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Ridwan Ah Sheikh
Email: ridwan@econdse.org
Department of Economics,
Delhi School of Economic
University of Delhi

Sunil Kanwar
Email: sunil_kanwar@econdse.org
Department of Economics,
Delhi School of Economic
University of Delhi

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Delhi School of Economics
Delhi- 110007

Revisiting the Impact of TRIPS on IPR-intensive Export Flows: Evidence from Staggered Difference-in-Differences

Ridwan Ah Sheikh[†] *and* Sunil Kanwar[‡]

Abstract

This paper evaluates the impact of the agreement on Trade-Related Aspects of Intellectual Property Rights (TRIPS) on IPR-intensive export flows. The compliance requirements or the transition period clause that countries enjoyed under the agreement means that TRIPS implementation is staggered. We employ a recent heterogeneity robust difference-in-differences research design based on the rollout of the agreement in 155 countries from 1990-2010. Utilizing various partial aggregation schemes, we summarize the treatment effect heterogeneity across different dimensions. The robustness of baseline model is tested by looking at the impact of agreement in specific industrial clusters, demonstrating increased sensitivity to intellectual property. Our summary parameters aggregated by cohort-time, event-time and calendar-time reveal: First, the aggregate treatment effect estimates suggest that TRIPS on average led to significant reduction in IPR-intensive export flows. Second, our results demonstrate significant heterogeneity both across treatment cohorts as well as across different time periods. In particular, TRIPS led to almost 36% increase in IPR-intensive export flows in early complied cohorts, while the agreement cause 38% decline in exports for the treatment cohorts that complied later. Third, the IPR-intensive exports exhibit U-shaped response to TRIPS, falling for about ten years and then rising again. Moreover, the significant positive impact of agreement based on event-study estimates kicks in with delay. Specifically, eleven years after the agreement's initiation, export flows are 41% higher in compliant cohorts relative to the comparison group and these effects persist until 14 years after the agreement's inception. Fourth, the conclusions based on aggregate trade flows remain valid when examining the impact of TRIPS on the composition of trade within disaggregated industry clusters.

JEL Classification: C21, C23, F13, F14, O34
Key words: TRIPS, Exports, Intellectual property rights, Difference-in-differences, Treatment effects, Event study

[†]Corresponding author: Department of Economics, Delhi School of Economics, University of Delhi-110007. E-mail address: ridwan@econdse.org

[‡]Department of Economics, Delhi School of Economics, University of Delhi-110007. E-mail address: sunil_kanwar@econdse.org

1 Introduction

The Trade-Related Aspects of Intellectual Property Rights (TRIPS) agreement of the World Trade Organization (WTO) came into effect on 1 January 1995. The agreement led to notable changes in the international framework of legal institutions governing the ownership and flow of knowledge, technology and other intellectual assets. When the TRIPS negotiations began, the majority of developing and several developed countries had considerably less stringent intellectual property (IP) protection than that required by the TRIPS agreement. The agreement required harmonization of the minimum threshold of intellectual property protection across member signatories.

However, critics contended that during the initial stages of the development process, the TRIPS mandated guidelines may slowdown the industrial development, and reduce the rate of innovation in developing countries, by limiting their ability to freely copy foreign technology. Most developing countries protested that the agreement would strengthen the dominance of corporate monopolies in controlling intellectual property, potentially worsening the North-South technology gap (Deere 2009). In light of these challenges, TRIPS provided different transitional periods to the member states of WTO to comply with the agreement, depending on their level of development. These differences in the length of the transition period across groups of countries implies that TRIPS adoption was staggered, creating a natural variation across countries and years, in the timing of legislative reforms vis-à-vis implementation of the TRIPS agreement.

