Reform Bundling and the Labour Share: Evidence from Korea's *Chaebol* Restructuring

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Abstract

Structural reform episodes typically bundle multiple institutional changes, yet the distributional consequences remain poorly understood. This paper exploits South Korea's 1998 reform package—combining competition enforcement, labour market liberalization, and state-directed corporate restructuring—targeting its top 30 *chaebol* conglomerates. Using firm-level panel data (1991–2011) and generalized synthetic control, I find the reform bundle reduced treated firms' labour share by 7–8 percentage points, offset by rising profit shares. The decline is overwhelmingly within-firm, with near-unitary elasticities of substitution ruling out capital deepening. Competition reform's distributional effects depend critically on the institutional environment in which it is embedded.

Keywords — Reform bundling, labour share, competition policy, *chaebols*, market power, Korea

***JEL Classification*:** D22, D33, E25, J21, J31, L40, O53

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1 Introduction

Structural reform episodes—whether triggered by financial crises, conditionality agreements, or deliberate policy reorientation—typically combine multiple institutional changes simultaneously (Williamson, 2000; Rodrik, 2006). Competition enforcement arrives alongside labour market flexibility, corporate governance overhauls, and macroeconomic stabilization; these complementary reforms interact in ways that a single-policy framework cannot capture (Blanchard and Giavazzi, 2003). This bundling is not a research inconvenience; it is the central reality of reform design. Yet virtually all existing evidence on the distributional consequences of competition policy comes from cross-country correlations that capture steady-state regulatory environments (Ennis et al., 2019; Zac et al., 2023; Cheng et al., 2024), not the abrupt, crisis-driven reform episodes in which multiple institutional changes coincide. Whether the positive relationship between competition enforcement and labour income shares documented in these studies survives when reforms are bundled with labour market liberalization and large-scale corporate restructuring is an open and policy-critical question.

This paper studies the distributional consequences of one of the most ambitious bundled reform episodes in modern economic history: South Korea's post-1997 restructuring of its dominant *chaebol*[1] conglomerates in the aftermath of the Asian Financial Crisis. The reform package combined three distinct institutional interventions: (i) pro-competitive reforms—stricter debt-equity requirements, cross-shareholding restrictions, and enhanced corporate transparency rules aimed at curbing market power; (ii) IMF-mandated labour market liberalization that relaxed employee protection rules and expanded the use of non-regular employment; and (iii) a state-directed corporate restructuring program known as the *Big Deals* (1998–1999), which required the five largest conglomerates—Samsung, Hyundai, LG, Daewoo, and SK—to swap business units and consolidate sectoral specialization across strategic industries. These overlapping interventions targeted the same set of firms simultaneously, making the Korean episode an ideal setting to study how reform bundling shapes

[1] *Chaebols* are conglomerates typically characterized by family-based ownership and management, with many vertically or horizontally integrated subsidiaries. According to the Korea Fair Trade Commission (KFTC), business groups whose total assets exceed 5 trillion Korean Won (KRW) (approximately USD 4.47 billion as of 2021) are designated as *chaebol*. Most originated during the colonial and post-war periods and grew rapidly during the 1970s and 1980s through selective credit policies favouring heavy and chemical industries.

the functional income distribution.

The raw distributional dynamics already pose a puzzle. Although the crisis initially drove down labour shares economy-wide, the labour share among treated *chaebols* failed to durably recover. After a brief post-crisis rebound in the early 2000s, it fell sharply after 2003 and remained persistently depressed through the end of the sample period. This persistent divergence raises a fundamental question: did the bundled reform package—combining competition enforcement with labour market liberalization and corporate restructuring—contribute to a redistribution of income away from labour?

Using firm-level data on publicly listed Korean firms from 1991 to 2011, I estimate the causal effect of the 1998 reform package on the functional income distribution using the generalized synthetic control (GSC) method (Xu, 2017). Exploiting the fact that the top 30 *chaebols* were explicitly targeted while smaller listed firms were largely unaffected, I find that, relative to untreated firms, the reforms reduced the labour share among the top 30 *chaebols* by approximately 7–8 percentage points in the medium to long run, with the decline fully offset by a corresponding increase in the profit share. These estimates are robust to conventional difference-in-differences specifications and to alternative treatment group definitions.

A key identification challenge—and a central focus of this paper—is that the bundled nature of the reforms makes it difficult to attribute the labour share decline to any single policy component. I address this through two decomposition strategies. First, a bracketing decomposition isolates the contribution of the state-directed *Big Deal* restructuring program by comparing estimates from the full sample with those excluding Big Five *chaebol* affiliates. The labour share decline survives this exclusion, though the *Big Deal* component accounts for roughly half the long-run effect. Second, a proxy-based channel decomposition exploits cross-sectional variation in pre-reform labour market exposure to separate the competition reform channel from the labour liberalization channel. Across four alternative proxies, the competition channel accounts for a substantial share—ranging from 41% to over 100%—of the average treated firm's labour share decline, with labour liberalization contributing comparably in some specifications. These exercises narrow—though do not fully eliminate—the attribution gap. The irreducible bundling is itself informative: it reflects the policy reality that reformers face, and the estimated joint effect is the policy-relevant quantity for evaluating such episodes.

I then investigate the transmission mechanisms. Firm-level elasticities of substitution estimated using the cost-share approach of Oberfield and Raval (2021) are statistically indistinguishable from unity ($\hat{\sigma} \approx 1.0$), ruling out the capital-deepening channel emphasised by Karabarbounis and Neiman (2013). An Olley–Pakes decomposition (Olley and Pakes, 1996) reveals that the decline is overwhelmingly within-firm: individual *chaebols* reduced their labour shares while holding their relative output positions approximately constant, with corresponding within-firm increases in markups and profit shares. Between-firm reallocation actually partially offset the decline. Combined with the widening productivity–wage gap among treated firms, the evidence points to a transformation in bargaining power and surplus division as the dominant mechanism—consistent with the concurrent weakening of employment protections—rather than capital-biased technological substitution. While firm-level markups rose among treated firms (estimated following De Loecker and Warzynski, 2012), the aggregate markup treatment effect is not consistently significant, suggesting that shifts in bargaining power rather than product-market power are the primary channel.

The paper makes three main contributions. First, and most centrally, it provides evidence on a fundamental policy design problem: the distributional consequences of pro-competitive reform depend critically on institutional bundling and complementary safeguards. When competition enforcement is implemented alongside labour market liberalization and large-scale corporate restructuring, the gains from improved efficiency need not accrue to workers. The Korean episode demonstrates that the *same* reform that enhances productivity can simultaneously reduce labour's claim on value added—not because competition policy is inherently regressive, but because the concurrent weakening of bargaining institutions shifts surplus division within firms. This finding carries direct implications for contemporary debates on competition policy, industrial strategy, and structural adjustment: reform packages must be evaluated as packages, and distributional outcomes depend on what accompanies competition enforcement.

Second, the paper advances the debate on transmission mechanisms by ruling out the capital-deepening channel and identifying surplus reallocation *within* firms as the operative margin. The decomposition results, combined with markup and profit-share evidence, indicate that redistribution occurred through internal rent division rather than technological substitution or reallocation toward more capital-intensive producers. In this respect, the Korean experience connects to the broader literature on markups and profit share dynamics (e.g., De Loecker et al., 2020; Barkai, 2020; van Vlokhoven, 2024), demonstrating how similar distributional outcomes can arise from policy-induced restructuring rather than purely market-driven concentration.

Third, the paper provides the first firm-level causal evidence from a well-identified reform episode on the joint effect of bundled market liberalization on the functional income distribution. While existing work documents a global decline in labour's income share (e.g., Karabarbounis and Neiman, 2013; Autor et al., 2020; Barkai, 2020), much of it relies on aggregate correlations or technological explanations. Cross-country studies find that competition law enforcement is associated with higher labour income shares (Zac et al., 2023; Cheng et al., 2024), but these estimates capture steady-state regulatory environments rather than crisis-driven reform episodes. By exploiting a quasi-experimental setting in Korea's *chaebol*-dominated economy, I show that the cross-country relationship can reverse when competition reform is bundled with labour market flexibilization—a finding that qualifies rather than contradicts the existing literature.

The remainder of the paper is organised as follows. Section 2 reviews the related literature. Section 3 describes the institutional context of Korea's pro-competitive reforms. Section 4 discusses the data and variable construction. Section 5 presents the empirical strategy. Section 6 reports the main results and transmission mechanism analysis. Section 7 conducts several robustness checks, and Section 8 concludes with policy implications.

2 Related Literature

A growing literature documents the persistent decline in labour's share of income across advanced and emerging economies since the 1980s. Explanations span rising firm-level market power and markups (Hall, 2018; De Loecker et al., 2020), globalization and weakening bargaining positions (Rodrik, 1998; Elsby et al., 2013), and capital-augmenting technological change (Karabarbounis and Neiman, 2013; Acemoglu and Restrepo, 2018; Grossman and

Oberfield, 2022). Yet most evidence remains descriptive or cross-country in nature, and relatively little is known about how specific institutional reform episodes causally shape the functional distribution of income at the firm level.

Recent work on "superstar'," firms posits that the growing disparity between corporate profit share and labour share reflects the dominance of a few high-markup firms (De Loecker and Eeckhout, 2018; Autor et al., 2020). This implies that competition policy plays a central role in shaping rent distribution (Stiglitz et al., 2013; Baker and Salop, 2015; Zac, 2021). Cross-country evidence suggests that effective competition law is associated with higher labour income shares (Ennis et al., 2019; Zac et al., 2023), but most studies rely on correlational designs rather than clearly identified reform episodes. Importantly, the reform complementarity literature highlights that the effects of product market deregulation depend on concurrent labour market institutions (Blanchard and Giavazzi, 2003; ?), yet this interaction has not been examined through the lens of the functional income distribution. The Korean reform literature, in turn, has focused predominantly on efficiency outcomes: Aghion et al. (2021) document that pro-competitive reforms facilitated Korea's transition to innovation-based growth, but do not address how these reforms affected the functional income distribution.

A central empirical challenge is distinguishing capital–labour substitution from changes in surplus division. Under CES production, falling labour share requires $\sigma > 1$ (Karabarbounis and Neiman, 2013), yet firm-level evidence frequently finds elasticities near or below unity (Oberfield and Raval, 2021; Gechert et al., 2022). Rising profit shares (Barkai, 2020) suggest that redistribution may operate through market power rather than technological substitution. We address this by combining a first-order-condition approach to estimate σ with an Olley–Pakes decomposition (Olley and Pakes, 1996) that separates within-firm changes from compositional reallocation, and find that the post-reform decline is overwhelmingly driven by within-firm changes in surplus allocation.

3 Background: Institutional Context and Reform Bundling

The 1998 pro-competitive reforms marked a structural shift in South Korea's political economy. For several decades prior to the Asian Financial Crisis, Korea's development model combined rapid industrialization with extensive state coordination of *chaebols*. Industrial policy, directed credit, and implicit debt guarantees enabled these conglomerates to expand rapidly across sectors, often under high leverage. Competition policy existed formally under the Monopoly Regulation and Fair Trade Act (MRFTA), enacted in 1981, but enforcement remained secondary to growth objectives throughout the high-industrialization period (Wise, 2000; Cherry, 2005).

The 1997 Asian Financial Crisis disrupted this equilibrium. The collapse of highly leveraged business groups exposed systemic vulnerabilities tied to debt accumulation, cross-guarantees, and opaque governance structures. In response, the government entered into an IMF stabilization program that required sweeping structural reforms. Product market liberalization, corporate restructuring, and labour market reforms were introduced in rapid succession. These measures were not implemented independently; rather, they formed a bundled reform package reshaping both market structure and institutional bargaining conditions.

This section clarifies how the reforms targeted specific firms, how concurrent restructuring interacted with competition enforcement, how labour market reforms altered bargaining conditions, and how post-reform developments complicate attribution.

3.1 Reform Targeting and Corporate Restructuring (*Big Deals*)

A central component of the reform package imposed strict debt-equity ceilings on the top 30 *chaebols*, requiring ratios below 200 percent and dismantling mutual debt guarantees (Chang, 2003; Haggard et al., 2003). Antitrust enforcement was strengthened and internal capital market practices subjected to tighter scrutiny.

In parallel, the government initiated the *Big Deals*—mandated asset swaps, mergers, and consolidations among the largest conglomerates aimed at reducing excess capacity. Implementation often took the form of one-sided acquisitions rather than mutually negotiated exchanges (Shin, 2003; Cherry, 2005). The five largest conglomerates were particularly exposed.

The bundled nature of the reforms is critical: the same firms facing heightened competition enforcement were simultaneously subject to debt constraints, asset reallocation mandates, and governance reforms. These overlapping interventions create heterogeneity within the treated group and motivate the subgroup analysis below. The treated group comprises affiliates of the top 30 *chaebols* as designated by the KFTC at the onset of the reform period; the control group consists of firms outside this designation. Section 5 provides full treatment assignment criteria. A detailed reform chronology is provided in the Supplemental Appendix (Figure A.1).

3.2 Labour Market Reform

The IMF stabilization program required substantial labour market liberalization. Legislative changes in February 1998 expanded firms' ability to dismiss workers under "urgent managerial necessity,',' legalized temporary employment agencies, and broadened non-regular employment contracts (Kwon, 1998; Haggard et al., 2003)—a significant departure from the pre-crisis regime, where dismissals were heavily constrained both legally and through implicit long-term employment norms within large *chaebol* affiliates (Wise, 2000).

Prior to the crisis, Korea combined relatively low aggregate union density with strong firm-level employment protections. Although union density had been declining since the late 1980s (Kuruvilla et al., 2002), internal labour markets, seniority-based promotion, and constrained dismissals sustained a stable distribution of surplus within conglomerates.

The 1998 reforms altered this framework through multiple channels. Expanded legal scope for layoffs weakened credibility of implicit long-term employment guarantees underpinning firm-level wage bargaining. The legalization of temporary and dispatched employment facilitated growth of a non-regular workforce outside collective bargaining coverage, producing marked labour market dualization (Grubb et al., 2007). These developments eroded not only formal union influence but also the broader institutional complementarities—internal labour markets, promotion ladders, and de facto dismissal constraints—that had shaped surplus division within large firms.

The simultaneity of labour market liberalization with product-market reform raises an identification concern: if the 1998 labour law changes disproportionately affected *chaebol* affiliates, differential movements in labour share may partly reflect weakened bargaining rather than competition enforcement alone. The estimated treatment effects should therefore be interpreted as capturing the joint impact of a bundled reform package; the empirical strategy below constructs counterfactual trajectories to help disentangle these overlapping channels.

