Table 3.1) and find that results are unchanged.

bank could gather to serve its customers.[24] Of course, one could think that the government bond markets are not necessarily the kind that soft information would matter the most. Indicators (such as tax revenue or fiscal balance) showing the strength of government's ability to pay back its debt are publicly available and easily accessible by market participants. Nevertheless, an interesting feature of the government debt markets is that, while corporate bankruptcy is always about the (in)ability of a company to repay, a sovereign default is -in most cases- a political decision and directly related to the degree of governing party's willingness to cut back government spending or increase tax rates. This crucial difference between corporate and sovereign debt arises due to the lack of a legal mechanism to enforce repayment on sovereigns (Panizza, Sturzenegger, & Zettelmeyer, 2009) and makes it especially important in times of stress to have insider information on government's willingness to honour its promises or country's political capacity to endure further budget cuts. Such soft information could be obtained via domestic banks' local/political connections or simply being more familiar with the country, its daily news and local economic and political climate.[25] In that respect, Butler (2008) illustrates a case in which local investment banks underwriting municipal bonds have comparative advantage in accessing and assessing soft information, especially when the bond is risky.

What is then so special about domestic banks over other types of domestic agents? First of all, domestic banks are the main players in the government debt markets. Figure 3.3 clearly illustrates that even before the crisis in Euro periphery, domestic banks held almost as much sovereign debt as that of all other domestic agents combined. This could give the banks a comparative edge in pricing of government securities.[26] Secondly, banks are natural information-gatherers for their economies. They transact with almost every sector of the domestic businesses and gain in-advance information on how well the overall economy may perform over the coming months/quarters, which would have a tremendous effect over government's ability to raise tax revenues and pay back its debt. Thirdly, banks are the agents with the greatest access to liq-

[24] See, among many others, Mian (2006), Alessandrini, Presbitero, & Zazzaro (2009) and Agarwal & Hauswald (2010).

[25] Here, I interpret familiarity as an accumulated informational advantage rather than a behavioural bias although the previous literature is somewhat ambiguous on this (see Huberman, 2001).

[26] Home bias might also arise simply due to domestic banks' responsibility to act as primary dealers or market makers in the sovereign debt markets. Ongena et al. (2016) provide contrary evidence that most of the market makers in periphery countries during crisis were foreign banks and this did not have any effect on domestic banks' home bias.

uidity (via central banks) in times of financial crises. Hence, in a liquidity crunch, governments may find it easier to signal their intentions/plans to local banks than any other local agent. Last but not least, public ownership in the banking sector is still more common relative to other sectors, which does not only give the government a tool to pressure banks, but also opens the possible communication channels that can transmit crucial soft information during times of sovereign stress (Ilzetzki, 2014).

In light of the above discussion, I expect cross-country informational linkages to be important for the European banks' sovereign exposures both at home and abroad. Figure 3.4 pictures the bank branch network in 30 EEA countries and it seems that Eurozone crisis struck the countries located in the outer sphere of this network, which may have caused these sovereigns to be especially susceptible to informational frictions. Additionally, larger nodes in crisis countries imply that their banking sector is dominated by the domestic banks which might be the reason why debt flew back to these countries in large quantities. Figure 3.5 with bank merger network tells more or less the same story. Hence, I go on to formally estimate the effect of informational distance on European banks' behaviour towards crisis countries:

$$\begin{aligned} SovereignPortion_{l,b,c,t} = \beta_1(CrossCountryDistance_{l,c} \times Crisis_{c,t}) \\ + \theta_{b,t} + \gamma_{c,t} + \mu_{l,c} + \varepsilon_{l,b,c,t} \end{aligned} \quad (3.5)$$

where, in addition to the previous ones, I also include fixed effects at the level of interaction between home country and exposure country ($\mu_{l,c}$) so that all non-time-varying structural cross-country linkages could be implicitly controlled. Hence, $CrossCountryDistance_{l,c}$ only enters the regression in interaction. Alternatively, I use $CrossCountryBranches_{l,c}$ and $CrossCountryMergers_{l,c}$ as proxies that would capture the informational channel between countries.

Table 3.7 presents the effects of informational distance on banks' exposures to crisis countries. First thing to notice is that the explanatory power (adjusted-r-square) of the model massively increases due to the fixed effects at HomeCountry*ExposureCountry level, implying that cross-country linkages matter substantially for the European banks' sovereign portfolios. Although geography could be thought of as a noisy proxy for informational linkages across countries,[27] especially in Europe given the fully open borders and easy transportation, columns I and IV illustrate that physical distance has a significant negative effect on bank exposures in times of crisis. One standard deviation increase in distance (0.83) lowers a bank's sovereign portion holding of a crisis country by almost one percent. Given that the sample mean of sovereign portion is 0.012 in the full sample, the effect is quite sizeable and economically meaningful. Similarly, branch and merger connections, which are better proxies for information, are also significant and positively associated with the banks' exposures to crisis countries (see columns II-III-V-VI).

However, full sample in these estimations also contain domestic observations, which are highly correlated with information variables; and thus may bias the results if there is a moral suasion or secondary market effect in these domestic observations. Thus, I take a much more conservative approach and drop all the domestic observations from the sample. All remaining observations denote the foreign exposures of the banks. Notice that this is a very conservative approach in the sense that the concept of informational linkages that this paper has argued for so far has mostly emphasised the link between governments and their domestic banks. Furthermore, there is the possibility of "reverse moral suasion" on foreign banks, in which the national regulators may have forced their banks to specifically drop their exposures to the troubled countries (Ongena et al., 2016). In that case, such pressure would be most pronounced for better-connected banks which, even before the crisis, may have had higher exposures to crisis countries. Thus, focusing only on foreign bank observations would severely underestimate the importance of information channel during crisis.

With the above concerns in mind, columns VII-XII in Table 3.7 report the results for foreign-banks sample and show that the effect of geographical distance becomes statistically indistinguishable from zero, which is not surprising given the noisy nature of this proxy. On the other hand, branch and merger variables are still influential on the behaviour of foreign banks towards

[27]One could also think that distance should be positively associated with asset holdings since more distant countries would offer better diversification benefits due to the lower correlation in business cycles across countries (Portes & Rey, 2005).

crisis countries. Although standard errors get relatively larger in the subsample, magnitude of the coefficients goes up in the meantime. One standard deviation increase in *CrossCountry-Branches* (1.86) shoots up the sovereign portion by more than 1 percent, which is sizeable given the sample average of 1.2 percent for *SovereignPortion*. Corresponding one-standard-deviation effect for the *CrossCountryMergers* variable is around 0.8 percent, still sizeable but lower than the one for branches. These findings confirm the main prediction of this paper: European banks located informationally-closer to troubled countries have relatively increased their exposures to these sovereigns during Eurozone crisis.

One potentially confounding factor might be the possibility that countries struck by crises may also be better connected to each other. In such a case, information variables may capture the effect of risk-shifting which was documented in Table 3.4. To control for this possibility, I include *StressedBank*Crisis* interaction as an additional control in Equation 3.5. Table 3.8 updates the results with this extra "risk-shifting" control and it turns out to be significant only in the full sample. Furthermore, none of the previous findings regarding information effects change in any meaningful way.

A further criticism might be due to Brutti & Sauré (2016) who argue that political strength of the bank's home country might be important for sovereign debt reallocation. Since banks from politically influential countries may feel more confident about enforcing repayments, they may tend to buy foreign government bonds while others are selling. If large and politically strong Eurozone countries have also banking systems closely-connnected to the troubled countries, then I might simply be capturing this political strength effect rather than the informational-closeness. To incorporate this into my framework, I construct two alternative control variables that Brutti & Sauré (2016) propose as a measure of political strength. One is the share of total Eurozone GDP that the home country of the bank produces, namely $Euroshare_l$; and second is simply a dummy for the German banks, $GermanBank_l$, since Germany has been arguably the most important decision-maker in Eurozone debt renegotiations so far.

Table 3.9 and Table 3.10 report the results with these two variables in addition to the previous control for risk-shifting. It is clearly evident from both tables that the effect of information variables does not depend on these alternative channels and robust to controlling for them in various ways. When it comes to individual controls, they usually have positive coefficients as expected; however there is no statistical evidence that either risk-shifting or political strength was instrumental in the sovereign exposures of foreign banks in Europe. Overall, independent of alternative explanations, findings in this section constitute a direct and strong evidence for the view that information channel played a key role in the recent sovereign debt reallocation across Europe.

3.4.6 Further analysis and policy implications

Eurozone crisis has been characterized by sudden changes in periphery countries' bond prices and various policy responses in the face of rising market speculation. Especially the actions taken by European Central Bank (ECB) seem to have been instrumental in preventing the self-fulfilling market sentiments (Saka et al., 2015). It is also possible to argue that cheap financing provided by the ECB to commercial banks in the form of long term refinancing operations (LTROs) may have led some of these banks to increase their exposures to risky government bonds. Given that periphery country banks were more likely to be undercapitalised, this might be the reason behind the rising domestic exposures of those banks to their own governments. However, this logic skips the fact that there were various countries in crisis at the same period and cheap financing together with risk-shifting tendency would lead these banks to also increase their exposures to other crisis-countries, for which I find only weak evidence in my data and show that information channel is independent of such motives.

One further extension of empirical strategy could be to check whether previous results might be driven by real exchange rate risk. Since my sample includes banks located in non-Eurozone countries such as HSBC in United Kingdom or Danske Bank in Denmark, differences in banks' currency exposures may affect their hedging strategies via government bonds. To account for this scenario, I construct a subsample composed of only banks headquartered in Eurozone countries. Hence, all banks in this subsample use Euro as the main currency and, given that inflation differences were minimal across European economies during my sample period, these banks should on average face similar real exchange rate risks towards other countries. Table 3.A2 updates all of the main results with this subsample. As can be clearly seen, there is no material change in any of my previous findings.

Despite accounting for differences in real exchange rates, one can still argue that there was a substantial redenomination risk within the Eurozone. As some countries may have started planning to get out of the monetary union, banks may have optimally started selling government bonds to hedge against such countries in order to avoid potential currency mismatches after a Eurozone break-up. However, it is not straightforward to list which countries actually planned to exit or which countries were perceived by the market as potentially preparing to exit. Thus, to test whether such motives are important in explaining my results, I follow a strategy similar to Brutti & Sauré (2016) and drop from my sample all the bank exposures towards Greece. It can be easily argued that, if any break-up expectations were evident during the sample period, this would be especially valid for Greece as it has been the country that suffered the most from Eurozone crises both economically and politically (Lane, 2012). Therefore, Table 3.A3 presents the results with Eurozone banks, but this time without any Greek exposures. Again, there does not seem to be any significant change in my main findings, supporting the notion that they are not driven substantially by the redenomination risk.

Another robustness check that comes to mind is to test whether the estimations are robust to reasonable changes in crisis definition. Table 3.A4 and Table 3.A5 present all the main results with crisis thresholds of 300 and 500 basis points for bond spreads instead of my main definition of 400bps. All the main results still hold although, expectedly, they get stronger with a lower threshold as this increases the size of crisis-country observations in the sample and weaker with a higher threshold as this decreases the number of crisis-countries.[28]

Furthermore, choosing an arbitrary threshold for crisis dummy restricts the relationship between information channel and sovereign risk to be non-linear. That is, we assume that information channel gets activated only at the peak levels of sovereign stress (i.e., crises). However, as mentioned earlier, information asymmetry theory of home bias should be applicable for risky assets

[28]Note that Spain and Italy are never in crisis with the higher threshold of 500bps.

in more general terms (both in tranquil and stressful times). Hence, one would expect that even for non-crisis countries, information channel should intensify at relatively higher levels of sovereign risk. To check for this possibility, I get rid of the crisis dummy and instead directly use bond spreads in my estimations. Results are illustrated in Table 3.A6 and strongly support the latter assumption: two-way interaction of *CrossCountryBranches*ExpSpread* is statistically significant at 1 percent level in foreign-bank sample with any combination of controls. This observation supports the intuition that information matters even for the tranquil countries/times and informationally-closer foreign banks absorb more of the sovereign debt as the default risk of a country rises in general. On the other hand, previous literature states that bond spreads may be influenced by factors other than the default risk, such as market liquidity or inflation expectations. Hence, a less noisy proxy for the true default probability of the government could be CDS spreads which are less likely to be affected by such contract-specific or market-specific factors (Longstaff et al., 2011). Therefore, in Table 3.A7, I repeat the same exercise by replacing bond spreads with CDS rates. All previous predictions, especially the ones on information channel, are again confirmed and leave no doubt behind regarding the general role information plays at higher levels sovereign risk.

These findings clearly challenge the recent literature of Eurozone studies focusing on the rising home bias in sovereign debt. One might argue that, in the age of technology and well-integrated markets such as in Europe, information must be cheap to attain; so huge asymmetries in the markets should not arise. However, theoretical literature illustrates that even initially-small differences in informational standings of domestic and foreign agents may lead them to focus on these differences rather than spending effort to get the information related to foreign assets (Van Nieuwerburgh & Veldkamp, 2009). Furthermore, recent studies on the sovereign credit risk prices in the Eurozone provide evidence that, at the peak of the crisis, there were great discrepancies between bond yields (or CDS spreads) and macro fundamentals of the countries in the Euro periphery, which is interpreted as a sign of market panic (De Grauwe & Ji, 2013; Saka et al., 2015). In such circumstances, it is not unreasonable to expect domestic or government-related banks to benefit from their superior informational position and collect sovereign bonds while foreign banks were leaving the debt markets in rush. In fact, some studies already show that banks that loaded up periphery country bonds during crisis benefited from this as the crisis pressures eased (Acharya, Eisert, Eufinger, & Hirsch, 2016b).

Another counter-argument might be that part of the literature shows how increasing sovereign exposures had negative spillovers on European banks' private lending, which may signal that sovereign exposure behaviour was partly involuntary for these banks (Acharya, Eisert, Eufinger, & Hirsch, 2016a; Altavilla et al., 2016b; Popov & Van Horen, 2015). Still, Broner, Erce, Martin, & Ventura (2014) clearly illustrate that, in the existence of frictions in financial markets, sovereign exposures may crowd out private lending without necessarily implying an involuntary or forced behaviour on the part of banks. Additionally, some recent studies that argue in favour of moral suasion do not find any negative effect of sovereign exposures on private lending (Ongena et al., 2016).

As a key policy conclusion: if information channel gets active between governments and domestic banks in the midst of a crisis, this may be considered as a stabilizing force compared to a situation where even domestic banks would rush out of the market and governments would find it impossible to roll-over their debt. Therefore, the close link between governments and their domestic banks may create positive externalities in terms of mitigating the effects of sudden stops and preventing possibly inefficient sovereign defaults. Nevertheless, policy discussions have so far emphasised shifting the regulatory power from national to supranational institutions to avoid moral suasion or coming up with various innovations of debt issuance in order to cut off the diabolic loop between sovereigns and their banks (see Brunnermeier, Garicano, Lane, Pagano, Reis, Santos, Thesmar, Van Nieuwerburgh, & Vayanos, 2016). Taken at face value, my results imply that these precautions would not be sufficient to prevent the rising home bias problem (to the extent that it constitutes a problem) during crises. Instead further policy discussions may also focus on increasing transparency in the sovereign debt markets especially in times of crisis or encouraging more cross-border banking activities to improve informational ties across countries.

3.5 Concluding Remarks

Deviating from the growing literature on home bias in European banks' sovereign debt portfolios, this paper argues that recent rise in this bias is not a surprising phenomenon if one takes into account one of the most conventional (albeit lately-forgotten) theories of the home bias in asset markets: informational asymmetries.

By taking a global portfolio approach and using a novel bank-level dataset compiled from various stress-tests, transparency and capital exercises of the European Banking Authority (EBA), I show that home bias increased and sovereign debt was indeed reallocated from foreign to domestic banks at the peak of the crisis. Though it cannot fully explain the rising home bias in response to crisis, risk-shifting tendency of crisis-country banks seems to have a contribution. In contrast with the secondary market theory of sovereign home bias, this reallocation was not visible at all for the domestic agents other than banks, which is not incompatible with the information channel of this paper given the relative advantages that banks enjoy in government bond markets. Additionally, I demonstrate that, in response to crisis, private forms of debt (retail and corporate) in bank balance sheets have experienced an equally large (if not larger) jump in home bias than the one observed for public debt, which is slightly at odds with the moral suasion story unless one assumes that government's priority for moral suasion would be on private sector debt during a sovereign debt crisis. On the other hand, this finding is what one would expect from less transparent assets (such as private debt) if crisis episodes were associated with informational frictions. Finally, I present a direct information channel and demonstrate that foreign banks that are informationally better-linked to crisis countries have relatively increased their exposures during crisis. This effect is independent of the previous channels proposed in the literature, not driven by exchange rate or redenomination risk and more strongly exists in general terms rather than being specific to the episodes of extreme sovereign stress. Hence, this paper mainly contributes to the extant empirical literature on the role informational asymmetries play in asset markets and extends it to the context of government bonds and high risk periods.

Taken at face value, my results have direct implications for policymakers. To the extent that information was at play during recent crises, increasing home bias in bank portfolios may have been a stabilising force rather than a destabilising one. Despite the well-illustrated adverse mechanism between governments and banks, the possibility that domestic banks acted as a buyer of last resort may have helped many of the crisis-governments to continue borrowing from the market and service their maturing debt payments. In the absence of a national central bank acting as a lender of last resort, this may have mitigated the sharp effects of a sudden stop triggered by foreign banks who potentially had very little soft information about the default probability of the governments. In that case, future policy discussions may benefit from focusing on increasing transparency in the sovereign debt market and encouraging cross-border banking activities to mitigate the rising home bias in advance of the next Eurozone crisis.

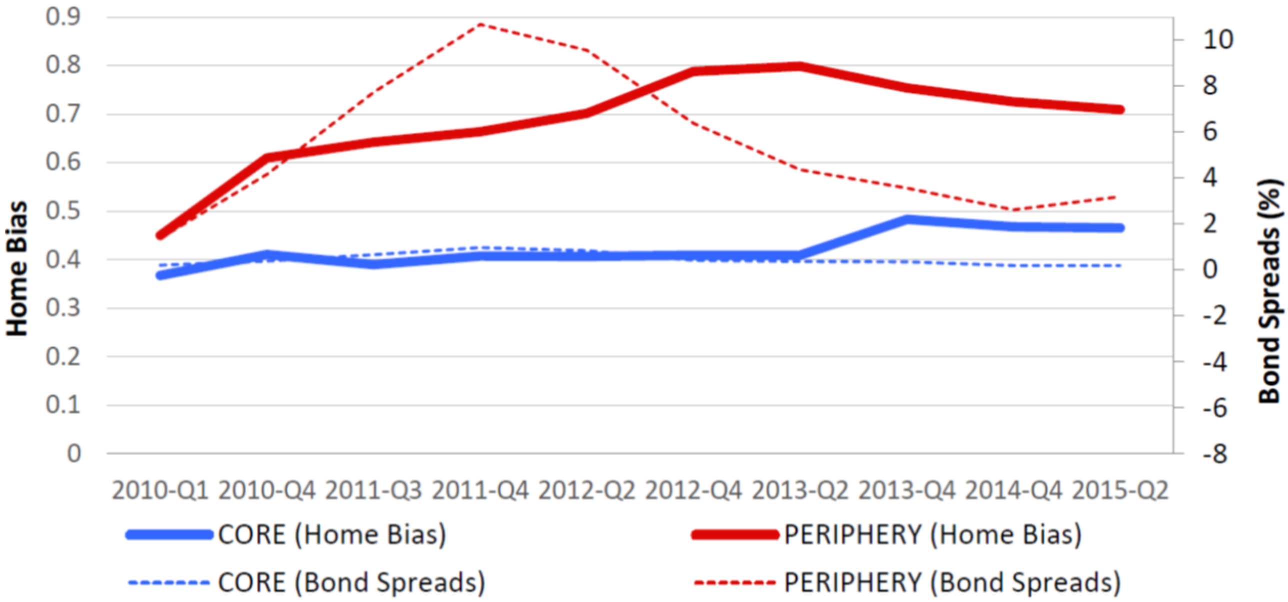


Figure 3.1: **Home bias in core and periphery Euro countries during crisis.** The graph shows simple country averages of home bias and bond spreads for each country group (core vs. periphery). Home Bias is defined as the portion of the total sovereign debt of a country held by its domestic banks. Bond Spreads are computed as the average daily bond spreads for a country (with respect to Germany) over the 3-month period before each observation date. Sovereign bond exposure data come from various stress-tests, transparency and recapitalization exercises undertaken by the European Banking Authority (EBA) and include 10 observation dates from 2010-Quarter1 to 2015-Quarter2 (see Table 3.1). Bond yields are obtained from Datastream. Core (non-crisis) countries: Austria, Belgium, Finland, France, Germany and Netherlands. Periphery (crisis) countries: Greece, Ireland, Italy, Portugal, Spain.

