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Hisaki Kono, Hoang-Minh Le, Manabu Nose, Yasuyuki Sawada

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Graduate School of Economics
Kyoto University
Yoshida-Hommachi, Sakyo-ku
Kyoto City, 606-8501, Japan

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Hisaki Kono[*] Hoang-Minh Le[†] Manabu Nose[‡] Yasuyuki Sawada[§]

Abstract

This paper examines the local economic impacts of industrial zones (IZs) in Vietnam, focusing on how their sectoral orientation within production networks shapes effectiveness. Using panel data on registered firms and a newly compiled dataset on IZ locations and sectoral compositions, we estimate the dynamic effects of IZ establishment on firm entry and employment through staggered difference-in-differences and synthetic control methods. We find that IZs lead to sustained increases in both firm and worker density over a 6–10 year horizon, indicating substantial local economic gains. These effects are particularly pronounced in zones oriented toward downstream industries—those that create demand for upstream suppliers—while upstream orientation does not predict stronger outcomes. We further show that backward production linkages mediate these gains, suggesting that demand-side constraints, rather than input frictions, may be more binding in developing country contexts. The results highlight not only the overall effectiveness of IZs but also the importance of aligning industrial policy design with the structure of production networks to maximize spatial development benefits.

JEL Classification: O12; O14; R11

Keywords: Industrial zones, production linkage

[*]Graduate School of Economics, Kyoto University. E-mail: kono@econ.kyoto-u.ac.jp.
[†]Graduate School of Economics, Kyoto University. E-mail: lehoangminhapu95@gmail.com
[‡]Graduate School of Economics, Keio University. E-mail: mnose@keio.jp
[§]Graduate School of Economics, University of Tokyo. E-mail: sawada@e.u-tokyo.ac.jp

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1 Introduction

Place-based industrial policies (PBIPs) are a central tool in many developing countries for promoting economic development, reducing regional disparities, and facilitating structural transformation (Atalay et al., 2023; Bailey et al., 2023). A widely adopted form of PBIP is the establishment of industrial zones (IZs)—geographically defined areas offering preferential treatment to firms—to attract investment, foster agglomeration, and stimulate industrial growth.

Despite the proliferation of IZs and a growing body of research on their effects, little is known about how the industrial composition of these zones influences their local economic impact. Many IZs target specific sectors, such as high-tech industries, automobile manufacturing, or sectors with strong production linkages. If the sectoral orientation of a zone matters for its effectiveness, then the design of PBIPs should consider which types of industries to prioritize. However, the existing empirical literature focuses primarily on the average effects of IZs (Wang, 2013; Neumark and Simpson, 2015; Chaurey, 2017; Lu et al., 2019; Kahn et al., 2021; Incoronato and Lattanzio, 2024; McCaig et al., 2024; Garin, 2025; Rothenberg et al., 2025), offering limited insight into which sectors or linkages drive the observed impacts.

This paper addresses that gap by examining how the sectoral orientation of IZs, specifically, their degree of upstreamness and downstreamness, affected local firm entry and employment in Vietnam. Studies about industrial policy offer competing perspectives. Liu (2019) argues that when economies face input distortions, targeting upstream sectors yields the largest welfare gains. Yet this reasoning omits demand-side frictions, which may be more salient in settings where firms struggle to find customers rather than suppliers. Historically, many Asian countries have pursued industrialization by promoting downstream, labor-intensive sectors like garment manufacturing and assembly (World Bank, 1993; Gereffi, 2018). While recent evidence from developed economies suggests symmetric amplification of shocks through both backward and forward linkages (Carvalho et al., 2021), there is limited empirical evidence on how such linkages shape the local effects of industrial policy in developing countries.

We utilize firm-level panel data on registered enterprises in Vietnamese, combined with detailed information on the location, timing, and sectoral composition of IZs across the country. Drawing on the industry-level upstreamness and downstreamness measures developed by Antràs and Chor (2018), we calculate zone-level indices that characterize each IZ's position in the production network. Using this data, we estimate the causal impact of IZ establishment on the number of firms (extensive margin) and the number of employed workers (intensive margin) at both the district and commune levels by using the staggered difference-in-differences (DID) framework (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; Borusyak et al., 2024) and synthetic DID method (Arkhangelsky et al., 2021). We further extend the imputation-based method by Borusyak et al. (2024) to evaluate how variation in treatment characteristics, namely, upstreamness and downstreamness, explains differences in treatment effects, controlling for other observable district and IZ characteristics.

Our results reveal that IZ establishment leads to sustained increases in local firm entry and employment, with effects becoming more pronounced over a 6–10 year horizon. Notably, we find that zones with higher downstreamness—those that create demand for upstream suppliers—generated significantly larger impacts on the number of firms (extensive margin) compared to zones with similar upstreamness. These results are robust across geographic levels of analysis and estimation strategies. In contrast, we observe no significant differences in the impact on worker density (intensive margin) between upstream- and downstream-oriented zones, suggesting symmetric labor responses regardless of linkage direction, consistent with findings by Carvalho et al. (2021).

This paper contributes to the literature on the design of place-based policies in developing countries. Unlike prior studies that estimate the impact of IZs either in aggregate or within specific contexts, we directly examine how zone-level characteristics shape their effectiveness. Our findings also speak to the literature on industrial policy under distortions (Liu, 2019;

Bartelme et al., 2019; Baqaee and Farhi, 2020), emphasizing the importance of sales-side constraints. In many developing contexts, firms may be more constrained by limited access to markets than by input availability, highlighting the potential for downstream-oriented policies to relax demand bottlenecks and unlock agglomeration spillovers.

We also propose an empirical strategy to evaluate the role of continuous, multidimensional treatment characteristics in shaping program impacts. By leveraging recent advances in imputation-based DID methods, we provide a framework for identifying the importance of treatment heterogeneity without relying on arbitrary discretization or subgroup analyses. Our empirical results exemplify that simple sample splits based on a single characteristic can yield misleading conclusions due to omitted variable bias. This approach can be broadly applied to other settings where treatment intensity or characteristics varies considerably across units.

The paper is structured as follows. The next section provides background on IZ policies in Vietnam and describes the data, including the construction of upstreamness and downstreamness measures. Section 3 presents the dynamic impacts of IZs, followed by an analysis of impact heterogeneity by upstreamness and downstreamness in Section 4. Section 5 offers additional evidence on the role of production linkages to firms in IZs. Section 6 concludes.

2 Background and Data

2.1 Industrial zones in Vietnam

IZs are one of the major place-based policy instruments in Vietnam. Introduced in 1994 under Decree No. 192-CP, IZs offer a range of incentives to attract investment, including preferential tax treatment (such as corporate income tax exemptions and reductions), import duty exemptions, land rent reductions or exemptions, simplified administrative procedures for licensing and customs, and infrastructure support. Beyond these direct incentives, firms also benefit from agglomeration economies, which further enhance the attractiveness of operating within IZs.

To establish an IZ in Vietnam, the managing entity must first submit an application to the provincial People's Committee. According to the 2018 Decree on Industrial and Economic Zones (Decree No. 82/2018/ND-CP), the provincial People's Committee may issue the official decision to establish an industrial zone (IZ) only after the following conditions are met:

- The IZ is listed in the province's approved Master Plan for Socio-Economic Development.
- The construction plan for the IZ is approved by the Ministry of Construction.
- The developer receives an investment registration certificate, which authorizes them to implement the construction plan and grants the right to operate the zone, typically under a 50-year leasehold arrangement.

Once these requirements are met and the Investment Registration Certificate is issued, the provincial government can formally establish the IZ. We define the establishment date of an IZ as the date on which this certificate is issued, as it is typically available through public sources such as government announcements and news reports.

Following approval, the developer undertakes land clearance and compensation to prepare the site for construction. This process often takes several years and is influenced by factors such as the completeness of application documents, the administrative efficiency of provincial authorities, and the complexity of land acquisition. In particular, delays often arise when there is insufficient land available to resettle displaced residents. For instance, the Amata City Long Thanh IZ in Dong Nai Province received its investment certificate in 2015, but did not commence operations until 2022 due to prolonged delays in land clearance and compensation. In contrast, the Amata City Ha Long IZ in Quang Ninh Province, managed by the same developer, began operations just three years after receiving its certificate in 2018, reflecting smoother implementation processes.

The development of IZs typically follows a phased expansion model. Rather than waiting for the completion of the entire zone, operations often begin in a limited section and expand gradually over time. For example, the Amata City Ha Long IZ (714 ha in total) began operations in only 123 ha in year 2021, extended to an additional 120 ha in 2023, and reached full operational capacity by 2025. This phased approach allows IZs to commence operations earlier, facilitating investment and firm entry while the remaining infrastructure is still under development.

2.2 Data

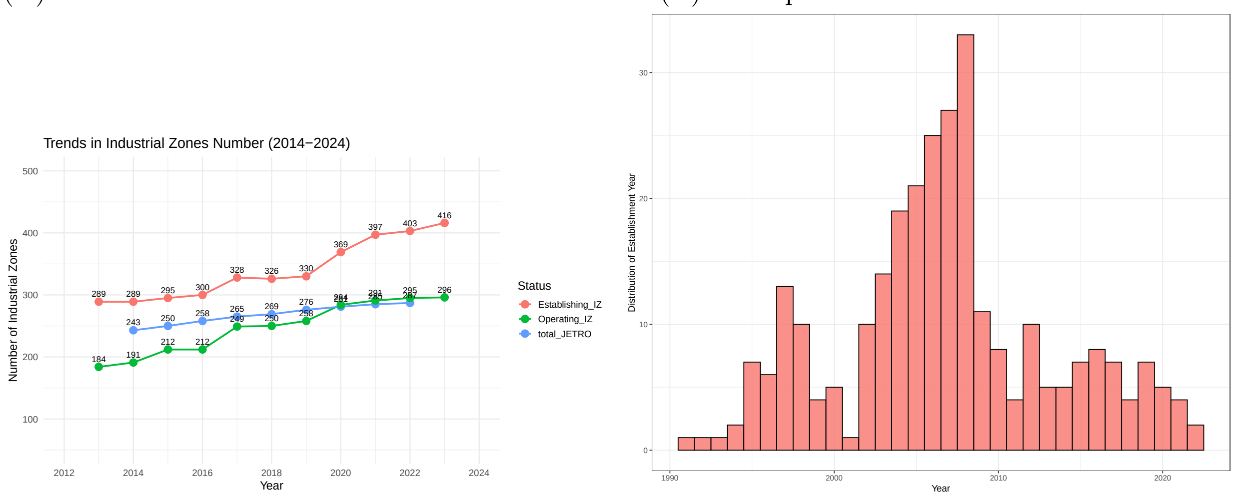
2.2.1 Industrial zone data

Panel (A) of Figure 1 presents the total number of IZs from 2013 onward. Data from 2014 to 2022 are based on annual reports published by the Ministry of Planning and Investment (MPI), while figures for 2013 and 2023 are supplemented with information from various news sources. The red line represents the cumulative number of established IZs, while the green line shows the number of IZs currently in operation.

Figure 1: Total number of Industrial Zones

(A) Cummulative number of IZs

(B) IZs reported in JETRO database



Source: Ministry of Planning and Investment. JETRO Industrial Zone Database

Since establishment year data prior to 2014 were not available from MPI reports, we rely on an alternative source: the Industrial Zone Database compiled by the Hanoi and Ho Chi Minh City offices of the Japan External Trade Organization (JETRO) as of 2024.[1] This database, designed to promote foreign direct investment (particularly from Japan), contains detailed geographic information and lists of firms located in each IZ. It was compiled through a questionnaire survey distributed to IZs across Vietnam, primarily those in operation, with assistance from local coordinators.

The JETRO database offers high coverage. Panel (A) of Figure 1 includes the number of established IZs derived from the JETRO database, shown alongside the number of operating

[1]The JETRO database is regularly updated.

IZs reported in MPI annual reports. In the JETRO dataset, establishment years are inferred from lease terms, calculated as 50 years prior to the lease expiration date.[2] The JETRO data cover approximately 85% of the IZs identified as established in MPI reports. Discrepancies between the two sources become more pronounced in recent years, largely because the JETRO database focuses on IZs that are already operational. Consequently, the number of IZs in the JETRO dataset more closely aligns with the number of operating zones reported by MPI in recent years.[3]

Panel (B) of Figure 1 displays the distribution of IZ establishment years as reported in the JETRO data. Several zones were established before 2000, primarily in districts within or near major cities.[4] A large number of IZs were established between 2002 and 2008, with a modest deceleration in the pace of establishment after 2010.

Note that the JETRO database also provides a more comprehesive list of IZs in Vietnam, including 117 IZs for which no questionnaire response was received or to which no survey was sent. When these IZs are added to the total count, the number of IZs in the JETRO database closely approximates the number reported by MPI. However, establishment years for these additional IZs are not available.

2.2.2 Firm-level data

To measure local economic activity, we aggregate firm-level data from the Vietnam Enterprise Survey (VES) at either the district or commune level. For ease of exposition, we refer to "districts" throughout this section; the construction of commune-level data follows the same procedure, simply replacing "district" with “commune.”[5]

The VES has been conducted annually since 2000, but coverage in the early years was

[2]Some IZs lack the lease period information. For these IZs, we conducted targeted internet searches to determine establishment dates. Using this approach, we identified establishment years for 287 out of the 296 IZs listed in the JETRO database. The remaining nine IZs were not yet established as of 2023.

[3]While the start of operation is not formally defined in legal documents, it likely corresponds to the point at which the zone is functionally ready to host external firms.

[4]McCaig et al. (2024) documents that most IZs established before 2000 were located in more urbanized areas, whereas those established after 2000 were typically in more agrarian regions.

[5]Some studies use nighttime light (NTL) intensity as a proxy for local economic activity (Henderson et al., 2012; Michalopoulos and Papaioannou, 2014). However, NTL data often capture illumination from public infrastructure and are poorly suited to tracking time-series variation in economic activity, especially at finer spatial scales (Chen and Nordhaus, 2019; Gibson et al., 2021; Perez-Sindin et al., 2021). Moreover, because the establishment of industrial zones (IZs) typically coincides with infrastructure development, using NTL may confound infrastructure improvements with actual economic growth, potentially biasing the estimates.

limited.[6] We therefore use the data from 2004 to 2023, when coverage becomes sufficiently broad.

Although the VES is conducted every year, the full questionnaire is administered only to a subset of the firms. For the remaining firms, the survey collects basic information such as the number of employees and imputes other variables based on previous years' data and observable firm characteristics. Given these limitations, we focus on two consistently reported indicators: the number of firms and the total number of employees at the district or commune level. Both variables are transformed using the inverse hyperbolic sine function to address skewness and accommodate observations with zero values.

2.3 Selection of control localities

To estimate the causal impact of IZs, it is crucial to construct an appropriate comparison group. Importantly, IZs were rarely established in city centers due to limited land availability of land, or in remote areas with poor accessibility. To address this non-random placement, we exclude from our sample all districts located within 1 km of a provincial capital, as well as those with a population density below 100 persons per km^2 as of 2001.[7]

We also exclude districts where IZs were established before 2004, in order to ensure that treatment occurs after the start of our study period. In addition, districts with IZs lacking establishment year information are excluded. After applying these criteria, our final sample comprises 388 districts and 5518 communes. Spatial distribution of the sampled districts is shown in Appendix Figure 1.

