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Causal analysis of central bank holdings of corporate bonds under interference☆

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ABSTRACT

We investigate whether the transfer of corporate bonds from the private sector to the balance sheet of the central bank permanently alters their relative prices. Answering this question complements the literature on central bank asset purchase programs, documenting significant relative price changes over shorter horizons. We use data on bonds issued over the duration of the European Central Bank's corporate bond purchase program and a novel regression discontinuity design to quantify the causal effect of interest. The estimates indicate that the program did not, on average, permanently alter the yield spreads of eligible bonds relative to those of similar noneligible bonds. This finding suggests that central bank holdings of even relatively illiquid private sector securities can have no distortionary effects on the relative prices of such assets.

1. Introduction

In several advanced economies, central bank policy rates have remained close to their effective lower bounds since the global financial crisis, substantially constraining the conduct of monetary policy (Bank for International Settlements, 2019). At the same time, monetary authorities have been confronted with the challenge of persistently low inflation, and more recently the need to respond to the pandemic crisis. These developments have prompted central banks to make greater use of asset purchase programs. Asset purchases have been conducted both to address financial market disruptions and to attain a target inflation rate (Committee on the Global Financial System, 2019; Aßhoff et al., 2021).

The effects of central bank asset purchases, despite having been eagerly studied, remain imperfectly understood.[1] Based on surveys of central bank governors and academics, Blinder et al. (2017) find that there is skepticism 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. Moreover, central bank asset purchases have the potential to distort relative security prices, increasing the risk of sharp asset-price corrections (Dell'Ariccia et al., 2018). Yet, central banks continue to pursue their objectives by making use of such policies. 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 (ECB). 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 the private sector to the balance sheets of the Eurosystem. Given that the corporate bond market is relatively

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[1] There is burgeoning literature studying the effects of unconventional monetary policies such as central bank asset purchases (for a review, see Kuttner, 2018; Dell'Ariccia et al., 2018 and Bhattarai and Neely, 2020).

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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.

The effects of a market intervention such as the CSPP, especially over longer periods of time, are likely not to be restricted to the units targeted by the policy.[2] This possibility poses a serious challenge to the identification of the causal effects, in the spirit of Angrist et al. (2018), of such interventions. We illustrate this challenge by considering a framework of causal inference in which not only the treated but also the nontreated units are affected by the intervention of interest. The framework differs from those commonly employed in the literature on causal inference under interference (see, e.g., Sobel, 2006; Hong and Raudenbush, 2006 and Huber and Steinmayr, 2021) in that no group or network structure is imposed on the effects of the intervention. In this framework, it is impossible, under standard identifying assumptions, to separately identify the treatment and the spillover effect of the intervention as the latter is felt by both the treated and the nontreated. For this reason, we instead focus on identifying and estimating economically meaningful differential effects. We show that such effects can be identified under a set of assumptions which restricts in no way the conditional distributions of the potential outcomes under the program.

We use the framework to quantify the causal effect of the CSPP on the yield spreads at issuance of the bonds eligible for purchase relative to those of similar noneligible bonds. Estimating this effect over the duration of the program can shed light on whether relative asset prices are permanently altered when the central bank becomes a large holder of certain types of securities. 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 adopt a regression discontinuity design specifically developed for applications in which the treatment-determining variable is ordered categorical (Li et al., 2021), 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 differential effect of interest locally, around the eligibility threshold.

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 similar 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 was not significant 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.

More generally, our results support the view that the central bank can purchase large amounts of assets from the private sector without permanently distorting their relative prices.[3] The absence of such a distortion can be seen as a prerequisite for asset purchases to have effects similar to those of a monetary policy rate cut exerting, through arbitrage, downward pressure also on longer-term interest rates (Reis, 2013). At the same time, our findings are congruous with the sticky price view of the monetary transmission mechanism, according to which monetary policy innovations do not have permanent effects on relative prices.[4]

In the next section, we discuss central bank asset purchase programs 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 (Committee on the Global Financial System, 2019). 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 of the European Central Bank 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 (for further details, see De Santis, 2020). The aim of the CSPP was twofold. First, together with the other asset purchases,

[2] The presence of general equilibrium effects through which also the prices of noneligible bonds are affected is rather a prerequisite for asset purchase programs to stimulate economic activity (Bauer, 2012).

[3] The finding of no alteration of relative asset prices accords with the principle of "market neutrality" which guides the asset purchase programs of the ECB (Pelizzon et al., 2018). To the best of our knowledge, there are no analogous results for government bonds in the literature on unconventional monetary policies. However, the horizon over which changes in their relative prices are examined tends to be short (see D'Amico and King, 2013 for a typical setting).

[4] This implication of stick price models is supported by Boivin et al. (2009), providing evidence that monetary policy innovations do not have permanent effects on relative sectoral prices.

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.[5]

Under the CSPP, corporate bonds denominated in euro issued by euro-area non-bank corporations 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 than purchases of government bonds.[6]

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. Ertan et al. (2018), Grosse-Rueschkamp et al. (2019), Betz and De Santis (2019) and Arce et al. (2021) 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. Abidi et al. (2019) examine how bond ratings changed over the course of the program. Todorov (2020) analyzes how the CSPP affected yields, liquidity and issuance in the European corporate bond market. Bartocci et al. (2021) evaluates the macroeconomic effects of the program. Boneva et al. (2021) study how the CSPP affected liquidity in the German corporate bond market. De Santis and Zaghini (2021) estimate the effects of the program on bond issuance.

Differently from these works and from our preliminary evaluation of the CSPP (Li et al., 2021), we quantify the effect of the program on bond yields over its whole duration. Given the relatively long sample period, we explicitly take into account the possibility that the program affected the prices of both eligible and noneligible bonds. This feature of our empirical framework and its implications for the identification of causal effects are the main dimensions along which our study differs from the previous literature. The framework we employ 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. In this way we are able to shed light on the important, yet so far unanswered, policy question of whether asset purchase programs permanently distort the relative prices of the acquired assets.

[5] 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.

[6] 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 (Committee on the Global Financial System, 2019). However, this difference could have been offset by the larger share of buy-to-hold investors.

3. Empirical strategy

The empirical approach exploits the feature of the program that the highest rating, known as the first-best 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.[7] 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 build on the RD approach developed in our previous work (Li et al., 2021), specifically constructed for settings in which the variable determining assignment to treatment, i.e. the running variable, is ordered categorical.

The novelty of our approach lies in the relaxation of the assumption of no interference between units. Our setup allows for the possibility that the program affected the yield spreads of both the eligible and the noneligible bonds. More specifically, we model the potential outcomes of a unit as functions of its exposure to the program, defined in terms of its treatment and of the share of units that the program assigns on average to treatment. We then introduce a set of assumptions under which a meaningful differential effect of the program can be identified.

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 discontinuity 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).

3.1. General framework and notation

We formalize the RD design in terms of a potential outcomes framework (Imbens and Rubin, 2015), explicitly allowing for interference between units. Consider a sample of $i = 1, \ldots, N$ bonds drawn from a super-population Ω. Let $\boldsymbol{Z}$ denote the N-vector of treatment assignments whose element Z_i is 1 if bond i is assigned to treatment and 0 if the bond is assigned to control. The assignment to either condition depends on an observable ordinal pre-treatment variable R_i, the first-best rating. The policy rule is such that $Z_i = \mathbf{1}(R_i \geq r_c)$, where $\mathbf{1}(\cdot)$ is the indicator function and r_c represents the eligibility threshold (BBB-). For each bond, besides the running variable, a set of pre-treatment covariates $\boldsymbol{X}_i$ is also available. The propensity score, i.e. the probability of being assigned to the treatment condition conditional on the covariates $Pr(Z_i = 1|\boldsymbol{X}_i = \boldsymbol{x}_i)$, is denoted by $e(\boldsymbol{x}_i)$.

The potential outcomes for bond i, i.e. its yield spreads, are functions of the vector of treatment assignments and are denoted by $Y_i(\boldsymbol{Z})$.[8] Without interference between units we would have $Y_i(\boldsymbol{Z}) = Y_i(Z_i)$. We relax the assumption of no interference between units by allowing that, for two vectors of treatment assignment $\boldsymbol{Z}'$ and $\boldsymbol{Z}''$ and for a generic element i, $Y_i(\boldsymbol{Z}') \neq Y_i(\boldsymbol{Z}'')$ even when $Z_i' = Z_i''$. This possibility leads to a considerable increase in the cardinality of the set of potential outcomes because, at the limit, a change in the treatment assignment of only one unit can change the potential outcome of any other unit. With the aim of capturing the kind of interference present in our

[7] For a historical overview of RD designs, see Cook (2008), and for recent surveys Imbens and Lemieux (2008); Cattaneo et al. (2019).

[8] This notation presupposes that the potential outcomes depend only on the vector of treatment assignments and not also on the realized values of the running variable. This assumption is formally stated in Section 3.2.

application and to remain at the same time within the super-population framework for inference,[9] we define potential outcomes using the following notation. Consider a generic program j and let $e_j(\boldsymbol{x}_i)$ denote the probability of being assigned to treatment conditional on the covariates under the program. The potential outcomes for unit i under program j are functions of Z_i and of the share of units that the program assigns on average (over a hypothetical large number of repetitions) to treatment: $Y_i(Z_i, \psi_j)$, where $\psi_j = \mathbb{E}_j[Z_i] = \int \mathbb{E}_j(Z_i|\boldsymbol{X}_i)\mathrm{d}F(\boldsymbol{x}_i) = \int e_j(\boldsymbol{x}_i)\mathrm{d}F(\boldsymbol{x}_i)$. This notation implies that, over a set of assignment models $\{e_j(\boldsymbol{x}_i)\}_j$, the same treatment level Z_i will lead to different individual potential outcomes depending on the share of units assigned to treatment by each assignment model.[10] We denote the share of units assigned to the treatment condition under the actual policy by $\psi = \int e(\boldsymbol{x}_i)\mathrm{d}F(\boldsymbol{x}_i)$.

Meaningful comparisons of potential outcomes (causal effects) are the differences between potential outcomes under the actual assignment mechanism and under a hypothetical one which assigns units to treatment with probability zero, i.e. $\psi_j = 0$. The latter is equivalent to a benchmark policy under which no bond is eligible for purchase. Our individual causal effects of interest are: $\tau_i^1 = Y_i(1, \psi) - Y_i(0, 0)$ and $\tau_i^0 = Y_i(0, \psi) - Y_i(0, 0)$, which we call the treatment effect and the spillover effect of the program, respectively.[11] Note that we do not impose any functional form restrictions on how the treatment and the spillover effect depend on ψ. As a result, our estimates are not directly informative about the effects of similar policies of different size. Given that our interest lies in a retrospective evaluation of the CSPP, we prefer not to introduce additional assumptions that would allow such extrapolations.

