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# A REGRESSION DISCONTINUITY DESIGN FOR CATEGORICAL ORDERED RUNNING VARIABLES APPLIED TO CENTRAL BANK PURCHASES OF CORPORATE BONDS

by Fan Li[*], Andrea Mercatanti[**], Taneli Mäkinen[**] and Andrea Silvestrini[**]

## Abstract

We propose a regression discontinuity design which can be employed when assignment to a treatment is determined by an ordinal variable. The proposal first requires an ordered probit model for the ordinal running variable to be estimated. The estimated probability of being assigned to a treatment is then adopted as a latent continuous running variable and used to identify a covariate-balanced subsample around the threshold. Assuming the local unconfoundedness of the treatment in the subsample, an estimate of the effect of the programme is obtained by employing a weighted estimator of the average treatment effect. We apply our methodology to estimate the causal effect of the corporate sector purchase programme of the European Central Bank on bond spreads.

**JEL Classification**: C21, G18.
**Keywords**: programme evaluation, regression discontinuity design, asset purchase programmes.

## Contents



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# 1 Introduction[1]

Regression discontinuity (RD) design is a quasi-experimental strategy for causal inference. In the conventional sharp RD setting, the treatment status is a deterministic step function of a pre-treatment variable, commonly referred to as the *running variable*. All units with a realized value of the running variable on one side of a pre-fixed threshold are assigned to one regime and all units on the other side are assigned to the other regime. The basic idea of RD is that one can compare units with similar values of the running variable, but different levels of treatment, to draw causal inference of the treatment at or around the threshold. First introduced in 1960 (Thistlethwaite and Campbell, 1960), RD has become increasingly popular since the late 1990s in economics and policy, with many influential applications (e.g. Angrist and Krueger, 1991; Imbens and van der Klaauw, 1995; Angrist and Lavy, 1999; Lee, 2001; van der Klaauw, 2002, among others).

In the standard RD setting, the running variable is continuous; one usually assumes continuity (namely, potential outcomes are continuous functions of the running variable at the threshold), and then employs local linear regressions or polynomials to extrapolate the counterfactual potential outcome under the opposite treatment status and estimate the causal effects at the threshold (Hahn et al., 2001; Imbens and Lemieux, 2008). A recent strand of research instead views that RD designs lead to locally randomized experiments around the threshold (Lee, 2008; Lee and Lemieux, 2010). Building on this interpretation, several recent works provide formal identification conditions and inferential strategies to estimate the causal effects (e.g. Cattaneo et al., 2015; Li et al., 2015).

RD methods have been mostly developed in the context of continuous running variables. However, in many empirical applications, assignment to treatment is determined by covariates which are inherently discrete or only take on a limited number of values (Lee and Card, 2008; Kolesár and Rothe, 2017). Examples are numerous and include the test score of a student, the year of birth of an individual (Carpenter and Dobkin, 2011) and the credit

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score of a person (Keys et al., 2010). A categorical running variable poses challenges to RD estimation for two reasons. First, RD estimation always involves measuring the distance of each unit to the threshold. When the running variable is categorical, ordered or not, values of the running variable provide little information on the distance to the threshold. Consequently, one can no longer compare outcomes within arbitrarily small neighborhoods of the threshold to identify the causal effects, and thus has to account for the uncertainty about the relationship between the running variable and the outcomes (Lee and Card, 2008). Second, if the number of categories is small, even considering only units in the two categories bordering the threshold may lead to misleading results, particularly when the units within the two categories differ considerably from each other. Indeed, existing literature provides limited insights on how to apply RD in such settings. Lee and Card (2008) assume a parametric functional form relating the outcome to the running variable and account for the uncertainty in the choice of this functional form. Dong (2015) considers a setting in which the running variable is discrete due to rounding and shows that in this case standard RD estimation leads to biased estimates of the average treatment effect and provided formulas to correct for this discretization bias.

Two recent works shed further light on the issues with discrete running variables. Kolesár and Rothe (2017) show that the confidence intervals proposed by Lee and Card (2008) have poor coverage properties and suggest to calculate alternative confidence intervals under suitable restrictions on the functional form of the relationship between the outcome and the running variable. Imbens and Wager (2018) propose a general optimization-based approach that minimizes the worst-case conditional mean-squared error among all linear estimators, which is applicable to both continuous and discrete running variables. However, these recent advances are not directly applicable to problems in which the running variable is ordered categorical rather than discretized from an underlying continuous variable.

This paper addresses this limitation by developing a framework for conducting RD inference when the running variable is ordered categorical. Our methodological innovation is motivated by our interest in the evaluation of the European Central Bank's (ECB) corporate sector purchase programme (CSPP), which illustrates the challenges posed by categorical running variables in a RD context. The CSPP entails the acquisition of corporate bonds, with the aim of strengthening the pass-through of unconventional monetary policy measures to the financing conditions of the real economy. Under the CSPP, the Eurosystem purchases investment-grade bonds issued by non-bank corporations. Cast into a RD framework, the assignment to treatment in the CSPP is determined by an ordered categorical running variable, the rating of the bond. Specifically, only bonds with an investment-grade rating (i.e., BBB+ or above) receive the treatment, taking the form of being eligible for purchase by the Eurosystem.

The rating of a bond is determined by the financial strength of its issuer and bond-specific characteristics. This observation motivates us to develop a new approach in which we quantify the distance of each unit to the threshold in terms of a continuous latent variable which determines the assignment of each unit to a category. That is, we use the supplementary pre-treatment information to estimate a latent continuous running variable. Specifically, we adopt the local randomization perspective to RD and propose a three-step procedure with several new features. *First*, we postulate an ordered probit model for the categorical running variable, i.e., the bond rating, employing as predictors issuer and bond characteristics, and take the estimated probability of being assigned an investment-grade rating as the surrogate continuous running variable. *Second*, based on the estimated probability, we identify a subset of units in which the covariates in the treatment and control groups are similar. *Third*, within such a subset, we invoke a *local unconfoundedness* assumption and use the estimated probability to construct a weighted sample to estimate the causal effect of the treatment. The weighted sample represents a population of interest, namely units which could conceivably have been assigned to either treatment status. This is the population which we consider to be close to the threshold. This strategy is similar to propensity score weighting in causal inference (Hahn, 1998; Hirano and Imbens, 2001; Hirano et al., 2003; Li et al., 2018), which, to our knowledge, has not been discussed in the RD literature.

The rest of the paper proceeds as follows. Section 2 introduces the general framework, the methodology and the estimation strategy. Section 3 spells out the main institutional features of the CSPP. Section 4 presents the dataset. Section 5 describes the empirical application. Section 6 discusses some methodological issues which emerge from the analysis and Section 7 concludes.

# 2 Methods

## 2.1 Basic setup and assumptions

We proceed under the potential outcomes framework to causal inference (Rubin, 1974; Imbens and Rubin, 2015). Consider a sample of $N$ units indexed by $i = 1, \ldots, N$ drawn from a super-population $\Omega$. Let $R_i : \{r_1, r_2, ..., r_j, ..., r_J\}$ be the ordered categorical running variable with $J$ categories and $r_j > r_{j-l}$ for any integer $1 \leq l \leq (j-1)$. Based on $R_i$, a binary treatment $Z_i$ is assigned according to a RD rule: If a unit has a value of $R_i$ falling above (or below, depending on the specific application) a pre-specified threshold, $r_t$, that unit is assigned to treatment; otherwise, that unit is assigned to control. That is, the treatment status $Z_i = \mathbf{1}(R_i \geq r_t)$, where $\mathbf{1}(\cdot)$ is the indicator function. For each unit, besides the running variable, a set of pre-treatment covariates $\mathbf{X}_i$ is also observed. Each unit has a potential outcome $Y_i(z)$ corresponding to each treatment level $z \in \{0, 1\}$, and only the one corresponding to the observed treatment status $Y_i = Y_i(z_i)$ is observed. Define the propensity score $e(\mathbf{x}_i)$ as the probability of unit $i$ receiving the treatment conditional on the covariates: $e(\mathbf{x}_i) \equiv \Pr(Z_i = 1|\mathbf{X}_i) = \Pr(R_i \geq r_t|\mathbf{X}_i)$.

For valid causal inference, we focus on the subpopulations whose units all have non-zero probability of being assigned to either treatment condition. Formally, we make the assumption of *local overlap*.