3.3 Institutional and Employment Dynamics

Figure 1 presents the joint evolution of labour share, aggregate trade union density, and firm-level employment for treated firms between 1991 and 2011. The vertical dashed lines mark the 1997 crisis and the recession episodes of 2003 and 2008; the solid line marks the 1998 reform implementation. Three phases of adjustment are apparent.

Pre-reform (1991–1997): trade union density declined from approximately 16 to 11 percent, and mean employment among treated firms fell by roughly 13 percent. Despite these changes, labour share remained stable between 0.49 and 0.52, suggesting that institutional features of the *chaebol* employment system—long-term employment norms, constrained dismissal, and internal labour markets—buffered surplus allocation even as formal bargaining power weakened.

Crisis and restructuring (1998–2003): employment contracted by about 14 percent over four years, reflecting both post-reform restructuring and the 2003 domestic credit card crisis.[2] Labour share fell during the immediate crisis, partially recovered, and then resumed its decline—likely reflecting transitional bargaining adjustments and denominator effects during recovery.

Post-restructuring (2003–2011): employment stabilized while union density leveled off near 10 percent, yet labour share continued falling from approximately 0.47 in 2003 to below 0.30 by 2009. The decline persists through both the mid-2000s expansion and the 2008 global

[2]See OECD (2004) for details.

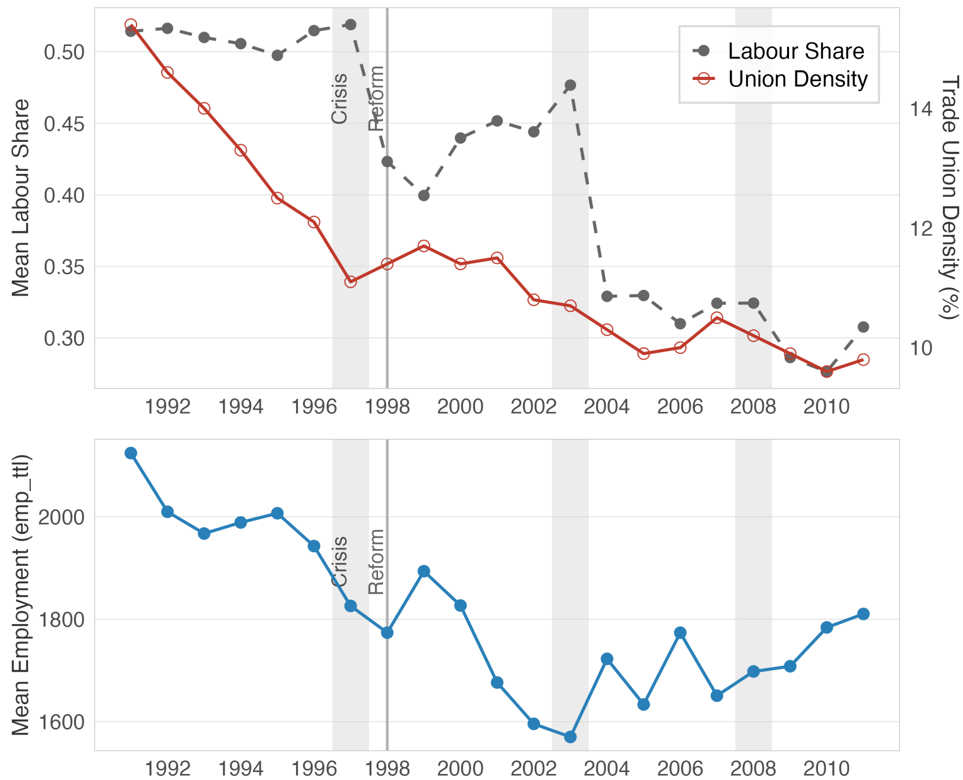


Figure 1: Labour Share, Trade Union Density, and Employment

Notes: The top panel reports mean labour share among treated firms (left axis) and Korea's aggregate trade union density (right axis). The bottom panel reports mean total employment among treated firms. The dashed vertical lines indicate the 1997 crisis and subsequent recession episodes (2003 recession, also known as credit card crisis, 2008 global financial crisis); the solid vertical line indicates the 1998 pro-competitive reforms. Union density is defined as the percentage of union members among all wage and salaried workers. Union density data are from the OECD/AIAS ICTWSS database: https://www.oecd.org/en/data/datasets/oecdaias-ictwss-database.html.

financial crisis, consistent with a structural shift in surplus allocation rather than workforce contraction, union erosion, or business-cycle dynamics.

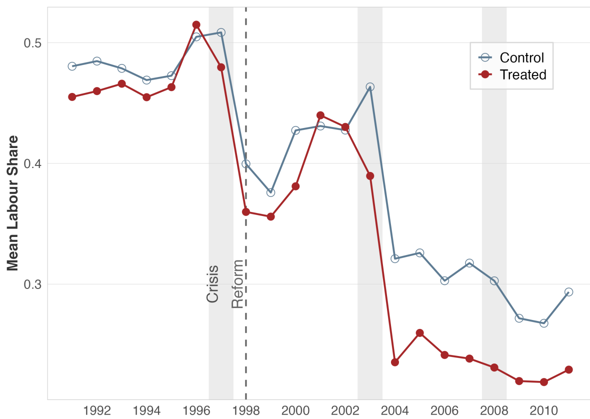
Two additional findings, examined in Section 6, support this interpretation. First, the estimated elasticity of substitution satisfies $\sigma \leq 1$, under which capital deepening should *raise* labour share (Glover and Short, 2020); the coexistence of capital deepening and declining labour share therefore points toward market-power channels. Second, treated firms exhibit a widening productivity–wage gap, indicating that productivity gains were not transmitted to compensation at pre-reform rates.

3.4 Distributional Patterns Across Factor Shares

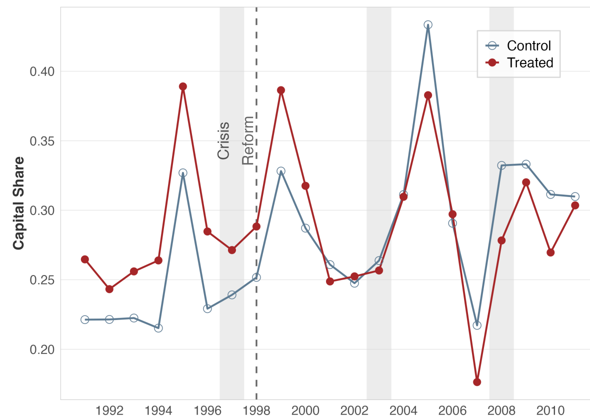
Figure 2 presents average labour, capital, and profit shares for treated and control firms. Prior to the crisis, treated and control firms follow broadly similar labour-share trajectories, with treated firms exhibiting slightly lower levels. Following 2003, labour share among treated firms declines sharply relative to controls and remains persistently lower. Capital share patterns are more volatile and do not display a sustained monotonic increase among treated firms in the post-reform period. Profit share movements mirror the labour share divergence: after a contraction in the early 2000s, profit share among treated firms rises from the mid-2000s onward and remains persistently above that of the control group, consistent with redistribution within value added rather than a contraction in total surplus.

These patterns are descriptive and do not establish causality. Differential exposure to crisis intensity, sectoral composition shifts during the 2000s export expansion, and contemporaneous macroeconomic shocks may also contribute.[3] The next section describes the data and sample construction, followed by the empirical strategy used to identify the causal impact of the reform episode.

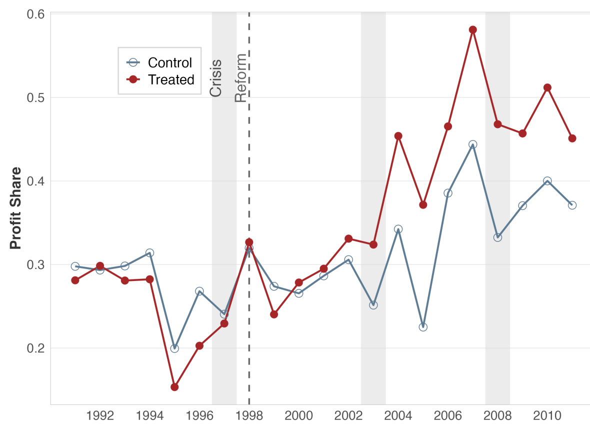
[3] The early 2000s were characterized by a semiconductor-led export expansion and a domestic credit-card crisis (2002–2003), both of which could influence firm-level outcomes independently of the reform package.



(a) Labour Share



(b) Capital Share



(c) Profit Share

Figure 2: Factor Share Trends by Group

Notes: The figure reports mean labour, capital, and profit shares for treated and control firms between 1991 and 2011. Vertical lines indicate the 1997 Asian Financial Crisis, the 1998 pro-competitive reforms, and subsequent recession episodes. Each share is winsorized at the 2.5 and 97.5 percentiles to mitigate the influence of extreme values that may reflect measurement error while preserving the overall distribution of the data.

4 Data

Data on the financial statements of the companies listed on the Korean stock market were provided by the Data Analysis, Retrieval and Transfer System (DART), operated by the Financial Supervisory Service.[4] Since 1992, the Korea Fair Trade Commission (KFTC) has designated the top 30 largest *chaebols* annually by asset size.[5] The KFTC defines a large business group as one whose combined assets exceed KRW 5 trillion[6] or that controls more than 30% market share in a given industry. The summary is presented in Table 1.

Table 1: Sample Composition of *Chaebols* and Non-*Chaebols*

	Top 30 *Chaebols*	Non Top 30 *Chaebols*	Total
Number of unique firms	66	384	450
Percent	14.67%	85.33%	100%
Number of observations	1,382	7,978	9,360
Percent	14.76%	85.24%	100%

Notes: The dataset is an unbalanced firm-level panel spanning 1991–2011. Panel length varies across firms due to entry, exit, and intermittent reporting. The treated group accounts for 14.7 percent of unique firms and 14.8 percent of firm-year observations, indicating similar panel coverage across groups.

Industries are classified at the two-digit level of the Korean Standard Industrial Classification (KSIC), yielding 45 sectors after dropping public administration and other non-market activities (see Table A.3 in the Supplemental Appendix). We further distinguish 18 *chaebol*-dominant sectors (top 30 market share above the median) from 28 *chaebol*-present sectors (Table 2).

Table 2: Firm Counts in the *Chaebol*-Dominant Sectors

	Treated	Control	Frequency	Percent
Firms in non-*chaebol*-dominant sectors	26	247	273	60.67
Firms in *chaebol*-dominant sectors	40	137	177	39.33
Total	66	384	450	100

Note: This classification is based on the top 30 largest *chaebols*' market share during the pre-treatment years in each industry sector.

[4] http://englishdart.fss.or.kr/.

[5] See the Supplemental Appendix.

[6] Approximately USD 0.84 billion as of 2021.

4.1 Dependent Variables

The baseline model focuses on three factor shares —labour share (S^L), capital share (S^K), and profit share (S^Π)—and the productivity–wage gap as key outcome variables.

Labour Share: Labour share is defined as the ratio of total labour cost to value added, $S^L_{ft} = w_{ft}L_{ft}/Y_{ft}$, computed directly from firm-level financial disclosures sourced from the Financial Supervisory Service's Data Analysis, Retrieval and Transfer System (DART) and value added figures. Because the sample consists entirely of incorporated firms reporting audited financial statements, the measure reflects payroll share and avoids several contentious measurement issues that arise in aggregate data. In particular, firm-level labour share does not require adjustment for self-employment income—a source of disagreement in the literature, with Elsby et al. (2013) arguing that equating payroll and self-employment income overstates labour share and Gollin (2002) showing that failing to adjust understates it, especially in developing countries. The measure also excludes housing capital, which Rognlie (2015) shows can inflate the aggregate capital share.

Both labour cost and value added are deflated by the Producer Price Index (PPI) and winsorized at the 1st and 99th percentiles before computing the ratio. The resulting labour share is winsorized at the 2.5 and 97.5 percentiles and restricted to the unit interval $[0, 1]$.[7]

This yields a main estimation sample of 9,144 firm-year observations. The constructed labour share correlates at $\rho \approx 0.99$ with the raw accounting labour share reported in the original data, confirming that results are not sensitive to the deflation or winsorization procedure. Importantly, the estimated treatment effects remain stable across alternative sample definitions, including relaxed trimming rules and restriction to the common decomposition sample where labour, capital, and profit shares are jointly defined (Supplemental Appendix).

Capital Share: Capital share is defined as the ratio of capital cost to value added, $S^K_{it} = R_{jt}K_{it}/Y_{it}$. Unlike labour share, the capital share cannot be read directly from financial statements: most firms own their capital stock, so the cost of using production capital—which

[7] Observations with labour share exceeding unity—indicating that a firm's wage bill exceeds its value added—arise for 251 observations (2.7 percent of the panel), predominantly among control firms (238 of 251; the Supplemental Appendix). These cases reflect temporary financial distress rather than distributional outcomes of analytical interest. Results are robust to including these observations and to relaxing the upper bound to 1.5 or 2.0 (Supplemental Appendix).

includes both depreciation and the opportunity cost of the funds tied up in physical assets—is not separately reported. The accounting-based capital share, defined mechanically as one minus the labour share, is essentially uncorrelated with our cost-based measure ($\rho \approx 0.08$), underscoring the importance of distinguishing capital costs from profits.

We recover the user cost of capital following the production-function-based method of van Vlokhoven (2024), which identifies capital cost from cross-sectional variation in firms' input choices within industry–year cells. The estimation equation is:

$$\frac{PY_f}{X_f} = \psi + \psi R \frac{K_f}{X_f} + \varepsilon_f \tag{4.1}$$

where PY_f denotes firm-level sales revenue, X_f variable costs, ψ the price-to-average-cost ratio, and R the user cost of capital.[8] The user cost is recovered as the ratio of the slope to the intercept: $\hat{R} = \widehat{\psi R}/\hat{\psi}$. Capital share is then calculated as $S^K_{ft} = \hat{R}_{jt} \cdot K_{ft}/Y_{ft}$, where the subscript jt reflects that the user cost is estimated at the industry–year level and is common to all firms within each cell.

To ensure broad coverage, we implement the estimation hierarchically when industry–year cells contain fewer than 30 observations. We first estimate the structural VV model at the industry–year level. When this is infeasible, we relax the level of aggregation while preserving the structural framework, using industry-pooled regressions with year fixed effects. If structural estimates remain unavailable, we impute missing values using the year median of valid structural estimates, followed by firm-level approximations based on $r_f + \delta_f$, and finally a conservative benchmark rate of 15 percent as a last resort.[9]

Supplemental Appendix Table A.4 reports the distribution across imputation levels:

[8] In practice, the regression includes a quadratic term in K_f/X_f and a set of control variables—capital-weighted demeaned risk proxies, depreciation rates, sales growth, and leverage ratios, as well as their interactions with capital intensity—to absorb heterogeneity in markups and risk premia across firms. All regressions are weighted by firm-level capital stock. Two identification assumptions are required (van Vlokhoven, 2024): (i) the capital-input ratio is uncorrelated with the price-to-average-cost ratio, $\mathrm{Cov}(K_f/X_f, \psi_f) = 0$; and (ii) the capital-input ratio is uncorrelated with $\psi_f R_f$, which requires that the cost of capital is equalized across firms within each industry–year cell.

