

The Effects of Corporate Purchase Programmes on Market Power

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Abstract

Does unconventional monetary policy affect the market power of firms? I address this question both empirically and theoretically. Comparing firms purchased under the European Central Bank's Corporate Sector Purchase Programme with eligible but unpurchased ones in a staggered difference-in-differences design, I show that a purchase raises a firm's markups by 7.1 percent. The transmission channel runs from issuing conditions to capital: purchased bonds are issued at a 22 basis point lower yield to maturity, and purchased firms invest 2.2 percentage points more. These effects materialise only under two conditions: purchases must be repeated over the years, and they must happen within a week of the bond issuance. A general equilibrium model rationalises these findings, combining endogenous markups and financial frictions. In a segmented bond market, a purchase raises the price of the very issue it absorbs, relaxes the issuer's capital constraint, and thus raises its output, market share and markup. A quantitative version compares different purchase designs: several, including the ECB's own with its deviations from market neutrality, deliver almost the same welfare gain, while concentrating purchases on a few large firms or directing them to the smallest eligible ones reduces it.

Keywords: Unconventional Monetary Policy, CSPP, Financial Frictions, Market Power, Endogenous Markups.

JEL Classification: D4, E44, E52, E58, G32, L1

1 Introduction

After the Great Financial Crisis and the Sovereign Debt Crisis, the European Central Bank (ECB), like other central banks, turned to unconventional monetary policy to stimulate and stabilise inflation and aggregate economic activity. This paper focuses on one component of this policy, the Corporate Sector Purchase Programme (CSPP), which involves purchasing corporate bonds to strengthen the pass-through of asset purchases to the financing conditions of the real economy (European Central Bank, 2016b). The ECB has become a major player in the Eurozone’s corporate bond market, holding up to 27% of all bonds eligible for the programme (Appendix Figure A-1, from Corell et al. (2025)). A key aspect of the CSPP is its intention to implement purchases in a (financial) market-neutral way (European Central Bank, 2017, p. 40; Draghi, 2018). To do so, the ECB guides its purchases with an internal benchmark that allocates them across issuer groups in proportion to their eligible outstanding bonds.

However, two main concerns suggest that such policy design might fail to achieve market neutrality. First, the ECB allows itself room to deviate from the benchmark guideline (Draghi, 2017a). While sensible for responding to different shocks, sustained deviations could be a source of distortion, and this has been the case over time across countries, sectors and rating buckets (Appendix Figure A-2). The second one is the set of criteria imposed on bonds to be considered eligible for the CSPP. There are several, but the most important criterion for non-neutrality is having an investment-grade rating (Appendix A.1). While sensible for limiting the credit risk on the Eurosystem’s balance sheet, this requirement implies that, within the universe of bond-issuing firms in Europe, only a small subset of big corporations could be part of the stimulus. First, even among listed non-financial groups, more than 63% had never participated in the bond market as of 2018 (Cappiello et al., 2021). Importantly, Darmouni and Papoutsis (2025) document that only 15.5% of euro-area bond issuers carry an investment-grade rating, and that such firms are concentrated at the very top of the size distribution: 39% of the issuers in the top quartile by total assets are investment grade, against less than 3% in each of the other three quartiles. These firms sit at the top of the size distribution, which is exactly where the literature on market power locates the global rise of markups (De Loecker and Eeckhout, 2025). In the euro area, Kouvavas et al. (2021) find that reallocation toward high-markup firms drives the aggregate increase while the median firm’s markup stayed basically unchanged (Appendix A.6). This trend has severe consequences for the labour and capital share and for startup rates (Autor et al., 2020; Barkai, 2020; De Loecker et al., 2026). The natural question arising from the CSPP’s design is whether purchasing bonds from this specific group of firms can imply unequal effects on input costs through financing, and thus whether this can affect the markup distribution.

This paper first shows empirically that the CSPP affected firms’ markups. Exploiting publicly available data, I can identify which bonds the ECB purchased. With this information, I find that bonds absorbed by the Eurosystem were priced with a lower yield to maturity at issuance than similar bonds eligible for the programme. Second, leveraging firm-level data, I examine whether this

price advantage translated into real variables of their issuers. Comparing firms reached by the CSPP against eligible but not purchased ones, I find that persistent purchases entail a higher investment rate and higher markups. In other words, the paper finds evidence on the entire transmission channel of the policy: from improved bond pricing, to issuers' capital and investment, to finally pricing power in the product market. These results are consistent with predictions from both theoretical models of unconventional monetary policy and of endogenous markups and market power. Indeed, the second part of the paper builds a three-period general equilibrium model that aims to rationalise and replicate the empirical findings, combining exactly these two classes of models with a segmented bond market, in which purchases move the price of the issues they absorb. Finally, I quantify the proposed theoretical framework to evaluate the effects of potential policy designs. This exercise shows that the current design delivers among the highest welfare gains.

The first contribution of my work is empirical: it studies the effects of actual CSPP purchases, rather than its announcement, over the programme's full life and within the set of firms it could reach, i.e., comparing purchased with eligible-but-not-purchased units. A few papers have looked at this purchase margin before, in the euro area and for the Federal Reserve's corporate purchases, but on short windows, stopping at financing conditions or finding no real response. To go beyond this, I identify from the ECB's daily eligible-assets lists and weekly portfolio publications which bonds were eligible, which were purchased and when, at the issuer-group level the ECB uses for its benchmark, separating purchases within a week of issuance (a proxy for primary market acquisitions) from later purchases on the secondary market. The resulting security-level dataset is complemented with bond-level data on all Euro-denominated corporate bonds and firm-level information on their issuers. An interesting feature of the policy implementation that emerges from the constructed datasets is that purchases are persistent: a purchased firm has a 0.82 probability of being repurchased at the next issuance; this feature is itself a treatment property and will be central to the empirical and theoretical results.

Studying actual purchases also requires identifying their effects, since CSPP purchases are not randomly assigned and the paper's empirical design does not assume otherwise. Instead, identification relies on the variation in purchases that the ECB's allocation rule and observable characteristics leave unexplained. To do so, I combine CSPP eligibility information with bond-level data to reconstruct the benchmark rule the ECB used internally to guide its purchases, which previous studies do not use.¹ This measure serves two purposes: it shows that the rule predicts which firms the ECB purchases – but not when it purchases them – and it enters as a control in every regression, so that the estimated effects come from departures from the rule rather than from the rule itself. Beyond the rule, observable firm and bond characteristics explain little of the remaining variation in purchases, and the examined firms' past outcome variables do not predict purchase timing.

¹ The benchmark is defined at the issuer-group level as the ratio of the nominal outstanding amount of eligible corporate bonds issued by the group to the entire eligible universe, so that it reflects all eligible outstanding issues proportionally through market-capitalisation weighting; the purchase limits that apply to each issuer group are based on it (European Central Bank, 2016a,b; Draghi, 2017b).

The second contribution is also empirical: it estimates the described purchase effects on firms’ market power and provides evidence for the transmission channel behind this outcome. Market power is only one possible consequence of the policy’s non-neutralities; for example, another, already examined in the literature, is the effect on climate ([Schnabel, 2021](#)). On market power, instead, the ECB’s own assessment is that the effects of its corporate bond purchases on competition and market concentration are “expected to have been minimal at most” ([Lagarde, 2021](#)). The available evidence points the other way: [Acharya et al. \(2025\)](#) find that the US firms subsidised through QE-induced portfolio rebalancing gained market share at the expense of their rivals, and [Adelino et al. \(2023\)](#) document that CSPP-eligible suppliers expanded their customer base and raised the concentration of their industries; neither of them, however, observes a purchase. The analysis starts with two hypotheses. The first is that the CSPP improves the pricing of the purchased bonds, which in turn allows the firm to increase its capital investment and, only then, its pricing power in the product market. The other is that the real effects should be driven mainly by primary market purchases, since this is when funds go directly to the issuing firm. The three main empirical analyses that follow test them in the order of the chain: bond pricing, capital, markups.

First, I run a cross-sectional bond-level analysis to test whether bonds the ECB purchased at issuance were priced differently from comparable but unpurchased bonds. Identification compares bonds of the same issuer, rating, market conditions, and eligibility status, so the estimate complements the announcement effects on the entire corporate bond market documented in the literature; every remaining design imperfection pushes it toward zero, so it can be read as conservative. The main empirical finding in this part of the paper is that bonds purchased at issuance have a lower yield to maturity ranging from 17 to 25 basis points. This effect is about a fifth of the spread over the safe rate that these issuers pay on average at issuance (around 120 basis points). This result is stable and robust to conditioning on prior purchases.

Then, I test whether these improved financing conditions translate into real variables. The hypothesised connecting variable is firms’ capital stock and investment, as in the main theoretical models of unconventional monetary policy. The same channel is the one studied by the empirical literature on the investment response to monetary policy, which finds that the firms responding the most are those with the safest balance sheets and the most liquid assets ([Ottonello and Winberry, 2020](#); [Jeenas, 2026](#)); the firms purchased by the ECB, large and investment grade, are exactly this type of firms, and the result below is in line with that evidence. In the firm panel, I restrict the sample to firms that issued at least one eligible bond during the policy period, so that purchased firms are compared with the comparable firms the programme could have reached ([Darmouni and Papoutsis, 2025](#)). I employ the local-projections difference-in-differences (LP-DiD) estimator of [Dube et al. \(2025\)](#), built for staggered, non-absorbing treatments with heterogeneous and dynamic effects: the coefficient at each horizon is identified from firms that remain purchased through that horizon, against firms whose purchase status does not change over the same window. The results of this empirical section support the hypothesis: issuers that are continuously purchased at least once a

year increase their investment rate by 2.2 percentage points (pooled estimate).

The paper then turns to its main question, whether this unequal treatment of financing costs moves the markup distribution. It is important to note that affecting markups is not necessarily welfare-reducing, since in principle the policy could have reduced the dispersion of their distribution. Using the same framework as the capital investment enquiry, the results show that firms reached by the CSPP increased their (gross) markup level by 7.1 percent; for example, the average firm in the sample starting with a markup level of 1.48 increases it to about 1.58.

Finally, the empirical part also reports four interesting null results from the transmission-channel analysis. First, the bond-pricing analysis does not detect spillover effects, in the sense that no effect of a purchase on the same issuer's later yields can be established. Second, I identify two necessary conditions for CSPP purchases to affect real variables, confirmed for both capital investment and markups: only primary market purchases have an effect, while secondary ones do not, and real variables respond only when such primary purchases are repeated over time, while a single purchase has no significant effect. Finally, the announcement of the programme has no detectable effect on capital or markups, in line with the small real response to eligibility documented by previous studies (Grosse-Rueschkamp et al., 2019; De Santis and Zaghini, 2021).

To rationalise the narrative depicted by the empirical part of the paper, I develop a real three-period general equilibrium model, where heterogeneous firms are subject to financial frictions that determine their capital investment and thus their size, as in the literature started by the seminal papers by Bernanke et al. (1999) and Cooley and Quadrini (2001). I then incorporate two key ingredients from two separate strands of literature. First, these firms compete strategically in their own product market, as canonical in models featuring endogenous markups (Atkeson and Burstein, 2008; De Loecker et al., 2026). Second, borrowing from the literature on unconventional monetary policy (Gertler and Karadi, 2011; Papoutsis et al., 2022), the economy includes a central bank that purchases firms' bonds to stimulate economic activity by alleviating financial frictions.

Specifically, the model follows the same decision sequence twice across the three periods. Firms with heterogeneous productivity and initial net worth finance capital by issuing bonds that a private intermediary holds at a cost that increases with its portfolio position (as in Papoutsis et al., 2022), which can be considered a reduced form capturing investors' habitat preferences (Vayanos and Vila, 2021). This segments the bond market: each issue's price depends on how much the private intermediary must absorb, so each firm's user cost of capital embeds a spread over the frictionless rate that increases with its leverage. In the second period, capital is financed by retained earnings, new bonds, or both. In both periods, firms make production decisions competing oligopolistically within the same market. The corporate purchase policy has two elements: an announced share bought from every eligible issue, priced in at issuance, and surprise deviations for some firms at the first purchase, repeated at the next (with persistence set to the measured repurchase probability mentioned earlier). The policy has real effects by reducing the private intermediary's position, raising the price of the purchased issue, relaxing the financing of capital, and finally raising output, market

share and markup, at the expense of within-sector rivals – which is the pattern that [Acharya et al. \(2025\)](#) document for the firms reached by the Federal Reserve’s QE-induced portfolio rebalancing.

The third contribution is theoretical: the model delivers a set of predictions about which purchases can have real effects, which maps directly onto the null results of the empirical section. A purchase moves a firm’s capital only when it is known at issuance: a purchase arriving after a bond has been placed, as in the secondary market, changes neither its sunk quantity nor its realised price, leaving capital, output, and markups unchanged. Similarly, a single purchase has no sizeable effect because it works only through an ex-post price revaluation, without changing financing quantities. In other words, the programme’s real effects come from its deviations from market neutrality, repeated over time. This is also what separates the model from the two existing frameworks of corporate purchases closest to it, the preferred-habitat model of [Gilchrist et al. \(2024\)](#) and the intermediary-based model of ECB purchases of [Holm-Hadulla and Leombroni \(2025\)](#): both price the purchases through the investors that absorb them, but on a representative security or for price-taking firms, so that the allocation of purchases across firms has no consequence for market shares and markups.

In the last part of the paper, I quantify the proposed model using the empirical estimates and data features from the empirical section. Specifically, I calibrate it to my first empirical result on the yield effect of purchases and to two additional moments of the data: the spread at bond issuance and firms’ dividend payout sensitivity. The model is then validated against the remaining empirical findings. First, it also replicates quantitatively the non-detectable response of real variables to a single purchase. Moreover, the capital response is within the empirical confidence interval but below the point estimate. This stems from a structural limitation of the framework. While it matches the programme’s size and the measured share of eligible issuances, the model has only two purchase events, whereas the empirical results average seven to eight purchases over three years. Finally, the markup response is understated by construction, so every market-power effect is a lower bound.

The fourth contribution is the comparison of purchase designs: I use the quantified model as a laboratory to compare different policy designs, in the spirit of the comparisons of purchase rules across sectors in the climate-tilt literature ([Nakov and Thomas, 2026](#)), but across firms with product-market power: I compare eight possible proposals with the programme as implemented, i.e. the announced share plus the measured deviations and their persistence. The possibilities range from a no-deviation case, targeting mid-size firms (the smallest eligible ones) or large firms (the entire fourth quartile of the size distribution), only the 10% biggest producers or the most financially constrained ones, to increasing the set of eligible issuers for purchase. They deliver stark differences in their aggregate outcomes. The results on output and average markups are intuitive: targeting the most constrained firms yields the largest output increase, purchasing from the smallest ones reduces the economy-wide markup, while purchasing from the top 10% yields the smallest output increase and the highest markup. With the caveat about the undersized markup effect, a surprising result is that several purchase designs perform almost identically in terms of welfare effects, including the current ECB design, which deviates from market neutrality. A decomposition analysis shows

that this is due to different implementation schemes balancing different drivers: the extra output delivered and the disutility of the extra work that it implies, the social cost of the extra capital, and the private and public financial holding costs.

Related literature. This paper relates to five strands of literature. The first is the empirical literature on corporate purchase programmes, most of which identifies the effects of the CSPP from the eligibility margin around its announcement: eligible yields fall and issuance rises ([Todorov, 2020](#); [Abidi and Miquel-Flores, 2018](#); [Pegoraro and Montagna, 2025](#)), eligible firms substitute bonds for bank loans while their real investment responds little ([Grosse-Rueschkamp et al., 2019](#); [De Santis and Zaghini, 2021](#)), and the credit freed at the banks reaches smaller firms ([Arce et al., 2021](#); [Betz and De Santis, 2022](#)). Fewer papers look at the actual purchases, and they stop at financing conditions: [Zaghini \(2019\)](#) finds a discount for the bonds bought in the primary market in the programme’s first two quarters, [Galema and Lugo \(2021\)](#) and [Bats \(2020\)](#) show that targeted issuers lean further into market debt, and [de Haes et al. \(2025\)](#), [Gilchrist et al. \(2024\)](#) and [Li and Ringgenberg \(2026\)](#) find small or no price effects and no real response of the purchased firms. On the indirect channel, [Selgrad \(2026\)](#) shows that the Fed’s Treasury purchases lower corporate yields through portfolio rebalancing and that exposed issuers borrow and invest more. Closest to my question are [Acharya et al. \(2025\)](#) and [Adelino et al. \(2023\)](#), discussed above, neither of which observes a purchase. This paper follows the actual purchases over the programme’s life, against the benchmark rule that guided them, from the issue price to the capital and the markup of the purchased firm.

The second is the literature on market power: the rise of markups and its concentration in the upper tail ([De Loecker et al., 2020](#); [De Loecker and Eeckhout, 2025](#)), which in the euro area comes from a reallocation towards high-markup firms while the average markup within European firms has stayed flat ([Kouvavas et al., 2021](#); [Bighelli et al., 2023](#)); the way market power shapes the transmission of monetary policy ([Mongey, 2021](#); [Baqaee et al., 2024](#); [Meier and Reinelt, 2024](#); [Chiavari et al., 2026](#)); and, closest to my mechanism, the evidence that less constrained firms charge higher markups and gain market share from their rivals ([Li et al., 2026](#); [Frésard, 2010](#)). Markups are measured with the production approach of [De Loecker and Warzynski \(2012\)](#), an input rather than a contribution. This paper turns the transmission question around: it asks whether a monetary policy instrument, by choosing whom to fund, moves the markup distribution, and provides a firm-level causal estimate of this effect.

The third is the literature on financial frictions and the investment channel of monetary policy, from the external finance premium of [Bernanke et al. \(1999\)](#) and [Cooley and Quadrini \(2001\)](#) to the heterogeneous responses by financial position of [Ottonello and Winberry \(2020\)](#) and [Jeenas \(2026\)](#) and, in the euro area, the margin between bank and bond finance ([De Fiore and Uhlig, 2011](#); [Darmouni et al., 2022](#); [Darmouni and Siani, 2025](#)). This paper adds the next link in the chain, from the financing advantage delivered at issuance to the firm’s product-market position, and shows why only purchases anticipated at issuance and repeated reach the capital stock.

The fourth is the theoretical literature on unconventional monetary policy, in which purchases of private assets relax financial intermediaries' constraints (Gertler and Karadi, 2011, 2013; Cúrdia and Woodford, 2011; Sims and Wu, 2021) and their price effects rest on imperfect substitutability and holding costs (Vayanos and Vila, 2021; Papoutsis et al., 2022; Selgrad, 2026). The composition of purchases across firms and sectors is studied by the climate-tilt literature (Nakov and Thomas, 2026; Ferrari and Nispi Landi, 2024; Giovanardi et al., 2023), by Kurtzman and Zeke (2020), and by the two models of corporate purchases closest to mine, Gilchrist et al. (2024) and Holm-Hadulla and Leombroni (2025). This paper puts the purchase programme into an oligopoly economy, so which firms are bought, how often, and how much matter for market shares and markups, and it compares alternative designs on welfare.

Finally, the paper builds on the oligopoly macro literature, where markups respond to market shares through the nested-CES Cournot structure of Atkeson and Burstein (2008), taken to general equilibrium with heterogeneous firms by Edmond et al. (2015), De Loecker et al. (2026), Burstein et al. (2025) and Deb et al. (2024), with the strategic interaction confined to the production stage. When a dynamic choice is itself strategic, the equilibrium is Markov perfect and solved with few firms (Wang and Werning, 2022; Mongey, 2021; Maskin and Tirole, 1988; Ericson and Pakes, 1995); the dynamic-games literature distinguishes the open-loop equilibrium, in which firms take their rivals' plans as given, from the closed-loop one, in which they internalise the rivals' response to their own capital and accumulate more (Fudenberg and Tirole, 1983; Reynolds, 1987; Fershtman and Kamien, 1987; Reinganum, 1982), and Doraszelski et al. (2022) compute a dynamic oligopoly with financing constraints in partial equilibrium. This paper adds a financing stage to the Cournot block in general equilibrium, with firm-specific bonds whose price depends on who absorbs them, so that buying some issues and not others reallocates market share within sectors. Capital is chosen as a Nash equilibrium in plans, the open-loop solution, which keeps a twelve-firm sector with a financing stage tractable; the quantitative part measures the term that the closed-loop solution would add.

Outline. The remainder of the paper is organised as follows. Section 2 presents the empirical analyses and their results. Section 3 describes the analytical model developed to rationalise these empirical findings. Section 4 quantifies the model, validates its performance and compares different purchase designs in terms of aggregate output, markup and welfare. Section 5 concludes.

2 Empirical Analysis

This section tests the two hypotheses of the introduction in the order of the transmission channel suggested by the main theoretical models of unconventional monetary policy (e.g. Gertler and Karadi, 2011): the pricing of purchased bonds, firms' capital investment, markups; and whether only purchases at issuance, when the funds reach the firm, move real variables.

2.1 Data

Identifying CSPP purchased bonds. The first step in constructing the dataset is to identify which bonds (and thus which firms) have been part of the CSPP. To do so, the paper exploits publicly available weekly updates published by the ECB on the portfolio of securities currently held.² There is one main limitation implied by this approach: the paper can identify which bonds have been purchased (extensive margin) but not the purchased amounts (intensive margin). Moreover, another important caveat is that the programme started on the 8th of June 2016, but the first available publication is dated only the 23rd of June 2017. I differenced consecutive portfolio snapshots (by ISIN code) to identify every security that entered the programme and the week it did so. Thus, each portfolio entry is classified in one of the following three categories:

- Issuance-week purchases: bonds purchased within a week of issuance;
- Secondary market purchases: bonds purchased more than a week after issuance;
- Initial-portfolio bonds: bonds already part of the first available publication.

This approach identifies 2,395 purchased bonds: 1,110 observed issuance-week purchases, 476 observed secondary purchases, and 809 initial-portfolio bonds. Notice that bonds in the third category have imputed - instead of observed - purchase timing. Thus, all subsequent analyses will use only observed purchases, treating the initial-portfolio-inclusive variant as a robustness check.

Identifying CSPP-eligible bonds. The second key step is to define the group of eligible securities for the programme. To do so, I exploit daily ECB publications on the entire universe of assets eligible for collateral by the Eurosystem.³ All these data are then filtered by issuer group (non-bank corporate), asset type and remaining maturity, in order to be in line with CSPP-specific eligibility criteria. A bond is defined as eligible if it passes this filtering within a window of $[-30, +90]$ days from its issuance date; validated against the purchased bonds identified above, this definition codes 99.1% of issuance-week purchases as eligible. Commercial paper is excluded from the headline eligible set because a March 2020 rule made it about 90% of the eligible universe thereafter (Appendix B.1); a commercial paper-inclusive alternative serves as a robustness check.

Dataset construction. The datasets are built from Datastream. First, I downloaded information on all Euro-denominated bonds issued by non-financial companies between 2010 and 2022. Then, to estimate the policy’s effect at the correct corporate level, I manually linked each asset to the issuing company’s main parent (e.g., bonds issued by “Mercedes-Benz International Finance BV” are linked to “Mercedes-Benz Group AG”). From the bonds issued and outstanding by issuer and year, I construct the “CSPP benchmark”, the announced purchase guideline used as a control throughout the empirical analysis (Section 2.2): the ratio of eligible outstanding bonds for firm i in period t over the total eligible outstanding bonds of all issuers in the data in the same period t .

² Available at <https://www.ecb.europa.eu/mopo/implement/app/html/index.en.html>.

³ Available at <https://www.ecb.europa.eu/paym/coll/assets/html/list-MID.en.html>.

Finally, I downloaded firm-level data for all identified firms to construct a panel dataset spanning 2010 to 2024.

These steps produce two datasets: a cross-sectional dataset at the bond level and a firm-year panel dataset. More specifically, the first merges security-level information with issuer characteristics (sector and country), along with the eligibility and purchase flags. The sample is then restricted as follows: financial, utility, real-estate, and public-administration sectors are excluded; only bonds with rating information (including a “Not rated” category) are selected; outliers are trimmed at the 1% level. Appendix Table A-3 reports descriptive statistics for the resulting sample: 4,255 bonds from 657 issuer groups, of which 16.4% were purchased at issuance.

The firm-level panel starts from the same universe of issuers. Firm-year treatment, eligibility and issuance dummies are merged in from the constructions above, together with the CSPP “benchmark” control. Nominal variables are deflated with a country-by-sector value-added deflator (Appendix B.5). This allows markups to be estimated with the production approach of De Loecker and Warzynski (2012): the markup is recovered as the output elasticity of a flexible input (in this case, cost of goods sold COGS) times the inverse of that input’s revenue share. This output elasticity is one Cobb-Douglas coefficient per NACE-2 sector, held constant over time; Appendix B.5 gives the details.

The resulting dataset contains 8,554 firm-year observations for 769 firms. Of these, 180 firms issued at least one eligible bond (referred to as “ever-eligible” hereafter), and 136 firms ever received an issuance-week purchase (159 ever received any purchase). Appendix Table A-4 reports descriptive statistics, divided into two panels by the outcome variable in each analysis. Almost every firm-year in both samples pays dividends (97 percent), and 98 percent of purchased firms pay in the year before their first purchase; this fact matters for the model in Section 4.

Some facts about the purchase process. Two facts about the implementation of the CSPP emerge from the bond-level dataset, restricted to issuance-week purchases: 2,252 eligible issues by 340 issuer groups, of which 1,083 were purchased in their issuance week. Table 1 reports them. Panel A shows that issuance-week purchases are common but not automatic: the unconditional probability is 0.481 over the dataset’s time span. The likelihood differs across years, however, in line with the ECB’s decisions.⁴

Additionally, Panel B shows that these probabilities depend heavily on the issuer’s own history within the CSPP. An issuer’s first eligible issue is purchased with probability 0.574.⁵ Regarding persistence, if the previous one was purchased, the next one is purchased with probability 0.760, increasing up to 0.903 for issues less than ninety days apart. On the other hand, if the CSPP did

⁴ In the first observed years of the policy (2017–2018) the chance of being purchased is high; it collapses in 2019, when net purchases were suspended, rises sharply again in 2020 with the PEPP, then declines over the next two years before collapsing again in 2023, when reinvestment replaced net purchases.

⁵ This probability exceeds the unconditional 0.481 because later issues come disproportionately from issuers in the unpurchased state, a composition effect rather than a puzzle.

Table 1: Issuance-week purchases, unconditional and by the previous issue’s status

	Issues	Purchased	Share
<i>Panel A. Eligible issues and issuance-week purchases, by year</i>			
2017 (from 1 July)	109	83	0.761
2018	235	162	0.689
2019	299	29	0.097
2020	411	308	0.749
2021	309	204	0.660
2022	277	159	0.574
2023	291	80	0.275
2024	321	58	0.181
Total	2,252	1,083	0.481
<i>Panel B. Issuance-week purchase probability, by the issuer’s previous issue</i>			
First eligible issue of the issuer	340	195	0.574
Previous issue purchased in its issuance week, any gap	957	727	0.760
within 1 year	762	627	0.823
within 90 days	495	447	0.903
Previous issue not purchased in its issuance week, any gap	955	161	0.169
within 1 year	843	126	0.149
within 90 days	531	55	0.104
Last two issues both purchased in their issuance week	637	493	0.774

Eligible bond issues other than commercial paper, by any issuer, issued from the 1st of July 2017, the first date at which an issuance-week purchase can be observed in the ECB’s weekly holdings publications; 2,252 issues by 340 issuer groups. An issue is purchased if it entered the CSPP portfolio within seven days of issuance, with observed timing. Panel A: the share of issues purchased, by year of issuance; the 2019 rate reflects the suspension of net purchases between January and October 2019. Panel B: the previous issue is the same issuer’s immediately preceding eligible issue; gaps are measured between issue dates; rows overlap by construction. Appendix Table A-5 gives the three-way split (bought at issuance, bought later, never bought). Source: ECB CSPP holdings and eligible-assets publications; author’s construction.

not select the previous one, the following security is purchased with probability 0.169, decreasing to 0.104 within the ninety-day window.⁶ Notice that part of this persistence is structural: issuers are consolidated at the group level, and large groups have more subsidiaries, more issues and therefore more purchase events.

2.2 Identification of Purchase Effects

The statistics just reported raise two questions about the identification assumptions underlying the entire empirical analysis. The focus of the analysis is on the effects of CSPP purchases within eligible bonds and firms, i.e. comparing purchased versus eligible-but-not-purchased units. The first worry is level selection: the ECB buys according to a rule that favours some issuers, so purchased and non-purchased units may differ systematically. The second is timing selection: the central bank could time the purchases of a given firm according to that firm’s outcome path,

⁶ Including the initial-portfolio bonds lowers the first-issue rate to 0.331, entirely through imputation uncertainty; the conditional rates of Panel B, which require an observed previous purchase, stay at 0.822–0.855 at the one-year horizon across every window, with the purchased-minus-unpurchased gap within 0.654–0.673.

which is the variation the panel analyses exploit. Identification therefore relies on the variation in purchases that the ECB’s allocation rule and observable characteristics leave unexplained. Three checks compose the identification argument for the treatment margin: the rule explains who is purchased in the cross-section, but not when (levels, differenced and controlled); the rule is followed only approximately (deviations); the residual variation is not driven by outcomes and observable characteristics (selection-into-purchase diagnostics).

Benchmark purchasing rule. Appendix Table A-6 decomposes the level-selection concern into its levels and timing components (a decomposition, not an instrumental-variable first stage, since the benchmark is not used as an instrument). The purchase dummy (linear probability model, LPM) and the number of purchases in a given year (dose count) are regressed on the lagged benchmark share, each under two fixed-effect structures. In the cross-section (within country and sector×year fixed effects), the lagged benchmark share strongly predicts *which* firms are purchased: both the linear probability model and the dose count give high point estimates and cluster-robust F-statistics higher than 26. Within firm, however, the disclosed purchasing criterion does not predict *when* a given firm is purchased: with firm and year fixed effects, neither coefficient is significant. These results imply that identification of the policy effects comes from within-firm variation. Two remarks complete this decomposition. First, the cross-sectional margin is neutralised. In the panel analysis, the long differences remove any fixed purchase-scale component of the firm; moreover, in both the bond- and firm-level exercises, the lagged benchmark share sits in the control set of every specification. Second, had the rule determined timing, the timing variation would be either absent conditional on the benchmark control or contaminated by whatever drives the benchmark within the firm; the null result implies that the variation exploited comes from elsewhere.

Descriptive evidence on deviations existence. The following concern is whether there is any purchase variation left once the benchmark is controlled. To answer this question, I exploit another ECB publication that compares holding shares and eligible-universe shares by country of risk, rating bucket and sector. These data yield clear descriptive evidence of consistent deviations between the two sets, as shown in Appendix Figure A-2 and Table A-1.

Selection-into-purchase at the bond level. Identification of the purchase effects in the cross-sectional analysis on bonds’ yield relies on fixed-effects cells. Even within them, selection could remain: among bonds of the same issuer, rating, market conditions, and eligibility status, the ECB may still have purchased securities that would have priced tightly anyway. Appendix Table A-7 shows that observable design explains almost none of who is purchased (a within- R^2 below one percent), and that the only imbalance, issue size (+0.108 log points, against −0.004 for maturity), is exactly the one the allocation rule announces rather than a symptom of yield-seeking.

Other potential sources of selection from policy design include purchase execution by six national central banks assigned by issuer geography, carried out in calendar phases of net purchases and

reinvestment (European Central Bank, 2016b, 2025). The specification absorbs each of these aspects: the benchmark share as a control, geography interacted with time, and the purchase calendar through the year-interacted effects.

Selection-into-purchase at the firm level. Finally, what the identification design requires is that purchase *timing* does not respond to the firm’s outcome path. Appendix Table A-8 regresses the treatment dummy on lagged firm characteristics: selection is on size and balance-sheet position, a level selection that the long-difference differences out, while selection on the lagged outcomes is absent, and lagged characteristics explain only 8.6 percent of the variation in treatment within country- and sector-year cells and 2.0 percent within firms.

2.3 Testing the Transmission Channel: Bond Issuing Conditions

The transmission’s first step is financial: if the purchase matters, it should show up in the price the firm pays for its bonds at issuance. Thus, this section estimates the effect of an issuance-week purchase on the bond’s own yield to maturity at issuance, in the bond-level dataset described earlier in Section 2.1.

2.3.1 Additional Identification Details

The estimand and the assumption it needs. The counterfactual for a purchased bond is a non-purchased bond of the same issuer, carrying the same rating, issued into the same market conditions, and equally eligible for the programme. How exactly “same market conditions” is defined (e.g. sector-by-year and country-by-year cells in the main specification) is a specification choice that does not affect identification. Let $D_b = 1$ if bond b enters the CSPP portfolio as an issuance-week purchase. Let $Y_b(1)$ and $Y_b(0)$ be the yields at issuance the bond would carry with and without the purchase. The estimand is the average of $Y_b(1) - Y_b(0)$ over purchased issues; the assumption that delivers it is conditional independence within the design’s cells: $Y_b(0) \perp D_b$ conditional on the cells and controls of the specification at hand. Identification rests on unconfoundedness within those cells, not on surprise: anticipation is the mechanism rather than a threat to it, since the Eurosystem’s order is in the book when the deal is priced. Appendix B.3 discusses the pricing effect, eligibility treatment, and residual selection within cells.

The direction of every remaining concern. After the arguments above and in Appendix B.3, four imperfections of the design remain open, and each pushes the estimated effect toward zero. First, from the measurement of treatment: purchases are read from weekly portfolio snapshots, so a bond allocated at the deal but settled after the first snapshot is coded as untreated. This concerns about 4.4% of plausible primary allocations, driven by the day of the week on which a bond happens to be issued (Appendix B.3), not bond quality. Misclassification of this kind (i.e. having some potentially treated bonds in the control set) attenuates the estimate. Second, the size imbalance deferred from the selection discussion admits two readings the data cannot separate: either the

benchmark rule mechanically favours large issues, in which case size is a harmless control and there is no quantity margin; or issuers expecting ECB demand design larger bonds, in which case size is a mediator and conditioning on it removes part of the effect. The analysis states the disjunction without claiming either; under the second, the estimate understates the true effect. Third, from the sample: bonds issued before the programme could never have been purchased and enter the estimation only through the nuisance parameters, and as the eligibility ladder of Appendix B.3 shows, restricting the sample to treatable bonds moves every estimate away from zero. Fourth, from the segment-wide effects documented in the literature: if the programme repriced eligible bonds broadly, then some control bonds are partially treated, and comparing against partially treated controls understates the total effect by construction (as in the first imperfection discussed). The estimates reported below are therefore to be read as conservative; the uncertainty attached to them survives re-derivation by bootstrap and by randomisation methods (Appendix B.3).⁷

2.3.2 Empirical Strategy

I assess whether CSPP purchases affect yield to maturity at issuance using a cross-sectional OLS analysis. Formally, equation 1 describes the regression specification:

$$y_b = \beta D_b + \delta X_b + \alpha_{fe} + \varepsilon_b \quad (1)$$

As anticipated, the outcome variable y is the bond’s yield to maturity (YTM) at issuance. The treatment variable D_b measures exposure to the CSPP; in this case, it equals 1 if bond b enters the CSPP portfolio as an issuance-week purchase. The main fixed-effects set α_{fe} comprises issuer, bond rating, sector×year, country×year, and the discussed eligible×year. Controls are included to refine further identification: lagged CSPP benchmark, (log) maturity and (log) amount issued. Finally, standard errors are clustered at the issuer level, and the data are trimmed at the 1% level to address outliers.

