MANUFACTURING REVOLUTIONS: INDUSTRIAL POLICY AND INDUSTRIALIZATION IN SOUTH KOREA*

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I study the effect of industrial policies on industrial development by considering an important episode during the East Asian miracle: South Korea's heavy and chemical industry (HCI) drive, 1973–1979. Based on newly assembled data, I use the introduction and termination of industrial policies to study their effects during and after the intervention period. I reveal that HCI policies promoted the expansion and dynamic comparative advantage of directly targeted industries. Using variation in exposure to policies through the input-output network, I demonstrate that the policy indirectly benefited downstream users of targeted intermediates. The benefits of HCI persisted even after the policy ended, and some results took time to materialize. The findings suggest that the temporary drive shifted Korean manufacturing into more advanced markets and supported durable change. This study helps clarify the lessons drawn from the East Asian growth miracle. *JEL codes:* L5, O14, O25, N6.

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I. Introduction

Miracles by nature are mysterious. The forces behind the East Asian growth miracle are no exception. Industrial policy (IP) has defined Asia's postwar transformation (Rodrik 1995). Historically, economists focusing on development saw these policies as essential for industrial development (Rosenstein-Rodan 1943; Nurkse 1953; Hirschman 1958), and some have argued that they were instrumental to East Asia's ascent (Wade 1990; Amsden 1992). However, many economists have been skeptical of their use (Baldwin 1969; Krueger 1990), and others have argued that they played a counterproductive role in Asia (Pack 2000). South Korea exemplifies Asia's rapid transformation and controversies around industrial policy. At the beginning of the 1960s, South Korea was a politically unstable industrial laggard; by the 1980s, it had undergone the kind of manufacturing transformation that had taken Western economies over a century to achieve (Nelson and Pack 1999). What role did industrial policy play in this transformation? As conversations about industrial policy have returned (Juhász et al. 2025), empirical evidence surrounding its efficacy is scant, especially for the East Asian miracle (Lane 2020).

I use the context of the heavy industrial drive and employ a dynamic difference-in-differences (DD) estimation strategy. I evaluate the effects of South Korean industrial policy by comparing changes in outcomes between targeted (treated) and non-targeted (untreated) manufacturing industries each year before and after the policy's launch. My baseline DD results are based on traditional two-way fixed effect (TWFE) estimators. I build on these results in two ways. First, I show that the core results are robust to using a double-robust DD estimator (Sant'Anna and Zhao 2020; Callaway and Sant'Anna 2021) that combines outcome-regression and propensity-score models to adjust the counterfactual. Second, I employ a cross-country, triple differences (DDD) estimation strategy, comparing Korean manufacturing sectors with foreign placebo manufacturing sectors.

The main DD estimates capture the effects of heavy industry policies, which emphasized directed credit and, to a lesser extent, trade policy. Industry pre-trends inform Korea's counterfactual sectoral structure. Without these interventions, industries would have evolved according to an earlier pattern of comparative advantage. I refer to Korea's comparative advantage without intervention as its static comparative advantage. Differences after 1973 reveal the effect of industrial policy in promoting dynamic comparative advantage, where the overarching policy was associated with the ascent of new industries and new patterns of specialization.[1]

To estimate these effects, I construct a new data set on industrial outcomes spanning Korea's miracle period (1967–1986). I harmonize material from digitized industrial surveys and historical machine-readable statistics into consistent panel data and then combine this industry-level data with digitized input-output (IO) accounts. This process results in panel data that cover a key episode of industrial development.

I highlight three empirical results. First, I find that the policy package resulted in significant positive effects across industrial development outcomes in targeted industries. Relative to pre-intervention levels, targeted heavy-chemical industries (HCIs) expanded their output by more than 100% over non-treated manufacturing sectors. Furthermore, labor productivity was more than 15% higher. This divergence was not driven by a decline in non-treated industries. Moreover, because industrial development is multidimensional, I consider it across outcomes and find effects on employment growth, export performance, and output prices. HCI appears to have durable, longer-run effects on treated industries, and I find evidence of persistent effects on plant-level, total factor productivity (TFP) in the post-1979 period.

I emphasize the role of investment policy and find evidence that supports dynamic learning by doing. Reduced-form estimates show that HCI sectors are correlated with stronger learning-by-doing forces, and the results are consistent with industry-wide, cross-plant learning spillovers. Importantly, I do not find that the HCI policy crowded out investment in non-treated industries.

Second, HCI coincided with a shift in the longer-term dynamic comparative advantage of the targeted export industry. Post-1979 outcomes, such as the share of activity in manufacturing sectors (employment or output), remained significantly higher than in non-treated sectors. In addition, treated industries were 10 percentage points more likely to achieve comparative advantage in global markets after 1973. Indeed, the revealed

1. These definitions build on Redding (1999), who defines dynamic comparative advantage more generally as a time-varying version of classic static comparative advantage.

comparative advantage (RCA) of HCI products increased 13% more than other manufacturing exports over the same period, and I observe similar patterns using gravity-based methods (Costinot, Donaldson, and Komunjer 2012). However, these patterns only emerged over time. Consistent with infant-industry theory (e.g., Bardhan 1971), shorter-term evaluations may fail to capture the full, dynamic effects of a given policy.

Third, HCI-drive policies correspond with the development of downstream industries. I find that downstream sectors with strong links to targeted industries expanded during the policy period. During the drive, comparative advantage emerged among downstream exporters and fully materialized after the end of the policy period (1979). However, given that policies targeted more upstream industries, the backward-linkage effects of the policy seem limited. Hence, I find evidence that the policy may have supported network spillovers. These results are consistent with quantitative research on optimal-policy approaches in networks, such as Liu (2019), which uses IO data from this study. Accounting for linkages reduces the precision of the main effects but preserves the core pattern of industrial-development estimates.

This study makes three contributions. First, I build on emerging research that uses contemporary econometrics to study the effect of industrial policies, including cross-country explorations of trade policy by Nunn and Trefler (2010), seminal case studies by Aghion et al. (2015) and Criscuolo et al. (2019), and sector-specific studies by Blonigen and Prusa (2016). It also complements the structural literature in industrial organization, which analyzes sector-specific industrial policies (Kalouptsidi 2018; Barwick, Kalouptsidi, and Zahur 2019), including earlier calibration-based evaluations (Baldwin and Krugman 1988; Irwin 1991; Head 1994). Similarly, relevant research in development economics by Rotemberg (2019) and Martin, Nataraj, and Harrison (2017) has explored industrial policy in India's small and medium-sized enterprise policy.

I contribute to the empirical study of industrial policy via natural experiments. This article is the first study to use modern empirical techniques to evaluate the HCI drive episode. I join Juhász (2018) and related work by Inwood and Keay (2013) and Harris, Keay, and Lewis (2015), who use historical experiments to estimate the effects of output market protection on manufacturing development. I consider the efficacy of infant-industry policy in a contemporary setting and with outward-oriented (e.g., export-facing) policies. My findings align with studies that use temporary historical episodes to explore the process of dynamic comparative advantage (Pons-Benaiges 2017; Hanlon 2020;). For example, Jaworski and Smyth (2018) and Giorcelli (2019) explore the impact of temporary government policies on industrial development. More broadly, I complement historical research highlighting the potential of transitory policy to promote the longer-run development of nascent industries. I do so by examining a purposeful, targeted intervention. By considering targeted policies, I connect with studies evaluating place-based policies, notably Criscuolo et al. (2019) and Becker, Egger, and Ehrlich (2010), who use exogenous spatial variation to study the effect of targeted support on distressed regions.

Finally, I add to debates on the role of industrial policy in development, especially those surrounding the East Asian miracle. On the one hand, rich qualitative research has emphasized the role of industrial strategies in newly industrializing economies (Johnson 1982; Wade 1990; Amsden 1992; Chang 1993). On the other hand, economists have generally expressed skepticism of such interventions (Noland and Pack 2003; Pack and Saggi 2006), especially their role in East Asia's ascent (Krueger 1995; Pack 2000). This study is the first modern empirical attempt to revisit debates on the East Asian episode, which is summarized in Section III. By using contemporary econometrics, I build on early correlational studies (Weinstein 1995; Beason and Weinstein 1996) and more recent quantitative research (Liu 2019).

My analysis is organized in the following way. In Section II, I discuss the institutional setting of the heavy industry drive and detail the policies. In Section III, I describe the general theoretical case for industrial policies. Section IV provides an overview of the data. In Section V, I present estimates of the direct effect of the heavy industry push on targeted industries, and in Section VI, I turn to policy mechanisms. Finally, I consider the estimates of HCI's spillovers into external sectors through IO linkages in Section VII. I conclude in Section VIII with a discussion of my findings.

II. Institutional Context

I first consider the institutional and historical context of the HCI drive. This section describes the policy's launch, sectoral choice, and variation over time. Finally, I synthesize my use of these features in the empirical research design.

II.A. The Nixon Shock and Launch

Political crises in South Korea catalyzed its 1973 industrial drive, which was fundamentally security-driven (H.-A. Kim 2011; Moon and Jun 2011). Among the factors behind Korea's crisis were (i) North Korea's increasing militarization and offensive actions (Kim 1997; Moon and Lee 2009) and, critically, (ii) a shift in U.S. foreign policy toward Asia. In 1969, President Richard Nixon declared that the United States would no longer provide direct military support to its allies in the Asia-Pacific region, creating the risk of U.S. troop withdrawal from the Korean Peninsula (Kim 1970; Nixon 1970; Kwak 2003). Unfortunately, this U.S. pivot coincided with North Korea's growing military antagonism. Like its South Vietnamese allies, South Korea believed it would need to defend itself against an impending communist-backed invasion. However, South Korea had no domestic arms industry, and the North rivaled the South militarily, having pursued a military industrialization campaign through the 1960s (Hamm 1999)—South Korea had not kept up. Without U.S. troops, South Korean armaments would not be able to absorb a North Korean blitz (Cushman 1979; Eberstadt 1999).[2] Military exigencies drove the timing of the heavy industry push and shaped its sectoral scope.

II.B. Sectoral Choice

1. *Sectoral Rationale and Selection.* The HCI drive targeted six strategic sectors: steel, nonferrous metals, shipbuilding, machinery, electronics, and petrochemicals (Stern et al. 1995; Castley 1997). Throughout this study, I define treated or targeted (I use the terms interchangeably throughout) industries as those listed in major policy acts—specifically, the enforcement decrees and national sectoral acts that undergirded the drive. Section IV specifies how I coded policy treatment separately from legislation, and I provide legislative details in Online Appendix 1.

Why were these sectors chosen, and what might deliberations over their selection tell us about expectations for their success?

2. Online Appendix A.1 describes the so-called Nixon shock and the subsequent political crisis. Online Appendix H.1 describes Korea's military status.

Two rationales dominated the choice of HCI sectors, and both have been documented by scholars and policy makers.

First, heavy industrial intermediates were seen as key for the military and industrial modernization (Lee 1991; Woo-Cumings 1998; H.-A. Kim 2011). In the early 1970s, unlike North Korea, direct military production was largely beyond the South's capabilities. Early failures in arms manufacturing were specifically mired by inputs of "inadequate" quality (Horikane 2005, 375). One former government official reported it was "apparent that the development of the heavy and chemical industries to the level of advanced countries was required to develop the defense industry" (C.-Y. Kim 2011, 409). Hence, industrial intermediates were a means of promoting military industrialization and future hardware production. For planners, the steel and nonferrous metals sectors supplied crucial upstream materials for basic defense components, electronic components for electronic weaponry, and machinery for precision military production (C.-Y. Kim 2011). Former officials from the government of South Korean President Park Chung-hee echoed these rationales (Kwang-Mo 2015). Thus, unlike downstream weaponry, upstream inputs were within Korea's capabilities and less controversial to lenders.

Second, relative to advanced military hardware, South Korea saw a potential advantage in targeting upstream production. Where Korea lacked the prerequisites to manufacture arms at scale, upstream intermediates were more practical—they were technologically within reach and featured economies of scale that could be supported through export markets (C.-Y. Kim 2011). To consider feasibility, the regime studied its contemporaries, including those in Western Europe and Japan (Perkins 2013), though the latter was less a metaphor than a blueprint. Japan's experience gave South Korea a guide to the sectors in which they had potential (Kong 2000; B.-K. Kim 2011; Moon and Jun 2011), and components of Korea's drive were borrowed from the New Long-Range Economic Plan of Japan (1958–1968). I discuss the overlap between Japanese and Korean policies in Online Appendix 2.

2. *Selection Skepticism by Foreign Lenders.* Ex ante, the potential of Korea's heavy-chemical industries was not obvious, and international investors expressed doubts, famously rejecting the financing of erstwhile heavy industrial projects (Amsden 1992; Redding 1999). The International Monetary Fund (IMF), the U.S. Agency for International Development (USAID), and several

European nations refused to provide loans for less ambitious, proto-HCI schemes (Woo 1991; Rhyu and Lew 2011). In 1969, both the U.S. Export-Import (EXIM) Bank and the World Bank blocked an early integrated steel mill, with the World Bank concluding that Korea "had no comparative advantage in the production of steel" (C.-Y. Kim 2011; Rhyu and Lew 2011, 324). The skepticism of lenders toward Korea's proto-HCI projects continued through the early 1970s, and such practicalities constrained early forays into heavy industrial projects—that is, until South Korea's political turn in late 1972,[3] when President Park's autocratic self-coup and breakthroughs due to international capital finally enabled a heavy industrial push.