For our main analysis, we further restrict the sample by excluding districts where the IZs host only agriculture-related firms or where firm lists are unavailable. This restriction is necessary because our primary outcome variables–the number of registered firms and total number of employed workers in these firms–are based on the VES, which does not capture economic activity involving agricultural households. As a result, IZs accommodating agricultural sectors only are not well represented in our dataset.

[6] Appendix B.1 reports the number of communes included in each survey wave. In the first four years, only 60 to 70 communes were covered, whereas coverage expanded to between 7,000 and 10,000 communes in subsequent years.

[7] In our data, only four IZs were established in communes with a population density below per km^2 as of 2001.

2.4 Upstreamness and downstreamness of IZs

We measure the upstreamness and downstreamness of IZs using the weighted average of industry-level indices developed by Antràs and Chor (2018). Let US_s^{AC} and DS_s^{AC} denote the upstreamness and downstreamness of industry s, respectively, as defined by Antràs and Chor:

$$US_s^{AC} = 1 \times \frac{F_s}{Y_s} + 2 \times \frac{\sum_{r=1}^{S} a_{sr} F_r}{Y_s} + 3 \times \frac{\sum_{r=1}^{S} \sum_{q=1}^{S} a_{sr} a_{rq} F_q}{Y_s} + \cdots = \left([\mathbf{I} - \mathbf{A}]^{-1} \mathbf{Y} \right)_s$$

$$DS_s^{AC} = 1 \times \frac{VA_s}{Y_s} + 2 \times \frac{\sum_{r=1}^{S} b_{rs} VA_r}{Y_s} + 3 \times \frac{\sum_{r=1}^{S} \sum_{q=1}^{S} b_{qr} b_{rs} VA_q}{Y_s} + \cdots = \left([\mathbf{I} - \mathbf{B}]^{-1} \mathbf{Y} \right)_s ,$$

where Y_s and VA_s are the gross output and value added of sector s, respectively. The coefficients $a_{sr} = Z_{sr}/Y_r$ and $b_{rs} = Z_{rs}/Y_r$ are the input-output coefficients, where Z_{sr} is the value of sector s's output used in sector r. Specifically, a_{sr} is the dollar amount of sector s's output needed to produce one dollar worth of sector r's output, while b_{rs} is the share of sector r's output that is used in sector s. Z_{sr} corresponds to the (s, r)-th entry of I-O table. The expressions $\left([\mathbf{I} - \mathbf{A}]^{-1}\mathbf{Y}\right)_s$ and $\left([\mathbf{I} - \mathbf{B}]^{-1}\mathbf{Y}\right)_s$ denote the s-th elements of the Leontief inverse times output vector. We computed these indices using Vietnam Input-Output (I-O) table for year 2012. Appendix Table 2 lists the top 20 industries by upstreamness and downstreamness.

We then compute the upstreamness and downstreamness of IZs in district i as

$$US_i^{IZ} = \frac{\sum_{j=1}^{n_i} w_{jis} \cdot US_s^{AC}}{\sum_{j=1}^{n_i} w_{jis}},$$

$$DS_i^{IZ} = \frac{\sum_{j=1}^{n_i} w_{jis} \cdot DS_s^{AC}}{\sum_{j=1}^{n_i} w_{jis}},$$

where j indexes firms located in IZs in district i, and n_i is the total number of firms located in those IZs. The weight w_{jis} is designed to give greater importance to larger firms and is defined as

$$w_{jis} = \mathrm{med}_t \left(\frac{sales_{jis,t}}{employment_{jis,t}} \right) \times employment_{jis,t_{0,j}},$$

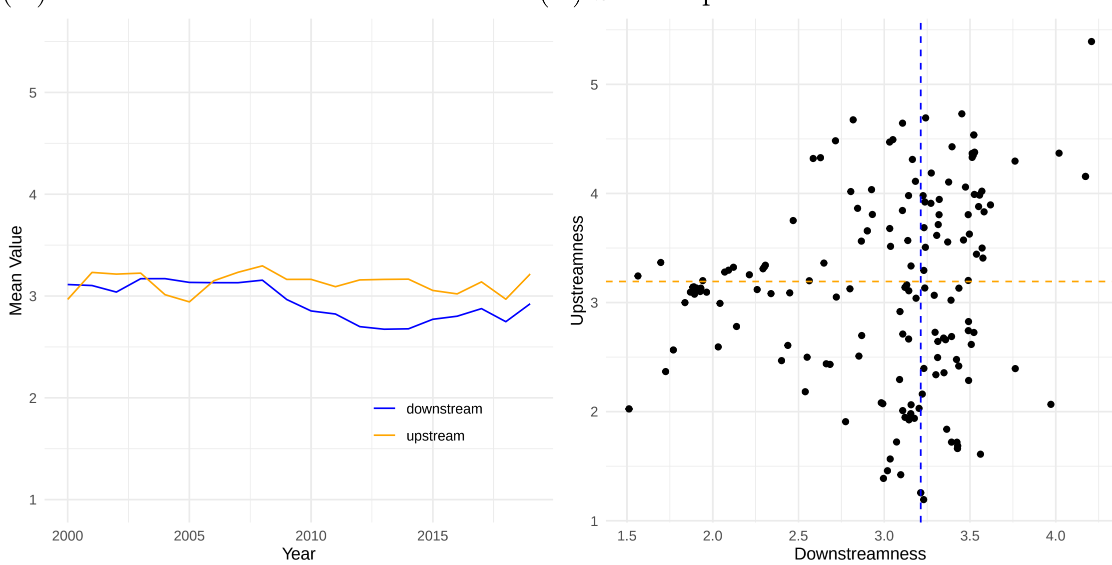
where $t_{0,j}$ indicates the first year of operation for firm j. To mitigate potential reverse causality, where firm entry is influenced by contemporaneous local economic conditions, we restrict the sample to firms established within three years of IZ opening in computing the weight. The choice of this bandwidth also reflects the fact that IZ operation expands gradually, which often results in a limited number of firms entering in the very first year of operation. The median sales-to-employment ratio is used to mitigate the impact of short-term fluctuations in sales. This construction allows us to account for the stronger input-output linkages of larger firms.

We exclude agricultural sectors when computing US_i^{IZ} and DS_i^{IZ}, as the establishment of agro-processing plants may enhance agricultural productivity without stimulating the non-agricultural activity measured in the VES. Consequently, IZs that contain only agricultural-related industries are dropped from the analysis.[8] To prevent the presence of low-linkage sectors from unduly lowering the IZ-level indices, we set the weights w_{jis} to zero for firms whose US_s^{AC} or DS_s^{AC} values fall below the 25th percentile.

Figure 2 presents the national trends in average US_k^{IZ} and DS_k^{IZ}, along with a scatter plot of US_k^{IZ} against DS_k^{IZ}.[9]

Figure 2: IZ-level Upstreamness and Downstreamness

(A) Trend (B) Scatter plots



[8]Examples of high downstream agricultural sectors include: vegetable and animal oils and fats ($DS_s^{AC} = 5.55$, $US_s^{AC} = 3.12$), processed meat products ($DS_s^{AC} = 4.00$, $US_s^{AC} = 1.54$), poultry products ($DS_s^{AC} = 3.97$, $US_s^{AC} = 1.42$), dairy products ($DS_s^{AC} = 3.97$, $US_s^{AC} = 1.84$), and pig products ($DS_s^{AC} = 3.95$, $US_s^{AC} = 3.00$). The list of the top 20 industries by upstreamness and downstreamness excluding agricultural sectors are provided in Appendix Table 3.

[9]Our results remain robust across different cutoff values, although using higher thresholds tends to reduce variation across IZs.

Summary statistics for district-level variables and IZ characteristics are reported in Panel (A) of Table 1, while the corresponding statistics for the commune-level data are presented in Panel (B).

Table 1: Summary statistics

(A) District level data

variable	N	mean	sd	max	min
Total Operating Firms	7760	543.6	1375.0	14448.0	0.0
Total Workers in Operating Firms	7760	14064.1	32966.4	524651.0	0.0
Firms Per 10 Km2	7760	29.2	131.0	1864.7	0.0
Worker Per 10 Km2	7760	716.0	3654.5	67714.5	0.0
District Area(Km2)	7760	333.7	303.4	2105.4	4.2
Population Density	7760	1826.2	4558.9	27573.8	105.4
IZ Area(ha)	1200	1404.1	5915.4	45332.0	50.5
Dist to Capital(km)	7760	25.3	18.4	91.8	1.0
Upstream of IZ	1200	3.1	0.9	4.7	1.3
Downstream of IZ	1200	3.0	0.6	4.2	1.5
IZs in Districts	7760	0.2	0.7	6.0	0.0
Employment in IZ	1200	2214.1	3752.1	22622.7	8.0

(B) Commune level data

variable	N	mean	sd	max	min
Total Operating Firms	110360	37.5	123.9	4823.0	0.0
Total Workers in Operating Firms	110360	988.4	4077.9	211099.0	0.0
Firms Per 10 Km2	110360	38.7	163.4	6377.7	0.0
Worker Per 10 Km2	110360	899.7	6374.0	700308.6	0.0
Commune Area(Km2)	110360	14.4	15.5	172.0	0.1
Population Density	110360	1976.7	4996.5	35812.8	100.1
IZ Area(ha)	1840	1180.0	4831.3	45332.0	10.2
Dist to Capital(km)	110360	24.4	17.5	98.2	1.0
Has IZ	110360	0.0	0.2	1.0	0.0
Upstream (of IZ)	1840	3.1	0.9	4.7	1.2
Downstream (of IZ)	1840	2.9	0.6	4.2	1.5
Employment in IZ	1840	2680.0	4799.8	28805.8	8.0

3 Impact of Industrial Zone Establishments

3.1 Empirical Strategy

We estimate the average treatment effect on the treated (ATT) of IZ establishment using a staggered DID framework. Specifically, we consider the following regression model:

$$Y_{it} = \alpha_i + \delta_t + \sum_{s=0}^{10} \gamma_s 1[E_i - t = s] + \sum_{s=-10}^{-2} \rho_s 1[E_i - t = s] + \mathbf{X}_i \boldsymbol{\beta} + \epsilon_{it} \tag{1}$$

where E_{it} is the year of the first IZ establishment in district i, α_i denotes district fixed effects, δ_t year fixed effects, and $\mathbf{X}_i$ is a vector of baseline district characteristics. The outcome variable Y_{it} is defined either as the number of operating firms per $10km^2$ (firm density), or the number of workers employed in these firms per $10km^2$ (worker density). Firm density captures the extensive margin, while worker density reflects the intensive margin. The covariates $\mathbf{X}_i$ include the logarithm of population density as of 2001, the logarithm of distance to the provincial capital, and the logarithm of district area. The parameters of interests are γ_s, which capture the ATT in the s-years after the IZ establishment.

To estimate γ_s, we apply three recent approaches to staggered DID: (A) Callaway and Sant'Anna (2021), (B) Sun and Abraham (2021) and (C) (Borusyak et al., 2024) (BJS, hereafter). To allow for the possibility that outcome trends vary with observable characteristics, we also implement inverse probability weighting (IPW) based on estimated propensity scores, following Callaway and Sant'Anna (2021) and Roth et al. (2023). This method relaxes the parallel trends assumption by conditioning on observables, thus allowing trends to differ systematically across treatment and control groups depending on their baseline characteristics. To ensure the overlap assumption required for IPW, we exclude control group districts with propensity scores below 0.1[10]

In equation (1), the coefficients ρ_s capture pre-treatment dynamics and are commonly used to assess the validity of the parallel trends assumption.

In addition to the parallel trend assumption, DID analyses rely on the no-anticipation assumption, that is, observed outcomes in the pre-treatment periods reflect the potential outcome in the absence of treatment. However, as discussed earlier, the establishment of an

[10] Appendix Figure 3 shows the distributions of estimated propensity scores for treated and control groups, indicating the validity of the overlap assumption in the range above 0.1.

IZ typically involves a preparatory process that may take several years, potentially leading to anticipation effects in a couple of years before IZ establishment. To address this concern, we relax the no-anticipation assumption for the two years prior to IZ establishment and impose it only for periods three or more years before treatment. Accordingly, we omit the estimation of the pre-treatment coefficients ρ_1 and ρ_2.

Following BJS, we also perform a pre-trend test using only the pre-treatment observations of treated units, exploiting the variation in treatment timing in the staggered DID setting. Specifically, we examine whether the outcome trends differ systematically by the number of years until treatment, after controlling for district fixed effects and year fixed effects:

$$Y_{it} = \alpha_i + \delta_t + \sum_{s=-7}^{-3} \rho_s 1[E - t = s] + \epsilon_{it}. \tag{2}$$

The null is $H_0 : \rho_s = 0 \ \forall s$, indicating no differential trends prior to treatment.

In addition, we implemented the synthetic DID method proposed by Arkhangelsky et al. (2021). Unlike the conventional DID approaches (including staggered DID), which assume that the error term ϵ_{it} in 1 is mean independent of the treatment status, the synthetic DID method relaxes this assumption by constructing a synthetic control so that the evolution of its error term ϵ_{it} closely matches that of the treated units during the pre-treatment periods. Whereas IPW assigns positive weights to all control units, the synthetic DID selects only a small subset of control units to construct the synthetic control. Because this method requires at least two pre-treatment periods to estimate the weights, we exclude districts where IZs were established before 2006. Standard errors are calculated using the permutation-based inference procedure recommended by Arkhangelsky et al. (2021).

3.2 Results

3.2.1 District-level data

Figure 3 presents event-study plots obtained from the staggered DID analysis using the district-level data. Panel (i) displays the estimated effects on firm density, while panel (ii) shows the effects on worker density. Since IZs typically take several years to become operational due to land clearance, infrastructure development, and construction, immediate impacts are expected to be limited. To reflect this, we exclude the estimated coefficients for the two years immediately after IZ establishment. As discussed earlier, we also exclude the two years preceding IZ establishment from the pre-treatment period to account for potential anticipation effects.

Panels (A), (B), and (C) present the results from Callaway-Santa'Anna, Sun-Abraham, and BJS estimators, respectively. Orange dots and bars represent point estimates and 95% confidence intervals based on the full sample, while green markers show results from a restricted sample that excludes districts with IZs for which upstreamness and downstreamness could not be calculated, either due to missing firm-level data or the absence of non-agricultural firms within the first three years after IZ establishment.

All three estimation methods yield similar patterns. The establishment of IZs had positive effects on both the number of firms and the number of employed workers in local districts. These effects grew over time, reflecting the gradual operational rollout of IZs, and took approximately six to ten years to fully materialize. The estimated effects are generally larger in the restricted sample, likely because it excludes non-performing zones where IZs failed to attract substantial economic activity.

The figures also reveal no clear evidence of pre-treatment trends. Additional pre-trend tests using the BJS approach further support this finding, as reported in Table 2, showing no systematic differences in outcomes prior to treatment. These results help alleviate concerns about potential violations of the parallel trends assumption.