In this analysis, we leverage this differential timing of compliance with the TRIPS agreement, to specify a dynamic difference-in-differences (DiD) framework for examining the impact of TRIPS-induced intellectual property reform on the level of IPR-intensive export flows. Our baseline approach uses the recent heterogeneity-robust estimation procedure proposed by Callaway and Sant'Anna (2021) that identifies the policy relevant parameters in DiD framework under staggered adoption design. The Callaway and Sant'Anna (2021) approach estimates the family of relevant estimands referred to as *group-time average treatment effects* - $ATT(g, t)$ i.e., the average treatment effect for units in group g at time t, where a "group" is defined by the time period when the country (or countries) first complied with TRIPS agreement [1]. It compares the outcome for units in group

[1] The $group - time$ ATT's in Callaway and Sant'Anna (2021) are referred as $cohort - time$ effects in Sun and Abraham (2021), therefore, $group - time$ and $cohort - time$ ATT's are used synonymously in this paper.

g which complied with the TRIPS agreement during time t with the reference group of units that do not comply during that interval. The attractive feature of this approach is that it completely avoids the problems associated with the two-way fixed effects (TWFE) model, which is not robust to treatment effect heterogeneity under staggered rollout design (De Chaisemartin and d'Haultfoeuille 2020, Goodman-Bacon 2021, Callaway and Sant'Anna 2021, Sun and Abraham 2021, Wooldridge 2021, Borusyak et al. 2024).

Our results reveal several interesting dynamics on treatment effect heterogeneity across different groups (g), at different points in time (t), and across different lengths of treatment exposure ($e = t - g$). Based on the single overall treatment effects summary parameter (simple average weighted by group size), we find a significant negative impact of TRIPS implementation on IPR-intensive exports in countries that complied with the agreement compared to the reference group of countries that didn't. Similarly, the estimated parameters obtained by averaging across all cohort-time and calendar-time dimensions suggest similar conclusions. Second, the partially aggregated parameters reveal significant heterogeneity across different lengths of exposure to the treatment. In particular, any positive impact of TRIPS kicks in with delay, suggesting that complying countries take several time periods to expand their full potential on trade flows. Third, TRIPS led to substantial increase in IPR-intensive exports among early-compliant cohorts, particularly in high-income countries. Conversely, the agreement resulted in a reduction in exports for countries self-designated as developing that adhered to the agreement later. Fourth, the robustness of the baseline model is tested by looking at the impact of TRIPS in specific industrial clusters, demonstrating increased sensitivity to intellectual property. The cumulative impact of adherence to the TRIPS agreement across all compliant cohorts indicates a significant decrease in exports in biopharmaceuticals, information and communication technology (ICT), and production technology, but a significant increase in exports of medical devices. Fifth, the event study estimates reveal a negative initial response of TRIPS on export flows across all industry clusters, with any positive effects manifesting eleven years after the agreement's initiation. Sixth, the average effect of TRIPS differs among various treatment cohorts and industries. Specifically, early compliant cohorts ($g = 1995$, 1996) witnessed a significant positive impact in exports for biopharmaceuticals, chemicals, ICT, medical devices, and production technology, whereas cohorts complying later experienced a negative impact. Moreover, the discerned influence of TRIPS across all treatment cohorts suggests a significant positive effect of the agreement on exports of medical devices.

From a methodological perspective, our contribution is to adopt the recent heterogeneity robust staggered difference-in-differences to examine the average causal impact of TRIPS on IPR-intensive export flows. In particular, we rely on Callaway and Sant'Anna (2021) that yield more interpretable estimands in settings with staggered treatment timing and treatment effect heterogeneity. The estimation procedure in Callaway and Sant'Anna (2021) offers various aggregation methods that can be used to highlight treatment effect heterogeneity across different treatment-timing cohorts, at different points in time, and across different lengths of treatment exposure as well as to summarize the overall effect of participating in the treatment. However, the outcome based on aggregate exports in IPR-intensive category is rather extensive. Additionally, we narrow our focus to examine the heterogeneous sectoral impacts of TRIPS within distinct industrial clusters that exhibit heightened sensitivity to IPRs. The disaggregation has two broad implications: (i) it allows us to test for differences in treatment effects by sector (ii) it enables us to access the response of an agreement within each sector across different cohorts, in different years, and at different lengths of event horizon.