II.C. Policy Instruments and Variation: Before, During, and After HCI

The drive's January 1973 announcement broke with Korea's earlier horizontal export-first industrial policy regime (Frank, Kim, and Westphal 1975; Krueger 1979; Westphal and Kim 1982; Westphal 1990), which famously was not sectoral but aimed at export activity writ large (Hong 1979, 28).[4] Before the HCI drive, export incentives were essentially "administered uniformly across all industries" (Westphal and Kim 1982, 217–218; Westphal 1990, 44). Exporters were exempted from so many restrictions that scholars have argued that the export drive essentially "allowed exporters to operate under a virtual free trade regime" (Nam 1980, 91; Lim 1981). In other words, the HCI drive represented a pivot to a fundamentally sector-specific strategy.

What was the industrial policy bundle? I consider two classes of policies in detail—investment policy and trade policy—and their variation across the period.

1. *The Bias of Lending and Investment Incentives.* Directed lending was a central lever of the heavy industrial drive (Lee 1991; Woo 1991).[5] Half of all domestic credit consisted of subsidized "policy loans," which were allocated by financial

3. See Online Appendix 3 for details on these constraints.

4. Before 1973, Korea implemented no fewer than 38 different incentives to promote exports (Lim 1981, 18). See Online Appendix 4 for details on Korean economic planning.

5. Woo summarizes that Korean policy sought to "hemorrhage as much capital as possible into the heavy industrialization program" (Woo 1991, 59).

institutions—less traditional nonbanking institutions (development banks) and more traditional commercial banks (Koo 1984; Lee 1996). Broadly defined, policy-targeted loans were designed to advance government objectives and were automatically rediscounted by the central bank at a preferential rate.[6] For example, over this period, policy loans had longer repayment schedules, and average interest rates were 5 percentage points lower than benchmark loans (Cho and Kim 1995).

Figure I illustrates the shift from (pre-1973) sector-agnostic policies to (post-1973) sector-specific investment policies. Panels B and C track the rise in new credit to the heavy-chemical sector after 1973 and the decline in direct state lending after 1979. Specifically, Panels B and C plot the change in loans issued by the Korea Development Bank (KDB), the source of around 90% of attractive policy loans lent by nonbanking financial institutions (Cho and Kim 1995, 42). Panel B presents the real value of all new KDB loans by industry, and Panel C presents these values for machinery and intermediates, a major focus of the industrial drive policy. The thin lines correspond to two-digit industries, and the thick lines are averages for targeted (red/lighter gray; color version available online) and non-targeted (darker gray) industries. Parallel lines denote the average lending for each period.

The sectoral bias of lending by state institutions is also seen in more traditional commercial deposit banks, which allocated a significant share of policy loans (World Bank 1993; Cho and Kim 1995). Online Appendix Figure A.2 shows similar growth for total credit and intermediate equipment loans by commercial banks. Indeed, across lending institutions, aggregate data in both figures (Figure I and Online Appendix Figure A.2) plot a trend-break in sectoral-specific lending after 1979, which marks the beginning of policy liberalization.

Similarly, Figure I, Panel A traces the sectoral bias of tax policy over the period, using the estimated effective marginal tax rate. These estimates account for myriad period-specific investment incentives, notably tax breaks, investment tax credits, and special depreciation rates (Kwack 1985; Stern et al. 1995; Lee 1996); see Online Appendix I.5 for details. Panel A presents the divergence in rates after 1973, when tax laws were reformed to concentrate investment in heavy industry (Kwack 1984; Kim

6. Historically, Korean policy loans have served rural and infrastructural development objectives and were a prominent lever of heavy industry targeting.

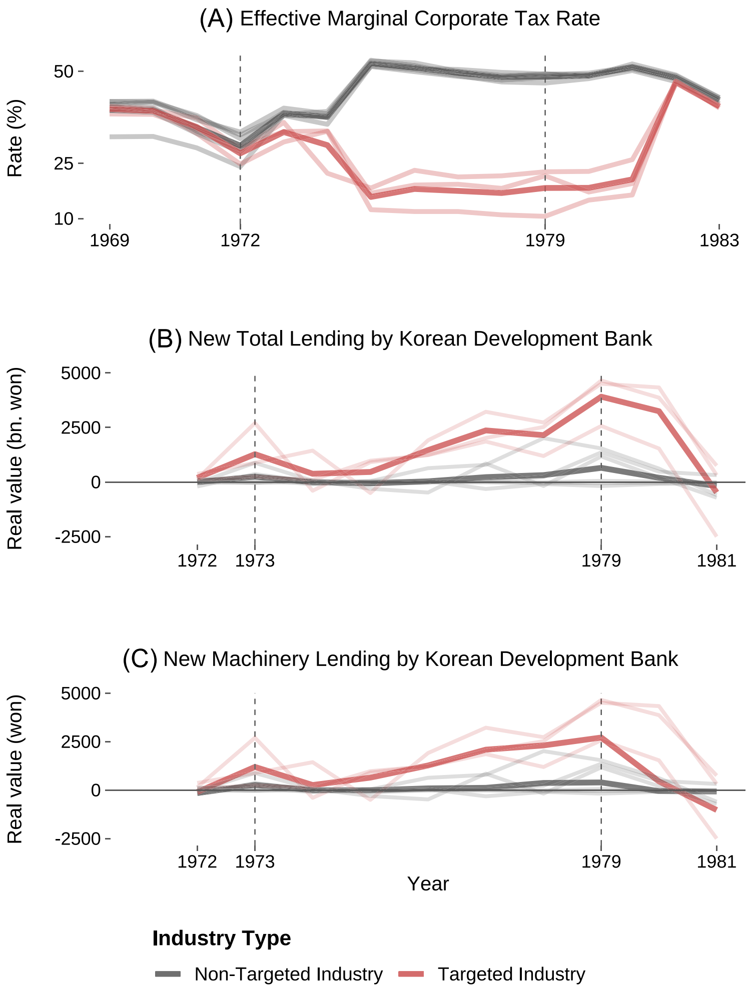

FIGURE I

Investment Policy and the Heavy and Chemical Industry Drive

This figure shows the patterns of investment policy through time by industry. Panel A plots estimates of the average effective marginal tax rate (percentage) on the returns to capital, accounting for changes in industry-specific tax subsidies (1969–1983). Thin lines are estimates for two-digit manufacturing industries. Thick lines are averages for treated and non-treated industries. Gray lines correspond to non-targeted sectors, and red (lighter gray; color version available online) lines correspond to targeted sectors. Panel B plots the change in the (real) value of total loans issued by the Korea Development Bank, 1972–1981, a representative state lending institution. Values are real values in won. Panel C plots only changes in lending for machinery, a major component of HCI lending and policy loans.

1990).[7] Like directed credit, tax policies converged after the start of the liberalization period in 1979.

2. *The Bias of Trade Policy.* The heavy and chemical industry drive also altered biases related to trade policy. Pre-1973, government policies broadly exempted exporters from import restrictions (Nam 1980; Westphal 1990). Indeed, measures of nominal protection were lower for heavy industry during this period (see Online Appendix 5 for pre-1973 trade policy). After 1973, exemptions were aimed at heavy industry (Woo 1991; Cho and Kim 1995), and HCI producers were exempted from up to 100% of import duties. Park (1977, 212) estimates that "key industries," on average, enjoyed 80% tariff exemptions. Although the post-1973 trade policy was refocused toward heavy industry, the nominal protection of output markets did not appear to rise substantially, especially relative to other policies (see Section VI.B).

3. *Post-1979 Liberalization.* President Park Chung-hee's assassination in 1979 prompted the withdrawal of his signature policy. With the fall of Park's regime, South Korea dismantled heavy-chemical industrial incentives and pursued structural economic reforms. I provide details of the post-1979 liberalization in Online Appendix 6. For example, the state-controlled banking sector was liberalized, with notable reforms in 1981 and 1983. Special rates on policy loans were eliminated, and they took a different form after 1979 (see Online Appendix A.2). Although the role of government policy loans shrank (Cho and Cole 1992; Nam 1992), fiscal reforms closed the gap in effective marginal corporate tax rates between strategic and nonstrategic industries (Kwack and Lee 1992). Meanwhile, the post-Park autocracy accelerated Korea's trend toward trade policy liberalization.

II.D. Summary: Features for Empirical Study

The policy context informs the research design of this study, which I summarize in four points. First, the episode introduced sectoral variation over time, as the heavy and chemical industry drive shifted national policy toward a discrete set of nascent

7. Packages included the "Special Tax Treatment for Key Industries" (Tax Exemption and Reduction Control Law), which gave strategic industries the choice of a five-year tax holiday, an 8% tax credit toward machinery investment, or a 100% special depreciation allowance (Lee 1996, 395).

industries. This shift began and ended because of external political events: the Nixon doctrine and Park's assassination, respectively. The liberalization of HCI is also useful, as theoretical justifications often entail temporary policy.

Second, policy variation was purposeful. Notably, I consider an actual policy and not a random variation mimicking industrial policy. Given the complications of estimating the effect of industrial policies, researchers have used important natural experiments that mimic policy variations (Juhász 2018; Hanlon 2020; Mitrunen 2025). Nevertheless, the case for industrial strategy hinges on the policy being intentional (Juhász et al. 2025), and it may be difficult to glean insights from random, accidental policy variation (Rodrik 2004).

Third, although targeted, Korea did not believe HCIs would develop without intervention, and financiers doubted the viability of the Korean heavy industry sector. Foreign lenders rejected financing for early prototype projects on the grounds of comparative advantage. Korean planners countered that investment could cultivate comparative advantage in targeted sectors.

Fourth, the political context of the HCI drive reduces the role of political confounders. This setting, including the existential threat facing South Korea, meant industrial policies were binding and coherent. Clientelism and political demands often divert resources to industries with a comparative disadvantage (Rodrik 2005; Lin 2012), and policy estimates may reflect political failures rather than the potential of a given policy. Korea's heavy and chemical industry drive was driven by top-down changes in national economic and defense strategy—the sectoral bias was not driven by lobbying or heavy industrial constituents.

III. Conceptual Cases for Industrial Policy

Mainstream neoclassical justifications often rely on the existence of externalities (Corden 1997; Juhász, Lane, and Rodrik 2024). I discuss two externalities relevant to the South Korean policy episode: dynamic economies of scale and linkage effects. I consider each in the context of earlier empirical work on East Asia.

III.A. Dynamic Economies of Scale

Dynamic externalities have long guided justifications for infant-industry policy (Bardhan 1971; Succar 1987; Young 1991). Intra-industry learning-by-doing externalities embody this class of justifications, whereby firms accumulate production experience over time; in turn, this experience benefits other firms in the same industry. Hence, individual firms may not internalize the benefits of learning, producing, or underinvesting in a socially beneficial activity. Interventions may also be justified even without across-firm spillovers, such as when firm-level learning occurs alongside other imperfections (Lucas 1984; Corden 1997). For instance, a firm may have strong learning-by-doing forces, yet if it faces capital constraints, it may be unable to survive turbulent nascent periods.

Such dynamic economies of scale are the means by which industrial policy can, in theory, cultivate a dynamic comparative trade advantage (Redding 1999). Theoretically, if learning-by-doing conditions are suitable (i.e., within-industry learning spillovers or firm-level learning combined with imperfections), a successful infant industrial policy in a new sector can promote the evolution of comparative advantage on the international market.

Correlational studies of East Asian industrial policy have suggested that interventions may not correspond to industrial development or externalities. For Korea, Lee (1996) identifies a negative relationship between postwar interventions and industry-level outcomes, specifically, protection and manufacturing productivity (see also Dollar and Sokoloff 1990). Beason and Weinstein (1996) find that Japanese industrial policy is not positively correlated with industry development. Similarly, Yoo (1990) argues that HCI may have harmed South Korea's export development performance relative to its contemporaries.

III.B. Linkage Effects

Second, pecuniary externalities through linkages have been another justification for implementing a particular industrial policy (Krueger and Tuncer 1982; Grossman 1990; Krugman 1993), where policies targeting one sector benefit external industries through IO connections. Development economists have long considered how industrial interventions impart benefits beyond the direct targets of the policies through IO linkages (Scitovsky 1954; Rasmussen 1956; Hirschman 1958). They argue that intuitive targeting is likely justified where the social benefits conferred to others are considerable. These benefits are transmitted in two directions. The first is through backward linkages to upstream industries selling inputs to targeted sectors. For example, if an industrial policy increases the size of targeted industries, it increases the demand for upstream producers. Second, industrial policy can confer benefits through forward linkages to downstream industries purchasing inputs from targeted sectors. For example, if a policy increases the productivity of a treated industry, it may lower prices, which benefits firms using those inputs.

As with dynamic externalities, tests of industrial policy justifications with linkage spillovers have attempted to explore the relationship between targeting—or often, policy levers—and the existence of linkage spillovers. Incisive studies of East Asia, in particular, have rejected industrial policy on the grounds that it has not corresponded to these externalities. Noland (2004) argues that Korean policy did not target sectors with high linkage spillovers. Using measures of IO linkages, Pack (2000) finds that industries targeted by South Korea and Japan had low linkages with non-targeted industries and questions whether the policy targeted externalities. Taken together, Noland and Pack (2003) and Pack and Saggi (2006) argue that industrial development and targeting seem uncorrelated with growth in key historical episodes. A recent applied theoretical study by Liu (2019) reveals that common features of IO tables may correspond to optimal targeting, using evidence for South Korea and China.

