Manufacturing Revolutions: Industrial Policy and Industrialization in South Korea

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Abstract

I study the impact of industrial policy 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 impacts during and after the intervention period. (1) I reveal that the heavy-chemical industrial policies promoted the expansion and dynamic comparative advantage of directly targeted industries. (2) Using variation in exposure to policies through the input-output network, I demonstrate that policy indirectly benefited downstream users of targeted intermediates. (3) The benefits of HCI persisted even after it ended, some of which took time to manifest. These 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: L5, O14, O25, N6. Keywords: industrial policy, East Asian miracle, economic history, industrial development, Heavy-Chemical Industry Drive, Heavy and Chemical Industry Drive.*

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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, Grossman, and Norman 1995). Early development economists saw these policies as essential for industrial development (Rosenstein-Rodan 1943; Nurkse 1953; Hirschman 1958), and scholars 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 their role in Asia may have been counterproductive (Pack 2000). South Korea exemplifies both Asia's rapid transformation and controversies around industrial policy. At the beginning of the 1960s, South Korea was a politically unstable industrial laggard; however, 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 industrial policy returns Juhász et al. 2022, empirical evidence surrounding its efficacy is scant, especially for the East Asian miracle (Lane 2020).

I use the context of the heavy industrial drive to employ a dynamic differences-in-differences (DD) estimation strategy. I evaluate the impact 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 then build on these results in two ways: First, I show that the core results are robust to using a doubly robust DD estimator (Callaway and Sant'Anna 2020; Sant'Anna and Zhao 2020) 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 to foreign placebo manufacturing sectors.

The main DD estimates capture the impacts of heavy industry policies, which emphasized directed credit and, to a lesser extent, trade policy. Industry pre-trends inform Korea's counterfactual sectoral structure. Absent 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 is associated with the ascent of new industries and new patterns of specialization.[1]

To estimate these effects, I construct new data 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. The result is panel data covering a key episode of industrial development.

I highlight three empirical results. First, I find significant positive impacts of the policy package across industrial development outcomes in targeted industries. Relative to pre-intervention levels, targeted heavy-chemical industries expanded their

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

output by more than 100% over non-treated manufacturing sectors. Furthermore, labor productivity was more than 15% higher. This divergence is not driven by a decline in non-treated industries. Moreover, since industrial development is multidimensional, I consider it across outcomes and find impacts on employment growth, export performance, and output prices. HCI not only appears to have durable, longer-run effects on treated industries, but I also find evidence of persistent impacts 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. Additionally, treated industries were 10 percentage points more likely to achieve comparative advantage in global markets after 1973. Indeed, the revealed 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 impacts of policy.

Third, heavy and chemical industry drive policies correspond to the development of downstream industries. I find that downstream sectors with strong linkages 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 appear limited. Hence, I find evidence that the policy may have supported network spillovers. These results are consistent with quantitative research on optimal policy approaches within networks, such as Liu (2019), which uses IO data from this study. Accounting for linkages reduces the precision of the main effects yet 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 impact of industrial policy. This stream of literature includes 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). This analysis also complements the structural literature in industrial organization, which analyzes sector-specific industrial policy (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 the context of India's small and medium-sized enterprise policy.

Second, I contribute to the empirical study of industrial policy via natural experiments. This paper is the first study to deploy modern empirical techniques to evaluate the heavy and chemical industry 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 impacts of output market protection on manufacturing development. I consider the efficacy of infant-industry policy in (a) a contemporary setting and (b) 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 (Hanlon 2020; Mitrunen 2019; Pons-Benaiges 2017). Likewise, Jaworski and Smyth (2018) and Giorcelli (2019) explore the impact of temporary government policies on industrial development. Moreover, I contribute to 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 speak to research evaluating place-based policies, notably, Criscuolo et al. (2019) and Becker, Egger, and Ehrlich (2010), who use exogenous spatial variation to study the impact of targeted support on distressed regions.

Third, I contribute 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 broadly been skeptical of such interventions (Pack and Saggi 2006; Noland and Pack 2003), 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, summarized in Section III). By employing contemporary econometrics, I build on early correlational studies (Weinstein 1995; Beason and Weinstein 1996) and recent quantitative research (Liu 2019).

To summarize, this study uses variations from the heavy and chemical industry drive to study the impacts of industrial policy in South Korea. It attempts to provide a disciplined, data-driven account of the episode. 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 then provides an overview of the data. In Section V, I present estimates of the direct impact 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 input-output 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 heavy and chemical industry 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 (Kim 2011c; Moon and Jun 2011). Among the factors behind Korea's crisis were (a) North Korea's increasing militarization and offensive actions (Kim 1997; Moon and Lee 2009) and, critically, (b) 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 (Nixon 1970; Kim 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 not only the timing of the heavy industry push, but also shaped its sectoral scope. I turn to this next.

II.B Sectoral Choice

a. Sectoral rationale and selection. The heavy and chemical industry 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 the Online History Appendix I.2.1.

Why were these sectors chosen, and what might deliberations over their selection tell us about expectations for their success? Two rationales dominated the choice of heavy-chemical sectors, documented by scholars and policymakers.

First, heavy industrial intermediates were seen as key for military-industrial modernization (Lee 1991; Woo-Cumings 1998; Kim 2011c). In the early 1970s, unlike the North, 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, p.375). One former government official, Kim Chung-yum, 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" (Kim 2011b, p.409). Hence, industrial intermediates were a means to promote 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 (*ibid*). Former officials from the government of South Korean

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

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—technologically within reach and featuring economies of scale that could be supported through export markets (Kim 2011b). To consider feasibility, the regime studied its contemporaries, including those in Western Europe and Japan (Perkins 2013). 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; Moon and Jun 2011; Kim 2011a), 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 I.2.2.

b. Selection skepticism by foreign lenders. Yet, *ex-ante*, the potential of Korea's heavy-chemical industries was not obvious, and international investors raised 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 multiple European nations declined loans for less ambitious, proto-HCI schemes (Rhyu and Lew 2011; Woo 1991). 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* [emphasis my own] in the production of steel" (Kim 2011b; Rhyu and Lew 2011, p.324). 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, which I detail next.

II.C Policy Bundle 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 rather aimed at export activity *writ large* (Hong 1977, p.28).[4] Before the HCI drive, export incentives were essentially "administered uniformly across all industries" (Westphal and Kim 1982, pp.217–218; Westphal 1990, p.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, p.91; Lim 1981). In other words, the heavy-chemical drive represented a pivot to a fundamentally sector-specific strategy.

3. See Online History Appendix I.2.3 for details on these constraints.

4. Before 1973, Korea implemented no less than 38 different incentives to promote exports (Lim 1981, p.18). See Online Appendix I.2.4 for details on Korean planning.

Yet, what was the industrial policy bundle? I consider two classes of policies in detail—(i) investment policy and (ii) trade policy—and their variation across the period.

a. The bias of lending and investment incentives. Directed lending was a central lever of the heavy industrial drive (Woo 1991; Lee 1991).[5] Half of all domestic credit consisted of subsidized "policy loans," which were allocated by financial institutions—both less traditional non-banking and more traditional commercial banking institutions (Koo 1984; Lee 1996). Broadly defined, policy loans were designed to advance government objectives and were automatically re-discounted by the central bank at a preferential rate.[6] For example, over the policy period, policy loans had longer repayment periods, and average interest rates were five 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 of Figure I 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 non-banking financial institutions (p.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 HCI drive policy. The thin lines correspond to two-digit industries, and the thick lines are averages for targeted (red) and non-targeted (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 also allocated a significant share of policy loans (World Bank 1993; Cho and Kim 1995). Appendix Figure A2 shows a similar growth in total credit and intermediate equipment loans by commercial banks. Likewise, across lending institutions, aggregate data in both figures (Fig. I and Appendix Fig. A2) plot a trend-break in sectoral-specific lending after 1979, for the period of policy liberalization.

Similarly, Panel A of Figure I traces the sectoral bias of tax policy over the period, using the estimated effective marginal tax rate. These estimates account for a myriad of period-specific investment incentives, notably tax incentives for investment. For example, investment tax credits and special depreciation rates (Kwack 1985; Stern et al. 1995; Lee 1996); see Online Appendix II.3 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 1990).[7] Like directed credit above, tax policies converged after the start of liberalization in 1979.

5. Woo summarizes that Korean policy sought to "hemorrhage as much capital as possible into the heavy industrialization program" (1991, p.159).

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

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, p.395).

b. The bias of trade policy. The heavy and chemical industry drive also altered the bias of trade policy. Pre-1973, policy broadly exempted exporters from import restrictions on inputs (Nam 1980; Westphal 1990). Indeed, measures of nominal protection were lower for heavy industry for the period (see Online Appendix I.2.5 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 on inputs. Park (1977) estimates that "key industries," on average, enjoyed 80% tariff exemptions (1977, p.212). Although post-1973 trade policy was refocused toward heavy industry, the nominal protection of output markets does not appear to rise substantially, especially relative to other policies (see Section VI.B).

c. 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 I.2.6. 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 over the post-1979 period (see: Appendix A.2). While the role of government policy loans shrank (Cho and Cole 1986; Nam 1992), fiscal policy reforms closed the gap in effective marginal corporate tax rates between strategic and non-strategic industries (Kwack and Lee 1992). Meanwhile, the post-Park autocracy only accelerated Korea's trend toward trade policy liberalization. Throughout this study, I use 1979 as the *de facto* end of the episode.

II.D Summary: Features for Empirical Study

The policy context above informs the research design of this study, which I summarize in four points:

First, the episode introduces sectoral variation over time, as the heavy and chemical industry drive shifted national policy toward a discrete set of nascent industries. This shift began and ended because of external political events, with 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. I consider actual policy and not random variation mimicking industrial policy. Given the complications of estimating the impact of IP, researchers have used important natural experiments that mimic policy variation (Juhász 2018; Hanlon 2020; Mitrunen 2019). Nevertheless, the case for industrial strategy hinges on the policy being intentional (Juhász et al. 2022) and may make it difficult to glean insights from random, accidental policy variation (Rodrik 2004).

Third, although targeted, Korea did not believe heavy-chemical industries would develop without intervention, and financiers doubted the viability of Korean heavy industry. 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 heavy and chemical industry drive reduces the role of political confounders. This setting, including the existential threat facing South Korea, meant industrial policies were binding and coherent. Clientalism and political demands often divert resources to industries with a comparative disadvantage (Rodrik 2005; Lin 2012), and policy estimates may reflect political failures rather the potential of 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 sectoral lobbying and 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). In this section, I discuss two externalities relevant to the South Korean policy episode: *(a)* dynamic economies of scale and *(b)* linkage effects. I consider each in the context of earlier empirical work on East Asia.

a. Dynamic economies of scale. First, 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 and, in turn, this experience benefits other firms within the same industry. Hence, individual firms may not internalize the benefits of learning, producing, or under-investing in 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 they face capital constraints, they may be unable to survive turbulent nascent periods.

Such dynamic economies of scale are the means by which industrial policy can, in theory, cultivate dynamic comparative advantage in trade (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 new sectors can promote the evolution of comparative advantage on the international market.

Correlational studies of East Asian industrial policy have argued that interventions may not correspond to industrial development or externalities. For Korea, Lee (1996) identifies a negative relationship between post-war interventions and industry-level outcomes, specifically, protection and manufacturing productivity (also see 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.

b. Linkage effects. Second, pecuniary externalities through linkages have been another justification for industrial policy (Krueger and Tuncer 1982; Grossman 1990; Krugman 1993), where policies targeting one sector benefit external industries through input-output (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 large. These benefits are transmitted in two directions. The first is through *backward linkages* to upstream industries selling inputs to targeted sectors. For example, if 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 given policy increases the productivity of a treated industry, it may lower prices to the benefit of 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 heavy and chemical industry 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, aggregated from establishment (or plant) level surveys. [8] In addition to industry-level data, I also 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.

a. Long and short industry panels. This study uses two harmonized industry panels. Table A1 presents pre-1973 statistics (mean and standard deviation, non-normalized values) for key industrial variables. Part A of Table A1 reports values from the "short" granular five-digit industry panel, harmonized from 1970 to 1986. Part B reports values from the "long" more aggregated four-digit panel, harmonized from 1967 to

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

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 the Online Data Appendix II.2 and the concordance within the MMS and across other data series.

The harmonization process introduces a trade-off between the two panels above. 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 critical pre-1973 ("pre-treatment") periods. Although the long panel adds three years to the set of pre-treatment years, (i) four-digit observations and (ii) the harmonization process significantly reduces the number of industry observations relative to the short, disaggregated panel.

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 such as shipbuilding, aggregate sectors from acts and census industries are closely aligned. However, care is required for more complex industries and their acts, such as chemicals. I hand-match the industries in the legislation to the harmonized data, both long and short panels. This process entails matching industry labels in legislation to industries in the five-digit KSIC industry codes. See Online Appendix I.2.1 for legislation and matching.

c. Linkages. Inter-industry linkage data are constructed from the Bank of Korea's 1970 "basic" input-output 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 Section E.1. The Bank of Korea data and MMS surveys use different coding schemes. Thus, combining IO accounts with industry data requires further harmonization (see Online Data Appendix II.2).

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 Standard International Trade Classification (SITC, Rev.1) four-digit-level trade data. The trade flow data come principally from the UN Comtrade database. Trade policy—product-level measures of quantitative restriction (QRs) coverage and 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 non-tariff barriers, notably QRs. Most empirical studies of Korean trade policy use highly aggregated data. For 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): e.g., input-tariff$_i$ = $\sum_j \alpha_{ji} \times$ output-tariff$_j$, where α_{ji} are cost-shares for industry i and for input j. Cost weights come from 1970 input-output accounts.

V The Main Impacts of Industrial Policy

This section considers the empirical impact of the heavy and chemical industry 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). Second, I report estimates of the average impact of policy and consider estimates from the double-robust DD estimator (Section V.E). Finally, I employ a DDD estimation strategy to study the impact of HCI using cross-country variation (Section V.F).

V.A Estimation

To estimate the impact of industrial policy, I use the temporal and sectoral variation from the heavy and chemical industry drive to employ a differences-in-differences 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 follow 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, and 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 pre-treatment 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 pre-treatment averages X_i' are time-invariant, I interact them with year effects to estimate their impact over time.

The impact 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_js is the differences between targeted and non-targeted industries for each year j, relative to the pre-treatment year 1972; 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 doubly 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 heavy and chemical industry 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 impacts of the industrial policy drive. In the parlance of the industrial policy literature, the longevity of these effects 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 also coincide with a permanent shift in levels of development between 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]

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 policymakers are often interested in the impact of a policy on targeted units rather than the average unit in an economy (ATE); as such, the ATT requires different—and less stringent—assumptions.

Theoretically, sectoral industrial policies often target industries that are most responsive to policy 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 Average Treatment effect on the Treated (ATT), the common trend assumption accounts for this issue under certain assumptions: if selection is not changing over time (irrespective of policy), the common trends assumption addresses this form of selection between targeted and non-targeted industries (Heckman et al. 1998; Blundell and Dias 2009). That is, 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 leadership and foreign lenders estimated that South Korea could not cultivate dominance in heavy industries without intervention. Nev-

9. Although null results provide information about DD pre-trend assumptions, they cannot validate the pre-trend assumption alone or decisively. This situation is particularly true for more detailed five-digit estimates, which have limited pre-treatment periods.

ertheless, 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 re-weighting 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 doubly robust DD estimator—a method that re-weights 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 deploy a triple difference estimation strategy. This DDD strategy attempts to directly address the issues discussed above by comparing Korean industries to similar international industries. Let us first, however, consider the baseline estimates.

V.B Direct Impact on Industrial Development: Results

a. Key patterns and output expansion. Figure II plots baseline dynamic DD estimates for the impact 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, while the right columns show estimates with controls. The top row of each panel in Figure II presents the average log output for targeted (red) and non-targeted (black) industries using the fit model from eq. (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. These patterns will 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 is clearest in the longer aggregate four-digit panel, and pre-period coefficients are individually and jointly insignificant (Online Appendix Table B1).

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 since differences between treated and non-treated industries may emerge if policies harm control industries (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 impacts of the drive were not transitory. In terms of real output, in Figure II, the gap between treated and non-treated industries persists through 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 vs. five-digit panels), and specifications in Appendix Figure B1.

b. Industrial development outcomes. Figure III presents the impact of the heavy-chemical policy across various industrial development outcomes. Panel A of Figure III 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 relative decline in prices for heavy industry. Appendix B.1 shows that heavy industry prices increased less than other industries over the inflationary 1970s.

Panel A (Fig. III) also demonstrates that 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 B2 jointly rejects pre-trends. Additionally, Figure III shows a rise in the number of plants operating in HCI markets.

V.C Direct Impact on Exports Development

Export performance provides another view of industrial development, and exports were central to the policy program, as was the case 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 (World Bank 1987; Hong 1987). Figure III, Panel B reports the impact 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 index is defined as the ratio of Korea's export share of good k relative to the world's export share of commodity k: $\text{RCA}_k = (X_k^{\text{Korea}}/X_{\text{Total}}^{\text{Korea}})/(X_k^{\text{World}}/X_{\text{Total}}^{\text{World}})$, where X denotes the value of exports. Korea has a comparative advantage in k when RCA_k is larger than one.

Additionally, I estimate the relative export productivity (CDK) using the gravity model methods proposed by Costinot, Donaldson, and Komunjer (2012). Their CDK estimate provides a theoretically consistent measure of revealed comparative advantage beyond the classic RCA calculations. For industry k, I estimate relative export productivity for country i, where $\widehat{\text{CDK}}_k = \exp(\delta_{ik}/\hat{\theta})$; 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 non-zero trade flows and takes positive non-zero values.

Across measures of export development, Panel B in Figure III reports a strong positive relationship between industrial policy and treatment. For the classic RCA index, I employ Poisson pseudo-maximum likelihood (PPML), given the prevalence of 0s. I also provide linear estimates using with (inverse hyperbolic sine) 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 grows markedly after 1973. Second, before 1973, pre-trends were absent across trade outcomes, except for RCA, which trended downward. Third, estimates grow and become highly significant in the post-policy period. Hence, relative comparative advantage 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

V.D.1 Total Factor Productivity: Plant-Level Persistence and Industry-Level Trends

Above, Section V.B presented indirect productivity measures. I now turn to total factor productivity. However, features of the data and the historical context pose constraints for estimating TFP (e.g., microdata availability). Nevertheless, I study TFP in two ways:

First, I consider the persistence of plant-level TFP using microdata (available after 1979). Specifically, I study the correlation between targeting and plant-level TFP after the termination 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, t are years after 1979. The term Targeted_p indicates plants operating in industries targeted by the 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 following the HCI drive (1980–1986), treated establishments have significantly higher TFP than non-treated establishments. Across specifications and measures of TFP, estimates in Table I are significant and imply that HCI plants have between 2 and 8 % higher productivity than non-targeted plants in the 1980s. These correlational results are compatible with the industry-level dynamics shown in Section V.B.

Second, I turn to industry-level dynamics using aggregate TFP, which I present in Appendix B.2.1. 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 over the post-1979 period. 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 B1 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 policy effects taking time to manifest. Perhaps just as 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.

V.D.2 Continuous Treatment and Limited "Horizontal" Spillovers

For robustness, Online Appendix III.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 Fig. B2) track the binary estimates in Section V.B. Broadly, however, multi-product plants in heavy industry tend not to produce significant output in control industries. Consequently, there is limited 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

a. Double robust estimator. I now employ the doubly-robust DD estimator proposed by Sant'Anna and Zhao (2020) and Callaway and Sant'Anna (2020). Doing so 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,

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

where equation (3) refers to the weighted average differences in industry outcomes. More precisely, eq. (3) is the difference in outcomes between targeted industries (Targeted) and non-targeted industries (1 − Targeted). Weights in eq. (3) are defined as follows (Sant'Anna and Zhao 2020; Callaway and Sant'Anna 2020): the term $\pi(X) \equiv \mathbf{E}[\text{Targeted}\,|\,\text{X}]$ 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, pre-treatment 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 (3) is doubly robust in that if either component is correctly specified, it provides a consistent estimate of the ATT.

The doubly 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 other DD estimators. The average effects in eq. (3) do not rely on zero pre-trends over *all* pre-treatment periods, instead using a long-difference (between post-period t and the last pre-treatment period, 1972). Confidence intervals for (3) are calculated using a bootstrap procedure, which allows industry-level clustering (Callaway and Sant'Anna 2020). I use the same controls as the TWFE estimates above. Note that (3) requires a binary treatment and is not used for cases of continuous treatment, such as the indirect analysis in Section VII.

b. Results: average impacts. 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 to TWFE estimates using controls. Panels A and B present estimates for the five-digit and four-digit panels, respectively.

The estimates in Table II reveal that the overall average impact of HCI targeting was meaningful and significant. The preferred estimates in Panel A, column (1) indicate 128 % growth in output for HCI manufacturers relative to non-HCI manufacturers.[10] Similarly, linear TWFE estimates in Panel A (col. 2) suggest 124 % output growth, significant at the 1% level. The average impact on labor productivity (col. 1) translates into a 17.2 % increase in value added per worker for targeted industries after 1973. Labor productivity growth ranges from 14.3 –17.2 % across four-digit and five-digit panels. Table II shows relatively lower prices, implying prices were -9.55 % (Panel A, 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 Panel A, column (1), or a 31.4 % increase for four-digit data in Panel B, 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 0.36 , while the average RCA for non-targeted Korea was 0.88 (refer to Table A1). 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 and chemical industry drive plan (50% of manufacturing exports) was surpassed by 1983 (Kim and Leipziger 1993; Cho and Kim 1995).

c. Results: Robust dynamic estimates. Doubly robust event-study estimates show similar patterns to the direct effects above in Section V.B. Appendix B.2.2 records and provides the dynamic DD estimates using the re-weighting estimator above. Here, the patterns (Appendix Figures B4–B6) are qualitatively similar to the linear TWFE estimates (Sec. V.B), although the doubly 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 triple difference estimation strategy.

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

a. Cross-country variation and triple difference estimation. How did heavy-chemical industries in South Korea fare relative to the world? Cross-country data allows me

10. Calculated using $100 \times \left(\exp\left(\hat{\beta} - .5 \times (\mathrm{SE})^2\right) - 1\right)$.

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

to move beyond the within-country comparisons above. I use a DDD estimation strategy to expand on the DD analysis above—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:

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

where c denotes country, i denotes industry, and t denotes time. I estimate equation (4a) using cross-country trade data (SITC four-digit level). I focus on the triple interaction, $\mathrm{Korea}_c \times \mathrm{HCI}_i \times \mathrm{Year}_t$, 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, 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:

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

where Equation (4b) 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 eq. (4b) thus subsume the interactions $\mathrm{Korea}_c \times \mathrm{Year}_t$ and $\mathrm{HCI}_i \times \mathrm{Year}_t$ interactions from eq. (4a).

Triple difference estimates (eq. 4a-4b) capture the impact of Korea's industrial policy on industrial development. The coefficients of interest are β_{3j}, estimated from the three-way interaction term: $\mathrm{Korea}_c \times \mathrm{HCI}_i \times \mathrm{Year}_t$. 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] Triple difference 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 (Silva and Tenreyro 2006) for RCA outcomes, given the preponderance of zeros. I also show alternative transformations and estimators for completeness.

b. Results: cross-country trade development. Figure IV presents the triple differences estimates for the impact of Korean HCI on comparative advantage. The panels in Figure IV plot the coefficient from the interaction: $\mathrm{Korea}_c \times \mathrm{HCI}_i \times \mathrm{Year}_t$. I present multiple specifications: one using individual county, year, and industry effects; one using industry–year and country–year effects; and one that adds additional country–industry effects.

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

The DDD estimates in Figure IV show a substantial impact of policy on Korean heavy industry exports across trade outcomes. These outcomes include revealed comparative advantage (RCA, standard and normalized inverse hyperbolic sine), measures of relative export productivity (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 to using alternative, aggregate industry data, which I show in Online Appendix IV.1. The Online Appendix Figure C2 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 as those in Fig. IV.

How unusual is it for a country to cultivate export advantage in targeted industries? Perhaps it is inevitable that countries naturally cultivate comparative advantage in heavy or chemical industries. I assess the probability that an HCI industry achieves comparative advantage on the world market, $p(\text{RCA} > 1)$, in Korea versus foreign controls in Appendix C.1. Appendix Table C1 shows that Korea had a significantly higher probability (between 9.3 and 13.1 %) of achieving comparative advantage in HCI products after 1972, compared to countries with similar levels of development in 1972 (OLS estimates in Appendix Table C1).

