Heterogeneous Treatment Effects and Counterfactual Policy Targeting Using Deep Neural Networks: An Application to Central Bank Corporate Credit Facilities

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I present a novel two-step semi-parametric difference-in-differences (DiD) estimator for computing dynamic (heterogeneous) treatment effects and policy counterfactuals. In the first step, deep neural networks are used to compute non-parametric terms in a setting with high-dimensional controls. These are inputs into the estimator evaluated in the second step. The estimator is applied to study the effects of the Federal Reserve's Corporate Credit Facilities (CCFs) on the dynamics of firm cash holdings, leverage, payout, and investment. I show that the proposed estimator delivers comparable results to static (homogeneous) treatment effects obtained from DiD panel regressions and dynamic (homogeneous) treatment effects from event study regressions with two-way fixed effects, though with important differences attributable to selection bias and heterogeneity. Firms generally increased cash holdings and leverage, while payout and investment initially fell. Firms eligible for the CCFs accumulated less cash and began deleveraging in 2021, relative to ineligible firms. Eligible firms exhibit relatively larger payouts, while they do not invest more, suggesting that the CCFs failed to meet their objective of boosting real effects. Counterfactual eligibility criteria can possibly deliver an improvement in investment outcomes.

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All errors are my own.

1. Introduction

In announcing, and later expanding, the Corporate Credit Facilities (CCFs) in early 2020, the Federal Reserve references its dual-mandate to promote maximum employment and stable prices.[1] Through purchases of corporate bonds in the primary and secondary market, as well as exchange-traded funds (ETFs), the CCFs were intended to support credit to firms and business activity, despite the shock created by the COVID-19 pandemic.

While the CCFs could provide up to $750 billion in financing, actual purchases totaled just $14.1 billion at 2020 year-end. However, markets priced in significant contingent support by the Fed, especially if conditions were to deteriorate and tail risks materialized (Haddad, Moreira, and Muir 2025). Consequently, the bulk of the financial market effect of the CCFs were realized around its announcement, with a significant decline in bond spreads.[2] Record bond issuance followed,[3] as did equity issuance, particularly for more financially constrained firms.[4] Firms used the proceeds to satisfy their demand for cash,[5] paying back heavily utilized credit lines drawn on prior to the intervention.[6] Research on the implementation of corporate bond purchase program in Europe by the European Central Bank (ECB), operational since 2016, found decreases in financing costs for firms eligible for the program which translated into higher bond issuance and payouts to shareholders but not investment.[7] Was this also the case for the Fed CCF intervention during the pandemic?

There is very good reason to believe the CCFs should have supported investment by reducing financial constraints. Surveys of Chief Financial Offers (CFOs) by Barry et al. (2022) during the pandemic suggest that improving financial flexibility would improve hiring and capital spending. This echoes the CFO survey results of Campello,

[1] https://www.federalreserve.gov/newsevents/pressreleases/monetary20200323b.htm, https://www.federalreserve.gov/newsevents/pressreleases/monetary20200409a.htm

[2] See Boyarchenko, Kovner, and Shachar (2022); D'Amico, Kurakula, and Lee (2020); Flanagan and Purnanandam (2020); Gilchrist et al. (2021); Haddad, Moreira, and Muir (2021); Kargar et al. (2021); Momin and Li (2022); O'Hara and Zhou (2021).

[3] See Becker and Benmelech (2021); Boyarchenko, Kovner, and Shachar (2022); Darmouni and Siani (2024); Dutordoir et al. (2024); Halling, Yu, and Zechner (2020); Hotchkiss, Nini, and Smith (2022)

[4] See Dutordoir et al. (2024); Halling, Yu, and Zechner (2020); Hotchkiss, Nini, and Smith (2022).

[5] See Acharya and Steffen (2020); Darmouni and Siani (2024); Pettenuzzo, Sabbatucci, and Timmermann (2023)

[6] See: Acharya and Steffen (2020); Darmouni and Siani (2024); Greenwald, Krainer, and Paul (2020).

[7] See De Santis and Zaghini (2021); Grosse-Rueschkamp, Steffen, and Streitz (2019); Todorov (2020).

Graham, and Harvey (2010) during the Great Financial Crisis (GFC), where the vast majority of CFOs stated that financial constraints restricted investments in attractive projects. Firms do seem to have prioritized financial flexibility at the onset of the pandemic by initially cutting payouts.[8] Were these actions taken to both preserve cash and support operations? Becker and Benmelech (2021) and Darmouni and Siani (2024) find firms do not increase investment, but does this hold on a relative basis for firms targeted by the CCFs versus those that are not? How about after accounting for the heterogeneous reactions of firms?[9] And is this still true in the years following the pandemic, as the shock fades, and investment opportunities improve?

To answer these questions, I introduce a novel two-step semi-parameteric difference-in-differences (DiD) estimator to compute dynamic (heterogeneous) treatment effects from the onset of the pandemic in 2020 through 2023. To achieve identification, I use an extremely high-dimensional set of controls, allowing for rich, potentially, non-linear interactions. The number of controls far exceeds the number of observations used in estimation, thus requiring tools from the double/debiased machine learning (DML) and causal machine learning literature to perform proper inference. This is accomplished by using an influence function (IF) estimator, alternatively called a Neyman orthogonal score function.

The first step requires estimating the non-parametric terms in the structural equation for the potential outcomes model which specifies the treatment effect. The structural equation for potential outcomes is the linear combination of a non-parametric intercept term and the interaction of a treatment indicator (eligibility for the CCFs) and a non-parametric slope term. The slope term captures individual level heterogeneity, that is, conditional average treatment effects (CATEs). Another ingredient for the estimator is an estimation of propensity scores, the probability of a firm being classified as eligible for the CCFs, which is also modeled as a non-parametric function of a high-dimensional set of characteristics. The non-parametric terms are estimated using deep feed-forward neural networks. Deep nets are used because ability to approximate continuous functions of real variables arbitrarily well, showing exceptional performance in this regard (Chronopoulos et al. 2023). Farrell, Liang, and Misra (2021a) provides the theoretical justification for using deep nets to estimate non-parametric terms in the first step of two-step semi-parametric estimation

[8] See Ali (2022); Cejnek, Randl, and Zechner (2021); Gormsen and Koijen (2020); Krieger, Mauck, and Pruitt (2021); Pettenuzzo, Sabbatucci, and Timmermann (2023).

[9] See Darmouni and Siani (2024); Greenwald, Krainer, and Paul (2020); Haque and Varghese (2021); Hassan et al. (2023); Pagano and Zechner (2022).

and inference. The expression for the two-step semi-parametric DiD estimator is derived from the general expressions for IF estimators given in Farrell, Liang, and Misra (2021b).

Identification of average treatment effects (ATEs) requires that the assumptions of unconfoundedness and overlap are satisfied. I defend the assumption of unconfoundedness by appealing to the extremely large set of covariates used in the estimation of the non-parametric terms, along with the usage of deep nets allows for the estimation of rich interactions and potential non-linearities. The covariate set consists of the quarterly histories of 37 to 60 pre-treatment variables going back up to 10 years, along with indicator variables for industry classification. However, I note that at the cost of identifying the average treatment effect on the treated (ATET), instead of the ATE, given the DiD nature of the estimator, I can use the weaker assumptions of conditional no anticipation and parallel trends, instead of unconfoundedness. This requires that, conditional on the pre-treatment variables, firms did not anticipate the CCFs in 2019 and that comparable firms would have exhibited similar dynamics, absent intervention. A general lack of pre-trends in event study regressions suggests that conditional parallel trends is a justifiable assumption, and estimates of the ATE and ATET are not statistically different from zero. Overlap is justified by the slow-moving nature of credit ratings, which determined firm eligibility for the CCFs, and the significant overlap in the distributions of fundamental characteristics and market-based measures of risk (CDS spreads) across eligible and ineligible firms.

I compare the dynamic (heterogeneous) treatment effects from the novel estimator to static (homogeneous) treatment effects from a DiD panel regression and dynamic (homogeneous) treatment effects from an event study design with two-way fixed effects. The magnitudes of the point estimates and standard errors are similar. The results show that while all firms increased leverage and cash holdings as a proportion of 2019 year-end assets, firms eligible for the CCFs increased leverage and cash to a relatively lower extent than ineligible firms. Both the static (homogeneous) treatment effects and the dynamic (heterogeneous) treatment effects indicate that eligible firms do not show show an increased investment response over the treatment horizon, thus suggesting that the CCFs may not have met its objective for producing real effects. These results are robust to alternative proxies for investment. I argue in preference for the result from the two-step semi-parametric estimator, since the high-dimensional set of controls can better control for potential selection bias and account for heterogeneity. In contrast, all models general indicate that eligible firms did increase payouts to shareholders, at least on a relative basis, with the two-step semi-parametric estimator showing that this was apparent even in 2020.

Since an intermediary step to computing ATEs using the two-step semi-parametric estimator is to compute the distribution of CATEs, I can study the effects of counterfactual policy targeting schemes, particularly to see if investment can be improve. This is also identified if the unconfoundedness assumption and overlap condition holds. Although, without the unconfoundedness assumption, the estimator is still valid and recovers the predictive effects of alternative policy targeting schemes, which is still important for policy diagnostics. The CFO survey evidence of Campello, Graham, and Harvey (2010) and Barry et al. (2022) suggests that targeting weaker, more financially constrained credits may produce stronger real effects. This is also echoed in the simple theoretical setup of Brunnermeier and Krishnamurthy (2020). Counterfactual policy targeting loosening the CCFs eligibility criteria to also target BB-rated firms exhibits weak to inconclusive evidence of improving investment outcomes in 2020, while showing no evidence of improved outcomes for later years.

This paper contributes to several literatures. First, it contributes to the extensive literature on the financial and real dynamics of firms during the COVID-19 pandemic that was earlier cited. Among these, the paper closest to this one is that of Darmouni and Siani (2024). They also show that firms drastically increased bond issuance following the announcement of the Fed CCFs and that the proceeds from these were used to pay down previously drawn credit lines and build cash buffers. They find that firms maintained equity payouts but did not increase investment. While I echo most of their findings, I utilize a different identification strategy that involves inferring treatment effects from comparing the relative dynamics of eligible firms versus ineligible firms using high-dimensional controls in a non-linear setting, rather than an instrumental variable (IV) approach as was used in their approach. Additionally, to the best of my knowledge, I am the first to study the counterfactual effects of the Fed CCFs from counterfactual policy targeting.

Second, this paper contributes to the DML and CML literature. The canonical references to using DML for estimation and inference are Belloni, Chernozhukov, and Hansen (2014) and Chernozhukov et al. (2018).[10] The DML literature commonly uses a partially linear model for specifying the structural potential outcomes model where the intercept term is referred to as an infinite-dimensional nuisance parameter and the slope term is the product of a constant, homogeneous treatment effect and a

[10] See also the textbook Chernozhukov et al. (2024).

treatment indicator. Farrell, Liang, and Misra (2021b) provides the general expression for the IF estimator for smooth structural models. From this I derive a two-step semi-parametric DiD estimator with non-parametric, heterogeneous CATEs and show that the dynamic (heterogeneous) treatment effects estimated from this estimator is similar to the static (homogeneous) treatment effect estimated from a panel DiD regression and to the dynamic (homogeneous) treatment effects estimated from event study regressions with two-way fixed effects. This estimator has a similar functional form and is analogous to the doubly-robust DiD estimator of Sant'Anna and Zhao (2020) in the non-ML context and the DML DiD estimator of Chang (2020). While the derivation of the estimator that I utilize in this paper is straight-forward, to the best of my knowledge, I am the first to present this, at least in the context of an application in finance.

Third, this paper contributions to the finance and accounting literatures featuring applications of DML and two-step semi-parameteric estimators, more generally. The majority of these papers utilize DML for model selection and inference in high-dimensional settings. Among empirical asset pricing papers, specifically, on factor models for explaining the cross-section of stock returns, Feng, Giglio, and Xiu (2020) is the first to use DML to assess new factors given control factors from the factor zoo. Maasoumi et al. (2024) proposes a DML-based method to identify factors with the most significant explanatory power for explaining the cross-section of stock returns, rather than just evaluating new factors as in Feng, Giglio, and Xiu (2020). Borri et al. (2024) uses DML to compare their proposed novel, nonlinear asset pricing factor for explaining the cross-section of equity returns against the factor zoo, finding that their proposed factor significant while the majority of factor zoo is not. Other empirical asset pricing applications include Hansen and Siggaard (2024), who uses DML to revisit explanations of the post-earnings announcement drift (PEAD), and Gomez-Gonzalez, Uribe, and Valencia (2024), who employs DML to study the effect of economic complexity index on sovereign yield spreads, considering a large number of explanatory variables.

There are also numerous accounting and corporate finance applications of DML. Bilgin (2023) studies the significance of cash holdings, current ratio, and non-debt tax shield in determining firms' capital structure in the face of high-dimensional controls. De Marco and Limodio (2022) uses DML to understand which characteristics among a high dimensional set contributes the most to bank climate resilience. Movaghari, Tsoukas, and Vagenas-Nanos (2024) studies the determinants of cash holdings. Wasserbacher and Spindler (2024) studies the heterogeneous effects causal effect of ratings on the leverage ratio, also taking into consideration high dimensional controls. Finally, Yang, Chuang, and Kuan (2020) studies the 'Big N' audit quality effect.

Papers specifically using the two-step semi-parameteric estimation/inference methodology of Farrell, Liang, and Misra (2021a) and Farrell, Liang, and Misra (2021b), along with deep nets to estimate non-parametric terms, include Kim and Nikolaev (2024a) and Kim and Nikolaev (2024b). Kim and Nikolaev (2024a) uses the approach of Farrell, Liang, and Misra (2021b) to specify a semi-parameteric function that allows for interactions between numerical and narrative data to forecast operating profitability. Similarly, Kim and Nikolaev (2024b) studies the narrative context provided by disclosures around the release of numeric information to understand the effect of contextual information on earnings persistence, combining textual and numeric data via deep nets to uncover heterogeneous effects. In a spirit similar to Farrell, Liang, and Misra (2021b), Simon, Weibels, and Zimmermann (2022) embeds a structural model of portfolio allocation in a deep net via the loss function used to train the deep net and learn the parameters for portfolio weights.

While my application also utilizes the methods from this literature for model selection and inference in high-dimensional settings, and to infer heterogeneous effects, in the context of estimating non-parameteric terms using deep nets, I also compute counterfactual treatment effects for policy evaluation. To the best of my knowledge, this is the first such application of its kind in the finance and accounting literatures.

The rest of the paper is organized as follows. Section 2 provides the institutional background of the Fed CCFs. Section 3 describes the data and presents the descriptive statistics for eligible and ineligible firm variables. Section 4 presents the static (homogeneous) treatment effects obtained from a panel DiD regression. Section 5 presents the dynamic (homogeneous) treatment effects obtained from event study regressions with two-way fixed effects. Section 6 presents the dynamic (heterogeneous) treatment effects obtained from the two-step semi-parameteric DiD estimators. Section 7 presents the results from counterfactual policy targeting experiments. Section 8 concludes.

2. Institutional Background

The Federal Reserve initially announced the Primary Market Corporate Credit Facility (PMCCF) and the Secondary Market Corporate Facility (SMCCF) on March 23, 2020.[11] Both facilities were established with a liquidity backstop provided by the Treasury. Initially, the CCFs, along with the Term Asset-Backed Securities Loan Facility (TALF), had the potential to provide up to $300 billion in financing.[12]

Eligibility for the CCFs was determined at the issuer level with eligible issuers needing to be American companies with headquarters and material operations domestically. Additionally, eligible issuers needed to be rated investment-grade (IG). In the case issuers had multiple ratings, the plurality of these ratings were required to be IG. Depository institutions and depository holding companies were excluded from eligibility. Moreover, the SMCCF also targeted IG ETFs.

On April 9, 2020, the Federal Reserve increased the size of the facilities, such that the CCFs could provide up to $750 billion in financing.[13] Additionally, the eligibility criteria of the facilities were amended such that issuers meeting the rating criteria as of March 22, 2020 were deemed eligible for the facilities. Effectively, this meant that issuers ('Fallen Angels') downgraded out of eligibility between the initial and subsequent announcement dates had their eligibility restored. The term sheet of the SMCCF was also amended to expand eligible ETFs to include high-yield (HY) ETFs.

The Federal Reserve began the purchases of ETFs on May 12, 2020[14] and of secondary market cash bonds according to a "broad, diversified market index" on June 15, 2020.[15] Participation in the SMCCF initially required corporate issuers certify compliance with the eligibility criteria.[16] The SMCCF continued purchases until December 31, 2020, finishing with a total portfolio of $14.1 billion, while the PMCFF was not utilized.[17] The SMCCF began winding down its ETF holdings on June 7, 2021 and corporate bond holdings on July 12, 2021, completing the divestitures by August 31,

[11] https://www.federalreserve.gov/newsevents/pressreleases/monetary20200323b.htm

[12] https://home.treasury.gov/news/press-releases/sm951

[13] https://www.federalreserve.gov/newsevents/pressreleases/monetary20200409a.htm

[14] https://www.newyorkfed.org/newsevents/news/markets/2020/20200511

[15] https://www.federalreserve.gov/newsevents/pressreleases/monetary20200615a.htm

[16] https://www.newyorkfed.org/markets/primary-and-secondary-market-faq/archive/corporate-credit-facility-faq-201204

[17] https://newbagehot.yale.edu/docs/united-states-primary-market-corporate-credit-facility-and-secondary-market-corp

2021.[18] Equity capital was returned to the Treasury and the facilities were terminated by the end of 2021. For information on the full range of public sector interventions undertaken in the United States during the pandemic, see Clarida, Duygan-Bump, and Scotti (2021).

3. Data

3.1. Sample Construction

Firm fundamental characteristics are obtained from Compustat North America via Wharton WRDS and the Financial Ratios Suite by WRDS. Firms incorporated outside of the United States are dropped, as are firms with two-digit NAICS code 52, which corresponds to the Finance and Insurance industry. This drops firms outside the eligibility criteria for CCF cash bond purchases. Additionally, CDS spread data for five-year senior unsecured debt is obtained from IHS Markit through WRDS. Eligibility criteria is determined using issue ratings corresponding to senior unsecured debt (which correspond to issuer ratings), obtained from Mergent Fixed Income Securities Database (FISD) via Wharton WRDs.

3.2. Descriptive Statistics

Tables 1 and 2 report key fundamental characteristics and financial indicators for public eligible and ineligible traded firms, respectively, for the 2019 fiscal year. There are more eligible firms than ineligible firms, but the counts for each are sizeable. In general, eligible issuers are larger, more solvent, and more liquid. The larger size of eligible firms are reflected in far larger equity valuations, higher debt levels, greater asset holdings, more sales, and higher EBITDA. While eligible firms have more employees, the gap here is much smaller compared to ineligible firms. Ineligible firms are less solvent as reflected in higher book and market leverage and larger five-year senior unsecured CDS spreads. The lower liquidity of ineligible firms are reflected in lower EBITDA interest coverage, debt-to-EBITDA, and profit margin. These trends are reinforced in the distributions of size and performance indicators in Figure 1, and of solvency and liquidity indicators in Figure 2.

