When the State Takes Over: Nationalization, Firm Performance, and Political Backlash*

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Abstract

We study the economic effects of a large nationalization program using newly assembled firm-level data from Chile under Salvador Allende (1970–73). Using a difference-in-differences design, we show that nationalization substantially reduced firm performance and international business activity relative to comparable private firms. Return on assets fell sharply and importing activity declined, with negative effects concentrated in manufacturing, while firms in strategic and natural resource sectors were largely unaffected. We also document lower electoral support for the incumbent coalition in more exposed municipalities. Overall, nationalization generated sizable and uneven economic costs with significant political consequences.

Keywords: nationalization, state-owned enterprises, firm performance

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1 Introduction

The ownership and management of firms is a prominent way in which the state intervenes in the economy (Atkinson and Stiglitz, 1980). Motivations for the "nationalization" of existing firms include efficiency gains in natural monopolies, improved investment coordination, economic stabilization following emergencies, egalitarian objectives, and control over industries deemed strategic. Examples abound, ranging from the targeted nationalization of firms in France and the United Kingdom after World War II, to the nationalization of natural resources such as oil and gas in Latin America, and to the broad expansion of state ownership in Eastern Europe and the Soviet bloc.[1] Nationalization strategies are not confined to the past: in the first two decades of the twenty-first century, state ownership expanded considerably, particularly following the global financial crisis of 2008 and the COVID-19 pandemic (EBRD, 2020; Megginson et al., 2025). Despite their importance, empirical evidence on the consequences of nationalization programs remains scarce.[2]

We empirically evaluate one of the most ambitious nationalization programs ever implemented. Salvador Allende and his Popular Unity (UP) coalition governed Chile between November 1970 and September 1973 with the aim of pursuing deep economic and social transformations. Nationalization stood at the center of this agenda, alongside higher wages, price controls, and the expansion of state control over banks and large firms viewed as strategic to development (Popular Unity, 1969). Through a combination of constitutional reform, negotiated acquisitions, and administrative interventions, the state rapidly expanded its ownership: by 1973, state-owned enterprises accounted for nearly 40 percent of GDP (Hachette, 2000). This transformation reshaped firm ownership, disrupted business groups, and generated intense domestic and international conflict amid growing macroeconomic instability and political polarization. Because nationalization unfolded in such a contested environment, our analysis captures the economic and political consequences of being targeted by the UP program, rather than the effect of public ownership in isolation.

We assemble a new panel dataset of large Chilean firms using historical administrative records

[1] France nationalized electricity and automobile companies in 1945-46, while the United Kingdom pursued large-scale nationalization of railways, coal, steel, electricity, and gas under the leadership of Clement Attlee and the Labour Party. In Latin America, prominent examples include the nationalization of oil companies in Mexico (1938) and Venezuela (1976), as well as more recent takeovers of gas, mining, and key services in Bolivia and Argentina.

[2] We do not evaluate temporary nationalizations implemented as stabilization policies with explicit exit strategies, such as Northern Rock in the United Kingdom (2008) or AIG in the United States (2008).

that we manually collected from state libraries. At the time, firms with dispersed ownership were required to submit annual business reports to a regulatory agency. From these reports, we construct a panel dataset of 71 major firms observed annually between 1967 and 1973. We digitize balance-sheet and income-statement information and read approximately 400 reports to extract additional unstructured information on business activities. In particular, the reports contain "business letters" to shareholders that reveal whether firms imported inputs or exported products, disclose relationships with domestic and international banks, and provide descriptions that allow us to identify the sectors in which firms operated. We then identify which firms were nationalized under the Allende government using a range of historical sources. Descriptive statistics show that nationalized firms were initially larger and more internationally connected, but these differences largely disappear once we account for firm size and sector.

We use a difference-in-differences design that compares firms nationalized by the UP government with privately owned firms in the same sector. Our first main finding is that the nationalization program under the UP worsened the performance of newly public firms relative to otherwise similar firms that remained private throughout the period. Firms targeted by the UP program experienced an 11 percentage point decline in returns on assets, equivalent to about 60 percent of baseline returns. Firms taken over by the Allende government also disengaged from international business relations, as reflected in the disappearance of importing activities documented in business correspondence with company owners. These results are robust to a broad battery of specification checks and alternative estimators that account for baseline differences and the timing of the nationalization program. Evidence from aggregate trade flows between the United States and Chile further highlights the role of international business linkages in explaining these findings, and is particularly relevant in the Cold War context, when international trade has been shown to respond strongly to political alignments and diplomatic tensions between countries (Berger et al., 2013).

Importantly, these average effects mask substantial heterogeneity across types of firms. The negative impact of nationalization is concentrated in manufacturing, the sector where political conflict and resistance to the UP government were most intense, while we find no evidence of adverse performance effects among firms operating in strategic industries or in natural resource sectors. This pattern is consistent with the historical record, which emphasizes that manufacturing was the main arena of confrontation between the government and the opposition, whereas strategic

and resource-based sectors lay closer to the core of the UP's development strategy and administrative priorities. Moreover, analysis of contemporaneous surveys indicates that the decline in firm performance was accompanied by a decline in the wage premium paid by public firms, suggesting that workers directly experienced the economic costs of nationalization. Taken together, these results indicate that the economic consequences of the program were highly uneven across sectors and that its most negative effects emerged precisely where political and institutional frictions were strongest, a feature that is central for understanding its broader implications.

We then examine the downstream political consequences of the nationalization program. Building a panel of municipalities across seven elections between 1961 and 1973, and combining it with detailed information on the geographic location of nationalized plants, we study whether exposure to nationalization affected electoral support for the incumbent UP coalition. The 1973 Congress Election was perceived as a critical juncture with the potential to radically shape the country's future (Harmer, 2020). Using a difference-in-differences framework, we compare changes in the vote share of Socialist and Communist candidates across more- and less-exposed municipalities. We find that municipalities with greater exposure to nationalized firms exhibited systematically lower support for the coalition in the 1973 election—on the order of 1–4 percentage points—while pre-treatment trends are flat and estimates are small and imprecise before the nationalization program was implemented. These results are robust to alternative specifications, parametric restrictions, and controls for contemporaneous policies such as land reform, tariff protection, and the milk program, as well as to controls for the presence of non-nationalized plants, which proxy for local industrial density and urban political intensity. Taken together, the evidence suggests that the economic costs of nationalization translated into meaningful electoral punishment for the incumbent government.

Our main contribution is to provide an empirical evaluation of a large nationalization program using firm-level data and a rigorous research design. Nationalization programs have varied in prominence throughout the last century. The oil industry has been particularly exposed to nationalization, with expropriations rising in popularity in the 1970s and again in the 2000s, when oil prices increased (Stroebel and Van Benthem, 2013). Given the central role of oil in the economy, most empirical research on nationalization focuses on this industry (Chang et al., 2018).[3]

[3]There is also a rich theoretical literature studying how expropriations arise in equilibrium and how they affect investment and production, particularly in natural resource industries. See, for example, Tobin (1974); Thomas and Worrall (1994); Bohn and Deacon (2000); Engel and Fischer (2010).

Prominent findings from this literature show that states are more likely to take over foreign-owned oil companies when commodity prices are high and institutions are weak, and that such expropriations negatively affect productivity and foreign direct investment (Guriev et al., 2011; Melek, 2020; Lucke and Rehfeldt, 2023). Yet systematic evidence outside the oil sector, and based on firm-level panel data, remains limited. This gap is perhaps unsurprising, given the targeted nature of most expropriations, their endogenous timing, and the small number of affected firms, which complicate the construction of credible counterfactuals.

Notable exceptions include empirical studies examining the resurgence of state ownership in the early twenty-first century. In China, researchers document the political and economic determinants of firm selection into nationalisation, with mixed evidence on subsequent firm performance (Huang et al., 2021; Zhan, 2023). Similar questions regarding selection have been studied in the context of re-nationalizations in Russia (Chernykh, 2011).[4] In contrast, we study a country-wide nationalization program triggered by the unexpected outcome of an election (Girardi and Bowles, 2018). This setting mitigates concerns about the endogenous timing of state acquisitions and facilitates the implementation of a transparent difference-in-differences research design.

Our analysis of firm performance under state ownership also relates to the broader literature on ownership structure and firm outcomes. Most existing research studies transitions from state to private ownership, rather than the reverse process that we analyze. The focus on privatization reflects its prominence in the 1970s and 1980s, driven in part by political support from leaders such as Margaret Thatcher in the United Kingdom (Vickers and Yarrow, 1997). Empirical work in the 1990s documented substantial productivity advantages of private over state-owned firms, contributing to a broad consensus during a period of declining state ownership worldwide (Megginson et al., 1994; Megginson and Netter, 2001). Given large observable differences between public and private firms, much of this literature relies on privatization reforms to study ownership effects (e.g. La Porta and López-de-Silanes 1999). Explanations for superior private-sector performance emphasize political incentives that distort resource allocation in state-owned firms, and the efficiency gains from "depoliticization" under private ownership (Shleifer and Vishny, 1994; Barberis et al., 1996; Shleifer, 1998). More recent work adds nuance by highlighting how privatization processes

[4]Related empirical work also examines stock market reactions to nationalizations in France and the United States (Langohr and Viallet, 1986; Ang and Boyer, 2011).

are implemented in practice and how irregularities can generate negative consequences for firms and society (Fisman and Wang, 2014; González et al., 2020). In contrast, much less is known about large-scale nationalization programs that substantially expand state ownership.[5]

Finally, our results linking nationalization to lower electoral support for the incumbent coalition contribute to the literature on how policies shape political outcomes in electoral democracies. Prior research shows that health, education, financial, infrastructure, and anti-poverty policies can influence political preferences and voting behavior (e.g. Manacorda et al. 2011; Clinton and Sances 2018; Aidt et al. 2024; Voigtländer and Voth 2026). By comparison, relatively little is known about the political consequences of public ownership. While public firms may generate political support when they provide visible benefits to voters (Atal et al., 2024), their electoral effects likely depend on performance and perceived value added. We contribute to this literature by providing novel evidence that poorly performing public firms can erode political support for incumbent coalitions in high-stakes elections.

2 The Nationalization Program

By the late 1960s, Chile combined rising economic concentration with a growing and increasingly mobilized left (González, 2013). Demands for redistribution intensified, and support for a socialist alternative gained traction. In this context, Salvador Allende, a prominent leader of the Socialist Party, was elected president in September 1970 with 36.6 percent of the vote, heading a broad left-wing coalition known as Popular Unity (UP).[6] Because no candidate obtained an absolute majority, congressional confirmation was required, and the two months that followed the election were marked by intense political polarization. The United States closely monitored the process and covertly sought to prevent Allende's inauguration, while domestic actors debated the constitutional path forward (Kornbluth, 2003; Qureshi, 2009). After the assassination of General René Schneider, a key advocate of military non-intervention whose death galvanized support for constitutional continuity, Congress confirmed Allende, and he took office on November 4, 1970.

[5] A related literature compares outcomes across private and state-owned firms after adjusting for observable characteristics (e.g. DeWenter and Malatesta 2001; Borisova et al. 2015; De Haas et al. 2025).

[6] More details about the Salvador Allende government can be found in de Vylder (1976); Boorstein (1977); Sigmund (1977); Corvalán (2003); Harmer (2011); Fermandois (2013); Amoros (2013); Mansuy (2023), among others.

The UP program sought to chart a "democratic road to socialism," combining institutional continuity with economic transformation (Popular Unity, 1969). In his first message, Salvador Allende was clear: "The establishment of the area of social ownership does not signify the creation of state capitalism, but the beginning of a truly socialist structure." The associated economic strategy, commonly known as the *Vuskovic plan* after the Minister of the Economy Pedro Vuskovic, rested on the intensification of land expropriations in the context of agrarian reform, the nationalization of banks and firms in strategic sectors, price controls, and substantial increases in wages for workers (Larrain and Meller, 1991; Girardi and Bowles, 2018). The diagnosis motivating this strategy emphasized concentrated markets, monopolistic practices, and widespread "excess capacity" among leading firms, which were believed to constrain growth and perpetuate inequality (Dornbusch and Edwards, 1990; Edwards, 2023a). Nationalization and higher spending were intended to reorient production, coordinate investment, and redirect surpluses toward social and redistributive goals.

The early macroeconomic consequences were striking. Government spending expanded rapidly and was largely financed by increases in money supply and domestic credit. In 1971, GDP growth accelerated and unemployment fell, reflecting a short-lived boom. Yet the imbalances soon became apparent: fiscal deficits widened, inflation began to accelerate, and shortages and black markets became widespread from 1972 onwards. GDP growth turned negative in 1972 and 1973, and real wages fell sharply as the economy overheated (Dornbusch and Edwards, 1990; Edwards, 2023b). By 1973, inflation had reached 441 percent and the central government's fiscal deficit was about 25 percent of GDP (Lüders, 1993). These economic developments unfolded amid growing political conflict. Disagreements within the governing coalition, resistance from opposition parties, and foreign pressures in the context of the Cold War combined to produce one of the most polarized periods in Chilean history (Sigmund, 1977; Valenzuela, 1979; Haslam, 2005).

A central component of the UP's economic strategy was the nationalization of large firms and banks. The plan was to first take control of the largest companies in the country and then extend public ownership to a broader set of firms. Nationalization was conceived as a means of reshaping the productive structure, disciplining perceived monopolistic behavior, and channeling resources toward social and investment priorities (Popular Unity, 1969).[7] In parallel, the government sought to gain control over the banking system, which would allow state-owned firms to access credit on

[7]Online Appendix A1 presents the outline of the nationalization strategy as defined in the UP program.

favorable terms. The best-known component of this agenda was the 1971 nationalization of copper, which eliminated remaining U.S. ownership in the sector and became a focal point of conflict with American companies and the U.S. government (Sigmund, 1974; Qureshi, 2009; Edwards, 2025).[8]

The legal and institutional implementation of nationalization was heterogeneous and at times contentious.[9] Regarding the well-known case of major copper companies with U.S. investments, Congress approved a constitutional reform in 1971 that authorized nationalization with broad political support. In manufacturing, finance, and other sectors, the government relied on a combination of existing legislation, negotiated purchases, and administrative instruments. A particularly important tool was a 1932 law that allowed the executive to take control of firms "whenever a strike or a breakdown would be against the public interest." Allende's government invoked this law to intervene in firms facing labor conflicts or operational disruptions, a practice that expanded the public sector beyond the initially targeted "strategic" enterprises (Larrain and Meller, 1991). State agencies, especially the Production Development Corporation (CORFO), played a key role in acquiring ownership stakes, managing newly public firms, and coordinating activity across sectors.