The observed outcome Y_i is $Y_i(Z_i, \psi)$ for each i, implying that $Y_i(0, 0)$ is not observed for any unit. Thus, we cannot separately identify the population averages of the two unit-level effects τ_i^1 and τ_i^0. For this reason, we examine alternative estimands defined in terms of $\tau^1(\boldsymbol{x}, z) := \mathbb{E}[\tau_i^1|\boldsymbol{X}_i = \boldsymbol{x}, Z_i = z]$ and $\tau^0(\boldsymbol{x}, z) := \mathbb{E}[\tau_i^0|\boldsymbol{X}_i = \boldsymbol{x}, Z_i = z]$. Meaningful causal effects are obtained by averaging $\tau^1(\boldsymbol{x}, z)$ and $\tau^0(\boldsymbol{x}, z)$ over target distributions of $\boldsymbol{x}$. Consider, for instance, the following differential effect (DE):

$$\begin{aligned} \Delta^{DE} &:= \int \left[\tau^1(\boldsymbol{x}_i, 1) - \tau^0(\boldsymbol{x}_i, 0)\right] \mathrm{d}F(\boldsymbol{x}_i|Z_i = 1) \\ &= \mathbb{E}[\tau_i^1|Z_i = 1] - \int \tau^0(\boldsymbol{x}_i, 0)\mathrm{d}F(\boldsymbol{x}_i|Z_i = 1) \end{aligned} \tag{1}$$

Δ^{DE} represents the treatment effect on the treated relative to the spillover effect on similar controls. We show that this causal effect can be identified under the assumptions presented next.

3.2. Identifying assumptions

We take the local randomization perspective for identification (Lee and Card, 2008; Cattaneo et al., 2015), which is arguably more flexible in addressing complex situations such as ordinal running variables. The assumptions we rely on are similar to those invoked in Li et al. (2015), but modified to our framework, in which units can interfere with each other. All the assumptions are local in nature, referring to a subpopulation of units around the eligibility threshold. First, following Imbens (2004), we require that the units in the subpopulation could be assigned to the control condition with a non-zero probability conditional on the covariates (weak overlap).

Assumption 1. There exists a subpopulation Ω_o of the entire population Ω such that, for each i in Ω_o, $e(\boldsymbol{X}_i) < 1$.

Within the subpopulations satisfying Assumption 1, a stable unit treatment value assumption (SUTVA; Rubin, 1980) is often made. In our framework, SUTVA would imply: (i) the independence of the potential outcomes of the running variable given the treatment status of the unit, and (ii) the absence of interference between units. However, in order to accommodate potential spillover effects in our application, we relax SUTVA in a specific but plausible way. Specifically, we drop condition (ii), which would be equivalent to assuming that the program does not affect the units in the control group. We continue to impose the exclusion restriction (i), which is necessary to avoid defining potential outcomes as functions of the running variable.

Assumption 2. For each i in Ω_o and $\psi_j \in \{0, \psi\}$, 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 $r_i' < r_c$ and $r_i'' < r_c$, or $r_i' \geq r_c$ and $r_i'' \geq r_c$, then $Y_i(z_i', \psi_j) = Y_i(z_i'', \psi_j)$.

Assumption 2 implies that the strength of the spillover effect is only a function of the share of the treated units ψ_j, which can be viewed as a measure of the size of the program. This specific relaxation of SUTVA is justified by the fact that our analysis concerns an anonymous market setting, in which the spillover effect materializes through market prices. This feature distinguishes our setting from those with social interactions, such as educational interventions. In such settings, it is natural to adopt a finite sample perspective to causal inference, and to keep track of the identities of the units. The reason is that the outcomes of all units may depend on which units receive the treatment, rather than only on the share of units receiving the treatment as we assume.

The spillover effect captures how the program affected the units in the control group. It is plausible to presume that the program could have had a general equilibrium effect of this kind as the yield spreads of both the eligible and the noneligible bonds might have had to adjust to eliminate arbitrage opportunities. The spreads of the noneligible bonds could have been affected, for instance, through an improvement in the liquidity of the corporate bond market brought about by the central bank purchases.[12]

Identification of meaningful causal effects also requires assuming that the assignment mechanism is unconfounded, i.e. independent of potential outcomes conditional on the covariates. In our framework, such an assumption can take many forms as the potential outcomes for unit i depend not only on Z_i but also on ψ_j. We adopt the following, relatively weak unconfoundedness assumptions .[13]

Assumption 3. For each i in Ω_o, the treatment assignment satisfies: $\Pr(Z_i|Y_i(0, 0), \boldsymbol{X}_i) = \Pr(Z_i|\boldsymbol{X}_i)$.

In the absence of interference, the average treatment effect on the treated can be identified with the help of the assumption of unconfoundedness for controls: $\Pr(Z_i|Y_i(0), \boldsymbol{X}_i) = \Pr(Z_i|\boldsymbol{X}_i)$, where $Y_i(0)$ denotes the potential outcome under the control condition

[9] The literature on causal inference with interference, since the seminal papers by Sobel (2006) and Hudgens and Halloran (2008), has mainly focused on finite-sample inference for randomized experiments. There is a line of research on super-population inference under interference but it usually considers group-level interventions or a population of units connected through networks (e.g., Hong and Raudenbush, 2006; Tchetgen Tchetgen and VanderWeele, 2012; Papadogeorgou et al., 2019; Forastiere et al., 2021).

[10] Under the same treatment level Z_i, two different assignment mechanisms $e_k(\boldsymbol{x}_i)$ and $e_l(\boldsymbol{x}_i)$ will lead to the same potential outcome if $\mathbb{E}_k[Z_i] = \mathbb{E}_l[Z_i]$. Thus, the couple (Z_i, ψ_j) can be thought as the domain of an *exposure mapping* (Aronow and Samii, 2017) in our super-population framework.

[11] Given that under the benchmark policy ($\psi_j = 0$) the probability of being assigned to treatment is zero, it is natural to consider $Y_i(0, 0)$ as the baseline outcome for each i. That said, the potential outcome $Y_i(1, 0)$, which can be thought of as the limit $Y_i(1, \psi_j)$ when $\psi_j \to 0$, can also be of interest.

[12] Zaghini (2019) provides evidence of the CSPP having influenced also the spreads of the noneligible bonds.

[13] The unconfoundedness assumption 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.

(Heckman et al., 1997; Imbens, 2004). Assumption 3 can be viewed as an analogue of unconfoundedness for controls in our setting. It requires that the distribution of $Y_i(0,0)$ given $Z_i = 0$ and X_i is equal to the distribution of $Y_i(0,0)$ given $Z_i = 1$ and X_i.[14] Note that Assumption 3 concerns the distribution of a variable that is never observed. Given that $\psi_j = 0$ represents a hypothetical program under which the share of units assigned to treatment is equal to zero, $Y_i(0,0)$ is unobservable for each unit. Unconfoundedness for controls, on the other hand, restricts the conditional distributions of a variable that is observed for the controls.

Alternatively, we could require either that $\Pr(Z_i|Y_i(0,\psi),\boldsymbol{X}_i) = \Pr(Z_i|\boldsymbol{X}_i)$ or that $\Pr(Z_i|Y_i(0,\psi),Y_i(1,\psi),\boldsymbol{X}_i) = \Pr(Z_i|\boldsymbol{X}_i)$ as these assumptions concern the distributions of variables that are observed for either the treated or the controls. However, we deem Assumption 3 to be more plausible in our application on the basis of the following considerations. In the program that we evaluate, assignment to treatment depends on the models used by rating agencies to assign bond ratings since $\Pr(Z_i|\boldsymbol{X}_i) = \Pr(R_i \geq r_c|\boldsymbol{X}_i)$. Given that the vector of covariates $\boldsymbol{X}_i$ contains the variables that are known to be the key inputs to these models, we regard unconfoundedness of Z_i as plausible. Yet, if the program affected the behavior of rating agencies, as suggested by Abidi et al. (2019), ratings may have not been independent of *all* the potential outcomes conditional on $\boldsymbol{X}_i$. Specifically, suppose that the rating agencies were more likely to assign an investment-grade rating to bonds whose spreads were particularly responsive to becoming eligible for purchase, i.e. to units with $Y_i(1,\psi) - Y_i(0,\psi)$ large in absolute value. Then, it is likely that $\Pr(R_i \geq r_c|Y_i(0,\psi),Y_i(1,\psi),\boldsymbol{X}_i) \neq \Pr(R_i \geq r_c|\boldsymbol{X}_i)$. At the same time, Assumption 3, i.e. $\Pr(R_i \geq r_c|Y_i(0,0),\boldsymbol{X}_i) = \Pr(R_i \geq r_c|\boldsymbol{X}_i)$, could well be satisfied as the gain from becoming eligible $Y_i(1,\psi) - Y_i(0,\psi)$ does not directly depend on $Y_i(0,0)$.

The unconfoundedness assumption (Assumption 3) is strong and generally untestable. But it is arguably more plausible in a small neighorhood around the threshold than in the whole sample in the RD setting. Indeed, the same assumption was adopted by Angrist and Rokkanen (2015). Moreover, pre-program data can provide indirect evidence on its plausibility. For definiteness, suppose that the assumption $Z_{i,t} \perp\!\!\!\perp Y_{i,t}(0,0)|\boldsymbol{X}_{i,t}$ implies that $Z_{i,s} \perp\!\!\!\perp Y_{i,s}(0,0)|\boldsymbol{X}_{i,s}$, where t denotes the program period and s a pre-program period. Then, given that $Y_{i,s}(0,0)$ is observed for both eligible and noneligible bonds in any pre-program period, the validity of $Z_{i,s} \perp\!\!\!\perp Y_{i,s}(0,0)|\boldsymbol{X}_{i,s}$ can be assessed. Such indirect assessment of the alternative unconfoundedness assumption $\Pr(Z_i|Y_i(0,\psi),\boldsymbol{X}_i) = \Pr(Z_i|\boldsymbol{X}_i)$ is not possible since $Y_{i,s}(0,\psi)$ is unobserved for each unit in any pre-program period.

3.3. Estimators and their properties

As we are interested in the effect of the program on the eligible bonds which was not felt by the noneligible bonds, we consider two estimators developed for estimating the average treatment effect on the treated (ATT). When weak overlap, SUTVA and unconfoundedness hold, both are valid estimators for the ATT (Hirano and Imbens, 2001; Hirano et al., 2003; Mercatanti and Li, 2014).