Figure 3 graphs the distribution of log CDS spreads on March 20, 2020, the last business day before the CCF announcement on March 23, 2020, for both eligible

[18] https://www.newyorkfed.org/markets/secondary-market-corporate-credit-facility

Table 1. Descriptive Statistics - Eligible

	Median	Mean	Standard Deviation	Observations
Common Equity at Market Value (Millions)	22,421.93	58,526.71	122,246.57	321
Total Debt (Millions)	5,718.30	13,352.45	22,580.30	358
Total Assets (Millions)	17,642.35	39,488.83	73,815.12	358
Employees (Thousands)	16.30	56.42	146.44	345
Book Leverage (Percent)	49.03	49.86	17.21	345
Market Leverage (Percent)	21.84	24.00	13.64	321
Sales (Millions)	8,980.15	25,430.24	51,988.78	358
EBITDA (Millions)	2,211.30	5,106.62	10,418.35	340
EBITDA Interest Coverage	9.44	13.77	17.03	338
Debt-to-EBITDA	2.87	3.17	1.82	340

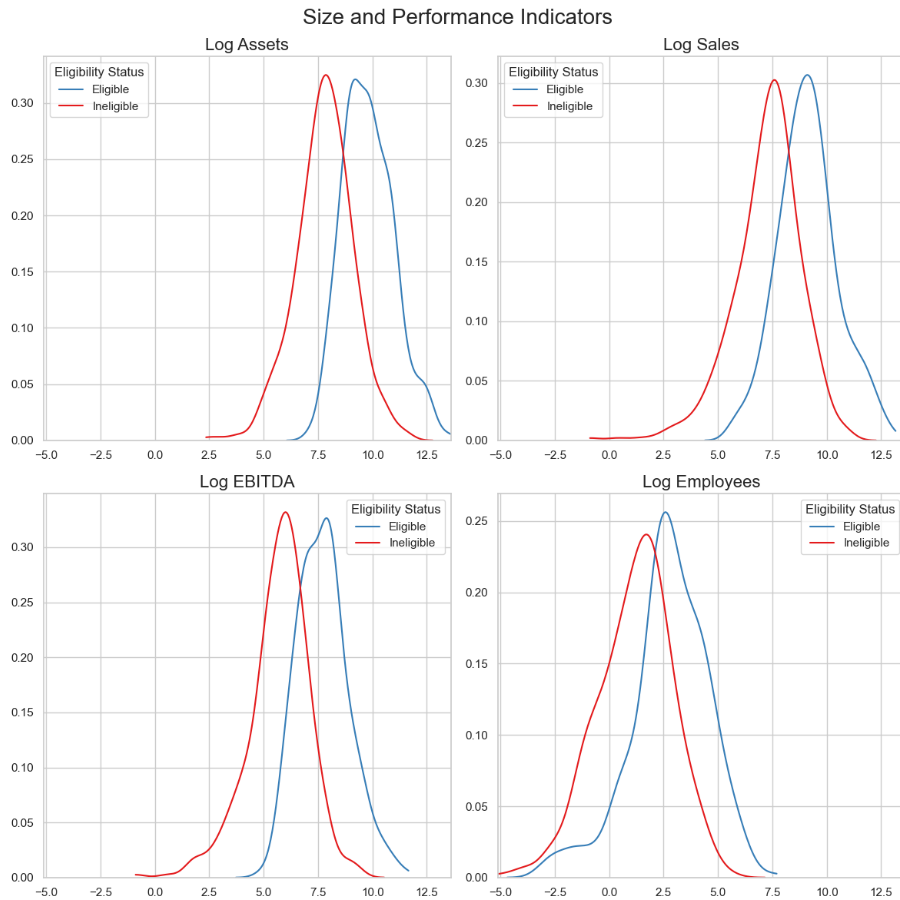
The table shows accounting and financial information for publicly traded firms who are identified to be eligible for direct cash bond purchases under the Fed CCFs based on their ratings. The data corresponds to fiscal year 2019. Compared to ineligible firms, eligible firms are far larger as measured by market equity, total assets, employee headcount, and sales. Moreover, they have stronger liquidity and solvency indicators.

Table 2. Descriptive Statistics - Ineligible

	Median	Mean	Standard Deviation	Observations
Common Equity at Market Value (Millions)	2,075.07	5,054.11	10,387.18	460
Total Debt (Millions)	1,043.55	2,532.42	4,979.49	464
Total Assets (Millions)	2,502.09	5,584.92	10,617.85	465
Employees (Thousands)	3.63	10.82	22.73	458
Book Leverage (Percent)	52.47	53.71	20.14	412
Market Leverage (Percent)	33.16	37.43	23.93	459
Sales (Millions)	1,667.11	3,556.65	6,182.18	462
EBITDA (Millions)	228.18	488.68	1,182.23	461
EBITDA Interest Coverage	3.86	3.89	16.70	452
Debt-to-EBITDA	3.65	3.92	25.65	460

The table shows accounting and financial information for publicly traded firms who are identified to be ineligible for direct cash bond purchases under the Fed CCFs based on their ratings. The data corresponds to fiscal year 2019. Compared to eligible firms, ineligible firms are far smaller as measured by market equity, total assets, employee headcount, and sales. Moreover, they have weaker liquidity and solvency indicators.

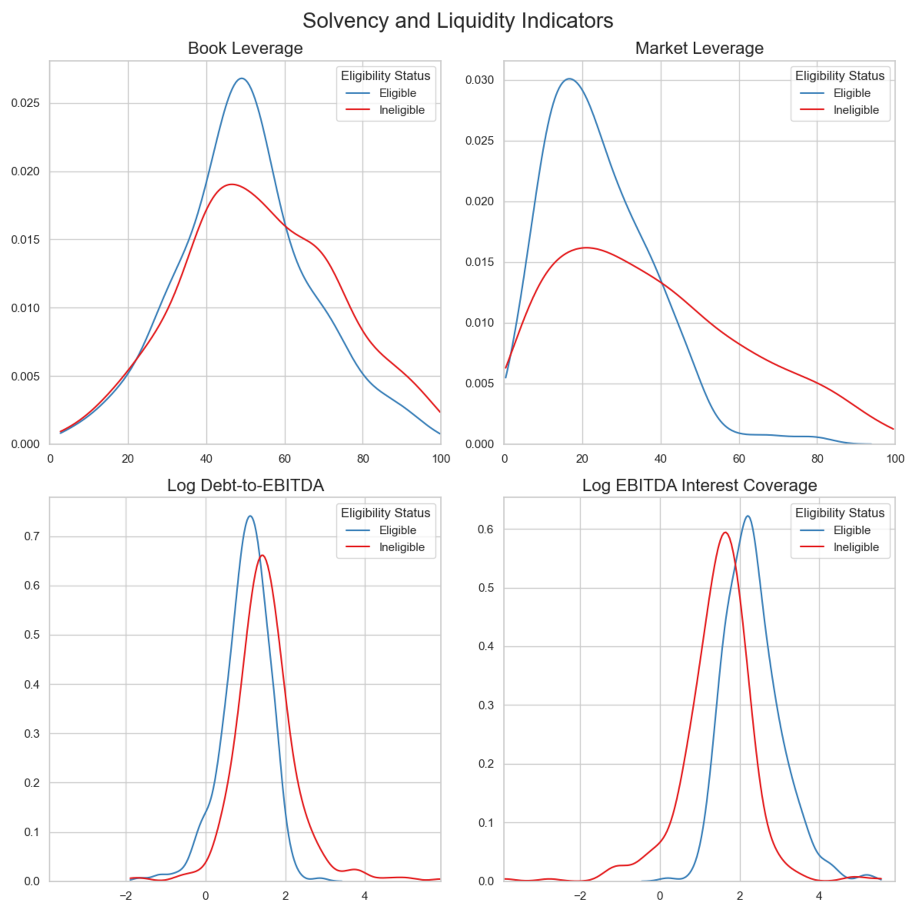
Figure 1. Eligible Issuers are Larger, with More Substantial Cash Flows



The figure shows the distributions of the logged values of several size and performance indicators across eligible and ineligible issuers of the Fed CCFs. Eligible issuers have more assets and higher employee headcounts. Additionally, they generate higher revenue and register higher EBITDA.

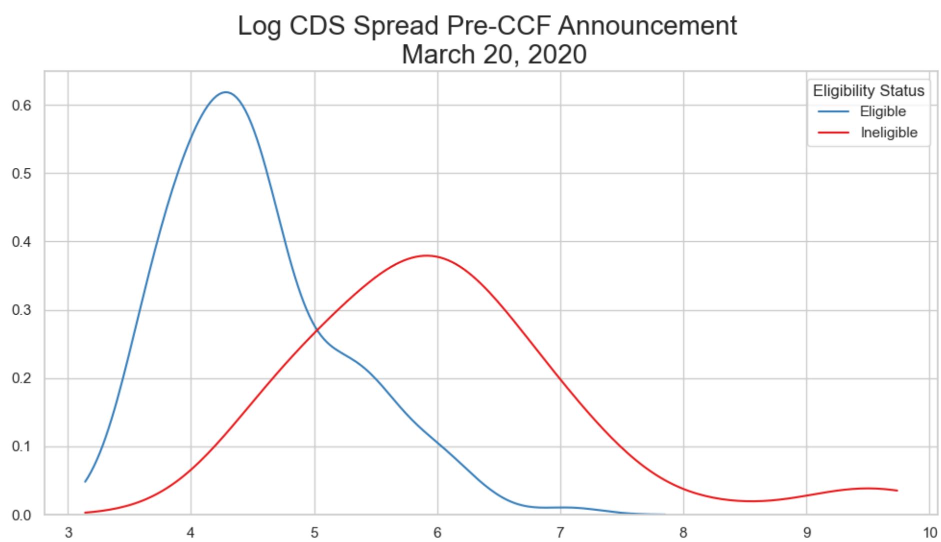
and ineligible firms. Firms with CDS spreads are a subset of all firms with public financials. The figure reinforces the information presented in the tables, but also reveals that eligible firms are not uniformly perceived to have lower default risk than ineligible firms. Notably, there is a significant overlap in the supports of the two distributions, with the support of eligible firms' CDS spread distribution almost entirely lying within the corresponding support for ineligible firms.

Figure 2. Eligible Issuers are also More Liquid with Lower Leverage



The figure shows the distributions of the logged values of several solvency and liquidity indicators across eligible and ineligible issuers of the Fed CCFs. While both sets of issuers have comparable distributions of book leverage, eligible issuers have far lower levels of market leverage (as measured with respect to firm market value). Additionally, eligible issuers have greater cash flow coverage of debt and debt servicing costs.

Figure 3. CDS Spreads Consistent with Higher Default Risk of Ineligible Firms



The figure shows the distributions of logged CDS spreads on March 20, 2020 prior to the Fed CCF announcement date on March 23, 2020, across eligible and ineligible firms. Consistent with the fundamental characteristics shown in Figures 1 and 2 and Tables 1 and 2, the market assessed ineligible firms to be riskier than eligible firms. However, there is a significant area of overlap between the two sets of firms.

4. Static Homogeneous Average Treatment Effects: Difference-in-Differences Regressions

Static (homogeneous) treatment effects are estimated using a difference-in-differences (DiD) regression. The specification is:

$$y_{i,t} = \beta_0 + \beta_1 \text{Eligible}_i + \beta_2 \text{Post}_t + \beta_3 (\text{Eligible}_i \times \text{Post}_t) + \gamma_i + \epsilon_{i,t} \tag{1}$$

where $y_{i,t}$ is the outcome variable of interest, Eligible_i is an indicator variable with value 1 if firm i was eligible for cash bond purchases under the CCFs, Post_t is an indicator variable equal to 1 if date t is 2020 or later, and γ are two-digit NAICS industry fixed effects. The static treatment effect is given by β_3. The DiD regressions are computed over 2017 to 2023. Standard errors are clustered by issuer and date.

4.1. Potential Selection Bias and Parallel Trends

For both the DiD panel regression in this section, and the event study design in Section 5, a key concern may be potential selection bias contaminating the estimated treatment effect, in addition to any biases attributable to ignoring heterogeneity. An obvious source of this selection bias may arise from the fact that eligibility for the CCFs is essentially a proxy for IG status.

Consequently, the treatment variable may simply be capturing the differing dynamics between IG and HY firms. Section 6 tackles this issue more seriously by using a large set of controls as well as permitting arbitrary interactions between these controls, motivated by Section 3.2 showing considerable overlap in the distributions of eligible and ineligible firms along fundamentals and market-based measures of risk. Nonetheless, the general lack of pre-trends observed in the event study regressions in Section 5 suggests that the parallel trends assumption can be justified and hence, the DiD regressions in this section identify the ATET. Section 6.6.1 includes further discussion about selection bias.

4.2. Results

Table 3 reports the DiD regression results for cash holdings (% 2019Q4 assets) and total debt (% 2019Q4 assets). Columns (1) and (3) show results without industry fixed-effects, while Columns (2) and (4) include industry fixed effects, but the results are

Table 3. Debt Levels and Cash Holdings Broadly Increased, With Negative Treatment Effect for Eligible Firms

Dependent Variables:	Cash (% 2019Q4 Assets)		Total Debt (% 2019Q4 Assets)	
Model:	(1)	(2)	(3)	(4)
Variables				
Constant	10.01***		36.37***	
	(0.9476)		(2.943)	
Eligible (Fed CCFs)	-4.477***	-3.120***	-10.54***	-12.49***
	(0.9565)	(1.009)	(1.969)	(2.010)
Post 2020	9.866***	9.903***	23.16***	23.17***
	(2.015)	(2.005)	(4.075)	(4.082)
Eligible (Fed CCFs) × Post 2020	-7.295***	-7.464***	-6.141**	-6.212**
	(2.042)	(2.046)	(2.733)	(2.729)
Fixed-effects				
NAICS (2-Digit)		Yes		Yes
Fit statistics				
Observations	9,912	9,912	9,502	9,502
R^2	0.03349	0.07229	0.07234	0.10256
Within R^2		0.02740		0.07712

Clustered (Issuer & Date) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The table reports the regression results from Equation 1 for cash holdings (% 2019Q4 assets) and total debt (% 2019Q4 assets). The results suggest that firms broadly increased leverage while increasing cash holdings in the treatment period (2020 onward). Additionally, the regressions pick up negative treatment effects for eligible firms for both variables, suggesting that these firms increased cash holdings and debt to a lesser extent than ineligible firms.

broadly consistent across the different specifications. The DiD regressions suggest that eligible firms generally hold less cash and have less debt than ineligible firms. Furthermore, the regressions suggest that firms broadly increased cash holdings and leverage in the treatment period (2020 onward). This result can be seen by the positive coefficient on the 'Post 2020' variable for ineligible firms and the sum of the coefficients for 'Post 2020' and 'Eligible (Fed CCFs) × Post 2020' for eligible firms. Interestingly, negative treatment effects are picked up on eligible firms' relative cash holdings and total debt. Hence, while eligible firms increased cash holdings and leverage in the treatment period, they appear to have done so proportionally less than ineligible firms.

Table 4. Eligible Firms' Payout Shows Positive Effect; No Effect Seen for Investment

Dependent Variables:	Dividends and Buybacks (% 2019Q4 Assets)		Capital Expenditures and R&D (% 2019Q4 Assets)	
Model:	(1)	(2)	(3)	(4)
Variables				
Constant	1.062***		2.456***	
	(0.2192)		(0.2956)	
Eligible (Fed CCFs)	0.9875***	0.8328***	-1.217***	-1.150***
	(0.2433)	(0.2519)	(0.3103)	(0.3168)
Post 2020	-0.1769	-0.1554	1.240*	1.305*
	(0.2470)	(0.2473)	(0.6771)	(0.6843)
Eligible (Fed CCFs) × Post 2020	1.180***	1.158***	-0.8407	-0.9016
	(0.2377)	(0.2345)	(0.6597)	(0.6642)
Fixed-effects				
NAICS (2-Digit)		Yes		Yes
Fit statistics				
Observations	9,641	9,641	9,798	9,798
R^2	0.00907	0.01695	0.00988	0.03614
Within R^2		0.00657		0.00882

Clustered (Issuer & Date) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The table reports the DiD regression results for payouts (% 2019Q4 assets),investment (% 2019Q4 assets), and capital expenditure (% 2019Q4 assets). Payouts are computed as the annual sum of dividends and share buybacks. Investment is proxied as the annual change in the gross value of Property, Plant, and Equipment. The results show a positive effect for firm payouts over the treatment period, while both investment and capital expenditure exhibit null effects, despite the general increases in cash holdings and leverage shown in Table 3.

Table 4 reports the DiD regression results for for payouts (% 2019Q4 assets) and investment (% 2019Q4 assets). Payouts are computed as the annual sum of dividends and share buybacks. Investment is proxied as the annual change in the gross value of property, plant, and equipment.[19] The results show a positive effect for firm payouts over the treatment period, while investment exhibit nulls effects. Together, Tables 3 and 4 suggest that while firms generally increased leverage and cash in the treatment period, this translated into higher payouts by eligible firms but not investment.

[19] In contrast, when investment is proxied by the annual change in the gross value of property, plant, and equipment, a positive coefficient is estimated for the eligible indicator, while the coefficient on the interaction term is negative. The causal ML results for the property, plant, and equipment investment proxy is discussed further in Section 6 and reported in Appendix 8.12.

5. Dynamic Homogeneous Average Treatment Effects: Event Study Regressions with Two-Way Fixed Effects

To study the dynamic impact of the CCF intervention, I employ event study regressions with two-way fixed effects. These have the functional form:

$$y_{i,t} = \sum_{\tau=-3}^{-2} \beta_\tau D_t^\tau \text{Eligible}_i + \sum_{\tau=0}^{3} \beta_\tau D_t^\tau \text{Eligible}_i + \gamma_i + \zeta_t + \epsilon_{i,t} \tag{2}$$

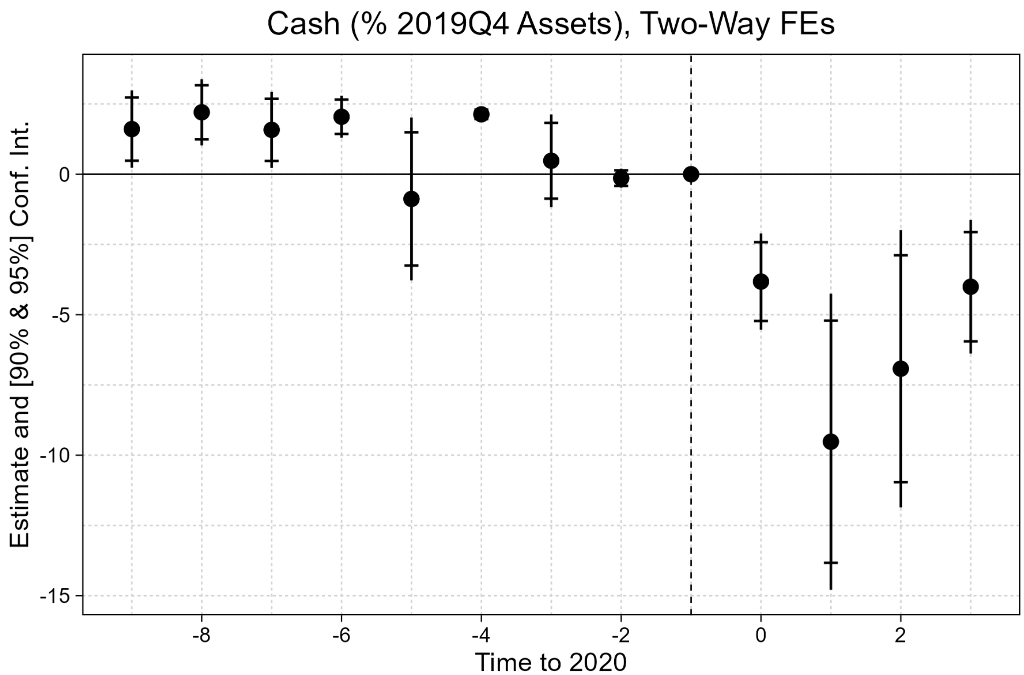
where $y_{i,t}$ is the outcome variable of interest, $D^\tau = \mathbf{1}\{t - 2020 = \tau\}$ is an indicator variable equal to 1 if the difference between the year t and 2020 is equal to τ, Eligible is an indicator variable with value 1 if the firm was eligible for direct cash bond purchases under the CCFs, 0 otherwise, and finally, β_τ are the coefficients being estimated. Two-way unit and time fixed effects are given by γ_i for issuer and ζ_t for year, respectively. The event study regressions are computed over the window 2017 to 2023. Standard errors are clustered by issuer and year.

Consequently, β_τ for $\tau \geq 0$ are dynamic treatment effects, while for $\tau < 0$, β_τ corresponds to a placebo or falsification test. However, notice that $\tau = -1$, $t = 2019$ is omitted from the regression specification; this is the baseline comparison group, which will also be the case for the two-step semi-parametric DiD estimator introduced in Section 6. Additionally, as discussed in Section 4, the estimation of treatment effects may suffer from potential selection bias and neglect of heterogeneity, which further motivates the method used in Section 6. Nonetheless, there generally appears to be a lack of pre-trends, which suggests that the parallel trends assumption holds for the treatment period.

Figure 4 shows the dynamic effects of the CCF intervention on firm cash balances as a proportion of total assets as of 2019Q4. The coefficient estimates prior to 2020 are null or positive, suggesting that either there are not meaningful differences in relative cash holdings between eligible and ineligible firms or that eligible firms hold more cash. However, after the onset of the pandemic, the dynamic treatment effects are negative, reaching a bottom in 2021 before reverting. This suggests that ineligible firms increased cash balances to a greater extent than eligible firms.