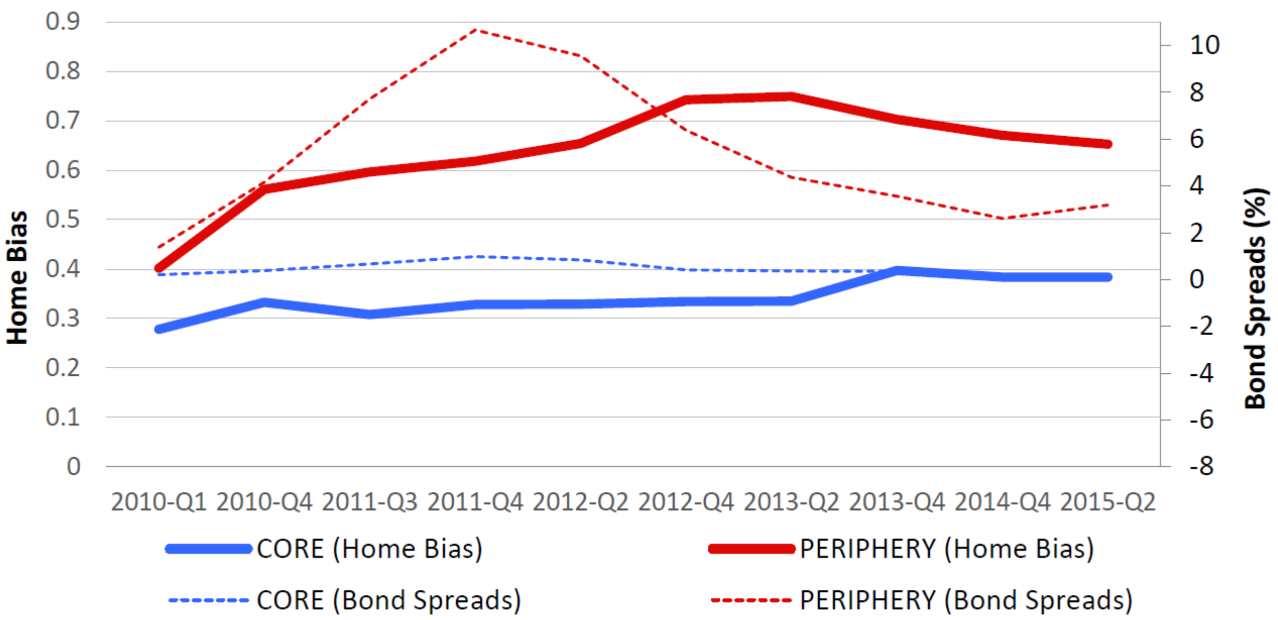
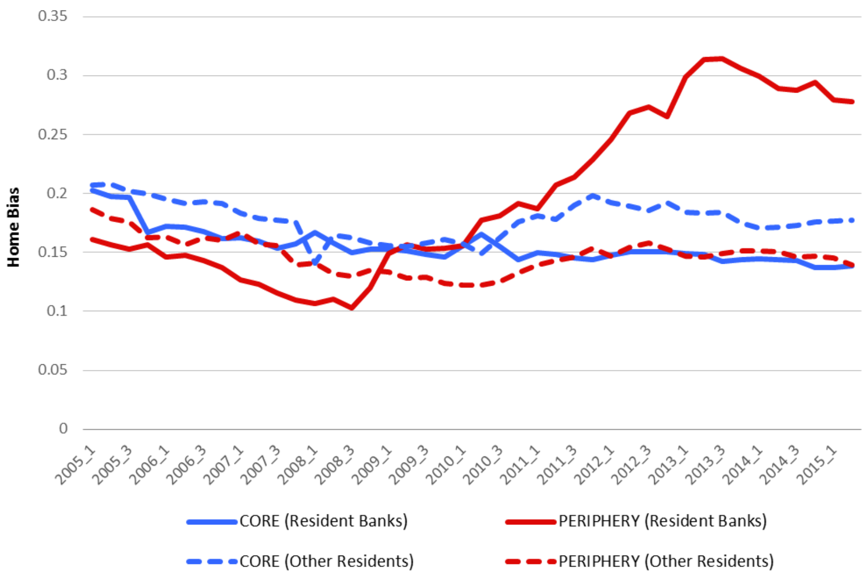
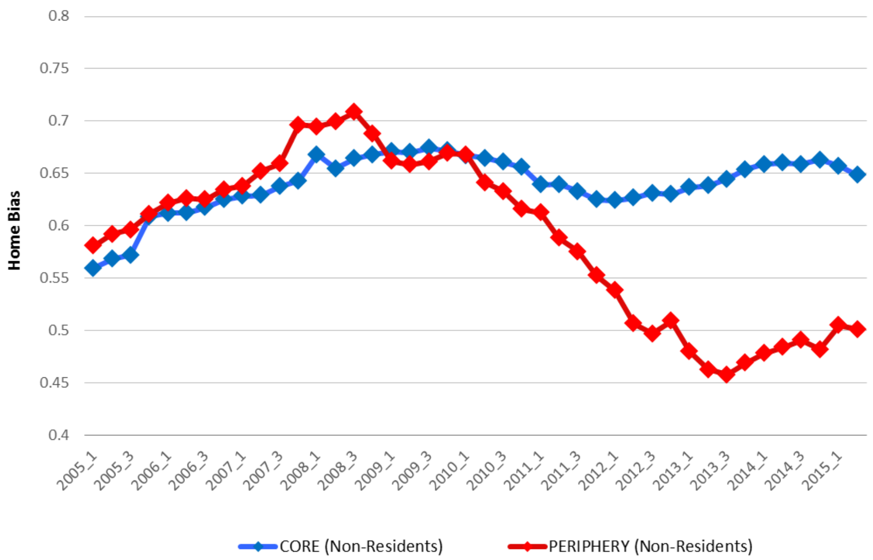


Figure 3.2: **Home bias (CAPM-adjusted) in core and periphery Euro countries during crisis.** The graph shows simple country averages of home bias and bond spreads for each country group (core vs. periphery). Home Bias is defined as the portion of the total sovereign debt of a country held by its domestic banks, after taking into account the portfolio size of these domestic banks according to a standard portfolio (CAPM) model (see the Data Description section). Bond Spreads are computed as the average daily bond spreads for a country (with respect to Germany) over the 3-month period before each observation date. Sovereign bond exposure data come from various stress-tests, transparency and recapitalization exercises undertaken by the European Banking Authority (EBA) and include 10 observation dates from 2010-Quarter1 to 2015-Quarter2 (see Table 3.1). Bond yields are obtained from Datastream. Core (non-crisis) countries: Austria, Belgium, Finland, France, Germany and Netherlands. Periphery (crisis) countries: Greece, Ireland, Italy, Portugal, Spain.



(a) Bank residents and Non-bank residents



(b) Non-residents

Figure 3.3: **Home bias for bank residents, non-bank residents and non-residents during crisis.** The graph shows simple country averages of home bias separately for bank residents, non-bank residents and non-residents. Home Bias is defined as the portion of the total sovereign debt of a country held by a particular creditor group. Sovereign debt exposures come from the dataset compiled from various national sources by Merler and Pisani-Ferry (2012) and include quarterly observations from 2005-Quarter1 to 2015-Quarter2. Core (non-crisis) countries: Belgium, Finland, France, Germany and Netherlands. Periphery (crisis) countries: Greece, Ireland, Italy, Portugal, Spain. Data for Belgium and Finland can only be found annually; so these data are linearly interpolated in order to obtain quarterly values.

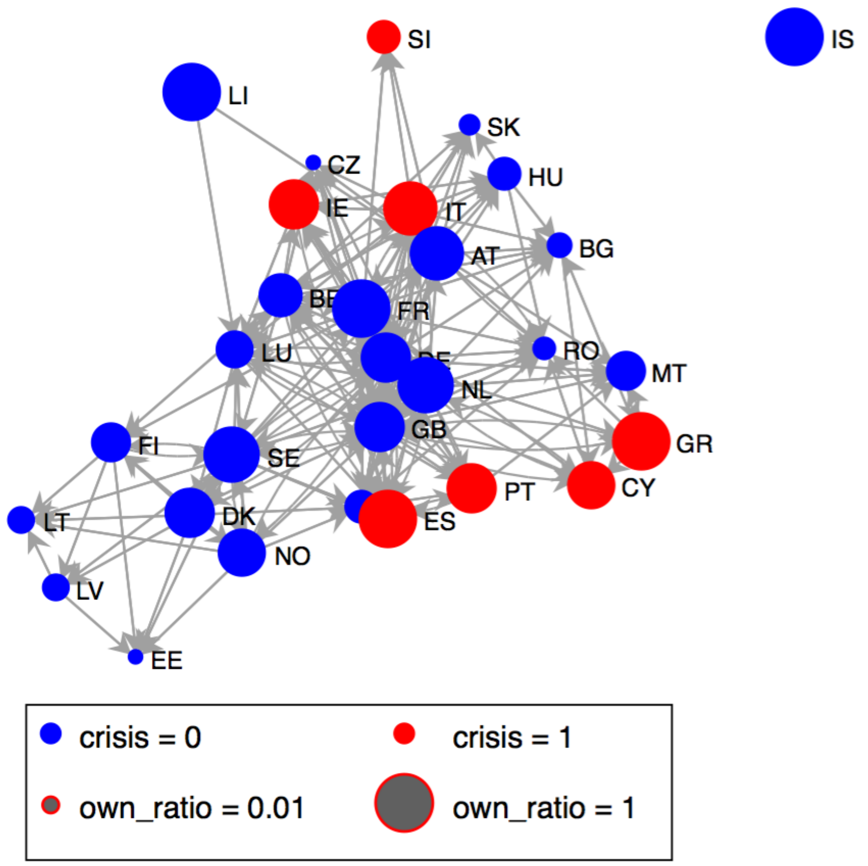


Figure 3.4: **Bank branch network across European countries.** The graph shows a simple network map for all the bank branch connections across 30 EEA countries. *Crisis* countries (Greece, Cyprus, Ireland, Portugal, Italy, Slovenia and Spain) are in red and others are in blue. Each arrow represents a connection between two countries with the direction of the arrow pointing from home country towards the host. Nodes are placed via multidimensional scaling procedure with a random component and the size of the nodes (*own_ratio*) represents the percentage of the total branches in a country that belongs to domestic banks. Bank branch data come from SNL Financial as of February, 2016.

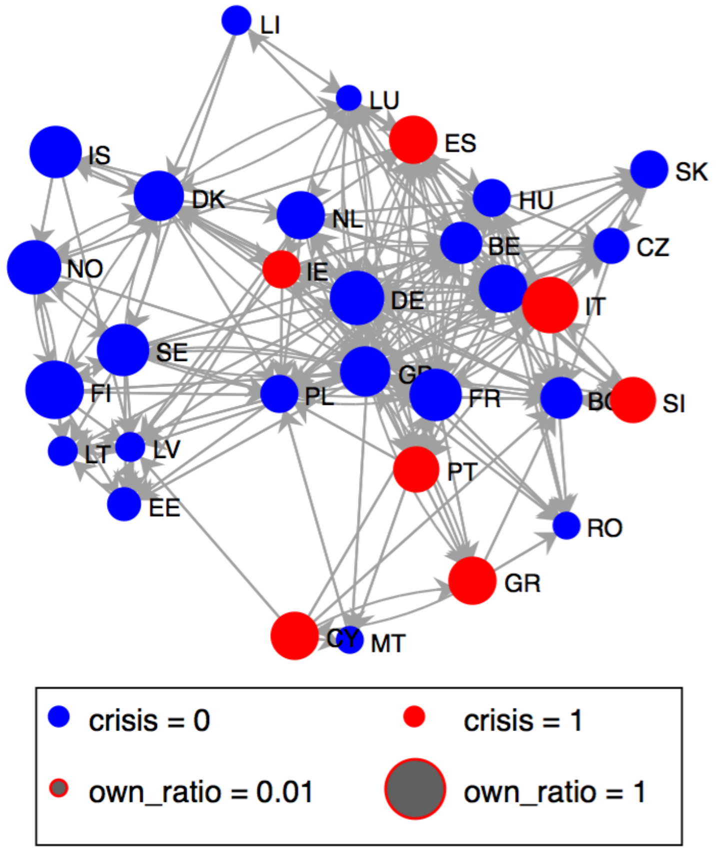


Figure 3.5: **Bank merger network across European countries.** The graph shows a simple network map for all the bank merger connections across 30 EEA countries. *Crisis* countries (Greece, Cyprus, Ireland, Portugal, Italy, Slovenia and Spain) are in red and others are in blue. Each arrow represents a connection between two countries with the direction of the arrow pointing from home country towards the host. Nodes are placed via multidimensional scaling procedure with a random component and the size of the nodes (*own_ratio*) represents the percentage of the total mergers in a country that belongs to domestic banks. Bank merger data come from SDC Platinum and cover the years between 1985 and 2008.

Disclosure date	*Disclosure name*	*Information date*	*Number of banks covered*	*Type of credit disclosure*
23/07/2010	2010 EU-wide stress testing exercise (CEBS)	2010-Q1	91	Sovereign
15/07/2011	2011 EU-wide stress testing exercise (EBA)	2010-Q4	90	Sovereign & Private
08/12/2011	EU Capital exercise 2011 (EBA)	2011-Q3	65	Sovereign
03/10/2012	EU Capital exercise 2012 (EBA)	2011-Q4 & 2012-Q2	62	Sovereign
16/12/2013	2013 EU-wide transparency exercise (EBA)	2012-Q4 & 2013-Q2	64	Sovereign & Private
26/10/2014	2014 EU-wide stress testing exercise (EBA)	2013-Q4	123	Sovereign & Private
24/11/2015	2015 EU-wide transparency exercise (EBA)	2014-Q4 & 2015-Q2	105	Sovereign & Private

Table 3.1: **Data disclosure details from European Banking Authority (EBA).** The table lists the disclosures of various exercise results as announced by the European Banking Authority (EBA). CEBS refers to the Committee of European Banking Supervisors, which was comprised of senior representatives of bank supervisory authorities and central banks of the European Union and later succeeded by the EBA. 2010 EU-wide stress testing exercise was conducted by the CEBS and made public by national regulators; however EBA does not provide the related data. Hence, this dataset was obtained from the Peterson Institute for International Economics while all other datasets were acquired from EBA. Private credit refers to the corporate and retail credit exposure of the banks covered in the respective datasets. Information date refers to the data time-points in each disclosure for which the values of bank credit positions can be found.

Variables	Mean	Median	Std. Deviation	Min	Max	Observations	Source
SovereignPortion	0.012	0	0.047	0	0.973	23,268	EBA
SovereignPortionBias	0	-0.004	0.047	-0.076	0.972	23,268	EBA
RetailPortion	0.012	0	0.070	0	1	13,665	EBA
SovereignPortion (Domestic)	0.126	0.092	0.128	0	0.841	831	EBA
SovereignPortionBias (Domestic)	0.115	0.072	0.128	-0.014	0.841	831	EBA
RetailPortion (Domestic)	0.164	0.075	0.208	0	1	497	EBA
DomesticPortion (ResidentBanks)	0.189	0.197	0.105	0.008	0.451	242	Bruegel
DomesticPortion (OtherResidents)	0.186	0.198	0.131	0.002	0.583	242	Bruegel
Bond Spreads (in basis points)	2.54	1.44	3.35	-0.96	28.70	280	Datastream
Crisis dummy (Spread > 400bps)	0.12	0	0.33	0	1	280	Datastream
CrossCountryDistance (in thousand kms)	1.45	1.36	0.83	0	4.88	616	MapQuest
CrossCountryBranches (in thousand branches)	0.22	0	1.86	0	28.72	616	SNL Financial
CrossCountryMergers (in hundred announcements)	0.05	0	0.34	0	6.10	616	SDC Platinum

Table 3.2: **Summary statistics for main variables.** The table lists the variables used in the main regressions. *SovereignPortion* is the portion of the total sovereign debt of a country held by a specific bank. *SovereignPortionBias* is the portion of total sovereign debt of a country held by a specific bank, after adjusting for a standard CAPM model (see the Data Description section). *RetailPortion* is the portion of the total retail debt in a country held by a specific bank. Domestic in parentheses denotes the observations where the country of exposure is the same as the home country of the bank. *DomesticPortion* is the portion of the overall sovereign debt of a country held by domestic agents, separately for *ResidentBanks* and *OtherResidents*. *Bond Spreads* are the spreads (in basis points) on 10-year maturity bond for each country in the sample (with respect to 10-year German bond) averaged over three-months daily values before each observation date. *Crisis* is a dummy variable which is equal to 1 if a Euro country's bond spread (with respect to Germany) is above 400 basis points at an observation date. *CrossCountryDistance* is the geographical distance (in thousand kilometres) between the capital city of the bank's home country and the capital city of the bank's exposure country. *CrossCountryBranches* is the total number of bank branches (in thousands) in the exposure country of the bank which ultimately belong to a bank from its home country. *CrossCountryMergers* is the total number of completed bank merger announcements (in hundreds) over the years 1985-2008 in which the acquirer is from the bank's home country and the target is from the bank's exposure country. The last column shows the source of the related data used for computations of each variable.

Dependent Variable:	*SovereignPortion*				*SovereignPortionBias*			
	I	*II*	*III*	*IV*	*V*	*VI*	*VII*	*VIII*
Domestic	0.126***	0.126***	0.113***	0.113***	0.127***	0.127***	0.114***	0.114***
	[10.430]	[10.276]	[9.363]	[9.210]	[10.511]	[10.356]	[9.437]	[9.284]
*Domestic*Crisis*			0.109***	0.110***			0.109***	0.110***
			[3.755]	[3.680]			[3.753]	[3.670]
Fixed Effects								
Bank	*Yes*		*Yes*		*Yes*		*Yes*	
ExpCountry x Time	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Bank x Time		*Yes*		*Yes*		*Yes*		*Yes*
Clustering	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank
Adj-R-sq	0.244	0.236	0.264	0.256	0.243	0.229	0.262	0.249
N	23268	23268	23268	23268	23268	23268	23268	23268

Table 3.3: **Sovereign debt reallocation across European banks during crisis.** The table summarizes the results of the equation (3.1) with dependent variables *SovereignPortion* (I-IV) and *SovereignPortionBias* (V-VIII) estimated over a time period fully spanning the Eurozone crisis on a biannual basis from early 2010 to mid-2015. *SovereignPortion* is the portion of the total bank-debt of a sovereign held by a specific bank. *SovereignPortionBias* is the portion of total bank-debt of a sovereign held by a specific bank, after adjusting for a standard CAPM model (see the Data Description section). *Domestic* is a dummy variable equal to 1 only if the country of exposure is the same as the home country of the bank. *Crisis* is a dummy variable which is equal to 1 only if a Euro country's bond spread (with respect to Germany) is above 400 basis points calculated as the average of daily bond spreads over the 3-month period preceding the observation date. Sovereign bond holding data come from various exercises of the European Banking Authority (EBA) and country exposures are included for 30 members of the European Economic Area (EEA). Bond yields for Crisis dummy are obtained from Datastream. Robust standard errors are clustered at the bank-level and t-statistics are reported in brackets. $*p \leq 0.1, **p \leq 0.05, ***p \leq 0.01$.

Dependent Variable:	*SovereignPortion*		*SovereignPortionBias*	
	I	*II*	*III*	*IV*
Domestic	0.123***	0.112***	0.124***	0.114***
	[10.186]	[9.204]	[10.263]	[9.278]
*StressedBank*Crisis*	0.029***	0.008***	0.029***	0.009***
	[4.089]	[3.162]	[4.073]	[3.089]
*StressedBank*Crisis*Domestic*		0.104***		0.104***
		[3.543]		[3.532]
Fixed Effects				
ExpCountry x Time	*Yes*	*Yes*	*Yes*	*Yes*
Bank x Time	*Yes*	*Yes*	*Yes*	*Yes*
Clustering	Bank	Bank	Bank	Bank
Adj-R-sq	0.241	0.256	0.234	0.249
N	23268	23268	23268	23268

Table 3.4: **Sovereign debt reallocation across European banks during crisis: Stressed Banks.** The table summarizes the results of the equation (3.2) estimated over a time period fully spanning the Eurozone crisis on a biannual basis from early 2010 to mid-2015. Dependent variables are *SovereignPortion* (I-II), which is the portion of the total sovereign debt of a country held by a specific bank, and *SovereignPortionBias* (III-IV), which is the portion of total sovereign debt of a country held by a specific bank after adjusting for a standard CAPM model (see the Data Description section). *Domestic* is a dummy variable equal to 1 only if the country of exposure is the same as the home country of the bank. *Crisis* is a dummy variable which is equal to 1 only if a Euro country's bond spread (with respect to Germany) is above 400 basis points calculated as the average of daily bond spreads over the 3-month period preceding the observation date. *StressedBank* is a dummy variable indicating those observations in which the home country of the bank is considered to be "in crisis" ($400bps \leq spread$). Sovereign bond holding data come from various exercises of the European Banking Authority (EBA) and country exposures are included for 30 members of the European Economic Area (EEA). Bond yields for Crisis dummy are obtained from Datastream. Robust standard errors are clustered at the bank-level and t-statistics are reported in brackets. $*p \leq 0.1, **p \leq 0.05, ***p \leq 0.01$.

