State Shareholding and Structural Transformation

Evidence from the Italian Economic Miracle*

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Early draft

Abstract

This paper focuses on the effects of state shareholding in enterprises as a tool of industrial policy. I outline evidence from post-war Italy (1946–1971), leveraging a panel of more than fifteen thousand firms which accounts for more than 90% of the total share capital of joint-stock firms in the country. Balance sheets are linked to ownership information from the three main state holdings (SHs), traced across three corporate control layers. At their peak, participated firms accounted for over 20% of total assets in the database, mainly targeting larger and underperforming firms in core industrial sectors. TWFE and staggered DD models show that participation is associated with a notable increase in size and a moderate increase in profitability. A pattern of "standardisation" explains the latter: under participation, underperformers—the majority—improve, while overperformers worsen. Participation also fosters net growth at the sector level, consistent with positive spillovers on private firms and reallocation from unrelated sectors.

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> *As the world stumbles from crisis to crisis, Leviathan is back* [...]*, touted as both the saviour and the gravedigger of the capitalist economy.*
>
> (Alami and Dixon 2024, p. 1)

1 Introduction

Can the state shape the market from within? Canonically, the "grabbing hand" bends the allocation of resources toward political, rather than economic, objectives, breeding rent-seeking and misallocation (Shleifer and Vishny 1994; Krueger 1974). Insulated from the discipline of the market and facing a soft budget constraint (Kornai 1986), state control is expected to preserve unviable activities and dissipate capital. State ownership creates market failures, rather than redressing them.

Some, however, credit precisely the preservation of unviable activities with engineering industrialisation where markets alone would not. Gerschenkron (1962) highlights the potential of state intervention when substituting for the capital and coordination markets lack in lagging economies. Accordingly, developmental-state scholarship documents governments successfully "governing the market" (Johnson 1982; Wade 1990), through bureaucracies at once autonomous and embedded in the private economy (Evans 1995; Woo-Cumings 1999). A growing body of work rehabilitates industrial policy more broadly (Chang and Andreoni 2020; Aiginger and Rodrik 2020; Juhász et al. 2024; Breznitz and Gingrich 2025), with large-N, plausibly causal evidence slowly surfacing (Lane 2019).

In most of these accounts, the state largely governs the market from the outside, leaving profit-maximising firms to the new incentives provided by regulation, tariffs, subsidies, and the like. What happens when the state tries to influence the development of industries from within, participating in the market through the firms it controls? While the acquisition of equity stakes is relevant (especially in developing economies—see Juhász et al. 2022; Evenett et al. 2024) and growing (Alami and Dixon 2024), evidence on SOEs is concentrated on privatisations, listed firms, and the Chinese experience. Mazzucato (2015) casts public capital as a patient, mission-oriented investor that private finance follows rather than leads, recasting the state as an agent that creates and shapes markets rather than merely correcting their failures (Mazzucato and Penna 2016); whether ownership actually delivers these promised benefits, however, remains largely untested.

This lack of evidence has not restrained Western governments. Despite the recent acquisition of a 10 per cent stake in the chipmaker Intel being described as "pretty darn

bonkers",[1] the US Government has been acquiring shares of multiple key domestic firms (US Steel, Lithium Americas, MP Materials) in pursuit of what has been termed a "technological sovereignty with American characteristics".[2] The United Kingdom has directed its National Wealth Fund and British Business Bank to take larger and "more strategic" equity positions in domestic firms, so as to "crowd in private finance" and "anchor future superstar firms in the UK" (Department for Business and Trade 2025, p. 51). Across the European Union, governments are assembling equity-backed industrial pipelines in green technology (Veugelers et al. 2024) and in a defence sector long dominated by partly state-owned national champions (Wilkinson 2020).

I contribute large-sample evidence to the analysis of state shareholding as an instrument of industrial policy. The paper leverages the system of state holdings that steered a large part of the Italian economy through the post-war "economic miracle". These holdings, initially more autonomous and later managed through a Ministry of State Participations, were tasked with guiding the industrialisation of the Italian economy and with spreading economic dynamism to lagging geographical areas. To analyse systematically their impact, I link Imita.db, a near-census of Italian joint-stock companies covering more than 90 per cent of the share capital of the corporate universe, to a firm-by-firm reconstruction of the stakes held by the three main state holdings—IRI, ENI, and EFIM—traced through the pyramids of corporate control. The resulting panel follows more than fifteen thousand firms between 1946 and 1971; at their peak the participated firms account for more than a fifth of all assets in the panel. The holdings targeted larger and systematically underperforming firms, concentrating in the core industrial sectors of manufacturing, energy, transport, and utilities.

Applying two-way fixed-effects and staggered difference-in-differences designs (Callaway and Sant'Anna 2021), I find that participation is followed by a pronounced expansion in firm size and a more modest improvement in profitability. Over the roughly six years for which the average firm is observed after acquisition, its share capital, debt, and assets rise by 24, 87, and 57 per cent, while its return on assets improves by about one percentage point. Event studies reveal no meaningful pre-trends and a gradual onset. The size effect is large—it exceeds that documented for private-equity buy-outs—whereas the profitability gain is modest.

The descriptive evidence on mechanisms suggests that the expansion works largely through firms' enhanced capacity to raise debt: leverage rises sharply upon acquisition, and current debt weakly predicts subsequent asset growth. This is compatible with two complementary channels—a derisking effect, whereby implicit state backing relaxes financial constraints, and a focal-point effect, whereby the visible commitment of a state holding coordinates private capital onto the firm; the latter is corroborated by the

[1] *Financial Times*, 25 August 2025.
[2] *The New York Times*, 2 September 2025; *Financial Times*, 12 October 2025.

finding that the size premium fades as the state saturates a sector with participations. On profitability, a "standardisation" pattern reconciles the modest average gain with traditional scepticism toward state ownership: participation compresses the distribution of returns, lifting the underperformers that holdings preferentially target while dragging down the rare overperformers they acquire.

As industrial policy is ultimately about the structure of the economy, I analyse sector-level outcomes. I find the size effect replicated on sectoral totals: participated firms do not merely capture growth that would otherwise have accrued to their private neighbours. On the contrary, private firms that share a sector with a participated firm fare better than comparable firms elsewhere—evidence against within-sector displacement and consistent with positive spillovers. Across sectors, the effect attenuates but survives as the comparison group is narrowed to related activities, pointing to a mixture of genuine net growth and reallocation drawn from very different sectors of the economy.

Taken together, these results assemble rare firm-level evidence on the purchase of equity stakes as an instrument of industrial policy. They speak to three literatures—on industrial policy, on state ownership of enterprise, and on the state as investor—from an unusual vantage: the state's entry into firms rather than its exit through privatisation, and its participation in ordinary, non-listed enterprises rather than the publicly traded firms on which existing evidence largely rests. Throughout the study, I highlight the observational equivalence between these results and the possibility that the holdings simply selected firms and sectors already poised to grow on the basis of time-varying unobservables.

2 Related Literature

This paper contributes to the reinvigorated discussion on industrial policy, to the study of state ownership of enterprises, and to the growing debate about the state as an investor.

I follow Juhász et al. (2024, p. 216) and define industrial policy as policy explicitly targeting the transformation of the structure of economic activity in pursuit of a public goal. The mandate of SHs during the Italian economic miracle, detailed in Section 3, squarely falls within this purview, as they were tasked with aiding industrialisation (see sectoral targets in Figure 5) and generating a more equal geographic distribution of economic activity (Ciocca 2015). While previously leading perspectives found limited scope, if any, for effective industrial policy (Shleifer and Vishny 1994), recent contributions ground beneficial industrial policy in the presence of externalities,[3] the need for

[3]Key externalities are connected to learning (e.g. Hausmann and Rodrik 2003), environmental preservation (Acemoglu et al. 2016), national security (Kooi 2025), and good jobs (Rodrik and Sabel 2019; Dani Rodrik 2022).

non-horizontal public inputs,[4] and a lack of coordination or agglomeration. For example, thin capital markets such as post-war Italy's (Amatori 2020) might be insufficient to provide critical financial mass to all promising sectors without coordinating on a subset. Theoretically, the first-best response to the first two—externalities and missing public inputs—would be Pigouvian subsidies and direct government investment, respectively. The government might resolve the third, a lack of coordination, simply by supplying the missing coordination itself: either by providing a credible focal point (Myerson 2004) for economic actors, or by extending public guarantees that will make expansion in the desired sectors more appealing without ever having to be paid out (Juhász et al. 2024). This function closely relates to the market-making role highlighted by Mazzucato and Penna (2016), which reaches beyond the correction of market failures.

State-owned enterprises as instruments of industrial policy bundle a degree of each of these interventions. The ability of the government to take losses allows for the refinancing of firms generating positive externalities. Public control of infrastructure operators (e.g. in the Italian case, port authorities and the national highway agency) amounts to indirect investment into non-horizontal public inputs. Finally, the government entering a given sector of economic activity signals commitment to develop that sector and can function as a coordination device for private capital. Since policy success depends on the credibility of this commitment—fear of reversals discourages private investment (Fang and Li 2026)—delegating policy to more insulated bureaucracies (such as industrial holdings) could be a solution (Bersch and Fukuyama 2023; Besley et al. 2022). However, the inherently bundled nature of the treatment makes studying SOEs in relation to structural transformation particularly challenging.

The paucity of empirical contributions reflects this. While Juhász et al. (2022) and Kim and Sumner (2021) highlight equity stakes as a relatively widespread industrial policy tool, plausibly causal evidence (Lane 2019) is mainly available for public R&D (Gross and Sampat 2023; Kantor and Whalley 2023; Gazzani et al. 2026), trade protection (Juhász 2018), targeted lending and corporate tax cuts (Lane 2025), subsidies (Criscuolo et al. 2019; Barwick et al. 2025), or place-based policies (Bartik 2020; Kline and Moretti 2014).[5]

The subject of state-owned enterprises sustains a mature literature, yet relatively little evidence on the purchase of equity stakes is available, both in terms of its effect on firm results and of its influence on the economic structure. Geographically, most contributions rely on the Chinese experience (e.g. Wang et al. 2024). Empirically, the effects of state

[4]Situations where the government is "doomed to choose", for example having to select the location of an airport (Hausmann and Rodrik 2006).

[5]Place-based policies have been more thoroughly investigated in the Italian context. Among others, Cingano et al. (2025) study public subsidies (1996–2007), chiefly aimed at southern firms, finding positive results on employment partially clouded by political discretion; Incoronato and Lattanzio (2026) find industrial development zones in the South positively and persistently affected the density of economic activity.

ownership are chiefly studied through privatisations (see Megginson and Netter (2001) and Estrin et al. (2009) for reviews), typically concluding that transfers to private owners tend to raise efficiency and profitability. However, the conclusions we draw about the impact of state ownership might differ if we look at its end or at its beginning.

Gonzalez and Prem (2026) provide evidence on firm acquisition, showing that Allende-era takeovers in Chile sharply lowered firms' return on assets—yet nationalisations are very different from the purchase of an equity stake in the firm. In a related study, Bortolotti et al. (2024) study European firms (2000–2009) and find positive effects of government acquisition on innovation, driven by public minority stakes, due to relaxed financial constraints. However, unlike this paper, the study focuses only on publicly traded firms: whether these insights apply to more typical and less liquid enterprises is an open question.

Lastly, the active historical debate on the role of SHs as industrial conglomerates or as investment holdings (Amatori 2020) relates this paper to a broader literature studying the state as an investor—through banks, sovereign wealth funds, and government venture capital funds (GVCs).

Government ownership of banks is generally associated with slower financial and economic development (La Porta et al. 2002) and profound political distortions (Dinç 2005; Khwaja and Mian 2005). In Italy, Sapienza (2004) leverages Imita.db to show that state-owned banks favour large firms and depressed areas, charging rates that fall as the affiliated party's local electoral results improve. However, encouraging evidence emerges in relation to minority stakes by development banks (see Musacchio and Lazzarini (2014) for the Brazilian case). Sovereign wealth funds tend to target poorly-performing firms, with short-lived stock-price boosts that do not translate into durable improvements in performance (Kotter and Lel 2011) and may worsen it (Bortolotti et al. 2015). Conversely, Beraja et al. (2025) finds that Chinese GVCs effectively back firms with weaker ex-ante quality signals, or in peripheral provinces, and crowd in private capital, suggesting government financial intermediaries achieve a developmental role in the Chinese context.

In sum, the system of state holdings during the Italian economic miracle offers the opportunity to provide empirical evidence on state-owned enterprises as agents of structural economic transformation, and on the impact of state participation on non-publicly traded firms. The next section introduces the case and the rich, mostly qualitative, scholarship about it.

3 Context

3.1 The economic miracle

At the end of World War II, Italy was a comparatively poor and structurally backward economy: its per-capita income on the eve of the war stood at just under two-thirds of the Western European average and below half the United States level (Ciocca 2015). The economy was profoundly dualistic. Sectorally, agriculture still employed about 41 per cent of the male labour force in 1951 (Broadberry et al. 2011; Daniele and Malanima 2011); territorially, the South was lagging across economic dimensions (Felice 2007). However, the industrial core had regained pre-war output levels by the late 1940s (Crafts and Magnani 2013). Much of the country's heavy industry and its largest banks were, moreover, already in public hands, having been absorbed by the state holding IRI during the banking crisis of the early 1930s (Amatori 2020). What followed was the fastest sustained expansion in the country's history: between 1951 and 1973 output and labour productivity grew at roughly 5.8 per cent a year, accompanied by mass internal migration from the agrarian South to the industrial North (Broadberry et al. 2011). By the end of the 1970s the transformation was structural and largely complete. Agriculture's share of employment had fallen below one-fifth, industry had become the leading sector, and Italy—a founding participant in the Group of Seven summits from 1975—had substantially closed the income gap with its larger Western European neighbours (Broadberry et al. 2011; Daniele and Malanima 2011; Felice and Vecchi 2015). The evolution of the economic structure is depicted in Figure 1 at the province level, showing a general movement towards higher GDP per capita, a noticeable drop in the weight of agriculture in GDP, and a rapid hump-shaped increase in the weight of industry.

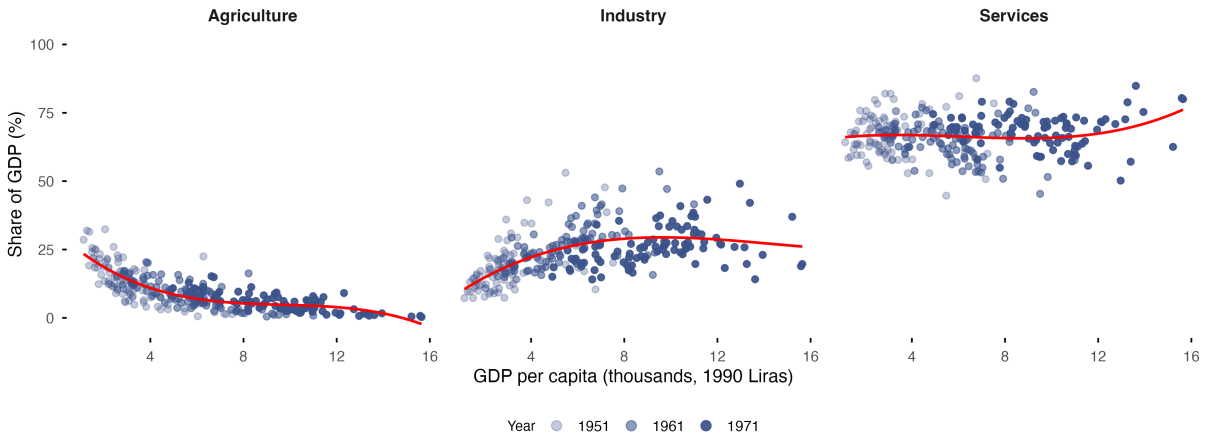


Figure 1: Structural transformation in Italy (1951–1971).

Data at the province level from Daniele and Malanima (2011), based on three population and industry censuses. The trends in the figure noisily replicate three of the canonical Kuznets facts (Herrendorf et al. 2014; Kuznets 1973) about structural transformation.

3.2 Economic policy and state holdings

With the Christian Democratic party at the helm of every ruling majority throughout the period (Ginsborg 1990), governments deployed multiple policies to influence the Italian economy. The dominant approach combined orthodox macroeconomic management (Toniolo 2013) and an actively developmental but market-compatible role for the state (Graziani 2000). The European Recovery Program aid financed reconstruction (Zamagni 1997; Bianchi and Giorcelli 2023) and membership of the European Economic Community (1957) underwrote increasing trade liberalisation (Crafts and Magnani 2013). Economic activity was characterised by highly concentrated ownership, pyramidal groups, a thin public stock market, and a weak market for corporate control, within which the state was a preeminent owner (Barca 1994; Amatori and Barca 2010).

The state-holding system was administered through several public management entities, but three accounted for almost all of its economic weight (Toninelli and Vasta 2011) and are the focus of this study. By far the largest and oldest was IRI (*Istituto per la Ricostruzione Industriale*, Institute for Industrial Reconstruction) created in 1933 to rescue Italy's three major banks by absorbing their distressed industrial shareholdings. After the war, it was repurposed as a standing instrument of industrial policy (Amatori 2020; Ciocca 2015). The public-law entity at the top of the pyramid controlled private-law sectoral sub-holdings and several major players directly (e.g. the motorway concessionaire, the national air carrier). By the early 1980s, IRI employed nearly 600,000 workers and was present across the bulk of national steel output, the telephone network, most of domestic shipbuilding, and more (Ciocca 2015; Amatori 2020; Gasperin 2022). ENI (*Ente Nazionale Idrocarburi*, National Hydrocarbons Board) was created in 1953 and built into a vertically integrated energy group active in exploration, pipelines, petrochemicals, and oilfield services, with the objective of reducing the country's reliance on energy imports (Ciocca 2015; Toniolo 2013). EFIM (*Ente partecipazioni e Finanziamento Industrie Manifatturiere*, Manufacturing Industries Participation and Financing Board), much smaller, was concentrated in aluminium, engineering, defence, and glass (Toninelli and Vasta 2011). Together these three holdings controlled between 17.9 and 29.7 per cent of the share capital of all Italian joint-stock companies across the period, having gained most of that share through the 1950s and 1960s (Toninelli and Vasta 2011, p. 83).