Table 2: BJS pre-trend tests

Test	Pr(>Chisq)	Chisq
Pre-trend F-Test (Firm)	0.42	3.89
Pre-trend F-Test (Worker)	0.22	5.79

The table reports the results of BJS pre-trend tests based on equation (2).

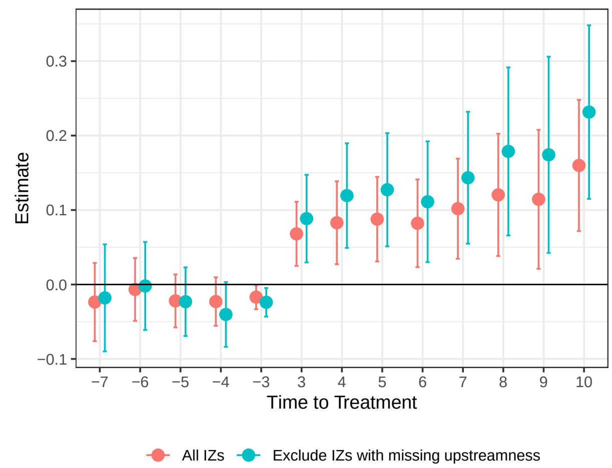
Figure 4 presents event-study plots based on the synthetic DID estimator, which addresses potential differences in pre-treatment trends by constructing synthetic controls that closely match the outcome trajectories of treated units prior to treatment.[11] The close alignment between the results from the synthetic DID estimator and those obtained from the three staggered DID methods suggests the absence of systematic differences in pre-treatment trends, thereby reinforcing the credibility of our staggered DID estimates.

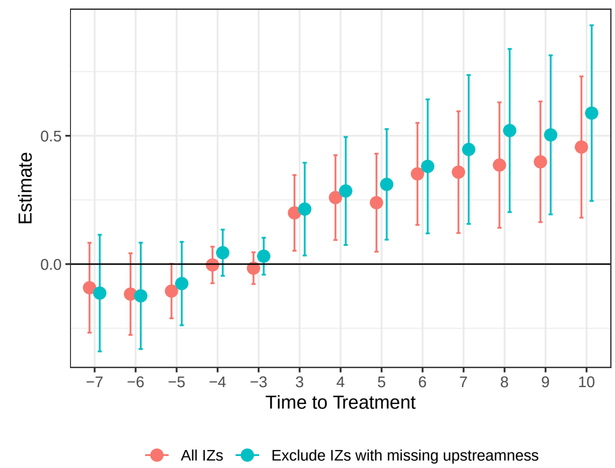
[11] Full estimation results from the synthetic DID (event-study plots by each cohort) are provided in Appendix Figures 4 and 5.

Figure 3: Event study analysis

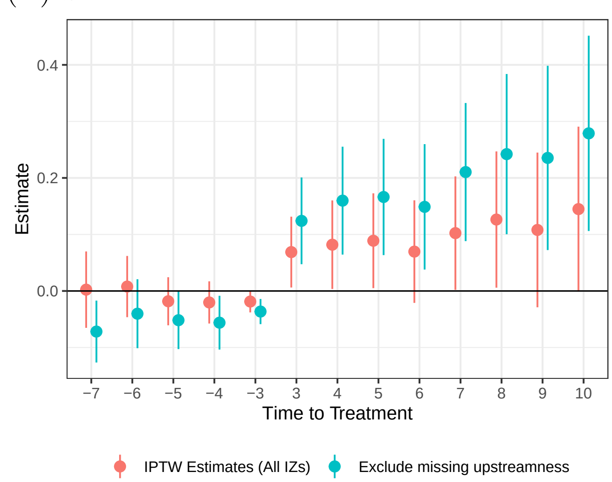
(i) Firm density (ii) Worker density

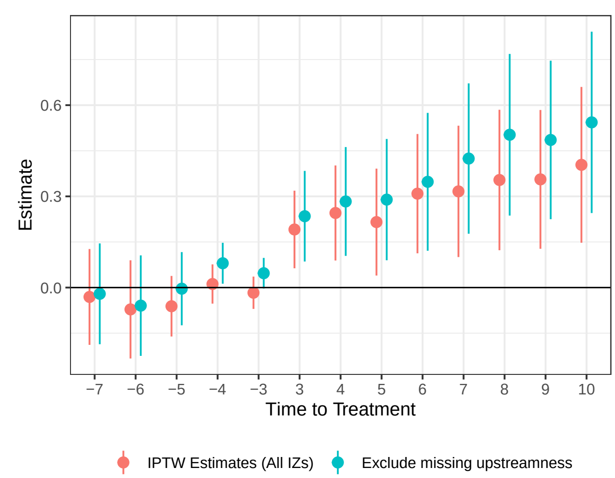
(A) Callaway-Sant'Anna



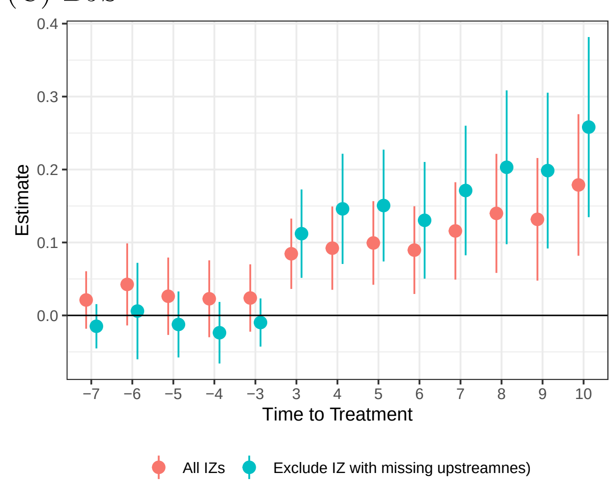


(B) Sun-Abraham





(C) BJS



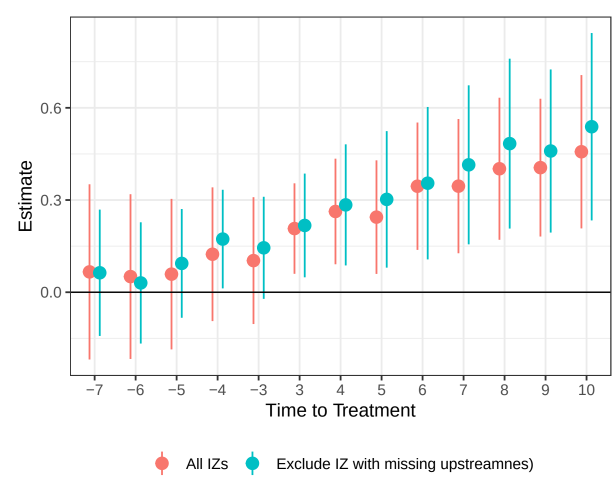
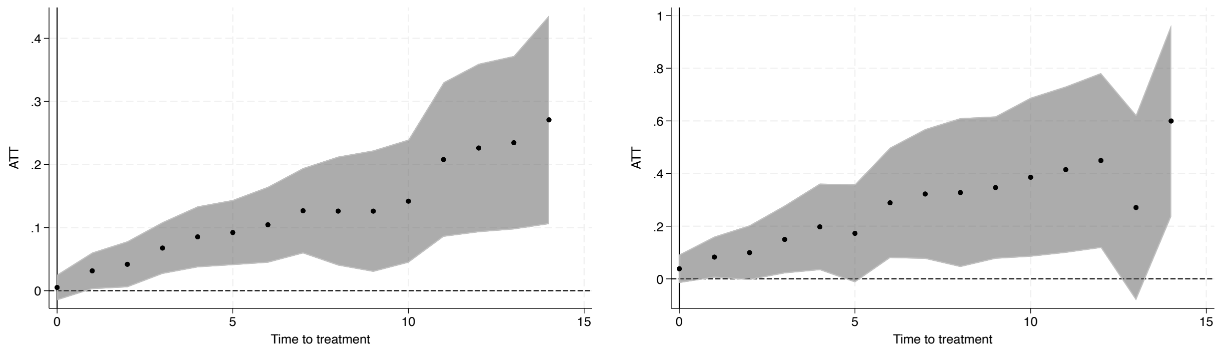


Figure 4: Effects of Industrial Zones using Synthetic DID Estimator

(i) Firm Density (ii) Worker Density



Dots represent the estimated treatment effects; shaded areas indicate 95% confidence intervals.

3.2.2 Commune-level data

Our main analyses above was based on the district-level analyses to account for potential spillover effects across communes. However, the district-level analyses also has their own limitation. First, the relatively small number of treated districts reduces statistical power. Second, the heterogeneity analyses discussed in the next section relies solely on the characteristics of the first IZ established in each district, which may introduce bias by ignoring subsequent IZ established in the same district.

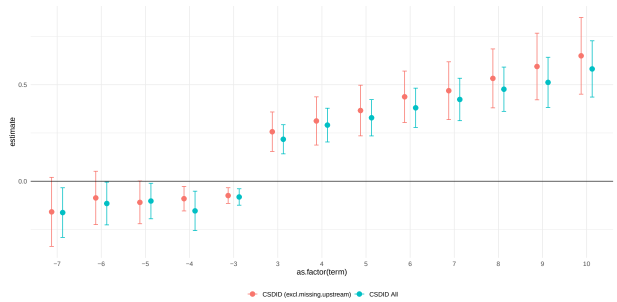
To address these limitations, we complement the district-level analysis with an alternative analysis at the commune level. Although potential spillover effects across communes remain a concern, the commune-level approach mitigates the issue of multiple IZs, as it is relatively uncommon for a single commune to host more than one industrial zone. Pre-treatment trends based on the commune-level data are illustrated in Appendix Figure 8, showing no systematic differences between treatment and control groups.

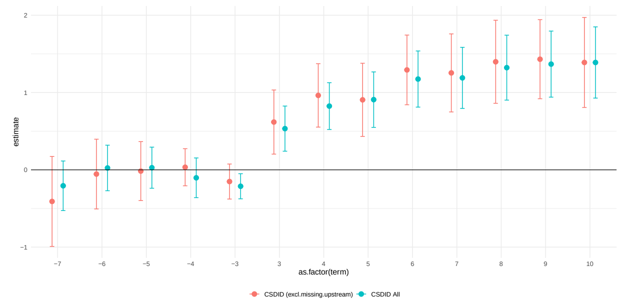
Figure 5 presents the commune-level event-study plots based on three staggered DID estimators. The results show patterns consistent with those observed in the district-level analysis, lending further support to the validity of the commune-level approach. Specifically, firm and worker densities begin to increase gradually around 3 to 5 years after IZ establishment and reach a peak between 6 to 10 years, consistent with the expected time lag due to construction and operational rollout. Importantly, no strong evidence of differential pre-trends is observed, supporting the identifying assumptions of the DID framework.

Figure 5: Event study graphs: commune-level analyses

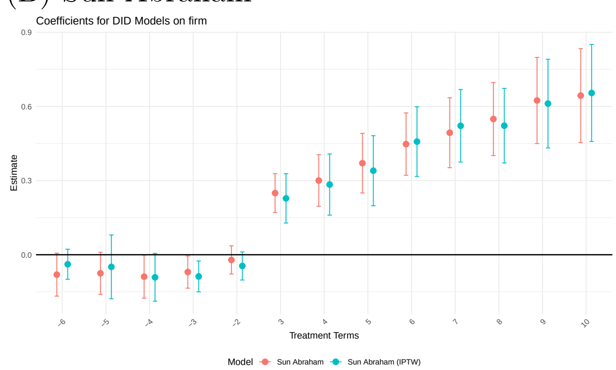
(i) Firm density (ii) Worker density

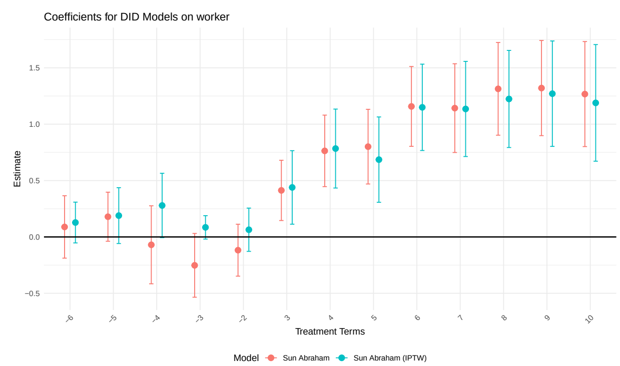
(A) Callaway-Sant'anna



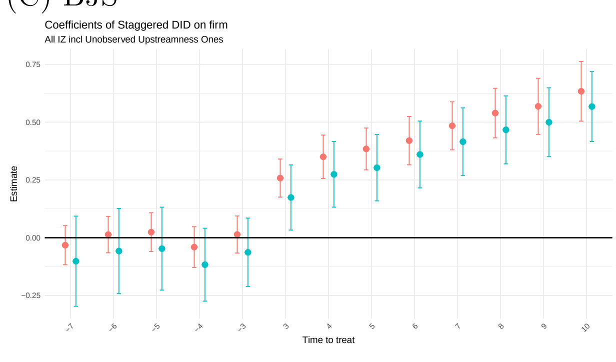


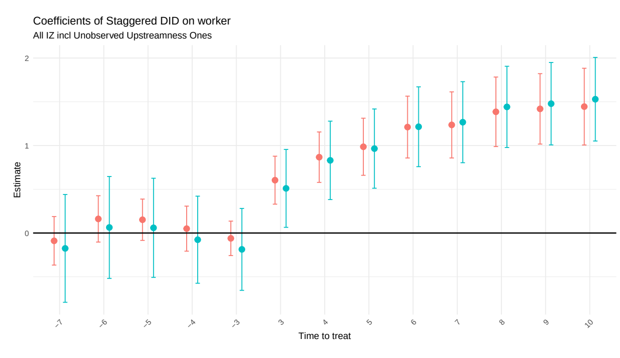
(B) Sun-Abraham





(C) BJS





4 Heterogeneity by upstreamness and downstreamness

Our analysis shows that IZs have had significant and growing effects on local economic activity over time. These effects are likely transmitted through production networks. In this section, we investigate how these impacts vary depending on the industrial composition of the zones, particularly their degree of upstreamness or downstreamness.

IZs with a greater share of upstream sectors are expected to stimulate local economic activity by supplying intermediate goods and inputs to other firms. In contrast, IZs with a stronger downstream sectors are likely to boost demand for local products and services. Consequently, the impact of IZs may depend on whether supply or demand was the primary constraint in the pre-existing local economy.

4.1 Sample splitting

To assess heterogeneity in IZ impacts by upstreamness and downstreamness, we first conduct staggered DID analyses on subgroups of treated districts defined by these characteristics. Specifically, we split treated districts into two groups: those above and below the median in terms of upstreamness (or downstreamness), and estimate the models separately for each subgroup. The control group remains the same as in the main analysis.