The scale of the nationalization program was unprecedented. While state-owned companies represented about 14 percent of gross domestic product (GDP) in 1965, by 1973 this share had nearly tripled to 39 percent, and CORFO owned more than 500 firms (Hachette, 2000). The expansion of public ownership affected a wide range of industries, from manufacturing and banking to services and natural resources, reshaping the landscape of Chilean capitalism and disrupting the development of modern business groups that had emerged in previous decades (Larrain and Meller, 1991; Rojas, 2015; Aldunate et al., 2020). Nationalization thus altered not only ownership structures, but also the networks linking firms, banks, and business elites, all with important international ramifications. The copper nationalization and related expropriations were interpreted in

[8]The copper nationalization primarily affected large US-owned multinationals such as Kennecott Copper Corporation and Anaconda Copper Company, whose Chilean operations were taken over through a constitutional reform and gave rise to protracted disputes over compensation. These firms do not appear in our firm-level dataset because they were foreign-incorporated corporations operating in Chile under special concessionary regimes and were not subject to domestic securities regulation or reporting requirements. By contrast, our data include Chilean-incorporated mining companies organized as joint-stock firms, which were legally required to submit annual reports to the Superintendencia de Valores y Seguros and were nationalized through ordinary administrative or negotiated channels.

[9]The legal and political process through which firms were nationalized was highly contested and relied on a mix of failed legislative initiatives, negotiated acquisitions, share purchases, and the use of pre-existing emergency laws. Opposition parties, courts, business owners, and even factions within the governing coalition actively resisted or reshaped these efforts. See Online Appendix A2 for a detailed discussion drawing on de Vylder (1976).

Washington as a direct challenge to U.S. economic interests, enabling the Nixon administration to invoke existing legislation to suspend bilateral assistance and to oppose new lending to Chile by multilateral institutions (Sigmund, 1974; Qureshi, 2009). Commercial banks cut back on credit, and access to foreign finance became more limited, complicating the government's ability to maintain imports and sustain production. Historians debate the relative importance of these external pressures vis-à-vis domestic policies, but there is broad agreement that nationalization became a central axis of conflict at home and abroad (Sigmund, 1974; Aldunate et al., 2024).

By 1973, a substantial segment of the industrial sector and the banking system had moved under state control, alongside intensified land reform and expanded social programs. These simultaneous transformations strained administrative capacity and fiscal resources. Mounting economic difficulties and political tensions culminated in the military coup of September 11, 1973, which deposed Allende and initiated a seventeen-year dictatorship (Huneeus, 2006). The new regime quickly reversed many of the nationalizations, returning firms to previous owners or privatizing them, and set the stage for a radical shift toward market-based economic policies.

3 Research Design

3.1 Data construction

We construct a new panel dataset of firms using historical administrative records that we manually collected from state libraries. During the period we study, large firms and firms with dispersed ownership were required to submit annual business reports to the *Superintendencia de Valores y Seguros* (SVS), a regulatory agency equivalent to the U.S. Securities and Exchange Commission. This reporting requirement applied to Chilean-incorporated joint-stock companies (*sociedades anónimas*), and as a result the SVS archives contain detailed information for many of the country's most economically important firms.[10] Together, these firms accounted for a substantial share of industrial output, employment, and domestic market activity during the period.

[10]These include major industrial producers such as Compañía Manufacturera de Papeles y Cartones, Cemento El Melón, and Elecmetal; key utilities and infrastructure firms such as Empresa Nacional de Electricidad and Compañía de Gas de Santiago; large transport and shipping companies such as Compañía Sud Americana de Vapores; and prominent food, beverage, and consumer goods producers such as Compañía Cervecerías Unidas and Carozzi.

Using all available reports from the SVS, we construct an unbalanced panel of 71 firms observed annually between 1967 and 1973. The panel is unbalanced because some reports are missing or lack information on specific variables: 29 firms are observed in all seven years, 32 firms are observed for four to six years, and 10 firms are observed for two to three years. We drop firms observed only once, as firm fixed effects cannot be estimated for them. These firms were key players in their industries, and SVS reports have been widely used in previous research to study Chilean firms and corporate governance in later decades (Khanna and Palepu, 2000; Martínez et al., 2007; González and Prem, 2018, 2020; González et al., 2020; Aldunate et al., 2020). Our contribution is to assemble and exploit these historical records to study firm behavior during the Allende period and to analyze one of the largest and most consequential nationalization programs in history. Before describing how we construct the main variables used in the analysis, we briefly address a potential concern arising from the unbalanced nature of the panel.

Because the panel is unbalanced, a potential concern is that missing observations are systematically related to firm characteristics. Table A1 anticipates this issue by examining whether observable firm characteristics—measured using the variables described below—predict the number of missing firm-year observations, separately for the pre- and post-Allende periods. We find no evidence that missingness is related to nationalization status, firm size, profitability, international exposure, or banking relationships. Joint tests fail to reject the null that these variables have no explanatory power, suggesting that attrition in the panel is unlikely to bias our estimates.

Business reports present structured and unstructured information about business operations and provide a unique window to understand the Chilean economy in the 1960s and 1970s. Using the reports as our main source of information, we proceed in three steps. First, we manually digitize measures of assets, debt, earnings (before interest and taxes), and sales from balance sheets and income statements. Although the exact format changes slightly, balance sheet information is relatively structured and homogenous across firms and reports. Second, we read all of the approximately 400 reports to extract additional information from unstructured parts of reports. One key part is a section with paragraphs describing business activities for the owners of the company. These "business letters" allow us to learn if a firm was importing (i.e. inputs) or exporting products each year in the 1967-1973 period, information that was critical for owners and thus always communicated. Similarly, the reports always begin with a description of operations, from which

we can classify the sector in which a firm operated. And finally, the cover of reports presents information about the banks—domestic, international, or both—providing financial services to the firm. And third, we identify which of these 71 firms were nationalized by the Salvador Allende government from a wide variety of historical sources including academic articles, books, the business reports themselves, state agencies, and special commissions (Farías, 2000; Corvalán, 2003; Congress Report, 2004; CORFO, 2009; Barros, 2013; Aguirre-Briones, 2019).

Table 1 provides descriptive statistics for the 71 firms in our final dataset. To characterize companies before the UP government, we restrict attention to the 1967–1970 period, when all firms were privately owned, and describe all the variables in our dataset. The first takeaway from this description is that nationalized and non-nationalized firms differed along several dimensions prior to Salvador Allende's rise to power. Nationalized firms were larger, more indebted, had more international business relations and banking ties, and were more likely to operate in the secondary sector, i.e. manufacturing industries such as textiles. However, most of these differences are accounted for once we control for firm size and the sector in which firms operated.[11] Column 1 presents the mean and standard deviation for the 32 firms that were nationalized, column 2 presents the same statistics for the 39 firms that remained privately owned throughout the period, and column 3 reports the difference in means between the two groups. Column 4 shows that most differences disappear after controlling for sector fixed effects and log assets. Finally, column 5 indicates that none of the variables in our dataset statistically predicts the timing of nationalization.

3.2 Difference-in-differences strategy

To estimate the impact of Salvador Allende's nationalization program on firm-level performance, we focus on the panel dataset of 71 firms observed annually between 1967 and 1973. In particular, we estimate the following difference-in-differences regression equation:

$$Y_{ijt} = \sum_{k} \beta_k \left(N_i \times T_k\right) + \phi_i + \phi_{jt} + \varepsilon_{ijt} \tag{1}$$

[11] The number of U.S. banks remains higher for firms that were later nationalized even after controlling for size and sector, motivating a robustness exercise which shows that our main results are not driven by this imbalance.

where Y_{ijt} is a performance outcome for firm i, which operates in sector j, in year t. The indicator N_i takes the value of one for firms that were nationalized during the Allende administration and zero otherwise. Importantly, we do not observe nationalizations between 1967 and the 1970 presidential election, i.e. the baseline period in our analysis. Each one of the indicators T_k takes the value of one for the corresponding year k, with $k = 1969$ as the omitted category. We absorb unobserved differences across firms with fixed effects ϕ_i and common shocks by sector with fixed effects ϕ_{jt}. Given the economic shocks experienced by the country in 1970-1973, the latter fixed effects are particularly important to account for the cycles of the economy. Finally, the robust error term ε_{ijt} is clustered by firm to allow for arbitrary serial correlation within firms over time.

Our interest is on the estimated parameters $\widehat{\beta_k}$, which we interpret as measuring the differential performance of firms nationalized by Salvador Allende during the 1970-1973 period. The comparison group is composed by other contemporaneous firms operating in the same sector j. Therefore, the identification assumption is that in the absence of the nationalization program implemented by the Allende government, firms that were nationalized would have evolved similarly to other firms in the same sector that were never nationalized. We provide some evidence supporting this assumption by looking at the prevalence of parallel trends in the 1967-1969 period.

To facilitate the interpretation of results, we also provide estimates from a more parsimonious econometric specification. More precisely, we complement equation (1) with the following:

$$Y_{ijt} = \beta (N_i \times T_t) + \phi_i + \phi_{jt} + \varepsilon_{ijt} \tag{2}$$

where everything is defined as in equation (2) but the main difference is that now T_t takes the value of one for the years 1971-1973 and zero otherwise. That is, we estimate the average differential performance of nationalized firms during the Salvador Allende period. In this case, we are interested on the singles estimated parameter $\widehat{\beta}$, which is a convex combination of $\widehat{\beta_k}$. In the presence of parallel trends between nationalized firms and comparison firms in the baseline period, the specification in equation (2) can also be interpreted as a parametric version of equation (1).

We use four variables related to firm performance: return on assets, sales over assets, an exporting indicator, and an importing indicator. The former two outcomes are related to the overall business operations of firms and capture their profitability. The latter two outcomes capture the

involvement of firms in international business relations. The Allende administration was characterized by the promotion of domestic activity, and the retraction from international business networks. Note that we are intentionally agnostic about the theoretical relationship between the two pairs of outcomes. Indeed, the profitability of firms could have been impacted by the involvement in international business relations. We view these outcomes as summarizing the operation of firms and leave the empirical examination of other firm-level outcomes for the Online Appendix.

In our context, the difference-in-differences design is preferable to methods that exploit the fact that firms were nationalized in different years under the Allende administration. Some firms were nationalized immediately in early 1971, while others only did so in 1972. Therefore, we could have chosen other research designs to estimate the parameter of interest. In particular, we could have relied on the staggered implementation of nationalizations and implement the appropriate recently developed difference-in-differences methods (e.g. Borusyak et al. 2024). However, we argue that the relevant shock to firms was the arrival of Salvador Allende to office in November 1970. The nationalization program was already in place in the UP economic program, and firms adapted their business decisions immediately after learning that a left-wing coalition would be in power. In econometric terms, we worry about anticipation effects when implementing a staggered design. Nevertheless, the Online Appendix presents results with that alternative method for completeness.

4 Firm Performance

4.1 Difference-in-differences results

We find that nationalization leads to lower firm performance and retraction from international business relations. Figure 1 presents estimates of equation (1) which we consider to be our main set of results. All panels present the estimated coefficients $\widehat{\beta}_k$ as a black dot and the 95 percent confidence interval as a vertical black line. The estimates reveal that nationalized firms have lower return on assets (panel A) and lower sales over assets (panel B) under the Allende administration (1971-73). Additionally, we observe that nationalized firms are less likely to export products (panel C), although those coefficients are statistically insignificant at conventional levels, and less likely

to engage in importing activities (panel D).[12] These results are similar when controlling for the baseline size of firms interacted by year fixed effects (Figure A1). Importantly, a joint test for the statistical significance of the coefficients $\widehat{\beta}_{1967}$ and $\widehat{\beta}_{1968}$ reveals that nationalized firms were on similar trends to other firms before Salvador Allende rose to power in November 1970, with p-values of 0.89, 0.14, 0.75, and 0.85 in panels A-D respectively.[13]

To facilitate the discussion of magnitudes, Table 2 presents estimates of equation (2). Even columns include as control the baseline size of firms as measured by assets interacted by year to account for differences across firms. Columns 1-2 show that nationalization leads to a decline in return on assets (ROA) of 11-13 percentage points, a decline of 60-70% from the baseline. The decline in sales over assets is smaller than the decline in ROA, as revealed by columns 3-4. After nationalization, sales decrease by 8-10 percentage points, which represent a 14% decline from the baseline. Taken together, these estimates suggest that the large decline in returns is likely explained by factors different from sales, e.g. costs. Columns 5-6 show a decrease of 3-4 percentage points in the probability of exporting, a 10% decline from baseline, but estimates are statistically insignificant. The last two columns (7-8) reveal a significant decline of 16-23 percentage points in the probability of importing, approximately 25% of the baseline. All of these results are similar when accounting for the size of firms. Crucially, Table A3 provides additional evidence that firms nationalized by Allende were on similar trends to other firms before 1971.[14]

Robustness. The decline in ROA and importing activity after nationalization is robust across a wide range of specifications and alternative estimators, and is not driven by any single firm. Column 1 of Table 3 shows that similar results obtain when collapsing the data into two periods (pre- and post-Allende), addressing concerns about serial correlation (Bertrand et al., 2004). Column 2 shows that the estimates are unchanged when replacing sector-by-year fixed effects with more narrowly defined industry-by-year fixed effects. Column 3 addresses the remaining imbalance in U.S.

[12] Consistent with the 1971 boom fn the chilean economy driven by a sharp increase in public spending, we find suggestive but ultimately statistically insignificant evidence of more sales among nationalized firms in 1971.

[13] Additional results in Figure A2 shows that the assets of nationalized firms were similar to the assets of other firms under Allende (panel A), alleviating concerns about changes in the denominator. In addition, the remaining panels in the same figure show a statistically insignificant decline in the leverage (debt over assets) of nationalized firms (panel B), similar fixed assets (panel C), and a relatively imprecise 50 log-points decline in total sales in 1972-73 (panel D).

[14] For completeness, Table A2 presents parametric estimates for the additional outcomes (log) total assets, leverage, fixed assets, and (log) sales. Again, we do not find systematic evidence of changes among nationalized firms.

banking relationships documented in Table 1 by controlling for pre-Allende links to U.S. banks interacted with the Allende period; the estimated effects remain similar, indicating that our results are not driven by pre-existing financial ties to U.S. institutions.[15] Columns 4–8 show that the results are robust to combining difference-in-differences with several matching procedures (Abadie, 2005; Crump et al., 2009; Yang and Ding, 2018), further strengthening comparability between nationalized and non-nationalized firms. Allowing for small deviations from parallel trends using the honest difference-in-differences approach leads to similar conclusions (Figure A3, Rambachan and Roth, 2023). We also obtain consistent results using staggered difference-in-differences estimators based on the year of nationalization (Figure A4, Tables A4–A5; Borusyak et al., 2024). Finally, Figure A5 shows that no single nationalized firm drives the results.