The holdings were instruments of structural transformation, charged with reshaping the sectoral and territorial composition of the economy rather than with managing an inherited portfolio on financial criteria alone (Saraceno 1975; Felisini 2013; Ciocca 2015).

Before a dedicated ministry existed, the holdings were governed through a model that featured a sharp separation between the public-law holding, which set strategy and answered to the government, and the private-law operating companies, which were run as ordinary firms on commercial criteria (Amatori 2020). This design gave

operating management substantial autonomy and, crucially, allowed the holdings to recruit technically competent managers outside the constraints and pay scales of the civil service. Early economic and business history scholarship identified this model as a distinctive "IRI formula" (Holland 1972; Posner and Woolf 1967). The creation of the Ministry of State Holdings (*Ministero delle Partecipazioni Statali*, PPSS) in 1957 formalised the previously fragmented oversight in a three-tier chain: the government set strategic guidelines, the PPSS issued directives and supervised the state holdings (SHs), which in turn controlled the operating companies directly or through sectoral sub-holdings (Posner and Woolf 1967; Holland 1972; Ciocca 2015).

The system was thus meant to combine political direction with managerial autonomy, a balance codified in Saraceno's (1975) principle that operating firms should be run according to profit-oriented management, while the non-commercial aims[6] of territorial rebalancing and job creation were compensated by the state through capital transfers (Felisini 2013).

3.3 A contested nexus

Scholars have long been divided on the developmental contribution of the SHs. Some argue distortions prevailed: holdings degenerated from inception into an instrument of patronage, managed by parcelled-out political appointees and fuelled by swelling public debt. Unable to "let losers go" (Juhász et al. 2024, p. 218) the holdings preserved employment in unviable firms and plants (Barca and Trento 1997; Conte and Piluso 2013), in a prime example of politically driven misallocation (Shleifer and Vishny 1994) enabled by a soft budget constraint (Kornai 1986). In the words of Barca and Trento (1997), the holdings "betrayed their mission".

Others identify the holdings as major drivers of industrial development, at least into the 1960s, operating chiefly through three channels: the provision of long-term, patient capital in an economy whose thin stock market and bank-centred finance limited private access to investment funds, with public-enterprise investment rates running at more than double the economy-wide average (Posner and Woolf 1967; Barca 1994; Pastorelli 2006); the injection of technically competent management; and consequent technological upgrading (Holland 1972; Posner and Woolf 1967; Felice 2010). Examples ranged from the integrated full-cycle steelworks at Cornigliano and Taranto (Ranieri and Romeo 2015) to internationally competitive aerospace, space, and electronics divisions (Felice 2010). Organisationally, IRI's operation as a genuinely integrated industrial group provided two advantages to participated firms: the group had its own research and engineering capabilities (Gasperin 2022), and it leveraged vertical integration to supply intermediate

[6] *Oneri impropri*, literally improper burdens.

inputs at a scale and price that enabled and coordinated downstream private investment (Amatori 2020; Ciocca 2015).

This paper focuses on the first period of activity of the holdings for which comprehensive data are available (1946–1971) and applies modern econometric techniques to assess their effect on firms and sectors. The following section describes the data enabling the quantitative analysis.

4 Data

The analysis relies on a long panel of firms, broadly representative of larger firms in the post-war Italian economy. I then leverage further archival work performed on the basis of the panel to trace networks of corporate ownership and map the extent of the SHs' shareholdings. After introducing these two main sources, I discuss their important limitations.

4.1 Sources

Panel of firms The backbone of the analysis is Imita.db (*IMprese ITAliane Data Base*), a relational database that digitises the *Notizie statistiche*, a serial record of Italian joint-stock companies (*società per azioni*) compiled from 1894 onwards, first by Credito Italiano and, from 1928, by the joint-stock companies' association ASIPA, and reconstructed in machine-readable form by Vasta (2006). Each volume reported every company quoted on an Italian stock exchange together with all joint-stock firms whose share capital exceeded a varying and relatively low reporting threshold (Pellegrino 2024). The database is organised around nine benchmark years (1911, 1913, 1921, 1927, 1936, 1952, 1960, 1971, and 1982) at which firm characteristics and boards are recorded exhaustively, while the balance-sheet archive is annual, spanning every year from 1900 to 1971 with the exception of the war years 1940–45. The balance sheet data is collected starting from firms mapped in benchmark years and moving backwards to the previous benchmark. This structure is represented in Figure 2.

Since virtually all large companies in Italy were incorporated as joint-stock firms in the period (Vasta 2006; Pellegrino 2024), the coverage of the panel allows us to consider it a census of the larger end of the Italian corporate universe.

The period of analysis begins in 1946 and ends in 1971 due to balance sheet data availability: a further benchmark year was collected (1982), but the previous balance sheets are not reported in the dataset.

While the growth in assets shown in Figure A1 is staggering, it is relatively similar to the growth experienced by the whole economy during the 25 years examined, as shown by Panel A2a. Moreover, Panel A2 shows that, within the period of analysis, the count of

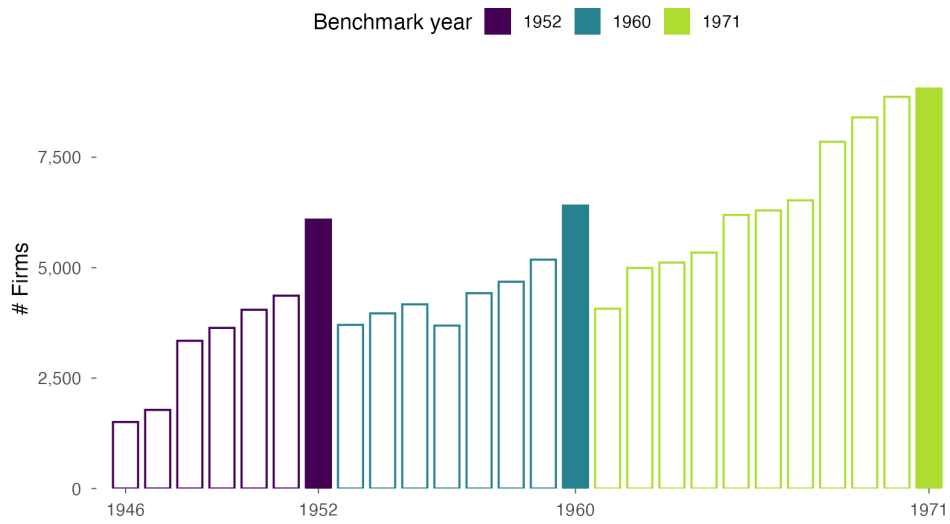


Figure 2: Benchmark structure of Imita.db, 1946–1971

Number of distinct firms observed in the sample in each year between 1946 and 1971. Solid bars mark the three benchmark years falling in the period of analysis (1952, 1960, and 1971), at which firms are recorded exhaustively. Outlined bars are the intervening years, each coloured after the benchmark year from which its balance sheets were collected backwards (1946–51 from 1952, 1953–59 from 1960, 1961–70 from 1971).

joint-stock firms in benchmark years is low relative to the universe, yet wide coverage in terms of size is achieved. The share capital in sample varies from 92.6% of the universe in 1971 to 97.6% in 1952 (Vasta 2006).

Combined, these two elements suggest that the dataset is well suited to capture trends related to the universe of large firms in the Italian economy.

For each firm-year the source reports the principal items of a standard balance sheet, with a limited degree of missingness. Share capital, assets, debt, and profits or losses are populated in almost all firm-years.[7] From these I construct the outcome variables used throughout: assets, share capital, debt, and net result. Based on them, I derive ratios: ROA, ROE, and debt-over-assets leverage. To study profitability, I chiefly rely on ROA, as the ROE calculated on share capital neglects other sources of equity and is thus a less informative measure.

Each company additionally carries a set of time-invariant attributes: a foundation year, the location of its head office, and a line of business, which I harmonise across benchmark years to NACE Rev. 1.1 (implemented in Italy as the ATECO 2002 classification) and aggregate to the division, group, and class levels (2-, 3- and 4-digits, respectively) for sectoral analyses. A persistent firm identifier follows each company across changes of name and across benchmark years, so that the successive annual balance sheets can be assembled into a continuous firm-level panel.

[7] The most missing item, net result, is missing from 2.94% of observations. Its missingness is not correlated with treatment.

State ownership *Notizie statistiche* records each company's capital and accounts but not the identity of its shareholders. To place the SHs within it I draw on the mapping of the three holdings' shareholdings assembled by Toninelli and Vasta (2011), who matched Imita.db against the holdings' own records, namely the consolidated balance sheets, yearbooks and board reports of the IRI and ENI archives, supplemented by the standard investors' handbooks of the period. For five benchmark years (1936, 1952, 1960, 1971 and 1982) they identify, firm by firm, the stakes held by each holding both directly and through its sub-holdings and other group companies. This reconstructs the pyramidal chains through which IRI, ENI and EFIM controlled large parts of the economy.

I leverage Toninelli and Vasta's measure of control, which they define as the *accounting* share, in accordance with the corporate-ownership literature (La Porta et al. 1999). This multiplies out shareholdings algebraically along each chain, filtering the weight of SHs down to the operating firms. The study also compiles *effective* control, which counts any accounting stake above 20%; *majority* control, which requires more than 50%; and *pyramidal* control, which follows chains of majority stakes. To run the most demanding test, I classify firms with any degree of state ownership as state-participated.

4.2 Scope and limitations

The resulting database traces the evolution of an unbalanced panel of 15,765 individual firms across 25 years, among which 361 experience state participation.

Imita.db is the most comprehensive source on firms' long-term performance in Italy, yet its design implies two key limitations. First, firms inactive during benchmark years are not observed in the dataset. Second, balance sheets were not collected backwards from the benchmark year 1982. This constrains the analysis to the furthest post-war mapped year available, i.e. 1971.

Moreover, the dataset disregards economic operators not incorporated as joint-stock firms. An overwhelming majority of mid- to large-sized enterprises in Italy have historically adopted this legal form (Pellegrino 2024), hence this constrains the validity of the analysis to relatively large enterprises.

The methodology of Toninelli and Vasta (2011) also limits the accuracy with which I can represent state ownership, both in terms of target firms and of chronology.

The most problematic limitation relates to the timing of the acquisitions. Toninelli and Vasta (2011) do not date individual acquisitions: they reconstruct, firm by firm, which stakes trace back to the holdings only at the five benchmark years (1936, 1952, 1960, 1971, and 1982), so the data records a membership snapshot at each benchmark rather than a transaction log. Ownership is therefore observed as a step function on a coarse and uneven grid, and an acquisition can only be bracketed to the interval between two benchmarks, not placed in a specific year. In practice, I take the benchmark at which a

firm first appears as state-owned to be its acquisition year, which is an upper bound on the true date; a firm acquired shortly after one benchmark is thus mistakenly treated as private for several intervening years.

Three problems of more limited consequence are also present. First, the dataset only focuses on participations by SHs. However, as noted in Section 3, these constituted the overwhelming majority of ownership exercised by the state at the national level (Toninelli and Vasta 2011). Municipalities also held participations in public service firms, yet they lacked the structural transformation aims which form the subject of this paper.[8]

Second, it does not track corporate control layers beyond the third.[9] Nonetheless, the omission is likely second-order as relatively few and small firms tend to be located from the fourth step onwards of the control ladder (Toninelli and Vasta 2011).

Finally, it is possible that a holding might own another entity which is not a joint-stock firm, and which in turn owns a joint-stock firm within the database. In this case, the data would misclassify the latter as fully private. The extent of the problem, however, is likely to be limited due to the same reasons listed above. Additionally, in a strictly empirical sense, this type of misclassification would bias the effect of state participation downwards.

While misclassifying observed firms dampens the potential effect of participation, this snapshot structure introduces measurement error with three more relevant empirical implications. Before treatment, it makes it harder to identify pre-trends, as these might be due to early acquisitions within the benchmark interval. After treatment, it is impossible to draw accurate conclusions on the time required for the onset of any effects. Additionally, short-lived spells of state ownership are missed entirely, so the mapping understates the churn of acquisitions and dismissals that the holdings may have generated. This might bias effects in a more positive direction if, contrary to what the literature suggests (Juhász et al. 2024, p. 218), SHs divested from bad acquisitions quickly.

In spite of these limitations, the data enables an analysis of the effect of state participation in large firms, complementing the thin large-N evidence produced so far. In the following section, I present some descriptive facts about the sample.

[8] Regions, which might have had the scope to attempt transforming their economies, received substantive powers only in 1970.

[9] Suppose IRI has a stake in firm X, which in turn has one in firm Y, which has one in firm Z, which has one in firm W. All firms besides W would be assigned a degree of SH participation.

5 Descriptives

Preliminary inquiry into the dynamics of state participation reveals the large footprint of state holdings in the period, driven by the participation of large firms in key industrial macro-sectors such as manufacturing and utilities.

Corporate control Figure 3 shows participation trends in the sample by level of share capital control. A relatively limited percentage of firms accounts for up to 34% of the total assets in the sample. At the end of the period, the total assets of participated firms exceed 25% of national GDP (see Panel A2a for reference). The dataset design is again evident in the data: benchmark years record additional participations, with a sizeable enlargement in 1971, for which no further balance sheet data are available.

State holdings participate in firms as minority or majority shareholders in roughly equal proportions at the beginning of the period; minority positions then experience a slight relative decline, driven by stakes smaller than 25% of the share capital.

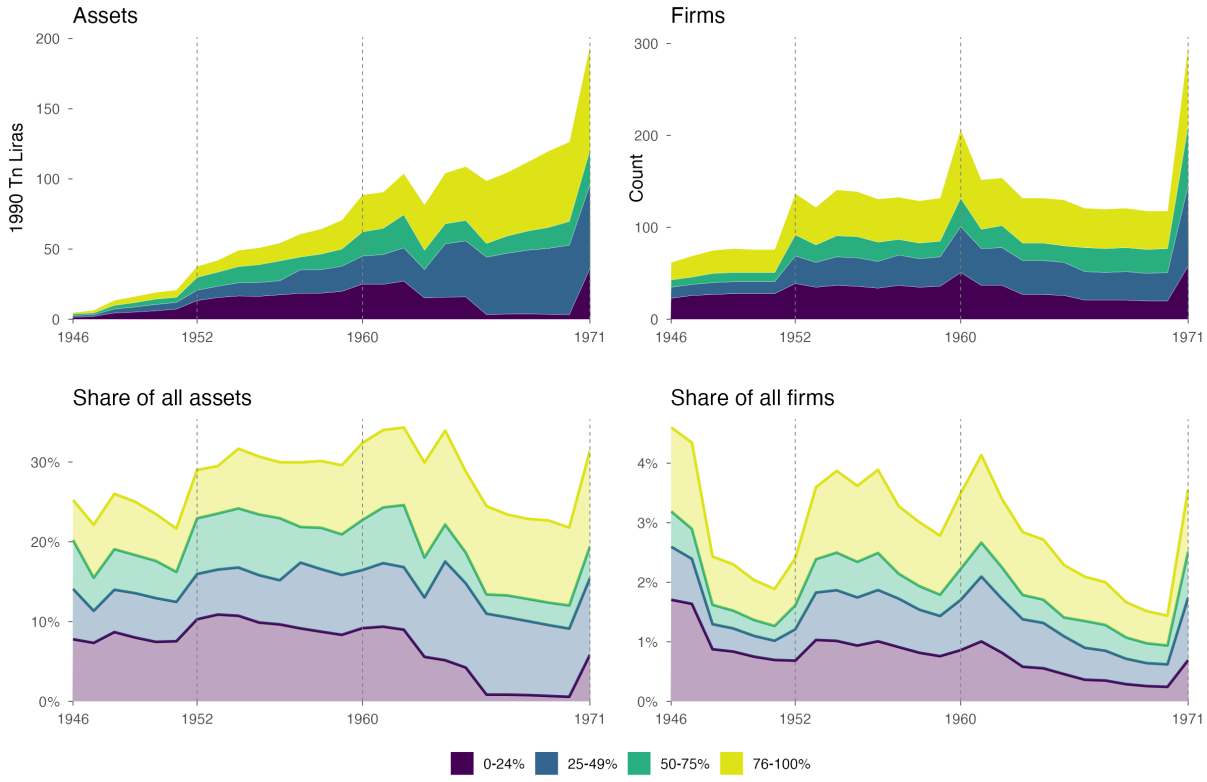


Figure 3: State holdings' participations by share capital control

Participations by SHs over 1946–1971, grouped by the state's accounting equity stake at the firm, in four brackets (0–24%, 25–49%, 50–75%, 76–100%); a firm counts as participated from the year it first appears as state-held onward, as in Equation 1. The top row reports levels as solid stacked areas: (left) participated firms' assets, summed and expressed in trillions of 1990 lire, and (right) the count of participated firms. The bottom row stacks each bracket's share of all sample assets (left) and all sample firms (right), so the top of each stack reads as total state participation. Vertical dashed lines mark the ownership-mapping benchmark years (1952, 1960, and 1971). Section J (financial intermediation) is excluded throughout.

Participation drivers How do target firms differ from their counterparts? Figure 4 compares never-treated firms with treated firms before and after treatment. The densities show that participation targets tend to be larger in terms of share capital, assets, and debt ahead of participation. These distributions shift further to the right in the participation period, foreshadowing the strong results on size outlined in Section 6.

Differences in profitability, as measured by raw profits and their ratios to share capital and assets, display less evident patterns. Notably, the ROA distribution of participated firms is thinner on the positive tail, suggesting that less profitable firms are more likely to be targeted by SHs. This hypothesis will be investigated in Section 6.3.2.