IV. Data

I use newly assembled industry-level data on industrial development during South Korea's miracle period, 1967–1986. Industry-level panels are constructed using digitized data from the Economic Planning Board's (EPB) Mining and Manufacturing Surveys and Census (MMS). MMS data are suitable for studying the HCI drive, which was fundamentally a sectoral policy. The survey is high quality and reports consistent manufacturing census outcomes over the study period. The MMS census data are published nearly every five years, with annual intercensal surveys. Manufacturing outcomes are published at the five-digit industry level and aggregated from establishment (or plant) level surveys.[8] In addition to industry-level data, I use post-1979 plant-level microdata from the MMS. Price data are digitized from historical and contemporary Bank of Korea producer price index publications and yearbooks.

IV.A. Long and Short Industry Panels

This study uses two harmonized industry panels. Online Appendix Table A.1 presents pre-1973 statistics (mean and standard deviation, non-normalized values) for key industrial variables. Panel B of Online Appendix Table A.1 reports values from the "short" granular five-digit industry panel, harmonized from 1970 to 1986. Panel A reports values from the "long" more aggregated four-digit panel, harmonized from 1967 to 1986. The terminal date of the study is 1986, the year before Korea's consequential democratic transition.

Creating these consistent industry panels from MMS data is not trivial and requires harmonization across multiple code revisions. Between 1967 and 1986, the EPB updated Korea's industrial codes (KSIC) four times, with a major revision in 1970. Thus, harmonizing MMS data alone requires multiple crosswalk schemas and their digitization. I describe this process in Online Appendix I.2 and the concordance in the MMS and across other data series.

The harmonization process introduces a trade-off between the two panels. The short panel (1970–1986) contains more industry observations (five-digit level) but covers a more limited timeline. The shorter panel requires less harmonization and thus is closer to the original MMS publication statistics. In contrast, the long panel (1967–1986) contains fewer industries (four-digit level) but covers a longer timeline. Thus, the longer panel requires more harmonization but encompasses the critical pre-1973 ("pretreatment") period. Although the long panel adds three years to the pretreatment period, four-digit observations and the harmonization process significantly reduce the number of industry observations relative to the short, disaggregated panel.

8. I supplement digitized MMS statistics with early machine-readable MMS data (1977–1986).

IV.B. Defining Treatment

I define treated or targeted industries as those appearing in major industry legislation. Section II described the industry scope of the HCI drive, which was built from six major sectoral acts. For sectors like shipbuilding, aggregate sectors from the acts and census industries are closely aligned. However, care is required for more complex industries, such as chemicals, and the relevant legislation. Consequently, I hand-match the industries in the legislation to the harmonized data, both the long and short panels. This process entails matching industry labels in legislation to industries in the five-digit KSIC industry codes. See Online Appendix 1 for legislation and matching.

IV.C. Linkages

Interindustry linkage data are constructed from the Bank of Korea's 1970 "basic" IO tables, which I digitized. These are the most disaggregated tables for the period, covering approximately 320 sectors. I used these tables to create the measures of exposure to industrial policy through linkages, which I detail in Online Appendix E.1. However, the Bank of Korea data and MMS surveys use different coding schemes. Thus, combining IO accounts with industry data requires further harmonization (see Online Appendix I.2).

IV.D. Trade Flows and Trade Policy

I also use international trade-flow and trade policy data. The "long" four-digit industry panels are hand-matched to the Standard International Trade Classification (SITC, rev.1) four-digit-level trade data. The trade-flow data come principally from the UN Comtrade database. For trade policy, the product-level measures of quantitative restrictions (QRs), for example, tariffs, are digitized from Luedde-Neurath (1988) and connected to modern nomenclatures. These data are available for 1968, 1974, 1976, 1978, 1980, and 1982, representing the most disaggregated, readily available data for the period (Westphal 1990). These statistics contain measures of core nontariff barriers, notably QRs. Most empirical studies of Korean trade policy use highly aggregated data. In terms of QRs, Luedde-Neurath (1988) codes the severity of restrictions from least to most severe (0 to 3).

I use trade policy data to calculate separate measures for output and input market protection exposure. Output protection for industry i is simply the average tariff (or quantitative restriction) score for that sector: output-tariff$_i$. Heavy industry policy also used exemptions from import barriers as a policy tool, and I calculated measures of input protection. Input tariffs (QRs) faced by industry i are calculated as the weighted sum of tariff (QR) exposure (Amiti and Konings 2007): for example, input-tariff$_i$ = $\sum_j \alpha_{ji} \times$ output-tariff$_j$, where α_{ji} are cost shares for industry i and input j. Cost weights come from the 1970 IO accounts.

V. The Main Effects of the HCI Drive

This section considers the empirical effect of the HCI drive in three parts. First, I introduce the main estimation strategy (Section V.A), which I use to identify how HCI targeting corresponds to industrial development (Section V.B). Then I report estimates of the average impact of the policy over time and consider estimates from the double-robust DD estimator (Section V.E). Finally, I use a DDD estimation strategy to study the effect of the heavy industry drive using cross-country variation (Section V.F).

V.A. Estimation

To estimate the impact of the industrial policy, I use the temporal and sectoral variation from the HCI drive to employ a DD strategy. I take the January 1973 announcement of HCI as the start date and the assassination of President Park in 1979 as the de facto end date. I compare differences between the set of targeted manufacturing industries versus the set of non-targeted manufacturing industries relative to 1972. I collect data on the industries until 1986, the year before Korea's formative democratic transition. I consider the following baseline specification:

$$\ln(y_{it}) = \alpha_i + \tau_t + \sum_{j \neq 1972} \beta_j \cdot \left(\text{Targeted}_i \times \text{Year}_t^j\right)$$
$$+ \sum_{j \neq 1972} X_i' \times \text{Year}_t^j \Omega_j + \epsilon_{it}, \tag{1}$$

where y_{it} are (log) industrial development outcomes, i indexes each manufacturing industry, and the year is denoted by t, which takes the values 1967–1986 for the long panel and 1970–1986 for the short panel. Equation (1) is a linear TWFE specification with industry fixed effects α_i and time effects τ_t. I cluster standard errors at the industry level, allowing for within-industry correlation. I also estimate equation (1) using pretreatment variables to control for unobserved productivity correlated with the intervention, including pre-1973 industry averages: total intermediate outlays (material costs), average wage bill (total wage bill per worker), average plant size (employment per plant), and labor productivity (value added per worker). Values are all in real terms and are in logs. Since the pretreatment averages X_i' are time-invariant, I interact them with year effects to estimate their impact over time.

The effect of the industrial policy drive is estimated using a binary variable Targeted_i, which is equal to one for a treated industry and zero otherwise (for assignment, see Section IV). The set of β_j's is the differences between targeted and non-targeted industries for each year j relative to the pretreatment year 1972, and coefficients for 1972 are normalized to zero. The binary treatment term allows me to visually assess counterfactual dynamics and pre-trends. I also compare TWFE estimates from equation (1) to the double-robust DD estimators below (Section V.E).

The coefficients of interest, β_j, convey three aspects of how targeted sectors evolved. First, estimates after 1972 describe the average impact of the targeting for each period after the start of the HCI drive. If the industrial policy is associated with short-term industrial development during the six-year drive, we should observe increasing differences in y_{it} between 1973 and 1979.

Second, estimates after 1979 describe the long-term effects of the industrial policy drive. In the parlance of the industrial policy literature, their longevity indicates the potential dynamic effects of industrial policy. This evolution may be realized through dynamic economies of scale (Section III). Even where differences stabilize in the later period, this may coincide with a permanent shift in levels of development between the two types of industries.

Third, estimates before 1972 describe average differences between targeted and non-targeted industries before the policy. Thus, they convey information about the common-trend assumption of the research design. Before 1972, we should not observe systematic differences between treated and control industries: $\hat{\beta}_{1967} \approx \hat{\beta}_{1968} \approx \hat{\beta}_{1972} \approx 0$. For key analyses, I report the full tables and plotted estimates, including full tests for the joint significance of pre-trends.[9]

9. Although null results provide information about DD pre-trend assumptions, they cannot validate the pre-trend assumption alone or do so decisively.

Ultimately, the goal of specification (1) is to understand the impact of the industrial policy package on treated industries or the ATT. This estimand is particularly relevant for industrial policy, where policy makers are often interested in the effect of a policy on targeted units rather than the average unit in an economy (ATE); as such, the ATT requires different, less stringent assumptions.

Theoretically, sectoral industrial policies often target industries that are most responsive to the relevant legislation or that idiosyncratically gain from interventions. In our setting, targeted industries may be those expected to respond the most to policies, for example, by having stronger dynamic economies of scale. For estimating the ATT, the common-trend assumption accounts for this issue under certain assumptions: if selection does not change over time (irrespective of policy), the common-trend assumption addresses this form of selection between targeted and non-targeted industries (Heckman et al. 1998; Blundell and Dias 2009). In other words, if the selection bias remains unchanged between the sectors at the time of treatment, then parallel trends remove unobserved idiosyncratic gains from estimates. This assumption is violated if unobserved factors such as productivity are expected to accelerate in targeted industries, regardless of treatment. Recall, however, that Section II documented how allies and foreign lenders believed that South Korea could not cultivate dominance in heavy industries without intervention. Nevertheless, the assumptions above mean that estimating the impact (ATT) of the policy drive requires a proper control group. To this end, the treatment-effects literature has emphasized the power of alternative estimators and reweighting methods (Heckman et al. 1998; Smith and Todd 2005).

I consider alternative estimation procedures and build on my baseline TWFE estimator for equation (1) in two ways. First, I use a double-robust DD estimator—a method that reweights observations in the control group through their propensity score and adjusts the counterfactual outcome using a linear regression model. Second, I estimate the takeoff of Korean targeted industries using cross-country and cross-industry variation and use a DDD estimation strategy. This strategy attempts to directly address the

This situation is particularly true for more detailed five-digit estimates, which have limited pretreatment periods.

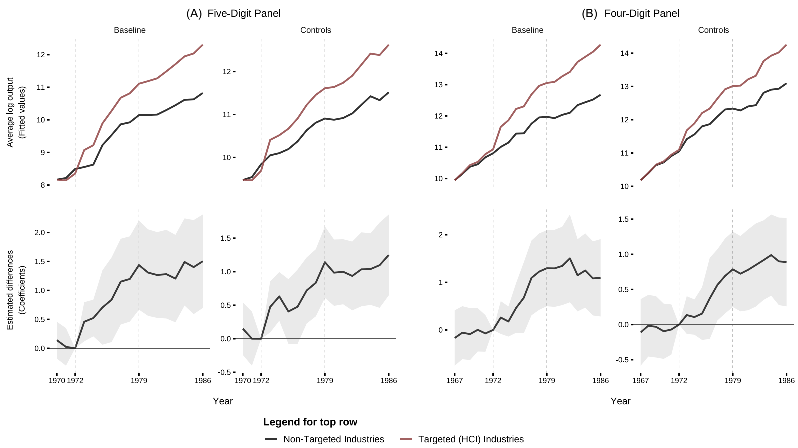

FIGURE II

Industrial Policy and Industry Output

This figure shows the dynamic DD estimates for the relationship between HCI and output, measured as (log) real value of gross output shipped. Coefficients in the plot are estimated using equation (1). The bottom row shows dynamic DD estimates: Panel A corresponds to estimates for the detailed (short) five-digit level panel. Panel B corresponds to estimates for the aggregate (long) four-digit level panel. Baseline columns are baseline two-way fixed effects regressions, and Plus Controls columns include pretreatment controls. The top row shows the predicted outcomes of the fitted model to show group-specific trends; lines correspond to predicted values for treated and control industries for each point before and after 1972. For specifications with controls, predictions use the mean values of the controls. All estimates are relative to 1972, the year before the HCI policy. 1979 demarcates the end of the Park regime. Standard errors are clustered at the industry level. Ninety-five percent confidence intervals are shown in gray.

issues discussed above by comparing Korean industries to similar international industries. First I consider the baseline estimates.

V.B. *Direct Impact on Industrial Development: Results*

1. *Key Patterns and Output Expansion.* Figure II plots baseline dynamic DD estimates for the effect of HCI on output, measured as real value shipped. Panel A provides estimates for the detailed (short) five-digit panel, which starts in 1970. Panel B presents estimates for the more aggregated (long) four-digit panel, which started in 1967. The left columns give estimates from the baseline fixed-effect specifications, and the right columns show estimates with controls. The top row of each panel in Figure II presents the average log output for targeted

(red/gray) and non-targeted (black) industries using the fit model from equation (1). The bottom row presents the traditional DD plots of the estimated differences between the two industries.

Figure II delivers three key patterns of industrial development associated with the policy drive, and these patterns reappear across outcomes throughout this study. First, Figure II shows that output from targeted and non-targeted industries evolved similarly over the pre-HCI period (1967–1972). This result is clearest in the longer aggregate four-digit panel, and pre-period coefficients are individually and jointly insignificant (Online Appendix Table B.1).