2.3.3 Results

Table 2 reports the results of the specification described above.

An issuance-week purchase lowers the bond’s yield at issuance by 0.217 percentage points (roughly 22 basis points) in the main specification reported in column (1). The remaining columns report other fixed-effects specifications, with the second interacting the bond rating by year, while the following two report the most saturated fixed-effect specifications (i.e., absorbing country-sector-year effects). The estimate is stable across these specifications, between -0.19 and -0.25 percentage points. Column (5) reports the tightest comparison the data allow, purchased versus non-purchased

⁷ The outcome is the raw yield to maturity at issuance rather than a spread over a benchmark curve. Nothing depends on this choice: country-by-year cells, the rating fixed effect and the duration control absorb what benchmarking would perform by construction. Raw yield is also the object the firm actually contracts on, which is what the quantification in Section 4 requires.

Table 2: Issuance-week purchases and the yield at issuance

	(1)	(2)	(3)	(4)	(5)
Issuance-week purchase	-0.217*** (0.081)	-0.195** (0.090)	-0.248** (0.116)	-0.243* (0.133)	-0.169* (0.103)
Issuer	✓	✓	✓	✓	
Issuer × year					✓
Sector × year	✓	✓			
Country × year	✓	✓			
Sector × country × year			✓	✓	
Eligible × year	✓	✓	✓	✓	✓
Bond rating	✓		✓		✓
Rating × year		✓		✓	
Observations	2,920	2,873	2,405	2,361	2,217
R ²	0.908	0.915	0.927	0.934	0.938

Outcome: yield to maturity at issuance, percentage points. *Treatment:* the bond entered the CSPP portfolio within seven days of issuance, observed timing. *Sample:* euro-denominated corporate bonds, 2010–2024, commercial paper excluded, trimmed at the first and ninety-ninth percentile. *Controls* in every column: lagged CSPP benchmark share, log duration, log amount issued. *Rows below the estimate:* fixed effects absorbed. In columns 3 and 4, the effect is identified from sector-country-year cells containing both purchased and non-purchased issues (35.3 percent of purchased bonds; the issuer count falls from 545 to 111); in column 5, from issuer-years with more than one issue (18.1 percent of purchased bonds, 44 issuers; $p = 0.10$, 287 issuer clusters). Standard errors clustered by issuer in parentheses; *, ** and *** denote significance at the 10, 5 and 1 percent level. The main specification is column 1, in bold.

bonds of the same issuer in the same year: -0.17 percentage points, significant at the ten percent level ($p = 0.10$), in line with the other columns. Table A-10 in Appendix B.3 completes the ladder at the coarse end: with year or quarter fixed effects alone, the estimate is small and insignificant, -0.08 and -0.01 , which is the pattern the identification subsection anticipated.

Appendix Table A-16 summarises the entire battery of robustness checks performed on the results just presented. As before, it carries out one perturbation at a time: conditioning on prior purchases, two two-way clustering schemes, broader rating buckets, the fixed coupon subsample, the results controlling for the coupon rate, two alternative yield measures, one employing the benchmark definition which excludes commercial papers (Section 2.1), the widened definition of treatment inclusive of initial-portfolio entries, and the eligibility ladder mentioned in the identification subsection. The estimated effect ranges between -0.18 and -0.29 percentage points across fifty specifications.

2.4 Testing the Transmission Channel: Capital Investment

The next link in the hypothesised chain, from the financing advantage to pricing power, is capital investment.

2.4.1 Additional Identification Details

Sample restriction. The sample is restricted to ever-eligible issuers: firms that at some point issued a CSPP-eligible bond. The comparison is therefore between purchased and non-purchased

firms within the population the programme could reach. This removes the largest and most obvious selection margin, as eligibility itself is a size-and-rating filter (Darmouni and Papoutsis, 2025). Within this restricted sample, Appendix Table A-4 shows that purchased and non-purchased firms are comparable on observables.

Clean-control assumption. The panel comparison assumes that firms whose purchase status does not change over the estimation window are the counterfactual for firms that switch into treatment, conditional on the fixed-effect cells and the lagged controls, and that the dynamic effects of a purchase stabilise after L years. The pre-trend placebos reported with every result are its testable part. Appendix B.4 reports the reasons for choosing the fixed-effects structure and the horizon L .

2.4.2 Empirical Strategy

LP-DiD estimator. The treatment is staggered (firms are first purchased in different years), non-absorbing (a firm can be purchased in one year and not the next), and its effects are plausibly heterogeneous across firms and dynamic over time; under these conditions, the standard two-way fixed-effects regression does not recover a proper average of treatment effects. To avoid such estimation issues, I employ the LP-DiD estimator developed by Dube et al. (2025), which combines the local projection methodology of Jordà (2005) with the “clean controls” condition of Cengiz et al. (2019). For outcome y and horizon h , the estimating equation is

$$y_{i,t+h} - y_{i,t-1} = \beta_h \Delta D_{i,t} + \delta'_h X_{i,t-1} + \alpha^h_{(.)} + \varepsilon^h_{i,t}, \quad (2)$$

where treated units are firms that switch into treatment at t after at least L untreated years and remain treated through $t + h$, and controls are firms with no treatment change over the same window, so that the effect at a given horizon h is identified only through units continuously treated up to that point in time. The estimator is run in a standard form, where the horizon-specific sample is allowed to vary, and in a “no-composition” form balanced across the post horizons; the pooled coefficient is identical across the two by construction, and the main text reports the standard variant. Appendix B.4 details the estimator and its no-composition variant.

Specification. The outcome variable y for this analysis is the investment rate of firms derived from their net capex position; at horizon h , this is defined as

$$\frac{\text{capex}_{i,t+h} - \text{capex}_{i,t-1}}{\text{assets}_{i,t-1}},$$

the h -year change in net capex scaled by total assets in the year before the event year t . The denominator is fixed across horizons, so treated firms’ balance-sheet growth cannot attenuate the

path.⁸ The treatment variable $D_{i,t}$ is the dummy variable for purchase (version without imputed benchmark bond timing). For control variables, I first include nine lagged firm-level variables: log assets, the CSPP benchmark, and, as ratios over assets: bonds outstanding, total debt, total liabilities, cash, gross profit, labour costs, and selling-general-administrative. Then I add two dummies for contemporaneous issuance and eligible-issuance, so that β_h captures the purchase itself, not the (potential) effect of eligibility and issuance. The main fixed effects absorbed are on country and sector $\times$ year; confidence intervals are firm-clustered at the 90% level, and outliers are trimmed at the 1% level. The post window is capped at $h = 2$ because very few firms are continuously treated for longer (Appendix Table A-21), so later horizons are not estimable with meaningful power. For the same reason, the paper’s main estimates are the pooled post coefficient - a variance-weighted average of the various coefficients - and the pooled pre placebo, rather than any single horizon. Finally, I set the L stabilisation parameter to one because the outcome is a flow variable; robustness checks at different values do not change the pooled coefficients and are presented in Appendix B.4.

2.4.3 Results

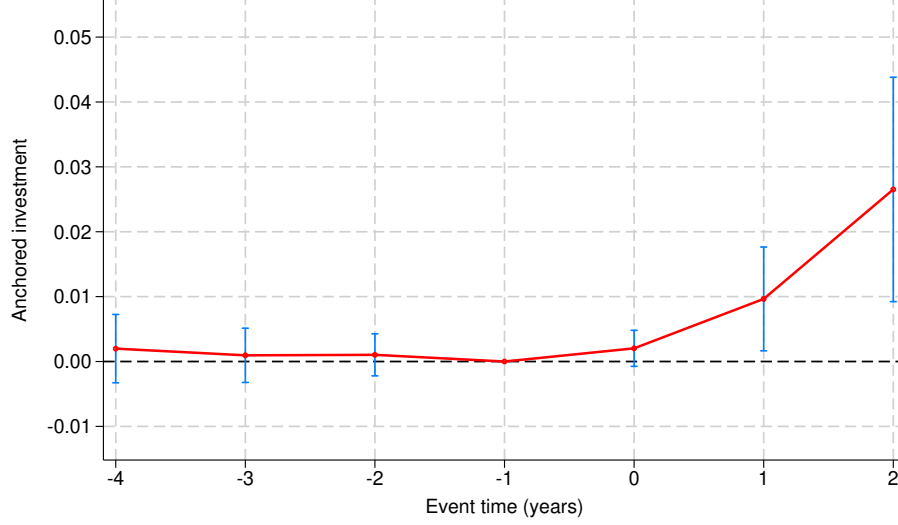
Figure 1 plots the horizon coefficients β_h and Table 3 reports them with the pooled coefficients across the fixed-effects ladder, the headline specification in bold.

The pooled effect is +2.2 percentage points, stable between +1.5 and +2.2 pp and significant across the entire fixed-effect ladder. In contrast, the placebo effects (both pooled and horizon) are not significant for the four years before baseline. The horizon coefficients shed additional light on the transmission mechanism. There is no significant change on impact, but it appears in the year after the purchase (+1.0 pp increase) and increases further two years later (+2.7 pp), consistent with time-to-build dynamics. The balanced version (Appendix B.4) again shows very similar magnitudes, with an even sharper increase once the compositional change is taken out. The cumulative three-year effect estimates a +6.4% increase in pre-event assets, which the quantitative part of the paper uses as its estimate of the purchase’s capital-stock effect.

I run a battery of robustness checks on the results just presented, changing only one element at a time. Appendix B.4 reports all results, and Table A-22 summarises them, showing the pooled estimates. The post-pooled estimate moves between 0.0 and 2.2 percentage points across the nineteen perturbations of Appendix Table A-22 (fixed effects, controls, the treatment and eligibility definitions, the COVID years, autoregressive controls, the stabilisation parameter, and the definition of the treated and control groups), never changing sign, and loses ten percent significance in four of them: the benchmark-inclusive treatment, the outcome blanked in 2020–21, the predetermined eligibility gate, and the lagged outcome in first differences.

⁸ Two estimands are reported throughout: the pooled post coefficient over $h = 0-2$ (the headline, a missing-intolerant window mean) and the cumulative three-year flow over assets in the base year, which approximates the capital-stock change $\Delta K/K$ over the post window and equals three times the pooled coefficient by construction. Both are computed on the same clean sample: they change the estimand, never the power.

Figure 1: Investment rate around an issuance-week purchase



Coefficients β_h of equation (2) by event time, outcome: the change in net capex from the base year scaled by base-year assets, estimated with the non-absorbing LP-DiD at the main specification (country and sector-by-year effects, nine lagged firm controls, the issuance and eligible-issuance dummies, ever-eligible sample, $L = 1$). Whiskers are 90 percent confidence intervals, standard errors clustered by firm. Horizons -4 to -1 are placebos relative to the base year. Point estimates in Table 3.

Table 3: Investment rate around an issuance-week purchase

Investment rate	(1)	(2)	(3)	(4)
Fixed effects	sector, country, year	country, sector $\times$ year	sector, country $\times$ year	sector $\times$ year, country $\times$ year
Pooled post, $h = 0$ to 2	0.015** (0.007)	0.022** (0.009)	0.019** (0.008)	0.018*** (0.006)
Cumulative three years ($\approx \Delta K/K$)	0.044** (0.022)	0.064** (0.028)	0.057** (0.023)	0.055*** (0.019)
Pooled pre placebo, $h = -4$ to -2	0.002 (0.002)	0.000 (0.003)	0.003 (0.003)	-0.001 (0.004)
<i>By horizon, main specification (column 2)</i>				
$h = 0$		0.002 (0.002)		
$h = 1$		0.010** (0.005)		
$h = 2$		0.027** (0.010)		
Observations (post)	727	599	682	530

Outcome: the investment rate, i.e. the change in net capex from the base year scaled by base-year assets, a level ratio, so 0.022 reads as 2.2 percentage points. *Treatment:* issuance-week purchase, observed timing. *Sample:* ever-eligible firms; four pre-periods, two post-periods, $L = 1$. *Controls* in every column: nine lagged firm financials, the lagged benchmark share, the issuance and eligible-issuance dummies. *Estimator:* non-absorbing LP-DiD; the pooled post coefficient over $h = 0$ to 2 is the headline; the cumulative three-year flow over base-year assets is computed on the same sample; the lower block reports the per-horizon coefficients at the main specification. The main specification is column 2, in bold. Standard errors clustered by firm in parentheses; *, ** and *** denote significance at the 10, 5 and 1 percent level. Full battery in Appendix Table A-22.

2.5 Testing Market Power Effects

The final step of the empirical analysis addresses the paper’s main research question: whether CSPP bond purchases affect firms’ markups. As the vast literature cited in the introduction has shown, these are among the best measures of market power available. This is particularly true for the paper’s multi-sector, multi-country dataset, as it lets me sidestep the challenging question of defining each firm’s market.

2.5.1 Identification and Empirical Strategy

The design follows Section 2.4 on capital investment, with two differences. The first and main one is the outcome variable, which becomes firms’ markups (in logs). The second, smaller difference is the value of the L stabilisation parameter for the main results, set to three to be as conservative as possible (since L equals the maximum estimated horizon). Robustness to different values is reported in Appendix B.4.

2.5.2 Results

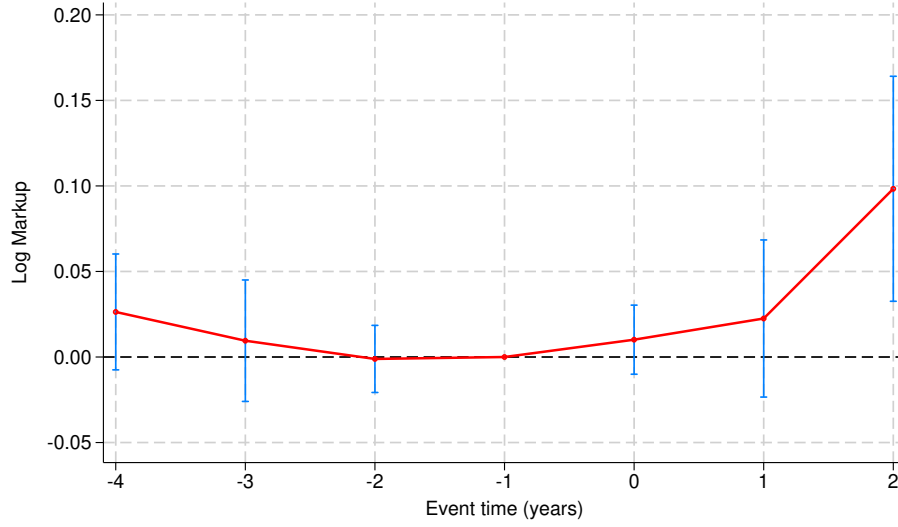
Figure 2 and Table 4 present the results of this last empirical investigation. A firm purchased under the Corporate Sector Purchase Programme raises its markup by a pooled 6.9 log points over the following two years (90% CI [0.020, 0.119]), i.e. by 7.1 percent: for the average firm in the estimation sample, a markup of 1.48 rising to 1.58. All markup effects in the paper are reported in log points and converted at this rate.⁹ The effect is very small and not significant in the same year as the first purchase and in the following year. In contrast, firms that are purchased continuously for three years show a considerable increase in markups of about 9.8 log points. This result confirms the timing of the hypothesised transmission channel: a financing advantage working through decisions that take time (investment, pricing, market share), not a contemporaneous price response. Pre-trends are non-significant, both horizon-specific and pooled ones.

The same battery of robustness checks as in the investment rate analysis is run also for this outcome variable (Appendix B.4).¹⁰ The results (Appendix B.4) are stable, except for one test at the stabilisation parameter $L = 1$, where the coefficient has the same sign but is not statistically significant. First, with the outcome blanked in 2020–21, the coefficient is basically unchanged (0.068): the effect is not a pandemic artefact. Second, the results do not rest on specific definitions of treatment or eligibility. The fully predetermined eligibility criterion yields a larger effect, though the results should be interpreted carefully given that the observation count falls to only 194. Moreover,

⁹ Estimating the same design on the markup in levels gives a pooled +0.089 (SE 0.061, $p = 0.148$), 6.0 percent of the sample mean, with the same dynamics as the log specification: nothing at $h = 0$ and $h = 1$, the whole effect at $h = 2$ (0.138, $p = 0.065$).

¹⁰ Two features of the markup measure do not affect these estimates. The output elasticities are constant within sector and over time, so they drop out of every within-firm change (Appendix B.5). The first-stage correction of the revenue share makes the estimate more conservative, as the uncorrected revenue-to-COGS ratio gives 9.1 log points (standard error 5.3) versus 6.9 for the headline (Appendix Table A-35).

Figure 2: Markups around an issuance-week purchase



Coefficients β_h of equation (2) by event time, outcome: the change in the log markup from the base year, estimated with the non-absorbing LP-DiD at the main specification (country and sector-by-year effects, nine lagged firm financials, the lagged benchmark share, the issuance and eligible-issuance dummies, ever-eligible sample, $L = 3$). Whiskers are 90 percent confidence intervals, standard errors clustered by firm. Horizons -4 to -1 are placebos relative to the base year. Point estimates in Table 4.

Table 4: Markups around an issuance-week purchase

Log markup	(1)	(2)	(3)	(4)
	sector, country, year	country, sector $\times$ year	sector, country $\times$ year	sector $\times$ year, country $\times$ year
Pooled post, $h = 0$ to 2	0.065** (0.026)	0.069** (0.030)	0.100** (0.039)	0.100 (0.064)
Pooled pre placebo, $h = -4$ to -2	0.012 (0.016)	0.013 (0.014)	0.005 (0.019)	0.002 (0.020)
<i>By horizon, main specification (column 2)</i>				
$h = 0$		0.010 (0.012)		
$h = 1$		0.023 (0.028)		
$h = 2$		0.098** (0.040)		
Observations (post)	716	592	669	517

Outcome: log markup, estimated with the production approach (Appendix B.5); coefficients in log points, 0.069 reads as 7.1 percent. *Treatment:* issuance-week purchase, observed timing. *Sample:* ever-eligible firms; four pre-periods, two post-periods, $L = 3$. *Controls* in every column: nine lagged firm financials, the lagged CSPP benchmark share, the contemporaneous issuance and eligible-issuance dummies. *Estimator:* non-absorbing LP-DiD; the pooled post coefficient over $h = 0$ to 2 is the headline; the lower block reports the per-horizon coefficients at the main specification. The main specification is column 2, in bold. Standard errors clustered by firm in parentheses; *, ** and *** denote significance at the 10, 5 and 1 percent level. Full battery in Appendix Table A-23.

the initial-portfolio-inclusive treatment dummy gives a coefficient of 0.060: adding the imputed-timing initial-portfolio firm-years dilutes it but does not overturn it. Finally, because the treatment process is highly persistent (paragraph “Some Facts about the Purchase Process”, Section 2.1), the three checks that condition on the firm’s previous-purchase history (lagged treatment indicator and lagged and cumulative purchased-issue counts) are particularly important. The resulting coefficient is the same as in the headline results in all three, so the effect is not legacy-treatment loading. The no-composition form of the estimator gives very similar results throughout (Appendix B.4).

A common concern with production-approach markups is that a rising markup could reflect a changing overhead share rather than pricing power. If so, operating profits should not be affected by the treatment. The same design on operating profits gives positive but insignificant point estimates with a clean pre-trend placebo. The reason is power, not sign: the change in operating profit over assets is sixty percent noisier than the investment rate, so the smallest effect this design could detect is four to six times the one the markup result implies.¹¹

2.6 Only Repeated Purchases at Issuance Have Real Effects

Four further margins yield null results that clarify which purchases produce the effects above (Table 5).

Table 5: Margins without an effect

	Yield at issuance	Investment rate	Log markup
Secondary-market purchases	—	0.011 [−0.004, 0.026]	−0.044 [−0.132, 0.044]
Single purchase	—	−0.0026 [−0.0075, 0.0023]	−0.005 [−0.048, 0.038]
Announcement	—	−0.0004 [−0.0071, 0.0062]	−0.010 [−0.047, 0.027]
Spillover to later issues	−0.129 [−0.291, 0.033]	—	—

Each cell reports the pooled point estimate with its 90 percent confidence interval in brackets. Rows: the secondary-market purchase treatment (Appendix Table A-26, Check 2); a single purchase not followed by another (Appendix Table A-28, one-shot column, no-composition LP-DiD); predetermined eligibility at the announcement, firm, country-by-year and size-quartile-by-year effects (Appendix Table A-29); a purchase of the issuer’s previous issue within one year, never-purchased sample, verdict specification (Appendix Table A-13). Units: yield in percentage points; investment rate as a level ratio of base-year assets; markup in log points. A dash marks a margin not defined for that outcome. 90 percent intervals are computed from the coefficient and its clustered standard error, as in the source tables.

2.6.1 A Purchase Does Not Lower the Issuer’s Later Yields

Does a previous purchase lower the yield of the issuer’s later bonds? Tables A-13–A-15 in Appendix B.3 show no detectable effect of this kind. The same specification of the main analysis is

¹¹ The pooled $h = 0-2$ coefficient yields a coefficient of +0.28pp of base-year assets per year (SE 1.44). The annual change in operating profit over lagged assets has a standard deviation of 2.14pp against 1.32pp for net capex, so the profit outcome is roughly 60% noisier than investment in the units the design uses.

augmented with additional dummy variables that take the value of one if the issuer of bond b was purchased within a given window before the current issuance (e.g., the previous year or quarter). Moreover, I run this analysis on two subsamples: the full sample and a cleaner one restricted to bonds never purchased at issuance. In neither case is the coefficient on a previous purchase statistically significant in the main specification, whatever the window used to define it. The two samples, however, deliver different pictures of the uncertainty around this null. On the full sample the estimate is a precise zero, -0.001 (0.080), with intervals that exclude an effect the size of the direct one in ten of the twelve fixed-effect combinations; on the never-purchased sample it is negative but imprecise, -0.129 (0.098), and does not exclude a spillover as large as the direct effect.

2.6.2 Secondary, Single and Announced Purchases Have No Effect

Only issuance-week purchases matter. The treatment measure used so far is the issuance-week purchase. The natural objection is that this is one arbitrary part of ECB activity: why should a purchase at issuance matter and a purchase of the same firm’s bond three months later on the secondary market not? Table A-26 answers this question with a detailed decomposition, run for both the investment rate and markups. Check 1 keeps the same treatment definition but drops firms purchased on the secondary market as well. Check 2 defines the treatment as being purchased only on the secondary market. Finally, Check 3 adds a measure of secondary market purchases to the headline specification described in the previous sections. Importantly, the decomposition is not a relabeling of one group, as the two treatments genuinely separate at the firm level: 77 firms are treated only in their issuance week, 17 only in the secondary market, and 59 in both (Appendix Table A-27).

The results clearly answer both questions. First, the secondary-market treatment is null under every fixed-effects structure, including the main one. For the investment rate, the point estimate is about half the primary one (1.1 against 2.2 percentage points), with a 90% interval of $[-0.4, +2.6]$: small and not distinguishable from zero, even if the interval contains the primary effect near its upper edge. For markups, the estimate has the opposite sign (-0.044), and its interval $[-0.132, 0.044]$ excludes the primary effect of 0.069. Second, dropping every firm that ever receives a secondary purchase leaves the investment effect essentially unchanged (1.8 percentage points in the main specification), while making the markup effect larger (0.139, significant at 1%). If anything, the coefficient of 6.9 log points in the main analysis is diluted by firms whose treatment mixes the two types of ECB intervention. Third, controlling for contemporaneous secondary purchases leaves both primary effects intact (2.1 percentage points and 0.061): the two margins are close to orthogonal.

Together, these results imply that only ECB purchases at the moment the firm raises funds move real variables. Simply being part of the Eurosystem’s portfolio is not enough. The evidence on the Federal Reserve’s corporate purchases of 2020, which were made only in the secondary market and over a few months, shows the same pattern: Li and Ringgenberg (2026) find that the firms whose bonds the Fed bought gained liquidity and cheaper new issues but did not change investment,

employment or capital.

Only repeated purchases matter. The non-absorbing estimator identifies each horizon from continuously purchased firms (Section 2.4), so the results so far cannot distinguish the dynamic effects of a single treatment from those of cumulative purchases over time. The no-composition version of the estimator addresses this concern by estimating the effect of a purchase that is not repeated. The treated group is restricted to firms purchased once - on at t , off at $t + 1$ and $t + 2$ - holding the clean-control pool and every other design choice fixed, so that the two exercises differ in exactly one thing: which firm-year observations count as treated.

Table A-28 reports the results for both outcomes. First, the single-purchase path is flat and centred on zero at every horizon. For the investment rate, the one-shot pooled coefficient is -0.3 percentage points versus a continuous one of $+2.2$ on the same balanced sample; for markups, it is -0.005 versus 0.069 , with a continuous $h = 2$ estimate of 0.101 .¹² Second, the null is not based on thin data: it rests on more treated variation than the standard results on cumulative purchases, as there are 53 one-shot treatment onsets (switch events) against 12 for the investment rate, and 51 against 11 for markups. Moreover, standard errors are similar across the two variants, and each one-shot interval excludes the corresponding continuous point estimate for both outcomes and at every horizon.

These results imply that only repeated purchases affect real variables; one-off purchases are not enough.¹³

No effects on announcement. Another exercise examines whether the announcement of the Corporate Sector Purchase Programme itself affected the variables of interest, rather than the actual purchases analysed so far, which is the standard approach in the literature, as in Todorov (2020) and De Santis and Zaghini (2021). Treatment is fully predetermined: a dummy equal to one if firm i had at least one non-commercial paper bond on the ECB eligible-assets list outstanding on the day before the announcement (9 March 2016), a requirement 88 of 769 panel firms satisfy. The specification is an event study of eligible versus never-eligible bond issuers around 2016, with firm, country $\times$ year, and size $\times$ year fixed effects; the latter is key because eligible firms are far larger than ineligible ones (Appendix B.4 details the design and its robustness checks). Table A-29 reports results for markups and the investment rate: predetermined eligibility has no detectable effect on either outcome, and announcement effects of the magnitude of actual purchases are ruled out. For markups, the 90% interval is $[-0.047, +0.027]$ against a purchase effect of $+0.069$; for the investment rate, it is $[-0.7, +0.6]$ percentage points of 2015 assets against $+2.2$. This replicates,

¹² The $h = 2$ dynamic coefficient differs from the headline one presented in Section 2.5 because this exercise uses the no-compositional version of the estimator, which is the same as the one reported in Appendix B.4. The pooled coefficient (0.069) is common to the two by construction.

¹³ A caveat is that selection seems to play an important role here, as the groups are defined by their own future path. A gap already exists at $h = 0$, where the two groups have, by construction, received identical treatment and differ only in what happens afterwards.

on a stricter and fully predetermined eligibility measure, the null on real investment that [De Santis and Zaghini \(2021\)](#) find around the announcement.

2.6.3 Revenues, Costs and Dividends

Revenues and COGS. A natural question is what happens to the two components from which markups are estimated. Table [A-35](#) in Appendix [B.5](#) decomposes the effect of the main result according to the identity between the log markup, log revenue, log cost of goods sold and the measurement correction of the production approach: the 6.9 log points are the result of revenue rising by 9.8 log points, against 0.8 for the cost of goods sold and 2.1 for the correction. Both components move in the direction the mechanism implies. However, neither is separately identified: their standard errors are five times the markup’s because the scale shocks common to both cancel in the ratio while remaining in each.

Dividend payments. The last variable examined is the payout of the purchased firms, on which the model of Section [3](#) draws a sharp distinction. The same panel design on cash dividends paid over base-year assets yields no detectable effect, a pooled coefficient of -0.8 percentage points (standard error 0.7) that is stable across the battery of Appendix Table [A-34](#); the upper end of its 90% interval, 0.4 percentage points of assets, is the ceiling against which the quantitative section validates the model’s payout response.

2.6.4 Summary of the Empirical Analysis

The three main empirical analyses and their additional checks present a clear narrative of how Corporate Purchase Programmes (and specifically the ECB’s CSPP) affect firms’ market power.

First, the policy announcement had no detectable effect on any real variable, in line with the existing literature. On the other hand, a purchase at issuance by the ECB delivers cheap funding exactly when the firm raises capital; if the purchases are repeated over time, the firm accumulates capital from the following year; its markup builds over the same period of time, peaking one year after the capital effect starts to be significant. One clarification is important. The paper does not claim that a 22 basis points yield reduction per se causes a 6.9 log-point markup increase through a price-of-debt elasticity, as no plausible cost-of-capital arithmetic can deliver that. The price-to-capital link is quantitatively accounted for by the measured dose: the typical continuously treated firm receives a three-year sequence of roughly seven to eight purchased issues with a combined face value near 9% of assets. The residual elasticity is roughly two to four, and sits on the capital-to-markup link.

3 Analytical Model

This section builds a general equilibrium model to rationalise the empirical evidence, with heterogeneous firms that finance part of their capital by issuing securities in advance. Before the details,

it is useful to state what the model delivers. Firms compete à la Cournot within their sector and finance capital one period in advance by placing bonds with a private intermediary, whose holding cost rises with its position in each issue, so that every firm faces its own downward-sloping demand for its bonds. A central bank purchase takes part of an issue off the intermediary’s book: the price of that issue rises, the firm’s user cost of capital falls, and it installs more capital, produces more and gains market share and markup at the expense of its sector rivals. This mechanism has three implications that map onto the null results of Section 2.6. A purchase after a bond has been placed changes neither its quantity nor its price, so secondary-market purchases are neutral. A single purchase only revalues an issue already sold, so its real effect is small. Only a purchase anticipated at the next issuance moves the firm’s capital decision, so the programme’s real effects come from repeated deviations from the announced market-neutral share. The model’s one limitation, that only firms paying no dividends respond, is what the quantitative version of Section 4 addresses.

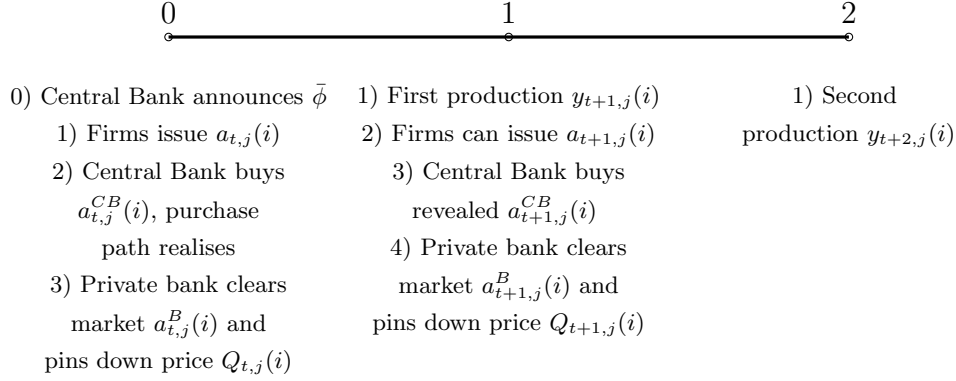
3.1 Environment

The model is written in real terms. Time is discrete and finite, with all agents living for three periods. The economy consists of a representative consumer who has preferences over final consumption and labour supplied, owns all the firms, and holds shares in the financial intermediary. The final good is produced by a representative perfectly competitive firm by using a bundle of intermediate goods. These are produced by firms that compete oligopolistically, specifically à la Cournot (i.e. setting quantities). As in [Atkeson and Burstein \(2008\)](#), they are divided into a continuum of markets (sectors) indexed by j , each with a finite number I of producers. This market structure allows firms to charge heterogeneous, endogenous markups. Moreover, their production function is Cobb-Douglas over capital and labour. Similarly to [Gertler and Karadi \(2011\)](#), they need to issue securities to finance capital investment beyond their initial net worth. On the other side of the financial market, a representative financial intermediary maximises shareholders’ (i.e. household) value. The securities market is perfectly competitive; however, there are financial frictions, modelled as convex holding costs, as in [Papoutsis et al. \(2022\)](#) and [Nakov and Thomas \(2026\)](#). This feature implies that each issuer faces a downward-sloping demand curve for its bonds. Finally, there is a central bank, which does not carry out conventional monetary policy but can intervene in the securities market. It buys private assets and finances this expenditure by issuing risk-free debt. There is no uncertainty in the economy, and there is perfect information about the idiosyncratic productivities; the central bank’s Purchase Programme is introduced as an unexpected (MIT) shock.

Timing. Figure 3 shows the model’s timing. At the beginning of the first period, each firm needs to issue securities $a_{t,j}(i)$ to finance the capital needed for production in the next one. These bonds are two-period, maturing in a fraction $1 - \lambda$ in the following period and the remaining λ in the last one. After that, action in the securities market takes place. First, the central bank purchases securities $a_{t,j}^{CB}(i)$ from some firms. Importantly, this intervention also reveals the purchasing scheme

in the subsequent period. Then, the private financial intermediary absorbs the entire supply of those left by policymakers, $a_{t,j}^B(i)$. In doing so, the price $Q_{t,j}(i)$ clears the security market. When the second period starts, the firm finds out the realised bond price they receive on the securities (and so the amount of installed capital), and the first production $y_{t+1,j}(i)$ happens. Then, essentially the same actions repeat: first, firms decide how many other (one-period) bonds to issue, and then the central bank and the private bank clear the market. Finally, in the last period, the last production $y_{t+2,j}(i)$ takes place.

Figure 3: Timing of the model



Household. The representative household maximises lifetime utility, given preferences over consumption adjusted for the disutility of work, as described by

$$\max_{C_t, N_t, S_t} \sum_{t=0}^2 \beta^t \left(C_t - \frac{N_t^{1+\frac{1}{\varphi}}}{1 + \frac{1}{\varphi}} \right)$$

where β is the discount factor for future utility and φ is the inverse Frisch elasticity of labour supply. The household chooses consumption of the final good C_t , the amount of labour to supply N_t and the amount of shares of the private financial intermediary S_t to buy. It faces the following series of budget constraints

$$\begin{aligned} C_0 + Q_0^s S_0 &= E && \text{for } t = 0 \\ P_t C_t + Q_t^s S_t &= W_t N_t + S_{t-1} + D_t + T_t && \text{for } t = 1, 2 \end{aligned}$$

At $t = 0$ the household receives an endowment E , which it spends on consumption and on the intermediary's shares at price Q_0^s ; with quasi-linear preferences and E common across policy scenarios, the endowment enters no optimality condition and no policy comparison in Section 4.6. At $t = 1, 2$ the household earns wages $W_t N_t$, receives firm profits D_t and pays lump-sum taxes T_t .