Heterogeneity analysis. Table 4 exploits the richness of our data and historical setting to study heterogeneity in the effects of nationalization across different types of firms. A first pattern is that the negative impact of nationalization on firm performance is concentrated among manufacturing firms. This is consistent with the historical record, which emphasizes that manufacturing was the main arena of political and economic conflict during the UP years: "it was in manufacturing industry that the most bitter fights between the Left and the rightist opposition took place after December 1970" (de Vylder, 1976, p. 134). In contrast, we do not find evidence of negative effects among firms classified as operating in strategic sectors, defined following the UP program (Popular Unity, 1969) as those central to development, economic sovereignty, and planning, such as energy, mining, transport, and basic industrial inputs. We also do not find negative effects among firms operating in natural resource industries.[16]

These results should be interpreted with caution, as the number of nationalized firms in the strategic and natural resource categories is small, which limits statistical power and implies relatively wide confidence intervals. The absence of statistically significant negative effects in these sectors should therefore be viewed as suggestive rather than definitive evidence of insulation from performance losses. Taken together, the estimates indicate that the adverse effects of nation-

[15]This also suggests that the estimates do not simply capture differential exposure to U.S. financial pressure, but reflect broader economic consequences of being targeted by the nationalization program.

[16]Note that these heterogeneity dimensions are not collinear with the sector fixed effects. Our sector classification follows the standard three-way division into primary, secondary, and tertiary activities, while the categories used here—manufacturing, strategic, and natural resource firms—are more fine-grained and cut across those broad sectors.

alization were most pronounced in manufacturing, where political conflict and resistance were strongest, while other sectors appear to have followed different and potentially more muted trajectories. This heterogeneity highlights the importance of institutional and political context in shaping the economic consequences of nationalization.

4.2 An exploration of trade flows

The historical context, together with results from the previous section, suggest that international business relations are particularly important to understand the effect of nationalization on firm performance. There are two sides to consider in the case of Chilean firms. First, after the arrival of Salvador Allende to power, U.S. President Richard Nixon implemented covert actions to unseat the Chilean socialist President. His actions were part of a broader Cold War strategy to prevent socialist ideas to spread over Latin America (Qureshi, 2009). An important part of the plan was to "make the economy scream," as revealed by the National Security Memorandum 93 and the Church Committee (U.S. Senate, 1975). To maximize the pressure on the economy, an "invisible blockade" was in place, with the U.S. actively blocking the flow of financial resources to Chile (Sigmund, 1974). Second, the economic strategy of the Allende government was to shift production to the domestic economy, which could have altered patterns of international trade. Imports and exports are equilibrium outcomes which reflect both sides of international business relations.

We explore the role of international business relations between the U.S. and Chile using annual bilateral trade data from the Direction of Trade Statistics of the International Monetary Fund. These panel data allow us to partially test if the differential relationship between the U.S. and the Allende administration can potentially explain part of the international disengagement that we observe among new public firms. Methodologically, we use a panel dataset of all countries in Latin America, measures of exports (FOB) and imports (CIF), and a synthetic control to construct a counterfactual of trade flows with Chile if other than Salvador Allende would have been elected president of the country in 1970 (e.g. Abadie et al. 2010). To construct the weights for the synthetic control, we use all years between 1955 and 1969, and the universe of Latin American countries as the donor pool. The treatment period is the Allende government, i.e. the 1970-1973 period.

We find that international trade between the US and Chile was significantly disrupted after the UP government rose to power. Figure 2 presents the main results from the synthetic control anal-

ysis. Overall, we observe both imports (panel A) and exports (panel B) decreasing sharply. These results are robust to using only odd years in the 1955-1969 period to construct the synthetic control while avoiding over-fitting (Ferman et al. 2020, see Figure A6). When comparing the results in panel A and panel B in Figure 2, we can also tell that imports decreased at least one year earlier than exports, consistent with the role of the U.S. changing significantly after the nationalization of copper companies in mid-1971 (Edwards, 2025). The decrease in imports corresponds to approximately 50% of the average imports in 1955-1969, while the change in exports corresponds to a disruption of increasing exports to Chile and a reversal to the level of exports in the early 1960s.[17]

In sum, we find that nationalization leads to lower firm performance and a disengagement from international business activities. In addition, we find evidence of trade flows between the U.S. (Chile's main trade partner) and Chile changing dramatically after 1970, which suggests that international relations could be the main explanation behind the lower performance of public firms.

4.3 Wages in public firms

The decline in firm performance under the UP government raises the question of its economic consequences for workers. Unfortunately, the business reports used in the previous sections do not contain information on wages. We therefore turn to the country's oldest labor survey, the *Encuesta de Ocupación y Desocupación* (EOD), to study wage outcomes in public firms. The survey was first mandated by the Central Bank in 1957 and was administered annually by the University of Chile until 2022. It collects detailed labor market information on all household members aged 14 and older from a random sample of approximately 3,000 households in the metropolitan area of Santiago. We use all June waves of the survey between 1965 and 1973.

Econometrically, we compare wages of workers in public and private firms by restricting attention to individuals between the ages of 18 and 65 who were employed and were surveyed by the EOD in 1965-1973. Then, we focus on the 32,276 individuals who work on either a public firm

[17]Previous research has documented fewer business relations between Chilean firms and U.S. banks after 1970 (Aldunate et al., 2024). This alternative explanation appears to be independent of the nationalization program. Table A6 shows that nationalized firms did *not* change their bank relationships differently than other firms.

($n = 1,514$) or a private firm 3 ($n = 30,762$), and estimate the following regression equation:

$$\log w_{it} = \sum_k \beta_k (P_i \times T_k) + \gamma x_i + \phi_t + \nu_{it} \quad (3)$$

where w_{it} is the wage of worker i in year t, the indicator P_i takes the value of one for workers in public firms and zero otherwise, and T_k is an indicator that takes the value of one for the year of the survey with $k = 1965, \ldots, 1973$.[18] To account for non-random sorting across public and private firms, we include the following controls x_i: an indicator for women, age, age squared, and fixed effects for five levels of education. To account for temporal effects that affect all workers, we include year fixed effects ϕ_t. Standard errors are robust to heteroskedasticity. Our interest is on the parameters β_k, which capture the differential change in wages of workers in public firms over time after adjusting for x_i and ϕ_t.

We find that wages in public firms decreased significantly in 1972 and 1973 when compared to wages in private firms. Panel A in Figure 3 presents estimates $\widehat{\beta_k}$ of equation (3). In 1965-1971, we observe an average wage premium in public firms of 20%, but the premium completely disappears in the 1972-1973 waves of the survey. Panel B in the same figure shows that the decline cannot be explained by differences in the number of hours worked, which remained similar across public and private firms throughout the 1965-1973 period. In fact, we observe the same decline when estimating equation (3) using wages per hour as dependent variable (Figure A7). Although the survey does not allow us to distinguish between workers in always-public or newly-public firms, these results suggest that workers in public firms could have had a poor contemporaneous evaluation of nationalization program, which motivates the analysis in the following section.

5 Downstream Political Effects

The lower performance of firms, together with the decline in wages paid by public firms, suggests that the nationalization program may have been perceived negatively by the population. Given the scale of the policy, exposure to nationalized firms could have led voters to form a worse evaluation of the incumbent Popular Unity (UP) coalition. In this section, we test this possibility by examining

[18]Note that we do not need an omitted category in equation (3) because of the repeated cross-sectional nature of the dataset and our chosen specification which compares workers across types of firms in which they are employed.

the empirical relationship between local exposure to the nationalization program and support for UP candidates in the March 1973 congressional election, held amid severe economic turmoil.

The UP administration confronted the 1973 election in the midst of severe macroeconomic stress, characterized by shortages and a sharp erosion of real wages. Despite these conditions and the presence of a unified opposition coalition (CODE), the government secured roughly 44 percent of the vote—enough to prevent an impeachment of President Allende (Fermandois, 2013, p. 598). Contemporary actors and observers frequently described the election as a de facto referendum on the regime, underscoring the perceived stakes for Chile's political future (Dooner, 1985; Harmer, 2011, p. 135). Opposition leaders expected a landslide, yet the result fell well short of their hopes, and foreign observers were struck by how limited the electoral punishment appeared given the deteriorating economy (Harmer, 2011; Amoros, 2013, p. 205,218). Against this backdrop, our analysis asks whether voters punished the government where the nationalization program was more salient, providing a test of the role of economic voting in the 1973 election.

5.1 Econometric strategy

To test for the relationship between the UP nationalization program and the vote share of UP candidates in the 1973 Congress Election, we begin by building a panel dataset of 307 municipalities observed in the 1960s and early 1970s. In terms of vote shares, we examine all of the four Congress Elections (1961, 1965, 1969, and 1973) and three Local Elections (1963, 1967, and 1971) during this period for a total of 2,143 observations.[19] Chile's democracy was of high standards, with universal voting and the healthiest elections in the region. To measure the exposure of municipalities to the nationalization program, we use the business reports to extract the municipality of location of every plant owned by each one of the 71 firms that we examined in section 3.1. We then consider three measures of exposure at the municipality level: (i) an indicator that takes the value of one for municipalities with at least one nationalized plant and zero otherwise, (ii) the number of nationalized plants, and (iii) the number of nationalized plants over total population in 1970.

Econometrically, we use the full panel data with the seven elections and implement a difference-

[19]To facilitate the comparison of vote shares within municipalities across elections, we follow González and Prem (2025) and study the vote shares of candidates from the Socialist and Communist parties, the core of the UP coalition.

in-differences strategy with the estimation of the following regression equation:

$$V_{mt} = \sum_k \beta_k (N_m \times T_k) + \phi_m + \phi_t + \epsilon_{mt} \quad (4)$$

where V_{mt} is the vote share of candidates running under the umbrella of the UP coalition in municipality m in election t. The local exposure to the nationalization program is measured by N_m, one of the three measures previously mentioned. The indicator T_k takes the value of one for the election in year k, with the 1969 Congress Election as the omitted category. We account for unobserved heterogeneity (i.e. local ideology) across municipalities with the use of municipality fixed effects ϕ_m, and absorb temporal changes that affect everyone in the country with the use of fixed effects by election year ϕ_t. Robust standard errors ϵ_{mt} are clustered by municipality.

The main parameter of interest is $\widehat{\beta}_{1973}$ and measures the differential vote share of the UP in municipalities more exposed to the nationalization program. The parameter $\widehat{\beta}_{1971}$ does not capture exposure to the program precisely because the nationalization of firms had not been fully implemented in 1971. In addition, the parameters $\widehat{\beta}_k$ with $k < 1969$ provide a test of parallel trends across treated and control municipalities before the arrival of the UP government. The identification assumption to interpret $\widehat{\beta}_{1973}$ as the causal effect of the program is that, if another candidate would have been elected President of Chile in 1970, then municipalities more exposed to the nationalization program would have had similar voting trends for UP candidates that other municipalities.

5.2 Economic voting against the incumbent

We find that exposure to the nationalization program is negatively associated with support for the incumbent UP coalition in the 1973 election. Figure 4 presents the main results, that is β_k estimates of equation (4). Black dots represent point estimates and vertical lines the 95 percent confidence interval. Different panels present results with different measures of the nationalization program: indicator for at least one nationalized plant in panel A, the number of plants nationalized in panel B, and the number of nationalized plants over 1970 population in panel C. Three patterns are key in these figures. First, municipalities more exposed to nationalizations exhibit a UP vote share that is 4 percentage points lower. Second, the UP also obtained a lower vote share in the 1971 local election, but the magnitude is smaller and statistically insignificant at conventional levels. This result is

consistent with the little or partial implementation of the nationalization program by April 1971. And third, the estimates for the years before 1969 are relatively smaller and indistinguishable from zero, lending support to the parallel trends assumption in our difference-in-differences strategy.[20]

A parametric estimation of equation (4) delivers similar results and facilitates the presentation of robustness exercises. Table 5 presents results from the baseline specification in which we restrict the β_k coefficients to be zero except the one in 1973. Columns 1-3 in this table present results for the same three outcomes. Reassuringly, we again find that exposure to the nationalization program is negatively associated with UP vote share in 1973. The estimates reveal a magnitude of approximately 1-4 percentage points depending on the specification.[21] The results are robust to a wide range of empirical exercises that modify the arguably arbitrary specification decisions that we made. Table A7 in the Online Appendix reports these additional results. In all, we find similar results when we restrict attention to Congress elections (column 1), when we collapse the panel to periods before and after 1973 (column 2), and little systematic deviations from parallel trends before 1970 (column 3). We also observe the same results controlling for state and institutional presence (military bases, church, social organizations; column 4), the spatial dependence of municipalities (column 5), local measures of education and labor market (column 6), and the presence of non-nationalized firms (column 7).[22] Similarly, we obtain the same results when including year fixed effects flexibly in each one of the 25 provinces in the country (column 8) and when excluding the 25 province capitals from estimation (column 9).

Importantly, the negative relationship between the nationalization program and support for the incumbent is robust to controlling for other large policies that were being implemented at the same time. Table 6 present results from these additional robustness exercises. The results are similar after accounting for the large number of hectares expropriated by the UP government by February 1973, the month before the election (Cuesta et al., 2017; González and Vial, 2021). We reach

[20] All of these results are similar in terms of magnitude and statistical significance if we restrict the sample to the four Congress elections for estimation. See Figure A8 in the Online Appendix for the corresponding results.

[21] While the magnitude in column 1 is immediate to grasp from the indicator, we interpret the magnitudes in columns 2 and 3 using an increase of 1 standard deviation in the 'Nationalizations' variable among the 78 municipalities exposed to the policy. The corresponding numbers are 3.3 and 1.5 in columns 2 and 3 respectively.

[22] The local presence of non-nationalized (i.e. private) firms also serves as an additional placebo check on our research design. Reassuringly, the coefficient of non-nationalized firms is economically smaller in magnitude, statistically unstable, and changes signs across specifications, providing further support for our causal interpretation.

the same conclusion after controlling for the exposure to trade protection using the local mix of agricultural production and prices that include the increase in tariffs during this period (Lederman, 2005; Cuesta et al., 2015), and the local exposure to the milk program, one of the flagship policies of the left-wing coalition which delivered free milk to all pre-schoolers in the country (Frens-String, 2021; González and Prem, 2025). Chile's economic activities is also characterized by mining and agriculture, and results remain robust after accounting for the local participation in these sectors. Salvador Allende also famously expanded the number of vacancies in higher education institutions and enfranchised the illiterate population (Bautista et al., 2025). Again, results remain similar after accounting for the distance to university campuses and the local illiteracy rate in 1970. Finally, the 1960s were characterized by rural-urban migration (Cousiño, 2001), but results are again unchanged after accounting for these migration patterns between 1960 and 1970.

Finally, we complement our baseline analysis using the recently developed synthetic difference-in-differences methodology (Arkhangelsky et al., 2021), which both confirms our main results and provides suggestive evidence on potential mechanisms. This approach compares each municipality with at least one nationalized firm (treated) to a weighted average of municipalities without nationalized firms (controls), where the weights are chosen to match pre-treatment trends in the outcome variable as closely as possible. Figure A9 reports these results. Panel A replicates our main finding using this alternative research design, while Panel B examines voter turnout.[23] The estimates in Panel B indicate no systematic decline in turnout in more exposed municipalities, suggesting that reduced participation by disaffected voters is unlikely to be the primary mechanism behind our results. Instead, the evidence points to changes in the political preferences of existing voters as the more plausible channel through which exposure to nationalization translated into lower electoral support for the incumbent coalition.