V.G Direct Impact: Discussion

The empirical relationship between industrial policy and industrial development is not a foregone conclusion. For many reasons, we may anticipate a negative relationship between 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). Above, I showed a positive relationship between 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 is durable through the 1980s. These results are robust across data sets (short-term and long-term panels), the type of estimator (TWFE v. doubly 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 heavy and chemical industry drive aimed to promote investment and input use through directed credit and investment incentives. This section examines the role and impacts of these policies. However, observing 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. 2022). The HCI drive is 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 respond differentially for treated industries. Then, drawing on (Bau and Matray 2021), I show that input use responds in ways consistent with credit policy, specifically in treated sectors.

a. Baseline results: input-use and policy variation. Figure V presents baseline DD estimates (equation 1) for input-related outcomes at the five-digit level. Plots A and B in Figure V show that this divergence is starkest for intermediate outlays (total and per worker), beginning in 1973 and widening throughout the drive. Plots C–E report estimates related to investment and capital formation. Estimates for material spending and capital formation become significantly different between the treated and non-treated industries soon after the start of the industrial policy drive. Additionally, I observe similar investment patterns across asset classes, especially those targeted by investment incentives (e.g., machinery equipment); see Online Appendix Table D2.

The increase in intermediate outlays and investment for heavy industry was substantial. Table IV provides doubly robust and TWFE DD estimates of the total average impact (ATT). Preferred estimates (Panel A) 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 a percent increase in 81.4 total investment for treated over non-treated industries, shown in column (1) of Table IV. 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 Appendix D.1).

b. 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 disproportionately impact firms with high wedges, and these wedges can be captured through the pre-treatment marginal revenue product of capital (MRPK) (Bau and Matray 2021). In other words, a policy should disproportionately impact investment in high-MRPK industries and increase the marginal revenue product of other inputs.

I find evidence consistent with investment policy operating in heavy-chemical 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 (2021). I present these calculations and details in 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: Appendix Figure D2). 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 impacted the heavy-chemical sector.

c. 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 thus far in favor of crowding out. Section II demonstrated that, although biased toward heavy-chemical industries, lending continued for the non-treated sectors throughout the drive. Recall that commercial banks continued to lend to non-targeted industries, which remained a major source of lending for the period; see Appendix A.2 for details. Trends in non-targeted industry growth support this. Recall, Section V.B suggests that the relative ascent of heavy industry was not driven by the absolute contraction of the non-targeted industry.

I now 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 Appendix D.3. I find that investment was high in targeted relative to non-targeted industries during the period, though it generally increased across both sectors. These results are consistent with the patterns of lending and growth described above.

Furthermore, although the heavy industry drive altered investment patterns, it did not reduce absolute investment in non-targeted industries. The analysis in Appendix Section D.3 shows that investment was not crowded out in untreated, capital-intensive industries (Appendix Figure D3). Although less striking than investment in heavy industry, investment in the light industry continued, as the non-heavy industry 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 the policy as overtly protectionist (Lall 1997). Evidence for the latter claim is weak in Appendix D.4. Conversely, I posit that targeted cuts on import duties for intermediates could have also 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 Appendix Table D3, Panel A). Appendix Table D3 also suggests that nominal output protection fell more for non-treated than treated industries. Likewise, assuming trade policy allowed for discounts on imported inputs (Section II.C), many heavy industrial producers enjoyed a significant discount in duties on foreign inputs (Appendix Table D3, 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 industrial policy promote industries with strong learning-by-doing forces? I now 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 with higher productivity or lower unit cost. To assess whether learning was particularly strong in treated sectors, I employ a simple, reduced-form regression for the post-1972 period. Specifically, I consider the following equation:

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

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. Additionally, I control for the effects of technological progress embodied in input use, X_{it} (e.g., total input outlays, capital intensity, etc.). 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 impact of Experience$_{it}$ for the treated industries. Hence, estimates from (5) test whether dynamic externalities are present in targeted industries and their strength in treated sectors relative to non-treated sectors (see Beason and Weinstein 1996 and 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 Targeted$_i \times$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. Appendix Table D1 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: (i) plant-level and (ii) industry-level (four-digit) experience, both measured from the beginning of the sample period. All regressions include plant and 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 following 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 (cols. 1–3). Moreover, the estimates for industry-level experience are also significant—and significantly stronger—for targeted industries (cols. 2–3). Similarly, industry-level experience has a positive impact on TFP (cols. 5–6). 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 heavy-chemical industry establishments, as shown at the bottom of Table VI. Predictably, plant-level experience generally exerts a larger effect than industry-level experience. Appendix Table D2 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 are plausible for targeted industries. Of course, this correlational analysis cannot definitively identify the strength of the externalities or whether they originate from plant-level learning or industry-wide learning. Nevertheless, taken together, the industry and plant-level analyses suggest the potential for learning-by-doing spillovers operating in heavy and chemical industries.

VII Indirect Impact of Industrial Policy

I now consider how the industrial policy drive may have impacted industries outside of the targeted sectors through linkages. I use the terms "backward" and "forward" links from the vantage point of the targeted industry. When the impact of industrial policy propagates from treated heavy-chemical industry to upstream suppliers, suppliers are impacted through backward linkages. When the impact of industrial policy propagates downstream to users of heavy-chemical industry products, buyers are impacted through forward linkages. I refer to either 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. 2015).

I measure an industry's linkage exposure to industrial policy using South Korea's 1970 input-output 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 heavy-chemical industries. For industry i, its Backward Linkage$_i$ (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$_i$ (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 input-output accounts. For further details on these calculations, refer to Appendix E.1.

The measures above (6a-6b) capture direct spillovers to industries one degree away from a heavy-chemical industry. 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-6b, but now employing weights derived from the Leontief inverse calculated from the 1970 IO accounts. For example, Total Backward Linkages$_i$ = $\sum_{j \in \mathrm{HCI}} \ell_{ij}$, where ℓ_{ij} is an element of the Leontief inverse matrix. See 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 (eq. 1), I consider the following specification:

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

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 (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 impact of policy using the interaction term Targeted$_i$ × 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 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 the first part of this paper (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 industrial policy increases the cost of key inputs over the policy period, we may expect negative estimates for $\widehat{\delta}_{1973}, ..., \widehat{\delta}_{1979}$. 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.

VII.A Indirect Effects: Results

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

a. Downstream industrial development. Figure VI plots the relationship between the strength of forward linkages and downstream output (eq. 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 columns in Fig. VI present estimates across different 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 only. This restriction 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 upwards in the 1960s and centered on zero.[13] Table VII reports the average pre-post version of Equation (7) and presents both forward linkage estimates and backward linkage estimates. For output, average forward linkages estimates imply a 1% rise in the share of links (between $0-1$) from treated industry is associated with 4.4 % more output (col. 2), estimated for the non-treated industry; estimates for the full sample imply a semi-elasticity of 2.83 (col. 1). Estimates across specifications are positive and significant for direct forward linkages. A similar pattern also holds for the total forward linkages exposure (see Appendix E.2 and Table E1).

Similarly, Table VIII shows that greater exposure to forward linkages is associated with reduced output prices. Panel A, 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, col. 1). Appendix Table E2 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 Fig. 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 industrial policies associated with increased prices for downstream firms (Blonigen 2016).

There was also a positive relationship between forward linkage exposure and development outcomes. This relationship is particularly strong and highlight sig-

13. Online Appendix Table E1 rejects pre-trends across specifications, except those for the non-HCI sample in the four-digit data.

nificant for relative employment and plant entry in downstream industries using large shares of treated inputs. This holds across datasets, but also for direct and total linkage measures (see average DD estimates in Tables E3–E4). I provide a more detailed analysis of these effects in Appendix E.2. The results show weakly positive estimates between forward linkages and labor productivity, wages, and TFP.

b. Evolution of downstream comparative advantage. What was the relationship between forward linkages and trade development? To explore this, I combine information on linkages with the SITC-level trade data and consider the same regressions as above. As before, I employ 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 E4 shows full estimates). Prior to 1973, forward-linked sectors did not demonstrate a relative export advantage or export productivity over other downstream sectors. Post-1973, Fig. 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 Appendix Figure E2.

The previous section presented evidence of positive, contemporaneous spillovers from industrial policy. However, 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.

c. Downstream linkages: mechanisms, investment, and intermediates. Where the industrial policy affected downstream industries, it likely did so by supplying domestic inputs for their benefit. I analyze this in 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 Appendix Figure E3 (Panels A and B).

d. 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 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 impact of forward linkage exposure. The same pattern is also seen for similar DD estimates using total backward linkages measures (Appendix Table E1). Similarly, the relationship between backward linkage exposure is undetectable for employment, plant entry, and other development outcomes, as seen in Appendix Table E3. I discuss the muted estimates of backward linkages further in Appendix F.

VII.B Robustness and the Stable Unit Treatment Value Assumption

The indirect effects above (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 both output and labor productivity, estimates using the "limited exposure" group (for both direct and total linkages) do not significantly alter the main policy effect (Appendix Figure G1).

Second, I report the main effects while controlling for linkage exposure in the control group. 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, Appendix G.3 provides additional evidence that investment is not crowded out when accounting for linkages.

VII.C Indirect Impact: Discussion

The analysis above demonstrates policy spillovers through linkages to and from treated heavy-chemical industries. 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 impact of backward linkages appears to have been limited and ambiguous, possibly because treated sectors were, by design, upstream. While 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 paper 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 the major elements of the policy 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 impacted the development of industries not targeted by the policy, both 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 labor market outcomes.

Aspects of these findings correspond to arguments posed 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, however, emphasize conventional policy forces rather than miraculous ones. These included the use of directed credit to facilitate investment, the purchase of key intermediates, and the promotion of sectors with dynamic economies and linkage spillovers.

History is not a clean laboratory, and South Korea's experience is no exception. Like many transformations, South Korea's was tumultuous and multifaceted. Nevertheless, this study attempts 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. By doing so, 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. Instead, they point to a potential direction for further empirical work.

The limitations of this study are manifold and show the necessity of further study. Although heavy industrial policy may have promoted forms of industrial development, it did so with costs. The multitude of these costs cannot be accounted for within the scope of this study. 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.

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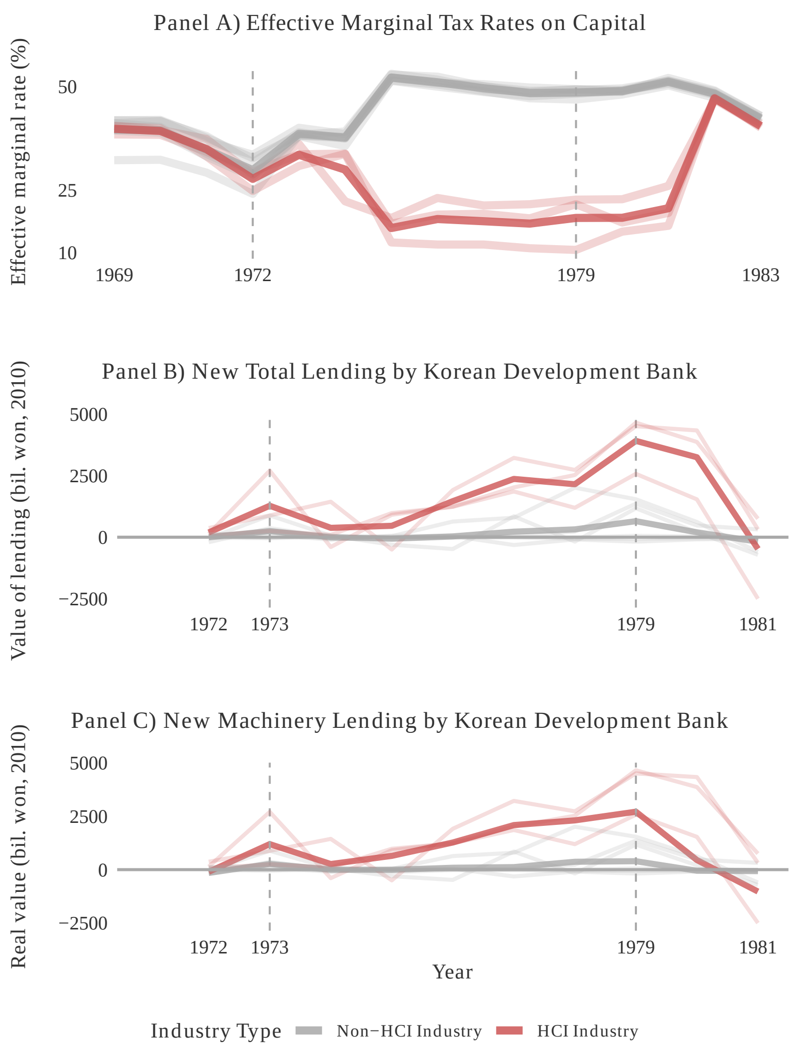

FIGURE I

Industrial Policy Lending and Public Finance: Before, During, and After The Heavy and Chemical Industry Drive

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 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. Panel C plots only changes in lending for machinery, a major component of HCI lending and policy loans.

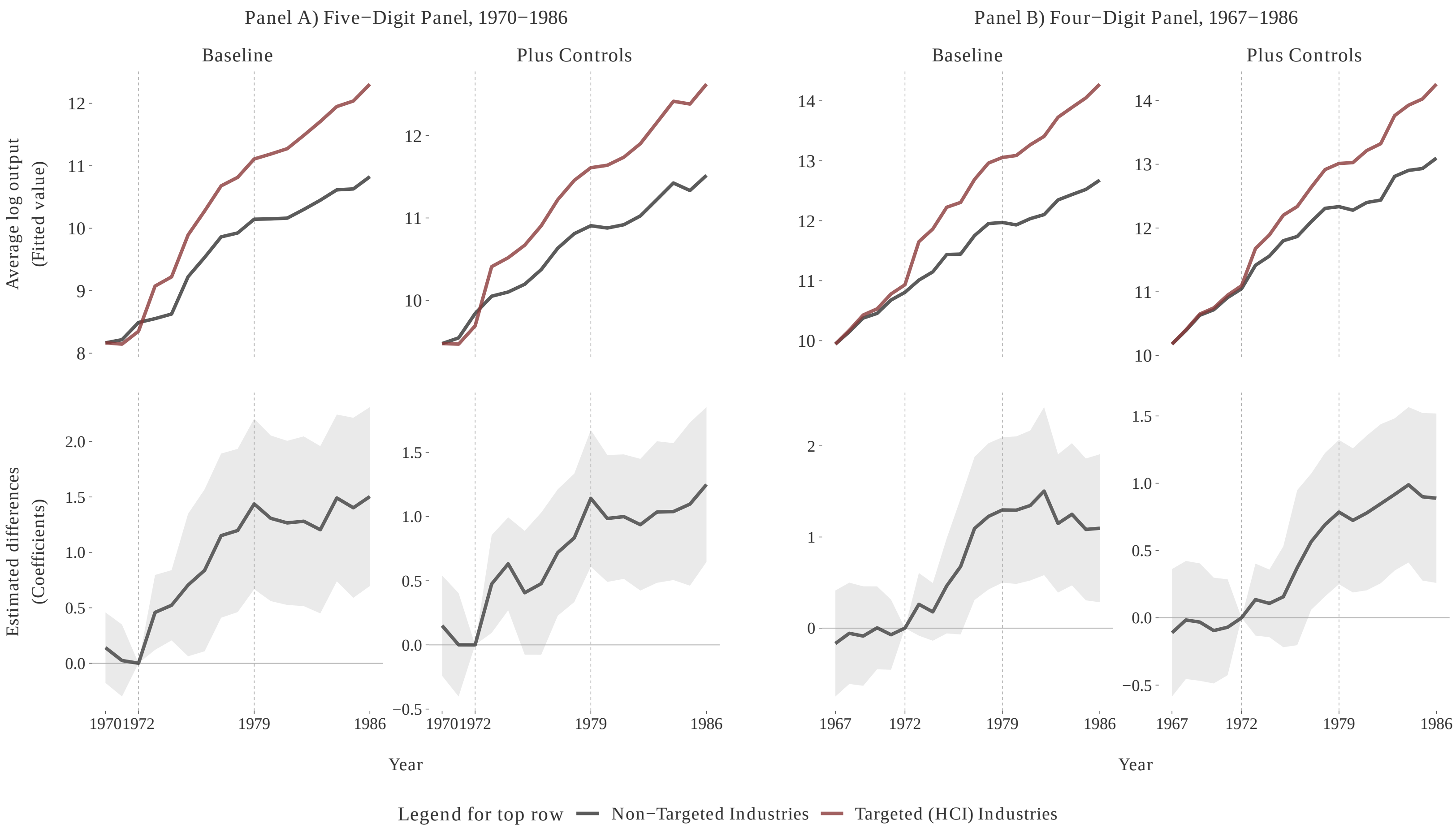

FIGURE II

Industrial Policy and Industry Output

This figure shows the dynamic differences-in-differences 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) 5-digit level panel. Panel B corresponds to estimates for the aggregate (long) 4-digit level panel. 'Baseline' columns are baseline two-way fixed effects regressions, and 'Plus Controls' columns include pre-treatment 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 in time 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. 95 percent confidence intervals are shown in gray.

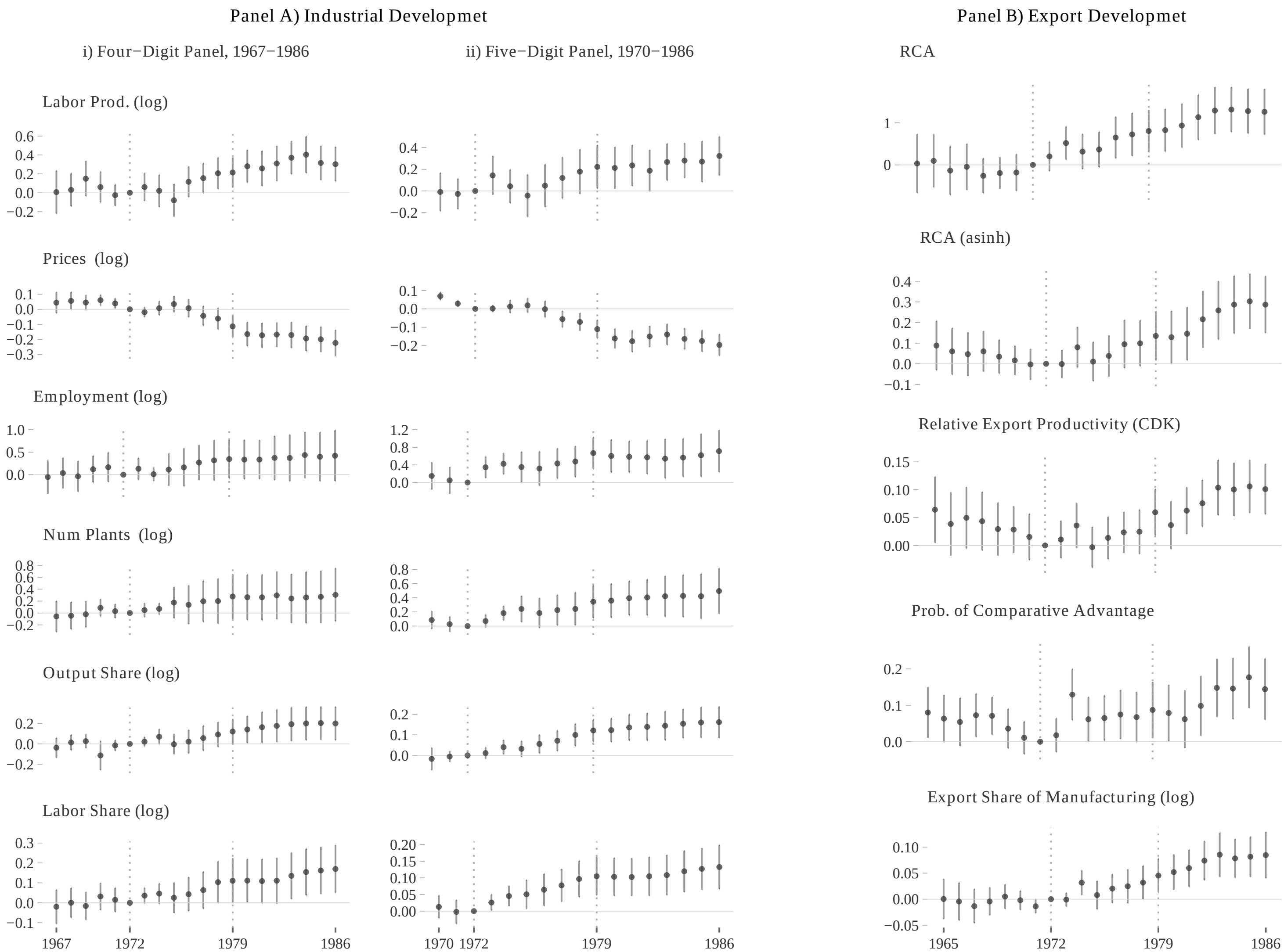

FIGURE III

INDUSTRIAL DEVELOPMENT AND TRADE DEVELOPMENT

This figure shows the dynamic differences-in-differences 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). For Panel A: Left (i) are estimates from long panel data (4-digit), right (ii) are estimates from detailed short panel data (5-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). For 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. 95 percent confidence intervals are shown in gray.

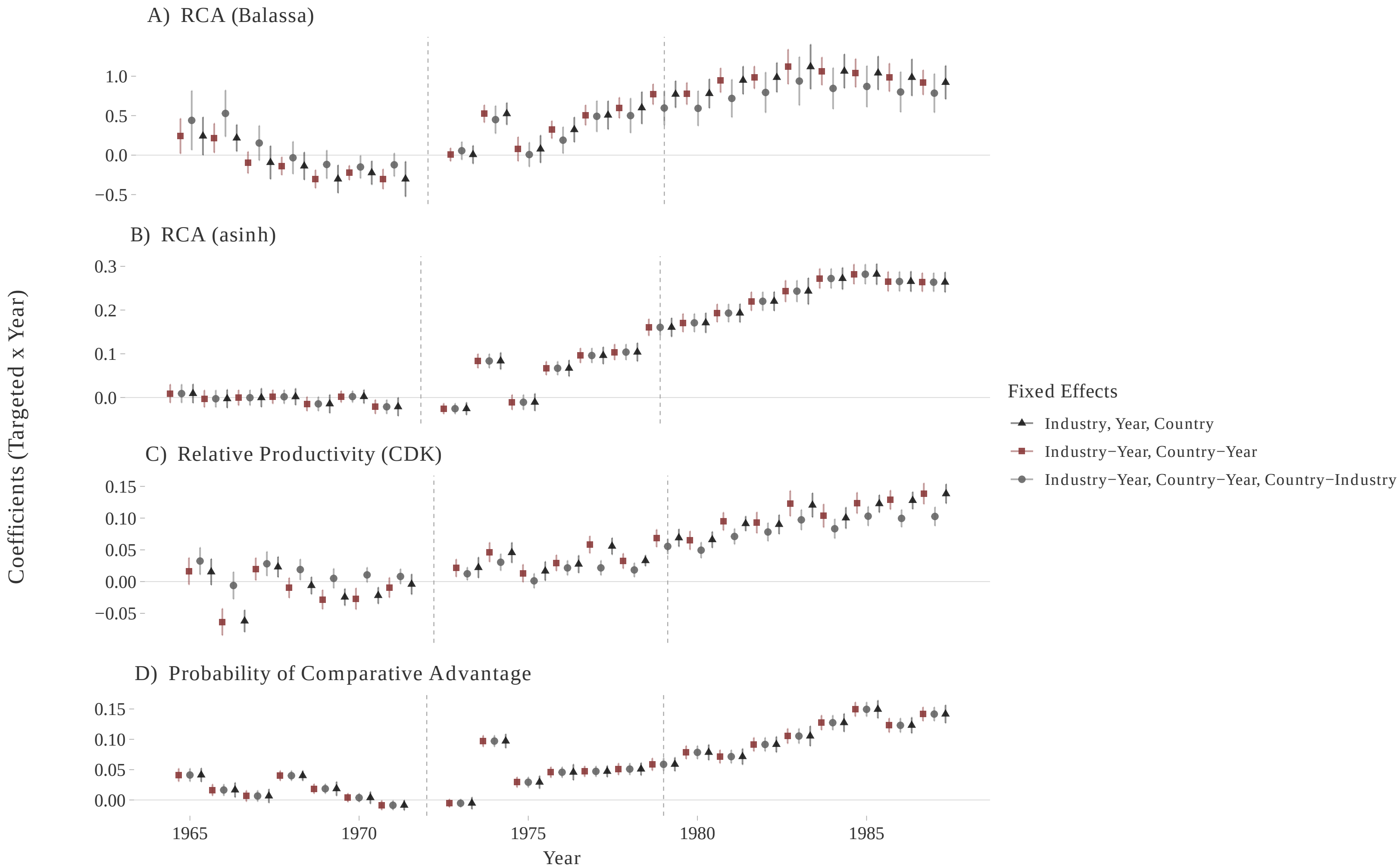

FIGURE IV

Cross-Country (Triple Difference) Estimates: Industrial Policy and Export Development

This figure plots triple difference estimates for the impact of the Korean HCI drive using SITC-level trade data. Specifically, plots show the interaction, Korea × Targeted × Year, estimated from equations (4a)-(4b). Fixed effects are shown in the legend. RCA (Balassa) specifications are estimated using PPML and are not transformed. Alternatively, RCA is transformed using inverse hyperbolic sine to accommodate zeros and 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. 95 percent confidence intervals are shown in gray.