Final good producer. A representative, perfectly competitive firm produces the final good Y_t using a bundle of intermediate goods, according to the double-nested CES structure first introduced

by Atkeson and Burstein (2008). Goods within the same market (i.e. the same sector) have a higher substitutability measure, indicated by η , than goods in different markets, denoted by θ . Thus, $\eta > \theta$.

$$Y_t = \left[\int_0^1 Y_{t,j}^{\frac{\theta-1}{\theta}} dj \right]^{\frac{\theta}{\theta-1}} \quad Y_{t,j} = \left[I^{-\frac{1}{\eta}} \sum_{i=1}^I y_{t,j}(i)^{\frac{\eta-1}{\eta}} \right]^{\frac{\eta}{\eta-1}} \quad (3)$$

where $Y_{t,j}$ is sector output and $y_{t,j}(i)$ the output of intermediate firm i in sector j . Taking the price of intermediate goods as given, the final good producer chooses the quantity of each intermediate good optimally, aiming to maximise its profits,

$$P_t Y_t - \int_j \sum_{i=1}^I p_{t,j}(i) y_{t,j}(i) dj$$

subject to equations (3).

Intermediate goods producers. Intermediate producer i in sector j (a continuum of sectors with I finite producers each) competes oligopolistically by setting quantities and produces with the Cobb-Douglas technology

$$y_{t+1,k}(i) = z_{t+1,k}(i) k_{t,k}(i)^\alpha n_{t+1,k}(i)^{1-\alpha} \quad (4)$$

Moreover, notice that capital must be financed and installed one period in advance (time-to-build). In the first period $t = 0$, the firm holds inherited net worth $e_j(i)$ and issues multi-period bonds to finance the rest of its capital position, as described by the constraint

$$k_{0,j}(i) = e_j(i) + Q_{0,j}(i) a_{0,j}(i). \quad (5)$$

At date 1, the firm installs capital out of retained earnings and/or new issuance, subject to a standard budget constraint. Capital depreciates at rate δ . Finally, each firm perceives itself as infinitesimally small to the entire economy and thus takes the wage level W_{t+1} as given.

Notice that from this point onwards the notation is simplified as follows: the time subscript is always kept; the sector index j appears only inside the aggregators and the demand system; the firm index i appears only where an object varies across firms within the same expression, as for the policy objects, the rivals' quantities and the integrals of the aggregation; everything else refers to a representative firm of a given type. For example, the assets issued by a given firm i, j at time $t = 0$ will be denoted by a_0 instead of $a_{t,j}(i)$.

The date-0 issue a_0 is a two-period bond, maturing in a fraction $(1 - \lambda)$ at $t = 1$ and λ at $t = 2$; on the other hand, date-1 issue a_1 is one-period. Thus, the firm's debt position is $b_0 = a_0$ at $t = 0$ and $b_1 = a_1 + \lambda a_0$ at $t = 1$. At each production date, the firm chooses labour n_t to maximise operating profits Υ_t , taking into account the strategic interaction with the other firms in its sector,

and repays maturing debt; at $t = 1$ it installs k_1 out of retained dividends div_1 and/or new bonds, and at $t = 2$ it pays out the residual as dividends div_2 . Formally, at $t = 0$ each producer solves

$$\begin{aligned} V_0 &= \max_{a_0} M_1 V_1(z_1, k_0, a_0, y_1(-i)) \\ \text{s.t. } k_0 &= e + Q_0 a_0 \end{aligned}$$

and at $t = 1$

$$\begin{aligned} V_1(z_1, k_0, a_0, y_1(-i)) &= \max_{div_1, k_1, a_1} \{div_1 + M_2 V_2(z_2, k_1, a_0, a_1, y_2(-i))\} \\ \text{s.t. } 0 \leq div_1 &\leq \Upsilon_1 - a_0(1 - \lambda) - k_1 + (1 - \delta)k_0 + Q_1 a_1 \end{aligned}$$

Dividends are non-negative (no equity financing) and bounded by the budget: undepreciated capital plus operating profits Υ_1 , net of the maturing tranche of a_0 and of next-period capital k_1 , plus the proceeds of the short-term issue a_1 . Operating profits are

$$\begin{aligned} \Upsilon_1(z_1, k_0, y_1(-i)) &\equiv \max_{n_1} [p_1 y_1 - W_1 n_1 - W_1 \psi_1 z_1^{\psi_2}] \\ \text{s.t. } y_1 &= z_1 k_0^\alpha n_1^{1-\alpha} \\ p_1 &= I^{-\frac{1}{\eta}} y_1^{-\frac{1}{\eta}} Y_{1,k}^{\frac{1}{\eta} - \frac{1}{\theta}} Y_1^{\frac{1}{\theta}} P_1 \end{aligned} \tag{6}$$

The overhead cost $\psi_1 z_1^{\psi_2}$ is denominated in labour (and thus hired at the wage level W_{t+1}) as in [De Loecker et al. \(2026\)](#). Unlike their formulation, I generalise it to scale with the firm's type, i.e., its date-1 productivity, at both production dates: it represents an administrative capacity proportional to firm size. This is needed because the model does not feature endogenous entry and exit of firms; a common overhead cost would imply an unrealistic number of loss-making firms. At $t = 2$ the firm only chooses n_2 , with Υ_2 defined as in equation (6), and pays out the residual as dividends:

$$\begin{aligned} V_2(z_2, a_0, a_1, b_1, y_2(-i)) &= \max_{div_2} div_2 \\ \text{s.t. } 0 \leq div_2 &\leq \Upsilon_2 - (a_1 + \lambda a_0) + (1 - \delta)k_1 \end{aligned}$$

Market competition and equilibrium. The equilibrium concept within each market is a Nash equilibrium. Given the continuum of sectors, firms in different markets do not interact strategically. In contrast, the finite number of firms I in each sector gives rise to strategic interactions. Specifically, each firm chooses its own quantity to produce $y_{t+1,j}(i)$ in order to maximise operating profits $\Upsilon_{t+1,j}(i)$, taking as given the quantities $y_{t+1,j}(-i)$ of its competitors. Notice that it can do so because there is no uncertainty in the economy, and there is perfect information about everyone's idiosyncratic productivity level $z_{t+1,j}(i)$; finally, they have rational expectations about the aggregate variables of wage W_{t+1} , output Y_{t+1} and price level P_{t+1} . An additional layer is that

firms live for three periods and produce (and thus compete) twice, so the capital a firm installs today is also a choice about the market it will compete in tomorrow. The paper's equilibrium concept keeps the two decisions separate. At each production date, the within-sector equilibrium is Cournot-Nash in quantities and, given the capital stocks, unique. When choosing capital, instead, each firm takes the output paths of its rivals as given: capital is a Nash equilibrium in plans, the open-loop solution of the dynamic game (Fudenberg and Tirole, 1983; Reynolds, 1987), in which a firm does not internalise the response of its rivals' output to its own capital. I make this choice for tractability, as it keeps a twelve-firm sector with a financing stage computable; Section 4.5 measures the term that the closed-loop, Markov-perfect solution (Maskin and Tirole, 1988) would add and reports its sign and size. The central bank's intervention does not alter this structure, as it is introduced as an unexpected shock and the central bank is not a strategic player: it changes the economy's equilibrium outcome, but not the game between firms.

Private financial intermediary. On the other side of the security market, there is a representative private financial intermediary that acts in a perfectly competitive manner. I model it by simplifying the framework proposed in Papoutsis et al. (2022). It is owned by the household, and its portfolio can be composed of both private securities $a_{t,j}(i)$ and risk-free debt d_t . Equivalently, d_t can be interpreted as interest-bearing reserves. Financial frictions are holding costs, which can also be read as the transaction costs of changing the portfolio's composition, as in Andres et al. (2004), Chen et al. (2012), and Gertler and Karadi (2013), among others.

Denote $a_0^B(i)$ as its position in firm i 's date-0 bond and $a_1^B(i)$ as its position in the date-1 bond. Financial frictions take the form of quadratic holding costs that the private financier has to pay to hold the risky part of the portfolio (reserves d_t are risk-free) between two consecutive periods, computed security by security:

$$\begin{aligned} h_0^B &= \int_j \sum_{i=1}^I \frac{\gamma}{2} \frac{\left(Q_{0,j}(i) a_{0,j}^B(i) \right)^2}{e_j(i)} dj, \\ h_1^B &= \int_j \sum_{i=1}^I \frac{\gamma}{2} \left[\frac{\left(Q_{1,j}(i) a_{1,j}^B(i) \right)^2}{k_{0,j}(i)} + \frac{\left(\lambda Q_{0,j}(i) a_{0,j}^B(i) \right)^2}{k_{0,j}(i)} \right] dj, \end{aligned} \quad (7)$$

γ is the parameter that embodies the strength of the financial friction in the economy. It is important to notice two features of the functions above. First, the cost on the financier position is scaled by the predetermined firm size, i.e. predetermined net worth $e_j(i)$ at date 0, capital $k_{0,j}(i)$ at date 1.¹⁴ This assumption makes the marginal cost proportional to the leverage of the position rather than to its absolute size, in turn implying that flat leverage implies flat bond spreads across sizes. The second assumed feature of the functional forms above is separability between different

¹⁴ The scale is predetermined for one technical reason. A scale that moved with the current issue would make the marginal holding cost depend on the firm's simultaneous capital choice, so the intermediary's schedule and the firm's demand would no longer separate, and the two decisions could no longer be solved sequentially.

maturities: the two types of securities enter the date-1 holding cost (7) as separate quadratic terms, not through the value of the combined position. This is an assumption made to match the paper’s empirical results. Having both types of bonds under the same argument of the quadratic function would imply that a past purchase improves the price of the firm’s next issues even if no further purchase occurs (a spillover effect), and that a purchase of the remaining tranche at date 1 raises the price of the current new issue and hence moves capital (an effective secondary-market purchase). The empirical investigation in Section 2.6 rejects both implications, so separability is the natural way to model holding costs. Moreover, this assumption aligns more closely with the preferred-habitat reading of γ , since habitat preferences in the tradition of Vayanos and Vila (2021) are specific to a security’s maturity segment.

Central bank. The central bank, as in Papoutsis et al. (2022), carries out no conventional monetary policy and is equivalent to a government: it intervenes only by purchasing intermediate firms’ securities, funded by issuing risk-free debt. Following previous notation, define $a_{t,j}^{CB}(i)$ as the securities of firm i, j in the central bank balance sheet. Then, the central bank finances its policy according to

$$Q_t^d d_t = \int_j \sum_{i=1}^I Q_{t,j}(i) a_{t,j}^{CB}(i) dj \equiv V_t^{CB}, \quad t = 0, 1,$$

Moreover, as the private financier, the central bank has to pay some holding costs on the securities in its balance sheet,

$$\begin{aligned} h_0^{CB} &= \int_j \sum_i \frac{\gamma + \kappa}{2} \frac{\left(Q_{0,j}(i) a_{0,j}^{CB}(i)\right)^2}{e_j(i)} dj, \\ h_1^{CB} &= \int_j \sum_i \frac{\gamma + \kappa}{2} \left[\frac{\left(Q_{1,j}(i) a_{1,j}^{CB}(i)\right)^2}{k_{0,j}(i)} + \frac{\left(\lambda Q_{0,j}(i) a_{0,j}^{CB}(i)\right)^2}{k_{0,j}(i)} \right] dj, \end{aligned} \quad (8)$$

In addition to the holding cost γ the private intermediary pays, the central bank incurs an additional efficiency cost κ for each asset intermediated. As in Gertler and Karadi (2011), this could reflect two aspects. The first is the cost of raising funds through government debt, while the other is the cost of identifying the correct private-sector investments. Putting all these elements together, the central bank budget constraint, reported in Appendix C.1, equates the holding cost, the purchases and the maturing reserves to the repayments received, the new reserves $Q_t^d d_t$, with Q_t^d their price, and lump-sum transfers T_t to the household.

Corporate purchase programme elements. To specify how purchases take place, the central bank first selects a set $\tilde{I}$ of firms eligible for this policy. Moreover, as the ECB does in practice, the central bank announces a fixed market-neutral share $\bar{\phi}$ to purchase from each eligible issuer. However, the realised purchased share $\phi_{0,j}(i)$ depends on possible deviations $\xi_{0,j}(i)$ from the an-

nounced policy implementation. These deviations arrive after firms have already made their first bond issuance decision according to the announced $\bar{\phi}$. In other words, the deviations at time $t = 0$ are modelled as an MIT shock in this economy. Moreover, I assume that the deviations happen only at $t = 0$: they reveal the entire purchase path (i.e. date-1 purchased share $\phi_{1,j}(i)$) through a persistence rule, governed by the parameter ρ . Summarising, policy is described by the collection $(\tilde{I}, \bar{\phi}, \xi_{0,j}(i), \rho)$, with

$$a_{t,j}^{CB}(i) = \phi_{t,j}(i) a_{t,j}(i), \quad \phi_{0,j}(i) = \begin{cases} \bar{\phi} + \xi_{0,j}(i) & \text{if } i \in \tilde{I}, \\ 0 & \text{otherwise,} \end{cases} \quad \phi_{1,j}(i) = \rho \phi_{0,j}(i), \quad \rho \in [0, 1]. \quad (9)$$

The market-neutral share $\bar{\phi}$ is a fixed primitive, as the issuer-level benchmark of Section 2.1: the central bank buys the fraction $\bar{\phi}$ of every eligible issue at every purchase date, so the portfolio represents the eligible universe proportionally and the programme's footprint is derived rather than targeted (Appendix C.1 gives the face-value form): $V_t^{CB} = \bar{\phi} V_t^{elig}$, coming from denoting the universe of eligible securities as

$$V_t^{elig} \equiv \int_j \sum_{i \in \tilde{I}} Q_t(i) a_t(i) dj$$

The date-0 deviations $\xi_{0,j}(i)$, whose real counterpart Section 2.2 documents, are the only remaining primitives, as the persistence rule generates the date-1 fractions; they are firm-specific and free, subject only to the fraction bounds

$$0 \leq \bar{\phi} + \xi_0(i) \leq \phi^{\max} \quad \text{for all } i \in \tilde{I},$$

where $\phi^{\max}$ is the programme's per-issue purchase limit: following the ECB decision in the real world, $\phi^{\max}$ is set equal to 0.7. The deviations profile partitions the eligible universe into two categories: purchased firms ($\phi_0(i) > 0$), and never-purchased firms ($\phi_0(i) = 0$). Notice that nothing requires the deviations to be small: a purchase rule that leaves a substantial fraction of the eligible universe unpurchased is a profile of large deviations, and is completely admissible under what was just described.

The persistence rule mirrors a conventional monetary policy shock, unexpected and decaying at rate ρ . Since the data report whether an issue was purchased but not the purchased share (Section 2.1), ρ is calibrated to the extensive-margin repurchase probabilities, and equation (9) translates them into the persistence of the purchased share.

Aggregation, resource constraints and welfare. Bond market clearing holds security by security,

$$a_t^B(i) = a_t(i) - a_t^{CB}(i) = a_t(i)(1 - \phi_t(i)), \quad t = 0, 1. \quad (10)$$

Additionally, let $K_t = \int_j \sum_{i=1}^I k_{t,j}(i)$ denote the aggregate capital in the economy $t = 0, 1$, and the implied aggregate investment is $I_t = K_t - (1 - \delta)K_{t-1}$. Moreover, denote with $N_t = \int_j \sum_i [n_{t,j}(i) +$

$\psi_1 z_{1,j}(i)^{\psi_2} dj$ the total labour supplied in the economy for $t = 1, 2$, which comprehends both production and overhead labour. The three resource constraints are

$$\text{date 0 : } E + \int_j \sum_i e_j(i) dj = C_0 + K_0, \quad (11)$$

$$\begin{aligned} \text{date 1 : } Y_1 &= C_1 + K_1 - (1 - \delta)K_0 + h_0^B + h_0^{CB}, \\ \text{date 2 : } Y_2 + (1 - \delta)K_1 &= C_2 + h_1^B + h_1^{CB}. \end{aligned} \quad (12)$$

Equations (11)–(12) follow from the household budget, firm dividends, the intermediary's and the central bank's budgets, as taxes and reserves net out. The date-0 resources $E + \int \sum e$ are primitives common to all policy scenarios, so $\Delta C_0 = -\Delta K_0$ across scenarios: any policy that raises aggregate capital lowers first-date consumption one for one. Welfare is reported in differences, which are free of the primitives,

$$\begin{aligned} \Delta \mathcal{W} &= -\Delta K_0 + \beta \left[\Delta Y_1 - \Delta K_1 + (1 - \delta)\Delta K_0 - \Delta h_0^B - \Delta h_0^{CB} - \Delta v(N_1) \right] \\ &\quad + \beta^2 \left[\Delta Y_2 + (1 - \delta)\Delta K_1 - \Delta h_1^B - \Delta h_1^{CB} - \Delta v(N_2) \right], \end{aligned} \quad (13)$$

with $v(N) \equiv N^{1+1/\varphi}/(1 + 1/\varphi)$.

3.2 Solution without Policy

This section solves the model by backward induction without policy ($\phi_t(i) = 0$, $a_t^B = a_t$).

Household. The household's Euler equation is standard:

$$Q_t^s = \beta \mathbb{E} t \left[\frac{P_t}{P_{t+1}} \right] \Rightarrow Q_t^s = M_{t+1} = \beta$$

Given linear preferences on consumption, the stochastic discount factor is constant and equal to the discount factor β . On the other hand, the aggregate amount of labour to supply to the economy is summarised by

$$N_t^{\frac{1}{\varphi}} = \frac{W_t}{P_t} \quad (14)$$

Final good producer. The final good producer's demand for each intermediate good is

$$p_t = I^{-\frac{1}{\eta}} y_t^{-\frac{1}{\eta}} Y_{t,j}^{\frac{1}{\eta} - \frac{1}{\theta}} Y_t^{\frac{1}{\theta}} P_t \quad (15)$$

which represents the demand that each intermediate producer faces. Moreover, as common with CES aggregators, the sector j price $P_{t,j}$ and the aggregate price level P_t are defined respectively as the levels that are consistent with $P_t Y_t = \int_0^1 P_{t,j} Y_{t,j}$ and $P_{t,j} Y_{t,j} = \sum_{i=1}^I p_{t,j}(i) y_{t,j}(i)$, resulting in

the following expressions

$$P_{t,j} = \left[I^{-1} \sum_{i=1}^I p_{t,j}(i)^{1-\eta} \right]^{\frac{1}{1-\eta}} \quad P_t = \left[\int_0^1 P_{t,j}^{1-\theta} \right]^{\frac{1}{1-\theta}}$$

Intermediate goods producers: production decision. The production decision is static and identical at $t = 1$ and $t = 2$ (at $t = 2$ it is the firm's only decision, dividends being the residual): the firm chooses labour n_{t+1} to maximise operating profits Υ_{t+1} , internalising the production function (4) and its demand curve (15) and taking rivals' quantities as given:

$$\Upsilon_{t+1} \equiv \max_{n_{t+1}} \left[p_{t+1} y_{t+1} - W_{t+1} n_{t+1} - W_{t+1} \psi_1 z_1^{\psi_2} \right] \quad \text{s.t. (4) and (15),}$$

Solving it yields the following optimality condition:

$$\frac{p_{t+1}}{\mu_{t+1}} (1 - \alpha) z_{t+1} k_t^\alpha n_{t+1}^{-\alpha} = W_{t+1}, \quad \text{i.e.} \quad p_{t+1} = \mu_{t+1} MC_{t+1}, \quad MC_{t+1} \equiv \frac{W_{t+1}}{\partial y_{t+1} / \partial n_{t+1}}. \quad (16)$$

The overhead term contains no choice variable, so it does not affect any first-order conditions. Its only equilibrium effects run through firm budgets (lower internal funds at every date) and through the labour market (higher total labour demand). Substituting the demand curve and the production function and solving gives the optimal labour choice as a function of own capital and sectoral aggregates,

$$n_{t+1}^* = \left[\frac{(1 - \alpha) \Lambda_{t+1}}{W_{t+1} \mu_{t+1}} \right]^{\frac{\eta}{G}} z_{t+1}^{\frac{\eta-1}{G}} k_t^{\frac{\alpha(\eta-1)}{G}}, \quad G \equiv \alpha(\eta - 1) + 1, \quad (17)$$

where $\Lambda_{t+1} \equiv I^{-1/\eta} Y_{t+1,j}^{1/\eta-1/\theta} Y_{t+1}^{1/\theta} P_{t+1}$ collects the demand shifters common to the sector.

The Cournot–Nash allocation within each sector is summarised by

$$\begin{aligned} \mu_{t+1}(i) &= \frac{p_{t+1}(i)}{MC_{t+1}(i)} = \frac{\varepsilon_{t+1,j}(i)}{1 + \varepsilon_{t+1,j}(i)} = \left[1 - \frac{1}{\eta} + \left(\frac{1}{\eta} - \frac{1}{\theta} \right) s_{t+1} \right]^{-1} \\ \frac{1}{\varepsilon_{t+1,j}(i)} &= \frac{\partial p_{t+1,j}(i)}{\partial y_{t+1,j}(i)} \frac{y_{t+1,j}(i)}{p_{t+1,j}(i)} = -\frac{1}{\eta} \left(1 - s_{t+1,j}(i) \right) - \frac{1}{\theta} s_{t+1,j}(i) \\ s_{t+1,j}(i) &= \frac{p_{t+1,j}(i) y_{t+1,j}(i)}{\sum_{i=1}^I p_{t+1,j}(i) y_{t+1,j}(i)} = \frac{(y_{t+1,j}(i))^{1-\frac{1}{\eta}}}{\sum_{i=1}^I (y_{t+1,j}(i))^{1-\frac{1}{\eta}}} \end{aligned} \quad (18)$$

The markup is a decreasing function of the residual demand elasticity, which falls with the firm's market share (De Loecker et al., 2026): it ranges from $\frac{\eta}{\eta-1}$ for a negligible share, when competition is within the market, to the monopolist's $\frac{\theta}{\theta-1}$ for a share of one, when only substitution across markets remains. Finally, recall the equilibrium concept from the environment section: the production stage is solved as a Cournot–Nash equilibrium at each date, while the capital decisions in Sections 3.2

and 3.3 take rivals' output paths as given.

Intermediate goods producers: date $t=1$. At $t = 1$ the firm chooses div_1 , k_1 and a_1 ; the first-order condition on capital equates its marginal return to the user cost, the firm's borrowing rate plus depreciation:

$$\frac{\partial \Upsilon_2}{\partial k_1} = \left(\frac{1}{Q_1} - 1 \right) + \delta = r_1 + \delta \quad (19)$$

Let ϑ_1 denote the Lagrange multiplier on the budget constraint; optimisation endogenously yields the following two types of firms. A firm with $\vartheta_1 = 0$ pays dividends div_1 in the middle period: for it, internal and external funds are perfect substitutes at the margin. Hence, the firm's bond supply a_1 is perfectly elastic at the frictionless price $Q_1 = M_2 = \beta$. The dividend level div_1 is pinned down as a residual from the budget constraint. I label this type *unconstrained firms*. In contrast, firms with $\vartheta_1 > 0$ pay no intermediate-period dividends, which I label *constrained firms*. For both types of firms, equation (19) pins down the level of installed capital. However, for unconstrained firms, the level of short-term bond issuance a_1 is pinned down by whatever amount clears the market at the frictionless rate; for a constrained firm, the budget constraint pins the amount of securities. They are priced according to the private intermediary's optimisation, which incorporates the financial friction that acts on the constrained firm through the price: issuing more depresses Q_1 in equilibrium, raising the user cost and lowering desired capital, even though the firm itself takes the price as given.

Intermediate goods producers: date $t=0$. At $t = 0$ the firm chooses a_0 to maximise $M_1[div_1 + M_2 div_2]$ subject to the financing constraint (5) and the date-1 conditions just derived in the previous paragraph. Using the interest convention of Appendix C.1 (paragraph "Interest-rate convention"), the not-yet-matured tranche is marked at the date-1 price; this implies $1 + r_0 = [(1 - \lambda) + \lambda Q_1]/Q_0$. Using this result, once again the first-order condition for capital is the same for both types of firms, and it takes the same form as in $t = 1$:

$$\frac{\partial \Upsilon_1}{\partial k_0} = \frac{(1 - \lambda) + \lambda Q_1}{Q_0} - 1 + \delta = r_0 + \delta, \quad (20)$$

Notice how the level of net worth e does not enter such optimality: since it enters the financing constraint (5) additively, it shifts only the issuance level.

Intermediate goods producers: closed forms. Denote by u_t the firm's user cost of capital. After substituting (17) into (19), I obtain a closed form for desired capital at both dates, and issuance follows from the budget constraints (Appendix C.1). It is important to notice from that closed form the log-sensitivity of desired capital to the user cost,

$$\frac{\partial \log k_t^*}{\partial \log u_t} = -G = -[\alpha(\eta - 1) + 1], \quad (21)$$

which is the structural object behind the policy pass-through of Section 3.3 (paragraph “Date-1 Allocation under Policy”).

Private financial intermediary. The private financial intermediary chooses the quantities of firms’ and risk-free debt to maximise shareholder value. Solving the problem backwards, in $t = 1$ this is described as

$$\max_{\{a_1^B\}, d_1} \int_j \sum_i \left[M_2 - Q_1 \right] a_1^B dj + \left[M_2 - Q_1^d \right] d_1 - M_2 h_1^B,$$

Under the maturity separability assumption, the term related to the not-yet-matured part of the long-term bond is additively separable and sunk, so the first-order conditions are (writing $\gamma_0 = \gamma_1 = \gamma$ for the slope on each security, which Section 4.1 lets differ)

$$[a_1^B] : \quad Q_1 = M_2 \left[1 - \gamma_1 Q_1 \frac{Q_1 a_1^B}{k_0} \right], \quad [d_1] : \quad Q_1^d = M_2 = \beta, \quad (22)$$

At date 0, the intermediary prices the multi-period bond, whose unit pays $1 - \lambda$ at date 1 and λ at date 2, and whose position thus generates holding costs in both periods:

$$\begin{aligned} \max_{\{a_0^B\}, d_0} \int_j \sum_i \left[(1 - \lambda) M_1 + \lambda M_1 M_2 - Q_0 \right] a_0^B dj + \left[M_1 - Q_0^d \right] d_0 \\ - M_1 \int_j \sum_i \frac{\gamma_0 (Q_0 a_0^B)^2}{2 e(i)} dj - M_1 M_2 \int_j \sum_i \frac{\gamma_1 (\lambda Q_0 a_0^B)^2}{2 k_0(i)} dj. \end{aligned}$$

The first-order conditions are

$$[a_0^B] : \quad Q_0 = (1 - \lambda) M_1 + \lambda M_1 M_2 - M_1 Q_0 \left[\gamma_0 \frac{Q_0 a_0^B}{e} + M_2 \lambda \gamma_1 \frac{\lambda Q_0 a_0^B}{k_0} \right], \quad (23)$$

with the risk-free central bank debt being priced again at $[d_0]$: $Q_0^d = M_1 = \beta$. Notice how the first-order condition at both periods yields the price segmentation result: $Q_t(i)$ depends only on firm (i, j) ’s own holding cost and own placed quantity.

Equilibrium. An equilibrium is a collection

$$\{a_0(i), a_1(i), Q_0(i), Q_1(i), k_0(i), k_1(i), div_1(i), div_2(i), n_t(i), y_t(i), p_t(i), \mu_t(i), s_t(i)\}_{i,j, t=1,2}$$

together with aggregates $(W_1, W_2, Y_1, Y_2, N_1, N_2, K_0, K_1, C_0, C_1, C_2)$ such that: the household and the final good producer optimise, (14)–(15); firms optimise at every date and moment of it, (16), (18), (19) and (20), with the product market in Cournot–Nash equilibrium within each sector at each production date; the intermediary optimises, (22) and (23); the bond market clears security by security, (10); and the resource constraints (11)–(12) hold.

3.3 Solution Introducing Policy

This subsection introduces the purchase programme of Section 3 (eligible set $\tilde{I}$, announced share $\bar{\phi}$, date-0 deviations $\xi_0(i)$ and their persistence) into the solution.

Date-0 allocation under policy. Following the market clearing condition of equation (10), when the policy is announced all eligible firms make their first bond decision expecting to be purchased according to the market-neutral fraction $\bar{\phi}$. In other words, they base their issuance on the equilibrium price determined by $a_0^B = a_0^*(1 - \bar{\phi})$, where a_0^* is the amount of long-term security each firm issues under this belief. Notice that the announced share is common to all eligible firms: it improves their financing conditions relative to ineligible ones by the same fraction of every issue, so its effect on the eligible-versus-ineligible margin is small but not zero, and it is differenced away in any comparison between purchased and eligible-but-not-purchased firms, which is the margin the empirical section measures. Moreover, the issued amount a_0^* is sunk when the central bank carries out the actual purchases and the deviations $\xi_0(i)$ realise. Thus, the security market in $t = 0$ clears under a different condition

$$a_0^B(i) = a_0^*(i)(1 - \bar{\phi} - \xi_0(i)),$$

which implies a different bond price $Q_0^\xi(i)$ and capital installed $k_0(i) = e(i) + Q_0^\xi(i)a_0^*(i)$ from what the firm planned. Differentiating the intermediary's first-order condition (23) at the sunk quantity gives $\partial k_0 / \partial \xi_0 = a_0^* \partial Q_0 / \partial \xi_0 > 0$. A positive date-0 deviation raises the realised price of the whole sunk issue, thus installing more capital; production, market share and markup at date 1 then respond through the production block of Section 3.2 (paragraph “Intermediate Goods Producers: Production Decision”), at the expense of within-sector rivals. Notice that this is the only real effect a deviation can have at $t = 0$: issuance is sunk, so the channel is a revaluation, bounded by the price response.

Date-1 allocation under policy. Consider now a purchased firm ($\phi_0 > 0$) in the following period $t = 1$. Given the persistence rule assumption, it knows its date-1 fraction $\phi_1 = \rho\phi_0$ in advance and can make its second issuance decision accordingly. A higher ϕ_1 reduces the quantity the intermediary absorbs at any issuance level, raising Q_1 and lowering r_1 ; in other words, the user cost of such a firm is lower. Thus, the firm moves along its capital demand schedule: from (21), it does so according to

$$d \log k_1 = -G d \log (r_1 + \delta) \tag{24}$$

Contrast this with the previous period: there, the deviation moves capital only by revaluing a sunk position $a_0^* \partial Q_0 / \partial \xi_0$. Here, it moves capital by re-optimisation, (24), an elasticity- $G = \alpha(\eta - 1) + 1$ response of desired capital to the user cost. The two channels differ by orders of magnitude for the same price movement whenever $G > Qa/k$, which is always the case for sensible parameter configurations.

3.3.1 What the Policy Implies, and the Connection to the Empirical Results

Throughout, recall that the empirical results are identified from the implemented deviations from the announced market-neutral share, i.e. from $\xi_0(i)$ in the model's language.

No effect of secondary market purchases and no spillovers. Although the secondary market is not modelled explicitly, the irrelevance of purchases there follows directly: a purchase of a security that has already cleared the market changes neither the sunk quantity nor the realised price, and thus not the firm's installed capital; in turn, the entire production block is unchanged. Notice that purchasing the outstanding part of the long-term bond at $t = 1$ would affect the economy, but only through the holding costs h_1^B and h_1^{CB} ,¹⁵ i.e. through a decrease in the resource loss that financial intermediation implies. This outcome replicates the empirical finding that only issuance-week purchases affect capital investment and markups, as reported in Section 2.6. Similarly, separability between different types of firm securities implies that purchasing one cannot influence the pricing of the other, consistent with the absence of a detectable spillover effect of a purchase on the yield of the same issuer's later bonds at any horizon examined.

Only repeated purchases, and no capital response from dividend-paying firms. The gap between the revaluation channel at $t = 0$ and the re-optimisation channel at $t = 1$ is the single-purchase null of Section 2.6; Section 4.5 quantifies it. The most important implication is that the central bank's second-period securities purchase affects only constrained firms. For dividend-paying firms (i.e., unconstrained), short-term bond supply is perfectly elastic at the frictionless price $Q_1 = \beta$, because their internal-funds buffer is enough to finance completely the optimal capital needed for $t = 2$ final production; their user cost of capital therefore does not depend on the potential policy-purchased share ϕ_1 . Thus, even if the central bank did so, bond price Q_1 and desired capital k_1 would not change; equilibrium issuance a_1 would expand, but its returns would be used exclusively to pay out more dividends. This contrasts with two empirical findings: purchased firms are all dividend-paying, and the CSPP did not have a significant positive effect on dividend payouts.

4 Quantitative Model

The simple analytical model described in Section 3 can qualitatively reproduce the main empirical results from the previous section. However, as the last paragraph above shows, it faces one fundamental problem that prevents it from replicating the results quantitatively: identifying which firms drive the policy response. The simple model predicts that only constrained, non-dividend-paying firms will increase their capital investment and markups. In contrast, as Section 2.1 reports, purchased firms come mainly from the upper quartiles of the firm distribution, and they all pay

¹⁵ Since holding costs are quadratic and paid separately by the two financial intermediaries, splitting the same security's placement between them reduces total frictions. This result holds as long as the additional efficiency cost κ paid by the central bank is lower than the strength of the financial friction γ .

dividends on a habitual basis. This section adds the features that resolve this discrepancy, maps the model to the programme, validates it against untargeted moments and compares purchase designs on welfare.

4.1 From the Analytical to the Quantitative Economy

Four ingredients are needed for the analytical model to speak to the data properly: an adjustment cost on dividends, a corporate tax with deductible interest payments, a reference holding in the intermediary’s holding cost function, and a separate financial friction parameter for each of the firm’s two securities. Everything else is unchanged: the household, the final good producer, the strategic interaction between firms, the central bank’s actions and the resource constraints are those of Section 3. Moreover, the welfare expression is unchanged because the dividend cost is modelled as a transfer to the household.

Payout adjustment cost. Paying out a level of dividends at $t = 1$ different from the habitual level costs the firm

$$\varphi(div_1) = div_1 + \frac{\kappa_d}{k_0}(div_1 - \bar{d}(i))^2, \quad \kappa_d \geq 0,$$

as in [Jermann and Quadrini \(2012\)](#). The target $\bar{d}(i)$ is the firm’s habitual payout: specifically, it is set equal to the dividends it pays in the no-policy equilibrium of the economy with the cost switched off. A purchase does not influence this target, as it is a model-internal, predetermined object. As with the holding costs paid by the private financier, the quadratic term is scaled by assets in place, so a unique κ_d gives firms of every size the same payout sensitivity. This cost applies only to dividends paid in the middle period, since those paid at $t = 2$ reflect the firm’s liquidation in the last period. Moreover, I keep the no-equity-issuance constraint $div_1 \geq 0$. Introducing this feature means the marginal value of internal funds varies continuously across firms. Specifically, in the interior of dividend payment,

$$div_1 = \bar{d} + \frac{k_0}{2\kappa_d}\left(\frac{1}{\vartheta_1} - 1\right),$$

so a firm that has cut its payout below its habit values a marginal unit above one while still paying a dividend. This addition implies that the anticipated purchase at $t = 1$ shifts desired capital for every purchased firm, with an intensity that depends on how far the firm’s payout sits below its habit.