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

$$\widehat{\Delta}^{ATT} = \frac{\sum_{i=1}^{n} Y_i Z_i}{\sum_{i=1}^{n} Z_i} - \frac{\sum_{i=1}^{n} Y_i(1-Z_i)\frac{\widehat{e}(\boldsymbol{X}_i)}{1-\widehat{e}(\boldsymbol{X}_i)}}{\sum_{i=1}^{n}(1-Z_i)\frac{\widehat{e}(\boldsymbol{X}_i)}{1-\widehat{e}(\boldsymbol{X}_i)}}, \tag{2}$$

where $\widehat{e}(\boldsymbol{X}_i)$ is the estimated propensity score and $i = 1,2,\ldots,n$ is the subsample of interest. A limitation of the weighting estimator in (2) is that it can be biased when the propensity score model is misspecified. For this reason, we also consider the augmented weighting estimator for the ATT, introduced by Mercatanti and Li (2014):

$$\widehat{\Delta}^{ATT}_{DR} = \frac{\sum_{i=1}^{n} Y_i Z_i}{\sum_{i=1}^{n} Z_i} - \frac{\sum_{i=1}^{n} \frac{Y_i(1-Z_i)\widehat{e}(\boldsymbol{X}_i)+\widehat{\mu}_0(\boldsymbol{X}_i)(Z_i-\widehat{e}(\boldsymbol{X}_i))}{1-\widehat{e}(\boldsymbol{X}_i)}}{\sum_{i=1}^{n} Z_i}, \tag{3}$$

where $\widehat{\mu}_0(\boldsymbol{X}_i)$ represents a regression model for $\mathbb{E}[Y_i|\boldsymbol{X}_i, Z_i = 0]$ under the actual policy. Mercatanti and Li (2014) prove that under SUTVA the estimator in (3) 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., 2021) has highlighted that model augmentation provides additional robustness to covariate imbalance. Thus, augmented estimators can be applied in principle even when covariates are moderately unbalanced between the treated and control units.

In our framework, it is not possible to identify and estimate the average treatment effect on the treated, $\mathbb{E}[\tau_i^1|Z_i = 1]$, as $Y_i(0,0)$ cannot be observed for any unit. Still, the estimators in (2) and (3) can potentially be used to estimate meaningful differential effects. Verifying this conjecture requires characterizing the asymptotic properties of the two estimators. Making use of the assumptions stated above, we obtain the following result.[15]

Proposition 1. *Under Assumptions* 1, 2 *and* 3, *the estimators in* (2) *and* (3) *satisfy:*

$$\widehat{\Delta}^{ATT} \to \Delta^{DE}, \quad \widehat{\Delta}^{ATT}_{DR} \to \Delta^{DE}, \tag{4}$$

The two estimators are consistent for the effect of the program on the treated relative to that on similar controls. More specifically, the latter refers to the effect of the program on controls which are similar in terms of the distribution of their covariates to the treated. On a more general level, Proposition 1 demonstrates that it is possible to identify causal effects of interest without requiring the potential outcomes under the program to be independent of the treatment assignment given the covariates. If we instead invoked the alternative unconfoundedness assumption $\Pr(Z_i|Y_i(0,\psi),\boldsymbol{X}_i) = \Pr(Z_i|\boldsymbol{X}_i)$, a somewhat different effect would be identified (see Appendix A.2).

Importantly, the augmented weighting estimator remains consistent for the differential effect identified in Proposition 1 even when either the propensity score model or the potential outcome model is misspecified.

Corollary 1. *The augmented weighting estimator in* (3) *is consistent for* Δ^{DE} *in Proposition* 1 *also when either of the following two sets of conditions holds:*

$$\widehat{e}(\boldsymbol{X}_i) \nrightarrow e(\boldsymbol{X}_i) \quad \text{and} \quad \widehat{\mu}_0(\boldsymbol{X}_i) \to \mathbb{E}[Y_i|\boldsymbol{X}_i, Z_i = 0] \tag{5}$$

$$\widehat{e}(\boldsymbol{X}_i) \to e(\boldsymbol{X}_i) \quad \text{and} \quad \widehat{\mu}_0(\boldsymbol{X}_i) \nrightarrow \mathbb{E}[Y_i|\boldsymbol{X}_i, Z_i = 0] \tag{6}$$

Conducting inference about the causal effects 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

[14] Assumption 3 is similar to the conditional independence assumption (CIA) in Angrist and Rokkanen (2015).

[15] Proofs of the results stated in the main text are reported in Appendix A.1.

estimator in (2) can be derived following the steps in Li et al. (2019),[16] while one for the augmented weighting estimator in (3) can be found in Li et al. (2021).

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 relative to those of the noneligible in the primary market. Focusing on the effect on these bonds is motivated by the following considerations. We seek to assess the differential effect of the CSPP that prevailed during the whole duration of the program. 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 has been found to be only temporary, as well as being small in magnitude (D'Amico and King, 2013).

Quantifying the effect of our interest using primary market prices has two important advantages.[17] First, the yields examined are based on prices in a market with significant trading activity, unlike many of those observed in the secondary market. The reason is that most corporate bonds are actively traded only in the days following their issuance (Chen et al., 2007; Dick-Nielsen et al., 2012).[18] Consequently, the yield spreads we use, which are calculated from the first available prices after issuance, are more likely to reflect investors' valuations of the bonds rather than liquidity conditions.[19] An added advantage of the spreads used is that they are not contaminated by the underpricing of corporate bond offerings (e.g., Wang, 2021).[20]

Second, 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.[21] The intensity of the treatment of our interest, understood as the probability of an eligible security being purchased by the Eurosystem, was thus higher in the primary market. Despite the fact that most of the purchases under the program were conducted in the secondary market,[22] the primary market is well suited to our analysis as we seek to quantify the bond-level rather than the macroeconomic effects of the CSPP.

Finally, it is worth pointing out that the bonds issued over the duration of the program appear representative, along important dimensions, of those on the secondary market when the program was announced. The bonds issued by companies in the sectors of consumer discretionary, utilities and industrials account for 23, 12 and 12 per cent of the total amount of euro-denominated bonds issued by euro-area non-banks, respectively. The corresponding percentages in the secondary market as of March 10, 2016 were 18, 17 and 13 per cent. In terms of the country of incorporation of the issuer, France, Netherlands and Germany make up 26, 25 and 15 per cent of the total in the primary, and 29, 22 and 12 per cent in the secondary market, respectively.

4.1. Data

Our interest lies in estimating the differential effect of the program on yields at issuance 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.[23] 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. Fig. 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.[24]

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

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

[16] The analytical formula for the variance of the ATT estimator is $\mathbb{V}(\widehat{\Delta}^{ATT}) = \frac{1}{(n\widehat{\theta})^2}\sum_{i=1}^{n}\widehat{\boldsymbol{I}}_i^2$, where $\widehat{\theta} = \sum_{i=1}^{n}\widehat{e}(\boldsymbol{X}_i)/n$ and $\widehat{\boldsymbol{I}}_i = Z_i(Y_i - \widehat{\tau}_1) - (1 - Z_i)(Y_i - \widehat{\tau}_0)\frac{\widehat{e}(\boldsymbol{X}_i)}{(1-\widehat{e}(\boldsymbol{X}_i)} - \widehat{\boldsymbol{H}}_{\boldsymbol{\eta}}^T(n\widehat{\boldsymbol{\Sigma}}_{\boldsymbol{\eta}})\boldsymbol{S}_i(\widehat{\boldsymbol{\eta}})$ with $\widehat{\tau}_0 = \sum_{i=1}^{n} Y_i(1 - Z_i)\frac{\widehat{e}(\boldsymbol{X}_i)}{(1-\widehat{e}(\boldsymbol{X}_i)} / \sum_{i=1}^{n}(1 - Z_i)\frac{\widehat{e}(\boldsymbol{X}_i)}{(1-\widehat{e}(\boldsymbol{X}_i)}$, $\widehat{\tau}_1 = \sum_{i=1}^{n} Y_i Z_i / \sum_{i=1}^{n} Z_i$, $\widehat{\boldsymbol{H}}_{\boldsymbol{\eta}} = \frac{1}{n}\sum_{i=1}^{n}\frac{(1-Z_i)(Y_i-\widehat{\tau}_0)}{(1-\widehat{e}(\boldsymbol{X}_i))^2}\widehat{e}_{\boldsymbol{\eta}}(\boldsymbol{X}_i)$. $\boldsymbol{S}_i(\widehat{\boldsymbol{\eta}})$ is the individual contribution to the gradient of the log-likelihood function of the ordered probit model, $\widehat{e}_{\boldsymbol{\eta}}(\boldsymbol{X}_i)$ the gradient of the propensity score and $\widehat{\boldsymbol{\Sigma}}_{\boldsymbol{\eta}}$ the variance-covariance matrix of the maximum likelihood estimates of the parameters of the ordered probit model.

[17] If we were interested in the effect of the program at a point in time, rather than over its entire duration, we would, on the contrary, have little choice but to base the analysis on secondary market prices.

[18] Due to the liquidity of new issues being higher than that of seasoned ones, the former are also used in earlier empirical investigations of corporate bond prices (see, e.g., Kessel, 1971; Sorensen, 1979; Fung and Rudd, 1986).

[19] See Chen et al. (2007), Bao et al. (2011), Dick-Nielsen et al. (2012) and Friewald et al. (2012) on how variations in liquidity conditions introduce noise to secondary-market prices.

[20] First reported prices after issuance, similar to the prices on which the yield spreads we employ are based on, have, on the contrary, been used to quantify the extent of underpricing in the primary corporate bond market (Chen et al., 2007).

[21] We estimated the latter percentage based on the eligible bonds in the ICE BofAML Euro Corporate Index (ER00) as of March 10, 2016.

[22] Between June 2016 and December 2018, 82 per cent of the 180 billion worth of bonds purchased were acquired in the secondary market.

[23] 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.

[24] 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.

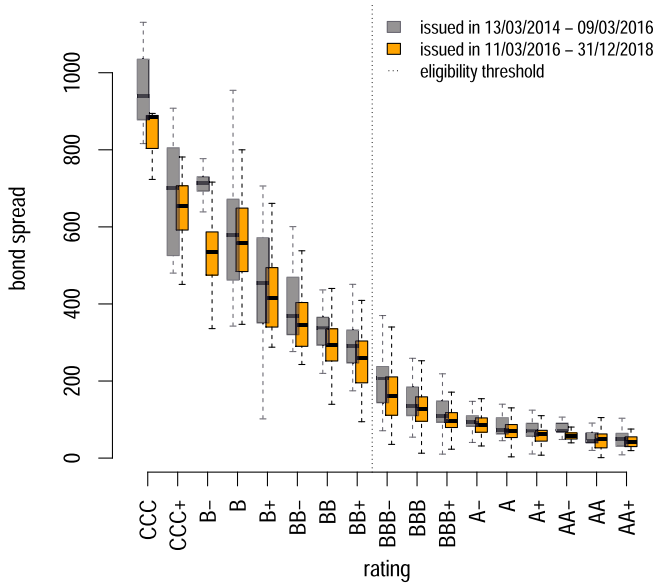

Fig. 1. Option-adjusted spreads by rating.
NOTE: The figure illustrates the distribution of the option-adjusted spread within each first-best rating category, for the bonds issued after (11/03/2016–31/12/2018) and before (13/03/2014–09/03/2016) the announcement of the CSPP.