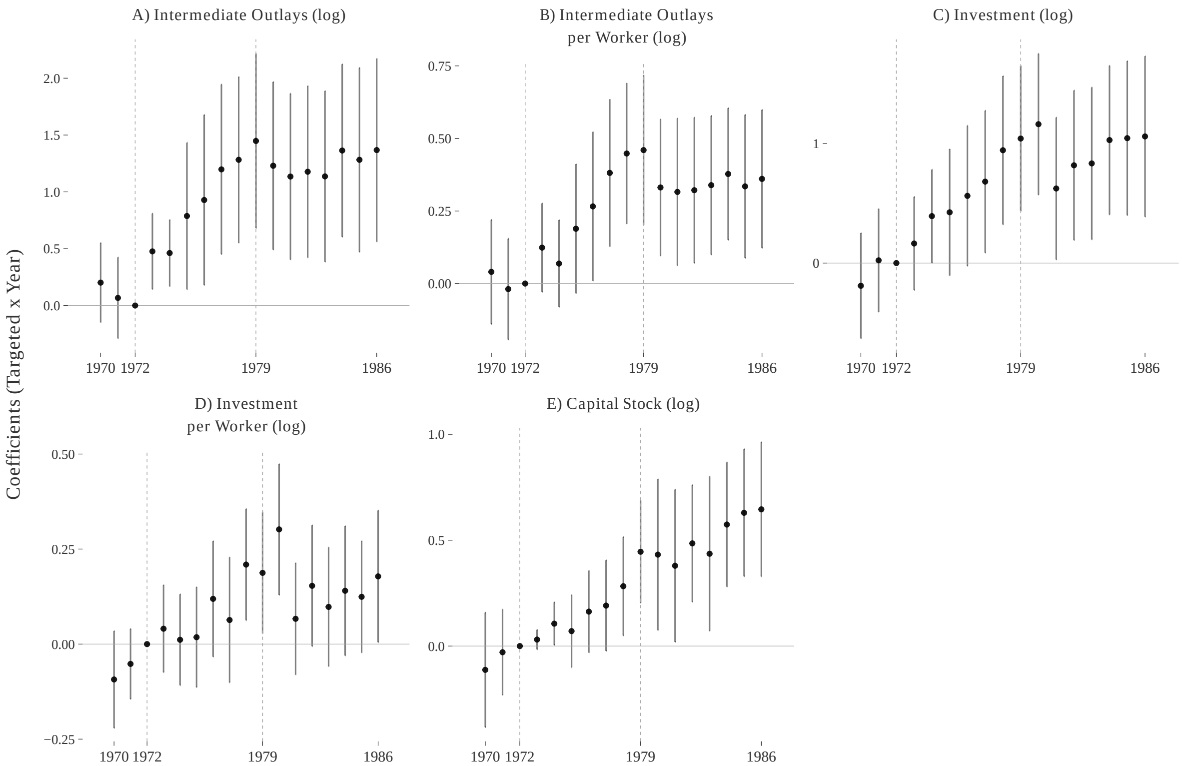

FIGURE V

Changes in Input Use and Investment

This figure plots dynamic differences-in-differences 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 5-digit industry panel (1970-1986). 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. 95 percent confidence intervals are shown in gray.

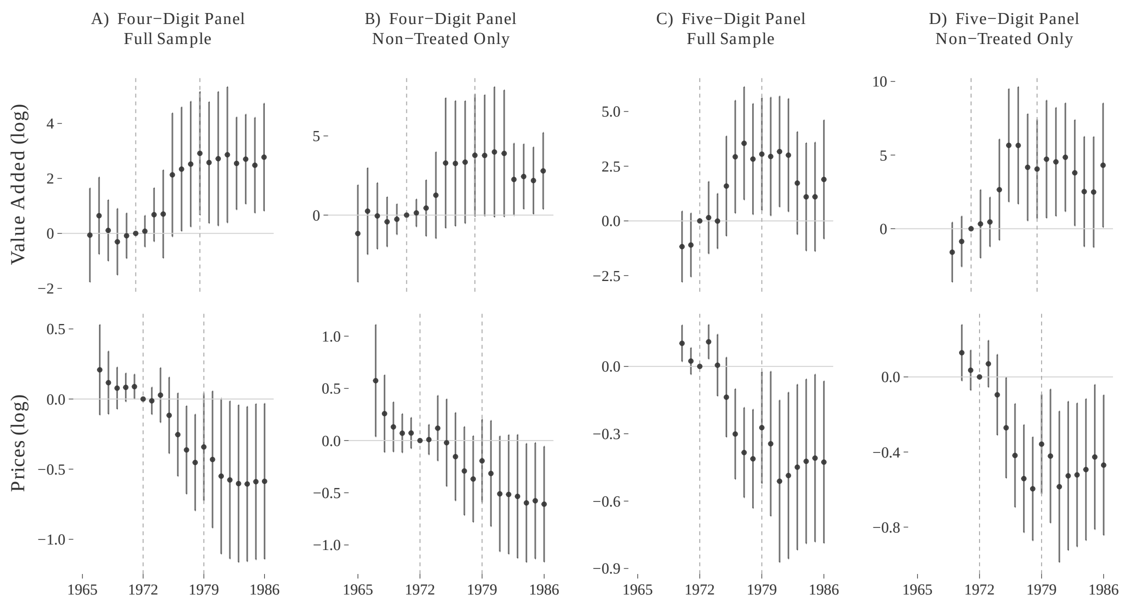

FIGURE VI
Forward Linkages Exposure to Policy: Value Added and Output Prices

This figure plots dynamic differences-in-differences 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). Linkage measures are calculated from the 1970 input-output tables; see 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 (direct forward) are on the y-axis: e.g., Linkage × Year. These estimates come from the DD specification that includes the impact of both measures. Full sample regressions control for the main Targeted × Year effect. 95 percent confidence intervals are shown in gray.

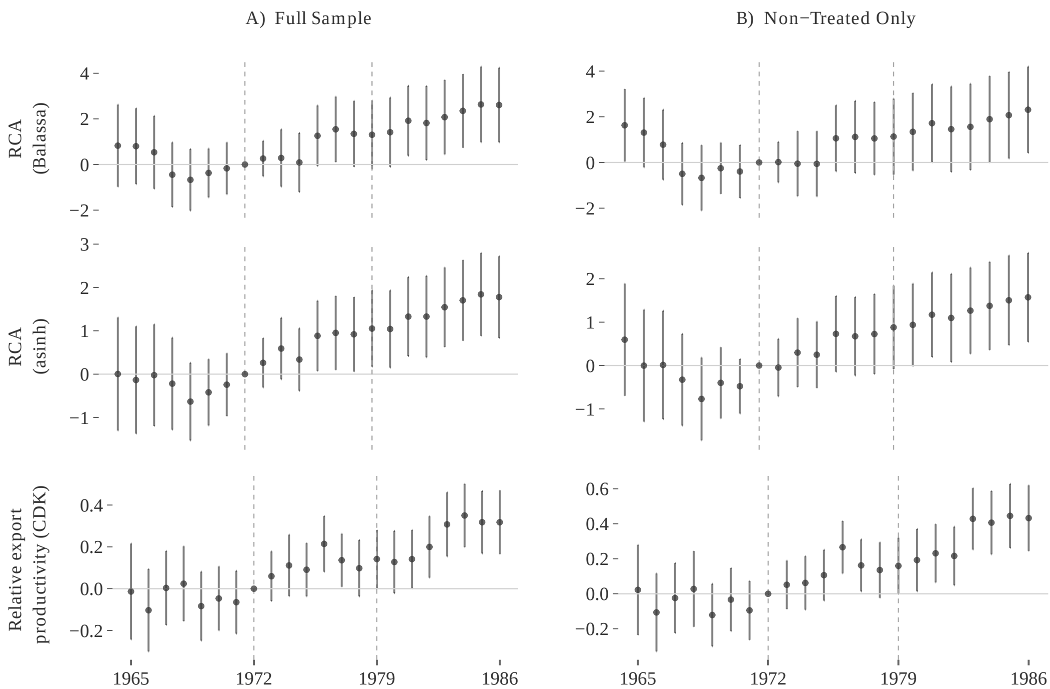

FIGURE VII

Forward Linkages Exposure to Policy: Export Development

This figure plots dynamic differences-in-differences estimates for the relationship between direct forward linkage exposure to HCI 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 0s, 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 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 (direct forward) are on the y-axis: e.g., Linkage × Year. These estimates come from the DD specification that includes the impact of both measures. Full sample regressions control for the main HCI × Year effect. 95 percent confidence intervals are shown in gray.

TABLE I

Differences in Plant-Level Total Factor Productivity, Post-HCI (1980-1986)

	Outcomes: Total Factor Productivity (TFP)				
	(1)	(2)	(3)	(4)	(5)
Targeted	0.061***	0.032***	0.072***	0.057***	0.018**
	(0.011)	(0.009)	(0.012)	(0.010)	(0.009)
Industry × Year	Yes	Yes	Yes	Yes	Yes
R^2	0.466	0.378	0.481	0.350	0.162
Observations	272479	272479	272479	272479	272479
Two-way Cluster (Industry Plant)	488 x 91141	488 x 91141	488 x 91141	488 x 91141	488 x 91141
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), using equation (2). TFP is estimated using Ackerberg-Caves-Frazer (ACF), Levinsohn-Petrin (LP), Olley-Pakes (OP), and Wooldridge (W) methods, shown at the bottom of the table. I also include TFP estimated from simple OLS as a baseline estimate. (TFP is estimated using a log-transformed value added production function). The specification regresses TFP measures on a binary, plant-level dummy indicator for targeted HCI industry. The Targeted indicator is defined by the plant's main industry. All regressions control for year-by-industry (4-digit level) fixed effects. Regressions use two-way clustered standard errors at the plant and industry levels. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.

TABLE II
AVERAGE IMPACT OF INDUSTRIAL POLICY: INDUSTRIAL DEVELOPMENT

	A) Five-Digit Panel		B) Four-Digit Panel	
	Double Robust	TWFE	Double Robust	TWFE
Outcomes (log)	(1)	(2)	(3)	(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 average treatment effect on the treated (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): avg. wages, avg. plant size, intermediate material costs, and labor productivity. Standard errors are clustered at the industry level. Double robust estimators use bootstrapped standard errors (10,000 iterations) and are adjusted to allow for within-industry correlation. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.

TABLE III

Average Impact of Industrial Policy: Export Development

	Type of Estimator		
	Double Robust	TWFE	PPML
Outcomes	(1)	(2)	(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 average treatment effect on the treated (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 text for their calculation. The indicator I[RCA>1] is a binary dummy variable equal to 1 when an industry has achieved comparative advantage, 0 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 material costs, and labor productivity. Standard errors are clustered at the industry level. Double robust estimators use bootstrapped standard errors (10,000 iterations) and are adjusted to allow for within-industry correlation. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.

TABLE IV

Average Impact of Industrial Policy: Input Use and Investment

	A) Five-Digit Panel		B) Four-Digit Panel	
	Double Robust	TWFE	Double Robust	TWFE
Outcomes (log)	(1)	(2)	(3)	(4)
Intermediate Outlays	0.7544***	0.7408***	0.5606**	0.5147**
	(0.1878)	(0.1894)	(0.2374)	(0.2428)
Intermediate Outlays (Per Worker)	0.213***	0.2074***	0.2823***	0.2659***
	(0.069)	(0.0719)	(0.0923)	(0.0975)
Investment	0.6198***	0.6089***	0.2445	0.21
	(0.2211)	(0.2205)	(0.2136)	(0.2186)
Investment (Per Worker)	0.134**	0.1316**	0.1124	0.1048
	(0.0596)	(0.0612)	(0.0855)	(0.0901)

Notes. This table shows the average treatment effect on the treated (ATT) for industrial policy. Average DD estimates are shown for double robust and TWFE estimators. Intermediate outlays (log) is real intermediate input costs. Investment Total (log) is real total gross capital formation. I include both in per worker terms as well. Specifications include controls for pre-1973 industry averages (log): avg. wages, avg. plant size, intermediate material costs, and labor productivity. Standard errors are clustered at the industry level. Double robust estimators use bootstrapped standard errors (10,000 iterations) and are adjusted to allow for within-industry correlation. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.

TABLE V
Mechanisms: Industry-Level Learning by Treatment Status

	Prices (log)		Unit cost (log)		Total Factor Productivity (ACF)		Total Factor Productivity (LP)		Total Factor Productivity (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	0.822	0.876	0.976	0.985	0.983	0.990
Observations	3890	3429	3890	3429	3512	3428	3512	3428	3512	3428
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
(St.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 vs. non-targeted industries. Estimates come from equation (5). The analysis is for the post-1972 period, using the 5-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 are clustered at the industry level. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.

TABLE VI
Mechanisms: Plant and Industry-Level Learning by Treatment Status

	Unit cost (log)			TFP		
	(1)	(2)	(3)	(4)	(5)	(6)
Plant Experience	-0.073***	-0.073***	-0.069***	0.454***	0.456***	0.456***
	(0.002)	(0.002)	(0.002)	(0.010)	(0.010)	(0.010)
Targeted × Plant Experience	-0.010***	-0.008***	-0.005***	0.015**	0.007	0.010
	(0.001)	(0.001)	(0.001)	(0.007)	(0.007)	(0.007)
Industry Experience		-0.008**	-0.006**		0.019*	0.023**
		(0.003)	(0.003)		(0.011)	(0.011)
Targeted × Industry Experience		-0.002*	-0.003**		0.014**	0.014**
		(0.001)	(0.001)		(0.006)	(0.006)
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	0.879	0.879	0.888	0.659	0.659	0.663
Observations	251166	251166	251166	236257	236257	236257
Clusters (Industry and Plant)	489 x 60030	489 x 60030	489 x 60030	489 x 57980	489 x 57980	489 x 57980
Linear Combination (Plant-Level)	-0.082	-0.081	-0.074	0.469	0.463	0.466
(St.Err.)	(0.002)	(0.002)	(0.002)	(0.012)	(0.012)	(0.012)
Linear Combination (Industry-Level)		-0.010	-0.009		0.033	0.037
(St.Err.)		(0.003)	(0.003)		(0.010)	(0.010)

Notes. This table shows the plant-level relationship between industrial outcomes and (log) Experience in targeted vs. 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 4-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. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.

TABLE VII

LINKAGE EXPOSURE AND VALUE ADDED, BEFORE AND AFTER 1973

	Outcome: Value Added (log)			
	A) Five-Digit Panel (1970-1986)		B) Four-Digit Panel (1967-1986)	
	Full Sample	*Non-HCI Sample*	*Full Sample*	*Non-HCI Sample*
	(1)	(2)	(3)	(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	4720	2986	1750	1096
Clusters	278	176	88	55

Notes. Average differences-in-differences 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 i) only non-treated industries and ii) the full sample of industries. Estimates for the full sample separately control for the Targeted × Year effects to account for the main impact of policy. Standard errors are clustered at the industry level. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.

TABLE VIII

LINKAGE EXPOSURE AND OUTPUT PRICES, BEFORE AND AFTER 1973

	Outcome: Output Prices (log)			
	A) Five-Digit Panel (1970-1986)		B) Four-Digit Panel (1967-1986)	
	Full Sample	*Non-HCI Sample*	*Full Sample*	*Non-HCI Sample*
	(1)	(2)	(3)	(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	4721	2987	1751	1097
Clusters	278	176	88	55

Notes. Average differences-in-differences 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 i) only non-treated industries and ii) the full sample of industries. Estimates for the full sample separately control for the Targeted × Year effects to account for the main impact of policy. Standard errors are clustered at the industry level. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.

Appendix

A History Appendix

A.1 United States Troop Withdrawal Threat and the Nixon Shock

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 full American troop withdrawal from the Korean Peninsula (Nixon 1970; Kim 1970; Kwak 2003). Panel B of Figure A1 shows American press coverage of the troop withdrawal, measured by the share of New York Times articles containing "South Korea" and "troop withdrawal." The first peak appeared around 1970 when the United States confirmed its withdrawal from the Peninsula. This confirmation "shocked" the South Korean leadership, who had expected exemptions from Nixon's doctrine (Kwak 2003; Rogers 1970; Trager 1972, p.34). Coverage increases during the 1971 troop pullout of 24,000 troops and three air force battalions. The second jump coincides with the 1976 U.S. presidential contest and Jimmy Carter's election, who further committed to an American pullout (Han 1978; Taylor, Smith, and Mazarr 1990). This pullout was later complicated by the fall of the Park regime during President Carter's administration.[14]

The United States' pivot coincided with growing antagonism from North Korea. Panel A of Figure A1 illustrates North Korea's increasing hostility during the U.S. policy shift, using the full-text archives of two major Korean newspapers, *Dong-A Ilbo* and *Kyunghyang Shinmun*. The Online Data Appendix II.1 describes the data construction and the use of a `word2vec`-style Korean language model. The data shows the number of articles covering military antagonism, counted using a dictionary of Korean-language keywords related to military hostility. Panel A traces a series of high-profile security emergencies that tipped the Park regime into crisis (Scobell and Sanford 2007; Kim and Im 2001). Online Data Appendix II.1 demonstrates that these patterns are robust to alternative data sources.

A.2 Commercial Banks and Policy Loans During the HCI Drive

Appendix Figure A2 illustrates commercial bank loans during the heavy-chemical drive period. Although technically private, the commercial banking sector was deeply intertwined with the state throughout the Park era. Commercial deposit banks played a significant role in this period. They distributed 60 percent of policy loans during the 1970s (Cho and Kim 1995; World Bank 1993).

Panel A of Appendix Figure A2 reveals that before the heavy industry drive, the value of new loans from commercial deposit banks was similar across sectors. However, it rose sharply for targeted sectors after 1973. After 1979, new total heavy industry lending declined. In contrast to the Korean Development Bank (Figure I in paper), total private lending continued. These post-1979 policy loans were qualitatively different; liberalization removed preferential rates and equalized borrowing

14. See Online History Appendix I for details.

costs across industries (Lee 1991; Woo 1991, pp.443-444). For more information about the liberalization of the banking sector, refer to Online Appendix I.2.6.

B Direct Impact Appendix

B.1 Further Analysis: Labor Productivity and Prices

An initial interpretation of the event study estimates in Figure III might suggest that prices declined for HCI versus non-HCI industries and that pre-1973 pre-trends indicate a literal downward trend in prices for HCI-targeted industries. However, the top row of Appendix Figure B2 Panel A (five-digit panel) reveals that the trends between the two industries are similar throughout the mid-1970s and diverge over the policy period.

Labor productivity rises through the HCI period, which is notable in the five-digit data and less precisely estimated in the four-digit data. The top row of Figure B2 also demonstrates that the effects on labor productivity stem from increased labor productivity for treated industries rather than a decline in non-treated industries.

Appendix Figure B2 (Panel B) shows that average prices increased during the inflationary 1970s. However, HCI prices diverged from the control industry averages and did not increase as sharply over this inflationary period. These price effects, shown in Figure III and Appendix Figure B2, contrast with industrial policy experiences elsewhere, where inefficient industrial policy has typically increased the prices for targeted outputs.

A positive relationship between prices and industrial policy may be the norm rather than the exception. For example, heavy industrial policy in Egypt, India, and Turkey may have effectively increased the relative price of capital and intermediate goods (Schmitz Jr 2001). For a case study on steel, see (Blonigen 2016), which shows how heavy industrial policies can raise output prices, to the detriment of downstream exporters.

B.2 Robustness: Direct Impacts

B.2.1 Robustness: Industry-Level TFP

This section explores the relationship between the HCI industrial policy package and estimated total factor productivity (TFP) using the more granular (five-digit) industry-level panel (1970-1986).[15] Although I estimate industry-level TFP over the study period, I emphasize caution. Modern best practices for estimating TFP focus on micro-econometric estimation strategies and corrections modeled by micro-level behavior (Beveren 2012). For this reason and more, the following industry-level estimates may have limitations.

Practically, aggregate data can limit the power to estimate production function parameters and may exacerbate measurement issues that confound TFP estimation

15. The short, five-digit data contains capital stock data and is subject to less harmonization/aggregation. See Data Section IV. For aggregation and harmonization of the four-digit data, refer to Online Data Appendix II.

(see Diewert 2000). Market imperfections may further complicate TFP estimation, especially in distorted miracle economies (Felipe 1999; Fernald and Neiman 2011). Aggregate data precludes some micro-level corrections. Nevertheless, I estimate industry-level TFP using common estimation strategies discussed in Section V.D.1.

I estimate (log-linearized value added) production function parameters at approximately the two-digit level. To improve power, I combine sectors with sparse observations to properly estimate production function parameters when additional power is required.[16] Following the empirical TFP literature, I Winsorize estimates for extreme values.

Figure B3 shows estimates of HCI's impact on industry-level TFP using five common measures. To be conservative, I use 1970 as the baseline for regression estimates of total factor productivity. Figure B3 demonstrates that 1972 was a particularly low year for HCI TFP and DD estimates; using 1972 as the baseline can overstate post-1972 TFP differences.

Figure B3 reveals a slow upward trend in TFP for targeted industries relative to non-targeted industries over the study period. Although estimates are noisy and vary across TFP outcomes, they show a slight increase in TFP for HCI industries. For the (limited) pre-1973 period, TFP in the targeted industries seemed stagnant, perhaps even declining; after 1973, this trend reverses and estimates gain momentum through the later 1970s. TFP measures become significant post-1979 across the board.

Earlier studies stressed that HCI industries experienced low productivity growth (Dollar and Sokoloff 1990), yet early work tends not to consider the *relative* trends in TFP before and after the intervention. Some limited relative growth (in TFP) over the period matches earlier analysis (Felipe 1999). Moreover, a slight upward trajectory appears compatible with a story of industrial learning taking time and perhaps being promoted further by post-1979 liberalization.

B.2.2 Robustness: Dynamic Doubly Robust DD Results

For robustness, I demonstrate that the patterns observed in two-way fixed effects (TWFE) difference-in-differences (DD) estimates also appear in estimates using the doubly robust estimator of Sant'Anna and Zhao (2020) and Callaway and Sant'Anna (2020) (see Section V.E for description). I use the same outcomes and controls as in the standard TWFE estimates for Equation 1. The adjustments performed by the doubly robust estimator rely on controls, so only specifications with controls are used.[17] I provide bootstrap confidence intervals at the 95 percent level.

Appendix Figures B4–B6 present estimates from the doubly robust estimator (eq. 3). Figure B6 reports estimates for export development outcomes aggregated to the four-digit KSIC industry level. The patterns in Figures B4–B6 are qualitatively similar to the linear TWFE estimates.

Consider first the relationship between HCI and industrial development given by Appendix Figures B4 (four-digit panels) and B5 (five-digit panels). Although the doubly robust DD relaxes some assumptions related to the traditional TWFE DD,

16. For example, some mining and minerals processing sectors contain limited five-digit industries, so a broader two-digit category is used.

17. Without controls, the estimator package defaults to a standard TWFE method.

the general dynamic pattern associated with HCI remains robust. This is particularly important because this estimator re-weights the treatment and control groups. In other words, the same dynamics shown in the OLS TWFE estimates are in the semi-parametric DD estimates in the main paper. See the main paper for the comparisons between the average estimates.

C Direct Impact on Trade Appendix

C.1 Probability of Achieving Comparative Advantage in Heavy Industry

Table C1 examines the probability of achieving a comparative advantage in heavy-chemical goods in Korea versus control countries. I restrict data to the post-1972 period and focus on HCI products only, using the regression:

$$Y_{ict} = \alpha_{kt} + \beta_1 \text{Korea}_i + \beta_2 \ln\left(\text{Income}_{i,1972}\right) + \epsilon_{ict} \tag{8}$$

For completeness, I present both PPML and linear probability estimates. The linear probability estimates, columns (1)-(4), provide a more straightforward interpretation.

For 1972–1986, the average country had a comparative advantage in 7.8 percent of HCI products; mean in column (1). Estimates in Table C1 show that—across samples and estimates—Korea had a significantly higher probability of achieving comparative advantage in heavy-chemical industry goods. The effect of the Korea indicator is highly significant across specifications, including after controlling for 1972 income per capita (PPP adjusted, 2010 dollars) in columns (2) and (6).

Likewise, additional estimates in Table C1 demonstrate that Korea had a significantly higher probability of achieving comparative advantage when we limit estimates: First, (i) to sample countries in the same pre-treatment income decile in columns (4) and (8). Second, (ii) to countries in similar income deciles, defined as those in the same decile *and* those in the immediate deciles above and below Korea's (1972) income group, in columns (3) and (7).

D Policy and Mechanisms Appendix

D.1 Investment and Industrial Policy Discussion

Is it obvious that we would observe responses to investment or production incentives from industrial policy? Based on the history of industrial policy, no. If financial policies are redundant, they may not create new investment (outlays)—investment (outlays) that would have still occurred without policy. Likewise, in many contexts, *de jure* investment policy may not bind. Work by Lazzarini et al. (2015) shows that for Brazil, capital from a major national development bank did not translate into increased investment and was allocated to politically connected firms where investments would otherwise have taken place. For East Asia, Yang (1993) argues that investment subsidies in Taiwan did not contribute to capital formation, echoing a common criticism of industrial investment schemes: that investment would have occurred anyways.