In terms of existing literature, our contribution is most closely related to Ivus (2010), Delgado et al. (2013) and Ivus and Park (2019) that study the impact of TRIPS or more general national patent reforms on the geography and composition of trade. However, these studies use conventional regression based two-way fixed effects (TWFE) estimator that has come under considerable scrutiny in the setting when treatment effects are heterogeneous and there is a variation in treatment timing (staggered design). Instead, we show that the results based on standard dynamic TWFE difference-in-differences does not identify the convex combination of cohort-specific treatment effects using the decomposition theorem in Goodman-Bacon (2021).

Finally, this paper provides first causal estimates of the impact of TRIPS on export flows in IPR-intensive industries using the recent advances in difference-in-differences with variation in treatment timing. Our approach can be further extended to revisit and improve on the related studies that examine the impact of membership in GATT/WTO (Rose 2004, Larch et al. 2019, Esteve-Pérez et al. 2020), regional trade agreements (Grant and Lambert 2008, Baier et al. 2019, Larch and Yotov 2024) and other trade policy instruments on the dynamics of trade and FDI (Baltagi et al. 2008, Anderson et al. 2019), respectively. [2]

2 Classification criteria for TRIPS compliance

As outlined in the introductory section, many developing countries resisted the implementation of TRIPS mandated guidelines with the apprehension that monopoly power granted by IP protection might restrict their accessibility to knowledge-based products or limit their ability to copy or adapt the foreign technology. During the early phase of TRIPS negotiations, developing countries contended that adherence to the Agreement may raise the prices of critical life-saving drugs, with adverse consequences on the health and well-being of their citizens. In this regard, Chaudhuri et al. (2006) find that TRIPS-induced pharmaceuticals product patents led to significant increase in prices of special class of antibiotics (quinolones) and large welfare loss for the Indian economy.

Importantly, the TRIPS council granted several extensions to member states to comply with the Agreement. Advanced countries were given one year, extending until January 1, 1996, to bring their laws and practices in conformity with TRIPS requirements. The member states that self-designated themselves as developing upon joining the WTO in 1995 were granted a transitional periods of five years (until January 1, 2000) to adhere to the Agreement. However, some countries did not extend patent protection to all technology areas (such as pharmaceuticals and agriculture chemical products), and the deadline for these countries was further extended until January, 2005. Finally, for least-developed countries (LDCs), the deadline to comply with TRIPS was initially set until January 1, 2006 which was further extended to January 1, 2016 to provide patent protection and exclusive marketing rights for pharmaceuticals and chemical products including trade secrets and undisclosed information during the 2001 Doha declaration on TRIPS agreement and public health, and extended to 2013 for all other categories.

[2]In a recent study, Nagengast and Yotov (2023) employ an extended two-way fixed effect (ETWFE) estimator for staggered difference-in-differences within structural gravity to estimate the effect of regional trade agreements (RTAs) on bilateral trade flows.

We identify the date of compliance to the TRIPS agreement using the classification criteria in Hamdan-Livramento (2009). We build on Delgado et al. (2013) to extend our analysis to more comprehensive sample of 155 countries using several approaches, to define when a particular country became TRIPS compliant, including WTO's original compliance schedule. First, for the WTO member countries not self-designated as developing in 1995, the estimated year of compliance is taken to be 1995. For the Czech Republic, Iceland, Portugal and Slovakia, the compliance year is taken to be 1996 (based on Ginarte and Park 1997 and Park 2008). Second, for the member states of WTO self-designated as developing in 1995, the estimated year of compliance is 2000. However, TRIPS granted numerous extensions for countries within this category that did not extend product patents in a particular area of technology such as pharmaceuticals and agricultural chemical products as of 2000 (as discussed previously), we draw on Park (2008), and Maskus and Ridley (2016) to identify countries that may be leveraging these exemptions or were non-compliant by 2000.[3] Third, for WTO member countries after 1995, their estimated compliance year is their membership date in accordance with WTO rules (Delgado et al. 2013, Maskus and Ridley 2016). Fourth, both the de facto and de jure system of patent laws in LDCs are not in compliance with TRIPS. Therefore, least-developed countries are classified as non-compliant for entire period of our analysis.[4]

The reluctance of most of developing countries to enact the TRIPS mandated policy changes suggests that compliance with the agreement was not an endogenous response to domestic innovation and other activities (Kanwar 2012, Delgado et al. 2013). Therefore, critical for our identification, the TRIPS agreement provides a natural experiment of sorts, by creating a variation in the rollout of an agreement across country and year.