Figure 6 presents the results using Callaway-Sant'Anna estimator. In each panel, red dots and lines show the estimated treatment effects and their 95% confidence intervals for treated districts with above-median levels of upstreamness (Panel A) or downstreamness (Panel B). Blue dots and lines represent the corresponding estimates for districts with below-median levels of upstreamness (Panel A) or downstreamness (Panel B).

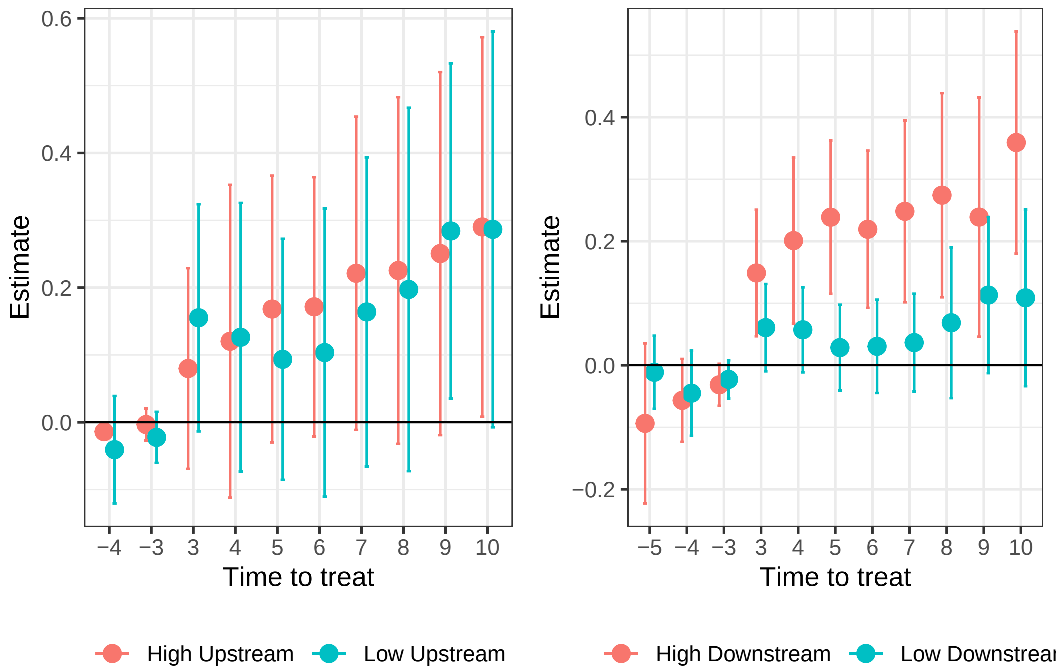
The results show no meaningful differences in estimated impacts between districts with high and low upstreamness (Panel A). However, we observe clear heterogeneity by downstreamness: in Panel B, treatment effects are statistically insignificant for districts with below-median downstreamness, whereas districts with above-median downstreamness experience significant increases in both firm density and worker density.

Figure 7 further illustrates these patterns by plotting the differences in estimated treatment effects between districts with above- and below-median levels of upstreamness and downstreamness. The differences between high and low upstreamness groups are not statistically significant, confirming the results from Panel A. In contrast, we find significant

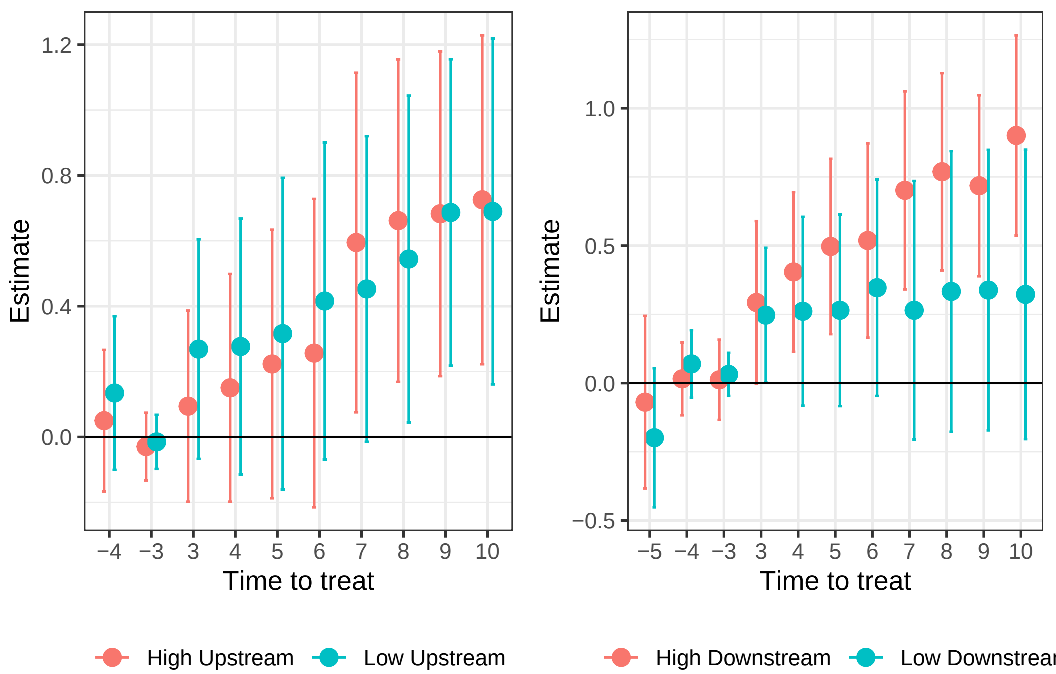
Figure 6: Heterogeneous impacts by upstreamness/downstreamness

(A) By upstream (B) By downstream

(i) Firm density



(ii) Worker density



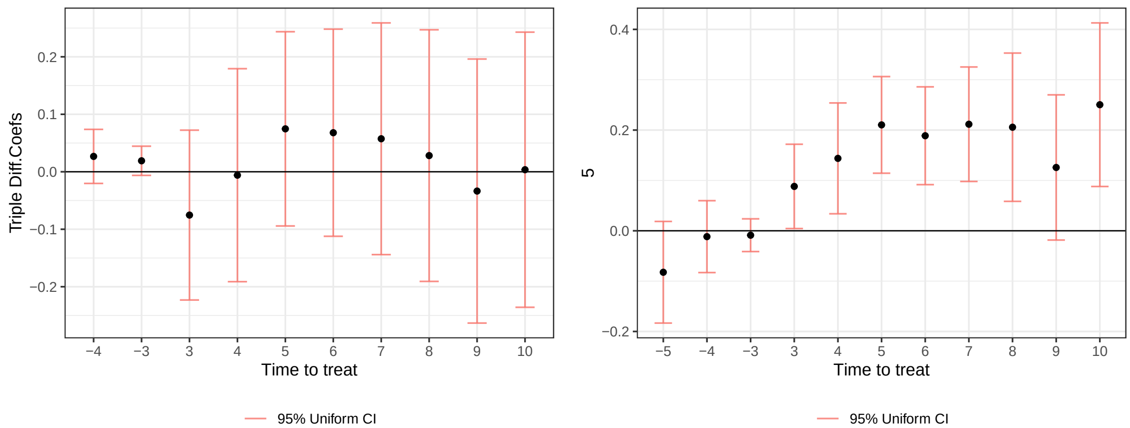
Estimation results are obtained by Callaway-Sant'Anna estimator. Covariates include log(population density 2001), log(distance to capital), and log(district area). red dots and lines show the estimated treatment effects and their 95% confidence intervals for treated districts with above-median levels of upstreamness (Panel A) or downstreamness (Panel B). Blue dots and lines represent the corresponding estimates for districts with below-median levels of upstreamness (Panel A) or downstreamness (Panel B).

differences between high and low downstreamness districts, suggesting that stronger downstream linkages play a key role in amplifying the local economic impact of IZs.

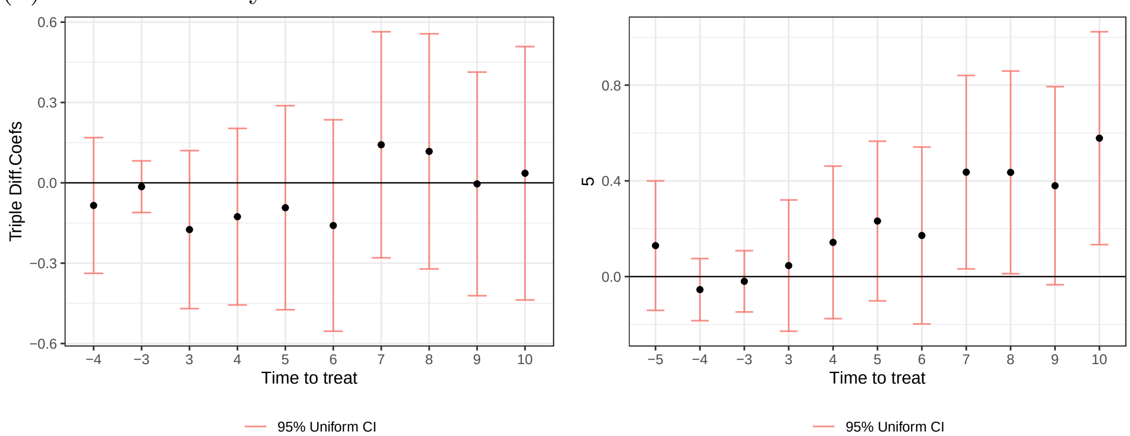
Figure 7: Difference in estimated coefficients between above- and below-median groups

(A) By upstream (B) By downstream

(i) Firm density



(ii) Worker Density



Results are obtained using the same procedures as in Figure 6.

4.2 Regression approach: Framework

The previous subsection demonstrated that the impacts of IZs vary systematically with their degree of upstreamness and downstreamness. However, these characteristics may be correlated with other attributes of the zones or districts, raising concerns about potential confounding. A standard regression approach that interacts treatment status with these characteristics is not feasible, as upstreamness and downstreamness are only defined for treated units. Additionally, categorizing IZs into multiple treatment groups based on these characteristics is not feasible, as they are continuous and multidimensional in nature.

To overcome these challenges, we extend the framework of BJS to examine how treatment effects vary with IZ characteristics, controlling for other IZ and district characteristics.

Let $Y_{it}(\infty)$ denote the potential outcome for district i in year t had no IZ ever been established, and $Y_{it}(r)$ denote the potential outcome when the first IZ is established in year r. Following BJS, we model the untreated potential outcome $Y_{it}(\infty)$ as

$$Y_{it}(\infty) = \mathbf{X}_i \boldsymbol{\beta}(\infty) + \alpha_i(\infty) + \delta_t(\infty) + \epsilon_{it}(\infty), \tag{3}$$

where $\mathbf{X}_i$ is a vector of covariates, $\alpha_i(\infty)$ and $\delta_t(\infty)$ are district and year fixed effects, respectively, and $\epsilon_{it}(\infty)$ is an idiosyncratic error term. BJS propose estimating this equation by OLS and computing the fitted untreated outcome as

$$\hat{Y}_{it}(\infty) = \mathbf{X}_i \hat{\boldsymbol{\beta}}(\infty) + \hat{\alpha}_i(\infty) + \hat{\delta}_t(\infty).$$

The estimates of the treatment effect τ_{it} is then given by

$$\hat{\tau}_{it} = Y_{it} - \hat{Y}_{it}(\infty).$$

The ATT can is computed as a weighted average, $\sum_{it:D_{it}=1} w_{it}\hat{\tau}_{it}$, where w_{it} are appropriate weights for the treated observations.

To explore heterogeneity in treatment effects, we regress the estimated treatment effect $\hat{\tau}_{it}$ on district-level covariates $\mathbf{X}_i$ and on IZ characteristics $\mathbf{W}_i$. This approach allows us to examine how the impact of IZs varies with continuous, multidimensional treatment attributes, while controlling for other observable factors. Standard errors are computed using bootstrap methods.

Since the IZ impacts tend to accumulate over time, as illustrated in the event-study plots in Figure 6, we focus on the estimated effect in the 10th year following IZ establishment. For districts where 10 years have not yet elapsed, we use the estimated effect based on the most recent available data. Districts with fewer than five years since their first IZ was established are excluded from the analysis. This restriction enhances interpretability, as the estimated coefficients reflect how IZ characteristics shape long-term treatment effects.

To clarify the assumptions underlying this procedure, suppose the potential outcome $Y_{it}(r)$ can be expressed in a form analogous to the untreated outcome model $Y(\infty)$ in equation (3), with the addition of a function capturing the influence of IZ characteristics $\mathbf{W}_i$:

$$Y_{it}(r) = \mathbf{X}_i\boldsymbol{\beta}(r) + F_r(\mathbf{W}_i) + \alpha_i(r) + \delta_t(r) + \epsilon_{it}(r). \tag{4}$$

Taking the difference between equations (4) and (3), we obtain

$$\begin{aligned}
&Y_{it}(r) - Y_{it}(\infty) \\
&= \mathbf{X}_i(\boldsymbol{\beta}(r) - \boldsymbol{\beta}(\infty)) + F_r(\mathbf{W}_{it}) + (\alpha_i(r) - \alpha_i(\infty)) + (\delta_t(r) - \delta_t(\infty)) + (\epsilon_{it}(r) - \epsilon_{it}(\infty)) \\
&= \mathbf{X}_i\tilde{\boldsymbol{\beta}}(r,\infty) + F_r(\mathbf{W}_i) + \tilde{\alpha}_i(r,\infty) + \tilde{\delta}_t(r,\infty) + \tilde{\epsilon}_{it}(r,\infty),
\end{aligned}$$

where $\tilde{\boldsymbol{\beta}}(r,\infty) \equiv \boldsymbol{\beta}(r) - \boldsymbol{\beta}(\infty)$, and similarly for $\tilde{\alpha}_i(r,\infty), \tilde{\delta}_t$, and $\tilde{\epsilon}_{it}(r,\infty)$. This equation shows that the coefficients on $\mathbf{X}_i$ reflect how the effect of the covariates differs between treated and untreated potential outcomes, while the parameters regarding $\mathbf{W}_i$ capture treatment effect heterogeneity related to IZ characteristics.