6 Conclusion

This paper provides an empirical evaluation of one of the largest nationalization programs implemented in a democratic setting. Using newly assembled firm-level panel data from Chile during the

[23] Difference-in-differences estimates display a differential change in turnout between 1961 and 1965 (Figure A10), reflecting the correlation between the location of nationalized firms and the expansion of electoral participation during that period, which was largely driven by Christian Democratic efforts to broaden the electorate (Szymanski, 1975).

Popular Unity government (1970–73), we show that the transition from private to state ownership was associated with substantial declines in firm performance and a retrenchment from international business activity. These effects were economically large and concentrated in manufacturing, the sector where political conflict around nationalization was most intense, while firms operating in strategic industries and in natural resources were largely insulated from performance losses. By linking firm outcomes to electoral data, we further document that municipalities more exposed to nationalized firms exhibited lower support for the incumbent coalition in the 1973 congressional election. Taken together, our results show that large-scale nationalization generated uneven economic effects with measurable political repercussions, and that its consequences depended critically on the type of firms involved and the political environment in which the policy unfolded.

Several caveats are important for interpreting these findings. First, the Chilean experience unfolded in a highly specific historical context shaped by Cold War geopolitics, intense domestic polarization, and external economic pressures, which may limit the external validity of our estimates. Moreover, the treatment we study bundles the change in ownership with the broader political and economic environment of the Allende period. As a result, our estimates should be interpreted as capturing the effects of being targeted by the UP nationalization program in this contested setting, rather than isolating the impact of public ownership in a neutral or stable institutional environment.

Second, our analysis captures only the short-run effects of nationalization, as the military coup of September 1973 abruptly terminated the policy and restricts the horizon over which firm performance can be observed. It is therefore possible that some potential benefits often attributed to state ownership, such as long-term investment coordination or structural transformation, require more time and institutional stability to materialize than our setting allows.

Third, declines in profitability and international business activity do not map one-to-one into losses in aggregate social welfare. State-owned firms may pursue objectives other than profit maximization, such as employment stability, wage protection, or strategic production goals. Consequently, our results should be interpreted as changes in firm behavior and performance rather than as direct measures of social inefficiency. Our results speak to how nationalization altered firm-level outcomes, but they do not constitute a complete welfare evaluation of the policy.

Our findings open several directions for future research. A first priority is to study the medium- and long-run effects of nationalization in contexts where public ownership persists, allowing an as-

sessment of whether the performance declines we document are temporary or persistent. Second, richer evidence on workers would help clarify how nationalization reshapes labor outcomes beyond average wages, including job stability, skill accumulation, and occupational mobility. Third, improved data on boards, managers, and internal governance could shed light on how changes in control and incentives translate into organizational performance. Finally, extending the analysis to market-level outcomes—such as prices, product quality, and consumer welfare—would provide a more complete evaluation of the broader economic consequences of nationalization policies.

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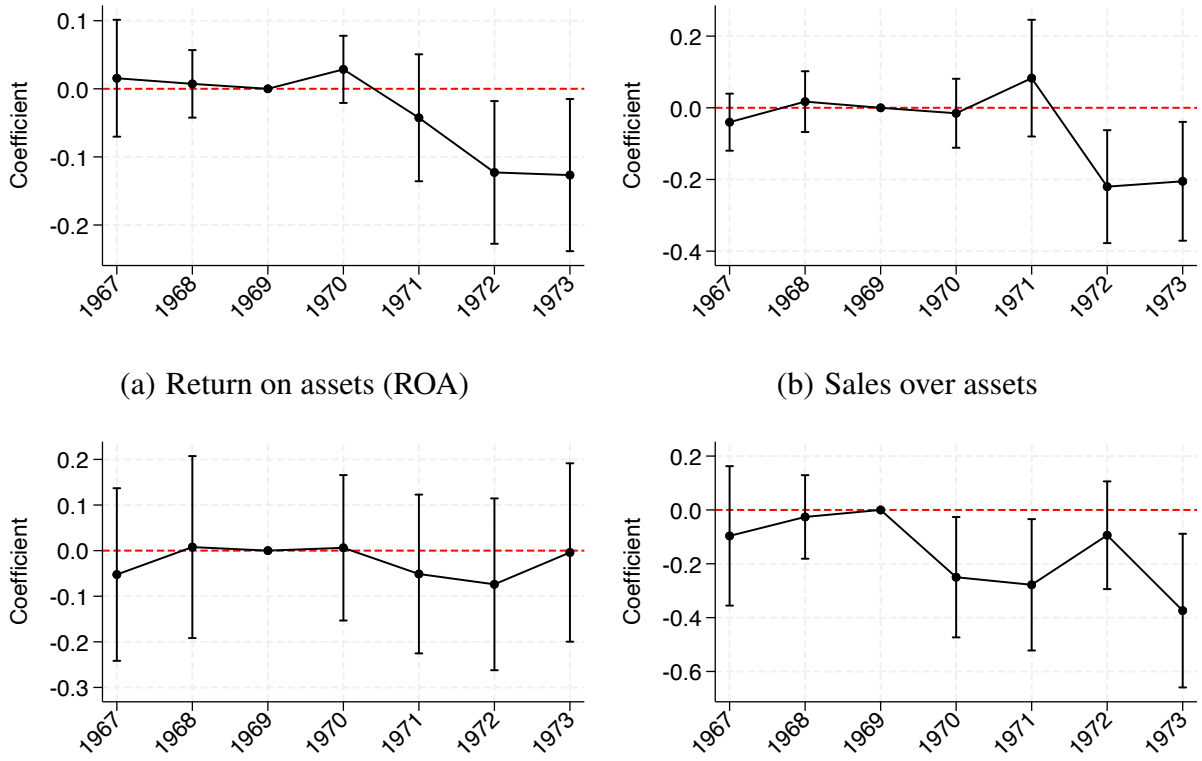
Figure 1: Nationalization and firm performance



(a) Return on assets (ROA)

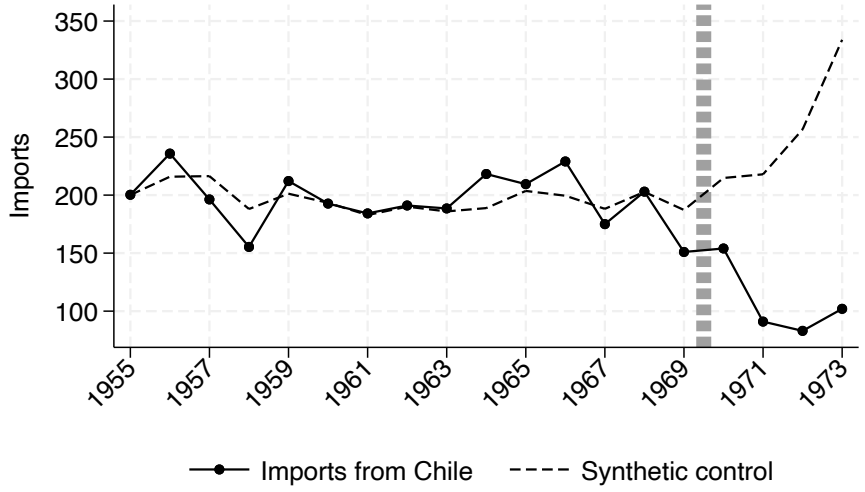
(b) Sales over assets

(c) Indicator exporting

(d) Indicator importing

Notes. Difference-in-differences estimates of the effect of nationalization on firm-level outcomes using a panel of 71 firms observed annually between 1967 and 1973. The nationalization program began after Salvador Allende took office in November 1970. The dependent variable is return on assets in panel A, sales over assets in panel B, an indicator equal to one if the firm exports in panel C, and an indicator equal to one if the firm imports in panel D. Black dots denote point estimates and vertical lines indicate 95 percent confidence intervals. All specifications include firm and sector-by-year fixed effects. Standard errors are clustered at the firm level.

Figure 2: Exploration of US-Chile trade flows using a synthetic control method

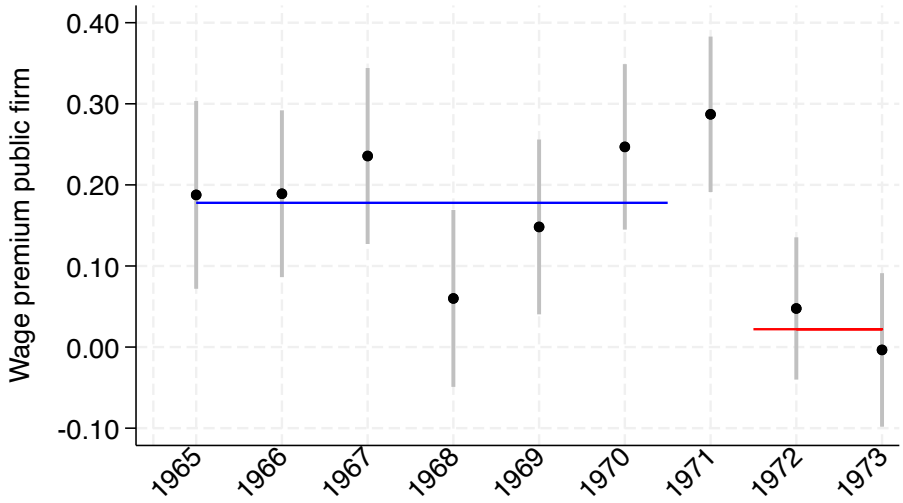


(a) Imports from Chile

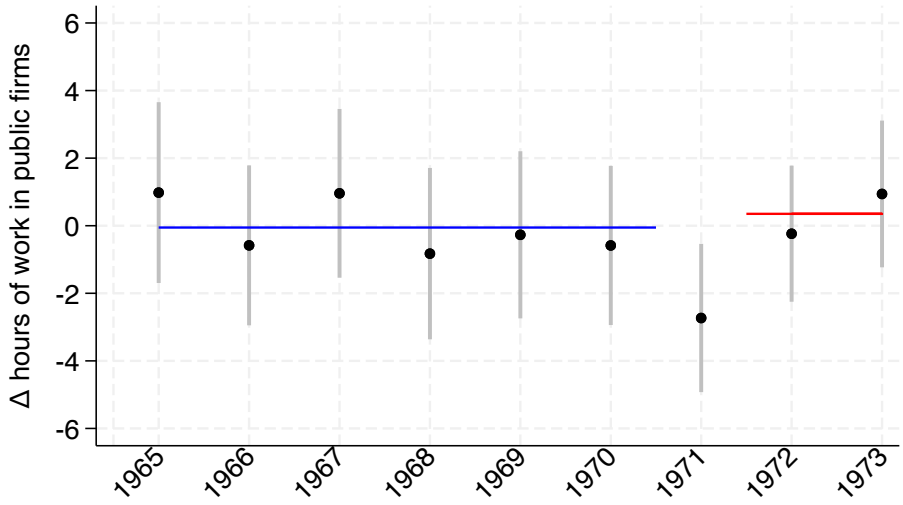
(b) Exports to Chile

Notes. Synthetic control estimates using a panel of Latin American countries and measures of exports (FOB) and imports (CIF) from the International Monetary Fund's Direction of Trade Statistics. The synthetic control weights are constructed using data from 1955 to 1969. The treatment period corresponds to 1970–1973. Trade flows are measured from the perspective of the United States, i.e. imports *from* Chile and exports *to* Chile.

Figure 3: Work in public firms



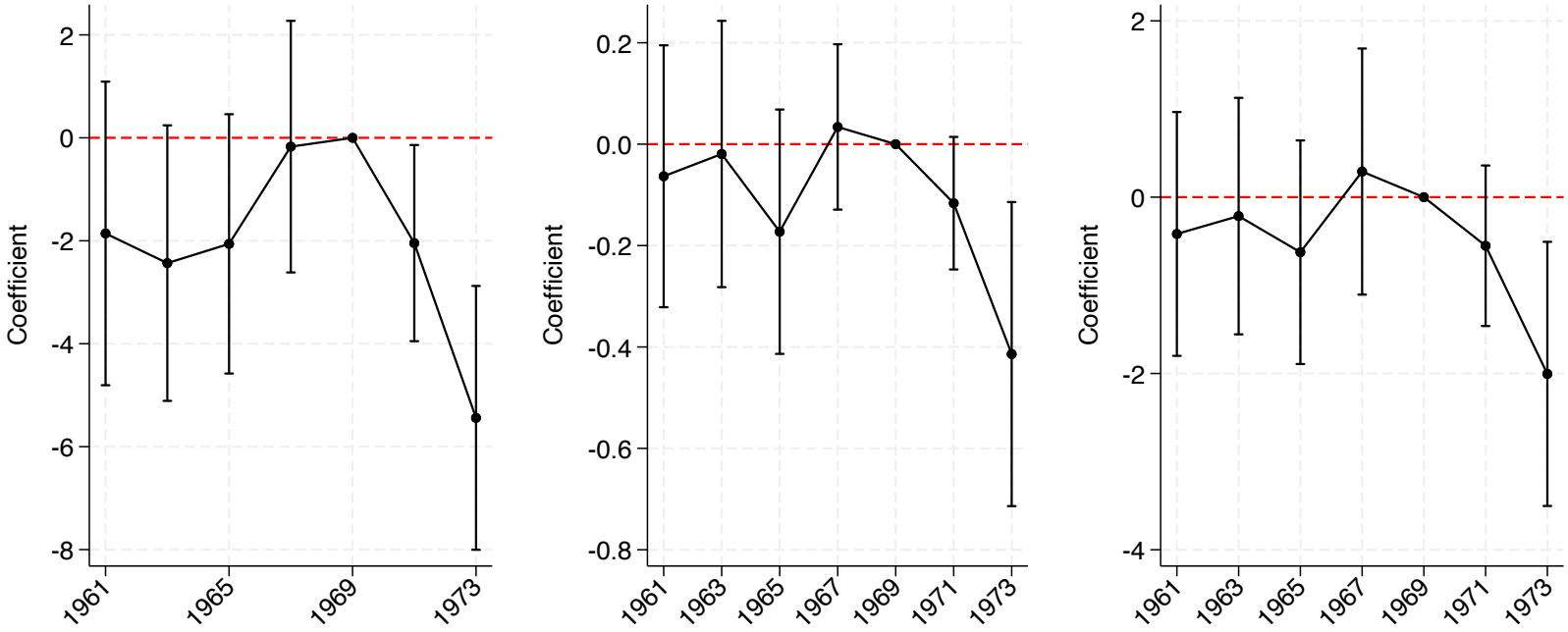
(a) Wages



(b) Hours of work

Notes. Both panels present regression estimates using repeated cross-sections of survey data from Chile's oldest labor survey, the *Encuesta de Ocupación y Desocupación* (EOD). The EOD collects labor market information for all household members aged 14 and older in a random sample of approximately 3,000 households in the city of Santiago. We use all June surveys between 1965 and 1973 and compare wages of workers employed in public and private firms. The sample is restricted to individuals aged 18–65 who report being employed at the time of the survey. The analysis focuses on 32,276 workers, of whom 1,514 are employed in public firms and 30,762 in private firms. All regressions control for gender, age, and education.