Figure 5 shows the dynamic effects on firm gross debt as a proportion of total assets as of 2019Q4. The coefficient estimates in the pre-treatment period are null or positive. Particularly, the recent coefficient estimates for 2017 and 2018 are positive,

Figure 4. Eligible Firm Cash Holdings Show Relative Decline, Before Reverting



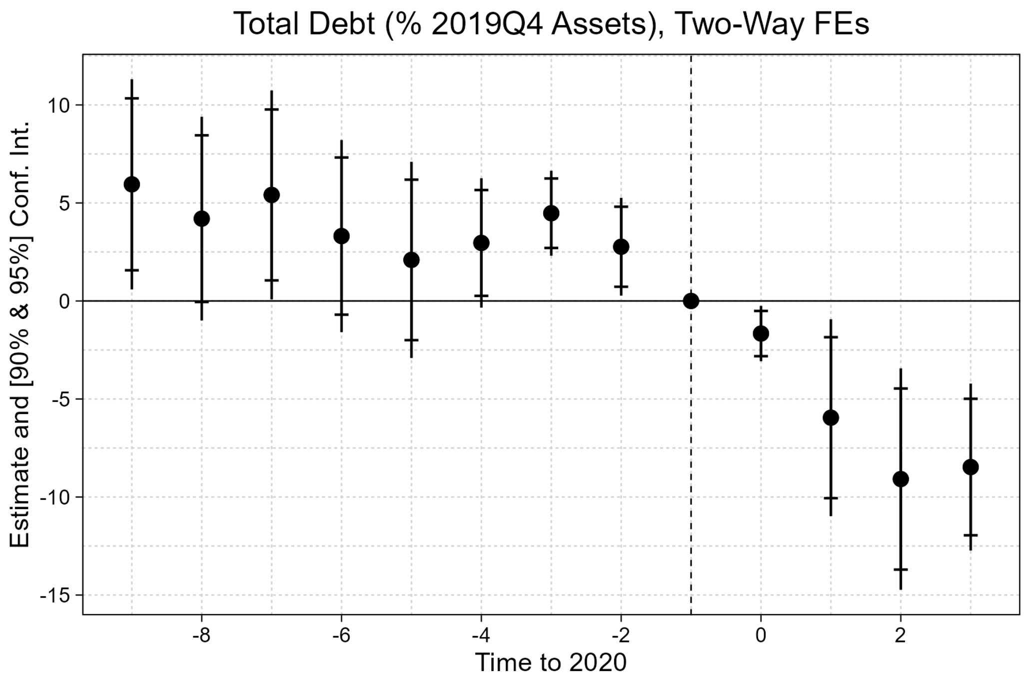
The figure plots the dynamic treatment effects from the regression given by Equation 2 for cash as proportion of 2019Q4 assets. The coefficient estimates prior to 2020 are null or positive, suggesting that either there are not meaningful differences in relative cash holdings between eligible and ineligible firms or that eligible firms hold more cash. However, after the onset of the pandemic, the dynamic treatment effects are negative, reaching a bottom in 2021 before reverting. This suggests that ineligible firms increased cash balances to a greater extent than eligible firms.

indicating that eligible firms are more leveraged than ineligible firms prior to the pandemic, with a possible declining trend. In the treatment horizon, the coefficients become negative and continue to decrease, indicating greater relative increases in leverage for ineligible firms.

Figure 6 plots the dynamic treatment effects for payouts as a portion of 2019Q4 total assets. The relative level of payouts between eligible and ineligible firms are generally null for the pre-treatment period. In the treatment horizon, the point estimates for the dynamic treatment effects are positive and increasing in the treatment horizon.

Figure 7 plots the dynamic treatment effects for investment as a proportion of 2019Q4 total assets. In the pre-treatment period, the relative levels of investment between eligible and ineligible firms were generally null in the pre-treatment period, although it was possibly negative and statistically significant more recently. While

Figure 5. Relative Leverage of Ineligible Firms Rise



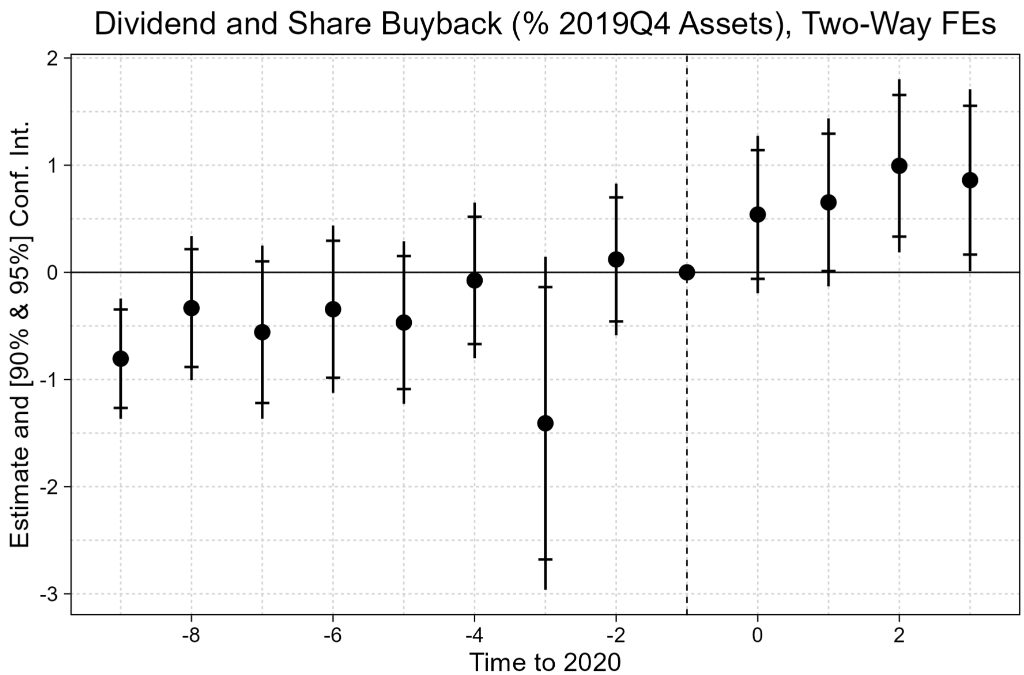
The figure plots the dynamic treatment effects from the regression given by Equation 2 for total debt as a proportion of 2019Q4 assets. The coefficient estimates in the pre-treatment period are null or positive. Particularly, the recent coefficient estimates for 2017 and 2018 are positive, indicating that eligible firms are more leveraged than ineligible firms prior to the pandemic, with a possible declining trend. In the treatment horizon, the coefficients become negative and continue to decrease, indicating greater relative increases in leverage for ineligible firms.

the point estimate falls in 2020, there is significant variation. It falls further in 2021 before starting to revert.

5.1. Discussion

Coefficients from the event study regressions are reported in Tables A1 in Appendix 8.1. The dynamic (homogeneous) treatment effects for cash and debt presented in this section broadly align with the static (homogeneous) treatment effects shown in Table 3. That is, the dynamic (homogeneous) treatment effects are consistently negative. Likewise, the dynamic (homogeneous) treatment effects from payouts are consistent with the positive effect found in Table 4. Interestingly, while the static (homogeneous) treatment effect was null over the entire treatment horizon, as seen in Table 4, the dynamic (homogeneous) treatment effects for investment were negative for 2021, 2022, and 2022. Moreover, the standard errors are particularly

Figure 6. Relative Payouts by Eligible Firms Rise

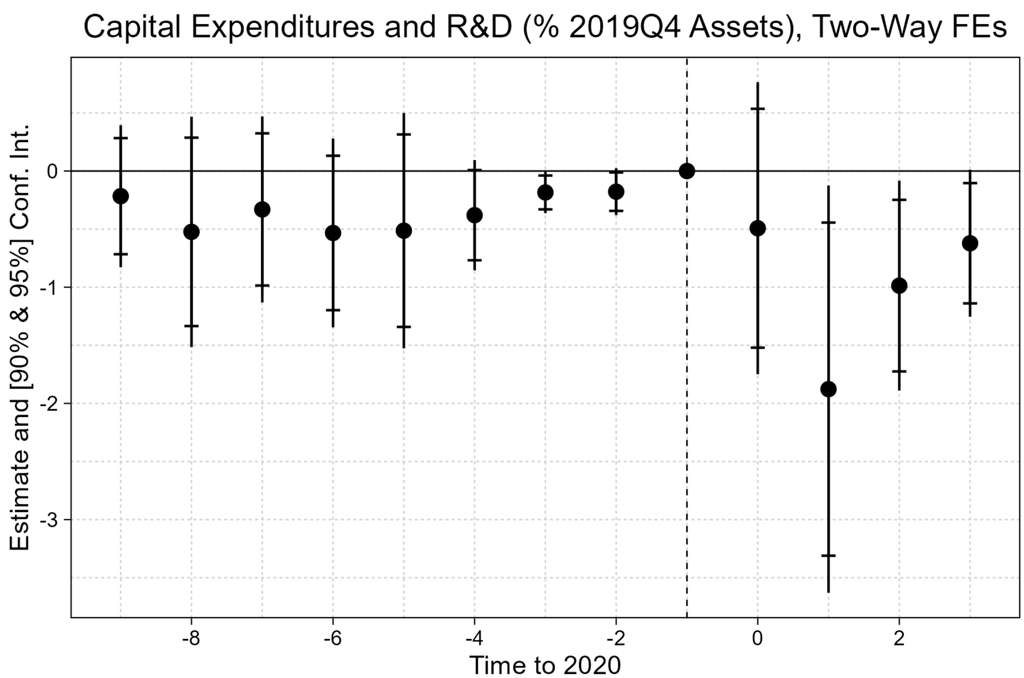


The figure plots the dynamic treatment effects from the regression given by Equation 2 for payouts as a portion of 2019Q4 total assets. The relative level of payouts between eligible and ineligible firms are generally null for the pre-treatment period. In the treatment horizon, the point estimates for the dynamic treatment effects are positive and increasing in the treatment horizon.

wide for investment, suggesting the presence of heterogeneous effects.

Overall, while the general absence of pre-trends may suggest that parallel trends may hold in the treatment period, there may still be concerns about potential selection bias or heterogeneous effects. Eligible issuers may have better navigated the pandemic by taking on relatively lower leverage and increasing cash buffers to a lesser extant, while being more cautious about investment and supporting payouts more as the crisis faded. The next section attempts to better address potential threats to identification from regression designs by using a deep nets in a setting with high-dimensional controls and flexible functional forms, in addition to accounting for heterogeneous effects.

Figure 7. Eligible Firms Display Relative Decline in Investment in 2020 Before Reversion



The figure plots the dynamic treatment effects for investment as a proportion of 2019Q4 total assets. In the pre-treatment period, the relative levels of investment between eligible and ineligible firms were generally null in the pre-treatment period, although it was possibly negative and statistically significant more recently. While the point estimate falls in 2020, there is significant variation. It falls further in 2021 before starting to revert.

6. Dynamic Heterogeneous Average Treatment Effects: Two-Step Semi-Parametric DiD Estimators

6.1. Overview of Empirical Design

As mentioned, a key concern of the regression frameworks in Sections 4 and 5 is the potential selection bias that is associated with the treatment variable being a proxy for IG status. IG firms may naturally have been more resilient than HY firms, taking on less leverage and increasing cash to a lesser extent, while maintaining firm payouts. The ideal experiment would compare two virtually identical firms that only differ based on treatment assignment (e.g. eligibility for the CCFs).

The causal ML approach used in this section attempts to take this idea to furthest extent possible by using a high-dimensional set of covariates that far exceeds the number of observations without imposing variable selection or functional form restrictions on the interactions across variables beforehand. The novel two-step semi-parametric DiD estimator for computing dynamic (heterogeneous) treatment effects presented here is comparable to an event study design with two-way fixed effects, which computes dynamic (homogeneous) treatment effects. The structural equation for potential outcomes consists of a linear combination of a non-parametric intercept term and the interaction between a treatment indicator and non-parametric slope term. The intercept term corresponds to the outcome of ineligible firms, or potential outcome of eligible firms had they not received treatment. The slope term corresponds to the unit-level heterogeneous treatment effect, otherwise called the CATE for eligible firms or the counterfactual, potential CATE for ineligible firms.

In the first step, the non-parametric intercept and slope terms are estimated using deep nets, as is the propensity score, which is a key ingredient in the estimators for the coefficients. The propensity score is the probability of a firm being classified as eligible given the high-dimensional set of characteristics. Deep nets are used because ability to approximate continuous functions of real variables arbitrarily well, showing exceptional performance in this regard (Chronopoulos et al. 2023). Farrell, Liang, and Misra (2021a) provides the theoretical justification for using deep nets to estimate non-parametric terms in the first step of two-step semi-parametric estimation and inference. Nonetheless, the results should be similar if using other high-quality ML algorithms, such as random forests (Belloni, Chernozhukov, and Hansen 2014; Chernozhukov et al. 2018).

However, because of the bias induced by regularization in a high-dimensional setting, an IF estimator, or Neyman orthogonal score function, is required. The expression for the DiD estimator used in this section is derived from the general formulation of IF estimators given in Farrell, Liang, and Misra (2021b). In addition, cross-fitting, the estimation and evaluation of models across different samples, is used to prevent overfitting and produce unbiased estimates (Chernozhukov et al. 2018). Section 6.3 goes into the requirements for identification of parameter estimates in greater detail, the key requirements are that the unconfoundedness (or selection on observables) assumption and the overlap condition holds (Farrell, Liang, and Misra 2021a). Given the DiD setup, unconfoundness can be relaxed to the conditional versions of the no anticipation and parallel trends to identify the average treatment effect on the treated (ATET), instead of the average treatment effect (ATE) (Chernozhukov et al. 2024). Indeed, the general lack of pre-trends in the event study regressions in Section 5 suggests that conditional parallel trends is a reasonable assumption. Moreover, estimates of the ATE and ATET are not statistically different from zero, as shown in Appendix 8.10.

The empirical design is used not only to address potential concerns around selection bias but also to account for the effects of heterogeneity. As mentioned, CATEs for all firms are recovered. The IF estimator appropriately weights the individual heterogeneous effects when constructing the estimate of the ATE. In addition, the knowledge of CATEs allows for the study the effects of counterfactual policy targeting, which is undertaken in Section 7. Counterfactual effects are also identified, so long as unconfoundedness and overlap holds (Farrell, Liang, and Misra 2021a). If unconfoundedness does not hold, the estimator is still valid and recovers the predictive effects from alternative policy schemes. This is still useful for policy analysis and diagnostics.

6.2. Modelling Framework

Let $\mathcal{F}$ denote the realized information for firms by the end of 2019. Let $h = t - 2020$, where t is the year. Define $\Delta y_i^h = y_i^h - y_i^{-1}$, which is the difference in the outcome variable for some year 2020 or later and its value in 2019. I restrict attention to all covariates realized by the end of 2019, with less than 1% of observations missing: $x_i \subset \mathcal{F}$. I further consider an expanded list of covariates by relaxing the tolerance for missing observations to 10%. Binary treatment, z_i, is defined to equal 1 if a firm's cash bonds were eligible for direct purchase by the Fed CCFs at the announcement date.

All together this gives the following potential outcomes model:

$$\Delta y_i^h = \alpha(x_i) + \beta(x_i)z_i + e_i \tag{3}$$

Note that this is a linear combination of a non-parametric intercept term, $\alpha(x_i)$, and the interaction between a non-parametric slope term, $\beta(x_i)$, and a binary variable, z_i.

Let $Y^h(z)$ be the potential outcome at time h where Z denotes the treatment status:

$$\mathbb{E}[\Delta Y^h | X = x, Z = z] = \mathbb{E}[\Delta Y^h(z) | X = x, Z = z] = \mathbb{E}[\Delta Y^h(z) | X = x] = \alpha(x) + \beta(x)z$$

where the first equality follows from the consistency assumption (the potential outcome is consistent with the treatment assignment) and the second equality follows from the unconfoundedness and overlap assumptions (these are discussed further in Section 6.3).

Taking the difference in the differences in the outcome variables yields:

$$\mathbb{E}[\Delta Y^h(1) - \Delta Y^h(0) | X = x] = \beta(x)$$

Hence, the CATE is given by $\beta(x)$ and ATE, incorporating in heterogeneity, is given by:

$$\mu = \mathbb{E}[\beta(x)] \tag{4}$$

Given the DiD setting, the assumption of unconfoundedness can be relaxed to the weaker assumptions of no anticipation and parallel trends, conditional on pre-treatment covariates.[20] This would identify the ATET, as is the case in Sections 4 and 5 (Cher-

[20]In effect, these assumptions require that, conditional on pre-treatment covariates, firms do not anticipate the treatment (CCFs) in 2019 and absent the treatment, comparable firms' dynamics would have evolved similarly.

nozhukov et al. 2024).[21] [22] The general lack of pre-trends observed in the event study regressions in Section 5 suggests that the parallel trends assumption can be justified.

I also estimate the quantity, $\mathbb{E}[\alpha(x)]$, and refer to it as the base effect. Plotting the evolution of the base effect gives insight into the dynamics for outcome variables for ineligible firms, as well as the potential outcome for eligible firms absent treatment.

Let the parameter vector be given by $\theta = (\alpha, \beta)$, then the general expression for the influence function estimator follows from Farrell, Liang, and Misra (2021b):

$$\psi(\Delta y_i^h, z_i, x_i, \theta(x_i)) = H(x_i, \theta(x_i)) - (\nabla_\theta H)(\mathbb{E}[l_{\theta\theta}|X = x]^{-1} l_\theta) \tag{5}$$

where l the loss function, $l_\theta = \frac{\partial}{\partial\theta} l$ is the score function, and $l_{\theta\theta} = \frac{\partial^2}{\partial\theta\partial\theta'} l$ is the Hessian.

Given a mean squared error loss function, we can express l as:

$$l(\Delta y^h, z, \theta(x)) = l(\Delta y^h, z, \alpha(x), \beta(x)) = \frac{1}{2}(\Delta y^h - \alpha(x) - \beta(x)z)^2$$

Consequently, the expression for the score is:

$$l_\theta = -\begin{pmatrix} 1 \\ z \end{pmatrix} (\Delta y^h - \alpha(x) - \beta(x)z)$$

[21] The ATET is given by the difference between the difference in the outcome variables for treated and untreated firms, averaging over the entire sample. This is expressed as:

$$\begin{aligned} \text{ATET} &= \mathbb{E}[\mathbb{E}[\Delta Y^h(1)|Z = 1, X] - \mathbb{E}[\Delta Y^h(0)|Z = 0, X]] \\ &= \mathbb{E}[\mathbb{E}[\alpha(x) + \beta(x)|Z = 1, X] - \mathbb{E}[\alpha(x)|Z = 0, X]] \\ &= \mathbb{E}[\alpha(x) + \beta(x)|Z = 1] - \mathbb{E}[\alpha(x)|Z = 0] \end{aligned}$$

Hence, the ATET is equal to the average of the CATEs among treated firms if the average of the potential outcome absent treatment is the same between treated and untreated firms. Section 8.5 derives the IF estimator for the ATET. Section 8.10 compares the ATE and ATET estimates for the benchmark model across all outcome variables, showing that the two are similar with no statistically significant difference in any instance.

[22] Another motivation of using a two-step semi-parametric DiD estimator is that adding controls to the linear models in Sections 4 and 5 may not recover causal effects without strong restrictions functional form and heterogeneity (Caetano et al. 2024).

And likewise, for the Hessian:

$$l_{\theta\theta} = \begin{pmatrix} 1 & z \\ z & z^2 \end{pmatrix}$$

Let $\Lambda(x) = \mathbb{E}[l_{\theta\theta}|X = x]$. Hence,

$$\Lambda(x) = \begin{pmatrix} 1 & p(x) \\ p(x) & p(x) \end{pmatrix} \tag{6}$$

where,

$$p(x) \equiv \Pr(z|X = x) \tag{7}$$

is the propensity score, or the probability of a firm being treated given its features.

The derivation for the ATE involves setting $H(x, \theta(x)) = \beta(x)$ and is shown in Appendix 8.4. The resultant estimator is analogous to the doubly-robust DiD estimator of Sant'Anna and Zhao (2020) in a non-ML setting and the DML DiD estimator of Chang (2020) for partially linear models.[23] The setup for all three models is the basic 2×2, or $N \times 2$, difference in differences model with 2 units, which are either treated or control, or N units split into these groups, and 2 time periods, pre- and post- treatment. All three estimators have the doubly-robust property: they are consistent estimators of the average treatment effect if either the potential outcome model given by Equation 3 or the propensity scores, Equation 7, are correctly estimated, but not necessarily both.

Consequently, in the results presented here, the dynamic (heterogeneous) base and treatment effects are computed by re-running the two-step semi-parameteric model period by period over the treatment horizon. Alternatively, this can be extended to estimating the dynamic effects simultaneously in a panel version of two-step semi-parametric estimators, as in Chronopoulos et al. (2023). Identification is similar in both cases (Miller 2023).

6.3. Discussion on Identification

As mentioned, Farrell, Liang, and Misra (2021a) provide the theoretical justification for using deep nets in the first step of two step estimation when inference is con-

[23]This corresponds to case where $\beta(x) = \beta$ is homogeneous in Equation 3.

ducted on the second step using an influence function estimator, as in Equation 5. In addition, they mention two requirements the estimation of a potential outcomes model, as in Equation 3, need to satisfy in order to identify causal parameters of interest. These are unconfoundedness (i.e. selection on observables) and overlap.[24]

6.3.1. Unconfoundedness

An advantage of using deep nets is that it allows for the consideration of a high-dimensional feature space along with arbitrary interactions and transformations among potential covariates. The list of firm characteristics used in estimation is given by Tables A2 and A3. In addition, indicator variables for industry classification are used. The histories of data used range from 1 year to 10 years. To accommodate such a high-dimensional feature space, deep architectures are used for the neural networks, as shown in Tables A4 and A5. Consequently, the claim is that any unobserved variable correlated with the treatment, and potentially biasing the results, is likely to be spanned by the high-dimensional feature space, the transformation of features, and their interactions. As such, the selection on observables assumption is likely satisfied.