Second, Figure II shows that marked differences between treated and non-treated sectors emerged after the 1973 intervention. These differences widen and become stark over the policy period. This divergence is most pronounced in estimates for the five-digit data in Panel A. Panel B reports a similar, though less precise, divergence in aggregate four-digit data. The top row of Figure II also shows that the estimated differences (bottom) are not driven by the decline in the control industries. This finding is useful because differences between treated and non-treated industries may emerge if policies harm industries in the control group (e.g., Cerqua and Pellegrini 2017), for instance, if policy crowds out investment for other manufacturing industries. I explore this issue in Section VI.A.

Third, the effects of the drive were not transitory. In terms of real output, in Figure II, the gap between treated and non-treated industries persists throughout the post-1979 period. The top row of Figure II also reveals that even though differences stabilize or diminish, the level effects are sticky. The patterns in Figure II are also robust and seen across alternative measures of log output, data sets (four- versus five-digit panels), and specifications in Online Appendix Figure B.1.

2. *Industrial-Development Outcomes.* Figure III presents the effect of the HCI policy across various industrial-development outcomes. Panel A illustrates that the policy drive coincided with a significant increase in simple measures of labor productivity (log real value added per worker) and relatively lower (log) output prices. Like the estimates above, five-digit data estimates are more precise than aggregate four-digit ones. Note that these estimates are not driven by a decline in prices for heavy industry.

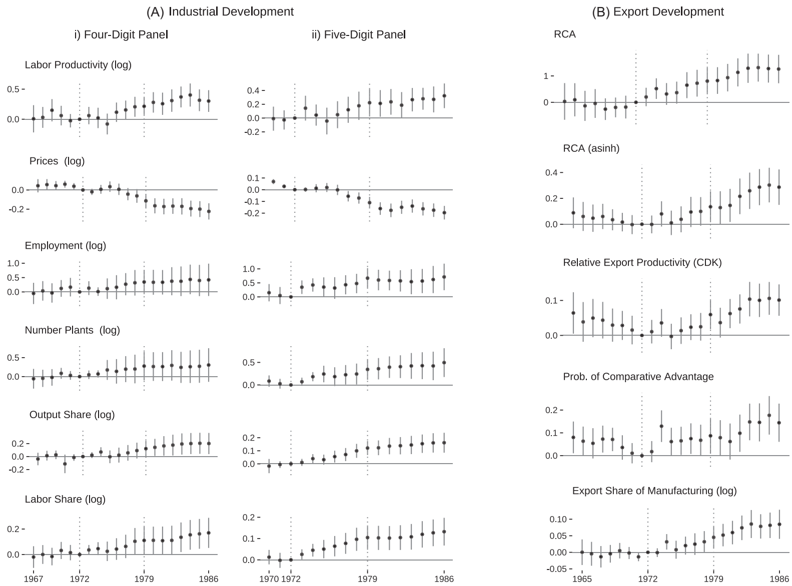

FIGURE III

Industrial Policy: Industrial and Export Development

This figure shows the dynamic DD estimates for the relationship between HCI and industrial development (log) outcomes (Panel A) and export development outcomes (Panel B). Coefficients in the plot are estimated using equation (1). Panel A: Left are estimates from long-panel data (four-digit), right are estimates from detailed short-panel data (five-digit). Panel A reports estimates for log outcomes: total employment; labor productivity (real value added per worker); output prices; number of plants; and output (labor) share is industry's share of total manufacturing output (employment). Panel B presents outcomes for trade data. RCA is the plain Balassa index, estimated using PPML; all other trade outcomes are estimated using OLS. RCA (asinh) is transformed using inverse hyperbolic sine. Relative export productivity is structurally estimated using CDK. The probability of reaching comparative advantage is defined as cases where the RCA index > 1. All estimates are relative to 1972, the year before the HCI policy. 1979 demarcates the end of the Park regime. Standard errors are clustered at the industry level. Ninty-five percent confidence intervals are shown in gray.

Online Appendix B.1 shows that heavy industry prices increased less than other industries over the inflationary 1970s.

Figure III, Panel A also demonstrates that the policy coincided with a shift in the share of total manufacturing activity toward targeted industries. The log manufacturing share of output and the log employment share both increase for the targeted industry. Moreover, this reallocation of manufacturing activity toward the heavy and chemical industry is durable. Estimates are less precise for aggregate data. Online Appendix Table B.2 jointly rejects pre-trends. In addition, Figure III shows a rise in the number of plants operating in HCI markets.

V.C. Direct Impact on Export Development

Export performance provides another view of industrial development, and exports were central to the policy program, as it was for earlier iterations of South Korean industrial policy. For instance, a distinct goal of the HCI drive was that heavy-chemical products would constitute 50% of exports by 1980 (Hong 1987; World Bank 1987). Figure III, Panel B reports the effect of industrial policy on export-development outcomes, now using SITC (rev. 1) trade-flow data, which are substantially more disaggregated than harmonized industry data.

The analysis in Panel B considers multiple measures of export development. First, I calculate a traditional measure of revealed comparative advantage (e.g., Balassa 1965) for each industry. The RCA (Balassa) index is defined as the ratio of Korea's export share of good k relative to the world's export share of commodity k:

$$\mathrm{RCA}_k = \frac{\left(\frac{X_k^{\mathrm{Korea}}}{X_{\mathrm{Total}}^{\mathrm{Korea}}}\right)}{\left(\frac{X_k^{\mathrm{World}}}{X_{\mathrm{Total}}^{\mathrm{World}}}\right)},$$

where X denotes the value of exports. Korea has a comparative advantage in k when RCA_k is larger than one.

In addition, I estimate the relative export productivity (CDK) using the gravity model methods proposed by Costinot, Donaldson, and Komunjer (2012). Their estimate provides a theoretically consistent measure of revealed comparative advantage beyond the classic calculations. For industry k, I estimate relative export productivity for country i, where $\widehat{\mathrm{CDK}}_k = \exp(\frac{\delta_{ik}}{\hat{\theta}})$; the trade elasticity $\hat{\theta}$ is taken from Costinot, Donaldson, and Komunjer (2012). The δ_{ik} term is the exporter-commodity fixed effect from the bilateral trade regression, $\ln(X_{ijk}) = \delta_{ij} + \delta_{jk} + \delta_{ik} + \epsilon_{ijk}$, where X are exports, i is an exporter, j is an importer, and k is a commodity. While the traditional RCA measure accommodates zero trade flows, CDK is estimated from nonzero trade flows and takes positive nonzero values.

Across measures of export development, Figure III, Panel B depicts a strong positive relationship between industrial policy and treatment. For the classic RCA index, I use Poisson pseudo-maximum likelihood (PPML), given the prevalence of zeros. I also provide linear estimates using transformed RCA for completeness. Panel B shows a consistent pattern: after 1973, there was a marked rise in the relative RCA and the share of manufacturing exports for targeted SITC industries. Furthermore, the probability of attaining comparative advantage grew markedly after 1973. Second, before 1973, pre-trends were absent across trade outcomes, except for RCA, which trended downward. Third, estimates grew and became highly significant in the post-policy period. Hence, RCA emerged during the drive and was fully articulated after the policy period. The ascent of heavy-chemical exports is also shown below (Section V.F) using cross-country trade data.

V.D. Direct Impact: Robustness

1. *TFP: Plant-Level Persistence and Industry-Level Trends.* Section V.B presented indirect productivity measures. I now turn to TFP. However, features of the data and the historical context pose constraints for estimating TFP (e.g., microdata availability). Nevertheless, I consider the persistence of plant-level TFP using microdata (which are available after 1979). Specifically, I study the correlation between targeting and plant-level TFP after the end of HCI in 1979 using a simple pooled panel regression:

$$\text{TFP}_{pit} = \alpha_{it} + \beta \text{Targeted}_p + \epsilon_{pit}, \tag{2}$$

where p denotes plant, and t are years after 1979. The term Targeted_p indicates plants operating in industries targeted by the HCI drive. Given that treatment is time invariant, I include (four-digit) industry-year effects, α_{it}. For completeness, I consider multiple estimates of TFP_{pit} (Olley and Pakes 1996; Levinsohn and Petrin 2003; Wooldridge 2009; Ackerberg, Caves, and Frazer 2015). I use two-way clustered standard errors to allow for within-industry and plant correlation.

Table I reports the relationship between plant-level productivity and plants in treated heavy industry. For the period immediately after the HCI drive (1980–1986), treated establishments have significantly higher TFP than do non-treated establishments. Across specifications and measures of TFP, estimates in Table I are significant and imply that heavy industry plants in the 1980s have 0%–6% higher productivity than non-targeted plants.

TABLE I

DIFFERENCES IN PLANT-LEVEL TOTAL FACTOR PRODUCTIVITY, BY TREATMENT STATUS (1980–1986)

	Outcomes: Total factor productivity				
	(1)	(2)	(3)	(4)	(5)
Targeted	0.043***	0.010	0.054***	0.042***	0.003
	(0.015)	(0.014)	(0.015)	(0.014)	(0.013)
Industry × Year	Yes	Yes	Yes	Yes	Yes
R^2	0.368	0.446	0.342	0.234	0.117
Observations	272,150	272,150	272,150	272,150	272,150
Two-way cluster (industry plant)	488 × 91,094	488 × 91,094	488 × 91,094	488 × 91,094	488 × 91,094
Estimation type (TFP)	W	ACF	LP	OP	OLS

Notes. This table shows the relationship between plant-level TFP and HCI (targeted industries) for the post-HCI period (1980–1986). TFP is estimated using Ackerberg-Caves-Frazer (ACF), Levinsohn-Petrin (LP), Olley-Pakes (OP), and Wooldridge (W) methods.

TFP is estimated using a log-transformed, value-added production function. I include TFP estimated using OLS as a baseline estimate. The Targeted indicator is defined by the plant's main industry. All regressions control for year-by-industry (four-digit level) fixed effects. Regressions use two-way clustered standard errors at the plant and industry levels. Standard errors are in parentheses. * $p < .10$; ** $p < .05$; *** $p < .01$.

These correlational results are compatible with the industry-level dynamics shown in Section V.B.

Next I turn to industry-level dynamics using aggregate TFP, which I present in Online Appendix 1. These industry-level estimates also reveal a gentle upward trend in total productivity for targeted industries relative to non-targeted industries. Differences in productivity became significant after 1979. This upward trajectory is compatible with the relatively high TFP in the cross section of post-1979 heavy industry plants (Table I). For further robustness, Online Appendix B.1 provides dynamic estimates for plant-level TFP, showing gentle upward trends over the limited post-1979 period. Together, the industry and plant-level estimates appear consistent with the slow emergence of policy effects. Perhaps equally important, I do not find a salient relative decline in TFP for the treated industries, which may be commonly associated with poorly performing industrial policy.

2. *Continuous Treatment and Limited "Horizontal" Spillovers.* For robustness, Online Appendix J.1 explores the patterns of industrial development using a more continuous industry-level measure of exposure to HCI. This measure captures the extent to which plants in HCI product markets produce output in other (non-HCI) markets. Dynamic estimates using this continuous measure (Online Appendix Figure B.2) track the binary estimates in Section V.B. Broadly, however, multiproduct plants in heavy industry tend not to produce significant output in control industries. Consequently, there is minimal variation in this type of continuous measure and limited potential for this form of horizontal spillover.

V.E. Direct Impact: Double-Robust DD and Average Effects

1. *Double-Robust Estimator.* I now use the double-robust DD estimator proposed by Sant'Anna and Zhao (2020) and Callaway and Sant'Anna (2021). This allows me to consider the policy bundle's overall effect (ATT) and provides a robustness check on the TWFE estimates. In particular, this procedure relaxes some of the constraints of the traditional DD estimators and coherently adjusts counterfactuals. I consider the following

specification:

$$\mathrm{ATT}_t = \mathbf{E}\left[\frac{\text{Targeted}}{\mathbf{E}\left[\text{Targeted}\right]} - \frac{\frac{\pi(X)(1-\text{Targeted})}{1-\pi(X)}}{\mathbf{E}\left[\frac{\pi(X)(1-\text{Targeted})}{1-\pi(X)}\right]}\right](Y_t - Y_{1972})$$
$$- f_{0,Y_t-Y_{1972}}(X), \tag{3}$$

which refers to the weighted average differences in industry outcomes. More precisely, equation (3) is the difference in outcomes between targeted industries (Targeted) and non-targeted industries ($1 -$ Targeted). Weights in equation (3) are defined as follows (Sant'Anna and Zhao 2020; Callaway and Sant'Anna 2021): the term $\pi(X) \equiv \mathbf{E}[\text{Targeted}|X]$ is the propensity score for the treated industries. The term $f_{0,Y_t-Y_{1972}}(X) \equiv \mathbf{E}[Y_t - Y_{1972}|\text{Targeted} = 0, X]$ is a regression for the change in outcomes for non-treated industries between post-period t and the baseline, pretreatment period, $t = 1972$. Propensity scores $\pi(X)$ and regression $f_{0,Y_t-Y_{1972}}(X)$ are estimated by logit and OLS, respectively. The estimator in equation (3) is doubly robust in that if either component is correctly specified, it provides a consistent estimate of the ATT.