Tax advantage of debt. Firms pay a flat corporate tax τ on operating profit, with the realistic feature of deductible interest payments and depreciation; the revenue is rebated to the household. With the per-period rates implied by the bond prices as already used in the analytical model and defined in Appendix C.1 (paragraph “Interest-rate convention”), the firm’s date-1 budget constraint

becomes

$$\varphi(div_1) \leq (1 - \tau)\Upsilon_1 - a_0(1 - \lambda - \tau Q_0 r_0) + (1 - (1 - \tau)\delta)k_0 - k_1 + Q_1 a_1, \quad (25)$$

where $\varphi(div_1)$ is the adjustment cost on dividends just defined. In turn, the final period dividend liquidation becomes $(1 - \tau)\Upsilon_2 - (1 - \tau Q_1 r_1)(a_1 + \lambda a_0) + (1 - (1 - \tau)\delta)k_1$. The role of this tax is to give a firm that pays dividends a reason to hold debt at a spread. The marginal value of internal funds for a firm issuing in the interior region now satisfies $\vartheta_1 = \beta[(1 - \tau)/Q_1 + \tau]$. Thus, a firm priced at the safe rate has $\vartheta_1 = 1 - \tau(1 - \beta) < 1$, implying an incentive to borrow until its spread reaches the tax wedge $\tau(1 - \beta)/[\beta(1 - \tau)]$. The tax does not affect the capital condition: for every firm interior in issuance, the benefit and the cost of debt scale by the same factor, and the marginal product of capital equals the same user cost of capital $r_1 + \delta$ at any value of the Lagrange multiplier.

Reference holdings and per-security slopes. As in Section 3, the private intermediary's positions are measured in leverage units, funds placed relative to the borrower's balance sheet at the pricing date. For simplicity of exposition, define

$$x_0(i) \equiv \frac{Q_0(i)a_0^B(i)}{e(i)}, \quad x_1(i) \equiv \frac{Q_1(i)a_1^B(i)}{k_0(i)},$$

This section modifies the holding function so that the cost is charged only on the part of each position that exceeds a reference holding $\bar{x}_t$, common to all firms and specific to each type of security:

$$h_0^B = \int_j \sum_i \frac{\gamma_0}{2} e(i) \left([x_0(i) - \bar{x}_0]^+ \right)^2 dj,$$

$$h_1^B = \int_j \sum_i \left[\frac{\gamma_1}{2} k_0(i) \left([x_1(i) - \bar{x}_1]^+ \right)^2 + \frac{\gamma_0}{2} \frac{([\lambda Q_0(i)a_0^B(i) - \lambda \bar{x}_0 e(i)]^+)^2}{k_0(i)} \right] dj,$$

with $[u]^+ = \max\{u, 0\}$ coming from the fact that no cost is charged on positions below $\bar{x}_t$. Both the slope and the reference are a reduced form of modelling the preferred habitat preferences of investors (Vayanos and Vila, 2021; Selgrad, 2026 calibrates such a model to the Fed's purchases): the reference is what this literature calls the preferred position, the slope the cost of departing from it. The intermediary's first-order conditions give a piecewise price for each issue. Within the reference, the issue is priced at the same safe rate as before.¹⁶ Above it, the price solves a quadratic, and the spread over the safe rate is the slope times the excess leverage: at the repeat

¹⁶ Notice that a purchase of a bond in this region does not affect real variables, for the same reason as purchasing from an unconstrained firm in the analytical model is irrelevant.

issue, from the intermediary’s first-order condition,

$$\frac{1}{Q_1} - \frac{1}{\beta} = \gamma_1(x_1 - \bar{x}_1), \quad (26)$$

and analogously at the first issue, with the multi-period adjustments of the interest-rate convention of Appendix C.1. The reason for introducing the reference point is identification. With a cost through the origin, the spread is proportional to leverage, so the price effect of a purchase pins both the slope and the level of the implied spread, leaving no parameter to reconcile the two. In contrast, the reference gives the intermediary’s demand an intercept and a slope. Two slopes, γ_0 and γ_1 , are needed because the first bond is placed against the firm’s net worth and the second against its assets in place: the two leverages differ several times over, so one slope would price the second issue several times cheaper than the first, which the data reject.

4.2 Mapping the Model to the Programme

Period and units. One model period is one year. Bond-level moments enter in the same units as in the empirical section, i.e. percentage points of decrease in the yield at issuance and basis points of spread. Firm-level moments are converted from the panel’s asset base to the model-coherent capital base using the measured assets-to-capital ratio of the treated firms (1.55). This implies that a cumulative capital response of 6.4 percent of assets corresponds to a change in log capital of 0.099, with a confidence interval of [0.028, 0.171]; the payout response is converted in the same way (Section 4.4).

Externally set parameters. Appendix Table A-36 lists the parameters set outside the model. The safe rate is set at two percent; results at half a percent, three percent, and four percent are reported in the robustness appendix. The Cobb-Douglas capital share is set to the standard value of $\alpha = 1/3$. The substitution elasticities within and across sectors, 5.75 and 1.2, follow the values estimated by De Loecker et al. (2021), while the Frisch elasticity of labour supply, 0.25, follows Chetty et al. (2011). Similarly, I take the central bank efficiency cost of intermediation, 0.001, from Gertler and Karadi (2011). Finally, the share of the long-term bond that matures in the intermediate period, 0.75, is the annual translation of the value imposed by Jeenas (2026) to match an average expected maturity of four years. Several others are instead estimated from the panel dataset described in Section 2.1: the depreciation rate, 0.008, is the panel’s depreciation-to-gross-capital ratio, while the corporate tax rate, 0.246, is the panel’s mean effective tax rate. The productivity process is log-normal with the panel’s within-sector dispersion, 0.35, and persistence corrected for measurement error, 0.959. Two groups of objects are fitted inside every solve rather than set: the overhead labour cost, which reproduces the panel’s overhead-to-revenue share of 0.283 and its elasticity to size of 0.887, and the net-worth endowment, a quadratic in the log type that reproduces the panel’s net-worth-to-assets ratio in the mean (0.357) and in the bottom and top size deciles (0.352 and 0.364). Table 6 reports these values in panel B.

The eligible and the purchased sets. The model economy has twelve firms in each of forty-four sectors, which are taken from the panel’s number of NACE-2 sectors and the mean number of firms across them. Within each productivity quartile, firms are ranked by productivity, and the most productive are eligible up to the quartile’s eligible share in the panel: none in the first quartile, 5.7 percent in the second, 29.7 percent in the third, and 58.3 percent in the fourth. This gives 122 eligible firms out of 528, of which 7, 39 and 76 sit in the second, third and fourth quartiles. Purchased firms are drawn from the eligible set at the measured rate: the first eligible issue of an issuer is purchased with probability 0.574 (Table 1), so 70 of the 122 eligible firms are purchased. The draw is weighted by size so that the purchased firms’ share of eligible issuance matches the 0.649 measured in the bond data; large issuers are much more likely to be bought, but not with certainty, and the purchased firms fall in the four quartiles at 0, 1, 16, and 53. The draw is made once and reused at every parameter vector, so that no comparison in this section carries sampling noise from the draw (Appendix C.2.1).

Programme size and persistence. The announced benchmark $\bar{\phi}$ is computed as the share of eligible issuance the ECB bought at issuance over the programme’s net-purchase years, 2017 to 2022: primary-market holdings (plus the redemptions they replaced), over the face value of the entire eligible universe (excluding commercial paper). This methodology gives a value of 0.0853. Applying the same approach to the purchased set, it implies a purchase intensity ϕ_0 of 0.131 on each issue. Nothing about the programme is calibrated to a real outcome: size, reach and persistence are measured, and the model is asked what such a programme does. The persistence of effective purchases is set equal to the measured probability (Table 1) that an issuer purchased at its previous issue is purchased again within a year, i.e. 0.823. The additional values coming from the any-gap and ninety-day windows (0.760 and 0.903, respectively) are used for robustness checks.

4.3 Identification: Which Moment Pins Which Parameter

Five parameters remain: the slope and reference point of each security in the holding-cost functions and the payout curvature in the payout-adjustment costs. Each is disciplined by one bond-level or one firm-level moment. Table 6 reports the values, moments, and fit. I run several robustness tests: Table A-39 repeats the full calibration at safe rates of half a percent, three percent, and four percent, and re-pins the first-issue parameters to the alternative yield effect of -0.266 percentage points, estimated on the sample restricted to policy years.

The first-issue parameters. The slope and reference of the first security are solved jointly on two moments of purchased issuers: the yield effect of a purchase at issuance, -0.217 percentage points (Table 2), and the level of the spread at issuance. The spread is defined as the bond’s yield at issuance over the ECB’s AAA euro-area government seven-year spot rate. It is measured on the purchased bonds issued between July 2017 and December 2018, the net-purchase phase in which issuance-week purchases are observed and the same interest-rate regime from which the safe rate

Table 6: Calibrated parameters and their moments

Parameter	Value	Description	Moment	Model	Data
<i>A. Calibrated on moments</i>					
γ_0	0.00796	Slope of the holding cost, first issue	Empirical result: Yield effect of a purchase (pp)	−0.217	−0.217
$\bar{x}_0$	0.3306	Reference holding, first issue	Data: Bond spread over the AAA euro-area government curve (bp)	119.9	120.0
γ_1	0.065	Slope of the holding cost, repeat issue	Empirical result: Yield effect of a purchase (pp)	−0.191	−0.217
$\bar{x}_1$	0.46	Reference holding, repeat issue	Data: Bond spread over the AAA euro-area government curve (bp)	44.7	120.0
κ_d	0.1226	Payout-adjustment cost	Payout sensitivity to operating profit	0.0545	0.0545
<i>B. Fitted inside every solve</i>					
ψ_1	3.60e-04	Overhead labour, scale	Overhead cost over revenue	0.283	0.283
ψ_2	2.920	Overhead labour, curvature in type	Overhead cost elasticity to size	0.887	0.887
a	−6.616	Net worth, level	Net worth over assets, mean	0.357	0.357
b	3.571	Net worth, slope in log type	Net worth over assets, bottom size decile	0.352	0.352
ζ	−1.632	Net worth, curvature in log type	Net worth over assets, top size decile	0.364	0.364

Panel A: parameters chosen to match the moment in the same row; “Empirical result” moments are the paper’s own estimates (the first-issue yield effect, Table 2), “Data” moments are sample statistics (the spread at issuance over the AAA euro-area government curve; the payout sensitivity to operating profit). The $t = 1$ parameters are reported at the admissible point closest to their targets (Section 4.3). Panel B: objects fitted inside every solve. Model values at the headline calibration, safe rate 2 percent, repurchase probability 0.823.

of the calibration is read; its median value is 120 basis points.¹⁷ Through (26) the level moment reads off the excess leverage once the slope is known, while the yield effect, the revaluation of the sunk placement, depends on both; so the two are solved simultaneously, not one after the other. The solution is $\gamma_0 = 0.00796$ and $\bar{x}_0 = 0.331$, which reproduce −0.217 percentage points and 119.9 basis points.

The second-issue parameters, chosen by a rule. The same two moments exist at the second issue. However, these parameters cannot reach both moments, because they pull the reference point in opposite directions: raising it collapses the spread, because firms lever up toward it and the purchase – by taking part of the issue off the intermediary’s book – pushes the remaining position below the reference, where the issue is priced at the safe rate; however, it raises the yield effect for the same reason. Steepening the slope does not help either, because firms cut the repeat issue as the cost steepens, and the purchase itself bounds the funds a purchase lets a firm raise

¹⁷ Over the whole policy period, up to December 2024, the median spread of purchased bonds is 136 basis points, as the issues of 2022–2024 price in a rate level about three percentage points higher. A different spread level moves the reference holding, not the yield effect, which is matched by construction.

(Appendix C.1, paragraph “The bound at the repeat issue”). This second pair of parameters is therefore chosen by a rule fixed before the search: pick the one closest to the yield effect among the admissible points on a grid, while keeping the first-issue parameters fixed. A point is admissible if it passes four tests, listed in Appendix C.2.1. The results give $\gamma_1 = 0.065$ and $\bar{x}_1 = 0.46$, implying a second-issuance yield effect of -0.191 percentage points and a spread of 44.7 basis points. Figure A-9 in Appendix C.2.2 shows the grid search map.

The payout curvature. The identifying object for κ_d is the response of a firm’s date-1 dividend to a marginal, transitory unit of date-1 operating profit, everything else held fixed. In the model, at the no-policy point,

$$s \equiv \frac{\partial \text{div}_1}{\partial \Upsilon_1} = \frac{1 - \tau}{1 + 2\kappa_d \omega}, \quad \omega \equiv \frac{1}{\beta(1 - \tau)} \left[\frac{\alpha(\eta - 1) + 1}{\tilde{u}_1} \frac{k_1}{k_0} + \frac{1}{\gamma_1} \right],$$

ω is the absorption capacity of the two other margins complementary to the payout: investment and issuance. $\alpha(\eta - 1) + 1$ is the elasticity of desired capital to its user cost, equation (21). The mapping from κ_d to s is a strictly decreasing bijection, so a measured s returns one κ_d ; it is inverted, not searched. In the data, s is the coefficient of the change in dividends over assets on the change in gross profit net of overhead over assets, in first differences with year effects: 0.054 (Appendix Table A-37).¹⁸ The implied curvature is $\kappa_d = 0.1226$.

4.4 Untargeted Moments

Everything else the empirical part of the paper measures is not targeted and is thus used to validate the calibrated economy. Table 7 reports four responses of purchased firms against the corresponding empirical estimate and its 90 percent confidence interval: the capital response, the payout response, the markup response, and the response to a purchase that is not repeated. In each case, the model object is the purchase part of the programme, i.e. the realised path, the announced benchmark plus the measured deviations, against the benchmark alone, purchased minus never-purchased eligible firms. This is the right comparison because it is the one the difference-in-differences estimates of Section 2 measure: the announced share is common to all eligible firms and is differenced away.

Capital response. In the data, continuously purchased firms raise their investment rate by 2.2 percentage points of base-year assets per year, which cumulates to a capital-stock increase of 6.4 percent of assets over three years, i.e. a change in log capital of 0.099 with a 90 percent interval of $[0.028, 0.171]$ (Section 4.2). In the model, the purchased-minus-never-purchased difference in log capital is 0.031 . The response is therefore inside the interval, in its lower part, and about a third of the point estimate. I read this gap as a limitation of the model’s horizon rather than of its mechanism. A purchased firm in the model takes one anticipated issuance decision, the repeat issue

¹⁸ The profit measure is net of the depreciation allocated to cost of revenues and overhead, so it corresponds to the model’s operating profit net of depreciation.

Table 7: Untargeted moments

Moment	Model	Data	
		Estimate	90% interval
<i>Responses of purchased firms (purchased minus never purchased)</i>			
Capital, $\Delta \log k_1$	0.0310	0.099	[0.028, 0.171]
Payout, $\Delta(\text{div}_1/k_0)$, on pre-purchase capital	0.00546	−0.0121	[−0.0298, 0.0055]
Markup, $\Delta \log \mu_2$	0.00205	0.069	[0.020, 0.119]
Single purchase effect on capital ($\Delta \log k_1$)	0.0009	−0.0122	[−0.0352, 0.0107]

Model responses are the purchase part: the realised path minus the announced benchmark, purchased minus never-purchased firms, at the headline calibration (safe rate 2 percent, repurchase probability 0.823). Data: capital, the cumulative three-year effect of the investment regression converted to log capital with the assets-to-capital ratio of treated firms, 1.55 (Table 3); payout, the pooled effect of the dividend regression in the same units (Appendix Table A-34); markup, the pooled effect of the markup regression (Table 4); single purchase, the one-shot column of the sequence split (Table 5). Intervals are 90 percent.

at $t = 1$, knowing that it will be bought with the measured probability. A continuously purchased firm in the data receives seven to eight over three years, with purchases worth close to nine percent of its assets. The model matches the dose correctly – the programme’s size and the purchased firms’ share of eligible issuance; what it cannot reproduce is how often the dose is supplied. The “Persistence” paragraph in Section 4.5 shows that the full capital response depends on anticipating a single repeat purchase so that a longer horizon would increase it.

Payout response. The empirical part finds no change in the dividends of purchased firms: in the model’s units, the pooled estimate is -0.012, with interval [-0.030, 0.0055]. The model’s purchased-minus-never-purchased change in the dividend-to-capital ratio is 0.00546, inside the interval and at its upper end. Notice that the interval width depends on the dividend regression window: over the two years after purchase instead of three, its upper end would be 0.015; thus, the model response would comfortably sit within it.

Markup response. Purchased firms raise their markup by 6.9 log points in the data, i.e., 7.1 percent, with a 90 percent interval of [2.0, 11.9] log points. In the model, the purchased-minus-never-purchased change in the log markup is only 0.2 log points, one tenth of the lower end of the interval. This is the one validation the model fails, and the failure is systematic: the pass-through of the capital response to the markup, the ratio of the two responses, is 0.066 in the model against about 0.7 in the data.¹⁹ The reason is the market structure of the model. In the nested-CES Cournot block, a firm’s markup rises with its market share, and the share responds to capital through the production function, so that the pass-through of capital to the markup is bounded by the capital share and the demand elasticities; at the calibrated values, no parameter brings it

¹⁹ The reason is not measurement. If part of the cost of goods sold were fixed, the production-approach markup of Section 2 would overstate the true response; but the elasticity of the cost of goods sold to revenue in the panel is 0.97, so the measured markup tracks the true one.

near the data. Appendix C.2.4 traces this pass-through gap. The consequence for everything that follows is that every market-power effect reported in this section is a lower bound on the effect the programme had.

Single purchase. The last check is a model prediction rather than a validation, since the analytical part of the paper derives it. A purchase that is not repeated acts only through the revaluation of the sunk first issue, so its capital response is 0.0009 at a repurchase probability of zero, against a single-purchase estimate of -0.012 with interval $[-0.035, 0.011]$. Two of the four ingredients that separate the quantitative economy from the analytical one – the payout adjustment cost and the reference holding – are not there for realism alone: three simpler economies, built and calibrated before this one, reproduce the measured yield effect but fail to deliver the capital response together with the payout null, and the way each fails motivates the corresponding ingredient in the model (Appendix C.2.3).

4.5 The Mechanism in the Calibrated Economy

Who transmits. In the calibrated economy, every firm prices both of its issues on the sloped part of its schedule: no firm, purchased or not, sits within its reference holding at either issuance, with or without the policy (Appendix Table A-40). The payout margin then determines who transmits. Of the 70 purchased firms, 67 pay a dividend under no policy and 69 on the realised path, so dividend-paying firms carry almost all of the capital response. This is the feature the analytical economy could not deliver, and it comes from the marginal cost of the payout: a firm that would pay out the full cost relief when unrestricted keeps part of it when paying out above its habitual level is costly; that part becomes capital.

The bound at the second issuance. A second feature of the mechanism explains why the yield effect at the repeat issue cannot be matched: the purchase raises the price of the issue only because the intermediary now holds less of it, so part of every unit the central bank buys merely replaces one the intermediary would have placed, and the firm cannot raise more than the purchase (Appendix C.1, paragraph “The bound at the repeat issue”). In the calibrated economy, against the announced benchmark, purchased firms raise 0.52 units of extra funds at the repeat issue for every extra unit the central bank buys there (0.53 for the median firm, between 0.43 and 0.80 across firms), and install 0.45 units of extra capital. The bound also explains why steepening the intermediary’s cost does not help match the second yield effect: a steeper cost makes the firm issue less, and the relief per euro bought falls with it.

Persistence. How much of the programme’s real effect depends on anticipating the repeat purchase can be read by varying the repurchase probability alone while keeping the rest of the calibration fixed (Appendix Table A-41). The capital response of purchased firms is 0.0009 when the purchase is not repeated, 0.0286 at a repurchase probability of 0.760, 0.0310 at the measured 0.823,

0.0341 at 0.903 and 0.0380 under certain repetition. In other words, 97 percent of the response at the measured persistence comes from the anticipated repeat purchase. A programme that does not repeat its purchases delivers the revaluation of an issue already in place and little else, which is the result of the analytical part and the counterpart of the single-purchase null.

Rivals and the common component. Table 8 decomposes the change in each group’s sales-weighted markup at $t = 2$ into the two parts of the programme: the announced benchmark, which lowers the cost of external finance of every eligible firm and is differenced away in the estimates of Section 2, and the realised purchases, the part those estimates measure. The announcement raises the markup of all eligible firms by 0.117 percent for those that will be purchased and 0.099 percent for those that will not, and lowers it for ineligible firms by 0.036 percent, as their eligible rivals expand. The two eligible groups differ under the same announced share because of their composition: the purchased set is drawn weighted by size, so it includes firms with larger market shares, whose markups respond more to the same expansion under Cournot competition. The purchases then move the same resources from the eligible firms that are not bought to the ones that are: the purchase part is +0.079 percent for purchased firms and −0.137 percent for the never-purchased eligible ones, which issued at date 0 expecting the announced share and were not bought; ineligible firms gain 0.007 percent, recovering part of the share that the never-purchased rivals in their sectors lose. In total, the economy-wide markup rises by 0.103 percent, of which 0.088 comes from the announcement. The purchase part is thus a reallocation of the announced support rather than a purchase on top of it (Appendix C.3.1 makes this reading clear). A purchased firm’s larger capital lowers the never-purchased market shares and their operating profits, and a constrained rival installs less capital. The levels in the table are lower bounds for the reason given in Section 4.4.

Table 8: Markup change at date 2 by group of firms

	Announcement part	Purchase part	Total	Single purchase
Purchased	+0.1170	+0.0794	+0.1964	+0.0117
Eligible, never purchased	+0.0986	−0.1369	−0.0383	+0.0065
Ineligible	−0.0359	+0.0074	−0.0285	−0.0033
All firms (economy-wide)	+0.0881	+0.0151	+0.1032	+0.0084

Percent change of each group’s sales-weighted average markup at date 2 from its no-policy level, headline programme. Announcement part: the announced, market-neutral benchmark against no policy; purchase part: the realised path at repurchase probability 0.823 against the benchmark; the two add to the total. Single purchase: the headline’s purchased firms bought only at the first issue, against no policy. Groups: purchased firms, eligible firms never purchased, ineligible firms; the last row is the whole economy.

What the equilibrium concept leaves out. Capital is chosen in the model as a Nash equilibrium in plans. Each firm takes its rivals’ output paths as given when it chooses how much capital to install, and the strategic interaction is confined to the production stage. A firm that anticipated

its rivals' output response to its own capital would value capital differently, and this subsection measures by how much. For each firm and each production date, I compute two objects at the calibrated equilibrium. The first is the direct return to capital: the marginal revenue product of the firm's capital with its rivals' outputs held fixed, which the first-order condition in Section 3 equates to the user cost. The second is the strategic term: the change in the firm's operating profit that comes from its sector rivals' output response to a marginal unit of its capital. The ratio, computed by perturbing one firm's capital and re-solving the sector's Cournot equilibrium at fixed aggregates (Appendix C.2.1), is reported as a sales-weighted average within groups of firms. The strategic term is positive for all 528 firms at both dates because a rival cuts output when the firm adds capital, raising the firm's price. Its size is 14.5 percent of the direct return for purchased firms, 10.8 percent for never-purchased eligible firms, 5.8 percent for ineligible firms and 9.8 percent economy-wide; it rises with size, from below one percent in the smallest quartile to 13 percent in the largest and 16 percent for the largest tenth. The sign is what matters for reading the results. A firm that internalises the term would invest more than the model's firm does, and more so as it grows, so the open-loop assumption understates the capital response of purchased firms and, in turn, their markup response. Both are therefore lower bounds on this account as well; I leave the closed-loop solution for future work.

4.6 Different Purchase Designs

The last question the quantitative model can answer is what the central bank could have done with the same policy size. Every design below spends the same amount of the headline programme at the first issue, keeps the measured repurchase probability and the announced benchmark, and changes only which issues the programme reaches. The per-issue intensity implied by each design is reported; where it would exceed the programme's 70 percent per-issue limit, the constraint binds, and the design is evaluated there. Table 9 reports the designs, Figure 4 shows their output, markup, and welfare effects, and Table 10 reports the welfare-effect decomposition. Welfare is the discounted utility of the representative household, which works, owns every firm, and receives every rebate; I report its change as a percent of no-policy welfare and per euro bought.²⁰ Before ranking the designs, it is important to recall the caveat of Section 4.4: the markup effects below are lower bounds, so the ranking of designs by welfare, which rests on the allocation of inputs rather than on the level of markups, is the robust object of this subsection.

The designs. The paper compares nine designs. The headline programme is the calibrated one, which buys 70 firms. The announced policy with equal shares buys 8.53 percent of every eligible firm in the first issue and 0.823 times that share in the repeat issue; it is the announcement carried out without deviations, at the headline programme size. Three designs concentrate the purchases on one size cell of the eligible set: the mid-size firms, i.e. all eligible firms in the second and third

²⁰ The welfare level includes the date-0 endowment, a normalisation that does not affect allocation. In units that do not depend on it, the headline programme's welfare gain is 0.26 percent of one year's output. Appendix C.3.1 discusses the units.

Table 9: Purchase designs at the headline programme’s size

Design	Firms bought	Share bought per issue	Programme size (headline = 1)	Capital gained per euro	Output, date 2 (%)	Markup, date 2 (%)	Welfare (%)	Welfare per euro
Headline programme	70	0.131	1.000	0.205	0.437	0.103	0.0124	0.0240
Announced policy (equal shares)	122	0.085	0.993	0.206	0.427	0.073	0.0124	0.0241
Mid-size firms (size quartiles 2–3)	46	0.373	1.032	0.238	0.449	−0.055	0.0084	0.0158
Large firms (top size quartile)	76	0.111	1.006	0.205	0.443	0.111	0.0126	0.0243
Largest 10% of large firms	8	0.700	1.041	0.117	0.303	0.188	0.0058	0.0108
Low payers (bottom quarter by payout)	31	0.433	1.064	0.254	0.539	0.038	0.0102	0.0184
Wider eligibility (headline purchase rule)	99	0.120	1.006	0.213	0.446	0.076	0.0126	0.0242
Wider eligibility (announced policy)	173	0.078	0.999	0.211	0.434	0.055	0.0125	0.0243
Wider eligibility (smallest firms first)	58	0.606	1.000	0.237	0.414	−0.094	0.0039	0.0076

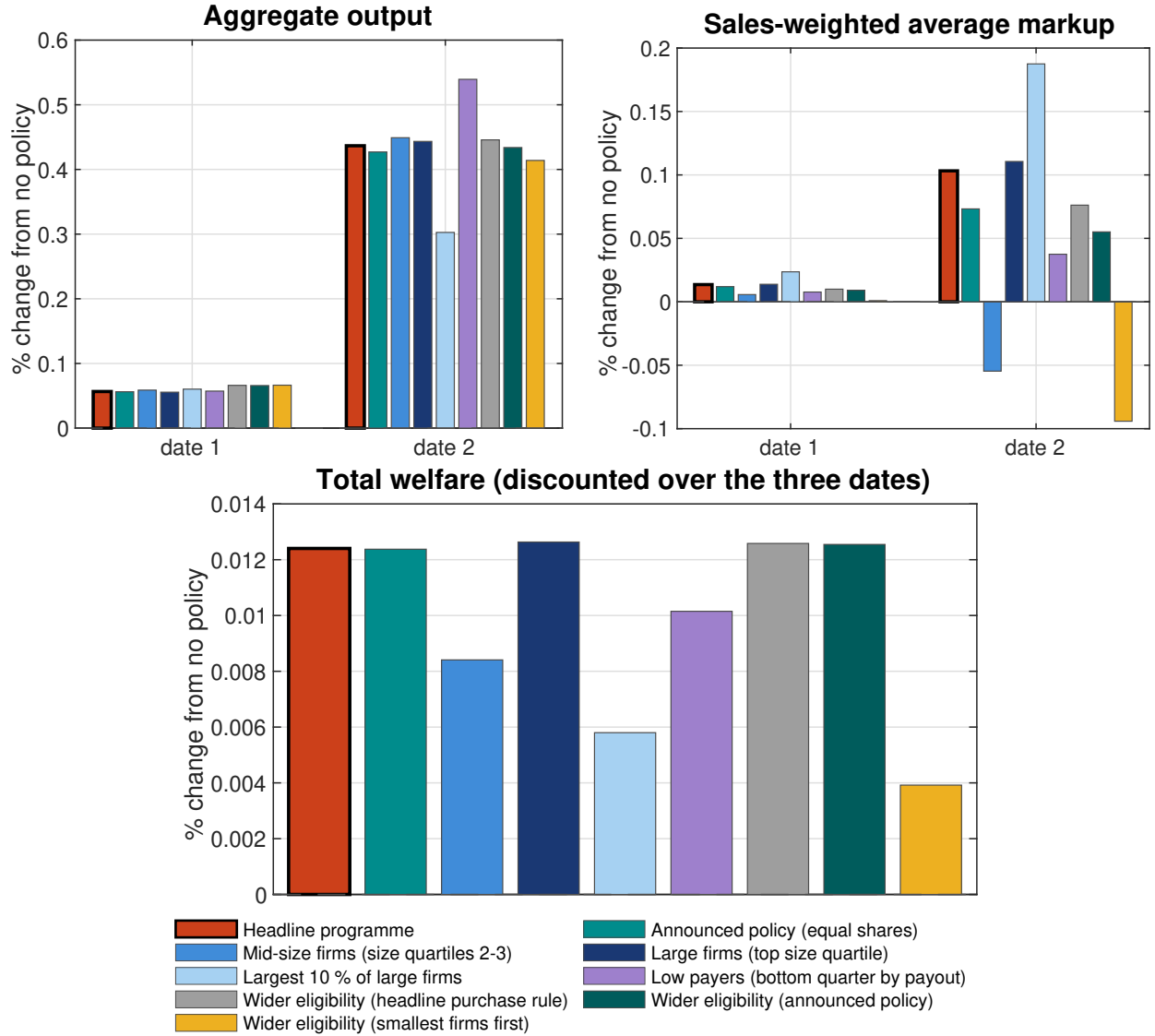
Every design has the headline programme’s size at the first issue; the size over both dates follows each design’s repeat issuance. Share bought per issue: the per-issue intensity on the purchased firms, euro-weighted where it varies. Capital gained: the economy’s date-1 capital against no policy, per euro bought at face value over both dates. Output and the sales-weighted average markup at date 2, and welfare, are percentage changes from no policy; welfare is the discounted sum over the three dates with the central bank’s holding cost in the resource constraint; welfare per euro is the change in levels per euro bought. Repurchase probability 0.823, safe rate 2 percent.

size quartiles (46 firms); the large firms, i.e. all eligible firms in the fourth quartile (76 firms); and the largest tenth of the large firms (8 firms, where the maximum constraint binds). The low payers are the eligible firms in the bottom quarter of the payout rate under the benchmark (31 firms). The last three designs widen eligibility, raising the eligible shares of the first and second size quartiles to 0.15 and 0.30, which brings the eligible set from 122 to 173 firms: with the headline purchase rule (99 firms bought), with no deviations from the announcement, and purchasing from the smallest eligible firms first at the cap (58 firms). The wider designs keep the announcement at 8.53 percent of every issue on the wider set, so their realised purchases fall short of the announcement by about nine percent; this holds for all three.

Comparison results. Two regularities emerge from the table, and the first of them is the expected one. Designs that reach smaller or more constrained firms add more capital per euro bought: 0.254 for the low payers, 0.238 for the mid-size firms and 0.237 for the smallest firms first, against 0.205 for the headline programme and 0.117 for the largest tenth. The same designs raise the economy-wide markup less, or lower it: the mid-size design lowers the sales-weighted markup at $t = 2$ by 0.055 percent and the smallest-first design by 0.094 percent, versus a rise of 0.103 percent under the headline programme and of 0.188 percent when the largest tenth is bought. The second regularity is that welfare follows neither ranking. Five designs deliver almost the same (and largest) amount of gains: the large firms (0.0126 percent of no-policy welfare), the wider eligibility with the headline rule (0.0126), the wider eligibility with equal shares (0.0125), the headline programme (0.0124) and the announced policy with equal shares (0.0124). The low payers follow at 0.0102, then the mid-size firms at 0.0084, the largest tenth at 0.0058 and the smallest firms first at 0.0039.

Where the welfare change comes from. In order to decompose the reason for the result on welfare, Table 10 writes the change of each design, through the resource constraints, as the sum of five terms: the extra output, the cost of the extra capital, the private holding cost saved, the central

Figure 4: Output, markups and welfare by purchase design



Percent change from no policy. The legend applies to all panels. Repurchase probability 0.823, safe rate 2 percent. The per-euro panel is in Appendix Figure A-10.

bank's holding cost and the extra work. Two features drive the ranking of designs. The first is the value of a unit of capital where it is added. A firm with markup μ equates the value of the marginal product of its capital to μ times its user cost, while the social cost of that unit is the safe rate plus depreciation; the gap between the two is a wedge of market power plus a wedge of finance, and it is largest at large, high-markup firms. To a first order, the welfare change is the sum over firms of this wedge times the capital added, plus the labour wedge times the hours added, minus the change in holding costs: the misallocation accounting of [Baqaee and Farhi \(2020\)](#) enhanced with a holding-cost analysis, which Appendix C.3.1 writes out. The second fact is the central bank's convex holding cost: concentrating the same programme size on fewer issues raises it more than proportionally. It costs 0.0047 percent of welfare when the largest tenth is bought, 0.0054 under the

smallest-first design, and 0.0033 for the low payers, versus 0.0008 under the headline programme. The largest tenth is an instructive example: it adds the least capital of any design, and of the 0.0066 percentage points by which its welfare falls short of the headline’s, about 60 percent is the central bank’s cost.²¹ In conclusion, the lesson for the design of a corporate purchase programme is negative as much as positive: at a given size, which of the large eligible firms the programme reaches matters little for welfare, while concentrating them on a few firms, or directing them to the smallest ones, costs welfare.

Table 10: Welfare change by component and design

Design	Extra output	Cost of the extra capital	Private holding cost saved	ECB holding cost	Extra work	Total	per euro bought
Headline programme	+0.0232	−0.0114	+0.0039	−0.0008	−0.0024	+0.0124	0.0240
Announced policy (equal shares)	+0.0227	−0.0114	+0.0040	−0.0005	−0.0025	+0.0124	0.0241
Mid-size firms (size quartiles 2–3)	+0.0239	−0.0135	+0.0041	−0.0028	−0.0032	+0.0084	0.0158
Large firms (top size quartile)	+0.0234	−0.0114	+0.0037	−0.0006	−0.0024	+0.0126	0.0243
Largest 10% of large firms	+0.0171	−0.0073	+0.0021	−0.0047	−0.0014	+0.0058	0.0108
Low payers (bottom quarter by payout)	+0.0280	−0.0147	+0.0034	−0.0033	−0.0033	+0.0102	0.0184
Wider eligibility (headline purchase rule)	+0.0241	−0.0122	+0.0041	−0.0007	−0.0027	+0.0126	0.0242
Wider eligibility (announced policy)	+0.0235	−0.0120	+0.0042	−0.0005	−0.0027	+0.0125	0.0243
Wider eligibility (smallest firms first)	+0.0226	−0.0134	+0.0036	−0.0054	−0.0034	+0.0039	0.0076

Each column is one term of the discounted welfare change from no policy, $\Delta\mathcal{W} = \sum_t \beta^t (\Delta C_t - \Delta v(N_t))$, rewritten with the resource constraints (equation 13), in percent of no-policy discounted welfare; each design’s total equals its welfare cell in Table 9. Extra output: the discounted change in output at dates 1 and 2. Cost of the extra capital: the discounted change in investment net of undepreciated capital. Private holding cost saved and central-bank holding cost: minus the discounted change in each intermediary’s holding cost. Extra work: minus the discounted change in the disutility of labour. Total per euro: the change in levels per euro bought at face value over both dates.