As previously noted, given the DiD setup, the assumption of unconfoundedness can be relaxed to the weaker assumptions of conditional no anticipation and parallel trends. This requires that, conditional on pre-treatment covariates, comparable firms did not anticipate the CCF interventions in 2019, which is reasonable given the unprecedented nature of the pandemic, and that treated firms would have had similar dynamics to their untreated counterparts, absent intervention. The general lack of pre-trends observed in the event study regressions in Section 5 suggests that the parallel trends assumption can be justified. The cost of moving to these weaker assumptions is that the model identifies the ATET rather than the ATE (Chernozhukov et al. 2024). Section 5 suggests that the parallel trends assumption can be justified. Section 8.10 compares the ATE and ATET estimates, finding statistically negligible differences.

[24] The assumption of consistency is also required to ensure that observed outcomes correspond to the assigned treatment, which is also assumed here.

6.3.2. Overlap

The overlap condition is satisfied if propensity scores, given by Equation 7 are bounded away from zero and one. Figures 1, 2, and 3 help support the argument that the overlap condition is satisfied. Graphically, both the data on firm characteristics and CDS spreads shows a significant overlap in distributions between eligible and ineligible firms.

Indeed, the CCF's reliance on ratings to determine eligibility is critical to the identification strategy. As argued by several papers, ratings lag and can be predicted by fundamental data (Altman and Rijken 2004), given ratings agencies' desire for ratings stability. In addition, there is some evidence of a loosening of ratings standards heading into the pandemic. Çelik, Demirtaş, and Isaksson (2020) document that within-rating leverage ratios increased by 2019, as the number of BBB-rated firms increased. Additionally, downgrade frequency declined relative to upgrades, with BB+ rated issuers having the highest probability of a 1-notch upgrade within a year and BBB- rated issuers having the lowest probability of a 1-notch downgrade. Consistent with this, Altman (2020) finds that based on 2019 data, 34% of BBB-rated firms can be classified as HY based largely on fundamental characteristics based on the Altman Z-score.

In the same vein, CDS spreads lead and predict future ratings changes CDS spreads (Lee, Naranjo, and Velioglu 2018; Lee, Naranjo, and Sirmans 2021). Not only does Figure 3 show a significant overlap between eligible and ineligible firms' CDS spread distribution, the support of the former lies almost entirely within the support of the latter. This suggests had the eligibility criteria been determined by CDS spreads rather than credit ratings, many eligible firms would have been deemed ineligible, and vice-versa.

In summary, there is rich overlap in the feature space across eligible and ineligible firms. This suggests that the overlap condition is satisfied and allows for the IF estimator given by Equation 5 to be well-defined.

6.4. Estimation Procedure

I obtain features for the deep nets from the Financial Ratios Suite by WRDS. Quarterly variables with less than 1% missing observations are used with histories going back 1, 5, and 10 years. Table A2 in the Appendix reports the list of 37 features where less than 1% of observations are missing over a 10 year history from 2010Q1 to 2019Q4.

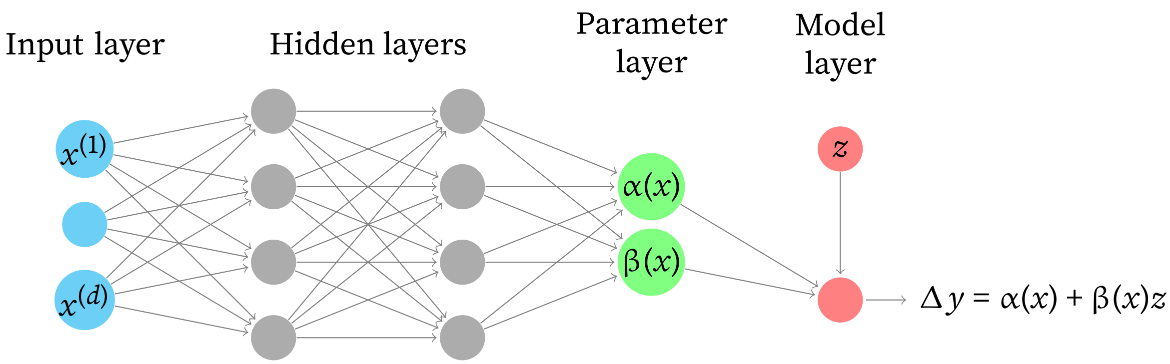


Figure 8. Deep Net Architecture for the Potential Outcomes Model

The figure represents the deep net architecture for estimating the parameters in the potential outcomes model given by Equation 3. Specific values for the number of inputs and hidden layer architecture are reported in Table A4 for models with features with less than 1% missing observations and Table A5 for models with features with less than 10% missing observations.

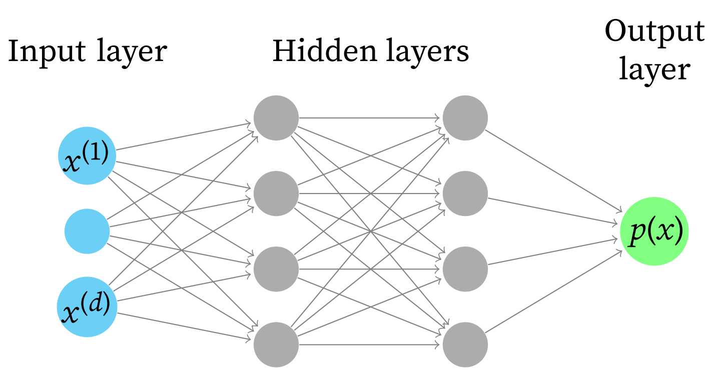


Figure 9. Deep Net Architecture for Propensity Scores

The figure represents the deep net architecture for estimating the propensity scores given by Equation 7. Specific values for the number of inputs and hidden layer architecture are reported in Table A4 for models with features with less than 1% missing observations and Table A5 for models with features with less than 10% missing observations.

As a robustness check, 23 additional features are added by increasing the tolerance of missing observations to 10%; these are reported in Table A3. Missing information is replaced with the quarter-industry median and dummy variables are added to track missing data. Additionally, dummy variables for two-digit NAICS industry codes are used.

Figure 8 illustrates the architecture used to estimate the parameters for Equation 3. Table A4 reports specific values for the number of inputs and the hidden layer architectures associated with the models with covariate histories of 1, 5, and 10 years, respectively. Propensity scores are estimated in a similar fashion and with the same architecture, as seen in Figure 9. The deep net models for the parameters for Equation 3 use rectified linear (ReLU) activation functions within the hidden layers. A linear output layer combines the parameter estimates and treatment indicator to get an estimated outcome. Then, a mean-squared error loss function is applied to the estimated and actual outcomes. The deep net models for propensity scores use hyperbolic tangent (tanh) activation functions within the hidden layers with a sigmoid output layer and a binary cross-entropy loss function. This is done to have propensity scores bounded within zero and one so that the IF estimator is well-defined.

The procedure to estimate any parameter of interest requires three folds of cross-fitting. Cross-fitting involves estimating deep nets on one set of data and evaluating it on another. This is done to prevent over-fitting and to produce unbiased estimators (Chernozhukov et al. 2018). The dataset is split into three random samples of equal size. A deep net is trained on each sample to produce models for the parameters in Equation 3. Separately, deep nets are trained to produce propensity scores. Finally, the influence function is computed by evaluating data from a third sample on models for the CATEs and propensity scores each trained on different samples.[25] Given the cross-fit procedure, dropout regularization is used in training the deep nets to reduce overfitting and so, increase efficiency.[26]

To further improve efficiency, I run multiple cross-fit iterations. I take the median of estimators computed across M cross-fit partitions and its associated variance:[27]

$$\tilde{\mu}_0 = \text{Median}\left((\mu_{0,m})_{m\in[M]}\right)$$

[25]Sample code demonstrating the ability of the estimator to recover parameters in simulated data can be found here: https://github.com/rmmomin/causal-ml-auto-inference.

[26]The reported results are from models trained with a dropout rate of 20% but similar results are obtained by using a dropout rate equal to 30%, 40%, or 50%.

[27]See https://docs.doubleml.org/stable/guide/resampling.html.

$$\hat{\sigma}^2 = \sqrt{\text{Median}\left(\hat{\sigma}_m^2 + (\tilde{\mu}_{0,m} - \tilde{\mu}_0)^2\right)_{m\in[M]}}$$

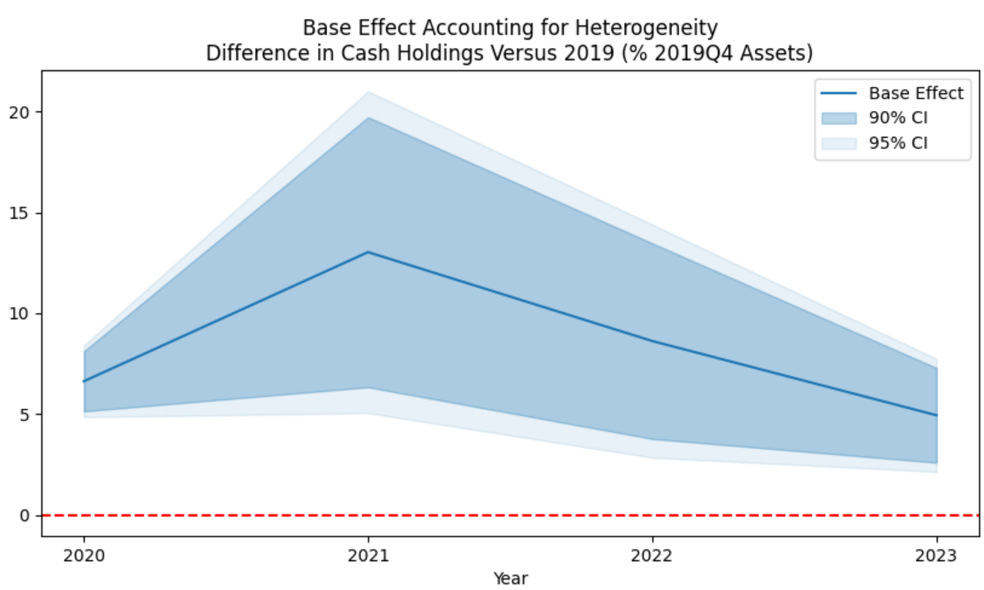
where $\tilde{\mu}_0$ is the parameter of interest, $\tilde{\mu}_{0,m}$ is the parameter estimate corresponding to partition m of cross-fitting, and $\hat{\sigma}^2$ is the variance. The asymptotic standard error is given by $\hat{\sigma}^2/\sqrt{N}$, where N is the number of observations. In the reported results, 10 cross-fit partitions are generated for each deep net estimation.

Additionally, as mentioned in Section 6.2, the model is estimated period by period over the treatment horizon. An extension to the framework would be to estimate these effects simultaneously in a panel setting of the two-step semi-parameteric model, as in Chronopoulos et al. (2023).

6.5. Base Effects

To estimate the base effect, set $H(x, \theta(x)) = \alpha(x)$ in Equation 5. Appendix 8.3 provides details on the derivation of the estimator.

Figure 10. Large Base Effect with Increase in Cash Holdings

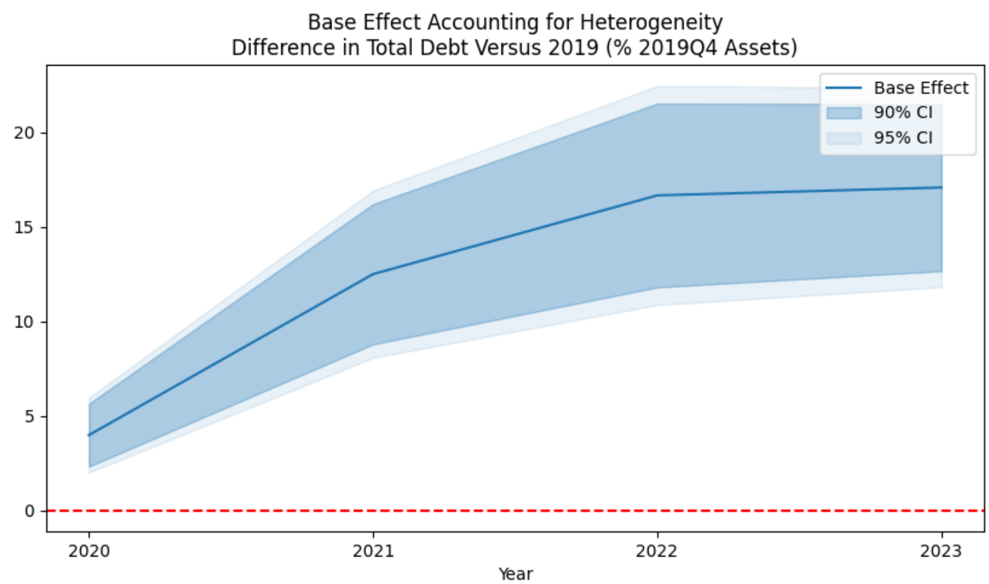


The figure plots the base effects for the change in cash holdings, as a percent of 2019Q4 assets. The model above uses 10 years of feature history and 1% tolerance for missing observations. Details on its architecture is reported in Table A4 in the Appendix. Table A6 reports results across all model specifications. Consistent with the DiD regressions reported in Table 3, a large base effect is identified for 2020 onwards.

Figure 10 plots the base effects for the change in cash holdings, as a percent of

2019Q4 assets, using a model with 10 years of feature history and 1% tolerance for missing observations. Details on its architecture is reported in Table A4 in the Appendix. Consistent with the DiD regressions reported in Table 3, a large base effect is identified for 2020 onwards. However, the dynamics of cash holdings suggests that these peaked for all firms in 2021 and then began to fall, perhaps as uncertainty from the pandemic and so, demand for precautionary liquidity began to fade. The finding of large, positive base effects, as well as its dynamics, are largely consistent across different model specifications, as shown in Table A6.

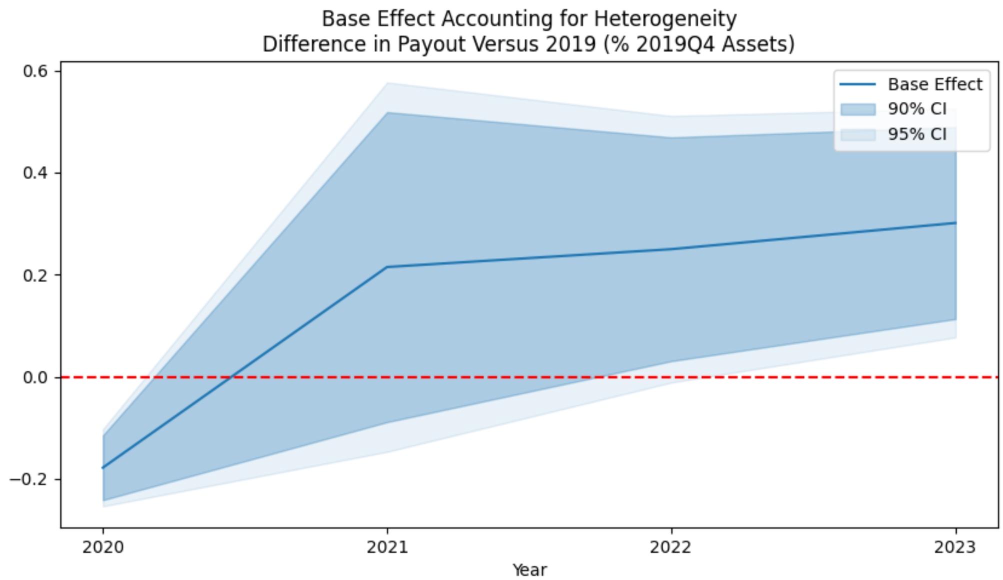
Figure 11. Large Base Effect with Increase in Total Debt



The figure plots the base effects for the change in debt, as a percent of 2019Q4 assets. The model above uses 10 years of feature history and 1% tolerance for missing observations. Details on its architecture is reported in Table A4 in the Appendix. Table A7 reports results across all model specifications. Consistent with the DiD regressions reported in Table 3, a large base effect is identified for 2020 onwards.

Figure 11 plots the base effects for the change in debt, as a percent of 2019Q4 assets, using a model with 10 years of feature history and 1% tolerance for missing observations. Details on its architecture is reported in Table A4 in the Appendix. Consistent with the DiD regressions reported in Table 3, a large base effect is identified for 2020 onwards. In contrast to cash holdings, the base effects for leverage has remain elevated. This suggests that ineligible firms did not deleverage as their cash reserves fell. The finding of large, positive base effects, as well as an increasing trend, are largely consistent across different model specifications, as shown in Table A7.

Figure 12. Payout Base Effect Initially Negative Then Increases

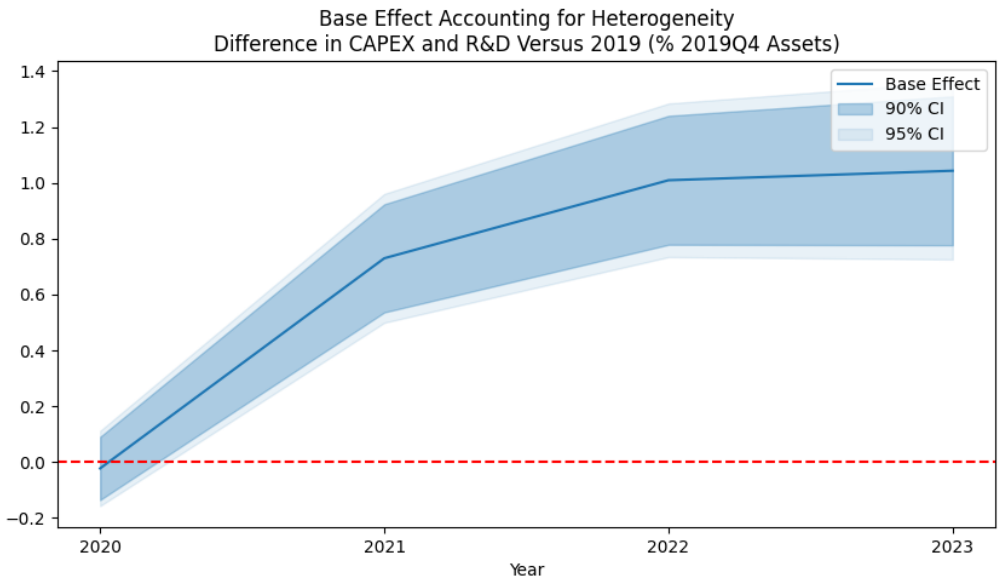


The figure plots the base effects for the difference in annual payouts versus 2019, scaled by 2019Q4 assets. The model above uses 10 years of feature history and 1% tolerance for missing observations. Details on its architecture is reported in Table A4 in the Appendix. Table A8 reports results across all model specifications. In contrast to the null results picked up by the DiD regressions reported in Table 4, the base effect here changes over the observation period, initially negative in 2020 and then increasing.

Figure 12 plots the base effects for the difference in annual payouts versus 2019, scaled by 2019Q4 assets, using a model with 10 years of feature history and 1% tolerance for missing observations. Details on its architecture is reported in Table A4 in the Appendix. In contrast to the null results picked up by the DiD regressions reported in Table 4, the base effect here changes over the observation period, initially negative in 2020 and then increasing. This is consistent with firms initially reducing payouts to preserve liquidity and then resuming them as conditions improved. These results are largely consistent across different model specifications, as shown in Table A8.

Figure 13 plots the base effects for the difference in annual investment versus 2019, scaled by 2019Q4 assets, using a model with 10 years of feature history and 1% tolerance for missing observations. Details on its architecture is reported in Table A4 in the Appendix. Consistent with the positive effect found for the post period in the DiD regressions reported in Table 4, positive effects are generally found over the treatment period. However, these results are not robust to an alternative proxy for in-

Figure 13. Investment Base Effect Null then Increasing



The figure plots the base effects for the difference in annual investment versus 2019, scaled by 2019Q4 assets. The model above uses 10 years of feature history and 1% tolerance for missing observations. Details on its architecture is reported in Table A4 in the Appendix. Table A9 reports results across all model specifications. Consistent with the positive effect found for the post period in the DiD regressions reported in Table 4, positive effects are generally found over the treatment period. Figure A1 plots the base effects corresponding to proxying investment with the annual change in gross property, plant, and equipment. In contrast to here, negative base effects are estimated for 2020 and 2021, which then become null for 2022 and 2023.

vestment. Figure A1 plots the base effects corresponding to proxying investment with the annual change in gross property, plant, and equipment. In contrast to here, negative base effects are estimated for 2020 and 2021, which then become null for 2022 and 2023. These results are largely consistent across different model specification, as shown in Table A20.