D.2 Policy Mechanisms: The Impact of Directed Credit and Marginal Revenue Product of Capital

I explore the relationship between high-MRPK versus low-MRPK industries and input use. Specifically, I test (i) whether input use increased differentially for industries with a high marginal revenue product of capital and (ii) whether this increase occurred specifically for treated industries.

The MRPK calculation is constrained by industry-level (as opposed to micro) data and is calculated for the most disaggregated five-digit panel. The marginal revenue product of capital for industry i is $\text{MRPK}_i = \alpha_i^k \times (\text{Revenue}_i/K_i)$. I calculate a version of the measure proposed by Bau and Matray (2021), using total sales (real shipments) divided by total tangible capital stock. I estimate capital coefficients α_i^k at the two-digit level.[18] Industries are then split into high-MRPK or low-MRPK groups based on whether they are above or below the median level of MRPK.

I then consider the following regression equation:

$$\ln\left(\text{input}_{it}\right) = \alpha_i + \alpha_t + \sum_{j \neq 1972} \beta_j \left(\text{High-MRPK}_i \times \text{Year}_t^j\right) + \epsilon_{it} \tag{9}$$

where the outcome $\ln\left(\text{input}_{it}\right)$ is investment or intermediate input use for industry i at year t. I estimate equation (9) separately for targeted and non-targeted industries. The set of coefficients β_j conveys differences in input use between high-MRPK and low-MRPK industries, relative to 1972. In other words, the estimates in (9) reveal whether inputs respond for those sectors most exposed to HCI credit policies (e.g., those described in Section II.B). Specifically, whether this relationship is seen for targeted industry during the drive period.

Appendix Figure D2 illustrates the relationship between MRPK and the increase in input use. I estimate regressions separately for targeted and non-targeted industries. Panels A-B show estimates for (log) total material outlays and real total investment. Panels A-B demonstrate that inputs increased in high-MRPK industries relative to low-MRPK industries after 1973, but only for targeted industries. Similarly, high-MRPK industries show increases in (log) labor (Panel C) and, consequently, output, measured as the log real output shipped (Panel D).

Thus, the estimates in Figure D2 suggest that policy differentially relaxed constraints for high-MRPK industries, leading to an increase in input use. Note that these results do not imply MRPK convergence or reduced misallocation due to the policy. Instead, they provide indirect evidence that credit expansion operated differentially for targeted industries.

The expansion in credit to targeted industries during the policy drive shares similarities with the directed credit literature and the macroeconomics literature on credit booms and instability (Gorton and Ordoñez 2020; Mendoza and Terrones 2008). While this literature has emphasized the aggregate correlates of credit booms, the sectors receiving credit may also have significant implications for the impact of credit booms in industrializing economies.

18. Capital shares are calculated using pre-HCI drive shares.

D.3 Robustness: Testing Investment Crowding Out

To explore crowding out, I compare patterns of investment in targeted and non-targeted sectors using a simple regression analysis. Specifically, I first regress (log) investment outcomes on year effects, controlling for five-digit industry fixed effects:

$$\ln\left(\text{investment}_{it}\right) = \alpha_i + \sum_{j \neq 1972} \beta_j \cdot \text{Year}_t^j + \epsilon_{it}. \tag{10}$$

I report the estimates for equation (10) separately in Panel A, Appendix Figure D3. Panel A shows investment patterns for each sector relative to 1972, revealing no evidence of crowding out during the drive. Instead, it demonstrates a relative increase in investment for both manufacturing sectors, with targeted heavy industry experiencing a more substantial increase.

To examine potential crowding out in capital-intensive, non-targeted industries, Panel B of Appendix Figure D3 illustrates the impact of pre-treatment capital intensity on investment during the HCI period. It plots coefficients from the interaction $\text{Year}_t \times \ln(\text{Capital Intensity})_{i0}$, where capital intensity is measured using pre-1973 capital stock per employee. Like Panel A, estimates in Panel B are presented separately for targeted and non-targeted samples.

Panel B of Appendix Figure D3 shows no relative decline in investment for capital-intensive, non-treated sectors during the drive. The relationship between capital intensity and investment remains mostly neutral for both HCI and non-HCI sectors during this period. An exception occurs in the early part of the drive when investment increases in capital-intensive industries across both sectors. Post-liberalization, more capital flowed to capital-intensive sectors. However, in HCI sectors, which typically have higher capital intensity, the relationship between capital intensity and investment remains neutral, even after liberalization. Additionally, Panel B of Figure D2 demonstrated that investment did not differentially change for high-MRPK versus low-MRPK industries during the drive.

D.4 HCI, Trade Policy, and Nominal Protectionism

This following analysis considers evidence of overt nominal protectionism of targeted heavy industry (Section VI.B). Before considering quantitative evidence, I first turn to the conceptual and historical context for South Korean trade policy over the period.

D.4.1 Historical Context: HCI as ISI?

Although the HCI period has been associated with rising protectionism or import substitution-style industrialization (ISI) policies (Kim 1990; Yoo 1990; Lee 1992), the reality is more complex. Since the 1960s, South Korea underwent a "continuous process of tariff reform" under Park Chung-hee (General Agreement on Tariffs and Trade 1992, p.52), including multiple rounds of tariff cuts during the HCI period (General Agreement on Tariffs and Trade: Balance-of-Payments Committee 1978;

Young 1988).[19] Average import liberalization ratios gradually increased from 1973 to 1979.[20] Exemptions from trade policy were widely used in the 1960s and during the heavy industry drive. Consequently, reported tariffs and quantitative restrictions may represent a theoretical upper bound for an industry's effective protection (Yoo 1993).[21]

D.4.2 Trade Policy Analysis

Having established the qualitative patterns above, I now quantitatively study the role of trade policy over the heavy industry drive period.

Before regression analysis, however, it is worth considering the aggregate data presented in Figure D1. Panels C and D of Figure D1 show two simple, aggregate measures of market protection across targeted and non-targeted industries for five periods: 1968, 1974, 1978, 1980, and 1982.[22] Panel D reports the average tariff rates (percent), and Panel C presents measures of quantitative restriction (QR) coverage. These panels demonstrate that output protection, measured in terms of tariffs and QR coverage, was lower in targeted sectors versus non-targeted sectors. Panel D shows that average measures of nominal tariff protection fell continually through the period. QRs in Panel C had a slight rise in the 1970s, but fell by 1982.

Furthermore, Figure D4 plots the distribution of protection by treatment status for the same period. The histograms in Figure D4 show a steady convergence in the distribution of nominal (output) protection between targeted and non-targeted sectors from 1968 to 1982. Liberalization would proceed fully after 1982. For details of liberalization refer to Section II and Online Appendix I.2.6. Note that in Figure D4 we also see a mass of low tariff and QR protection for targeted industry.

Let us now turn to a regression analysis and consider the following specification,

$$Y_{it} = \alpha + \beta \cdot \left(\text{Targeted}_i\right) + \tau_t + X_i'\Omega + \epsilon_{it} \tag{11}$$

where i are industries and t are the five periods. Specification (11) controls for period effects, τ_t, and includes baseline controls (log avg. wages, material outlays, avg. plant size, and labor productivity). I estimate this relationship in terms of levels *and* differences, Y_{it} and ΔY_{it}. The coefficient of interest, β, provides the difference in the average level—or change—in policy between heavy and non-heavy industries from 1968 to 1982.

Table D3 Panel A first considers differences in output protection between treated and non-treated sectors. Panel A reports that the *level* of output protection was, on average, significantly lower for targeted heavy industries: columns (1–4) show this for log tariffs, and columns (5–8), for QR coverage. Estimates (cols. 3–4) imply that the level of tariffs were significantly lower for targeted industry, even during HCI.

19. The 1978 GATT Consultation reports tariff reductions in 1973, 1974, July 1975, December 1976, January 1977, January - November 1977, and April-July 1978 (General Agreement on Tariffs and Trade: Balance-of-Payments Committee 1978, p.6).

20. Economic instability in 1979–1980 postponed further import liberalization, planned in 1978, until the post-HCI era (Kim 1988). See Online Appendix I.2.6 for more information.

21. For example, income from customs duties accounted for less than 14 percent of total tax revenue in 1975.

22. Trade policy data is limited to these periods; refer to Section IV.

Quantitative restrictions were also lower (cols. 7–8). Panel A, columns (9–12) of Table D3 report estimated changes in output protection between 1968 and 1982. Estimates are positive, though imprecisely estimated. Changes in log QRs are significantly higher for column (11). Despite some shallow growth in QRs, the level of output protection was significantly lower for treated industry.

However, heavy and chemical industries were also assisted by trade policy *vis-à-vis* exemptions on imported inputs (see Section II). Table D3 Panel B shows differential exposure to input protection using industry-level measures of input protection built from input-output tables (Section IV). These measures account for potential exemptions afforded to targeted industries during the drive. Panel B shows that targeted industry had significantly lower levels of input protection (cols. 1–8) than non-targeted industry. Likewise, targeted industry see significant *reductions* in input exposure for tariffs (cols. 9–10) and QRs (cols. 11–12).

In sum, the analysis above does not provide strong evidence that the heavy chemical-industry drive meant an appreciable rise in conventional means of market protection. The findings comport with general trends in liberalization and South Korea's incorporation into the multilateral institutions during the Park era.

E Linkage Appendix

E.1 Linkage Measurement

The linkage measures in this study capture exposure to HCI industrial policy through backward and forward linkages. Note that the measures below do not model the causal relations. Rather, they are proxies capturing the extent to which an industry is exposed to policy indirectly through inter-industry linkages. A long literature in input-output economics has considered far more complicated means of measuring and decomposing linkage effects. The following is a simple baseline implementation of backward and forward linkage measures.

E.1.1 Direct Linkages

First, consider exposure to industrial policy through backward linkages: this is when the impact of industrial policy propagates to upstream suppliers (through the backward linkages with treated sectors). Let i be a non-targeted industry that sells its output to a treated industry j. Industry i's exposure to the industrial policy through backward linkages is equal to,

$$\text{Backward Linkage}_i = \sum_{j \in \text{HCI}} \alpha_{ij} \quad \text{with} \quad \alpha_{ij} = \frac{\text{x}_{ij}}{\text{x}_j}, \tag{12}$$

where α_{ij} represents the share of i's sales to treated heavy-chemical industries j ($j \in$ HCI). Specifically, α_{ij} is the proportion of i used to produce one unit of output j, calculated as the value of i's sales to industry j, x_{ij}, divided by the value of j's total output: $x_j = \sum_{i=1}^{n} x_{ij}$.[23] The coefficients α_{ij} come from technical matrix:[24]

$$A = \begin{bmatrix} \alpha_{11} & \alpha_{12} & \cdots & \alpha_{1n} \\ \alpha_{21} & \alpha_{22} & \cdots & \alpha_{2n} \\ \vdots & \vdots & \ddots & \vdots \\ \alpha_{n1} & \alpha_{n2} & \cdots & \alpha_{nn} \end{bmatrix}. \tag{13}$$

Practically, to calculate the backward linkage measure (12), I take the row-wise sum of elements from the technical coefficient table (13). This means that for each row i, I add the coefficients across columns j corresponding to HCI sectors.

Second, consider exposure to industrial policy through forward linkages. In this case, industrial policy propagates *downstream* to purchasers (through *forward linkages* from HCI sectors). In this case, let i be an untreated sector that purchases inputs from a treated sector j. The forward linkage analog of equation (12) is the following,

$$\text{Forward Linkage}_i = \sum_{j \in \text{HCI}} \alpha_{ji} \quad \text{with} \quad \alpha_{ji} = \frac{\mathrm{x}_{ji}}{\mathrm{x}_i}. \tag{14}$$

where α_{ji} denotes the sales from treated industry j to downstream industry i (x_{ji}), per unit of i's total output (x_i). Practically, to calculate the exposure to policy through forward linkages (12), I take the column-wise sum of elements from table (13). That is, for each column i, I add coefficients across rows j corresponding to HCI sectors. As with backward linkages, I exclude diagonal elements.

E.1.2 Total Linkages

In addition, I also calculate the exposure of non-treated industries to HCI policy through total—direct and indirect—links with treated industries. Equations (12)-(14) above capture the extent to which HCI policy propagates through *direct*, or first-degree, connections. I now consider the total n-degree effects; I calculate Total Backward Linkages and Total Forward Linkage Measures using a method analogous to the direct linkages described above. Instead of using coefficients α_{ij} from the coefficient matrix A, I use coefficients ℓ_{ij} from the Leontief matrix:

23. The denominator x_j includes j's output sold to all sectors, including manufacturing, services, and final output. I follow the literature and do not count i's sales to itself and exclude diagonal elements α_{ii} in the input-output matrix (e.g., $\alpha_{11} = 0$).

24. I calculate the matrix A manually for 1970 from the table of inter-industry flows $X = [x_{ij}]_{n \times n}$. The vector x is a vector of the total output sold by each sector. I compute $A = X\left[\text{diag}(x)\right]^{-1}$, and each element is $\alpha_{ij} = x_{ij}/x_j$.

$$L = \begin{bmatrix} \ell_{11} & \ell_{12} & \cdots & \ell_{1n} \\ \ell_{21} & \ell_{22} & \cdots & \ell_{2n} \\ \vdots & \vdots & \ddots & \vdots \\ \ell_{n1} & \ell_{n2} & \cdots & \ell_{nn} \end{bmatrix}. \tag{15}$$

The Leontief inverse matrix in equation (15) is calculated from the technical coefficient matrix A (eq. 13). More precisely, $L = (I - A)^{-1}$, where I is the identity of matrix A. The matrix L, or the Leontief inverse, captures the full chain of inter-industry relationships between sectors.

I calculate the total exposure between treated HCI industry j and non-treated industry i using elements from table L. Formally, the two measures are

$$\text{Total Backward Linkages}_i = \sum_{j \in \text{HCI}} \ell_{ij} \tag{16a}$$

$$\text{Total Forward Linkages}_i = \sum_{j \in \text{HCI}} \ell_{ji}. \tag{16b}$$

Industry i's total exposure to policy through backward linkages is given by equation (16a), which equals the sum of coefficients between supplier i to each HCI purchaser j. To compute (16a), I perform row-wise calculations over matrix L: for each row i, I sum across columns j that correspond to HCI industries. Similarly, i's total exposure to policy through forward linkages is given by (16b), which equals the sum of coefficients between HCI supplier j and the purchasing industry i. To compute (16b), I perform column-wise calculations over elements of matrix L: for each column i, I sum across rows j that correspond to HCI industries.

E.2 *Forward Linkage Appendix: Developmental Effects*

a. Total Forward Linkages, Output, and Prices. This section considers the total linkage effects of policy in more detail. Table E1 reports pre-post estimates for total forward linkages, those accounting for n-degree linkages between downstream industries and HCI suppliers. Like the direct linkages, Table E1 reports a robust relationship between total forward linkage exposure and the change in downstream value added. These total effects are strongest in the non-HCI sample. Likewise, Table E2 shows the average pre-post impact of total linkages on output prices. The estimates for total forward linkages are negative across specifications in Table E2.

b. Forward Linkages and Other Development Outcomes. I now consider the impact of forward linkages on outcomes besides log output (value added) and log prices. These results are provided in Appendix Figure E1. Beyond these core outcomes, I observe similar patterns across outcomes, such as entry into and higher employment in downstream sectors with stronger connections. Likewise, I find a weak relationship between forward linkages and productivity outcomes. Appendix Tables E3 and E4 show pre-post estimates for direct and total linkages, respectively.

E.3 Forward Linkage Appendix: Mechanisms and Intermediate Input Use

Figure E3 examines input use and investment among industries with more versus less exposure to HCI suppliers. Pre-1973, differences in (log) total intermediate outlays and (log) investment were closing for sectors with differential forward links to HCI suppliers. After 1973, the trend reversed; Figure E3, Panel A shows a jump in material outlays (Panel A, top row) and total investment (Panel A, bottom row). The post-1973 divergence is seen in both non-HCI and full samples, and across data sets. Likewise, these estimates are strong when limited to non-HCI-targeted industries. Joint F-tests reject pre-trends across most specifications, shown in the Online Appendix E5, except four-digit panels, where inputs trended upward and converged before 1973.

Additionally, Panel B of Figure E3 shows qualitatively similar effects for the total forward linkage exposure. Although, the effects are less precisely estimated for the total linkage effects (see Online Table E5 for the full regression table). Thus, during the HCI period, direct downstream users of HCI inputs expanded outlays and inputs during the drive.

F Backward Linkage Results

Although estimates for forward linkages correspond with the industrial development of downstream industries, backward linkages do not. Broadly, the effects are weak and quite limited. This is seen in estimates for output in Table VII for direct linkages exposure and Appendix Table E1 for total linkage exposure. The impact of forward linkage exposure is consistently stronger than noisy backward linkage effects.

In the case of output, the higher backward linkage exposure–direct (Table VII) or total (App. Table E1)–is negatively related to log value added in upstream industry. Yet, estimates are mostly imprecise. The indeterminate impact of backward linkages is also seen in Appendix Figures F1-F2, which show dynamic estimates for output. The Figures show the ambiguous, weak relationship between backward linkage exposure and upstream output–both for direct and total backward linkage exposure.

G SUTVA and Linkage Appendix

G.1 Main Effects, Restricting Estimates to Low-Linkage Control Industries

Figure G1 shows TWFE event study estimates (eq. 1) for output and labor productivity, but with alternative control groups. Specifically, I restrict the control groups to *only* industries with low downstream linkages (triangles) or low upstream linkages (squares). To do so, I split non-targeted industries into those with low and high linkage exposure to HCI sectors. Specifically, I base these categories on whether they are below or above Forward Linkage$_i$ (Backward Linkage$_i$). I then re-run baseline DD specifications with these truncated control groups.

For both output and labor productivity, estimates using a "low forward linkage" control group increase slightly, and the baseline pattern is preserved. Intuitively, it would make sense that the main effects of HCI increases, since I remove the control industries most likely to benefit from positive policy spillovers (e.g. those with high forward linkage exposure). Standard errors increase, which is not surprising given the truncated sample.

Across outcomes, Figure G1 shows that limiting control industries to those with low upstream connections has a minimal impact on point estimates for the main, direct impact of HCI (e.g. Targeted$_i \times$Year$_t$). This is expected, as the upstream linkage effects of the policy were more muted than the downstream effects (and slightly negative); see: Section VII. In sum, limiting the impact of the strongest first-order linkage effects on the control group is insufficient to overcome the main direct impact of HCI.

G.2 *Main Effects, Controlling for Linkages*

I now test whether the main DD estimates survive after including these effects. I do so by rerunning the main regression equation (1), now saturated with linkage controls. That is, specifically controlling for linkage exposure for non-treated industries. Linkages are multiplied by an indicator equal to one for non-treated industry, zero for treated.

Figure G2 Panel A shows baseline results for the main effect, Targeted$_i \times$Year$_t$, versus estimates that include varieties of linkage controls. These results are given for both direct linkages (left) and total (Leontief) linkages (right). The baseline estimates are in red, and those controlling for linkages are in dark gray. I control for linkages using the interaction Forward Linkage$_i$ $\times$ Post$_t$, which controls for the linkages more parsimoniously. (Controlling flexibly for linkages, Forward Backward Linkage$_i$ $\times$ Year$_t$, significantly increases the number of parameters.)

Once I control for the positive downstream spillovers in non-treated industries, Panel A (Fig. G2) shows that the main direct effect Targeted$_i$ $\times$ Year$_t$ becomes larger. Furthermore, estimates are more prominent after controlling for the total linkage effects. This is intuitive, as positive spillovers means may also benefit the control group, and thus bias baseline estimates downward. Recall, I have demonstrated in Section VII.A (d.) that there may have been weak negative spillovers into backward-linked industries (direct linkages). This is seen specifically for five-digit panel estimates, more precisely pickup the linkage effects.

Panel B in Figure G2 builds off the regressions in Panel A, but now includes controls for *both* backward and forward linkage exposure. Panel B shows that including both linkages maintains the main pattern, while increasing the standard errors. The main effect estimates are now less positive than those in Panel A. Including backward linkages means we now control for the negative upstream spillovers. The main pattern is preserved in both Figure G2, Panels A and B, although slightly increased (along with standard errors), once we control for the most prominent linkage effects.

G.3 SUTVA: Investment Crowding Out and Linkages

The crowding out of investment is another way the SUTVA assumption is violated. Section VI demonstrated that investment, although higher in targeted industries, was not diminishing in non-targeted sectors, nor was this the case in capital-intensive non-HCI sectors. I now consider whether crowding out may occur after controlling for linkage intensity. Figure G3 shows the relationship between investment and capital intensity (log, pre-1973 capital stock divided by employment) controlling for linkages. Estimates are shown separately for HCI and non-HCI industry samples. The left panel plots estimates with controls for linkages using Forward or Backward Linkage$_i$ $\times$ Post$_t$. The right panel plots estimates using the more intensive Forward or Backward Linkage$_i$ $\times$ Year$_t$ control.

After controlling for linkages, I do not identify a negative relationship between measures of capital intensity and investment. Broadly, the relationship between capital intensity and investment in non-treated sectors is similar to the robustness estimates that did not account for linkages in Figure D3. The relationship between capital intensity and investment—now controlling for linkages—is similar in both industries during the drive. There is a positive relationship between capital intensity and investment after 1973 for both industries, although the relationship is zero during the HCI period. After capital market liberalization (see Online Appendix I), the relationship becomes more pronounced in both industries, with a stronger relationship among non-treated industries.

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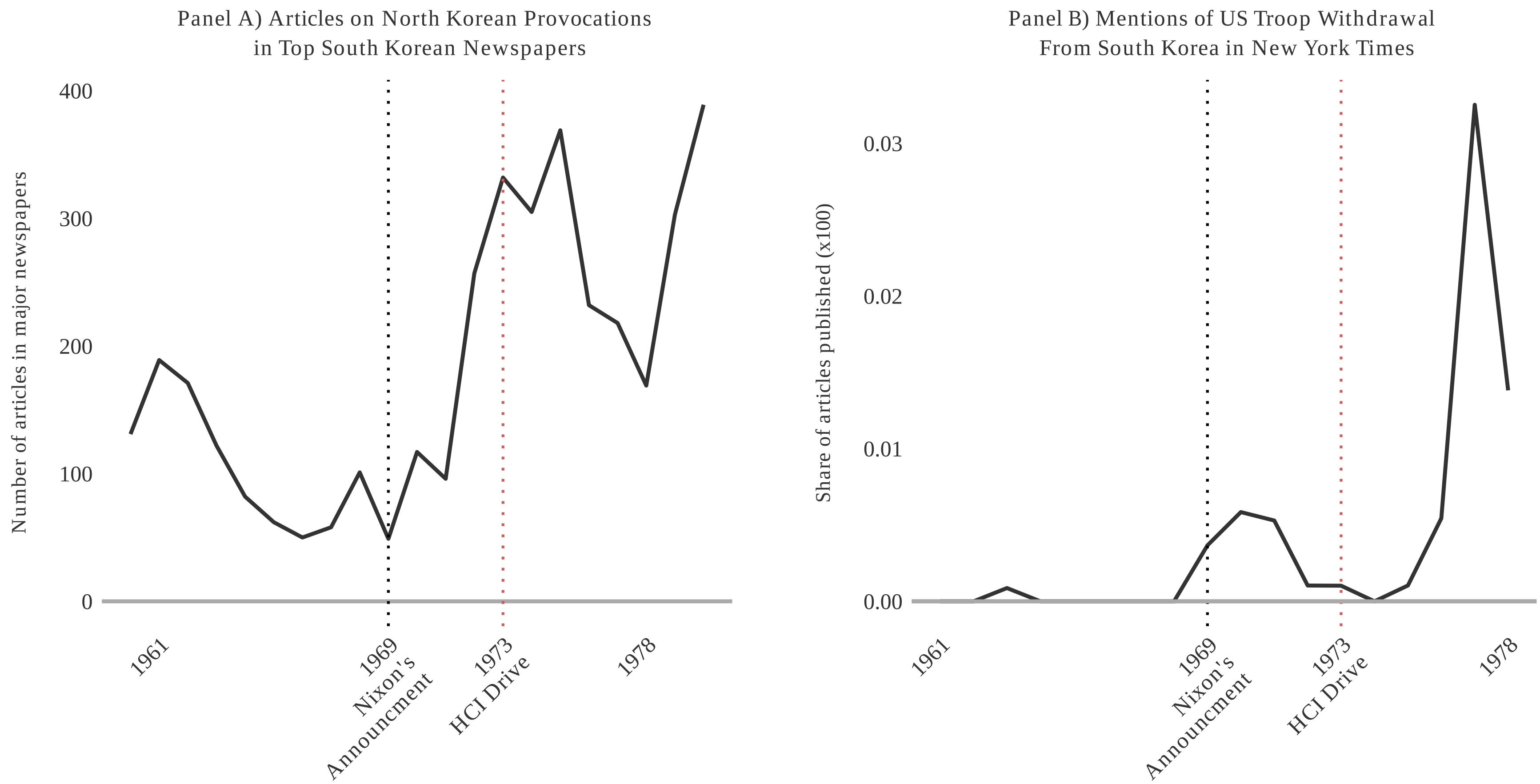

FIGURE A1

Political Events Surrounding Heavy and Chemical Industry Drive

This figure shows the political crisis facing South Korea via U.S. and South Korean media. Panel A (left) shows the number of articles (count) in Dong-a and Kyunghyang newspapers matching a Korean-language dictionary of 'provocation' keywords. See details in Supplemental Data Appendix; count includes articles matching dictionary terms appearing on the first five pages. Panel B (right) shows the share of New York Times news stories referring to troop withdrawal. Share is measured as the total number of full-text article hits ('South Korea+Troop Withdrawal') divided by the number of stories published, via New York Times.