Market power and welfare. Under Cournot competition, a purchased firm’s expansion and its markup increase are the same movement, and welfare, to first order, counts markups only through where the inputs go: moving inputs toward high-markup firms raises allocative efficiency even though their markups rise, because the markup increase only dampens the expansion. This is why the design that raises the aggregate markup most, the largest tenth, loses welfare through the central bank’s cost and the little capital it adds rather than through its markups, and why the designs that lower the aggregate markup do not gain from doing so. The market-power cost of the programme is real, and a lower bound, in the distribution of markups across firms, since in every design the within-sector rivals of the purchased firms lose market share and operating profits; in the welfare of a representative household that owns all the firms, however, it shows up only through the allocation of inputs.

²¹ Appendix Table A-43 removes that cost in two steps: without the efficiency cost of Gertler and Karadi (2011), which is about five percent of it, no design changes rank, while without the whole holding cost the low payers would rank first.

5 Conclusion

This paper studies the effects of central bank purchases of corporate bonds, focusing on the European Central Bank’s Corporate Sector Purchase Programme (CSPP). Specifically, it asks whether this policy affects firms’ financing and real activity, and whether this, in turn, affects their strategic interactions and pricing power in the product market.

The empirical analysis shows that firms purchased under the CSPP have been able to increase their markups compared to equally eligible but not purchased firms. The paper presents these results after constructing a security-level dataset from publicly available data that identifies which bonds and issuers were purchased and when, and distinguishes between issuance-week purchases – a proxy for primary market purchases – and secondary market purchases. This distinction turns out to be crucial for identifying which types of purchases have real effects.

The paper first establishes that bonds purchased at issuance by the ECB have been priced with lower yields to maturity, with an estimated reduction of 22 basis points. This effect is about a fifth of the spread over the safe rate that these issuers pay on average at issuance (around 120 basis points). Next, I analyse whether this financing advantage has translated into any effect on firms’ capital investment. Using a local-projections difference-in-differences estimator built for staggered and non-absorbing treatments, I document that firms purchased at least once a year over three consecutive years have increased their investment rate by a pooled 2.2 percentage points. Finally, applying the same empirical design to these firms’ estimated markups shows that purchased firms increased this measure of pricing power by 7.1 percent. Moreover, I conduct further analyses to better understand which types of purchases move real variables, a result not found in the literature. My findings reveal that only issuance-week (primary) purchases repeated over consecutive years can do so, while the announcement of the policy, secondary and isolated purchases have no detectable effect.

To rationalise these findings, I develop a real three-period general equilibrium model in which central bank purchases reduce the financial frictions to which heterogeneous firms are subject. These producers also engage in strategic interactions in the product market. This feature gives rise to the empirically documented transmission channel: the policy raises the price of a purchased issue, relaxes capital financing, and finally raises the output, market share, and markup of the selected issuer. The model also delivers the empirical null results as its own predictions: a purchase moves capital only when it is anticipated at issuance, so that purchases after placement are neutral and a single purchase acts only through the revaluation of an issue already sold.

After enriching the model with realistic features to better match the data, I calibrate it to the dataset and the estimates in the paper’s empirical section. In terms of validation, it replicates several untargeted findings, even if it under-delivers the markup response of purchased firms, so every market-power effect it produces is a lower bound. With this caveat in mind, I use this quantitative model to compare nine different policy designs, looking at the aggregate consequences

of targeting various types of firms with the same programme size. The result is that several purchase designs perform almost identically in terms of welfare effects, with one being the current ECB design with deviations from market neutrality, while concentrating the programme on a few large firms or directing it to the smallest eligible ones costs welfare.

One limitation points to future work. Firms choose capital taking their rivals' plans as given, and the strategic term the paper measures, positive for every firm, indicates that a closed-loop solution would raise both the capital and the markup response of purchased firms; solving the model under that equilibrium concept is the natural next step, and a candidate explanation for part of the markup gap. The empirical lesson, however, does not depend on it: a programme designed to be market-neutral, and implemented against a neutrality guideline that this paper controls for throughout, nonetheless changes relative positions in the product market, because central bank purchases, however neutral in portfolio weights, are not neutral in who, when and how gets funded at the margin.

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Appendix for “The Effects of Corporate Purchase Programmes on Market Power”

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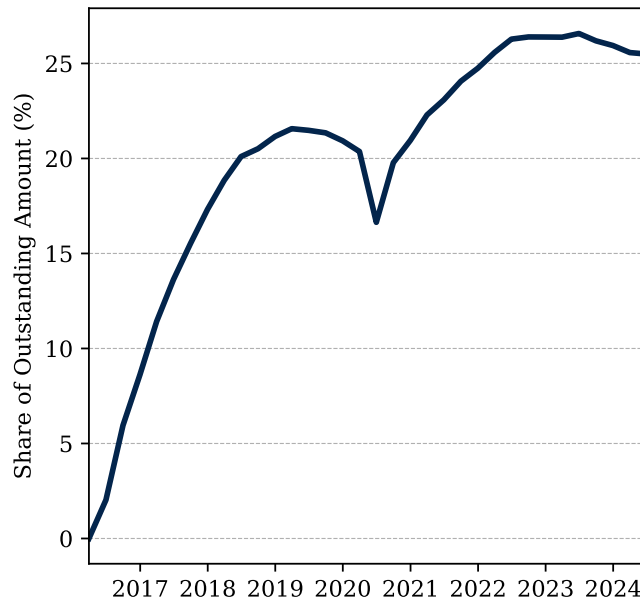
A Policy Background and Motivation

While in the US the Federal Reserve has limited itself to purchasing only government bonds and Asset-Backed Securities¹, the European Central Bank (ECB) has developed a more sophisticated policy package. The ECB’s “Asset Purchase Programme” (APP) consists of four main branches. The main one is the “Public Sector Purchase Programme” (PSPP), which buys central government bonds; as of September 2026, it accounts for around 80% of APP holdings (European Central Bank, 2026). The others are the “Covered Bond Purchase Programme” (CBPP3), the “Asset-Backed Securities Purchase Programme” (ABSPP), and the “Corporate Sector Purchase Programme” (CSPP). As their titles suggest, the first consists of the purchase of covered bonds, the second of asset-backed securities, and the last of bonds issued by private firms. With a portfolio of €214 billion, the CSPP constitutes the second most important programme, accounting for slightly more than 10% of the total APP holdings (European Central Bank, 2026).

This section of the APP, the focus of this paper, was announced on the 10th of March 2016 (European Central Bank, 2016b), while purchases began on the 8th of June of the same year. Six national central banks, coordinated by the ECB, carried out the purchases in both the primary and secondary markets. After that, the ECB adopted different approaches on how to invest in the corporate bond market (European Central Bank, 2026): (i) conducted net purchases (June 2016 to December 2018 and November 2019 to June 2022); (ii) only reinvested the principal payments from maturing securities (January to October 2019 and July 2022 to June 2023, only partially from March 2023); (iii) discontinued all reinvestments (from July 2023). The CSPP holdings stood at €92 billion in June 2017, around 11% of the eligible bond universe, spread over around 950 securities issued by around 200 issuer groups (European Central Bank, 2017); they reached €185 billion at the end of 2019 and €310 billion at the end of 2021 (Corsi and Mudde, 2022). Figure A-1 shows the weight that the Eurosystem acquired in this market over time. Moreover, the ECB gradually increased its primary-market operations: by mid-2017, it had purchased 15% of holdings there (European Central Bank, 2017), and 22.9% by the end of 2021 (Corsi and Mudde, 2022).

¹ On the 23rd of March 2020, the Fed announced the Secondary Market Corporate Credit Facility (SMCCF), a programme similar to the CSPP in the Eurozone. However, it consisted only of secondary-market purchases and ran until the 31st of December 2020. For such reasons, in this project I focus on the Corporate Purchases carried out by the ECB.

Figure A-1: Eurosystem holdings as a share of the corporate bond market



Reproduced from [Corell et al. \(2025\)](#), Figure D1, page 61.

A.1 Eligibility Criteria

As mentioned in the introduction, the CSPP has some criteria to determine which bonds can be eligible for the programme ([European Central Bank, 2016a](#), Article 2), which are:

- the bond must be eligible as collateral for Eurosystem credit operations;
- the bond must be denominated in euro;
- the bond must have a minimum first-best credit assessment of at least BBB- or equivalent (obtained from an external credit assessment institution);
- the bond must have a minimum remaining maturity of six months and a maximum remaining maturity of less than 31 years;
- the issuer must be a corporation established in the euro area, defined as the location of incorporation of the issuer;
- the issuer must not be a credit institution nor have any parent undertaking which is a credit institution.

Moreover, the Eurosystem cannot hold more than 70% of any single bond ([European Central Bank, 2016a](#), Article 4(1)).

A.2 Market Neutrality

In pursuing its objective, the ECB is mandated to act in accordance with the principle of an open market economy with free competition. As a consequence, the ECB aims for a market-neutral implementation of the APP ([European Central Bank, 2017](#), p. 40). In other words, the purchases should not alter the relative conditions of issuers within the eligible universe. In the words of the ECB’s President, the Eurosystem “does

not discriminate on the basis of the issuers’ economic activity when determining asset eligibility and actual purchases” (Draghi, 2018).

A.3 The Benchmark

The tool designed to reach such neutrality is a benchmark. Here I report an extract from the press release announcing the details of the programme (European Central Bank, 2016b):

A benchmark will be defined at issuer group level. The benchmark will be neutral in the sense that it will reflect, in proportion, all outstanding issues qualifying for the benchmark. This also implies that market capitalisation provides a weighting for each of the jurisdictions of issuance within the benchmark. Issuer group limits will be based on the benchmark to ensure a diverse portfolio, while still offering sufficient leeway to build the portfolio.

The legal act states the same rule: the purchase limits for issuer groups are based on “a benchmark allocation related to an issuer group’s market capitalisation” (European Central Bank, 2016a, Article 4(3)). Importantly, market capitalisation refers here to the nominal outstanding amount of eligible bonds issued by the issuer group, as a share of the entire CSPP-eligible universe (Draghi, 2017b; European Central Bank, 2019).² This is the measure that in the main text is defined as “CSPP benchmark”.

A.4 Deviations from the Benchmark

In other words, purchases follow a guideline, while allowing some deviations for specific acquisitions. The holdings are oriented toward an issuer’s share in the benchmark, and deviations are allowed to the extent determined by the limits that apply to each issuer, which include some leeway (Draghi, 2017a). Neither the limits nor the leeway have been published. Such deviations are visible in the data. First, the holdings contain a higher share of recently issued bonds than the benchmark (De Santis et al., 2018). Second, since 2017 the ECB has published twice a year a comparison of its holdings with the eligible universe by country, rating, and sector; Figure A-2 shows how the two differ persistently over time.

² European Central Bank (2017) describes the benchmark as reflecting the market value of eligible bonds, while reporting its own comparison in nominal values. I follow the definition in nominal amounts, as in all other sources.

Figure A-2: Deviations of CSPP holdings from the eligible universe



Deviation of the CSPP holdings share from the eligible-universe share, in percentage points, at semi-annual dates from 2017H2 to 2024H1, after which the ECB discontinued the comparison. Panel (a): five large countries, by the ECB's country of risk; panel (b): rating buckets; panel (c): the six ECB sectors with the most ever-eligible firms in the estimation panel. Shares are on a volume basis; holdings are stocks, not flows. Appendix Table A-1 reports all fourteen sectors. Source: ECB, CSPP holdings breakdowns by country, rating and sector (semi-annual).

Table A-1: Deviations of CSPP holdings from the eligible universe

	2017H2	2019H1	2021H1	2023H1	2024H1	Mean dev
<i>(a) Countries (country of risk)</i>						
France	-2.00	-1.00	0.00	3.00	4.00	2.07
Germany	0.00	-2.00	-2.00	0.00	-1.00	1.21
Italy	-1.00	0.00	2.00	0.00	0.00	1.14
Netherlands	1.00	1.00	0.00	-1.00	-2.00	0.86
Spain	1.00	2.00	1.00	2.00	4.00	1.86
<i>(b) Rating buckets</i>						
A	-2.00	-1.00	-1.00	0.00	-1.00	1.36
AA	-2.00	-1.00	-1.00	-1.00	0.00	0.71
BBB	4.00	2.00	1.00	0.00	1.00	1.71
<i>(c) Sectors (all)</i>						
Automotive and parts	0.00	-2.00	-1.00	-1.00	-1.00	1.00
Beverages	1.00	1.00	1.00	0.00	1.00	0.79
Chemicals	1.00	1.00	1.00	0.00	1.00	0.64
Construction and materials	1.00	1.00	0.00	1.00	0.00	0.50
Energy and basic resources	-1.00	0.00	0.00	0.00	0.00	0.36
Food	1.00	1.00	1.00	1.00	0.00	0.79
Health care and life science	1.00	0.00	1.00	1.00	1.00	0.64
Infrastructure and transportation	-2.00	-1.00	-1.00	-2.00	-2.00	1.43
Insurance	0.00	0.00	0.00	0.00	0.00	0.07
Other sectors	0.00	1.00	0.00	0.00	-1.00	0.50
Real estate	0.00	-1.00	-2.00	-2.00	0.00	1.21
Technology	0.00	0.00	0.00	1.00	2.00	0.57
Telecommunication	1.00	0.00	2.00	1.00	1.00	1.43
Utilities	-1.00	-1.00	0.00	-2.00	-2.00	1.07

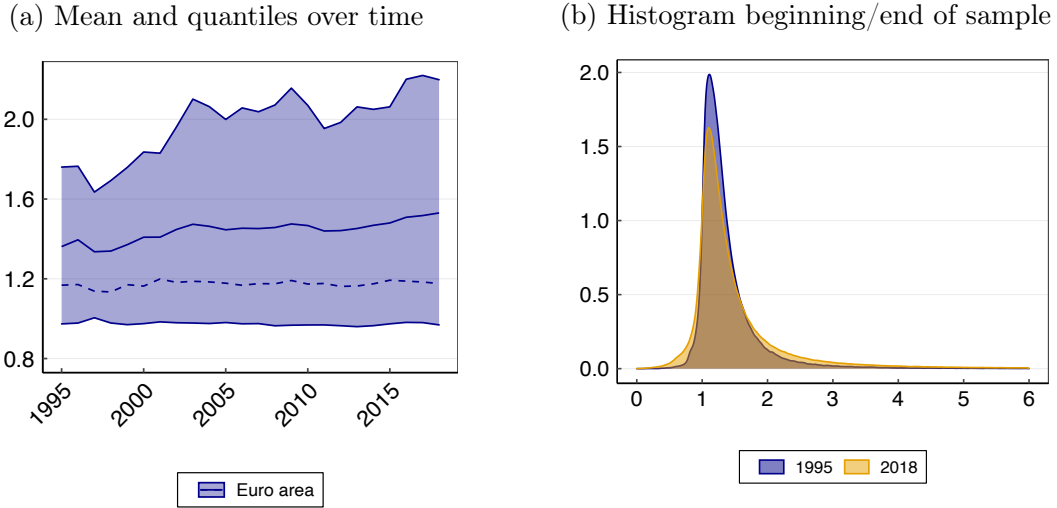
Deviation of the CSPP holdings share from the eligible-universe share, percentage points, at selected semi-annual dates, with the mean absolute deviation over 2017H2–2024H1. Country is the ECB’s country of risk; sector labels are the ECB’s own classification, all fourteen sectors. Shares are on a volume basis; holdings are stocks. Source: ECB, CSPP holdings breakdowns (semi-annual).

A.5 Later Statements of the ECB

Finally, it is worth noting how the ECB itself changed its position on market neutrality over time. In 2021, a member of its Executive Board acknowledged that applying this principle implies that the CSPP exhibits “an inherent bias towards large firms in carbon-intensive industries”, because such firms are more likely to enter the bond market (Schnabel, 2021). In 2022, the President described market neutrality as “an operational tool, not a legal requirement” (Lagarde, 2022). On the other hand, regarding the question of this paper, the position of the ECB is that the effects of its corporate bond purchases on competition and market concentration are “expected to have been minimal at most” (Lagarde, 2021).

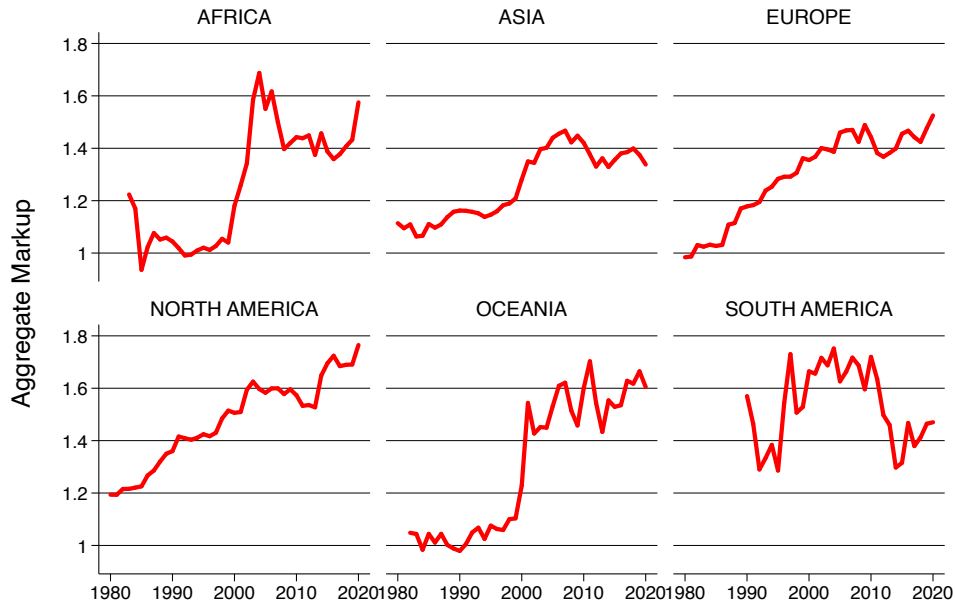
A.6 Market Power in Europe and the Firm Distribution

Figure A-3: Markup distribution in the euro area, 1995–2018



Reproduced from [Kouvavas et al. \(2021\)](#), Figure 3, page 15: weighted mean, median and the 10th–90th percentile band of firm-level markups. This paper uses the same production approach, with COGS as the flexible input and sector output elasticities constant over time.

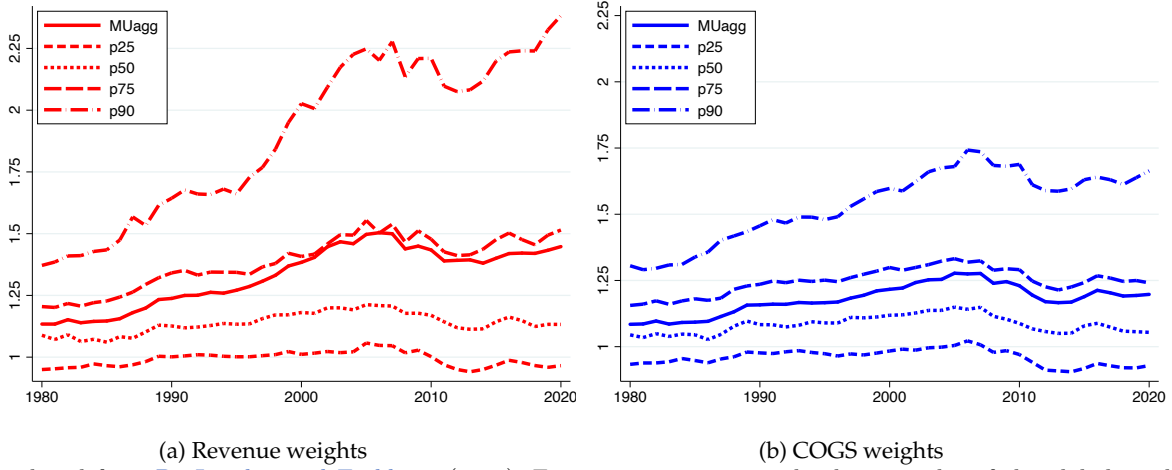
Figure A-4: Aggregate markup by continent, 1980–2020



(a) Revenue Weights

Reproduced from [De Loecker and Eeckhout \(2025\)](#), Figure 9, panel (a), page 30: revenue-weighted aggregate markup by continent. This paper's markups use the same production approach, with COGS as the flexible input and sector output elasticities constant over time.

Figure A-5: Percentiles of the markup distribution



Reproduced from [De Loecker and Eeckhout \(2025\)](#), Figure 2, page 22: weighted percentiles of the global markup distribution, sales weights in panel (a) and COGS weights in panel (b). This paper uses the same production approach, with COGS as the flexible input and sector output elasticities constant over time.

Table A-2: Credit ratings of euro-area bond issuers by size quartile

	Q1	Q2	Q3	Q4	All Firms
A (%)	1.90	0.45	1.27	9.97	4.51
BBB (%)	0.63	0.00	0.00	28.61	11.02
Eligible (%)	2.53	0.45	1.27	38.58	15.53
HY (%)	0.00	0.00	4.24	17.06	7.52
NR (%)	97.47	99.55	94.49	44.36	76.95
Not Eligible (%)	97.47	99.55	98.73	61.42	84.47

Share of euro-area non-financial bond-issuing groups in each rating class, within each quartile of total assets, percent; the last column pools all bond issuers. Shares are within size quartile of bond issuers, not of all firms. “Eligible” is the investment-grade proxy (A or BBB rating), not the CSPP eligibility measured in this paper; “Not Eligible” collects high-yield (HY) and not-rated (NR) issuers. Source: [Darmouni and Papoutsis \(2025\)](#), Table 1; the Eligible and Not Eligible rows are the sums of the rating rows.

B Empirical Appendix

B.1 Data

Commercial paper. One detail on this eligibility identification is about commercial paper assets. A March 2020 rule changed the minimum maturity for these securities from 183 to 28 days. This decision transformed the eligible universe, as commercial paper became about 90% of it from 2020 onwards. For this reason, the paper mainly drops these assets from the main eligible set, using a commercial paper-inclusive alternative as a robustness check.

The benchmark control in the two datasets. The bond-level and firm-level datasets compute the benchmark’s denominator, the total eligible outstanding bonds of all issuers in the data, over their own sets of issuers, so the two versions of the control differ only by a year-specific scale factor. Commercial paper enters the benchmark control from 2020 onwards, when it became eligible; the robustness variant of Appendix B.3 excludes it, in line with the eligibility definition above.

Table A-3: Summary statistics, bond sample

	Estimation sample		Mean by group	
	Mean	SD	Purchased	Not purch.
<i>Panel A: bond characteristics (4255 bonds, 657 issuers)</i>				
Yield to maturity (%)	2.783	1.888	1.799	3.011
Duration (days)	3000.663	2187.311	2952.225	3010.494
Log duration	7.843	0.561	7.876	7.836
Amount issued (EUR m)	622.785	301.743	743.753	599.040
Log amount issued	20.096	0.631	20.365	20.043
Purchased at issuance (share)	0.164	–	1.000	0.000
<i>Panel B: rating-class distribution</i>				
	N	Share of sample	Purch. within	
AAA	11	0.003	0.000	
AA	182	0.043	0.368	
A	651	0.153	0.270	
BBB	807	0.190	0.321	
BB	248	0.058	0.048	
B	92	0.022	0.000	
CCC and below	12	0.003	0.000	
NR (explicit not-rated grade)	2252	0.529	0.082	

The estimation sample of the yield regressions: euro-denominated bonds issued by non-financial companies, 2010–2024, commercial paper excluded, financial, utility, real-estate and public-administration sectors dropped, bonds with rating information only (the explicit NR grade retained as its own class), yield, log duration and log amount trimmed at the first and ninety-ninth percentile. Panel A splits the sample by the bond’s own issuance-week purchase status. Panel B reports, by rating class, the number of bonds, the sample share, and the purchased share. Source: Refinitiv Datastream; ECB CSPP holdings and eligible-assets publications; author’s construction (Section 2.1).

Table A-4: Summary statistics, firm panel

	Estimation sample		Mean by group	
	Mean	SD	Ever-tr.	Never-tr.
<i>Panel A: markup estimation sample (592 firm-years, 129 firms, 96 ever-treated)</i>				
Markup (level)	1.479	0.614	1.515	1.422
Log markup	0.323	0.354	0.349	0.282
CSPP benchmark (lagged)	0.004	0.011	0.005	0.002
Log assets (lagged)	23.356	1.254	23.543	23.058
Bonds outstanding / assets (lagged)	0.077	0.093	0.065	0.097
Total debt / assets (lagged)	0.284	0.142	0.285	0.282
Total liabilities / assets (lagged)	0.661	0.148	0.638	0.697
Cash / assets (lagged)	0.081	0.062	0.075	0.092
Gross profit / assets (lagged)	0.282	0.152	0.285	0.279
Labour cost / assets (lagged)	0.152	0.100	0.138	0.175
SG&A / assets (lagged)	0.177	0.109	0.166	0.194
Issuance dummy	0.557	0.497	0.582	0.518
Eligible-issuance dummy	0.257	0.437	0.286	0.211
Pays dividends	0.971	0.167	0.988	0.944
<i>Panel B: anchored-capex estimation sample (599 firm-years, 134 firms, 101 ever-treated)</i>				
Capex / lagged assets (level)	0.044	0.029	0.043	0.045
Anchored capex change (regression outcome, pooled τ_0 - τ_2)	0.005	0.015	0.005	0.004
CSPP benchmark (lagged)	0.004	0.011	0.005	0.002
Log assets (lagged)	23.357	1.281	23.563	23.017
Bonds outstanding / assets (lagged)	0.075	0.081	0.067	0.089
Total debt / assets (lagged)	0.281	0.137	0.288	0.268
Total liabilities / assets (lagged)	0.660	0.149	0.645	0.685
Cash / assets (lagged)	0.083	0.065	0.075	0.097
Gross profit / assets (lagged)	0.289	0.158	0.289	0.288
Labour cost / assets (lagged)	0.158	0.109	0.145	0.180
SG&A / assets (lagged)	0.182	0.118	0.173	0.198
Issuance dummy	0.559	0.497	0.587	0.513
Eligible-issuance dummy	0.272	0.445	0.311	0.208
Pays dividends	0.968	0.175	0.980	0.949

The two headline estimation samples of the panel analyses: ever-eligible issuers, trimmed at the first and ninety-ninth percentile within the sample, and clean-control observations with complete outcome and controls. Panel A is the markup regression sample; Panel B is the investment regression sample. The anchored capex change in Panel B is the outcome variable of the investment regression: the pooled change in net capex over base-year assets, a level ratio. “Pays dividends” is the share of firm-years with positive cash dividends. The last two columns split each sample by ever-purchased status. Financial controls are lagged one year and scaled by lagged total assets where indicated. Source: Refinitiv Worldscope; author’s construction (Section 2.1).

Table A-5: The purchase process by the status of the previous issue: three-way decomposition

	Issues	Share of issues		
		At issuance	Later	Never
First eligible issue of the issuer	340	0.574	0.191	0.235
Previous issue bought at issuance, any gap	957	0.760	0.061	0.180
within 1 year	762	0.823	0.051	0.126
within 90 days	495	0.903	0.022	0.075
Previous issue bought later, any gap	343	0.222	0.574	0.204
within 1 year	284	0.180	0.634	0.187
within 90 days	157	0.076	0.739	0.185
Previous issue never bought, any gap	612	0.139	0.087	0.775
within 1 year	559	0.134	0.082	0.784
within 90 days	374	0.115	0.075	0.810
Last two issues both bought at issuance	637	0.774	0.053	0.173

Eligible bond issues other than commercial paper, by any issuer, issued from 1 July 2017, 2,252 issues by 340 issuer groups, the sample of Table 1. Each issue is bought at issuance (entered the CSPP portfolio within seven days, observed timing), bought later, or never bought; rows condition on the same status of the issuer's immediately preceding eligible issue at the three gaps between issue dates. The bought-at-issuance column reproduces Panel B of Table 1. Rows overlap by construction. Source: ECB CSPP holdings and eligible-assets publications; author's construction.

B.2 Identification of Purchase Effects

Table A-6: The benchmark share and observed purchases

	(1) LPM C + S×Y	(2) LPM F + Y	(3) Dose C + S×Y	(4) Dose F + Y
Benchmark share	12.8879*** (2.4897)	1.0910 (6.0225)	36.4147*** (6.7098)	-7.7856 (17.0622)
Cluster-robust F	26.80	0.03	29.45	0.21
Observations	1289	1372	1289	1372

Firm-year regressions of the issuance-week purchase indicator (columns 1 and 2) and of the number of issuance-week purchases in the year (columns 3 and 4) on the lagged CSPP benchmark share, the issuer's share of the eligible outstanding bonds of the issuers in the data (Section 2.1). Columns 1 and 3 absorb country and sector-by-year effects; columns 2 and 4 absorb firm and year effects. Sample: ever-eligible firms, 2016 onwards. With a single regressor, the cluster-robust F is the squared t-statistic. The benchmark is measured on a volume basis, while the purchase measures are counts. S, C, Y and F denote sector, country, year and firm fixed effects; × denotes their interaction. Standard errors clustered by firm. Standard errors in parentheses; *, ** and *** denote significance at the 10, 5 and 1 percent level.

Selection-into-purchase at the bond level. Identification of the purchase effects in the cross-sectional analysis on bonds' yield relies on fixed-effects cells. Even within them, selection could remain: among bonds of the same issuer, rating, market conditions, and eligibility status, the ECB may still have purchased securities that would have priced tightly anyway. Regressing the treatment on bond characteristics within the cells shows that observable design explains almost none of who is purchased (a within- R^2 below one percent). Moreover, the fixed-effect-adjusted balance comparison locates the little there is: maturity does not differ between purchased and non-purchased issues (-0.004 log points) despite an eligibility window running from six months to thirty years (European Central Bank, 2016a) leaving ample room to shift it, while issue size does ($+0.108$ log points). Size, however, is exactly the imbalance the allocation rule announces, rather than a symptom of yield-seeking: purchases followed an internal benchmark proportional to the issuer's nominal outstanding eligible bonds.

Table A-7: Bond characteristics and the purchase decision

<i>Panel A. Within-R^2 of purchase (headline fixed effects)</i>			
Regressors	Within R^2	N	
Amount, duration	0.0029	2,920	
Amount, duration, coupon	0.0076	2,920	
Benchmark share, amount, duration	0.0038	2,920	
<i>Panel B. Balance</i>			
	Purchased	Non-purchased	Difference within cells
Log amount issued	20.380	20.198	0.108** (0.053)
Log duration	7.920	7.943	-0.004 (0.040)

Panel A: within- R^2 of regressions of the issuance-week purchase indicator on the listed bond characteristics (log amount issued, log duration, the coupon rate and the lagged benchmark share), with the fixed effects of the main yield specification absorbed (issuer, bond rating, sector-by-year, country-by-year, eligible-by-year); headline sample of Table 2, 2,920 bonds, 354 issuers. Panel B: means of log amount issued and log duration for purchased and non-purchased bonds, and their difference within the same fixed-effect cells, with the standard error of the difference in parentheses. Standard errors clustered by issuer. *, ** and *** denote significance at the 10, 5 and 1 percent level.

Selection-into-purchase at the firm level. Finally, what the identification design requires is that purchase *timing* does not respond to the firm’s outcome path. Table A-8 estimates selection into purchase directly, regressing the treatment dummy on lagged firm characteristics. The two columns are linear probability models, with the cross-sectional and the within-firm fixed-effect structures of the previous table. The results show that selection is on size and balance-sheet position: the main cross-sectional predictor is the CSPP benchmark share (mechanical, being the purchase guideline), followed by assets and leverage. This is level selection, which is not problematic in the results below because the long-difference differences it out and firm-level controls address it further.

Most importantly, selection on the lagged outcomes is absent, as the coefficients on lagged markup and investment rate are not significant. These characteristics also have limited explanatory power: lagged characteristics explain 8.6 percent of the variation in treatment within country- and sector-year cells, and 2.0 percent within firms. Most of the purchase variation is unrelated to anything the balance sheet carries, which is what the design requires.

Table A-8: Lagged firm characteristics and the purchase decision

	(1) LPM C + S×Y	(2) LPM F + Y
Benchmark share	8.4292*** (3.1409)	5.9164 (6.6361)
Log assets	0.0652*** (0.0187)	-0.0193 (0.0490)
Bonds outstanding / assets	0.3971** (0.1976)	-0.6752*** (0.2281)
Total debt / assets	0.1642 (0.1864)	-0.4921 (0.3682)
Total liabilities / assets	-0.4020** (0.1691)	0.0790 (0.3094)
Cash / assets	-0.5070* (0.2619)	-0.4331 (0.3018)
Gross profit / assets	-0.0372 (0.2100)	-0.5242 (0.3464)
Labour costs / assets	-0.0178 (0.1975)	-0.8999* (0.4643)
SG&A / assets	0.3350 (0.2623)	1.0118* (0.5737)
Log markup	0.0766 (0.0615)	0.1091 (0.1493)
Investment rate	0.4001 (0.6413)	0.2892 (0.8545)
R^2	0.3615	0.3456
Within R^2	0.0860	0.0197
Observations	1149	1219

Linear probability models of the issuance-week purchase indicator on lagged firm characteristics; ever-eligible firms, 2016 onwards. Column 1 absorbs country and sector-by-year effects, column 2 firm and year effects. Ratios are over lagged total assets; the markup is in logs; the investment rate is net capex over lagged assets. Within R^2 is computed after absorbing the fixed effects. S, C, Y and F denote sector, country, year and firm fixed effects; × denotes their interaction. Standard errors clustered by firm. Standard errors in parentheses; *, ** and *** denote significance at the 10, 5 and 1 percent level.

B.3 Bond Issuing Conditions

Pricing and timing. The same pricing structure disqualifies a natural control: the coupon rate is set at the same moment, and at-par pricing makes it the outcome under a different name. The data show this is the case: 88.7% of issues price within a point of par, the yield–coupon correlation is 0.9855, and the median gap is 5.6 basis points. In light of this, controlling for the coupon rate would be almost the same as regressing the outcome on itself, with the resulting estimates capturing only the purchase effect on the issue-price discount margin. Indeed, Table A-12 shows that doing so results in an estimated yield reduction of only 2.4 basis points, a magnitude in line with the median gap mentioned before.