[9] For capital stock, the hierarchy proceeds as follows: we begin with reported tangible assets; interpolate at the firm level using adjacent observations when missing; apply industryâĂŞyear tangible-to-total asset ratios to firm-level total assets; use year-specific global ratios; and, as a final fallback, scale industryâĂŞyear capitalâĂŞlabour ratios by firm-level employment. Supplemental Appendix Table A.4 reports the distribution of observations across imputation levels.

approximately 42 percent of user-cost observations are structurally estimated, with similar shares for treated and control groups, mitigating concerns about differential imputation intensity. Restricting to structurally estimated observations yields qualitatively identical treatment effects. Capital share is winsorised at the 1 and 99 percentiles.

Profit Share: Profit share is defined residually as $S^{\Pi}_{it} = 1 - S^{L}_{it} - S^{K}_{it}$, following Barkai (2020), who stresses the distinction between capital costs and pure economic profit when examining the sources of labour share decline. We note that this residual may include components such as indirect taxes that cannot be separately identified in our data. Profit share is winsorized at the 1st and 99th percentiles.

Sample Structure: The three-way decomposition requires that all factor share inputs pass through a common hierarchical imputation pipeline to ensure the adding-up identity $S^L + S^K + S^{\Pi} = 1$ holds by construction. This restricts the decomposition sample to 8,198 firm-year observations (87.3 percent of the panel). The labour share computed from this pipeline correlates at $\rho \approx 0.99$ with the main labour share measure described above. The main labour share results use the full bounded sample of 9,144 observations, which does not depend on the capital cost estimation or the hierarchical imputation of inputs. As a robustness check, the Supplemental Appendix reports the main results estimated on the full, bounded, and decomposition samples; point estimates are stable across specifications.

Labour Productivity and Productivity–Wage Gap: Labour productivity is defined at the firm level as the ratio of real value added to total employment, $\ell^p_{ft} = VA_{ft}/L_{ft}$, where VA_{ft} is deflated by the PPI and L_{ft} is the total number of employees. To capture the degree to which productivity gains are passed on to workers, we construct the productivity–wage gap as:

$$\ell^p_{ft} - w_{ft} = \ln\left(\frac{VA_{ft}}{L_{ft}}\right) - \ln\left(\frac{W_{ft}}{L_{ft}}\right) = \ln\left(\frac{VA_{ft}}{W_{ft}}\right) \tag{4.2}$$

where W_{ft} denotes the total wage bill—the narrow payroll component of labour compensation, excluding retirement pay and welfare expenditure.[10] A widening gap indicates that value added per worker is growing faster than direct compensation per worker, a standard indicator

[10]This distinction matters because W_{ft} (the wage bill) is a narrower concept than total labour cost, which additionally includes retirement pay and welfare expenditure. Using the wage bill isolates the gap between what workers produce and what they receive as direct pay.

of wage–productivity decoupling.

Markup Ratio: We estimate firm-level markups following the production approach of De Loecker and Warzynski (2012), defining the markup as the ratio between the output elasticity of a flexible input and its revenue share. Our preferred measure uses intermediate inputs from a gross output Cobb-Douglas production function estimated via Ackerberg et al. (2015):

$$\ln Y_{ft} = \beta_K \ln K_{ft} + \beta_L \ln L_{ft} + \theta^M \ln M_{ft} + \omega_{ft} + \varepsilon_{ft} \tag{4.3}$$

where K_{ft} is deflated tangible assets, L_{ft} total employment, M_{ft} real intermediate inputs, ω_{ft} unobserved productivity, and ε_{ft} an i.i.d. error. Labour and materials are treated as freely adjustable; capital is a state variable with lagged materials proxying for the productivity shock. Intermediate inputs aggregate core operational inputs and sales costs, each deflated by PPI and winsorized at the 1 and 99 percentiles.[11]

Using $\hat{\theta}^M$, firm-level markups are:

$$\mu_{ft} = \hat{\theta}^M \cdot \frac{Y_{ft}}{M_{ft}} \tag{4.4}$$

Markups are winsorized at the 2.5 and 97.5 percentiles.[12] Naive OLS fixed-effects estimates are reported as a benchmark in the Supplemental Appendix; comparing with the ACF estimates reveals the expected transmission bias correction.

4.2 Treatment Variables

The treatment group comprises firms affiliated with the top 30 *chaebols*, which were the explicit target of the government's reform agenda following the 1997 Asian Financial Crisis. We construct a hierarchy of treatment definitions that vary in stringency, forming a

[11] This corresponds to the "narrow" materials definition. As a robustness check, we also employ a broader definition including utilities and data processing expenses.

[12] To maximise coverage, we construct a hierarchical markup measure. When the materials-based DLW estimate is unavailable, we substitute, in order: an industry-specific markup based on industry-level production functions estimated separately by two-digit industry, a labour-based LP markup from the value-added specification of Levinsohn and Petrin (2003), a labour-based DLW markup using the revenue labour share, and finally a markup derived from the price-cost margin. All alternative measures are winsorized at the 2.5 and 97.5 percentiles. In our sample, the materials-based DLW estimate provides coverage for the vast majority of observations.

bracketing argument around the reform's effects, and introduce two additional measures to exploit within-*chaebol* variation in reform intensity.

Baseline specification: Our baseline specification ($Reform_f^B$) classifies all top 30 *chaebol* affiliates as treated, regardless of their pre-crisis debt-to-equity ratio. This definition directly mirrors the scope of the policy: the government's five reform directives—capital structure improvement, elimination of cross-debt guarantees, enhanced transparency, concentration of core competencies, and strengthened accountability of controlling shareholders—applied to all top 30 groups as designated by the Korea Fair Trade Commission (KFTC) in 1998. No group was exempted on the basis of its leverage. The baseline yields 66 treated and 385 control firms.[13]

Sensitivity and robustness specifications: A central pillar of the reform was the mandate that all *chaebols* reduce their debt-to-equity ratios below 200 percent by the end of 1999. While the reform applied to all top 30 groups, this threshold determined which affiliates faced binding restructuring pressure on their capital structure. To test whether the reform's effects concentrate among these more constrained firms, we construct two debt-conditioned variants.

Our sensitivity specification ($Reform_f^S$) restricts treatment to top 30 affiliates whose debt-to-equity ratio exceeded 200 percent as of 1997. This uses leverage observed at the onset of the reform—and is therefore closest to the information the government itself used when designing the restructuring mandates. Because 1997 was the crisis year, however, firms' year-end leverage may partly reflect crisis-induced distress rather than pre-existing financial structure.

Our robustness specification ($Reform_f^R$) replaces the 1997 debt-to-equity ratio with the 1996 value—the last pre-crisis year—ensuring that treatment assignment is determined

[13]Several top 30 groups underwent separation after the crisis. The original Hyundai group split into Hyundai Motor, Hyundai Heavy Industries, Hyundai Department Store, HDC, and Hyundai Corporation; LG separated into LG, GS, and LS; CJ separated from Samsung in 1996. We assign all successor-group affiliates to their original top 30 group, since they were subject to the reform as a unified entity during the treatment period. Seven of the thirty groups—Daewoo (#3), Kohap (#17), Shinho (#25), Newcore (#27), Geopyeong (#28), Kangwon Industries (#29), and Saehan (#30)—have no surviving affiliates in the DART filing system, as most were liquidated during the crisis. The sole surviving Daewoo entity is Daewoo Shipbuilding & Marine Engineering. By design, we exclude holding companies, captive intra-group service subsidiaries, and groups with no DART-filing affiliates.

entirely by leverage that predates both the crisis and the reform announcement. Both debt-conditioned specifications yield 50 treated firms.[14]

The three specifications form a coherent bracketing argument. The baseline captures the full scope of the policy mandate; the sensitivity variant tests whether effects are amplified among firms with binding leverage constraints; and the robustness variant confirms that this amplification is not an artifact of crisis-year contamination in the debt ratio. Treatment assignment under all three definitions is time-invariant and pre-determined; it is never updated based on post-reform outcomes.

For identification in the Generalized Synthetic Control (GSC) framework, treatment switches on in 1998, the first full year of reform implementation. In the standard difference-in-differences (DiD) specification, the treatment group indicator is interacted with a post-reform dummy. Our estimation sample restricts to firms observed in at least one pre-crisis year ($\leq$ 1996) and in 1998, to ensure a genuine pre-treatment baseline and identification at reform onset.

Treatment intensity: Within the top 30, reform pressure was not uniform. The five largest conglomerates—Hyundai (#1), Samsung (#2), Daewoo (#3), LG (#4), and SK (#5)—were subject to the majority of Big Deal directives and the strongest scrutiny. We define $\mathit{Top5}_f$ as an indicator for affiliates of these five groups (31 treated firms); groups ranked 6–30 form a moderate-scrutiny category ($N = 35$):

$$\underbrace{\text{Control}}_{N=385} < \underbrace{\text{Ranks 6–30}}_{\substack{N=35 \\ \text{moderate scrutiny}}} < \underbrace{\text{Top 5}}_{\substack{N=31 \\ \text{intense scrutiny}}} . \tag{4.5}$$

The identification argument is group-level: every affiliate was affected through disruptions to internal capital markets, forced asset disposals, and cross-guarantee unwinding. A monotone gradient across tiers would constitute evidence of dose-response in reform intensity.

Firm-level Big Deal participants: Between December 1998 and 1999, the government brokered eight sector-specific transactions directing affiliates to swap, merge, or divest business

[14] The 16 top 30 affiliates excluded by the debt condition have a mean 1996 debt-to-equity ratio of 144 percent (maximum: 189 percent), well below the policy threshold. Debt ratios used for treatment conditioning are raw (unwinsorized) to avoid mechanical misassignment at the 200 percent cutoff; outcome variables remain winsorized at the 1st and 99th percentiles.

lines.[15] Seven firms in our sample were direct participants.[16] These firms were simultaneously subject to the general reform package *and* targeted industrial restructuring, making their treatment qualitatively different. We re-estimate the baseline excluding Big Deal firms ($Reform_f^{B,-\text{B5}}$) and estimate a separate Big Deal effect with staggered treatment onset to test for dose-response within the most intensely treated groups.

Summary: Table 3 summarizes the treatment hierarchy.

Table 3: Treatment Variable Hierarchy

	Definition	N_{treated}	N_{control}	Role
Panel A: Bracketing specifications				
$Reform_f^B$	Top 30 affiliates	66	388	Baseline
$Reform_f^S$	Top 30, $\text{debt}_{1997} > 200\%$	56	388	Sensitivity
$Reform_f^R$	Top 30, $\text{debt}_{1996} > 200\%$	50	388	Robustness
Panel B: Treatment intensity				
$Top5_f$	KFTC ranks 1–5 affiliates	31	388	Intensity gradient
Ranks 6–30_f	$Reform^B \setminus Top5$	35	388	Intensity gradient
Panel C: Mechanism				
$BigFive_f^{\text{Firm}}$	Direct swap participants	7	388	Dose-response
$Reform_f^{B,-\text{B5}}$	Baseline excl. Big Deal	59	385	Exclusion robustness

Notes: Firm counts as of 1998 in the estimation sample. Treatment assignment is time-invariant and determined by pre-reform group membership. top 5 groups: Hyundai (#1), Samsung (#2), Daewoo (#3), LG (#4), SK (#5). Ranks 6–30 includes Hanjin (#6), Ssangyong (#7), Hanwha (#8), Kumho (#9), Lotte (#11), and 13 additional groups. Of the 7 Big Deal participants, 4 belong to top 5 groups and 3 to partner groups (Hanwha, Hanjin). Two Big Deal firms are excluded from the sample due to post-1996 DART entry (see text). $Reform^S$ and $Reform^R$ use raw (unwinsorized) debt ratios for treatment conditioning; outcome variables are winsorized at the 1 and 99 percentiles. N_{control} is common across all specifications: non-top 30 firms with at least one pre-crisis observation and observed in 1998.

4.3 Firm Characteristics Variables

Control variables include log total assets and log total employment to account for firm-size heterogeneity, and the Herfindahl-Hirschman Index (HHI) based on KOSPI-listed

[15]Of the eight Big Deals, two failed outright (automobiles and electronics) and one partially failed (petrochemicals). The remaining five were implemented between 1999 and 2001.

[16]Kia (acquired by Hyundai Motor, 1998), SK Hynix (semiconductors), Samsung Heavy Industries and Korea Shipbuilding & Offshore Engineering (power generation), Hanwha Aerospace and Hanwha Solutions (aerospace/oil refining), and Hanjin Heavy Industries Holdings (railway vehicles). Two additional participants—Hyundai Doosan Infracore and Daewoo Shipbuilding—entered DART after 1996 and are excluded.

firms' sales to measure industry-level concentration. Summary statistics are presented in the Supplemental Appendix.

5 Empirical Framework and Methodology

5.1 Baseline Model: Generalized Synthetic Control (GSC) Method

Our baseline estimator is the generalized synthetic control (GSC) method of Xu (2017), which accommodates multiple treated units and relaxes the parallel trends assumption by modeling latent time-varying confounders. This is particularly important in our setting, where the reforms were deterministically targeted at the top 30 *chaebols* and the concurrent financial crisis likely had heterogeneous effects across firms, violating the parallel trends required for standard DiD.

The GSC framework models outcomes as:

$$Y_{ft} = \delta_{ft} Reform_{ft} + X'_{ft}\beta + \mathcal{L}_{ft} + \epsilon_{ft}, \tag{5.1}$$

where δ_{ft} is the heterogeneous treatment effect, X_{ft} includes observed covariates (log total assets and employment), and $\mathcal{L}_{ft}$ is a time-varying latent factor structure capturing unobserved heterogeneity. Identification requires strict exogeneity: $\epsilon_{ft} \perp\!\!\!\perp Reform_{gs}, X_{gs}, \Lambda, F \;\; \forall f, g, t, s$, where $\mathcal{L}$ is decomposed into unit-specific loadings Λ and common factors F.

The estimand is the average treatment effect on the treated:

$$\widehat{ATT}_{t,t>T_0} := \frac{1}{N_{tr}} \sum_{f \in \mathcal{T}} \left[Y_{ft}(1) - \hat{Y}_{ft}(0) \right], \tag{5.2}$$

where $\hat{Y}_{ft}(0)$ is the counterfactual outcome constructed from untreated units' factor structure.

