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# EFFECTS OF ELIGIBILITY FOR CENTRAL BANK PURCHASES ON CORPORATE BOND SPREADS

by Taneli Mäkinen\*, Fan Li\*\*, Andrea Mercatanti\* and Andrea Silvestrini\*

## Abstract

The causal effect of the European Central Bank’s corporate bond purchase program on bond spreads in the primary market is evaluated, making use of a novel regression discontinuity design. The results indicate that the program did not, on average, permanently alter the yield spreads of eligible bonds relative to those of noneligible. Combined with evidence from previous studies, this finding suggests the effects of central bank asset purchase programs are in no way limited to the prices of the specific assets acquired.

**JEL Classification:** C21, G18.

**Keywords:** asset purchase programs, corporate bonds, causal inference.

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# 1 Introduction<sup>1</sup>

Persistently low inflation represents an important challenge to central banks in several advanced economies. Inflation rates have remained below central banks' targets even though monetary policy rates have been cut to historically low levels. These developments have substantially constrained the conduct of monetary policy (BIS, 2019a). The limited scope to lower policy rates further has prompted central banks to make use of asset purchase programs with the aim of raising inflation. Using asset purchases to attain a target inflation rate distinguishes these policy tools from the purchase programs implemented during the global financial and the European sovereign debt crisis, which primarily addressed financial market disruptions (BIS, 2019b).

The effects of central bank asset purchases, especially when conducted in relatively tranquil times, remain imperfectly understood. Based on surveys of central bank governors and academics, Blinder et al. (2017) find that there is scepticism about the usefulness of keeping large-scale asset purchases in the monetary policy toolkit due to uncertainty about their costs and benefits. Williamson (2016) adopts an even more cautious tone in arguing that asset purchase programs seem to have been ineffective in increasing inflation. At the same time, asset prices have been documented to respond strongly to the introduction of asset purchase programs (BIS, 2019b). Central banks also continue to pursue their objectives by making use of such policies. A case in point is the decision by the European Central Bank to restart in November 2019 net purchases of assets in response to inflation below its target.<sup>2</sup> In light of these observations, further research into the effects of central bank asset purchase programs appears warranted.

We contribute to the discussion about central bank asset purchases by studying the effects of the corporate sector purchase programme (CSPP) of the European Central Bank. Under the CSPP, 180 billion euros worth of corporate bonds were purchased between June 2016 and December 2018. As a result, close to a fifth of the bonds eligible for purchase were transferred from

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<sup>2</sup>See the "Introductory statement to the press conference (with Q&A)" (September 12, 2019), accessible at <https://www.ecb.europa.eu/press/pressconf/2019/html/ecb.is190912~658eb51d68.en.html>.

the private sector to the balance sheets of the Eurosystem. Given that the corporate bond market is relatively illiquid, with buy-to-hold investors playing a large role, the CSPP provides a compelling setting to evaluate the price effects of central bank asset holdings. Specifically, the program allows to assess whether relative asset prices are permanently altered when the central bank becomes a large holder of certain types of securities. In other words, we focus on the stock effects of the program, i.e. the changes in prices over its whole duration due to the permanent reduction in the amount of the eligible securities in the hands of the private sector (D'Amico and King, 2013).

Our study of the relative price changes induced by the CSPP focuses on the effects of the program on the bonds eligible for purchase. For these bonds, we estimate the causal effect of the CSPP on their yield spreads at issuance relative to those of bonds not eligible for purchase. The estimation strategy exploits the feature of the program that only bonds whose highest rating exceeded a given threshold were considered to be eligible for purchase. Namely, we make use of a regression discontinuity design specifically developed for applications in which the treatment-determining variable is ordered categorical (Li et al., 2020), as is the case with the rating in our setting. Employing both a simple weighting estimator and a doubly robust augmented weighting estimator, we estimate the local average treatment effect of the program on the yield spreads of eligible bonds.

We find that the program did not have a statistically significant causal effect on the yield spreads of the eligible bonds, relative to those of noneligible bonds, issued between the announcement of the program in March 2016 and the end of net purchases in December 2018. The differential effect of the program on the eligible bonds was not statistically different from zero also when the holdings of corporate bonds under the CSPP reached their highest level, and in countries in which a larger share of corporate bonds are held by long-term investors. These results suggest that the transfer of even relatively illiquid securities such as corporate bonds to the balance sheet of the central bank can have no permanent effect on the prices of such securities relative to those of their close substitutes. The absence of such an effect complements the finding from previous analyses that the program initially exerted a negative effect on the spreads of the eligible bonds relative to those of the noneligible (Zaghini, 2019; Li et al., 2020). Put together, the results are consistent with the view that the effects of central bank asset purchase programs are strongly felt also by securities not eligible to be acquired.

In the next section, we discuss central bank asset purchase program in general and the CSPP in particular, as well as the related literature. In Section 3,

we describe the empirical methodology employed. In Section 4, we first present the data used, then provide some preliminary results and finally examine the causal effects of the program. Section 5 contains some concluding remarks.

## **2 Motivation and background**

### **2.1 Central bank asset purchase programs**

Large-scale asset purchase programs have, over the last ten years, become to play an increasingly important role in the implementation of monetary policy for several central banks (BIS, 2019b). Such programs have taken many forms, involving purchases of not only government bonds but also securities issued by the private sector. Specifically, the acquired financial instruments have included asset-backed securities, commercial paper and corporate bonds.

Asset purchase programs have been resorted to as a means of providing additional monetary stimulus, as the scope to do so by cutting policy rates further has been limited. The designs of the programs have reflected a diversity of views about the channels through which they can affect financial conditions and ultimately the real economy.

In an environment in which short-term nominal interest rates are very low, an asset purchase program that expands the size of the central bank's balance sheet can affect the private sector's expectations about the future path of interest rates. Specifically, a central bank can more credibly commit to keep the policy rate low in the future if it acquires long-term financial assets as their price varies inversely with interest rates (Clouse et al., 2003; Eggertsson and Woodford, 2003).

Central bank purchases can affect security prices also if there are financial assets for which perfect substitutes cannot be constructed using other existing assets. Namely, an asset purchase program that alters the relative amounts of different financial assets outstanding can affect price changes by inducing changes in the relative scarcity of assets (see, e.g., Bernanke and Reinhart, 2004 and the references therein). Notably, imperfect substitutability would open the possibility of altering prices by changing the composition of assets on the central bank's balance sheet without increasing its size.

Finally, asset purchase programs which target private sector debt and securities markets, referred to as credit policies, have the potential to increase the availability and lower the cost of funding (Borio and Disyatat, 2010). Such effects can arise as the central bank can raise funds at a lower cost than private sector lenders and may demand a lower premium for holding illiquid securities.

Thus, purchases of claims on the private sector by the central bank can lead to lower risk premia on such claims.