Weekly snapshots and the measurement of treatment. The treatment variable in the yield analysis comes from the Eurosystem’s weekly publications of holdings, which appear on Fridays: 940 of the 941 observed purchase dates in the bond sample fall on a Friday. As a consequence, the lag between the issuance of a bond and its first appearance in the portfolio has a distinctive shape: 93, 131, 147, 186 and 133 bonds appear at zero to four days from issuance, none at five and six days, which are weekends, and four at seven days. In other words, the lag is simply the number of days between the issue date and the next Friday. This has two implications. First, the treatment variable measures not a purchase within seven days of issuance, but whether the Eurosystem holds the bond at the first Friday snapshot after issuance. The within-week variation in the lag is calendar arithmetic, uncorrelated with anything about the bond except the weekday of its issuance, and cannot act as a confounder. The second is a measurement error with a known sign. A bond allocated to the Eurosystem at the deal but not yet settled by the first Friday appears in the portfolio with a lag of eight days or more and is therefore coded as not purchased at issuance; this concerns 32 bonds, about 4.4% of the plausible primary allocations. The driver is the interaction between the weekday of issuance and the settlement lag (a bond issued on a Thursday has one day to reach the Friday snapshot, one issued on a Monday has four), which is mechanically unrelated to the quality of the bond. This is non-differential misclassification of a binary treatment, and it attenuates the estimated effect toward zero, which is the first entry in the list in Section 2.3 of imperfections that make the estimate conservative.

A pricing effect. The yield at issuance is fixed at pricing, at the close of bookbuilding, so the rate the firm pays over the bond’s life is a contracted object. No ECB portfolio entry is dated before its bond’s issue date (0 of 692), so the estimate is an effect on contracted terms, not the transitory price impact of a trade in a live market (paragraph “Pricing and timing” above). What the Eurosystem contributes to the book is an inelastic, price-insensitive order: it is unconstrained, does not mark the position against a funding cost, and does not withdraw as the deal tightens.³

Controlling for eligibility status. The literature on the effects of the CSPP on the bond market (Zaghini, 2019; Todorov, 2020; Abidi and Miquel-Flores, 2018; Li et al., 2021; De Santis and Zaghini, 2021; Pegoraro and Montagna, 2025) documents that the policy affected all the eligible bonds and, most importantly, had spillover effects beyond it through a rebalancing portfolio effect. Given this, estimating the purchase effect correctly requires accounting for bonds’ eligibility status. The premium on eligible bonds is large early in the sample and shrinks to almost zero once quantitative easing compressed corporate spreads (Appendix Figure A-6), so a pooled eligibility control over-corrects every post-2016 eligible bond and loads

³ The ECB’s own account of the programme’s first year describes a book in which that order is material without being privileged in the allocation: investor demand for eligible issues ran “on average around three times higher than the issued amount”, and issuers “generally treat the Eurosystem similarly to most other investors in terms of final allocations” (European Central Bank, 2017).

the premium onto the treatment dummy. To avoid this, all specifications carry an eligibility $\times$ year fixed effect.⁴ Six alternative ways to tackle the issue are tested in Appendix B.3; the cleanest of which is restricting the sample to eligible securities only, where no eligibility control is needed at all. All six deliver closely comparable magnitudes and the same sign. Moreover, this fixed effect makes the analysis in this paper complementary to what the literature has found so far. Those effects are on the entire corporate bond market; the ones reported in this section measure the additional purchased-unpurchased difference.

Additional details on selection-into-purchase. The selection that remains possible after the diagnostics shown in Section 2.2 is the unobservable kind (e.g., order-book depth, investor appetite) that the data do not capture. Two patterns address it. The most obvious version of “would have priced tightly anyway” is credit quality, and that channel is closed by construction. The bond’s own rating at issuance (i.e. the market’s summary of issuer quality at the moment of pricing) is absorbed in every specification, and refining it to rating-by-year cells leaves the estimate essentially unchanged. For what remains, the fixed-effects ladder (Table 2) provides the diagnosis. A hot-deal story would imply the following prediction: confining the comparison to ever-narrower cells - the same sector and country in the same year, ultimately the same issuer within the same year - holds fixed more and more of what “would have priced tightly” means, so the measured difference should shrink at every step. The results show the opposite: the estimate strengthens as the cells tighten, and the point estimate in the tightest comparison the data allow - purchased against non-purchased bonds of the same issuer in the same year, feasible for about a fifth of the purchased bonds - is in line with the rest of the ladder (Appendix B.3). Nor is this pattern a pre-programme artefact: re-estimated on bonds issued after the programme’s start, where every nuisance parameter is fitted on treatable bonds only, the ladder is flatter, but its direction is unchanged.

Table A-9: Identifying cells by fixed-effect structure, bond sample

FE dimension	cells	singl.	ident.	trt. share (%)	issuers
<i>Panel A: Purchased at issuance (headline treatment)</i>					
NACE $\times$ Year	640	173	137	80.5	330
Country $\times$ Year	338	95	79	95.9	375
NACE $\times$ Country $\times$ Year	1604	857	93	35.3	111
Rating $\times$ Year	192	42	70	97.3	396
Issuer $\times$ Year (excluded)	1959	1173	61	18.1	44
Eligible $\times$ Year (absorbed everywhere)	30	0	9	100.0	251
<i>Panel B: Purchased at issuance, wide window (incl. imputed benchmark)</i>					
NACE $\times$ Year	640	173	159	81.8	354
Country $\times$ Year	338	95	91	96.4	393
NACE $\times$ Country $\times$ Year	1604	857	108	35.2	124
Rating $\times$ Year	192	42	82	97.7	419
Issuer $\times$ Year (excluded)	1959	1173	69	17.9	49
Eligible $\times$ Year (absorbed everywhere)	30	0	10	100.0	252

For each fixed-effect dimension: the number of cells; singleton cells (one bond, absorbed entirely); identifying cells, containing both treated and untreated bonds; the share of treated bonds inside identifying cells; and the number of distinct issuers in those cells. Complete-case estimating sample: 3,341 bonds, 545 issuers.

⁴ Each simpler treatment of eligibility — a pooled level, a post-programme shift, or the two combined — imposes testable equality restrictions on the year-by-year premia, and all three are rejected ($p \leq 0.0001$).

Table A-10: Yield at issuance across the fixed-effects ladder

Yield to Maturity	(1)	(2)	(3)	(4)	(5)
Treated (issuance week)	-0.0769 (0.0870)	-0.0117 (0.0786)	-0.1694 (0.1028)	-0.1976 ^{**} (0.0826)	-0.2253 [*] (0.1173)
Controls	Yes	Yes	Yes	Yes	Yes
Issuer FE	✓	✓		✓	✓
Issuer $\times$ Year FE			✓		
NACE $\times$ Year FE				✓	
Country $\times$ Year FE				✓	
NACE $\times$ Country $\times$ Year FE					✓
Eligible $\times$ Year FE	✓	✓	✓	✓	✓
Year FE	✓				
Quarter FE		✓			
Bond Rating FE	✓	✓	✓	✓	✓
Bond Type FE				✓	✓
Coupon Type FE				✓	✓
Observations	3,293	3,293	2,217	2,918	2,402
R ²	0.8541	0.8798	0.9379	0.9101	0.9296
Adjusted R ²	0.8299	0.8576	0.8989	0.8599	0.8737

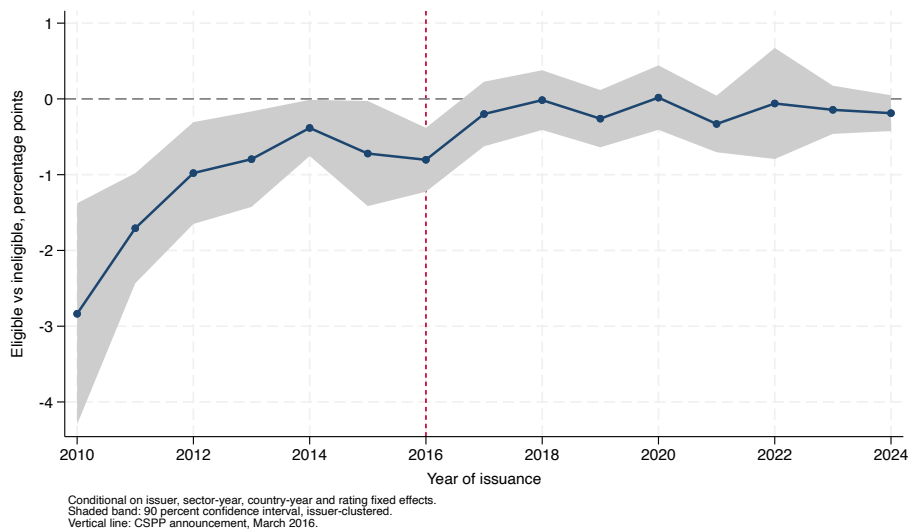
Dependent variable: yield to maturity at issuance, percentage points. Treatment: the bond entered the CSPP portfolio within seven days of issuance, observed timing. Controls: lagged CSPP benchmark share, log duration, log amount issued. Sample: euro-denominated corporate bonds, 2010–2024, commercial paper excluded, trimmed at the first and ninety-ninth percentile. Columns 1 and 2 absorb only year or quarter effects alongside the issuer; column 3 is the issuer-by-year column of Table 2; columns 4 and 5 the sector-and-country-by-year and the triple interaction with bond-type and coupon-type effects. Standard errors clustered by issuer. Standard errors in parentheses; *, ** and *** denote significance at the 10, 5 and 1 percent level.

Table A-11: Alternative treatments of eligibility

Yield to Maturity	(1)	(2)	(3)	(4)
Treated (issuance week)	-0.0720 (0.0952)	-0.1333 (0.0826)	-0.1274 (0.0840)	-0.2170*** (0.0809)
eligible	-0.4511** (0.1867)		-0.6687** (0.2760)	
Eligible and issued post-announcement		-0.1946 (0.1241)	0.3551* (0.2034)	
Controls	Yes	Yes	Yes	Yes
Issuer FE	✓	✓	✓	✓
NACE $\times$ Year FE	✓	✓	✓	✓
Country $\times$ Year FE	✓	✓	✓	✓
Eligible $\times$ Year FE				✓
Bond Rating FE	✓	✓	✓	✓
Observations	2,920	2,920	2,920	2,920
R ²	0.9044	0.9024	0.9049	0.9075
Adjusted R ²	0.8526	0.8496	0.8533	0.8564

Dependent variable: yield to maturity at issuance, percentage points. Treatment: the bond entered the CSPP portfolio within seven days of issuance, observed timing. Controls: lagged CSPP benchmark share, log duration, log amount issued. Sample: euro-denominated corporate bonds, 2010–2024, commercial paper excluded, trimmed at the first and ninety-ninth percentile. Four treatments of the bond's eligibility status: a single eligibility premium pooled over the sample (column 1), a post-announcement eligibility shift only (column 2), the two combined (column 3), and the eligible-by-year effects of the main specification (column 4). Standard errors clustered by issuer. Standard errors in parentheses; *, ** and *** denote significance at the 10, 5 and 1 percent level.

Figure A-6: The eligibility premium by year



Coefficients on eligibility by year of issuance from the main specification with the pooled eligibility effect replaced by year-specific ones; the shaded band is the 90 percent confidence interval, standard errors clustered by issuer.

Table A-12: Controlling for the coupon

Yield to Maturity	(1)	(2)	(3)	(4)
Treated (issuance week)	-0.0239 (0.0239)	-0.0154 (0.0232)	-0.0245 (0.0292)	-0.0334 (0.0311)
Controls	Yes	Yes	Yes	Yes
Coupon	Yes	Yes	Yes	Yes
Issuer FE	✓	✓	✓	✓
NACE $\times$ Year FE	✓	✓		
Country $\times$ Year FE	✓	✓		
NACE $\times$ Country $\times$ Year FE			✓	✓
Eligible $\times$ Year FE	✓	✓	✓	✓
Bond Rating FE	✓		✓	
Rating $\times$ Year FE		✓		✓
Observations	2,909	2,864	2,400	2,356
R ²	0.9916	0.9922	0.9911	0.9917
Adjusted R ²	0.9869	0.9870	0.9842	0.9839

Dependent variable: yield to maturity at issuance, percentage points. Treatment: the bond entered the CSPP portfolio within seven days of issuance, observed timing. Controls: lagged CSPP benchmark share, log duration, log amount issued. Sample: euro-denominated corporate bonds, 2010–2024, commercial paper excluded, trimmed at the first and ninety-ninth percentile. Columns follow Table 2 with the coupon rate added to the controls. The coupon is set at pricing together with the yield: 88.7 percent of issues price within a point of par, the yield–coupon correlation is 0.9855, the median gap 5.6 basis points. Standard errors clustered by issuer. Standard errors in parentheses; *, ** and *** denote significance at the 10, 5 and 1 percent level.

Table A-13: Previous purchases and the yield of later issues, never-purchased sample

Yield to Maturity	(1)	(2)	(3)	(4)
Prev. purchase, any (1y)	-0.1686** (0.0813)	-0.1289 (0.0983)	-0.0655 (0.1046)	-0.2765** (0.1349)
Controls	Yes	Yes	Yes	Yes
Issuer FE	✓	✓	✓	✓
NACE $\times$ Year FE		✓	✓	
Country $\times$ Year FE		✓	✓	
NACE $\times$ Country $\times$ Year FE				✓
Eligible $\times$ Year FE	✓	✓	✓	✓
Year FE	✓			
Bond Rating FE	✓	✓		✓
Rating $\times$ Year FE			✓	
Sample	Never-treated	Never-treated	Never-treated	Never-treated
Observations	2,622	2,244	2,189	1,813
R ²	0.8600	0.9216	0.9311	0.9365
Adjusted R ²	0.8316	0.8656	0.8707	0.8763

Dependent variable: yield to maturity at issuance, percentage points; controls: lagged CSPP benchmark share, log duration, log amount issued. The regressor is an indicator that the issuer had a purchase of any kind within one year before the issue; sample restricted to bonds not themselves purchased at issuance. Issuer-by-year effects are absent by construction. Column 2 is the verdict specification. Standard errors clustered by issuer. Standard errors in parentheses; *, ** and *** denote significance at the 10, 5 and 1 percent level.

Table A-14: Previous purchases and the yield of later issues, full sample

Yield to Maturity	(1)	(2)	(3)	(4)
Prev. purchase, any (1y)	-0.0798 (0.0617)	0.0010 (0.0795)	0.0181 (0.0811)	-0.0686 (0.1188)
Treated (issuance week)	-0.0818 (0.0863)	-0.2169*** (0.0813)	-0.1938** (0.0909)	-0.2485** (0.1144)
Controls	Yes	Yes	Yes	Yes
Issuer FE	✓	✓	✓	✓
NACE $\times$ Year FE		✓	✓	
Country $\times$ Year FE		✓	✓	
NACE $\times$ Country $\times$ Year FE				✓
Eligible $\times$ Year FE	✓	✓	✓	✓
Year FE	✓			
Bond Rating FE	✓	✓		✓
Rating $\times$ Year FE			✓	
Sample	Full	Full	Full	Full
Observations	3,293	2,920	2,873	2,405
R ²	0.8542	0.9075	0.9146	0.9267
Adjusted R ²	0.8300	0.8563	0.8586	0.8690

Dependent variable: yield to maturity at issuance, percentage points; controls: lagged CSPP benchmark share, log duration, log amount issued. The regressor is an indicator that the issuer had a purchase of any kind within one year before the issue; full sample, with the bond's own issuance-week purchase as a control. Column 2 is the verdict specification. Standard errors clustered by issuer. Standard errors in parentheses; *, ** and *** denote significance at the 10, 5 and 1 percent level.

Table A-15: Previous purchases by definition and window

Yield to Maturity	(1)	(2)	(3)	(4)	(5)	(6)
Prev. purchase, any (1y)	-0.1385 (0.0962)					-0.0610 (0.1081)
Prev. purchase, any (2y)	0.0625 (0.0843)					0.0604 (0.0997)
Prev. purchase, any – late timing (1y)		-0.0345 (0.1033)				
Prev. purchase, any – late timing (2y)		0.1348 (0.0922)				
Prev. purchase, precise timing (1y)			0.0247 (0.1149)			
Prev. purchase, precise timing (2y)			-0.0215 (0.0979)			
Prev. primary purchase (1y)				-0.0494 (0.1457)		
Prev. primary purchase (2y)				-0.0616 (0.1560)		
Prev. secondary purchase (1y)					-0.1988 (0.1398)	
Prev. secondary purchase (2y)					-0.1271 (0.1696)	
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Issuer FE	✓	✓	✓	✓	✓	✓
NACE $\times$ Year FE	✓	✓	✓	✓	✓	✓
Country $\times$ Year FE	✓	✓	✓	✓	✓	✓
Eligible $\times$ Year FE	✓	✓	✓	✓	✓	✓
Bond Rating FE	✓	✓	✓	✓	✓	✓
Observations	2,244	2,244	2,244	2,244	2,244	2,126
R ²	0.9216	0.9216	0.9215	0.9215	0.9217	0.9214
Adjusted R ²	0.8655	0.8654	0.8653	0.8653	0.8657	0.8617

Dependent variable: yield to maturity at issuance, percentage points; controls: lagged CSPP benchmark share, log duration, log amount issued. The previous-purchase coefficient at the verdict specification for each definition of a previous purchase and the one- and two-year windows, on both samples. Standard errors clustered by issuer. Standard errors in parentheses; *, ** and *** denote significance at the 10, 5 and 1 percent level.

Table A-16: Yield at issuance: robustness battery (a)

Specification (fixed effects)	Coefficient	Standard error	Bonds
<i>Prior-purchase controls, issuance-week purchases, one-year window</i>			
(1) NACE×Year, Country×Year, Bond Rating	-0.2152***	(0.0815)	2,920
(2) NACE×Year, Country×Year, Rating×Year	-0.1900**	(0.0903)	2,873
(3) NACE×Country×Year, Bond Rating	-0.2520**	(0.1159)	2,405
(4) NACE×Country×Year, Rating×Year	-0.2368*	(0.1346)	2,361
<i>Prior-purchase controls, issuance-week purchases, one- and two-year windows</i>			
(1) NACE×Year, Country×Year, Bond Rating	-0.2175***	(0.0807)	2,920
(2) NACE×Year, Country×Year, Rating×Year	-0.1920**	(0.0899)	2,873
(3) NACE×Country×Year, Bond Rating	-0.2616**	(0.1127)	2,405
(4) NACE×Country×Year, Rating×Year	-0.2403*	(0.1328)	2,361
<i>Prior-purchase controls, purchases of any kind, one-year window</i>			
(1) NACE×Year, Country×Year, Bond Rating	-0.2169***	(0.0813)	2,920
(2) NACE×Year, Country×Year, Rating×Year	-0.1938**	(0.0909)	2,873
(3) NACE×Country×Year, Bond Rating	-0.2485**	(0.1144)	2,405
(4) NACE×Country×Year, Rating×Year	-0.2432*	(0.1327)	2,361
<i>Prior-purchase controls, purchases of any kind, one- and two-year windows</i>			
(1) NACE×Year, Country×Year, Bond Rating	-0.2221***	(0.0813)	2,920
(2) NACE×Year, Country×Year, Rating×Year	-0.2003**	(0.0900)	2,873
(3) NACE×Country×Year, Bond Rating	-0.2518**	(0.1133)	2,405
(4) NACE×Country×Year, Rating×Year	-0.2508*	(0.1290)	2,361
<i>Clustering by issuer and year-quarter</i>			
(1) NACE×Year, Country×Year, Bond Rating	-0.2170**	(0.0936)	2,920
(2) NACE×Year, Country×Year, Rating×Year	-0.1948*	(0.1036)	2,873
(3) NACE×Country×Year, Bond Rating	-0.2484**	(0.1180)	2,405
(4) NACE×Country×Year, Rating×Year	-0.2427*	(0.1323)	2,361
<i>Clustering by issuer and year</i>			
(1) NACE×Year, Country×Year, Bond Rating	-0.2170**	(0.0771)	2,920
(2) NACE×Year, Country×Year, Rating×Year	-0.1948**	(0.0882)	2,873
(3) NACE×Country×Year, Bond Rating	-0.2484***	(0.0774)	2,405
(4) NACE×Country×Year, Rating×Year	-0.2427**	(0.0867)	2,361
<i>Rating classes</i>			
(1) NACE×Year, Country×Year	-0.2060**	(0.0826)	2,920
(2) NACE×Country×Year	-0.2411**	(0.1166)	2,407

Dependent variable: yield to maturity at issuance, percentage points. Each row is one specification of the robustness battery, one perturbation at a time from the main specification of Table 2, column 1: coefficient on the issuance-week purchase indicator, standard error clustered by issuer unless the row states another scheme, number of bonds. Groups: prior-purchase controls (issuance-week purchases or purchases of any kind, one- and two-year windows); two-way clustering by issuer and year-quarter, and by issuer and year; rating classes; fixed-coupon bonds; post-programme sample; alternative yield measures (recomputed and unified); benchmark without commercial paper; treatment including the initial portfolio. Standard errors in parentheses; *, ** and *** denote significance at the 10, 5 and 1 percent level.

Table A-17: Yield at issuance: robustness battery (b)

Specification (fixed effects)	Coefficient	Standard error	Bonds
<i>Fixed-coupon bonds</i>			
(1) NACE×Year, Country×Year, Bond Rating	-0.1920**	(0.0838)	2,841
(2) NACE×Year, Country×Year, Rating×Year	-0.1773*	(0.0928)	2,791
(3) NACE×Country×Year, Bond Rating	-0.2236*	(0.1205)	2,327
(4) NACE×Country×Year, Rating×Year	-0.2199	(0.1417)	2,274
<i>Post-programme sample</i>			
(1) NACE×Year, Country×Year, Bond Rating	-0.2658***	(0.0834)	2,000
(2) NACE×Year, Country×Year, Rating×Year	-0.2446***	(0.0942)	1,983
(3) NACE×Country×Year, Bond Rating	-0.2580**	(0.1128)	1,684
(4) NACE×Country×Year, Rating×Year	-0.2632**	(0.1247)	1,662
<i>Alternative yield measures, recomputed yield</i>			
(1) NACE×Year, Country×Year, Bond Rating	-0.2210***	(0.0751)	3,022
(2) NACE×Year, Country×Year, Rating×Year	-0.2072**	(0.0824)	2,977
(3) NACE×Country×Year, Bond Rating	-0.2673***	(0.0990)	2,467
(4) NACE×Country×Year, Rating×Year	-0.2633**	(0.1146)	2,422
<i>Alternative yield measures, unified yield</i>			
(1) NACE×Year, Country×Year, Bond Rating	-0.2435***	(0.0724)	3,098
(2) NACE×Year, Country×Year, Rating×Year	-0.2223***	(0.0791)	3,055
(3) NACE×Country×Year, Bond Rating	-0.2940***	(0.0947)	2,536
(4) NACE×Country×Year, Rating×Year	-0.2732**	(0.1060)	2,494
<i>Benchmark without commercial paper</i>			
(1) NACE×Year, Country×Year, Bond Rating	-0.2171***	(0.0808)	2,920
(2) NACE×Year, Country×Year, Rating×Year	-0.1950**	(0.0900)	2,873
(3) NACE×Country×Year, Bond Rating	-0.2492**	(0.1158)	2,405
(4) NACE×Country×Year, Rating×Year	-0.2435*	(0.1323)	2,361
<i>Treatment including the initial portfolio</i>			
(1) NACE×Year, Country×Year, Bond Rating	-0.2618***	(0.0788)	2,920
(2) NACE×Year, Country×Year, Rating×Year	-0.2300***	(0.0877)	2,873
(3) NACE×Country×Year, Bond Rating	-0.2785**	(0.1095)	2,405
(4) NACE×Country×Year, Rating×Year	-0.2785**	(0.1203)	2,361

Continued from Table A-16; notes there.

Table A-18: Randomisation inference

<i>Panel A. Observed estimate</i>	
Coefficient on purchase	-0.2170
Cluster-robust standard error	0.0809
t -statistic	-2.680
Analytical p -value	0.0077
<i>Panel B. Randomization inference</i>	
Replications	5,000
Permutations with $ b $ at least the observed	0
RI p -value, two-sided	0.0000
small-sample form $(1 + n)/(1 + R)$	0.0002
RI p -value, one-sided	0.0000
RI p -value on the t -statistic, two-sided	0.0070

Dependent variable: yield to maturity at issuance, percentage points. Treatment: the bond entered the CSPP portfolio within seven days of issuance, observed timing. Controls: lagged CSPP benchmark share, log duration, log amount issued; fixed effects of the main specification (issuer, bond rating, sector-by-year, country-by-year, eligible-by-year); sample of Table 2, column 1. Each of the 5,000 replications reassigns the purchase indicator at random among the bonds of each eligible-by-year cell, holding the cell's number of purchases at its observed value; the randomisation-inference p -value is the share of replications whose coefficient is at least as large in absolute value as the observed one. Seed 20260908.

Table A-19: Wild cluster bootstrap

	Analytical (cluster-robust, t_{G-1})	Wild cluster bootstrap (Rademacher, 9,999 reps)
Coefficient on purchase	-0.2170	-0.2170
Standard error	0.0809	—
t -statistic	-2.680	-2.680
p -value	0.0077	0.0106
95% confidence interval	[-0.3761, -0.0578]	[-0.3770, -0.0587]
90% confidence interval	[-0.3504, -0.0835]	[-0.3517, -0.0842]
Observations	2,920	2,920
Clusters (issuer groups)	354	354

Dependent variable: yield to maturity at issuance, percentage points. Treatment: the bond entered the CSPP portfolio within seven days of issuance, observed timing. Controls: lagged CSPP benchmark share, log duration, log amount issued; fixed effects of the main specification (issuer, bond rating, sector-by-year, country-by-year, eligible-by-year); sample of Table 2, column 1. Restricted (null-imposed) wild cluster bootstrap- t with Rademacher weights, 9,999 replications, clustered by issuer; symmetric two-sided p -value, intervals by test inversion. Seed 20260908.

B.4 Firm Panel

This subsection gives the details of the panel estimator employed in Sections 2.4 and 2.5, which the main text summarises, and then collects the tables and figures of the robustness batteries, the no-composition versions of every regression, the decompositions behind the null results and the additional outcomes.

Why not two-way fixed effects. The firm-level dataset is a panel of units with a variable measuring the exposure to the CSPP. The most common way to estimate causal effects in this setting is two-way fixed-effects (TWFE) regression, in which the outcome is regressed on unit effects, time effects, and a measure of treatment exposure, possibly lagged. Its justification is that, in a design with two groups and two periods, the TWFE coefficient coincides with the difference-in-differences estimate. Recent literature has shown that this equivalence does not extend to the richer designs to which TWFE regressions have been applied, with many groups and periods, staggered adoption, and treatments that switch on and off.⁵ The TWFE coefficient recovers a proper average of treatment effects only under parallel trends, no anticipation and a treatment effect that is constant across groups and over time. The first two assumptions are standard and can be at least partially tested; the third is unlikely to hold whenever effects build over time, and when it fails, the TWFE coefficient weights some comparisons negatively because it uses previously treated units as controls for newly treated ones. The treatment of this paper, the purchase of a firm’s bond by the Eurosystem, fails the constant-effect assumption in every respect: firms are first purchased in different years (staggered treatment), a firm can be purchased in one year and not in the next (non-absorbing treatment), and the effect on capital and on the product market is plausibly heterogeneous across firms and builds over time (heterogeneous and dynamic effects).

The LP-DiD estimator. For these reasons, the paper employs the local projections difference-in-differences (LP-DiD) estimator of Dube et al. (2025), which combines the local projection method of Jordà (2005) with the clean-control condition of Cengiz et al. (2019). The estimating equation at horizon h is equation (2) of the main text: the change in the outcome between the base year $t - 1$ and $t + h$ is regressed on the change in treatment status at t , on the lagged controls and on the fixed effects, one regression per horizon, so that no restriction is imposed on the shape of the dynamic response. What makes the estimator a difference-in-differences one is the restriction of the sample. Denoting by $D_{i,t}$ the treatment dummy and by L the number of periods after which the dynamic effects of a treatment are assumed to have stabilised, the regression at horizon h uses only the firm-years that satisfy

$$\begin{cases} \text{treated:} & D_{i,t+j} = 1 \text{ for } 0 \leq j \leq h \quad \text{and} \quad D_{i,t-j} = 0 \text{ for } 1 \leq j \leq L, \\ \text{clean control:} & \Delta D_{i,t+j} = 0 \text{ for } -L \leq j \leq h. \end{cases} \quad (\text{A-1})$$

In words, the treated group at horizon h consists of the firms that enter treatment at t after at least L untreated years and remain treated through $t + h$; the clean-control group consists of the firms whose treatment status does not change between $t - L$ and $t + h$, whether they are untreated or treated throughout. Firm-years that satisfy neither condition, for instance, a firm purchased at t after an untreated spell shorter than L , or a control whose status changes inside the window, are excluded from the regression at that horizon. This is the non-absorbing version of the estimator, which the package of Dube et al. (2025) implements, and the one required by a treatment that can switch off: in the absorbing version, the control group would be

⁵ Examples are Callaway and Sant’Anna (2021), Sun and Abraham (2021), Goodman-Bacon (2021) and Borusyak et al. (2024); see De Chaisemartin and d’Haultfoeuille (2023) for a survey.

defined by not-yet-treated units only. The stabilisation parameter L is set to one year for the investment rate (a flow) and to three years for markups (a stock-like outcome); the batteries report the other values.

Which firms contribute to which coefficient. Condition (A-1) is the key to reading the results, because the panel contains firms purchased in a single year, firms purchased continuously for several years, and firms purchased in several years but not continuously. The three cases contribute differently: a firm treated only at t enters the treated group at $h = 0$ only, so it contributes to β_0 and to nothing else; a firm treated continuously from t to $t+j$ contributes to $\beta_0, \dots, \beta_j$; a firm treated at t , untreated at $t+1$ and treated again at $t+j$ contributes its first purchase to β_0 only, while its second purchase counts as a new treatment event if and only if the untreated spell lasted at least L years, and otherwise the corresponding firm-years are excluded from both groups. The horizon coefficients are therefore identified from firms continuously treated up to that horizon, which is why the number of treatment events falls with h (Table A-21) and why the comparison between continuous and single purchases in Section 2.6 is a separate exercise.

Sector composition of the treated variation. The treated variation is concentrated in goods-producing firms (NACE A–F): about 72% of the treatments that reach $h = 2$. Neither sector group retains enough surviving treatment switches to support a split estimate, so the design answers the heterogeneity question with composition, not coefficients.

Pre-period placebos and pooled coefficients. The pre-period coefficients are obtained from the same regressions run on the backward changes $y_{i,t-k} - y_{i,t-1}$, $k = 2, \dots, 4$, on the same clean sample, so that they are placebo tests of the design rather than of a different comparison; the coefficient at $k = 1$ is zero by construction, being the normalisation. The pooled post coefficient, the headline estimand of the paper, regresses the average of the forward changes over $h = 0$ to 2 on the change in treatment status, on the sample that is clean at the longest horizon; it requires the full post window to be observed, and it equals the variance-weighted average of the horizon coefficients, the default weighting of Dube et al. (2025). The pooled pre coefficient is built in the same way from the backward changes. Standard errors are clustered by firm throughout, and the fixed effects listed in each table are absorbed horizon by horizon, with year effects always included.

Controls and the anchored outcome. The controls of the main text, nine lagged firm financials, the lagged benchmark share and the two contemporaneous issuance dummies, enter linearly with horizon-specific coefficients. The investment outcome is anchored, i.e. every forward change is scaled by assets in the base year rather than by assets at $t + h - 1$, so that the balance-sheet growth of treated firms cannot attenuate the path. The base year $t - 1$ is the year before the purchase for a treated firm and the year before the comparison date for a clean control, so the outcome is defined symmetrically for the two groups. An anchored outcome cannot be expressed in the package of Dube et al. (2025), whose outcome transformation is a plain difference; the investment regressions are therefore run with the same horizon-specific clean-control samples, reconstructed from (A-1), and with the same regressors, in a standard fixed-effects regression. The reconstruction reproduces the package’s estimates exactly wherever both can be computed, as for the markup outcome.

The no-composition variant. The sample of the regression at horizon h in (A-1) grows as h falls, since fewer years of continuous treatment are required. The horizon coefficients can therefore differ for two reasons: the effect dynamics and the changing composition of the treated group. The no-composition variant

removes the second: every horizon is estimated on the sample that is clean at the longest horizon, so that the same treatment events identify every coefficient. The pooled coefficient is identical across the two variants by construction, as the pooled outcome already requires the full post window; the choice affects only the display of the dynamics. The main text reports the standard variant, and this appendix reports the balanced alternative of every regression. The one-shot treatment of Section 2.6 is the no-composition variant with the treated group restricted to firms purchased at t and not at $t + 1$ and $t + 2$, with the clean-control pool unchanged.

Variants in the batteries. The last rows of the robustness batteries change the definition of the groups rather than the specification: treating only the first purchase of each firm as a treatment event, which turns the design into an absorbing one; using not-yet-treated firms only as controls, as in the absorbing version of the estimator; the two combined; and a standard local projection, which drops the clean-control restriction and is reported as a reference for what the restriction does. The COVID variant blanks the outcome in 2020 and 2021; blanking interior years degenerates the estimator’s native pooled average, so its pooled coefficient is computed with a version of the estimator that tolerates missing values.

Fixed-effects and horizon choice. The dataset under analysis is a multi-country, multi-sector panel. This implies three main variation dimensions that the researcher might want to control through fixed effects in the regressions: time, sector, and country. Several combinations are possible, but the feasibility of each should be considered, especially given the panel’s relatively short time dimension. Table A-20 reports, for each candidate, the number of cells that actually contain both a treatment switch and a clean control - the only cells that contribute to identification. The chosen headline specification absorbs country and sector $\times$ year effects: it is the best-identified non-trivial structure (115 identifying cells on the switch margin). On the other hand, because purchases cluster by country-year, country $\times$ year effects absorb much of the treatment variation, making identification weaker for any combination that includes them.

Table A-20: Identifying cells by fixed-effect structure, firm panel

FE spec	Treatment level				Treatment switch			
	cells	singl.	ident.	Events	cells	singl.	ident.	Events
S + C + Y	21	0	18	316	21	0	18	212
S + C $\times$ Y	296	73	76	304	270	69	69	206
C + S $\times$ Y	539	135	136	289	484	124	115	194
S $\times$ Y + C $\times$ Y	1615	1266	95	112	1376	1114	58	66
S $\times$ Y + C $\times$ Y + S $\times$ C	1615	1266	95	112	1376	1114	58	66
S $\times$ C $\times$ Y	1615	1266	95	112	1376	1114	58	66

Treatment level is the purchased-at-issuance indicator and treatment switch a change in purchase status. For each candidate structure, grouped at its finest interaction: the number of non-empty cells, singleton cells (absorbed entirely), identifying cells (containing both a treatment switch and a clean control) and the share of treatment switches inside identifying cells. Outcome-independent: the counts use the treatment side only, so the table is the same for every panel outcome. Sample: ever-eligible firms. S, C, Y and F denote sector, country, year and firm fixed effects; $\times$ denotes their interaction.