For this strategy to be valid, the error term differential, $\tilde{\epsilon}_{it}(r,\infty)$, must be uncorrelated with the IZ characteristics $\mathbf{W}_i$. This assumption would be violated if the evolution of unobservables (i.e., trends in the error term) is systematically related to $\mathbf{W}_i$. To assess the plausibility of this assumption, we examine whether pre-treatment outcome trends are correlated with IZ characteristics. Specifically, extending the BJS pre-trend test (2), we extend the BJS pre-trend test in equation (2) by regressing the outcome on IZ characteristics $\mathbf{W}_i$ and other observable covariates $\mathbf{X}_i$ as follows:

$$Y_{it} = \alpha_i + \delta_t + \sum_{s=-3}^{-7} \rho_s 1[E - t = s] + \sum_{s=-3}^{-7} 1[E - t = s]\mathbf{W}_i\boldsymbol{\theta}_s + \mathbf{X}_i\boldsymbol{\beta} + \epsilon_{it}. \tag{5}$$

The null is $H_0 : \boldsymbol{\theta}_s = 0\ \forall s$, which tests whether pre-treatment trends systematically vary with IZ characteristics. This specification extends the original BJS pre-trend test in equation (2) by incorporating interactions with continuous, multidimensional treatment characteristics.[12]

Note that several IZs either lack information on the firms established within them or do not include any firms in the non-agricultural sector. These observations were excluded

[12]Caution is warranted when including a large number of treatment characteristics, as doing so may reduce the statistical power of the pre-trend tests.

from the heterogeneity analysis. To assess whether this exclusion introduces sample selection bias, Appendix Figure 9 compares the distribution of the estimated treatment effect between included and excluded units. The figure shows that districts or communes with missing information tend to have lower estimated treatment effect on firm density, suggesting potential upward bias in the average estimated impact. However, no systematic differences are found for the estimated treatment effect on worker density. Therefore, while results related to firm density should be interpreted with caution, the findings on worker density are more robust to this selection issue.[13]

4.3 Regression approach: Results

We begin by presenting the results of the pre-trend test based on equation (5). Figure 8 plots the estimated coefficients $\boldsymbol{\theta}_s$ in a similar way to the BJS pre-trend test in Figure 6. Panel (i) displays the results for firm density, while Panel (ii) presents the results for worker density.

In each panel, the upper sub-figure corresponds to upstreamness, and the lower sub-figure to downstreamness. For firm density, the results show no significant pre-treatment trends related to either upstreamness or downstreamness, supporting the validity of our identification strategy. In contrast, for worker density, there is evidence of systematic pre-treatment differences with respect to upstreamness. Therefore, the results on heterogeneous impacts on worker density by upstreamness should be interpreted with caution.

Table 3 presents the results on impact heterogeneity by IZ upstreamness and downstreamness. All covariates are demeaned so that the intercept represents the ATT for districts with average characteristics. Columns (1)–(4) report results for firm density, while Columns (5)–(7) report results for worker density. As controls for district and IZ characteristics, we include pre-treatment firm and worker density (measured one year prior to IZ establishment), the logarithm of the distance to the capital, the logarithm of population density, the logarithm of IZ area, and the total employment of firms located in the IZ three years after its establishment.

Column (1) presents results from a linear specification. We find that IZs with a stronger

[13] The p-values from the Kolmogorov–Smirnov (K–S) test for firm density are 0.001 for district-level data and 0.038 for commune-level data, indicating significant differences. In contrast, the corresponding p-values for worker density are 0.65 and 0.79, respectively, suggesting no systematic differences.

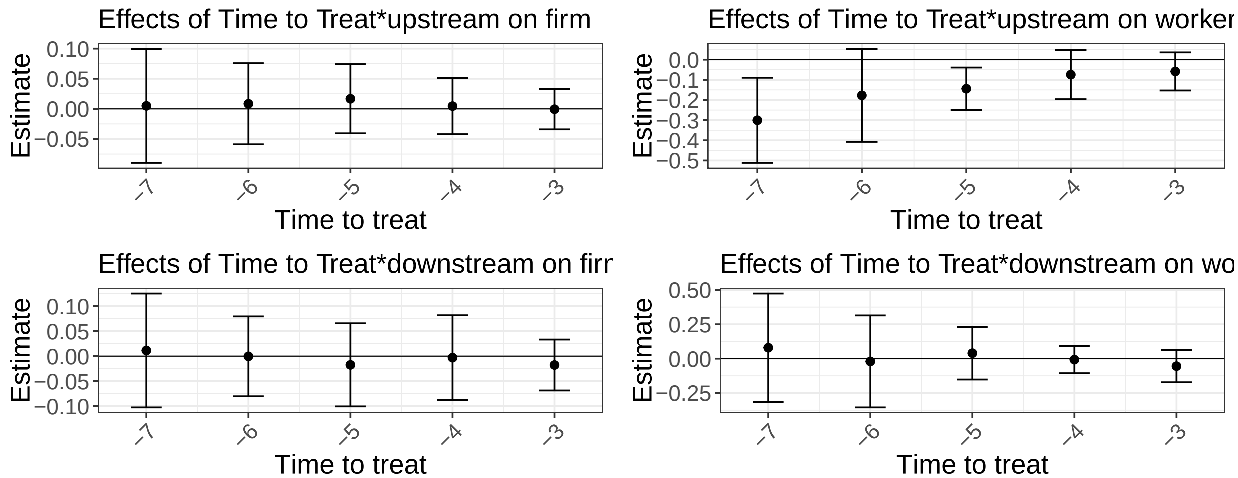
Table 3: Heterogeneous impact on firms and workers: District-level analyses

	Firm				Worker			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Intercept	0.210	0.168	0.282	0.182	0.284	0.855	1.095	0.856
	(0.568)	(0.536)	(0.086)	(0.699)	(1.420)	(1.049)	(0.319)	(1.382)
Upstream	0.002	-0.016	-0.011	-0.065	-0.045	-0.062	0.075	-0.085
	(0.038)	(0.038)	(0.068)	(0.039)	(0.095)	(0.084)	(0.177)	(0.098)
Downstream	0.138	0.192	0.223	0.308	0.236	0.341	0.238	0.336
	(0.049)	(0.057)	(0.105)	(0.056)	(0.130)	(0.139)	(0.279)	(0.151)
Upstream squared		-0.029	-0.051	-0.051		-0.457	-0.411	-0.314
		(0.041)	(0.081)	(0.049)		(0.108)	(0.261)	(0.162)
Downstream squared		0.155		0.217		-0.631	-0.730	-0.628
		(0.074)		(0.080)		(0.147)	(0.344)	(0.146)
Firms at t0-1	-0.332	-0.313		-0.120	-0.786	-0.825	-0.260	-0.786
	(0.070)	(0.067)		(0.083)	(0.188)	(0.185)	(0.174)	(0.216)
Workers at t0-1	0.083	0.063	0.055	0.044	-0.482	-0.458		-0.366
	(0.041)	(0.038)	(0.030)	(0.055)	(0.136)	(0.106)		(0.105)
Dist to Capital(log)	-0.064	-0.084		-0.023	-0.381	-0.354		-0.219
	(0.037)	(0.043)		(0.039)	(0.122)	(0.127)		(0.113)
Pop Density(log)	0.191	0.188		0.098	1.233	1.249		1.163
	(0.083)	(0.076)		(0.108)	(0.236)	(0.166)		(0.225)
IZ Area(log)	0.037	0.052	0.044	0.112	-0.019	-0.047	-0.164	-0.010
	(0.032)	(0.030)	(0.055)	(0.039)	(0.078)	(0.070)	(0.136)	(0.071)
Employment IZ (log)	-0.023	-0.011	-0.013	-0.017	0.027	-0.023	0.015	-0.037
	(0.017)	(0.016)	(0.041)	(0.021)	(0.050)	(0.049)	(0.125)	(0.062)
Num.Obs.	46	46	46	45	46	46	46	45
Weighted	N	N	N	Y	N	N	N	Y

Bootstrap standard errors are reported in parentheses. Columns (1) and (5) report results from regressions including linear terms for upstreamness and downstreamness. Columns (2) and (6) extend these models by adding quadratic terms for both variables. Columns (3) and (7) incorporate covariates selected using the post-double-selection (PDS) LASSO method. Columns (4) and (8) apply weights based on the average export-to-output ratio of industries within each IZ.

Figure 8: Pre-treatment correlation: District-level analyses

(i) Firm density (B) Worker density



The figures show the estimated coefficients $\hat{\boldsymbol{\theta}}_s$ and their confidence intervals in equation (5).

downstream orientation generated significantly larger impacts on firm density, even after controlling for district- and zone-level characteristics. A one standard deviation increase in downstreamness (approximately 0.7) is associated with an increase in the IZ effect of about 0.1. Given that the estimated ATT in the 10th year after IZ establishment is approximately 0.2 (see Figure 3), this represents a sizable effect.

The coefficients on the control variables suggest that IZ impacts were larger in districts that are closer to the provincial capital and have higher population density, highlighting the importance of geographic factors in shaping the effectiveness of place-based industrial policies.

In Column (2), we incorporate second-order polynomial terms for both upstreamness and downstreamness. Both the linear and quadratic terms for downstreamness are positive and statistically significant, indicating a convex relationship: the IZ impact is particularly strong for highly downstream-oriented zones. In contrast, we find no evidence of heterogeneity in IZ impacts with respect to upstreamness.

One potential concern is that the analysis relies on a relatively small sample, using only one observation per treated district. With limited degrees of freedom, including many covariates could reduce statistical power. To address this issue, Column (3) presents results using the post-double-selection (PDS) LASSO (Belloni et al., 2014), which selects a parsimonious set of covariates based on their predictive power. The key findings regarding downstreamness remain robust under this specification.

Column (4) shows results where upstreamness and downstreamness are further weighted by industry exportability, defined as $\frac{\text{export}_s}{\text{export}_s+\text{import}_s}$ based on the I-O table. This adjustment aims to reflect the notion that the entry of industries with a comparative advantage, i.e., those more oriented toward exports, has a stronger impact. Consistent with this hypothesis, the coefficient on downstreamness increases under this specification.[14]

Similar results for worker density are shown in Columns (5)–(8). In the linear specification (Column 5), greater downstreamness is associated with stronger IZ effects, significant at the 10% level, and upstreamness also shows significantly positive effect at the 5% level, unlike the result obtained from the simple sample splitting. These results underscore the importance of controlling for other IZ and district characteristics when evaluating heterogeneous impacts. Relying on simple subgroup comparisons based on upstreamness or downstreamness alone may yield misleading conclusions.

When quadratic terms are added (Column 6), the coefficient on downstreamness becomes statistically significant at the 5% level. However, the negative sign on the squared term suggests an inverse U-shaped relationship, which is best visualized through graphical illustration.

Figure 9 visualizes the heterogeneity in estimated impacts with respect to upstreamness and downstreamness. Following the Frisch–Waugh–Lovell theorem, it plots the partial residuals of the estimated treatment effects against the partial residuals of downstreamness (Panel A) and upstreamness (Panel B). Each dot represents an observation. The blue lines show the fitted regression lines, and the shaded areas represent their 95% confidence intervals.

The figure confirms that IZs with a stronger downstream orientation tend to generate larger impacts on firm density, and this result is not driven by some outliers. This suggesting that demand-side linkages play a key role in catalyzing local firm entry.

Table 4 presents the regression results based on commune-level data. Column (1), which reports estimates from a linear specification for firm density, confirms that downstreamness plays an important role in shaping the impact of IZs. It also suggests a significant role for upstreamness. In Column (2), where we include second-order polynomial terms for both upstreamness and downstreamness, the coefficient on upstreamness decreases slightly and

[14] Because many service sectors are non-tradable, some IZs composed solely of service industries were excluded from this analysis.

its quadratic term is negative, indicating diminishing returns. In contrast, the coefficient on downstreamness increases, and its quadratic term is positive and statistically significant. These results suggest that downstreamness is more strongly associated with the magnitude of IZ impacts, particularly in the upper range of the distribution. A graphical illustration similar to Figure 9 are provided in Appendix Figure 10.

Table 4: Heterogeneous treatment impacts: Commune-level analyses

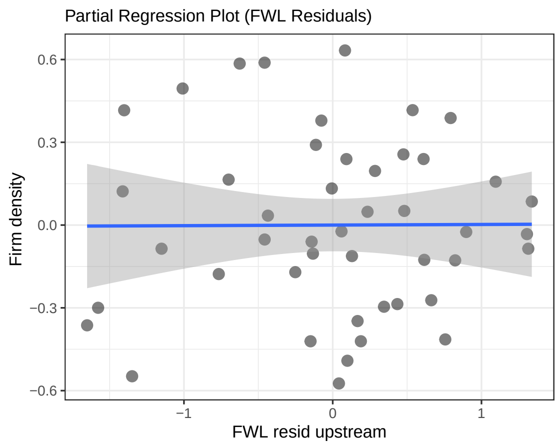
	Firm				Worker			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Intercept	0.661	0.635	0.793	0.616	1.390	1.812	1.683	1.812
	(0.633)	(0.606)	(0.129)	(0.622)	(1.541)	(1.646)	(0.274)	(1.742)
Upstream	0.114	0.091	0.079	0.119	0.259	0.185	0.146	0.214
	(0.046)	(0.058)	(0.095)	(0.059)	(0.133)	(0.143)	(0.189)	(0.153)
Downstream	0.165	0.258	0.297	0.210	0.300	0.459	0.543	0.385
	(0.085)	(0.084)	(0.158)	(0.102)	(0.199)	(0.220)	(0.354)	(0.251)
Upstream squared		-0.070	-0.155	-0.046		-0.406	-0.393	-0.334
		(0.074)	(0.123)	(0.082)		(0.175)	(0.245)	(0.212)
Downstream squared		0.207		0.226		-0.323		-0.304
		(0.108)		(0.107)		(0.272)		(0.314)
Firms at t0-1	-0.269	-0.205		-0.224	-0.592	-0.592	-0.511	-0.550
	(0.087)	(0.077)		(0.083)	(0.225)	(0.241)	(0.374)	(0.301)
Workers at t0-1	0.002	-0.007		0.008	-0.664	-0.640	-0.665	-0.624
	(0.037)	(0.034)		(0.033)	(0.089)	(0.089)	(0.133)	(0.097)
Dist to Capital(log)	-0.001	-0.011		0.078	-0.057	-0.052		0.215
	(0.089)	(0.080)		(0.083)	(0.168)	(0.198)		(0.216)
Pop Density(log)	0.426	0.378	0.220	0.417	1.411	1.417	1.373	1.478
	(0.072)	(0.087)	(0.095)	(0.075)	(0.208)	(0.216)	(0.346)	(0.272)
IZ Area(log)	0.100	0.103		0.094	0.113	0.097		0.092
	(0.046)	(0.045)		(0.051)	(0.124)	(0.111)		(0.165)
Employment IZ(log)	-0.003	0.010	-0.013	0.028	0.065	0.026	0.050	0.074
	(0.027)	(0.029)	(0.043)	(0.030)	(0.065)	(0.064)	(0.097)	(0.074)
Num.Obs.	70	70	70	69	70	70	70	69
Weighted	N	N	N	Y	N	N	N	Y

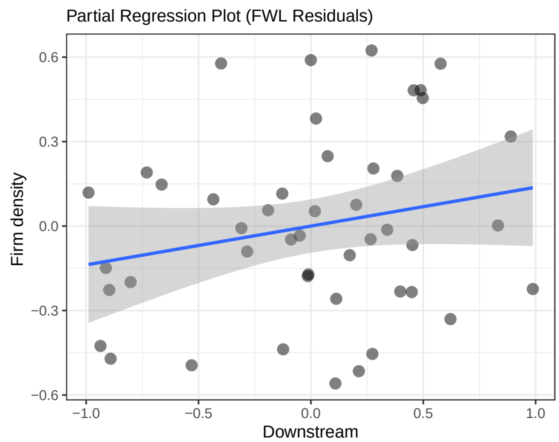
Bootstrap standard errors are reported in parentheses. Columns (1) and (5) report results from regressions including linear terms for upstreamness and downstreamness. Columns (2) and (6) extend these models by adding quadratic terms for both variables. Columns (3) and (7) incorporate covariates selected using the post-double-selection (PDS) LASSO method. Columns (4) and (8) apply weights based on the average export-to-output ratio of industries within each IZ.