Dependent Variable: *DomesticPortion*	*I*	*II*	*III*	*IV*
Crisis	-0.009	-0.092***	-0.101***	
	[-0.333]	[-3.609]	[-3.623]	
*Crisis*ResidentBanks*		0.167**	0.184***	0.184**
		[3.000]	[3.375]	[2.440]
Fixed Effects				
Country	*Yes*	*Yes*	*Yes*	
Time	*Yes*	*Yes*		
Creditor	*Yes*	*Yes*		
Creditor x Time			*Yes*	*Yes*
Country x Time				*Yes*
Clustering	Country	Country	Country	Country
R-sq	0.024	0.146	0.167	0.248
N	484	484	484	484

Table 3.5: **Sovereign debt reallocation during crisis: Resident banks vs non-bank residents.** The table summarizes the results of the equation (3.3) with dependent variable *DomesticPortion* (I-IV), which is the portion of the overall sovereign debt of a country held by a particular domestic agent (either by resident banks or other private residents), estimated over a time period fully spanning the Eurozone crisis on a quarterly basis from early 2010 to the mid-2015. *ResidentBanks* is a dummy variable equal to one only if the creditor is the resident banks of the country. *Crisis* is a dummy variable which is equal to 1 only if a Euro country's bond spread (with respect to Germany) is above 400 basis points calculated as the average of daily bond spreads over the 3-month period preceding the observation date. Domestic sovereign holding data come from the dataset compiled from various national sources by Merler and Pisani-Ferry (2012). Countries include Belgium, Finland, France, Germany, Greece, Ireland, Italy, Netherlands, Portugal, Spain and United Kingdom. Bond yields for Crisis dummy are obtained from Datastream. Robust standard errors are clustered at the country-level and t-statistics are reported in brackets. $*p \leq 0.1, **p \leq 0.05, ***p \leq 0.01$.

Dependent Variable:	*DebtPortion*				*DebtPortionBias*			
	I	*II*	*III*	*IV*	*V*	*VI*	*VII*	*VIII*
Domestic	0.141***				0.144***			
	[10.053]				[10.141]			
*Domestic*Retail*		0.167***	0.154***	0.152***		0.170***	0.157***	0.155***
		[8.313]	[7.747]	[7.578]		[8.373]	[7.816]	[7.664]
*Domestic*Sovereign*		0.126***	0.112***	0.113***		0.128***	0.114***	0.115***
		[10.348]	[9.068]	[9.288]		[10.427]	[9.133]	[9.344]
*Domestic*Crisis*			0.118***	0.135***			0.119***	0.133**
			[3.645]	[2.641]			[3.636]	[2.590]
*Domestic*Crisis*Sovereign*				-0.026				-0.022
				[-0.588]				[-0.503]
Fixed Effects								
Bank x Time	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
ExpCountry x Time	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Sector	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Clustering	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank
Adj-R-sq	0.209	0.213	0.228	0.229	0.206	0.210	0.225	0.225
N	36777	36777	36777	36777	36777	36777	36777	36777

Table 3.6: **Debt reallocation across European banks during crisis: Sovereign vs retail debt.** The table summarizes the results of the equation (3.4) estimated over a time period fully spanning the Eurozone crisis on a biannual basis from early 2010 to mid-2015. Dependent variables are *DebtPortion* (I-IV), which measures the portion of a specific type of total debt (sovereign or retail) of a country held by a specific bank and *DebtPortionBias* (V-VIII), which is the portion of total debt of a country held by a specific bank after adjusting for a standard CAPM model (see the Data Description section). *Sovereign* and *Retail* are dummy variables indicating the respective debt types held by the banks. *Domestic* is a dummy variable equal to 1 only if the country of exposure is the same as the home country of the bank. *Crisis* is a dummy variable which is equal to 1 only if a Euro country's bond spread (with respect to Germany) is above 400 basis points calculated as the average of daily bond spreads over the 3-month period preceding the observation date. Sovereign and retail debt data come from various exercises of the European Banking Authority (EBA) and country exposures are included for 30 members of the European Economic Area (EEA). Bond yields for Crisis dummy are obtained from Datastream. Robust standard errors are clustered at the bank-level and t-statistics are reported in brackets. $*p \leq 0.1, **p \leq 0.05, ***p \leq 0.01$.

Dependent Variable:	Full Sample						Foreign bank sample					
	SovereignPortion			*SovereignPortionBias*			*SovereignPortion*			*SovereignPortionBias*		
	I	*II*	*III*	*IV*	*V*	*VI*	*VII*	*VIII*	*IX*	*X*	*XI*	*XII*
*CrossCountryDistance*Crisis*	-0.011***			-0.012***			-0.001			-0.001		
	[-4.421]			[-4.427]			[-1.045]			[-1.017]		
*CrossCountryBranches*Crisis*		0.004***			0.004***			0.006**			0.006**	
		[4.806]			[4.748]			[2.214]			[2.202]	
*CrossCountryMergers*Crisis*			0.014***			0.015***			0.024*			0.025*
			[3.056]			[3.047]			[1.972]			[1.946]
Fixed Effects												
Bank x Time	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
ExpCountry x Time	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
HomeCountry x ExpCountry	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Clustering	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank
Adj-R-sq	0.511	0.513	0.511	0.500	0.503	0.500	0.223	0.223	0.223	0.215	0.215	0.215
N	23268	23268	23268	23268	23268	23268	22437	22437	22437	22437	22437	22437

Table 3.7: **Sovereign debt reallocation across European banks during crisis: Effect of informational distance.** The table summarizes the results of the equation (3.5) in full sample (I-VI) and in foreign sample (VII-XII) estimated over a time period fully spanning the Eurozone crisis on a biannual basis from early 2010 to mid-2015. Dependent variables are *SovereignPortion*, which measures the portion of total sovereign debt of a country held by a specific bank and *SovereignPortionBias*, which is the portion of total sovereign debt of a country held by a specific bank after adjusting for a standard CAPM model (see the Data Description section). *Crisis* is a dummy variable which is equal to 1 only if a Euro country's bond spread (with respect to Germany) is above 400 basis points calculated as the average of daily bond spreads over the 3-month period preceding the observation date. *CrossCountryDistance* is the geographical distance (in thousand kilometres) between the capital city of the bank's home country and the capital city of the bank's exposure country. *CrossCountryBranches* is the total number of bank branches (in thousands) located in the bank's exposure country which ultimately belong to a bank from its home country. *CrossCountryMergers* is the total number of completed bank merger announcements (in hundreds) over the years 1985-2008 in which the acquirer is from the bank's home country and the target is from the bank's exposure country. Sovereign bond holding data come from various exercises of the European Banking Authority (EBA) and country exposures are included for 30 members of the European Economic Area (EEA). Bond yields for Crisis dummy are obtained from Datastream. Robust standard errors are clustered at the bank-level and t-statistics are reported in brackets. $*p \leq 0.1, **p \leq 0.05, ***p \leq 0.01$.

Dependent Variable:	Full Sample						Foreign bank sample					
	SovereignPortion			*SovereignPortionBias*			*SovereignPortion*			*SovereignPortionBias*		
	I	*II*	*III*	*IV*	*V*	*VI*	*VII*	*VIII*	*IX*	*X*	*XI*	*XII*
*CrossCountryDistance*Crisis*	-0.011***			-0.011***			-0.001			-0.001		
	[-4.329]			[-4.339]			[-1.130]			[-1.097]		
*CrossCountryBranches*Crisis*		0.004***			0.004***			0.006**			0.006**	
		[4.655]			[4.601]			[2.242]			[2.228]	
*CrossCountryMergers*Crisis*			0.013***			0.014***			0.025**			0.026*
			[2.922]			[2.915]			[1.986]			[1.957]
*StressedBank*Crisis*	0.010***	0.006**	0.008***	0.011***	0.006**	0.008***	0.001	0.001	0.001	0.001	0.001	0.001
	[3.878]	[2.202]	[3.305]	[3.869]	[2.165]	[3.275]	[0.803]	[0.754]	[0.753]	[0.754]	[0.708]	[0.706]
Fixed Effects												
Bank x Time	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
ExpCountry x Time	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
HomeCountry x ExpCountry	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Clustering	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank
Adj-R-sq	0.511	0.513	0.511	0.501	0.503	0.501	0.223	0.223	0.223	0.215	0.215	0.215
N	23268	23268	23268	23268	23268	23268	22437	22437	22437	22437	22437	22437

Table 3.8: **Effect of informational distance: Controlling for risk-shifting.** The table summarizes the results of the equation (3.5) in full sample (I-VI) and in foreign sample (VII-XII) estimated over a time period fully spanning the Eurozone crisis on a biannual basis from early 2010 to mid-2015. For previous variable definitions, see Table 3.7. *StressedBank* is a dummy variable indicating those observations in which the home country of the bank is considered to be "in crisis" ($400bps \leq spread$). Sovereign bond holding data come from various exercises of the European Banking Authority (EBA) and country exposures are included for 30 members of the European Economic Area (EEA). Bond yields for Crisis dummy are obtained from Datastream. Robust standard errors are clustered at the bank-level and t-statistics are reported in brackets. $*p \leq 0.1, **p \leq 0.05, ***p \leq 0.01$.

Dependent Variable:	Full Sample						Foreign bank sample					
	SovereignPortion			*SovereignPortionBias*			*SovereignPortion*			*SovereignPortionBias*		
	I	*II*	*III*	*IV*	*V*	*VI*	*VII*	*VIII*	*IX*	*X*	*XI*	*XII*
*CrossCountryDistance*Crisis*	-0.011***			-0.011***			-0.001			-0.001		
	[-4.255]			[-4.267]			[-1.032]			[-1.003]		
*CrossCountryBranches*Crisis*		0.004***			0.004***			0.006**			0.006**	
		[4.665]			[4.610]			[2.063]			[2.057]	
*CrossCountryMergers*Crisis*			0.013***			0.014***			0.023*			0.024*
			[2.932]			[2.925]			[1.793]			[1.775]
*StressedBank*Crisis*	0.010***	0.005**	0.008***	0.010***	0.005**	0.008***	0.001	0.001	0.001	0.001	0.001	0.001
	[3.838]	[2.120]	[3.243]	[3.819]	[2.075]	[3.202]	[0.841]	[0.774]	[0.762]	[0.791]	[0.726]	[0.714]
*Euroshare*Crisis*	-0.004	-0.003	-0.001	-0.004	-0.003	-0.001	0.003	0.003	0.002	0.003	0.003	0.002
	[-0.522]	[-0.444]	[-0.192]	[-0.509]	[-0.449]	[-0.197]	[0.578]	[0.436]	[0.353]	[0.554]	[0.406]	[0.325]
Fixed Effects												
Bank x Time	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
ExpCountry x Time	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
HomeCountry x ExpCountry	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Clustering	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank
Adj-R-sq	0.511	0.513	0.511	0.501	0.503	0.500	0.223	0.223	0.223	0.215	0.215	0.215
N	23268	23268	23268	23268	23268	23268	22437	22437	22437	22437	22437	22437

Table 3.9: **Effect of informational distance: Controlling for risk-shifting and Eurozone share.** The table summarizes the results of the equation (3.5) in full sample (I-VI) and in foreign sample (VII-XII) estimated over a time period fully spanning the Eurozone crisis on a biannual basis from early 2010 to mid-2015. For previous variable definitions, see Table 3.7. *StressedBank* is a dummy variable indicating those observations in which the home country of the bank is considered to be "in crisis" ($400bps \leq spread$). *Euroshare* is the share of total Eurozone GDP that the home country of the bank produces (0 for Non-Eurozone). Sovereign bond holding data come from EBA and country exposures are included for 30 EEA members. Bond yields for Crisis dummy are obtained from Datastream. Robust standard errors are clustered at the bank-level and t-statistics are reported in brackets. $*p \leq 0.1, **p \leq 0.05, ***p \leq 0.01$.

	Full Sample						Foreign bank sample					
Dependent Variable:	*SovereignPortion*			*SovereignPortionBias*			*SovereignPortion*			*SovereignPortionBias*		
	I	*II*	*III*	*IV*	*V*	*VI*	*VII*	*VIII*	*IX*	*X*	*XI*	*XII*
*CrossCountryDistance*Crisis*	-0.011***			-0.011***			-0.001			-0.001		
	[-4.327]			[-4.337]			[-1.116]			[-1.083]		
*CrossCountryBranches*Crisis*		0.004***			0.004***			0.006**			0.006**	
		[4.659]			[4.604]			[2.232]			[2.219]	
*CrossCountryMergers*Crisis*			0.013***			0.014***			0.025*			0.025*
			[2.924]			[2.918]			[1.948]			[1.920]
*StressedBank*Crisis*	0.010***	0.006**	0.008***	0.011***	0.006**	0.008***	0.001	0.001	0.001	0.001	0.001	0.001
	[3.859]	[2.259]	[3.317]	[3.849]	[2.222]	[3.287]	[0.857]	[0.808]	[0.799]	[0.808]	[0.761]	[0.752]
*GermanBank*Crisis*	0.000	0.001	0.001	0.000	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001
	[0.085]	[0.666]	[0.449]	[0.075]	[0.656]	[0.438]	[0.479]	[0.451]	[0.404]	[0.463]	[0.434]	[0.388]
Fixed Effects												
Bank x Time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
ExpCountry x Time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HomeCountry x ExpCountry	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank
Adj-R-sq	0.511	0.513	0.511	0.501	0.503	0.500	0.223	0.223	0.223	0.215	0.215	0.215
N	23268	23268	23268	23268	23268	23268	22437	22437	22437	22437	22437	22437

Table 3.10: **Effect of informational distance: Controlling for risk-shifting and German banks.** The table summarizes the results of the equation (3.5) in full sample (I-VI) and in foreign sample (VII-XII) estimated over a time period fully spanning the Eurozone crisis on a biannual basis from early 2010 to mid-2015. For previous variable definitions, see Table 3.7. *StressedBank* is a dummy variable indicating those observations in which the home country of the bank is considered to be "in crisis" ($400bps \leq spread$). *GermanBank* is a dummy variable which is equal to one for banks located in Germany. Sovereign bond holding data come from EBA and country exposures are included for 30 EEA members. Bond yields for Crisis dummy are obtained from Datastream. Robust standard errors are clustered at the bank-level and t-statistics are reported in brackets. $*p \leq 0.1, **p \leq 0.05, ***p \leq 0.01$.

Dependent Variable:	*DebtPortion*				*DebtPortionBias*			
	I	*II*	*III*	*IV*	*V*	*VI*	*VII*	*VIII*
Domestic	0.140***				0.143***			
	[10.016]				[10.080]			
*Domestic*Corporate*		0.164***	0.152***	0.152***		0.167***	0.155***	0.155***
		[8.444]	[7.940]	[7.881]		[8.475]	[7.970]	[7.924]
*Domestic*Sovereign*		0.126***	0.113***	0.113***		0.128***	0.115***	0.115***
		[10.326]	[9.088]	[9.292]		[10.406]	[9.153]	[9.347]
*Domestic*Crisis*			0.110***	0.114**			0.110***	0.112**
			[3.319]	[2.102]			[3.302]	[2.059]
*Domestic*Crisis*Sovereign*				-0.006				-0.002
				[-0.119]				[-0.047]
Fixed Effects								
Bank x Time	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
ExpCountry x Time	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Sector	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Clustering	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank
Adj-R-sq	0.217	0.221	0.235	0.235	0.213	0.217	0.230	0.230
N	36777	36777	36777	36777	36777	36777	36777	36777

Table 3.A1: **Debt reallocation across European banks during crisis: Sovereign vs corporate debt.** The table summarizes the results of the equation (3.4) estimated over a time period fully spanning the Eurozone crisis on a biannual basis from early 2010 to mid-2015. Dependent variables are *DebtPortion* (I-IV), which measures the portion of a specific type of total debt (sovereign or retail) of a country held by a specific bank and *DebtPortionBias* (V-VIII), which is the portion of total debt of a country held by a specific bank after adjusting for a standard CAPM model (see the Data Description section). *Sovereign* and *Corporate* are dummy variables indicating the respective debt types held by the banks. *Domestic* is a dummy variable equal to 1 only if the country of exposure is the same as the home country of the bank. *Crisis* is a dummy variable which is equal to 1 only if a Euro country's bond spread (with respect to Germany) is above 400 basis points calculated as the average of daily bond spreads over the 3-month period preceding the observation date. Sovereign and corporate debt data come from various exercises of the European Banking Authority (EBA) and country exposures are included for 30 members of the European Economic Area (EEA). Bond yields for Crisis dummy are obtained from Datastream. Robust standard errors are clustered at the bank-level and t-statistics are reported in brackets. $*p \leq 0.1, **p \leq 0.05, ***p \leq 0.01$.

Dependent Variable:	*SovereignPortion*											
Table:	3	4	5	6	7 (Full)	7 (Foreign)	8 (Full)	8 (Foreign)	9 (Full)	9 (Foreign)	10 (Full)	10 (Foreign)
Domestic	0.099***	0.099***										
	[7.324]	[7.319]										
*Domestic*Crisis*	0.123***											
	[4.105]											
*StressedBank*Crisis*		0.006**					0.005**	0.001	0.005*	0.001	0.006**	0.001
		[2.292]					[2.146]	[0.555]	[1.965]	[0.545]	[2.231]	[0.613]
*StressedBank*Crisis*Domestic*		0.119***										
		[4.055]										
*Crisis*ResidentBanks*			0.145**									
			[2.415]									
*Domestic*Retail*				0.130***								
				[6.120]								
*Domestic*Sovereign*				0.099***								
				[7.409]								
*Domestic*Crisis*				0.156***								
				[3.094]								
*Domestic*Crisis*Sovereign*				-0.034								
				[-0.778]								
*CrossCountryBranches*Crisis*					0.004***	0.007**	0.004***	0.007**	0.004***	0.007**	0.004***	0.007**
					[4.859]	[2.463]	[4.727]	[2.482]	[4.738]	[2.416]	[4.732]	[2.498]
*Euroshare*Crisis*									-0.004	0.002		
									[-0.426]	[0.261]		
*GermanBank*Crisis*											0.001	0.001
											[0.759]	[0.379]
Fixed Effects												
Bank x Time	*Yes*	*Yes*		*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
ExpCountry x Time	*Yes*	*Yes*		*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
HomeCountry x ExpCountry					*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Sector				*Yes*								
Creditor x Time			*Yes*									
Country x Time			*Yes*									
Clustering	Bank	Bank	Country	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank
Adj-R-sq	0.287	0.287	0.334	0.261	0.525	0.216	0.525	0.216	0.525	0.216	0.525	0.216
N	18872	18872	440	29882	18872	18198	18872	18198	18872	18198	18872	18198

Table 3.A2: **Main results with only Eurozone banks.** The table summarizes the results of the equations (3.1), (3.2), (3.3), (3.4) and (3.5) with dependent variable *SovereignPortion* estimated over a time period fully spanning the Eurozone crisis on a biannual basis from early 2010 to mid-2015. This sample only includes the banks located in the Eurozone. For the definitions of variables, see Table 3.3, 3.4, 3.5, 3.6, 3.7, 3.8, 3.9 and 3.10. Robust standard errors are clustered and t-statistics are reported in brackets. $*p \leq 0.1, **p \leq 0.05, ***p \leq 0.01$.

Dependent Variable:	SovereignPortion											
Table:	3	4	5	6	7 (Full)	7 (Foreign)	8 (Full)	8 (Foreign)	9 (Full)	9 (Foreign)	10 (Full)	10 (Foreign)
Domestic	0.099***	0.099***										
	[7.253]	[7.248]										
*Domestic*Crisis*	0.128***											
	[3.810]											
*StressedBank*Crisis*		0.006**					0.005*	0.001	0.005	0.001	0.005*	0.001
		[2.312]					[1.783]	[0.868]	[1.646]	[0.686]	[1.853]	[0.823]
*StressedBank*Crisis*Domestic*		0.124***										
		[3.739]										
*Crisis*ResidentBanks*			0.179**									
			[2.594]									
*Domestic*Retail*				0.130***								
				[6.116]								
*Domestic*Sovereign*				0.099***								
				[7.340]								
*Domestic*Crisis*				0.200***								
				[3.034]								
*Domestic*Crisis*Sovereign*				-0.072								
				[-1.407]								
*CrossCountryBranches*Crisis*					0.004***	0.007**	0.004***	0.007**	0.004***	0.007**	0.004***	0.007**
					[4.764]	[2.546]	[4.617]	[2.576]	[4.631]	[2.576]	[4.618]	[2.570]
*Euroshare*Crisis*									-0.004	-0.001		
									[-0.517]	[-0.163]		
*GermanBank*Crisis*											0.001	0.000
											[0.538]	[-0.091]
Fixed Effects												
Bank x Time	*Yes*	*Yes*		*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
ExpCountry x Time	*Yes*	*Yes*		*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
HomeCountry x ExpCountry					*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Sector				*Yes*								
Creditor x Time			*Yes*									
Country x Time			*Yes*									
Clustering	Bank	Bank	Country	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank
Adj-R-sq	0.280	0.280	0.306	0.258	0.533	0.221	0.533	0.221	0.533	0.221	0.533	0.221
N	18198	18198	396	28803	18198	17548	18198	17548	18198	17548	18198	17548

Table 3.A3: **Main results with only Eurozone banks and without Greek exposures.** The table summarizes the results of the equations (3.1), (3.2), (3.3), (3.4) and (3.5) with dependent variable *SovereignPortion* estimated over a time period fully spanning the Eurozone crisis on a biannual basis from early 2010 to mid-2015. This sample only includes the banks located in the Eurozone and does not include their Greek exposures. For the definitions of variables, see Table 3.3, 3.4, 3.5, 3.6, 3.7, 3.8, 3.9 and 3.10. Robust standard errors are clustered and t-statistics are reported in brackets. $*p \leq 0.1, **p \leq 0.05, ***p \leq 0.01$.