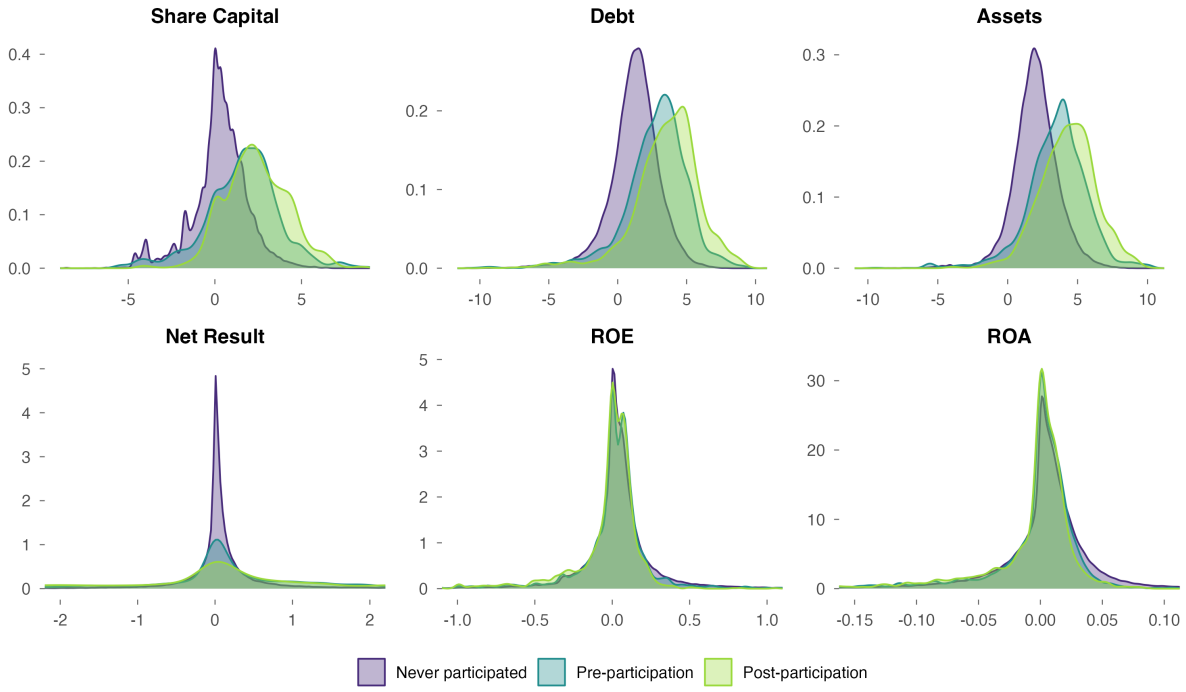


Figure 4: Firm characteristics by treatment status

Densities of firm-years by key balance sheet characteristics, split by state-participation status: never-participated and participated firm-years before and after acquisition. Share capital, debt, and assets are in natural logarithms; the net result in inverse hyperbolic sine, and ROE (net result over share capital) and ROA (over assets) winsorized at the top and bottom 0.5%. The sample spans 1946–1971 and excludes section J (financial intermediaries).

Sectoral distribution Figure 5 reports the allocation of SHs' participations across 1-digit macro-sectors. The distribution is consistent with the structural transformation aims described by historians and synthesised in Section 3: the overwhelming majority of state-holding involvements are targeted at the four macro-sectors of manufacturing, transport and communication, utilities, and mining, both in terms of firm size and number. Investigating the intensity of participation—i.e. the portion of assets or firms

participated in each sector—reveals a more nuanced picture. While the four key industrial sectors are also intensely participated, smaller non-industrial sectors reveal high percentages of state involvement (e.g. hospitality, agriculture and forestry).

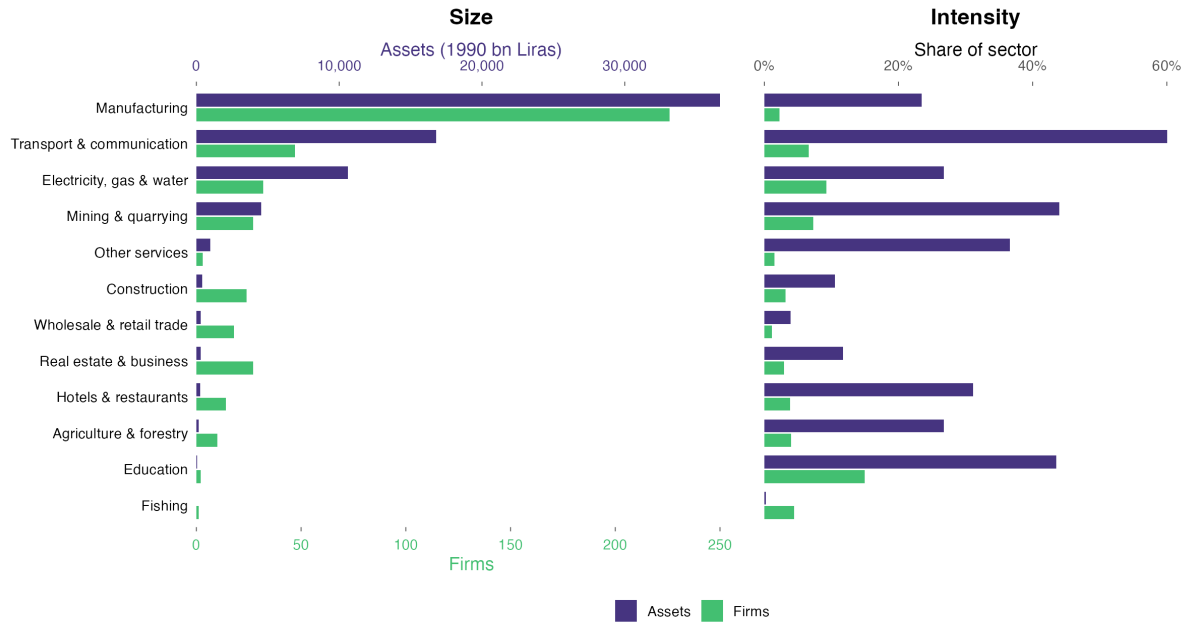


Figure 5: Participated firms and assets by 1-digit sector

Participations by SHs across one-digit NACE sectors (*sezione*) averaged over 1946–1971. A firm counts as participated from the year it first appears as state-held. The left panel ("Size") reports absolute magnitudes against two axes: the sector's participated assets, averaged across years and expressed in billions of 1990 lire (top axis), and its number of distinct participated firms (bottom axis). The right panel ("Intensity") reports the within-sector footprint of participation on a single axis: participated assets or firms as a share of the whole sector. The sample excludes section J (financial intermediaries).

Figure A4 represents the evolution of the sectoral mix over time. Participation in manufacturing remains stable above 20% of sectoral assets throughout the period, indicating that participations grew, in size or number, at a similar pace to the overall sector. Transport and communication were equally stable, albeit above 60%, driven by the weight of the national highway operator. Involvement in mining and quarrying sharply increases around 1954 with the creation of ENI, which intervened in a sector that was at the time small in absolute size. Conversely, utilities experience significant divestment relative to the size of the sector in the sample in the last part of the period.

Overall, preliminary descriptive analysis reveals the staggering size of SHs' involvement in key industries, privileging majority participations in very large enterprises. The following section analytically investigates the effect of such involvement on the performance of firms.

6 Firm-level Results

What happens to firms when they are participated by state holdings in this context? Given some time, they increase in size and, to a lesser extent, in profitability. This section will outline these firm-level findings, show their robustness, and offer suggestive evidence on the mechanisms driving them.

6.1 TWFE and Staggered DD Models

First, I estimate the firm-level effect of state participation with a two-way fixed-effects model in the following preferred specification. For firm i in year t, let $SP_{it} = \mathbb{1}\{t \geq \tau_i\}$ be a dummy indicating that i reports any level of state participation, where τ_i is the year at which the firm first appears as participated; the indicator is absorbing, switching and remaining on at τ_i. The simple estimating equation is therefore

$$y_{it} = \beta\, SP_{it} + \alpha_i + \delta_t + \varepsilon_{it}, \tag{1}$$

where α_i and δ_t are firm and year fixed effects and standard errors are clustered at the firm level. Consequently, β is identified from within-firm variation after acquisition, relative to the contemporaneous path of never- and not-yet-participated firms. The outcome y_{it} enters as the natural logarithm of equity, assets, and debt (on strictly positive observations) and as the inverse hyperbolic sine of the net result, so that β reads as an approximate percentage change; the return ratios are percentages winsorized at the top and bottom 0.5%. The sample excludes the few firms first recorded as state-owned at the out-of-sample 1936 benchmark year. Table 1 reports the estimates of β. As discussed in Section 6.2, the results are robust to different specifications and to employing the staggered DD framework by Callaway and Sant'Anna (2021).

As anticipated, state participation radically increases firms' size. Specifically, their share capital, debt, and assets become larger by 24, 87, and 57% after participation, on average. Because the typical participated firm-year is observed about six years after acquisition, the findings indicate a yearly differential growth of 3.6% for share capital, 10.8% for debt, and 7.7% for assets, respectively, relative to control-group firms.[10] No clear effect on profits, by far the most volatile of the items, emerges from the analysis. However, when divided by share capital (ROE) or assets (ROA), the regressions recover a positive effect of 11.6 and 0.8 percentage points on returns.

These effects are sizeable. A comparison can be drawn with the impact on firms of acquisition by private equity (PE) funds. While the two types of acquisitions are very

[10] Reading the gap as accruing linearly from acquisition, $\beta \approx c\,\bar{s}$, where $\bar{s} \approx 6$ is the mean number of years since acquisition of the participated firm-year. Hence, the implied yearly rate is $e^{\beta/\bar{s}} - 1$. Considering that acquisitions predate the benchmark year at which they are first recorded, these yearly rates are an upper bound.

Table 1: State participation and firms' balance sheet items

	Share Capital (1)	Debt (2)	Assets (3)	Net Result (4)	ROE (5)	ROA (6)
SP	0.219**	0.626***	0.452***	0.007	0.116***	0.008***
	(0.088)	(0.124)	(0.106)	(0.086)	(0.029)	(0.002)
Firm FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Dependent variable mean	0.38	1.27	2.13	0.12	0.05	0.00
Observations	131,530	127,925	129,709	125,910	125,849	125,863
Adjusted R^2	0.76	0.79	0.85	0.56	0.30	0.41
F-test	2,247.34	2,281.09	3,476.25	830.97	323.00	483.51

***, **, and * indicate $p < .01$, $p < .05$, and $p < .1$ significance levels, respectively. Standard errors are clustered at the firm level. State participation captures any extent of state share acquisition. Balance-sheet raw items are transformed for interpretability and to account for outliers: share capital, debt, and assets enter in logs (estimated on strictly positive observations only) and Net Result enters in inverse hyperbolic sine (IHS) to accommodate negative values. ROE, ROA are winsorized at the top and bottom 0.5%. The sample excludes firms acquired in 1936. Section 6.2 shows the estimates are widely robust to alternative outlier treatment, raw values, and specifications.

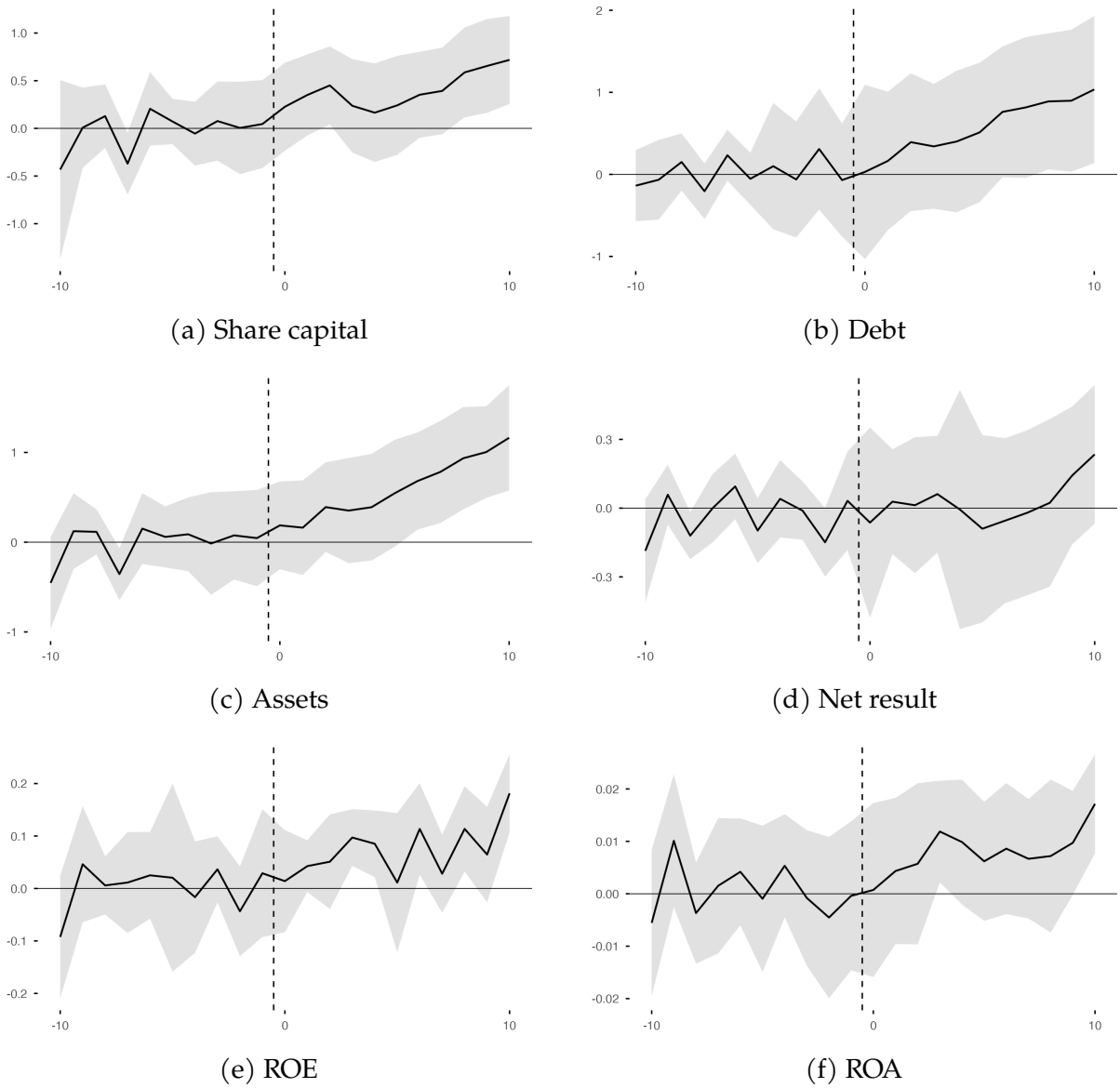
different, PE funds offer the chance of scaling by relaxing financial borrowing constraints (Boucly et al. 2011). This is relevant to state participation, as it is likely that one of its major effects will be derisking the target company.

I sketch a comparison based on two key studies on leveraged buy-outs (LBOs). Cohn et al. (2014) find a 30% increase in target firms' assets in the first two years after acquisition, and stable size afterwards. They estimate no improvement in profitability. Boucly et al. (2011) analyse French leveraged buy-outs by private equity funds and locate the average increase in asset size at 12% in a four-year window, while profitability sits at 4.4%. Projecting the trends implied by Boucly et al.'s findings linearly onto this study's six-year average treatment span, they would imply an 18% increase in assets and a 6.6% increase in profitability, which lands between Table 1's estimates for ROE and ROA.

While important caveats apply to the comparability of these studies, these back-of-the-envelope calculations suggest that state participation increases target firms' size much more than LBOs, while improving their profitability to a lesser extent.

These effects might be the result of positive pre-trends characterising target firms prior to state participation. I now present event studies (Callaway and Sant'Anna 2021) to inquire into the presence of such pre-trends and offer a clearer idea of treatment onset, bearing in mind that treatment time in this data is an upper bound, and actual participation might have started earlier (see Section 4.2 for details).

Visual inspection rules out the presence of meaningful pre-trends. Post-treatment increases are clear for share capital, debt, and assets (Panels 6a to 6c), more muted and lagging for returns (6e and 6f), absent for net results (6d). Overall, these event studies confirm the findings in Table 1, and suggest a relatively long time for treatment effect



(a) Share capital

(b) Debt

(c) Assets

(d) Net result

(e) ROE

(f) ROA

Figure 6: Difference-in-differences event studies (Callaway and Sant'Anna 2021)

Studies on the outcomes of Table 1. Each panel plots yearly coefficient estimates for SP, with 95% confidence bands; the vertical dashed line marks the last pre-participation year. Never-treated firms form the control group. As reported in Columns 1 and 2 of Table A3, all post-treatment estimates are jointly significant, and all pre-treatment ones are not, with the exception of Panel 6d.

onset across outcomes. Reassuringly, the yearly estimates are broadly consistent with the back-of-the-envelope calculations performed above, both in magnitude and linearity.

6.2 Robustness

These findings are robust to a wide range of alternative specifications and outlier corrections. Most importantly, they are robust to accounting for survivor bias by including only firms observed at the next benchmark year in the analysis, and to propensity-score matching to ensure the comparability between treated and non-treated firms (Sant'Anna

and Zhao 2020; Callaway and Sant'Anna 2021). Since the interpretation of the models rests on SUTVA (Imbens and Rubin 2015), evidence in support of it is reported in Section 7.2, which is concerned with sectoral spillovers.

First, Table A4 reports SP coefficients estimated according to specification 1 varying the handling of outliers. This is necessary due to the extreme tails in the three balance sheet items (see Table A1). Results on ROE and ROA lose significance when estimated on raw values. This is likely due to the volatility of net results, assets, and share capital, combined with the limited magnitude of the effect. All previously significant results remain unaffected throughout, with a slight positive trend emerging for the net result in Column 5, consistent with the positive results on returns.

Table A5 reports SP coefficients estimated with variations of specification 1. The first column reinstates the cohort of firms first recorded as state-participated in 1936, excluded elsewhere: even without contributing identifying variation—they are always treated in the analysis period—they may influence estimates through year fixed effects nonetheless. Estimates remain virtually unchanged. Columns 2 to 5 absorb sector-level yearly fixed effects and time trends, marginally reducing the magnitude of the estimates without affecting their significance.