We estimate counterfactual outcomes using the matrix completion (MC) approach of Athey et al. (2021), selected via cross-validation, which views the problem as missing data imputation with nuclear norm regularization to control rank.[17] Implementation uses the R

[17]The MC estimator solves: $\arg\min_{\mathcal{L}}\{|\Omega|^{-1} \sum_{(f,t)\in\Omega}(Y_{ft} - X'_{ft}\beta - \mathcal{L}_{ft})^2 + \lambda\|\mathcal{L}\|_*\}$, where $\|\mathcal{L}\|_*$ is the nuclear norm and λ the regularization parameter selected by cross-validation.

package `fect` (Liu et al., 2024) with bootstrapped confidence intervals.

5.2 Two-Way Fixed Effects DiD Benchmark

To benchmark and validate our findings, we additionally present results from a traditional two-way fixed effects DiD model, which accounts for both firm and time variations. To address the identification challenges, the model includes the same covariates used in the GSC model so that they may mitigate the bias introduced by non-random treatment assignment.

The baseline model is:

$$\begin{aligned} Y_{ft} = \alpha &+ \delta_1 \left(Reform_f \times \text{ImmediatePost}_t \right) \\ &+ \delta_2 \left(Reform_f \times \text{LongTermPost}_t \right) + X'_{ft}\beta + \gamma_f + \lambda_t + \epsilon_{ft} \end{aligned} \tag{5.3}$$

where $Reform_f$ is a binary indicator representing whether a firm f is in the treated group. ImmediatePost_t and LongTermPost_t are indicators for the immediate (1998-2000) and long-term (2001-2011) post-treatment periods, respectively. X_{ft} is time-varying firm characteristics, and γ_f and λ_t are firm and year fixed effects respectively, controlling for time-invariant firm characteristics and common shocks across all firms within a given year. ϵ_{ft} is the error term, clustered at the industry level to account for within-firm correlation over time.

This model captures the causal effect of the treatment by exploiting the variation in labour share across firms and over years. We also want to isolate the treatment effect from other confounding factors by adding relevant covariates as well as potential confounding factors related to various firm characteristics. A simple DiD method employs the interaction terms between the treatment dummy and the post-treatment period dummy to estimate the differential impact of the treatment during these periods.

The estimation process relies on the assumption that, in the absence of the treatment, the trends of key dependent variables would have followed a parallel path across treated and control firms. However, if the treatment is correlated with other unobserved factors that affect outcome variables, the parallel trends assumption (PTA) may be violated, causing the estimates to be biased. All the standard errors will be clustered at the industry level to ensure that our identification strategy will remain valid and that the estimated treatment effects accurately measure the causal impact of the treatment on the dependent variables.

6 Results and Discussion

6.1 Main Estimation Results

Figure 3 illustrates the GSC-estimated ATTs for six outcomes, with point estimates and bootstrapped standard errors reported in the Supplemental Appendix.

Labour share (Figure 3(a)) declines immediately following the reforms—approximately 2 pp at impact, deepening to 3–5 pp in years 1–2. A partial rebound in years 3–4 likely reflects temporary post-crisis stabilisation policies, but from year 5 onward the decline resumes, reaching approximately 9 pp at year 7. Long-run ATTs (years 8–14) fluctuate between 4 and 8 pp below counterfactual levels, consistent with a structural reallocation process in which competitive pressure continued to erode labour's claim on value added.[18]

Capital share (Figure 3(b)) follows a qualitatively similar pattern—modest early declines deepening to 10–14 pp by sample end. This parallel decline in both factor shares departs from classical capital-deepening expectations (van Vlokhoven, 2024) and reflects the gradual divestiture and capacity rationalisation of chaebol restructuring.

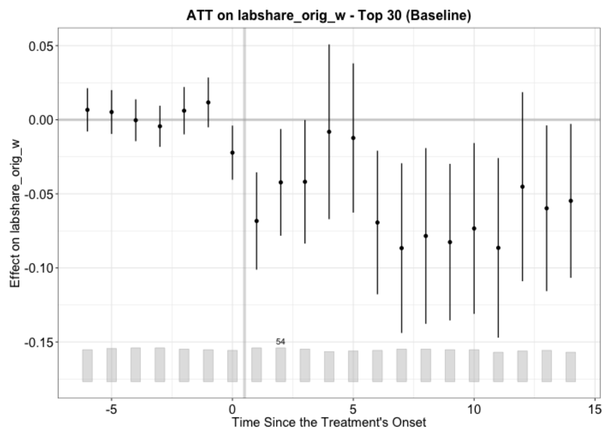
Profit share (Figure 3(c)) rises immediately to absorb the joint decline in labour and capital shares: approximately 5 pp at impact, growing to 13–19 pp by years 6–14. A growing portion of value added is captured by residual claimants rather than factor providers, echoing Barkai (2020) and van Vlokhoven (2024).

Markups (Figure 3(d)) increase among treated *chaebols*, with ATTs of 0.04–0.22 in periods 0–2 and 0.25–0.51 from year 6 onward. Intervening years show smaller estimates, suggesting initial gains were partially competed away before re-emerging as the most productive treated firms consolidated market positions. This finding qualifies the view that pro-competitive reforms should reduce markups: while market concentration declined (Figure 4), surviving dominant *chaebols* increased their pricing power.

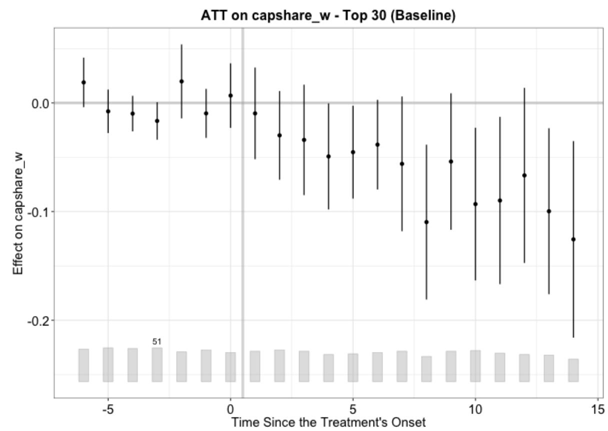
Labour productivity (Figure 3(e)) rises by approximately 0.10–0.44 log points,

[18] While the GSC estimator controls for unobserved time-varying confounders, group-specific policy interventions may not be fully absorbed by the interactive fixed effects (Bai, 2009, pp. 1237–1238).

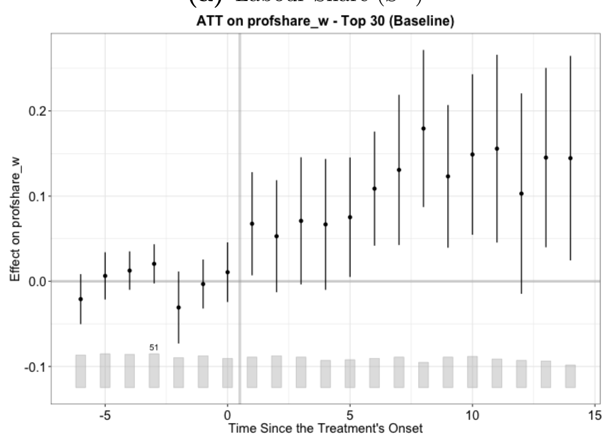
indicating resource reallocation and capital-intensive technology adoption. Critically, this productivity growth did not translate into higher labour shares, supporting a decoupling of productivity from compensation. The **productivity–wage gap** (Figure 3(f)) widens correspondingly, with later-period estimates exceeding 20–40 pp—consistent with broader evidence on productivity–compensation divergence (Stansbury and Summers, 2020; Autor et al., 2020).



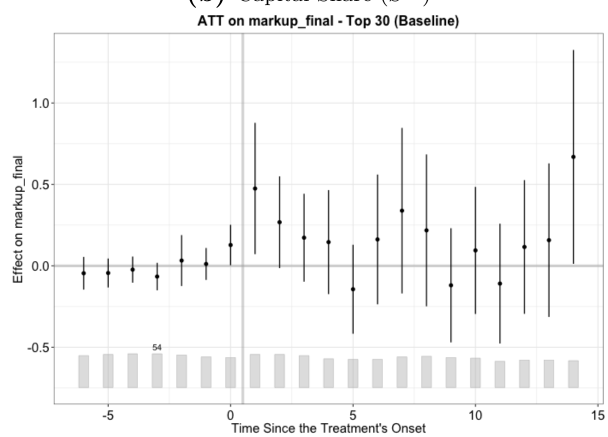
(a) Labour Share (S^L)



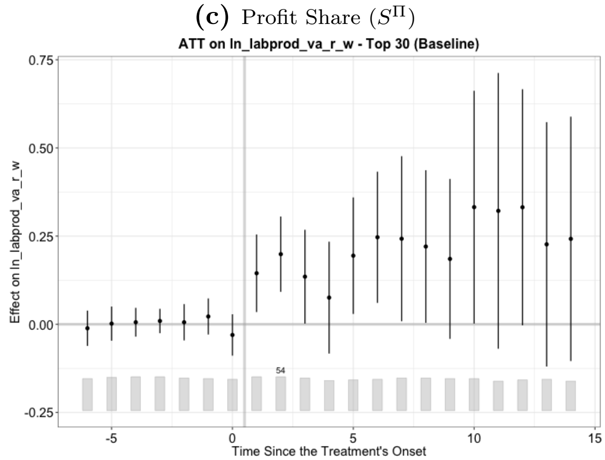
(b) Capital Share (S^K)



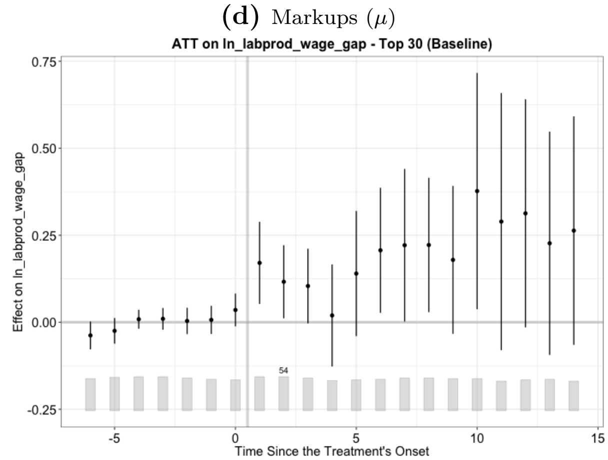
(c) Profit Share (S^Π)



(d) Markups (μ)



(e) Log Labour Productivity (ℓ^p)



(f) Labour Productivity-Wage Gap ($\ell^p - w$)

Figure 3: Estimated ATTs with the Baseline Treatment Group

Note: To obtain the uncertainty estimates for ATTs, we run nonparametric bootstrapping with 500 replications, which result in point estimates and 95% confidence intervals. The x-axis represents the time relative to the treatment, which aligns all firms based on their respective treatment times and creates a common time frame where the vertical line (grey coloured) represents the time of treatment. The pre- and post-treatment periods are standardised across all firms, facilitating the analysis of the treatment effect dynamics over a common time frame. The y-axis displays the point estimates of ATTs with confidence intervals. The height of the bars in each period shows the number of firm observations available for that period. The models were run after winsorising top and bottom 1% of each dependent and independent variable every year.

Meanwhile, although market concentration declined modestly in *chaebol*-dominated sectors (Figure 4), treated industries remained highly concentrated throughout the post-reform period (HHI consistently above 0.25). The coexistence of reduced industry-level concentration with rising firm-level markups among treated firms suggests that the reforms altered the distribution of market shares—benefiting the most productive incumbents—without dismantling the pricing power of dominant firms. This pattern is consistent with the superstar firm dynamic described by Autor et al. (2020): tougher competition reallocates market share toward the most efficient firms, which then command higher markups and lower labour shares.

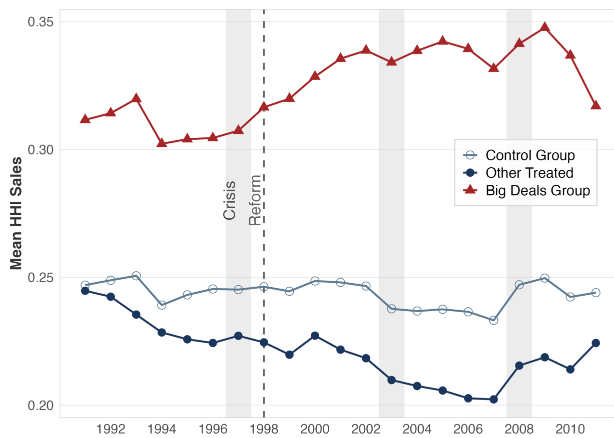


Figure 4: HHI Trends by Group

6.2 TWFE Validation and Estimator Selection

The GSC estimates reported above rely on interactive fixed effects to construct unit-specific counterfactuals. To assess the robustness of these findings, we estimate a TWFE event-study specification with firm and year fixed effects:

$$Y_{it} = \alpha_i + \delta_t + \sum_{s \neq 1996} \beta_s \left(D_i \times \mathbf{1}[t = s]\right) + \mathbf{X}_i' \boldsymbol{\gamma}_t + \varphi \, \text{Export}_{it} + \varepsilon_{it}, \tag{6.1}$$

where α_i and δ_t are firm and year fixed effects, D_i indicates Top 30 chaebol affiliation, and 1996 is the omitted reference year. Pre-treatment coefficients $\{\beta_s\}_{s<1997}$ serve as a direct test of the parallel trends assumption: if $\beta_s \approx 0$ for all pre-treatment years, the TWFE estimand is interpretable as the average treatment effect on the treated. Standard errors are clustered at the KSIC 2-digit industry level throughout.[19]

Figure 5 reports the event-study estimates for the four factor share outcomes and markups. The results reveal a sharp diagnostic contrast across outcomes.

Capital Share and Markups: Parallel Trends Validated

For the capital share (Figure 5(b)), pre-treatment coefficients are centred at zero throughout 1991–1996, confirming that treated and control firms followed parallel capital share trajectories before reform. Post-treatment, the capital share declines monotonically, reaching approximately -0.10 by 2004 and stabilising thereafter. This is the most robust finding in the paper: both GSC and TWFE agree in sign, magnitude, and statistical significance, and the identifying assumption is verified by the event-study diagnostic.

For markups (Figure 5(d)), pre-treatment coefficients similarly cluster at zero, with a sharp increase at 1998 that attenuates by 2005, confirming the transitory nature of the consolidation-driven market power increase. In both cases, the TWFE event study corroborates the GSC dynamic ATT estimates (Supplemental Appendix).