Dependent Variable:	SovereignPortion											
Table:	3	4	5	6	7 (Full)	7 (Foreign)	8 (Full)	8 (Foreign)	9 (Full)	9 (Foreign)	10 (Full)	10 (Foreign)
Domestic	0.109***	0.109***										
	[8.623]	[8.613]										
*Domestic*Crisis*	0.102***											
	[3.795]											
*StressedBank*Crisis*		0.008***					0.003	0.001	0.003	0.001	0.003	0.001
		[2.871]					[1.245]	[0.485]	[1.163]	[0.515]	[1.301]	[0.566]
*StressedBank*Crisis*Domestic*		0.097***										
		[3.634]										
*Crisis*ResidentBanks*			0.181**									
			[2.292]									
*Domestic*Retail*				0.150***								
				[7.318]								
*Domestic*Sovereign*				0.110***								
				[8.709]								
*Domestic*Crisis*				0.114***								
				[2.628]								
*Domestic*Crisis*Sovereign*				-0.014								
				[-0.392]								
*CrossCountryBranches*Crisis*					0.005***	0.005***	0.005***	0.005***	0.005***	0.005**	0.005***	0.005***
					[4.587]	[2.869]	[4.512]	[2.873]	[4.511]	[2.461]	[4.515]	[2.896]
*Euroshare*Crisis*									-0.003	0.003		
									[-0.411]	[0.371]		
*GermanBank*Crisis*											0.001	0.001
											[0.540]	[0.442]
Fixed Effects												
Bank x Time	*Yes*	*Yes*		*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
ExpCountry x Time	*Yes*	*Yes*		*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
HomeCountry x ExpCountry					*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Sector				*Yes*								
Creditor x Time			*Yes*									
Country x Time			*Yes*									
Clustering	Bank	Bank	Country	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank
Adj-R-sq	0.258	0.259	0.265	0.229	0.518	0.223	0.518	0.223	0.518	0.223	0.518	0.223
N	23268	23268	484	36777	23268	22437	23268	22437	23268	22437	23268	22437

Table 3.A4: **Main results with the crisis threshold of 300 basis points.** The table summarizes the results of the equations (3.1), (3.2), (3.3), (3.4) and (3.5) with dependent variable *SovereignPortion* estimated over a time period fully spanning the Eurozone crisis on a biannual basis from early 2010 to mid-2015. In this sample, *Crisis* is a dummy variable which is equal to 1 only if a Euro country's bond spread (with respect to Germany) is above 300 basis points calculated as the average of daily bond spreads over the 3-month period preceding the observation date. For the definitions of variables, see Table 3.3, 3.4, 3.5, 3.6, 3.7, 3.8, 3.9 and 3.10. Robust standard errors are clustered and t-statistics are reported in brackets. $*p \leq 0.1, **p \leq 0.05, ***p \leq 0.01$.

Dependent Variable:	*SovereignPortion*											
Table:	*3*	*4*	*5*	*6*	*7 (Full)*	*7 (Foreign)*	*8 (Full)*	*8 (Foreign)*	*9 (Full)*	*9 (Foreign)*	*10 (Full)*	*10 (Foreign)*
Domestic	0.116***	0.116***										
	[9.760]	[9.755]										
*Domestic*Crisis*	0.129***											
	[3.206]											
*StressedBank*Crisis*		0.016***					0.015**	0.006**	0.015**	0.006**	0.015**	0.006**
		[3.752]					[2.375]	[2.294]	[2.429]	[2.308]	[2.399]	[2.263]
*StressedBank*Crisis*Domestic*		0.115***										
		[2.985]										
*Crisis*ResidentBanks*			0.195**									
			[2.479]									
*Domestic*Retail*				0.160***								
				[7.845]								
*Domestic*Sovereign*				0.117***								
				[9.846]								
*Domestic*Crisis*				0.105*								
				[1.880]								
*Domestic*Crisis*Sovereign*				0.022								
				[0.446]								
*CrossCountryBranches*Crisis*					0.011**	0.019*	0.008	0.020*	0.008	0.020*	0.008	0.020*
					[2.147]	[1.745]	[1.623]	[1.800]	[1.626]	[1.805]	[1.630]	[1.818]
*Euroshare*Crisis*									0.003	0.005		
									[0.358]	[0.646]		
*GermanBank*Crisis*											0.001	0.001
											[0.614]	[0.419]
Fixed Effects												
Bank x Time	*Yes*	*Yes*		*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
ExpCountry x Time	*Yes*	*Yes*		*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
HomeCountry x ExpCountry					*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Sector				*Yes*								
Creditor x Time			*Yes*									
Country x Time			*Yes*									
Clustering	Bank	Bank	Country	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank
Adj-R-sq	0.254	0.254	0.236	0.223	0.510	0.223	0.510	0.223	0.510	0.223	0.510	0.223
N	23268	23268	484	36777	23268	22437	23268	22437	23268	22437	23268	22437

Table 3.A5: **Main results with the crisis threshold of 500 basis points.** The table summarizes the results of the equations (3.1), (3.2), (3.3), (3.4) and (3.5) with dependent variable *SovereignPortion* estimated over a time period fully spanning the Eurozone crisis on a biannual basis from early 2010 to mid-2015. In this sample, *Crisis* is a dummy variable which is equal to 1 only if a Euro country's bond spread (with respect to Germany) is above 500 basis points calculated as the average of daily bond spreads over the 3-month period preceding the observation date. For the definitions of variables, see Table 3.3, 3.4, 3.5, 3.6, 3.7, 3.8, 3.9 and 3.10. Robust standard errors are clustered and t-statistics are reported in brackets. $*p \leq 0.1, **p \leq 0.05, ***p \leq 0.01$.

Dependent Variable:	SovereignPortion											
Table:	3	4	5	6	7 (Full)	7 (Foreign)	8 (Full)	8 (Foreign)	9 (Full)	9 (Foreign)	10 (Full)	10 (Foreign)
Domestic	0.098***	0.113***										
	[7.571]	[9.450]										
*Domestic*ExpSpread*	0.017***											
	[3.664]											
*HomeSpread*ExpSpread*		0.000					0.000**	0.000	0.000**	0.000*	0.000**	0.000
		[1.592]					[2.034]	[1.406]	[2.255]	[1.703]	[2.094]	[1.490]
*HomeSpread*ExpSpread*Domestic*		0.001***										
		[2.900]										
*Spread*ResidentBanks*			0.015									
			[1.677]									
*Domestic*Retail*				0.131***								
				[7.072]								
*Domestic*Sovereign*				0.099***								
				[7.699]								
*Domestic*ExpSpread*				0.023***								
				[3.073]								
*Domestic*ExpSpread*Sovereign*				-0.006								
				[-0.948]								
*CrossCountryBranches*ExpSpread*					0.002***	0.007***	0.002***	0.007***	0.002***	0.007***	0.002***	0.007***
					[5.302]	[2.957]	[5.212]	[2.926]	[5.211]	[2.913]	[5.212]	[2.933]
*Euroshare*ExpSpread*									0.002	0.001		
									[1.585]	[1.360]		
*GermanBank*ExpSpread*											0.000	0.000
											[1.285]	[0.798]
Fixed Effects												
Bank x Time	*Yes*	*Yes*		*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
ExpCountry x Time	*Yes*	*Yes*		*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
HomeCountry x ExpCountry					*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Sector				*Yes*								
Creditor x Time			*Yes*									
Country x Time			*Yes*									
Clustering	Bank	Bank	Country	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank
Adj-R-sq	0.262	0.255	0.221	0.235	0.517	0.225	0.517	0.225	0.517	0.225	0.517	0.225
N	23268	23268	484	36777	23268	22437	23268	22437	23268	22437	23268	22437

Table 3.A6: **Main results with the crisis dummy replaced with bond spreads.** The table summarizes the results of the equations (3.1), (3.2), (3.3), (3.4) and (3.5) with dependent variable *SovereignPortion* estimated over a time period fully spanning the Eurozone crisis on a biannual basis from early 2010 to mid-2015. In this sample, *Crisis* dummy is replaced with *ExpSpread* measuring the average of exposure country's daily bond spreads (with respect to Germany) over the 3-month period preceding the observation date. For the definitions of variables, see Table 3.3, 3.4, 3.5, 3.6, 3.7, 3.8, 3.9 and 3.10. Robust standard errors are clustered and t-statistics are reported in brackets. $*p \leq 0.1, **p \leq 0.05, ***p \leq 0.01$.

Dependent Variable:	*SovereignPortion*											
Table:	*3*	*4*	*5*	*6*	*7 (Full)*	*7 (Foreign)*	*8 (Full)*	*8 (Foreign)*	*9 (Full)*	*9 (Foreign)*	*10 (Full)*	*10 (Foreign)*
Domestic	0.119***	0.120***										
	[9.895]	[9.959]										
*Domestic*ExpSpread*	0.000*											
	[1.863]											
*HomeSpread*ExpSpread*		0.000*					-0.000***	0.000	-0.000***	0.000	-0.000***	0.000
		[1.905]					[-3.872]	[1.613]	[-3.908]	[1.372]	[-3.894]	[1.399]
*HomeSpread*ExpSpread*Domestic*		-0.000*										
		[-1.668]										
*Spread*ResidentBanks*			0.000									
			[1.273]									
*Domestic*Retail*				0.167***								
				[7.990]								
*Domestic*Sovereign*				0.120***								
				[9.953]								
*Domestic*ExpSpread*				0.000								
				[0.986]								
*Domestic*ExpSpread*Sovereign*				0.000								
				[0.411]								
*CrossCountryBranches*ExpSpread*					0.000***	0.004***	0.001***	0.004***	0.001***	0.004***	0.001***	0.004***
					[3.055]	[2.860]	[4.657]	[2.931]	[4.680]	[2.878]	[4.674]	[2.929]
*Euroshare*ExpSpread*									-0.000**	0.000		
									[-1.988]	[-1.164]		
*GermanBank*ExpSpread*											0.000	0.000
											[-1.324]	[-0.732]
Fixed Effects												
Bank x Time	*Yes*	*Yes*		*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
ExpCountry x Time	*Yes*	*Yes*		*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
HomeCountry x ExpCountry					*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Sector				*Yes*								
Creditor x Time			*Yes*									
Country x Time			*Yes*									
Clustering	Bank	Bank	Country	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank
Adj-R-sq	0.234	0.233	0.131	0.220	0.511	0.227	0.515	0.228	0.515	0.228	0.515	0.228
N	22437	22059	484	35449	22437	21620	22059	21242	22059	21242	22059	21242

Table 3.A7: **Main results with the crisis dummy replaced with CDS spreads.** The table summarizes the results of the equations (3.1), (3.2), (3.3), (3.4) and (3.5) with dependent variable *SovereignPortion* estimated over a time period fully spanning the Eurozone crisis on a biannual basis from early 2010 to mid-2015. In this sample, *Crisis* dummy is replaced with *ExpSpread* measuring the average of exposure country's daily CDS spreads over the 3-month period preceding the observation date. For the definitions of variables, see Table 3.3, 3.4, 3.5, 3.6, 3.7, 3.8, 3.9 and 3.10. Robust standard errors are clustered and t-statistics are reported in brackets. $*p \leq 0.1, **p \leq 0.05, ***p \leq 0.01$.

Chapter 4

Political Lending Cycles and Real Outcomes: Evidence from Turkey

4.1 Introduction

Theories of political lending cycles predict that governments use loans by state-owned banks as a strategic tool for re-election purposes. In particular, bank credit can be significantly reallocated around election years and such targeted redistribution would often be aimed to shift local election outcomes in favour of the ruling party, or coalition parties in control of central government. Is it then possible to see that some regions would be favoured and others get punished on the basis of their attractiveness to politicians? More importantly, does this reallocation have real effects on the local economy?

We test theories of political cycles in Turkey for the period from 2003 to 2016 using the universe of bank credit for the country. We collect detailed data on election outcomes, banking activity for different bank types, and indicators of economic activity all observed at the province level. Unlike previous literature, we can draw on quarterly data to identify the exact timing of politically induced lending. Our data also allow us to differentiate between the effects of politically driven lending on firms and consumers separately.

We document two main sets of findings. First, we show that state-owned banks engage in strategic lending around election years when compared with private banks. In contrast to earlier findings, state-owned banks curb aggregate credit prior to local elections and increase lending immediately afterwards However, this result is driven by cross-sectional reallocation of credit between constituencies defined by their political alignment and the degree of electoral competition. In particular, state bank lending increases in provinces when an incumbent mayor aligned with the ruling party faces competition from opposition parties. In contrast, closely contested provinces get relatively less credit from state banks in the run up to elections if the incumbent mayor is from an opposition party. We interpret this vastly different behaviour of state banks around elections as strong evidence for the existence of a political lending cycle. It appears that the central government strategically targets provinces either to support their own mayors, or to punish opposition mayors, so that their candidates have a better chance in upcoming elections.

Election cycles and close election outcomes provide a quasi-exogenous variation in how aggregate credit is allocated across the country. In our second set of findings, we present evidence that local economic activity is influenced by this reallocation. In particular, economic output – as measured by private sector building activity – suffers in provinces with an opposition mayor and close electoral competition when compared against provinces with aligned mayors. In line with the interpretation that this reallocation of economic activity is driven by the political lending cycle, we find that credit extended to the corporate sector follows the same pattern.

Our identification strategy builds on difference-in-differences estimates that exploit the greater susceptibility of state-owned banks to political pressure compared with private banks. We use cross-sectional variation in electoral competition and political alignment across localities to identify elements of tactical redistribution and rule out alternative explanations. On the one hand, this helps us eliminate demand-driven explanations of the lending cycle, since local economic shocks that may be correlated with the election cycle should affect private banks equally. On the other hand, private banks may also be subject political influence, and they may respond to competition from state banks. In that case, our estimations constitute a lower bound for the true size of the political cycle.

Our setting also allows us to differentiate between some of the main mechanisms of political resource reallocation suggested in earlier work. The literature on targeted redistribution distinguishes between constant patronage, which refers to rewarding core supporters (Cox & McCubbins, 1986), and tactical redistribution, which aims to achieve electoral gains by targeting politically competitive regions around elections (Dixit & Londregan, 1996). "Patronage" involves awarding areas in which the incumbent party might enjoy strong support. Such constituencies would absorb a disproportionate amount of resources regardless of the electoral cycle. "Tactical redistribution" predicts that resources will be directed towards 'swing' districts either to change the election outcome, in which case we are more likely to see an impact prior to the election, or rewarding the party's strongholds, where one would expect to see a post-election impact. Our results provide strong evidence consistent with tactical redistribution, while we also find some evidence supporting the constant patronage argument.

We contribute to two strands of the literature. First, we provide new evidence on political cycles and mechanisms underlying tactical redistribution. Inspired by theories of opportunistic political cycles,[1] earlier studies investigate the effect of elections on governments' tax revenues and budget deficits.[2] Evidence shows that such political budget cycles are prevalent across the world, especially in developing countries and young democracies (Akhmedov & Zhuravskaya, 2004; Shi & Svensson, 2006; Brender & Drazen, 2008). A more recent set of papers asks whether lending by state-owned banks follows a political cycle. Dinç (2005) finds cross-country evidence that government-owned banks raise lending in national election years compared with private banks. Cole (2009) finds that state banks in India extend more agricultural credit during election years, but with no tangible effect on agricultural output, especially in 'swing' regions.[3] Similarly, Carvalho (2014) shows that Brazilian firms eligible for state-bank lending employ more people in politically attractive regions near elections and in return, these expansions are likely to be financed by state-bank loans. Most recently, Englmaier & Stowasser (2017) find that

[1] See Nordhaus (1975), MacRae (1977) and Rogoff & Sibert (1988).

[2] These studies explore the possibility that politicians in power may use the central government's fiscal muscles to boost the economy and improve their own re-election prospects. However, there is a chance that sophisticated voters might punish opportunistic governments as in Peltzman (1992), although this would require fully-informed voters with plenty of democratic experience (Brender & Drazen, 2005).

[3] Cole (2009) also finds that loan defaults increase after directed lending with no concurrent rise in output, which implies that election-induced loans are not used efficiently.

German savings banks, which are subject to political influence, change their lending behaviour in the run up to local elections.

Our work complements these studies. We take advantage of the Turkish electoral system, which differentiates between the election of district and metropolitan mayors, to create an exact match between political, credit, and real outcomes at the province level. Our identification is strongest in metropolitan provinces where a single mayor is elected by the majority of votes coming from all voters located in that province. This helps us derive more precise estimates for political competition and avoid vote aggregation issues encountered by earlier studies. Furthermore, we draw on a newly available quarterly dataset of bank loans to explore the lending cycle in a higher frequency and differentiate between pre- and post-election behaviour. This is a considerable improvement over previous studies, which analyse lending cycles using yearly observations that do not always correspond to exact election timing.

In terms of mechanisms, our setting is similar to the political capture mechanism described by Brollo & Nannicini (2012) and Carvalho (2014) for Brazil, where state-bank lending is controlled by the central government and reallocated among regions depending on their political attractiveness. We provide evidence that state-bank loans are reallocated towards politically competitive provinces when the incumbent mayor is allied with the ruling party in central government. However, in opposition provinces, this reallocation takes the form of punishment, as credit is withdrawn especially from competitive regions. Our findings suggest that the latter mechanism outweighs the former. Our evidence is therefore consistent with the incentives of "tying your enemy's hands in close races" (Brollo & Nannicini, 2012).

Our second contribution to the literature is on potential benefits and harms of state-owned banks. While government ownership can help solve credit market failures that arise due to coordination problems or information asymmetries (Stiglitz, 1993), they could also end up serving the private interests of the politicians (Shleifer & Vishny, 1994; Shleifer, 1998). In a seminal paper, La Porta, Lopez-de Silanes, & Shleifer (2002) show that state ownership of the banking sector across countries is associated with lower levels of growth, financial development, and government efficiency. Sapienza (2004) uses loan-level data to find that Italian state banks charge lower interest rates to similar firms. This tendency strengthens as the political party associated with the state bank has more support in the region, implying financial favours for its supporters. Similarly, Khwaja & Mian (2005) present evidence that firms in Pakistan with a politician on their board benefit from lower rates and default more often when they borrow from government banks, but not from private ones.[4]

Our paper contributes to this literature by showing that the political lending cycle in Turkey is driven mainly by corporate sector loans, implying that the government prefers enriching the (potentially connected) firms operating in allied regions while impoverishing the ones located on the opposition side. Since such reallocation has real economic effects in the same locality, this could lead to an increase in inequality among provinces of different political affiliations.

The rest of the paper is organised as follows. The next section briefly outlines the Turkish banking industry and gives the institutional background for local elections in Turkey. Section 3 describes the data. Our empirical methodology and results are presented in section 4. Section 5 concludes.

4.2 Institutional Background

4.2.1 The Turkish banking sector

The Turkish financial system is dominated by deposit-taking banks, which are the primary sources of funding in the economy as in other emerging markets. Both state-owned and private banks provide banking services through nation-wide branch networks, and there are no local or regional banks. Banks primarily lend to corporates and households with no particular sectoral specialisation, having left behind the episode of fiscal repression and funding government deficits of the 1980's and 1990's.