The announcement exercise. The analysis is designed as follows. Treatment is fully predetermined, being a dummy equal to one if firm i had at least one non-commercial paper bond outstanding on the day before the CSPP announcement (9 March 2016) that appeared on the ECB eligible-assets list. Nothing the firm did after the announcement enters; 88 of 769 panel firms satisfy this requirement. The specification is an event study of eligible against never-eligible bond issuers around 2016, with firm, country $\times$ year and

Table A-21: Treatment events surviving at each horizon

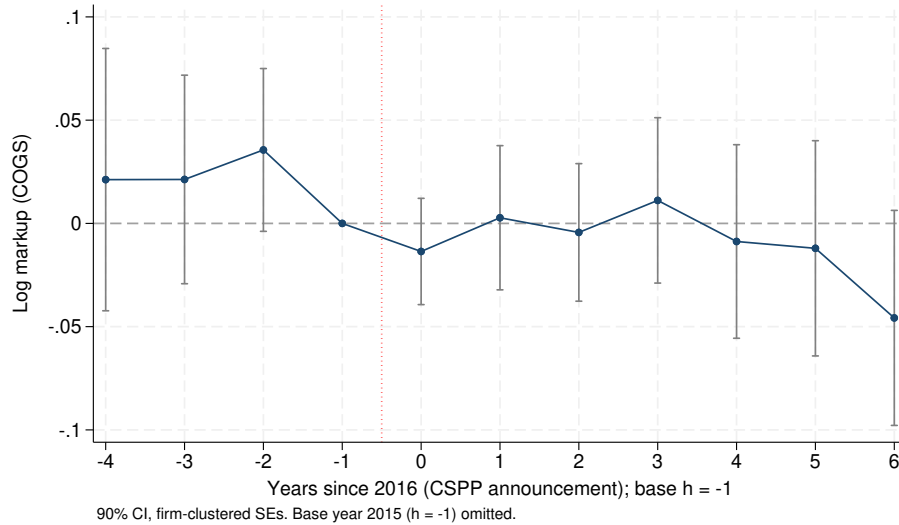
Horizon	Firm-years		Treat events		
	Standard	Balanced	Standard	Surviving	Survival %
<i>Panel A. Log markup ($L = 3$)</i>					
τ_0	1208	719	113	11	9.7
τ_1	921	719	34	11	32.4
τ_2	750	719	11	11	100.0
<i>Panel B. Investment rate ($L = 1$)</i>					
τ_0	1125	581	123	11	8.9
τ_1	796	581	36	11	30.6
τ_2	588	581	11	11	100.0

Firm-years and treatment events (switches into treatment after at least L untreated years) entering the regression at each horizon in the standard variant, and the events that survive to the longest horizon, which the no-composition variant uses at every horizon; the last column is the survival share. Panel A: log markup, $L = 3$; Panel B: investment rate, $L = 1$. Ever-eligible sample, main specification. Source: author's calculations on the estimation samples of Tables 3 and 4.

size $\times$ year fixed effects. The pooled version interacts eligibility with $\mathbf{1}[t \geq 2016]$ on 2012–2019, a window mechanically free of the COVID years. Because eligible firms are far larger than ineligible ones (normalised difference 1.46 in log assets), the size $\times$ year term is key for identification. Table A-29 shows that without it, eligible firms spuriously appear to cut capital after 2016.

Several robustness checks are shown in Appendix B.4, none of which change the implication: lagged size control, dropping size-missing firms, full and no-COVID windows, a balanced panel, and a no-firm-FE cross-sectional variant.

Figure A-7: The announcement and real outcomes: pre-determined eligibility



Log markup: coefficients on pre-determined eligibility interacted with year indicators, base year 2015, with firm, country-by-year and size-quartile-by-year effects; 90 percent confidence intervals with the normal approximation, standard errors clustered by firm.

The dividend exercise. The last variable examined is the payout of the purchased firms, given that almost every firm in the sample pays dividends (Section 2.1) and that the model of Section 3 draws a sharp

Table A-22: Investment rate: robustness battery

Perturbation	Pooled post	Cumul. ($\approx \Delta K/K$)	Pooled pre	N
Baseline: headline	2.2** (0.9)	6.4** (2.8)	0.0 (0.3)	599
FE: sector, country, year	1.5** (0.7)	4.4** (2.2)	0.2 (0.2)	727
FE: sector + country×year	1.9** (0.8)	5.7** (2.3)	0.3 (0.3)	682
FE: sector×year + country×year	1.8*** (0.6)	5.5*** (1.9)	-0.1 (0.4)	530
Controls: drop issuance dummies	2.2** (0.9)	6.8** (2.8)	-0.1 (0.2)	599
Control: + lagged treatment	2.2** (0.9)	6.5** (2.7)	0.0 (0.3)	599
Control: + lagged purchase count	2.2** (0.9)	6.5** (2.7)	0.0 (0.3)	599
Control: + lagged cumul. purchases	2.2** (0.9)	6.4** (2.8)	0.1 (0.3)	599
Treatment: benchmark-inclusive purchase	0.5 (0.7)	1.3 (2.0)	-0.0 (0.3)	526
COVID: 2020–21 outcome blanked	0.4 (0.4)	—	0.2 (0.2)	799
Gate: eligibility excl. commercial paper	2.2** (1.0)	6.6** (2.9)	0.0 (0.3)	556
Gate: pre-determined eligibility	0.2 (1.3)	0.6 (3.9)	0.1 (0.4)	191
AR: lagged outcome level	2.2** (0.9)	6.4** (2.8)	-0.2 (0.2)	599
AR: lagged outcome first difference	1.5 (1.0)	4.5 (2.8)	-0.1 (0.1)	461
$L = 2$	2.2** (1.0)	6.6** (3.1)	0.1 (0.3)	586
$L = 3$	2.2** (1.0)	6.5** (3.1)	0.1 (0.3)	578
Variant: first purchase only treated	2.1** (1.1)	6.3** (3.2)	0.1 (0.3)	575
Variant: not-yet-treated controls	2.2** (0.9)	6.4** (2.8)	0.1 (0.3)	576
Variant: first purchase + not-yet-treated	1.7* (1.0)	5.1* (2.9)	0.1 (0.3)	575
Variant: standard LP	0.0 (0.1)	0.0 (0.3)	0.0 (0.1)	1188

Pooled post coefficient, cumulative three-year flow and pooled pre placebo, one row per perturbation of the main specification, which is the first row. Unless perturbed: issuance-week treatment, country and sector-by-year effects; nine lagged firm financials, the lagged benchmark share, the issuance and eligible-issuance dummies; ever-eligible sample, $L = 1$, four pre- and two post-periods, trimming within the sample. Coefficients $\times 100$, in percentage points of base-year assets. The standard local-projection row drops the clean-control restriction and is a different estimand, shown for reference. Standard errors clustered by firm. Standard errors in parentheses; *, ** and *** denote significance at the 10, 5 and 1 percent level. Row definitions: the three previous-purchase control rows add, respectively, the lagged treatment indicator, the lagged number of purchased issues and the number of purchased issues accumulated through $t - 1$; the COVID row blanks a horizon when either endpoint year falls in 2020–21; the two AR rows add the lagged ratio of capex to assets in levels and in first differences.

Table A-23: Markups: robustness battery

Perturbation	Pooled post	Pooled pre	N
Baseline: headline	6.9 ^{**} (3.0)	1.3 (1.4)	592
Outcome: log input share	6.9 ^{**} (3.0)	1.3 (1.4)	592
FE: sector, country, year	6.5 ^{**} (2.6)	1.2 (1.6)	716
FE: sector + country×year	10.0 ^{**} (3.9)	0.5 (1.9)	669
FE: sector×year + country×year	10.0 (6.4)	0.2 (2.0)	517
Controls: drop issuance dummies	6.7 ^{**} (2.6)	1.1 (1.2)	592
Control: + lagged treatment	6.9 ^{**} (3.0)	1.3 (1.4)	592
Control: + lagged purchase count	6.9 ^{**} (3.0)	1.3 (1.4)	592
Control: + lagged cumul. purchases	6.9 ^{**} (3.0)	1.3 (1.4)	592
Treatment: benchmark-inclusive purchase	6.0 ^{**} (2.7)	0.4 (1.4)	502
COVID: 2020–21 outcome blanked	6.8 ^{***} (2.6)	3.2 (2.2)	690
Gate: eligibility excl. commercial paper	7.1 ^{**} (3.3)	1.8 (1.5)	557
Gate: pre-determined eligibility	12.4 ^{**} (5.8)	3.6 (2.8)	194
AR: lagged outcome level	8.2 ^{***} (3.0)	1.5 (1.3)	592
AR: lagged outcome first difference	9.0 ^{***} (2.8)	1.3 (0.9)	454
$L = 2$	6.9 ^{**} (2.9)	1.4 (1.4)	603
$L = 1$	5.1 [*] (2.9)	1.5 (1.4)	617
Variant: first purchase only treated	6.9 ^{**} (3.0)	1.3 (1.4)	588
Variant: not-yet-treated controls	6.9 ^{**} (3.0)	1.3 (1.4)	588
Variant: first purchase + not-yet-treated	6.2 ^{**} (2.8)	1.3 (1.4)	589
Variant: standard LP	1.0 [*] (0.5)	0.0 (0.4)	1203

Pooled post coefficient and pooled pre placebo, one row per perturbation of the main specification, which is the first row. Unless perturbed: issuance-week treatment, country and sector-by-year effects; nine lagged firm financials, the lagged benchmark share, the issuance and eligible-issuance dummies; ever-eligible sample, $L = 3$, four pre- and two post-periods, trimming within the sample. Coefficients $\times 100$, in log points. The standard local-projection row drops the clean-control restriction and is a different estimand, shown for reference. Standard errors clustered by firm. Standard errors in parentheses; *, ** and *** denote significance at the 10, 5 and 1 percent level. Row definitions: the drop-issuance-dummies row removes the issuance and eligible-issuance dummies; the three previous-purchase control rows add, respectively, the lagged treatment indicator, the lagged number of purchased issues and the number of purchased issues accumulated through $t - 1$; the COVID row blanks the outcome in 2020–2021; the log-input-share row is a different outcome, in log points of the input share.

Table A-24: Investment rate: robustness battery, balanced samples

Perturbation	Pooled-post	Cumul. ($\approx \Delta K/K$)	Pooled-pre	N
Baseline: headline	0.022** (0.009)	0.065** (0.028)	0.000 (0.003)	599
FE: sector, country, year	0.015** (0.007)	0.046** (0.022)	0.002 (0.002)	727
FE: sector + country×year	0.019** (0.008)	0.056** (0.023)	0.003 (0.003)	682
FE: sector×year + country×year	0.018*** (0.006)	0.053*** (0.019)	-0.001 (0.004)	530
Controls: drop issuance dummies	0.022** (0.009)	0.067** (0.028)	-0.001 (0.002)	599
Control: + lagged treatment	0.022** (0.009)	0.066** (0.027)	0.000 (0.003)	599
Control: + lagged purchase count	0.022** (0.009)	0.065** (0.028)	0.000 (0.003)	599
Control: + lagged cumul. purchases	0.022** (0.009)	0.065** (0.028)	0.001 (0.003)	599
Treatment: benchmark-inclusive purchase	0.005 (0.007)	0.014 (0.020)	-0.000 (0.003)	526
COVID: 2020–21 outcome blanked	0.004 (0.004)	—	0.002 (0.002)	799
Gate: eligibility excl. commercial paper	0.022** (0.010)	0.067** (0.029)	0.000 (0.003)	556
Gate: pre-determined eligibility	0.002 (0.013)	0.006 (0.039)	0.001 (0.004)	191
AR: lagged outcome level	0.022** (0.009)	0.065** (0.028)	-0.002 (0.002)	599
AR: lagged outcome first difference	0.015 (0.010)	0.044 (0.029)	-0.001 (0.001)	461
$L = 2$	0.022** (0.010)	0.067** (0.031)	0.001 (0.003)	586
$L = 3$	0.022** (0.010)	0.066** (0.031)	0.001 (0.003)	578
Variant: first purchase only treated	0.021** (0.011)	0.064** (0.032)	0.001 (0.003)	575
Variant: not-yet-treated controls	0.022** (0.009)	0.065** (0.028)	0.001 (0.003)	576
Variant: first purchase + not-yet-treated	0.017* (0.010)	0.052* (0.029)	0.001 (0.003)	575

One row per perturbation of the main specification, which is the first row, estimated on samples balanced across the post horizons. Unless perturbed: issuance-week treatment, country and sector-by-year effects; nine lagged firm financials, the lagged benchmark share, the issuance and eligible-issuance dummies; ever-eligible sample, $L = 1$, four pre- and two post-periods. Standard errors clustered by firm. Standard errors in parentheses; *, ** and *** denote significance at the 10, 5 and 1 percent level.

Table A-25: Markups: robustness battery, balanced samples

Perturbation	Pooled-post	Pooled-pre	N
Baseline: headline	0.069** (0.030)	0.013 (0.014)	592
Outcome: log input share	0.069** (0.030)	0.013 (0.014)	592
FE: sector, country, year	0.065** (0.026)	0.012 (0.016)	716
FE: sector + country×year	0.100** (0.039)	0.005 (0.019)	669
FE: sector×year + country×year	0.100 (0.064)	0.002 (0.020)	517
Controls: drop issuance dummies	0.067** (0.026)	0.011 (0.012)	592
Control: + lagged treatment	0.069** (0.030)	0.013 (0.014)	592
Control: + lagged purchase count	0.069** (0.030)	0.013 (0.014)	592
Control: + lagged cumul. purchases	0.069** (0.030)	0.013 (0.014)	592
Treatment: benchmark-inclusive purchase	0.060** (0.027)	0.004 (0.014)	502
COVID: 2020–21 outcome blanked	0.068** (0.026)	0.005 (0.015)	690
Gate: eligibility excl. commercial paper	0.071** (0.033)	0.018 (0.015)	557
Gate: pre-determined eligibility	0.124** (0.058)	0.036 (0.028)	194
AR: lagged outcome level	0.082*** (0.030)	0.015 (0.013)	592
AR: lagged outcome first difference	0.090*** (0.028)	0.013 (0.009)	454
$L = 2$	0.069** (0.029)	0.014 (0.014)	603
$L = 1$	0.051* (0.029)	0.015 (0.014)	617
Variant: first purchase only treated	0.069** (0.030)	0.013 (0.014)	588
Variant: not-yet-treated controls	0.069** (0.030)	0.013 (0.014)	588
Variant: first purchase + not-yet-treated	0.062** (0.028)	0.013 (0.014)	589

One row per perturbation of the main specification, which is the first row, estimated on samples balanced across the post horizons. Unless perturbed: issuance-week treatment, country and sector-by-year effects; nine lagged firm financials, the lagged benchmark share, the issuance and eligible-issuance dummies; ever-eligible sample, $L = 3$, four pre- and two post-periods. Standard errors clustered by firm. Standard errors in parentheses; *, ** and *** denote significance at the 10, 5 and 1 percent level.

Table A-26: Issuance-week against secondary-market purchases

	(1)	(2)	(3)	(4)
	S + C + Y	S + C×Y	C + S×Y	S×Y + C×Y
Panel A. Investment rate (anchored capex)				
<i>Pooled τ_0-τ_2 (treatment effect)</i>				
Primary, full sample	0.015** (0.007)	0.019** (0.008)	0.022** (0.009)	0.018*** (0.006)
Check 1: drop secondary firms	0.017* (0.010)	0.020** (0.009)	0.018 (0.012)	0.029*** (0.009)
Check 2: secondary	0.007 (0.007)	0.006 (0.007)	0.011 (0.009)	0.007 (0.010)
Check 4: primary + sec. control	0.015** (0.008)	0.019** (0.008)	0.021** (0.009)	0.018*** (0.006)
<i>Pooled-pre (placebo)</i>				
Primary, full sample	0.002 (0.002)	0.003 (0.003)	0.000 (0.003)	-0.001 (0.004)
Check 1: drop secondary firms	0.004 (0.003)	0.004 (0.004)	0.002 (0.004)	-0.007 (0.008)
Check 2: secondary	-0.002 (0.002)	-0.003 (0.002)	-0.003 (0.002)	-0.003 (0.003)
Check 4: primary + sec. control	0.002 (0.002)	0.003 (0.003)	0.000 (0.003)	-0.001 (0.004)
N (pooled τ_0 - τ_2 , primary rows)	727	682	599	530
Panel B. Log markup				
<i>Pooled-post (treatment effect)</i>				
Primary, full sample	0.065** (0.026)	0.100** (0.039)	0.069** (0.030)	0.100 (0.064)
Check 1: drop secondary firms	0.070** (0.034)	0.127* (0.070)	0.139*** (0.044)	0.281** (0.121)
Check 2: secondary	-0.074 (0.062)	-0.064 (0.063)	-0.044 (0.053)	-0.029 (0.062)
Check 4: primary + sec. control	0.065** (0.026)	0.103*** (0.039)	0.061* (0.036)	0.100* (0.055)
<i>Pooled-pre (placebo)</i>				
Primary, full sample	0.012 (0.016)	0.005 (0.019)	0.013 (0.014)	0.002 (0.020)
Check 1: drop secondary firms	0.018 (0.024)	0.007 (0.028)	0.012 (0.024)	0.021 (0.047)
Check 2: secondary	0.012 (0.012)	0.001 (0.016)	0.015 (0.016)	0.011 (0.024)
Check 4: primary + sec. control	0.013 (0.016)	0.006 (0.019)	0.015 (0.014)	0.003 (0.021)

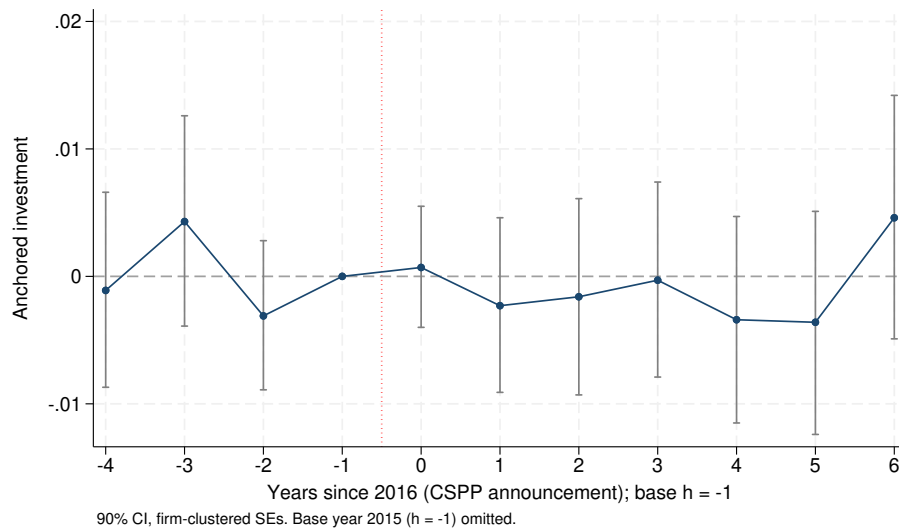
Panel A: investment rate; Panel B: log markup; the main specification of each in column 3. Rows: the issuance-week treatment on the full sample; Check 1 drops every firm with any secondary-market purchase; Check 2 uses the secondary-market treatment; Check 4 adds the contemporaneous secondary-market indicator as a control. Pooled post coefficients. Firm counts by treatment type in Appendix Table A-27. Standard errors clustered by firm. Standard errors in parentheses; *, ** and *** denote significance at the 10, 5 and 1 percent level. S, C, Y and F denote sector, country, year and firm fixed effects; × denotes their interaction.

Table A-27: Firms by type of purchase received

	Firms (ever-)	Treated firm-years
Primary (Purchased at issuance)	136	316
Any (Any purchase)	158	478
Observed secondary (Secondary-market purchase)	75	114
<i>Primary $\times$ secondary (firm level)</i>		
both primary and secondary	59	
primary only	77	
secondary only	16	
never treated (eligible controls)	22	

Ever-eligible estimation sample. Issuance-week and secondary-market purchases as defined in Section 2.1; the secondary measure excludes the initial portfolio.

Figure A-8: The announcement and real outcomes: pre-determined eligibility



Investment rate, the change in net capex since 2015 per euro of 2015 total assets: coefficients on pre-determined eligibility interacted with year indicators, base year 2015, with firm, country-by-year and size-quartile-by-year effects; 90 percent confidence intervals with the normal approximation, standard errors clustered by firm. Drawn from the saved horizon rows of the announcement regression.

distinction on this margin. Running the same panel design on cash dividends paid over base-year assets yields no detectable effect: the pooled coefficient is -0.8 percentage points (standard error 0.7), and it is stable across the battery reported in Table A-34. The upper end of its 90% interval, 0.4 percentage points of assets, is the ceiling against which the quantitative section validates the model's payout response.

Table A-28: Repeated against single purchases

	Continuous (3 consecutive years)	One-shot (single purchase year)
Panel A. Investment rate (anchored capex)		
τ_0	0.0081 (0.0108)	-0.0040 (0.0030)
τ_1	0.0301*** (0.0109)	-0.0006 (0.0034)
τ_2	0.0266*** (0.0101)	-0.0033 (0.0047)
Pooled τ_{0-2}	0.0216** (0.0093)	-0.0026 (0.0030)
Pooled τ_{1-2}	0.0283*** (0.0099)	-0.0020 (0.0037)
Cumulative 3-yr	0.0648** (0.0278)	-0.0079 (0.0090)
Pooled pre (placebo)	0.0004 (0.0028)	0.0004 (0.0028)
N (pooled-post)	599	658
N (pooled-pre)	536	536
Treated anchors	12	53
Anchor firms	12	51
Panel B. Log markup		
τ_0	0.042 (0.031)	-0.015 (0.019)
τ_1	0.064** (0.031)	-0.002 (0.025)
τ_2	0.101** (0.041)	0.002 (0.039)
Pooled τ_{0-2}	0.069** (0.030)	-0.005 (0.026)
Pooled pre (placebo)	0.013 (0.014)	0.013 (0.014)
N (pooled-post)	592	647
N (pooled-pre)	536	536
Treated anchors	11	51
Anchor firms	11	51

No-composition (balanced) LP-DiD, main specification (country and sector-by-year effects; nine lagged firm financials, the lagged benchmark share, the issuance and eligible-issuance dummies; ever-eligible sample), horizons 0 to 2. “Sequence”: firms purchased at t and in both following years; “single”: firms purchased at t and not at $t+1$ or $t+2$; the clean-control pool and every other design choice are identical. Panel A: investment rate; Panel B: log markup. Treated onsets and firms reported in the last rows. Standard errors clustered by firm. Standard errors in parentheses; *, ** and *** denote significance at the 10, 5 and 1 percent level.

Table A-29: The announcement and real outcomes: pre-determined eligibility

Fixed effects	Estimate	SE	<i>p</i>	N	Firms
Panel A. Log markup (log points)					
F + C×Y	1.1	(2.1)	0.593	3,920	624
F + C×Y + Size quartile×Y (headline)	-1.0	(2.3)	0.663	3,920	624
F + C×Y + Size quartile×Y + S×Y	0.3	(1.7)	0.845	3,918	624
F + C×Y + S×Y	2.4	(1.7)	0.168	3,918	624
Panel B. Investment rate (anchored capex)					
F + C×Y	-0.0070*	(0.0040)	0.0805	3,476	496
F + C×Y + Size quartile×Y (headline)	-0.0004	(0.0040)	0.9124	3,476	496
F + C×Y + Size quartile×Y + S×Y	-0.0043	(0.0043)	0.3119	3,470	496
F + C×Y + S×Y	-0.0104**	(0.0041)	0.0115	3,470	496

Intention-to-treat effect of eligibility at the announcement, an eligible non-commercial-paper bond outstanding on 9 March 2016, interacted with an indicator for 2016 onwards, estimated on 2012–2019, all firms, unbalanced panel. Panel A: log markup, coefficients in log points. Panel B: change in net capex since 2015 per euro of 2015 total assets, a level ratio. Fixed-effect initials: F firm, C country, Y year, S sector; size quartiles are of 2015 assets; the specification with size-quartile-by-year effects is the headline, in bold. Standard errors clustered by firm. Standard errors in parentheses; *, ** and *** denote significance at the 10, 5 and 1 percent level.

Table A-30: Announcement: balance

	Ineligible	Eligible	Difference	Normalised difference	Eligible firms
Log assets	21.330	23.830	2.500	1.460	74
Log revenue	21.006	23.410	2.404	1.323	74
Markup	1.486	1.487	0.002	0.003	73
Leverage	0.300	0.286	-0.014	-0.079	74
Cash over assets	0.097	0.077	-0.020	-0.305	74
Investment rate	0.046	0.050	0.004	0.114	61

Means over 2013–2015, before the announcement, of log assets, log revenue, the markup, leverage (total debt over assets), cash over assets and the investment rate (capital expenditure over lagged total assets) for panel firms with and without a non-commercial-paper eligible bond outstanding on 9 March 2016 (pre-determined eligibility), the difference, the normalised difference and the number of eligible firms with data. Firm panel of Section 2.1. Source: Refinitiv Worldscope; ECB eligible-assets list; author’s construction.

Table A-31: Announcement: robustness, markups

	Coef	SE	Pvalue	N	Firms
LagAssets	-0.002	0.023	0.923	3416	583
DropSizeMiss	-0.010	0.022	0.666	3766	569
FullWindow	-0.029	0.026	0.281	5681	694
FullWinNoCov	-0.022	0.024	0.358	4476	648
BalancedPanel	-0.008	0.027	0.767	2792	363
NoFirmFE	-0.019	0.030	0.521	3969	673

Log markup, pooled eligibility intention-to-treat, firm, country-by-year and size-quartile-by-year effects, one row per robustness variant. Standard errors clustered by firm; p-values from the normal approximation.

Table A-32: Announcement: markups, pooled

	Coef	SE	Pvalue	N	Firms
FEA	0.011	0.021	0.593	3920	624
FEB	-0.010	0.023	0.663	3920	624
FEC	0.003	0.017	0.845	3918	624
FED	0.024	0.017	0.168	3918	624

Log markup: pooled eligibility intention-to-treat, post = 2016–2019, sample 2012–2019. FEA: firm and country-by-year effects; FEB: FEA plus size-quartile-by-year effects (headline); FEC: FEB plus sector-by-year effects; FED: FEA plus sector-by-year effects. Standard errors clustered by firm; p-values from the normal approximation.

Table A-33: Announcement: net capex, pooled

	Coef	SE	Pvalue	N	Firms
FEA	-0.004	0.003	0.214	3402	582
FEB	0.000	0.004	0.999	3402	582
FEC	-0.005	0.004	0.210	3400	582
FED	-0.009	0.003	0.013	3400	582

Net capex over lagged assets: pooled eligibility intention-to-treat, post = 2016–2019, sample 2012–2019. FEA: firm and country-by-year effects; FEB: FEA plus size-quartile-by-year effects (headline); FEC: FEB plus sector-by-year effects; FED: FEA plus sector-by-year effects. Standard errors clustered by firm; p-values from the normal approximation.

Table A-34: Dividend payments: robustness battery and ceiling

Panel A. Robustness battery					
Perturbation	Pooled $h=0-2$	Cumul. 3-yr	Pooled-pre	N	
Baseline (headline specification)	-0.0078 (0.0069)	-0.0235 (0.0206)	0.0013 (0.0016)	541	
FE: sector, country, year	-0.0014 (0.0032)	-0.0041 (0.0095)	0.0007 (0.0011)	647	
FE: sector + country×year	0.0006 (0.0025)	0.0017 (0.0074)	0.0011 (0.0013)	586	
FE: sector×year + country×year	-0.0027 (0.0039)	-0.0082 (0.0117)	0.0009 (0.0022)	449	
Treatment: benchmark-inclusive purchase	-0.0021 (0.0033)	-0.0063 (0.0100)	0.0023 (0.0018)	470	
COVID: 2020–21 blanked	-0.0071** (0.0032)	–	0.0017 (0.0015)	723	
$L = 2$	-0.0097 (0.0085)	-0.0290 (0.0254)	0.0012 (0.0016)	531	
$L = 3$	-0.0096 (0.0086)	-0.0287 (0.0258)	0.0012 (0.0016)	523	
Controls: drop lagged cash/assets	-0.0079 (0.0070)	-0.0236 (0.0209)	0.0014 (0.0016)	541	
Controls: drop lagged cash/assets and gross profit/assets	-0.0075 (0.0070)	-0.0225 (0.0209)	0.0014 (0.0017)	541	
Panel B. Bounds on the headline pooled $h=0-2$ estimate					
	Estimate	SE	Upper end of 90% interval	Upper end $\times 1.55$	MDE
Headline (balanced)	-0.0078	(0.0069)	0.0036	0.0055	0.0201

Anchored LP-DiD on balanced samples, outcome the change in cash dividends paid from the base year per euro of base-year assets, a level ratio shown to four decimals. Columns: pooled $h = 0$ to 2, the cumulative three-year sum, the pooled pre placebo. Panel A: one row per perturbation of the main specification, as in the investment battery. Panel B: the headline estimate, the upper end of its 90 percent interval, and that bound in the model's units (times the assets-to-capital ratio, 1.55), which the quantitative section validates against. Standard errors clustered by firm. Standard errors in parentheses; *, ** and *** denote significance at the 10, 5 and 1 percent level.

B.5 Markup Estimation

Firm-level markups are the outcome variable of the analysis in Section 2.5. To estimate them, I rely on the production approach developed by De Loecker and Warzynski (2012), and later used, among others, by De Loecker et al. (2020), Díez et al. (2021) and De Loecker and Eeckhout (2025). It builds on the insight of Hall (1988) that the markup can be recovered from the firm’s cost-minimisation problem, without specifying the demand system or the mode of competition.

The markup formula. Consider a firm i which in year t produces output $Q_{i,t}$ and sells it at price $P_{i,t}$, using a vector of inputs of which at least one, denoted $V_{i,t}$ with price $P_{i,t}^V$, is variable, i.e. freely adjustable within the period and free of adjustment costs. The first-order condition of cost minimisation with respect to this input yields

$$\mu_{i,t} = \theta_{i,t}^v \frac{P_{i,t} Q_{i,t}}{P_{i,t}^V V_{i,t}}, \quad (\text{A-2})$$

where $\mu_{i,t}$ is the markup of price over marginal cost and $\theta_{i,t}^v$ is the output elasticity of the variable input. In other words, the markup is the ratio of two quantities: the output elasticity of the variable input, which must be estimated, and that input’s revenue share, which can be read from the balance sheet. The variable input used throughout the paper is the cost of goods sold (COGS), so the denominator of the share is the firm’s COGS and the numerator is its net sales, both deflated with a country-by-sector value-added deflator at the NACE-2 level, base 2015, built from Eurostat and OECD national accounts, with a country-level GDP deflator as the fallback where no sector series exists. Two alternative definitions of the variable input, materials only (COGS net of the wage bill and depreciation) and labour costs, are available in the data; the first delivers the same patterns, while the second yields a median markup below one in this panel, which makes it unsuitable as a measure of market power, and neither is used in the paper.

Estimating the output elasticity. The output elasticity is estimated sector by sector, assuming that all firms within a NACE-2 sector share the same Cobb-Douglas technology:

$$y_{i,t} = \beta^v v_{i,t} + \beta^k k_{i,t} + \omega_{i,t} + \varepsilon_{i,t}, \quad (\text{A-3})$$

where lower-case letters denote logs, $y_{i,t}$ is deflated net sales, $v_{i,t}$ deflated COGS, $k_{i,t}$ the deflated gross stock of property, plant and equipment, $\omega_{i,t}$ the productivity of the firm, which it observes but the econometrician does not, and $\varepsilon_{i,t}$ an error term collecting measurement error and unanticipated shocks to output. Under Cobb-Douglas, $\theta_{i,t}^v = \beta^v$ is constant within the sector. The difficulty is the classical one: since the firm chooses its inputs knowing $\omega_{i,t}$, an OLS estimate of (A-3) suffers from simultaneity bias. The control-function approach addresses it in two stages. In the first stage, productivity is proxied by inverting the demand for the variable input, $\omega_{i,t} = h_t(v_{i,t}, k_{i,t})$, and expected output is separated from the error term by estimating

$$y_{i,t} = \phi_t(v_{i,t}, k_{i,t}) + \varepsilon_{i,t},$$

where $\phi_t(\cdot)$ is approximated by a complete polynomial of degree three in $v_{i,t}$ and $k_{i,t}$, including all cross-products up to the third power in each input, together with year fixed effects. This stage delivers the fitted value $\hat{\phi}_{i,t}$ and the residual $\hat{\varepsilon}_{i,t}$. In the second stage, productivity is assumed to follow a first-order Markov process, $\omega_{i,t} = g(\omega_{i,t-1}) + \xi_{i,t}$, with $g(\cdot)$ a cubic polynomial and $\xi_{i,t}$ the innovation, which is unanticipated by the firm when it chooses its state variables. For any candidate (β^v, β^k) , productivity is computed as $\omega_{i,t}(\beta) = \hat{\phi}_{i,t} - \beta^v v_{i,t} - \beta^k k_{i,t}$; the innovation $\xi_{i,t}(\beta)$ is the residual of the regression of $\omega_{i,t}(\beta)$ on the cubic

in $\omega_{i,t-1}(\beta)$, and the parameters are the ones that make the innovation orthogonal to the instruments:

$$\mathbb{E} \left[\xi_{i,t}(\beta) \begin{pmatrix} 1 \\ v_{i,t-1} \\ v_{i,t} \\ k_{i,t} \end{pmatrix} \right] = 0. \quad (\text{A-4})$$

The lagged variable input and the current capital stock are the standard instruments: capital is predetermined, as it results from past investment, and the lagged input cannot respond to the current innovation. Following [Díez et al. \(2021\)](#), I add the contemporaneous value of the variable input to the set. This pins down the coefficient on the variable input directly from the second-stage moments, where it is identified: as [Akerberg et al. \(2015\)](#) show, the first stage alone cannot separate β^v from the proxy function. The moment conditions (A-4) are solved by minimising the GMM criterion with the identity weighting matrix using the Nelder-Mead algorithm; with four instruments for three parameters, the system is over-identified by one restriction.

Sample and implementation. The production function is estimated on the full firm-year panel of Section 2.1, 2010 to 2024, before the restriction to ever-eligible issuers, so that the output elasticities are estimated on as many firms as possible and are the same for purchased and non-purchased firms. The estimation sample keeps firm-years with strictly positive sales, COGS and gross fixed assets, and with a COGS-to-sales ratio between one percent and one, dropping holding companies with almost no production costs and firm-years with a non-positive gross margin. The utility, financial, real-estate, and public-administration sectors are excluded, as in the rest of the paper, and so are sectors with fewer than fifty firm-year observations, leaving forty-four NACE-2 sectors, each with its own pair $(\hat{\beta}^v, \hat{\beta}^k)$. The elasticities are estimated once and held constant over time, so a change in a firm’s markup after a purchase cannot originate from a re-estimated elasticity.