Profit Share: Mild Pre-Trend Drift

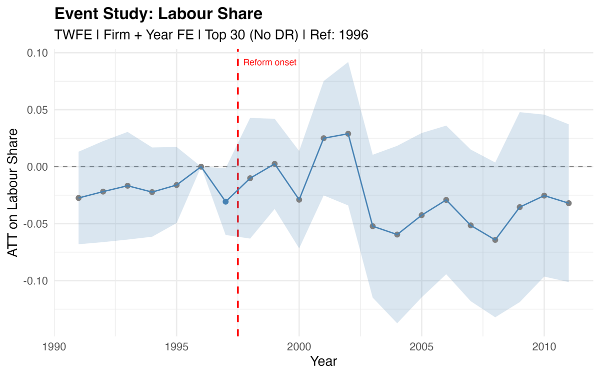
For the profit share (Figure 5(c)), pre-treatment coefficients exhibit a mild positive drift of approximately 0.03 in 1991–1993 before converging to zero by 1996. This drift is the mechanical mirror of the labour share pre-trend through the accounting identity $S^L + S^K + S^\Pi = 1$: if the labour share exhibits a negative pre-trend, the profit share must absorb part of that deviation. The post-treatment break is large relative to the pre-trend magnitude (+0.10 to +0.18 versus +0.03), and the TWFE two-period estimates remain significant ($+0.055^{**}$, $+0.112^{***}$), indicating that the pre-trend does not account for the observed effect.

[19] Following Abadie et al. (2022), we also report results with firm-level clustering (approximately 450 clusters versus 25 at the industry level), which aligns with the level of treatment assignment. All results that are significant under KSIC 2-digit clustering remain significant under firm-level clustering; the converse does not hold for the labour share, where inference is sensitive to clustering level—consistent with the pre-trend diagnostic discussed below (the Supplemental Appendix).

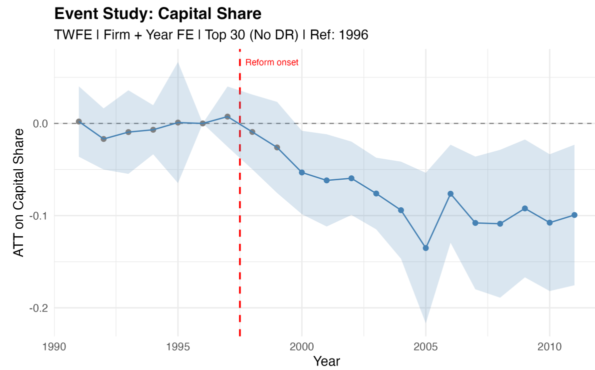
Labour Share: Parallel Trends Failure Motivates GSC

For the labour share (Figure 5(a)), pre-treatment coefficients exhibit a systematic negative drift of approximately -0.025 relative to the 1996 reference year, indicating that chaebol affiliates' labour shares were on a different trajectory than control firms prior to reform. This violation of the parallel trends assumption renders the TWFE estimand unreliable for this outcome: the standard TWFE event-study coefficients are imprecise and centred near zero post-treatment—not because the reform had no effect on labour's share, but because the estimator cannot separate the treatment effect from the pre-existing differential trend.

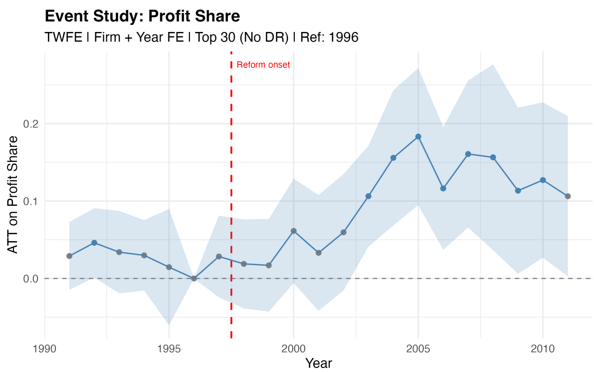
This diagnostic motivates our reliance on the GSC estimator for the labour share. Unlike TWFE, the GSC method constructs unit-specific counterfactuals via interactive fixed effects, accommodating heterogeneous trends across units without requiring parallel trends in levels. Figure 6 reports the GSC pre-treatment diagnostics for the labour share, which stand in sharp contrast to the TWFE event study. The pre-treatment residuals are tightly centred at zero within the equivalence bounds (F-test p-value: 0.084; equivalence test p-value: 0.002), and the placebo test fails to reject the null of zero pre-treatment effects ($p = 0.377$). The equivalence test is the more relevant statistic: it rejects the null that pre-treatment effects are *meaningfully different from zero*, confirming that the interactive fixed effects successfully absorb the differential dynamics that confound TWFE (?). Having passed these diagnostics, the GSC dynamic ATTs identify a persistent labour share decline of 2–9 percentage points over the decade following reform (Supplemental Appendix).



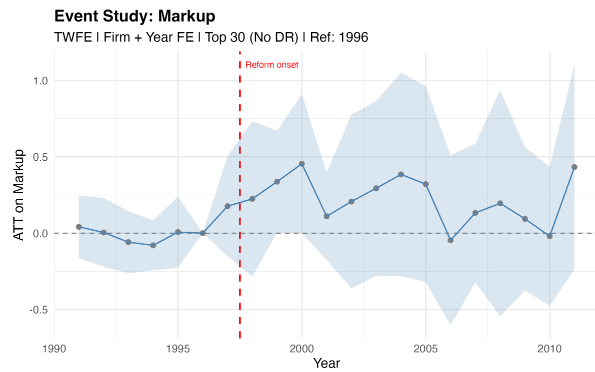
(a) Labour Share (S^L)



(b) Capital Share (S^K)



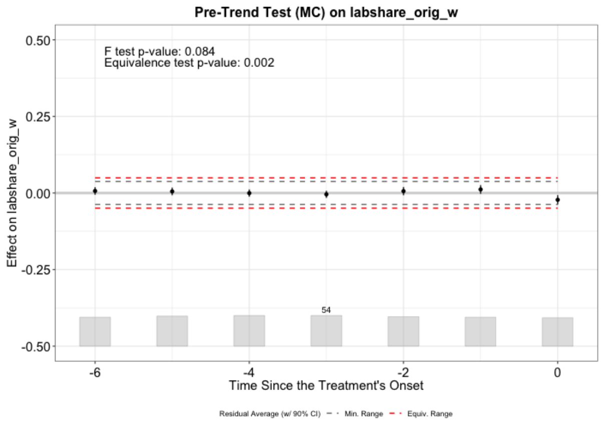
(c) Profit Share (S^Π)



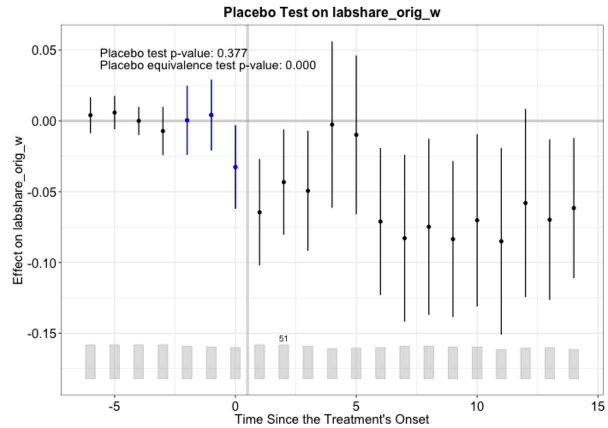
(d) Markups (μ)

Figure 5: TWFE Event-Study Estimates (Firm + Year FE, Ref: 1996)

Note: Each panel reports the TWFE event-study coefficients $\hat{\beta}_s$ from Equation (6.1), with 1996 as the omitted reference year. Pre-treatment coefficients test the parallel trends assumption; post-treatment coefficients estimate the dynamic ATT, directly comparable to the GSC estimates in Figure 3. Shaded bands indicate 95% confidence intervals based on standard errors clustered at the KSIC 2-digit level. The dashed red line marks the reform onset (1997/98).



(a) Pre-Trend Test (MC)



(b) Placebo Test

Figure 6: GSC Pre-Treatment Diagnostics for Labour Share (S^L)

Note: Panel (a) reports the pre-treatment residual averages from the matrix completion estimator with 90% confidence intervals. The dashed red lines indicate the equivalence range. The F-test ($p = 0.084$) fails to reject the null of zero pre-treatment effects at the 5% level; the equivalence test ($p = 0.002$) strongly rejects the null that pre-treatment effects are large. Panel (b) reports the placebo test, in which the treatment is artificially advanced by one period. The placebo test p-value of 0.377 fails to reject the null; the placebo equivalence test p-value of 0.000 confirms that pre-treatment effects are meaningfully small.

Evidence Hierarchy

Table 4 summarises the cross-method comparison. The capital share decline is the most robust finding, identified under both TWFE and GSC with consistent magnitudes. The labour share decline is credibly identified by GSC, with the event-study diagnostic providing a transparent explanation for why TWFE cannot detect it. The cross-method agreement on outcomes where TWFE assumptions hold strengthens the credibility of the GSC estimates on the outcome where they do not.

Table 4: Evidence Hierarchy: Estimator Validity by Outcome

Outcome	Parallel Trends	TWFE Result	GSC Result	Primary Estimator
Capital share (S^K)	Valid	-0.044^{***} / -0.092^{***}	-0.045^{**} to -0.110^{***}	Both
Markup (μ)	Valid	$+0.309^{**}$ / $+0.177$	$+0.128^{**}$ to $+0.475^{**}$	Both
Profit share (S^{Π})	Mild drift[a]	$+0.055^{**}$ / $+0.112^{***}$	$+0.011$ to $+0.179^{***}$	Both
Labour share (S^L)	Fails	-0.013 / -0.022	-0.022^{**} to -0.087^{***}	GSC only

[a] Mechanical mirror of S^L pre-trend via the accounting identity $S^L + S^K + S^{\Pi} = 1$.
TWFE reports Immediate / Long-Run post-reform coefficients from the two-period specification.
GSC reports the range of dynamic ATT estimates over periods 0–10.

Three additional pieces of evidence support the labour share finding despite the TWFE insignificance. First, the accounting identity provides an internal consistency check: since S^K declines significantly by approximately 9 percentage points and S^{Π} rises significantly by approximately 11 percentage points under TWFE, the residual S^L must decline by approximately 2 percentage points—precisely the magnitude estimated by GSC at the early post-treatment horizon. Second, the dose-response gradient is consistent across methods: the subgroup analysis in Section 6.3 shows that Big 5 chaebols—the most directly reformed firms—exhibit larger labour share point estimates than ranks 6–30, with the difference driven by the same capital rationalisation channel that produces the significant S^K result. Third, the TWFE pre-trend failure for labour's share has a natural economic interpretation: prior to reform, Korea's labour market was undergoing structural transformation with differential wage growth across firm types, particularly as chaebol affiliates expanded employment in the early 1990s at rates that diverged from smaller firms. This pre-existing divergence in labour compensation dynamics is precisely the type of heterogeneous trend that the GSC method is designed to accommodate.

These results raise a natural identification concern. The 1998 reforms bundled pro-competitive deregulation with the state-directed *Big Deal* restructuring and labour market liberalization. Whether the observed labour share decline reflects competitive pressure, forced mergers, or weakened bargaining cannot be determined from the aggregate ATT alone. The following subsections address each source of bundling.

6.3 Separating the Big Deal Channel

The *Big Deal* program directed large-scale mergers among the five largest *chaebol* groups, likely having independent effects on factor shares through capital concentration, layoffs, and reinforced monopolistic structures. This subsection isolates the Big Deal component from the pro-competitive reform effect.

Identification Strategy

We adopt a bracketing approach. Re-estimating the GSC model excluding all Big Five affiliates isolates the combined effect of competition reform and labour liberalization, since these firms are precisely those subjected to the Big Deals:

$$\underbrace{\widehat{ATT}_{\text{full}}}_{\text{Competition + Labour + Big Deals}} = \underbrace{\widehat{ATT}_{\text{excl. Top 5}}}_{\text{Competition + Labour}} + \underbrace{\left(\widehat{ATT}_{\text{full}} - \widehat{ATT}_{\text{excl. Top 5}}\right)}_{\text{Big Deal component}} \tag{6.2}$$

This requires no additional assumptions beyond those underlying the GSC estimator, exploiting the fact that Big Deal exposure is deterministic and perfectly observed.

Results

The GSC estimates for the restricted sample excluding Big Five affiliates confirm that the labour share decline persists.[20] At period 6, the labour share ATT is −4.9 percentage points ($p < 0.05$), and point estimates remain negative through the end of the sample. While this is smaller in absolute magnitude than the full-sample estimate—which reaches 6–9 percentage points at the same horizon—the sign and significance confirm that the labour share decline is not purely an artefact of the Big Deal restructuring. Table 5 summarises the bracketing decomposition across all six outcomes at two horizons.

Several patterns emerge. First, and most importantly, the labour share decline survives the exclusion of the Big Five in both panels. At period 6, the excluding-top-5 ATT is −4.9 pp ($p < 0.05$), compared with −6.3 pp ($p < 0.01$) for the full sample. Over the longer window (periods 6–11), the excluding-top-5 average is −4.1 pp, roughly half the full-sample average of −8.0 pp. The Big Deal component accounts for approximately 22% of the medium-term effect and 49% of the long-run effect, confirming that state-directed restructuring amplified but did not create the labour share decline.

Second, the profit share and markup effects also persist after excluding the Big Five. At period 6, the profit share ATT is +9.5 pp ($p < 0.01$) for the excluding-top-5 sample,

[20]The excluding-top-5 specification uses the same GSC model, covariates, and cross-validation procedure as the baseline. The only difference is the exclusion of firms affiliated with the five largest *chaebols* (Hyundai, Samsung, Daewoo, LG, and SK) from the treated group. The full period-by-period ATT estimates are reported in the Supplemental Appendix in the Appendix.