Figure 9: Estimated treatment effects and upstreamness/downstreamness: District-level analyses

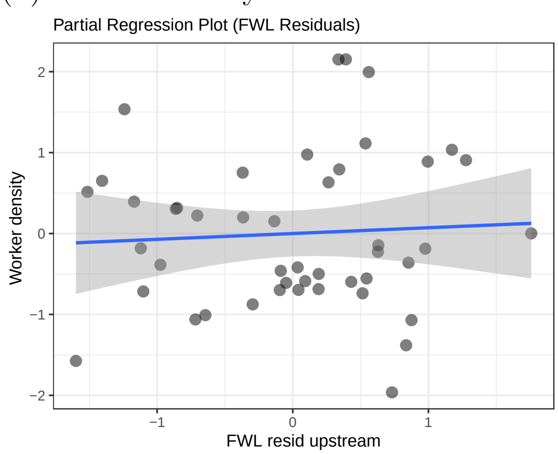
(A) Upstream (B) Downstream

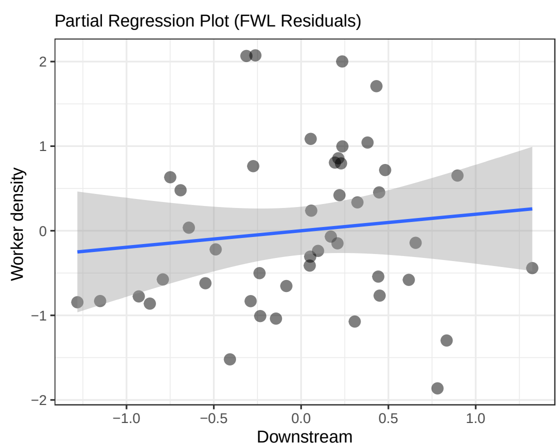
(i) Firm density





(ii) Worker density





Figures plot the partial residuals of the estimated treatment effects against the partial residuals of downstreamness (Panel A) and upstreamness (Panel B), based on the Frisch–Waugh–Lovell theorem. Each dot represents a district-level observation. Blue lines indicate fitted regression lines; shaded areas show 95% confidence intervals.

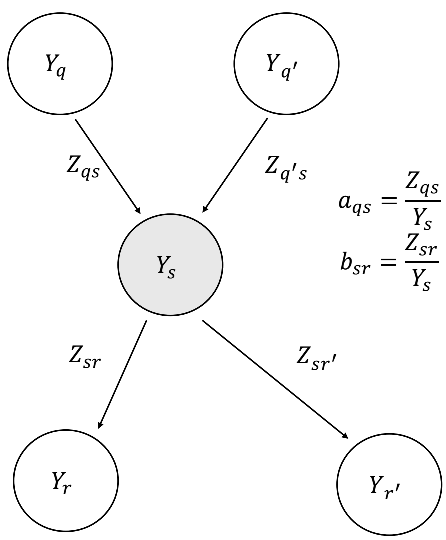
5 Production linkages to firms in IZs

We have shown that downstreamness plays a key role in determining the impact of IZs: zones with a higher concentration of downstream sectors tend to generate greater effects, likely through backward linkages—that is, by creating demand for upstream sectors.

In this section, we examine whether such production linkages actually mediate the observed impacts. To do so, we construct outcome measures that are weighted by the strength of backward and forward linkages with firms operating in IZs. We then compare the evolution of these weighted outcomes to their unweighted counterparts to assess the contribution of production networks.

Specifically, we construct outcome variables that account for how strongly each sector is connected to firms within IZs, using the I-O table to measure linkage intensities. Consider a firm k located in an IZs in district i and operating in sector s. The I-O table tells us that sector s producing output Y_s will generates demand for inputs from its upstream sector q by an amount $Z_q s$ (backward linkage), and supplies inputs to its downstream sector r by $Z_s r$ (forward linkage), as illustrated in Figure 10.

Figure 10: Backward and forward linkage weight



Let $a_{qs} = Z_{qs}/Y_s$ denote the intensity of the backward linkage from sector s to sector q. The contribution of firm k to the backward linkage in sector q is measured by $w_{kis}a_{qs}$, where w_{kis} is the firm-specific weight defined in equation 2.4. Using these weights, we compute the backward-linkage-weighted outcome as

$$BLY_{it} = \frac{\sum_{j \in N(i)} \sum_{k \in M(i)} \sum_{q=1}^{S} w_{kis} a_{qs} y_{jiqt}}{\sum_{k \in M(i)} \sum_{q=1}^{S} w_{kis} a_{qs}}$$

where $N(i)$ is the set of all firms in district i, $M(i)$ is the set of firms located in IZs within district i, and y_{jiqt} denotes the outcome of interest (i.e. an indicator for firm operation or the number of workers) for sector q in district j at time t.

Similarly, the forward-linkage-weighted outcome is computed as

$$ULY_{it} = \frac{\sum_{j \in N(i)} \sum_{k \in M(i)} \sum_{r=1}^{S} w_{kis} b_{sr} y_{jirt}}{\sum_{k \in M(i)} \sum_{r=1}^{S} w_{kis} b_{sr}}$$

By comparing the trajectories of these weighted outcomes to their unweighted counterparts, we can assess whether the strength of production linkages moderates the effects of IZ establishment. While the previous analyses examined heterogeneity based on the overall upstreamness and downstreamness of the IZ, this exercise shifts the focus to the degree of production linkage between local firms and those established within the IZ. This distinction allows us to test whether stronger backward or forward ties to IZ firms lead to greater spillover effects on the surrounding economy.

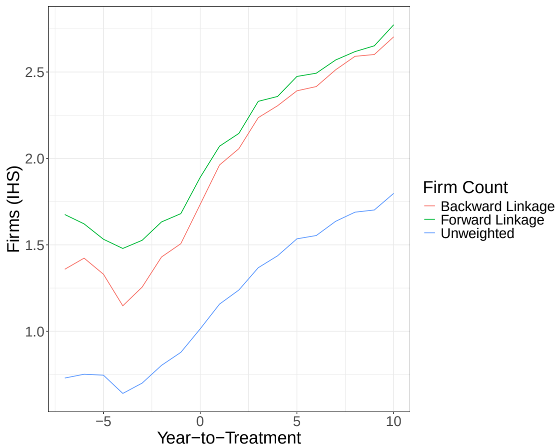
Figure 11 plots the evolution of both weighted and unweighted outcomes for firm density (Panel A) and worker density (Panel B). The plots show that linkage-weighted firm outcomes consistently exceed the unweighted ones, suggesting that production linkages played an important role in shaping local firm dynamics. Notably, even before IZ establishment, sectors with stronger connections to IZ firms were already more prevalent, reflecting broader structural patterns in the national economy.

The figure also shows that the backward-linkage-weighted firm outcomes rise markedly following IZ establishment, indicating that IZs created localized demand that spurred the entry or expansion of upstream suppliers. In contrast, no distinct pattern is observed for worker density, consistent with our earlier finding that IZs had limited effects on employment at the intensive margin.

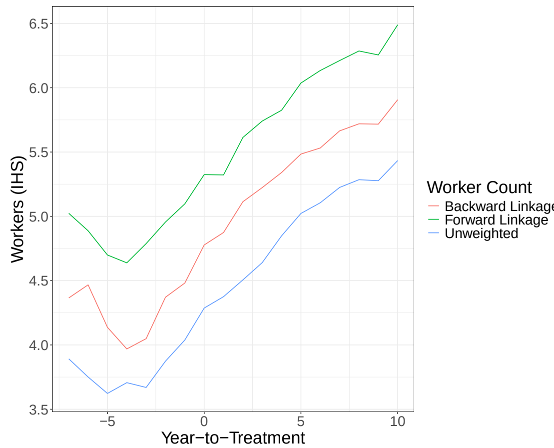
Note that these analyses do not identify the causal impacts of linkages to firms in IZs; rather, they illustrate empirical patterns in the evolution of outcomes when greater weight is placed on firms with such linkages. Nonetheless, the findings provide suggestive evidence

Figure 11: Evolution of linkage weighted / unweighted outcomes (Firms and Workers)

(i) Firm Density



(B) Worker density



that production linkages—particularly backward linkages to the local economy—may play an important role and should be taken into account in the design of place-based industrial policies (PBIPs).

6 Concluding remarks

This paper examines how the characteristics of place-based industrial policies—specifically, the sectoral orientation of industrial zones (IZs)—influence their local economic impacts. By combining firm-level panel data with a comprehensive IZ database, and constructing novel measures of IZ-level upstreamness and downstreamness based on input–output linkages, we assess the heterogeneous effects of IZs on firm entry and employment across both space and time.

Our findings reveal substantial variation in IZ impacts depending on their sectoral orientation. IZs with a stronger downstream focus—those that generate demand for intermediate goods and services from the local economy—consistently lead to larger increases in firm density, even after controlling for a range of district- and zone-level characteristics. In contrast, we find little evidence that upstream orientation enhances IZ effectiveness, nor do we observe significant impacts on worker density. These patterns suggest that employment responses may be weaker or operate through different mechanisms.

To further explore the role of production linkages, we construct outcome measures weighted by the strength of backward and forward linkages between local firms and those established within IZs. The results confirm that firms more strongly connected to IZs—particularly as upstream suppliers—experience greater gains following IZ establishment. This pattern indicates that downstream-oriented IZs may stimulate local firm growth by creating demand for intermediate inputs, thereby catalyzing production spillovers through backward linkages. However, we find no comparable effects on the intensive margin, reinforcing the conclusion that IZs primarily influence firm entry rather than employment expansion within existing firms.

Taken together, these findings offer new insights into the design of place-based industrial policy in developing countries. They highlight that not all IZs yield equal economic returns, and that targeting downstream sectors—especially those capable of inducing strong backward linkages—can generate greater local benefits. More broadly, the study underscores the importance of incorporating production network structures into the evaluation and design of spatial development policies. While this study does not assess the aggregate impact of IZs or determine the optimal sectoral composition under market distortions, it demonstrates that understanding where and how linkages operate can guide more effective approaches to promoting structural transformation and inclusive growth.

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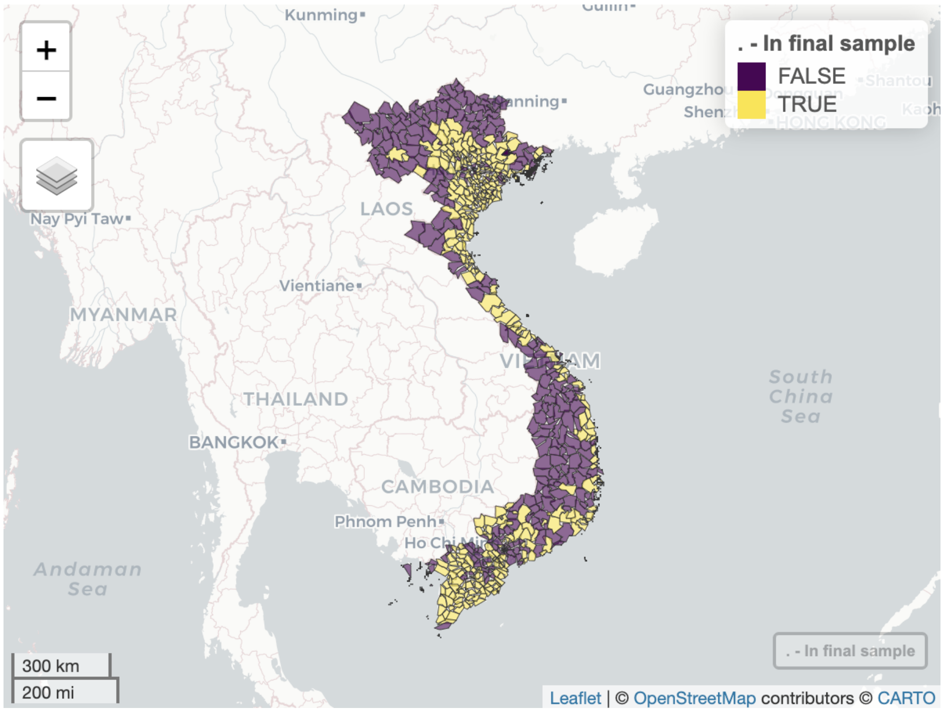
A Appendix Figures

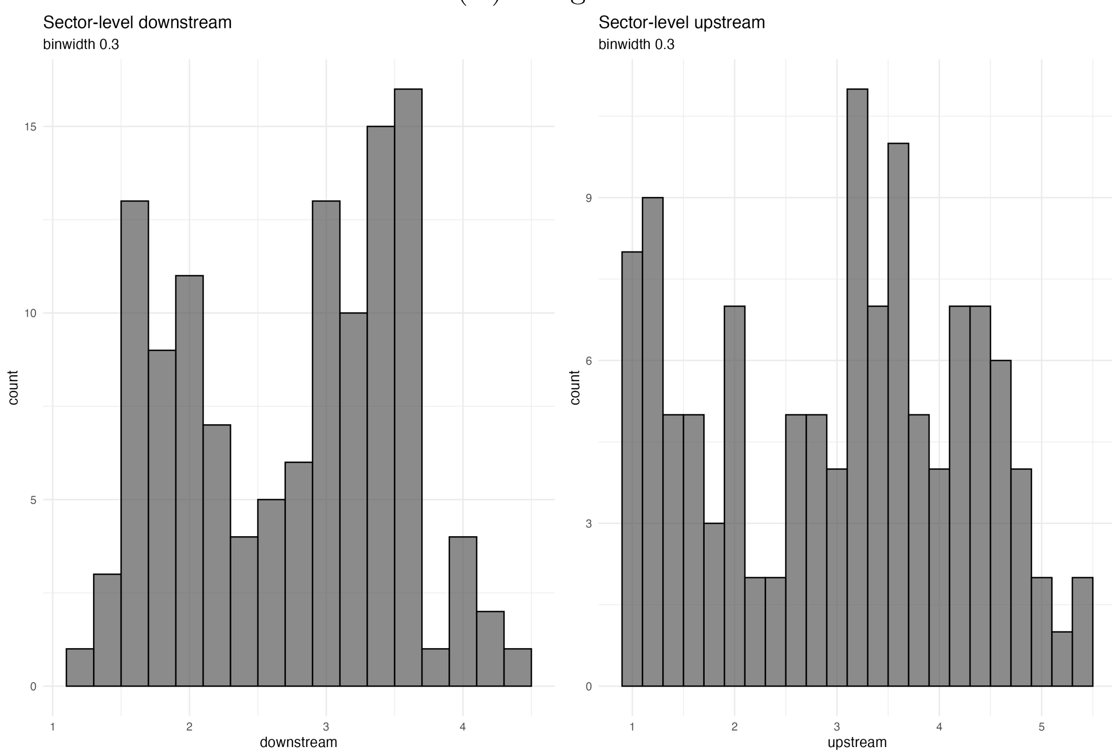
Appendix Figure 1: Location of districts used for the analyses



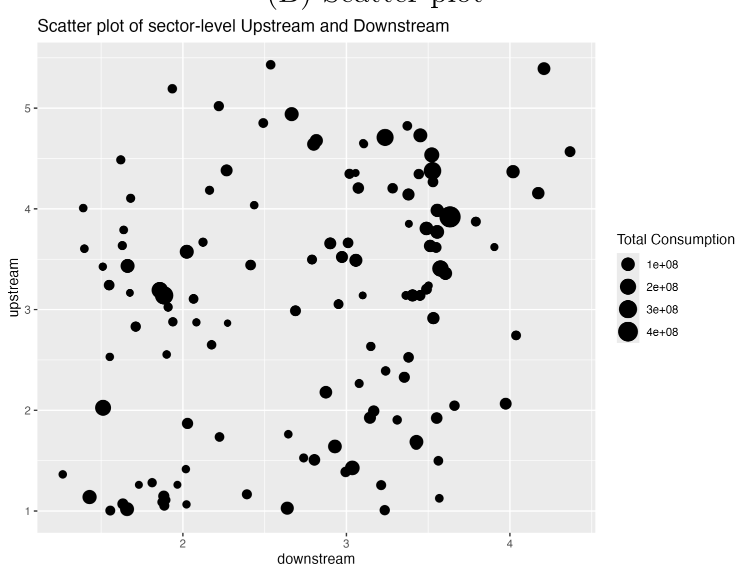
Note: Yellow-colored areas indicate the districts used for the analyses, which satisfy (1) population density in 2001 no less than 100 per km^2, (2) distance to the provincial capital exceeding 1 km, and (3) distance to the provincial capital less than 100 km.