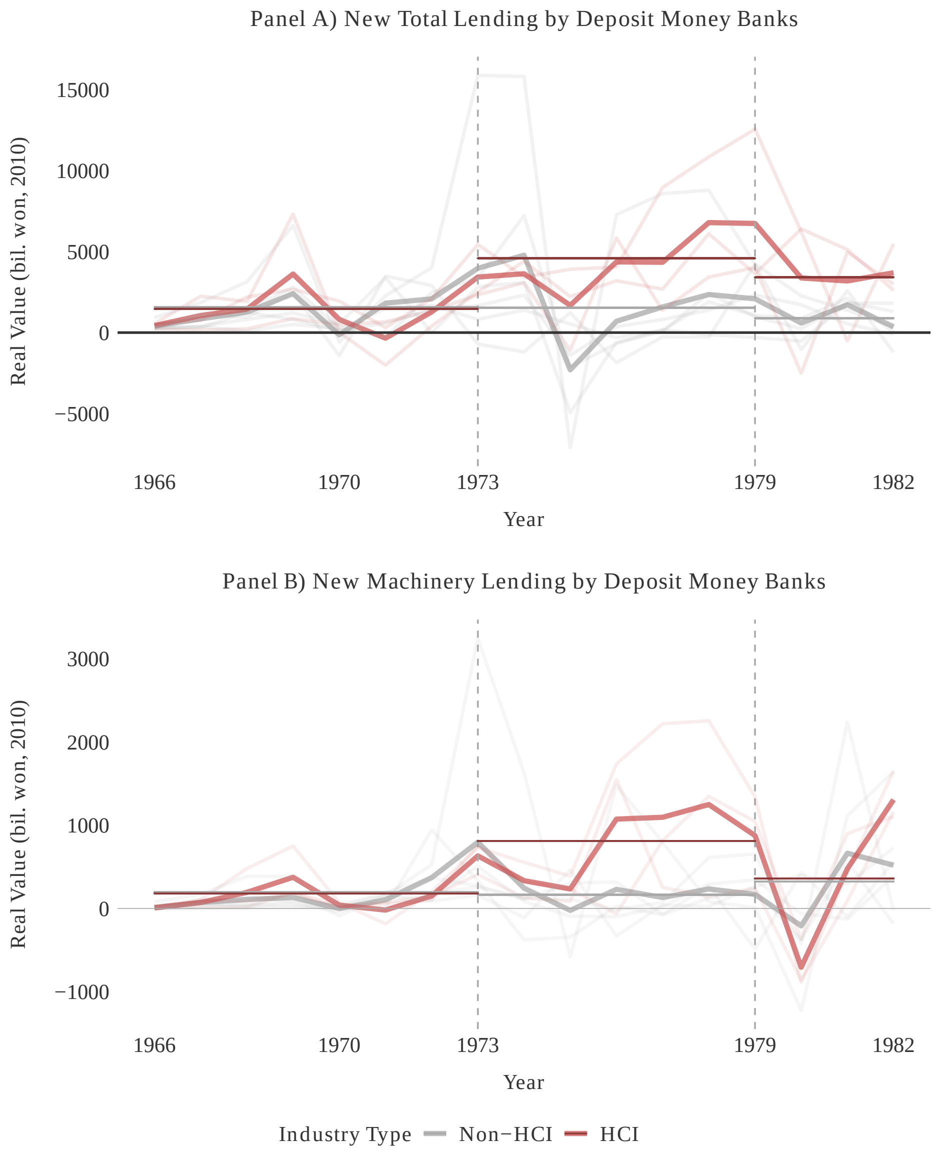

FIGURE A2

New Loans Issued By Commercial Deposit Money Banks

This figure shows the change in the real value of loans issued by South Korean commercial banks (traditional deposit money banks). The top panel plots changes in total new lending. The bottom panel plots new lending for machinery loans only. Units are real won (2010 base year). Gray lines correspond to non-targeted (non-HCI) sectors, red corresponds to targeted (HCI) sectors. Thick lines are averages by treatment status. Subsidized policy loans were lent through the commercial banking sector. After 1979, the banking sector was liberalized, and the differences in policy interest rates were eliminated. See text for details. Source: Korean Yearbooks.

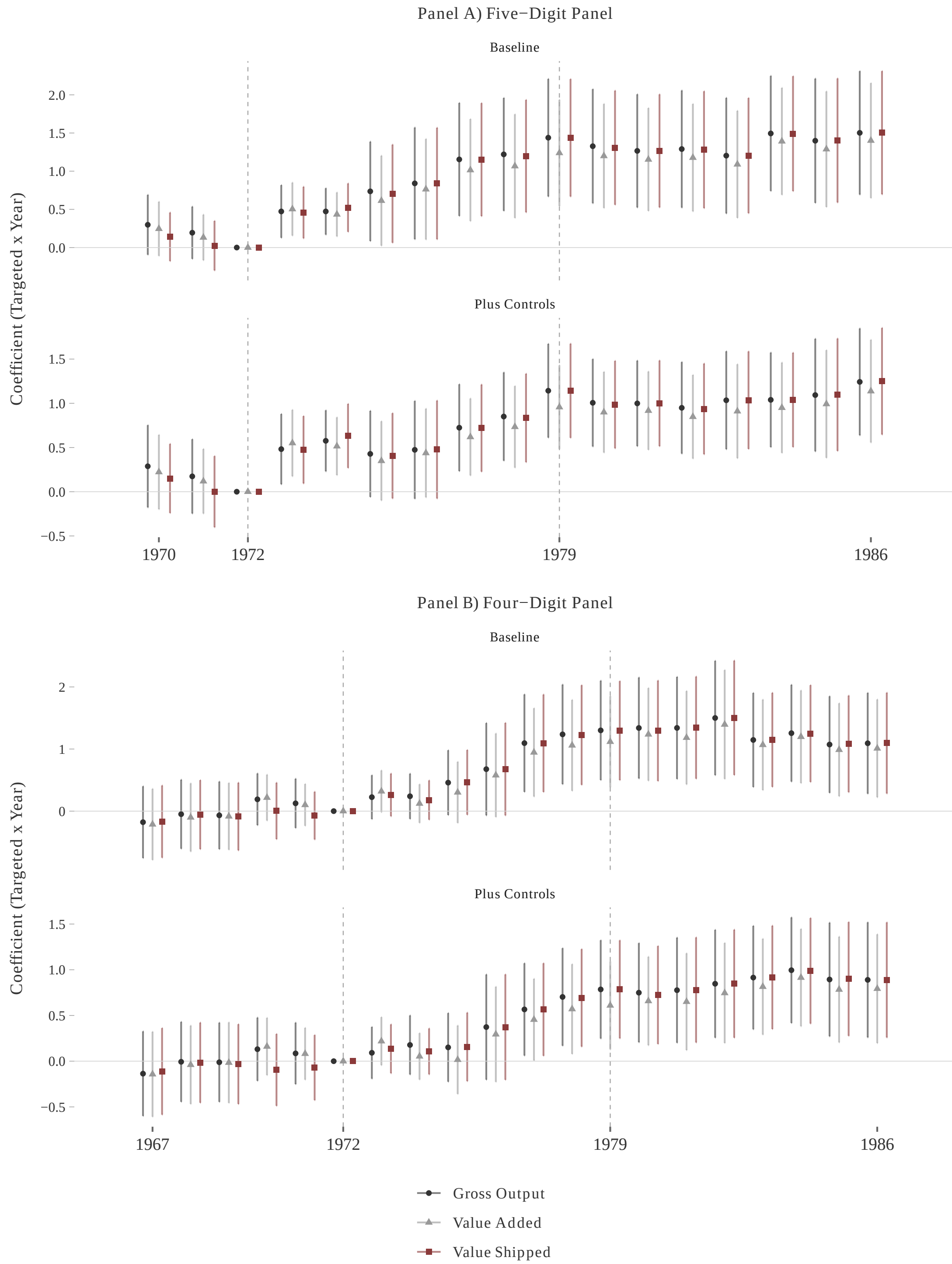

FIGURE B1

Robustness: Industrial Policy and Measures of Output

This figure shows dynamic differences-in-differences estimates for the relationship between HCI and industrial output outcomes. Plots show regression coefficients from equation (1) for three measures of log output: gross output, value added, and value of gross output shipped. Panel A shows results for 4- and 5-digit panels. Each column of the panels corresponds to a specification: the baseline two-way fixed effect specification and specifications adding additional controls. Controls are log pre-1973 industry averages: avg. industry wages, avg. industry plant size, labor productivity, and intermediate costs, interacted with time effects. The figure plots coefficients of interest estimated from equation (1). All estimates are relative to 1972, the year before the HCI policy. The line at 1979 demarcates the end of the Park regime. Standard errors are clustered at the industry-level. 95 percent confidence bands are in gray.

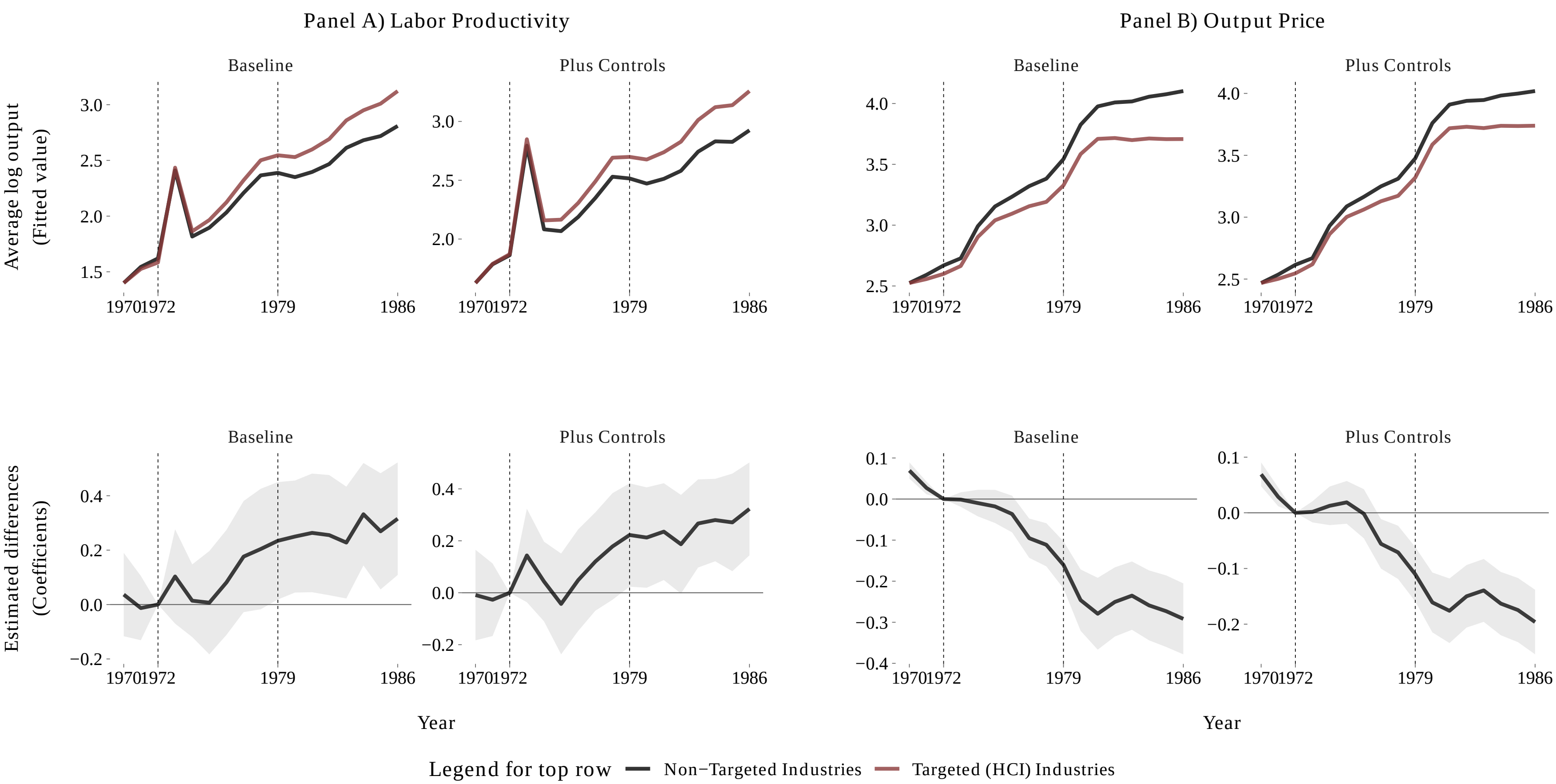

FIGURE B2

Differences in Value Added Per Worker and Output Prices

This figure plots dynamic differences-in-differences estimates for the relationship between HCI and labor productivity (value added per worker) in Panel A and output prices in Panel B. Estimates come from equation (1). The top row shows the average outcomes for targeted (red) and non-targeted industries (black) using the fitted model. For specifications with controls, the model is evaluated using means of the controls. The bottom row plots the differences-in-differences estimates. All estimates are relative to 1972, the year before the HCI policy. The line at 1979 demarcates the end of the Park regime. Standard errors are clustered at the industry level. 95 percent confidence intervals are shown in gray.

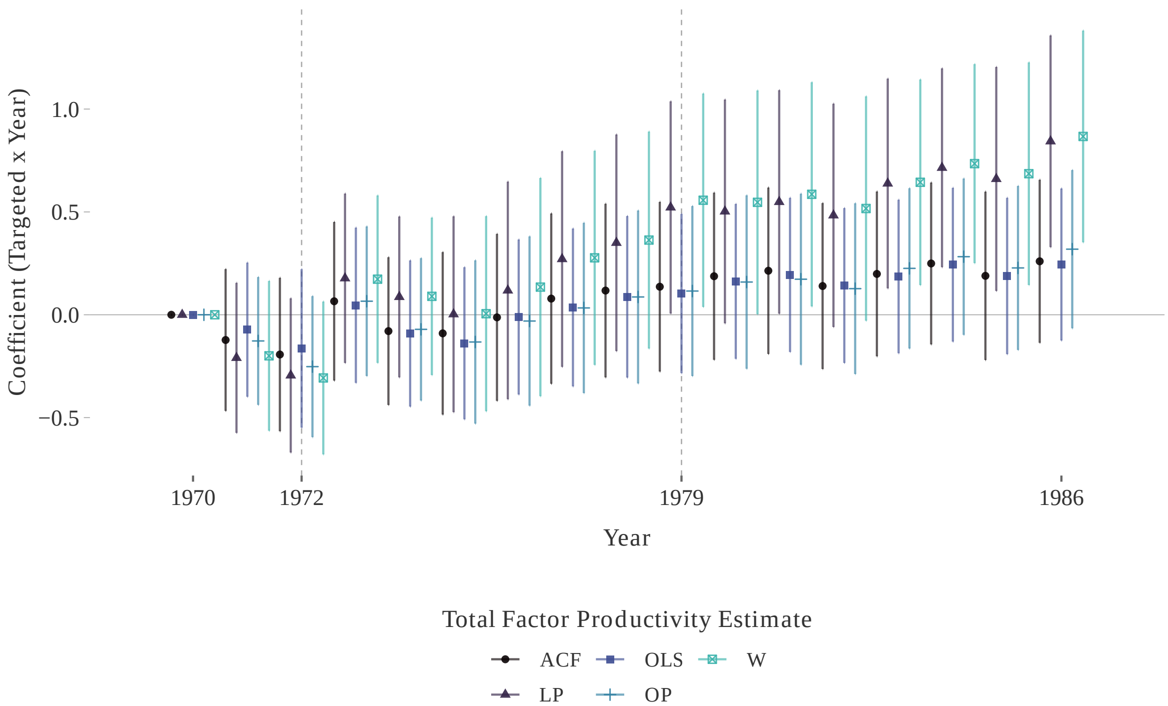

FIGURE B3

Robustness: Industry Policy and Industry-Level Total Factor Productivity

This figure shows the relationship between HCI and total factor productivity. The coefficients in the figure are estimated from (1). TFP outcomes are estimated using Ackerberg-Caves-Frazer (ACF), Levinsohn-Petrin (LP), Olley-Pakes (OP), Wooldridge (W) methods, as well as baseline OLS using the Solow residual. Data are estimated using the 5-digit (long) panel, where capital stocks are available; log-transformed production functions are structurally estimated at the 2-digit level. Event study estimates are performed relative to the start year of the panel, 1970, as opposed to 1972, due to the significant dip in TFP in 1972. This is done for transparency; using 1972 as the omitted category may overstate event study estimates. Standard errors are clustered at the industry level. Bars show 95 percent confidence intervals.

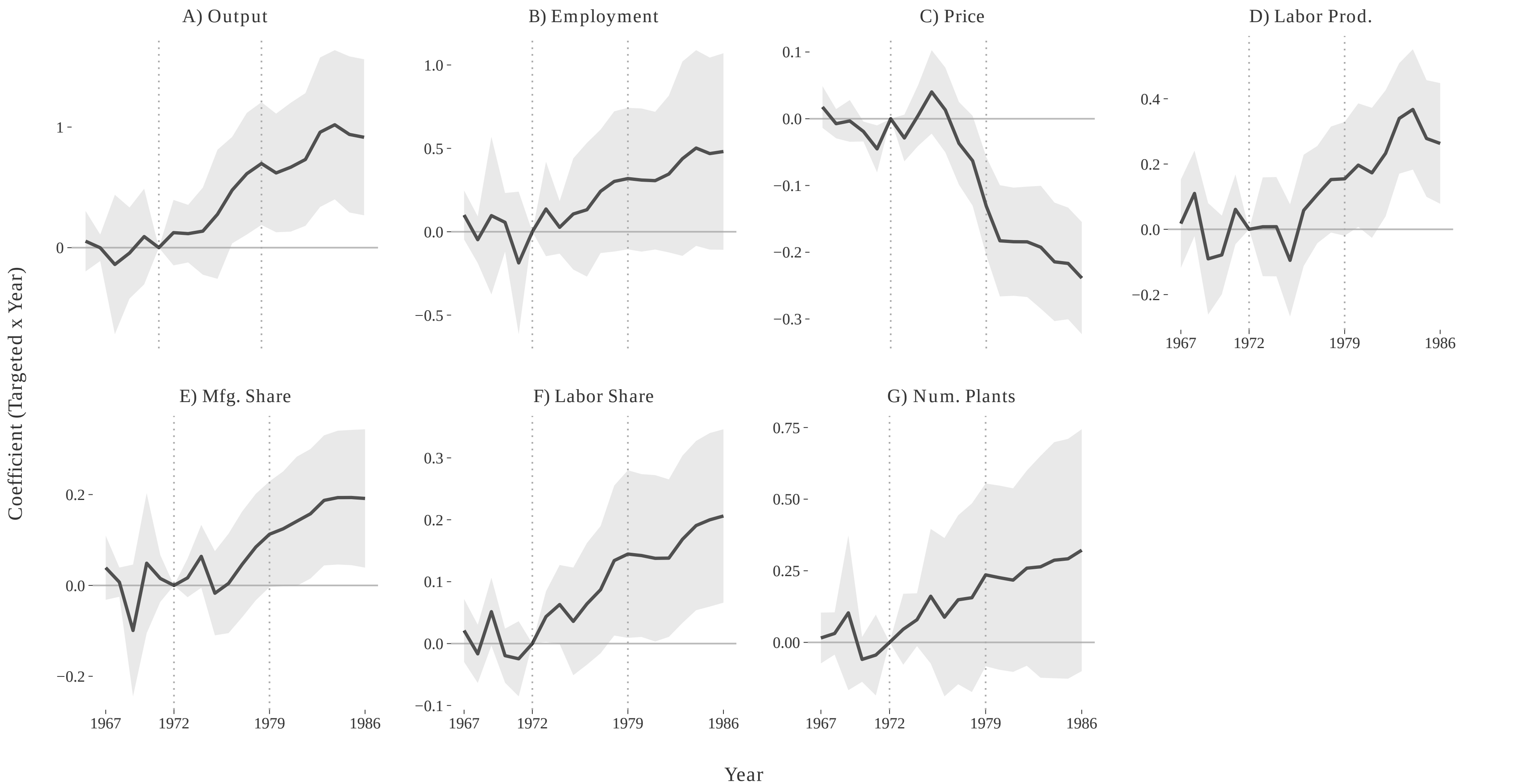

FIGURE B4

Double Robust Estimates: Industrial Policy and Industrial Development, Four-Digit Panel

This figure plots semiparametric (doubly-robust) differences-in-differences estimates for the impact of HCI on core (log) industrial development outcomes. Log outcomes include real value of shipments, employment, output prices, labor productivity (value added per worker), mfg. share (manufacturing share of output), lab. share (manufacturing share of employment), and number of plants. This figure reports estimates for the aggregated 4-digit panel (1967-1986). Black lines correspond to estimates from (3). All point estimates are relative to the 1972 baseline level (coefficients normalized to 0). 95

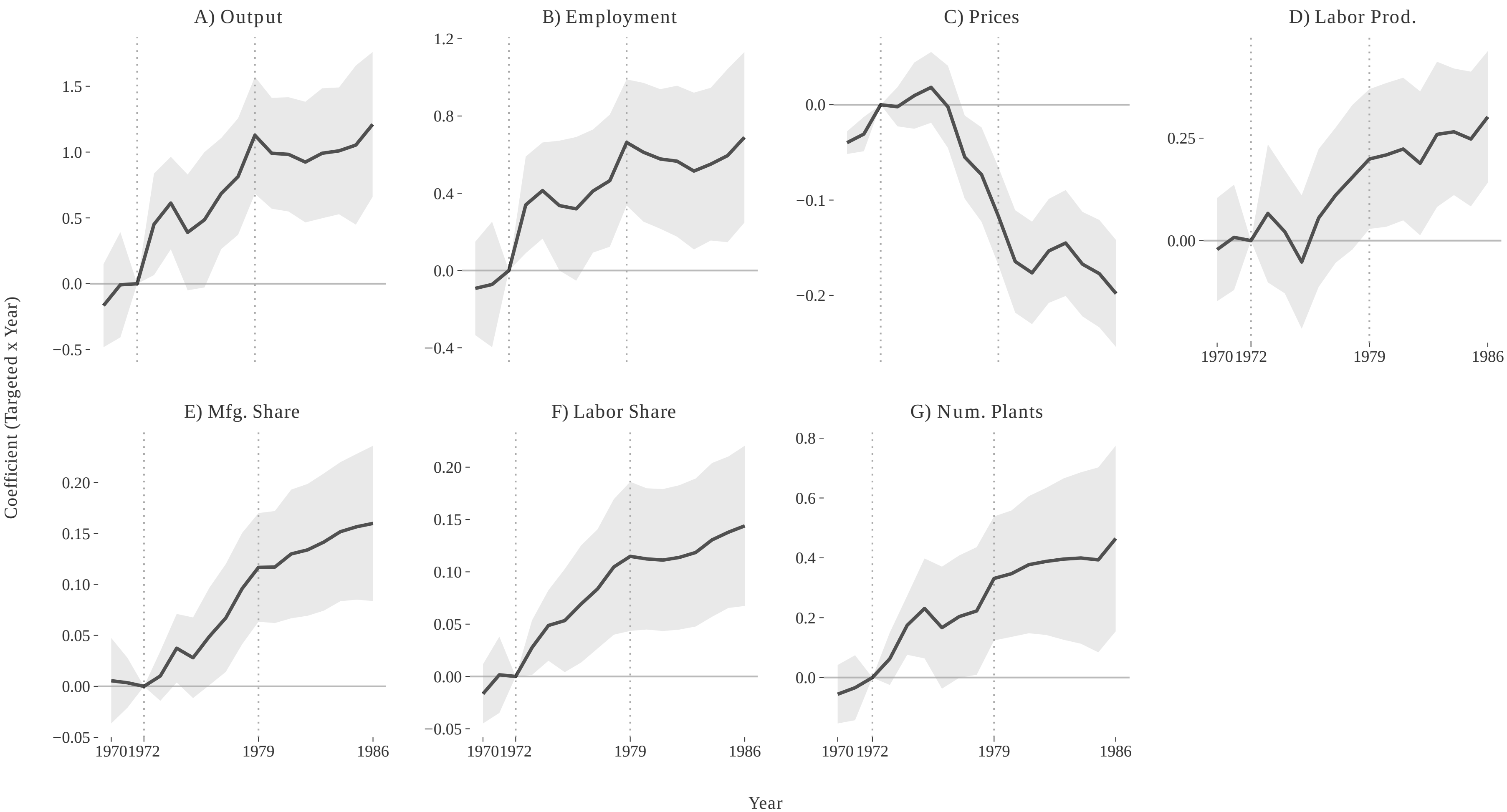

FIGURE B5

Double Robust Estimates: Industrial Policy on Industrial Development, Five-Digit Panel