The double-robust estimator ensures a balance between targeted and non-targeted industries. The two-step procedure relaxes some of the functional-form assumptions of the evolution of potential outcomes. The pre-trend assumptions are also less stringent than those of other DD estimators. The average effects in equation (3) do not rely on zero pre-trends over all pretreatment periods, instead using a long-difference (between postperiod t and the last pretreatment period, 1972). Confidence intervals for equation (3) are calculated using a bootstrap procedure, which allows industry-level clustering (Callaway and Sant'Anna 2021). I use the same controls as the TWFE estimates above. Note that equation (3) requires a binary treatment and is not used for cases of continuous treatment, such as the indirect analysis in Section VII.

2. *Results: Average Effects.* I first consider the overall average impact of the policy before and after 1972. Table II reports the ATTs, comparing double-robust and OLS estimates. Columns (1) and (3) list the doubly robust results, and columns (2) and (4) list the linear TWFE results. Because the double-robust estimator uses controls, I compare them only with TWFE estimates using

TABLE II

THE AVERAGE EFFECT OF INDUSTRIAL POLICY: INDUSTRIAL DEVELOPMENT

	Five-digit panel		Four-digit panel	
Outcomes (log)	Double robust (1)	TWFE (2)	Double robust (3)	TWFE (4)
Output (shipm.)	0.8378***	0.8235***	0.5923***	0.5452**
	(0.1764)	(0.1846)	(0.217)	(0.2223)
Value added	0.7426***	0.7292***	0.5063**	0.4586**
	(0.1696)	(0.1742)	(0.1973)	(0.209)
Gross output	0.8383***	0.8236***	0.5962***	0.5481**
	(0.1718)	(0.1852)	(0.2033)	(0.2217)
Employment	0.504***	0.4972***	0.2941	0.2679
	(0.1451)	(0.1509)	(0.204)	(0.1915)
Prices	−0.1002***	−0.1012***	−0.1154***	−0.1152***
	(0.0207)	(0.0205)	(0.0305)	(0.0304)
Labor prod.	0.1608**	0.1548**	0.1602**	0.1371*
	(0.0654)	(0.068)	(0.0688)	(0.0829)
Output share	0.0996***	0.0993***	0.1072*	0.097
	(0.0258)	(0.0261)	(0.0551)	(0.0599)
Labor share	0.0979***	0.0967***	0.1254**	0.116**
	(0.0284)	(0.028)	(0.0534)	(0.0495)
Num. plants	0.297***	0.2908***	0.1986	0.1831
	(0.0977)	(0.1018)	(0.1419)	(0.1549)

Notes. This table shows the ATT for industrial policy. Average DD estimates are shown for double-robust and TWFE estimators. Outcomes are log: output is the real value of gross output shipped (shipments), alongside other measures of real output: value added and gross output. Employment is the total number of workers. Prices are industry output prices. Labor prod. is real value added per employee. Output share is the manufacturing share of industry output. Labor share is the manufacturing share of industry employment. Specifications include controls for pre-1973 industry averages (log): average wages, average plant size, intermediate outlays, and labor productivity. Standard errors, clustered at the industry level, are in parentheses. Double-robust DD estimates come from equation (3). Double-robust estimators use bootstrapped standard errors (10,000 iterations) and are adjusted to allow for within-industry correlation. * $p < .10$; ** $p < .05$; *** $p < .01$.

controls. Estimates are presented for the five-digit and four-digit panels.

The estimates in Table II reveal that the overall average effect of HCI targeting was meaningful and significant. The preferred estimates in column (1) indicate 128% growth in output for targeted manufacturers relative to non-targeted manufacturers.[10] Similarly, linear TWFE estimates in column (2) suggest 124% output growth, significant at the 1% level. The average effect on labor productivity (column (1)) translates into a 17.2% increase in value added per worker for targeted industries after 1973. Labor productivity growth ranges from 14.3% to 17.2%

10. Calculated using $100 \times \left(\exp\left(\hat{\beta} - 0.5 \times (\mathrm{SE})^2\right) - 1\right)$.

TABLE III
THE AVERAGE EFFECT OF INDUSTRIAL POLICY: EXPORT DEVELOPMENT

	Type of Estimator		
Outcomes	Double robust (1)	TWFE (2)	PPML (3)
RCA	0.4853***	0.4701***	0.9142***
	(0.1841)	(0.1806)	(0.26)
RCA (log)	0.1251***	0.1192***	0.5939***
	(0.0419)	(0.042)	(0.1535)
RCA (asinh)	0.1633***	0.1557***	0.6059***
	(0.0513)	(0.0537)	(0.1577)
RCA (CDK)	0.0502***	0.0498***	0.0302
	(0.0182)	(0.0176)	(0.0189)
Prob. comparative adv.	0.1057***	0.1021***	0.6486***
	(0.0281)	(0.0307)	(0.1945)
Export share	0.071**	0.0727**	0.8346***
	(0.0299)	(0.0293)	(0.2582)
Export share (log)	0.0481***	0.048***	0.7658***
	(0.0145)	(0.0147)	(0.1991)
Export share (asinh)	0.0596***	0.0599***	0.7998***
	(0.0196)	(0.0191)	(0.2166)

Notes. This table shows the ATT for industrial policy. Average DD estimates are shown for double-robust, PPML TWFE, and linear TWFE estimators. RCA is the standard Balassa index measure of revealed comparative advantage. RCA (CDK) is relative productivity estimated using CDK. See the text for their calculation. The indicator $\mathbb{1}[\text{RCA} > 1]$ is a binary dummy variable equal to one when an industry has achieved comparative advantage, zero otherwise. I also show transformed versions of RCA (asinh and log). Specifications include controls for pre-1973 industry averages (log): avg. wages, avg. plant size, intermediate outlays, and labor productivity. Standard errors, clustered at the industry level, are in parentheses. Double-robust DD estimates come from equation (3). Double-robust estimators use bootstrapped standard errors (10,000 iterations) and are adjusted to allow for within-industry correlation. * $p < .10$; ** $p < .05$; *** $p < .01$.

across four-digit and five-digit panels. Table II shows relatively lower prices, implying prices were −9.55% (column (1)) lower relative to other industries over the period. The average employment effects of HCI in Table II are also substantial. Preferred double-robust DD estimates imply a 63.8% increase in employment in column (1), or a 31.4% increase for four-digit data in column (3). The reallocation of labor share is also positive and significant across specifications.

Table III shows substantial development in the heavy export industry. These results are significant and similar across measures of export development. Before 1973, the mean RCA index for targeted sectors was 0.4, whereas the average RCA for non-targeted Korea was 1.27 (refer to Online Appendix Table A.1). Table III, column (1) reports a significant increase in (log) RCA. These estimates translate into a 13.2% rise in RCA for targeted industry products. Column (1) implies that targeted industries saw a 10.6 percentage point increase in the probability of attaining comparative advantage, or, alternatively, a 4.92% increase in the (log) share of manufacturing exports (over non-targeted sectors).[11] The grand export target of the original heavy industry plan (50% of manufacturing exports) was surpassed by 1983 (Kim and Leipziger 1993; Cho and Kim 1995).

3. *Results: Robust Dynamic Estimates.* Double-robust event-study estimates show similar patterns to the direct effects discussed in Section V.B. Online Appendix 2 records and provides the dynamic DD estimates using the reweighting estimator. Here, the patterns (Online Appendix Figures B.4–B.6) are qualitatively similar to the linear TWFE estimates (Section V.B), although the double-robust DD relaxes some assumptions relative to the traditional TWFE DD. The general dynamic pattern associated with HCI is robust across estimators. Do these same patterns hold when using cross-country variation? I turn to this next using a DDD estimation strategy.

V.F. Direct Effect on Trade Development: Cross-Country Evidence

1. *Cross-Country Variation and DDD Estimation.* How did heavy-chemical industries in South Korea fare relative to the rest of the world? Cross-country data allow me to move beyond the within-country comparisons. I use a DDD estimation strategy to expand on the DD analysis—intuitively, I compare the original DD estimates between HCI and control manufacturers in Korea to placebo DDs across international markets. I start with the following baseline specification:

$$\begin{aligned} Y_{ict} = \alpha_i + \tau_t + \sigma_c + \sum_{j\neq 1972} \beta_{1j} \cdot \left(\mathrm{HCI}_i \times \mathrm{Year}_t^j\right) \\ + \sum_{j\neq 1972} \beta_{2j} \cdot \left(\mathrm{Korea}_c \times \mathrm{Year}_t^j\right) \\ + \sum_{j\neq 1972} \beta_{3j} \cdot \left(\mathrm{Korea}_c \times \mathrm{HCI}_i \times \mathrm{Year}_t^j\right) + \epsilon_{ict}, \end{aligned} \tag{4a}$$

11. The World Bank calculated that for HCI industries, the export share of output tripled during the period (Kim and Leipziger 1993; Cho and Kim 1995).

where c denotes country, i denotes industry, and t denotes time. I estimate equation (4a) using cross-country trade data (based on the SITC four-digit level). I focus on the triple interaction, $\text{Korea}_c \times \text{HCI}_i \times \text{Year}_t$, where Korea_c is a dummy indicator for Korean observations. The simplest specification (4a) includes industry, time, and country effects: α_i, τ_t, and σ_c, respectively. However, using cross-country trade data allows me to control for a rich set of higher-dimensional fixed effects. Hence, I also consider a more stringent specification:

$$Y_{ict} = \alpha_{it} + \tau_{ct} + \sigma_{ci} + \sum_{j \neq 1972} \beta_{3j} \cdot \left(\text{Korea}_c \times \text{HCI}_i \times \text{Year}_t^j\right) + \epsilon_{ict}, \tag{4b}$$

which controls for aggregate industry-year shocks (α_{it}), aggregate country-year shocks (τ_{ct}), and time-invariant country-by-industry factors (σ_{ic}). The effects in equation (4b) thus subsume the $\text{Korea}_c \times \text{Year}_t$ and $\text{HCI}_i \times \text{Year}_t$ interactions from equation (4a).

DDD estimates (equations (4a) and (4b)) capture the effect of Korea's industrial policy on industrial development. The coefficients of interest are β_{3j}, estimated from the three-way interaction term $\text{Korea}_c \times \text{HCI}_i \times \text{Year}_t$. In effect, I compare the conventional DD for Korea to placebo DDs over the same period. The identifying assumptions of DDD require differences in targeted and non-targeted outcomes for Korea to have trended similarly to differences in targeted and non-targeted industries (elsewhere) before the intervention.[12] DDD estimates use two-way standard errors clustered at the industry and country level. I follow the empirical trade literature and estimate DDD specifications using PPML (Santos Silva and Tenreyro 2006) for RCA outcomes, given the preponderance of zeros. I also show alternative transformations and estimators for completeness.

2. *Results: Cross-Country Trade Development.* Figure IV presents the DDD estimates for the effect of Korean HCI on comparative advantage. The panels plot the coefficient from the interaction $\text{Korea}_c \times \text{HCI}_i \times \text{Year}_t$. I present multiple specifications: one using individual country, year, and industry effects; one us-

12. Note that the difference between two biased DD estimators is considered unbiased when the bias is similar in both (Olden and Møen 2022).

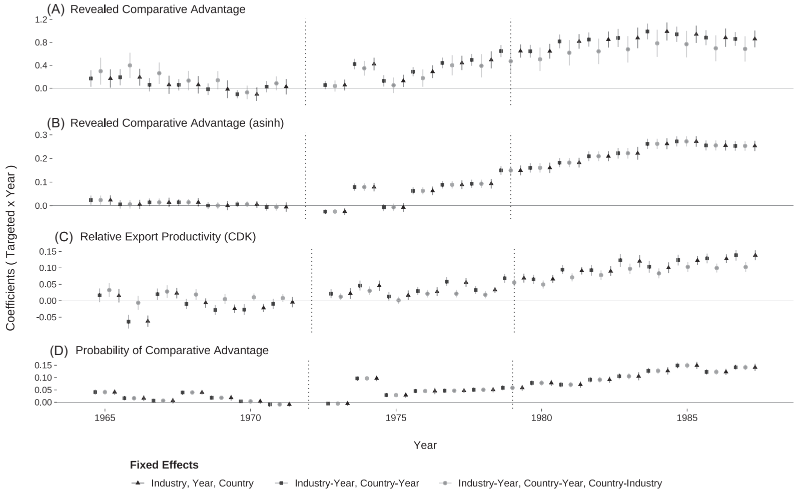

FIGURE IV

Cross-Country Triple Differences and Export Development

This figure plots DDD estimates for the effect of the Korean HCI drive using SITC-level trade data. Specifically, plots show the interaction, Korea × Targeted × Year, estimated from equations (4a) and (4b). Fixed effects are shown in the legend. RCA (Balassa) specifications are estimated using PPML. Alternatively, RCA is transformed using inverse hyperbolic sine to accommodate zeros and is estimated using OLS. Relative export productivity (CDK) specifications are estimated using OLS. Estimates are relative to 1972, the year before the HCI policy intervention. The line at 1979 demarcates the end of the Park regime. All specifications use two-way clustering at the country and industry level. Ninety-five percent confidence intervals are shown.

ing industry-year and country-year effects; and one that adds additional country-industry effects.

The DDD estimates in Figure IV show a substantial policy effect on Korean heavy industry exports across trade outcomes. These outcomes include RCA (standard and normalized inverse hyperbolic sine), measures of CDK, and the probability of achieving comparative advantage. RCA estimates use PPML to accommodate zeros; all others use OLS. The four plots in Figure IV show similar patterns across the export development measures: muted differences before 1973 and a post-1973 shift in comparative advantage for the targeted Korean industry, which continued to ascend after the end of the drive.