6.6. ATE with Heterogeneity

To estimate the ATE, set $H(x, \theta(x)) = \beta(x)$ in Equation 5. For each cross-fit fold, the estimator becomes:

(8)

$$\hat{\mu}_s = \frac{1}{N}\sum \psi(\Delta y^h, z, x, \theta) = \frac{1}{N}\sum\left[\beta(x) + \frac{z(\Delta y^h - \alpha(x) - \beta(x)z)}{p(x)} - \frac{(1-z)(\Delta y^h - \alpha(x))}{1-p(x)}\right]$$

Appendix 8.4 provides details on the derivation. The final estimate of Equation 4 is then obtained by averaging the estimates from each fold:

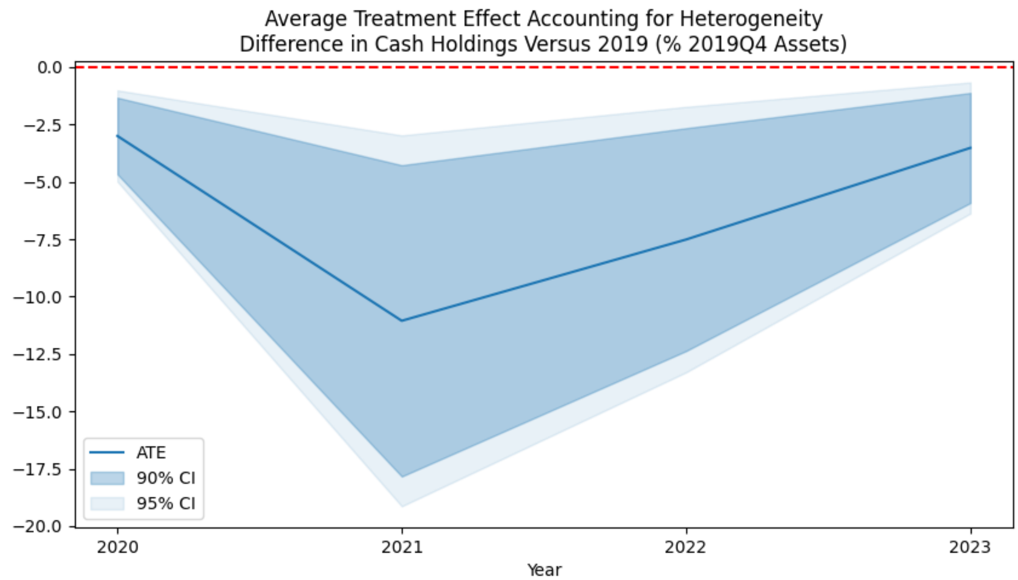
(9) $$\hat{\mu} = \frac{1}{3}\sum \hat{\mu}_s$$

While the ATE is identified under the assumption of unconfoundedness, this assumption can be relaxed to conditional no anticipation and parallel trends to identify the ATET instead. The general lack of pre-trends observed in the event study regressions in Section 5 suggests that the parallel trends assumption can be justified. Section 8.10 compares the ATE and ATET estimates, finding statistically negligible differences.

Figure 14 plots the average treatment effects for the change in cash holdings over different horizons, accounting for heterogeneity, using a model with 10 years of feature history and 1% tolerance for missing observations. Details on its architecture is reported in Table A4 in the Appendix. Large negative treatment effects are estimated over the entire horizon, consistent with static (homogeneous) treatment effects estimated by the DiD regressions and the dynamic (homogeneous) treatment effects estimated by the event study regressions. These are summarized in Table 5. Table A10 reports the results across different model specifications, showing that the estimates are robust.

Figure 15 plots the average treatment effects for change in total debt over different horizons, accounting for heterogeneity, using a model with 10 years of feature history and 1% tolerance for missing observations. Details on its architecture is reported in Table A4 in the Appendix. While an initial null effect is picked up for 2020, this becomes negative and large for the remainder of the horizon. This suggests that both eligible and ineligible firms initially increased leverage in 2020, but subsequently, eligible firms began deleveraging, while ineligible firms did not. Table 6 compares the treatment effect estimates across different models and suggests that the results are broadly in line. These results are robust to other model specifications, as shown

Figure 14. Cash ATE With Heterogeneity Shows Large Negative Effect



The figure plots the average treatment effects for the change in cash holdings, as a percent of 2019Q4 assets. This corresponds to the estimator reported in Equation 9. The model above uses 10 years of feature history and 1% tolerance for missing observations. Details on its architecture is reported in Table A4 in the Appendix. Table A10 reports results across all model specifications. Large negative treatment effects are estimated over the entire horizon. Table 5 compares the treatment effect estimates across different models and suggests that results are broadly in line.

in Table A11.

Figure 16 plots the average treatment effects for the difference in annual payouts versus 2019, as a percent of 2019Q4 assets. The estimates are from the model using 10 years of feature history and 1% tolerance for missing observations; complete details on its architecture is reported in Table A4 in the Appendix. The payout treatment effect is initially positive in 2020, then null for 2021, and again positive for 2022 and 2023. Table 7 compares the treatment effect estimates across different models. Interestingly, the point estimates for the dynamic (heterogeneous) treatment effects are smaller than both the static (homogeneous) treatment effects and the dynamic (heterogeneous) treatment effects, but as are the standard errors. In general, this could suggest that the selection bias for the regression models results in an upward bias to treatment effects, consistent with IG firms being more resilient and maintaining payouts. These results are consistent across models using 5 and 10 years of feature history, as seen in Table A12.

Table 5. Cash Treatment Effect Comparison

Treatment Effect Estimates				
Cash (% 2019Q4 Assets)				
Year	Static (Homogeneous)	Dynamic (Heterogeneous) (1)	Dynamic (Homogeneous) (2)	Difference (1)-(2)
2020		-3.00	-3.82	0.82
		(1.01)	(0.79)	
2021		-11.05	-9.52	-1.53
		(4.12)	(2.42)	
2022		-7.51	-6.92	-0.59
		(2.95)	(2.27)	
2023		-3.52	-4.01	0.48
		(1.46)	(1.09)	
Eligible × Post 2020	-7.46			
	(2.05)			

Standard-errors in parentheses

The table reports the treatment effect estimates for cash, as a percent of 2019Q4, across the three models examined in this paper. The static (homogoeneous) treatment effect comes from the DiD regressions reported in Table 3, while the dynamic (homogeneous) treatment effects correspond to the event study regressions with two-way fixed effects, reported in Table A1 and shown in Figure 4. The different treatment effect estimates are broadly in line.

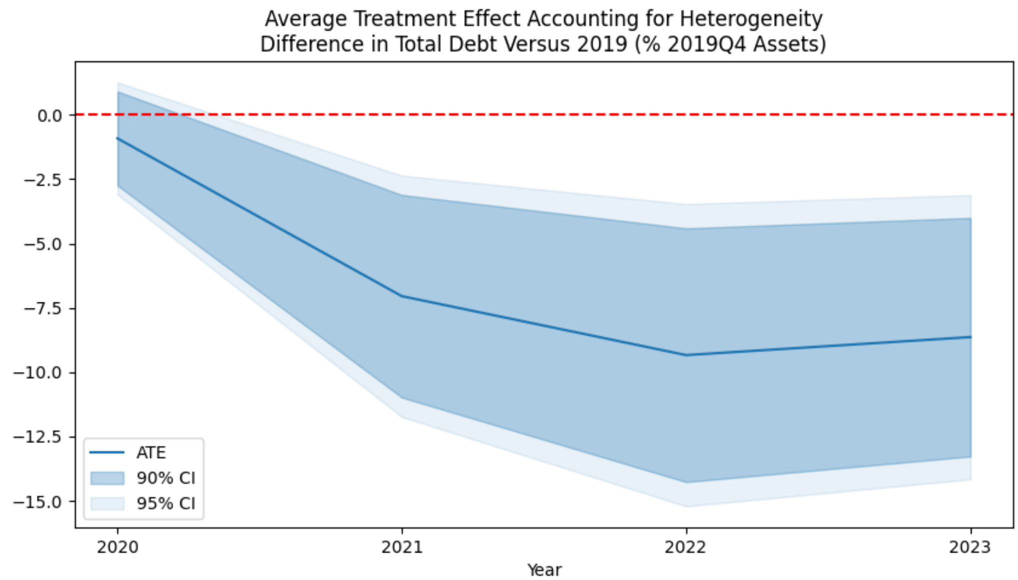
Table 6. Debt Treatment Effect Comparison

Treatment Effect Estimates				
Total Debt (% 2019Q4 Assets)				
Year	Static (Homogeneous)	Dynamic (Heterogeneous) (1)	Dynamic (Homogeneous) (2)	Difference (1)-(2)
2020		-0.91	-1.66	0.75
		(1.11)	(0.65)	
2021		-7.04	-5.95	-1.09
		(2.39)	(2.30)	
2022		-9.34	-9.08	-0.25
		(3.00)	(2.59)	
2023		-8.64	-8.47	-0.17
		(2.82)	(1.95)	
Eligible × Post 2020	-6.21			
	(2.73)			

Standard-errors in parentheses

The table reports the treatment effect estimates for debt, as a percent of 2019Q4, across the three models examined in this paper. The static (homogoeneous) treatment effect comes from the DiD regressions reported in Table 3, while the dynamic (homogeneous) treatment effects correspond to the event study regressions with two-way fixed effects, reported in Table A1 and shown in Figure 5. The different treatment effect estimates are broadly in line.

Figure 15. Debt ATE With Heterogeneity Negative After 2020

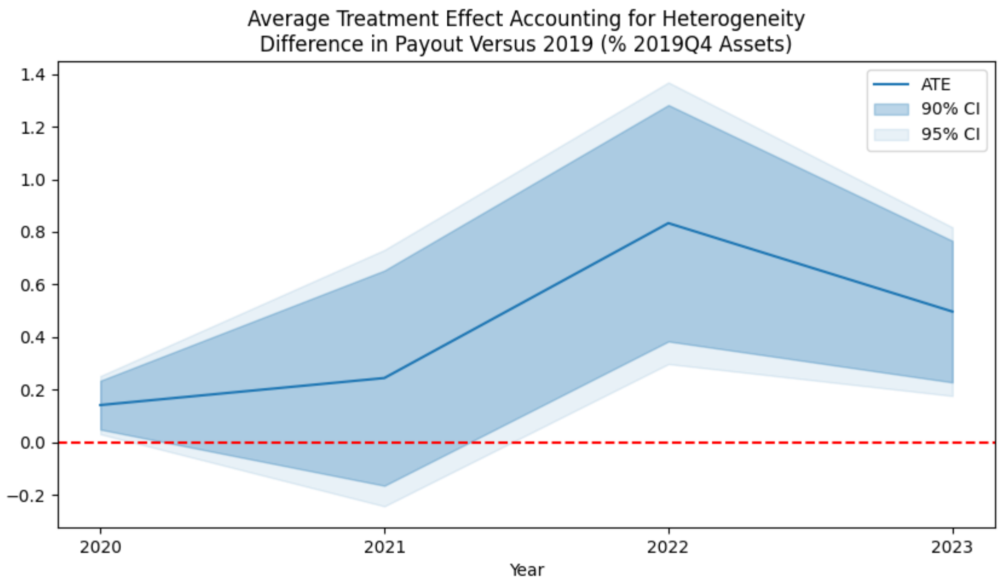


The figure plots the average treatment effects for change in total debt, as a percent of 2019Q4 assets. This corresponds to the estimator reported in Equation 9. The model above uses 10 years of feature history and 1% tolerance for missing observations. Details on its architecture is reported in Table A4 in the Appendix. Table A11 reports results across all model specifications. While an initial null effect is picked up for 2020, this becomes negative and large for the remainder of the horizon. Table 6 compares the treatment effect estimates across different models and suggests that the results are broadly in line.

Figure 17 plots the average treatment effects for the difference in annual investment versus 2019, scaled by 2019Q4 assets, using a model with 10 years of feature history and 1% tolerance for missing observations. Details on its architecture is reported in Table A4 in the Appendix. Table A13 reports results across all model specifications, showing that null-to-negative effects are estimated in every instance. Table 8 compares the different treatment effects estimated by each model, showing that incorporating high-dimensional controls and heterogeneity actually increases point estimates for the dynamic effects. Figure A2 and Table A21 show the treatment effect dynamics when proxying investment by the annual change in gross property, plant, and equipment. For this proxy, null effects are estimated for every specification.

The dynamic (heterogeneous) treatment effects estimated in this section suggest that eligible firms increased cash holdings to a lesser extent than ineligible firms and also took on less leverage, as well. These are consistent with both the static (homogeneous) treatment effects estimated by the DiD panel regression in Section 4 and

Figure 16. Payout ATE Generally Positive



The figure plots the average treatment effects for the difference in annual payouts versus 2019, as a percent of 2019Q4 assets. This corresponds to the estimator reported in Equation 9. The model above uses 10 years of feature history and 1% tolerance for missing observations. Details on its architecture is reported in Table A4 in the Appendix. Table A12 reports results across all model specifications. The payout treatment effect is initially positive in 2020, then null for 2021, and again positive for 2022 and 2023. Table 7 compares the treatment effect estimates across different models. While the treatment effects are comparable, the standard errors for the dynamic (heterogeneous) treatment effects are smaller.

the dynamic (homogeneous) treatment effects estimated by the event study regressions with two-way fixed effects in Section 5. A comparison of the dynamic treatment effects across the two designs reveals relatively similar point estimates with no uniform direction in the difference (positive or negative).

In contrast, the dynamic (heterogeneous) treatment effects estimated in this section are generally smaller than those estimated in Section 5, with the exception of investment. This could be due to the effects of either selection bias, which is accounted for by high-dimensional controls, or the effects of heterogeneity. For payouts, the lower point estimates are also accompanied by smaller standard errors and reinforce the result that eligible firms had relatively higher levels of payouts. Even though the dynamic (heterogeneous) treatment effects for investment are larger, these are still null or negative, similar to the dynamic (homogeneous) treatment effect found in 2020 in Section 5. An alternative proxy for investment reinforces this conclusion.

Table 7. Payout Treatment Effect Comparison

Treatment Effect Estimates				
Payout (% 2019Q4 Assets)				
Year	Static (Homogeneous)	Dynamic (Heterogeneous) (1)	Dynamic (Homogeneous) (2)	Difference (1)-(2)
2020		0.14	0.54	-0.40
		(0.06)	(0.34)	
2021		0.24	0.65	-0.41
		(0.25)	(0.36)	
2022		0.83	0.99	-0.16
		(0.27)	(0.37)	
2023		0.50	0.86	-0.36
		(0.16)	(0.39)	
Eligible ×	1.16			
Post 2020	(0.23)			

Standard-errors in parentheses

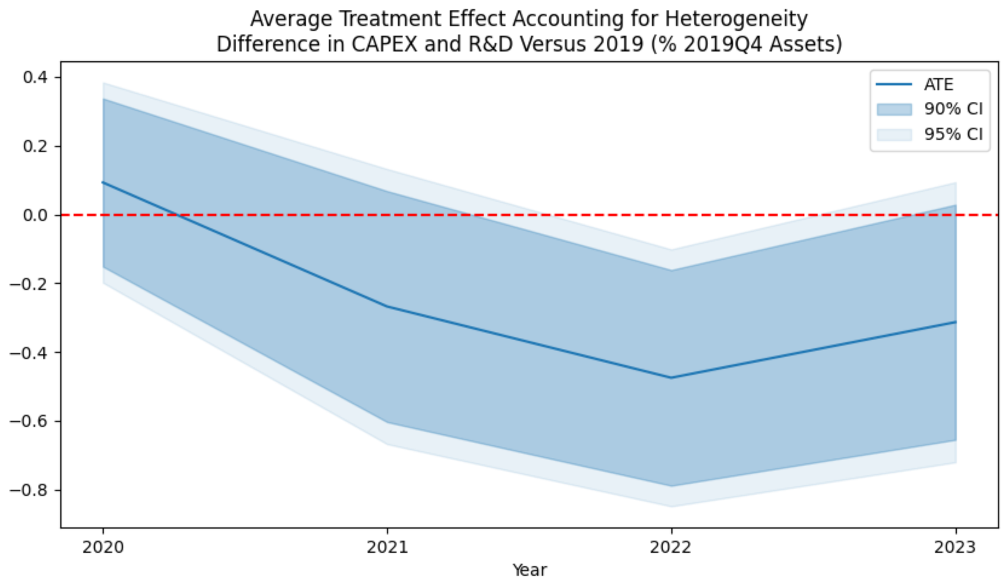
The table reports the treatment effect estimates for payout, as a percent of 2019Q4, across the three models examined in this paper. The static (homogeneous) treatment effect comes from the DiD regressions reported in Table 4, while the dynamic (homogeneous) treatment effects correspond to the event study regressions with two-way fixed effects, reported in Table A1 and shown in Figure 6. The point estimates for the dynamic (heterogeneous) treatment effects are smaller than both the static (homogeneous) treatment effects and the dynamic (heterogeneous) treatment effects, but as are the standard errors. The systematic negative difference in the point estimates can be attributed to either selection bias in the regression models which is better controlled for by covariates or heterogeneous effects.

6.6.1. Discussion on Selection Bias for Investment

Selection bias for investment may be positive or negative. Given that the treatment was assigned on the basis of IG status, positive selection bias may arise if IG rated firms have more investment opportunities, while negative selection bias may arise if managers of IG firms are more cautious/disciplined about investment, as examples. The lack of pre-trends in the event study regressions in Section 5 suggests that there is no systematic difference in relative investment in the pre-treatment period. The causal ML estimator with a high-dimensional control structure presented in this section should provide additional safeguards against this by spanning any potential omitted variable. To the extent that the effect of such omitted variables are not controlled, then a positive selection bias due to investment opportunities would suggest that the estimated treatment effect is biased upwards. Given that negative or null effects are found for both investment proxies, this should strengthen the argument that the Fed CCFs did not meet its objectives of improving real outcomes.

However, if negative selection bias is present, then the results shown here underes-

Figure 17. Investment ATE With Heterogeneity Consistent With Previous Estimates



The figure plots the average treatment effects for the difference in annual investment versus 2019, scaled by 2019Q4 assets. This corresponds to the estimator reported in Equation 9. The model above uses 10 years of feature history and 1% tolerance for missing observations. Details on its architecture is reported in Table A4 in the Appendix. Table A13 reports results across all model specifications, null-to-negative treatment effects are estimated, particularly for the models with longer covariate histories. Table 8 compares the different treatment effects estimated by each model, showing that incorporating high-dimensional controls and heterogeneity increases the point estimates for the dynamic effects. Figure A2 and Table A21 show the treatment effect dynamics when proxying investment by the annual change in gross property, plant, and equipment. For this proxy, null effects are estimated for every specification.

timate the true effect and do not necessarily provide evidence that Fed CCF eligibility failed to spur investment. This would require that differencing out the trend in the outcome variable for ineligible firms as well as controlling for a high dimensional set of variables fails to properly address (negative) selection effects. More precise identification strategies exploiting plausibly exogenous variation may better assuage fears around selection bias but would come at the cost of external validity. Uniquely, the casual ML approach presented here allows for the estimation of heterogeneous effects and permits counterfactual analysis.

Table 8. Investment Treatment Effect Comparison

Year	Static (Homogeneous)	Dynamic (Heterogeneous) (1)	Dynamic (Homogeneous) (2)	Difference (1)-(2)
Treatment Effect Estimates				
CAPEX and R&D (% 2019Q4 Assets)				
2020		0.09	-0.49	0.59
		(0.15)	(0.58)	
2021		-0.27	-1.88	1.61
		(0.20)	(0.80)	
2022		-0.48	-0.99	0.51
		(0.19)	(0.41)	
2023		-0.31	-0.62	0.31
		(0.21)	(0.29)	
Eligible × Post 2020	-0.90			
	(0.66)			

Standard-errors in parentheses

The table reports the treatment effect estimates for investment, as a percent of 2019Q4, across the three models examined in this paper. The static (homogeneous) treatment effect comes from the DiD regressions reported in Table 4, while the dynamic (homogeneous) treatment effects correspond to the event study regressions with two-way fixed effects, reported in Table A1 and shown in Figure 7. The point estimates for the dynamic (heterogeneous) treatment effects are larger than the dynamic (homogeneous) treatment effects. The systematic positive difference in the point estimates can be attributed to either selection bias in the regression models which is better controlled for by covariates or heterogeneous effects.