**Assumption 1** (Local overlap)**.** *There exists a subpopulation $\Omega_o \subset \Omega$ such that, for each $i$ in $\Omega_o$, we have $0 < e(\mathbf{x}_i) < 1$.*

Within the subpopulation $\Omega_o$, we further make two assumptions.

**Assumption 2** (Local SUTVA)**.** *For each unit $i$ 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 $r_j$ of any other unit $j \neq i$ in $\Omega_o$.*

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

Local SUTVA implies (i) the absence of interference between units, and (ii) independence of the potential outcome on the running variable given the treatment status for the same unit. Local unconfoundedness forms the basis for causal inference under RD: it entails the existence of a subpopulation around the threshold for which the assignment to treatment is unconfounded given the observed pre-treatment variables. We will elaborate on the selection of this subpopulation in Section 2.2.3. Local unconfoundedness extends the local randomization assumption in Lee and Card (2008), and is similar to the bounded conditional independence assumption in Angrist and Rokkanen (2012). As explained by Lee and Card (2008, p. 655), the local randomization assumption means that "it may be plausible to think that treatment status is 'as good as randomly assigned' among the subsample of observations that fall just above and just below the threshold." For instance, suppose that a policy grants a sum of money to households when their income (i.e., a continuous running variable) is below a given threshold. Local randomization means that for each household with an income level inside a small window around the threshold the observed income is assumed to be governed by chance. Given that the probability to enter in this window around the threshold depends on household characteristics (e.g., households with high level of education and wealth plausibly have much less probability to have an income in an interval around the threshold compared to households with low level of education and wealth) and given the continuity of the running variable, the local randomization assumption implies that, for each unit in the window, the probability to observe a value of income above the threshold is 0.5 in the limit. Our local unconfoundedness assumption extends the local randomization assumption by allowing the probability to be randomly assigned to the treatment to depart from 0.5 and to depend on the pre-treatment variables (education and wealth in this example). Therefore, relaxation of the local randomization hypothesis allows us to enlarge the subsample of units around the threshold for which randomization can be assumed to hold, given that it lets units for which the probability progressively departs from 0.5 enter in the subsample. However, our unconfoundedness assumption is still "local" in nature: indeed, we maintain the RD hypothesis according to which, for each unit in the population, treatment assignment also depends on the unobserved units' characteristics: as a result, the interval around the threshold for which the unconfoundedness hypothesis holds has to be bounded.

## 2.2 Design and analysis

The key to our proposal is to treat the probability $\Pr(R_i = r_j|\mathbf{X}_i)$ as a latent continuous running variable instead of using the observed category $R_i$ as an ordinal running variable. Under this perspective, we will define the causal estimand as a weighted average treatment effect on a subpopulation with particular policy interest, namely, the overlap population.

Our estimation strategy consists of three steps. *First*, we fit an ordered probit model for the categorical running variable conditional on the observed covariates and take the estimated probability of being assigned to treatment as the latent continuous running variable. *Second*, based on the estimated probability, we identify a subset of units in which the local unconfoundedness assumption is plausible by checking the covariate balance. *Third*, within this subpopulation, we estimate the average treatment effect for the target population. The three steps of the estimation strategy are illustrated in Figure 1.

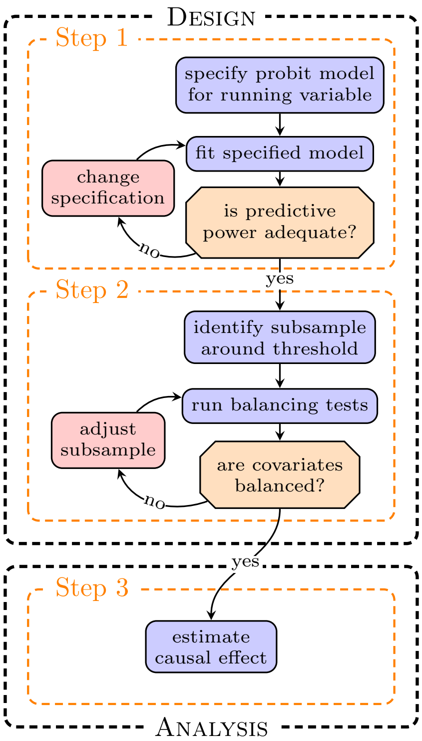


Figure 1: Three-step estimation procedure for causal estimand

### 2.2.1 Probit model for the ordered running variable

We postulate an ordered probit model for the distribution of the ordered running variable, $\Pr(R_i = r_j|\mathbf{X}_i)$, and consequently for the propensity score $e(\mathbf{x}_i)$. Specifically, we assume that each unit's observed category $R_i$ is determined by a latent normally distributed variable $R_i^*$ as follows:

$$R_i^* = \mathbf{X}_\mathrm{i}\beta + U_i, \quad U_i \sim N(0,1) \tag{1}$$

and

$$R_i = \begin{cases} r_1, & \text{if } R_i^* \leq \mu_1, \\ r_j, & \text{if } \mu_{j-1} < R_i^* \leq \mu_j, \\ r_J, & \text{if } R_i^* > \mu_{J-1}, \end{cases} \tag{2}$$

where $\mu_j \in \{\mu_0, \mu_1, \ldots, \mu_{J-1}, \mu_J\}$ is a series of cutoff points, with $\mu_0 = -\infty$ and $\mu_J = \infty$.[2] That is, $R_i$ falls in category $r_j$ when the latent variable $R_i^*$ falls in the interval between $r_{j-1}$ and $r_j$. A probit model for $R_i$ is plausible in contexts where the category classifies units by ordered levels of "quality", which can be for example the grade a student achieves in a subject, or in our case the credit quality of a bond. In these examples, the quality of a unit is supposed to be a continuous variable (e.g., the student's level of knowledge in a subject, or the issuer's capacity to honor its debts) we cannot observe, but for which we can observe the interval where it falls. Based on the ordered probit model (1)-(2), we have

$$\Pr(R_i = r_j|\mathbf{X}_\mathrm{i}) = \Pr(\mu_{j-1} < R_i^* \leq \mu_j) = \Phi(\mu_j - \mathbf{x}_i\beta) - \Phi(\mu_{j-1} - \mathbf{x}_i\beta).$$

The ordered probit model belongs to the class of generalized linear models suitable for ordinal responses. The link function is the inverse of the normal CDF, which implies that the probability of response is a monotonic function of the linear transformation $\mathbf{x}_\mathrm{i}\beta$ (Agresti, 2013), namely, for any $\mathbf{x}_1$ and $\mathbf{x}_2$, $\Pr(R_i \leq r_j|\mathbf{X}_1 = \mathbf{x}_1) \leq \Pr(R_i \leq r_j|\mathbf{X}_2 = \mathbf{x}_2)$ when $\mathbf{x}_1\beta > \mathbf{x}_2\beta$. Given the deterministic relationship $Z_i = \mathbf{1}(R_i \geq r_t)$, the monotonicity also holds for the propensity score $e(\mathbf{x}_i)$. Therefore, we expect the estimated propensity scores, $\hat{e}(\mathbf{x}_i)$, to be close to 1 for units for which we observe high values of $R_i$, while being close to 0 for units for which we observe low value of $R_i$.

Moreover, given the monotonicity of $e(\mathbf{x}_i)$ in $\mathbf{x}_i\beta$, and provided that $\mathbf{X}_i$ is a good predictor of the ordinal responses, we expect the average $\hat{e}(\mathbf{x}_i)$ to be below 0.5 for units whose value of $R_i$ is just below the threshold $r_t$, i.e., $\sum_i \mathbf{1}(i \in r_{t-1})\hat{e}(\mathbf{x}_i)/\sum_i \mathbf{1}(i \in r_{t-1}) < 0.5$, and above or equal to 0.5 for

[2] The Gaussianity assumption in (1) is not crucial and can be relaxed in favour of semi-nonparametric specifications. See, for instance, Stewart (2004).

units whose value of $R_i$ is at the threshold $r_t$, i.e., $\sum_i \mathbf{1}(i \in r_t)\hat{e}(\mathbf{x}_i)/\sum_i \mathbf{1}(i \in r_t) \geq 0.5$. Therefore, values of the propensity score around 0.5 pertain to units which fall in categories around the threshold. These units form a target population of policy interest because they can be assigned with non-negligible probability to either treatment condition and therefore are the mostly affected by, even small, changes in the policy. This target population can be formally defined using the concept of "overlap weights" (Li et al., 2018), as described in Section 2.2.2.