This figure plots semiparametric (doubly-robust) differences-in-differences estimates for the impact of HCI on core (log) industrial development outcomes. Log outcomes include: real value of shipments, employment, output prices, labor productivity (value added per worker), mfg. share (manufacturing share of output), lab. share (manufacturing share of employment), and number of plants. This figure reports estimates for the more detailed 5-digit panel (1970-1986). Black lines correspond to estimates from (3). All point estimates are relative to the 1972 baseline level (coefficients normalized to 0). 95

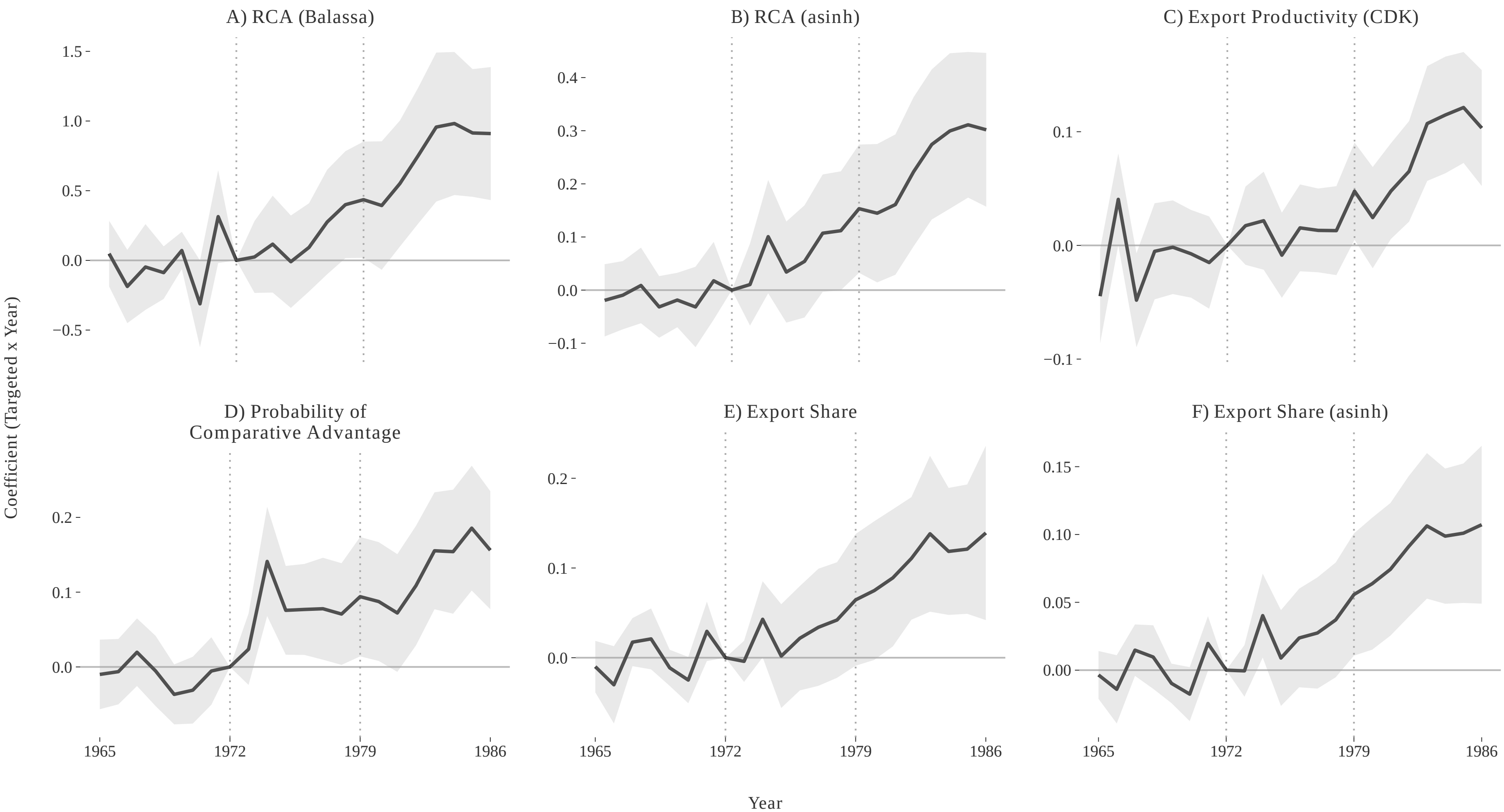

FIGURE B6

Double Robust Estimates: Industrial Policy and Export Development

This figure plots semiparametric (doubly-robust) differences-in-differences estimates for the impact of HCI on trade development and comparative advantage outcomes: RCA, CDK, and log export share. For RCA measures, I show the normal raw (Balassa) index alongside log and asinh-transformed RCA. CDK is structrually estimated. This figure reports estimates from 4-digit SITC panel data (1965-1986). Black lines correspond to estimates from (3). All point estimates are relative to the 1972 baseline level (coefficients normalized to 0). 95

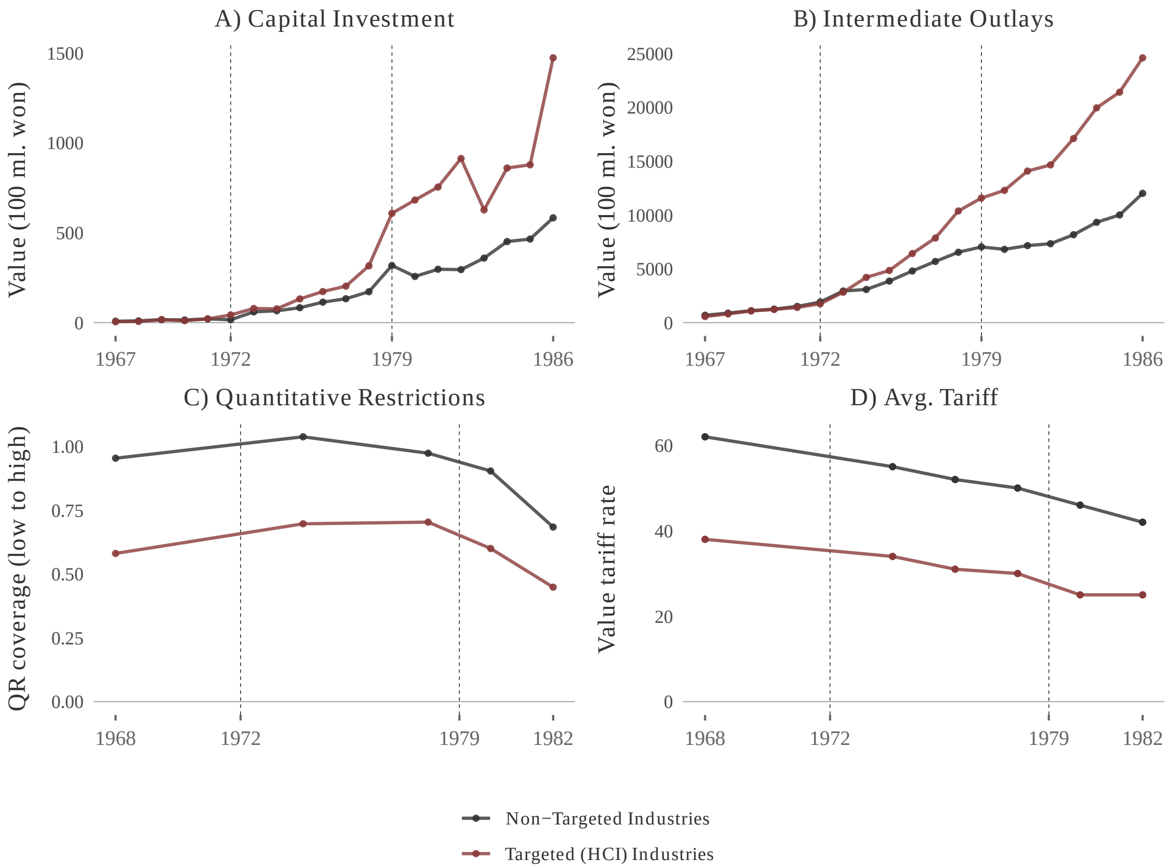

FIGURE D1
AVERAGE AGGREGATE INVESTMENT AND TRADE POLICY

Each panel plots outcomes related to investment and protection. Points are averages across targeted (HCI) and non-targeted (non-HCI) industries. The top row, Panels A-C, shows outcomes related to investment incentives. Panel A reports mean real total capital formation across targeted and non-targeted industries. Panel B shows real total material costs. Note: average intermediate material outlays can exceed investment. Panels C and D show outcomes for trade policy: C reports average ad valorem tariff rates (percent), and D shows quantitative restriction measures (QR). QR is a qualitative ranking of coverage on products within an industry, 0 being minimal coverage and 3 being high coverage.

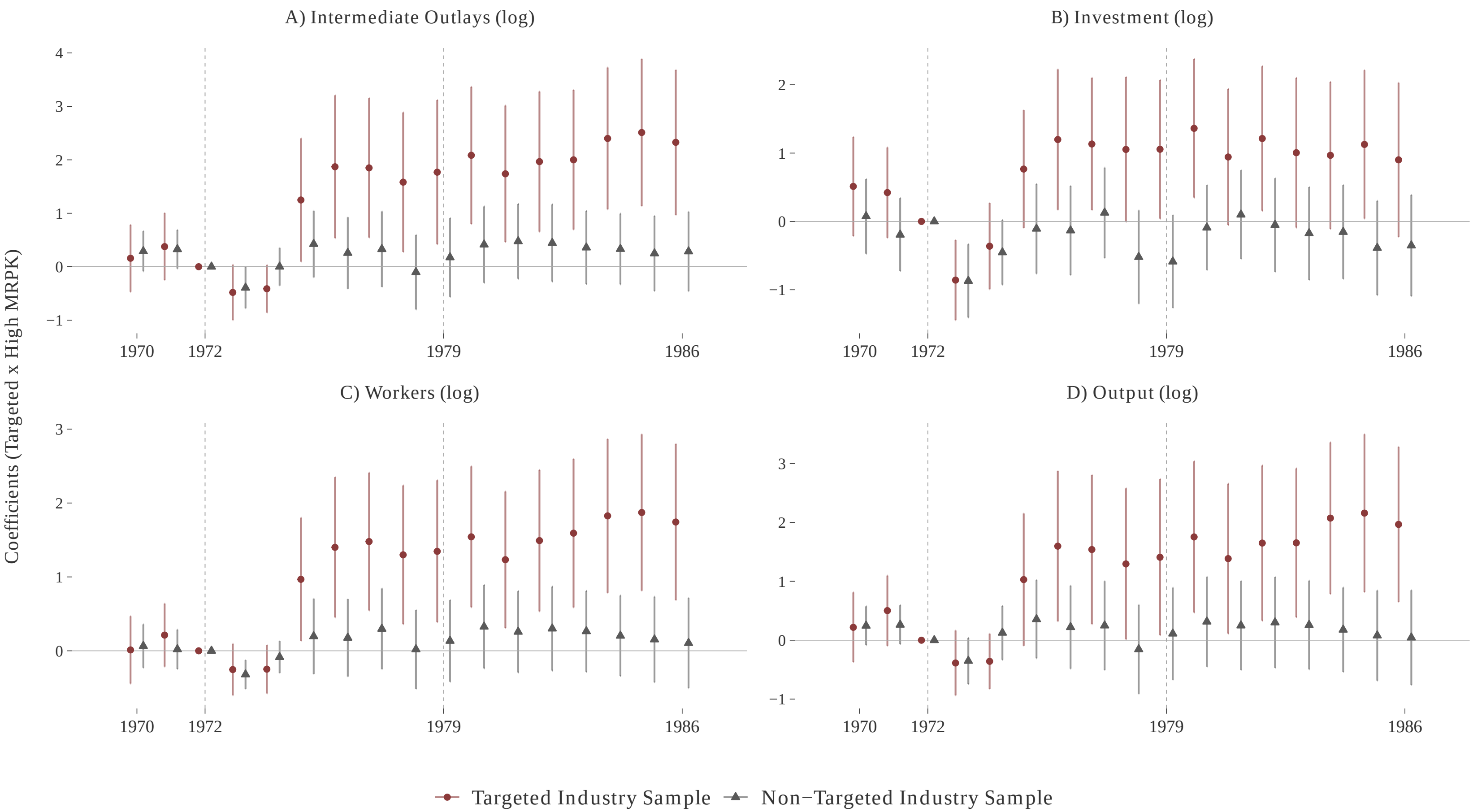

FIGURE D2

INPUT USE AND MARGINAL REVENUE PRODUCT OF CAPITAL

This figure shows dynamic differences-in-differences estimates for the relationship between HCI and responses to in input use by high versus low marginal revenue product of capital (MRPK) industries. The figure plots coefficient estimates from 9, estimated separately for (red) targeted and (gray) non-targeted industries. These coefficients convey the differences in input use between high-MRPK and low-MRPK industries, relative to 1972. See Appendix D.1 for MRPK calculation. Outcomes are log values: real material outlays, real investment, employment, and real gross output shipped. Error bars show the 95 percent confidence interval. The figure plots coefficients of interest estimated from equation (1). All estimates are relative to 1972, the year before the HCI policy. The line at 1979 demarcates the end of the Park regime. Standard errors are clustered at the industry-level. 95 percent confidence bands are in gray.

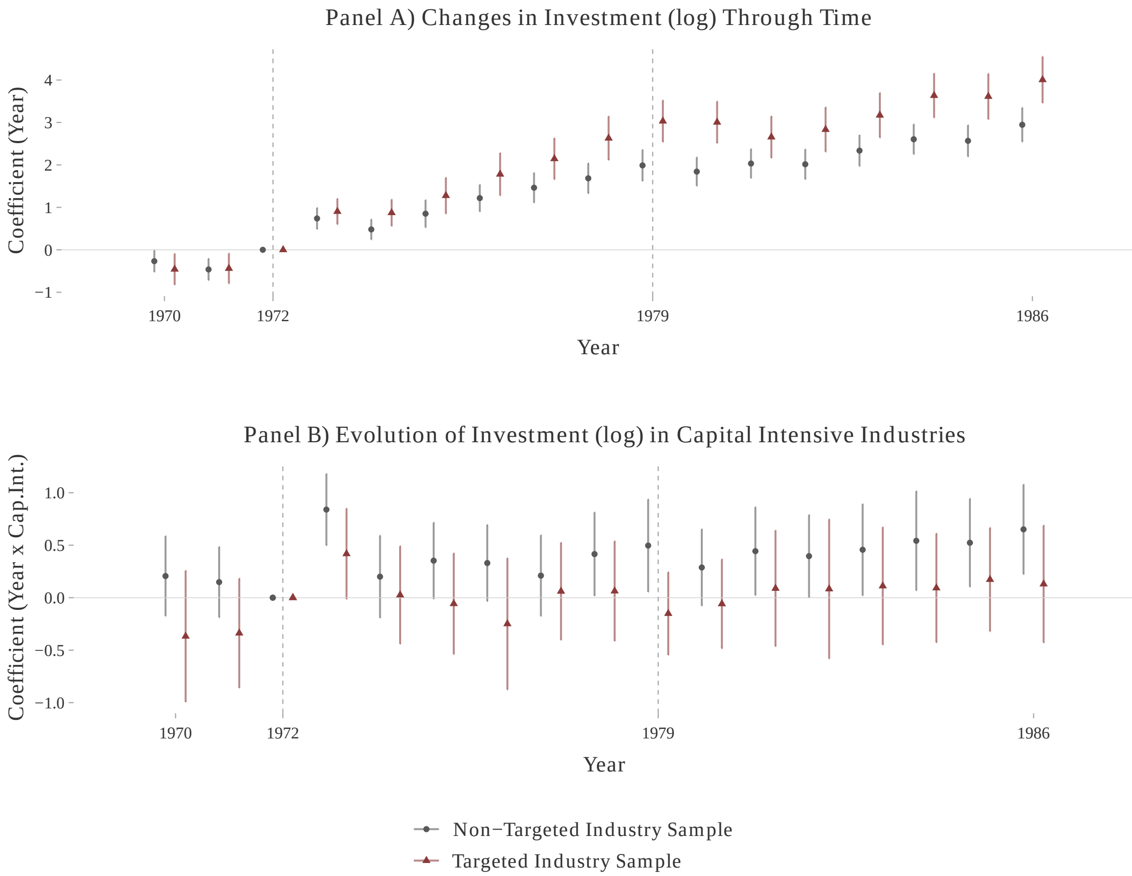

FIGURE D3

CROWDING OUT AND INVESTMENT BY TREATMENT STATUS

This figure shows dynamic differences-in-differences estimates for the relationship between HCI and responses to investment incentives. Panel (A) shows the changes in (log, real) investment for targeted and non-targeted industries, relative to 1972. Panel A plots the coefficients from equation (10), estimated separately by treatment status. Panel B assesses the degree to which non-treated, capital-intensive industries may have been squeezed by HCI drive credit policy. Panel (B) shows the evolution of investment in high versus low capital-intensive industries, estimated separately by treatment status. Coefficients are from the interaction Year × log Capital Intensity, with 1972 as the omitted category. Pre-treatment capital intensity is the pre-1973 real capital stock per worker. The figure plots coefficients of interest estimated from equation (1). All estimates are relative to 1972, the year before the HCI policy. The line at 1979 demarcates the end of the Park regime. Standard errors are clustered at the industry-level. 95 percent confidence bands are in gray.

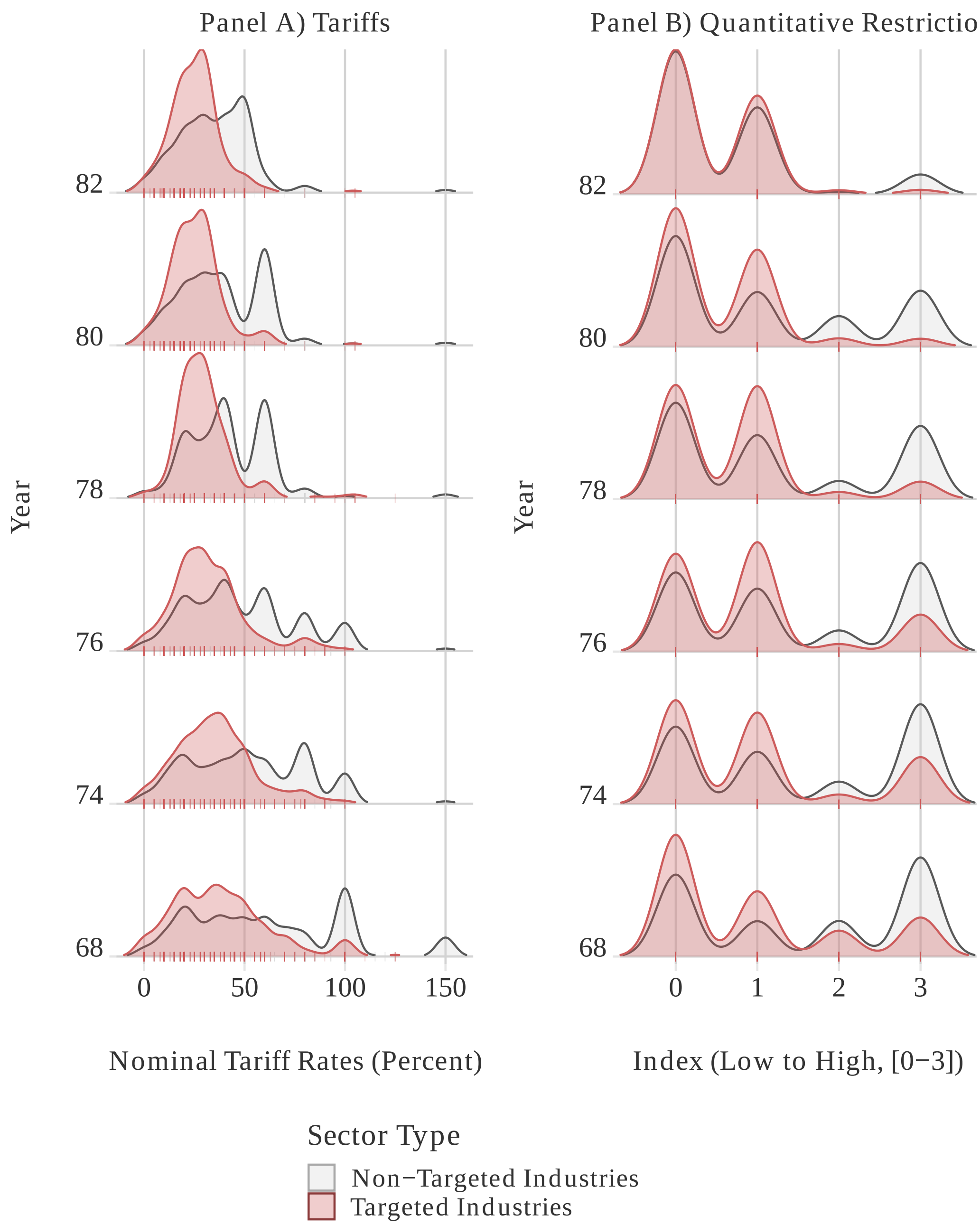

FIGURE D4

Changes in Distribution of Trade Policies, 1968-1982

This figure shows the decline and convergence in (A) nominal tariff rates (percent) and (B) quantitative restrictions (severity scores 0-3). The kernel density distribution for targeted products is in red; non-targeted products are in gray. Distributions are estimated over annual product-level data (unweighted, CCCN code, 4-digit level) for years 1968, 1974, 1978, 1980, and 1982. The severity of quantitative restrictions within 4-digit products is measured using a qualitative 0-3 scale, from (0) no restrictions to (3) high restrictions.

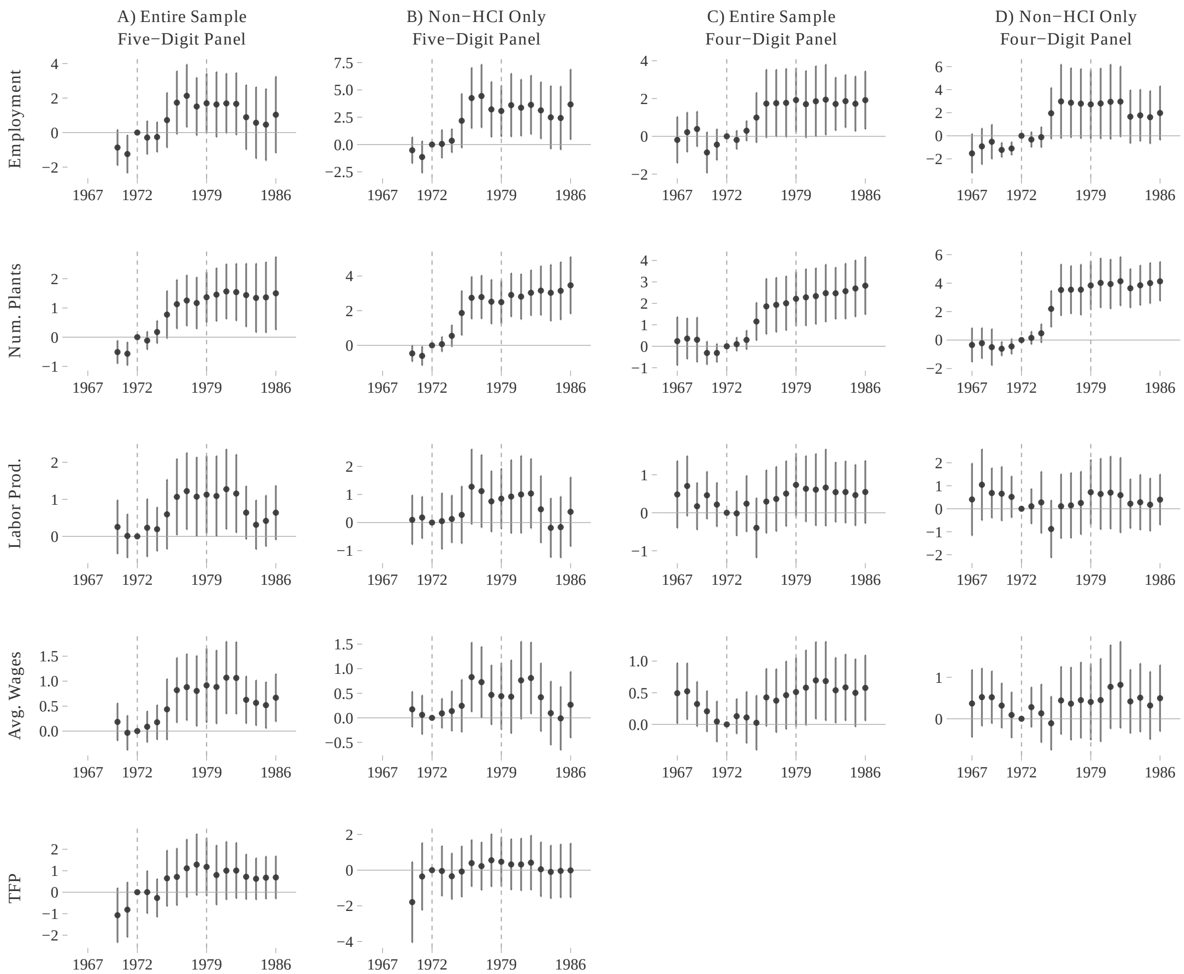

FIGURE E1

Direct Forward Linkages and Development Outcomes

This figure plots dynamic differences-in-differences estimates for the relationship between direct forward linkage exposure and outcomes: (log) employment, number of plants (plant entry), labor productivity, average wages, and TFP (ACF). Coefficients are estimated from equation (7). Estimates are relative to, 1972, the year before HCI. The year 1979 corresponds to collapse of Park regime. Years are on the x-axis. Estimates for the effect of direct forward (Linkage × Year) linkages are on y-axis. Full sample regressions control for the main HCI × Year effect. All regressions include controls for direct backward linkage connections, interacted with time. 95 percent confidence intervals are shown in gray.