7. Counterfactual Targeting

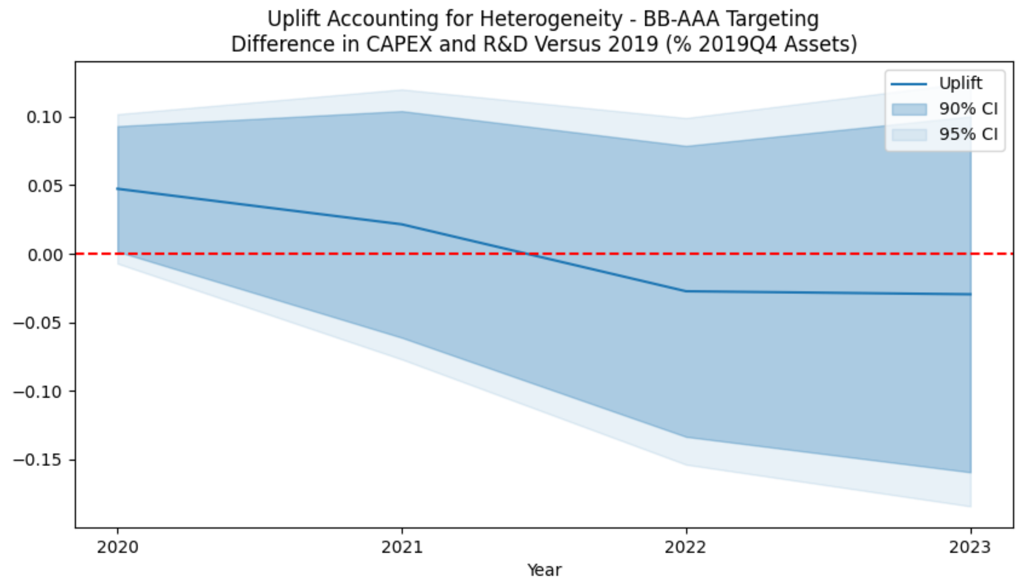
Let z be the realized vector of treatment indicators and z' a vector of counterfactual treatments. To assess the benefits of counterfactual targeting, I focus on the difference in the average of the CATEs between the counterfactual target set and the realized set of targeted firms: $\mathbb{E}[\beta(x)(z' - z)]$. Hence, set $H(x, \theta(x)) = \beta(x)(z' - z)$ in Equation 5. The derivation for the closed-form of the IF estimator is given in Appendix 8.6. It is identical to the ATE IF estimator except for a term $(z' - z)$ multiplying the summands in Equation 8. Although unconfoundedness can be relaxed to conditional parallel trends and no anticipation to identify the ATET instead of the ATE, unconfoundedness is needed here to have a causal interpretation of the counterfactual effect of changing policy targeting. If this fails to hold, the estimator is still valid, but instead identifies a predictive effect, which would still be useful for policy analysis.

A simple framework, such as in Brunnermeier and Krishnamurthy (2020), would suggest that targeting lower-rated firms should result in a stronger decrease in borrowing costs, and so, should stimulate more real activity. This argument is further strengthened by the CFO survey evidence of Campello, Graham, and Harvey (2010) and Barry et al. (2022). As seen in Figure 2, lower-rated firms are less liquid and solvent, suggesting that these firms could be more sensitive to credit conditions. Based on this reasoning, I consider counterfactual policy targeting based on different ratings criteria: BB-AAA and BB-A. The first policy expands eligibility to HY firms, while the second policy does the same while also restricting eligibility to firms that are not too highly rated. I focus on potential relaxations of the eligibility criteria to the BB-rated category, since overlap in characteristics around the BB/BBB IG/HY threshold is likely to be strongest.[28]

Figure 18 plots the expected difference in the average CATEs among targeted firms from changing the set of target firms from BBB-AAA to BB-AAA for the model using 10 years of feature history and 1% tolerance for missing observations. Table A18 in the Appendix shows the estimated effects across different models. Although a positive effect is detected for 2020, this is not robust to other model specifications. In addition, positive effects are not estimated for other years. Figure A3 and Table A22 shows the corresponding results when proxying investment by the annual change in gross property, plant, and equipment. Those results provide stronger evidence that counterfactual targeting may have improved average outcomes among treated firms

[28] See Section 6.3.2 for additional discussion.

Figure 18. Slight Improvement in Investment Outcome from BB-AAA Targeting



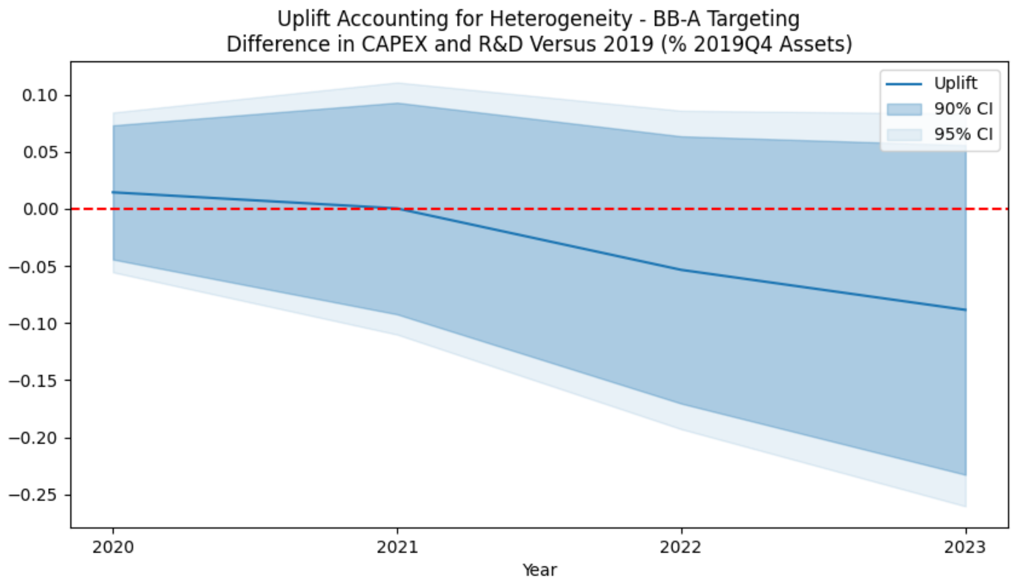
The figure plots the expected difference in the average CATEs among targeted firms in annual investment versus 2019, as a percent of 2019Q4 assets, from expanding the targeted firms from BBB-AAA to BB-AAA for the model using 10 years of feature history and 1% tolerance for missing observations. Table A18 in the Appendix shows the estimated effects across different models. While a positive effect is detected for 2020, this is not robust to other model specifications. Additionally, positive effects are not estimated for other years. Figure A3 and Table A22 shows the corresponding results when proxying investment by the annual change in gross property, plant, and equipment. Those results provide stronger evidence that counterfactual targeting may have improved average outcomes among treated firms in 2020, although significant results are not picked up across all model specifications.

in 2020, although significant results are not picked up across all model specifications.

Similarly, Figure 19 plots the expected difference in the average CATEs among targeted firms from changing the set of target firms from BBB-AAA to BB-A for the model using 10 years of feature history and 1% tolerance for missing observations. As seen, there is no improvement in investment. Table A19 in the Appendix shows that the finding of no improvement in outcomes is generally robust across different models. Figure A4 and Table A23 shows the corresponding results when proxying investment by the annual change in gross property, plant, and equipment. In contrast to here, those results provide some evidence that counterfactual targeting may have improved average outcomes among treated firms in 2020, although significant results are not picked up across all model specifications.

Overall, the results in this section provide weak to inconclusive evidence that ex-

Figure 19. No Improvement in Investment Outcome from BB-A Targeting



The figure plots the expected difference in the average CATEs among targeted firms in annual investment versus 2019, as a percent of 2019Q4 assets, from changing the set of targeted firms from BBB-AAA to BB-A for the model using 10 years of feature history and 1% tolerance for missing observations. As seen, there is no improvement in investment. Table A19 in the Appendix shows that the finding of no improvement in outcomes is generally robust across different models. Figure A4 and Table A23 shows the corresponding results when proxying investment by the annual change in gross property, plant, and equipment. In contrast to here, those results provide some evidence that counterfactual targeting may have improved average outcomes among treated firms in 2020, although significant results are not picked up across all model specifications.

panding the eligibility criteria of firms to include BB-rated firms would have led to an improvement in the average investment outcomes among targeted firms in 2020. The results are not entirely robust across different proxies for investment or model specifications. However, there is no evidence to suggest that outcomes would have been improved for other years.

8. Conclusion

I present a novel two-step semi-parametric difference-in-differences estimator for computing dynamic (heterogeneous) treatment effects that is comparable to an event study design with two-way fixed effects. The structural equation for potential outcomes is the linear combination of a non-parametric intercept term and the interaction of a treatment indicator and a non-parametric slope term. The slope term captures individual level heterogeneity, that is, conditional average treatment effects. Another ingredient for the estimator is an estimation of propensity scores, the probability of a firm being classified as eligible for the CCFs, which is also modeled as a non-parametric function of a high-dimensional set of characteristics. The non-parametric terms are estimated using deep neural networks. Given that the assumptions of unconfoundedness and the overlap condition are satisfied, this allows for the identification of average treatment effects that account for heterogeneity and counterfactual treatment effects from alternative policy targeting. Given the difference-in-differences setup, the assumption of unconfoundedness can be relaxed to weaker assumptions of (conditional) no anticipation and parallel trends, thus identifying the average treatment effect on the treated, instead. Given a general lack of pre-trends in the event study regressions, conditional parallel trends is a justifiable assumption, and estimates of the ATE and ATET from the two-step estimator are not statistically different from zero.

The estimator is applied to study the financial and real effects of the Federal Reserve's Corporate Credit Facilities launched in 2020 amid the COVID-19 pandemic, as well as the effects of counterfactual eligibility criteria. Dynamic (heterogeneous) treatment effects from the novel estimator are comparable to static (homogeneous) treatment effects from a difference-in-differences panel regression and dynamic (homogeneous) treatment effects from an event study design with two-way fixed effects. The results show that while all firms increased leverage and cash holdings as a proportion of 2019 year-end assets, firms eligible for the CCFs increased leverage and cash to a relatively lower extent than ineligible firms. Moreover, eligible firms do not show an increased investment response, which suggests that the CCFs may not have met its objective for producing real effects. This is robust to alternative proxies for investment. In contrast, eligible firms did increase payouts to shareholders. Counterfactual policy targeting loosening the CCFs eligibility criteria to target weaker credits with possibly more binding financially constraints produces weak to inconclusive evidence of improved investment outcomes in 2020, while there is no evidence of improved outcomes found for later periods.

Noting that both in the United States, as well as in Europe, CCFs failed to stimulate investment (De Santis and Zaghini 2021; Grosse-Rueschkamp, Steffen, and Streitz 2019; Todorov 2020), Momin (2025) explores changes to the design of the CCFs to encourage firm investment.

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Appendix

8.1. Event Study Regressions with Two-Way Fixed Effects

Table A1. Dynamic (Homogeneous) Treatment Effects

Dependent Variables:	Cash (% 2019Q4 Assets)	Total Debt (% 2019Q4 Assets)	Dividends and Buybacks (% 2019Q4 Assets)	Capital Expenditures and R&D (% 2019Q4 Assets)
Model:	(1)	(2)	(3)	(4)
Variables				
2011	1.605**	5.950**	-0.8059***	-0.2164
	(0.6316)	(2.460)	(0.2578)	(0.2804)
2012	2.202***	4.198	-0.3333	-0.5236
	(0.5401)	(2.385)	(0.3084)	(0.4549)
2013	1.577**	5.409**	-0.5584	-0.3305
	(0.6211)	(2.446)	(0.3708)	(0.3674)
2014	2.043***	3.310	-0.3442	-0.5331
	(0.3430)	(2.249)	(0.3585)	(0.3729)
2015	-0.8817	2.094	-0.4685	-0.5136
	(1.330)	(2.297)	(0.3482)	(0.4647)
2016	2.130***	2.960*	-0.0752	-0.3801
	(0.0944)	(1.514)	(0.3333)	(0.2178)
2017	0.4773	4.476***	-1.408*	-0.1844**
	(0.7562)	(0.9942)	(0.7130)	(0.0817)
2018	-0.1416	2.766**	0.1208	-0.1778*
	(0.1550)	(1.145)	(0.3248)	(0.0927)
2020	-3.822***	-1.662**	0.5398	-0.4923
	(0.7861)	(0.6474)	(0.3370)	(0.5767)
2021	-9.520***	-5.954**	0.6531*	-1.877**
	(2.418)	(2.304)	(0.3596)	(0.8043)
2022	-6.923***	-9.084***	0.9944**	-0.9863**
	(2.265)	(2.591)	(0.3708)	(0.4143)
2023	-4.005***	-8.471***	0.8602**	-0.6217*
	(1.091)	(1.954)	(0.3895)	(0.2904)
Fixed-effects				
Issuer	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	9,912	9,502	9,641	9,798
R^2	0.44205	0.56682	0.17201	0.39251
Within R^2	0.00827	0.00736	0.00212	0.00116

Clustered (Issuer & Date) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The table reports the coefficients related to the event study regressions presented in Section 5 corresponding to Figures 4, 5, 6, and 7. Negative, sizeable effects are found for cash and total debt over the treatment period. Positive effects are found for payouts, which are statistically significant for 2021, 2022, and 2023. Similarly, negative effects are found for investment, which are statistically significant for 2021, 2022, and 2023.

8.2. Features

Variable	Description
accrual	Accruals/Average Assets
adv_sale	Advertising Expenses/Sales
aftret_eq	After-tax Return on Average Common Equity
aftret_equity	After-tax Return on Total Stockholders Equity
aftret_invcapx	After-tax Return on Invested Capital
at_turn	Asset Turnover
capital_ratio	Capitalization Ratio
cash_debt	Cash Flow/Total Debt
cash_lt	Cash Balance/Total Liabilities
cfm	Cash Flow Margin
de_ratio	Total Debt/Equity
debt_assets	Total Debt (ltq)/Total Assets
debt_at	Total Debt (dlcq+dlttq)/Total Assets
debt_capital	Total Debt/Capital
debt_ebitda	Total Debt/EBITDA
debt_invcap	Long-term Debt/Invested Capital
equity_invcap	Common Equity/Invested Capital
evm	Enterprise Value Multiple
gpm	Gross Profit Margin
gprof	Gross Profit/Total Assets
lt_debt	Long-term Debt/Total Liabilities
lt_ppent	Total Liabilities/Total Tangible Assets
npm	Net Profit Margin
opmad	Operating Profit Margin After Depreciation
opmbd	Operating Profit Margin Before Depreciation
pcf	Price/Cash flow
pe_exi	P/E (Diluted, Excl. EI)
pe_inc	P/E (Diluted, Incl. EI)
pe_op_basic	Price/Operating Earnings (Basic, Excl. EI)
pe_op_dil	Price/Operating Earnings (Diluted, Excl. EI)
ps	Price/Sales
ptpm	Pre-tax Profit Margin
rd_sale	Research and Development/Sales
roa	Return on Assets
roce	Return on Capital Employed
staff_sale	Labor Expenses/Sales
totdebt_invcap	Total Debt/Invested Capital

Table A2. Features with Less than One Percent Missing Observations

Variable	Description
bm	Book/Market
capei	Shillers Cyclically Adjusted P/E Ratio
cash_ratio	Cash Ratio
curr_debt	Current Liabilities/Total Liabilities
curr_ratio	Current Ratio
dltt_be	Long-term Debt/Book Equity
int_debt	Interest/Average Long-term Debt
intcov	After-tax Interest Coverage
intcov_ratio	Interest Coverage Ratio
ocf_lct	Operating CF/Current Liabilities
pay_turn	Payables Turnover
peg_1yrforward	Forward P/E to 1-year Growth (PEG) ratio
pretret_earnat	Pre-tax Return on Total Earning Assets
pretret_noa	Pre-tax return on Net Operating Assets
profit_lct	Profit Before Depreciation/Current Liabilities
ptb	Price/Book
quick_ratio	Quick Ratio (Acid Test)
rect_act	Receivables/Current Assets
rect_turn	Receivables Turnover
roe	Return on Equity
sale_equity	Sales/Stockholders Equity
sale_invcap	Sales/Invested Capital
short_debt	Short-Term Debt/Total Debt

Table A3. Additional Features with Less than Ten Percent Missing Observations

8.3. Base Effect Estimator Derivation

Set $H(x, \theta(x)) = \alpha(x)$. Then, $\nabla_\theta H = \begin{bmatrix} 1 & 0 \end{bmatrix}$.
Compute the inverse of Equation 6:

$$
\begin{aligned}
\Lambda(x)^{-1} &= \frac{1}{p(x)(1-p(x))} \begin{bmatrix} p(x) & -p(x) \\ -p(x) & 1 \end{bmatrix} \\
&= \begin{bmatrix} \frac{1}{1-p(x)} & -\frac{1}{1-p(x)} \\ -\frac{1}{1-p(x)} & \frac{1}{p(x)(1-p(x))} \end{bmatrix}
\end{aligned}
$$

This gives:

$$
\begin{aligned}
(\nabla_\theta H)\Lambda(x)^{-1} &= \begin{bmatrix} 1 & 0 \end{bmatrix} \begin{bmatrix} \frac{1}{1-p(x)} & -\frac{1}{1-p(x)} \\ -\frac{1}{1-p(x)} & \frac{1}{p(x)(1-p(x))} \end{bmatrix} \\
&= \begin{bmatrix} \frac{1}{1-p(x)} & -\frac{1}{1-p(x)} \end{bmatrix}
\end{aligned}
$$

Plug these in.

$$
\begin{aligned}
\psi(\Delta y_i^h, z_i, x_i, \theta(x_i)) &= \alpha(x) - (\nabla_\theta H)\Lambda(x)^{-1} l_\theta \\
&= \alpha(x) - (\nabla_\theta H)\Lambda(x)^{-1} \left(- \begin{bmatrix} 1 \\ z \end{bmatrix} (\Delta y^h - \alpha(x) - \beta(x)z) \right) \\
&= \alpha(x) + \begin{bmatrix} \frac{1}{1-p(x)} & -\frac{1}{1-p(x)} \end{bmatrix} \begin{bmatrix} 1 \\ z \end{bmatrix} (\Delta y^h - \alpha(x) - \beta(x)z) \\
&= \alpha(x) + \left(\frac{1}{1-p(x)} - \frac{z}{1-p(x)} \right) (\Delta y^h - \alpha(x) - \beta(x)z) \\
&= \alpha(x) + \frac{(1-z)(\Delta y^h - \alpha(x) - \beta(x)z)}{1-p(x)} \\
&= \alpha(x) + \frac{(1-z)(\Delta y^h - \alpha(x))}{1-p(x)}
\end{aligned}
$$

where the last line uses the fact that $(1-z)z = 0$.

8.4. ATE Estimator Derivation

Set $H(x, \theta(x)) = \beta(x)$. Then, $\nabla_\theta H = \begin{bmatrix} 0 & 1 \end{bmatrix}$. The inverse of Equation 6 is the same as in Appendix 8.3.

This gives:

$$
\begin{aligned}
(\nabla_\theta H)\Lambda(x)^{-1} &= \begin{bmatrix} 0 & 1 \end{bmatrix} \begin{bmatrix} \frac{1}{1-p(x)} & -\frac{1}{1-p(x)} \\ -\frac{1}{1-p(x)} & \frac{1}{p(x)(1-p(x))} \end{bmatrix} \\
&= \begin{bmatrix} -\frac{1}{1-p(x)} & \frac{1}{p(x)(1-p(x))} \end{bmatrix}
\end{aligned}
$$

Plug these into Equation 5.

$$
\begin{aligned}
\psi(\Delta y_i^h, z_i, x_i, \theta(x_i)) &= \beta(x) - (\nabla_\theta H)\Lambda(x)^{-1} l_\theta \\
&= \beta(x) - (\nabla_\theta H)\Lambda(x)^{-1} \left(- \begin{bmatrix} 1 \\ z \end{bmatrix} (\Delta y^h - \alpha(x) - \beta(x)z) \right) \\
&= \beta(x) + \begin{bmatrix} -\frac{1}{1-p(x)} & \frac{1}{p(x)(1-p(x))} \end{bmatrix} \begin{bmatrix} 1 \\ z \end{bmatrix} (\Delta y^h - \alpha(x) - \beta(x)z) \\
&= \beta(x) + \left(-\frac{1}{1-p(x)} + \frac{z}{p(x)(1-p(x))} \right) (\Delta y^h - \alpha(x) - \beta(x)z) \\
&= \beta(x) + \frac{(z - p(x))(\Delta y^h - \alpha(x) - \beta(x)z)}{p(x)(1-p(x))}
\end{aligned}
$$

Add and subtract $p(x)z$ to the numerator of the second term.

$$
\begin{aligned}
\psi(\Delta y_i^h, z_i, x_i, \theta(x_i)) &= \beta(x) + \frac{(z - p(x) + p(x)z - p(x)z)(\Delta y^h - \alpha(x) - \beta(x)z)}{p(x)(1-p(x))} \\
&= \beta(x) + \frac{[(1-p(x))z - p(x)(1-z)]\Delta y^h - \alpha(x) - \beta(x)z)}{p(x)(1-p(x))} \\
&= \beta(x) + \frac{(1-p(x))z(\Delta y^h - \alpha(x) - \beta(x)z)}{p(x)(1-p(x))} - \frac{p(x)(1-z)(\Delta y^h - \alpha(x) - \beta(x)z)}{p(x)(1-p(x))} \\
&= \beta(x) + \frac{z(\Delta y^h - \alpha(x) - \beta(x)z)}{p(x)} - \frac{(1-z)(\Delta y^h - \alpha(x) - \beta(x)z)}{1-p(x)} \\
&= \beta(x) + \frac{z(\Delta y^h - \alpha(x) - \beta(x)z)}{p(x)} - \frac{(1-z)(\Delta y^h - \alpha(x))}{1-p(x)}
\end{aligned}
$$

where the last line uses the fact that $(1-z)z = 0$.