These cross-country patterns are robust even when using alternative, aggregate industry data, which I show in Online

Appendix K. Online Appendix Figure C.2 also shows a DD version of Figure IV, comparing only targeted Korean industries to targeted placebo industries in "non-treated" counties. These results show a qualitatively similar pattern to those in Figure IV.

How unusual is it for a country to cultivate an export advantage in targeted industries? Perhaps it is inevitable that countries naturally cultivate a comparative advantage in heavy or chemical industries. I assess the probability that an HCI achieves comparative advantage on the world market, $p(\text{RCA} > 1)$, in Korea versus foreign controls in Online Appendix C. Online Appendix Table C.1 shows that Korea had a significantly higher probability (between 11% and 13.1%) of achieving comparative advantage in HCI products after 1972 when compared with countries with similar levels of development (OLS estimates in Online Appendix Table C.1).

V.G. Direct Impact: Discussion

The empirical relationship between industrial policies and industrial development is not a foregone conclusion. For many reasons, we may anticipate a negative relationship between an industrial policy and development outcomes (see Harrison and Rodríguez-Clare 2010; Lane 2020; Juhász, Lane, and Rodrik 2024). Historically, there is no shortage of failures (Pack 2000). I showed a positive relationship between an industrial policy and industrial-development outcomes from output growth to export development. The impact of these policies is seen throughout the HCI period (1973–1979) and continued through the 1980s. These results are robust across data sets (short-term and long-term panels), the type of estimator (TWFE versus double-robust), and cross-country variation (DDD). Next I turn to the forces underlying these results.

VI. Policy and Mechanisms

VI.A. Policy: Credit Expansion, Investment, and Input Use

The HCI drive aimed to promote investment and expand the sector through directed credit and investment incentives. This section examines the role and effects of these policies. However, observing an explicit effect of these policy levers is challenging. Industrial statistics rarely capture such policy details, and these issues of observability are common in studies of industrial policy (Kalouptsidi 2018; Juhász et al. 2025). The HCI drive is

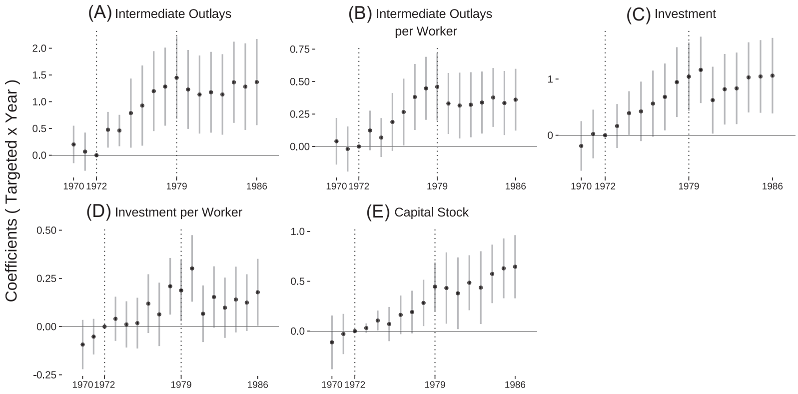

FIGURE V

Changes in Input Use and Investment

This figure plots dynamic DD estimates for responses to investment incentives. The coefficients in the plot are estimated using equation (1). All outcomes are real log values: real total intermediate outlays (material costs), intermediate outlays per worker, total investment, investment per worker, and capital stock. Panels report baseline estimates from the five-digit industry panel (1970–1986). The estimates are relative to 1972, the year before the HCI drive. The line at 1979 demarcates the end of the Park regime. Standard errors are clustered at the industry level. Ninety-five percent confidence intervals are shown in gray.

no exception. Given these limitations, I examine indirect outcomes related to investment policy, following approaches in the credit-policy literature (Banerjee and Duflo 2014; Manova, Wei, and Zhang 2015). First, I demonstrate that intermediate outlays and investments responded differentially for treated industries. Then, drawing on Bau and Matray (2022), I show that input use responded in ways consistent with credit policy, specifically in treated sectors.

1. *Baseline Results: Input Use and Policy Variations.* Figure V presents baseline DD estimates (equation (1)) for input-related outcomes at the five-digit level. Panels A and B show that this divergence is starkest for intermediate outlays (total and per worker) beginning in 1973 and widening throughout the drive. Panels C–E report estimates related to investment and capital formation. These estimates grow significantly different between the treated and non-treated industries soon after the start of the industrial policy drive. In addition, I observe similar investment patterns across asset classes, especially those targeted

TABLE IV
THE AVERAGE EFFECT OF INDUSTRIAL POLICY: INPUT USE AND INVESTMENT

	Five-digit panel		Four-digit panel	
Outcomes (log)	Double robust (1)	TWFE (2)	Double robust (3)	TWFE (4)
Intermediate outlays	0.7544*** (0.1878)	0.7408*** (0.1894)	0.5606** (0.2374)	0.5147** (0.2428)
Intermediate outlays (per worker)	0.213*** (0.069)	0.2074*** (0.0719)	0.2823*** (0.0923)	0.2659*** (0.0975)
Investment	0.6198*** (0.2211)	0.6089*** (0.2205)	0.2445 (0.2136)	0.21 (0.2186)
Investment (per worker)	0.134** (0.0596)	0.1316** (0.0612)	0.1124 (0.0855)	0.1048 (0.0901)

Notes. This table shows the ATT for industrial policy. Average DD estimates are shown for double-robust and TWFE estimators. Intermediate outlays (log) are real intermediate input costs. Investment total (log) is real total gross capital formation. I also show outcomes in per worker terms. Specifications include controls for pre-1973 industry averages (log): average wages, average plant size, intermediate outlays, and labor productivity. Standard errors, clustered at the industry level, are in parentheses. Double-robust DD estimates come from equation (3). Double-robust estimators use bootstrapped standard errors (10,000 iterations) and are adjusted to allow for within-industry correlation. * $p < .10$; ** $p < .05$; *** $p < .01$.

by investment incentives (e.g., machinery equipment; see Online Appendix Table D.2).

The increase in intermediate outlays and investment for heavy industry was substantial. Table IV provides double-robust and TWFE DD estimates of the ATT. Preferred estimates in column (1) translate into a 109% relative increase in total intermediate outlays for treated over non-treated manufacturers. DD estimates for materials are highly significant for five-digit data and noisily estimated in four-digit panels. Similarly, for investment, double-robust DD estimates translate into an 81.4% increase in total investment for treated over non-treated industries, as shown in Table IV, column (1). Investment estimates for four-digit data are imprecise, and investment per worker is negative, given the high employment growth. In light of industrial policy history, it is not obvious that we should expect positive effects on investment and input outlays (e.g., if lending leads to crowding out; see Online Appendix D.1).

2. *Policy Mechanisms: Changes in Investment and Input Wedges.* In theory, directed credit should reduce wedges on inputs for the treated industry during the heavy industry drive. Capital market policies that expand credit should disproportion-

ately affect firms with high wedges, and these wedges can be captured through the pretreatment marginal revenue product of capital (MRPK) (Bau and Matray 2022). In other words, a policy should disproportionately affect investment in high-MRPK industries and increase the marginal revenue of other inputs.

I find evidence consistent with investment policy operating in HCI sectors, reducing wedges for high-MRPK industries, specifically among targeted producers. I analyze the differential impact of industrial policy targeting on high-MRPK versus low-MRPK industries, using a basic measure of industry-level MRPK à la Bau and Matray (2022). I present these calculations and details in Online Appendix D.2 and show a marked increase in input use across intermediate inputs (intermediate materials, investment, and labor) for high-MRPK industries relative to low-MRPK industries (see: Online Appendix Figure D.2). Importantly, this change is only seen in targeted industries—no such effect is seen for non-treated industries. These results suggest that credit expansion differentially affected the heavy-chemical sector.

3. *Robustness: Investment and Crowding Out.* Was the policy deleterious for investment in non-targeted industries? Although higher in treated industries, investment did not decline for non-HCI industries. Before formal tests, it is worth considering the evidence in favor of crowding out. Section II demonstrated that although biased toward HCIs, lending continued for the non-treated sectors throughout the drive. Recall that commercial banks continued to lend to non-targeted industries and remained a major source of financial support during the period (see Online Appendix A.2 for details). Trends in non-targeted industry growth support this finding. Recall that Section V.B suggested that the relative ascent of heavy industry was not driven by the absolute contraction of non-targeted industries.

I consider whether investment dynamics were different across treated versus untreated industries. I do so by regressing investment outcomes on time effects separately for each class of industry, as shown in Online Appendix D.3. I find that investment was high in targeted relative to non-targeted industries during the time period, though it generally increased across both sectors. These results are consistent with the patterns of lending and growth already described.

Furthermore, although the heavy industry drive altered investment patterns, it did not reduce absolute investment in non-targeted industries. The analysis in Online Appendix D.3 shows that investment was not crowded out in untreated, capital-intensive industries. Although less striking than investment in heavy industry, investment in the light industry sector continued, as non–heavy industry companies had access to domestic commercial credit and credit from countries like Japan (Castley 1997).

VI.B. Policy: Trade Policy and the Weak Case for Nominal Protection

Scholars have emphasized the role of trade policy, and some have characterized it as overtly protectionist (Lall 1997). Evidence for the latter claim is weak, as shown in Online Appendix D.4. Instead, I posit that targeted cuts on import duties for intermediates could have been advantageous. Using a simple fixed-effects regression and five periods of disaggregated protection data, I find that the average level of output protection was significantly lower for treated versus non-treated industries during the policy drive (see Online Appendix Table D.3, Panel A). Online Appendix Table D.3 also suggests that nominal output protection fell more for non-treated than treated industries. Similarly, assuming that trade policy allowed for discounts on imported inputs (Section II.C), many heavy industrial producers enjoyed significantly lower duties on foreign inputs (Online Appendix Table D.3, Panel B). Together, the evidence does not suggest a surge in overt, nominal protectionism over the period.

VI.C. Mechanisms: Targeted Industry and Learning

Did the industrial policy promote industries with strong learning-by-doing forces? I examine the potential learning-by-doing effects in treated industries. If learning-by-doing forces were at work, we would expect increased cumulative experience to correspond to higher productivity or a lower unit cost. To assess whether learning was particularly strong in treated sectors, I use a simple, reduced-form regression for the post-1972 period. Specifically, I consider the following equation:

$$
\begin{aligned}
Y_{it} = {} & \beta_1 \text{Experience}_{it} + \beta_2 \left(\text{Experience}_{it} \times \text{Targeted}_i\right) + \theta \text{Size}_{it} \\
& + \alpha_i + \tau_t + X_{it}'\Omega + \epsilon_{it}, \qquad (5)
\end{aligned}
$$

where Y_{it} represents industry (or plant) log prices, log unit cost, or TFP, following Gruber (1998), Barrios and Strobl (2004), and

Fernandes and Isgut (2005). I measure unit cost as total intermediate costs per unit of real gross output. Equation (5) examines a reduced-form relationship between these outcomes and log Experience_{it}, measured as real cumulative gross output up to time t. All baseline regressions control for measures of plant size (Size_{it}) to account for conventional scale effects. In addition, I control for the effects of technological progress embodied in input use, X_{it} (e.g., total input outlays, capital intensity). These (log) covariates are normalized by the number of workers to further account for scale effects. I include year effects (τ_t) and industry effects (α_i) in industry-level regressions or plant effects in micro-level regressions.

The correlations in equation (5) indicate potential learning externalities over the policy period. The coefficient β_1 is the general impact of cumulative output (Experience_{it}), and β_2 is the differential effect of Experience_{it} for the treated industries. Hence, estimates from equation (5) examine whether dynamic externalities are present in targeted industries and their strength in treated sectors relative to non-treated sectors (see Beason and Weinstein 1996; Pons-Benaiges 2017). These estimates are indicative and not causal.

First, consider the industry-level estimates of equation (5). Table V, columns (1)–(4) demonstrate that experience is positively related to reductions in prices and unit costs, with the effect being significantly stronger for targeted sectors. Estimates for the interaction $\text{Targeted}_i \times \text{Experience}_{it}$ are negative and highly significant. Similarly, columns (5)–(10) show a positive relationship between experience and productivity using three measures of TFP. In these cases, the correlation between experience and TFP is stronger for targeted industries, with interactions being significant for Levinsohn-Petrin (LP) measures of TFP. Furthermore, the combined effect of experience for treated industries (shown at the bottom of Table V) is strong and significant across all specifications. Online Appendix Table D.1 confirms that these results are robust to alternative measures of experience, unit cost, and TFP.