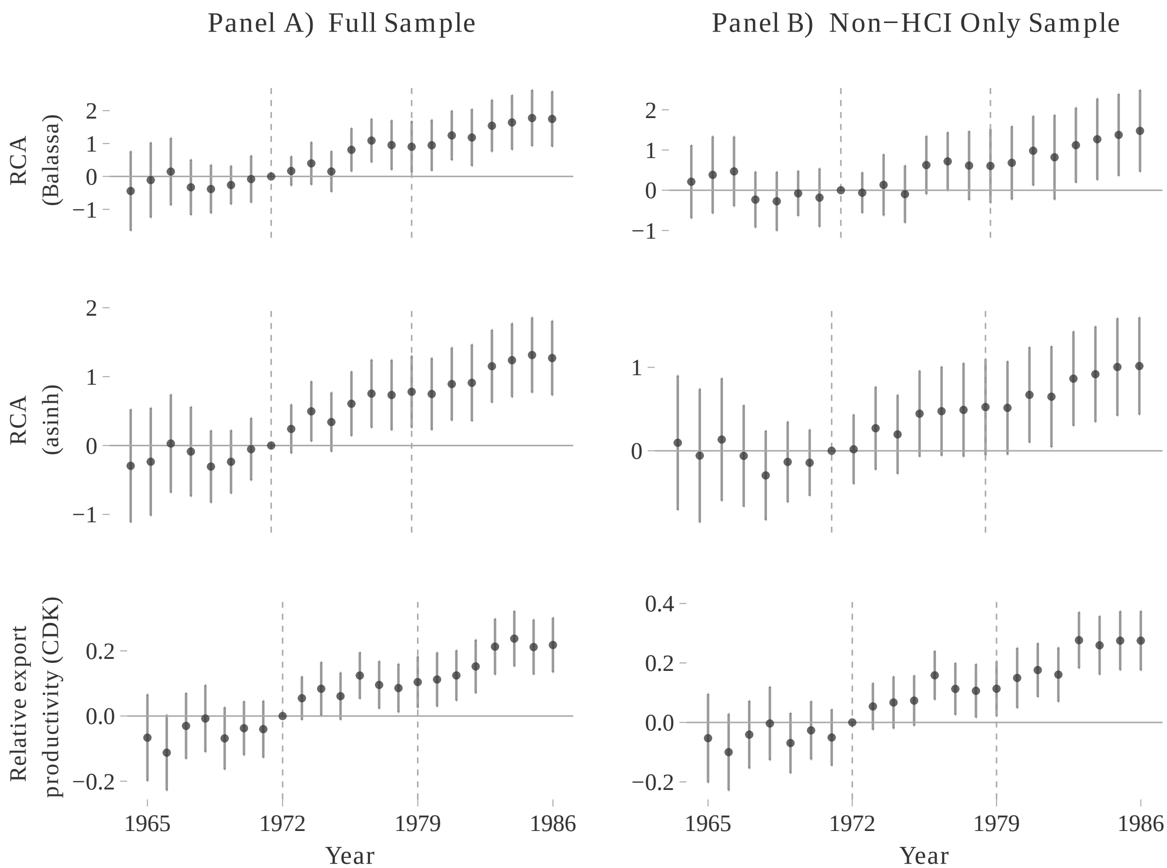

FIGURE E2

Total Forward Linkages and Export Development

The figure plots dynamic differences-in-differences estimates for the relationship between total (Leontief) forward linkage exposure and export development. The coefficients in the plot are estimated from equation (7), using total linkage measures. For the raw RCA (Balassa) index, regressions are estimated using PPML. RCA (asinh) and relative export productivity (CDK) are estimated using OLS. Linkage measures are calculated from the 1970 input-output tables. 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 (total (Leontief) forward) are on the y-axis: e.g., Linkage × Year. These estimates come from the DD specification that includes the impact of both measures. Full sample regressions control for the main HCI × Year effect. 95 percent confidence intervals are shown in gray.

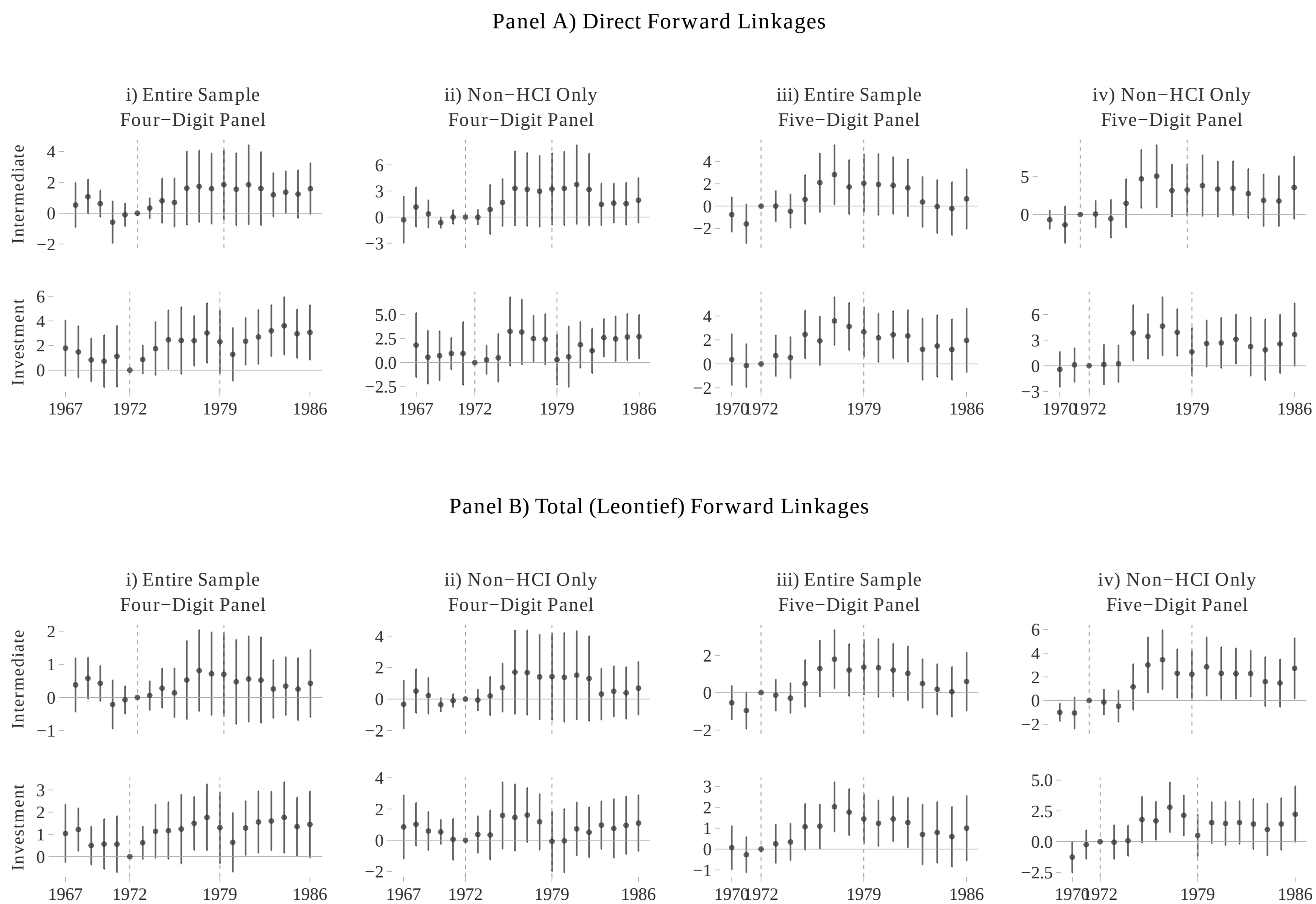

FIGURE E3

Linkage Mechanisms - Direct Forward Linkages, Intermediate Outlays, and Investment

This figure plots dynamic differences-in-differences estimates for the relationship between direct forward linkage exposure and outcomes: log real intermediate input outlays (Intermediates), and log real total gross capital formation (Investment). Coefficients are estimated from equation (7). Estimates are relative to, 1972, the year before HCI. The year 1979 corresponds to collapse of Park regime. Years are on the x-axis. Estimates for the effect of direct forward (Linkage × Year) linkages are on y-axis. Full sample regressions control for the main HCI × Year effect. All regressions include controls for direct backward linkage connections, interacted with time. 95 percent confidence intervals are shown in gray.

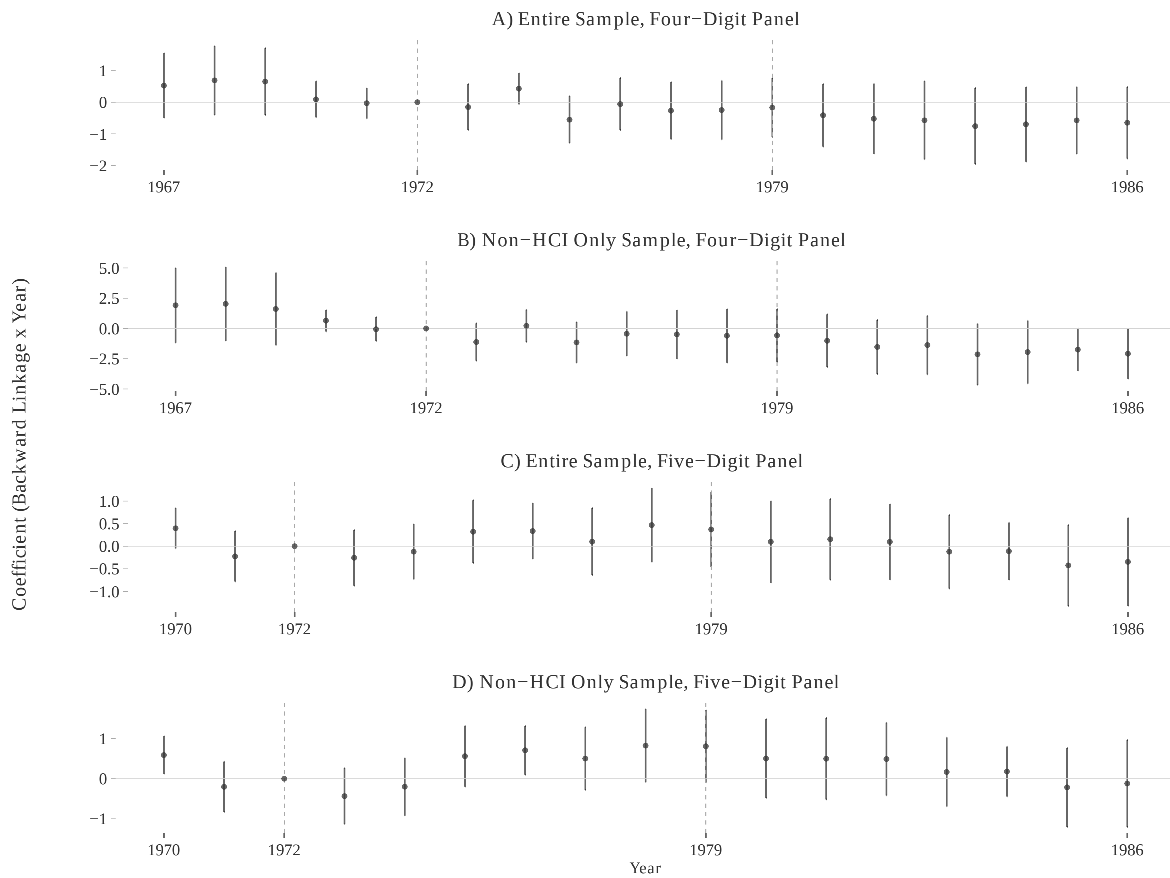

FIGURE F1

Relationship Between Direct Backward Linkages on Upstream Output

This figure plots dynamic differences-in-differences estimates for the relationship between direct backward linkage exposure and outcomes: log real value added. Coefficients are estimated from equation (7). Estimates are relative to, 1972, the year before HCI. The year 1979 corresponds to collapse of Park regime. Years are on the x-axis. Estimates for the effect of direct backward (Linkage × Year) linkages are on y-axis. Full sample regressions control for the main HCI × Year effect. All regressions include controls for direct forward linkage connections, interacted with time. 95 percent confidence intervals are shown in gray.

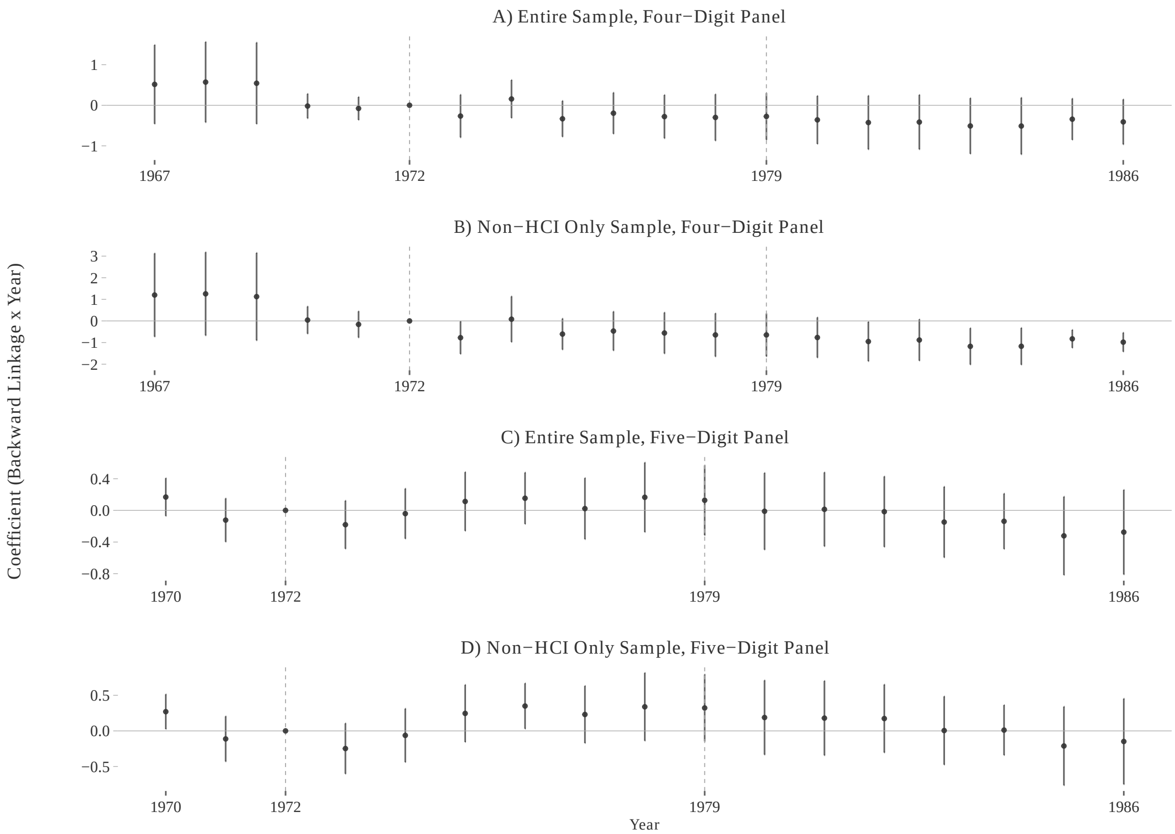

FIGURE F2

Relationship Between Total Backward Linkages and Upstream Output

This figure plots dynamic differences-in-differences estimates for the relationship between total backward linkage exposure and outcomes: log real value added. Coefficients are estimated from equation (7). Estimates are relative to, 1972, the year before HCI. The year 1979 corresponds to collapse of Park regime. Years are on the x-axis. Estimates for the effect of total backward (Linkage × Year) linkages are on y-axis. Full sample regressions control for the main HCI × Year effect. All regressions include controls for total forward linkage connections, interacted with time. 95 percent confidence intervals are shown in gray.

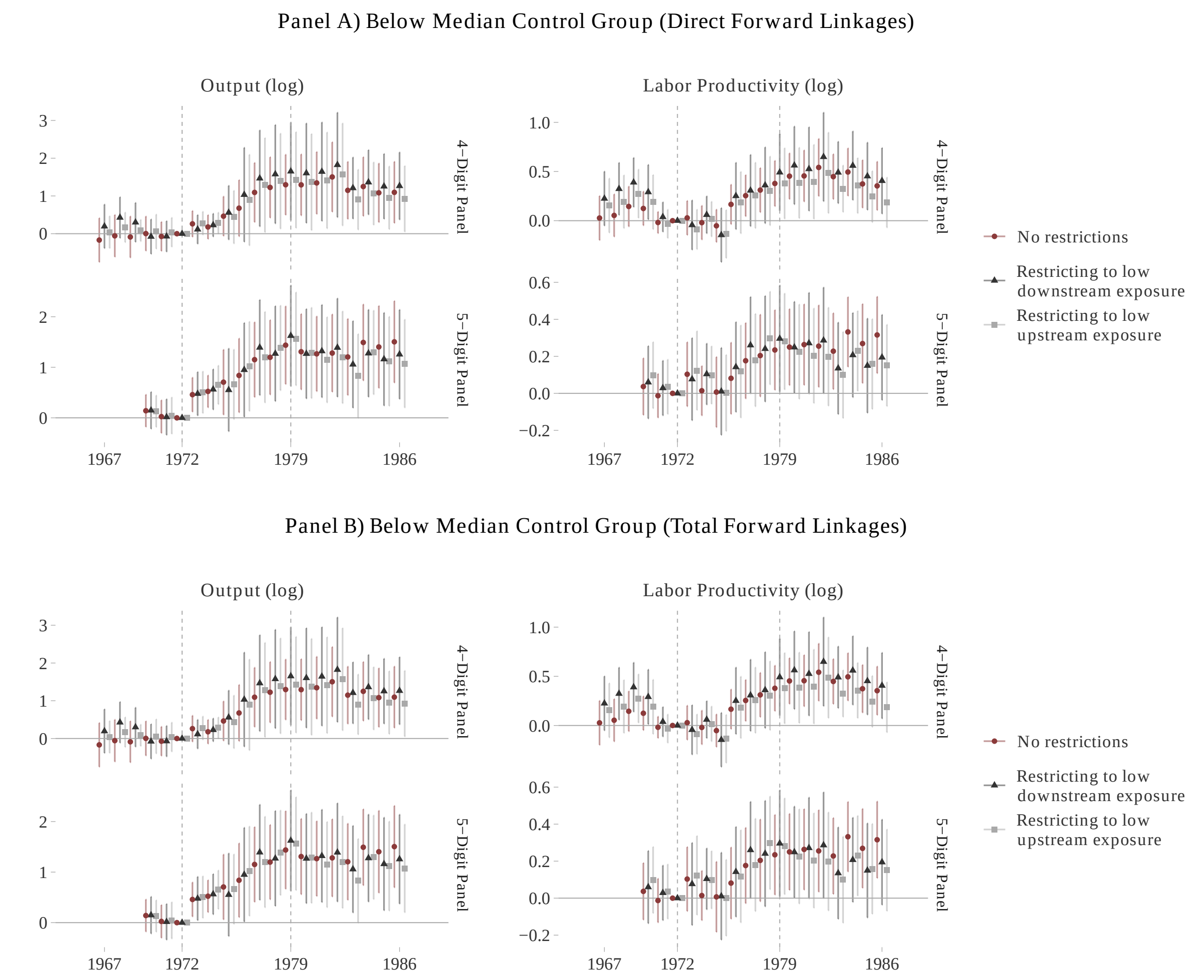

FIGURE G1

Robustness: Impact of Industrial Policy on Development, Restricting to Control Industries with Low Linkages

This figure shows dynamic differences-in-differences estimates for the relationship between HCI and responses to industrial development outcomes. The figure show estimates with and without controlling for linkage effects. Panel A limits the control group to industries with below median forward linkage exposure. Panel B limits the control group to industries below median exposure to total forward linkages. The figure plots coefficients of interest estimated from equation (1). All estimates are relative to 1972, the year before the HCI policy. The line at 1979 demarcates the end of the Park regime. Standard errors are clustered at the industry-level. 95 percent confidence bands are in gray.

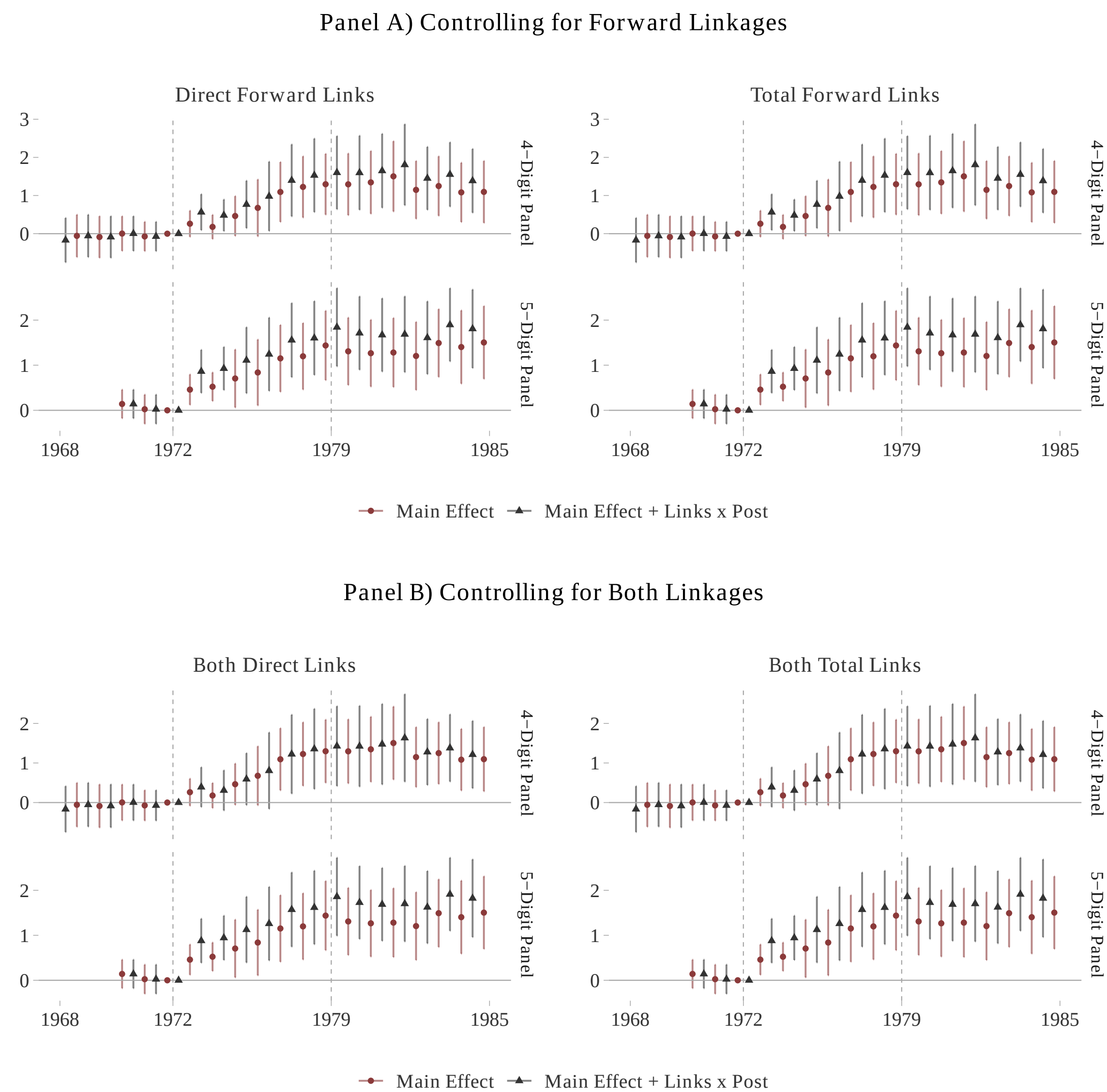

FIGURE G2

Robustness: Impact of Industrial Policy on Development, Controlling for Non-Treated Linkages

This figure shows dynamic differences-in-differences estimates for the relationship between HCI and responses to industrial development outcomes. Estimates with and without controls for linkage effects in non-treated sectors (linkage effects only for non-treated industry). Panels A compares baseline estimates from equation (1) to estimates that control for forward linkage exposure. Panel B compares baseline estimates from equation (1) to estimates controlling for both measures of linkage exposure. The figure plots coefficients of interest estimated from equation (1). All estimates are relative to 1972, the year before the HCI policy. The line at 1979 demarcates the end of the Park regime. Standard errors are clustered at the industry-level. 95 percent confidence bands are in gray.