8.5. ATET Estimator Derivation

Let $c = 1 - z$. Set $H(x, \theta(x)) = (\alpha(x) + \beta(x))z - \alpha(x)c$. Then, $\nabla_\theta H = \begin{bmatrix} z - c & z \end{bmatrix}$. The inverse of Equation 6 is the same as in Appendix 8.3.
This gives:

$$\begin{aligned}
(\nabla_\theta H)\Lambda(x)^{-1} &= \begin{bmatrix} z - c & z \end{bmatrix} \begin{bmatrix} \frac{1}{1-p(x)} & -\frac{1}{1-p(x)} \\ -\frac{1}{1-p(x)} & \frac{1}{p(x)(1-p(x))} \end{bmatrix} \\
&= \begin{bmatrix} \frac{z-c}{1-p(x)} - \frac{z}{1-p(x)} & -\frac{z-c}{1-p(x)} + \frac{z}{p(x)(1-p(x))} \end{bmatrix} \\
&= \begin{bmatrix} -\frac{c}{1-p(x)} & -\frac{(z-c)\,p(x)}{p(x)(1-p(x))} + \frac{z}{p(x)(1-p(x))} \end{bmatrix} \\
&= \begin{bmatrix} -\frac{c}{1-p(x)} & \frac{c\,p(x)+(1-p(x))z}{p(x)(1-p(x))} \end{bmatrix}
\end{aligned}$$

Plug these into Equation 5.

$$\begin{aligned}
\psi(\Delta y_i^h, z_i, x_i, \theta(x_i)) &= (\alpha(x) + \beta(x))z - \alpha(x)c - (\nabla_\theta H)\Lambda(x)^{-1} l_\theta \\
&= (\alpha(x) + \beta(x))z - \alpha(x)c - (\nabla_\theta H)\Lambda(x)^{-1} \left(- \begin{bmatrix} 1 \\ z \end{bmatrix} (\Delta y^h - \alpha(x) - \beta(x)z) \right) \\
&= (\alpha(x) + \beta(x))z - \alpha(x)c + \begin{bmatrix} -\frac{c}{1-p(x)} & \frac{c\,p(x)+(1-p(x))z}{p(x)(1-p(x))} \end{bmatrix} \begin{bmatrix} 1 \\ z \end{bmatrix} (\Delta y^h - \alpha(x) - \beta(x)z) \\
&= (\alpha(x) + \beta(x))z - \alpha(x)c + \left(-\frac{p(x)}{p(x)(1 - p(x))} + \frac{z}{p(x)(1 - p(x))} \right) (\Delta y^h - \alpha(x) - \beta(x)z) \\
&= (\alpha(x) + \beta(x))z - \alpha(x)c + \frac{(z - p(x))(\Delta y^h - \alpha(x) - \beta(x)z)}{p(x)(1 - p(x))}
\end{aligned}$$

Add and subtract $p(x)z$ to the numerator of the second term, as in Section 8.4.

$$\psi(\Delta y_i^h, z_i, x_i, \theta(x_i)) = (\alpha(x) + \beta(x))z - \alpha(x)c + \frac{z(\Delta y^h - \alpha(x) - \beta(x)z)}{p(x)} - \frac{(1 - z)(\Delta y^h - \alpha(x))}{1 - p(x)}$$

8.6. Counterfactual Effect Estimator Derivation

Set $H(x,\theta(x)) = \beta(x)(z' - z)$. Then, $\nabla_\theta H = \begin{bmatrix} 0 & z' - z \end{bmatrix}$. The inverse of Equation 6 is the same as in Appendix 8.3.
Plug these into Equation 5.

$$\begin{aligned}
\psi(\Delta y_i^h, z_i, x_i, \theta(x_i)) &= \beta(x)(z' - z) - (\nabla_\theta H)\Lambda(x)^{-1} l_\theta \\
&= \beta(x)(z' - z) - (\nabla_\theta H)\Lambda(x)^{-1}\left(-\begin{bmatrix} 1 \\ z \end{bmatrix}(\Delta y^h - \alpha(x) - \beta(x)z)\right) \\
&= \beta(x)(z' - z) + (z' - z)\begin{bmatrix} -\frac{1}{1-p(x)} & \frac{1}{p(x)(1-p(x))} \end{bmatrix}\begin{bmatrix} 1 \\ z \end{bmatrix}(\Delta y^h - \alpha(x) - \beta(x)z) \\
&= (z' - z)\left[\beta(x) + \left(-\frac{1}{1-p(x)} + \frac{z}{p(x)(1-p(x))}\right)(\Delta y^h - \alpha(x) - \beta(x)z)\right] \\
&= (z' - z)\left[\beta(x) + \frac{(z - p(x))(\Delta y^h - \alpha(x) - \beta(x)z)}{p(x)(1-p(x))}\right]
\end{aligned}$$

The remainder of the derivation simplifying the bracketed terms follows Appendix 8.4. The final expression is given by:

$$\psi(\Delta y_i^h, z_i, x_i, \theta(x_i)) = (z' - z)\left[\beta(x) + \frac{z(\Delta y^h - \alpha(x) - \beta(x)z)}{p(x)} - \frac{(1-z)(\Delta y^h - \alpha(x))}{1-p(x)}\right]$$

8.7. Deep Net Architectures

	Feature History (Years)		
	1	5	10
Number of Features	333	1342	3204
Hidden Layer Architecture	[300, 150, 75, 35, 15]	[1500, 750, 375, 150, 75, 35, 15]	[2700, 1350, 675, 300, 150, 75, 35, 15]
Dropout Rate	20%		

Table A4. Architecture for Deep Nets with 1% Tolerance for Missing Observations

	Feature History (Years)		
	1	5	10
Number of Features	517	2502	5314
Hidden Layer Architecture	[500, 300, 150, 75, 35, 15]	[3000, 1500, 750, 375, 150, 75, 35, 15]	[5000, 2700, 1350, 675, 300, 150, 75, 35, 15]
Dropout Rate	20%		

Table A5. Architecture for Deep Nets with 10% Tolerance for Missing Observations

8.8. Base Effects

Table A6. Cash Base Effect

Difference in Cash Holdings Versus 2019 (% 2019Q4 Assets)						
Base Effect Accounting for Heterogeneity						
	Model (Feature History, Missingness Tolerance)					
Year	(1,1)	(1,10)	(5,1)	(5,10)	(10,1)	(10,10)
2020	10.15*	6.49***	6.39***	6.78***	6.62***	6.75***
	(5.98)	(1.61)	(0.90)	(0.98)	(0.91)	(0.97)
2021	9.81*	9.76***	11.81***	12.96***	13.03***	13.32***
	(5.85)	(3.29)	(3.83)	(4.19)	(4.07)	(4.26)
2022	9.33	6.92***	7.83***	8.38***	8.62***	8.56***
	(6.02)	(2.58)	(2.86)	(3.01)	(2.95)	(3.12)
2023	7.63*	5.40***	5.16***	5.09***	4.94***	4.90***
	(4.19)	(1.77)	(1.46)	(1.43)	(1.43)	(1.38)

Standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The table reports the base effects for the change in cash holdings, as a percent of 2019Q4 assets. Results for all model specifications are reported here; the corresponding architectures are reported in Tables A4 and A5 in the Appendix. The finding of large, positive base effects are largely consistent across different model specifications, as well as the DiD regressions reported in Table 3.

Table A7. Debt Base Effect

Difference in Total Debt Versus 2019 (% 2019Q4 Assets)						
Base Effect Accounting for Heterogeneity						
	Model (Feature History, Missingness Tolerance)					
Year	(1,1)	(1,10)	(5,1)	(5,10)	(10,1)	(10,10)
2020	6.04	5.68***	3.66***	3.53***	3.97***	4.13***
	(4.70)	(1.99)	(0.92)	(0.99)	(1.01)	(1.05)
2021	19.13**	14.21***	11.53***	11.20***	12.48***	12.53***
	(8.03)	(3.25)	(2.00)	(1.96)	(2.25)	(2.26)
2022	19.83	20.97***	16.16***	16.67***	16.65***	16.92***
	(17.93)	(4.89)	(2.51)	(3.02)	(2.95)	(3.02)
2023	27.45**	24.83***	16.50***	17.88***	17.06***	17.73***
	(12.32)	(5.15)	(2.66)	(2.84)	(2.69)	(2.76)

Standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The table reports the base effects for the change in debt, as a percent of 2019Q4 assets. Results for all model specifications are reported here; the corresponding architectures are reported in Tables A4 and A5 in the Appendix. The finding of large, positive base effects are largely consistent across different model specifications, as well as the DiD regressions reported in Table 3.

Table A8. Payout Base Effect

Difference in Payout Versus 2019 (% 2019Q4 Assets)						
Base Effect Accounting for Heterogeneity						
	Model (Feature History, Missingness Tolerance)					
Year	(1,1)	(1,10)	(5,1)	(5,10)	(10,1)	(10,10)
2020	-0.21	-0.16**	-0.18***	-0.16***	-0.18***	-0.18***
	(0.18)	(0.08)	(0.04)	(0.04)	(0.04)	(0.04)
2021	0.13	0.17	0.23	0.19	0.21	0.16
	(0.42)	(0.23)	(0.20)	(0.17)	(0.18)	(0.18)
2022	-0.07	0.18	0.22**	0.27**	0.25*	0.24*
	(0.65)	(0.17)	(0.10)	(0.13)	(0.13)	(0.13)
2023	0.12	0.27*	0.29**	0.33***	0.30***	0.29**
	(0.31)	(0.16)	(0.11)	(0.12)	(0.11)	(0.12)

Standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The table reports the base effects for the change in payouts with respect to 2019, scaled by 2019Q4 assets. Results for all model specifications are reported here; the corresponding architectures are reported in Tables A4 and A5 in the Appendix. Generally consistent results are found whereby the base effect is initially negative in 2020, null in 2021, and then positive in 2022 and 2023. This is in contrast to the null results picked up by the DiD regressions reported in Table 4.

Table A9. Investment Base Effect

Difference in CAPEX and R&D Versus 2019 (% 2019Q4 Assets)						
Base Effect Accounting for Heterogeneity						
	Model (Feature History, Missingness Tolerance)					
Year	(1,1)	(1,10)	(5,1)	(5,10)	(10,1)	(10,10)
2020	-0.22	0.10	-0.01	-0.02	-0.02	-0.02
	(0.54)	(0.19)	(0.07)	(0.07)	(0.07)	(0.07)
2021	0.76	0.85***	0.67***	0.72***	0.73***	0.72***
	(0.72)	(0.25)	(0.12)	(0.12)	(0.12)	(0.12)
2022	1.04	1.07***	0.92***	1.02***	1.01***	1.02***
	(0.85)	(0.30)	(0.14)	(0.15)	(0.14)	(0.15)
2023	1.24	1.25***	1.00***	1.04***	1.04***	1.08***
	(0.94)	(0.33)	(0.16)	(0.16)	(0.16)	(0.16)

Standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The table reports the base effects for the difference in annual investment versus 2019, scaled by 2019Q4 assets. Results for all model specifications are reported here; the corresponding architectures are reported in Tables A4 and A5 in the Appendix. Consistent with the positive coefficient found for the post period in Table 4 for the DiD regressions, positive base effects are generally found. However, this is not robust to an alternative proxy for investment. As seen in Table A20, proxying investment by the annual change in gross property, plant, and equipment suggests a negative effect which reverts to null.

8.9. Average Treatment Effects

Table A10. Cash ATE

Difference in Cash Holdings Versus 2019 (% 2019Q4 Assets)						
Average Treatment Effect Accounting for Heterogeneity						
	Model (Feature History, Missingness Tolerance)					
Year	(1,1)	(1,10)	(5,1)	(5,10)	(10,1)	(10,10)
2020	-17.63	-6.24*	-3.30***	-3.27***	-3.00***	-3.37***
	(14.22)	(3.64)	(1.02)	(1.06)	(1.01)	(1.09)
2021	-19.78	-9.68**	-9.56**	-11.56***	-11.05***	-11.11**
	(15.96)	(4.07)	(3.92)	(4.07)	(4.12)	(4.48)
2022	-15.64	-7.22*	-6.92**	-7.64**	-7.51**	-7.61**
	(13.81)	(4.28)	(2.87)	(2.98)	(2.95)	(3.05)
2023	-15.36	-4.88	-3.93**	-3.62**	-3.52**	-3.22**
	(11.19)	(3.92)	(1.53)	(1.48)	(1.46)	(1.44)

Standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The table reports the average treatment effects for the change in cash holdings, as a percent of 2019Q4 assets. This corresponds to the estimator reported in Equation 9. Results for all model specifications are reported here; the corresponding architectures are reported in Tables A4 and A5 in the Appendix. The results are robust across model specifications, showing large, negative treatment effects. These are also consistent with the DiD regressions reported in Table 3.

Table A11. Debt ATE

Difference in Total Debt Versus 2019 (% 2019Q4 Assets)						
Average Treatment Effect Accounting for Heterogeneity						
	Model (Feature History, Missingness Tolerance)					
Year	(1,1)	(1,10)	(5,1)	(5,10)	(10,1)	(10,10)
2020	-20.48	-2.76	-0.98	-0.91	-0.91	-0.86
	(18.15)	(4.69)	(1.20)	(1.12)	(1.11)	(1.13)
2021	-36.12*	-15.45**	-6.66***	-5.93***	-7.04***	-6.96***
	(18.79)	(7.85)	(2.43)	(2.21)	(2.39)	(2.40)
2022	-39.72	-19.50**	-9.72***	-9.60***	-9.34***	-9.75***
	(27.89)	(9.50)	(2.88)	(3.00)	(3.00)	(3.11)
2023	-43.82	-16.12*	-9.33***	-9.29***	-8.64***	-8.90***
	(30.06)	(8.50)	(3.18)	(2.91)	(2.82)	(2.94)

Standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The table reports the average treatment effects for change in total debt, as a percent of 2019Q4 assets. This corresponds to the estimator reported in Equation 9. Results for all model specifications are reported here; the corresponding architectures are reported in Tables A4 and A5 in the Appendix. The finding of an initial null effect and subsequent large, negative effects are generally consistent across specifications. Table 6 compares the treatment effect estimates across different models and suggests that the results are broadly in line.

Table A12. Payout ATE

Difference in Payout Versus 2019 (% 2019Q4 Assets)						
Average Treatment Effect Accounting for Heterogeneity						
	Model (Feature History, Missingness Tolerance)					
Year	(1,1)	(1,10)	(5,1)	(5,10)	(10,1)	(10,10)
2020	-0.62	-0.25	0.13**	0.11**	0.14**	0.14***
	(0.75)	(0.30)	(0.07)	(0.06)	(0.06)	(0.05)
2021	-2.28	-0.05	0.22	0.24	0.24	0.32
	(1.98)	(0.70)	(0.26)	(0.25)	(0.25)	(0.23)
2022	-1.16	1.14	0.96***	0.78***	0.83***	0.86***
	(4.61)	(1.84)	(0.34)	(0.27)	(0.27)	(0.28)
2023	3.28	0.38	0.67**	0.46***	0.50***	0.53***
	(4.57)	(0.79)	(0.26)	(0.17)	(0.16)	(0.16)

Standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The table reports the average treatment effects for the different in annual payouts versus 2019, as a percent of 2019Q4 assets. This corresponds to the estimator reported in Equation 9. Results for all model specifications are reported here; the corresponding architectures are reported in Tables A4 and A5 in the Appendix. The model results are consistent across models using 5 and 10 years of feature history, showing an initial positive effect in 2020 followed by a null effect in 2021 before returning to positive effects for 2022 and 2023. Table 7 compares the treatment effect estimates across different models. While the treatment effects are comparable, the standard errors for the dynamic (heterogeneous) treatment effects are smaller.

Table A13. Investment ATE

Difference in CAPEX and R&D Versus 2019 (% 2019Q4 Assets)						
Average Treatment Effect Accounting for Heterogeneity						
	Model (Feature History, Missingness Tolerance)					
Year	(1,1)	(1,10)	(5,1)	(5,10)	(10,1)	(10,10)
2020	-0.60	-0.49	-0.03	-0.00	0.09	-0.00
	(2.44)	(0.62)	(0.25)	(0.16)	(0.15)	(0.13)
2021	-2.93	-0.82	0.11	-0.01	-0.27	-0.41***
	(3.03)	(0.75)	(0.48)	(0.42)	(0.20)	(0.14)
2022	-3.16	-0.65	-0.37	-0.48**	-0.48**	-0.58***
	(3.47)	(1.14)	(0.23)	(0.20)	(0.19)	(0.17)
2023	-3.04	-0.83	-0.25	-0.35	-0.31	-0.46**
	(2.62)	(0.84)	(0.26)	(0.23)	(0.21)	(0.20)

Standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The table reports the average treatment effects for the difference in annual investment versus 2019, scaled by 2019Q4 assets. This corresponds to the estimator reported in Equation 9. Results for all model specifications are reported here; the corresponding architectures are reported in Tables A4 and A5 in the Appendix. Particularly for the models with longer covariate histories, null-to-negative treatment effects are estimated. Table 8 compares the different treatment effects, showing that incorporating high-dimensional controls and heterogeneity increases the point estimates for the dynamic effects. Additionally, when proxying investment using the annual change in gross plants, property, and equipment, Table A21, null effects are generally estimated across different models and cumulation horizons.

8.10. ATE and ATET Comparison

Table A14. Cash Treatment Effect Comparison

Dynamic (Heterogeneous) Treatment Effect Comparison			
Cash (% 2019Q4 Assets)			
Year	Average Treatment Effect (1)	ATE on Treated (2)	Difference (1)-(2)
2020	-3.00	-3.97	0.97
	(1.01)	(1.02)	(1.44)
2021	-11.05	-10.91	-0.14
	(4.12)	(4.19)	(5.88)
2022	-7.51	-6.78	-0.74
	(2.95)	(3.02)	(4.22)
2023	-3.52	-3.38	-0.15
	(1.46)	(1.47)	(2.07)

Standard-errors in parentheses

The table compares the dynamic (heterogeneous) ATE and ATET estimates for cash, as a percent of 2019Q4, for the model which uses 10 years of feature history and 1% tolerance for missing observations. The architecture for the model is given by Tables A4 and A5 in the Appendix. Section 8.4 and 8.5 derives the IF estimator for the ATE and ATET, respectively. The estimates are comparable with the difference not being statistically significant.

Table A15. Debt Treatment Effect Comparison

Dynamic (Heterogeneous) Treatment Effect Comparison			
Total Debt (% 2019Q4 Assets)			
Year	Average Treatment Effect (1)	ATE on Treated (2)	Difference (1)-(2)
2020	-0.91	-1.51	0.59
	(1.11)	(1.12)	(1.57)
2021	-7.04	-7.86	0.81
	(2.39)	(2.36)	(3.36)
2022	-9.34	-10.92	1.58
	(3.00)	(3.07)	(4.29)
2023	-8.64	-10.02	1.39
	(2.82)	(2.90)	(4.04)

Standard-errors in parentheses

The table compares the dynamic (heterogeneous) ATE and ATET estimates for debt, as a percent of 2019Q4, for the model which uses 10 years of feature history and 1% tolerance for missing observations. The architecture for the model is given by Tables A4 and A5 in the Appendix. Section 8.4 and 8.5 derives the IF estimator for the ATE and ATET, respectively. The estimates are comparable with the difference not being statistically significant.