Table 5: Bracketing Decomposition: Full Sample vs. Excluding Big Five

	Full Sample $\widehat{ATT}_{\text{full}}$	Excl. Top 5 $\widehat{ATT}_{\text{excl. 5}}$	Big Deal Component	% Big Deal
Panel A: Medium-Term (Period 6)				
Labour Share (S^L)	-0.063^{***}	-0.049^{**}	-0.014	22.2
Labour Productivity (ℓ^p)	$+0.289^{***}$	$+0.257^{**}$	$+0.032$	11.1
Capital Share (S^K)	-0.077^{***}	-0.064^{**}	-0.013	16.9
Profit Share (S^Π)	$+0.127^{***}$	$+0.095^{***}$	$+0.032$	25.2
Prod.-Wage Gap ($\ell^p - w$)	$+0.251^{***}$	$+0.262^{***}$	-0.011	—
Markups (μ)	$+0.316^{***}$	$+0.293^{**}$	$+0.023$	7.3
Panel B: Long-Run Average (Periods 6–11)				
Labour Share (S^L)	-0.080	-0.041	-0.039	48.9
Labour Productivity (ℓ^p)	$+0.278$	$+0.199$	$+0.080$	28.6
Capital Share (S^K)	-0.075	-0.065	-0.010	13.1
Profit Share (S^Π)	$+0.142$	$+0.092$	$+0.050$	35.5
Prod.-Wage Gap ($\ell^p - w$)	$+0.271$	$+0.223$	$+0.048$	17.6
Markups (μ)	$+0.442$	$+0.243$	$+0.200$	45.1

Notes: Full Sample reports the ATT from the baseline GSC model (tr30gsc0: top 30 *chaebols*, 1997 debt-ratio condition). Excl. Top 5 reports the ATT from the GSC model excluding Big Five *chaebol* affiliates (tr25gsc0). The Big Deal Component is the difference ($\widehat{ATT}_{\text{full}} - \widehat{ATT}_{\text{excl. 5}}$); % Big Deal expresses this as a share of the full-sample ATT. Panel A reports period-specific estimates at period 6 (representative medium-term horizon; significance from bootstrapped SEs). Panel B reports the mean ATT across periods 6–11 (sustained long-run window; significance stars omitted for averages). "—" indicates the Big Deal component has the opposite sign to the full-sample ATT. All GSC models use the matrix completion estimator with the same covariates and 500 bootstrap replications. $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

compared with +12.7 pp in the full sample. Markups increase by 0.29 ($p < 0.05$) without the Big Five, versus 0.32 in the full sample. This indicates that the rising market power documented in Section 6.1 was not confined to the Big Deal firms.

Third, the Big Deal component grows over time. In Panel A (period 6), the Big Deal contribution to the labour share decline is modest (−1.4 pp, or 22% of the total). By Panel B (periods 6–11 average), it has grown to −3.9 pp (49%). This temporal pattern is consistent with the forced mergers taking time to generate their full effects on market structure and factor shares—the initial consolidation of assets was followed by a gradual strengthening of market power among the restructured groups.

Fourth, the productivity-wage gap is an informative exception: at period 6, the excluding-top-5 estimate (+26.2 pp) actually *exceeds* the full-sample estimate (+25.1 pp), yielding a negative Big Deal component. This suggests that the Big Deal firms did not experience a wider productivity-wage divergence than other treated *chaebols* at the medium-term horizon—the gap widened broadly across all treated firms, not exclusively among the restructured groups.

Supplementary Evidence: Subgroup Interaction Model

As a complementary check, we estimate a DiD model with firm and year fixed effects that allows the treatment effect to differ between Big 5 and ranks 6–30 treated firms through explicit interaction terms.[21]

The results confirm the dose-response gradient expected from the bracketing exercise. For the labour share, the Big 5 long-run coefficient (−0.052) is an order of magnitude larger than the ranks 6–30 coefficient (−0.004). The same ordering holds across outcomes: Big 5 capital share effects are approximately 60% larger (-0.119^{***} versus -0.073^{**}), and Big 5 profit share effects are approximately 85% larger ($+0.154^{***}$ versus $+0.083^{*}$) in the long-run period. Markups are significant only for the Big 5 ($+0.441^{*}$), consistent with the Big Deal mergers generating lasting market power among the restructured groups.

[21] The subgroup interaction model replaces the single treatment indicator with two binary group indicators—ranks 6–30 and Big 5—each interacted with the immediate and long-term post-reform period dummies. Pre-reform firm size (log assets, log employment) is interacted with post-period indicators to control for differential size-driven trends, following Abadie et al. (2022). We test for dose-response by a Wald test of equal coefficients across subgroups within each period. The full specification and results are reported in the Supplemental Appendix (Table A.13).

Two additional features merit attention. First, the ranks 6–30 coefficients are significant for the capital share (-0.040^{***} immediate, -0.073^{**} long-run) and profit share ($+0.044^{*}$ immediate, $+0.083^{*}$ long-run), confirming that the reform effects extend beyond the Big Deal firms—consistent with the GSC bracketing exercise. Second, the dose-response pattern is economically coherent: the Big 5, which faced the most intense restructuring, show the largest effects, while the broader group shows smaller but directionally consistent effects driven by the competition and labour liberalization channels.

Interpretation

These results establish that the aggregate labour share decline documented in Section 6.1 reflects two distinct forces. The Big Deal restructuring program, which affected only the largest five *chaebol* groups, accounts for a substantial share of the total effect—consistent with the fact that forced mergers and asset consolidation mechanically increase capital concentration and reduce labour's bargaining position. However, the decline also extends to the broader set of treated *chaebols* that were not subject to the Big Deals, indicating that pro-competitive reform and labour market liberalization exerted independent downward pressure on the labour share.

Having separated the Big Deal channel, a natural question remains: among the non-Big-Deal treated firms, is the labour share decline driven primarily by the competition reform or by labour market liberalization? These two policy changes hit all top 30 *chaebols* simultaneously, including those outside the Big Deal program, and cannot be separated by sample exclusion alone. The next subsection addresses this remaining source of bundling through a heterogeneous treatment effect approach.

6.4 Transmission Mechanisms

The main results establish that the pro-competitive reforms led to a substantial and persistent decline in the labour share among treated *chaebols*. This subsection investigates the transmission channels through which this decline materialised. We proceed in three steps. First, we test whether capital deepening—operating through the elasticity of substitution between capital and labour—can account for the observed decline (Section 6.4.1). Second, we decompose the aggregate labour share change into within-firm and between-firm components using the Olley and Pakes (1996) shift–share methodology, which identifies the *margin* along which the decline occurred (Section 6.4.2). Third, we extend the decomposition to markups and profit shares, showing that the within-firm channel that drives the labour share decline is mirrored by within-firm increases in markups and profitability—a pattern consistent with rising market power as the primary mechanism (Section 6.4.3).

6.4.1 Capital Deepening and the Elasticity of Substitution

Whether capital deepening can account for the declining labour share depends on the magnitude of σ. Under a CES production function, rising capital intensity (K/L) reduces labour share only if $\sigma > 1$; when capital and labour are gross complements $(\sigma < 1)$, capital deepening raises labour share instead. Cross-country estimates of σ remain contested. Karabarbounis and Neiman (2013) estimate $\hat{\sigma} \approx 1.26$ and attribute the global labour share decline to declining investment prices. However, Glover and Short (2020) show that omitting consumption growth from the rental rate proxy biases σ upward; correcting for this yields estimates consistent with unity. A comprehensive meta-analysis by Gechert et al. (2022) covering 3,186 estimates from 121 studies finds a best-practice estimate of approximately 0.3 after correcting for publication bias, aggregation bias, and omitted capital-cost information—well below the values needed for the capital-deepening channel to operate.

To estimate the elasticity based on our listed firm sample data, we consider a standard CES production function with two factors of capital (K) and labour (L), and factor-augmenting technology:

$$VA_{ft} = \left[\alpha(A_t^K K_t)^{\frac{\sigma-1}{\sigma}} + (1-\alpha)(A_t^L L_t)^{\frac{\sigma-1}{\sigma}}\right]^{\frac{\sigma}{\sigma-1}}, \tag{6.3}$$

where A_t^K and A_t^L are factor-augmenting productivity terms. The standard approach regresses the log capital–labour ratio on the log wage–rental ratio in the reduced form:[22]

[22] For a detailed derivation, see Supplemental Appendix B.

$$\ln\left(\frac{K_{ft}}{L_{ft}}\right) = \alpha_0 + \sigma \ln\left(\frac{w_t}{r_t}\right) + \varepsilon_{ft} \tag{6.4}$$

where $\alpha_0 = \sigma \ln \frac{\alpha}{1-\alpha} + (\sigma - 1) \ln \frac{A_t^K}{A_t^L}$ and the wage–rental ratio w_t/r_t is constructed at the industry–year level.

The Supplemental Appendix (Figure A.8) presents the reduced-form relationship between $\ln(K/L)$ and $\ln(w/r)$, estimated separately for our three firm groups by pooling observations across time.[23] The OLS slopes suggest heterogeneity: the Big Deal group exhibits the lowest slope ($\hat{\sigma} \approx 0.79$), suggesting limited substitutability between capital and labour or complementarity. The control group shows a slightly higher value ($\hat{\sigma} \approx 0.85$), while the non–Big Deal treated group exhibits a value near unity ($\hat{\sigma} \approx 1.05$).

However, the reduced-form regression faces well-documented identification problems with firm-level panel data. The wage–rental ratio is constructed at the industry–year level—the finest level at which factor prices can be credibly measured.[24] Including firm fixed effects absorbs nearly all identifying variation, producing severe attenuation bias: the pooled OLS estimate of $\hat{\sigma} = 0.28$ collapses to 0.03 with firm and year fixed effects (Panel A of Table 6). Additionally, as Glover and Short (2020) and Gechert et al. (2022) document, omitting the first-order condition for capital from the specification biases σ upward.

The First-Order Condition Approach: To address these identification challenges, we follow the cost-share approach developed by Oberfield and Raval (2021) and Raval (2019). The key insight of their methodology is to exploit information embedded in firm-level factor *payments* rather than industry-level factor *prices*, thereby circumventing the attenuation problem that plagues the standard reduced form.

The derivation begins from the same CES production function (Equation (6.3)) and the cost-minimization first-order conditions. At the optimum, each factor is paid its marginal revenue product:

[23] Since σ is a deep technology parameter that characterises the curvature of the isoquant, it should not vary with the timing of the reforms. The relevant variation is cross-sectional: different firm types may operate different production technologies. We therefore pool pre- and post-reform observations within each group.

[24] Firm-level wage bills and capital stocks reflect both prices and quantities; the industry–year construction aggregates across firms within a two-digit KSIC industry to approximate the factor price environment.

$$r_t = \lambda \cdot \alpha \left(\frac{VA}{K_t}\right)^{\frac{1}{\sigma}} (A_t^K)^{\frac{\sigma-1}{\sigma}} \tag{6.5}$$

$$w_t = \lambda \cdot (1-\alpha) \left(\frac{VA}{L_t}\right)^{\frac{1}{\sigma}} (A_t^L)^{\frac{\sigma-1}{\sigma}} \tag{6.6}$$

Multiplying each FOC by the respective factor quantity gives total factor payments. Taking the ratio of labour payments to capital payments and simplifying:

$$\frac{w_t L_t}{r_t K_t} = \frac{1-\alpha}{\alpha} \left(\frac{A_t^L}{A_t^K}\right)^{\frac{\sigma-1}{\sigma}} \left(\frac{K_t}{L_t}\right)^{\frac{1}{\sigma}} \tag{6.7}$$

Taking natural logarithms and rearranging yields the estimating equation:

$$\ln\left(\frac{w_t L_t}{r_t K_t}\right) = c - \frac{1}{\sigma} \ln\left(\frac{K_t}{L_t}\right) + \varepsilon_{ft} \tag{6.8}$$

where $c \equiv \ln \frac{1-\alpha}{\alpha} + \frac{\sigma-1}{\sigma} \ln \frac{A_t^L}{A_t^K}$, the coefficient on $\ln(K/L)$ is $\beta = -1/\sigma$, and the elasticity of substitution is recovered as $\sigma = -1/\hat{\beta}$.[25]

Identification advantages: The FOC specification resolves the reduced-form identification problems because both sides use firm-level variables (exhibiting within-firm variation even with fixed effects), the specification exploits the capital FOC directly (Gechert et al., 2022), and it sidesteps the omitted variable problem of Glover and Short (2020) by working with factor payments rather than prices.

Econometric implementation: We estimate Equation (6.8) by OLS with two-way fixed effects:

$$\ln\left(\frac{w_{it} L_{it}}{r_{it} K_{it}}\right) = \alpha_i + \gamma_t + \beta \cdot \ln\left(\frac{K_{it}}{L_{it}}\right) + \varepsilon_{it} \tag{6.9}$$

where α_i absorbs time-invariant heterogeneity in the distribution parameter and the level of factor-augmenting technology, and γ_t captures aggregate shocks common to all firms. Standard errors are clustered at the two-digit industry level (KSIC), and the elasticity is recovered as $\hat{\sigma} = -1/\hat{\beta}$ with delta method standard errors. Our implementation follows

[25] Equivalently, the specification can be written as a regression of the log labour-to-capital cost share on the log capital–labour ratio. The negative sign on β has a natural interpretation: firms with higher K/L devote a larger share of total factor payments to capital, so the *labour* cost share falls.

the "micro elasticity'," framework of Raval (2019) and Oberfield and Raval (2021); we note that our resulting estimate ($\hat{\sigma} \approx 1.0$) is somewhat higher than their 0.5–0.7 range for U.S. manufacturing, which likely reflects the composition of our sample of large Korean conglomerates operating in capital-intensive heavy industries.[26]

Table 6 reports the results. Panel A shows the standard reduced-form regressions: progressively richer fixed effects attenuate the estimate from 0.28 (pooled OLS) toward near zero (0.03 with firm and year FE), confirming the attenuation problem. Panel B presents our preferred FOC estimates: $\hat{\sigma} = 0.99$ (SE$= 0.02$), statistically indistinguishable from unity and consistent with the Cobb-Douglas benchmark. Panel C reports a DiD specification interacting the factor price ratio with *chaebol* status and the post-reform indicator; the triple interaction is small and statistically insignificant ($p > 0.3$), indicating that the reforms did not differentially alter σ for treated firms.

To examine temporal stability, we estimate the FOC specification in 5-year rolling windows (Supplemental Appendix, Figure A.9). The rolling estimates fluctuate closely around unity throughout, with no discrete break or persistent upward shift around the 1998 reforms. Even at the upper end of the rolling-window range ($\hat{\sigma} \approx 1.12$), the quantitative implications for factor shares remain limited: a 10% decline in the relative price of investment goods would shift the labour share by roughly 1 percentage point—far too small to account for the 7–31 percentage-point decline observed among treated *chaebols*.

Interim conclusion: The evidence from our preferred FOC estimate ($\hat{\sigma} \approx 1.0$), the insignificant DiD treatment effect, the smooth temporal convergence toward Cobb-Douglas, and the broader literature consensus that $\sigma \leq 1$ (Glover and Short, 2020; Gechert et al., 2022) all point in the same direction: *capital deepening cannot account for the observed decline in the labour share among treated firms.* This motivates a decomposition exercise that can identify whether the decline occurred within firms—consistent with a market-power mechanism—or through reallocation across firms.

[26] Labour cost is total employee compensation from financial statements, deflated to constant prices. For capital cost, we use two measures: observed capital expenditures and an imputed rental rate of 15% of tangible assets (Oberfield and Raval, 2021). A recent replication by Yang and Zhang (2024) using Chinese data finds an aggregate elasticity of 0.9–1.0, consistent with our estimate.