## 2.2 The CSPP

The corporate sector purchase programme (CSPP) of the European Central Bank (ECB) was announced on March 10, 2016 and purchases of eligible securities began on June 8, 2016. The program was announced in a context of falling actual and expected inflation. Prior to the announcement of the CSPP, the ECB already had asset purchase programs in place, involving purchases of public sector securities, covered bonds and asset-backed securities. The aim of the CSPP was twofold. First, together with the other asset purchases, the program sought to provide additional monetary policy accommodation and to raise inflation rates. Second, the program aimed to improve the financing conditions of the real economy.<sup>3</sup>

Under the CSPP, corporate bonds issued by euro-area non-bank corporations denominated in euro were purchased. Securities eligible for purchase were required to be rated investment-grade by at least one rating agency. Moreover, the remaining maturity of the securities was restricted to lie between 6 months and 30 years at the time of purchase. Purchases of eligible securities were carried out both in the primary and the secondary market. The bonds acquired under the CSPP were made available for securities lending by the six Eurosystem central banks that carried out the purchases.

The CSPP differed from central bank purchases of government bonds along important dimensions. Most of the differences are related to the features which distinguish the corporate bond market from that of government bonds. The corporate bond market is significantly more heterogeneous, as issued bonds are often embedded with options to better suit the financing needs of the issuer. For instance, corporate bonds are often callable, allowing the issuer to redeem the bond before it matures. In addition, the number of issuers far exceeds that in the government bond market. The composition of investors in the corporate bond market is also quite different from that in the market for sovereign debt. Indeed, large fractions of some corporate bond issues are bought by institutional investors who hold them until maturity (Biais et al., 2006). Due to these differences between corporate and government bond markets, the CSPP can be expected to have had significantly different effects

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<sup>3</sup>See the press release "ECB announces the details of the corporate sector purchase programme (CSPP)" (April 21, 2016), accessible at [https://www.ecb.europa.eu/press/pr/date/2016/html/pr160421\\_1.en.html](https://www.ecb.europa.eu/press/pr/date/2016/html/pr160421_1.en.html).

than purchases of government bonds.<sup>4</sup>

There is a growing number of studies analyzing different aspects of the CSPP. Zaghini (2019) estimates the effects of the program over the first year of purchases, relying on a regression model for bond spreads at issuance. Focusing instead on the secondary market, Abidi and Miquel-Flores (2018) quantify the announcement effect of the program, exploiting differences between investors and the ECB in identifying investment-grade bonds. Arce et al. (2017), Ertan et al. (2018), Grosse-Rueschkamp et al. (2019) and Betz and De Santis (2019) study the indirect effects of the CSPP on the composition of bank lending. Galema and Lugo (2017) investigate the capital structure of the issuers whose bonds were purchased under the CSPP. De Santis and Zaghini (2019) instead study the effects of the program on bond issuance. Finally, Abidi et al. (2019) examine how bond ratings changed over the course of the program. Differently from these works and from our preliminary evaluation of the CSPP (Li et al., 2020), we quantify the effect of the program on bond yields over its whole duration. Doing so allows us to assess whether the program permanently altered the yield spreads of eligible bonds *vis-à-vis* those of bonds not eligible for purchase under the CSPP.

### 3 Methodology

The empirical approach exploits the feature of the program that the highest rating of eligible bonds exceeded a given threshold. This policy rule allows us to employ a regression discontinuity (RD) design to evaluate the causal effects of the program.<sup>5</sup> However, due to the ordinal nature of the treatment-determining variable, i.e. the rating, the standard RD methods are not applicable. For this reason, we adopt the RD approach developed in our previous work (Li et al., 2020), specifically constructed for settings in which the variable determining assignment to treatment, i.e. the running variable, is ordered categorical. The general framework underlying our approach is formally described in Appendix A.1.

The estimation strategy combines the advantages conferred by the classical RD design with benefits deriving from the use of weighting estimators (Hirano and Imbens, 2001; Li et al., 2018). Namely, exploiting the disconti-

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<sup>4</sup>At the same time, the CSPP was smaller, in terms of the acquired holdings as a percentage of the eligible assets, than the purchases of public sector securities by the ECB (BIS, 2019b). However, this difference could have been offset by the larger share of buy-to-hold investors.

<sup>5</sup>For a historical overview of RD designs, see Cook (2008), and for recent surveys Imbens and Lemieux (2008); Lee and Lemieux (2010).

nity in the assignment rule ensures internal validity of the analysis, since the treatment can be considered “as good as randomized in a local neighborhood” of the threshold (Lee, 2008). Weighting estimators allow us to evaluate the causal effects of the program around, rather than at, the threshold, improving external validity. The stability of the estimates is enhanced by augmenting the weighting estimators with regression models for the potential outcomes, which guards against both model misspecification and covariate imbalance between the treated and control units (Robins et al., 1995; Lunceford and Davidian, 2004).

The weighting estimators we employ are designed to quantify the average treatment effect on the treated (ATT), which is the average of the unit level effects for those units that received the treatment.<sup>6</sup> Our analysis aims at identifying the ATT locally, around the eligibility threshold. This local approach is in the spirit of the RD framework of Angrist and Rokkanen (2015) and increases the plausibility of the necessary identification assumptions, which are stated in Appendix A.2.

The estimation strategy requires first specifying an ordered probit model for the ordinal running variable. Estimating such a model yields a predicted propensity score for each bond, which is the probability of being eligible for purchase conditional on a set of observed covariates (Rosenbaum and Rubin, 1983). The propensity score is adopted as a surrogate running variable, whose continuity allows us to measure the distance of each unit to the threshold. A propensity score equal to 0.5 is employed as the threshold on the continuous probability scale, given that it corresponds to a bond rating around the actual ordinal threshold.

The second step of the strategy aims at maximizing the external validity of our estimates. To this end, we seek to identify the largest subset of units for which the causal effect can be estimated. Such a subsample should be characterized by the distributions of the covariates being balanced between the treatment and control group.<sup>7</sup> Covariate balance can be assessed by checking the difference in the weighted average of each pre-treatment covariate between the treatment and control group. Thus, we select for use subsamples of units with estimated propensity scores around 0.5 in which these differences are small in absolute value. When employing the augmented weighting estimator, we can tolerate some mild imbalance, which allows us to consider larger subsamples,

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<sup>6</sup>In our preliminary evaluation of the program (Li et al., 2020), we also consider its counterfactual effect on the whole population of interest.

<sup>7</sup>The equality of the weighted distributions of each pre-treatment covariate for units under the treatment and the control is a consequence of the local unconfoundedness assumption, stated in Appendix A.2 (Li et al., 2018).

with units further away from the eligibility threshold.

In the third step of the strategy, the causal effect of interest is estimated by applying a suitable propensity score weighting estimator to the chosen subsamples. A natural choice is the simple difference of weighted average outcomes between the treatment and control group. The robustness of this weighting estimator can be increased by augmenting it with regression models for the potential outcomes (Robins et al., 1995; Lunceford and Davidian, 2004). Doing so leads to the so-called “augmented weighting estimator”, which for the ATT has been introduced by Mercatanti and Li (2014). In our evaluation of the CSPP, we employ both the simple and the augmented weighting estimator, which are described more in detail in Appendix A.3.