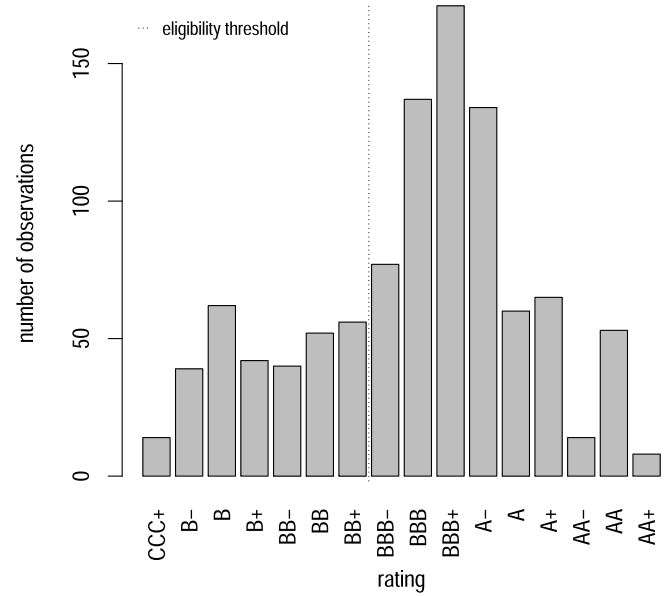

Fig. 2. Number of observations by rating.
NOTE: The figure illustrates the number of observations within each first-best rating category, for the bonds issued between March 11, 2016 and December 31, 2018.

Table 1
Summary statistics for the bond characteristics.

variable	mean	sd	Q_1	Q_2	Q_3	N
coupon rate	2.7	2.1	1.1	2.0	4.0	1654
original maturity	8.0	4.2	5.0	7.0	10	1654
amount sold	459	385	150	450	650	1643
OAS	199	180	78	125	277	1131

NOTE: The table presents summary statistics for the characteristics of the bonds issued between March 11, 2016 and December 31, 2018. Coupon rate is expressed in percentage points, original maturity in years, amount sold in millions of euros and OAS in basis points.

year to ensure that the issuer information predates the program being evaluated. Financial statement data, through not always complete, is obtained in this way for 1482 units. For 929 of them the data refer to the issuer of the bond, for 352 to its immediate parent company and for 201 to a parent company on a higher level in the business group.[25]

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.

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 1058 such bonds, of which 635 are callable, 364 bullet bonds, 52 convertible and 1 putable. The convertible and putable 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.

Given that we identify the effect of the CSPP on bonds around the eligibility threshold, it is important to make sure that such units are not too few. To this end, we illustrate in Fig. 2 the number of units in each rating category. Even though there are fewer noneligible than eligible bonds, the number of bonds also just below the threshold is adequate for our analysis.

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 causal 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

[25] If we include in the sample only the bonds for which the financial statement data refer to its issuers or the immediate parent of the issuer, our estimates of the differential effect remain statistically not different from zero (see Table B.1 in Appendix B).

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	0.34	0.046	0.10	0.18	1370
cf	$\frac{\text{cash from operations}}{\text{total assets}}$	0.055	0.089	0.032	0.066	0.095	1232
liq	$\frac{\text{cash from operations}}{\text{total liabilities}}$	0.094	0.14	0.047	0.095	0.15	1232
cov	$\frac{\text{EBIT}}{\text{interest expenses}}$	7.9	18	1.4	3.8	8.0	1295
lev	$\frac{\text{total debt}}{\text{total assets}}$	0.37	0.20	0.23	0.36	0.49	1332
solv	$\frac{\text{common equity}}{\text{total assets}}$	0.29	0.20	0.18	0.29	0.41	1365
size	log(totalrevenue)	3.5	1.1	2.9	3.7	4.4	1379
age	2017 − year founded	74	72	22	58	114	1277
ltdebt	$\frac{\text{long-term debt}}{\text{total assets}}$	0.32	0.31	0.16	0.26	0.40	1325

NOTE: The table presents summary statistics for the issuer characteristics of the bonds issued between March 11, 2016 and December 31, 2018. The variable size is calculated with total revenue recorded in millions of euros.

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 first-best 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 first-best 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.[26] 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 first-best 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 Fig. 3.

To assess the goodness of fit of the propensity score model, we inspect how well it predicts ratings around the BBB- eligibility threshold. From Fig. 3, we observe 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. The model predicts the first-best rating accurately, especially in the case of the eligible bonds. The estimated propensity scores for the BBB- 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

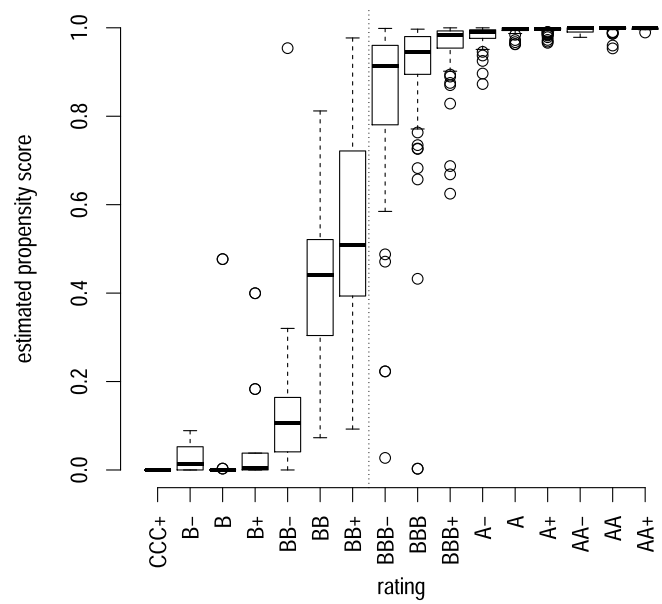

Fig. 3. Estimated propensity scores by rating in the full sample.
NOTE: The figure illustrates the distribution of the estimated propensity scores within each first-best rating category in the full sample. The ordered probit specification used to estimate the propensity scores contains the variables: cpn, mat, prof, cov, solv, size and size^2.

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.[27] Moreover, Fig. 3 shows that all the 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.

The predictive accuracy of the propensity score model remains high also out of sample. Specifically, the estimated propensity scores are little changed when the sample is split randomly and predictions for units in each half are obtained using the model estimated with the other half (see Figure B.1 in Appendix B). This finding constitutes further evidence in favor of the adopted propensity score model.

[26] 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).

[27] 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.

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

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: The table presents the number of controls (n_0), the number of treated units (n_1) and the standardized bias of each covariate in symmetric subsamples around the eligibility threshold, constructed using the full sample.

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):

$$\mathrm{SB} = \left(\frac{\sum_{i=1}^{n} x_i z_i w_i}{\sum_{i=1}^{n} z_i w_i} - \frac{\sum_{i=1}^{n} x_i (1 - z_i) w_i}{\sum_{i=1}^{n} (1 - z_i) w_i} \right) \Big/ \sqrt{s_0^2/n_0 + s_1^2/n_1},$$

where s_z^2 denotes the sample variance of the unweighted covariate and n_z the number of units in group $z = 0, 1$ (Hirano and Imbens, 2001). 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.[28] 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 covariate imbalance, 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 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 Fig. 4, 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.[29] For values of the estimated propensity scores

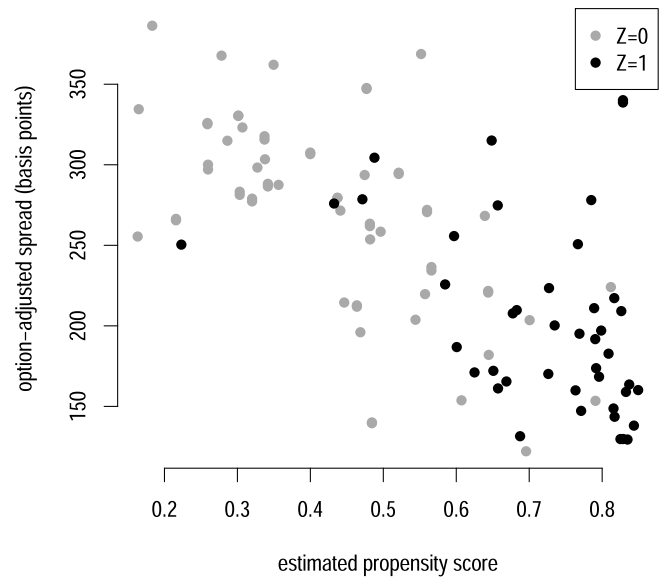

Fig. 4. The OAS as a function of the estimated propensity score.
NOTE: The figure illustrates the relationship between the estimated propensity scores and the option-adjusted spread for the control (Z = 0) and treated units (Z = 1) in the full sample whose estimated propensity scores lie between 0.15 and 0.85.

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 similar noneligible bonds. However, definite conclusions are difficult to draw due to the relatively large dispersion in the outcomes of the two groups.[30]

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

[28] 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.

[29] 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.

[30] 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 estimated contain a larger number of units.

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*.[31] These variables enter the model linearly. We estimate this outcome model separately for the noneligible and eligible bonds, and use the estimates for the former in the augmented weighting estimator.

4.2.3.1. 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.[32] The choice is supported also by the fact that the eligibility criterion that we exploit became known when the program was announced.[33] 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 one covariate is no longer balanced.[34] These estimates are obtained employing the augmented weighting estimator, which can reduce the possible bias which may arise when considering these subsamples.[35]

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 similar noneligible bonds. This finding confirms the preliminary conclusion drawn from an inspection of the distribution of the spreads at issuance in Fig. 4. 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 similar 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.

It is worthwhile to highlight that all the point estimates in Panel B of Table 4 are positive, rather than negative as should be the case had the program permanently lowered the yield spreads of the eligible bonds but not those of the noneligible. As a result, even though the effects are not estimated with great precision, we can reject the hypothesis of an economically meaningful, negative differential effect. Specifically, we consider the null of the program having had a differential effect of at most −20 basis points, representing a 10 per cent reduction with respect to the average yield spread. The results in Table B4 show that this null can be rejected with high confidence.

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

n_0	n_1	estimate	se (*p*-val.)
Panel A. Weighting est.			
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)
Panel B. Aug. weighting est.			
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: The table presents the number of controls (n_0), the number of treated units (n_1) and the estimates obtained using the simple weighting estimator (Panel A) and the augmented weighting estimator (Panel B) in symmetric subsamples around the eligibility threshold, constructed using the full sample. The standard errors are calculated using the M-estimation-based analytical formulae.