Figure 4: Nationalization and support for the incumbent UP coalition



(a) Indicator exposure to nationalization (b) Number of nationalized plants (c) Nationalized plants over 1970 pop.

Notes. These figures present difference-in-differences estimates of changes in electoral support for the left-wing coalition (y-axis) between 1961 and 1973. The definition of the cross-sectional treatment variable varies across panels: an indicator for municipalities hosting at least one plant belonging to a nationalized firm in panel A, the number of nationalized plants located in the municipality in panel B, and the number of nationalized plants per 1970 population in panel C. Black dots represent point estimates and vertical lines denote 95 percent confidence intervals. The dependent variable is the vote share of candidates affiliated with the Socialist and Communist parties in congressional elections (1961, 1965, 1969, and 1973) and local elections (1963, 1967, and 1971). All specifications include municipality and election-year fixed effects. Standard errors are clustered at the municipality level.

Table 1: Descriptive statistics in 1967-1969 by type of firm

	Nationalized	Non-Nationalized	Difference	Adding covariates	Year of nationalization
	(1)	(2)	(3)	(4)	(5)
Log assets	17.91	15.81	2.11***		-0.12
	(0.90)	(2.03)	(0.39)		(0.26)
Log fixed assets	17.48	15.18	2.30***	0.23*	-0.16
	(1.17)	(2.15)	(0.42)	(0.14)	(0.29)
Sales over assets	0.70	0.60	0.10	0.09	0.02
	(0.30)	(0.50)	(0.10)	(0.11)	(0.06)
Return on assets (ROA)	0.19	0.18	0.01	-0.02	0.04
	(0.15)	(0.27)	(0.05)	(0.06)	(0.03)
Fixed assets on total assets	0.71	0.61	0.09	0.10	-0.04
	(0.22)	(0.29)	(0.06)	(0.07)	(0.05)
Log debt	16.63	14.25	2.38***	0.13	0.39
	(2.52)	(3.02)	(0.58)	(0.81)	(0.63)
Leverage	0.39	0.39	0.00	-0.06	-0.01
	(0.14)	(0.24)	(0.04)	(0.06)	(0.04)
Int'l market: Indicator exporter	0.45	0.23	0.22**	0.13	-0.07
	(0.50)	(0.42)	(0.11)	(0.14)	(0.13)
Int'l market: Indicator importer	0.79	0.47	0.31***	0.04	0.02
	(0.41)	(0.50)	(0.10)	(0.10)	(0.08)
Sector: Primary	0.15	0.26	-0.11		-0.02
	(0.36)	(0.44)	(0.10)		(0.12)
Sector: Secondary	0.78	0.33	0.45***		-0.00
	(0.42)	(0.47)	(0.11)		(0.12)
Sector: Tertiary	0.07	0.41	-0.34***		0.02
	(0.25)	(0.49)	(0.09)		(0.03)
Number of national banks	4.93	3.53	1.41**	-0.82	-0.88
	(2.00)	(2.68)	(0.61)	(0.78)	(0.54)
Number of international banks	3.08	1.31	1.77***	0.63	-0.33
	(1.74)	(1.45)	(0.40)	(0.40)	(0.55)
Number of US banks	1.40	0.54	0.87***	0.52**	-0.24
	(1.01)	(0.71)	(0.21)	(0.23)	(0.25)
Firms	32	39	71	71	32

Notes. Columns 1 and 2 report the mean and standard deviation for nationalized and non-nationalized firms, respectively. Column 3 reports the difference in means between nationalized and non-nationalized firms. Column 4 reports the same difference as in column 3 after adjusting for firm size (log assets) and sector fixed effects; standard errors are reported in parentheses. Column 5 reports estimates from a cross-sectional regression using the 32 nationalized firms, where the dependent variable is the year of nationalization and the regressors include all firm-level variables in the dataset; standard errors are reported in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2: Difference-in-differences estimates

	Return on assets		Sales over assets		Indicator exports		Indicator imports	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Nationalized × Allende	-0.108***	-0.130***	-0.098*	-0.083	-0.033	-0.040	-0.159*	-0.231**
	(0.037)	(0.045)	(0.052)	(0.051)	(0.087)	(0.088)	(0.086)	(0.103)
Observations	390	390	390	390	389	389	389	389
Firm fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Sector-year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Covariates	N	Y	N	Y	N	Y	N	Y
Firms	71	71	71	71	71	71	71	71
Avg. dependent variable	0.188	0.188	0.643	0.643	0.326	0.326	0.611	0.611

Notes. The unit of observation is a firm-year. The table reports difference-in-differences estimates using alternative measures of firm performance as dependent variables: return on assets (ROA) in columns 1–2, sales over assets in columns 3–4, an indicator equal to one if the firm exports in columns 5–6, and an indicator equal to one if the firm imports in columns 7–8. Odd-numbered columns include firm and sector-by-year fixed effects, while even-numbered columns additionally control for firm size in the pre-treatment period (1967–1969) interacted with year fixed effects. Standard errors are clustered at the firm level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3: Robustness of changes in firm performance

	Collapse to two periods (before and during Allende)	Replaces *sector* f.e. by narrower *industry* f.e.	Controls for the number of U.S. banks pre-Allende	Controls for probability (pscore) of being nationalized by Allende	Diff-in-diff + matching (Crump et al., 2009)	Diff-in-diff + matching (Yang and Ding, 2018)	Diff-in-diff + matching Abadie (2005)	Diff-in-diff + matching Doubly robust method
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Return on assets								
Nationalized × Allende	-0.151***	-0.113***	-0.128***	-0.115***	-0.090***	-0.091***	-0.123***	-0.124***
	(0.042)	(0.041)	(0.038)	(0.042)	(0.033)	(0.033)	(0.025)	(0.028)
Avg. dependent variable	0.199	0.194	0.188	0.188	0.171	0.188	0.188	0.188
Panel B: Indicator imports								
Nationalized × Allende	-0.220**	-0.255**	-0.180	-0.200**	-0.165*	-0.165*	-0.280***	-0.253***
	(0.104)	(0.115)	(0.106)	(0.098)	(0.089)	(0.089)	(0.082)	(0.075)
Avg. dependent variable	0.622	0.628	0.611	0.611	0.632	0.611	0.611	0.611
Observations	120	370	389	389	362	389	389	389
Firms	60	67	71	71	65	71	71	71
Firm fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Sector-year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y

Notes. The unit of observation is a firm-year in 1967-1973. The table reports robustness checks for the baseline difference-in-differences estimates using two measures of firm performance as dependent variables: return on assets (ROA) in panel A and an indicator equal to one if the firm imports in panel B. Column 1 reports estimates from a specification that collapses the panel into two periods (pre- and post-Allende). Column 2 reports estimates using more narrowly defined industry-by-year fixed effects. Column 3 controls for the number of U.S. banks in the pre-Allende period interacted by the Allende period to assess the effect of the unbalancedness of this variable in the descriptive statistics table. Columns 4–8 report estimates using alternative matching procedures combined with the difference-in-differences design. Standard errors are clustered at the firm level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4: Nationalization and performance by type of firm

	Z = Manufacturing		Z = Strategic sector		Z = Nat. resources	
Dependent variable:	ROA	Importing	ROA	Importing	ROA	Importing
	(1)	(2)	(3)	(4)	(5)	(6)
Nationalized × Allende	-0.031	-0.083	-0.188***	-0.028	-0.178***	-0.125
	(0.047)	(0.083)	(0.046)	(0.085)	(0.042)	(0.078)
× Indicator for firm in Z	-0.154**	-0.148	0.202***	-0.331*	0.296***	-0.101
	(0.069)	(0.169)	(0.074)	(0.187)	(0.063)	(0.143)
Indicator for firm in Z × Allende	0.130**	0.002	-0.130**	0.209	-0.026	0.327**
	(0.064)	(0.137)	(0.054)	(0.137)	(0.049)	(0.143)
Observations	390	389	390	389	390	389
Firm fixed effect	Y	Y	Y	Y	Y	Y
Sector-year fixed effect	Y	Y	Y	Y	Y	Y
Firms	71	71	71	71	71	71
Firms nationalized and in Z	18	18	13	13	5	5
Average dep variable	0.188	0.611	0.188	0.611	0.188	0.611

Notes. The unit of observation is a firm-year. The table reports difference-in-differences estimates with heterogeneous effect by type of firm: in manufacturing or not (columns 1-2), operating in strategic industries or not (columns 3-4), and operating in industries extracting natural resources (columns 5-6). We use two measures of firm performance as dependent variables: return on assets (ROA) in odd columns and an indicator equal to one if the firm imports in even columns. Firms are classified as strategic following the original economic program of the Unidad Popular (Popular Unity, 1969). Standard errors are clustered at the firm level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5: Nationalizations and voting

Dependent variable: Left-wing vote share			
Nationalizations:	*At least one*	*Total number*	*Total number over 1970 pop.*
	(1)	(2)	(3)
Nationalizations × 1973 Election	-4.02***	-0.36***	-1.75***
	(1.11)	(0.08)	(0.67)
Observations	2143	2143	2143
Municipality fixed effects	Y	Y	Y
Year fixed effects	Y	Y	Y
Avg. dependent variable	28.22	28.22	28.22
Municipalities	307	307	307

Notes. The unit of observation is a municipality-election. The table reports difference-in-differences estimates of the relationship between exposure to firm nationalizations and electoral support for the left-wing coalition. The sample includes 307 municipalities observed in four congressional elections (1961, 1965, 1969, and 1973) and three local elections (1963, 1967, and 1971). Left-wing vote share is defined as the vote share of Communist and Socialist candidates in each election. Exposure to nationalization is measured as an indicator for municipalities hosting at least one nationalized firm (column 1), the number of nationalized firms located in the municipality (column 2), and the number of nationalized firms per population, standardized by the mean and standard deviation (column 3). A total of 110 municipalities experienced at least one nationalization; conditional on exposure, the average number of nationalized firms is 3 (standard deviation 2). All specifications include municipality and election-year fixed effects. Standard errors, reported in parentheses, are clustered at the municipality level. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Other policies implemented by the UP government

Policy control:	Dependent variable: Left-wing vote share							
	Land reform	Trade protection	Milk distributed	Share mining workers	Share agricultural workers	Distance closest university	Illiteracy rate	Rural-urban migration
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Any nationalization								
Nationalizations × 1973	-3.12***	-3.88***	-4.05***	-4.00***	-2.83**	-3.42***	-2.55**	-3.90***
	(1.09)	(1.17)	(1.11)	(1.11)	(1.14)	(1.27)	(1.10)	(1.09)
Policy × 1973	2.38***	-0.47	0.32	-1.89***	2.19***	0.36	1.76***	-1.47*
	(0.58)	(0.50)	(0.54)	(0.69)	(0.62)	(0.57)	(0.50)	(0.79)
Panel B: Number of nationalizations								
Nationalizations × 1973	-0.25***	-0.34***	-0.36***	-0.39***	-0.28***	-0.30***	-0.24***	-0.33***
	(0.07)	(0.07)	(0.08)	(0.09)	(0.06)	(0.10)	(0.06)	(0.08)
Policy × 1973	2.31***	-0.41	0.38	-2.24***	2.55***	0.46	1.80***	-1.09
	(0.60)	(0.44)	(0.55)	(0.72)	(0.67)	(0.57)	(0.58)	(0.76)
Panel C: Number of nationalizations over pop.								
Nationalizations × 1973	-1.37**	-1.71**	-1.74**	-1.83***	-1.27**	-1.30**	-1.11*	-1.72***
	(0.57)	(0.68)	(0.68)	(0.69)	(0.60)	(0.60)	(0.57)	(0.62)
Policy × 1973	2.58***	-0.64	0.19	-1.98***	2.91***	0.85*	2.03***	-1.62**
	(0.57)	(0.47)	(0.55)	(0.72)	(0.72)	(0.48)	(0.61)	(0.76)
Observations	2143	2143	2143	2143	2143	2143	2143	2143
Municipality fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Avg. dependent variable	28.22	28.22	28.22	28.22	28.22	28.22	28.22	28.22
Municipalities	307	307	307	307	307	307	307	307

Notes. The unit of observation is a municipality-election. Each column reports a difference-in-differences estimate using 307 municipalities observed in four congressional elections (1961, 1965, 1969, and 1973) and three local elections (1963, 1967, and 1971). Columns differ only in the inclusion of an alternative policy exposure variable, each interacted with an indicator for the 1973 election, to account for the local impact of major contemporaneous policies implemented between 1970 and 1973. All policy variables are standardized. Column 1 controls for land reform (share of land expropriated), column 2 for trade protection (tariffs interacted with local agricultural production), column 3 for exposure to the milk program, columns 4 and 5 for employment shares in mining and agriculture, column 6 for distance to the nearest university, column 7 for local illiteracy rates, and column 8 for changes in rurality between 1960 and 1970. All specifications include municipality and election-year fixed effects. Standard errors are clustered at the municipality level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

ONLINE APPENDIX

When the State Takes Over: Nationalization, Firm Performance, and Political Backlash

Felipe González *Mounu Prem*

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A Historical Addendum

A1 Nationalization in the UP program

As noted by de Vylder (1976, p. 112), "the decisive passage in the UP program" (Popular Unity, 1969) that outlined the strategy for the nationalization program reads as follows:

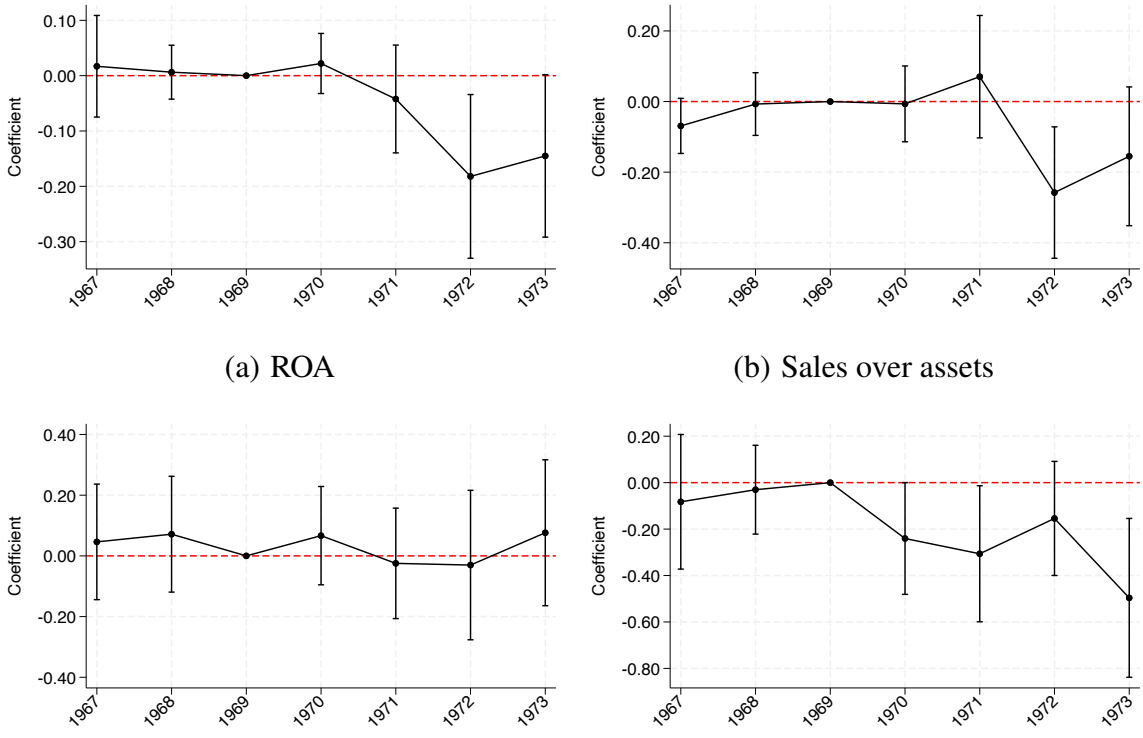
> "The process of transforming our economy will begin with a policy intended to create a dominant state area made up of enterprises already owned by the state plus those which are to be expropriated. As an initial step we will nationalize those basic riches—large-scale mining of copper, iron, nitrates, and others—which are now controlled by foreign capital and by domestic monopolies. This sector of nationalized activities will thus include the following activities:
>
> 1. large-scale mining of copper, nitrate, iodine, iron and coal;
> 2. the financial system of the country, in particular the private banks and insurance companies;
> 3. foreign trade;
> 4. large-scale enterprises and monopolies in the field of distribution;
> 5. strategic industrial monopolies;
> 6. in general those activities which condition the economic and social development of the country, such as the production and distribution of electrical energy, transportation by rail, air, and sea; communications; the production, refining and distribution of petroleum and its derivatives, including liquid gas; the iron and coal industry; cement, petrochemicals and heavy chemicals, cellulose, and paper.
>
> All these expropriations will always be carried out with full regard for the interests of the small shareholders."