Column 6 performs a key robustness check related to survivor bias. As balance sheets are collected backwards from the benchmark censuses (Section 4.1), a firm's post-acquisition accounts are observed only if it resurfaces at a later benchmark. Every participated firm-year in the sample is therefore conditional on the firm surviving to the next census. Were SHs routinely to wind down the firms they acquire, these failures would disappear before their poor outcomes could be recorded, and the estimates of Table 1 would reflect only the performance of survivors, overstating the effect of participation. Column 6 of Table A5 guards against this possibility by re-estimating the preferred specification of Equation 1 only on firms observed at the next benchmark year, so any residual effect cannot be an artefact of survivor bias. The results are reassuring: the effects on share capital, debt, and assets remain positive and significant, while the estimates on profitability, if anything, strengthen—the net result, insignificant in the main specification, turns positive and significant, and the coefficients on ROE and ROA grow larger. Although the smaller sample weakens the significance of the size estimates, restricting the comparison to survivors thus leaves the core findings in place, indicating that differential attrition of participated firms does not drive them.

I additionally report a variety of DD specifications based on Callaway and Sant'Anna (2021) in Table A3. Columns 1 and 2 report the joint pre-treatment ATT and the post-treatment ATT across cohorts: these synthesise the event-study findings already presented in Figure 6. The sole significant pre-trend is in net result and it is negative, which would be consistent with SHs targeting underperforming firms. This is investigated further in the following section.

Columns 3 to 5 perform another key robustness test related to the comparability of treated and control firms. State-participated firms are not a random draw from the corporate universe: they are systematically larger than never-participated ones, so comparing the two groups risks attributing to participation an effect that is the result of different characteristics interacting differently with confounders in the post-treatment period. For example, larger firms might be systematically better able to internationalise, and reap the benefits of the opening of Italy to the European Economic Community in 1957.

The literature usually meets such imbalance by matching treated units to comparable controls, and the Callaway and Sant'Anna (2021) estimator performs an analogous correction inside its own machinery, when provided with pre-treatment covariates. Building on Sant'Anna and Zhao (2020), it estimates each cohort-time effect with a doubly-robust estimator that, for participation cohort g and year t, takes the form

$$\mathrm{ATT}_t^g = \mathbb{E}\left[\left(\frac{T_i^g}{\mathbb{E}[T_i^g]} - \frac{C_i \pi_i^g}{\mathbb{E}[C_i \pi_i^g]}\right)\left(y_{it} - y_i^{g-1} - m_t^g(X_i)\right)\right]. \tag{2}$$

The central element is $p_g(X_i) \equiv \mathbb{E}[T_i^g \mid X_i,\, T_i^g + C_i = 1]$, i.e. a logit propensity score for belonging to cohort g estimated on pre-treatment covariates X_i for each firm i. From it, the odds of being treated in cohort g are $\pi_i^g = \dfrac{p_g(X_i)}{1 - p_g(X_i)}$. $T_i^g = \mathbb{1}\{\tau_i = g\}$ flags firms first participated in g, while C_i flags never-participated firms. Finally, $m_t^g(X_i) \equiv \mathbb{E}[y_{it} - y_i^{g-1} \mid X_i,\, C_i = 1]$ is an OLS regression of the outcome change over the never-participated firms.

The second term in the first factor reweights controls based on π_i^g, so that never-participated firms resembling the participated ones count for more, while $m_t^g(X)$ nets out the change that firm characteristics alone would predict. The comparison is thereby purged of the differential impact observable heterogeneity might have in the post-treatment period, matching acquired firms to similar private ones rather than to the never-participated pool as an undifferentiated whole. It is doubly robust in that consistency requires only one of its two ingredients—the propensity model $p_g(X_i)$ or the outcome regression $m_t^g(X_i)$—to be correctly specified: if the reweighting is misspecified the regression still recovers the counterfactual, and if the regression is misspecified the reweighting corrects it, so that the estimate is biased only when both fail.

Columns 3 to 5 progressively enrich this covariate vector, adding pre-treatment (i.e. dataset entry-year) variables for size in assets, then leverage (debt over assets), then profitability. The size results are, if anything, reinforced: the effect on share capital, debt, and assets stays positive and significant and, comparing firms of similar initial scale, is estimated more precisely and somewhat larger. The returns are more nuanced. ROE and ROA are essentially unmoved by matching on size and leverage, but attenuate once pre-treatment profitability enters the covariate set. This hints at the standardisation

mechanism discussed below, whereby part of the measured gain reflects convergence from the low base at which underperformers are acquired. Holding observable heterogeneity fixed thus leaves the central conclusions intact and leads into the discussion of mechanisms.

6.3 Mechanisms

I now inquire into the reasons behind a large increase in size and a modest increase in profitability after state participation. However, I must preliminarily note that a time-varying, unobserved confounder might be determining both participation choices by SHs and the observed changes. Policymakers and managers at the time might have been aware of systematic elements that do not appear in pre-trends and are not captured by fixed effects.

6.3.1 Size

If participation is affecting the accumulation of firms' assets, the most straightforward explanations for the increase in size both pass through participated firms' ability to take on debt more easily. Moreover, while in some cases state involvement might spell bad news for the firm's efficiency, it likely boosts its solvency, for both economic and political reasons. As extensively noted in the literature, it is hard for governments to let losers go without suffering political backlash (Juhász et al. 2024, p. 218). Investors might thus anticipate that the state will offer exceptionally strong financial backing to firms (derisking effect)—conditional, that is, on the solvency of the state itself, which in the present case was not in question. This is consistent with Colonnelli et al. (2024, p. 218) finding that potential VC investors in China are more attracted by entities already invested in by government investors, and with Khwaja and Mian (2005), who show that "political" firms receive more credit in Pakistan.

Moreover, state participation signals to private investors that SHs have bet on a given firm among many. If firms need to clear a certain threshold of financial support to viably scale and ensure returns, investors might find it reasonable to coordinate on state-invested firms, knowing that this signal of state commitment is public (focal point effect). This crowding-in might be particularly plausible in a setting with a thin financial market (Amatori and Barca 2010).

I provide some descriptive evidence consistent with these two interpretations.

Derisking First, Column 2 of Table 1 reports a staggering increase in debt under participation. Additionally, Table A7 shows that current debt weakly but persistently predicts asset growth in subsequent years. This implies debt might be helping participated firms grow in size.

Lacking data on loan conditions, bond covenants, or debt service, I cannot directly test for the presence of derisking, which would be consistent with cheaper credit being extended to participated firms. Table 2 nonetheless shows that firms notably increase their leverage once SHs take a stake, which is consistent with state backing allowing them to sustain higher debt ratios. Certainly, the same increase could reflect a softened budget constraint, whereby state participation loosens market discipline and lets firms accumulate debt that would otherwise signal or produce distress; under this reading, rising leverage is a symptom of SHs absorbing losses rather than evidence of derisked, viable credit sustaining growth. The limited positive elasticity of future assets on current debt displayed in Table A7 suggests both mechanisms might be at work.

Table 2: State participation and leverage

	Debt/Assets			
	(1)	(2)	(3)	(4)
SP	0.042**	-0.004	0.079***	0.064***
	(0.018)	(0.017)	(0.017)	(0.017)
Firm FE		✓	✓	✓
Year FE			✓	
Subsection × Year FE				✓
Dependent variable mean	0.51	0.51	0.51	0.51
Observations	129,130	129,130	129,130	129,130
R^2	0.00	0.65	0.68	0.68
Adjusted R^2	0.00	0.60	0.64	0.64

***, **, and * indicate $p < .01$, $p < .05$, and $p < .1$ significance levels, respectively. Standard errors are clustered at the firm level. SP (State Participation) equals one once a firm is state-acquired. The outcome is the debt-over-assets ratio, winsorized at the top and bottom 0.5%. The sample is 1946–1974 and excludes firms acquired in 1936.

Focal point Intuitively, a singular coordination target should be more effective in fostering coordination than multiple targets. Hence, if representing a target to coordinate on for private investors is an advantage for participated firms, crowding-in should be reduced as SHs participate in more firms in the same economic sector. To test this conjecture, I compute a measure of state saturation, i.e. the share of firms held by a state holding in the same 3-digit sector as each participated firm at the start of the participation. I then interact it with the participation dummy. The results in Table 3 show that, as state saturation increases, the size increase diminishes. This is compatible with the focal point effect fading as SHs own more firms in the same sector. Note that, by acquiring more firms in the same sectors, SHs might also be more able to transfer

expertise, offering a countervailing positive channel of influence. The lack of effects on return ratios, however, does not support this channel.

Table 3: State participation and state sectoral saturation (TWFE, 1945–75)

	Share Capital (1)	Debt (2)	Assets (3)	Net Result (4)	ROE (5)	ROA (6)
SP	0.326***	0.810***	0.623***	0.037	0.102***	0.008***
	(0.107)	(0.141)	(0.126)	(0.104)	(0.032)	(0.003)
× State Saturation	-0.138***	-0.164***	-0.178***	-0.008	-0.003	-0.001
	(0.039)	(0.049)	(0.042)	(0.058)	(0.012)	(0.002)
Firm FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Dependent variable mean	0.37	1.26	2.12	0.11	0.06	0.00
Observations	130,409	126,819	128,594	124,906	124,845	124,859
Adjusted R^2	0.76	0.79	0.85	0.56	0.30	0.41
F-test	2,137.75	2,175.69	3,323.79	790.11	309.56	461.34

***, **, and * indicate $p < .01$, $p < .05$, and $p < .1$ significance levels, respectively. Standard errors are clustered at the firm level. This is the preferred firm-level specification (Table 1) interacted with *State Saturation*: the share of firms in the target's 3-digit sector already held by any state holding (IRI, ENI or EFIM, with an earlier acquisition year) over all firms in the sector, measured in the cross-section of the acquisition year. Unlike a holding-specific measure of the same exposure (the acquiring holding's own prior footprint in the sector, not reported), saturation counts the state's total sectoral footprint, regardless of which holding owns each firm. The measure is standardized, so the interaction is the additional effect per one-standard-deviation increase in saturation; it enters only through the interaction, leaving never-treated and pre-acquisition firm-years unaffected. A negative interaction on the size outcomes is consistent with the focal-point (coordination) advantage fading as the state offers more targets in the same sector. Sectoral participation is observed only over firms with a balance sheet, so the measure is a lower bound. The sample excludes firms acquired in 1936.

6.3.2 Profitability

While an increase in size is far from surprising, an increase in profitability conflicts with the traditional narrative around state-owned enterprises. I offer descriptive evidence consistent with a "standardisation" story: once participated, firms converge to a middling level of profitability, as measured by their ability to produce profits given their assets. This implies that high performers will lose out from state participation, while poor performers will gain. Since SHs tend to acquire underperformers, this nets out to an overall improvement in profitability. This targeting behaviour would also be consistent with SHs anticipating their effect on firms' performance while noisily identifying underperformers—or acquiring overperformers for non-economic reasons. I now present evidence consistent with this interpretation.

Standardisation While the negative pre-trend on profits surfacing in Column 1 of Table A3 hints at SHs targeting underperformers, more direct evidence is needed. First, I identify underperformers using a simple Mincer-style regression aimed at predicting yearly ROA from the information available on the firm, with the following specification:

$$\widehat{\mathrm{ROA}}_{it} = \beta \log A_{i,t-1} + \gamma_{s(i)} + \rho_{r(i)} + \lambda_{d(i),t}, \tag{3}$$

where ROA_{it} is the net result of firm i in year t over its assets A_{it}, winsorized, and $\log A_{i,t-1}$ controls for firm size, measured at the beginning of the period so as not to be mechanically tied to the current-year ROA denominator. This control captures scale-related returns. The remaining terms are fixed effects for the firm's 3-digit sector ($\gamma_{s(i)}$), its province ($\rho_{r(i)}$), and the interaction of its 2-digit sector with the year ($\lambda_{d(i),t}$), the last of which absorbs sector-specific annual shocks and subsumes the common year effects; standard errors are clustered at the firm level. $\widehat{\mathrm{ROA}}_{it}$ is therefore the rate of return a firm of a given size would be expected to earn given its sector, location, and the contemporaneous sectoral conjuncture.

I then compute the residuals of this regression: in a given year, a negative residual corresponds to a firm performing worse than expected given its characteristics. I aggregate these residuals over a rolling window that includes the three preceding years to identify persistent underperformance, which is more likely to undergird an acquisition decision, and obtain the measure of overperformance employed.

Table 4: Does overperformance predict state acquisition?

	Participation Start				
	(1)	(2)	(3)	(4)	(5)
Overperformance	-4.462***	-5.711***	-5.147***	-4.776***	-4.795***
	(0.945)	(1.314)	(1.247)	(1.136)	(1.022)
Assets_{t-1}		0.381***	0.413***	0.445***	0.439***
		(0.034)	(0.036)	(0.048)	(0.052)
Year FE			✓	✓	✓
Sector FE				✓	✓
Province FE					✓
Observations	78,424	77,363	14,418	12,640	10,636
Pseudo R^2	0.004	0.045	0.082	0.181	0.251

***, **, and * indicate $p < .01$, $p < .05$, and $p < .1$ significance levels, respectively. Standard errors are clustered at the firm level. Discrete-time logit hazard: the dependent variable equals 1 in the year a firm first enters state participation and 0 in prior at-risk firm-years; firm-years after acquisition are dropped from the risk set. Overperformance is the firm's average residual ROA (net result net of size, time-sector, and province effects) over the previous three calendar years ($t-1$ to $t-3$). The sample excludes firms acquired in 1936. Sector is the NACE division. The negative relationship is robust to entering overperformance lags individually, and is replicated using the firm's actual ROA. Pseudo-R^2 is McFadden's.

Table 4 checks whether SHs target underperformers, by estimating increasingly stringent logit models of the probability of being acquired by SHs as a function of overperformance, controlling for firm size for consistency. The results robustly confirm the

hypothesis, although the sample drops substantially when introducing year fixed effects, as many years record no variation in participations.

What, then, is the change in profitability under state participation, taking into account previous performance?

Table 5: State participation and ROA, by overperformance at acquisition

	ROA			
	(1)	(2)	(3)	(4)
SP	0.002	0.007**	-0.002	0.005
	(0.003)	(0.003)	(0.003)	(0.004)
$SP\times$ Overperformance	-0.232**	-0.231**	-0.262**	-0.273**
	(0.108)	(0.104)	(0.103)	(0.121)
Overperformance	0.614***	0.614***		0.633***
	(0.079)	(0.072)		(0.081)
Year FE		✓		
Firm FE			✓	
Division $\times$ Year FE				✓
Dependent variable mean	-0.01	-0.01	-0.01	-0.01
Observations	2,984	2,984	2,984	2,984
Adjusted R^2	0.15	0.20	0.39	0.25
F-test	14.13	2.10	88.04	0.23

***, **, and * indicate $p < .01$, $p < .05$, and $p < .1$ significance levels, respectively. Standard errors are clustered at the firm level. The outcome is ROA (net result over assets), winsorized at the top and bottom 0.5%. SP (State Participation) equals one once a firm is state-acquired; Overperformance is the firm's average residual ROA over the three years before acquisition (net of size, sector, and province effects), constant within firm. The coefficient of interest is $SP\times$Overperformance: a negative value means the ROA effect of participation is smaller (or more negative) for firms that were overperforming before acquisition. The sample is treated firms with a computable pre-acquisition overperformance, over 1946–1974, excluding firms acquired in 1936. The Overperformance main effect is absorbed by the firm fixed effects (column 3).

Table 5 focuses on treated firms: the interaction coefficient shows that worse-performing target firms experience a larger increase in ROA after state participation, while better-performing firms experience a decrease, on average. The finding is clearly represented in Figure 7.

It is important to note that the findings are also consistent with simple mean reversion of performance. Yet, overperformance at acquisition generally predicts higher returns for firms, as shown by the third coefficient in Table 5. While part of this correlation is mechanical, it also suggests that the variable need not be subject to significant mean reversion, but instead captures something structural about the firm. It is then reasonable

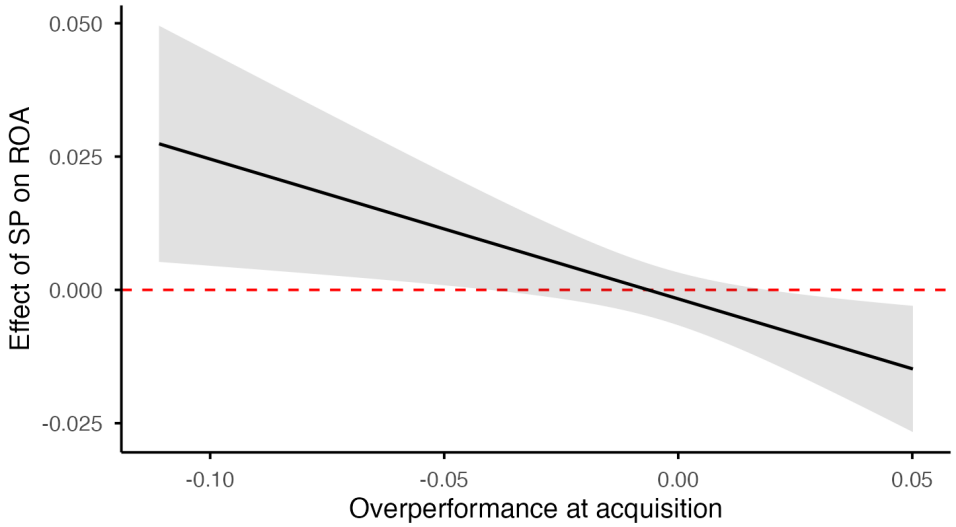


Figure 7: The "standardising" impact of state participation.
Interaction reported in Table 5, Column 3.

to expect that SHs will interact with that structural element as follows: (a) target underperformers and (b) slightly mistarget acquisitions, then (c) improve firms' performance up to a standard, but not exceptional, level, while interfering with top-performers.

7 Sectoral Results

In Italy's post-war context, state participation seems to have had positive effects on target firms, strongly in terms of size and more modestly in terms of profitability. However, these are within-firm comparisons that do not inform us about the weight of the sectors comprising these firms in the economy. As the aim of industrial policy is to provoke a change in the economic structure (Juhász et al. 2024), I now move to a brief sector-level analysis, cognisant that the data is suited to represent the dynamics of large firms rather than the whole economy.

The core empirical approach is analogous to the firm-level one outlined in Equation 1, albeit aggregating balance-sheet outcomes at the sector level. As sectors, I employ the two most granular distinctions available to ensure statistical power: 188 3-digit "groups" or 460 nested 4-digit "classes", comprising on average 53 and 34 firms, respectively. A sector is considered treated from the moment the first firm within it is participated—excluding the cohort of firms acquired in 1936, well before the sample period, which would otherwise mark their sectors as treated throughout—replicating as closely as possible the setup in Section 6.