4.2.3.2. 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.[36]

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.[37] If the program was expected to affect the spreads of eligible bonds by altering the composition of their holders, 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.[38] The effect of the program on the eligible bonds is statistically significant neither when applying the simple weighting estimator nor when

[31] In addition to these variables, we also considered including indicator variables for subperiods of the sample (and alternatively a linear time trend) to account for potential time variation in the spreads of the eligible and noneligible bonds. However, such indicator variables had statistically insignificant coefficients and did not materially alter the estimates obtained using the augmented weighting estimator. For this reason, we did not include them in the outcome model.

[32] In light of the limited number of observations in the subsamples around the eligibility threshold, it is worth pointing out that both the propensity score model and the outcome models are estimated using all the observations, and not just those in the subsamples.

[33] The results do not, however, change if only the period of positive net purchases, starting in June 8, 2016, is considered.

[34] Specifically, in some of the subsamples the SB of the variable size exceeds 1.96 in absolute value.

[35] With the purpose of mitigating concerns that the statistical insignificance of the estimates is due to the moderate sizes of the subsamples, we also consider larger subsamples in which at most two covariates, rather than one, are unbalanced. The results, showing that the estimates remain statistically not significantly different from zero, along with the corresponding balancing statistics, are reported in Tables B.2 and B.3 in Appendix B.

[36] In this, as well as the next exercise examining another dimension of heterogeneity, the propensity score model and the outcome model for the noneligible bonds are re-estimated using only bonds in the sample of interest.

[37] 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.

[38] The SBs of the pre-treatment variables in the subsamples considered in Table 5 are reported in Table B5 and the number of bonds issued during the subperiod of interest in Figure B.2, both in Appendix B. Even though the number of observations around the eligibility threshold is lower than in the full sample, the standard errors of the estimates in Panel B are smaller than in Table 4. Thus, there is no evidence that the precision of the estimates deteriorates due to too few observations.

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

n_0	n_1	estimate	se (*p*-val.)
Panel A. Weighting est.			
15	5	−8.9	50.1 (0.859)
15	6	3.8	46.1 (0.935)
Panel B. Aug. weighting est.			
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: The table presents the number of controls (n_0), the number of treated units (n_1) and the estimates obtained using the simple weighting estimator (Panel A) and the augmented weighting estimator (Panel B) in symmetric subsamples around the eligibility threshold, constructed using the bonds issued in March 1 – December 31, 2018. The standard errors are calculated using the M-estimation-based analytical formulae.

the augmented weighting estimator is employed.

4.2.3.3. 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 (Committee on the Global Financial System, 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, for each euro-area country, we calculate the share of the stock of debt securities issued by resident non-financial corporations held by euro-area insurance corporations and pension funds.[39] Then, we only consider the bonds issued by companies incorporated in countries in which this share 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 Fig. 5).

The results of this analysis are presented in Table 6.[40] Differently 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 spread differential between eligible and similar noneligible bonds differently in markets in which a large share of corporate bonds are held by insurance companies and pension funds.

These findings suggest the CSPP did not permanently lower the yields spreads of the eligible bonds relative to those of the noneligible even in countries in which a larger share of corporate bonds are held by less price-sensitive investors. In other words, such investors would not, according to our analysis, appear to have materially shaped the propagation of the effects of the program. That said, drawing more definite conclusions would require more granular data than is available to us. Ideally, the measure used to capture the role of long-term investors would be bond-specific and capture the share of the holdings of the Eurosystem purchased from such investors.

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 similar noneligible bonds, even though it entailed the Eurosystem becoming an increasingly large holder of euro-dominated corporate bonds. Given that the securities purchased under the CSPP make up only a fraction of the total asset purchases of the Eurosystem, it is legitimate to ask whether this finding can be expected to carry over to the other asset purchase programs of the ECB, such as the PSPP under which public sector securities were purchased. It can be argued that it may indeed be so, for two reasons. First, our analysis concerns a market which is more illiquid than that for government bonds.

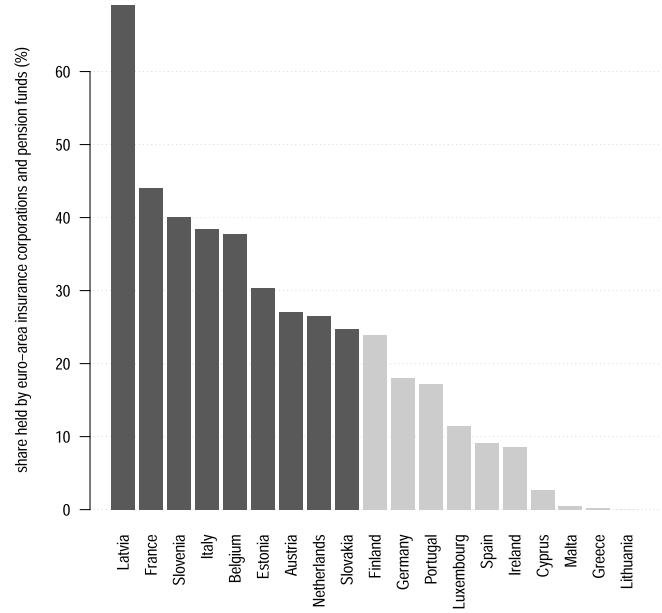

Fig. 5. Stock of long-term debt securities issued by resident NFCs, 2016Q1.
NOTE: For each country, the ratio between the long-term debt securities issued by resident non-financial corporations held by euro-area insurance corporations and pension funds and the outstanding amount of long-term debt securities issued by resident non-financial corporations. The latter are obtained from the Securities Issues (SEC) and the former from the Securities Holding Statistics (SHS) of the ECB.

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

n_0	n_1	estimate	se (*p*-val.)
Panel A. Weighting est.			
34	12	−12.8	22.1 (0.563)
34	13	−17.8	21.5 (0.407)
Panel B. Aug. weighting est.			
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: The table presents the number of controls (n_0), the number of treated units (n_1) and the estimates obtained using the simple weighting estimator (Panel A) and the augmented weighting estimator (Panel B) in symmetric subsamples around the eligibility threshold, constructed using the bonds issued in Latvia, France, Slovenia, Italy, Belgium, Estonia, Austria, the Netherlands and Slovakia. The standard errors are calculated using the M-estimation-based analytical formulae.

[39] Ideally, we would include in the numerator also the securities held by non-euro-area insurance corporations and pension funds. However, we are unable to do so as the data we employ does not contain information about their holdings.

[40] The corresponding SBs are reported in Table B.6 and the number of bonds issued in the countries of interest in Figure B.3, both in Appendix B.

As a result, if anything it could be expected that central bank holdings have a larger distortionary effect than in the government bond market. Second, the corporate bond market is considerably more heterogeneous than that for government bonds. Consequently, it can be more difficult for investors to replace the bonds purchased by the Eurosystem with similar securities, exerting upward pressure on the prices of the eligible bonds.

The results of our analysis also shed light on the channels through which corporate bond prices could have been influenced by the CSPP. Our finding of no permanent effect on the relative prices of the eligible and noneligible bonds speaks against the program having improved the liquidity of the market segment in which the purchases took place. Such an effect should have lowered the yield spreads of the eligible bonds but not those of the noneligible. This channel having played a limited role can be rationalized by the fact that the purchases under the CSPP took place over a relatively tranquil period, rather one of market stress.

The absence of a differential effect on the eligible bonds even when the holdings of the Eurosystem were at their highest level suggests that also changes in the relative scarcity of corporate bonds due to the CSPP did not have material consequences. That is, the eligible bonds seem not to be highly imperfectly substitutable with other securities, as otherwise changes in their outstanding amount should influence their prices. Another reason could be the increase in the issuance of CSPP-eligible securities documented by De Santis and Zaghini (2021). Finally, also the preferred habitat theory is unable to explain our findings. Specifically, the fact that we do not find a significant differential effect in countries in which less price-sensitive investors hold more corporate bonds does not support the view that such investors are key players in the chain of transmission of central bank asset purchases.

Our results suggest that, if anything, the CSPP raised the prices of the eligible and noneligible bonds proportionally. This interpretation is consistent with central bank asset purchase programs operating in times of low financial distress predominantly through "broad channels". When their effects are transmitted through such channels also assets not targeted by the policy are affected (Altavilla et al., 2021). In the case of the CSPP, it is plausible that the program improved the bargaining position not only of the eligible issuers but also that of those noneligible *vis-à-vis* their creditors. Such an effect could have materialized, for instance, due to the noneligible issuers finding it easier to borrow also from banks, as the eligible issuers substituted bank loans with bonds (Grosse-Rueschkamp et al., 2019).

There could be a concern that our estimates are distorted by other policies of the ECB in place during the period analyzed. Such a concern could arise especially in light of the fact that the CSPP was announced together with two other monetary policy measures: a five basis point decrease in the main policy rate of the ECB (the interest rate on the main refinancing operations) and a new series of targeted longer-term refinancing operations (TLTRO II). The possibility that these measures affected the yield spreads of the corporate bonds in our sample does not in itself constitute a problem. If, however, they affected the eligible bonds differently than the noneligible, the interpretation of our results could be more complicated. It can be postulated that the TLTRO II might have had such a differential effect as the eligible bonds were eligible to be pledged as collateral in them. In this case, we would expect to find a negative differential effect on the eligible bonds. Thus, the fact that we obtain positive point estimates constitutes evidence against such an effect of the TLTRO II, allaying the concern that our estimates capture the effects of policies other than the CSPP.

5. Conclusion

Motivated by the increasing reliance of advanced-economy central banks on asset purchase programs and the considerable uncertainty about their effectiveness as well as their side effects, we propose a framework of causal inference suitable for evaluating their effects. The framework allows the program of interest to affect all the units in the population of interest, irrespective of whether or not they qualify for it. Due to the presence of such general equilibrium effects, it is not possible, under standard identifying assumptions, to identify the treatment effect of the program for any target population. Yet, meaningful differential effects can be identified. Specifically, under relatively mild assumptions, it is possible to identify the effect of the program on the treated relative to its effect on similar controls. This effect can be consistently estimated using weighting estimators developed for estimating the average treatment effect on the treated.

We use the empirical framework to estimate the causal effect of the corporate sector purchase programme (CSPP) of the European Central Bank on the yield spreads of the corporate bonds that were eligible for purchase under the CSPP, relative to that on the spreads of similar noneligible bonds. This effect is of economic interest as it measures the degree to which the program distorted the relative prices of the eligible and the noneligible bonds. We find that the differential effect on the yield spreads of bonds issued over the duration of the program was not significantly different from zero. In other words, we do not find evidence of the program having permanently altered the relative prices in the corporate bond market where the purchases were conducted. This result is robust to considering periods and jurisdictions in which the Eurosystem's holdings can be expected to have been most relevant.