Second, I analyze microdata to investigate the correlation between learning and targeting after 1979, when microdata first became available. Expanding on equation (5), I regress plant TFP and log unit cost on two types of log cumulative experience: plant-level and industry-level (four-digit), both measured from the beginning of the sample period. All regressions include plant and

TABLE V

INDUSTRY-LEVEL LEARNING BY TREATMENT STATUS

	Prices (log)		Unit cost (log)		Total factor productivity					
					ACF		LP		W	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Experience	−0.011	−0.195***	0.015***	−0.109***	0.182***	0.369***	0.053	0.360***	0.060	0.348***
	(0.008)	(0.029)	(0.004)	(0.014)	(0.065)	(0.059)	(0.083)	(0.060)	(0.081)	(0.064)
Targeted × Experience	−0.042***	−0.044***	0.005	−0.035***	0.014	0.033	0.087**	0.120***	0.094***	0.125***
	(0.008)	(0.012)	(0.007)	(0.009)	(0.031)	(0.022)	(0.039)	(0.025)	(0.036)	(0.024)
Controls for size-scale	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls for capital intensity	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Controls for intermediates	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Controls for investment	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Industry effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.951	0.961	0.845	0.900	0.823	0.877	0.976	0.985	0.983	0.990
Observations	3,890	3,429	3,890	3,429	3,512	3,428	3,512	3,428	3,512	3,428
Clusters	278	263	278	263	264	263	264	263	264	263
Linear combination	−0.053	−0.239	0.020	−0.143	0.196	0.402	0.140	0.480	0.154	0.474
(std. err.)	(0.009)	(0.029)	(0.007)	(0.016)	(0.060)	(0.054)	(0.073)	(0.059)	(0.072)	(0.062)

Notes. This table shows the industry-level relationship between industrial outcomes and (log) experience in targeted versus non-targeted industries. Estimates come from equation (5). The analysis is for the post-1972 period, using the five-digit industry panel. The outcomes are log unit cost (total intermediate costs per unit of real gross output) and TFP, estimated using Ackerberg-Caves-Frazer (ACF), Levinsohn-Petrin (LP), and Wooldridge (W) methods. (log) Experience is measured as cumulative output (the sum of real gross output until the current year). All equations control for size/scale, measured as (log) industry employment and (log) average plant size. Additional controls include log: capital intensity, investment per worker, and intermediate input intensity per worker. Linear combination, at the bottom, gives the combined effects. All specifications are estimated using industry and year fixed effects. Standard errors, clustered at the industry level, are in parentheses. * $p < .10$; ** $p < .05$; *** $p < .01$.

industry-level fixed effects to account for time-invariant factors (micro and sectoral) that influence learning. As before, I control for year effects. I employ two-way clustered standard errors to allow for sectoral and plant-level correlation.

The plant-level estimates in Table VI provide evidence of learning, even in the period after infant-industry policy. Similar to the industry estimates (Table V), columns (1)–(3) of Table VI show a negative relationship between experience and unit cost reduction, now decomposing learning into plant and industry levels. The estimates for plant-level experience are differentially stronger among targeted establishments (columns (1)–(3)). Moreover, the estimates for industry-level experience are also significant—and significantly stronger—for targeted industries (columns (2) and (3)). Similarly, industry-level experience has a positive effect on TFP (columns (5) and (6)). Including the industry learning reduces estimates for the Targeted × (Plant Experience) interaction; however, the effect of Targeted × (Industry Experience) remains significant.

These micro estimates indicate that plant- and industry-level learning may be more pronounced for treated establishments. The combined effects of plant and industry-level estimates are substantial for HCI establishments, as shown at the bottom of Table VI. Predictably, plant-level experience generally exerts a larger effect than does industry-level experience. Online Appendix Table D.2 demonstrates the robustness of these results across alternative measures of experience, unit costs, and TFP. However, it is important to note that the plant-level estimates only cover the post-1979 period, thereby excluding potentially steep learning curves during the earlier stages of the industrial drive.

The correlational results in this section indicate that learning externalities plausibly affect targeted industries. Of course, this analysis cannot definitively identify the strength of the externalities or whether they originate from plant-level learning or industry-wide learning. Even so, taken together, the industry- and plant-level analyses suggest the potential for learning-by-doing spillovers operating in heavy and chemical industries.

VII. Indirect Effects of Industrial Policy

I consider how the industrial policy drive may have affected industries outside of the targeted sectors through linkages. I use

TABLE VI

PLANT- AND INDUSTRY-LEVEL LEARNING BY TREATMENT STATUS

	Unit cost (log)			TFP		
	(1)	(2)	(3)	(4)	(5)	(6)
Plant experience	−0.072***	−0.072***	−0.069***	0.479***	0.485***	0.484***
	(0.002)	(0.002)	(0.002)	(0.011)	(0.011)	(0.011)
Targeted × plant experience	−0.009***	−0.008***	−0.005***	0.032***	0.014*	0.018**
	(0.001)	(0.001)	(0.001)	(0.007)	(0.008)	(0.007)
Industry experience		−0.008***	−0.006**		0.022	0.026*
		(0.003)	(0.003)		(0.014)	(0.015)
Targeted × industry experience		−0.002**	−0.003***		0.030***	0.030***
		(0.001)	(0.001)		(0.009)	(0.009)
Control for plant size	Yes	Yes	Yes	Yes	Yes	Yes
Control for capital	Yes	Yes	Yes	Yes	Yes	Yes
Control for skill ratio	Yes	Yes	Yes	Yes	Yes	Yes
Control for investment	Yes	Yes	Yes	Yes	Yes	Yes
Control for intermediates	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial controls	No	No	Yes	No	No	Yes
Plant effect	Yes	Yes	Yes	Yes	Yes	Yes
Year effect	Yes	Yes	Yes	Yes	Yes	Yes
Industry effect	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.882	0.882	0.890	0.691	0.691	0.694
Observations	250,989	250,989	250,989	235,940	235,940	235,940
Clusters (industry and plant)	489 × 60,009	489 × 60,009	489 × 60,009	489 × 57,942	489 × 57,942	489 × 57,942

TABLE VI
CONTINUED

	Unit cost (log)			TFP		
	(1)	(2)	(3)	(4)	(5)	(6)
Linear combination (plant level)	−0.081	−0.080	−0.073	0.510	0.499	0.502
(std. err.)	(0.002)	(0.002)	(0.002)	(0.013)	(0.013)	(0.013)
Linear combination (industry level)		−0.010	−0.009		0.052	0.056
(std. err.)		(0.003)	(0.003)		(0.014)	(0.015)

Notes. This table shows the plant-level relationship between industrial development and (log) experience in targeted versus non-targeted industries. Estimates come from a plant-level version of equation (5). Outcomes are the following: log unit cost (total intermediate costs per unit of real gross output) and TFP (estimated using Ackerberg-Caves-Frazer). Experience is measured as cumulative output (the sum of real gross output until the current year). Plant experience refers to plant-level cumulative learning, and industry experience refers to industry-level learning, calculated at the four-digit industry level. All equations control for log plant size (employment). Additional controls include log: capital intensity, skill ratio, investment per worker, and intermediate input intensity per worker. Linear combination, at the bottom, gives the combined effects. All specifications are estimated using plant, industry, and year fixed effects. Polynomial controls adds cubic polynomials in the control variables. Two-way standard errors are clustered at the industry and plant levels and are in parentheses. * $p < .10$; ** $p < .05$; *** $p < .01$.

the terms "backward" and "forward" from the vantage point of the targeted industry. When the impact of industrial policy propagates from the treated HCI to upstream suppliers, suppliers are affected through backward linkages. When the effect of industrial policy propagates downstream to users of HCI products, buyers are affected through forward linkages. I refer to both as linkage effects. The following analysis draws on the empirical study of foreign direct investment (FDI) spillovers, particularly Javorcik (2004), and empirical work on the propagation of policy shocks (e.g., Acemoglu et al. 2016).

I measure an industry's linkage exposure to industrial policy using South Korea's 1970 IO accounts, which predate the HCI drive. Specifically, I calculate industry i's exposure to industrial policy through backward and forward linkages as follows:

$$\text{Backward Linkage}_i = \sum_{j \in \text{HCI}} \alpha_{ij}, \tag{6a}$$

$$\text{Forward Linkage}_i = \sum_{j \in \text{HCI}} \alpha_{ji}, \tag{6b}$$

where j represents the treated HCIs. For industry i, its Backward Linkage$_i$ (equation (6a)) equals the weighted sum of output supplied to treated industries j. The weight α_{ij} denotes the value of i's output used by j as a share of j's total output and comes from the IO accounts. For industry i, Forward Linkage$_i$ (equation (6b)) equals the weighted sum of inputs sourced from treated industries j. The weights α_{ji} denote the value of j's output sold to i as a share of i's total value of output in the IO accounts. For further details on these calculations, refer to Online Appendix E.1.

The measures above (equations (6a) and (6b)) capture direct spillovers to industries one degree away from the heavy-chemical sector. To account for both direct and indirect effects, I extend this analysis using the Leontief inverse, which captures the full network of linkages (first, second, . . . and n-degree) between Korean industries. For industry i, I construct Total Backward Linkages$_i$ and Total Forward Linkages$_i$ using a method analogous to equations (6a) and (6b), but now using weights derived from the Leontief inverse calculated from the 1970 IO accounts. For example, Total Backward Linkages$_i$ = $\sum_{j \in \text{HCI}} \ell_{ij}$, where ℓ_{ij} is an element of the Leontief inverse matrix. See Online Appendix E.1 for details.

To study the impact of linkages, I compare outcomes across industries with strong versus weak linkages to treated industries relative to 1972. In the spirit of the main DD analysis (equation (1)), I consider the following specification:

$$\ln(y_{it}) = \alpha_i + \tau_t + \sum_{j \neq 1972} \gamma_j \cdot \left(\text{Backward Linkage}_i \times \text{Year}_t^j\right) + \sum_{j \neq 1972} \delta_j \cdot \left(\text{Forward Linkage}_i \times \text{Year}_t^j\right) + \epsilon_{it}, \tag{7}$$

where Y_{it} is an outcome and i indexes each five-digit (or four-digit) industry. Subscript t denotes the years, which are 1967–1986 for the four-digit panel and 1970–1986 for the five-digit panel. As before, equation (7) uses two-way fixed effects for time τ_t and industry α_i. I first estimate equation (7) using only non-treated industries. I show these estimates alongside estimates from the full sample, which provide additional power. For the full-sample estimation, I control separately for the direct effect of policy using the interaction term $\text{Targeted}_i \times \text{Year}_t$.

The coefficients of interest, γ_j and δ_j, reflect the differential evolution of industries with strong versus weak exposure to treated industries, measured by $\text{Backward Linkage}_i$ and Forward Linkage_i. The set of estimates, $\widehat{\gamma_j}$ ($\widehat{\delta_j}$), captures the differential development of industries with strong backward (forward) linkages to targeted industries relative to those with weaker linkages. Note that specification (7) uses a continuous treatment, whereas estimates in Section V used a binary treatment. I estimate the model using the baseline linear TWFE estimator.

Before 1972, the set of coefficients should be zero, reflecting no prior differences between industries with stronger linkages. Estimates over the policy period suggest the potential strength and direction of linkage spillovers to non-treated industries. For instance, if the industrial policy increases the cost of key inputs over the policy period, we may expect negative estimates for $\widehat{\delta}_{1973}, \ldots, \widehat{\delta}_{1979}$. Estimates for the post-1979 period indicate, among other things, longer-term spillovers from the policy. The identifying assumption is that differences in industrial development between stronger or weaker backward (forward) linked industries would have evolved similarly in the absence of the heavy and chemical industry policy.

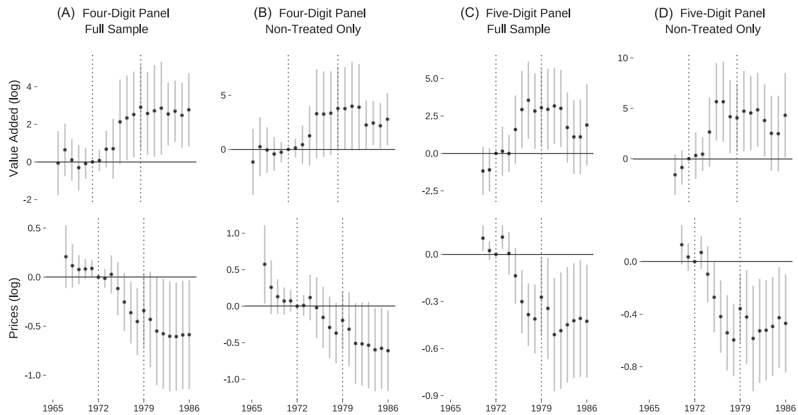

FIGURE VI

Forward-Linkages Exposure: Value Added and Output Prices

This figure plots dynamic DD estimates for the relationship between direct forward-linkage exposure and log outcomes: real output (value shipped; top) and output prices (bottom). The coefficients in the plot are estimated from equation (7). Linkages are calculated from the 1970 IO tables; see the text for details. All estimates are relative to 1972, the year before HCI. The year 1979 corresponds to the collapse of the Park regime. Years are on the x-axis. Estimates for the main linkage interaction (forward) are on the y-axis: for example, linkage $\times$ year. These estimates come from the DD specification that includes the impact of both measures. Full-sample regressions control for the main Targeted $\times$ Year effect. Ninety-five percent confidence intervals are shown in gray.

VII.A. Indirect Effects: Results

I find that industries with relatively strong forward linkages with targeted industries developed more robustly over the policy period. Specifically, downstream industries that were more dependent on inputs from targeted sectors showed greater industrial development. In contrast, the effect of backward linkages—where industries supply inputs to targeted sectors—appears to have been more limited.