## 4 Evaluation of the CSPP

We evaluate the effect of the CSPP on the yield spreads of the bonds eligible for purchase under the program in the primary market. Focusing on the effect on these bonds is motivated by the following considerations. We seek to assess the effect of the CSPP on yield spreads of the eligible bonds relative to those of the noneligible that prevailed during the whole duration of the program, i.e. its stock effect. If we instead defined the treatment in terms of actual purchases, we would probably capture a more transitory effect that partly reflects the illiquidity of the corporate bond market. Indeed, even in the case of central bank purchases of government bonds, the effect of actual purchases, referred to as the flow effect, has been found to be only temporary, as well as being small in magnitude (D’Amico and King, 2013).

The advantage of focusing on the primary market is that a yield at issuance accurately reflects investors’ valuation of the bond. Secondary market prices, on the contrary, are more noisy due to variations in liquidity conditions (Friewald et al., 2012). Moreover, under the CSPP, the Eurosystem purchased a higher percentage of the eligible bonds that were issued after than before the announcement of the program. Specifically, 85 per cent of the eligible bonds we analyze were purchased by the Eurosystem, while the percentage acquired is approximately 60 per cent for the bonds that were issued prior to the announcement of the program.<sup>8</sup>

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<sup>8</sup>We estimated the latter percentage based on the eligible bonds in the ICE BofAML Euro Corporate Index (ER00) as of March 10, 2016.

## 4.1 Data

Our interest lies in estimating the effect of the program on yields at issuance of the eligible bonds in the population of euro-denominated bonds issued by euro-area non-bank corporations. To this end, we obtained from Bloomberg all the bonds issued after the announcement of the program until the end of net purchases, i.e. between March 11, 2016 and December 31, 2018, satisfying all the eligibility criteria of the program referring to characteristics other than the rating of the bond. This sample is representative of the population of our interest; the maturity criterion eliminates only 2 per cent of the euro-denominated bonds issued by euro-area non-bank corporations over the period considered.

For the bonds in the sample constructed in this manner, we obtained information about their yield spreads and credit ratings, as well as other characteristics that can potentially explain these two variables. The yield spread measure we use is the option-adjusted spread (OAS), which is defined as the difference between the yield to maturity of the bond, adjusted to take into account its embedded options, and the yield of a government bond of a similar maturity. We employ the first available value of the OAS in the nine-day period starting from the issue date.<sup>9</sup> The ratings of each bond, assigned by Standard & Poor's, Moody's, Fitch and DBRS, if any, are as of its issue date. These ratings allow us to determine whether the bond was eligible for purchase under the CSPP when it was issued. Figure 1 summarizes, within each rating category, the distribution of the OAS of the bonds issued over the duration of the program. For comparison, also the spreads of the bonds issued in the two years preceding the announcement of the program are illustrated. It is worth pointing out that the spreads of the eligible bonds, especially of those with ratings just above the eligibility threshold, were lower during the period which we analyze than before it.<sup>10</sup>

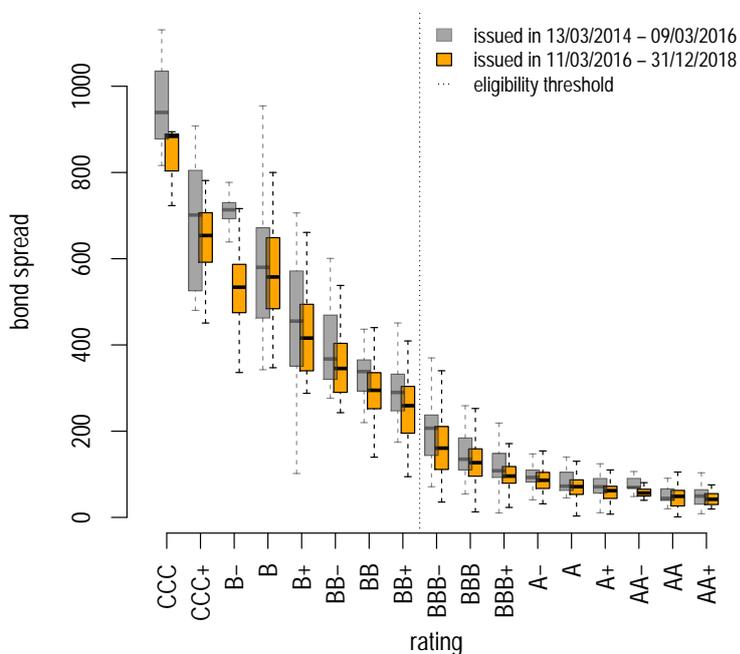
The other bond characteristics that we obtained from Bloomberg are: coupon rate (cpn), original maturity (mat), maturity type, issue date, coupon type and amount sold. Maturity type (callable, putable, convertible or at maturity) indicates the embedded options of the bond, with at maturity indicating a bullet bond. Coupon type (fixed, zero-coupon, pay-in-kind or variable) refers to the coupon payments that an investor holding the bond obtains. We excluded from the analysis the 9 bonds with variable coupon rates in our sample

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<sup>9</sup>In this way the number of bonds with missing data on the OAS is significantly reduced. However, for most bonds the OAS is as of the issue date.

<sup>10</sup>This fact is not compatible with the CSPP having been associated with an increase in the riskiness of the eligible bonds which fully offset its negative effect on the spreads.

Figure 1: Option-adjusted spreads by rating.



due to the unavailability of the OAS for these securities. Summary statistics of the bond characteristics are reported in Table 1.

Table 1: Summary statistics for the bond characteristics.

variable	mean	sd	Q <sub>1</sub>	Q <sub>2</sub>	Q <sub>3</sub>	N
coupon rate	2.7	2.1	1.1	2.0	4.0	1,654
original maturity	8.0	4.2	5.0	7.0	10	1,654
amount sold	459	385	150	450	650	1,643
OAS	199	180	78	125	277	1,131

NOTE: Coupon rate in per cent, original maturity in years, amount sold in millions of euros, OAS in basis points.

The information on the bonds was complemented with data on their issuers obtained from S&P Capital IQ. Specifically, we employ balance sheet and income statement data for the issuers or, in case they were subsidiaries, their ultimate parent companies. When data for the ultimate parent company is unavailable, because of, for instance, it being a private company, we use data referring to the parent of the issuer on the highest level in the business group.

We employ data for the 2015 fiscal year to ensure that the issuer information predates the program being evaluated.

We obtained all the balance sheet and income statement items necessary to construct the following variables: profitability (prof), cash flow (cf), liquidity (liq), interest coverage (cov), leverage (lev), solvency (solv), size, age and long-term debt (ltdebt), defined in Table 2. These variables were chosen because of their good predictive power for credit ratings (Mizen and Tsoukas, 2012). A few anomalous values of the variables, suggesting incorrectly reported data, were removed. Specifically, we excluded the units for which profitability was smaller than -400 (1 issuer), interest coverage was below -500 (1 issuer) or above 250 (4 issuer), leverage exceeded 1 (6 issuers) or solvency was below -2 (3 issuers). As a result, 41 observations were removed. Summary statistics of the issuer variables, calculated after the removal of the anomalous observations, are reported in Table 2.