Our analysis concerns distortions in the corporate bond market as the empirical approach requires the control units not to be too dissimilar from the treated. Yet, the treatment and control group represent different segments of the corporate bond market. For this reason, the absence of a differential effect on the treated is a policy-relevant result as it suggests that relative prices of targeted and nontargeted securities may not be altered even if they differ from each other in terms of the investor composition and liquidity of the market segments in which they are traded. Whether central bank asset purchase programs also have no distortionary effects on relative prices across asset classes constitutes an important avenue of future research.

A limitation of our analysis is that it only characterizes conditions under which meaningful differential effects can be identified in the presence of general equilibrium effects. Identification of the average treatment and spillover effect separately would require the imputation of the outcomes that would have been observed had the program of interest not been in place. Control units that are informative about these missing outcomes may, however, be significantly different from the treated units. Consequently, existing methods of causal inference may not be applicable. For this reason, we leave the identification of these causal effects for future research.

Declaration of competing interest

The authors have no conflict of interest to declare.

Appendix A.

A.1. Proofs of the results in the main text

Proof of Proposition 1. Consider first the estimator $\widehat{\Delta}^{ATT}$ in (2). It can be easily shown that the first term converges to $\mathbb{E}[Y_i|Z_i = 1] =$

$\mathbb{E}[Y_i(1,\psi)|Z_i=1]$. As for the second term, we obtain:

$$\frac{\sum_{i=1}^n Y_i(1-Z_i)\frac{\widehat{e}(\boldsymbol{X}_i)}{1-\widehat{e}(\boldsymbol{X}_i)}}{\sum_{i=1}^n (1-Z_i)\frac{\widehat{e}(\boldsymbol{X}_i)}{1-\widehat{e}(\boldsymbol{X}_i)}} \to \frac{\mathbb{E}\left[Y_i(1-Z_i)\frac{\widehat{e}(\boldsymbol{X}_i)}{1-\widehat{e}(\boldsymbol{X}_i)}\right]}{Pr(Z_i=1)}, \tag{A.1}$$

where:

$$\begin{aligned}
& \frac{\mathbb{E}\left[Y_i(1-Z_i)\frac{\widehat{e}(\boldsymbol{X}_i)}{1-\widehat{e}(\boldsymbol{X}_i)}\right]}{Pr(Z_i=1)} \\
= & \frac{\mathbb{E}_{\boldsymbol{X}_i}\left[\frac{Pr(Z_i=1|\boldsymbol{X}_i=\boldsymbol{x}_i)}{Pr(Z_i=0|\boldsymbol{X}_i=\boldsymbol{x}_i)}\mathbb{E}[Y_i(0,\psi)|\boldsymbol{X}_i=\boldsymbol{x}_i,Z_i=0]Pr(Z_i=0|\boldsymbol{X}_i=\boldsymbol{x}_i)\right]}{Pr(Z_i=1)} \\
= & \int \frac{Pr(Z_i=1|\boldsymbol{X}_i=\boldsymbol{x}_i)}{Pr(Z_i=1)}\mathbb{E}[Y_i(0,\psi)|\boldsymbol{X}_i=\boldsymbol{x}_i,Z_i=0]\mathrm{d}F(\boldsymbol{x}_i) \\
= & \int \mathbb{E}[Y_i(0,\psi)|\boldsymbol{X}_i=\boldsymbol{x}_i,Z_i=0]\mathrm{d}F(\boldsymbol{x}_i|Z_i=1) \\
= & \int \left\{\mathbb{E}[Y_i(0,0)|\boldsymbol{X}_i=\boldsymbol{x}_i,Z_i=0]+\mathbb{E}[\tau_i^0|\boldsymbol{X}_i=\boldsymbol{x}_i,Z_i=0]\right\}\mathrm{d}F(\boldsymbol{x}_i|Z_i=1) \\
= & \int \left\{\mathbb{E}[Y_i(0,0)|\boldsymbol{X}_i=\boldsymbol{x}_i,Z_i=1]+\mathbb{E}[\tau_i^0|\boldsymbol{X}_i=\boldsymbol{x}_i,Z_i=0]\right\}\mathrm{d}F(\boldsymbol{x}_i|Z_i=1) \\
= & \mathbb{E}[Y_i(0,0)|Z_i=1]+\int \mathbb{E}[\tau_i^0|\boldsymbol{X}_i=\boldsymbol{x}_i,Z_i=0]\mathrm{d}F(\boldsymbol{x}_i|Z_i=1)
\end{aligned} \tag{A.2}$$

The penultimate equality follows from Assumption 3. Thus, $\widehat{\Delta}^{ATT}$ converges to:

$$\mathbb{E}[\tau_i^1|Z_i=1]-\int \mathbb{E}[\tau_i^0|\boldsymbol{X}_i=\boldsymbol{x}_i,Z_i=0]\mathrm{d}F(\boldsymbol{x}_i|Z_i=1) \tag{A.3}$$

As regards the estimator $\widehat{\Delta}_{DR}^{ATT}$ in (3), its first term is identical to that of $\widehat{\Delta}^{ATT}$ and thus converges to $\mathbb{E}[Y(1,\psi)|Z_i=1]$. The second term can be rearranged as follows:

$$\frac{\sum_{i=1}^n \frac{Y_i(1-Z_i)\widehat{e}(\boldsymbol{X}_i)+\widehat{\mu}_0(\boldsymbol{X}_i)(Z_i-\widehat{e}(\boldsymbol{X}_i))}{1-\widehat{e}(\boldsymbol{X}_i)}}{\sum_{i=1}^n Z_i} = \frac{\sum_{i=1}^n Y_i(1-Z_i)\frac{\widehat{e}(\boldsymbol{X}_i)}{1-\widehat{e}(\boldsymbol{X}_i)}}{\sum_{i=1}^n Z_i} + \frac{\sum_{i=1}^n \widehat{\mu}_0(\boldsymbol{X}_i)\frac{Z_i-\widehat{e}(\boldsymbol{X}_i)}{1-\widehat{e}(\boldsymbol{X}_i)}}{\sum_{i=1}^n Z_i}, \tag{A.4}$$

where:

$$\frac{\sum_{i=1}^n Y_i(1-Z_i)\frac{\widehat{e}(\boldsymbol{X}_i)}{1-\widehat{e}(\boldsymbol{X}_i)}}{\sum_{i=1}^n Z_i} \to \frac{\mathbb{E}\left[Y_i(1-Z_i)\frac{\widehat{e}(\boldsymbol{X}_i)}{1-\widehat{e}(\boldsymbol{X}_i)}\right]}{Pr(Z_i=1)} \tag{A.5}$$

$$\frac{\sum_{i=1}^n \widehat{\mu}_0(\boldsymbol{X}_i)\frac{Z_i-\widehat{e}(\boldsymbol{X}_i)}{1-\widehat{e}(\boldsymbol{X}_i)}}{\sum_{i=1}^n Z_i} \to 0 \tag{A.6}$$

Therefore, $\widehat{\Delta}_{DR}^{ATT}$ converges to the same limit as $\widehat{\Delta}^{ATT}$.

Proof of Corollary 1. Suppose that $\widehat{e}(\boldsymbol{X}_i) \nrightarrow e(\boldsymbol{X}_i)$ and $\widehat{\mu}_0(\boldsymbol{X}_i) \to \mathbb{E}[Y_i|\boldsymbol{X}_i,Z_i=0]$. The first term of the estimator $\widehat{\Delta}_{DR}^{ATT}$ in (3) converges also in this case to $\mathbb{E}[Y(1,\psi)|Z_i=1]$. As for the second term, it can be rearranged as follows:

$$\frac{\sum_{i=1}^n \frac{Y_i(1-Z_i)\widehat{e}(\boldsymbol{X}_i)+\widehat{\mu}_0(\boldsymbol{X}_i)(Z_i-\widehat{e}(\boldsymbol{X}_i))}{1-\widehat{e}(\boldsymbol{X}_i)}}{\sum_{i=1}^n Z_i} = \frac{\sum_{i=1}^n \widehat{\mu}_0(\boldsymbol{X}_i)Z_i}{\sum_{i=1}^n Z_i} + \frac{\sum_{i=1}^n \frac{Y_i(1-Z_i)\widehat{e}(\boldsymbol{X}_i)-\widehat{\mu}_0(\boldsymbol{X}_i)(1-Z_i)\widehat{e}(\boldsymbol{X}_i)}{1-\widehat{e}(\boldsymbol{X}_i)}}{\sum_{i=1}^n Z_i} \tag{A.7}$$

where:

$$\frac{\sum_{i=1}^n \widehat{\mu}_0(\boldsymbol{X}_i)Z_i}{\sum_{i=1}^n Z_i} \to \frac{\mathbb{E}_{\boldsymbol{X}_i}\left[\mathbb{E}[Y_i|\boldsymbol{X}_i,Z_i=0]|Z_i=1\right]Pr(Z_i=1)}{Pr(Z_i=1)} \tag{A.8}$$

$$\frac{\sum_{i=1}^n \frac{Y_i(1-Z_i)\widehat{e}(\boldsymbol{X}_i)-\widehat{\mu}_0(\boldsymbol{X}_i)(1-Z_i)\widehat{e}(\boldsymbol{X}_i)}{1-\widehat{e}(\boldsymbol{X}_i)}}{\sum_{i=1}^n Z_i} \to 0, \tag{A.9}$$

The limit in (A.8) is equal to:

$$\mathbb{E}_{\boldsymbol{X}_i}\left[\mathbb{E}[Y_i|\boldsymbol{X}_i,Z_i=0]|Z_i=1\right] = \int \mathbb{E}[Y_i(0,\psi)|\boldsymbol{X}_i=\boldsymbol{x}_i,Z_i=0]\mathrm{d}F(\boldsymbol{x}_i|Z_i=1) \tag{A.10}$$

Under Assumption 3, the integral in (A.10) satisfies:

$$\begin{aligned}
&\int \mathbb{E}[Y_i(0,\psi)|\boldsymbol{X}_i=\boldsymbol{x}_i, Z_i=0]\mathrm{d}F(\boldsymbol{x}_i|Z_i=1)\\
=&\int \mathbb{E}[Y_i(0,0)|\boldsymbol{X}_i=\boldsymbol{x}_i, Z_i=0]\mathrm{d}F(\boldsymbol{x}_i|Z_i=1)+\int \mathbb{E}[\tau_i^0|\boldsymbol{X}_i=\boldsymbol{x}_i, Z_i=0]\mathrm{d}F(\boldsymbol{x}_i|Z_i=1)\\
=&\int \mathbb{E}[Y_i(0,0)|\boldsymbol{X}_i=\boldsymbol{x}_i, Z_i=1]\mathrm{d}F(\boldsymbol{x}_i|Z_i=1)+\int \mathbb{E}[\tau_i^0|\boldsymbol{X}_i=\boldsymbol{x}_i, Z_i=0]\mathrm{d}F(\boldsymbol{x}_i|Z_i=1)\\
=&\mathbb{E}[Y_i(0,0)|Z_i=1]+\int \mathbb{E}[\tau_i^0|\boldsymbol{X}_i=\boldsymbol{x}_i, Z_i=0]\mathrm{d}F(\boldsymbol{x}_i|Z_i=1),
\end{aligned} \tag{A.11}$$

where the penultimate equality follows from Assumption 3. Thus:

$$\widehat{\Delta}_{DR}^{ATT} \to \mathbb{E}[\tau_i^1|Z_i=1]-\int \mathbb{E}[\tau_i^0|\boldsymbol{X}_i=\boldsymbol{x}_i, Z_i=0]\mathrm{d}F(\boldsymbol{x}_i|Z_i=1), \tag{A.12}$$

as stated in Proposition 1.