In practice, a well-specified ordered probit model would produce in-sample predictions of $e(\mathbf{x}_i)$ that satisfy the above patterns, which can be visually checked by the box plots of the estimated $\Pr(R_i = r_j|\mathbf{X}_i)$ in each category of the observed running variable.

### 2.2.2 Causal estimands

Within a subpopulation $\Omega_0$ where Assumptions 1-3 hold, we can define a class of average treatment effects estimands, each corresponding to a different target population. To formalize, we assume that the marginal distribution of the pre-treatment variables $\mathbf{X}_i$ in $\Omega_0$, $Q(\mathbf{x}_i)$, exists. Denote the density of the pre-treatment variables $\mathbf{X}_i$ in the entire, treated and control population in $\Omega_0$ by $f(\mathbf{x}_i)$, $f_1(\mathbf{x}_i)$, $f_0(\mathbf{x}_i)$, respectively. Representing the target population density by $f(\mathbf{x}_i)h(\mathbf{x}_i)$, where $h(\mathbf{x}_i)$ is pre-specified function of $\mathbf{x}_i$, we can define a general class of weighted average treatment effect (WATE) estimands (Hirano et al., 2003):

$$\tau_h \equiv \frac{\int \mathbb{E}[Y(1) - Y(0)|\mathbf{x}_i]f(\mathbf{x}_i)h(\mathbf{x}_i)dQ(\mathbf{x}_i)}{\int f(\mathbf{x}_i)h(\mathbf{x}_i)dQ(\mathbf{x}_i)}. \tag{3}$$

Li et al. (2018) show that for any $h(\mathbf{x}_i)$,

$$f(\mathbf{x}_i)h(\mathbf{x}_i) = f_1(\mathbf{x}_i)h(\mathbf{x}_i)/e(\mathbf{x}_i) = f_0(\mathbf{x}_i)h(\mathbf{x}_i)/(1 - e(\mathbf{x}_i)).$$

This implies that applying the balancing weights—$w_1(\mathbf{x}_i) = h(\mathbf{x}_i)/e(\mathbf{x}_i)$ for the treated units and $w_0(\mathbf{x}_i) = h(\mathbf{x}_i)/(1 - e(\mathbf{x}_i))$ for the controls—balances the distribution of the pre-treatment variables between the treatment groups, and thus enables inferring the causal effect $\tau_h$ defined on the target population $f(\mathbf{x}_i)h(\mathbf{x}_i)$. A nonparametric consistent estimator of $\tau_h$ is the sample difference in the weighted average outcomes between treatment groups

$$\hat{\tau}_h = \frac{\sum_i w_1(\mathbf{x}_i)Z_iY_i}{\sum_i w_1(\mathbf{x}_i)Z_i} - \frac{\sum_i w_0(\mathbf{x}_i)(1 - Z_i)Y_i}{\sum_i w_0(\mathbf{x}_i)(1 - Z_i)}. \tag{4}$$

Among the general class of balancing weights, of particular relevance to our application is the overlap weights, $(w_0 = e(\mathbf{x}_i), w_1 = 1 - e(\mathbf{x}_i))$, corresponding to $h(\mathbf{x}_i) = e(\mathbf{x}_i)(1 - e(\mathbf{x}_i))$, the maximum of which is attained at

$e(\mathbf{x}_i) = 0.5$. This defines a target population whose pre-treatment characteristics could appear with substantial probability in either treatment group, i.e., with the most overlap. The corresponding causal estimand $\tau_h$ is called the *average treatment effect for the overlap population (ATO)*. Arguably, the overlap population consists of the units whose treatment assignment might be most responsive to a policy shift as new information is obtained. In our RD framework, this *overlap* population is exactly the subpopulation around the threshold: with overlap weights, the units are smoothly downweighted as their latent running variable moves away from the threshold, i.e., $e(\mathbf{x}_i) = 0.5$. Li et al. (2018) show that the overlap weights lead to the minimal asymptotic variance of $\hat{\tau}_h$ among all $h(\cdot)$ functions under mild regularity conditions.

Other two estimands relevant to our application are the average treatment effect (ATE) and the average treatment effect for the treated (ATT). The ATE corresponds to $h(\mathbf{x}_i) = 1$ and the balancing weight $w_0 = 1/(1 - e(\mathbf{x}_i)), w_1 = 1/e(\mathbf{x}_i)$ while for the ATT $h(\mathbf{x}_i) = e(\mathbf{x}_i)$ and $w_0 = e(\mathbf{x}_i)/(1 - e(\mathbf{x}_i)), w_1 = 1$. Though the ATE and ATT do not have a natural connection to the ordinal RD setting considered here, they are estimands of common interest in the economics literature and we will compare them with the ATO in our empirical application.

### 2.2.3 Select the subpopulation

An important issue in practice is how to select the subpopulation $\Omega_0$ where Assumption 3 holds. There can be many choices of the shape of the subpopulation. Following the convention in the literature, we first focus on the symmetric intervals around the threshold: $(0.5 - d) < \hat{e}(\mathbf{x}_i) < (0.5 + d)$. To select the bandwidth $d$, we adopt the idea of balancing tests (Cattaneo et al., 2015; Li et al., 2015). Specifically, given the "local" nature of Assumption 3, we expect the pre-treatment covariates to be balanced between treatment groups close to the threshold, but the balance will break down when moving away from the threshold. Therefore, starting from a small $d$, we check the covariate balance of units in the interval $(0.5 - d) < \hat{e}(\mathbf{x}_i) < (0.5 + d)$ and gradually increase $d$ until significant imbalance is detected. The "optimal" bandwidth will be set to be the maximum $d$ such that the covariates are balanced. We also consider subpopulations defined by asymmetric intervals. This allows us to find covariate-balanced subsamples with a larger number of units. As a result, asymmetric intervals allow us to increase the external validity of our findings.

# 3 The corporate sector purchase programme

## 3.1 Operational features and implementation

On March 10, 2016, the European Central Bank (ECB) announced a new asset purchase program, termed the corporate sector purchase programme (CSPP), to be implemented in conjunction with the other non-standard monetary policy measures already in place. The CSPP consists of purchases of investment-grade corporate bonds issued by euro-area non-bank corporations, and is a part of the ECB's expanded asset purchase programme (APP).[3] It aims at strengthening the pass-through of the Eurosystem asset purchases to the financing conditions of the real economy, in pursuit of the ECB's price stability objective.

On April 21, 2016, the ECB provided detailed guidelines concerning the main operational features of the CSPP.[4] The CSPP has to comply with the prohibition of monetary financing with regard to the purchase of eligible debt instruments issued by public undertakings.[5] In addition, the purchases must be conducted respecting the principle of an open market economy with free competition, without hampering the smooth functioning of European financial markets. The purchases can occur both in the primary and in the secondary market. The principal payments of the instruments have to be reinvested as the underlying debt instruments mature.[6]

The CSPP has to be implemented following adequate risk management and due diligence procedures. To this end, the ECB has established a number of safeguards to ensure that financial risks are adequately taken into account. In particular, to be *eligible* for outright purchase under the CSPP, *debt instruments issued* must satisfy the following conditions: (i) have a remaining maturity between 6 months and 31 years at the time of purchase; (ii) be denominated in euro; (iii) have a minimum first-best credit assessment of at least rating of BBB- or equivalent (i.e., investment-grade) obtained from an external and independent credit assessment institution; (iv) provide a yield to maturity, which can also be negative, above the deposit facility rate.

In addition, the *bond issuer* has to comply with the following requirements: (i) is a corporation established in the euro area; (ii) is not a credit institution

[3]See `https://www.ecb.europa.eu/press/pr/date/2016/html/pr160310_2.en.html`

[4]See `https://www.ecb.europa.eu/press/pr/date/2016/html/pr160421_1.en.html`

[5]Meaning any undertakings over which the State may directly or indirectly exercise a dominant influence by virtue of its ownership or its financial participation therein.

[6]See Decision (EU) 2016/948 of the European Central Bank of 1 June 2016 on the implementation of the corporate sector purchase programme (ECB/2016/16) in the Official Journal of the European Union.

supervised under the Single Supervisory Mechanism; (iii) does not have a parent undertaking that is also a credit institution; (iv) is not an investment firm, an asset management vehicle or a national asset management fund created in order to support financial sector restructuring; (v) has not issued an asset-backed security, a 'multi cedula' or a structured covered bond; (vi) must not have a parent company which is under banking supervision inside or outside the euro area, and must not be a subsidiary of a supervised entity or a supervised group; (vii) is not an eligible issuer for the public sector purchase programme (PSPP).