The shift in Turkish banking activity toward private sector financing followed an intensive

[4] See also Leuz & Oberholzer-Gee (2006), who detect a negative relationship between political connectedness of Indonesian firms and their foreign financing; this is consistent with the view that connected firms can obtain cheap financing from government banks and do not benefit from foreign financing.

restructuring phase, which was instigated by the twin currency and banking crises that struck the country between 1999 and 2001.[5] More than 15 banks failed during the episode and many were taken over by the country's Savings Deposit Insurance Fund (SDIF). An extensive reform package was initiated under the guidance of the International Monetary Fund (IMF) to strengthen the operational efficiency and financial stability of the banking sector. The central bank gained its institutional independence by law, while an independent Banking Regulation and Supervision Agency (BRSA) was established to solve the conflict of interest problem in bank supervision.[6] The BRSA was also given the sole right to issue new banking permits, which had been at the hands of the central government's Council of Ministers and therefore heavily politicised. In early 2003, BRSA pushed through the early adoption of Basel II capital adequacy standards. In 2004, a limited deposit insurance scheme was introduced and replaced the previously unlimited coverage for all financial institutions.

These reforms have undeniably improved the institutional quality of the Turkish banking sector, which escaped the global financial crisis of 2008-09 unscathed. They also arguably minimised government interference in banking, except via direct ownership. State authorities retain controlling shares in all three deposit-taking state banks – *Ziraatbank, Halkbank,* and *Vakifbank* –, while they have no direct influence over private banks. Therefore, our sample period, which starts around the time that these reforms took effect, constitutes an ideal period to investigate the influence of the central government on state-owned banks. Even though such influence has always existed in the Turkish political sphere, we expect the ownership to be the only channel through which government may exert pressure on the banking system during the period under study.[7]

Table 4.1 shows how deposit-taking banks in Turkey have evolved over the past two decades.[8]

[5] One of the root causes of these crises was the heavy involvement of the banks in the domestic government debt market, which has since receded. For a detailed discussion, see Akyüz & Boratav (2003).

[6] Up until 2000, the Treasury and the Central Bank shared the responsibility for bank supervision. These institutions were not able to step in to prevent the excessive carry-trade tendency when weakly-capitalised banks started financing Turkish government debt with cheap borrowing from abroad and exposed themselves to massive currency risks (see Baum, Caglayan, & Talavera, 2010).

[7] In the coalition governments of 1990s, for instance, it was common practice to share control of state banks among coalition parties based on their vote shares (Önder & Özyıldırım, 2013).

[8] Note that information in Table 4.1 does not include investment banks, development banks, or banks under the management of SDIF.

Panel A indicates that the sector has shrunk in size considerably between 1999 and 2004 following the financial stability programme. In total 20 banks were closed down, while state banks became much leaner by shedding branches and personnel. However, both state and private banks flourished since then, expanding their branch network and employees considerably. The sector consolidated on the private side through entry or mergers involving new and foreign banks. The three state banks were initially aimed to be privatised as part of the post-crisis restructuring programme, but these plans were never put into practice by the government.

Panel B shows that the formation of a uniform supervisory and regulatory system levelled the playing field for private and state banks. State banks have substantially improved their loan quality and capital buffers since 2004. More importantly, private and state banks have converged to a similar level of financial performance over time. This ensures that our identification strategy is immune to operational differences or balance sheet effects between these two sets of banks. State and private deposit-taking banks have typically controlled 30% and 60% of total banking assets, respectively. Their shares in total deposits and lending have been similar. Banking sector in general has experienced a strong growth, nearly doubling its size with respect to country's GDP since 2004.[9]

4.2.2 Politics and local elections in Turkey

Turkey is a parliamentary democracy with a multi-party political system. The Prime Minister, typically the leader of a political coalition, serves as the head of government and exercises executive powers with the Council of Ministers. The current ruling party, AKP (*Adalet ve Kalkinma Partisi*), has been in power since 2002 and retained its majority of seats in parliament through several general elections. The AKP inherited the IMF-led reforms of 1999-2001 and successfully implemented them, bringing public expenditures under control, strengthening the overall quality of institutions, and starting accession negotiations with the European Union in 2005.[10]

[9] See Table 4.A1 for the growth in assets, loans and deposit activity separately for state and private banks since 1999.

[10] See Acemoglu & Ucer (2015) for a discussion of Turkish politics and institutions under the AKP rule.

Turkey is divided into 81 provinces (or cities) for administrative purposes, which are further divided into 923 districts. Each district corresponds to a constituency in a local election. Out of the 81 provinces, 30 are designated as metropolitan municipalities. A metropolitan municipality consists of all districts within the borders of that province, and a metropolitan mayor is elected by the majority of votes cast in that province.[11] The electorate in metropolitan areas also votes for district mayors on the same election day. Voters in non-metropolitan areas only vote for mayoral candidates of the district they live in. The major contest among political parties is to have their candidate elected as the metropolitan mayor in metropolitan provinces, and as the mayor of the central district in the remaining provinces.

Local elections are held every five years on the same day throughout the country. Our sample period covers three local elections held in 2004, 2009, and 2014, at the end of March in each case. On the one hand, this means that we cannot exploit time variation across provinces in elections. On the other hand, it removes any bias from endogeneity of election timing, which may arise if early elections are called when the local economy is doing particularly well (Cole, 2009). Although early local elections are possible *de jure* in Turkey, *de facto* they do not exist in the country's political culture.[12] We focus on political cycles based on local, as opposed to general, elections to identify possible effects on bank lending and economic outcomes.[13] The reasons for this are twofold.

First, as Turkey gradually shifted from coalition governments to single-party governments over the past two decades, local elections have become more instrumental in expanding the power base of the ruling party. Mayors have become more visible in national politics, and some metropolitan municipalities have commanded substantial political clout.[14] These developments are consistent with the political model of Brollo & Nannicini (2012), in which voters are unable to distinguish the sources of government transfers and political spillovers occur in favour

[11] As discussed later, this helps us have a better correspondence between election and credit data in metropolitan provinces.

[12] There has never been an early local election in Turkey since 1982.

[13] General elections are held in different years from local elections, and frequently called early by the central government opportunistically. There were four national elections in our sample period: 2007, 2011, 2015 (June), and 2015 (November).

[14] Indeed, current President Recep T. Erdogan served as mayor of Istanbul between 1994 and 1998, before he set up the AKP that has ruled the country since 2002. See İncioğlu (2002) and Sayarı (2014) for the rising importance of local elections in Turkey.

of municipal governments. The central government may then use transfers to favour political friends or to punish political enemies at the local level, since mayoral candidates can be important allies for the central government once elected (Brollo & Nannicini, 2012). In addition, the single-party AKP government has rarely faced any competition at national elections during our sample period. Thus, it is reasonable to expect that any potential reallocation of resources should follow local elections, especially where the ruling party in central government faces real competition to "win" or "lose" certain provinces.

Second, province-level vote shares of political parties at national elections do not translate directly into the number of seats gained in parliament, and thereby into political influence over resource transfers. This is due to the presence of a relatively high election threshold, which requires each political party to receive at least 10% of the national vote to enter the parliament. This makes it impossible to have a clear measure of the actual province-level electoral contest, since votes for parties that fail to clear the national threshold are redistributed among remaining parties in each province. The number of legislators that go to parties with at least 10% of the national vote are artificially increased as a result. We believe that such uncertainty regarding the number of legislative seats that can be won at the province level deters the central government from pursuing a regional targeting policy.[15] In contrast, competition in a local election is straightforward to quantify and more visible as it resembles to a single-winner voting system, in which the party that gets the most votes wins the constituency. Therefore, our focus on local elections helps us understand tactical reallocation by the central government when it faces a clear competitive threat to win or lose a region.

4.3 Data

There are three main data sets that we exploit in our analysis. Our first dataset combines various sources with detailed banking information. We use annual bank credit data provided

[15] Baum et al., 2010 check for parliamentary election cycles in the Turkish banking sector from 1963 to 2007 and find no evidence of a meaningful difference between state and private banks. This could be due to two possible reasons. Either governments do not resort to such tactics for general elections, or political influence also affects private banks, as it used to be the case before 2001.

by the Central Bank of the Republic of Turkey (CBRT) and the Banks Association of Turkey (BAT). We combine these two datasets and eliminate the pre-crisis era, focusing instead on the period characterised by the single-party government. This gives us the year-end total cash loan exposure of each bank type (state or private) in each of Turkey's 81 provinces from 2003 to 2016. Additionally, we benefit from the FinTürk database maintained by the Banking Regulatory and Supervisory Agency (BRSA). BRSA provides quarterly province-level data on credit extended by state and private banks since the fourth quarter of 2007. These data constitute the universe of bank cash and non-cash loans in the country, and they are further broken down by credit extended to different sectors (e.g. corporate vs consumer). They cover 81 provinces over 37 quarters for different bank types, which gives us the opportunity to employ higher frequency data around elections and differentiate between pre- and post-election effects. In addition, we collect quarterly data on bank branches from FinTürk, again at the level of province and bank type.

Our second dataset contains measures of real economic outcomes. Since Turkey provides economic indicators typically at a more aggregate subregional level, we resort to a different proxy for economic activity at the province level.[16] In particular, we obtain records of construction permits issued by local municipalities from the Turkish Statistical Institute (TurkStat). These permits are a standard requirement for any entity to start a construction project. We believe that new construction activity in a province provides a good proxy for local economic activity. We obtain information on all buildings constructed in each province between 2003 and 2016, including total number of flats and houses, square-meters covered, and monetary value (in Turkish Liras). These data are also broken down by ownership (private vs public sector); we only keep private sector construction in our sample to avoid the possibility that state-funded projects might be targeted independent of credit conditions.[17]

Our third data set consists of local election outcomes. We obtain information on district- and metropolitan-level votes for each political party from TurkStat. Based on these data, we create

[16] Turkey follows EuroStat's NUTS (Nomenclature of Territorial Units for Statistics) designation for regions. There are 81 provinces at the NUTS-3 level, 26 subregions at the NUTS-2 level, and 12 regions at the NUTS-1 level.

[17] Marschall, Aydogan, & Bulut (2016) provide evidence consistent with the view that government-funded building projects in Turkey might be politically motivated.

two political variables. The first is a measure of political competition (or contestedness) that captures the margin of victory/loss by the ruling-party (AKP) candidate against the most popular opposition (non-AKP) candidate. Formally, we construct the following *Competition* variable:

$$Competition_{p,t} = 1 - |WinMargin_{p,t}|$$

where p stands for province, t indicates the particular election and *WinMargin* denotes the difference in the share of votes won by the ruling party's candidate and the most popular opposition candidate. Thus, *Competition* takes values between 0 and 1, with values closer to 1 indicating close electoral competition. For instance, in the extreme case that the top two candidates get the exact same share of votes (which is never observed in our sample), *Competition* would equal 1. To capture province-level competition, we work with the win margin in the election of metropolitan mayors in metropolitan areas. For non-metropolitan areas, we use the corresponding value for the central district of the province.

Our second political variable is a dummy for political alignment (or incumbency), which indicates whether the ruling-party (AKP) candidate wins (i.e., gets the highest number of votes) in that province or not. Recall that voters elect both district and metropolitan mayors in metropolitan provinces, while they elect only a district mayor in non-metropolitan provinces. However, our credit data are only available at the province level, which means we need to aggregate voting outcomes to define a province-level measure of alignment. Previous literature deals with this problem by averaging voting outcomes across constituencies of a region (see, for instance, Cole, 2009). However, this approach may be inappropriate in our setting. Unlike most previous studies, in which political pressure is applied by *local* governments on *local* state banks, our setting predicts political influence by the *central* government on *national* state banks. Thus, tactical reallocation not only depends on electoral competition in a province, but also crucially on whether the province is currently aligned or not.[18] This forces us to have a cleaner measure of alliance than averaging across districts.

[18] Alliance with the central or federal government does not matter in the political settings of Sapienza (2004), Cole (2009) or Englmaier & Stowasser (2017), where locally elected governments have a direct influence on state banks that operate locally. Carvalho (2014) has a setting similar to ours, in which the central government in Brazil manipulates state-bank lending to help re-elect allied state governors.

We tackle this problem by concentrating on the metropolitan mayors and, in non-metropolitan provinces, on the central district mayors. This gives us a direct measure of alliance for each province. However, this matching is still not ideal for non-metropolitan provinces, since some central districts – even though they are the largest by population within a province – do not always represent the political dynamics of the whole province. This can be seen in Figure 4.1, which shows the alliance of elected district mayors in two non-metropolitan provinces during 2004 elections. Panel A shows that in Muş, the only aligned district was the central district, where the electorate represented less than half of all voters (48.3%) in that province. In contrast, the central district in Kastamonu (Panel B) was not aligned with the ruling party; however, a large portion of the province (43.9% by votes) was still governed by an aligned mayor. If politically induced lending occurs at the level of districts, this may create some measurement error and lead to attenuation bias in our estimates. We therefore base our main findings on results from metropolitan provinces, where the elected mayor represents the whole electorate and acts as the main political figure in the province.[19] Our estimates from the metropolitan sample should thus be free of measurement error. Nevertheless, we will also report our findings from a full sample that also includes non-metropolitan provinces.

Table 4.2 presents summary statistics for the main variables in our analysis. During our sample period, 60% of provinces on average are classified as politically aligned with the ruling party. There is a fair degree of electoral competition, as the win margin in the median province is 14 percentage points.

[19]Given the rising importance of metropolitan mayors in the Turkish political sphere and their importance in the overall economy, we also believe that the central government is more likely to strategically target metropolitan provinces.

4.4 Methodology & Results

4.4.1 Identification strategy

We start with a simple *difference-in-differences* (DD) methodology in a balanced panel setting to investigate political cycles. We use government ownership of banks as our 'treatment', which captures political influence by the central government over local lending. Our control group includes all privately-owned banks that operate in the same provinces. If there is politically induced lending, then political pressure on state-owned banks should *intensify* around election years. We therefore expect state banks to alter their lending behaviour closer to elections compared with private banks. To the extent that the effect of politicians on lending decisions by state banks is stable over time, or that politicians might also affect private banks around elections, our DD estimates provide a lower bound for the true size of politically induced lending.

The essence of DD relies on the premise that treated and untreated groups share a parallel trend in the absence of treatment (Angrist & Pischke, 2009). Figure 4.2 shows the evolution of total cash loans extended by state and private banks since 2003 (in levels on the left panel and in logs on the right panel). Aggregate credit has been on a stable trajectory for both state and private banks throughout this period. Two exceptions to these trends appear in 2009 and 2016, when lending by private banks have actually contracted due to significant slowdown in the Turkish economy.[20] Our DD strategy should be immune to year-specific shocks to the extent that economic slowdowns affect all provinces or bank types similarly. Nevertheless, we carry out extensive checks to ensure that no single election or unobserved province- or bank type-specific shocks drive our results. Moreover, we include the number of local branches by bank type in each of our regressions. This should help us control for any long-term credit demand and supply conditions in each province by bank type, and potential sorting of banks that may be linked to regional unobservables.

As discussed before, we mainly search for tactical redistribution prior to elections in our context

[20]Turkey experienced a recession in 2009 due to the global financial crisis while growth slowed down in 2016 due to increased uncertainty, heightened by a failed coup attempt in July.

while still being open to the possibility of patronage in non-election years. To test this idea, we make use of the full time-series and cross-sectional dimensions of our dataset. Formally, we adopt a *triple difference-in-differences* (DDD) model and test whether highly contested provinces get more/less credit from state banks around elections when compared with private banks. The DDD model allows us to control for a full set of province-by-year or bank type-by-year fixed effects. This helps us eliminate any unobserved province- or bank-specific shocks that may be correlated with election cycles.

Indeed, a key feature of our identification comes from the fact that we test the differential allocation of state-bank credit towards 'swing' provinces over the entire election cycle instead of only comparing election versus non-election years. This gives us a full picture of the evolution of political pressure on state-banks, and provides a much more powerful test of election-induced lending. In fact, bank credit cycles over time could be explained by reasons unrelated to politics (such as banks' different sensitivities to political uncertainty). Cross-sectional allocation of credit towards certain provinces could be related to province-specific factors (such as concentration of certain sectors in certain provinces). However, it is almost impossible to explain why such cross-sectional relationships would vary over time specifically around elections without resorting to an explanation based on political incentives (Cole, 2009).

4.4.2 Is there an election cycle in state-bank credit?

We start by testing whether state banks adjust their overall lending behaviour around elections compared with private banks using a standard DD model. Consider:

$$LogCredit_{b,p,t} = \beta_\tau StateBank_b \times Election_{t+\tau} + \delta X_{b,p,t-1} + \theta_b + \gamma_p + \lambda_t + \varepsilon_{b,p,t} \quad (4.1)$$

where b is an index for bank type (state or private), p stands for province, and t denotes years in the yearly data (CBRT) and year-quarters in the quarterly data (FinTürk). $StateBank_b$ is a dummy variable indicating state-owned banks. Importantly, $Election_t$ equals one in the year before a local election and zero otherwise. Since all three elections are held in March, this strategy ensures that we capture a pre-election rather than a post-election effect in our yearly regressions.[21] To document the full election cycle, we generalise the definition of $Election_t$ to $Election_{t+\tau}$ and re-run regressions where τ takes values from -2 to +2 indicating the number of years around elections. For instance, we have $\tau = 1$ to indicate the first year-end after an election (corresponding to 2004, 2009 and 2014).

Our coefficient of interest in Equation 4.1 is β_τ and captures the behaviour of state banks compared with private banks at each point over the election cycle. We include fixed effects at the levels of bank type, province, and time in our baseline. Lastly, $X_{b,p,t-1}$ includes lagged number of bank branches, which control for local market shares separately for each bank-type. We cluster standard errors in all of our regressions at the province level, since local credit outcomes are likely to be correlated across time within localities.

Table 4.3 presents results on the election year (i.e., $\tau = 0$) for the full sample and the subsample of metropolitan provinces. In both samples and across different sets of controls, state banks decrease credit supply with respect to private banks in the run up to local elections.[22] This is the case even when all province-specific and time-varying factors are non-parametrically controlled (Columns IV and VIII), where all relevant local shocks to credit demand such as unemployment or growth are absorbed. State bank lending is between 10.3% and 14.2% lower compared with private-bank lending in election years.

Figure 4.3 shows results for the whole election cycle from regressions that control for local branches, baseline fixed effects, and province time trends. Each plotted coefficient corresponds to a single regression with an estimate of β_τ when τ is equal to -2, -1, 0, +1 or +2. Hence, coefficient estimates for $\tau = 0$ in Panels A and B equal estimates reported in Columns III and VII, respectively, of Table 4.3. The figure shows that state banks start curbing credit with respect to private banks one year before an election, and they further reduce lending in an election year. However, they increase lending on a larger scale than private banks directly afterwards. This cycle seems slightly stronger in metropolitan provinces than in our full sample.

[21] Note that this approach is also in line with previous literature (Englmaier & Stowasser, 2017).

[22] Table 4.A2 shows that this result is not driven by a particular local election in our sample period.

This finding may at first seem counter-intuitive, since most earlier studies document a rise in state-bank lending in the run up to elections. There are two reasons why earlier findings and ours actually complement, rather than contradict, each other. First, our focus is on local election cycles rather than general elections that have been studied by previous literature (Dinç, 2005). In local elections, a central government's control over state banks leads to different incentives across provinces depending on their political attractiveness (Brollo & Nannicini, 2012; Carvalho, 2014). Therefore, local elections do not necessarily imply an overall pre-election credit boom in the country. Second, earlier studies that investigate local elections and bank credit typically have political settings in which local governments are in direct control of local state banks (Cole, 2009; Englmaier & Stowasser, 2017). In that case, each local government would have an incentive to encourage pre-election lending to increase their re-election prospects, and thus there would be an overall credit boom in the country before elections. However, our political pressure channel goes from central government to state banks, which predicts a reallocation of credit across provinces but does not necessitate a rise in aggregate lending.

We next zoom in on the whole election cycle and check how lending by state banks evolves in the quarters immediately up to and after local elections. For this purpose, we estimate Equation 4.1 with the quarterly data provided by FinTürk over the period between 2007 and 2016, which covers two local elections. $Election_t$ now takes the value of 1 in the first quarters of 2009 and 2014 as well as in the preceding three quarters (and 0 otherwise).[23] Thus, we can differentiate exactly between pre- and post-election outcomes since the $Election_t$ dummy covers the four quarters immediately before the election takes place.

Table 4.4 presents the results. In line with our earlier findings from yearly data, state banks reduce their lending in the four quarters up to and including elections when compared with private banks. Point estimates range from 6.4% in the full sample to 11.2% in the metropolitan sample; all coefficients are estimated with a high level of statistical significance across different sets of controls.

The main advantage of working with quarterly data is that we can pinpoint exactly when state

[23] Exact election dates are 29 March in 2009 and 30 March in 2014.

banks alter their lending behaviour. We therefore extend our definition of the election variable to the whole cycle by employing a rolling definition of $Election_{t+\tau}$, where τ corresponds to the quarters before and after elections. For instance, $Election_{t-2}$ equals 1 for two to six quarters prior to the election and 0 otherwise.

Figure 4.4 plots coefficients for the entire credit cycle. Lending by state banks hits rock bottom compared with private banks either in the quarter in which elections take place or just before. In metropolitan provinces, state-bank credit hits a trough at -11.4% two quarters before local elections, while it hits a trough at -6.7% in the election quarter in the full sample. This negative effect is estimated with precision in the five quarters leading up to the election and persists for another two to three quarters following it. These findings clearly illustrate that state bank credit is subject to a cycle around local elections. State banks reduce their lending prior to local elections and boost it afterwards compared with private banks, especially in metropolitan provinces.