7.1 TWFE and Staggered DD Models

Table 6 and Table A9 in the Appendix show the results of fitting a two-way fixed-effects model analogous to Equation 1, aggregating outcomes at the 3- and 4-digit level, respectively.

Table 6: State participation and 3-digit sector aggregates

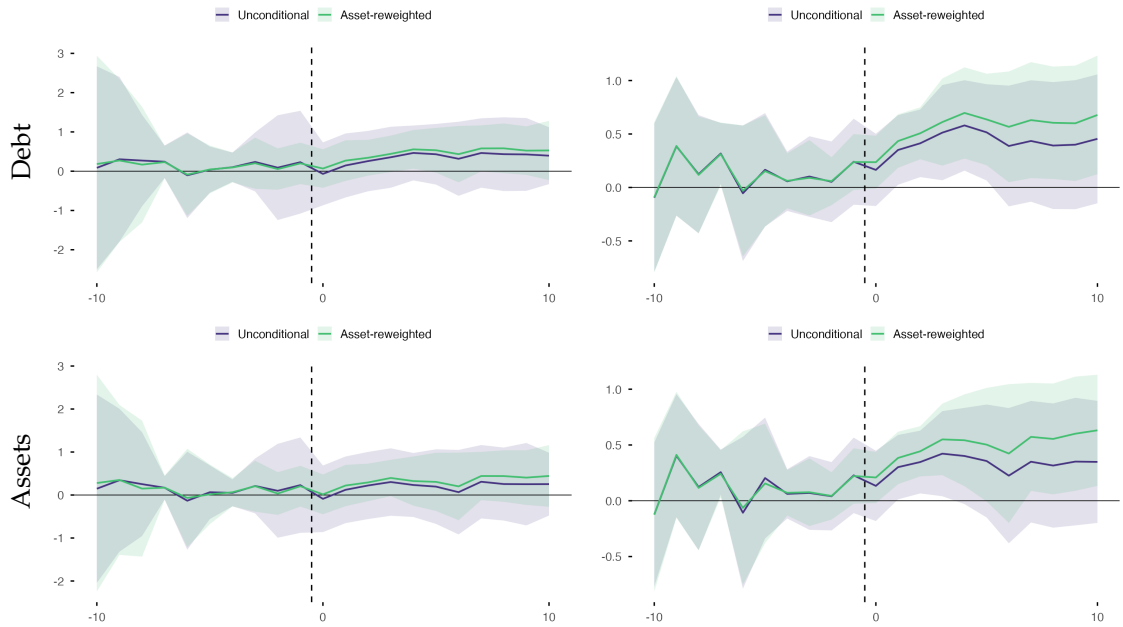
	Share Capital (1)	Debt (2)	Assets (3)	Net Result (4)	ROE (5)	ROA (6)	# Firms (7)
State participation	0.114	0.432***	0.243**	-0.116	0.034	0.003	0.051
	(0.126)	(0.121)	(0.115)	(0.194)	(0.023)	(0.003)	(0.075)
3-digit FE	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Dependent variable mean	3.231	4.370	5.060	0.739	0.019	0.002	2.281
Observations	4,326	4,320	4,330	4,332	4,326	4,330	4,332
Adjusted R^2	0.883	0.886	0.913	0.499	0.308	0.415	0.890

***, **, and * indicate $p < .01$, $p < .05$, and $p < .1$ significance levels, respectively. The unit is the 3-digit NACE sector-year, 1946–71, excluding the financial section J. State participation is a sector dummy equal to one from the first year the group contains a firm acquired by the state (excluding the 1936 cohort). Outcomes are balance-sheet items aggregated at the 3-digit level: share capital, assets, and debt (sums over the group's firms) and the number of firms in the group enter in logs on strictly positive sector-years; the net result (sum) enters in inverse hyperbolic sine; ROE and ROA are the group's aggregate ratios, winsorized at the top and bottom 0.5%. Standard errors are clustered by sector.

Both tables underscore the growth in sectors' size upon participation. Table A9, with greater granularity, also displays positive results on return on assets and the number of firms, albeit at lower significance. Appendix Tables A10–A13 replace the participation dummy with the time-varying share of participated firms in the sector—by simple firm count and, alternatively, by assets—and confirm the size result; the weaker profitability and firm-count results of Table A9, by contrast, do not survive.

Table A15 reports the staggered difference-in-differences aggregate ATTs for the same outcomes—unconditional, and reweighting the never-treated control sectors to match the treated sectors' baseline size. The estimates mirror those in Table 6, the most robust being positive effects on assets and debt. Figure 8 plots the dynamic event-study counterparts, with the remaining non-significant estimates for the other outcomes in Appendix Figure A3.

In sum, it appears that the increase in size (assets, and—less robustly—debt) identified at the level of the single firm is replicated at more aggregate sectoral levels. This suggests that participated firms do not merely capture the growth otherwise destined for the fully private firms in their sector; instead, their own growth drives an increase in total sector size. However, telling these two very different types of growth apart warrants an explicit investigation, outlined in the following section.



(a) 3-digit group (b) 4-digit class

Figure 8: Sector-level DD event studies: unconditional vs. asset-reweighted (assets and debt)

Sector-level Callaway and Sant'Anna (2021) dynamic event studies for sector debt and assets, at the 3-digit group (column a) and 4-digit class (column b) levels; the remaining outcomes of Table A15 appear in Figure A3. Each panel overlays the unconditional estimate and the asset-reweighted estimate—inverse-probability weights on the sector's baseline (entry-year) log assets; see the in-panel legend—with 95% uniform confidence bands; the vertical dashed line marks the last pre-participation year. Never-treated sectors form the control group; section J is excluded and the panel spans 1946–71.

7.2 Growth or displacement?

The evidence on within-sector variation just outlined indicates that the net effect of participation by SHs is to enlarge sectors targeted. However, this growth might have two very different components: some might reflect a genuine addition to overall economic activity, some might reflect the simple displacement of resources from some economic actors to participated firms. Displacement is quite plausible in the short to medium term, as most economic activity relies on finite rival inputs (e.g. deployable capital, energy). In the language of the potential-outcomes framework, this dynamic would imply a violation of SUTVA (Imbens and Rubin 2015): if participated firms subtract rival inputs from other firms, the positive effect we observe might merely reflect the hindering of private firms.

While a general-equilibrium counterfactual for the whole economy is beyond the scope of the present study, this section tries to gauge the relevance of displacement, in response to participation, within and between sectors.

7.2.1 Within sectors

First, I consider the possibility that participated firms and sectors grow at the expense of private firms within the sector.

To inquire into this, I refit Equation 1 marking firms as treated from the moment they share their (3- or 4-digit) sector with a participated firm, while dropping ever-participated firms altogether. This analysis is meant to capture what happens to a fully private firm when a firm in its sector is participated. Results are reported in Table 7 (3-digit aggregation) and in Appendix Table A14 (4-digit aggregation).

Table 7: Same-sector (3-digit) state participation and never-treated firms' balance sheets

	Share Capital (1)	Debt (2)	Assets (3)	Net Result (4)	ROE (5)	ROA (6)
Same-sector SP	0.015	0.076***	0.066***	0.024*	0.019**	0.002***
	(0.023)	(0.025)	(0.020)	(0.013)	(0.009)	(0.001)
Firm FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Dependent variable mean	0.33	1.26	2.07	0.11	0.06	0.00
Observations	85,460	84,300	85,168	83,047	83,028	83,014
Adjusted R^2	0.76	0.79	0.85	0.52	0.30	0.41

***, **, and * indicate $p < .01$, $p < .05$, and $p < .1$ significance levels, respectively. Firm-year panel of never-treated firms, 1946–71, excluding financial section J. Same-sector participation is a dummy equal to one from when the firm's 3-digit NACE sector contains at least one state-acquired firm. Share capital, debt and assets in logs (positive observations only), net result in inverse hyperbolic sine, ROE and ROA winsorized at the top and bottom 0.5%. Standard errors are clustered at the firm level.

Coefficients show that the positive results observed on participated firms are unlikely to be due to their squeezing out or steamrolling private competition. If anything, fully private firms in participated sectors seem to do better than their counterparts in non-participated sectors.

We may interpret this in two ways. Most generously, it might reflect positive spillovers of participation—coordination effects, or market-making (Mazzucato and Penna 2016) by SHs—that also reach private firms. Alternatively, it might reflect an independent sectoral trend, which SHs detect and direct investment towards.[11]

These results point to the implausibility of within-sector displacement being a first-order driver of the observed results. However, displacement might also happen across sectors.

[11]The difference between the coefficient in this table and in Table 1 could then be either the positive impact of participation, or the ability of SHs to select the best firms within growing sectors.

7.2.2 Between sectors

Are these growing sectors simply hoovering up economic activity from elsewhere? And if so, from related or unrelated sectors?

This distinction (Frenken et al. 2007; Boschma 2017; Hidalgo et al. 2018) is useful in terms of likelihood and desirability.[12] First, reallocation between related sectors is more plausible, owing to lower frictions in the reallocation of inputs (e.g. trained workers) and shared enablers (e.g. agglomeration economies, related established supply chains). Second, reallocation between related sectors does not foster structural transformation, which typically sees reallocation of output from agriculture to industry (Herrendorf et al. 2014).

I can probe the relative prevalence of related- and unrelated-sector reallocation by manipulating the comparison pool, i.e. by introducing progressively stringent sector fixed effects. If reallocation between related sectors dominates, the estimated effect should increase as the fixed effects become more granular, because the close comparison group suffers the negative spillovers of treatment. If, conversely, reallocation between unrelated sectors dominates, the estimate should weaken as the comparison pool becomes more similar.

The result of the exercise, displayed in Figure 9, is more consistent with the prevalence of unrelated-sector reallocation: when compared within very large groups—that is, against largely unrelated sectors—participated sectors do better than when compared against more closely related sectors (e.g. within the same division). The rightmost blue and green estimates reproduce the assets column of Tables 6 and A9, respectively—the within-group and within-class specifications, whose full progressive counterparts appear in Appendix Tables A16 and A17. That the estimate decreases—yet survives—as the comparison group becomes more stringent suggests that the observed effect of participation is a mix of net growth and unrelated-sector reallocation.

This is relevant for the substantive interpretation of the impact of SHs, which seems to foster either meaningful reallocation of productive forces across very different sectors, or a genuine enlargement of the sum of economic activity. As discussed in the case of within-sector variation, it is also impossible to rule out that SHs predicted which macro-areas of the economy were going to grow faster, predicted which sectors within those would be the best-performing, and invested in them.

A more thorough analysis of spillovers would require accounting for sectoral linkages. Unfortunately, no sufficiently granular input-output table was produced for the period in question. This is one of the manifold limitations of this study, which the following section recaps, non-exhaustively, together with implications for policy and avenues for future research.

[12] Another closely related distinction is short and long "jumps" across the product space (Hidalgo et al. 2007). The angle I explore, however, is reallocation rather than diversification.

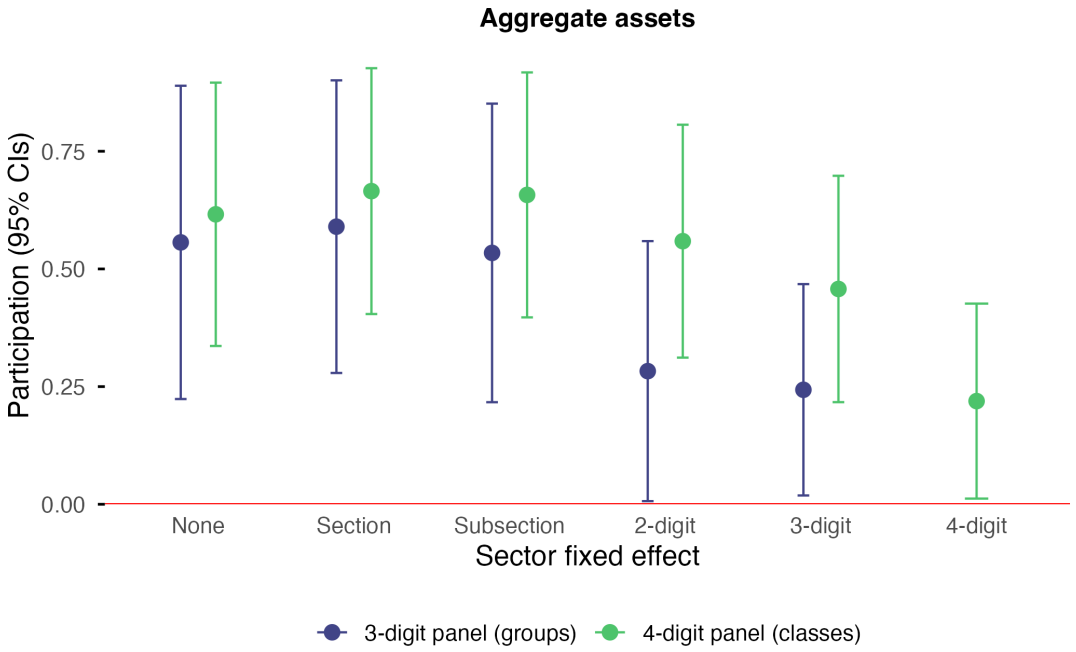


Figure 9: State participation and log sector assets under progressively finer comparison groups

State participation (Treat × Post) coefficients on log sector assets, estimated as the comparison pool is narrowed by adding progressively finer sector fixed effects (none, section, subsection, division, and the 3-digit group or 4-digit class), each with a 95% confidence interval. The two series are the 3-digit group and 4-digit class panels; the full specifications are reported in Appendix Tables A16 and A17, and the rightmost (within-sector) estimates reproduce the assets column of the sector TWFE Tables 6 and A9. Treatment is the staggered Treat × Post interaction defined on the sector's first (non-1936) participation year, and every model additionally controls for the sector's log assets in its first observed year. Section J is excluded, the panel spans 1946–71, standard errors are clustered by sector.

8 Conclusion

This paper has assembled what is, to my knowledge, the most comprehensive firm-level evidence to date on state shareholding as an instrument of industrial policy, linking a near-census of Italian joint-stock companies during the economic miracle (1946–1971) to a firm-by-firm reconstruction of the participations held by the three main state holdings. Three sets of findings emerge. Participated firms undergo a pronounced expansion in size and a more modest improvement in profitability. Share capital, debt, and assets rise by 24, 87, and 57 per cent, respectively, over the roughly six years for which the average firm is observed after acquisition, while returns on assets increase by 0.8 percentage points. Event studies reveal no meaningful pre-trends and a gradual onset. The size effect exceeds that documented for private-equity buy-outs, whereas the profitability gain is smaller. I highlight suggestive evidence of derisking and focal point effects to explain the former, while a "standardisation" pattern emerges in relation to the latter.

Crucially, the firm-level size effect is replicated at the sector level, indicating that participated firms do not merely capture growth that would otherwise have accrued

to their private counterparts. Private firms sharing a sector with a participated firm fare, if anything, better than comparable firms elsewhere—evidence against within-sector displacement and consistent with positive spillovers. Between sectors, the effect attenuates but persists as the comparison group is narrowed to related activities, pointing to a mixture of genuine net growth and reallocation drawn from unrelated parts of the economy—the kind of reallocation that structural transformation entails. Taken together, these results offer an encouraging view of state holdings as instruments of ambitious industrial policy, in complementary contrast to views emphasising patronage and soft budget constraints (Shleifer and Vishny 1994; Kornai 1986). Whether this reflects the state genuinely creating and coordinating markets (Mazzucato and Penna 2016), or merely its skill in selecting sectors and firms already poised to grow, is an interpretive tension that the data cannot fully resolve.

That tension points to this study's principal limitation, which is one of identification rather than of data availability. The design compares participated firms with never- and not-yet-participated ones after purging additive firm, year, and—in robustness—sector-by-year heterogeneity, and it documents flat pre-trends and stability across survivor-bias corrections, propensity-score reweighting, and a battery of alternative specifications. None of this can rule out selection on time-varying unobservables. The managers of the holdings, and the ministry that supervised them, might have possessed information—about a firm's prospects, its markets, its political usefulness—that appears nowhere in the balance sheets and need not manifest as a pre-trend. Were they systematically to acquire firms poised to expand for reasons unrelated to participation, the size effect would be overstated; in the mirror case, understated. The standardisation result is, likewise, observationally close to simple mean reversion, although the persistence of the overperformance measure and its predictive content for future returns suggest that it captures something structural about firms, rather than purely transitory. And because acquisition dates can only be bracketed to benchmark years, the analysis speaks to protracted participation, not to the immediate impact of acquisition or to short-lived spells. Absent a clean source of exogenous variation in the timing or allocation of participations, the estimates are best read as informative associations carrying a plausibly causal interpretation, rather than as point-identified treatment effects.

Read with that caution, the findings still carry practical lessons for the contemporary revival of state investment. State equity appears able to enlarge firms and, through them, the sectors deemed strategic: a government that wishes to build up a particular part of its economy can plausibly do so by taking stakes in it, provided it accepts that much of the effect operates through relaxed financial constraints rather than through efficiency gains. If, instead, the goal is to raise productivity, the evidence counsels selectivity—acquiring underperformers, firms trading below their expected results, yields improvement, whereas taking over already-excellent firms tends to erode their

edge. The corollary is a caution against acquiring firms for non-economic reasons, since participation appears to pull performance toward a middling standard from both directions. Coordination benefits, finally, seem strongest where the state acts as a singular focal point rather than blanketing a sector with holdings.

These conclusions open several research avenues. The most immediate is to open the black box of the size and profitability effects: data on credit terms, bond covenants, and debt service would allow the derisking and soft-budget-constraint channels to be told apart, while management- or plant-level records would clarify whether standardisation led to genuine operational restructuring. A second avenue concerns the political economy of participation itself. Board-composition data can reveal the networks of political connection through which acquisitions were decided, and matching participations to the geography of electoral competition—core, opposition, or swing districts—would test whether the holdings also served clientelistic or electoral ends. Third, the innovation consequences of participation remain underexplored; linking firms to historical Italian patents held in the state archives, or to US patents granted to Italian firms, would establish whether state backing translated into the technological upgrading that the qualitative literature on IRI's engineering capabilities suggests. Fourth, this study largely abstracts from the spatial dimension that motivated much of the holdings' mandate: situating participations in their geography, and measuring their effect on the industrialisation of the lagging South, would show how far they fostered regional convergence. Finally, export data would permit a test of whether state participation shifted Italy's revealed comparative advantage toward the sectors it targeted.