Table 2: Summary statistics for the issuer characteristics.

variable	definition	mean	sd	Q <sub>1</sub>	Q <sub>2</sub>	Q <sub>3</sub>	N
prof	$\frac{\text{EBIT}}{\text{total revenue}}$	0.14	0.34	0.046	0.10	0.18	1,370
cf	$\frac{\text{cash from operations}}{\text{total assets}}$	0.055	0.089	0.032	0.066	0.095	1,232
liq	$\frac{\text{cash from operations}}{\text{total liabilities}}$	0.094	0.14	0.047	0.095	0.15	1,232
cov	$\frac{\text{EBIT}}{\text{interest expenses}}$	7.9	18	1.4	3.8	8.0	1,295
lev	$\frac{\text{total debt}}{\text{total assets}}$	0.37	0.20	0.23	0.36	0.49	1,332
solv	$\frac{\text{common equity}}{\text{total assets}}$	0.29	0.20	0.18	0.29	0.41	1,365
size	log(total revenue)	3.5	1.1	2.9	3.7	4.4	1,379
age	2017 – year founded	74	72	22	58	114	1,277
ltdebt	$\frac{\text{long-term debt}}{\text{total assets}}$	0.32	0.31	0.16	0.26	0.40	1,325

NOTE: The variable size is calculated with total revenue recorded in millions of euros.

In the analysis that follows, we employ the bonds for which we have data on their coupon rate, original maturity and the issuer characteristics listed in Table 2. We have 1,058 such bonds, of which 635 are callable, 364 bullet bonds, 52 convertible and 1 puttable. The convertible and puttable bonds are, however, not used to estimate the causal effect of the program because the option-adjusted spread is unavailable for them. For future reference, let *call* be the indicator function taking the value 1 when the bond is callable and 0 otherwise.

## 4.2 Results

Our analysis consists of three parts. First, we postulate and estimate a model for the probability of being eligible for purchase under the CSPP. The estimated probabilities allow us to quantify, on a continuous scale, the distance of each bond to the eligibility threshold, around which we estimate the effect of the program. Second, we use the estimated probabilities of eligibility to provide preliminary evidence on the effects of the program on the bonds of our interest. Finally, we present and discuss our estimates of the effect of the program, obtained using the methodology described in the previous section.

### 4.2.1 Eligibility for the CSPP

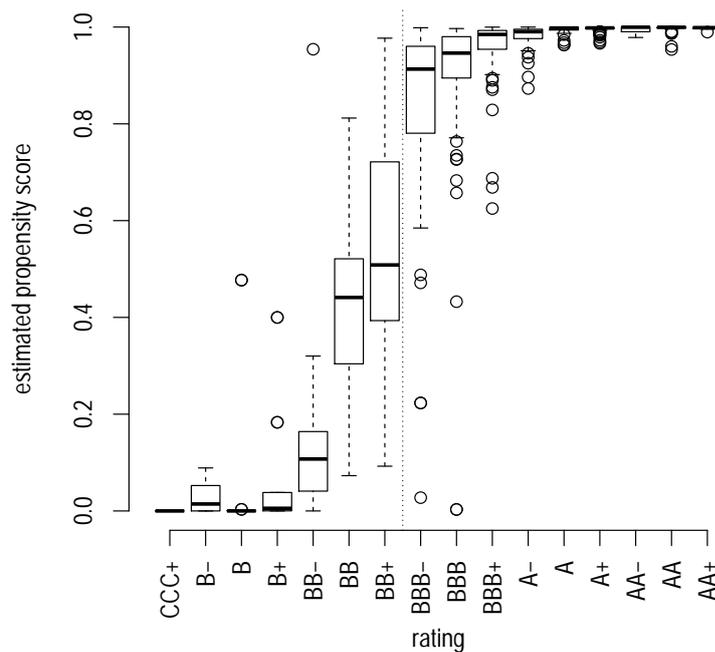
A key input in our estimation strategy is the probability of being eligible for purchase under the CSPP. This probability is the propensity score, i.e. the probability of receiving the treatment of our interest conditional on the covariates. When conditioning on the propensity score, the distribution of the covariates is the same for the treatment and control group (Rosenbaum and Rubin, 1983). Consequently, covariate balance is an important diagnostic in evaluating the estimated propensity scores. Yet, our primary concern in the search for an adequate specification of the propensity score model is its predictive power. Specifically, we seek a specification that yields accurate predictions around the eligibility threshold. A further reason for proceeding in this manner is that our doubly robust augmented weighting estimator can reduce any bias due to covariate imbalance.

Let us recall that the sample under study comprises bonds that satisfy all the eligibility criteria of the program with the exception of the rating requirement. Consequently, we can define the eligibility of each bond solely in terms of its highest rating. Specifically, all bonds whose maximum rating is greater than or equal to BBB-, or equivalent, are classified to be eligible for purchase under the program; the remaining bonds constitute the control group. It is important to distinguish this rating threshold from that employed by market participants to identify investment-grade and high-yield bonds. The latter classification is based on either the average or the minimum rating of a bond (Abidi and Miquel-Flores, 2018). Therefore, eligibility for purchase under the CSPP does not coincide with having the status of an investment-grade bond in the market. Due to this distinction, we employ the term eligibility rating for the highest rating of a bond, which is above that determining whether the bond is considered to be investment grade.

As explained in Section 3, we postulate an ordered probit model for the

eligibility rating. In specifying the model, we are guided by the literature on the determinants of bond and issuer ratings. Specifically, we consider specifications including our bond and issuer characteristics, that are typically employed in this literature.<sup>11</sup> Naturally, we require the variables to be determined before the bond is issued, which excludes the amount sold and the OAS. We seek a subset of the variables which accurately predicts the eligibility rating. Guided by this objective, we include in the specification the following variables: coupon rate, original maturity, profitability, interest coverage, solvency and size. We also consider all the quadratic terms formed from these variables and inspect whether adding them improves the predictive power of the model. This procedure leads us to adopt the specification whose in-sample predictions are illustrated in Figure 2.

Figure 2: Estimated propensity scores by rating in the full sample.



NOTE: The ordered probit specification contains: cpn, mat, prof, cov, solv, size and size<sup>2</sup>.

The model is able to predict the eligibility rating accurately, especially in the case of the eligible bonds. The estimated propensity scores for the BBB-

<sup>11</sup>See, e.g., Hickman (1958); Pogue and Soldofsky (1969); Pinches and Mingo (1973); Ang and Patel (1975); Pinches and Mingo (1975); Kaplan and Urwitz (1979); Kao and Wu (1990); Blume et al. (1998).

and BBB bonds, just above the eligibility threshold, are above 0.5 for 93 and 97 per cent of these units, respectively. For the bonds in the two rating categories just below the threshold, BB+ and BB, 38 and 70 per cent of the estimated propensity scores are below 0.5, respectively. The less precise predictions below the eligibility threshold imply that the subsamples of units around the threshold that we consider contain more control than treated units. However, this difference between the two groups is moderated by the larger overall number of treated than control units.<sup>12</sup>

We also assess the adequateness of the ordered probit specification in terms of the resulting covariate balance. The measure that we use for doing so is the standardized bias (SB):

$$SB = \left( \frac{\sum_{i=1}^N x_i z_i w_i}{\sum_{i=1}^N z_i w_i} - \frac{\sum_{i=1}^N x_i (1 - z_i) w_i}{\sum_{i=1}^N (1 - z_i) w_i} \right) / \sqrt{s_0^2/N_0 + s_1^2/N_1},$$

where  $s_z^2$  denotes the sample variance of the unweighted covariate and  $N_z$  the number of units in group  $z = 0, 1$  (Mercatanti and Li, 2014). When all units are weighted equally, the standardized bias equals the two-sample  $t$ -statistic. Consequently, we consider the distributions of the covariates for which the SB in absolute value exceeds 1.96 to be unbalanced between the treated and control units.