The CSPP started on June 8, 2016, and ended on December 31, 2018. The purchases were carried out in a decentralized manner by six national central banks, acting on behalf of the Eurosystem: Banco de España, Banca d'Italia, Banque de France, Deutsche Bundesbank, Nationale Bank van België/Banque Nationale de Belgique, and Suomen Pankki.

## 3.2 Potential effects and previous literature

The effect of the CSPP on bond spreads is, from the viewpoint of economic theory, *a priori* ambiguous. On the one hand, arbitrage pricing theory (Ross, 1976) asserts that no-arbitrage implies the existence of strictly positive state prices, such that the price of an asset equals the state-price weighted sum of its payoff across states. Thus, any asset purchase program will not influence bond prices unless it alters the state prices used to price all assets in the economy or bond payoffs. However, any such effects on investors' risk aversion or default risk would require the program to have macroeconomic effects (Krishnamurthy and Vissing-Jorgensen, 2011). This is unlikely to be the case for the CSPP, which constitutes only a small part of the ECB's expanded asset purchase programme.[7] On the other hand, according to the literature on liquidity and asset prices (Culbertson, 1957; Acharya and Pedersen, 2005), if the program altered the expected liquidity of the eligible bonds, then it could have also affected their prices. Any such liquidity effects could have influenced also the non-eligible bonds (Newman and Rierson, 2004). However, previous evidence from the secondary bond market shows that such liquidity effects, if any, only affected the eligible bonds (Abidi and Flores, 2017).

There are a few earlier works analyzing the CSPP. Zaghini (2019) assesses the effects of the program in the context of the primary bond market, by controlling for many possible determinants of bond spreads. Looking instead

[7] If it were the case, not only the eligible bonds but also the non-eligible ones as well as all other assets in the economy would be affected. Our approach is purposely designed not to detect such general equilibrium effects.

at the secondary market, Abidi and Flores (2017) employ differences in credit rating standards between investors and the ECB to shed light on the market's reaction to the announcement of the program. Similarly, Cecchetti (2017) studies the announcement effects of the CSPP, using an econometric model which decomposes corporate bond spreads. Arce et al. (2017) and Grosse-Rueschkamp et al. (2018), conversely, investigate how the program affected bank lending. Finally, Galema and Lugo (2017) examine the individual bond purchases under the CSPP and their effects on the financing decisions of the issuers. We complement these works by providing estimates of the effect of the program which rely on a formal statistical framework of causal inference.

Our evaluation of the CSPP is also related to the growing literature, initiated by Fleming (2003), Cohen and Shin (2003) and Brandt and Kavajecz (2004), on the price impact of trades in bond markets.[8] However, this literature, with the exception of Mitchell et al. (2007) and Ellul et al. (2011), is concerned with the government bond market. Given that the extreme safety and liquidity of government bonds render them similar to money (Krishnamurthy and Vissing-Jorgensen, 2012), the price impact of trades in the corporate bond market can be expected to differ substantially from that in the government bond market.

# 4 Data

We consider bonds issued after the program was announced as we wish to focus on the primary market. This is motivated by the relatively low liquidity of the secondary corporate bond markets in Europe (Biais et al., 2006; Gündüz et al., 2017), rendering secondary market quotes noisy indicators of going prices. Primary market prices, on the contrary, provide accurate information about the market valuation of bonds at the time of their issuance. Our data comes from two sources. The first source is Bloomberg, from which we obtained all the corporate bonds satisfying the eligibility criteria of the program with the exception of that pertaining to ratings, and issued between March 10, 2016 and September 30, 2017.[9] A total of 899 such bonds were found.

For each bond, we obtained from Bloomberg the following information: International Securities Identification Number (ISIN), coupon rate (cpn), maturity type, issue date, original maturity (mat), amount sold, coupon type,

[8]More recent contributions analyzing the price impact of central bank purchases of government bonds include Joyce and Tong (2012), D'Amico et al. (2012), D'Amico and King (2013), Eser and Schwaab (2016) and Ghysels et al. (2017).

[9]The choice of the start date is motivated by the fact that already when the program was announced it became known that only investment-grade bonds are eligible for purchase.

rating at issuance by Standard & Poor's, Moody's, Fitch and DBRS along with its option-adjusted and government bond spreads. Maturity type refers to any embedded options the bond contains (callable, putable, convertible) or it being a bullet bond (at maturity). Coupon type is one of the following: fixed, zero-coupon, pay-in-kind or variable. The government bond spread (G-spread) is simply the difference between the yield to maturity of the bond and the yield to maturity of a government bond with similar maturity, while the option-adjusted spread (OAS) further accounts for any embedded option features of the bond.[10] For both spreads, the first available value between the issue date and the subsequent eight days was employed. We also obtained from Bloomberg the country of incorporation and the industry (as given by the Bloomberg Industry Classification System) of the issuer of each bond. Due to the difficulty of comparing bonds with variable coupon rates to fixed rate bonds, we excluded the former (6 bonds) from the analysis. Summary statistics for the bond characteristics are provided in Table 1.

Table 1: Summary statistics for the bond characteristics.

| variable | mean | sd | $Q_1$ | $Q_2$ | $Q_3$ | N |
|---|---|---|---|---|---|---|
| coupon rate | 2.6 | 2.2 | 1.0 | 1.9 | 3.9 | 893 |
| original maturity | 7.9 | 3.9 | 5.0 | 7.0 | 10 | 893 |
| amount sold | 490 | 410 | 200 | 500 | 650 | 883 |
| OAS | 200 | 181 | 82 | 125 | 276 | 641 |
| G-spread | 276 | 254 | 99 | 160 | 408 | 893 |

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

We also illustrate, in Figure 2, how the option-adjusted spreads vary across bonds issued during the program with different ratings (right in each pair). For the sake of comparison, the distributions of the OAS are presented also for bonds issued before the announcement of the CSPP, between March 13, 2014 and March 9, 2016. This time frame was chosen to obtain a similar number of bonds as in the program data. For all rating categories, apart from the highest two, the option-adjusted spreads were lower during the program than before it. A particularly notable difference is observed for the lowest investment-grade category, BBB-.

The second source of data that we employ is S&P Capital IQ, from which

[10] Both spreads are calculated with respect to a synthetic euro-area government bond. The OAS relies on modeling the stochastic evolution of the benchmark rate to be able to evaluate the probabilities of the embedded options being exercised.

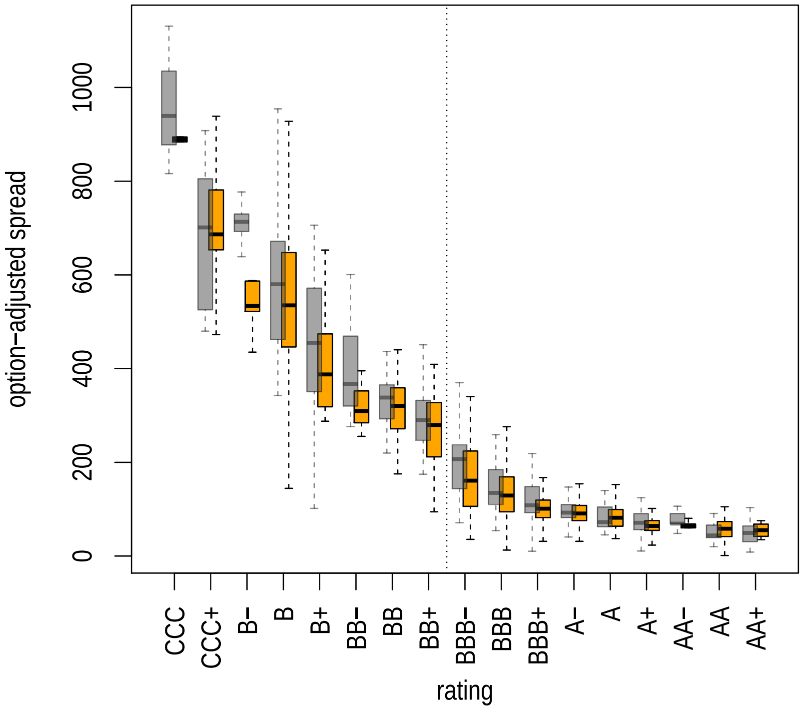


Figure 2: OAS by rating, before (left in each pair) and during the program.