[3]We extend our gratitude to Maskus and Ridley (2016) for sharing their data on TRIPS compliance for extended sample of countries used in their study.

[4]In the final analysis using Callaway and Sant'Anna (2021) DiD design, we drop least-developed countries (*never-complied*) from the sample and use only *not-yet-complied* as a valid comparison group (see Subsection 4.3 for discussion).

3 Existing scholarship

The extant literature on the relationship between trade and IPR protection reveal mixed results. Ivus (2010) finds that stronger patent rights in the developing countries who had been the former colonies of British and French raise technology-intensive exports from advanced countries into former. Delgado et al. (2013) finds that TRIPS implementation was associated with higher exports in high-IP products. While Rafiquzzaman (2002) identified a positive and statistically significant response on Canadian exports to the strength of patent rights (PRs) in high-income countries, the estimated impact of stronger PRs in middle and low-income group on the other hand is found to reduce Canadian export flows in most of the sectors through the exercise of market power effect. In a recent study, De Rassenfosse et al. (2022) utilize the firm level data on French firms and found that firms export more to the destination markets where its goods are protected by patents. Sheikh and Kanwar (2022) find that de facto patent rights protection has a strong positive effect on developing country knowledge-intensive imports. However, a number of other studies find on average either a negative or insignificant effect of intellectual property protection on trade. Maskus and Penubarti (1995) find that stringent patent rights in the host country have an insignificant effect on OECD export flows in patent-intensive industries; Co (2004), Smith (1999), Braga and Fink (2005) report similar findings. Campi and Dueñas (2016) examine the impact of IPRs on trade in agricultural products during a post-TRIPS period and find that IPRs have negative and uneven effect on agriculture trade flows. Palangkaraya et al. (2017) identify the two cases through which destination country patent system affects the trade. The first one is the degree of difficulty for the would-be-exporter to obtain a patent in the destination country (*foreign patent bias*); and second, their expectation of a patent infringement suit in that country (*block local*). They found that both *foreign patent bias* and *block local* were statistically significant and negatively related to trade.

The empirical studies discussed above mainly focused on the extent to which stronger IPR protection in destination (importing) country help in attracting imports from source country. Relatively, few studies examine the reverse flows; that is, the extent to which stringent IPR laws affect the outward technology transfer via exporting. In this direction, Maskus and Yang (2018) find that countries with stringent patent protection have significantly greater exports in R&D-intensive

sectors and the estimated impact is higher in countries that were compliant with TRIPS relatively early. Using a difference-in-differences approach, Ivus and Park (2019) find that high-IP exports expand along the extensive margin when patent reforms occur in developing countries. However, Shin et al. (2016) contended that source country exports are blocked by stronger patent protection in their destination market. At an income-group levels, they found that stronger IPR protection in advanced countries acts as a strong barrier to the exports of rapidly emerging economies. In yet another study, Maskus and Ridley (2016) find strong negative effect of TRIPS on high-IP exports in both low-income and high-income countries. Briggs and Park (2014) find similar conclusions that patent rights have stronger impact on exports and licensing activities of firms in developed countries, but such effects in terms of exporting are not observed for firms in developing countries.

Despite all this evidence, the effect of stronger IPR protection on trade is still in infancy and the estimated impacts tend to vary both across income-groups and product levels. This paper uses a natural experiment to identify the IPR policy shock via TRIPS rollout exogenous to economic fundamentals to examine its impact on IPR-intensive export flows in a staggered difference-in-differences setting.