Table 3 contains the SBs of each pre-treatment variable in several symmetric subsamples around the eligibility threshold.<sup>13</sup> The units are weighted by the ATT weights. The first five subsamples feature no significant imbalance, as all the SBs in absolute value are below 1.96. In the successive subsamples, containing units further away from the propensity score threshold of 0.5, signs of covariate imbalance, on the contrary, begin to appear. For this reason, in what follows, the simple weighting estimator is applied only to the first five subsamples. In the remaining subsamples, the augmented weighting estimator, which can reduce any bias due to violations of unconfoundedness, is employed instead.

#### 4.2.2 Preliminary evidence

The estimated propensity scores can be used to provide preliminary evidence on the effect of the program on bond yields, exploiting the fact that conditional on

<sup>12</sup>The fact that the sample contains fewer controls than treated units reduces the precision of the predictions from the ordered probit model for the former group.

<sup>13</sup>In constructing the subsamples, the maximum permitted distance between the estimated propensity score and 0.5 is adjusted such that each successive subsample contains at least one additional unit.

Table 3: SBs of the covariates in symmetric intervals around  $\hat{e}(x_j) = 0.5$ .

$N_0$	$N_1$	cpn	mat	prof	cf	liq	cov	lev	solv	size	age	ltdebt
25	4	-0.31	-0.143	1.02	-0.65	-0.55	-0.88	1.85	1.57	-1.89	-0.29	0.50
25	5	-0.39	-0.130	1.18	-0.64	-0.50	-1.14	1.84	1.59	-1.36	0.17	0.32
27	5	-0.45	-0.158	1.12	-0.66	-0.54	-1.24	1.91	1.47	-1.33	0.13	0.28
27	6	-0.88	-0.179	1.15	-0.19	-0.07	-1.08	1.74	1.34	-1.79	-0.26	0.47
28	6	-0.58	-0.003	1.16	-0.24	-0.15	-0.96	1.78	1.21	-1.68	-0.45	0.52
28	7	-0.87	0.59	-0.53	-0.70	-0.56	-1.35	0.07	1.48	-2.18	-0.82	-0.42
29	7	-0.78	0.60	-0.52	-0.60	-0.48	-1.41	0.23	1.35	-2.24	-0.78	-0.22
30	7	-0.82	0.67	-0.52	-0.63	-0.57	-1.52	0.30	1.29	-2.27	-0.83	-0.14
32	7	-0.52	0.72	-0.52	-0.75	-0.69	-1.66	0.39	1.28	-2.30	-0.94	-0.10
33	7	-0.47	0.72	-0.50	-0.90	-0.84	-1.70	0.57	1.28	-2.37	-0.83	0.15
33	8	-0.33	0.97	-0.46	-0.92	-0.85	-1.84	0.74	1.26	-1.95	-0.45	0.16
34	8	-0.39	1.02	-0.47	-0.49	-0.38	-1.87	0.73	1.25	-1.82	-0.42	0.21

NOTE:  $N_z$  is the group  $z$  sample size.

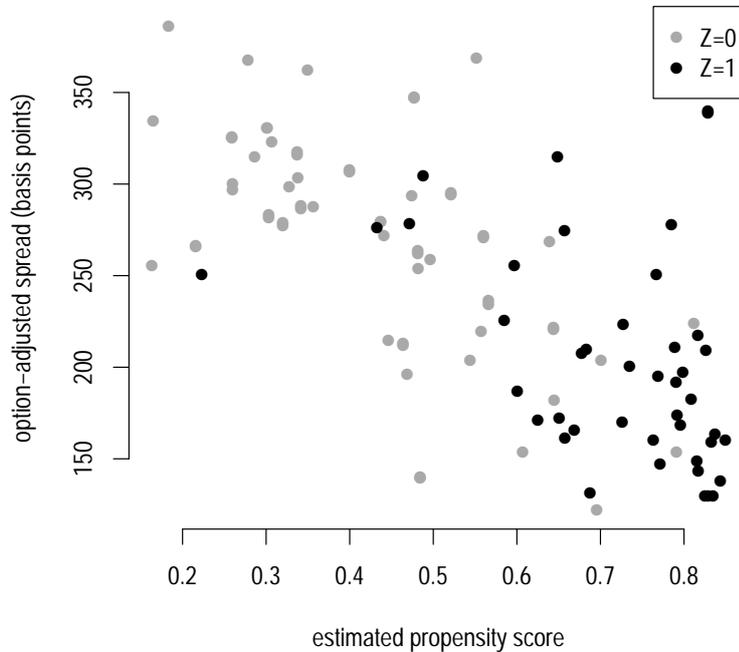
the propensity score the distributions of the covariates are balanced between the treatment and control group. Thus, also units whose estimated propensity scores are within a given narrow range should be similar in terms of their covariates. Consequently, any differences in the relation between the outcome of interest and the estimated propensity score between the treated and control units provide suggestive evidence about the effect of the program.

Motivated by these observations, we illustrate, in Figure 3, the option-adjusted spread as a function of the estimated propensity score, separately for the treated and control units. The scatter plot excludes all the units with propensity scores below 0.15 and above 0.85, this way providing a clearer illustration of the distribution of the outcome around the eligibility threshold.<sup>14</sup> For values of the estimated propensity scores for which there are both treated and control units, there is no noticeable difference between the two groups in terms of their option-adjusted spreads. This would suggest that the program did not appreciably affect the spreads of the bonds eligible for purchase relative to those of the noneligible bonds. However, definite conclusions are difficult to draw due to the relatively large dispersion in the outcomes of the two groups.<sup>15</sup>

<sup>14</sup>The observations around the threshold are used to estimate the causal effect of the program in Section 4.2.3. That being the case, it is worth mentioning that the few eligible bonds in the immediate vicinity of the threshold are in no way unusual observations. On the contrary, they represent bonds whose issuers resorted to the bond market also before the CSPP was announced.

<sup>15</sup>At first sight, it may seem puzzling that several noneligible bonds have propensity scores significantly above 0.5. A closer inspection reveals that this pattern is related to the fact that the estimated propensity score model yields less precise predictions for the BB+ and BB bonds, just below the eligibility threshold. A beneficial effect of this additional noise is that the subsamples around the threshold in which the effect of the program is identified and

Figure 3: The OAS as a function of the estimated propensity score.