Table 6: Elasticity of Substitution Estimates

	(1)	(2)	(3)	(4)
Panel A: Reduced-Form Regressions $\ln(K/L) = \alpha_0 + \sigma \ln(w/r) + \varepsilon$				
Pooled OLS	0.283			
Industry + Year FE		0.028		
Firm + Year FE			0.026	
5-Year Long Diff				0.217
Panel B: FOC / Cost-Share Approach $\ln(wL/rK) = c - (1/\sigma) \ln(K/L) + \varepsilon$				
FE-consistent FOC	**0.983**		(0.010)	
Panel C: DiD (Chaebol $\times$ *Post* $\times$ $\ln(w/r)$*)*				
Triple interaction	-0.012		$[p = 0.34]$	
Benchmarks				
Cobb-Douglas	$\sigma = 1.00$			
Song (2021) Korea	$\sigma = 1.18$			
Oberfield & Raval (2021) U.S. mfg	$\sigma = 0.5$–0.7			
Glover & Short (2020) cross-country	$\sigma \leq 1$			
Gechert et al. (2022) meta-analysis	$\sigma \approx 0.3$ (best practice)			

Notes: Panel A: regressions of $\ln(K/L)$ on $\ln(w/r)$ with progressively richer fixed effects. Panel B: preferred FOC approach following Oberfield and Raval (2021); the specification regresses $\ln(wL/rK)$ on $\ln(K/L)$ with firm and year FE and recovers $\sigma = -1/\hat{\beta}$. Standard errors (parentheses) are clustered at the two-digit industry level and computed via the delta method. Panel C: DiD triple interaction testing whether the reforms differentially affected σ for *chaebols*.

6.4.2 Decomposing the Labour Share Decline: Within-Firm versus Between-Firm Channels

Having ruled out the capital-deepening channel, we ask whether the aggregate labour share decline reflects changes within individual firms (the within-firm channel) or reallocation toward firms with already-low labour shares (the between-firm channel). A within-firm-dominated decline is consistent with rising markups or shifts in bargaining; a between-firm decline would suggest Melitz-type selection (Melitz, 2003; Autor et al., 2020).

Olley–Pakes shift–share decomposition: We decompose the change in the aggregate (value-added-weighted) labour share using the dynamic extension of the Olley and Pakes (1996) decomposition, which partitions the change between any two periods into three additive components. Define the aggregate labour share for group g at time t as $\bar{S}_{gt} = \sum_{f \in g} \theta_{ft} s_{ft}$, where s_{ft} is firm f's labour share and $\theta_{ft} = VA_{ft} / \sum_{j \in g} VA_{jt}$ is its value-added weight. The change from period $t-1$ to t can be written as:

$$\underbrace{\Delta \bar{S}_{gt}}_{\text{Total change}} = \underbrace{\sum_i \theta_{i,t-1}\, \Delta s_{it}}_{\text{Within}} + \underbrace{\sum_i \Delta\theta_{it}\, s_{i,t-1}}_{\text{Between}} + \underbrace{\sum_i \Delta\theta_{it}\, \Delta s_{it}}_{\text{Cross}} \tag{6.10}$$

where Δ denotes the first difference and the summation runs over the set of firms observed in both periods (the `common'' or`continuing',' firm set). The *within* component captures changes in labour shares holding each firm's output weight fixed at its initial value—this is the intensive margin. The *between* component captures the reallocation of value-added shares across firms holding each firm's labour share fixed at its initial level—this is the extensive margin. The *cross* term captures the covariance between changes in output shares and changes in labour shares; it is negative when firms that are growing in relative size are simultaneously reducing their labour shares.

We compute this decomposition in two ways. First, we estimate it year-to-year for each consecutive pair of years in the sample (1991–2011), which provides a dynamic picture of how the three components evolve over time and across the pre- and post-reform periods. Second, we compute a cumulative two-period decomposition directly comparing 1996 (the last pre-reform year) to 2011 (the end of our sample), restricted to firms observed in both years.

This “long difference’,’ decomposition provides the cleanest summary of the total within- and between-firm contributions over the full post-reform window. Both decompositions are performed separately for the Top 30 *chaebols* and control firms, as well as for the three-group split (Control, Top 30 excluding Top 5, and Top 5 or Big Deal group).

Results: the within-firm channel dominates: Table 7 reports the cumulative two-period decomposition for 1996–2011. The results are striking in their clarity. Among the Top 30 *chaebols*, the aggregate labour share fell by 31.2 percentage points (from 47.5% to 16.3%). The vast majority of this decline—28.9 percentage points, or 93% of the total—is attributable to the within-firm component: individual *chaebol* firms reduced their labour shares while holding their relative output positions approximately constant. The between-firm component is near zero (+0.4 pp), indicating that reallocation played essentially no role. The cross term (−2.7 pp) is negative but quantitatively secondary.

The three-group decomposition reveals important heterogeneity. The Big Deal Top 5 experienced the largest aggregate decline (−33.9 pp, from 48.7% to 14.8%), of which 95.6% (−32.4 pp) is within-firm. The Top 30 excluding Big Deal Top 5 shows a smaller but still substantial decline (−23.6 pp), with the within-firm component contributing 88.6% (−20.9 pp) of the total change. Even among control firms, the within-firm component dominates (−16.9 pp out of a −16.0 pp total decline), though the magnitude is smaller.

Period-by-period dynamics: the Supplemental Appendix (Table A.16) summarizes the year-to-year decomposition averaged by period. Several patterns merit emphasis. First, the within-firm component is negligible for treated firms in the pre-reform period (annual mean: +0.9 pp for Top 30), confirming that the decline in firm-level labour shares was not a pre-existing trend. The within-firm contribution turns sharply negative during the crisis year (−2.7 pp) and remains the dominant driver throughout the post-reform period, averaging −1.3 pp per year in the long-term (2004–2011). Second, the between-firm component is small and fluctuates in sign across periods, never exceeding 1 pp in absolute value on average. This rules out a narrative in which the reforms triggered large-scale reallocation toward low-labour-share firms. Third, the cross term is persistently negative in the immediate post-reform period, consistent with expanding firms simultaneously reducing their labour shares—a pattern suggestive of rising market power among the largest survivors.

Table 7: Olley–Pakes Decomposition of Aggregate Labour Share Change, 1996–2011

	$\bar{S}_{1996}$	$\bar{S}_{2011}$	$\Delta\bar{S}$	Within	Between	Cross	N
Panel A: Baseline (Top 30 vs. Control)							
Control	0.361	0.201	−0.160	−0.169	0.063	−0.054	327
				(1.056)	(−0.394)	(0.338)	
Top 30	0.475	0.163	−0.312	−0.289	0.004	−0.027	61
				(0.926)	(−0.014)	(0.087)	
Panel B: Three-Group Split							
Control	0.361	0.201	−0.160	−0.169	0.063	−0.054	327
Top 30 excl. Top 5	0.447	0.211	−0.236	−0.209	−0.033	0.006	31
				(0.886)	(0.140)	(−0.025)	
Big Deal Top 5	0.487	0.148	−0.339	−0.324	0.012	−0.026	30
				(0.956)	(−0.035)	(0.077)	

Notes: Each row decomposes the change in the value-added-weighted aggregate labour share between 1996 and 2011 using an Olley–Pakes (1996) style shift-share decomposition, restricting to firms observed in both years. The within component equals $\sum_i \theta_{i,1996}(s_{i,2011} - s_{i,1996})$; the between component equals $\sum_i(\theta_{i,2011} - \theta_{i,1996})s_{i,1996}$; the cross term equals $\sum_i(\theta_{i,2011} - \theta_{i,1996})(s_{i,2011} - s_{i,1996})$. Within, between, and cross components sum to $\Delta\bar{S}$ up to rounding. Numbers in parentheses report each component's share of $\Delta\bar{S}$. N denotes the number of firms observed in both 1996 and 2011.

Interpretation: The within-firm dominance confirms that the aggregate decline reflects broad-based reductions in labour's share at the firm level—consistent with rising markups or bargaining shifts—rather than compositional artifacts from reallocation. The positive between-firm component suggests the Melitz-type selection channel (Melitz, 2003) actually worked in the opposite direction, partially cushioning the within-firm decline, echoing patterns documented in transition economies (Bartelsman et al., 2013).

6.4.3 Markups, Profit Shares, and the Within-Firm Channel

The within-firm dominance of the labour share decline naturally raises the question: what happened *within* these firms? If individual firms reduced their labour shares while maintaining near-Cobb-Douglas production technology ($\hat{\sigma} \approx 1$), the residual claimant must be either capital or economic profit. We investigate this by extending the Olley–Pakes decomposition to two additional outcomes: firm-level markups (estimated via the production-function approach of De Loecker and Warzynski (2012)) and the profit share of value added

($\pi_{it} = 1 - s^L_{it} - s^K_{it}$, where s^L and s^K denote the labour and capital shares, respectively).

Markup decomposition: The Supplemental Appendix (Table A.14) reports the two-period decomposition for markups. Aggregate (value-added-weighted) markups rose substantially for all groups between 1996 and 2011, but the increase was markedly larger for treated firms. Among the Top 30, aggregate markups rose by 0.42 (from 1.53 to 1.96), while control firms experienced a decline of 0.31 (from 1.62 to 1.31). The Big Deal Top 5 experienced the largest increase: 0.44 (from 1.72 to 2.16). Critically, the within-firm component accounts for the majority of the markup increase among treated firms. For the Top 30, within-firm changes explain 72.4% of the total increase (+0.31 out of +0.42), with the between and cross terms contributing the remainder. For the Big Deal Top 5, the within-firm share is 79.9% (+0.36), with a moderate cross term (+0.11) indicating that the firms that grew most also experienced the largest markup increases—a pattern consistent with the superstar firm dynamic.

Profit share decomposition: The Supplemental Appendix (Table A.15) extends the analysis to profit shares. The aggregate profit share rose for both treated and control firms, but substantially more for treated firms: 28.8 percentage points for Top 30 versus 20.1 pp for controls. Among the Top 30, the within-firm component accounts for 70.5% of the total increase (+20.3 pp out of +28.8 pp), confirming that individual treated firms retained a larger share of value added as profit. The between-firm component is small (+3.4 pp), and the cross term moderate (+5.1 pp). The Big Deal Top 5 again stand out: within-firm changes explain 90.2% of their profit share increase (+24.0 pp out of +26.6 pp), with between-firm reallocation near zero (−0.2 pp).

By contrast, among control firms, the within-firm component is much smaller (only 27.9% of the total, or +5.6 pp), with the between (+5.5 pp) and cross (+9.0 pp) terms playing a proportionally larger role. This asymmetry is revealing: while all firms experienced rising profitability over this period, the within-firm channel was disproportionately concentrated among treated *chaebols*, particularly the Big Deal group.

Synthesis: Figure 7 summarizes the labour share decompositions across all three groups. The results jointly establish the following chain of evidence. The decline in the labour share among treated *chaebols* is overwhelmingly a within-firm phenomenon (89–96%

of the total change), not a reallocation story. The within-firm decline in labour's share is mirrored by within-firm increases in markups (72–80% of the total markup increase among treated firms) and profit shares (51–90% of the total profit share increase). These within-firm patterns are especially pronounced for the Big Deal Top 5—the firms subjected to the most intensive restructuring. Notably, markups among control firms actually *declined*, while treated firms experienced within-firm markup increases, confirming the differential impact of the reforms. Combined with the near-unitary elasticity of substitution ($\hat{\sigma} \approx 1.0$), these findings rule out both the capital-deepening and reallocation channels and point decisively toward rising firm-level market power as the primary transmission mechanism.

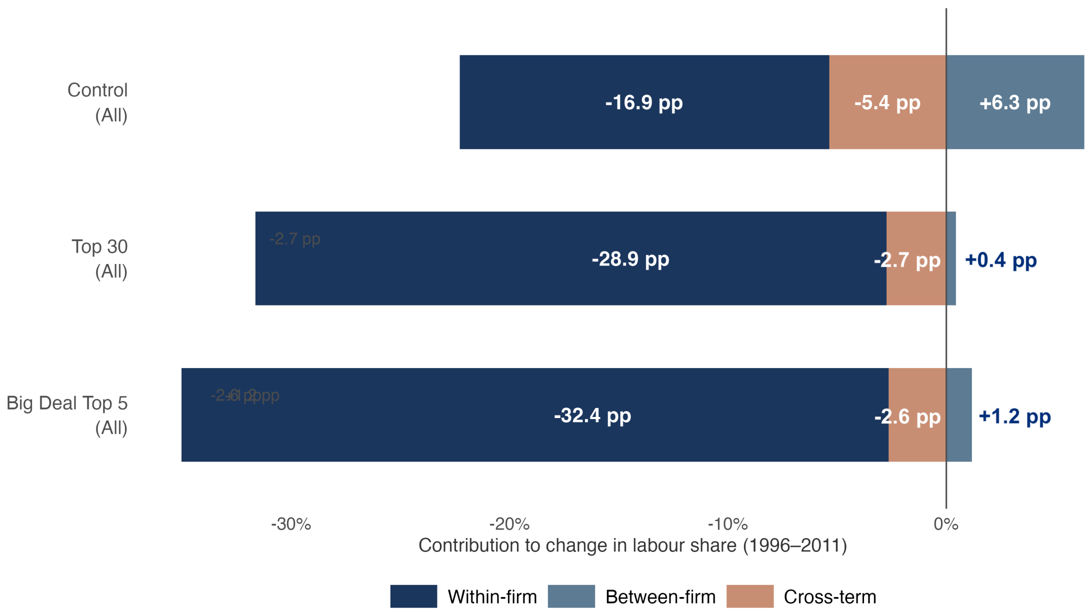


Figure 7: Contribution of Labour Share: Between vs. Within Firms

Broader implications: Beyond the decomposition results, the analysis highlights that distributional outcomes depend critically on the interaction between competition reform, complementary policies, and enforcement stringency. Despite reducing market concentration, the reformed sectors remained highly concentrated (HHI above 0.25), and the Big Deal program consolidated monopolistic structures in several industries. The concurrent labour market deregulation weakened workers' bargaining power and likely amplified the labour share decline. These implications are developed further in Section 8.

7 Robustness Checks

7.1 Placebo and Pre-trend Tests

We perform placebo tests using the `fect` R package, which designates pre-treatment periods as placebo periods and tests whether the model generates spurious pre-treatment effects. The MC method passes the placebo test for all outcome variables (see the Supplemental Appendix in the Appendix).

For pre-trend tests, the results (the Supplemental Appendix) confirm that outcomes generally satisfy the parallel trends assumption. For profit share, the F-test suggests a potential early pre-treatment deviation, but the equivalence test confirms that pre-treatment ATT estimates fall within a region of practical equivalence, supporting identification validity.