Appendix Figure 2: Sector-level upstreamness and downstreamness

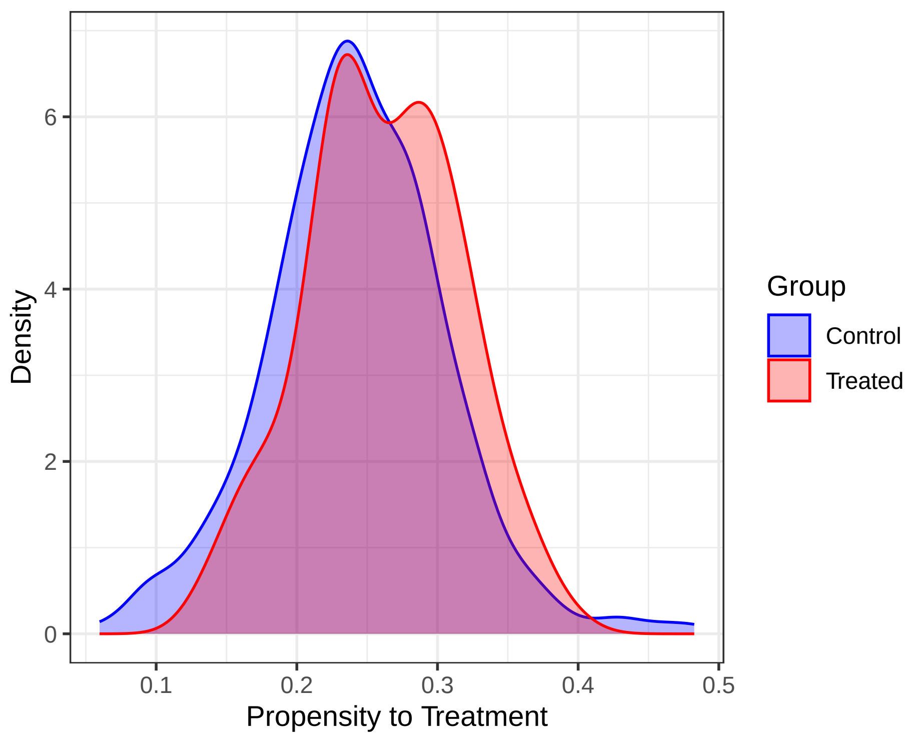
(A) Histgorams



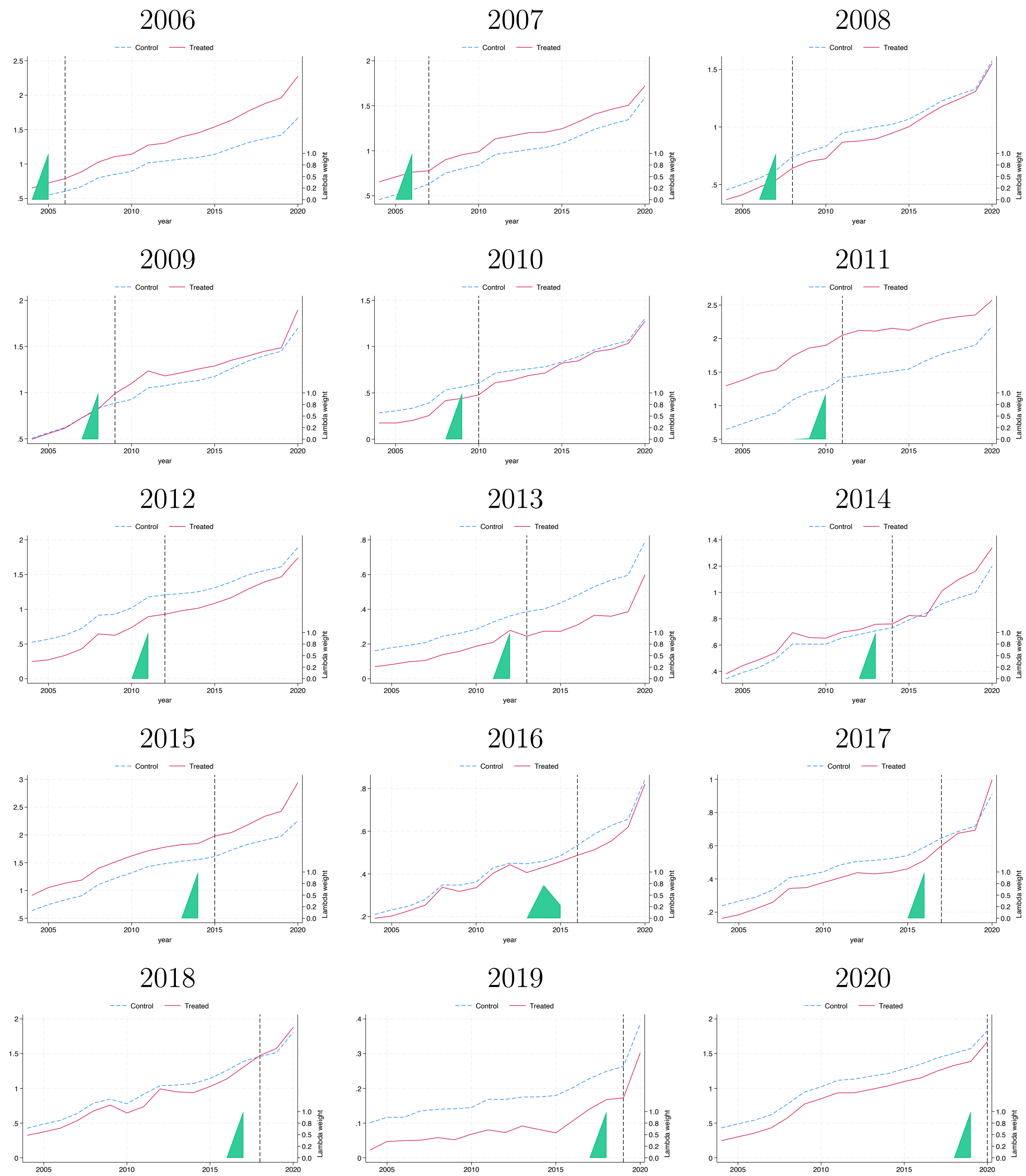
(B) Scatter plot



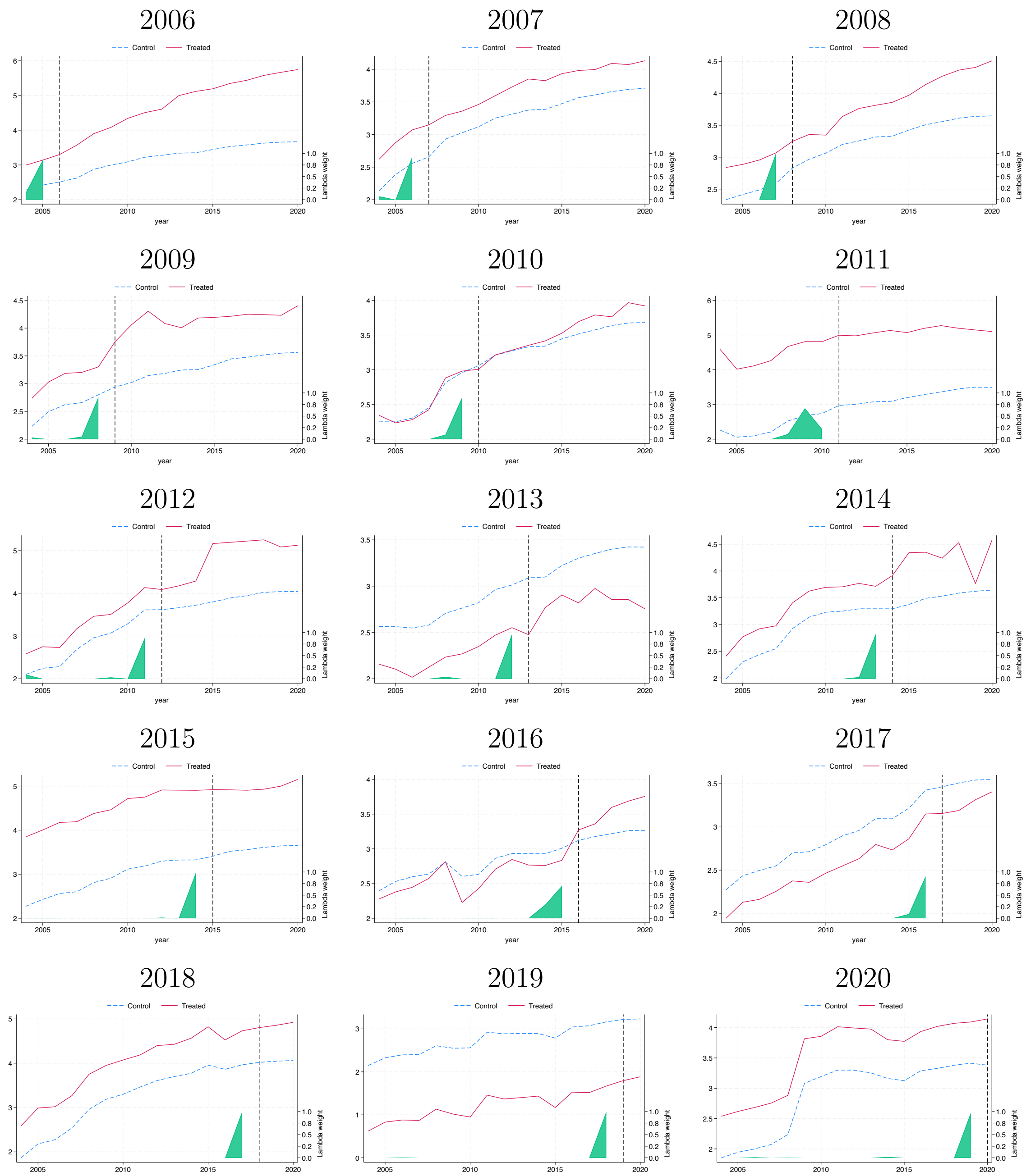
Appendix Figure 3: Common Support Plot



Appendix Figure 4: Detailed results of synthetic DID: Firm density (district level)

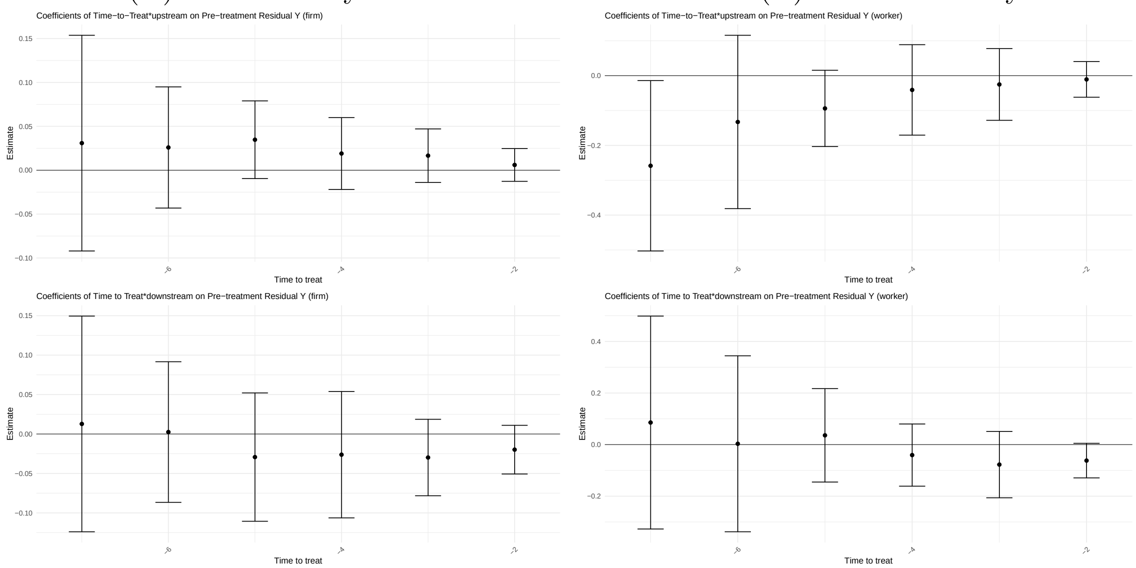


Appendix Figure 5: Detailed results of synthetic DID: Worker density (district level)