### 4.2.3 Causal effects of the CSPP

We proceed by examining whether the preliminary findings of the previous section are confirmed when employing the estimators described in Section 3. First, we estimate the effect of the program on spreads at issuance over the whole sample period. Then, we investigate whether the effect changed in the course of the program. Finally, we inspect the heterogeneity of the effect along other dimensions.

Before being able to apply the augmented weighting estimator we need to specify a model for our outcome variable, the spread at issuance. We are guided by economic theory in choosing the variables to include in the model. According to Merton (1974), the rate of return of a corporate bond above that of riskless debt is determined by the terms of the bond issue (maturity, coupon rate, call provisions, etc.) and the probability of default of the issuer. Consistent with this theory, we model the spread of a bond as a function of its coupon rate (*cpn*), maturity (*mat*), the solvency of its issuer (*solv*) and the indicator variable *call*. These variables enter the model linearly. We estimate

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estimated contain a larger number of units.

this outcome model separately for the noneligible and eligible bonds and use the estimates for the former in the augmented weighting estimator.

Table 4: Estimates of the effect of the CSPP in the full sample.

$N_0$	$N_1$	ATT	se ( $p$ -val.)
<i>Panel A. Weighting est.</i>			
25	4	15.9	20.2 (0.432)
25	5	12.8	19.4 (0.510)
27	5	10.2	18.7 (0.585)
27	6	-3.4	18.5 (0.856)
28	6	2.4	20.2 (0.906)
<i>Panel B. Aug. weighting est.</i>			
28	6	35.1	29.9 (0.241)
28	7	29.7	25.6 (0.247)
29	7	18.4	26.5 (0.487)
30	7	17.1	26.5 (0.518)
32	7	11.2	26.1 (0.669)
33	7	23.9	28.7 (0.404)
33	8	29.2	25.9 (0.259)
34	8	25.0	26.1 (0.337)

NOTE:  $N_z$  is the group  $z$  sample size.

*Full sample.* Table 4 contains the estimates of the causal effect for the whole sample period, covering the period from the announcement of the program until the end of net purchases. This period is chosen to obtain the largest possible sample to evaluate the effect of the program on the eligible bonds. The choice is supported also by the fact that the eligibility criterion that we exploit became known when the program was announced.<sup>16</sup> Panel A contains the estimates of the effect of the program on the eligible bonds obtained by applying the weighting estimator to the subsamples presented in Table 3. Given that the covariate distributions are not significantly unbalanced in these subsamples, applying the simpler weighting estimator is justified. In Panel B, we report the estimates obtained from larger subsamples, in which some covariates are no longer balanced.<sup>17</sup> These estimates are obtained employing the augmented weighting estimator, which can reduce the possible bias

<sup>16</sup>The results do not, however, change if only the period of positive net purchases, starting in June 8, 2016, is considered.

<sup>17</sup>Specifically, in some of the subsamples the SB of the variable size exceeds 1.96 in absolute value.

which may arise when considering these subsamples.

All of the estimates in Table 4 suggest that the program did not have a significant effect on the spreads of the eligible bonds at issuance *vis-à-vis* those of the noneligible. This finding confirms the preliminary conclusion drawn from an inspection of the distribution of the spreads at issuance in Figure 3. We are thus led to conclude that the program did not permanently alter the primary market prices of the bonds that were eligible for purchase relative to those of the noneligible bonds. This conclusion accords with Zaghini (2019), finding that the differential effect of the program on the eligible bonds vanished in 2017 when there was a reduction in the spreads of noneligible bonds, similar in magnitude to that observed for the eligible bonds after the announcement of the program.

*Selected subperiod.* The results presented thus far concern the whole sample period. During this period, the Eurosystem's holdings of eligible bonds gradually increased. It is therefore possible that the CSPP significantly affected the relative prices of the eligible bonds only during the later part of the program. We investigate this possibility formally by applying the two weighting estimators to bonds issued during the last ten months of the program, March–December 2018.

The Eurosystem's holdings of eligible bonds had reached 140 billion euros by March 2018, and increased further to 180 billion by the end of the year. As a percentage of the outstanding eligible bonds, the holdings at these two points in time amounted to 17 and 18 per cent, respectively.<sup>18</sup> If the program was expected to affect the spreads of eligible bonds by altering their amount in the hands of the private sector, it could have exerted a substantial effect during this later subperiod. However, the estimates for the bonds issued during the last ten months of the program, presented in Table 5, do not lend support to this conjecture.<sup>19</sup> The effect of the program on the eligible bonds is statistically significant neither when applying the simple weighting estimator nor when the augmented weighting estimator is employed.

It is worth pointing out that in our preliminary evaluation of the CSPP (Li et al., 2020) we analyze an earlier part of the program, between March 2016 and September 2017. For this period, employing the weighting estimator, we obtain a marginally statistically significant, negative effect on the eligible bonds. The effect becomes somewhat smaller in absolute value but more

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<sup>18</sup>The percentages were estimated based on the eligible bonds in the ICE BofAML Euro Corporate Index (ER00) as of February 28, 2018 and December 28, 2018.

<sup>19</sup>The SBs of the pre-treatment variables in the subsamples considered in Table 5 are reported in Table B.1 in Appendix B.

Table 5: Estimates of the effect of the CSPP in Mar. 1 – Dec. 31, 2018.

$N_0$	$N_1$	ATT	se ( $p$ -val.)
<i>Panel A. Weighting est.</i>			
15	5	-8.9	50.1 (0.859)
15	6	3.8	46.1 (0.935)
<i>Panel B. Aug. weighting est.</i>			
15	6	25.4	29.2 (0.384)
15	7	25.5	26.1 (0.328)
15	8	23.0	22.8 (0.313)
15	9	22.2	19.6 (0.257)
17	9	23.1	19.8 (0.244)
17	10	22.9	18.0 (0.202)

NOTE:  $N_z$  is the group  $z$  sample size.