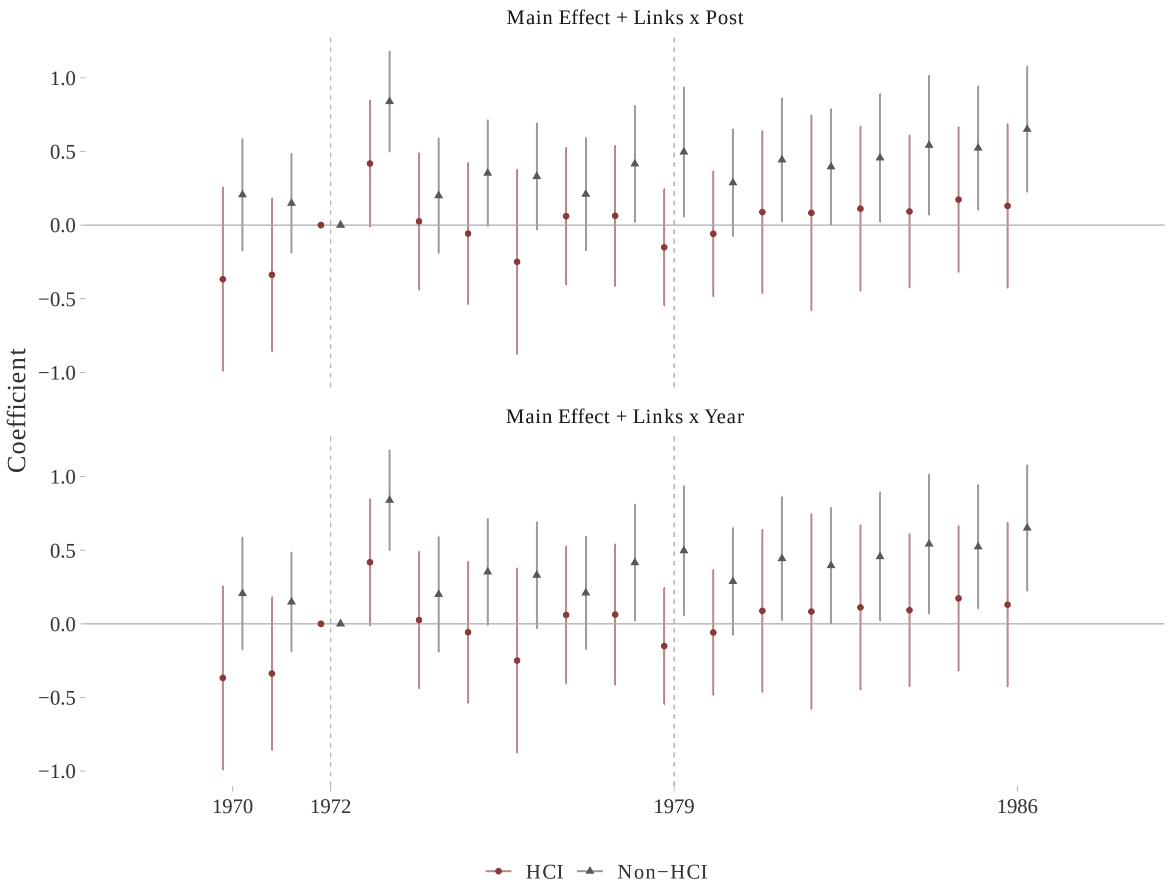

FIGURE G3
Robustness: Relationship Between Investment and Capital Intensity, HCI Versus Non-HCI Industry

This figure shows dynamic differences-in-differences estimates for the relationship between HCI and responses to investment incentives. Panels show the changes in investment for targeted and non-targeted, relative to 1972, controlling for IO linkages. Regressions are performed on either the targeted-only or non-targeted samples. Coefficients are from the interaction Year × Log Capital Intensity, with 1972 as the omitted category. Pre-treatment capital intensity is measured as the pre-1973 levels of capital stock per worker. Left panel plots the Year × Capital Intensity (main effects), controlling for forward and backard linkages (interacted with Post). Right panel plots the Year × Capital Intensity (main effects), controlling for forward and backard linkages (interacted with Year). The figure plots coefficients of interest estimated from equation (1). All estimates are relative to 1972, the year before the HCI policy. The line at 1979 demarcates the end of the Park regime. Standard errors are clustered at the industry-level. 95 percent confidence bands are in gray.

TABLE A1
Pre-HCI Drive Statistics by Treatment Status

		A) Four-Digit Panel (1967-1972)		B) Five-Digit Panel (1970-1972)	
Variable	Industry	Mean	N	Mean	N
i) Industrial Statistics					
	Non-HCI	7624.67	330	6933.08	528
Average Size	HCI	6811.94	198	7020.13	306
	Non-HCI	406.48	330	106.35	528
Establishments	HCI	162.23	198	50.22	306
	Non-HCI	182647.67	330	75534.11	528
Gross Output	HCI	149872.90	198	61968.10	306
	Non-HCI	6704.31	330	2261.10	528
Investment	HCI	8123.71	198	3351.19	306
	Non-HCI	766.17	330	708.96	528
Labor Productivity	HCI	917.37	198	744.37	306
	Non-HCI	1.39	330	0.42	528
Labor Share	HCI	0.72	198	0.24	306
	Non-HCI	9.70	330	10.97	528
Prices	HCI	29.20	198	29.14	306
	Non-HCI	85689.51	330	31335.02	528
Value Added	HCI	51414.51	198	22038.69	306
	Non-HCI	1.35	330	0.42	528
Value Added Share	HCI	0.78	198	0.26	306
	Non-HCI	12983.87	330	4117.89	528
Workers	HCI	6775.03	198	2351.84	306
ii) Linkage Exposure to HCI Sectors					
	Non-HCI	0.09	330	0.15	528
Backward Linkage	HCI	0.16	198	0.20	306
	Non-HCI	0.11	330	0.10	528
Forward Linkage	HCI	0.31	198	0.34	306
iii) Trade Statistics (SITC trade data, 1965-1972)					
	Non-HCI	0.88	3464		
RCA (Balassa)	HCI	0.36	1448		
	Non-HCI	0.19	3464		
Export Share	HCI	0.09	1448		
	Non-HCI	0.13	3464		
Import Share	HCI	0.24	1448		

Notes. Table reports pre-1973 statistics for a selection of core industrial variables. Panel A shows statistics for aggregated ('long') 4-digit industrial panel, 1967 to 1972. Panel B shows statistics for disaggregated ('short') 5-digit industrial panel, 1970 to 1972. Part i) of table reports Mining and Manufacturing Survey/Census (MMS) outcomes, with the exception of prices, which come from the Bank of Korea publications. Part ii) shows data from the 1970 input-output tables published by the Bank of Korea (1970), harmonized and matched to industry-level data. Part iii) shows trade variables (from UN-Comtrade). All values are deflated using 2010 baseline won, except for real USD trade values.

TABLE C1

PROBABILITY OF ATTAINING COMPARATIVE ADVANTAGE IN TARGETED INDUSTRY, SOUTH KOREA V. OTHER COUNTRIES, POST-1972

	Outcomes: Probability of Comparative Advantage							
	Estimates with OLS				Estimates with PPML			
	Full Sample		Similar GDP	Same GDP	Full Sample		Similar GDP	Same GDP
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Korea	0.131***	0.093***	0.137***	0.119***	1.002***	0.914***	1.085***	0.853***
	(0.011)	(0.013)	(0.011)	(0.021)	(0.141)	(0.089)	(0.143)	(0.212)
GDP per capita		0.046***				0.656***		
		(0.008)				(0.072)		
Industry X Year Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.025	0.090	0.065	0.053	0.052	0.165	0.082	0.064
Observations	251160	251160	76440	24570	246652	246652	55692	13824
Mean of Dependent Variable	0.078	0.078	0.075	0.102	0.079	0.079	0.103	0.180
Clusters (Country-Industry)	92 x 182	92 x 182	28 x 182	9 x 182	92 x 182	92 x 182	28 x 178	9 x 160

Notes. The probability of attaining RCA (RCA>1) in HCI products for Korea versus other countries in the post-1972 period. Regressions include industry-by-year effects. Data is restricted to treated industries. Two-way standard errors are clustered at the industry and country levels. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.

TABLE D1

Robustness: Learning in Industrial-Level Data, by Treatment Status

	Outcomes													
	Price (log)		Unit Cost (log)		Unit Cost (revenue, log)		TFP (OP)		TFP (ACF)		TFP (LP)		TFP (W)	
\textit{Alternative measures:}	Experience per worker (1)	Experience (alternative) (2)	Experience per worker (3)	Experience (alternative) (4)	Experience per worker (5)	Experience (alternative) (6)	Experience per worker (7)	Experience (alternative) (8)	Experience per worker (9)	Experience (alternative) (10)	Experience per worker (11)	Experience (alternative) (12)	Experience per worker (13)	Experience (alternative) (14)
Experience	-0.197***	-0.155***	-0.101***	-0.110***	-0.101***	-0.115***	0.355***	0.409***	0.403***	0.456***	0.439***	0.459***	0.428***	0.444***
	(0.029)	(0.027)	(0.015)	(0.014)	(0.015)	(0.015)	(0.060)	(0.055)	(0.060)	(0.061)	(0.059)	(0.058)	(0.065)	(0.062)
Targeted × Experience	-0.058***	-0.061***	-0.045***	-0.042***	-0.043***	-0.039***	0.039	0.050**	0.036	0.039	0.087***	0.138***	0.092***	0.144***
	(0.014)	(0.014)	(0.010)	(0.010)	(0.011)	(0.010)	(0.031)	(0.025)	(0.031)	(0.024)	(0.032)	(0.026)	(0.033)	(0.026)
Controls														
Controls for Size	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls for Capital Intensity	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls for Intermediates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls for Investment	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.961	0.960	0.899	0.901	0.903	0.905	0.981	0.981	0.878	0.882	0.986	0.986	0.990	0.990
Observations	3427	3429	3427	3429	3426	3428	3426	3428	3426	3428	3426	3428	3426	3428
Clusters	263	263	263	263	263	263	263	263	263	263	263	263	263	263
Combined Effects														
Linear Combination	-0.255	-0.216	-0.146	-0.152	-0.145	-0.154	0.394	0.459	0.439	0.495	0.526	0.597	0.519	0.588
(St.Err.)	(0.029)	(0.026)	(0.016)	(0.016)	(0.016)	(0.016)	(0.051)	(0.056)	(0.051)	(0.059)	(0.054)	(0.061)	(0.057)	(0.064)

Notes. This table shows the robustness of industry-level estimates from equation eqrefref:eq:lbdindustry for alternative outcomes. Unit Cost is the baseline unit cost measure: (log) total real intermediate cost per real gross output; Unit Cost (revenue) is measured using total real intermediate costs per unit of real revenue. TFP outcomes are estimated using Ackerberg-Caves-Frazer (ACF), Levinsohn-Petrin (LP), Olley-Pakes (OP), and Wooldridge (W) methods. Table shows estimates for each outcome using two alternative Experience measures: (log) Experience per worker, and Experience (alternative), which is experience calculated using cumulative value added units. All equations control for size and scale: (log) average plant size and total industry 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 for Experience for targeted industries. All specifications are estimated using industry and year fixed effects. Standard errors are clustered at the industry level. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.

TABLE D2
Robustness: Plant and Industry-Level Learning, by Treatment Status

	Panel A) Experience					Panel B) Experience (per worker)				
		TFP					TFP			
	Unit cost (revenue)	(ACF)	(LP)	(W)	(OP)	Unit cost (revenue)	(ACF)	(LP)	(W)	(OP)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Plant Experience	-0.072***	0.456***	0.454***	0.457***	0.456***	-0.076***	0.466***	0.465***	0.473***	0.465***
	(0.002)	(0.010)	(0.010)	(0.010)	(0.010)	(0.003)	(0.011)	(0.012)	(0.012)	(0.011)
Targeted × Plant Experience	-0.007***	0.007	0.016**	0.019**	0.009	-0.005**	-0.005	-0.001	-0.011	-0.003
	(0.002)	(0.007)	(0.008)	(0.007)	(0.007)	(0.002)	(0.009)	(0.010)	(0.010)	(0.010)
Industry Experience	-0.011***	0.019*	0.032***	0.028**	0.024**	-0.000	-0.000	0.008	0.003	0.003
	(0.003)	(0.011)	(0.012)	(0.011)	(0.011)	(0.003)	(0.009)	(0.011)	(0.010)	(0.010)
Targeted × Industry Experience	-0.004***	0.014**	0.008	0.007	0.012**	-0.010***	0.046***	0.048***	0.054***	0.046***
	(0.001)	(0.006)	(0.006)	(0.006)	(0.006)	(0.003)	(0.010)	(0.010)	(0.010)	(0.010)
Controls										
Control for Plant Size	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control for Capital	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control for Skill Ratio	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control for Investment	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control for Intermediates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Polynomial Controls	No	No	No	No	No	No	No	No	No	No
Plant Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.889	0.659	0.759	0.731	0.670	0.889	0.657	0.757	0.729	0.668
Observations	251029	236257	236257	236257	236257	251029	236257	236257	236257	236257
Clusters (Industry and Plant)	489 x 59999	489 x 57980	489 x 57980	489 x 57980	489 x 57980	489 x 59999	489 x 57980	489 x 57980	489 x 57980	489 x 57980
Combined Effects										
Linear Combination (Plant-Level)	-0.079	0.463	0.471	0.475	0.465	-0.081	0.461	0.464	0.462	0.462
(St.Err.)	(0.003)	(0.012)	(0.012)	(0.012)	(0.012)	(0.003)	(0.013)	(0.013)	(0.013)	(0.013)
Linear Combination (Industry-Level)	-0.015	0.033	0.040	0.034	0.036	-0.010	0.045	0.055	0.058	0.048
(St.Err.)	(0.003)	(0.010)	(0.011)	(0.011)	(0.011)	(0.003)	(0.011)	(0.012)	(0.012)	(0.012)

Notes. This table shows the robustness of plant-level estimates from equation eqrefref:eq:lbdindustry for alternative outcomes. Unit Cost is measured using total real intermediate costs per unit of (real) revenue. TFP outcomes are estimated using Ackerberg-Caves-Frazer (ACF), Levinsohn-Petrin (LP), Olley-Pakes (OP), and Wooldridge (W) methods. Panel A shows estimates for log Experience, and Panel B shows log Experience per worker. 'Plant Experience' refers to plant-level cumulative learning, and 'Industry Experience' refers to industry-level learning, calculated at the 4-digit industry level. All equations control for log plant size (workers). 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 for Plant and Industry Experience for HCI establishments. All specifications are estimated using plant, industry, and year fixed effects. Two-way standard errors are clustered at the industry and plant levels. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.

TABLE D3

Differences in Trade Policy by Treatment Status, 1968-1982

	Outcomes: Levels								Outcomes: Changes			
	Tariff Rate (log)				Quantitative Restrictions (log)				Tariff Rate (log)		Quantitative Restrictions (log)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A) Output Protection												
Targeted	-0.438***	-0.492***	-0.430***	-0.483***	-0.146**	-0.190***	-0.138**	-0.182***	0.012	0.012	0.028	0.022
	(0.123)	(0.104)	(0.123)	(0.104)	(0.057)	(0.052)	(0.058)	(0.053)	(0.022)	(0.024)	(0.017)	(0.014)
Year Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls		Yes		Yes		Yes		Yes		Yes		Yes
Sample	Full	Full	Post-1973	Post-1973	Full	Full	Post-1973	Post-1973	Full	Full	Full	Full
R^2	0.149	0.527	0.131	0.533	0.088	0.265	0.097	0.291	0.160	0.203	0.062	0.250
Observations	522	516	435	430	435	430	348	344	261	258	435	430
Clusters	87	86	87	86	87	86	87	86	87	86	87	86
Panel B) Exposure to Input Protection												
Targeted	-0.199**	-0.314***	-0.234**	-0.356***	-0.044*	-0.070***	-0.051*	-0.076***	-0.042***	-0.052***	-0.021**	-0.015
	(0.098)	(0.076)	(0.102)	(0.079)	(0.026)	(0.024)	(0.026)	(0.023)	(0.012)	(0.014)	(0.009)	(0.011)
Year Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls		Yes		Yes		Yes		Yes		Yes		Yes
Sample	Full	Full	Post-1973	Post-1973	Full	Full	Post-1973	Post-1973	Full	Full	Full	Full
R^2	0.123	0.256	0.091	0.243	0.158	0.252	0.169	0.265	0.184	0.297	0.198	0.263
Observations	522	516	435	430	435	430	348	344	435	430	261	258
Clusters	87	86	87	86	87	86	87	86	87	86	87	86

Notes. Table shows trade policy by treatment status (targeted vs. non-targeted), using nominal trade policy data for 1968-1982 (intermittent). Columns (1-8) show estimates in levels and columns (9-12) show changes. All regressions are at the 4-digit industry level. Columns (1-4) report estimates for log tariffs. Columns (5-8) report estimates for log quantitative restriction coverage. Columns (9-10) show estimates for changes in log tariff rates. Columns (11-12) show estimates for changes in log quantitative restrictions. Panel A presents tariff and quantitative restriction outcomes for output market protection (industry-level): the average level or change in tariff or quantitative restriction coverage. Panel B shows outcomes for input protection. Exposure to input protection is calculated using the weighted sum of tariffs or QRs for an industry's input basket, with weights taken from the 1970 input-output accounts. See text for calculation. Sample refers to whether all five periods are used, or whether only post-HCI (1973) observations are used.

TABLE E1
TOTAL LINKAGE EXPOSURE AND OUTPUT

	Outcome: Value Added (log)			
	A) Five-Digit Panel (1970-1986)		B) Four-Digit Panel (1967-1986)	
	Full Sample	*Non-HCI Sample*	*Full Sample*	*Non-HCI Sample*
	(1)	(2)	(3)	(4)
Post × Forward Linkage	1.909***	3.388***	0.988**	1.512*
	(0.516)	(0.857)	(0.485)	(0.853)
Post × Backward Linkage	-0.0536	0.0452	-0.574	-1.316**
	(0.175)	(0.197)	(0.393)	(0.511)
Industry Effects	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
Targeted X Year	Yes	No	Yes	No
R^2	0.777	0.765	0.849	0.828
Observations	4720	2986	1750	1096
Clusters	278	176	88	55

Notes. This table shows average differences-in-differences estimates, before and after 1973. Estimates correspond to equation eqrefeq:networkflexible where regressions interact linkage measures with a Post indicator. Both linkage interactions (forward and backward) are shown. Note that dynamic figures present only estimates for the linkage of interest. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.

TABLE E2

Total Linkage Exposure and Output Prices

	Outcome: Prices (log)			
	A) Five-Digit Panel (1970-1986)		B) Four-Digit Panel (1967-1986)	
	Full Sample	*Non-HCI Sample*	*Full Sample*	*Non-HCI Sample*
	(1)	(2)	(3)	(4)
Post × Forward Linkage	-0.289***	-0.406***	-0.344***	-0.421***
	(0.0726)	(0.0883)	(0.111)	(0.158)
Post × Backward Linkage	0.0500***	0.0463***	0.103	0.241***
	(0.0127)	(0.0100)	(0.0651)	(0.0318)
Industry Effects	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
Targeted X Year	Yes	No	Yes	No
R^2	0.958	0.943	0.963	0.959
Observations	4721	2987	1751	1097
Clusters	278	176	88	55

Notes. This table shows average differences-in-differences estimates, before and after 1973. Estimates correspond to equation eqrefeq:networkflexible where regressions interact linkage measures with a Post indicator. Both linkage interactions (forward and backward) are shown. Note that dynamic figures present only estimates for the linkage of interest. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.

TABLE E3

Direct Linkage Exposure and Industrial Development Outcomes

	Panel A) Five-Digit Panel (1970-1986)										Panel B) Four-Digit Panel (1967-1986)							
	Outcomes (log)										Outcomes (log)							
	Employment		Num. Plants		Labor Prod.		TFP (ACF)		Avg. Wage.		Employment		Num. Plants		Labor Prod.		Avg. Wage.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post × Forward Linkage	1.788***	3.403***	1.499***	2.828***	0.699**	0.473	1.350***	0.868	0.629***	0.330	1.645**	2.925**	1.897***	3.564***	0.0722	-0.283	0.182	0.107
	(0.685)	(1.108)	(0.393)	(0.562)	(0.325)	(0.390)	(0.499)	(0.588)	(0.220)	(0.233)	(0.777)	(1.143)	(0.639)	(0.677)	(0.297)	(0.424)	(0.173)	(0.219)
Post × Backward Linkage	-0.0917	0.116	0.105	0.177	0.116	0.104	0.0536	0.00233	0.0424	0.0307	-0.373	-1.084	0.279	0.0609	-0.137	-0.890	-0.140	-0.702**
	(0.258)	(0.279)	(0.104)	(0.115)	(0.105)	(0.123)	(0.0925)	(0.0680)	(0.0736)	(0.0866)	(0.323)	(0.727)	(0.210)	(0.397)	(0.287)	(0.539)	(0.193)	(0.319)
Industry Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Targeted × Year	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
R^2	0.797	0.807	0.867	0.873	0.750	0.687	0.705	0.673	0.774	0.714	0.853	0.848	0.892	0.895	0.847	0.777	0.853	0.791
Observations	4726	2992	4726	2992	4714	2981	4214	2682	4721	2987	1760	1100	1760	1100	1750	1096	1751	1097
Clusters	278	176	278	176	278	176	264	167	278	176	88	55	88	55	88	55	88	55
Sample	Full	Non-HCI	Full	Non-HCI	Full	Non-HCI	Full	Non-HCI	Full	Non-HCI	Full	Non-HCI	Full	Non-HCI	Full	Non-HCI	Full	Non-HCI

Notes. This table shows average differences-in-differences estimates, before and after 1973. Estimates correspond to equation eqrefeq:networkflexible where regressions interact linkage measures with a Post indicator. Both linkage interactions (forward and backward) are shown. Note that dynamic figures present only estimates for the linkage of interest. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.

TABLE E4
Total Linkage Exposure and Industrial Development

	Panel A) Five-Digit Panel (1970-1986)										Panel B) Four-Digit Panel (1967-1986)							
	Outcomes (log)										Outcomes (log)							
	Employment		Num. Plants		Labor Prod.		TFP (ACF)		Avg. Wage.		Employment		Num. Plants		Labor Prod.		Avg. Wage.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post × Forward Linkage	1.139***	2.449***	0.882***	1.868***	0.534***	0.550**	0.820***	0.737**	0.432***	0.382***	0.632	1.404*	0.747**	1.919***	0.0721	-0.172	0.107	0.0401
	(0.386)	(0.637)	(0.227)	(0.338)	(0.182)	(0.227)	(0.252)	(0.299)	(0.124)	(0.138)	(0.421)	(0.779)	(0.371)	(0.571)	(0.175)	(0.298)	(0.104)	(0.162)
Post × Backward Linkage	-0.0856	0.0311	0.0287	0.0803	0.0482	0.0289	-0.0120	-0.0194	0.0148	0.000387	-0.352	-0.731**	-0.0472	-0.188	-0.151	-0.508***	-0.120	-0.366***
	(0.136)	(0.143)	(0.0552)	(0.0573)	(0.0567)	(0.0682)	(0.0459)	(0.0453)	(0.0390)	(0.0468)	(0.229)	(0.347)	(0.117)	(0.212)	(0.168)	(0.176)	(0.112)	(0.117)
Industry Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Targeted × Year	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
R^2	0.798	0.808	0.867	0.874	0.750	0.687	0.706	0.674	0.775	0.715	0.855	0.852	0.890	0.894	0.849	0.786	0.855	0.803
Observations	4726	2992	4726	2992	4714	2981	4214	2682	4721	2987	1760	1100	1760	1100	1750	1096	1751	1097
Clusters	278	176	278	176	278	176	264	167	278	176	88	55	88	55	88	55	88	55
Sample	Full	Non-HCI	Full	Non-HCI	Full	Non-HCI	Full	Non-HCI	Full	Non-HCI	Full	Non-HCI	Full	Non-HCI	Full	Non-HCI	Full	Non-HCI

Notes. This table shows average differences-in-differences estimates, before and after 1973. Estimates correspond to equation eqrefeq:networkflexible where regressions interact linkage measures with a Post indicator. Both linkage interactions (forward and backward) are shown. Note that dynamic figures present only estimates for the linkage of interest. * Significant at the 10 percent level. ** Significant at the 5 percent level. *** Significant at the 1 percent level.