An important implication of these findings is that low frequency data may not be optimal to explore electoral cycles in bank lending. This point was first made by Akhmedov & Zhuravskaya (2004) in the context of political budget cycles. As the use of annual data do not allow a clear differentiation between pre- and post-election outcomes, studies may misinterpret the post-election rise in credit as direct evidence of political incentives. For instance, if $Election_t$ dummy in Equation 4.1 was defined as the actual election year instead of the year before, our estimates in Table 4.3 would come out as significantly positive.[24]. However, as can be seen from Figure 4.3/ 4.4, this would only be a post-election effect, which may not be directly driven by political motives.[25]

Although we find evidence that state banks' lending behaviour changes around elections, it is important to note that such intertemporal reallocation does not strictly imply political manipulation. It is possible that state banks are more sensitive than private banks to overall political

[24]Notice that estimates for β_τ would then be the same as current estimates for $\beta_{\tau+1}$ in Figure 4.3.

[25]In fact, Önder & Özyıldırım (2013) find that state banks in Turkey increase their share in the credit market during local elections; the authors use the same yearly dataset as we do but with the definition of 'actual election year' and interpret their findings as a sign of political manipulation. As obvious from the discussion above, such a conclusion might be biased.

uncertainty induced by local elections. As a result they may choose to postpone lending decisions until after elections take place. Since we document a recovery in state-bank lending a few quarters after elections, we do not yet rule out this possibility.

4.4.3 Is there tactical redistribution across provinces?

We now test the existence of political incentives behind the intertemporal reallocation of state-bank credit over the local election cycle. Note that redistributing credit is not costless and that the central government's incentive to distort bank policies increases with the marginal utility of receiving additional votes (Englmaier & Stowasser, 2017). Undoubtedly, this marginal utility is highest where a small number of votes can determine the outcome; that is, in closely contested elections. We should therefore find stronger reallocation of credit in provinces with high electoral competition if the election-induced cycle is driven (at least partly) by political goals. To test this idea, we extend Equation 4.1 to a *triple difference-in-differences* model as in the following:

$$\begin{aligned} LogCredit_{b,p,t} = {} & \beta_\tau Comp_{p,t} \times StateBank_b \times Election_{t+\tau} + \alpha_1 Comp_{p,t} \times StateBank_b \\ & + \alpha_2 StateBank_b \times Election_{t+\tau} + \alpha_3 Comp_{p,t} \times Election_{t+\tau} \\ & + \alpha_4 Comp_{p,t} + \delta X_{b,p,t-1} + \theta_b + \gamma_p + \lambda_t + \varepsilon_{b,p,t} \end{aligned} \tag{4.2}$$

where $Comp_{p,t}$ represents the *Competition* variable created in Section 4.3. Notice that $Comp_{p,t}$ is time-varying and we need to make an assumption on political contestedness for non-election years. We follow the literature in assuming that competition for the next two years after an election is captured by the previous election outcome, while it is captured by an upcoming election outcome for the two years before an election in that constituency (Cole, 2009; Englmaier & Stowasser, 2017). Despite the obvious endogeneity concern between credit as a dependent variable and competition as an independent variable in Equation 4.2, we believe it is reasonable to assume that political redistribution of credit would not change election outcomes by such a high margin as to make an election uncompetitive.[26]

Our main coefficient of interest in Equation 4.2 is the triple-interaction effect denoted by β_τ. It captures the impact of rising political competition in a province on the difference between state-bank and private-bank lending during an election year (i.e., when $\tau = 0$). The two-way interactions underlying the triple effect absorb economically important effects and are also of interest. Based on the discussion in Section 4.4.1, α_1 accounts for the possibility that state banks may differ in their local lending behaviour depending on the political attractiveness of a province independent of an election cycle. Similarly, α_2 captures any election-induced effects that may differ between the two types of banks, while α_3 accounts for any responses to elections that may vary across provinces but not bank types. Hence, the model captures any shocks to banks or provinces that may be correlated with either the electoral cycle or the degree of contestedness in an election.

A central government's incentives to redistribute resources across provinces depends not only on political attractiveness, but also on whether the incumbent mayor is a political ally or not. In particular, if a province is currently ruled by a mayor from the ruling party, then the central government has an interest in increasing voter appreciation and the re-election chances of the incumbent mayor. However, the opposite would be true if a mayor from opposition is currently in charge. It is thus optimal from the central government's perspective to increase credit and positively influence economic conditions in politically aligned provinces, and to decrease credit and reduce economic activity in non-aligned provinces. Therefore, we divide our sample into two subsamples based on current mayoral incumbency and condition our expectations of β_τ on political alliance. If *tactical redistribution* exists, we expect $\beta_\tau > 0$ in aligned provinces and $\beta_\tau < 0$ in non-aligned provinces just prior to the elections ($\tau = 0$). As for the *constant patronage* argument: central government would 'normally' (i.e., in non-election periods) be expected to favour its strong supporters (less competitive areas) in allied provinces and more competitive areas in non-allied provinces. Hence, we would expect β_τ to switch its sign further away from elections (for very low or high values of τ).

[26] This does not mean that the central government would not be able to win an election by manipulating credit. It means that any extra lending allocated to a province through state banks would not be able to change the nature of the election, making it competitive or uncompetitive.

We estimate Equation 4.2 on both our yearly and quarterly data. Table 4.5 shows the yearly results for metropolitan provinces, where we expect our identification to be strongest (see Section 4.3). In line with a tactical redistribution mechanism, there is evidence that state banks lend more in provinces with higher political contestedness and an aligned incumbent mayor (i.e., $\beta_\tau > 0$ in columns I-V), while they significantly cut credit in provinces with higher political contestedness but ruled by an opposition mayor (i.e., $\beta_\tau < 0$ in columns VI-X). We report estimates in each sub-sample with varying degrees of saturation in our fixed effects and find especially strong results in non-aligned provinces.

Figure 4.5 reports our yearly results for the whole election cycle by plotting the coefficient estimates of the triple-interaction term (β_τ) for different values of τ.[27] Panel A shows estimates from metropolitan provinces. In politically aligned provinces, state banks lend more than private banks in the election year especially when political competition is high, and this effect persists in the post-election period. In non-aligned provinces, the drop in state-bank lending in the election year similarly persists one year after the election before recovering. These findings suggest that the central government may continue its tactical redistribution even after elections by rewarding constituencies in which it narrowly won, and punishing regions in which it narrowly lost elections. There is also some support for constant patronage hypothesis as β_τ switches signs when the central government does not have electoral concerns but would rather favour areas where it faces stronger support in general ($\tau = -2$ and $+2$). The same patterns are also observed in Panel B, which shows estimates from the full sample of provinces, although coefficients have less precision.

It is crucial to differentiate between pre- and post-election effects to understand the exact nature of the lending cycle. We re-estimate Equation 4.2 with the quarterly data to see the effects of tactical redistribution in a more granular timeline.[28] Table 4.6 shows the corresponding estimates. In line with our yearly results, politically aligned provinces benefit from a relative

[27]The exact model used for the estimates shown in the figure includes our baseline controls and *Province time trends* as in Columns III and VIII in Table 4.5.

[28]As noted earlier, our quarterly observations start from the end of 2007 and hence do not cover the first local election in 2004. However, we do not expect this to be driving our previous results. Indeed, one could predict a more intense pre-election manipulation in the last two local elections since they correspond to a later period in which the ruling party has consolidated its control over government institutions.

rise in credit supply by state banks when elections are closely contested, while non-aligned provinces suffer from a relative reduction. Estimates are statistically significant for non-aligned provinces and comparable to those reported in Table 4.5. A one standard-deviation rise in the competitiveness of an opposition province leads to a decline of almost 6% in state-bank loans on the election year compared with private banks. This effect is quite sizeable given that our credit measure covers the entire state-bank lending in a province. It is also comparable to results by Cole (2009), who finds that state banks increase agricultural lending by 5-10 percentage points in an election year.

Figure 4.6 illustrates the presence of tactical reallocation over the full election cycle, which covers ten quarters before and after an election. It is clear from Panel A that targeted redistribution starts at least four quarters before an election. It is strongest in two to three quarters prior to an election, but it quickly disappears following an election. In both the metropolitan and full samples, politically non-aligned provinces suffer from a relative reduction in lending by state banks for multiple quarters in the run up to closely contested elections. Again, for constant patronage argument, it is clear that β_τ switches signs further away from elections (though not always statistically significant) consistent with the view that without election concerns, central government would favour its strongholds.

We believe that this visual representation of state-bank credit reallocation over the election cycle provides strong evidence of political incentives behind state-bank lending. There could be alternative explanations for why state banks in general would behave differently around elections (e.g. flight to safety amongst depositors induced by political uncertainty). There could also be reasons why certain provinces get a higher share of state-bank loans than others (e.g. banks may specialise in lending to certain industries, which agglomerate in certain provinces). However, without resorting to the argument of political incentives, it is very difficult to explain why such cross-sectional relationships would vary in different directions based on local political alignment and exactly prior to local elections.

In order to shed more light on political incentives, we explore the channels through which the central government engages in tactical redistribution. Our aggregate credit data can be broken down by lending to different segments of the economy. This allows us to test whether targeted lending occurs in certain segments but not others, which helps us understand what voters respond to. On the one hand, politicians may try to induce a quick and direct impact on voters by raising their instant consumption. Healy & Lenz (2014) find that voters judge U.S. presidential candidates on the election-year economy because this is the most immediately available metric to them for judging a president's performance. However, given that province mayors have no direct control over bank credit supply in Turkey which is widely known by the public, it is difficult to argue that a change in consumer loans would have a direct impact on consumers' perception about the incumbent mayor.

On the other hand, politicians may be tempted to use bank credit to boost or contain corporate activity in a region. This would be more likely to influence voting patters if corporates have a say in local politics and voters – at least partly – attribute corporates' economic outcomes to local politicians. For instance, Carvalho (2014) finds evidence in line with this view and shows that the central government in Brazil provides favourable credit to firms in aligned regions, who in turn expand employment to increase the re-election chances of incumbents. Although the consumer and corporate channels are not mutually exclusive, we expect the latter to be dominant in the Turkish political setting given its similarity to that of Brazil.

Figure 4.7 plots quarterly estimates from Equation 4.2 separately for corporate and consumer loans for different values of τ. A simple comparison between Panels A and B confirms our expectation that tactical redistribution is mainly targeted at corporate loans. The coefficient estimates are sizeable and statistically significant for both aligned and non-aligned provinces in the case of corporate loans. On average, a one standard deviation change in competition leads state banks to increase corporate loans by 9.8% in aligned municipalities and reduce it by 15.7% in non-aligned ones in the election year when compared with private banks. The positive impact in aligned provinces peaks precisely on the election quarter, while the negative impact in non-aligned provinces hits the bottom two quarters prior to the election. There is also statistical evidence that these patterns reverse in periods away from elections, supporting the notion that government might be pursuing patronage in those quarters.

In contrast, estimates for consumer loans are all insignificant and show no meaningful visible pattern around elections. We therefore conclude that the central government's reallocation of state-bank credit targets firms' credit access and aims to influence local economic and voting outcomes through the corporate channel. This leads us to investigate the effects of such redistribution on economic activity in the next section.

4.4.4 How does political lending affect economic outcomes?

We have so far established that there is an election-induced cycle in state-bank loans, which affects especially the corporate sector. Such lending is targeted at politically competitive provinces based on their political alignment. In ruling-party constituencies, it takes the form of rewarding the competitive region by increasing credit supply, and in opposition regions, it takes the form of punishment by lowering state-bank lending. This gives us a quasi-exogenous source of variation in the amount of total bank credit around local election times that provinces receive depending on their alignment. We now ask whether this variation in credit translates into real outcomes. If it does, then 'swing' provinces ruled by an opposition mayor are expected to suffer from lower economic activity around elections compared with provinces governed by a politically aligned mayor.

Since there is no data currently available on province-level GDP, we draw on a new dataset that contains all construction permits issued over the sample period in Turkey as a proxy for local economic activity. To abstract from the possibility that central government may directly interfere in the construction industry via state-funded institutions,[29] we only keep private sector activity in our sample. Construction by private entities is likely to be a good proxy for overall economic activity, since it tends to have a high correlation with an economy's growth rate.

[29]See Marschall et al. (2016).

Formally, we estimate the following model:

$$\begin{aligned} LogActivity_{p,t} = &\beta_\tau Opposition_{p,t} \times Comp_{p,t} \times Election_{t+\tau} + \alpha_1 Comp_{p,t} \times Election_{t+\tau} \\ &+ \alpha_2 Comp_{p,t} \times Opposition_{p,t} + \alpha_3 Opposition_{p,t} \times Election_{t+\tau} \\ &+ \alpha_4 Comp_{p,t} + \alpha_5 Opposition_{p,t} + \delta X_{b,p,t-1} + \theta_b + \gamma_p + \lambda_t + \varepsilon_{b,p,t} \end{aligned} \quad (4.3)$$

where $LogActivity_{p,t}$ is computed in three different ways. First, *Log Flats* measures (in logs) the total number of flats constructed by the private sector in province p in year t. Second, *Log SqMtr* measures the total square meter area covered by new construction. Third, *Log Value* measures the total monetary value of new construction. We use all three measures as alternative dependent variables. In order to control for potential drivers of construction activity, we include the lagged population size of each province over time (in logs). $Opposition_{p,t}$ indicates whether a province is governed by a mayor affiliated with an opposition party or not. The main coefficient of interest is β_τ, which measures the economic impact of being in an opposition province with high political contestedness around election times compared with being in an aligned province. If opposition regions suffer from a credit squeeze as we have shown previously, then one would expect to find a negative impact on local economic activity as captured by $\beta_\tau < 0$ around elections.[30]

Table 4.7 presents estimates of this regression for the election year (i.e., $\tau = 0$). For all three of our dependent variables and across varying sets of controls, the triple-interaction term carries a significantly negative value. The estimated effects are economically substantial. *Ceteris paribus*, a one standard deviation increase in electoral competition would decrease the number of flats constructed in an opposition province by almost 10% in an election year. Given that new construction activity accounts directly for around 8-9% of GDP in Turkey, only the effect of credit on construction itself would translate into almost a 1% reduction in total economic output.

To observe the full election cycle in local economic activity, Figure 4.8 plots estimates of β_τ

[30] Since construction sector usually responds to local economic factors with a lag, we define $Election_t$ dummy according to 'actual election' years.

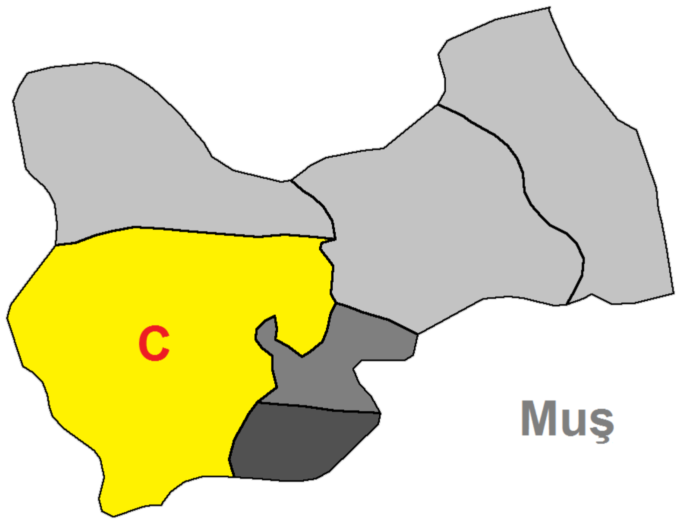
for $\tau = -2, -1, 0, 1, 2$. Panels A-C report results on *Log Flats*, *Log SqMtr*, and *Log Value*, respectively. For all three measures of activity, there is a visible downward trend in opposition areas with high political competition as elections get closer. In line with the persistence of the lending cycle beyond elections documented earlier, we find that construction activity lags in opposition provinces one year after elections take place. Hence, withdrawal of credit by state banks in politically competitive provinces under an opposition mayor leads to a significant distortion of economic activity.

4.5 Concluding Remarks

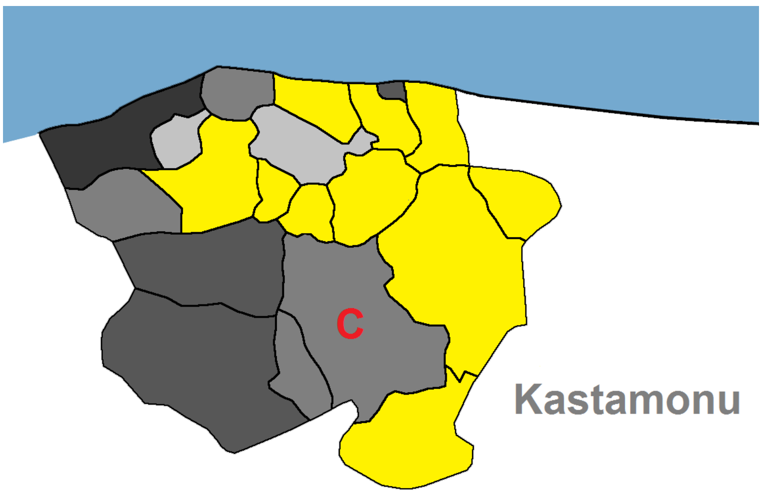
In this paper, we test for the presence of politically motivated distribution of resources in Turkey using a dataset with detailed information on banking activity and local economic outcomes. Our dataset is novel along several dimensions and helps us achieve stronger identification than earlier studies, while shedding light on some of the theoretical arguments voiced in the literature.

Our main findings are two-fold. First, we show that state banks in Turkey engage in politically motivated lending around local elections when compared with private banks. In particular, they increase lending to the corporate sector in politically attractive provinces when an incumbent mayor is aligned with the ruling party, while they reduce it if the incumbent mayor is from an opposition party. Second, this redistribution of credit has real consequences. Specifically, it leads to a significant reduction in local economic activity in opposition provinces that are politically contested.

Our findings around elections support the idea of tactical redistribution. Rolling estimations in non-election years show some evidence that central government may have resorted to patronage when it did not have election concerns. In ongoing work, we ask whether reallocation of bank credit helps the central government increase the electoral success of its allied mayoral candidates. To the extent that it does, it may provide one of the first pieces of evidence on how voters can be manipulated via financial intermediaries.



(a) An allied province in 2004 elections



(b) A non-allied province in 2004 elections

Figure 4.1: **District-level alliances in two non-metropolitan provinces.** Panel A shows a province in which the elected central district mayor is allied with the central government and Panel B shows a province in which the elected central district mayor is non-allied. 'C' in red colour stands for the central district. Allied districts are given in yellow and non-allied districts are given in varying shades of grey corresponding to different opposition parties.