7.2 Sensitivity to Treatment Group Definition

The baseline GSC estimates use a treatment group defined as affiliates of the top 30 *chaebols* whose debt-equity ratios in 1997 exceeded the policy threshold (tr30gsc0). This definition involves two researcher choices: the group size cutoff (top 30) and the debt-ratio screening year (1997). Because 1997 was the crisis year, debt ratios observed in that year may already reflect crisis-driven balance-sheet deterioration, potentially introducing selection on an outcome-related variable. To assess sensitivity, we re-estimate the GSC model under three alternative treatment definitions that systematically vary these margins:

- **(i) Narrower group, same debt screen** (tr25gsc0): top 25 *chaebols* with 1997 debt-ratio condition. This tests whether the results are driven by the marginal firms ranked 26–30.
- **(ii) Same group, pre-crisis debt screen** (tr30gsc1): top 30 *chaebols* with 1996 debt-ratio condition. This addresses the concern that 1997 debt ratios are contaminated by crisis dynamics, using instead a pre-crisis year that is unambiguously upstream of the treatment.
- **(iii) Same group, no debt screen** (tr30gsc2): top 30 *chaebols* regardless of debt-equity ratio. This tests whether the debt-ratio screen—itself a potential source of selection—affects the estimated treatment effect.

All specifications use the same covariates, cross-validation procedure, and bootstrapping parameters as the baseline. Table 8 reports the ATT estimates for the labour share across all four specifications at key post-reform horizons. The full period-by-period estimates for each alternative specification are reported in the Supplemental Appendix.

Table 8: Sensitivity of Labour Share ATT to Treatment Group Definition

	Baseline tr30gsc0	**(i) Top 25** tr25gsc0	**(ii) 1996 DR** tr30gsc1	**(iii) No DR** tr30gsc2
Panel A: Labour Share (S^L)				
Period 0 (impact)	-0.021^{**}	-0.014	-0.015	-0.016^{*}
Period 1	-0.048^{***}	-0.051^{**}	-0.050^{***}	-0.060^{***}
Period 2	-0.033^{*}	-0.033^{*}	-0.043^{**}	-0.042^{**}
Periods 3–4 (rebound)	-0.011/$+0.018$	-0.004/$+0.034$	-0.008/$+0.004$	-0.024/$+0.001$
Period 6	-0.057^{***}	-0.049^{**}	-0.060^{***}	-0.069^{***}
Period 7	-0.092^{***}	-0.057^{**}	-0.083^{***}	-0.080^{***}
Periods 8–11 (long run)	-0.059 to -0.079	-0.027 to -0.046	-0.085 to -0.107	-0.072 to -0.101
Periods 12–14	-0.038 to -0.047	-0.005 to $+0.034$	-0.029 to -0.061	-0.035 to -0.053
Panel B: Selected Other Outcomes (Period 6 – representative medium-term)				
Capital Share (S^K)	-0.097^{***}	-0.064^{**}	-0.071^{***}	-0.089^{***}
Profit Share (S^{Π})	$+0.153^{***}$	$+0.095^{***}$	$+0.119^{***}$	$+0.144^{***}$
Markup (μ)	$+0.254^{***}$	$+0.293^{**}$	$+0.291^{***}$	$+0.326^{***}$
L Productivity (ℓ^p)	$+0.253^{***}$	$+0.257^{**}$	$+0.301^{***}$	$+0.290^{***}$
LP-Wage Gap	$+0.233^{***}$	$+0.262^{***}$	$+0.242^{***}$	$+0.263^{***}$

Notes: Each column reports ATT estimates from the GSC model (matrix completion) with the indicated treatment group definition. All specifications use the same covariates (`ln_assets_ttl_pre96`, `ln_emp_pre96`), cross-validation, and nonparametric bootstrap (500 replications). Panel A reports the labour share ATT at key horizons; long-run ranges report the minimum and maximum point estimates across the indicated periods. Panel B reports all six outcomes at period 6 as a representative medium-term comparison. $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Three findings emerge. First, the qualitative pattern is identical across all four specifications: every definition produces the same dynamic signature for the labour share and the same directional results for all other outcomes. Second, the pre-crisis debt screen (tr30gsc1) produces the strongest results, with long-run labour share ATTs of −9.9 to −10.7 pp (all significant at 1%), consistent with cleaner identification from pre-crisis debt ratios. Third, removing the debt screen entirely (tr30gsc2) yields results nearly indistinguishable from the baseline (−6.9 vs. −5.7 pp at period 6), indicating that the debt-ratio screen does not materially alter treated group composition.

The narrower group (tr25gsc0) shows attenuated long-run estimates, likely reflecting reduced statistical power with 25 rather than 30 treated firms, though short- and medium-term effects remain significant. Table 8 confirms that the sign, significance, and approximate magnitude of all six outcomes are stable across all four definitions at a representative medium-term horizon.

7.3 Alternative Proxies for Labour Market Exposure

The channel decomposition in Section 6.4 uses pre-reform labour share as the lead proxy for a firm's exposure to labour market liberalization. Because this proxy is mechanically related to the outcome variable—firms with higher initial labour shares have more room to decline, raising a mean-reversion concern—we verify that the decomposition results are robust to three alternative proxies that are not direct functions of the dependent variable: pre-reform log employment (firm size), pre-reform labour cost relative to value added (labour cost intensity), and pre-reform log wage rate. Each proxy captures a different dimension of a firm's pre-reform reliance on labour and hence its exposure to the labour liberalization channel.

The Supplemental Appendix (Table A.17) reports the decomposition across all four proxies. The competition channel coefficient ($\hat{\beta}_{\text{main}}$) is stable across specifications, ranging from -0.018 (Pre Emp) to -0.054 (Pre LC/VA), confirming that the estimate is not an artifact of proxy choice. The competition channel accounts for 41% (Pre Emp) to 149% (Pre LC/VA) of the total ATT. In two specifications (Pre LabShare and Pre LC/VA), the labour liberalization channel actually offsets the competition channel because treated *chaebols* were already capital-intensive with relatively low labour dependence.

The Pre Emp specification provides the most informative lower bound: treated *chaebols* had substantially above-average employment ($\bar{z} = 1.18$), making them particularly exposed along the size dimension. Even so, the competition channel accounts for 41% of the total effect. The full regression table is reported in Table A.13 in the Supplemental Appendix.[27]

[27] In the dynamic specification, both the immediate and long-term triple interactions with Pre LabShare carry the same sign, confirming no temporal heterogeneity.

7.3.1 Subgroup Interaction Effect Model: Impact of Big Deal Policy

To examine whether the Big Deals amplified or attenuated the reform effects, we estimate a subgroup DiD model that interacts treatment indicators for Big 5 and Ranks 6–30 separately with immediate and long-run post-reform dummies:

$$\begin{aligned} Y_{ft} = \alpha &+ \delta_1 \left(\text{Treated-Other}_f \times \text{ImmediatePost}_t\right) \\ &+ \delta_2 \left(\text{Treated-Other}_f \times \text{LongTermPost}_t\right) + \delta_3 \left(\text{BigDeals}_f \times \text{ImmediatePost}_t\right) \\ &+ \delta_4 \left(\text{BigDeals}_f \times \text{LongTermPost}_t\right) + X'_{ft}\beta + \gamma_f + \lambda_t + \epsilon_{ft} \end{aligned} \tag{7.1}$$

where δ_3 and δ_4 capture the differential effects for Big Deal participants relative to non-Big-Deal treated firms.

Results: The full results are reported in the Supplemental Appendix (Table A.13); we summarize the key patterns here.

The results reveal two key patterns. First, the Big 5 *chaebols* exhibit substantially larger effects on the capital share (-0.055^{**} immediate, -0.126^{***} long-run) and profit share ($+0.118^{***}$ long-run) relative to the Ranks 6–30 group, consistent with the intensive restructuring these firms underwent through the Big Deal program. For the labour share, neither subgroup shows a statistically significant effect, though the point estimates are directionally negative. Second, the Big 5 show large and significant increases in markups ($+0.432^{***}$ immediate) and labour productivity ($+0.354^{**}$ long-run), suggesting that the restructuring program intensified surplus reallocation within these firms.

Subgroup analysis thus reveals meaningful heterogeneity in the treatment effects. For the broader set of treated *chaebols* (Ranks 6–30), the reform effects operate primarily through the capital and profit share channels, while for the Big 5, the effects are amplified across all margins—consistent with the additional impact of state-directed corporate restructuring.

8 Concluding Remarks

This study investigates the distributional consequences of South Korea's 1998 bundled reform package targeting its top *chaebols*, combining competition enforcement, labour market liberalization, and state-directed corporate restructuring. Exploiting these reforms as a quasi-natural experiment, I combine a generalised synthetic control method with difference-in-differences estimation on a panel of publicly listed firms (1991–2011) to identify the causal impact of the reform bundle and trace the transmission mechanisms through which it operated.

Main findings: Three sets of results emerge. First, the bundled reform package significantly reduced the labour share among treated *chaebols*—a finding that qualifies the conventional expectation that pro-competitive reforms promote more equitable income distribution. The reforms also widened the labour productivity gap between treated and control groups and exacerbated the productivity–wage compensation gap among treated firms. Second, these effects were amplified by the state-led corporate restructuring programs known as the *Big Deals*: the *Big Five chaebols* that underwent the most intensive restructuring account for roughly half the long-run labour share decline, though the effect persists even after excluding these firms. Third, the reforms led to a significant decline in the capital share as well, with the combined reduction in both labour and capital shares offset by a substantial rise in the profit share.

Transmission mechanisms: Our analysis systematically evaluates three candidate channels. The capital-deepening channel is ruled out: our preferred first-order condition estimate yields an elasticity of substitution $\hat{\sigma} \approx 1.0$, consistent with Cobb-Douglas, and a DiD specification finds no reform-induced change in the elasticity. Rolling-window estimates confirm smooth convergence toward unity with no structural break around 1998. Even at the upper bound of our estimates, the quantitative implications for the labour share are an order of magnitude too small to explain the observed decline.

The Olley–Pakes shift–share decomposition identifies the operative margin. Among the Top 30 *chaebols*, 93% of the cumulative 31.2 percentage-point decline in the aggregate labour share between 1996 and 2011 is attributable to the within-firm component—individual firms reducing their labour shares while holding their relative output positions approximately constant. Between-firm reallocation actually *partially offset* the decline. This within-firm dominance is mirrored by within-firm increases in markups (72% of the total markup increase for Top 30 firms) and profit shares (71% of the total profit share increase), with the Big Deal

Top 5 exhibiting the most pronounced pattern—consistent with a superstar firm dynamic (Autor et al., 2020). Notably, while treated firm markups rose, control firm markups actually declined over the same period, reinforcing the differential impact of the reforms.

These findings parallel the U.S. evidence of Barkai (2020), where declining labour and capital shares were offset by rising profit shares driven by market power. However, our setting adds an important nuance: while the value-added-weighted aggregate markup rose, the median markup across treated firms declined in the immediate post-reform period. This divergence indicates that markup gains were concentrated among the largest survivors, not distributed across the treated group—precisely the superstar firm pattern.

Discussion: Three broader observations carry implications for the design of pro-competitive reforms. First, distributional outcomes depend on reform effectiveness and enforcement (Zac et al., 2023). The 1998 reforms reduced market concentration in *chaebol*-dominated sectors, but these sectors remained highly concentrated (HHI consistently above 0.25), and the Big Deal program consolidated monopolistic structures in several key industries. Competition policy that does not adequately dismantle entrenched concentration may inadvertently widen the gap between capital and labour income. The reforms reduced the *flow* of markup adjustment while leaving the *stock* of structural market dominance largely intact.

Second, the effects of pro-competitive reforms are shaped by complementary policies. Labour market deregulation weakened workers' bargaining power and likely amplified the labour share decline, while the Big Deal restructuring program consolidated market power among the largest *chaebols*. Concurrent large-scale layoffs during the financial crisis accelerated this process. The net distributional impact of competition reform cannot be assessed in isolation from the broader policy environment.

Third, political economy constraints matter. Despite the transition toward a market-oriented regime, the Korean government continued to view *chaebols* as indispensable partners for economic recovery, limiting the stringency of reform enforcement. The Big Deals were driven more by the demands of international creditors and the urgency of financial stabilisation than by distributional concerns — prioritising efficiency over equity.

Policy implications: These findings should not be interpreted as a rejection of pro-competitive reforms, which remain essential for market efficiency. Rather, they underscore that competition policy alone is insufficient to ensure equitable distributional outcomes when implemented alongside labour market flexibilisation and large-scale corporate restructuring. The Korean experience carries direct lessons for three contemporary policy contexts. First, the United States' renewed antitrust enforcement under recent executive orders occurs alongside ongoing labour market deregulation in many states—the Korean case suggests that antitrust action without complementary labour protections may not deliver the distributional benefits its proponents expect. Second, the European Union's Digital Markets Act targets platform dominance, but platform workers' bargaining power is simultaneously contested—the bundling problem recurs in digital markets. Third, developing countries undergoing IMF-supported structural adjustment programs continue to face precisely the reform bundling documented here, where competition reform, labour flexibility, and privatization are packaged together.

Three policy directions follow: complementing competition policy with SME support to narrow the wage gap between large firms and smaller enterprises (OECD, 2019; Aghion et al., 2015); pairing competition reform with robust labour protections as AI and automation continue to erode lower-skilled workers' bargaining power (Leduc and Liu, 2024); and designing innovation and competition policy as a coordinated mix that directs productivity benefits toward broader distributional objectives (Mazzucato, 2018).

In an era of renewed protectionism and strategic industrial policies, policymakers remain prone to selecting superstar firms as national champions. Without adequate countervailing institutions, this tendency risks exacerbating functional income inequality. This study challenges the presumption that market liberalization inherently leads to equitable income distribution, exposing the tension between the efficiency gains from bundled reform packages and their distributional consequences. The central lesson is not that competition reform is harmful, but that its distributional effects are determined by the institutional environment in which it is embedded.

Supplemental Appendix (Available Online)

A Supplemental Appendix accompanies this paper, containing: a timeline of pro-competitive reforms and the Big Deals (Figure A.1); estimation sample structure and industry distribution (Tables A.1–A.5); production function estimates, including the naive OLS benchmark (Table A.6); summary statistics for main variables (Table A.7); GSC ATT estimates across specifications (Table A.8); event-study coefficient plots and clustering robustness checks (Figures A.3–A.4, Table A.10); two-way fixed-effects DiD results (Tables A.11–A.12); subgroup interaction model results (Table A.13); subgroup analysis by chaebol rank (Figure A.5); placebo and pre-trend tests (Figures A.6–A.7); Olley–Pakes decomposition detail for markups and profit shares (Tables A.14–A.15); average annual decomposition contributions by period (Table A.16); group-specific elasticity of substitution scatter plots and rolling-window estimates (Figures A.8–A.9); channel decomposition regression results (Table A.17); and CES production function derivation.

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