we obtained balance sheet (BS) and income statement (IS) data for the bond issuers. More specifically, we first identified the ultimate parent company of each subsidiary issuer. Then, for these ultimate parents and the issuers with no parent companies, we obtained the following BS and IS items for the fiscal year 2015: earnings before interest and taxes (EBIT), total revenue, cash from operations, total assets, total liabilities, interest expenses, total debt, common equity and long-term debt. In addition, we recorded the year in which the company had been founded. When no data existed for the ultimate parent company, for instance due to it being a private company, we obtained data for the parent on the highest level in the corporate structure for which data was available. From the recorded data, we constructed the following variables: profitability (prof), cash flow (cf), liquidity (liq), interest coverage (cov), leverage (lev), solvency (solv), size, age and long-term debt (ltdebt). They are described in Table 2. We chose these variables as they are known to be determinants of credit quality (Blume et al., 1998; Mizen and Tsoukas, 2012). Units for which we obtained anomalous variable values suggesting erroneously recorded BS or IS items were excluded from the calculation of the summary statistics and the rest of the analysis.[11]

Table 2: Summary statistics for the issuer characteristics.

| variable | definition | mean | sd | $Q_1$ | $Q_2$ | $Q_3$ | N |
|---|---|---|---|---|---|---|---|
| prof | $\frac{\text{EBIT}}{\text{total revenue}}$ | 0.14 | 0.27 | 0.046 | 0.098 | 0.17 | 766 |
| cf | $\frac{\text{cash from operations}}{\text{total assets}}$ | 0.055 | 0.095 | 0.033 | 0.067 | 0.096 | 699 |
| liq | $\frac{\text{cash from operations}}{\text{total liabilities}}$ | 0.10 | 0.11 | 0.049 | 0.095 | 0.15 | 699 |
| cov | $\frac{\text{EBIT}}{\text{interest expenses}}$ | 7.2 | 17 | 1.4 | 3.6 | 6.9 | 727 |
| lev | $\frac{\text{total debt}}{\text{total assets}}$ | 0.37 | 0.20 | 0.24 | 0.35 | 0.49 | 746 |
| solv | $\frac{\text{common equity}}{\text{total assets}}$ | 0.29 | 0.20 | 0.17 | 0.28 | 0.41 | 756 |
| size | log(total revenue) | 3.6 | 1.0 | 3.0 | 3.8 | 4.4 | 772 |
| age | 2017 − year founded | 77 | 76 | 22 | 61 | 115 | 709 |
| ltdebt | $\frac{\text{long-term debt}}{\text{total assets}}$ | 0.33 | 0.35 | 0.16 | 0.26 | 0.40 | 747 |

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

In the following analysis, we restrict attention to bonds for which data about their coupon rate, original maturity and all the characteristics of their issuers, i.e., the pre-treatment variables, is available. There are 591 such bonds, of which 29 are convertible, 351 callable and 211 bullet bonds.[12] In what follows, we will denote by *call* the indicator variable equal to 1 if the bond is callable and 0 otherwise.

# 5 Empirical application

We employ the methods proposed in Section 2 to evaluate the effects of the CSPP on bond spreads in the primary market. More specifically, we assess how the eligibility for purchase under the CSPP affects bond spreads at the time of their issuance. We define the treatment as the eligibility for purchase rather than the actual purchase of the bond for the following reasons. First, purchases under the CSPP are not pre-announced, making it impossible for

[11] More specifically, we excluded the bonds issued by companies for which interest coverage exceeded 250 (3 companies), leverage exceeded 1 (3 companies) and solvency was below -1 (1 company). These exclusions led to the removal of 29 bonds.

[12] The convertible bonds are not used in assessing the effect of the program as OAS is not available for them.

the market participants to react to them. Second, given that most eligible bonds issued after the program was announced have been purchased by the Eurosystem, market participants are likely to take the eligibility for purchase into consideration when pricing a bond at its issuance.[13] Finally, due to the relatively low liquidity of the secondary bond market, the effect of the actual purchase can be expected to be highly bond-specific and potentially only short-lived.[14] Any permanent effect of the program on spreads of eligible bonds is, instead, likely to be largely observed already at issuance. Defining the treatment in this manner implies that its effect can be evaluated using a *sharp* RD design.

Having defined the treatment as the eligibility for purchase, we classify all bonds whose highest rating is equal to or greater than BBB- as treated units and the remaining bonds as control units. It should be pointed out that this does not imply that the treatment is equivalent to being assigned an investment-grade rating. This is due to the fact that market participants employ either the average or the minimum rating to identify investment-grade bonds (Abidi and Flores, 2017). Therefore, the threshold employed by market participants is above that defining eligibility for purchase under the CSPP.

## 5.1 Design

In the design phase, our first objective is to obtain a well-specified ordered probit model for the running variable conditional on the pre-treatment variables. In particular, we are concerned about how well the ordered probit model predicts ratings around the BBB- eligibility threshold. Good predictive power around the threshold ensures that the subset of units which we ultimately employ to evaluate the program are close to the threshold in terms of their ratings.

Relying on substantive knowledge, we first include the economically most relevant pre-treatment variables which help predict ratings. This leads to the following seven variables: cpn, mat, prof, cov, size, ltdebt and call. Then, we form all the possible interaction and quadratic terms from these variables and include a combination of them which yields a model specification with adequate predictive power. The final specification is given in Table 3.

To assess the predictive power of the model, we inspect how well it predicts the probability of being assigned to the treatment group around the

[13]Of the 346 eligible bonds that we ultimately use in our analysis, more than 85 per cent had been purchased by the Eurosystem as of January 26, 2018.

[14]Due to the limited liquidity of the corporate bond market, a reliable evaluation of the effect of actual purchases would require focusing on the relatively few frequently traded bonds in the secondary market.

Table 3: Estimated coefficients of the ordered probit model.

| variable | estimate | $\widehat{\text{s.e.}}$ | $t$-stat. |
|---|---|---|---|
| cpn | 1.263 | 0.335 | 3.78 |
| mat | 0.218 | 0.030 | 7.27 |
| prof | 2.959 | 1.185 | 2.50 |
| cov | −0.002 | 0.007 | −0.28 |
| size | 1.685 | 0.210 | 8.03 |
| ltdebt | 7.496 | 2.429 | 3.09 |
| call | −0.033 | 0.226 | −0.15 |
| cpn×size | −0.466 | 0.068 | −6.81 |
| cpn×ltdebt | −0.026 | 0.187 | −0.14 |
| cpn×call | −0.607 | 0.127 | −4.79 |
| mat×prof | −0.096 | 0.068 | −1.41 |
| mat×ltdebt | −0.043 | 0.066 | −0.65 |
| prof×prof | −0.863 | 1.126 | −0.77 |
| prof×call | 1.107 | 0.542 | 2.04 |
| cov×call | 0.011 | 0.008 | 1.38 |
| size×ltdebt | −1.066 | 0.463 | −2.30 |
| ltdebt×ltdebt | −5.114 | 1.374 | −3.72 |

NOTE: Maximum likelihood estimates of the coefficients of the ordered probit model.

investment-grade threshold. Figure 3 illustrates the distribution of the estimated propensity scores for each rating category.[15] One observes that for high-yield bonds with a rating lower than BB and for investment-grade bonds with a rating higher than BBB the model predicts them to be with a high probability in the control and in the treatment group, respectively. Moreover, even for the four rating categories from BB to BBB around the threshold, the model correctly predicts the treatment status of most units. The estimated propensity score is less than 0.5 for 46% of the BB+ bonds and for 79% of the BB bonds. For the lowest investment-grade categories BBB- and BBB, the estimated propensity scores is greater than 0.5 for 98% of the bonds in both of these two categories.[16] Most importantly, Figure 3 shows that all the

[15]The circles represent outliers, i.e., data points that are further than 1.5 times the interquartile range away from the first and the third quartile.

[16]Given that the model correctly predicts the treatment status better for investment-grade than for high-yield bonds, one may be concerned that the vast majority of the units with estimated propensity scores around 0.5 belongs to the control group. This is not, however, the case due to the larger number of treated units around BBB- threshold.

bonds with estimated propensity scores around 0.5 have ratings that are close to the investment grade threshold BBB-, suggesting the probit model is well specified.