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Appendix

A Additional Tables

A.1 Descriptives

Table A1: Distribution of firms' balance-sheet items

Percentile	Share Capital	Debt	Assets	Net Result
0%	0.000	0.000	0.000	-1,738.152
0.5%	0.010	0.000	0.000	-8.662
10%	0.193	0.333	1.080	-0.305
20%	0.527	0.949	2.256	-0.074
30%	0.883	1.683	3.626	-0.006
40%	1.109	2.604	5.369	0.008
50%	1.472	3.838	7.610	0.031
60%	2.044	5.619	10.936	0.069
70%	2.985	8.508	16.650	0.133
80%	4.722	14.302	28.967	0.265
90%	9.814	33.148	69.827	0.703
99.5%	208.870	705.246	2,150.080	17.693
100%	7,653.278	71,833.024	123,636.921	694.318

Percentiles of the four balance-sheet items across firm-years in the estimation sample (1946–71), in billions of 1990 Italian liras. The 0.5% and 99.5% rows are the winsorization thresholds used in the main specifications.

A.2 Robustness

Table A2: State participation and firms' balance sheet items, excluding financial intermediaries

	Share Capital (1)	Debt (2)	Assets (3)	Net Result (4)	ROE (5)	ROA (6)
SP	0.209**	0.527***	0.403***	0.004	0.126***	0.008***
	(0.090)	(0.103)	(0.100)	(0.090)	(0.033)	(0.003)
Firm FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Dependent variable mean	0.34	1.29	2.07	0.09	0.05	0.00
Observations	121,285	118,141	119,454	115,654	115,632	115,612
Adjusted R^2	0.75	0.78	0.84	0.53	0.29	0.41
F-test	2,047.80	2,073.07	2,961.15	680.87	292.68	448.98

***, **, and * indicate $p < .01$, $p < .05$, and $p < .1$ significance levels, respectively. Standard errors are clustered at the firm level. State participation captures any extent of state share acquisition. Balance-sheet raw items are transformed for interpretability and to account for outliers: share capital, debt, and assets enter in logs (estimated on strictly positive observations only) and Net Result enters in inverse hyperbolic sine (IHS) to accommodate negative values. ROE and ROA are winsorized at the top and bottom 0.5%. The sample excludes firms acquired in 1936 and sector J (Financial intermediaries).

Table A3: State participation, staggered DD: robustness to propensity score reweighting on pre-treatment covariates (Callaway and Sant'Anna 2021)

No controls		Covariate-adjusted ATT		
Pre-ATT	ATT	Size	+ Leverage	+ ROA
Share Capital				
-0.032	0.513***	0.670***	0.607***	0.613***
(0.035)	(0.158)	(0.121)	(0.118)	(0.122)
Debt				
0.020	0.714**	0.934***	0.929***	0.905***
(0.033)	(0.344)	(0.222)	(0.212)	(0.226)
Assets				
-0.017	0.772***	1.037***	1.013***	1.021***
(0.027)	(0.189)	(0.132)	(0.129)	(0.145)
Net Result				
-0.034***	0.035	0.088	0.075	0.007
(0.013)	(0.084)	(0.079)	(0.074)	(0.079)
ROE				
0.002	0.092***	0.087***	0.089***	0.063***
(0.007)	(0.022)	(0.022)	(0.023)	(0.020)
ROA				
0.001	0.010**	0.009**	0.009**	0.006*
(0.001)	(0.004)	(0.004)	(0.004)	(0.003)

***, **, and * indicate $p < .01$, $p < .05$, and $p < .1$. All columns report the Callaway and Sant'Anna (2021) doubly-robust estimator with never-participated controls and firm-clustered multiplier-bootstrap standard errors (in parentheses); the sample is 1946–71. ATT is the overall (simple-aggregation) post-participation effect; Pre-ATT, reported for the no-controls baseline, is the equal-weighted average of the pre-participation event-time effects (-10 to -1), whose significance would flag a pre-trend. The three right-hand columns progressively enrich the estimator's propensity-score and outcome-regression models (`xformla`) with firm-constant pre-treatment (entry-year) covariates: *Size* is log entry assets, *+Leverage* adds the entry debt ratio, and *+ROA* adds entry ROA. Reweighting never-participated firms toward those resembling the participated ones—chiefly in size—holds observable heterogeneity fixed, so the adjusted columns compare acquired firms with similar private ones rather than with the never-participated pool as a whole.

Table A4: State participation and firms' balance sheets: outlier-robustness

Raw	Winsorize		Drop		IHS
	0.5%	1%	0.5%	1%	
Share Capital					
40.062***	14.178***	9.659***	9.727***	7.127***	0.335***
(12.089)	(2.927)	(1.615)	(1.830)	(1.181)	(0.064)
Debt					
122.435***	67.983***	44.548***	43.761***	27.409***	0.574***
(38.651)	(11.414)	(6.021)	(6.674)	(3.660)	(0.099)
Assets					
195.260***	156.342***	104.208***	124.231***	67.603***	0.406***
(69.495)	(30.546)	(17.268)	(21.625)	(10.394)	(0.086)
Net Result					
-2.317	0.250	0.175	0.237	0.206*	0.007
(3.839)	(0.285)	(0.174)	(0.192)	(0.115)	(0.086)
ROE					
0.799	0.116***	0.082***	0.059***	0.045***	0.115***
(0.747)	(0.029)	(0.018)	(0.014)	(0.011)	(0.033)
ROA					
0.074	0.008***	0.007***	0.006***	0.006***	0.011**
(0.068)	(0.002)	(0.002)	(0.002)	(0.002)	(0.005)

***, **, and * indicate $p < .01$, $p < .05$, and $p < .1$. Each cell is the coefficient of SP on the row outcome applying specification 1. Columns 2 to 6 vary on how outliers are accounted for: winsorizing or dropping the top/bottom 0.5% or 1%; or the inverse-hyperbolic-sine transform applied to all observations (IHS). The sample is 1946–71 excluding firms participated in 1936.

Table A5: State participation and firms' balance sheets: robustness to alternative specifications

Including 1936	Sector×Year FE		Sector-time trends		Firm in next
	Ss	D	Ss	D	benchmark
Share Capital					
0.227**	0.209**	0.236***	0.209**	0.214**	0.341**
(0.088)	(0.087)	(0.090)	(0.086)	(0.088)	(0.172)
Debt					
0.626***	0.561***	0.589***	0.559***	0.562***	0.335*
(0.124)	(0.120)	(0.121)	(0.117)	(0.117)	(0.194)
Assets					
0.458***	0.410***	0.436***	0.411***	0.403***	0.266*
(0.106)	(0.100)	(0.103)	(0.100)	(0.101)	(0.161)
Net Result					
0.002	-0.039	-0.072	-0.020	-0.060	0.300*
(0.086)	(0.085)	(0.082)	(0.085)	(0.082)	(0.156)
ROE					
0.113***	0.086***	0.088***	0.092***	0.090***	0.208***
(0.029)	(0.027)	(0.028)	(0.028)	(0.029)	(0.066)
ROA					
0.008***	0.005*	0.004*	0.005**	0.004*	0.010**
(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.005)

***, **, and * indicate $p < .01$, $p < .05$, and $p < .1$. Each cell is the coefficient of the state participation dummy SP regressed on the row outcome under alternative modifications of specification 1; outliers are accounted for following Table 1, and standard errors are clustered at the firm level. Column 1 repeats the baseline on the sample that retains the 1936 participation cohort; columns 2–3 add subsection (Ss) or division (D) by-year fixed effects, absorbing sector-specific annual shocks; columns 4–5 instead add subsection- or division-specific linear time trends; column 6 re-estimates the baseline on the survivor subsample of firm-years whose firm is also observed at the following benchmark census, a check against survivor bias. The sample is 1946–71.

Table A6: rp_main_log × US-aid recipient (TWFE, 1945-75)

	log Equity (1)	log Assets (2)	asinh P&L (3)	log Debt (4)	ROE (5)	ROA (6)	Debt Ratio (7)
State Participation	0.211**	0.461***	0.014	0.633***	0.117***	0.008***	0.079***
	(0.090)	(0.110)	(0.085)	(0.129)	(0.031)	(0.002)	(0.018)
State Participation × aid_anyTRUE	0.154	-0.190	-0.132	-0.146	-0.021	-0.005	-0.012
	(0.420)	(0.328)	(0.639)	(0.415)	(0.089)	(0.017)	(0.058)
Firm FE	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Observations	131,530	129,709	125,910	127,925	125,849	125,863	129,130
Adjusted R^2	0.76	0.85	0.56	0.79	0.30	0.41	0.64

State participation interacted with an indicator for firms Martinez (2024) identifies as US-aid recipients. The interaction tests whether the state-participation effect is concentrated among firms that also received US aid.

A.3 Firm-level Mechanism

Table A7: Current debt predicts future assets (balanced sample across horizons)

	Assets (t+1) (1)	Assets (t+2) (2)	Assets (t+3) (3)	Assets (t+4) (4)	Assets (t+5) (5)
Debt (t)	0.025***	0.028***	0.022***	0.021***	0.019**
	(0.007)	(0.007)	(0.007)	(0.008)	(0.009)
Assets (t)	0.602***	0.403***	0.290***	0.220***	0.166***
	(0.016)	(0.017)	(0.016)	(0.014)	(0.015)
Firm FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Observations	62,192	62,192	62,192	62,192	62,192
Adjusted R^2	0.960	0.949	0.949	0.949	0.949

***, **, and * indicate $p < .01$, $p < .05$, and $p < .1$ significance levels, respectively. Standard errors are clustered at the firm level. Debt and assets are limited to strictly positive observations and logged. The sample is held fixed across columns: only firm-years with strictly positive debt and assets at t and strictly positive assets at every horizon $t+1, \ldots, t+5$ are included, so all columns share the same observations. The findings are robust to letting the sample composition vary across columns (see Table A8).

Table A8: Current debt predicts future assets, controlling for current assets

	Assets (t+1) (1)	Assets (t+2) (2)	Assets (t+3) (3)	Assets (t+4) (4)	Assets (t+5) (5)
Debt (t)	0.034***	0.034***	0.024***	0.023***	0.021**
	(0.006)	(0.007)	(0.007)	(0.008)	(0.009)
Assets (t)	0.604***	0.401***	0.294***	0.217***	0.159***
	(0.012)	(0.015)	(0.014)	(0.013)	(0.015)
Firm FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Observations	111,773	98,500	86,234	75,340	66,611
Adjusted R^2	0.957	0.945	0.944	0.944	0.945

***, **, and * indicate $p < .01$, $p < .05$, and $p < .1$ significance levels, respectively. Standard errors are clustered at the firm level. Debt and assets are limited to strictly positive observations and logged.

A.4 Sectoral Results

Table A9: State participation and 4-digit sector aggregates

	Share Capital (1)	Debt (2)	Assets (3)	Net Result (4)	ROE (5)	ROA (6)	# Firms (7)
State participation	0.105	0.368***	0.219**	-0.035	0.059***	0.005**	0.123*
	(0.110)	(0.111)	(0.106)	(0.144)	(0.020)	(0.002)	(0.064)
4-digit FE	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Dependent variable mean	2.459	3.582	4.266	0.486	0.044	0.004	1.747
Observations	9,830	9,809	9,832	9,836	9,830	9,832	9,836
Adjusted R^2	0.830	0.822	0.852	0.465	0.266	0.363	0.818

***, **, and * indicate $p < .01$, $p < .05$, and $p < .1$ significance levels, respectively. The unit is the 4-digit NACE sector-year, 1946–71, excluding the financial section J. State participation is a sector dummy equal to one from the first year the class contains a firm acquired by the state (excluding the 1936 cohort). Outcomes are balance-sheet items aggregated at the 4-digit level: share capital, assets, and debt (sums over the class's firms) and the number of firms in the class enter in logs on strictly positive sector-years; the net result (sum) enters in inverse hyperbolic sine; ROE and ROA are the class's aggregate ratios, winsorized at the top and bottom 0.5%. Standard errors are clustered by sector.

Table A10: State participation intensity (firm share) and 3-digit sector aggregates

	Share Capital (1)	Debt (2)	Assets (3)	Net Result (4)	ROE (5)	ROA (6)	# Firms (7)
Share of participated firms	0.576	1.363***	0.914*	0.074	0.010	0.007	-0.443**
	(0.364)	(0.390)	(0.499)	(0.438)	(0.107)	(0.013)	(0.191)
3-digit FE	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Dependent variable mean	3.231	4.370	5.060	0.739	0.019	0.002	2.281
Observations	4,326	4,320	4,330	4,332	4,326	4,330	4,332
Adjusted R^2	0.883	0.886	0.914	0.499	0.306	0.415	0.891

***, **, and * indicate $p < .01$, $p < .05$, and $p < .1$ significance levels, respectively. The unit is the 3-digit NACE sector-year, 1946–71, excluding the financial section J. Participation is measured by intensity rather than by the absorbing dummy of Table 6: the share of the sector's firms that are state-participated in that year. A firm counts as participated from the year it is first acquired onward, excluding the 1936 cohort, so the regressor lies in $[0, 1]$ and the coefficient is the effect of moving the group from entirely private to entirely state-participated. Outcomes are the 3-digit aggregates of Table 6: share capital, assets, debt (sums) and the number of firms in logs; the net result (sum) in inverse hyperbolic sine; ROE and ROA as aggregate ratios, winsorized at the top and bottom 0.5%. The negative coefficient in column (7) is in part mechanical: the number of firms in the sector is the denominator of the regressor, so the entry of a private firm raises the dependent variable and lowers the share at once. The asset-share specification of Table A12, which carries no such link, returns a small and insignificant coefficient on the firm count. Standard errors are clustered by sector.

Table A11: State participation intensity (firm share) and 4-digit sector aggregates

	Share Capital (1)	Debt (2)	Assets (3)	Net Result (4)	ROE (5)	ROA (6)	# Firms (7)
Share of participated firms	0.700*	1.497***	1.014**	0.122	0.014	0.005	-0.310*
	(0.364)	(0.350)	(0.434)	(0.413)	(0.085)	(0.011)	(0.170)
4-digit FE	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Dependent variable mean	2.459	3.582	4.266	0.486	0.044	0.004	1.747
Observations	9,830	9,809	9,832	9,836	9,830	9,832	9,836
Adjusted R^2	0.830	0.823	0.853	0.465	0.264	0.362	0.818

***, **, and * indicate $p < .01$, $p < .05$, and $p < .1$ significance levels, respectively. The unit is the 4-digit NACE sector-year, 1946–71, excluding the financial section J. Participation is measured by intensity rather than by the absorbing dummy of Table A9: the share of the sector's firms that are state-participated in that year. A firm counts as participated from the year it is first acquired onward, excluding the 1936 cohort, so the regressor lies in $[0, 1]$ and the coefficient is the effect of moving the class from entirely private to entirely state-participated. Outcomes are the 4-digit aggregates of Table A9: share capital, assets, debt (sums) and the number of firms in logs; the net result (sum) in inverse hyperbolic sine; ROE and ROA as aggregate ratios, winsorized at the top and bottom 0.5%. The negative coefficient in column (7) is in part mechanical: the number of firms in the sector is the denominator of the regressor, so the entry of a private firm raises the dependent variable and lowers the share at once. The asset-share specification of Table A13, which carries no such link, returns a small and insignificant coefficient on the firm count. Standard errors are clustered by sector.

Table A12: State participation intensity (asset share) and 3-digit sector aggregates

	Share Capital (1)	Debt (2)	Assets (3)	Net Result (4)	ROE (5)	ROA (6)	# Firms (7)
Share of participated assets	0.811**	1.536***	1.110***	0.276	0.074	0.013	-0.058
	(0.326)	(0.293)	(0.333)	(0.525)	(0.077)	(0.009)	(0.158)
3-digit FE	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Dependent variable mean	3.234	4.370	5.060	0.739	0.019	0.002	2.282
Observations	4,324	4,320	4,330	4,330	4,324	4,330	4,330
Adjusted R^2	0.884	0.890	0.916	0.499	0.308	0.417	0.889

***, **, and * indicate $p < .01$, $p < .05$, and $p < .1$ significance levels, respectively. The unit is the 3-digit NACE sector-year, 1946–71, excluding the financial section J. Participation is measured by intensity rather than by the absorbing dummy of Table 6: the share of the sector's total assets held by state-participated firms in that year, which weights each participated firm by its size. A firm counts as participated from the year it is first acquired onward, excluding the 1936 cohort, so the regressor lies in $[0, 1]$ and the coefficient is the effect of moving the group from entirely private to entirely state-participated. Outcomes are the 3-digit aggregates of Table 6: share capital, assets, debt (sums) and the number of firms in logs; the net result (sum) in inverse hyperbolic sine; ROE and ROA as aggregate ratios, winsorized at the top and bottom 0.5%. Unlike the firm-share specification of Table A10, the regressor does not contain the sector's firm count, so column (7) is free of any mechanical link to the dependent variable. Standard errors are clustered by sector.

Table A13: State participation intensity (asset share) and 4-digit sector aggregates

	Share Capital (1)	Debt (2)	Assets (3)	Net Result (4)	ROE (5)	ROA (6)	# Firms (7)
Share of participated assets	0.765***	1.514***	1.042***	-0.190	0.039	0.005	0.132
	(0.294)	(0.276)	(0.282)	(0.429)	(0.058)	(0.007)	(0.126)
4-digit FE	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Dependent variable mean	2.461	3.582	4.266	0.486	0.044	0.004	1.747
Observations	9,826	9,809	9,832	9,832	9,826	9,832	9,832
Adjusted R^2	0.830	0.825	0.854	0.465	0.264	0.362	0.818

***, **, and * indicate $p < .01$, $p < .05$, and $p < .1$ significance levels, respectively. The unit is the 4-digit NACE sector-year, 1946–71, excluding the financial section J. Participation is measured by intensity rather than by the absorbing dummy of Table A9: the share of the sector's total assets held by state-participated firms in that year, which weights each participated firm by its size. A firm counts as participated from the year it is first acquired onward, excluding the 1936 cohort, so the regressor lies in $[0, 1]$ and the coefficient is the effect of moving the class from entirely private to entirely state-participated. Outcomes are the 4-digit aggregates of Table A9: share capital, assets, debt (sums) and the number of firms in logs; the net result (sum) in inverse hyperbolic sine; ROE and ROA as aggregate ratios, winsorized at the top and bottom 0.5%. Unlike the firm-share specification of Table A11, the regressor does not contain the sector's firm count, so column (7) is free of any mechanical link to the dependent variable. Standard errors are clustered by sector.