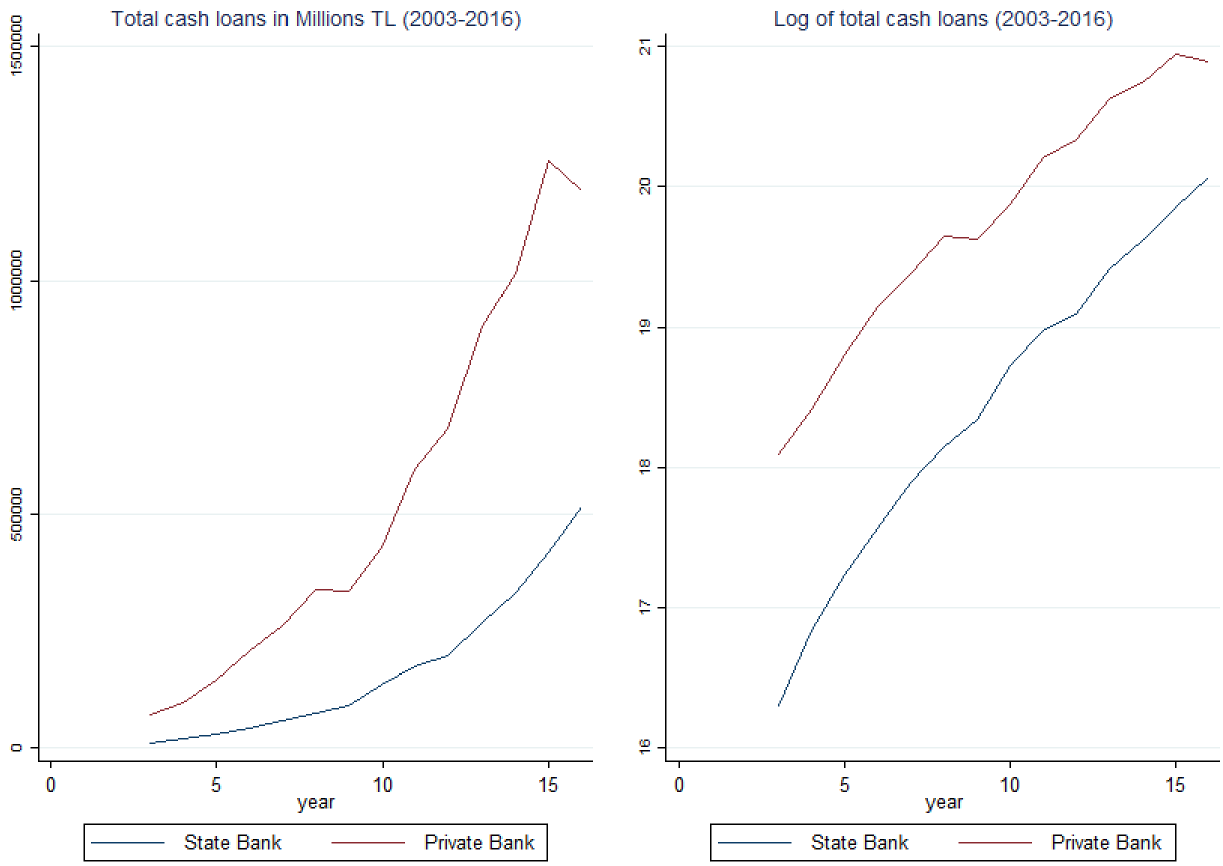
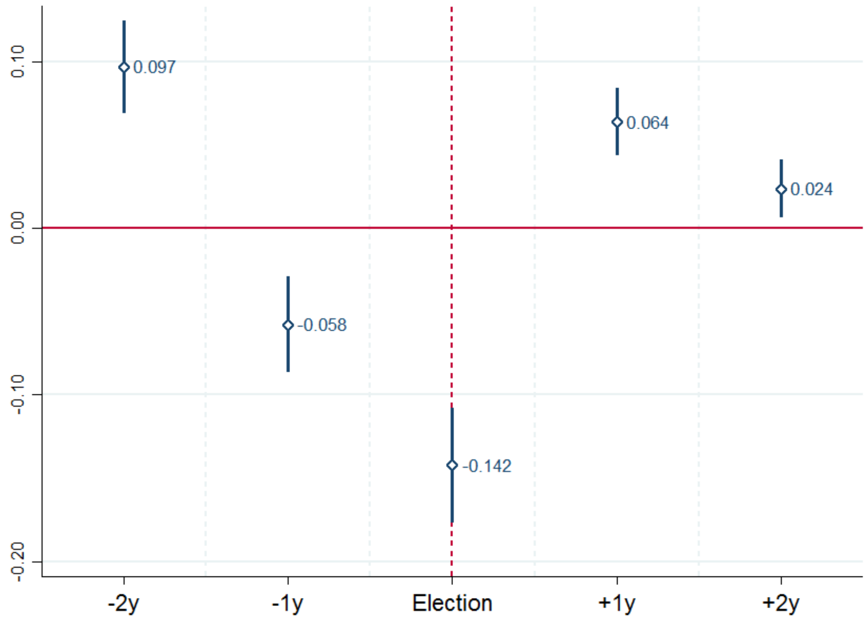
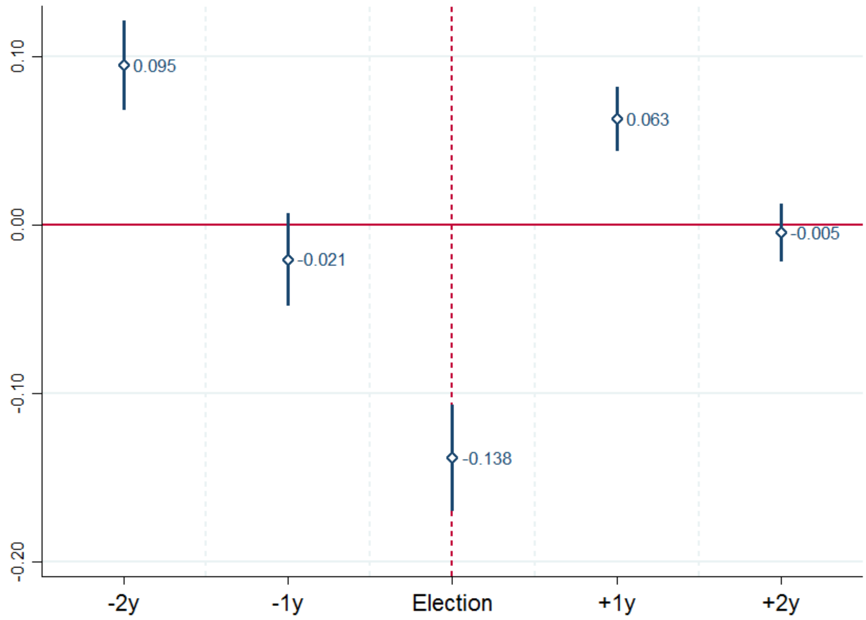


Figure 4.2: **Evolution of aggregate credit by bank type, 2003-2016.** This figure shows the evolution of the stock of all cash loans extended by state-owned and private banks during the period 2003-2016.

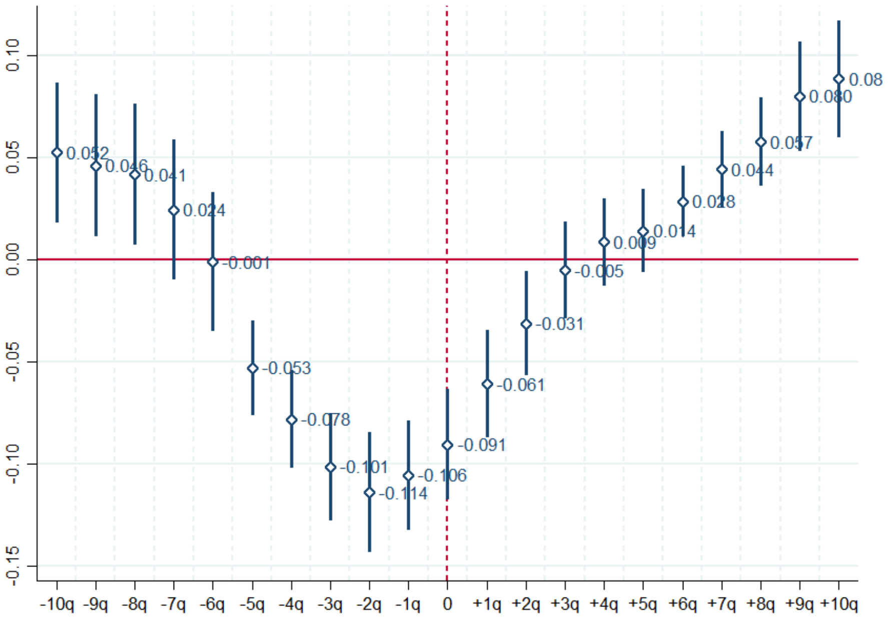


(a) Metropolitan provinces

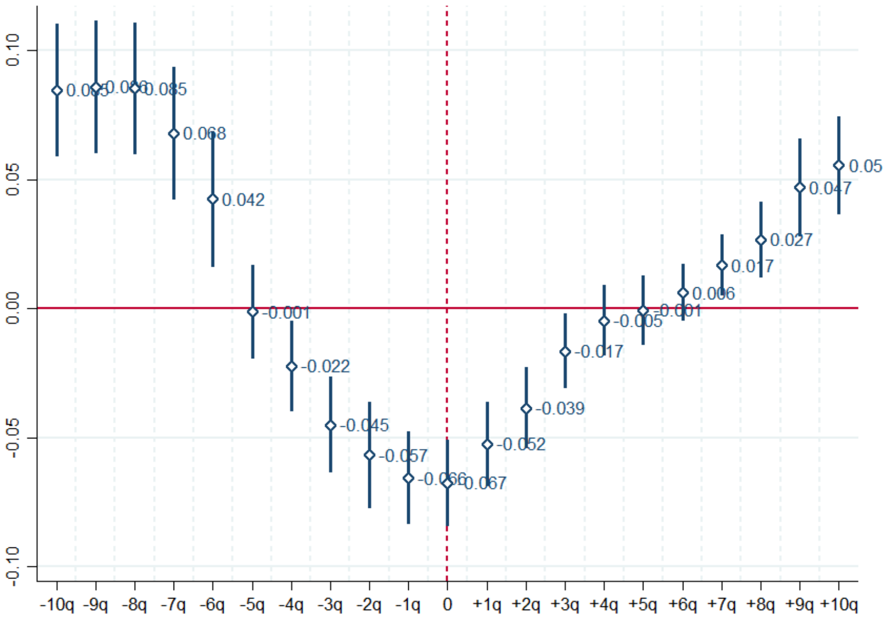


(b) All provinces

Figure 4.3: **State vs private bank behaviour over the full election cycle: Yearly estimates (2003-2016).** This figure shows results of Equation (4.1) estimated on yearly data when τ takes values from -2 to +2, indicating the number of years around elections. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for local branches, our baseline set of fixed effects, and province time trends. Panel A includes metropolitan provinces and panel B includes the full sample.

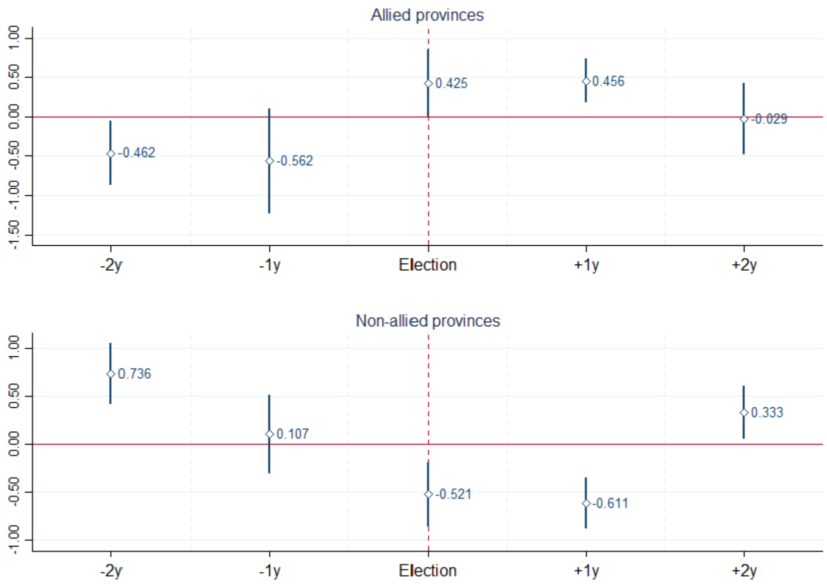


(a) Metropolitan provinces

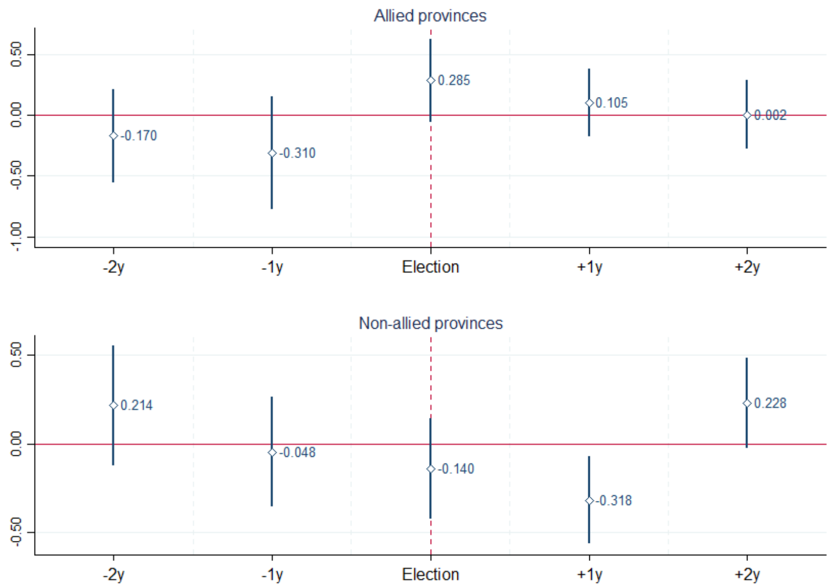


(b) All provinces

Figure 4.4: **State vs private bank behaviour over the full election cycle: Quarterly estimates (2007q4-2016q4).** This figure shows results of Equation (4.1) estimated on quarterly data when τ takes values from -10 to +10, indicating the number of quarters around elections. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for local branches, our baseline set of fixed effects, and province time trends. Panel A includes metropolitan provinces and panel B includes the full sample.

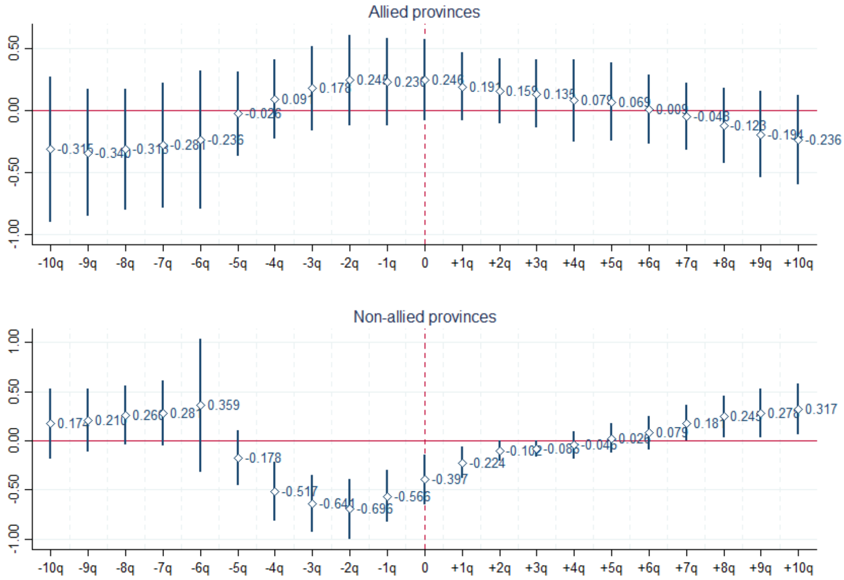


(a) Metropolitan provinces

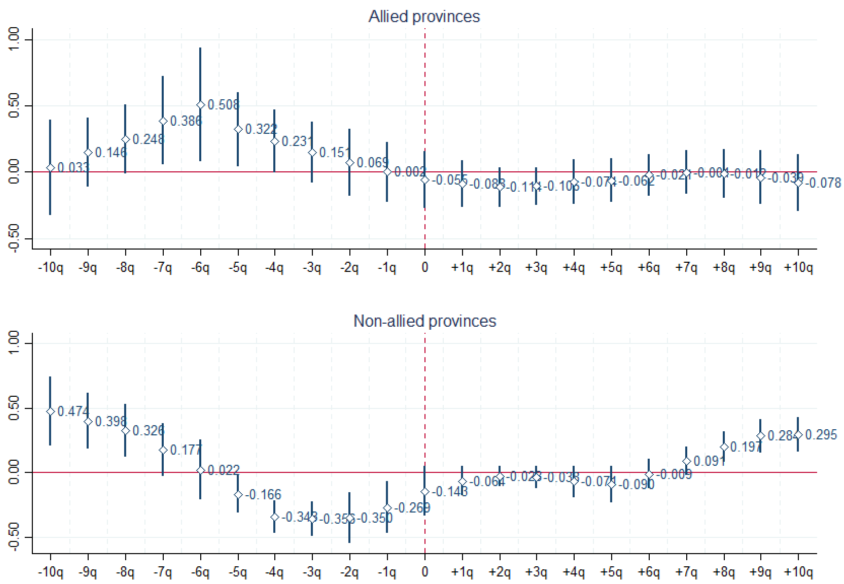


(b) All provinces

Figure 4.5: **Tactical redistribution of state-bank credit over the full election cycle: Yearly estimates (2003-2016).** This figure shows results of Equation (4.2) estimated on yearly data when τ takes values from -2 to +2, indicating the number of years around elections. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for local branches, our baseline set of fixed effects, and province time trends. Panel A includes metropolitan provinces and panel B includes the full sample; estimates are reported separately for aligned and non-aligned provinces in each panel.

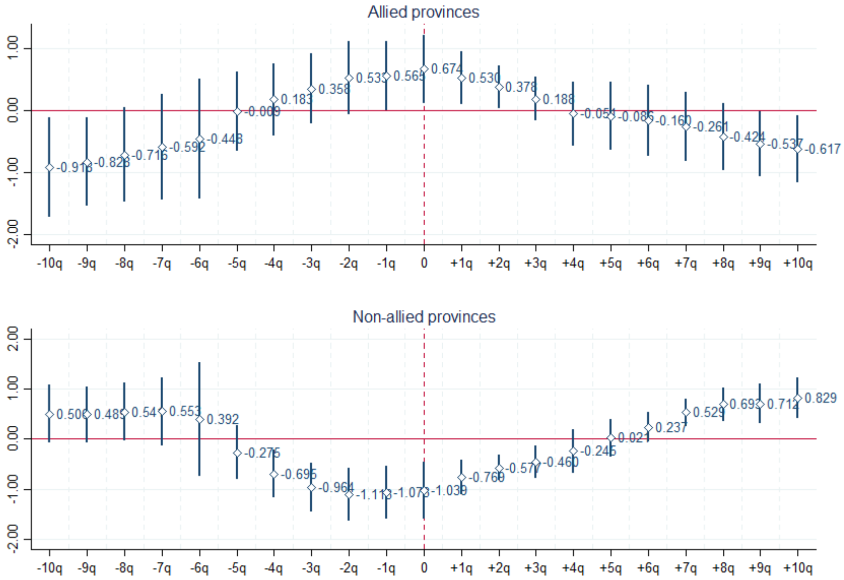


(a) Metropolitan provinces

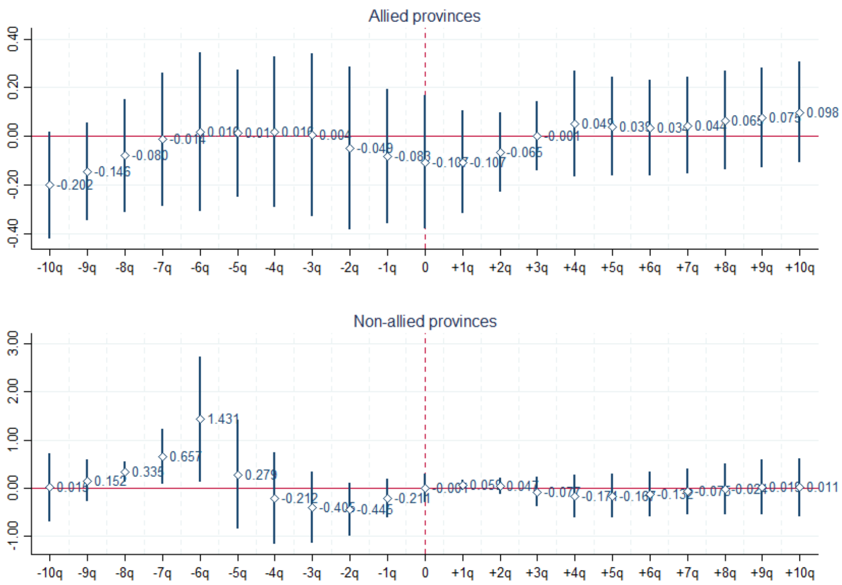


(b) All provinces

Figure 4.6: **Tactical redistribution of state-bank credit over the full election cycle: Quarterly estimates (2007q4-2016q4).** This figure shows results of Equation (4.2) estimated on quarterly data when τ takes values from -10 to +10, indicating the number of quarters around elections. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for local branches, our baseline set of fixed effects, and province time trends. Panel A includes metropolitan provinces and panel B includes the full sample; estimates are reported separately for aligned and non-aligned provinces in each panel.