4 Methods

The objective of this paper is to first use the conventional regression-based dynamic TWFE framework. Subsequently, the estimates derived from the TWFE will be juxtaposed with those obtained from a heterogeneity-robust estimator, allowing us to make some important comparisons (Schmidheiny and Siegloch 2023, Callaway and Sant'Anna 2021). Next, in Subsection 4.2, we discuss the pitfalls of TWFE estimator in a setting when treatment effects are heterogeneous and the design is staggered using the decomposition theorem in Goodman-Bacon (2021). Finally, in subsection 4.3, we depart from the conventional practice to adopt the recent heterogeneity-robust staggered difference-in-differences estimator proposed by Callaway and Sant'Anna (2021), our focus in this section will be on the effects of TRIPS rollout on IPR-intensive exports flows, both at aggregate and disaggregated industry clusters.

4.1 TWFE event study estimator

The causal effects in staggered adoption difference-in-differences (DiD) designs with a binary treatment variable have traditionally been estimated via Ordinary Least Squares (OLS) with two-way fixed effects. The fully dynamic event-study specification used in most of the studies takes the following functional form:

$$ln\ X_{it} = \lambda_i + \zeta_t + \sum_{\substack{l=-K \\ l\neq -1}}^{L} \beta_l \mathbf{1}\{F_i = t - l\} + \epsilon_{it} \tag{1}$$

The panel event study design seeks to estimate the impact of TRIPS compliance in countries that signed the agreement in given year and to use as the counterfactual the countries that did not sign the agreement in that year [5]. It considers the variation in outcomes around the adoption of TRIPS with the baseline reference period [6]. Moreover, this method enables one to check both the degree to which post-treatment effects were dynamic, and whether treatment and control groups were comparable on outcome dynamics pre-treatment, commonly referred to as the parallel trend assumption (Cunningham 2021).

$Ln\ X_{it}$ is the logarithm of IPR-intensive exports flows (nominal) in country i at time t, λ_i and ζ_t are country and year fixed effects, respectively, and relative time indicators $\mathbf{1}\{F_i = t - l\}$ equal to 1 if country i becomes TRIPS compliant l periods ago[7]. For $l \geq 0$, β_l is supposed to estimate the cumulative effect of $l + 1$ treatment periods after the TRIPS agreement. For $l \leq -2$, β_l either capture anticipation effects or, in their absence, is interpreted as placebo coefficients used in testing the validity of the parallel trends assumption.

[5] Nevertheless, the plethora of recent DiD literature indicates that the event study approach employing TWFE estimator also involves problematic comparisons that renders the estimated effects difficult to interpret causally, we will discuss about the pitfalls of TWFE event study design in the next section.

[6] The treatment is assumed to be an absorbing state, i.e., countries once complied to the agreement are assumed to be complied forever, Schmidheiny and Siegloch (2023) In contrast, De Chaisemartin and d'Haultfoeuille (2020) estimator considers a case of non-absorbing treatment status in a staggered DiD setting.

[7] As is standard in this setup, one of the event periods must be dropped to avoid perfect multicollinearity, therefore, the event study dummy for the year before TRIPS treatment $l = -1$ is often excluded as a normalization.

4.2 Goodman-Bacon decomposition

The regression based TWFE model is valid in a canonical 2×2 case, where we have only two groups, the treated and the comparison group, and two-time periods, the pre-treatment and post-treatment, such that the comparison group is untreated in both time periods whereas the treatment group receives the treatment in post-treatment period only. However, in a setting, when treatment effects are heterogeneous and units are treated at different points in time (i.e., treatment timings are staggered over time), the traditional TWFE erroneously employs already-treated units as a comparison for units treated later, potentially producing misleading results (Goodman-Bacon 2021, De Chaisemartin and d'Haultfoeuille 2023, Borusyak et al. 2024).