1. *Downstream Industrial Development.* Figure VI plots the relationship between the strength of forward linkages and downstream output (equation (7))). Rows in Figure VI correspond to estimates for real value added (top) and output prices (bottom). For this analysis, I consider output measured in terms of the value added, given different stages of production and input intensity. The panels in Figure VI present estimates across differ-

TABLE VII

LINKAGE EXPOSURE AND VALUE ADDED, BEFORE AND AFTER 1973

	Outcome: Value added (log)			
	Five-digit panel (1970–1986)		Four-digit panel (1967–1986)	
	Full sample (1)	Non-HCI sample (2)	Full sample (3)	Non-HCI sample (4)
Post × forward linkage	2.832***	4.405***	2.095**	2.906**
	(0.914)	(1.504)	(0.802)	(1.174)
Post × backward linkage	−0.0167	0.176	−0.693	−2.163*
	(0.334)	(0.375)	(0.559)	(1.279)
Industry effects	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes
Targeted × year	Yes	No	Yes	No
R^2	0.776	0.763	0.847	0.819
Observations	4,720	2,986	1,750	1,096
Clusters	278	176	88	55

Notes. Average DD estimates, before and after 1973. Estimates correspond to equation (7). Regressions interact linkage measures with a Post indicator. The outcome is real log value added. Both linkage interactions (forward and backward) are shown. Analysis is performed for the sample of only non-treated industries and the full sample of industries. Estimates for the full sample separately control for the Targeted × Year effects to account for the main effect of policy. Standard errors, clustered at the industry level, are in parentheses. * $p < .10$; ** $p < .05$; *** $p < .01$.

ent data sets (four-digit versus five-digit) and samples (full sample versus only non-treated). Panels B and D restrict the sample to non-targeted industries, which significantly reduces the sample size and power, especially in aggregate four-digit data. Alternatively, Panels A and C provide estimates using the entire sample of industries and flexibly control for targeted industries ($\text{Targeted}_i \times \text{Year}_t$).

Figure VI shows that industries with stronger forward-linkage exposure expanded more often following the policy drive. Before 1973, differences among the industries were noisy, trending upward in the 1960s, and centered on zero.[13] Table VII reports the average pre-post version of equation (7) and presents forward-linkage estimates and backward-linkage estimates. For output, average forward-linkage estimates imply that a 1% rise in the share of links (between zero and one) from a treated industry is associated with 4.4% more output (column (2)) for the

13. Online Appendix Table E.1 rejects pre-trends across specifications, except those for the non-HCI sample in the four-digit data.

TABLE VIII

LINKAGE EXPOSURE AND OUTPUT PRICES, BEFORE AND AFTER 1973

	Outcome: Output prices (log)			
	Five-digit panel (1970–1986)		Four-digit panel (1967–1986)	
	Full sample (1)	Non-HCI sample (2)	Full sample (3)	Non-HCI sample (4)
Post × forward linkage	−0.359***	−0.459***	−0.483**	−0.510***
	(0.128)	(0.144)	(0.184)	(0.176)
Post × backward linkage	0.103***	0.0880***	0.251	0.673***
	(0.0213)	(0.0142)	(0.154)	(0.226)
Industry effects	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes
Targeted × year	Yes	No	Yes	No
R^2	0.957	0.942	0.962	0.956
Observations	4,721	2,987	1,751	1,097
Clusters	278	176	88	55

Notes. Average DD estimates, before and after 1973. Regressions interact linkage measures with a Post indicator. Estimates correspond to equation (7). The outcome variable is log output price. Both linkage interactions (forward and backward) are shown. Analysis is performed for the sample of only non-treated industries and the full sample of industries. Estimates for the full sample separately control for the Targeted × Year effects to account for the main effect of policy. Standard errors, clustered at the industry level, are in parentheses. * $p < .10$; ** $p < .05$; *** $p < .01$.

non-treated industry; estimates for the full sample imply a semi-elasticity of 2.83 (column (1)). Estimates across specifications are positive and significant for direct forward linkages. A similar pattern holds for the total forward linkages (see Online Appendix E.2 and Table E.1).

Similarly, Table VIII shows that greater exposure to forward linkages is associated with reduced output prices. Column (2) implies that a 1% rise in the share of direct HCI linkages is associated with −0.459% lower output prices of non-HCI industry (−0.359 for the full sample, column (1)). Online Appendix Table E.2 shows a similar strong negative relationship for total forward linkages. Dynamic estimates plotted in Figure VI demonstrate that industries using more treated inputs had relatively low output prices during and after the drive. However, prices were relatively higher and began converging before the policy introduction. Thus, the price effects in Figure VI may have already been in motion before HCI. Nevertheless, the policy is associated with declining output prices in the downstream industry. This result contrasts with arguments that similar industrial policies are associated with increased prices for downstream firms (Blonigen 2016).

There is also a positive relationship between forward-linkage exposure and development outcomes. This relationship is particularly strong and significant for relative employment and plant entry in downstream industries using large shares of treated inputs. This finding holds across datasets and applies to direct- and total-linkage measures (see average DD estimates in Online Appendix Tables E.3 and E.4). I provide a more detailed analysis of these effects in Online Appendix E.2. The results show weakly positive estimates between forward linkages and labor productivity, wages, and TFP.

2. *Evolution of Downstream Comparative Advantage.* What was the relationship between forward linkages and trade development? To explore this question, I combine information on linkages with the SITC-level trade data and consider the same regressions as before. I use a PPML estimator for trade-flow outcomes.

Like output, Figure VII shows a positive relationship between the strength of forward-linkage exposure and improved export development in downstream industries (Online Appendix Table E.4 shows full estimates). Before 1973, forward-linked sectors did not demonstrate a relative export advantage or export productivity over other downstream sectors. After 1973, Figure VII shows a shift in comparative advantage that emerged over the 1973–1979 period. However, it took time for a comparative advantage to manifest, and estimates appear strongest in the 1980s. These patterns hold across traditional and modern measures of RCA. These effects are also seen in measures of total forward linkages, reported in Online Appendix Figure E.2.

The previous section presented evidence of positive, contemporaneous spillovers from industrial policy. Other spillovers may take time to materialize. Furthermore, the positive relationship between forward linkages and export development further supports the direct main effects of policy shown in Section V. Had the policy been unsuccessful, it may well have harmed downstream exports.

3. *Downstream Linkages: Mechanisms, Investment, and Intermediates.* Where the industrial policy affected downstream

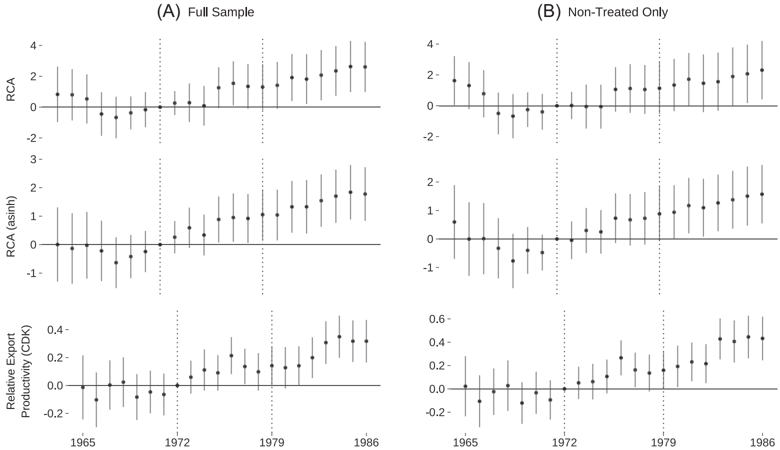

FIGURE VII

Forward-Linkages Exposure: Export Development

This figure plots dynamic DD estimates for the relationship between direct forward-linkage exposure and export-development outcomes. The coefficients in the plot are estimated from equation (7). Top row shows estimates using the raw RCA (Balassa) index, estimated using PPML. The middle row shows alternative RCA, transformed using inverse hyperbolic sine to account for zeros, and estimated using OLS. The bottom row shows OLS estimates for the relative export productivity (CDK) outcome. Linkage measures are calculated from the 1970 input-output tables (zero to one); see the text for details. All estimates are relative to 1972, the year before HCI. The year 1979 corresponds to the collapse of the Park regime. Years are on the x-axis. Estimates for the main linkage interaction (forward) are on the y-axis: for example, linkage $\times$ year. These estimates come from the DD specification that includes the impact of both measures. Full sample regressions control for the main HCI $\times$ year effect. Ninety-five percent confidence intervals are shown in gray.

industries, it likely did so by supplying domestic inputs for their benefit. I analyze this in Online Appendix E.3 and find that material outlays expanded relatively more for downstream users (both direct and indirect) of heavy industrial goods. This finding is illustrated in Online Appendix Figure E.3 (Panels A and B).

4. *Backward Linkage: Weak Relationship with Industrial Development.* The expansion of a targeted sector may promote upstream suppliers by increasing the demand for their goods. However, for this episode, the spillovers from the heavy industry drive to upstream suppliers appear to have been limited. This minimal effect may be because policy planners (Section II) chose relatively

upstream industries (Liu 2019), potentially constraining the extent of spillovers through backward linkages.

For instance, Table VII shows that an upstream industry with high backward-linkage exposure is not associated with a differential increase in output, unlike the positive effect of forward-linkage exposure. The same pattern is seen for similar DD estimates using total backward-linkage measures (Online Appendix Table E.1). Similarly, the relationship between backward-linkage exposure is undetectable for employment, plant entry, and other development outcomes, seen in Online Appendix Table E.3. I discuss the muted estimates of backward linkages further in Online Appendix F.

VII.B. Robustness and the Stable Unit Treatment Value Assumption

The indirect effects (Section VII.A) pose a dilemma in light of the direct effects of the policy highlighted in Section V.A. That is, the network effects of the policy may contaminate the control group by virtue of linkage spillovers, violating the stable unit treatment value assumption (SUTVA). For robustness, I demonstrate that the pattern of direct effects largely survives after accounting for the indirect effects in three analyses.

First, I examine how the main effects change when limiting the control group to industries with lower exposure to forward linkages from treated industries. Specifically, I restrict control sectors to industries with below-median linkage measures. For output and labor productivity, estimates using the "limited exposure" group (for direct and total linkages) do not significantly alter the main policy effect (Online Appendix Figure G.1).

Second, I report the main effects while controlling for linkage exposure in the control group. Online Appendix G.2 shows that after controlling for positive downstream spillovers in non-treated industries, the main impact of HCI becomes more pronounced. This finding is intuitive, as positive spillovers may cause the control group to benefit, slightly biasing estimates downward. While controlling for linkages increases the standard errors, the main effects persist.

Third, Online Appendix G.3 provides additional evidence that investment is not crowded out when accounting for linkages.

VII.C. Indirect Impact: Discussion

The analysis demonstrates policy spillovers through linkages to and from treated HCIs. I find that non-treated industries with high exposure to policy through forward linkages are associated with higher development outcomes and increased use of intermediates. This positive relationship extends to the later export development of downstream sectors. However, the effect of backward linkages appears to have been limited and ambiguous, possibly because treated sectors were, by design, upstream. Although indirect effects, even if weak, may influence the control group, Section VII.B shows that these linkage effects do not significantly alter the qualitative pattern observed in the main policy effects.

VIII. Conclusion

This article shows that Korea's heavy and chemical industry drive promoted industrial development in the manufacturing sectors targeted by the policy. I find that this intervention had wide ramifications. First, the drive created positive effects in treated industries long after its major elements had been retrenched. In the case of export performance, policy effects took time and fully materialized after the policy had ended. I provide cursory evidence that the dynamic effects may correspond to learning mechanisms. Moreover, the regime's policy likely affected the development of industries not targeted by the policy in the short and long run. Thus, this study takes a multidimensional view of industrial development, demonstrating that HCI targeting corresponded to improvements across an array of outcomes, from export performance to the labor market.

Aspects of these findings correspond to arguments proposed by Wade (1990) and Amsden (1992), mainly that active policy may have contributed to Korea's industrialization and its shift in comparative advantage to more advanced industries. My results emphasize conventional policy forces rather than miraculous ones. These included using directed credit to facilitate investment, purchasing key intermediates, and promoting sectors with dynamic economies and linkage spillovers.

History is not a clean laboratory, and South Korea's experience is no exception. Like many transformations, this one was tumultuous and multifaceted. Nevertheless, this study tries to decipher a key episode of industrial policy using the contemporary econometric toolbox. The goal is to structure coherent insights around a key historical case of industrial transformation. I hope to extract more coherent workings of the policy—those that are useful more broadly—and emphasize a more empirically grounded narrative around East Asian interventions. The findings here are not final, nor could they be. They point to a potential direction for further empirical work.

The limitations of this study are manifold and show the necessity of further work. Although heavy industrial policy may have promoted forms of industrial development, it did so at a cost that cannot be accounted for in the scope of this article. Nor have I examined the aggregate or allocative consequences of the episode. I leave those questions to future quantitative and empirical work. Importantly, the context of this study suggests that successful industrial policy likely hinges on bureaucratic capacity and political incentive compatibility (Haggard 1990; Evans 1995; Robinson 2010; Juhász and Lane 2024). Such factors highlight the importance of future research on the political economy of industrial policy.

Supplementary Material

An Online Appendix for this article can be found at *The Quarterly Journal of Economics* online.

Data Availability

The data underlying this article are available in the Harvard Dataverse, https://doi.org/10.7910/DVN/VJECHN (Lane 2025).

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