Suppose instead that $\widehat{e}(\boldsymbol{X}_i)\to e(\boldsymbol{X}_i)$ and $\widehat{\mu}_0(\boldsymbol{X}_i)\nrightarrow \mathbb{E}[Y_i|\boldsymbol{X}_i, Z_i=0]$. By the proof of Proposition 1:

$$\widehat{\Delta}_{DR}^{ATT} \to \mathbb{E}[Y(1,\psi)|Z_i=1]-\frac{\mathbb{E}\left[Y_i(1-Z_i)\frac{\widehat{e}(\boldsymbol{X}_i)}{1-\widehat{e}(\boldsymbol{X}_i)}\right]}{Pr(Z_i=1)} \tag{A.13}$$

since $\widehat{e}(\boldsymbol{X}_i)\to e(\boldsymbol{X}_i)$ implies that:

$$\frac{\sum_{i=1}^n \widehat{\mu}_0(\boldsymbol{X}_i)\frac{Z_i-\widehat{e}(\boldsymbol{X}_i)}{1-\widehat{e}(\boldsymbol{X}_i)}}{\sum_{i=1}^n Z_i}\to 0 \tag{A.14}$$

Being independent of $\widehat{\mu}_0(\boldsymbol{X}_i)$, the second term of the limit in (A.13) satisfies (A.2) under Assumption 3. Thus, $\widehat{\Delta}_{DR}^{ATT}$ converges to Δ^{DE} in Proposition 1 also in this case.

A.2. Identification under an alternative set of assumptions

Consider the following alternative to Assumption 3.

Assumption A.3. For each i in Ω_o, the treatment assignment satisfies: $\Pr(Z_i|Y_i(0,\psi),\boldsymbol{X}_i) = \Pr(Z_i|\boldsymbol{X}_i)$.

As unconfoundedness for controls, Assumption A.3 imposes restrictions on the conditional distributions of a potential outcome that is only observed for the controls. Combining this version of unconfoundedness with Assumptions 1 and 2 yields the following result on the asymptotic properties of the estimators in (2) and (3).

Proposition A.1. *Under Assumptions* 1, 2 *and* A.3, *the estimators in* (2) *and* (3) *satisfy:*

$$\begin{aligned}
&\widehat{\Delta}^{ATT}\to\Delta^{DE'},\quad \widehat{\Delta}_{DR}^{ATT}\to\Delta^{DE'},\\
&\Delta^{DE'}=\int\left[\tau^1(\boldsymbol{x}_i,1)-\tau^0(\boldsymbol{x}_i,1)\right]\mathrm{d}F(\boldsymbol{x}_i|Z_i=1)\\
&\quad=\mathbb{E}[\tau_i^1-\tau_i^0|Z_i=1]
\end{aligned} \tag{A.15}$$

Proof of Proposition A.1. By the proof of Proposition 1, the first term of the estimator $\widehat{\Delta}^{ATT}$ in (2) converges to $\mathbb{E}[Y_i(1,\psi)|Z_i=1]$ and the second term to the limit in (A.1). The latter satisfies:

$$\begin{aligned}
&\frac{\mathbb{E}\left[Y_i(1-Z_i)\frac{\widehat{e}(\boldsymbol{X}_i)}{1-\widehat{e}(\boldsymbol{X}_i)}\right]}{Pr(Z_i=1)}\\
=&\frac{\mathbb{E}_{\boldsymbol{X}_i}\left[\frac{Pr(Z_i=1|\boldsymbol{X}_i=\boldsymbol{x}_i)}{Pr(Z_i=0|\boldsymbol{X}_i=\boldsymbol{x}_i)}\mathbb{E}[Y_i(0,\psi)|\boldsymbol{X}_i=\boldsymbol{x}_i, Z_i=0]Pr(Z_i=0|\boldsymbol{X}_i=\boldsymbol{x}_i)\right]}{Pr(Z_i=1)}\\
=&\int\frac{Pr(Z_i=1|\boldsymbol{X}_i=\boldsymbol{x}_i)}{Pr(Z_i=1)}\mathbb{E}[Y_i(0,\psi)|\boldsymbol{X}_i=\boldsymbol{x}_i, Z_i=0]\mathrm{d}F(\boldsymbol{x}_i)\\
=&\int\mathbb{E}[Y_i(0,\psi)|\boldsymbol{X}_i=\boldsymbol{x}_i, Z_i=0]\mathrm{d}F(\boldsymbol{x}_i|Z_i=1)\\
=&\int\mathbb{E}[Y_i(0,\psi)|\boldsymbol{X}_i=\boldsymbol{x}_i, Z_i=1]\mathrm{d}F(\boldsymbol{x}_i|Z_i=1)\\
=&\mathbb{E}[Y_i(0,\psi)|Z_i=1]
\end{aligned} \tag{A.16}$$

The penultimate equality follows from Assumption A.3. Thus, $\widehat{\Delta}^{ATT}$ converges to:

$$\mathbb{E}[Y_i(1,\psi) - Y_i(0,\psi)|Z_i = 1] = \mathbb{E}[\tau_i^1 - \tau_i^0|Z_i = 1] \tag{A.17}$$

As for the estimator $\widehat{\Delta}_{DR}^{ATT}$ in (3), by the proof of Proposition 1, it converges to:

$$\mathbb{E}[Y(1,\psi)|Z_i = 1] - \frac{\mathbb{E}\left[Y_i(1-Z_i)\frac{\widehat{e}(\boldsymbol{X}_i)}{1-\widehat{e}(\boldsymbol{X}_i)}\right]}{Pr(Z_i = 1)} \tag{A.18}$$

Therefore, also $\widehat{\Delta}_{DR}^{ATT}$ converges to the limit in (A.17).

The alternative set of assumptions (1, 2 and A.3) identity a different effect than that employed in the main text (1, 2 and 3), namely the differential effect of the program on the treated.[41] More specifically, the estimators are consistent for the difference between the treatment effect on the treated and the spillover effect they would have felt had they been instead assigned to the control condition. The augmented weighting estimator is consistent for this differential effect also when either the propensity score model or the potential outcome model is misspecified.

Corollary A.1. *The augmented weighting estimator in* (3) *is consistent for* $\Delta^{DE\prime}$ *in Proposition* A.1 *also when either of the following two sets of conditions holds:*

$$\widehat{e}(\boldsymbol{X}_i) \nrightarrow e(\boldsymbol{X}_i) \quad \text{and} \quad \widehat{\mu}_0(\boldsymbol{X}_i) \rightarrow \mathbb{E}[Y_i|\boldsymbol{X}_i, Z_i = 0] \tag{A.19}$$

$$\widehat{e}(\boldsymbol{X}_i) \rightarrow e(\boldsymbol{X}_i) \quad \text{and} \quad \widehat{\mu}_0(\boldsymbol{X}_i) \nrightarrow \mathbb{E}[Y_i|\boldsymbol{X}_i, Z_i = 0] \tag{A.20}$$

Proof of Corollary A.1. Suppose that $\widehat{e}(\boldsymbol{X}_i) \nrightarrow e(\boldsymbol{X}_i)$ and $\widehat{\mu}_0(\boldsymbol{X}_i) \rightarrow \mathbb{E}[Y_i|\boldsymbol{X}_i, Z_i = 0]$. By the proof of Corollary 1, the estimator $\widehat{\Delta}_{DR}^{ATT}$ in (3) converges to:

$$\mathbb{E}[Y(1,\psi)|Z_i = 1] - \int \mathbb{E}[Y_i(0,\psi)|\boldsymbol{X}_i = \boldsymbol{x}_i, Z_i = 0]\mathrm{d}F(\boldsymbol{x}_i|Z_i = 1) \tag{A.21}$$

Given that Assumption A.3 is satisfied, we have that $\mathbb{E}[Y_i(0,\psi)|\boldsymbol{X}_i = \boldsymbol{x}_i, Z_i = 0] = \mathbb{E}[Y_i(0,\psi)|\boldsymbol{X}_i = \boldsymbol{x}_i, Z_i = 1]$. Hence, the integral in (A.21) is equal to $\mathbb{E}[Y_i(0,\psi)|Z_i = 1]$. Thus, $\widehat{\Delta}_{DR}^{ATT} \rightarrow \mathbb{E}[Y_i(1,\psi) - Y_i(0,\psi)|Z_i = 1]$, as stated in Proposition A.1.

Suppose instead that $\widehat{e}(\boldsymbol{X}_i) \rightarrow e(\boldsymbol{X}_i)$ and $\widehat{\mu}_0(\boldsymbol{X}_i) \nrightarrow \mathbb{E}[Y_i|\boldsymbol{X}_i, Z_i = 0]$. By the proof of Corollary 1:

$$\widehat{\Delta}_{DR}^{ATT} \rightarrow \mathbb{E}[Y(1,\psi)|Z_i = 1] - \frac{\mathbb{E}\left[Y_i(1-Z_i)\frac{\widehat{e}(\boldsymbol{X}_i)}{1-\widehat{e}(\boldsymbol{X}_i)}\right]}{Pr(Z_i = 1)} \tag{A.22}$$

Being independent of $\widehat{\mu}_0(\boldsymbol{X}_i)$, the second term of the limit in (A.22) satisfies (A.16) under Assumption A.3. Thus, $\widehat{\Delta}_{DR}^{ATT}$ converges to $\Delta^{DE\prime}$ in Proposition A.1 also in this case.