A2 Legal and political complexity of nationalization

The legal and political implementation of nationalization under the Allende government was highly contested and far from straightforward. As documented by de Vylder (1976, p. 135-144), the Popular Unity (UP) initially sought to establish a clear legal framework distinguishing between private, mixed, and social sectors of the economy, with explicit guarantees for small and medium-sized firms while reserving "strategic" activities for public ownership. A comprehensive nationalization bill presented to Congress in late 1971 proposed public control over large-scale mining, banking, transport, utilities, and other key sectors, alongside size-based criteria for transferring major corporations to the social or mixed area. Compensation rules favored small shareholders and relied largely on long-term bonds rather than cash. Despite these concessions, congressional opposition remained unified and consistently blocked new legislation, viewing the proposal as a threat to the private sector and to economic and political power more broadly.

In the absence of legislative approval, the government relied on existing legal instruments to expand state control. Most notably, a 1932 decree-law allowed expropriation, requisition, or intervention of firms producing "goods of prime necessity" in cases of production stoppages, labor conflicts, or violations of price controls. While outright expropriation under this law required full cash compensation and was therefore used sparingly, temporary interventions and requisitions—formally intended as emergency measures—became increasingly common. In parallel, the government negotiated direct purchases with owners, acquired shares through public agencies, and in some cases proposed mixed enterprises, especially when foreign interests were involved. Which mechanism was chosen depended on firm size, ownership structure, foreign dependence, labor militancy, and political resistance. As de Vylder (1976) emphasizes, nationalization outcomes were shaped less by a uniform legal blueprint than by ongoing political conflict among the government, opposition parties, courts, business owners, workers, and foreign actors, making the formation of the "social area" a fluid, conflict-driven, and highly heterogeneous process.

Figure A1: Difference-in-difference results, controlling for the size of firms



(a) ROA

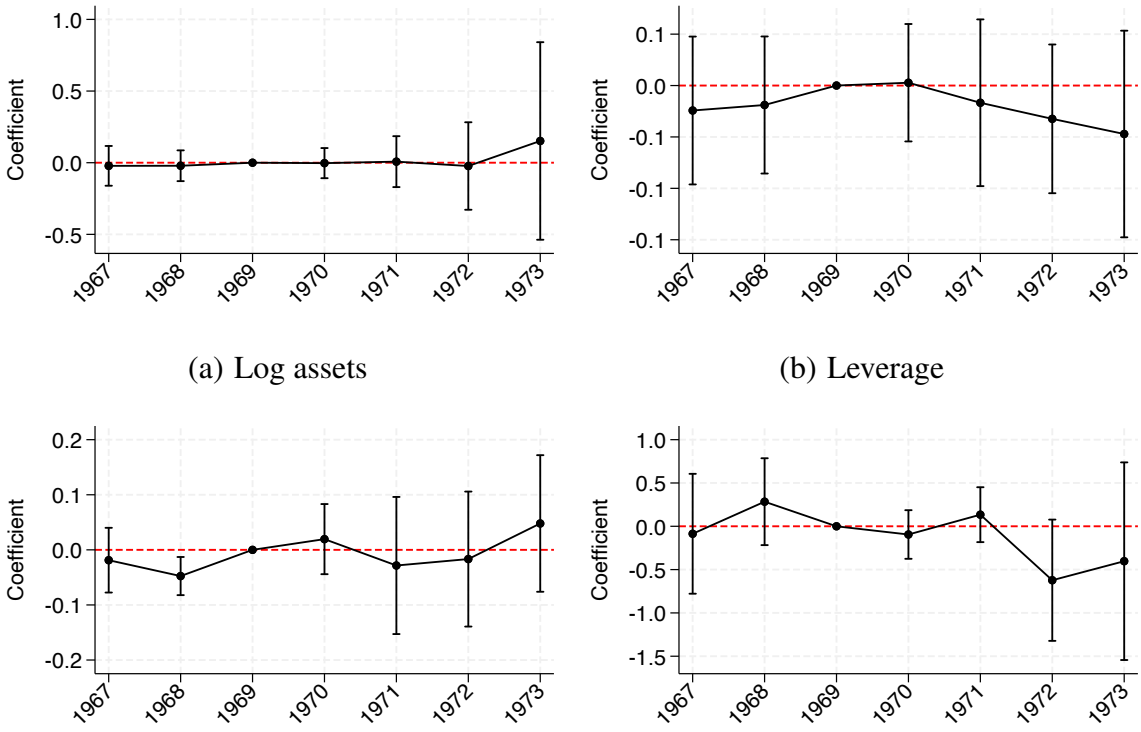
(b) Sales over assets

(c) Export

(d) Import

Notes. Difference-in-differences estimates for the effect of nationalization on firm-level outcomes using a panel of 71 firms observed annually between 1967 and 1973. The nationalization program began after Salvador Allende took office in November 1970. The dependent variable is return on assets in panel A, sales over assets in panel B, an indicator equal to one if the firm exports in panel C, and an indicator equal to one if the firm imports in panel D. Black dots represent point estimates and vertical lines denote 95 percent confidence intervals. All specifications include firm and sector-by-year fixed effects and additionally control for firm size in the pre-treatment period (1967–1969) interacted with year fixed effects. Standard errors are clustered at the firm level.

Figure A2: Difference-in-differences estimates, additional outcomes



(a) Log assets

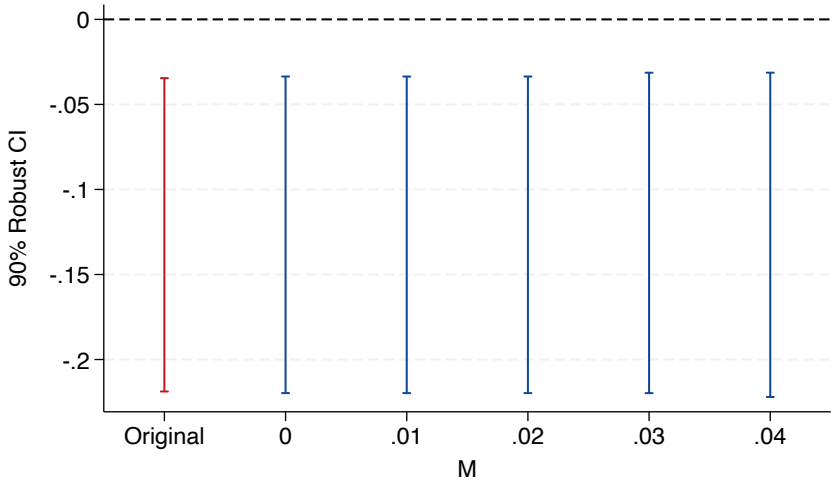
(b) Leverage

(c) Fixed assets over total assets

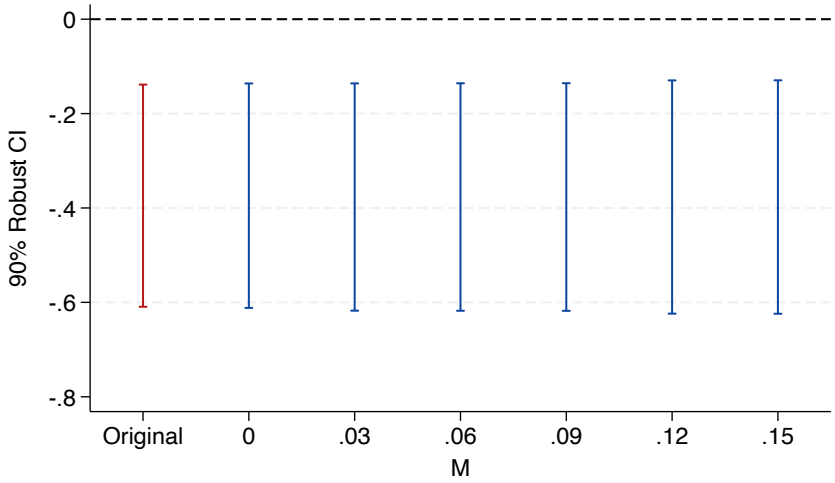
(d) Log sales

Notes. Difference-in-differences estimates for the effect of nationalization on alternative firm-level outcomes using a panel of 71 firms observed annually between 1967 and 1973. The nationalization program began after Salvador Allende took office in November 1970. The dependent variable is log total assets in panel A, leverage (debt over assets) in panel B, fixed assets over total assets in panel C, and log sales in panel D. Black dots represent point estimates and vertical lines denote 95 percent confidence intervals. All specifications include firm and sector-by-year fixed effects. Standard errors are clustered at the firm level.

Figure A3: Honest difference-in-differences



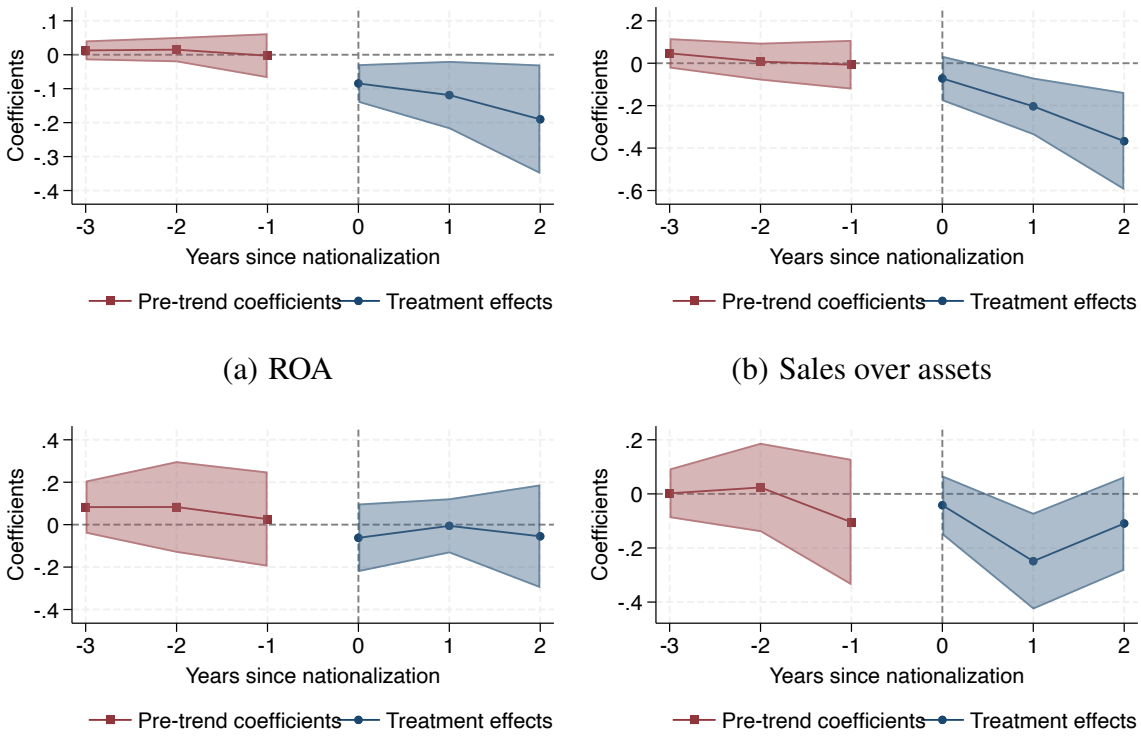
(a) Return over assets



(b) Indicator imports

Notes. This figure assesses the robustness of our main difference-in-differences estimates to potential violations of the parallel trends assumption using the honest difference-in-differences approach of Rambachan and Roth (2023). Panel A reports results for return on assets (ROA), and panel B for an indicator equal to one if the firm imports. The y-axis shows 90% robust confidence intervals, while the x-axis reports the original point estimate together with estimates under different values of the hyperparameter M which bounds post-treatment deviations from parallel trends. The labels in the x-axis, i.e. 0-0.04 in panel A and 0-0.15 in panel B, are values of M. The vertical lines provide the honest-DiD 10% confidence interval for each M. The "original" estimates in both panels correspond to a standard 10% confidence interval in the specifications presented in columns 2 and 8 in Table 2. All specifications include firm and sector-by-year fixed effects, and standard errors are clustered at the firm level.