Table A14: Same-sector (4-digit) state participation and never-treated firms' balance sheets

	Share Capital (1)	Debt (2)	Assets (3)	Net Result (4)	ROE (5)	ROA (6)
Same-sector SP	0.078***	0.060***	0.071***	0.027**	0.016*	0.003***
	(0.020)	(0.021)	(0.017)	(0.013)	(0.008)	(0.001)
Firm FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Dependent variable mean	0.28	1.21	2.00	0.09	0.05	0.00
Observations	116,623	113,523	114,803	111,322	111,300	111,280
Adjusted R^2	0.74	0.77	0.83	0.50	0.29	0.41

***, **, and * indicate $p < .01$, $p < .05$, and $p < .1$ significance levels, respectively. Firm-year panel of never-treated firms, 1946–71, excluding financial section J. Same-sector participation is a dummy equal to one from when the firm's 4-digit NACE sector contains at least one state-acquired firm. Share capital, debt and assets in logs (positive observations only), net result in inverse hyperbolic sine, ROE and ROA winsorized at the top and bottom 0.5%. Standard errors are clustered at the firm level.

Table A15: State participation and sector aggregates: staggered-DD ATTs, unconditional vs. asset-reweighted

	Unconditional		Asset-reweighted	
	3-digit	4-digit	3-digit	4-digit
Share Capital (log)	-0.024	0.127	0.165	0.377**
	(0.309)	(0.183)	(0.201)	(0.158)
Debt (log)	0.325	0.413**	0.460***	0.620***
	(0.318)	(0.191)	(0.161)	(0.166)
Assets (log)	0.179	0.306*	0.356**	0.559***
	(0.341)	(0.180)	(0.167)	(0.147)
# Firms (log)	0.015	0.070	0.054	0.132
	(0.184)	(0.099)	(0.136)	(0.091)
Net Result (asinh)	0.117	-0.034	0.030	-0.015
	(0.362)	(0.208)	(0.377)	(0.225)
ROE (win.)	0.017	0.025	0.002	0.003
	(0.033)	(0.023)	(0.036)	(0.029)
ROA (win.)	0.004	0.005	0.003	0.004
	(0.009)	(0.004)	(0.004)	(0.003)

Each cell is the ATT of a Callaway and Sant'Anna (2021) staggered difference-in-differences with never-treated controls at the sector level, reporting sector-clustered multiplier-bootstrap standard errors in parentheses; section J is excluded and the panel spans 1946–71. The treatment cohort is the first year the sector holds a state-participated firm. The asset-reweighted estimate is based on inverse-probability-weights on the sector's baseline (entry-year) log assets (the doubly-robust estimator is unfeasible on thin sector cohorts). Outcomes are sector aggregates: share capital, debt, assets and firm count in logs, net result in inverse hyperbolic sine, ROE and ROA as winsorized aggregate ratios. ***, **, and * indicate $p < .01$, $p < .05$, and $p < .1$.

Table A16: State participation (Treat × Post) and log sector assets, progressive sector FE (3-digit)

	Log assets				
	(1)	(2)	(3)	(4)	(5)
Treated (ever)	0.597***	0.734***	0.623***	0.507***	
	(0.159)	(0.159)	(0.171)	(0.165)	
Treat × Post	0.556***	0.590***	0.534***	0.283**	0.243**
	(0.170)	(0.159)	(0.162)	(0.141)	(0.115)
Log assets, first observed year	0.704***	0.645***	0.639***	0.681***	
	(0.041)	(0.045)	(0.045)	(0.044)	
Year FE	✓	✓	✓	✓	✓
Section FE		✓			
Subsection FE			✓		
Division FE				✓	
Group FE					✓
Dependent variable mean	5.064	5.064	5.064	5.064	5.064
Observations	4,326	4,326	4,326	4,326	4,326
Adjusted R^2	0.727	0.751	0.763	0.802	0.913

***, **, and * indicate $p < .01$, $p < .05$, and $p < .1$ significance levels, respectively. The outcome is the 3-digit NACE group's log aggregate assets, 1946–71, excluding the financial section J and restricted to strictly positive sector-years. Treatment is `first_treat_no1936`, the 3-digit NACE group's earliest firm-acquisition year excluding the 1936 cohort: *treat* $= 1$ if the 3-digit NACE group is ever treated, *post* $= 1$ from that year onward, and Treat×Post is the absorbing DiD indicator. Never-treated sectors are always pre-period, so *post* is collinear with Treat×Post and dropped; *treat* is absorbed once a sector fixed effect is added. Every column controls for the 3-digit NACE group's log assets in its first observed year and includes year fixed effects; columns add the sector fixed effect at progressively finer granularity: none, section, subsection, division, and 3-digit group. Standard errors are clustered by 3-digit NACE group.

Table A17: State participation (Treat × Post) and log sector assets, progressive sector FE (4-digit)

	Log assets					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated (ever)	0.616***	0.677***	0.639***	0.663***	0.674***	
	(0.109)	(0.105)	(0.104)	(0.107)	(0.145)	
Treat × Post	0.616***	0.665***	0.657***	0.559***	0.457***	0.219**
	(0.143)	(0.133)	(0.133)	(0.126)	(0.123)	(0.106)
Log assets, first observed year	0.623***	0.573***	0.555***	0.553***	0.532***	
	(0.033)	(0.032)	(0.033)	(0.031)	(0.040)	
Year FE	✓	✓	✓	✓	✓	✓
Section FE		✓				
Subsection FE			✓			
Division FE				✓		
Group FE					✓	
Class FE						✓
Dependent variable mean	4.270	4.270	4.270	4.270	4.259	4.270
Observations	9,818	9,818	9,818	9,818	8,193	9,818
Adjusted R^2	0.621	0.640	0.653	0.664	0.765	0.852

***, **, and * indicate $p < .01$, $p < .05$, and $p < .1$ significance levels, respectively. The outcome is the 4-digit NACE class's log aggregate assets, 1946–71, excluding the financial section J and restricted to strictly positive sector-years. Treatment is `first_treat_no1936`, the 4-digit NACE class's earliest firm-acquisition year excluding the 1936 cohort: *treat* $= 1$ if the 4-digit NACE class is ever treated, *post* $= 1$ from that year onward, and Treat×Post is the absorbing DiD indicator. Never-treated sectors are always pre-period, so *post* is collinear with Treat×Post and dropped; *treat* is absorbed once a sector fixed effect is added. Every column controls for the 4-digit NACE class's log assets in its first observed year and includes year fixed effects; columns add the sector fixed effect at progressively finer granularity: none, section, subsection, division, 3-digit group, and 4-digit class. Standard errors are clustered by 4-digit NACE class.

B Additional Figures

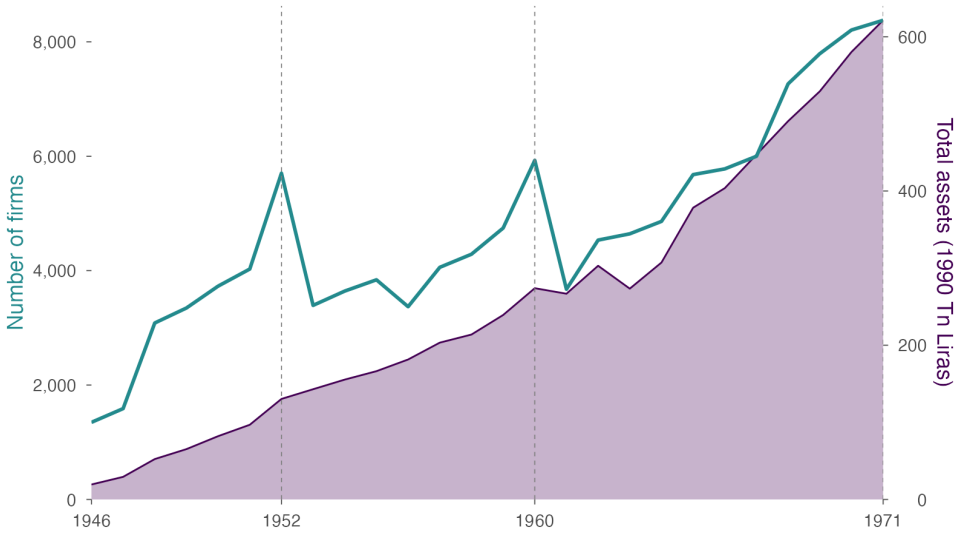
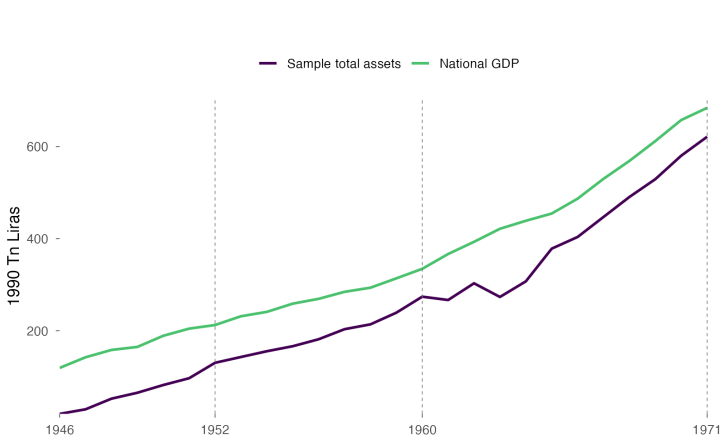
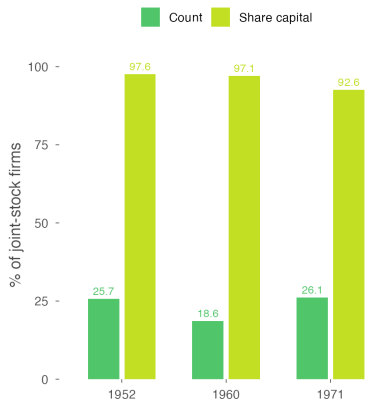


Figure A1: Count and size of firms in sample.



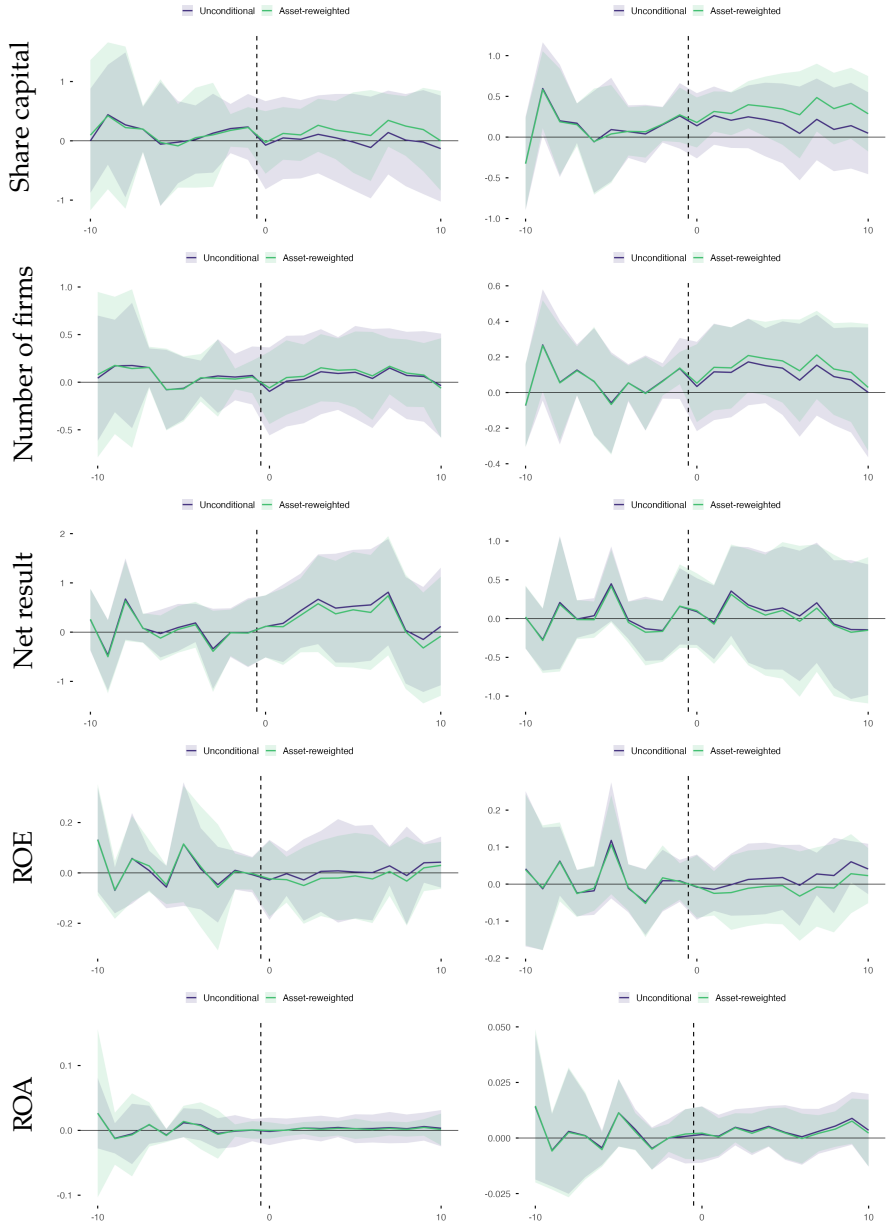
(a) Sample growth and GDP growth.



(b) Coverage of joint-stock firms.

Figure A2: Imita.db's representativeness.

GDP figures from Baffigi (2011), database firm coverage from Vasta (2006).



(a) 3-digit group (b) 4-digit class

Figure A3: Sector-level DD event studies: unconditional vs. asset-reweighted (remaining outcomes)

Companion to Figure 8 for the remaining outcomes of Table A15: share capital, number of firms, net result, ROE and ROA, at the 3-digit group (column a) and 4-digit class (column b) levels. Each panel overlays the unconditional estimate and the asset-reweighted estimate—inverse-probability weights on the sector's baseline (entry-year) log assets; see the in-panel legend—with 95% uniform confidence bands; the vertical dashed line marks the last pre-participation year. Never-treated sectors form the control group; section J is excluded and the panel spans 1946–71.

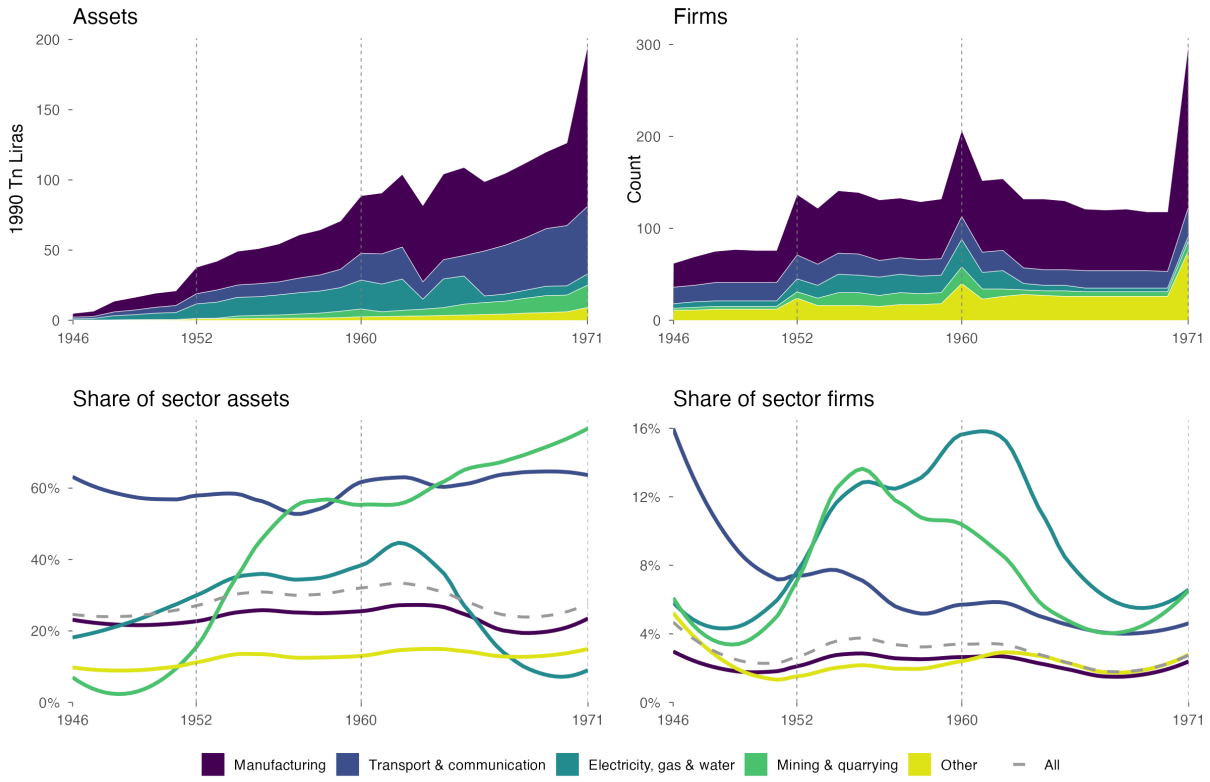
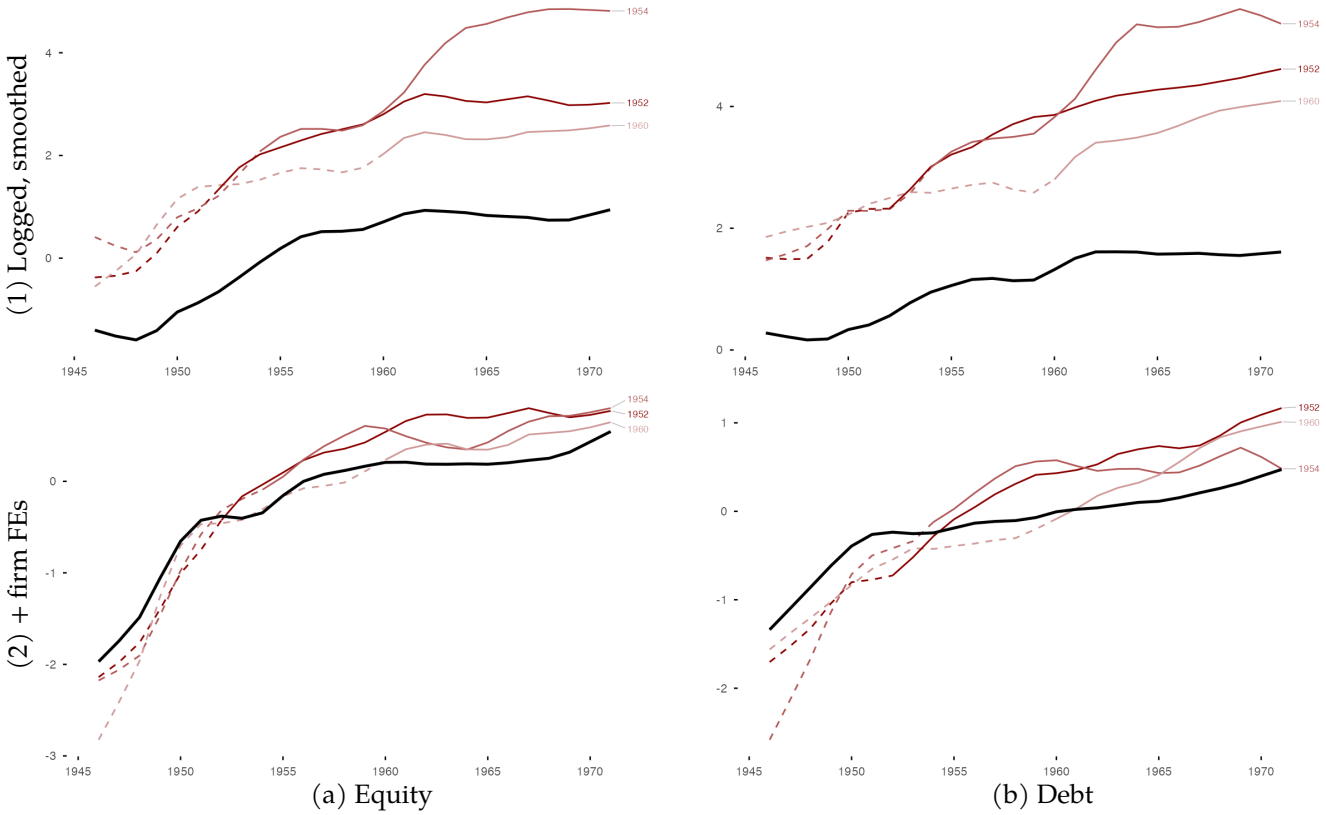