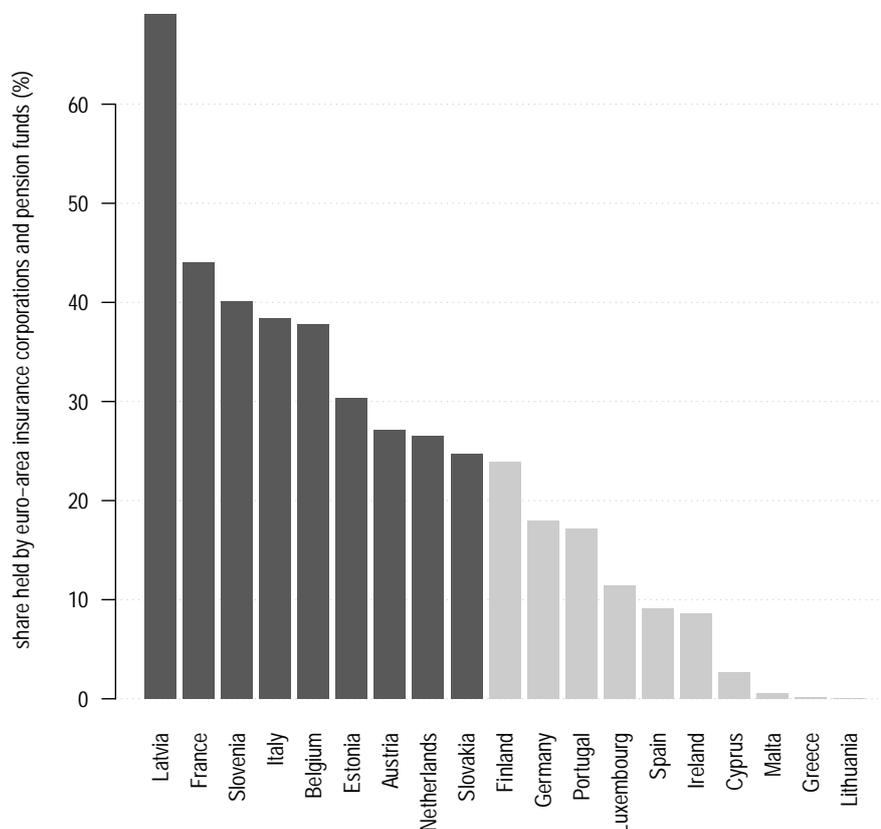
statistically significant if the augmented weighting estimator is applied instead. These findings suggest that after the program was announced it exerted a negative effect on the yield spreads of the eligible bonds, as also documented in Zaghini (2019).

*Selected jurisdictions.* Another dimension of heterogeneity that we wish to explore relates to different institutional sectors' holdings of corporate bonds. Certain classes of investors have a preference for long-term assets, such as corporate and government bonds. Pension funds and insurance companies, for instance, prefer to match their long-term liabilities with asset of similar maturities (BIS, 2011). Such definite preferences can give rise to market segmentation by which the net supply of a given security affects its price (Modigliani and Sutch, 1966). Were this the case, central bank asset purchases would likely exert a stronger price impact in jurisdictions in which a larger share of the acquired assets are held by such long-term investors (LTI). We examine this conjecture by estimating the effect of the program in countries in which pension funds and insurance companies hold a larger share of corporate bonds. Specifically, we only consider the bonds issued by companies incorporated in countries in which the share of the stock of debt securities issued by resident non-financial corporations held by euro-area insurance corporations and pension funds exceeds 24 per cent, the median in the sample. These high-LTI-share countries are Latvia, France, Slovenia, Italy, Belgium, Estonia, Austria, the Netherlands and Slovakia (see Figure 4).

The results of this subsample analysis are presented in Table 6.<sup>20</sup> Dif-

<sup>20</sup>The corresponding SBs are reported in Table B.2 in Appendix B.

Figure 4: Stock of long-term debt securities issued by resident NFCs, 2016Q1.



NOTE: Calculated using the ECB's Securities Issues (SEC) and Holding Statistics (SHS).

ferently from the results obtained using the full sample, the estimates of the effect of the program are negative when employing the simple weighting estimator. However, they are not statistically significant. Similarly, the augmented weighting estimator yields estimates which are not statistically different from zero. Thus, the program does not appear to have affected the spreads of eligible bonds differently in markets in which a large share of corporate bonds are held by insurance companies and pension funds.

Taken together, the estimates presented in this section suggest that the program did not appreciably affect the yield spreads of the eligible bonds relative to those of the noneligible, even though it entailed the Eurosystem becoming an increasingly large holder of euro-dominated corporate bonds. The results do not, however, rule out the possibility that the CSPP, along with the ECB's other asset purchase programs that were in place during the period analyzed, raised the prices of the eligible and noneligible bonds proportionally.

Table 6: Estimates of the effect of the CSPP in high LTI-share countries.

$N_0$	$N_1$	ATT	se ( $p$ -val.)
<i>Panel A. Weighting est.</i>			
34	12	-12.8	22.1 (0.563)
34	13	-17.8	21.5 (0.407)
<i>Panel B. Aug. weighting est.</i>			
34	13	8.2	17.7 (0.642)
34	14	9.3	16.4 (0.568)
36	14	9.3	16.3 (0.569)
36	15	9.8	15.3 (0.521)
38	15	11.2	15.3 (0.465)

NOTE:  $N_z$  is the group  $z$  sample size.

Moreover, it is possible that the CSPP allowed euro-area companies to issue a larger amount of CSPP-eligible bonds. The program could have had such an effect if there was limited demand for these securities before the announcement of the program.

## 5 Conclusion

The persistence of low inflation despite the historically low levels of monetary policy rates have impelled central banks to rely increasingly on large-scale asset purchase programs in pursuit of their objectives. How the effects of such programs propagate is, however, not yet fully understood. We try to shed light on this question by studying the corporate sector purchase programme (CSPP) of the European Central Bank. Specifically, we estimate the causal effect of the program on the yield spreads of the corporate bonds that were eligible for purchase under the CSPP. We do so by employing weighting estimators expressly developed for evaluating the causal effects of the program.

We find that the average effect of the program on the spreads of the eligible bonds was not statistically different from that on the spreads of the noneligible bonds. This result is robust to considering periods and jurisdictions in which the Eurosystem's holdings can be expected to have been most relevant.

The present study, however, only addresses the issue of whether central bank asset purchase programs permanently alter the prices of eligible assets relative to those of securities not eligible for purchase. The question about how they affect the level of asset prices is not investigated. Given that providing convincing estimates that can shed light on this second question may require developing novel methods of causal inference, we leave it for future research.

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## Appendix A

### A.1 General framework and notation

The RD design is formalized in terms of the potential outcomes approach (Rubin, 1974; Imbens and Rubin, 2015). Consider a sample of  $i = 1, \dots, N$  bonds. For each bond  $i$ , let  $Y_i(1)$  and  $Y_i(0)$  be the potential outcomes, i.e. the bond spreads, under the treatment ( $Z_i = 1$ ) and control ( $Z_i = 0$ ) conditions. The assignment to either condition depends on an observable ordinal pre-treatment variable  $R_i$ , the bond rating. The policy rule is such that  $Z_i = \mathbf{1}(R_i \geq r_t)$ , where  $\mathbf{1}(\cdot)$  is the indicator function and  $r_t$  represents the eligibility threshold. For each bond, besides the running variable, a set of pre-treatment covariates  $\mathbf{X}_i$  is also available. The propensity score, i.e. the probability of being assigned to the treatment condition conditional on the covariates, is denoted by  $e(\mathbf{X}_i)$ .

### A.2 Identifying assumptions

The following assumptions, invoked also in (Li et al., 2020), allow to identify the average treatment effect on the treated, i.e.  $\mathbb{E}[Y_i(1) - Y_i(0)|Z = 1]$ .

**Assumption 1** (Local overlap). *There exists a subpopulation  $\Omega_o$  of the entire population  $\Omega$  such that, for each  $i$  in  $\Omega_o$ ,  $0 < e(\mathbf{X}_i) < 1$ .*

That is, we require the existence of a subpopulation whose units could be assigned to either the treatment or the control condition with a non-zero probability conditional on the covariates. Within such a subpopulation, we further invoke the stable unit treatment value assumption (SUTVA; Rubin, 1980) and unconfoundedness.