Figure A4: Staggered difference-in-differences estimates



(a) ROA

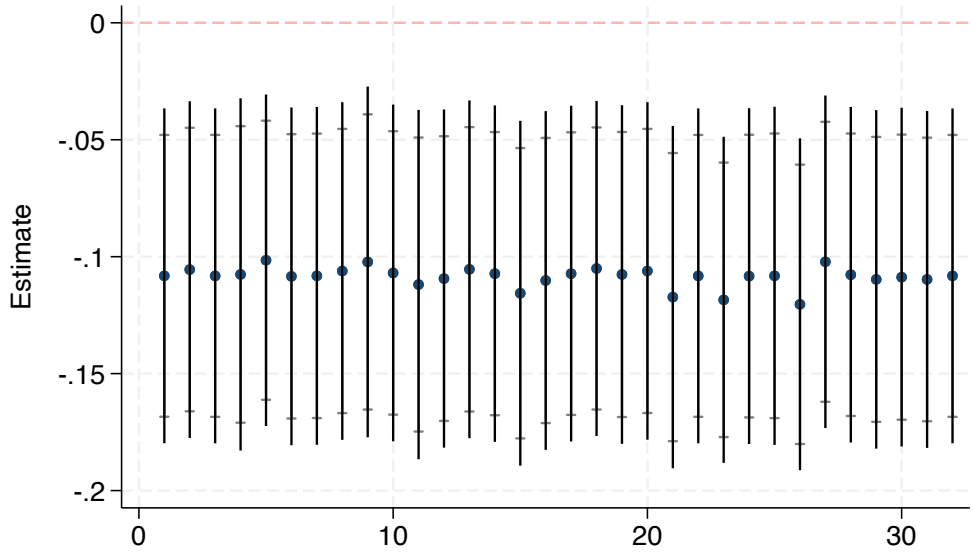
(b) Sales over assets

(c) Export

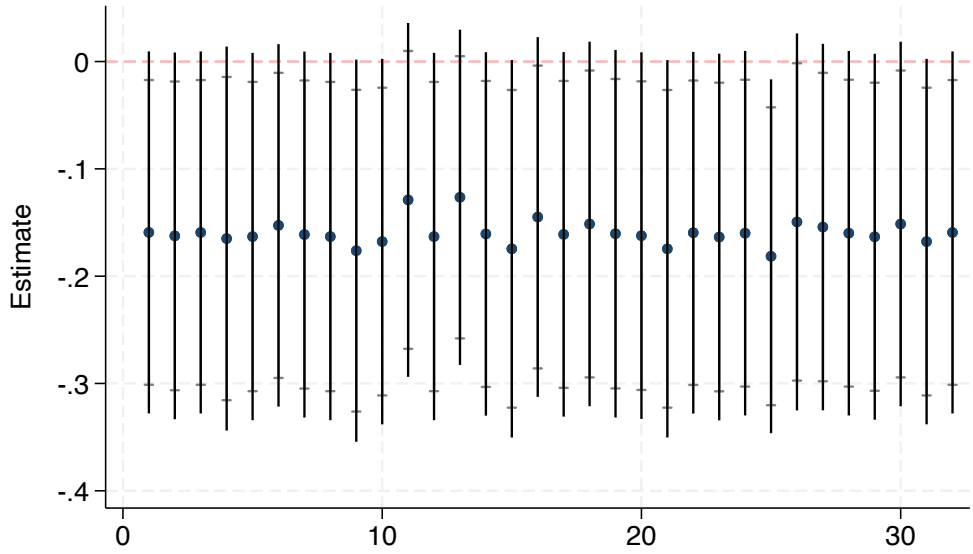
(d) Import

Notes. Difference-in-differences estimates for the effect of nationalization on firm-level outcomes using alternative estimators designed for settings with treatment timing variation. The four panels correspond to different outcomes: return on assets (panel A), sales over assets (panel B), an indicator for exporting firms (panel C), and an indicator for importing firms (panel D). Estimates are obtained using the estimator proposed by Borusyak et al. (2024). Black dots represent point estimates and shaded areas indicate 95% confidence intervals. All specifications include firm and sector-by-year fixed effects. Standard errors are clustered at the firm level.

Figure A5: Excluding single firms



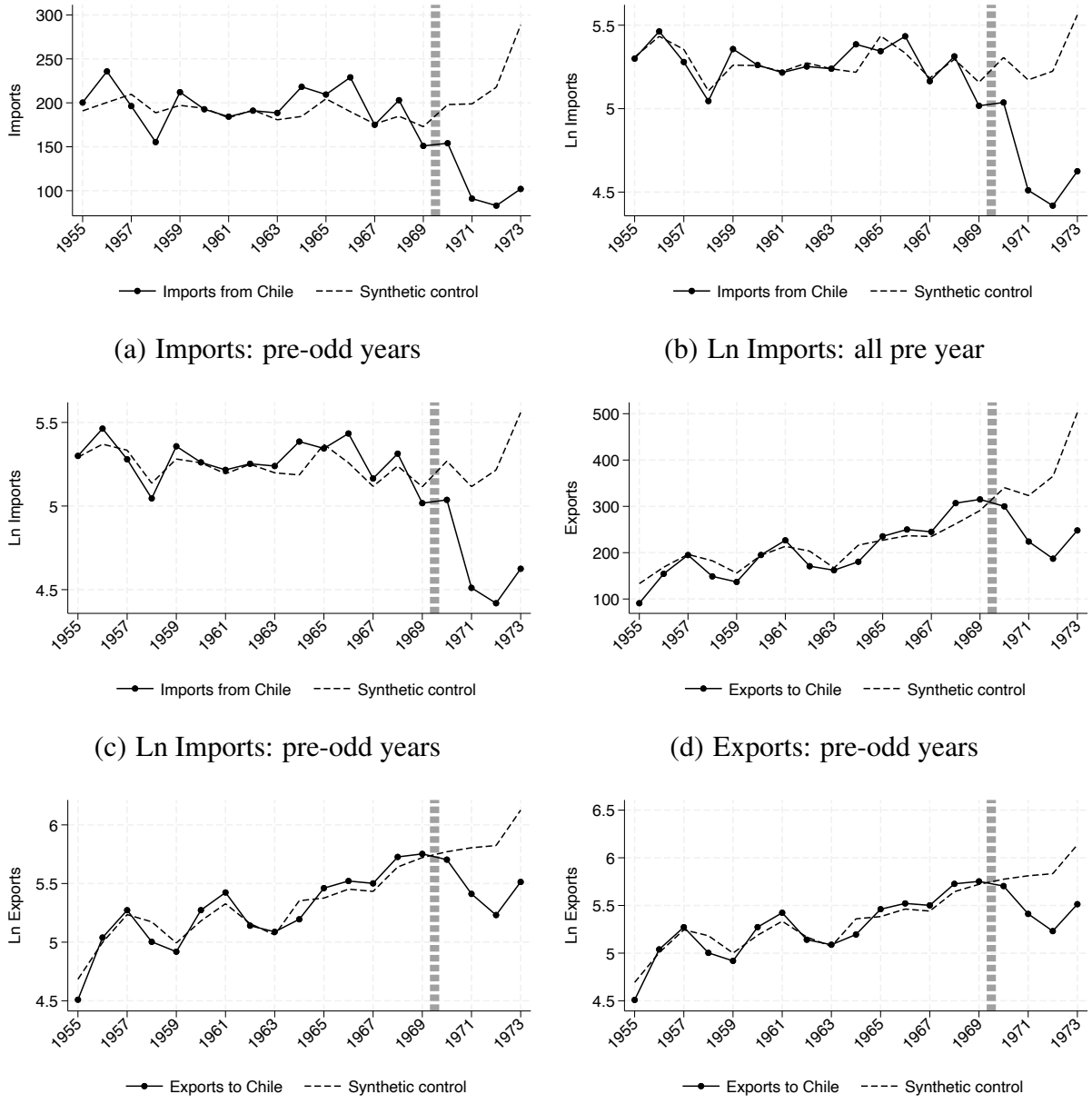
(a) ROA



(b) Import

Notes. Leave-one-out difference-in-differences estimates for the effect of nationalization on firm performance. Panel A reports estimates using return on assets (ROA) as the dependent variable, and panel B uses an indicator equal to one if the firm imports. In each estimation, one of the 32 nationalized firms is excluded from the sample; the x-axis identifies the excluded firm, and the y-axis reports the corresponding estimate. All specifications include firm and sector-by-year fixed effects. Standard errors are clustered at the firm level.

Figure A6: Additional synthetic control results



(a) Imports: pre-odd years

(b) Ln Imports: all pre year

(c) Ln Imports: pre-odd years

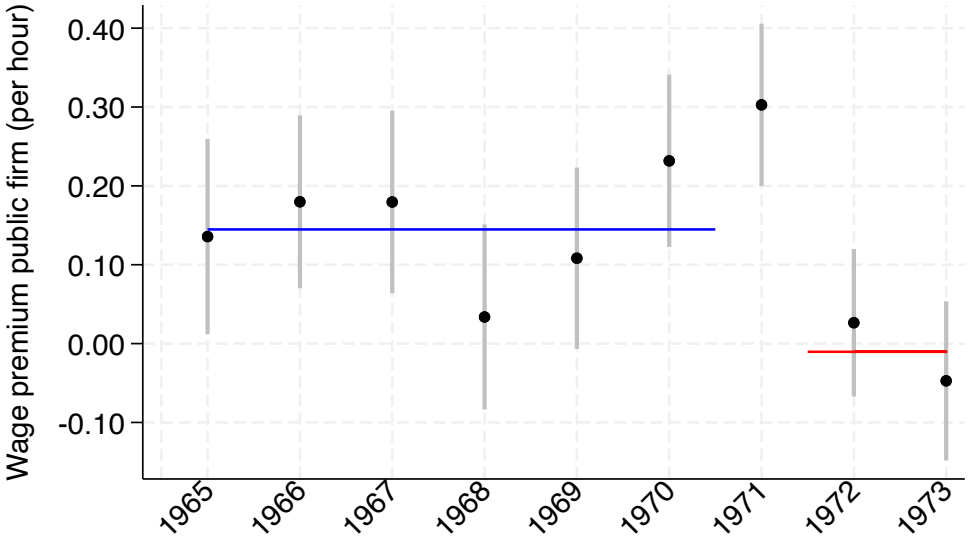
(d) Exports: pre-odd years

(e) Ln Exports: all pre year

(f) Ln Exports: pre-odd years

Notes. Synthetic control estimates using a panel of all Latin American countries and trade data from the International Monetary Fund (Direction of Trade Statistics). Panels A, C, D, and F construct the synthetic control using only odd years in the pre-treatment period. Panels B and C use log imports (CIF) as the dependent variable, while panels E and F use log exports (FOB); remaining panels use trade levels as in Figure 2. The pre-treatment period spans 1955-1969, and the treatment period is 1970–1973.

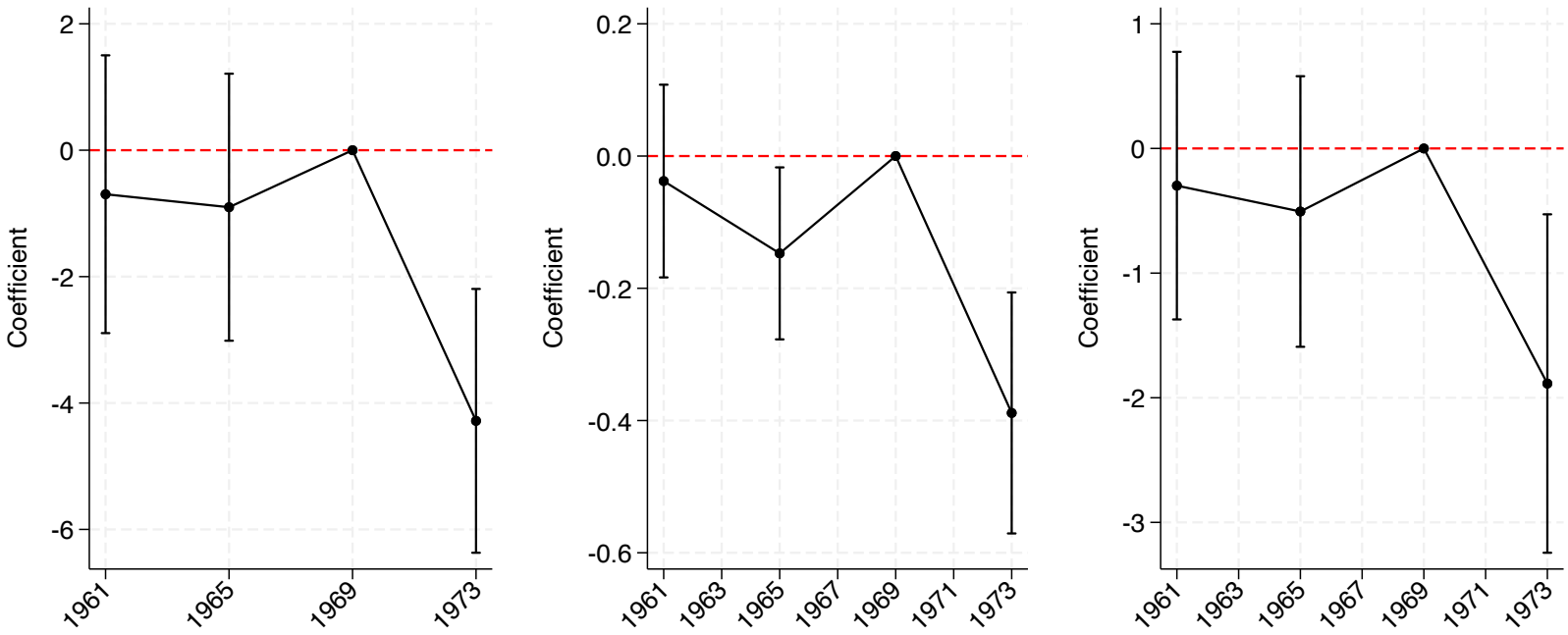
Figure A7: Wages per hour in public firms



(a) Wages per hour

Notes. This figure presents regression estimates using repeated cross sections of survey data from Chile's oldest labor survey, the *Encuesta de Ocupación y Desocupación* (EOD). The survey collects labor market information for all household members older than 14 years in a random sample of approximately 3,000 households in the capital city of Santiago. We use all June surveys between 1965 and 1973 and restrict attention to individuals aged 18 to 65 who are employed and work in either a public or a private firm. The dependent variable is log hourly wages. All regressions control for gender, age, and education.

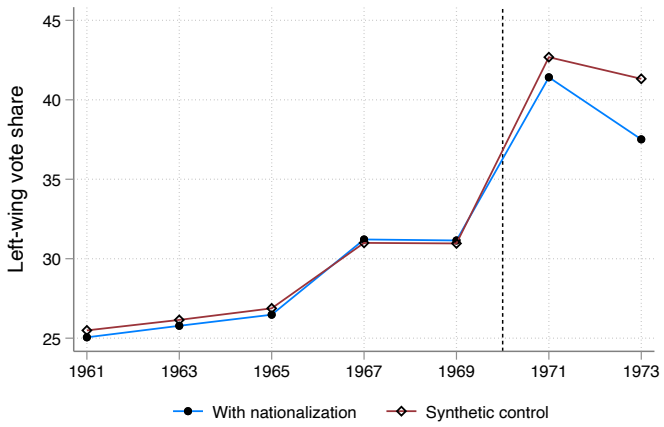
Figure A8: Nationalizations and UP voting, only Congress



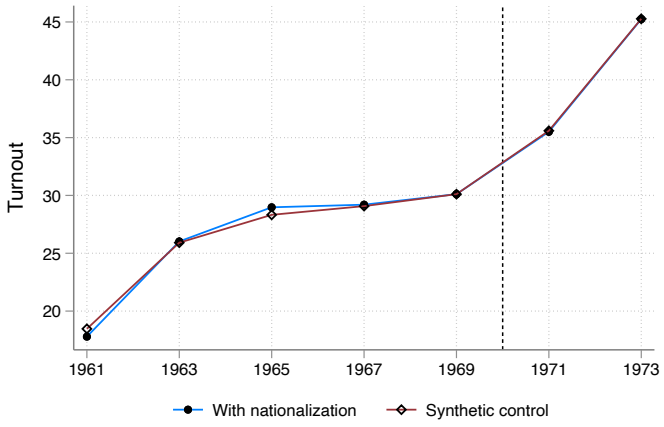
(a) Indicator exposure to nationalization (b) Number of nationalized plants (c) Nationalized plants over 1970 pop.