Table A16. Payout Treatment Effect Comparison

Dynamic (Heterogeneous) Treatment Effect Comparison			
Payout (% 2019Q4 Assets)			
Year	Average Treatment Effect (1)	ATE on Treated (2)	Difference (1)-(2)
2020	0.14	0.11	0.03
	(0.06)	(0.05)	(0.08)
2021	0.24	0.07	0.18
	(0.25)	(0.25)	(0.35)
2022	0.83	0.44	0.39
	(0.27)	(0.27)	(0.39)
2023	0.50	0.28	0.22
	(0.16)	(0.16)	(0.23)

Standard-errors in parentheses

The table compares the dynamic (heterogeneous) ATE and ATET estimates for payout, as a percent of 2019Q4, for the model which uses 10 years of feature history and 1% tolerance for missing observations. The architecture for the model is given by Tables A4 and A5 in the Appendix. Section 8.4 and 8.5 derives the IF estimator for the ATE and ATET, respectively. The estimates are comparable with the difference not being statistically significant.

Table A17. Investment Treatment Effect Comparison

Dynamic (Heterogeneous) Treatment Effect Comparison			
CAPEX and R&D (% 2019Q4 Assets)			
Year	Average Treatment Effect (1)	ATE on Treated (2)	Difference (1)-(2)
2020	0.09	0.08	0.02
	(0.15)	(0.13)	(0.20)
2021	-0.27	-0.28	0.01
	(0.20)	(0.21)	(0.29)
2022	-0.48	-0.42	-0.06
	(0.19)	(0.20)	(0.27)
2023	-0.31	-0.38	0.06
	(0.21)	(0.21)	(0.30)

Standard-errors in parentheses

The table compares the dynamic (heterogeneous) ATE and ATET estimates for investment, as a percent of 2019Q4, for the model which uses 10 years of feature history and 1% tolerance for missing observations. The architecture for the model is given by Tables A4 and A5 in the Appendix. Section 8.4 and 8.5 derives the IF estimator for the ATE and ATET, respectively. The estimates are comparable with the difference not being statistically significant.

8.11. Counterfactual Targeting

Table A18. BB-AAA Targeting

Difference in CAPEX and R&D Versus 2019 (% 2019Q4 Assets)						
Uplift Accounting for Heterogeneity, BB-AAA Targeting						
	Model (Feature History, Missingness Tolerance)					
Year	(1,1)	(1,10)	(5,1)	(5,10)	(10,1)	(10,10)
2020	0.00	0.00	0.05	0.04	0.05*	0.05
	(0.24)	(0.08)	(0.03)	(0.03)	(0.03)	(0.04)
2021	0.05	-0.01	0.08	0.02	0.02	-0.07
	(0.21)	(0.12)	(0.06)	(0.05)	(0.05)	(0.06)
2022	-0.22	-0.14	0.02	-0.07	-0.03	-0.20**
	(0.37)	(0.19)	(0.07)	(0.07)	(0.06)	(0.08)
2023	0.10	-0.05	0.07	-0.10	-0.03	-0.28***
	(0.42)	(0.17)	(0.09)	(0.08)	(0.08)	(0.10)

Standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The table reports the expected difference in the average CATE among targeted firms in annual investment versus 2019, as a percent of 2019Q4 assets, from expanding the targeted firms from BBB-AAA to BB-AAA. Results for all model specifications are reported here; the corresponding architectures are reported in Tables A4 and A5 in the Appendix. While a positive effect is detected for 2020 for the model with 10 years of feature history and 1% missingness tolerance, this is not robust to other model specifications. Additionally, positive effects are not estimated for other years. Figure A3 and Table A22 shows the corresponding results when proxying investment by the annual change in gross property, plant, and equipment. Those results provide stronger evidence that counterfactual targeting may have improved average outcomes among treated firms in 2020, although significant results are not picked up across all model specifications.

Table A19. BB-A Targeting

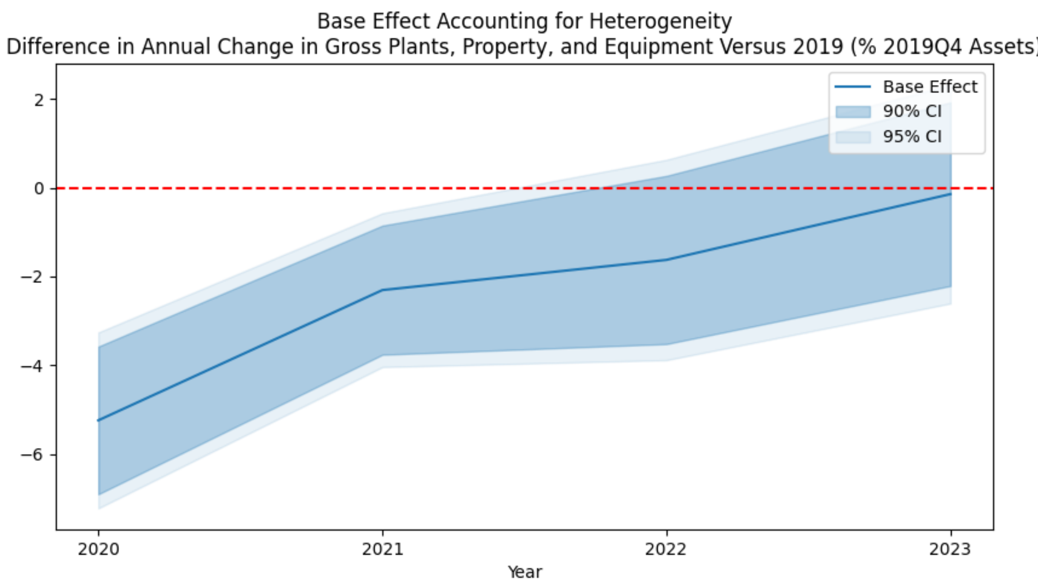
Difference in CAPEX and R&D Versus 2019 (% 2019Q4 Assets)						
Uplift Accounting for Heterogeneity, BB-A Targeting						
	Model (Feature History, Missingness Tolerance)					
Year	(1,1)	(1,10)	(5,1)	(5,10)	(10,1)	(10,10)
2020	-0.04	-0.02	0.03	0.01	0.01	0.00
	(0.34)	(0.08)	(0.04)	(0.04)	(0.04)	(0.06)
2021	-0.21	-0.11	0.06	0.01	0.00	-0.13
	(0.37)	(0.15)	(0.07)	(0.06)	(0.06)	(0.09)
2022	-0.22	-0.17	0.02	-0.09	-0.05	-0.31***
	(0.51)	(0.21)	(0.09)	(0.08)	(0.07)	(0.11)
2023	0.05	-0.09	0.01	-0.17*	-0.09	-0.48***
	(0.39)	(0.19)	(0.10)	(0.10)	(0.09)	(0.14)

Standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The table reports the expected difference in the average CATE among targeted firms in annual investment versus 2019, as a percent of 2019Q4 assets, from expanding the targeted firms from BBB-AAA to BB-A. Results for all model specifications are reported here; the corresponding architectures are reported in Tables A4 and A5 in the Appendix. As seen, no improvement in outcomes are detected across any specification.

8.12. Alternative Investment Proxy: Change in Gross Property, Plant, and Equipment

Figure A1. Base Effect for PPE Investment Proxy Negative Before Reverting to Null



The figure plots the base effects for the difference in annual investment versus 2019, scaled by 2019Q4 assets. Investment here is proxied by the annual change in gross property, plant, and equipment, in contrast to Figure 13 where investment is proxied by capital expenditures and R&D. The model above uses 10 years of feature history and 1% tolerance for missing observations. Details on its architecture is reported in Table A4 in the Appendix. Table A20 reports results across all model specifications. In contrast to the null-to-positive effects found using the CAPEX and R&D proxy for investment, here there are negative base effects are estimated for 2020 and 2021, which then become null for 2022 and 2023.

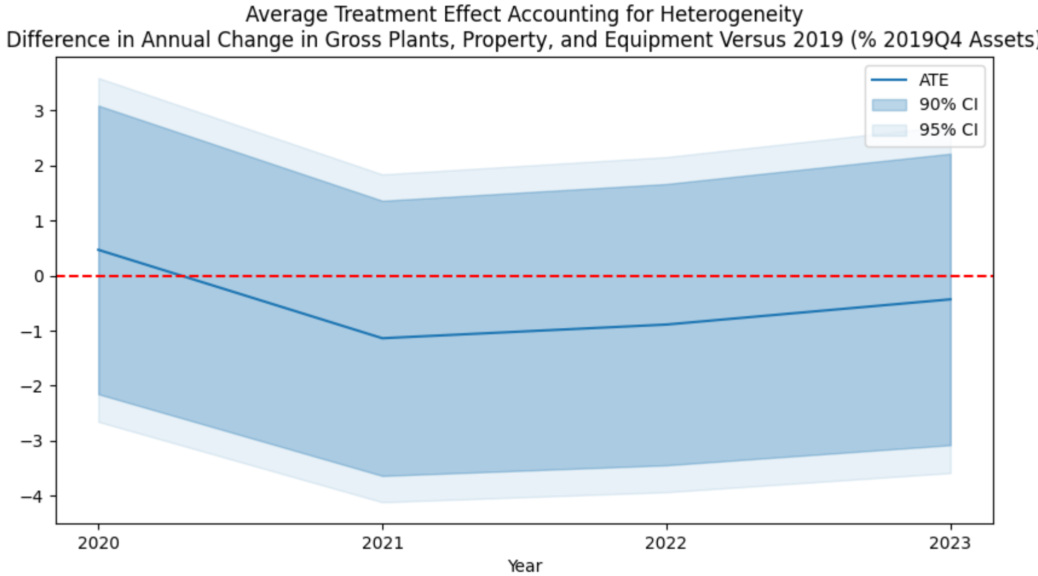
Table A20. Base Effect for PPE Investment Proxy

Difference in Annual Change in Gross Plants, Property, and Equipment Versus 2019 (% 2019Q4 Assets)						
Base Effect Accounting for Heterogeneity						
	Model (Feature History, Missingness Tolerance)					
Year	(1,1)	(1,10)	(5,1)	(5,10)	(10,1)	(10,10)
2020	-1.01	-6.49***	-4.87***	-5.25***	-5.24***	-5.10***
	(7.81)	(1.72)	(1.03)	(0.97)	(1.01)	(1.06)
2021	-2.41	-2.23	-2.28***	-2.43***	-2.31***	-2.21**
	(5.05)	(1.99)	(0.88)	(0.84)	(0.88)	(0.91)
2022	-4.19	-2.26	-1.86*	-2.16**	-1.63	-1.28
	(6.33)	(1.85)	(0.97)	(0.90)	(1.15)	(1.13)
2023	2.66	-0.58	-0.32	-0.41	-0.14	-0.02
	(4.48)	(2.31)	(1.10)	(1.06)	(1.26)	(1.24)

Standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The table reports the base effects for the difference in annual investment versus 2019, scaled by 2019Q4 assets. Investment here is proxied by the annual change in gross property, plant, and equipment, in contrast to Table A9 where investment is proxied by capital expenditures and R&D. Results for all model specifications are reported here; the corresponding architectures are reported in Tables A4 and A5 in the Appendix. The models produce relatively consistent results, with an initially negative base effect found for 2020 and 2021, becomes null for 2022 and 2023. This is in contrast to the null-to-positive effects found using the CAPEX and R&D proxy for investment.

Figure A2. ATE for PPE Investment Proxy Null



The figure plots the average treatment effects for the difference in annual investment versus 2019, scaled by 2019Q4 assets. Investment here is proxied by the annual change in gross property, plant, and equipment, in contrast to Figure 17 where investment is proxied by capital expenditures and R&D. This corresponds to the estimator reported in Equation 9. The model above uses 10 years of feature history and 1% tolerance for missing observations. Details on its architecture is reported in Table A4 in the Appendix. Table A21 reports results across all model specifications, showing that null effects are estimated in every instance. In contrast, the proxy for investment using CAPEX and R&D indicates a negative treatment effect for 2022.

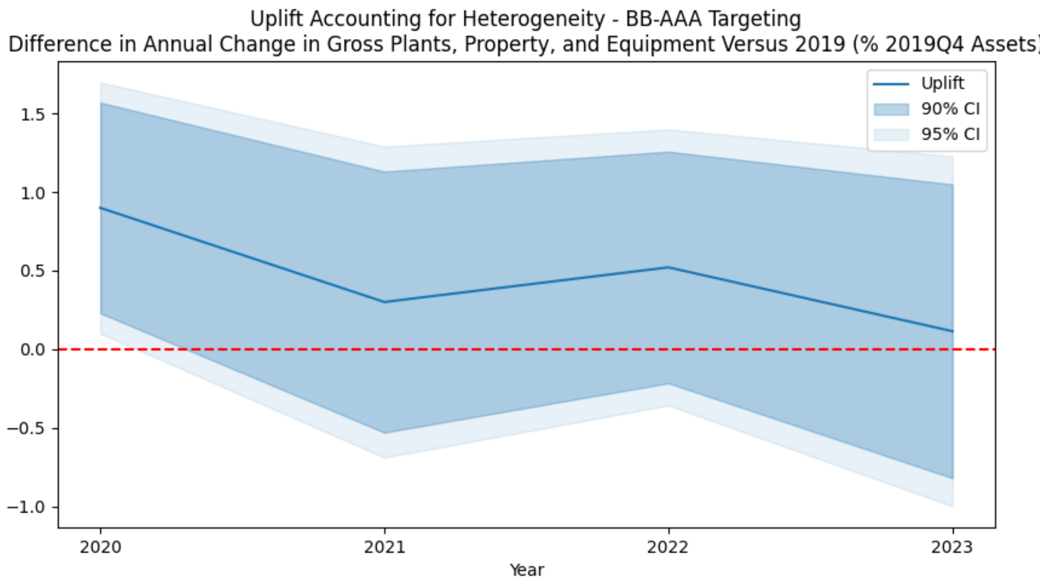
Table A21. ATE for PPE Investment

Difference in Annual Change in Gross Plants, Property, and Equipment Versus 2019 (% 2019Q4 Assets)						
Average Treatment Effect Accounting for Heterogeneity						
	Model (Feature History, Missingness Tolerance)					
Year	(1,1)	(1,10)	(5,1)	(5,10)	(10,1)	(10,10)
2020	38.02	9.63	-1.18	0.01	0.47	0.19
	(34.56)	(15.40)	(3.08)	(2.14)	(1.59)	(2.10)
2021	61.37	17.95	-0.01	0.03	-1.14	-0.87
	(94.61)	(17.78)	(2.05)	(1.91)	(1.52)	(1.59)
2022	52.72	6.29	0.91	0.99	-0.89	-1.71
	(48.18)	(9.77)	(2.76)	(2.05)	(1.55)	(1.47)
2023	20.16	12.82	1.61	0.66	-0.43	-0.85
	(36.17)	(8.38)	(2.06)	(1.48)	(1.61)	(1.60)

Standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The table reports the average treatment effects for the difference in annual investment versus 2019, scaled by 2019Q4 assets. Investment here is proxied by the annual change in gross property, plant, and equipment, in contrast to Table A13 where investment is proxied by capital expenditures and R&D. This corresponds to the estimator reported in Equation 9. Results for all model specifications are reported here; the corresponding architectures are reported in Tables A4 and A5 in the Appendix. Null effects are generally estimated across different models and cumulation horizons. In contrast, the proxy for investment using CAPEX and R&D indicates a negative treatment effect for 2022.

Figure A3. Possible Improvement in Investment Outcome from BB-AAA Targeting



The figure plots the the expected difference in the average CATEs among targeted firms in annual investment versus 2019, as a percent of 2019Q4 assets, from expanding the targeted firms from BBB-AAA to BB-AAA for the model using 10 years of feature history and 1% tolerance for missing observations. The results suggest an improvement in investment outcomes among treated firms for 2020. Table A23 shows that this effect is found for the models using 10 years of feature history. However, this is not robust across all specifications.

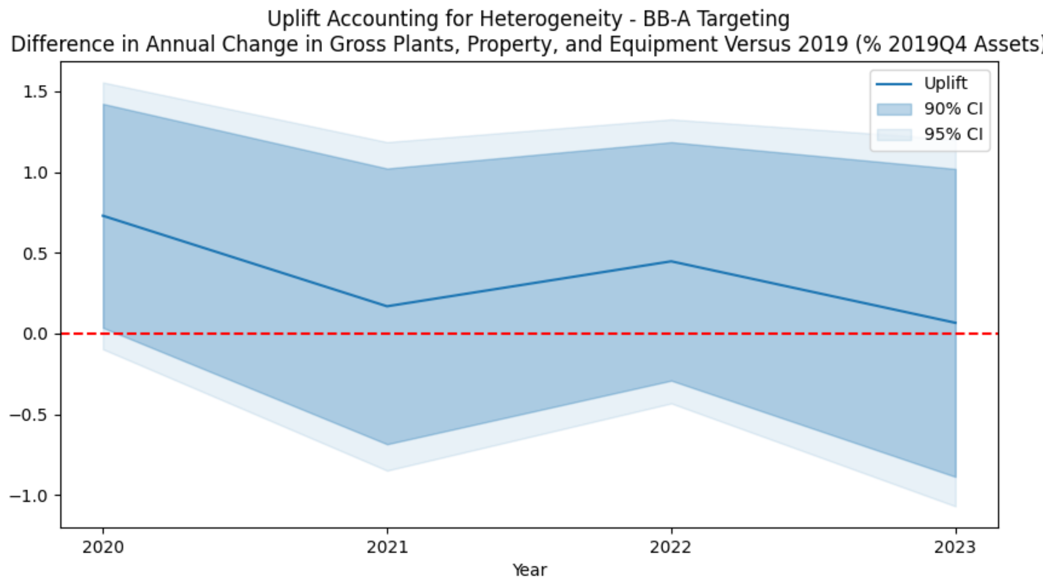
Table A22. BB-AAA Targeting

Difference in Annual Change in Gross Plants, Property, and Equipment Versus 2019 (% 2019Q4 Assets)						
Uplift Accounting for Heterogeneity, BB-AAA Targeting						
	Model (Feature History, Missingness Tolerance)					
Year	(1,1)	(1,10)	(5,1)	(5,10)	(10,1)	(10,10)
2020	0.34	1.30	0.59	0.85*	0.90**	2.04***
	(2.55)	(1.11)	(0.51)	(0.43)	(0.41)	(0.54)
2021	2.34	0.63	-0.35	0.20	0.30	0.72
	(3.07)	(1.15)	(0.62)	(0.54)	(0.50)	(0.68)
2022	1.90	1.23	0.65	0.66	0.52	1.09*
	(2.29)	(1.09)	(0.55)	(0.46)	(0.45)	(0.57)
2023	-1.70	0.62	0.00	0.17	0.11	0.19
	(1.94)	(1.21)	(0.64)	(0.58)	(0.57)	(0.69)

Standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The table reports the expected difference in the average CATEs among targeted firms in annual investment versus 2019, as a percent of 2019Q4 assets, from expanding the targeted firms from BBB-AAA to BB-AAA. Results for all model specifications are reported here; the corresponding architectures are reported in Tables A4 and A5 in the Appendix. The results suggest an improvement in investment outcomes among treated firms for 2020, particularly for the models using 10 years of feature history. However, this is not robust across all specifications.

Figure A4. Slight Improvement in Investment Outcome from BB-A Targeting



The figure plots the expected difference in the average CATEs among targeted firms in annual investment versus 2019, as a percent of 2019Q4 assets, from changing the set of targeted firms from BBB-AAA to BB-A for the model using 10 years of feature history and 1% tolerance for missing observations. The results suggest an improvement in investment outcomes among treated firms for 2020. Table A23 shows that this effect is found for the models using 10 years of feature history. However, this is not robust across all specifications.

Table A23. BB-A Targeting

Difference in Annual Change in Gross Plants, Property, and Equipment Versus 2019 (% 2019Q4 Assets)						
Uplift Accounting for Heterogeneity, BB-A Targeting						
	Model (Feature History, Missingness Tolerance)					
Year	(1,1)	(1,10)	(5,1)	(5,10)	(10,1)	(10,10)
2020	-1.49	1.47	0.15	0.66	0.73*	2.50***
	(2.13)	(1.49)	(0.60)	(0.45)	(0.42)	(0.67)
2021	1.15	0.12	-0.16	0.20	0.17	0.88
	(3.60)	(1.41)	(0.64)	(0.53)	(0.52)	(0.83)
2022	2.37	0.66	0.58	0.62	0.45	1.17
	(2.16)	(0.96)	(0.62)	(0.46)	(0.45)	(0.71)
2023	-1.85	0.18	-0.28	0.06	0.07	0.17
	(2.55)	(1.64)	(0.71)	(0.61)	(0.58)	(0.81)

Standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

The table reports the expected difference in the average CATEs among targeted firms in annual investment versus 2019, as a percent of 2019Q4 assets, from changing the set of targeted firms from BBB-AAA to BB-A. Results for all model specifications are reported here; the corresponding architectures are reported in Tables A4 and A5 in the Appendix. The results suggest an improvement in investment outcomes among treated firms for 2020, particularly for the models using 10 years of feature history. However, this is not robust across all specifications.