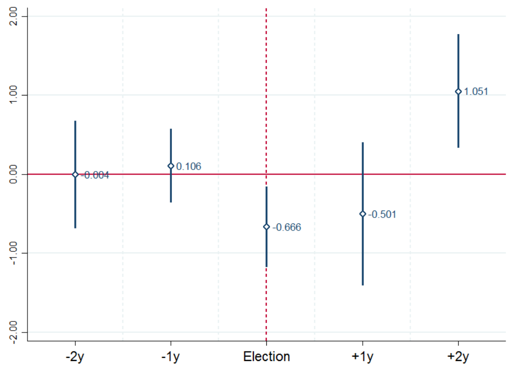


(a) Corporate loans

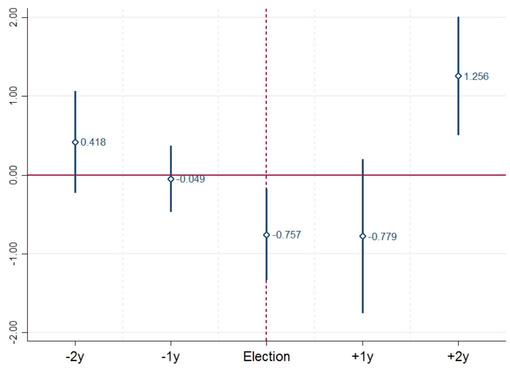


(b) Consumer loans

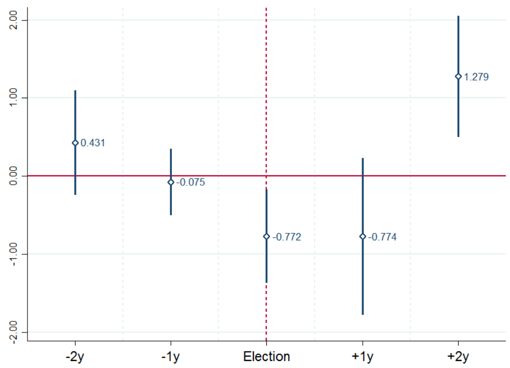
Figure 4.7: **Corporate vs consumer loans: Tactical redistribution of state-bank credit over the full election cycle (2007q4-2016q4).** This figure shows results of Equation (4.2) estimated on quarterly data when τ takes values from -10 to +10, indicating the number of quarters around elections. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for local branches, our baseline set of fixed effects, and province time trends. Panel A shows estimates for corporate loans and panel B shows estimates for consumer loans; estimates are reported separately for aligned and non-aligned provinces in each panel.



(a) Dependent variable: LogFlats



(b) Dependent variable: LogSqMtr



(c) Dependent variable: LogValue

Figure 4.8: **Effect of competition and alliance on local economic activity (2003-2016).** This figure shows results of Equation (4.3) estimated on yearly data when τ takes values from -2 to +2, indicating the number of years around elections. Each plotted coefficient comes from a single regression; bars around estimates show 90% confidence intervals. Each regression controls for our baseline set of fixed effects. Panel A shows estimates for the dependent variable total number of flats, panel B shows estimates for total square meter area and panel C shows estimates for total value in Turkish liras.

Panel A		*Years*		
		1999	2004	2015
Number of banks		**54**	**34**	**33**
	State	4	3	3
	Private	50	31	30
Number of branches		**6,946**	**6,087**	**11,150**
	State	2,865	2,149	3,681
	Private	4,081	3,938	7,469
Number of employees		**152,578**	**122,227**	**195,613**
	State	72,007	39,467	58,211
	Private	80,571	82,760	137,402
Panel B				
NPL/Loans	State	10.0%	11.1%	2.7%
	Private	3.6%	4.9%	3.3%
ROA	State	1.1%	2.5%	1.4%
	Private	4.5%	1.6%	1.0%
Equity/Assets	State	4.1%	9.4%	10.1%
	Private	12.9%	15.8%	11.0%

Table 4.1: **Composition and performance of Turkish banking sector over time.** This table summarizes the composition and financial performance of the banking sector in Turkey. State banks are defined as banks in which the central government has a controlling stake. Private banks are defined as all other banks. We exclude investment banks, development banks, and participation banks. NPL denotes non-performing loans. ROA denotes Return on Assets. Source: Banks Association of Turkey (BAT) & authors' calculations.

Variables	*Mean*	*Median*	*Std. Deviation*	*Min*	*Max*	*Observations*	*Source*
LogCredit	13.28	13.29	1.77	8.05	20.36	2,268	CBRT, BAT
LogCredit (state-bank)	13.14	13.17	1.59	8.51	18.83	1,134	CBRT, BAT
LogCredit (private-bank)	13.42	13.44	1.92	8.05	20.36	1,134	CBRT, BAT
LogCredit	13.95	13.88	1.47	9.41	20.15	5,994	FinTürk
LogCredit (state-bank)	13.85	13.77	1.27	10.80	19.07	2,997	FinTürk
LogCredit (private-bank)	14.05	14.05	1.65	9.41	20.15	2,997	FinTürk
Alliance (dummy)	0.60	1.00	0.49	0.00	1.00	243	TurkStat
Competition	0.8231	0.8568	0.1459	0.2391	0.9996	243	TurkStat
LogFlats	7.90	7.89	1.40	2.56	12.23	1,133	TurkStat
LogSqMtr	13.20	13.19	1.38	7.86	17.32	1,133	TurkStat
LogValue	19.49	19.53	1.53	13.82	24.19	1,133	TurkStat

Table 4.2: **Summary statistics.** This table presents summary statistics for the main variables in our analysis. Credit data from CBRT/BAT are annual, while credit data from FinTürk are quarterly. Alliance indicates whether a province is ruled at the time by a mayor from the ruling party or not. Competition is defined as 1 minus the win margin. Flats, SqMtr, and Value refer to the number of flats, total area measured in m2, and value in Turkish Liras, respectively, of newly constructed buildings.

	Metropolitan Sample				*Full Sample*			
	I	*II*	*III*	*IV*	*V*	*VI*	*VII*	*VIII*
StateBank x Election	-0.128***	-0.142***	-0.142***	-0.142***	-0.103***	-0.138***	-0.138***	-0.139***
	[0.019]	[0.020]	[0.020]	[0.027]	[0.018]	[0.019]	[0.019]	[0.026]
Controls								
Local branches		*Yes*	*Yes*	*Yes*		*Yes*	*Yes*	*Yes*
Bank type FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Province FE	*Yes*	*Yes*	*Yes*		*Yes*	*Yes*	*Yes*	
Time FE	*Yes*	*Yes*	*Yes*		*Yes*	*Yes*	*Yes*	
ProvinceTime trends			*Yes*				*Yes*	
Province x Time FE				*Yes*				*Yes*
Clustering	Province	Province	Province	Province	Province	Province	Province	Province
N	840	840	840	840	2,268	2,268	2,268	2,268
Adj-R-sq	0.947	0.963	0.967	0.950	0.898	0.945	0.951	0.928

Table 4.3: **State-bank behaviour in election years: Yearly estimates (2003-2016).** This table shows results of Equation (4.1) estimated on yearly data. Columns I-IV include metropolitan provinces and columns V-VIII include the full sample. Standard errors are provided in brackets; *, **, *** indicate statistical significance at the level of 10%, 5%, and 1%, respectively.

	Metropolitan Sample				*Full Sample*			
	I	*II*	*III*	*IV*	*V*	*VI*	*VII*	*VIII*
StateBank x Election	-0.112***	-0.090***	-0.091***	-0.090***	-0.064***	-0.068***	-0.067***	-0.068***
	[0.017]	[0.016]	[0.016]	[0.022]	[0.011]	[0.010]	[0.010]	[0.014]
Controls								
Local branches		*Yes*	*Yes*	*Yes*		*Yes*	*Yes*	*Yes*
Bank type FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Province FE	*Yes*	*Yes*	*Yes*		*Yes*	*Yes*	*Yes*	
Time FE	*Yes*	*Yes*	*Yes*		*Yes*	*Yes*	*Yes*	
ProvinceTime trends			*Yes*				*Yes*	
Province x Time FE				*Yes*				*Yes*
Clustering	Province	Province	Province	Province	Province	Province	Province	Province
N	2,220	2,220	2,220	2,220	5,994	5,994	5,994	5,994
Adj-R-sq	0.932	0.967	0.975	0.965	0.833	0.944	0.956	0.932

Table 4.4: **State-bank behaviour in election years: Quarterly estimates (2007q4-2016q4).** This table shows results of Equation (4.1) estimated on quarterly data. Columns I-IV include metropolitan provinces and columns V-VIII include the full sample. Standard errors are provided in brackets; *, **, *** indicate statistical significance at the level of 10%, 5%, and 1%, respectively.

	Allied Provinces					Non-allied Provinces				
	I	*II*	*III*	*IV*	*V*	*VI*	*VII*	*VIII*	*IX*	*X*
Comp x StateBank x Election	0.399*	0.424*	0.425*	0.425	0.448	-0.571***	-0.517**	-0.521**	-0.520*	-0.482**
	[0.228]	[0.240]	[0.245]	[0.322]	[0.321]	[0.146]	[0.191]	[0.192]	[0.249]	[0.210]
Comp x StateBank	-0.547	0.138	0.143	0.152	0.112	-0.971*	-0.439	-0.480	-0.473	-0.575
	[0.585]	[0.369]	[0.381]	[0.498]	[0.494]	[0.540]	[0.526]	[0.542]	[0.705]	[0.510]
StateBank x Election	-0.422**	-0.452**	-0.452**	-0.452		0.324**	0.269	0.273	0.272	
	[0.188]	[0.197]	[0.202]	[0.264]		[0.115]	[0.157]	[0.158]	[0.205]	
Comp x Election	-0.269**	-0.314***	-0.306***			0.555***	0.502***	0.374**		
	[0.104]	[0.104]	[0.104]			[0.081]	[0.098]	[0.129]		
Controls										
Local branches		*Yes*	*Yes*	*Yes*	*Yes*		*Yes*	*Yes*	*Yes*	*Yes*
Bank type FE	*Yes*	*Yes*	*Yes*	*Yes*		*Yes*	*Yes*	*Yes*	*Yes*	
Province FE	*Yes*	*Yes*	*Yes*			*Yes*	*Yes*	*Yes*		
Time FE	*Yes*	*Yes*	*Yes*			*Yes*	*Yes*	*Yes*		
ProvinceTime trends			*Yes*					*Yes*		
Province x Time FE				*Yes*	*Yes*				*Yes*	*Yes*
Bank x Time FE					*Yes*					*Yes*
Clustering	Province	Province	Province	Province	Province	Province	Province	Province	Province	Province
N	502	502	502	502	502	338	338	338	338	338
Adj-R-sq	0.937	0.961	0.964	0.945	0.964	0.959	0.965	0.969	0.955	0.977

Table 4.5: **Tactical reallocation in metropolitan provinces: Yearly estimates (2003-2016).** This table shows results of Equation (4.2) estimated on yearly data. Columns I-V include allied provinces and columns VI-X include non-allied provinces. Standard errors are provided in brackets; *, **, *** indicate statistical significance at the level of 10%, 5%, and 1%, respectively.

	Allied Provinces					Non-allied Provinces				
	I	*II*	*III*	*IV*	*V*	*VI*	*VII*	*VIII*	*IX*	*X*
Comp x StateBank x Election	0.296	0.244	0.246	0.245	0.266	-0.501***	-0.390**	-0.397**	-0.395*	-0.492*
	[0.203]	[0.191]	[0.192]	[0.261]	[0.249]	[0.167]	[0.146]	[0.145]	[0.198]	[0.261]
Comp x StateBank	-0.898**	-0.220	-0.239	-0.231	-0.206	-0.550	0.263	0.217	0.230	0.159
	[0.433]	[0.170]	[0.170]	[0.232]	[0.235]	[0.372]	[0.360]	[0.370]	[0.507]	[0.487]
StateBank x Election	-0.326**	-0.254*	-0.256*	-0.255		0.299**	0.221*	0.225*	0.224	
	[0.146]	[0.138]	[0.139]	[0.189]		[0.130]	[0.109]	[0.108]	[0.147]	
Comp x Election	-0.186	-0.179*	-0.286**			0.326	0.248	0.240***		
	[0.126]	[0.101]	[0.119]			[0.198]	[0.156]	[0.070]		
Controls										
Local branches		*Yes*	*Yes*	*Yes*	*Yes*		*Yes*	*Yes*	*Yes*	*Yes*
Bank type FE	*Yes*	*Yes*	*Yes*	*Yes*		*Yes*	*Yes*	*Yes*	*Yes*	
Province FE	*Yes*	*Yes*	*Yes*			*Yes*	*Yes*	*Yes*		
Time FE	*Yes*	*Yes*	*Yes*			*Yes*	*Yes*	*Yes*		
ProvinceTime trends			*Yes*					*Yes*		
Province x Time FE				*Yes*	*Yes*				*Yes*	*Yes*
Bank x Time FE					*Yes*					*Yes*
Clustering	Province	Province	Province	Province	Province	Province	Province	Province	Province	Province
N	1,236	1,236	1,236	1,236	1,236	984	984	984	984	984
Adj-R-sq	0.929	0.969	0.974	0.964	0.966	0.948	0.971	0.978	0.966	0.967

Table 4.6: **Tactical reallocation in metropolitan provinces: Quarterly estimates (2007q4-2016q4).** This table shows results of Equation (4.2) estimated on quarterly data. Columns I-V include allied provinces and columns VI-X include non-allied provinces. Standard errors are provided in brackets; *, **, *** indicate statistical significance at the level of 10%, 5%, and 1%, respectively.

Dependent variable:	*LogFlats*			*LogSqMtr*			*LogValue*		
	I	*II*	*III*	*IV*	*V*	*VI*	*VII*	*VIII*	*IX*
Opposition x Comp x Election	-0.680**	-0.666**	-0.519*	-0.764**	-0.757**	-0.605*	-0.780**	-0.772**	-0.628*
	[0.307]	[0.307]	[0.305]	[0.349]	[0.349]	[0.351]	[0.361]	[0.360]	[0.361]
Comp x Election	0.225	0.209	0.091	0.183	0.175	0.061	0.183	0.175	0.058
	[0.216]	[0.218]	[0.221]	[0.213]	[0.214]	[0.219]	[0.212]	[0.214]	[0.220]
Comp x Opposition	0.271	0.257	0.009	0.264	0.257	-0.113	0.132	0.125	-0.206
	[0.468]	[0.466]	[0.491]	[0.387]	[0.385]	[0.397]	[0.406]	[0.403]	[0.411]
Opposition x Election	0.502**	0.489*	0.385	0.596**	0.590**	0.481*	0.604**	0.597**	0.495*
	[0.246]	[0.246]	[0.244]	[0.279]	[0.279]	[0.282]	[0.288]	[0.288]	[0.289]
Controls									
Local population		*Yes*			*Yes*			*Yes*	
Province FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Time FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
ProvinceTime trends			*Yes*			*Yes*			*Yes*
Clustering	Province	Province	Province	Province	Province	Province	Province	Province	Province
N	1,133	1,133	1,133	1,133	1,133	1,133	1,133	1,133	1,133
Adj-R-sq	0.607	0.611	0.689	0.628	0.629	0.686	0.807	0.807	0.837

Table 4.7: **Political competition, alliance, and local economic activity (2003-2016).** This table shows results of Equation (4.3). Standard errors are provided in brackets; *, **, *** indicate statistical significance at the level of 10%, 5%, and 1%, respectively.

	Assets / GDP			*Loans / GDP*			*Deposit / GDP*		
Years	*State*	*Private*	*Total*	*State*	*Private*	*Total*	*State*	*Private*	*Total*
1999	0.23	0.37	0.60	0.06	0.12	0.17	0.18	0.22	0.40
2000	0.21	0.32	0.53	0.05	0.11	0.17	0.16	0.19	0.35
2001	0.22	0.40	0.62	0.04	0.11	0.14	0.15	0.30	0.46
2002	0.19	0.35	0.54	0.03	0.11	0.13	0.14	0.24	0.38
2003	0.18	0.32	0.50	0.03	0.11	0.13	0.13	0.20	0.33
2004	0.19	0.32	0.51	0.04	0.13	0.17	0.14	0.20	0.34
2005	0.18	0.38	0.57	0.05	0.17	0.22	0.14	0.23	0.38
2006	0.18	0.41	0.59	0.06	0.20	0.26	0.14	0.25	0.40
2007	0.19	0.43	0.61	0.07	0.23	0.31	0.15	0.26	0.41
2008	0.21	0.48	0.69	0.09	0.27	0.35	0.16	0.29	0.46
2009	0.25	0.52	0.77	0.10	0.26	0.36	0.19	0.32	0.51
2010	0.26	0.54	0.80	0.13	0.30	0.42	0.20	0.33	0.53
2011	0.24	0.56	0.80	0.13	0.32	0.46	0.17	0.33	0.50
2012	0.24	0.55	0.79	0.13	0.34	0.47	0.17	0.32	0.49
2013	0.27	0.60	0.86	0.16	0.38	0.53	0.18	0.34	0.52
2014	0.27	0.61	0.88	0.17	0.39	0.56	0.17	0.35	0.52
2015	0.29	0.62	0.91	0.19	0.40	0.59	0.18	0.36	0.53

Table 4.A1: **Growth in Turkish banking sector (1999-2015).** This table shows the relative size of the banking activities in Turkey with respect to country's GDP in each year between 1999 and 2015.

	2003 - 2006 dropped				*2007 - 20011 dropped*				*2012 - 2016 dropped*			
	Metropolitan		*Full*		*Metropolitan*		*Full*		*Metropolitan*		*Full*	
StateBank x Election	-0.143***	-0.143***	-0.125***	-0.125***	-0.122***	-0.122***	-0.114***	-0.114***	-0.143***	-0.143***	-0.159***	-0.160***
	[0.015]	[0.020]	[0.010]	[0.013]	[0.028]	[0.036]	[0.027]	[0.035]	[0.031]	[0.040]	[0.028]	[0.037]
Controls												
Local branches	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Bank type FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Province FE	*Yes*		*Yes*		*Yes*		*Yes*		*Yes*		*Yes*	
Time FE	*Yes*		*Yes*		*Yes*		*Yes*		*Yes*		*Yes*	
ProvinceTime trends	*Yes*		*Yes*		*Yes*		*Yes*		*Yes*		*Yes*	
Province x Time FE		*Yes*		*Yes*		*Yes*		*Yes*		*Yes*		*Yes*
Clustering	Province	Province	Province	Province	Province	Province	Province	Province	Province	Province	Province	Province
N	600	600	1,620	1,620	540	540	1,458	1,458	540	540	1,458	1,458
Adj-R-sq	0.96	0.94	0.938	0.905	0.973	0.959	0.962	0.945	0.946	0.919	0.912	0.873

Table 4.A2: **State-bank behaviour in election years: Election combinations.** This table shows results of Equation (4.1) estimated on yearly data with different election combinations. Standard errors are provided in brackets; *, **, *** indicate statistical significance at the level of 10%, 5%, and 1%, respectively.

Chapter 5

Conclusion

In this PhD thesis, I have investigated three topics on sovereign risk and banking.

In the first study (*Chapter 2*), we look at the sovereign contagion links during Eurozone crisis and how they change in response to an exogenous ECB policy announcement in order to differentiate between alternative theories in the literature. Our findings are: (*i*) principal components analysis reveals that the perceived commonality in default risk among peripheral and core Eurozone countries increased after the announcement. In the meantime, the link between country fundamentals and spreads strengthened implying that there might be non-fundamental factors at play prior to the announcement. (*ii*) An event study detects significant pre-announcement news transmission from Spain to Italy, Belgium, France and Austria that clearly dissipates post-announcement. This is consistent with the view that news in one country could act as a trigger (sunspot) for self-fulfilling market movements against other countries. (*iii*) Country-specific regressions of CDS spreads on systematic risk factors illustrate frequent days of large adverse shocks affecting simultaneously those same Eurozone countries during the pre-announcement period; but not afterwards. Altogether these findings support the view that market expectations during Eurozone crisis were at least partially self-fulfilling and ECB policy helped to contain such adverse dynamics.

Chapter 3 studies the relationship between sovereign debt crisis in the Eurozone and European banks' government debt exposures. (*i*) I first re-confirm that the crisis led to the reallocation of sovereign debt from foreign to domestic banks. (*ii*) However, this reallocation was only visible for banks as opposed to other domestic private agents, which does not seem to be consistent with the secondary market or exchange rate channel of the rising home bias. (*iii*) I find weak evidence for risk-shifting tendency of the troubled country banks; nonetheless this does not come close to explaining the full extent of the preference for local government bonds. (*iv*) In contrast to the recent literature focusing only on sovereign debt, I also show that banks' private sector exposures were (at least) equally affected by a rise in home bias, which implies that the specific channel of moral suasion on sovereign debt has limited explanatory power in sample. Given the insufficiency of the existing explanations, I propose a new debt reallocation channel based on informational frictions and (*v*) show that crisis-country debt was not only reallocated to domestic banks, but also to the informationally closer foreign banks. I further confirm that this effect is independent of the previous channels proposed in the literature, robust to various sample recompositions and exists more generally rather than being specific to the periods of extreme sovereign stress. Hence, these results imply that informational asymmetries among banks played a key role in the recent fragmentation across Eurozone debt markets.

Lastly, in *Chapter 4*, we look at the political economy aspects of the relationship between governments and banks. (*i*) We find that state-owned banks in Turkey systematically adjust their provincial lending around local elections relative to the private banks in the same province. There is considerable tactical redistribution: state-owned banks increase loans in politically competitive provinces with a current mayor aligned with the ruling party but reduce it in similar provinces with a current mayor from opposition. Besides, rolling estimations in non-election years show some evidence that central government may have resorted to patronage when it did not have election concerns. (*ii*) Political cycle only exists in corporate lending as opposed to consumer loans, suggesting that tactical redistribution targets job creation to increase electoral success. In line with this conjecture, real local outcomes seem to be influenced by the political cycle as the credit-constrained opposition areas suffer a drop in economic output measured by local construction activity.

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