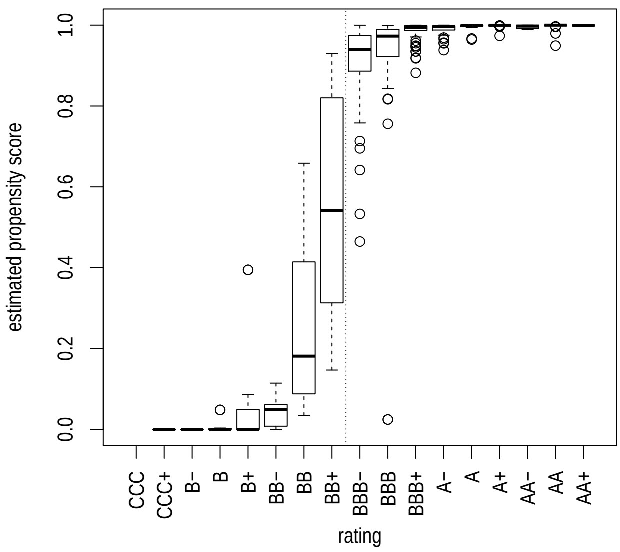


Figure 3: Estimated propensity scores by rating.

Our second objective in the design phase is to identify subsamples in which the distributions of the covariates are balanced between the treatment and control groups. Following the procedure in Section 2.2.3, we construct subsets of units in which the estimated propensity score of each unit falls in the interval $(0.5 - d, 0.5 + d)$, for some $d$. Then, in each subsample and for each pre-treatment variable, we assess covariate balance as measured by the standardized bias (SB):

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

where $s_z^2$ is the sample variance of the unweighted covariate and $N_z$ the sample size in group $z = 0, 1$. When each unit is assigned a weight of unity, the SB is simply the two-sample $t$-statistic. We employ the unweighted standard errors in the denominator to be able to compare the values of the statistic across different sets of weights. We first calculate the SB using the overlap weights. Our goal is to find subsamples in which all the covariates are well balanced, ensuring, at the same time, that the number of units in them is not too small. We identify five such values of $d$, and present the corresponding SBs of the covariates in Panel A of Table 4. All of the absolute values of the SBs are smaller than 1.96 (the critical value of the two sample $t$-statistic at 0.05 level), suggesting overall satisfactory covariate balance between the treatment and control groups. This supports the plausibility of local unconfoundedness in the subsamples under consideration.

Table 4: Standardized bias of the covariates when $0.5 - d < \hat{e}(x_i) < 0.5 + d$.

| $d$ | N | cpn | mat | prof | cf | liq | cov | lev | solv | size | age | ltdebt | call |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| *Panel A. ATO weights* | | | | | | | | | | | | | |
| 0.34 | 27 | −1.31 | 1.19 | 1.06 | 0.97 | 1.51 | 0.52 | −0.28 | 1.26 | −1.89 | −1.76 | −0.34 | −0.43 |
| 0.35 | 28 | −1.43 | 1.38 | 0.75 | 0.81 | 1.45 | 0.37 | −0.34 | 1.40 | −1.74 | −1.39 | −0.45 | −0.40 |
| 0.36 | 32 | −1.59 | 1.65 | 0.83 | 0.62 | 1.40 | 0.52 | −0.69 | 1.68 | −1.66 | −1.17 | −0.80 | −0.38 |
| 0.37 | 33 | −1.76 | 1.74 | 0.85 | 0.38 | 1.18 | 0.45 | −0.17 | 1.34 | −1.72 | −1.31 | −0.23 | −0.36 |
| 0.38 | 36 | −1.29 | 1.84 | 1.03 | 0.72 | 1.54 | 0.40 | 0.12 | 1.75 | −1.89 | −1.07 | 0.01 | −0.40 |
| *Panel B. ATE weights* | | | | | | | | | | | | | |
| 0.34 | 27 | −1.64 | 0.97 | 0.94 | 0.77 | 1.51 | 0.59 | −0.48 | 1.52 | −1.64 | −1.50 | −0.65 | −0.61 |
| 0.35 | 28 | −1.77 | 1.33 | 0.53 | 0.55 | 1.39 | 0.36 | −0.55 | 1.69 | −1.42 | −1.05 | −0.79 | −0.56 |
| 0.36 | 32 | −1.96 | 1.81 | 0.62 | 0.26 | 1.26 | 0.56 | −1.05 | 2.05 | −1.24 | −0.64 | −1.30 | −0.52 |
| 0.37 | 33 | −2.23 | 1.88 | 1.04 | −0.07 | 0.94 | 0.43 | −0.24 | 1.53 | −1.53 | −0.88 | −0.37 | −0.47 |
| 0.38 | 36 | −1.30 | 1.94 | 1.29 | 0.56 | 1.53 | 0.37 | 0.28 | 2.13 | −1.74 | −0.46 | 0.08 | −0.53 |
| *Panel C. ATT weights* | | | | | | | | | | | | | |
| 0.34 | 27 | −1.40 | 1.00 | 1.03 | 0.57 | 1.42 | 0.50 | −0.08 | 1.39 | −1.80 | −1.53 | −0.35 | −0.57 |
| 0.35 | 28 | −1.50 | 1.46 | 0.54 | 0.33 | 1.26 | 0.21 | −0.11 | 1.54 | −1.56 | −1.02 | −0.46 | −0.48 |
| 0.36 | 32 | −1.51 | 2.09 | 0.54 | 0.01 | 1.07 | 0.33 | −0.47 | 1.81 | −1.33 | −0.69 | −0.79 | −0.33 |
| 0.37 | 33 | −1.83 | 2.17 | 1.15 | −0.35 | 0.70 | 0.15 | 0.32 | 1.23 | −1.72 | −1.01 | 0.13 | −0.25 |
| 0.38 | 36 | −0.39 | 2.18 | 1.46 | 0.75 | 1.69 | 0.24 | 0.99 | 2.14 | −1.92 | −0.30 | 0.71 | −0.45 |
| *Panel D. Unitary weights* | | | | | | | | | | | | | |
| 0.34 | 27 | −2.37 | 0.69 | 0.75 | 0.35 | 1.40 | 0.92 | −1.37 | 2.12 | −0.96 | −0.85 | −1.63 | −0.97 |
| 0.35 | 28 | −2.52 | 1.19 | 0.28 | 0.10 | 1.24 | 0.65 | −1.47 | 2.31 | −0.70 | −0.36 | −1.80 | −0.91 |
| 0.36 | 32 | −2.76 | 1.78 | 0.43 | −0.22 | 1.09 | 0.92 | −2.10 | 2.72 | −0.51 | 0.17 | −2.43 | −0.87 |
| 0.37 | 33 | −3.05 | 1.83 | 1.10 | −0.58 | 0.73 | 0.77 | −0.92 | 2.07 | −1.00 | −0.14 | −1.08 | −0.82 |
| 0.38 | 36 | −2.95 | 1.81 | 1.27 | −0.57 | 0.80 | 0.51 | −0.74 | 2.46 | −1.10 | 0.10 | −0.94 | −0.76 |

We further investigate whether covariate balance is sensitive to the specific overlap weighting scheme. Specifically, we calculate the SB for each covariate in each of the identified subsamples when employing instead the weights corresponding to the two alternative estimands of our interest: ATE and ATT. The SBs obtained in this manner can be found in Panels B and C of Table 4. For both the ATE and ATT weighting scheme, the covariates remain balanced in the two subsamples with the fewest units. However, for the subsamples defined by $d > 0.35$, the SBs of two covariates exceed 1.96 in absolute value, signaling that local unconfoundedness is less likely to hold. For this reason, when we estimate the ATE and ATT, we focus on the first two subsamples ($d \in \{0.34, 0.35\}$).

We also assess covariate balance in the five subsamples considered so far when all the units are weighted equally. This allows us to evaluate whether applying the three sets of weights materially improves covariate balance. That is, for each variable, we conduct a $t$-test for the equality of the unweighted means of the variable in the two groups, the results of which are shown in Panel D of Table 4. They indicate significant imbalance in some of the covariates. Specifically, the $t$-statistics for cpn and solv exceed the relevant 5% critical values in all the five subsamples. Taken together, the results in Table 4 suggest that applying any of the three sets of weights to the samples under consideration improves the overall covariate balance, even though not for each individual covariate. The greatest improvement is observed when employing the overlap weights. Note that, unlike in Li et al. (2018), here the balancing tests are conducted in subsamples, while the weights are estimated using the whole sample. Consequently, covariate balance is not an immediate consequence of applying the overlap weights.