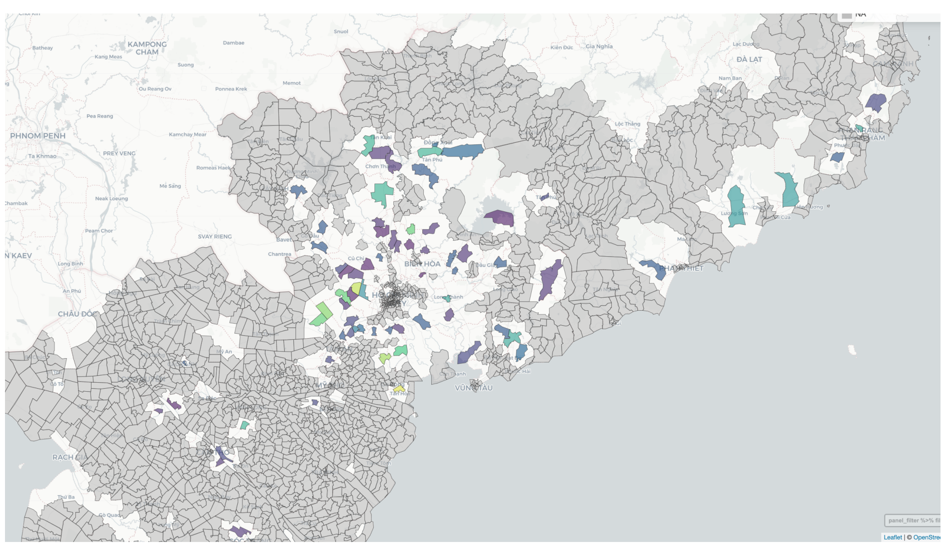


Appendix Figure 6: BJS pre-trend tests

(A) Firm density (B) Worker density

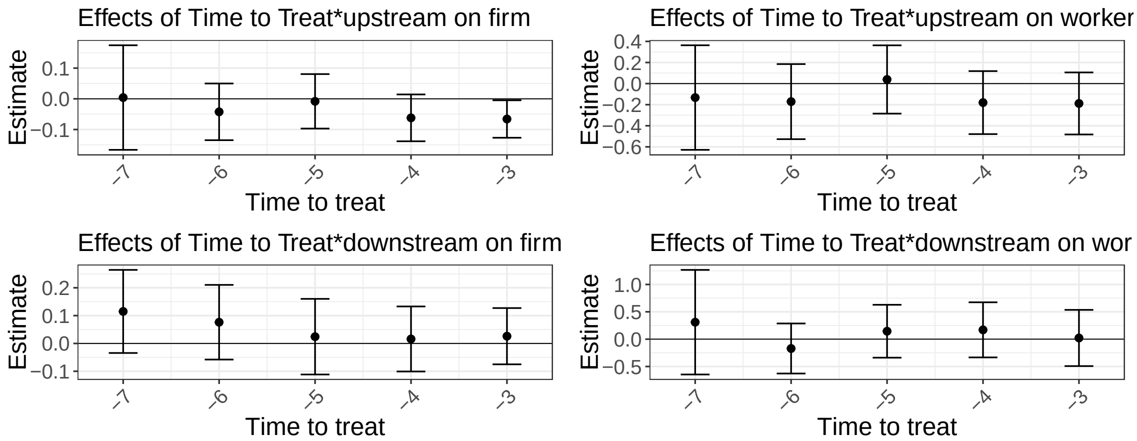


Appendix Figure 7: Control group map



The map shows control communes (gray) and treated communes (colored). The communes that are adjacent to treated ones are removed from the all commune-level analysis.

Appendix Figure 8: Pre-trend: commune level

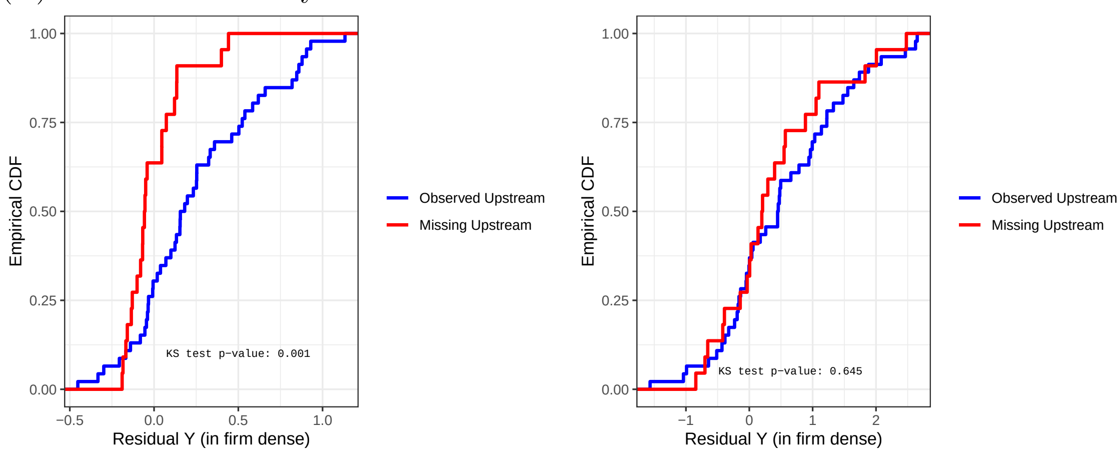


Appendix Figure 9: Distribution of the estimated ATT for districts/communes with complete upstream/downstream data and those with missing data

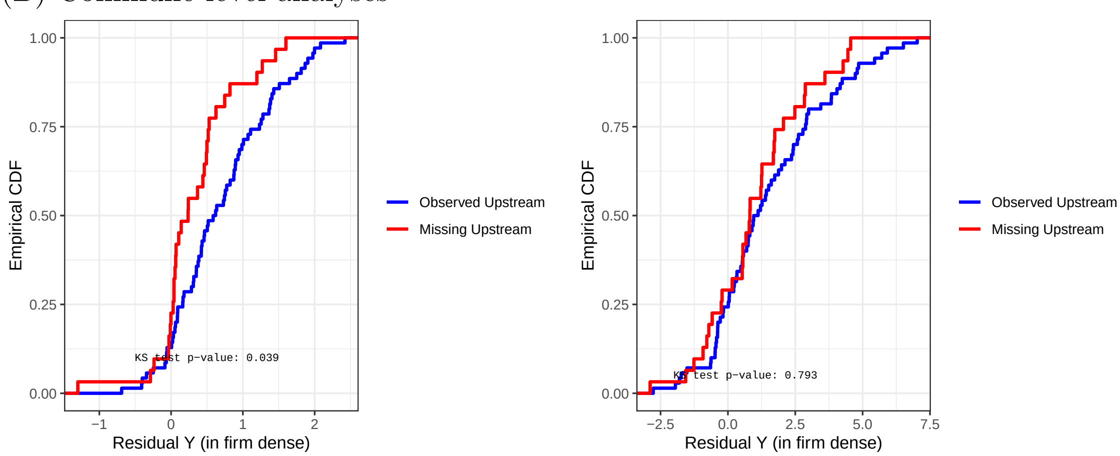
(i) Firm density

(ii) Worker density

(A) District-level analyses



(B) Commune-level analyses

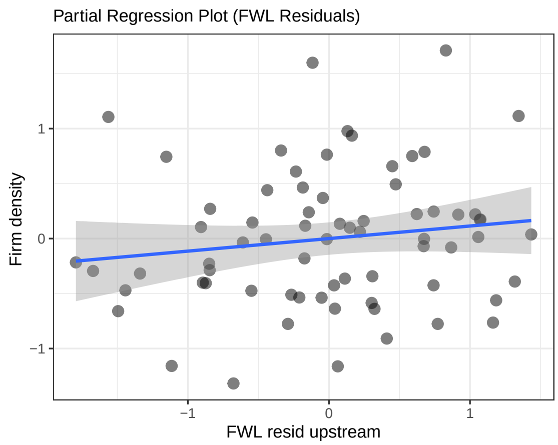


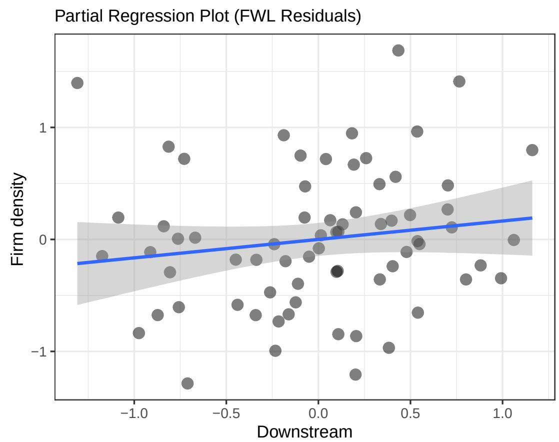
Appendix Figure 10: Estimated treatment effects and downstreamness/upstreamness: Commune-level analyses

(A) Upstream

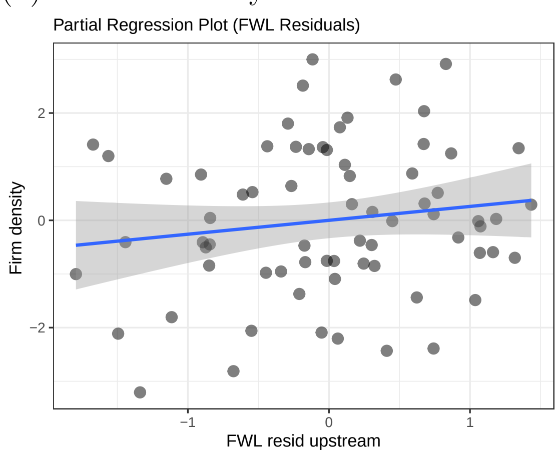
(B) Downstream

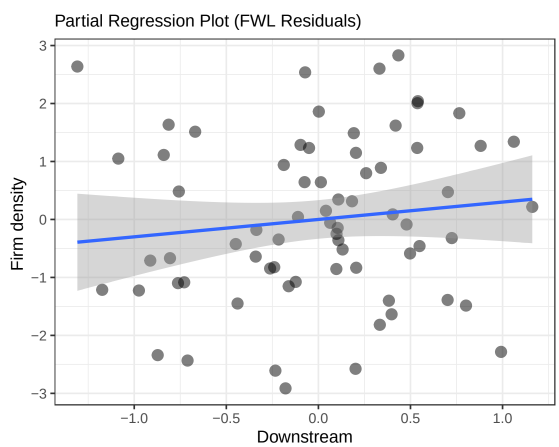
(i) Firm Density





(ii) Worker Density





Figures plot the partial residuals of the estimated treatment effects against the partial residuals of downstreamness (Panel A) and upstreamness (Panel B), based on the Frisch–Waugh–Lovell theorem. Each dot represents a district-level observation. Blue lines indicate fitted regression lines; shaded areas show 95% confidence intervals.

B Appendix Tables

B.1 Number of Communes appeared in the VES

Appendix Table 1: Number of Commune presence in VES data per year

Year	Commune	Percent	Cum.
2000	62	0.04	0.04
2001	63	0.04	0.08
2002	71	0.05	0.13
2003	70	0.05	0.17
2004	6828	4.40	4.57
2005	7602	4.89	9.46
2006	7847	5.05	14.51
2007	8281	5.33	19.84
2008	9268	5.97	25.81
2009	9009	5.80	31.61
2010	9053	5.83	37.44
2011	9438	6.08	43.52
2012	9475	6.10	49.62
2013	9501	6.12	55.73
2014	9550	6.15	61.88
2015	9597	6.18	68.06
2016	9745	6.27	74.33
2017	9850	6.34	80.68
2018	9983	6.43	87.10
2019	9856	6.35	93.45
2020	10 177	6.55	100.00
Total	**155326**	**100.00**	**100.00**

Appendix Table 2: Top 20 upstream and downstream industries

(i) Top 20 Upstream Industries

sector	upstream	Export share	Non agriculture	Rank
Metal ores	5.43	0.37	Yes	1
Special purpose machinary	5.39	0.11	Yes	2
Support services to mining	5.19	0.00	Yes	3
Other mining and quarying	5.02	0.22	Yes	4
Crude petroleum	4.94	0.85	Yes	5
Natural rubber	4.87	0.90	No	6
Other petroleum products	4.85	0.07	Yes	7
Coke oven products	4.82	0.53	Yes	8
Agricultural services	4.75	0.00	No	9
Plastic and synthetic rubber	4.73	0.10	Yes	10
Products of iron and	4.71	0.12	Yes	11
Basic chemicals	4.68	0.15	Yes	12
Roads, construction works for	4.65	0.00	Yes	13
Construction works for utility	4.64	0.00	Yes	14
Fertilizers and nitrogen products	4.64	0.25	Yes	15
Batteries and accumulators	4.57	0.47	Yes	16
Computer, electronic products	4.54	0.39	Yes	17
Other financial services	4.49	0.00	Yes	18
Warehousing and support services	4.38	0.00	Yes	19
Fabricated metal products, except	4.38	0.37	Yes	20

(ii) Top 20 Downstream Industries

sector	downstream	Export share	Non-Agriculture	Rank
Vegetables and animal oils	5.55	0.09	No	1
Batteries and accumulators	4.37	0.47	Yes	2
Special purpose machinary	4.21	0.11	Yes	3
Wiring and wiring devices	4.17	0.41	Yes	4
Electric lighting equipment	4.04	0.40	Yes	5
Colour and precious metals,	4.02	0.16	Yes	6
Processed and preserved meat	4.00	0.77	No	7
Products of poultry	3.97	0.48	No	8
Motorcycles	3.97	0.92	Yes	9
Dairy products	3.97	0.19	No	10
Products of pigs	3.95	0.87	No	11
Sewerage services	3.90	0.00	Yes	12
Processed and preserved fish,	3.81	0.92	No	13
Electric motors, generators	3.79	0.55	Yes	14
Trailers and semi-trailers	3.66	0.41	Yes	15
Refined petroleum products	3.63	0.00	Yes	16
Wood, products of wood	3.61	0.81	Yes	17
Textiles	3.58	0.33	Yes	18
Jewellery, bijouterie and related	3.57	0.70	Yes	19
Domestic appliances	3.56	0.30	Yes	20

Appendix Table 3: Top 20 upstream and downstream industries (excluding agriculture-related sectors

Top 20 Upstream Industries

sector	upstream	Export share	RankUpstream
Metal ores	5.43	0.37	1
Special purpose machinary	5.39	0.11	2
Support services to mining	5.19	0.00	3
Other mining and quarying	5.02	0.22	4
Crude petroleum	4.94	0.85	5
Other petroleum products	4.85	0.07	6
Coke oven products	4.82	0.53	7
Plastic and synthetic rubber	4.73	0.10	8
Products of iron and	4.71	0.12	9
Basic chemicals	4.68	0.15	10
Roads, construction works for	4.65	0.00	11
Construction works for utility	4.64	0.00	12
Fertilizers and nitrogen products	4.64	0.25	13
Batteries and accumulators	4.57	0.47	14
Computer, electronic products	4.54	0.39	15
Other financial services	4.49	0.00	16
Warehousing and support services	4.38	0.00	17
Fabricated metal products, except	4.38	0.37	18
Colour and precious metals,	4.37	0.16	19
Railways, construction works for	4.36	0.00	20

Top 20 Downstream Industries

sector	downstream	Export share	Rank Downstream
Batteries and accumulators	4.37	0.47	1
Special purpose machinary	4.21	0.11	2
Wiring and wiring devices	4.17	0.41	3
Electric lighting equipment	4.04	0.40	4
Colour and precious metals,	4.02	0.16	5
Motorcycles	3.97	0.92	6
Sewerage services	3.90	0.00	7
Electric motors, generators,	3.79	0.55	8
Trailers and semi-trailers	3.66	0.41	9
Refined petroleum products	3.63	0.00	10
Wood, products of wood	3.61	0.81	11
Textiles	3.58	0.33	12
Jewellery, bijouterie and related	3.57	0.70	13
Domestic appliances	3.56	0.30	14
Plastic products	3.56	0.73	15
Other chemical products	3.56	0.23	16
Other electrical equipment	3.55	0.15	17
Specialized construction works	3.55	0.00	18
Other textiles	3.53	0.23	19
Measuring, testing and navigating	3.53	0.21	20