Notes. This figure presents difference-in-differences estimates of changes in electoral support for the left-wing coalition using congressional elections only. The sample includes 307 municipalities observed in four congressional elections (1961, 1965, 1969, and 1973). The definition of the cross-sectional treatment variable varies across panels: an indicator for municipalities hosting at least one plant belonging to a nationalized firm in panel A, the number of nationalized plants located in the municipality in panel B, and the number of nationalized plants per 1970 population in panel C. Black dots represent point estimates and vertical lines denote 95 percent confidence intervals. The dependent variable is the vote share of candidates affiliated with the Socialist and Communist parties. All specifications include municipality and election-year fixed effects. Standard errors are clustered at the municipality level.

Figure A9: Political backlash using synthetic difference-in-differences



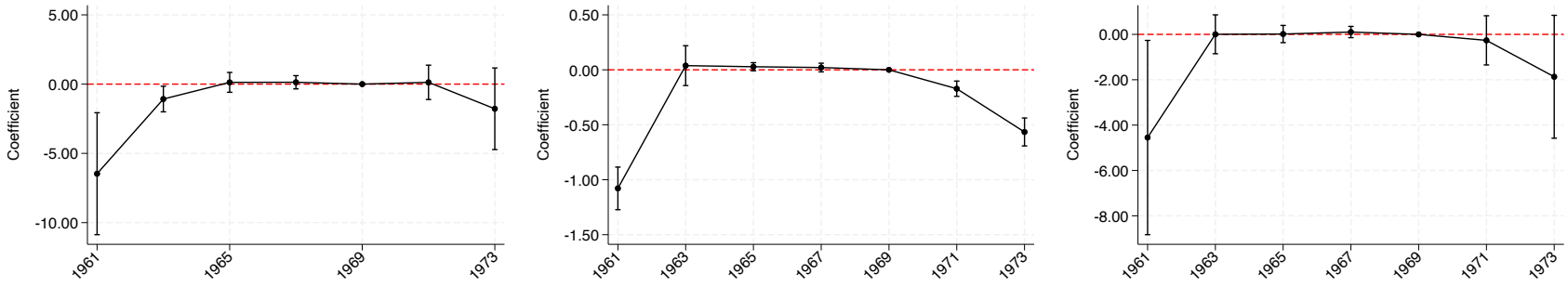
(a) Left-wing vote share



(b) Turnout

Notes. These figures present synthetic difference-in-differences estimates of changes in electoral support for the left-wing coalition (y-axis, panel A) and turnout (panel B) between 1961 and 1973 (Arkhangelsky et al., 2021). The "With nationalization" and "Without nationalization" groups represent group averages of treated municpalities versus their synthetic controls constructed from donor municipalities. The definition of the cross-sectional treatment variable is an indicator for municipalities hosting at least one plant belonging to a nationalized firm. The dependent variable is the vote share of candidates affiliated with the Socialist and Communist parties (panel A) or turnout (panel B) in congressional elections (1961, 1965, 1969, and 1973) and local elections (1963, 1967, and 1971). Point estimates are -3.7 in panel A with a 95 percent confidence interval of $[-6.1, -1.3]$ and -0.10 in panel B with a 95 percent confidence interval of $[-2.2, 2.0]$.

Figure A10: Nationalizations and turnout



(a) Indicator exposure to nationalization (b) Number of nationalized plants (c) Nationalized plants over 1970 pop.

Notes. This figure presents difference-in-differences estimates of changes in electoral participation, i.e. turnout. The sample includes 307 municipalities observed in in congress elections (1961, 1965, 1969, and 1973) and local elections (1963, 1967, and 1971). The definition of the cross-sectional treatment variable varies across panels: an indicator for municipalities hosting at least one plant belonging to a nationalized firm in panel A, the number of nationalized plants located in the municipality in panel B, and the number of nationalized plants per 1970 population in panel C. Black dots represent point estimates and vertical lines denote 95 percent confidence intervals. The dependent variable is the total number of votes over the 1970 local population. All specifications include municipality and election-year fixed effects. Standard errors are clustered at the municipality level.

Table A1: Analysis of missing business reports

	Dependent variable: Number of missing years					
	All years		Pre-Allende years		Allende years	
	(1)	(2)	(3)	(4)	(5)	(6)
Nationalized	-0.07	-0.11	0.18	0.20	-0.25	-0.31
	(0.39)	(0.52)	(0.23)	(0.28)	(0.27)	(0.37)
Log total assets		0.14		0.05		0.09
		(0.14)		(0.08)		(0.12)
Indicator for importing		-0.32		-0.07		-0.24
		(0.66)		(0.38)		(0.48)
Number of U.S. banks		-0.59		-0.39		-0.20
		(0.58)		(0.32)		(0.39)
Return on assets		1.03		0.40		0.64
		(0.87)		(0.57)		(0.61)
Sales over assets		0.17		0.25		-0.08
		(0.45)		(0.34)		(0.45)
Firms	71	71	71	71	71	71
Avg. dependent variable	1.51	1.51	0.62	0.62	0.89	0.89
Joint test p-value	–	0.61	–	0.48	–	0.68

Notes. The unit of observation is a firm. The dependent variable is the number of years with missing firm-level information: total missing years (columns 1–2), missing years in the pre-Allende period (columns 3–4), and missing years in the Allende period (columns 5–6). Regressions include nationalization status and, in even columns, baseline firm characteristics measured before 1970. Robust standard errors are reported in parentheses. The last row reports p-values from a joint test that all coefficients are equal to zero. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A2: Other firm-level outcomes

	Log total assets		Leverage		Fixed assets over total assets		Log sales	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Nationalized × Allende	0.058	0.151	-0.022	-0.016	0.012	-0.014	-0.307	-0.241
	(0.182)	(0.228)	(0.038)	(0.045)	(0.055)	(0.064)	(0.283)	(0.352)
Observations	390	390	385	385	388	388	390	390
Firm fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Sector-year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Covariates	N	Y	N	Y	N	Y	N	Y
Firms	71	71	71	71	71	71	71	71
Avg. dependent variable	16.73	16.73	0.391	0.391	0.654	0.654	15.82	15.82

Notes. The unit of observation is a firm-year. The table reports difference-in-differences estimates using alternative firm-level outcomes as dependent variables: log total assets in columns 1–2, leverage (debt over assets) in columns 3–4, fixed assets over total assets in columns 5–6, and log sales in columns 7–8. Odd-numbered columns include firm and sector-by-year fixed effects, while even-numbered columns additionally control for firm size in the pre-treatment period (1967–1969) interacted with year fixed effects. Standard errors are clustered at the firm level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A3: Differential pre-trends

	ROA		Sales over assets		Export		Import	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Nationalized × Linear trend	0.006	0.004	0.006	0.019	0.024	0.002	-0.034	-0.037
	(0.011)	(0.013)	(0.021)	(0.023)	(0.044)	(0.036)	(0.044)	(0.046)
Observations	234	234	234	234	233	233	233	233
R-squared	0.936	0.936	0.923	0.925	0.831	0.837	0.823	0.825
Firm fixed effect	Y	Y	Y	Y	Y	Y	Y	Y
Sector-Year fixed effect	Y	Y	Y	Y	Y	Y	Y	Y
Covariates	N	Y	N	Y	N	Y	N	Y
Firms	65	65	65	65	65	65	65	65
Average dep. variable	0.186	0.186	0.637	0.637	0.326	0.326	0.609	0.609

Notes. The unit of observation is a firm-year. The table reports difference-in-differences estimates testing for differential pre-trends prior to the Allende government. The sample is restricted to the pre-treatment period 1967–1970 because Salvador Allende only took office late in 1970, but results are robust to using 1967–1969 as pre-treatment period. The main regressor is an interaction between an indicator for firms that were later nationalized and a linear time trend. The absence of statistically significant coefficients provides evidence consistent with parallel trends between nationalized and non-nationalized firms before nationalization. All specifications include firm and sector-by-year fixed effects. Standard errors are clustered at the firm level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A4: Staggered difference-in-differences estimates

	ROA		Sales over assets		Export		Import	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post nationalization	-0.106***	-0.116***	-0.111**	-0.089**	-0.030	-0.029	-0.166**	-0.215***
	(0.031)	(0.037)	(0.045)	(0.039)	(0.072)	(0.067)	(0.072)	(0.076)
Observations	390	390	390	390	389	389	389	389
R-squared	0.820	0.822	0.824	0.828	0.795	0.799	0.794	0.798
Firm fixed effect	Y	Y	Y	Y	Y	Y	Y	Y
Sector-Year fixed effect	Y	Y	Y	Y	Y	Y	Y	Y
Covariates	N	Y	N	Y	N	Y	N	Y
Firms	71	71	71	71	71	71	71	71
Average dep. variable	0.188	0.188	0.643	0.643	0.326	0.326	0.611	0.611

Notes. The unit of observation is a firm-year. The table reports staggered difference-in-differences estimates using the two-way fixed effects (TWFE) estimator. The dependent variables are return on assets (ROA) in columns 1–2, sales over assets in columns 3–4, an indicator equal to one if the firm exports in columns 5–6, and an indicator equal to one if the firm imports in columns 7–8. Odd-numbered columns include firm and sector-by-year fixed effects, while even-numbered columns additionally control for firm size in the pre-treatment period (1967–1969) interacted with year fixed effects. Standard errors are clustered at the firm level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A5: Staggered difference-in-differences estimates (Borusyak et al., 2024)

	ROA	Sales over assets	Export	Import
	(1)	(6)	(7)	(8)
Panel A				
Post nationalization	-0.118***	-0.180***	-0.038	-0.137**
	(0.039)	(0.050)	(0.075)	(0.067)
Panel B: Adding sector-year FE				
Post nationalization	-0.114***	-0.132***	-0.023	-0.196**
	(0.036)	(0.050)	(0.083)	(0.078)
Avg. dependent variable	0.188	0.643	0.326	0.611
p-value pre-trend (panel A)	0.840	0.501	0.385	0.793
p-value pre-trend (panel B)	0.894	0.594	0.420	0.688
Treated	32	32	32	32
Never treated	39	39	39	39

Notes. The unit of observation is a firm-year. The table reports staggered difference-in-differences estimates using the imputation-based estimator proposed by Borusyak et al. (2024). The dependent variables are return on assets (ROA) in column 1, sales over assets in column 2, an indicator equal to one if the firm exports in column 3, and an indicator equal to one if the firm imports in column 4. Panel A includes firm and year fixed effects, while panel B includes firm and sector-by-year fixed effects. Standard errors are clustered at the firm level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A6: Difference-in-differences estimates for banking relations

	Any international		Any US		Any local private		Any state	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Nationalized × Allende	-0.125	-0.161	-0.013	0.060	-0.008	-0.014	-0.171*	-0.115
	(0.128)	(0.152)	(0.128)	(0.143)	(0.109)	(0.128)	(0.092)	(0.101)
Observations	390	390	390	390	390	390	390	390
R-squared	0.663	0.668	0.695	0.699	0.665	0.673	0.478	0.493
Firm fixed effect	Y	Y	Y	Y	Y	Y	Y	Y
Sector-Year fixed effect	Y	Y	Y	Y	Y	Y	Y	Y
Covariates	N	Y	N	Y	N	Y	N	Y
Firms	71	71	71	71	71	71	71	71
Average dep variable	0.650	0.650	0.504	0.504	0.604	0.604	0.854	0.854

Notes. The unit of observation is a firm-year. The table reports difference-in-differences estimates using measures of firms' banking relationships as dependent variables: an indicator equal to one if the firm has a relationship with an international bank in columns 1–2, an indicator equal to one if the firm has a relationship with a U.S. bank in columns 3–4, an indicator equal to one if the firm has a relationship with a domestic private bank in columns 5–6, and an indicator equal to one if the firm has a relationship with a state-owned bank in columns 7–8. Odd-numbered columns include firm and sector-by-year fixed effects, while even-numbered columns additionally control for firm size in the pre-treatment period (1967–1969) interacted with year fixed effects. Standard errors are clustered at the firm level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A7: Robustness of voting results

	Dependent variable: Left-wing vote share								
				Controlling by covariates × 1973					
	Congress elections	Collapse pre/post	*Variable* is linear pre-trend	Institutional presence	Spatial dependence	Socio-economic	*Variable* is non-nationalized large firms	Province-year fixed effects	Excl. province capitals
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Any nationalization									
Nationalizations × 1973	-4.13***	-3.97***		-4.12***	-3.52***	-1.51	-3.67***	-2.73***	-5.42***
	(1.26)	(1.12)		(1.11)	(1.14)	(1.33)	(1.08)	(0.84)	(1.13)
Nationalizations × *Variable*			0.29*				-0.75		
			(0.16)				(1.18)		
Panel B: Number of nationalizations									
Nationalizations × 1973	-0.34***	-0.36***		-0.40***	-0.34***	-0.18***	-0.75***	-0.27***	-1.66***
	(0.09)	(0.08)		(0.10)	(0.11)	(0.06)	(0.21)	(0.08)	(0.39)
Nationalizations × *Variable*			0.01				0.43**		
			(0.01)				(0.20)		
Panel C: Nationalizations over pop.									
Nationalizations × 1973	-1.66**	-1.73**		-1.76**	-1.35**	-1.02**	-1.61***	-0.95*	-1.11
	(0.69)	(0.67)		(0.69)	(0.68)	(0.51)	(0.61)	(0.55)	(0.69)
Nationalizations × *Variable*			0.03				-0.61		
			(0.08)				(0.49)		
Observations	1224	614	1529	2143	2143	2143	2143	2143	1968
Municipality fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Avg. dependent variable	27.01	30.99	24.81	28.22	28.22	28.22	28.22	28.22	28.33
Municipalities	307	307	307	307	307	307	307	307	282

Notes. This table presents robustness checks for the municipality-level voting results using difference-in-differences estimates. The sample includes 307 municipalities observed in four congressional elections (1961, 1965, 1969, and 1973) and three local elections (1963, 1967, and 1971). The cross-sectional treatment varies by panel: panel A uses an indicator for exposure to nationalization, panel B the number of nationalized plants, and panel C the number of nationalized plants per 1970 population. Each column implements a different robustness exercise: Column 1 restricts the sample to congressional elections; Column 2 collapses the data into pre- and post-1973 periods; Column 3 tests for pre-trends using a linear trend interacted with exposure; Columns 4–7 add alternative controls interacted with a 1973 indicator; Column 8 includes province-by-year fixed effects; and Column 9 excludes from the estimation the 25 municipalities that were province capitals in 1973. Standard errors in parentheses are clustered by municipality. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.