Table 5: Standardized bias of the covariates when $\hat{e}_{\min} < \hat{e}(x_i) < \hat{e}_{\max}$.

| $\hat{e}_{\min}$ | $\hat{e}_{\max}$ | N | cpn | mat | prof | cf | liq | cov | lev | solv | size | age | ltdebt | call |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | *Panel A. ATO weights* | | | | | | | |
| 0.10 | 0.88 | 37 | −1.36 | 1.85 | 1.05 | 0.72 | 1.55 | 0.43 | 0.07 | 1.77 | −1.84 | −1.04 | −0.03 | −0.42 |
| 0.09 | 0.88 | 39 | −1.48 | 1.84 | 1.05 | 0.67 | 1.36 | 0.33 | 0.10 | 1.61 | −1.88 | −0.95 | −0.01 | −0.44 |
| 0.08 | 0.88 | 40 | −1.54 | 1.82 | 1.05 | 0.67 | 1.37 | 0.35 | 0.06 | 1.65 | −1.88 | −0.97 | −0.05 | −0.45 |
| 0.07 | 0.88 | 43 | −1.72 | 1.84 | 1.08 | 0.74 | 1.43 | 0.39 | 0.08 | 1.64 | −1.87 | −1.00 | −0.03 | −0.48 |
| 0.06 | 0.88 | 45 | −1.82 | 1.86 | 1.09 | 0.78 | 1.47 | 0.43 | 0.09 | 1.63 | −1.84 | −1.04 | −0.03 | −0.50 |
| 0.05 | 0.88 | 47 | −1.92 | 1.88 | 1.11 | 0.78 | 1.48 | 0.46 | 0.04 | 1.66 | −1.80 | −1.02 | −0.07 | −0.51 |
| | | | | | | | *Panel B. ATE weights* | | | | | | | |
| 0.15 | 0.86 | 30 | −1.72 | 1.87 | 0.48 | 0.25 | 1.21 | 0.46 | −0.83 | 1.86 | −1.14 | −0.76 | −1.03 | −0.46 |
| | | | | | | | *Panel C. ATT weights* | | | | | | | |
| 0.13 | 0.85 | 30 | −1.61 | 1.44 | 0.58 | 0.33 | 1.29 | 0.24 | −0.19 | 1.65 | −1.63 | −0.99 | −0.55 | −0.50 |
| 0.11 | 0.85 | 31 | −1.65 | 1.45 | 0.60 | 0.33 | 1.29 | 0.26 | −0.23 | 1.68 | −1.60 | −0.97 | −0.59 | −0.51 |
| 0.09 | 0.85 | 33 | −1.69 | 1.44 | 0.61 | 0.31 | 1.19 | 0.22 | −0.22 | 1.59 | −1.63 | −0.92 | −0.61 | −0.53 |
| 0.08 | 0.85 | 34 | −1.73 | 1.44 | 0.61 | 0.31 | 1.20 | 0.23 | −0.25 | 1.62 | −1.64 | −0.93 | −0.64 | −0.54 |
| 0.07 | 0.85 | 37 | −1.84 | 1.45 | 0.65 | 0.34 | 1.24 | 0.25 | −0.25 | 1.65 | −1.64 | −0.93 | −0.66 | −0.56 |
| 0.06 | 0.85 | 39 | −1.91 | 1.46 | 0.67 | 0.36 | 1.26 | 0.27 | −0.26 | 1.66 | −1.63 | −0.95 | −0.67 | −0.57 |

Finally, we investigate whether covariate-balanced subsamples with a larger number of units can be found by rendering asymmetric the intervals in which the estimated propensity scores are required to lie. Specifically, for each of the three weighting schemes (ATO, ATE and ATT), we first identify from Table 4 the largest value of $d$ for which all the SBs are smaller than 1.96 in absolute value, indicating satisfactory covariate balance. Then, starting from these symmetric intervals ($d = 0.38$ for ATO and $d = 0.35$ for ATE and ATT), we gradually increase the length of the interval on the right or left of 0.5 until significant imbalance emerges. Table 5 contains the asymmetric intervals identified in this way along with the corresponding SBs. In the case of both the ATO and ATT weighting scheme, we are able to identify subsamples with a significantly larger number of units, allowing us to more precisely estimate the effect of the program, as well as to improve the external validity of our results.

## 5.2 Results

Having identified the subsamples in which local unconfoundedness plausibly holds, we can proceed to estimate the average causal effect of being eligible for purchase under the CSPP for the overlap population, namely the effect for units which could conceivably have been assigned to either the treatment or control group. To estimate the treatment effect, we use the sample estimator $\hat{\tau}$ in (4). We estimate the asymptotic variance of each weighted average treatment effect estimator (ATO, ATT and ATE) by using the empirical or "sandwich" estimator which can be shown to be directly linked to the influence function of an M-estimator (Huber, 1964, 1967; van der Vaart, 1998). The standard errors calculated in this way incorporate the uncertainty arising from both the design and analysis stages.

Table 6: Estimates of the weighted treatment effect. Symmetric intervals.

| $d$ | $N_0$ | $N_1$ | estimate | $p$-val. |
|---|---|---|---|---|
| | | *Panel A. ATO* | | |
| 0.34 | 10 | 17 | −37.1 | 0.116 |
| 0.35 | 11 | 17 | −39.5 | 0.088 |
| 0.36 | 13 | 19 | −42.7 | 0.046 |
| 0.37 | 14 | 19 | −45.9 | 0.029 |
| 0.38 | 16 | 20 | −38.4 | 0.096 |
| | | *Panel B. ATE* | | |
| 0.34 | 10 | 17 | −42.2 | 0.069 |
| 0.35 | 11 | 17 | −45.3 | 0.044 |
| | | *Panel C. ATT* | | |
| 0.34 | 10 | 17 | −36.4 | 0.186 |
| 0.35 | 11 | 17 | −39.9 | 0.128 |

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

Table 6 contains the estimated effects in the five covariate-balanced subsamples identified above. The estimates of the ATO suggest that eligibility for purchase under the CSPP had a statistically significant and negative, albeit moderate, effect on bond spreads (a reduction in the range of 35–50 basis points). This is slightly lower than the 60 basis point reduction in the primary market found by Zaghini (2019). However, the difference could simply reflect the more "local" nature of our estimates compared to those in Zaghini (2019), which are based on all the bonds issued in the primary market. Relative to the announcement effect of the program in the secondary bond market, of 15 basis points according to Abidi and Flores (2017), our estimates are slightly higher. This difference could reflect the higher liquidity of the bonds which are actively traded in the secondary market.

Given the weighted average maturity of 7.5 years in the subsample defined by $d = 0.38$, 35–50 basis point reduction in yield to maturity corresponds approximately to a 2.6–3.8 per cent increase in the price of a zero-coupon bond at issuance. Relative to the weighted average amount sold of 620 million euros in the subsample under consideration ($d = 0.38$), this represents a significant decrease in the funding costs of the issuers of the eligible bonds.

As the effect of the program on bond spreads at issuance could have been due to higher expected liquidity of the eligible bonds, it is instructive to compare the effect that we have estimated to liquidity premia of corporate bonds. Dick-Nielsen et al. (2012) estimate the liquidity premia of BBB US corporate bonds to lie in the range of 4–93 basis points. Also relative to these additional yields required by investors to compensate for the illiquidity of corporate bonds, our estimates of the effect of the program are sizable.

Let us next examine how the estimates of the treatment effect vary when changing the target population. Namely, we calculate the estimates of the ATE and ATT. On the one hand, ATE refers to the effect of the program for all units irrespective of their treatment status, without downweighting units further away from the threshold. On the other hand, ATT refers to the effect for the units effectively treated. The estimates can be found in Table 6, along with those of the ATO. For both the ATE and ATT, we consider only the first two subsamples, defined by $d \in \{0.34, 0.35\}$, given that that covariate imbalance emerges for larger values of $d$. The results suggest that the ATE is slightly larger than the ATT. In other words, the effect of the program on investment-grade bonds was slightly higher than the effect on high-yield bonds that would have been observed had they also been treated.