The revenue share and the measurement correction. The second ingredient of (A-2), the revenue share of the variable input, is corrected for the first-stage residual, as in [De Loecker and Warzynski \(2012\)](#): the share is computed as $\alpha_{i,t} = P_{i,t}^V V_{i,t} / \exp(y_{i,t} - \hat{\varepsilon}_{i,t})$, i.e. with revenue purged of measurement error and unanticipated shocks, which would otherwise enter the markup one for one. The markup of firm i in year t is then $\hat{\mu}_{i,t} = \hat{\beta}_{s(i)}^v / \alpha_{i,t}$, with $s(i)$ the sector of the firm. In logs,

$$\ln \hat{\mu}_{i,t} = \ln \hat{\beta}_{s(i)}^v + \ln P_{i,t} Q_{i,t} - \ln P_{i,t}^V V_{i,t} - \hat{\varepsilon}_{i,t},$$

which is the identity used by the decomposition of Table A-35. Two implications follow for the panel analysis of Section 2.5. First, since elasticity is constant within sector and over time, it drops out of every within-firm change in the log markup: the treatment effects estimated in the paper are invariant to the estimated output elasticities, which matter only for the markup level and for cross-sectional comparisons in the quantitative section. Second, the only estimated object that enters the within-firm variation is the correction $\hat{\varepsilon}_{i,t}$; without it, the treatment effect on the uncorrected ratio of revenue to COGS is 9.1 log points (standard error 5.3) against the 6.9 of the headline, as reported in Table A-35, so the correction makes the estimate more conservative rather than generating it.

Table A-35: Accounting decomposition of the markup effect

Pooled post ($h = 0-2$)	Coefficient	SE	p
Log markup (headline)	0.069**	(0.030)	0.022
ln revenue	0.098	(0.159)	0.539
$-\varepsilon$, DLW measurement correction (0.091 $\rightarrow$ 0.069)	0.021	(0.045)	0.638
$-\ln \text{COGS}$	0.008	(0.161)	0.962
Uncorrected ratio, ln revenue $-\ln \text{COGS}$	0.091*	(0.053)	0.088
Observations	592		

Pooled post coefficients of the main markup specification on each component of the identity log markup = log revenue $-\log \text{COGS} - \varepsilon$, where ε is the measurement correction of the production approach (the output elasticity's first-stage term); rows with a minus sign enter the identity with a minus. The last row is the uncorrected ratio, log revenue minus log COGS. Sample: ever-eligible issuers, the estimation sample of the main markup regression, 592 observations. Standard errors clustered by firm. Standard errors in parentheses; *, ** and *** denote significance at the 10, 5 and 1 percent level.

C Model

C.1 Derivations and Conventions

Interest-rate convention. The model is written in prices; interest rates are introduced only for interpretation, for the mapping to observable spreads and for the tax code of Section 4.1, which sees interest. Following Jeenas (2026), I define the implicit per-period rates from gross returns. A one-period bond sold at Q_1 pays one unit at date 2, so that $1 + r_1 = 1/Q_1$: of the unit paid, Q_1 is principal and $Q_1 r_1 = 1 - Q_1$ is interest. A multi-period bond sold at Q_0 pays $1 - \lambda$ at date 1 and λ at date 2; its one-period gross return marks the unmatured tranche at the date-1 price, $1 + r_0 = [(1 - \lambda) + \lambda Q_1]/Q_0$, so that the date-1 payment splits into principal $Q_0 - \lambda Q_1$ and interest $Q_0 r_0$. The remaining tranche is treated as rolled over at date 1 at the price Q_1 , with interest $\lambda Q_1 r_1$. Total interest over the life of the bond is then $Q_0 r_0 + \lambda Q_1 r_1 = 1 - Q_0$, the face value in excess of the issue proceeds; however, λ splits it across dates. This convention is what makes the date-0 first-order condition (20) free of the tax: the benefit and the cost of date-0 debt scale by the same factor for every firm interior in issuance.

Footprints in face value. The programme's objects of Section 3 can be defined equivalently in market value, as in the text, or in face value, more in line with the empirical analysis. Defining $A_t^{CB} \equiv \int_j \sum_{i=1}^I a_{t,j}^{CB}(i) dj$ and $\bar{A}_t^{elig} \equiv \int_j \sum_{i \in \tilde{\mathcal{I}}} a_t(i) dj$, the same steps give the footprint relationship $A_t^{CB} = \bar{\phi} \bar{A}_t^{elig}$.

Central bank: budget constraint. Putting all these elements together, the central bank budget constraint is

$$h_t^{CB} + \int_j \sum_{i=1}^I Q_{t,j}(i) a_{t,j}^{CB}(i) dj + d_{t-1} = \int_j \sum_{i=1}^I a_{t-1,j}^{CB}(i) dj + Q_t^d d_t + T_t$$

where, in addition to the variables already defined, Q_t^d is the price of the risk-free reserves in period t and T_t are lump-sum transfers to the household.

Intermediate goods producers: closed forms and issuance. Equating the user cost of capital u_t to (19) after having substituted (17) into it, the exponent on capital collapses because $1 - \alpha(\eta - 1)/G = 1/G$,

and desired capital is in closed form at both dates:

$$k_t^* = \alpha^G (1 - \alpha)^{(1-\alpha)(\eta-1)} I^{-1} z_{t+1}^{\eta-1} \mu_{t+1}^{-\eta} u_t^{-G} W_{t+1}^{(\alpha-1)(\eta-1)} Y_{t+1,j}^{1-\frac{\eta}{\theta}} Y_{t+1}^{\frac{\eta}{\theta}} P_{t+1}^{\eta} \quad t = 0, 1. \quad (\text{A-5})$$

Issuance then follows from the budget constraints: at date 0,

$$a_0 = \frac{k_0^* - e}{Q_0},$$

and at date 1, for the constrained firm, from (25) at $\text{div}_1 = 0$,

$$a_1 = \frac{k_1^* - \left[(1 - \tau) \Upsilon_1 - a_0 (1 - \lambda - \tau Q_0 r_0) + (1 - (1 - \tau) \delta) k_0 \right]}{Q_1}.$$

The bound at the repeat issue. Let F be the funds a purchased firm raises at the repeat issue, P the central bank's purchase of that issue and H the intermediary's holding, so that $F = H + P$. By the intermediary's first-order condition (22), the price of the issue is decreasing in H , so a purchase that raises the price does so by lowering H below the holding the intermediary would have taken without the purchase: $\Delta H < 0$ whenever $\Delta Q_1 > 0$. Hence $\Delta F = \Delta H + P < P$: the extra funds the purchase lets the firm raise are smaller than the purchase itself, and a fortiori so is the extra capital, since part of the funds replaces retained earnings at the margin. The bound is reached only when the intermediary's holdings do not respond to the price, i.e., when the slope of the holding cost is zero.

C.2 Quantitative

C.2.1 Computation

The economy. Twelve firms in each of forty-four sectors, 528 firms, with productivity types drawn once from the calibrated log-normal. Eligibility applies the panel's size-quartile shares to the model's size distribution and consumes no random numbers; 122 firms are eligible. The purchased set is drawn once from the eligible set, size-weighted on each firm's issuance under the announced benchmark, with weighted sampling keys in the manner of Efraimidis and Spirakis (2006), a scale found by bisection so that the purchased share of eligible issuance equals the measured 0.649, and a pairwise-swap refinement run to exhaustion. Seventy firms are purchased. The draw is fixed at the first converged evaluation of a calibration and reused at every later evaluation, so that the map of Figure A-9, the comparative statics and the designs carry no sampling noise from the draw.

One equilibrium. A single unanticipated shock at date 0 reveals the whole purchase path. The solution is computed in four steps. The no-policy equilibrium; the equilibrium under the announced benchmark, priced into every eligible issue before date-0 issuance; the surprise deviation at date 0 after issuance, which revalues the sunk issue and installs the corresponding capital; and the date-1 equilibrium with the known repeat purchase on purchased firms, solved from the date-0 state with the aggregates of the benchmark as starting values. The date-0 issuance problem and the date-1 sectoral Cournot problem are solved as fixed points, with damping in the sector loop and a guard against firms alternating between the two branches of the price schedule; the date-2 aggregates close an outer loop. Every no-policy solve checks that no firm is within its reference holding at the repeat issue, so that the reference is identified from firms on the slope.

One evaluation of the parameters. An evaluation at a parameter vector starts from the nearest converged solution, walks the four friction parameters to their new values one at a time in a fixed order with midpoint retries, re-fits the net-worth quadratic and the overhead cost to their panel anchors, reads the habitual payout from the no-policy solve with the cost switched off, inverts the payout curvature on the measured sensitivity over the firms on the slope, and then computes the four steps above at the measured persistence. One evaluation takes between one hour and an hour and a quarter on one core, most of it in the benchmark solve. Moments are determined to about 10^{-3} percentage points of yield and one basis point of spread, the tolerance of the inner fits; the first-issue parameters are declared converged when both residuals fall within that tolerance in a single evaluation.

The calibration. Because the repeat-issue moments are out of reach, the calibration is not a root-finding problem in four unknowns. The repeat-issue plane is mapped with a scrambled Sobol design (Sobol', 1967; scrambling as in Owen, 1995) over the log slope and the reference, holding the first-issue parameters fixed; the map has 21 points, of which 15 converged, and 11 pass the admissibility tests. The repeat-issue parameters are chosen from the map by the rule of Section 4.3. The first-issue parameters are then solved by design: four evaluations around the candidate, a linear fit of the two first-issue moments on the log slope and the reference, a two-by-two solve, and one confirming evaluation. The first-issue moments are nearly invariant to the repeat-issue parameters under security-specific slopes, which is what makes the two steps separable. Each rate of the ladder repeats the map at its own rate with the first-issue parameters re-solved; the alternative anchor re-solves the first-issue parameters only.

The admissibility tests. A point of the repeat-issue map is admissible if it passes four tests: the payout response of purchased firms stays below the upper end of the data's interval, measured on pre-purchase capital; date-1 consumption is positive; the intermediary still places a positive amount at the repeat issue; and the spread before the purchase is positive, so that the repeat issue is priced on the slope. The search along the direction of a larger yield effect ends at the first failure, and the rule records what ends it: the payout ceiling, the solver's convergence limit (a region of the map where the equilibrium cannot be computed), or the spread test.

Counterfactual designs. Each design changes only the purchase profile: who is bought and at which intensity, with the first-issue programme size of the headline programme. The announced benchmark and the no-policy economy are those from the calibration (for the wider-eligibility designs, the benchmark is re-solved on the wider list), and each design is solved at the measured persistence and with a repurchase probability of zero. The announced policy at repurchase probability one reproduces the saved benchmark to 10^{-10} , a check on the counterfactual code.

The strategic term. The measurement in Section 4.5 takes the headline economies, the no-policy, the announced-benchmark, and the realised-path solutions, and re-solves each sector's share fixed point to a tolerance of 10^{-14} to obtain the base point. For each firm, its log capital is perturbed by $\pm 10^{-4}$ (and by $\pm 10^{-3}$ as a check), the twelve-firm Cournot equilibrium of its sector is re-solved at the saved wage and aggregates, and each rival's output elasticity to the firm's capital is the central difference. The strategic term relative to the direct return is $(\mu_i/\alpha)(1/\eta - 1/\theta) \sum_{m \neq i} s_m \varepsilon_{m,i}$, with s_m the rival's sector share and $\varepsilon_{m,i}$ its output elasticity. The discrepancy between the two step sizes is below 10^{-8} , the elasticities agree with the implicit-function Jacobian of the share fixed point to 10^{-11} , and a closed-form profit identity holds at every firm to 10^{-10} . Sales-weighted means by group and date are reported in the text.

Hardware and reproducibility. All runs are single-threaded on one machine, at most four in parallel, on MATLAB R2024b. Every run prints a checksum of its moment definitions and a certificate of 128 property checks; the economy at the switch-off point of Appendix C.1 reproduces the earlier version’s equilibria to machine precision, and the same solution was certified byte-identical across two machines.

C.2.2 Identification Details and the Reachable Set

Table A-36: Parameters set outside the model

Parameter	Value	Description	Source
<i>Preferences and technology</i>			
β	0.9804	Discount factor	2% safe rate
α	0.333	Capital share	Standard
δ	0.08	Depreciation rate	Data
η	5.75	Within-sector elasticity of substitution	De Loecker et al. (2021)
θ	1.2	Across-sector elasticity of substitution	De Loecker et al. (2021)
φ	0.25	Frisch elasticity of labour supply	Chetty et al. (2011)
$I \times J$	12×44	Firms per sector $\times$ sectors	Data
σ_z	0.35	Dispersion of log productivity	Data
ρ_z	0.959	Persistence of log productivity	Data
<i>Firms and finance</i>			
τ	0.246	Corporate tax rate	Data
λ	0.75	Share of the first bond repaid at date 2	Jeenas (2025), annualised
κ	0.001	Central bank’s intermediation cost	Gertler and Karadi (2011)
$\phi^{\max}$	0.70	Per-issue purchase cap	ECB legal rule
E	25	Household endowment at date 0	Normalisation
<i>Programme, measured</i>			
$\bar{\phi}$	0.0853	Announced share of eligible issuance bought	Data
ϕ_0	0.131	Per-issue intensity on purchased firms	Data
ρ	0.823	Repurchase probability	Data
	0.574	Purchased share of eligible firms	Data
	0.649	Purchased share of eligible issuance	Data
	0	Eligible share of firms, smallest size quartile	Data
	0.057	Eligible share of firms, second size quartile	Data
	0.297	Eligible share of firms, third size quartile	Data
	0.583	Eligible share of firms, largest size quartile	Data

One model period is one year. “Data” in the Source column refers to the firm panel of Section 2.1 and to the bond data of the same section: δ is depreciation and amortisation over gross PPE; $I \times J$ are the NACE two-digit sectors and the mean number of firms per sector; σ_z and ρ_z the within-sector dispersion of log productivity and its persistence corrected for measurement error; τ the mean effective tax rate; $\bar{\phi}$ the ECB’s primary-market purchases over eligible issuance and ϕ_0 the same over the issuance of purchased bonds, both 2017–2022; ρ the one-year repurchase probability of Table 1; the three unnamed rows are the purchased share of eligible firms and of eligible issuance, and the eligible share of firms in each size quartile; the productivity process is a discretised log-normal AR(1) with the panel’s within-sector dispersion and persistence; the overhead scale and the net-worth endowment are fitted inside every solve to the panel’s overhead-to-revenue share and net-worth-to-assets ratio (Table 6).

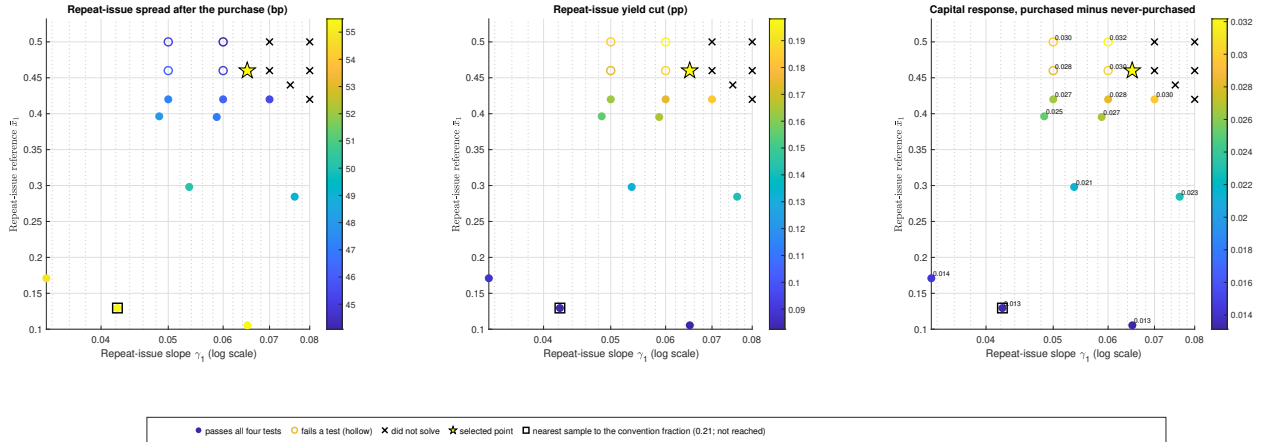
Table A-37: The payout sensitivity in the data

	Δ dividends / assets
Δ (gross profit – overhead) / assets	0.054 (0.010)
Year effects	Yes
Firm-years	5,127
Firms	609
Sample	Full panel

Regression of the change in cash dividends over assets on the change in gross profit net of overhead over assets, in first differences with year effects, on the firm panel of Section 2.1; the coefficient is the moment that pins the payout curvature of Section 4.3. Standard errors clustered by firm. Standard errors in parentheses.

Figure A-9: The repeat-issue parameters: reachable set, safe rate 2 percent

Repeat-issue parameters explored, safe rate 2.0 % (first issue held at $\gamma_0 = 0.0079$, $\bar{\pi}_0 = 0.32$): 21 runs, 6 did not solve; the search ends at the solver wall



Three panels over the repeat-issue slope (log scale) and reference holding, first-issue parameters held at their calibrated value: the repeat-issue spread after the purchase (basis points), the repeat-issue yield cut (percentage points) and the capital response, purchased minus never-purchased firms. Markers distinguish points that pass all four tests, fail a test, or did not solve; the star is the selected point, the square the sample nearest to the convention fraction.

C.2.3 Why Simpler Economies Fail

Two of the four ingredients that separate the quantitative economy from the analytical one – the payout adjustment cost and the reference holding – are not there for realism alone. Three simpler economies, built and calibrated before this one, reproduce the measured yield effect but fail to deliver the capital response together with the payout null; the way each fails motivates the corresponding ingredient in the model. Table A-38 reports the three, alongside the economy of this paper, on the same validations. The first is the analytical economy of Section 3 itself, with a holding cost through the origin and a binary payout decision. Pinned to the yield effect, it fails to reproduce the empirical transmission channel: the constrained firms are small, while the purchased ones are large. Thus, the capital response of the purchased firms is exactly zero, as the irrelevance result of that section predicts. The second economy adds a minimum dividend in the spirit of Gilchrist et al. (2014): a firm-specific floor below which the firm does not cut its date-1 payout, fitted to the firm's type in the same way as the net-worth endowment of Section 4.2.⁶ However, at that

⁶ The ratio of the floor to capital reproduces the panel's mean of dividends paid over total assets, 2.3 percent per year, overall and in the bottom and top size deciles.

level, and at any level consistent with the data, the floor constrains no purchased firm, because large firms carry a buffer of internal funds far above it, and the capital response stays at zero. The third trial adds the payout adjustment cost but keeps a holding cost through the origin – i.e. without the reference holding. This economy transmits, but too little and at the wrong margin: pinned to the yield effect, its capital response is 0.012, below the interval, and its payout response is 0.0098, above it. Without a reference holding, the price effect of a purchase pins the slope of the holding cost, leaving nothing to separate the relief a purchase delivers from the level of the spread. Only the combination of a marginal cost of paying out, which the buffer of a large firm cannot absorb, and a reference holding, which separates the price effect from the spread, delivers both validations at once.

Table A-38: Simpler economies, same validations

Economy	Safe rate	First-issue yield effect (pp)	Capital response	Payout response	Purchased firms at a payout limit
Price only (analytical economy)	4%	−0.216	0.000	–	0 of 70
Payout floor	4%	−0.216	0.000	–	0 of 70
Payout cost, no reference holding	4%	−0.216	0.012	0.0098	0 of 70
This paper	2%	−0.217	0.031	0.0055	–
			0.099	−0.012	
Data		−0.217	[0.028, 0.171]	[−0.030, 0.0055]	

Model values are purchased minus never-purchased firms at repurchase probability 0.823, except the first two rows, whose capital response is zero at every repurchase probability. Price only: the analytical economy of Section 3. Payout floor: dividends bounded below at zero. Payout cost: the quadratic payout-adjustment cost without the reference holding. This paper: the full quantitative economy. Last column: purchased firms at the payout corner. Data row: the paper’s estimates with 90 percent intervals (Tables 3 and A-34).

C.2.4 Why the Markup Validation Fails

The gap between the model’s markup response and the data’s is first traced to the pass-through from capital to the markup; three candidate explanations are then considered: measurement, the equilibrium concept and the demand side.

The pass-through from capital to the markup. In the nested-CES Cournot block, firm i ’s markup is $\mu_i = \varepsilon_i / (\varepsilon_i - 1)$, where the demand elasticity it faces is a share-weighted average of the within-sector and across-sector elasticities: $1/\varepsilon_i = s_i/\theta + (1 - s_i)/\eta$. The elasticity of the markup to the firm’s own market share is therefore $(\mu_i - 1) s_i (1/\theta - 1/\eta) \varepsilon_i$, and the share responds to capital through the production function with an elasticity that cannot exceed the capital share, dampened by the rivals’ response. At the calibrated elasticities and the markups of purchased firms, this pass-through is about 0.066 per unit of log capital, the ratio of the model’s two responses in Table 7; the data’s ratio of the markup response to the capital response is about 0.7. The gap is a factor of ten, and no admissible value of the two demand elasticities closes it while keeping the panel’s markup levels.

Measurement. The production-approach markup of Appendix B.5 is the output elasticity of the cost of goods sold times the ratio of revenue to that cost. A purchase that raises revenue relative to a partly fixed cost of goods sold would raise the measured markup without raising the true one. In the panel, the elasticity of the cost of goods sold to revenue is 0.97, so this channel can account for at most a few percent of the measured response; the accounting decomposition of Table A-35 shows that the increase is revenue rising relative to the cost of goods sold, 9.1 log points on the uncorrected ratio, which the measurement

correction lowers to 6.9 of the headline rather than generating it. The measured markup is the true one for this comparison.

The strategic term as a candidate. The measurement of Section 4.5 shows that a purchased firm that internalised its rivals’ output response would value its capital about 15 percent more than the model’s firm does. This raises the capital response of purchased firms, and through the pass-through above their markup response, by a similar proportion: a candidate for a part of the gap, not for the whole of it.

C.2.5 Robustness

Table A-39: Calibration at other safe rates and anchors

	First-issue yield effect (pp)	First-issue spread (bp)	Repeat-issue yield effect (pp)	Repeat-issue spread (bp)	Capital response	Payout response	Consumption, date 1	K_1/K_0	What ends the search
Safe rate 0.5%	−0.217	119.4	−0.087	13.7	0.0165	0.00086	0.821	1.093	the spread floor and the solver wall
Safe rate 2.0% (headline)	−0.217	119.9	−0.191	44.7	0.0310	0.00546	0.940	1.053	the solver wall
Safe rate 3.0%	−0.216	119.7	−0.213	75.3	0.0308	0.00531	1.129	0.973	the payout ceiling and the solver wall
Safe rate 4.0%	−0.217	120.0	−0.219	107.3	0.0286	0.00528	1.243	0.909	the payout ceiling
Safe rate 2.0%, first-issue anchor −0.266	−0.267	120.3	−0.192	43.7	0.0311	0.00546	0.924	1.058	first issue re-pinned; repeat issue held at the headline point

All values are model values; one confirmed calibration per row. Data: first-issue yield effect −0.217 pp (−0.266 pp in the last row), first-issue spread 120 bp; the repeat-issue parameters target −0.217 pp and 120 bp; capital response 0.099 [0.028, 0.171]; payout response −0.0121 [−0.0298, 0.0055] (90 percent intervals, Table 7). Responses are the purchase part, purchased minus never-purchased firms; the payout response is over pre-purchase capital. The repeat-issue parameters are the admissible point closest to the repeat-issue yield effect; the last column names what ends that search.

Table A-40: Pricing and payout regimes by scenario and group

	Flat, pays dividends	Flat, no dividend	Sloped, pays dividends	Sloped, no dividend	Flat at date 0	Firms
No policy, all	0	0	416	112	0	528
No policy, purchased	0	0	67	3	0	70
No policy, never	0	0	47	5	0	52
Announced benchmark, all	0	0	419	109	0	528
Announced benchmark, purchased	0	0	67	3	0	70
Announced benchmark, never	0	0	49	3	0	52
Realised path ($\rho = 0.823$), all	0	0	420	108	0	528
Realised path ($\rho = 0.823$), purchased	0	0	69	1	0	70
Realised path ($\rho = 0.823$), never	0	0	47	5	0	52

Date 1: flat = the placement within the reference holding (priced at the safe rate), slope = above it; payer/corner = the dividend margin interior / at zero. The date-0 flat count refers to the multi-period bond’s regime at the benchmark classification.

Table A-41: Date-1 outcomes by repurchase probability

	Repeat-issue yield effect (pp)	Repeat-issue spread (bp)	Capital response	Payout gap, pre-purchase capital	Markup gap (log)
Repurchase probability 0.823	-0.1913	44.7	+0.0310	+0.0055	+0.00205
Repurchase probability 0.760	-0.1760	46.2	+0.0286	+0.0050	+0.00187
Repurchase probability 0.903	-0.2108	42.8	+0.0341	+0.0060	+0.00229
Repurchase probability 0.000	-0.0003	64.1	+0.0009	-0.0001	-0.00021
Repurchase probability 1.000	-0.2348	40.3	+0.0380	+0.0068	+0.00258

The purchased firms and the date-0 purchase are fixed; only the expected repurchase at date 1 varies. Probability 0 is a single purchase, 0.823 the measured repurchase probability, 0.760 and 0.903 its any-gap and ninety-day alternatives, 1 full persistence; headline programme, anchor -0.217 .

C.3 Additional Tables and Figures

Table A-42: Further purchase designs

Design	Firms bought	Share bought per issue	Programme size (headline = 1)	Capital gained per euro	Output, date 2 (%)	Markup, date 2 (%)	Welfare (%)	Welfare per euro
Headline programme	70	0.131	1.000	0.205	0.437	0.103	0.0124	0.0240
Non-payers (capped at 70% per issue)	6	0.700	0.293	0.278	0.162	0.013	0.0024	0.0160
Non-payers, 5% of the programme size	6	0.134	0.049	0.458	0.048	0.008	0.0017	0.0680
Non-payers, 10% of the programme size	6	0.269	0.102	0.366	0.077	0.010	0.0023	0.0437
Non-payers, 15% of the programme size	6	0.403	0.159	0.343	0.111	0.013	0.0027	0.0333
Non-payers, 20% of the programme size	6	0.537	0.221	0.328	0.145	0.014	0.0029	0.0254
Non-payers, 25% of the programme size	6	0.672	0.281	0.287	0.160	0.014	0.0025	0.0175
Peak (2017) programme, headline frictions	70	0.231	1.790	0.210	0.796	0.185	0.0205	0.0222
Single purchase (no repeat purchase)	70	0.131	0.567	0.026	0.033	0.008	0.0048	0.0163

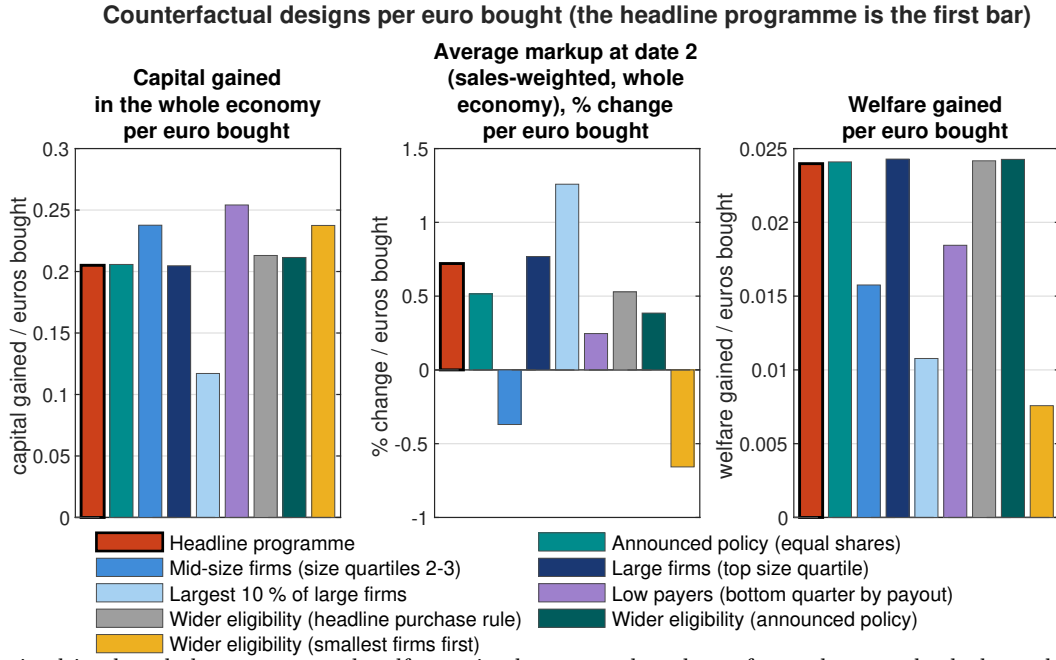
Every design has the headline programme's size at the first issue; the size over both dates follows each design's repeat issuance. Columns: firms bought; share bought per issue, euro-weighted where it varies; programme size relative to the headline; capital gained in the economy per euro bought at face value over both dates; output and the sales-weighted average markup at date 2, percent change from no policy; welfare, percent change of no-policy discounted welfare, and per euro bought. Repurchase probability 0.823, safe rate 2 percent; the headline programme is the reference row. Non-payers: the eligible firms paying no dividend at the announced benchmark; at the headline programme's size the 70 percent per-issue cap binds, so the programme falls short of them; the five scan rows spend 5 to 25 percent of the headline programme's size.

C.3.1 Reading the Results

This appendix explains the results in Sections 4.5 and 4.6. Every number is recomputed from the solved economies of the headline calibration.

The purchase part is a reallocation. The announcement part of a change is the benchmark against no policy; the purchase part is the realised path against the benchmark; together they make the total. Moving from the benchmark to the realised path, never-purchased eligible firms lose the announced support

Figure A-10: Capital and welfare per euro bought, by purchase design



Capital gained in the whole economy and welfare gained per euro bought at face value over both dates, by design; the headline programme is the first bar, the other designs in the order of Table 9. Repurchase probability 0.823, safe rate 2 percent.

Table A-43: Welfare with and without the central bank's holding cost

Design	Baseline	No efficiency cost	
		No central-bank holding cost	
Headline programme	0.01240	0.01245	0.01320
Announced policy (equal shares)	0.01238	0.01241	0.01288
Mid-size firms (size quartiles 2–3)	0.00841	0.00857	0.01122
Large firms (top size quartile)	0.01263	0.01267	0.01328
Largest 10% of large firms	0.00580	0.00605	0.01047
Low payers (bottom quarter by payout)	0.01015	0.01033	0.01349
Wider eligibility (headline purchase rule)	0.01258	0.01263	0.01333
Wider eligibility (announced policy)	0.01255	0.01258	0.01302
Wider eligibility (smallest firms first)	0.00392	0.00420	0.00936

The central bank's holding cost (equation (8)) is quadratic in its full position in each bond, with the private intermediaries' slopes plus the efficiency cost κ of Gertler and Karadi (2011). It is paid out of the central bank's rebate to the household and enters no price or firm decision, so removing it changes welfare only through the resource constraint, with the allocation unchanged. No efficiency cost: $\kappa = 0$. No central-bank holding cost: the whole term removed.

(their capital falls by 2.3 to 2.6 percent in every design), while purchased firms gain; the sector share the never-purchased lose goes to purchased firms and ineligible rivals. When the purchased firms grow little, or are few (the headline, +0.7 percent; the large firms, +0.15; the largest tenth), the ineligible rivals gain share, and their markups rise (+0.005 to +0.030 percent). When the purchased firms grow a lot (the mid-size firms and the low payers, +8 percent; the smallest first, +13), the ineligible firms lose share, and their markups fall. In every design, the purchase part moves the same resources from all eligible firms to the purchased ones; it is not a purchase on top of the announcement.

Why buying the non-payers yields less per euro as the programme grows. Four reasons act together. As the programme grows, the six non-payers leave the payout corner one by one (all six are at the corner under the announcement; three, two, one, none, and none at 5 to 25 percent of the programme size), and a firm off the corner pays part of the relief out. The relief runs out: the repeat-issue spread after the purchase falls from 75 to 55, 34, 8 and 0 basis points, and at 20 and 25 percent of the programme size two and five of the six firms are priced at the safe rate, where purchases do nothing. The central bank's convex cost per euro rises. Moreover, part of the gain does not scale with the programme size, because the announcement is kept in every design; extrapolated to a programme of zero size it leaves 0.0013 of capital and 0.00033 of welfare, which inflates the per-euro values at the small end. The decline is gradual, with no threshold.

The welfare accounting. Welfare is the discounted sum of consumption minus the disutility of work, and consumption is output minus investment minus holding costs. At firm i , the value of the marginal product of capital is $VMPK_i = p_i \alpha y_i / k_i$, and that of labour is $VMPN_i = p_i (1 - \alpha) y_i / n_i$. A firm with markup μ_i equates marginal revenue, value over μ_i , to cost, so $VMPN_i = \mu_i W$ and $VMPK_i = \mu_i u_i$, with u_i the safe rate plus the firm's spread plus depreciation. The social cost of a unit of capital is $r + \delta$ and of an hour is W , the household's marginal disutility of work; the spread is the intermediary's holding cost and is counted in its own line. To first order,

$$\Delta \mathcal{W} \approx \sum_i [VMPK_i - (r + \delta)] \Delta k_i + \sum_i (\mu_i - 1) W \Delta n_i - \Delta h, \quad (\text{A-6})$$

where Δh is the change in private and central bank holding costs; the capital wedge is $(\mu_i - 1)(r + \delta)$ from market power plus $\mu_i s_i$ from finance, about $0.45 \times 0.10 + 1.45 \times 0.007 \approx 0.055$ for a large firm. The date-2 terms of (A-6) reproduce the change in date-2 output within about one percent. The value of the marginal product of capital is 0.135, 0.135, 0.139 and 0.154 across the size quartiles, with markups of 1.22, 1.25, 1.30 and 1.45 and a cost of capital nearly flat at 0.107 to 0.110. The capital each design adds, and its value per unit, are 0.017 and 0.196 for the largest tenth, 0.029 and 0.164 for the headline, 0.039 and 0.156 for the low payers, 0.035 and 0.145 for the mid-size firms and 0.034 and 0.145 for the smallest first. The largest tenth has the highest value per unit and adds the least capital: eight firms at the cap, payers passing part of the relief to dividends, and 114 eligible firms losing the announced support. The mid-size and smallest-first designs add the same capital at the same value per unit; the smallest-first design loses more in the central bank's cost because it buys 58 to 70 percent of each issue against 37 percent.

The largest tenth against the headline. Its welfare is 0.0066 percent lower. Of that gap, 0.0039 is the central bank's convex cost, 0.0018 is the smaller private holding cost saved, and 0.0010 is less net output: output -0.0061 , a smaller cost of capital $+0.0041$ and less work $+0.0010$. About 60 percent of the gap is the central bank's cost; the rest is the little capital the design adds. The giants themselves grow under this

design: their capital rises by 7.6 percent, output by 3.3, hours by 1.1 and market share by 2.0 percent, and their markup by 1.1 percent.

Welfare units. Welfare changes are reported in percent of no-policy discounted welfare. Eighty-four percent of that level is date-0 consumption, which the endowment normalisation funds; the endowment enters date-0 consumption only and, with utility linear in consumption, affects no allocation. In units that do not depend on it, the headline programme's gain is 0.26 percent of one year's output, 0.13 percent of the present value of output at dates 1 and 2, and 0.024 per euro bought.