**Assumption 2** (Local SUTVA). *For each  $i$  in  $\Omega_o$ , consider two realizations of the running variable  $r'_i$  and  $r''_i$  with possibly  $r'_i \neq r''_i$ . If  $z'_i = z''_i$ , that is, if either  $r'_i \leq r_t$  and  $r''_i \leq r_t$ , or  $r'_i > r_t$  and  $r''_i > r_t$ , then  $Y_i(z'_i) = Y_i(z''_i)$ , irrespective of the realized value of the running variable  $r_j$  for any other  $j \neq i$  in  $\Omega_o$ .*

Local SUTVA implies that potential outcomes for each unit are independent (i) of the running variable given the treatment status of the unit and (ii) of the treatment assignments of other units. It is worth pointing out that the latter implication is equivalent to assuming that the program does not affect the units in the control group. If this assumption is violated, the approach we employ still allows us to estimate the differential effect of the program on the

eligible bonds *vis-à-vis* those not eligible for purchase.<sup>21</sup>

**Assumption 3** (Local unconfoundedness). *For each unit  $i$  in  $\Omega_o$ , the treatment assignment is unconfounded given  $\mathbf{X}_i$ :  $\Pr(Z_i|Y_i(1), Y_i(0), \mathbf{X}_i) = \Pr(Z_i|\mathbf{X}_i)$ .*

Local unconfoundedness requires that in the subpopulation around the threshold potential outcomes are independent of the assignment to treatment conditional on the covariates. This assumption is similar to that of bounded conditional independence in Angrist and Rokkanen (2012) and allows us to identify the causal effect of interest in a wider window around the threshold than if we instead invoked the local randomization assumption in Lee and Card (2008). In this way a higher degree of external validity of the RD estimates can be achieved.

### A.3 Estimation

The simple weighting estimator for the ATT takes the following form:

$$\hat{\Delta}^{ATT} = \frac{\sum_{i=1}^n Y_i Z_i}{\sum_{i=1}^n Z_i} - \frac{\sum_{i=1}^n Y_i (1 - Z_i) \frac{\hat{e}(\mathbf{X}_i)}{1 - \hat{e}(\mathbf{X}_i)}}{\sum_{i=1}^n (1 - Z_i) \frac{\hat{e}(\mathbf{X}_i)}{1 - \hat{e}(\mathbf{X}_i)}}, \quad (\text{A.1})$$

where  $\hat{e}(\mathbf{X}_i)$  is the estimated propensity score and  $i = 1, 2, \dots, n$  is the subsample of interest. Under Assumptions 1–3, (A.1) is a valid estimator for the ATT (Hirano et al., 2003; Imbens, 2004). However, the weighting estimator in (A.1) can be biased when the propensity score model is misspecified. This limitation does not apply to the augmented weighting estimator for the ATT, introduced by Mercatanti and Li (2014):

$$\hat{\Delta}_{DR}^{ATT} = \frac{\sum_{i=1}^n Y_i Z_i}{\sum_{i=1}^n Z_i} - \frac{\sum_{i=1}^n \frac{Y_i (1 - Z_i) \hat{e}(\mathbf{X}_i) + \hat{\mu}_0(\mathbf{X}_i) (Z_i - \hat{e}(\mathbf{X}_i))}{1 - \hat{e}(\mathbf{X}_i)}}{\sum_{i=1}^n Z_i}, \quad (\text{A.2})$$

where  $\hat{\mu}_0(\mathbf{X}_i)$  represents the regression model for the potential outcome of the control group. Mercatanti and Li (2014) prove that the estimator in (A.2) is “doubly robust” (DR), meaning that it is consistent if either the propensity score model or the potential outcome model is correctly specified, but not necessarily both. Moreover, a recent literature (Abadie and Imbens, 2011; Ben-Michael et al., 2018) has highlighted that model augmentation provides additional robustness to covariate imbalance. Thus, augmented estimators

<sup>21</sup>Given that we define the treatment as the eligibility for purchase, the assumption would be violated if the bonds in the treatment group being eligible for purchase were to influence the spreads of the noneligible bonds. In the context of the CSPP, Zaghini (2019) considers the possibility of such an effect.

can be applied in principle even when covariates are moderately unbalanced between the treated and control units.

Conducting inference about the causal effect of our interest requires evaluating the variances of the two weighting estimators. This task is complicated by the fact that the estimators are functions of estimated model parameters; both estimators depend on the estimated propensity score model and the augmented weighting estimator additionally on the estimated outcome model. The additional uncertainty stemming from estimating these models can be accounted for by M-estimation. M-estimation-based analytical formula for the variance of the weighting estimator in (A.1) can be derived following the steps in Li et al. (2019), while one for the augmented weighting estimator in (A.2) can be found in Li et al. (2020).

## Appendix B

Table B.1: SBs of the covariates for bonds issued in Mar. 1 – Dec. 31, 2018.

$N_0$	$N_1$	cpn	mat	prof	cf	liq	cov	lev	solv	size	age	ltdebt
15	5	-0.37	1.04	-0.78	-0.58	-1.40	-0.08	1.38	-0.69	-1.52	-1.20	1.55
15	6	-0.24	0.57	-0.71	-0.28	-0.40	0.37	1.20	-0.37	-0.64	-1.61	1.37
15	7	-0.71	0.74	-1.29	-0.54	-0.63	-0.03	0.86	0.07	-1.05	-2.03	1.09
15	8	-0.93	0.81	-1.08	-0.56	-0.68	-0.14	1.22	0.06	-1.26	-2.27	1.51
15	9	-1.26	0.83	-0.99	-0.50	-0.79	-0.06	1.42	-0.57	-1.44	-2.37	1.77
17	9	-1.40	0.82	-0.99	-0.56	-0.86	-0.03	1.38	-0.58	-1.47	-2.44	1.70
17	10	-1.43	1.19	-1.03	-0.67	-0.94	-0.20	1.45	-0.47	-1.26	-2.67	1.83

NOTE:  $N_z$  is the group  $z$  sample size.

Table B.2: SBs of the covariates for bonds issued in high LTI-share countries.

$N_0$	$N_1$	cpn	mat	prof	cf	liq	cov	lev	solv	size	age	ltdebt
34	12	-1.22	1.37	0.05	-1.02	-1.46	0.24	0.90	-0.31	-1.57	-1.22	0.75
34	13	-1.49	1.31	0.23	-0.39	-0.78	0.27	1.05	-0.26	-1.79	-1.39	0.96
34	14	-1.79	1.38	-0.49	-0.80	-1.11	-0.09	0.63	0.20	-2.15	-1.67	0.59
36	14	-1.90	1.38	-0.49	-0.85	-1.20	-0.15	0.66	0.14	-2.16	-1.62	0.62
36	15	-2.06	1.45	-0.45	-0.72	-0.94	-0.11	0.60	0.27	-2.10	-1.69	0.55
38	15	-2.16	1.45	-0.45	-0.75	-0.99	-0.09	0.60	0.25	-2.09	-1.76	0.54

NOTE:  $N_z$  is the group  $z$  sample size.

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