The estimates of the ATE, ATE and ATT in Table 6 are based on subsamples which are rather limited in size. For this reason, we estimate the effect of the program also when employing the subsamples defined by asymmetric intervals, identified in Section 5.1. The estimates are presented in Table 7. The magnitude of the estimates change little when considering these subsamples with a larger number of units. The estimates of the ATO appear to settle

Table 7: Estimates of the weighted treatment effect. Asymmetric intervals.

| $\hat{e}_{\min}$ | $\hat{e}_{\max}$ | $N_0$ | $N_1$ | estimate | $p$-val. |
|---|---|---|---|---|---|
| | | *Panel A. ATO* | | | |
| 0.10 | 0.88 | 21 | 16 | −39.6 | 0.082 |
| 0.09 | 0.88 | 23 | 16 | −41.3 | 0.064 |
| 0.08 | 0.88 | 24 | 16 | −42.7 | 0.054 |
| 0.07 | 0.88 | 27 | 16 | −44.8 | 0.037 |
| 0.06 | 0.88 | 29 | 16 | −45.0 | 0.033 |
| 0.05 | 0.88 | 31 | 16 | −45.5 | 0.029 |
| | | *Panel B. ATE* | | | |
| 0.15 | 0.86 | 17 | 13 | −48.0 | 0.023 |
| | | *Panel C. ATT* | | | |
| 0.13 | 0.85 | 19 | 11 | −40.6 | 0.114 |
| 0.11 | 0.85 | 20 | 11 | −41.2 | 0.106 |
| 0.09 | 0.85 | 22 | 11 | −42.2 | 0.094 |
| 0.08 | 0.85 | 23 | 11 | −43.0 | 0.087 |
| 0.07 | 0.85 | 26 | 11 | −44.2 | 0.074 |
| 0.06 | 0.85 | 28 | 11 | −44.3 | 0.071 |

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

around −45 basis points and the positive difference between the ATE and ATT decreases slightly. Not surprisingly, the standard errors of the estimates decrease as the sample sizes grow.

# 6 Discussion

## 6.1 Alternative approaches

Angrist and Rokkanen (2015) propose to identify causal effects away from the threshold by relying on a conditional independence assumption. In particular, they take advantage of the availability of a set of predictors of the dependent variable which does not contain the running variable. Conditional on this set of predictors, potential outcomes are assumed to be mean-independent of the running variable. Given the similarity between this and our local unconfoundedness assumption, it is instructive to compare results obtained using the two approaches.

To this end, we estimate the effect of being eligible for purchase under the CSPP employing the framework of Angrist and Rokkanen (2015). Given that their conditional independence assumption, $\mathbb{E}[Y_i(z)|R_i, \mathbf{X}_i] = \mathbb{E}[Y_i(z)|\mathbf{X}_i]$, $z = 0, 1$, is unlikely to be satisfied in our application, we invoke the alternative assumption introduced in Angrist and Rokkanen (2012), the bounded conditional independence assumption (BCIA). The BCIA requires that there exists $d > 0$ such that $\mathbb{E}[Y_i(z)|R_i, \mathbf{X}_i, |R_i - r_t| < d] = \mathbb{E}[Y_i(z)|\mathbf{X}_i, |R_i - r_t| < d]$. That is, conditional mean-independence is assumed to hold in a $d$-neighborhood of the threshold.

Given that in our case the running variable is categorical, the measure of distance in the definition of the BCIA is not directly applicable. However, we take advantage of the ordered nature of the running variable and identify the set of units around the threshold as those with a rating BB+ (the highest rating category in the control group) or BBB- (the lowest category in the treatment group). We then assume that conditional independence holds in this subset of units around the threshold.

Angrist and Rokkanen (2012) propose to assess the BCIA by testing the statistical significance of coefficients in regressions of the outcome on the running variable and the pre-treatment variables on either side of the threshold. This procedure is however not applicable to our selected subsample because we are only considering one rating category on each side of the threshold. Therefore, we cannot assess the validity of the conditional independence assumption in our subsample.

We obtain the estimates of the average treatment effect on the treated (ATT) presented in Table 8. The estimates are in line with those obtained using our framework even though the two approaches rely on rather different assumptions.

Table 8: Alternative estimates of the treatment effect.

| $N_0$ | $N_1$ | $ATT_1$ | $ATT_2$ |
|---|---|---|---|
| 26 | 43 | $-36.7$ (0.367) | $-51.8$ (0.205) |

NOTE: The numbers in parentheses are $p$-values, computed using a nonparametric bootstrap with 500 replications. $ATT_1$ refers to the linear reweighting estimator and $ATT_2$ to inverse propensity score weighting. $N_z$ is the group-$z$ sample size.

Angrist and Rokkanen (2012) propose to identify the subset of units to analyze based on the value of the running variable. In the context of our application, this can mean basing inference on a subsample in which the distributions of the covariates are imbalanced between the treated and the control groups. In the subsample used to obtain the above estimates, this would indeed appear to be the case (see Table 9).

Table 9: Balancing tests for the covariates in the subsample defined by rating.

| N | cpn | mat | prof | cf | liq | cov | lev | solv | size | age | ltdebt | call |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 69 | −4.10 | 1.47 | 3.57 | 1.78 | 2.76 | 1.02 | 1.18 | 3.10 | −1.64 | −1.16 | 1.31 | −0.44 |

NOTE: The numbers of in the columns for the covariates are standardized biases.
The subsample contains all bonds whose rating is either BB+ or BBB-.

A strength of our approach is that we define the candidate subsamples based on richer covariate information, encoded in the estimated propensity scores obtained from the ordered probit model. Our results suggest that this can help identify subsets of units in which the distributions of the covariates are more balanced between the treated and the control groups. Covariate balance lends powerful support to the validity of local unconfoundedness, being a stronger consequence of this assumption than that of regression independence assessed by Angrist and Rokkanen (2012).

### 6.2 Methodological issues

The analysis above suggests that our approach may give rise to a trade-off between variance and bias. Namely, when the model for the ordered categorical variable provides a good in-sample fit, the estimated propensity scores of most units are close to either 0 or 1. Consequently, covariate-balanced subsamples, identified using the estimated propensity scores, are likely to have moderate sample sizes. This may lead to elevated standard errors of the estimates of the treatment effect.

Finally, given that both the identification of the target population and the weighting scheme rely on the estimated model for the categorical running variable, sensitivity to the specification of the model could be analyzed. To do so, one might follow the approach adopted in Schwartz et al. (2012) and Mercatanti and Li (2017).

## 7 Conclusions

In this paper we have developed a regression discontinuity (RD) design applicable when the running variable, determining assignment to treatment, is categorical. The estimation strategy is based on the following steps. We first estimate an ordered probit model for the categorical running variable conditional on pre-treatment variables. The estimated probability of being assigned to treatment is then adopted as a continuous surrogate running variable. In order to provide external validity to the analysis, we move away from the standard inference *at* the threshold by assuming local unconfoundedness of the treatment in an interval *around* the surrogate threshold. Then, once this interval has been identified via an overlap-weighted balancing assessment of the pre-program variables across treatments, an estimate of the effect of the program in the interval is obtained employing a weighted estimator of the average treatment effect.

We have applied our methodology to estimate the causal effect of the corporate sector purchase programme on corporate bond spreads. Under the CSPP, the Eurosystem can purchase investment-grade corporate bonds issued by non-bank corporations. This eligibility criterion enables evaluating the program via a sharp RD design. Specifically, eligibility can be considered as a treatment, while the bond rating is a categorical ordered running variable determining assignment to treatment. We have estimated the effect of the program in a subpopulation defined by the estimated conditional probability to be eligible for purchase. This is composed of bonds that can be assigned with non-negligible probability to either eligibility status, and therefore are the most affected by, even small, change in the program.

Our results suggest that eligibility for purchase under the CSPP had a negative effect, in the order of 35–50 basis points, on bond spreads at issuance. This is somewhat higher than previous estimates of the announcement effect of the program on bonds traded in the secondary market (Abidi and Flores, 2017). Given that in the sample which is used to conduct inference the average amount issued exceeded 600 million euros, the 35–50 basis point reduction in the yield to maturity corresponds to a non-negligible decrease in the funding costs of the eligible issuers.

Several methodological extensions of our method can be explored in the future. In specifying the ordered probit model for the categorical running variable, we might use alternative, more objective, procedures to guide the choice of predictors to include, such as k-fold cross validation. It would also be useful to relax – at least partially – the local SUTVA assumption, which rules out interference between units as well as any "externality effects". At the same time, the empirical application could be extended in a a number of ways. First, it would be interesting to assess whether our results are sensitive to an extension (after September 2017) of the sample period. Second, so far we have focused on the effect of the CSPP on bond spreads. Thus, a further topic to be investigated relates to the causal effect on quantities (i.e., amounts issued), bond characteristics and their liquidity (as measured by standard indicators such as the bid-ask spread). We leave all these extensions for future research.

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