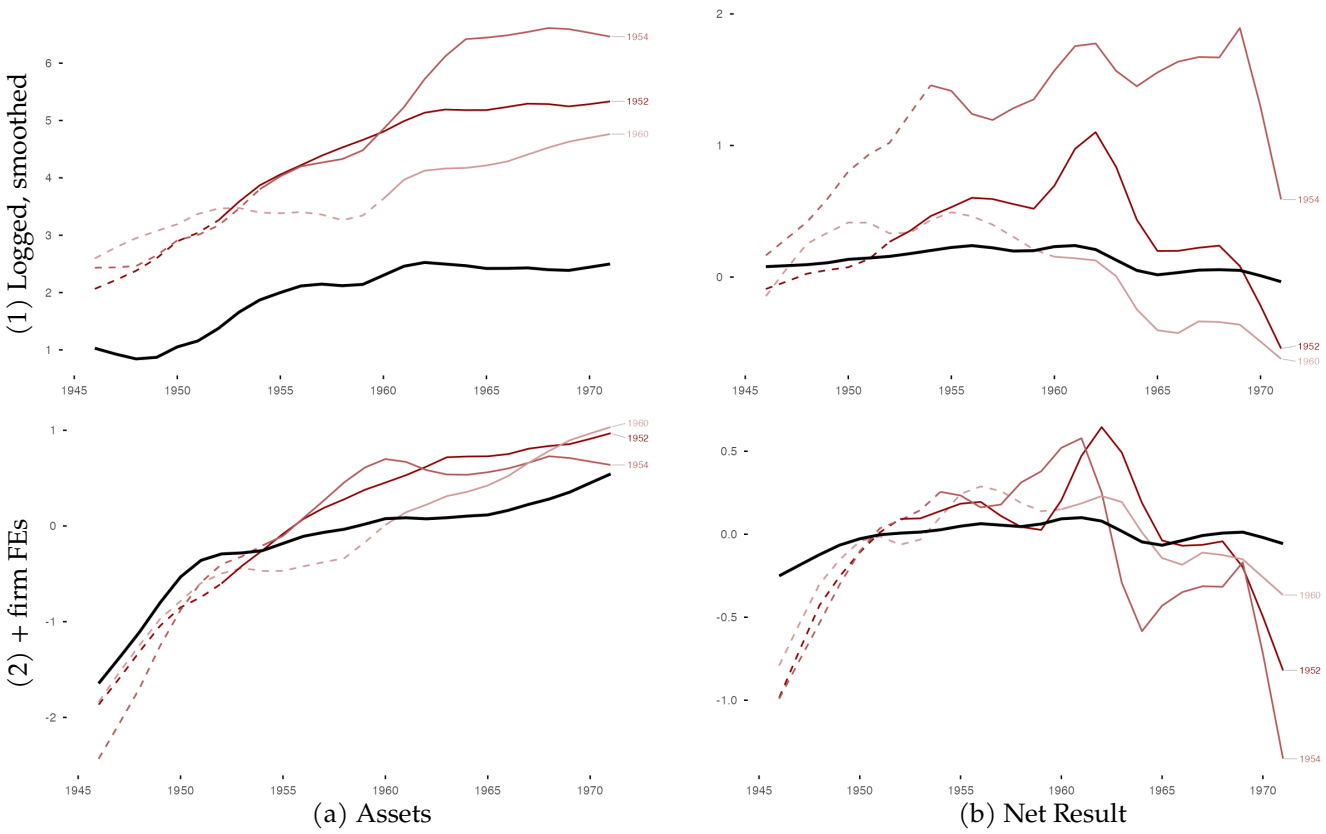
Figure A4: State holdings' participations by economic sector

Participations by SHs across one-digit NACE sectors (*sezione*) over 1946–1971; a firm counts as participated from the year it first appears as state-held onward, as in Equation 1. The top row reports levels: (left) participated firms' assets, summed within section and expressed in trillions of 1990 lire, and (right) the count of participated firms, both as stacked areas. The bottom row reports each sector's within-sector participation intensity: (left) participated assets as a share of all assets in the sector, and (right) participated firms as a share of all firms in the sector, loess-smoothed. The four sections with the largest participation footprint are shown individually; section J (financial intermediation) is excluded; all remaining sectors are pooled into "Other".



(a) Equity (b) Debt

Figure A5: Cohort-level averages over time, treated (1952, 1954, 1960; lines dashed before treatment) and never-treated.



(a) Assets (b) Net Result

Figure A6: Cohort-level averages of assets and net result, treated (1952, 1954, 1960; lines dashed before treatment) and never-treated.

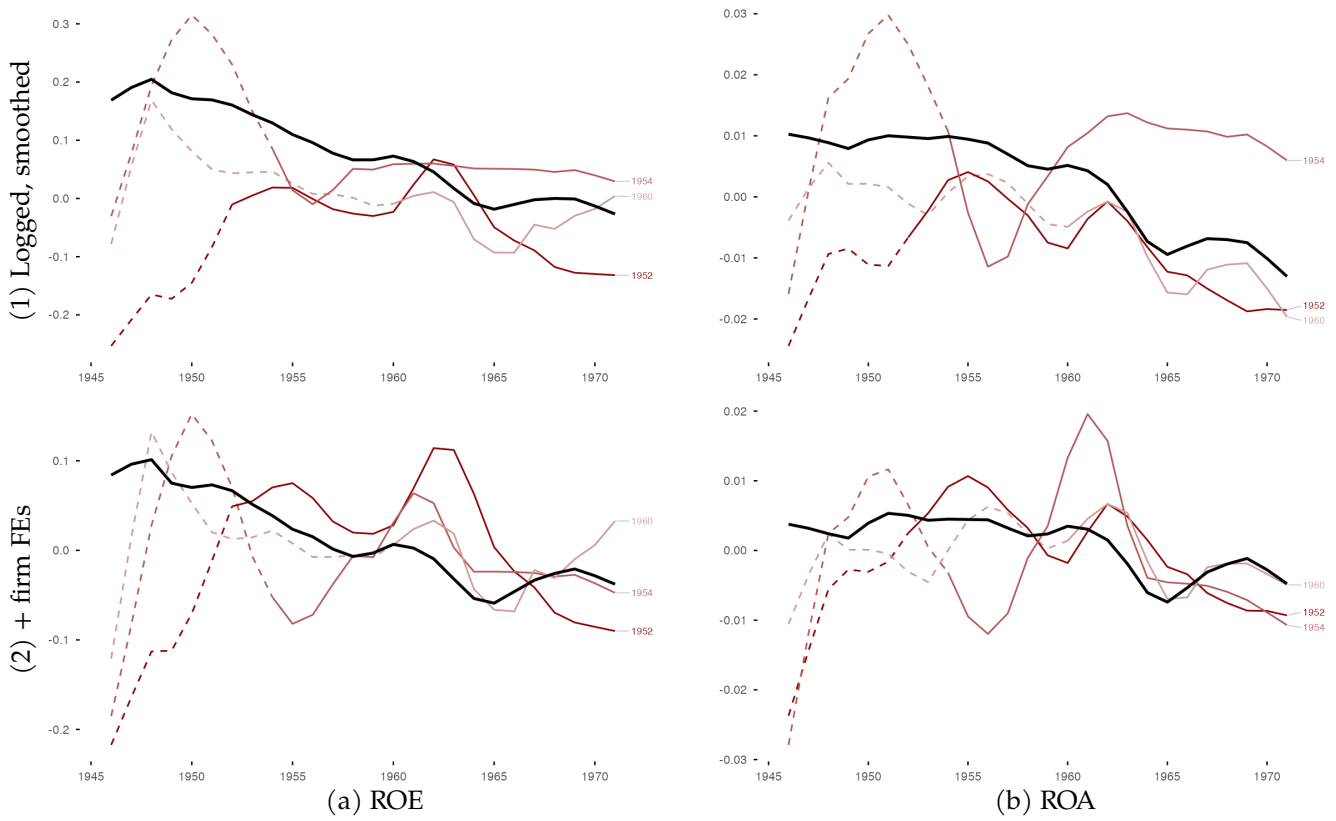


Figure A7: Cohort-level averages of balance sheet ratios, treated (1952, 1954, 1960; lines dashed before treatment) and never-treated.

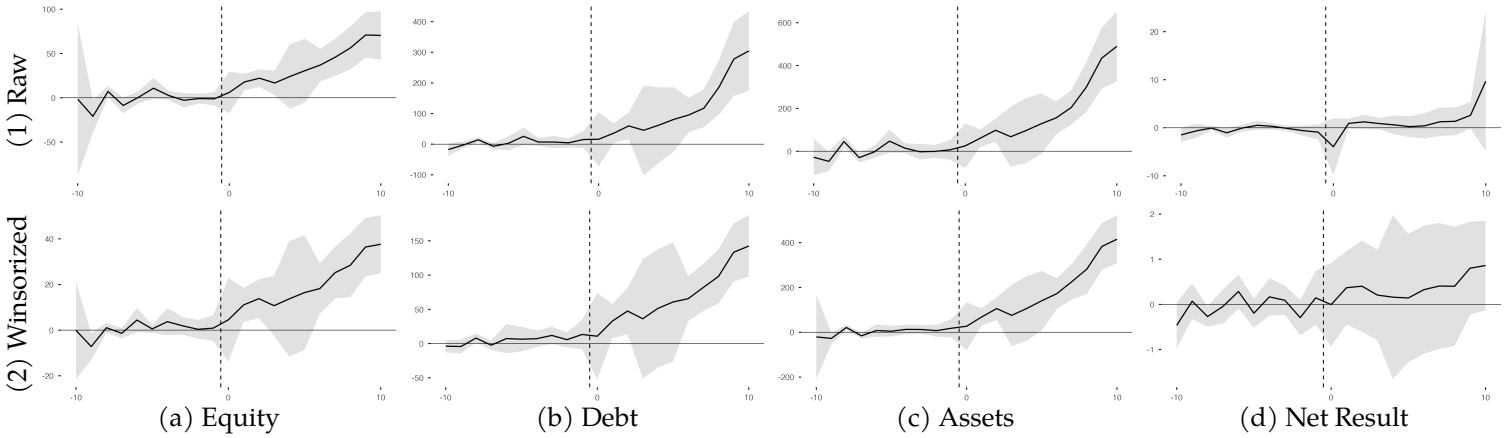
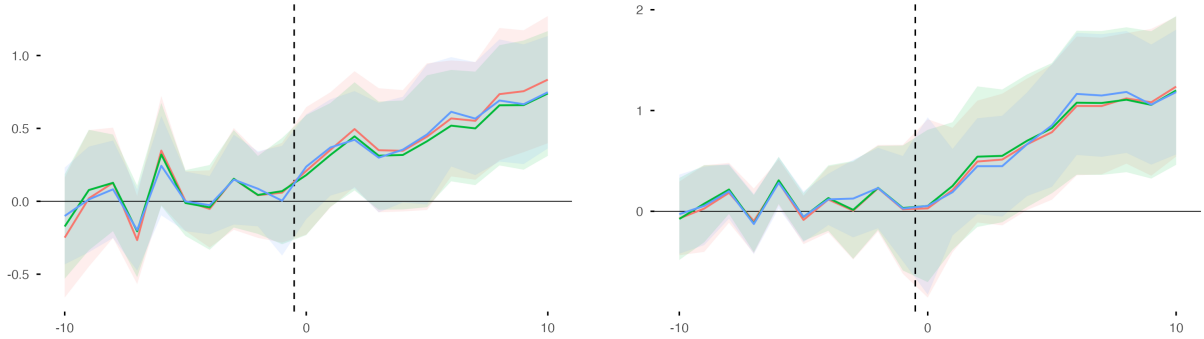
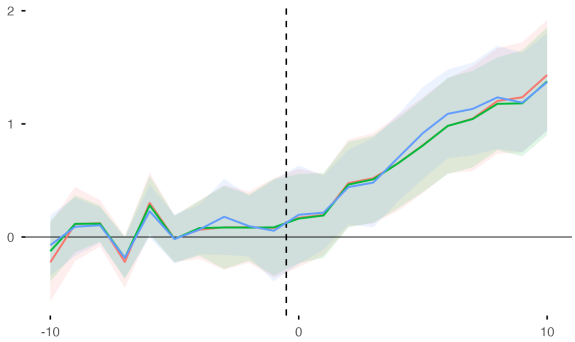


Figure A8: Difference-in-differences event-study estimates on balance sheet values. Columns correspond to equity, debt, assets, and net result; rows report the raw and winsorized specifications.

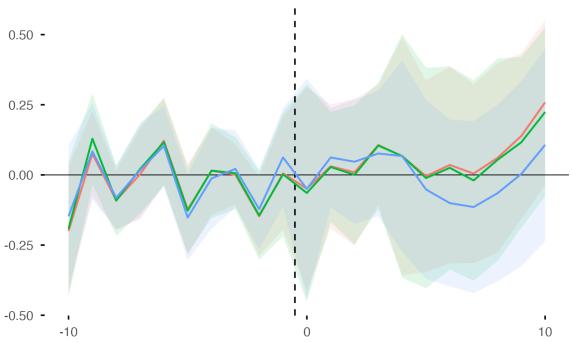


(a) Share capital

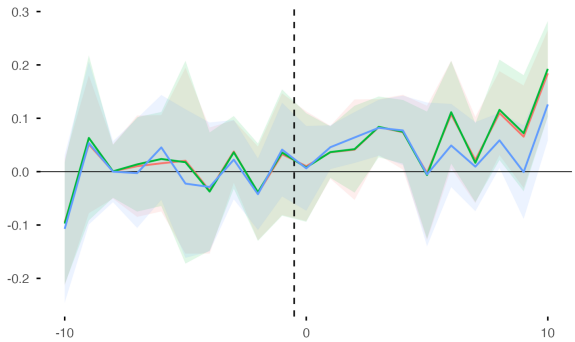
(b) Debt



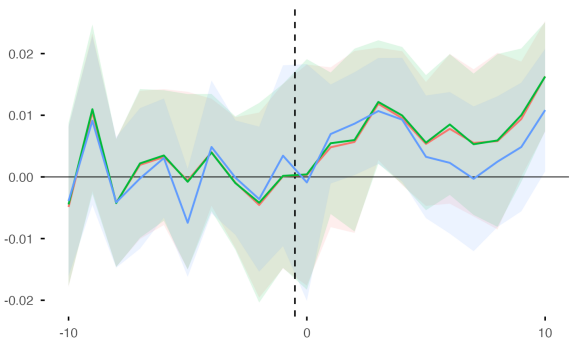
(c) Assets



(d) Net result



(e) ROE



(f) ROA

Figure A9: DD event studies: robustness to propensity score reweighting on pre-treatment covariates

Studies on the outcomes of Table A3. Each panel plots yearly coefficient estimates for SP, with 95% confidence bands; the vertical dashed line marks the last pre-participation year. Never-treated firms form the control group, reweighted based on propensity scores estimated on baseline covariates (Size, + Leverage, + ROA) as outlined in Callaway and Sant'Anna (2021). Corresponding joint ATTs reported in Table A3.