Appendix B

Table B.1
Estimates obtained using data on the issuers and their immediate parents only.

n_0	n_1	estimate	se (*p*-val.)
Aug. weighting est.			
28	8	−26.1	26.3 (0.320)
29	8	−13.6	26.9 (0.612)
30	8	−9.4	26.8 (0.725)
31	8	−18.3	28.7 (0.525)
32	8	−19.4	28.8 (0.500)
33	8	−22.0	29.2 (0.452)
35	8	−21.4	29.1 (0.462)

NOTE: The table presents the number of controls (n_0), the number of treated units (n_1) and the estimates obtained using the augmented weighting estimator in symmetric subsamples around the eligibility threshold, constructed using data on the issuers and their immediate parents only. The standard errors are calculated using the M-estimation-based analytical formulae.

[41] It can, however, be proven that, if $\Pr(Z_i|Y_i(0,0), Y_i(0,\psi), \boldsymbol{X}_i) = \Pr(Z_i|\boldsymbol{X}_i)$, which is stronger than either Assumption 3 or Assumption A.3, then $\Delta^{DE} = \Delta^{DE\prime}$.

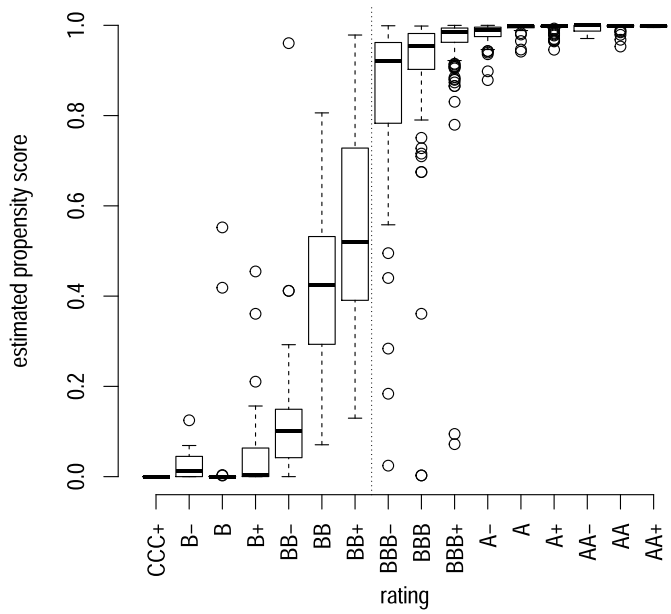

Fig. B.1 Predicted propensity scores out of sample by rating. NOTE: The figure illustrates the distribution of the estimated propensity scores within each first-best rating category obtained by splitting the sample randomly. The propensity scores for units in each half are predicted using the model estimated with the other half. The ordered probit specification contains the variables: cpn, mat, prof, cov, solv, size and size^2.

Table B.2
Estimates in the full sample obtained using the augmented weighting estimator.

n_0	n_1	estimate	se (*p*-val.)
34	8	25.0	26.1 (0.337)
34	9	22.2	23.3 (0.341)
34	10	26.3	21.5 (0.219)
34	11	20.7	20.4 (0.311)
36	11	22.1	20.4 (0.280)
37	11	23.4	20.6 (0.255)
39	11	20.4	20.4 (0.315)
39	12	16.3	19.4 (0.401)
40	12	15.4	19.3 (0.427)
40	13	14.7	17.8 (0.410)
42	13	14.1	17.7 (0.427)
42	14	15.3	16.5 (0.353)
42	15	15.3	15.4 (0.318)
43	15	14.7	15.4 (0.339)

NOTE: The table presents the number of controls (n_0), the number of treated units (n_1) and the estimates obtained using the augmented weighting estimator in symmetric subsamples around the eligibility threshold, constructed using the full sample. The standard errors are calculated using the M-estimation-based analytical formulae.

Table B.3
SBs of the covariates in the subsamples of Table B2.

n_0	n_1	cpn	mat	prof	cf	liq	cov	lev	solv	size	age	ltdebt
34	8	−0.39	1.02	−0.47	−0.49	−0.38	−1.87	0.73	1.25	−1.82	−0.42	0.21
34	9	−0.86	0.76	−0.52	−0.77	−0.66	−2.09	0.96	1.50	−2.17	−0.34	0.18
34	10	−1.05	0.62	−0.48	−0.73	−0.61	−2.21	1.10	1.48	−1.85	−0.04	0.19
34	11	−1.36	0.56	−0.39	−0.50	−0.45	−1.84	1.35	1.33	−2.12	−0.34	0.55
36	11	−1.50	0.54	−0.39	−0.55	−0.59	−2.01	1.45	1.16	−2.14	−0.27	0.63
37	11	−1.58	0.48	−0.38	−0.52	−0.55	−2.04	1.53	1.21	−2.17	−0.27	0.71
39	11	−1.64	0.46	−0.38	−0.52	−0.53	−2.02	1.45	1.32	−2.18	−0.35	0.62
39	12	−2.00	0.43	−0.30	−0.44	−0.64	−1.44	1.33	0.25	−2.42	−0.34	0.56
40	12	−2.04	0.46	−0.30	−0.45	−0.68	−1.53	1.39	0.20	−2.42	−0.40	0.61
40	13	−2.26	0.41	−0.30	−0.42	−0.64	−1.68	1.42	0.16	−2.18	−0.51	0.58
42	13	−2.26	0.43	−0.30	−0.45	−0.62	−1.62	1.08	0.33	−2.19	−0.42	0.25
42	14	−2.58	0.09	−0.18	−0.60	−0.79	−0.06	0.34	0.19	−2.43	−0.59	−0.30
42	15	−2.89	0.17	−0.81	−0.86	−0.99	−0.41	−0.07	0.64	−2.75	−0.84	−0.63
43	15	−2.95	0.18	−0.80	−0.84	−0.98	−0.41	−0.04	0.62	−2.77	−0.84	−0.60

NOTE: The table presents the number of controls (n_0), the number of treated units (n_1) and the standardized bias of each covariate in symmetric subsamples around the eligibility threshold, constructed using the full sample.

Table B.4
Estimates in the full sample with the *p*-values of a one-sided hypothesis test

n_0	n_1	estimate	se (*p*-val.)
Panel A. Weighting est.			
25	4	15.9	20.2 (0.038)
25	5	12.8	19.4 (0.046)
27	5	10.2	18.7 (0.053)
27	6	−3.4	18.5 (0.185)
28	6	2.4	20.2 (0.134)
Panel B. Aug. weighting est.			
28	6	35.1	29.9 (0.033)
28	7	29.7	25.6 (0.026)
29	7	18.4	26.5 (0.074)
30	7	17.1	26.5 (0.081)
32	7	11.2	26.1 (0.116)
33	7	23.9	28.7 (0.063)
33	8	29.2	25.9 (0.029)
34	8	25.0	26.1 (0.042)

NOTE: The table presents the number of controls (n_0), the number of treated units (n_1) and the estimates obtained using the simple weighting estimator (Panel A) and the augmented weighting estimator (Panel B) in symmetric subsamples around the eligibility threshold, constructed using the full sample. The standard errors are calculated using the M-estimation-based analytical formulae. The *p*-values refer to the hypothesis test with H_0: $\Delta^{DE} \leq -20$ and H_a: $\Delta^{DE} > -20$.

Table B.5
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: The table presents the number of controls (n_0), the number of treated units (n_1) and the standardized bias of each covariate in symmetric subsamples around the eligibility threshold, constructed using the bonds issued in March 1 – December 31, 2018.

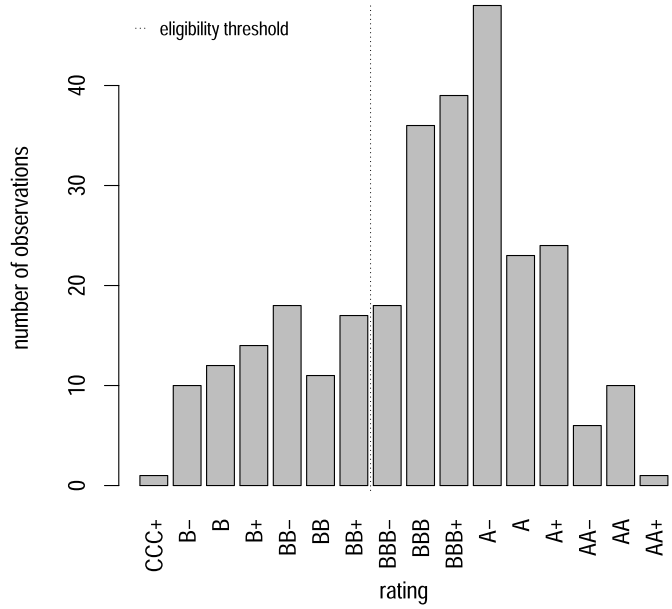

Fig. B.2 Number of bonds issued in Mar. 1 – Dec. 31, 2018 by rating. NOTE: The figure illustrates the number of observations within each first-best rating category, for the bonds issued between March 1 and December 31, 2018.

Table B.6
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: The table presents the number of controls (n_0), the number of treated units (n_1) and the standardized bias of each covariate in symmetric subsamples around the eligibility threshold, constructed using the bonds issued in Latvia, France, Slovenia, Italy, Belgium, Estonia, Austria, the Netherlands and Slovakia.

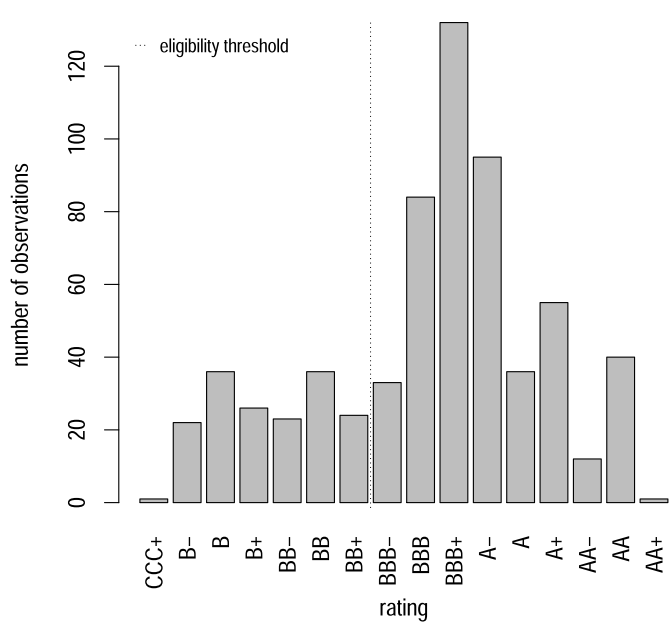

Fig. B.3 Number of bonds issued in high LTI-share countries by rating. NOTE: The figure illustrates the number of observations within each first-best rating category, for the bonds issued in Latvia, France, Slovenia, Italy, Belgium, Estonia, Austria, the Netherlands and Slovakia.

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