In the specific context we are considering, the average treatment effects of TRIPS on export flows may vary across different factors, rendering the TWFE estimator less favorable. First, the enforcement mechanism may vary across countries, and in some countries it may have been slow to develop. Therefore, any discernible effects of TRIPS may become manifest over time. Second, the impact of TRIPS-induced IPR reforms depends crucially on complementary investments, (such as the provision of electricity in ICT and availability of trained staff in the case of pharmaceutical, electronics and professional goods, etc.). Given the heterogeneity in the level of such investments across countries, the impact of TRIPS may vary accordingly. Additionally, the development of such complementary investments may take time, leading to the gradual emergence of export markets over time. Third, exporters may have required time to learn about the potential of exports in foreign market. Fourth, the impact of TRIPS could be different for the countries that complied with the agreement earlier, versus those that complied later (Egger et al. 2023). Finally, the impact of TRIPS compliance on exports could also be different at different lengths of treatment exposure, i.e., the average treatment effect may increase or decrease with elapsed treatment time.

4.2.1 Potential pitfalls of TWFE estimator

In this subsection, we provide an illustration of the potential pitfalls of the TWFE estimator. Consider a regression of the logarithm of IPR-intensive exports ($ln\ X_{it}$) on country fixed effects λ_i, year fixed effects ζ_t, and a binary treatment indicator D_{it} that equals 1 if unit i is TRIPS-compliant at period t and zero otherwise.

$$ln\ X_{it} = \lambda_i + \zeta_t + \beta_{fe} D_{it} + \epsilon_{it} \tag{2}$$

If the parallel trend assumption is satisfied, it follows that:

$$E[\beta_{fe}] = E\left[\sum_{(i,t)\,:\,D_{it}=1} w_{i,t}\ TE_{it}\right] \tag{3}$$

where $TE_{i,t} = ln\ X_{it}(1) - ln\ X_{it}(0)$, is the treatment effect of unit i at time t. In this representation, β_{fe} is the estimated impact of TRIPS on export flows, obtained as a weighted average of a series of 2×2 unit-specific difference-in-differences estimates. The weights sum to 1, but may be negative (De Chaisemartin and d'Haultfoeuille 2020, Roth et al. 2023). Goodman-Bacon (2021) demonstrate that, with differential treatment timings, some of these 2×2 estimates leverage on "forbidden comparisons" that compare the outcomes between groups that got treated at a certain point in time with the reference group which got treated earlier, leading to negative weights [8]. While the forbidden contrasts are appropriate, and enhances efficiency in situations where treatment effects are homogeneous, they become problematic when treatment effects exhibit heterogeneity across units or time, as is the case in our scenario (Borusyak et al. 2024). The negative weights are an issue, because even when all the average treatment effects (TE_{it}) are positive (indicating a positive impact of TRIPS on IPR-intensive export flows across all units), the overall average treatment effect of the treated (β_{fe}) could be negative due to the negative weights, which may be difficult to interpret causally.

To see the problem of negative weights in TWFE model in the presence of treatment effect heterogeneity and staggered design, let us consider a simple example (presented in the table below) from Borusyak et al. (2024). Consider two units A and B, both observed in periods $t = 1, 2, 3$. The early-treated unit A is untreated in period 1, but treated in periods 2 and 3. Unit B is untreated in periods 1 and 2, but treated in period 3. Based on equation (2) and normalizing $\zeta_1 = 0$, the

[8] The already complied (been treated early) countries form an invalid comparison, in such a scenario they do not provide the correct counterfactual of what the later complied countries would look like in the absence of compliance with the agreement, thereby placing negative weights ($w_{it} < 0$)

$E[Y_{it}]$	$i = A$	$i = B$
$t = 1$	λ_A	λ_B
$t = 2$	$\lambda_A + \zeta_2 + \beta_{A_2}$	$\lambda_B + \zeta_2$
$t = 3$	$\lambda_A + \zeta_3 + \beta_{A_3}$	$\lambda_B + \zeta_3 + \beta_{B_3}$
Event date	$E_i = 2$	$E_i = 3$

outcome evolution of units A and B in a no-treatment state $t = 1$ depends on unit fixed effects λ_A and λ_B, respectively. At $t = 2$, outcome evolution of unit A (treated) depends on unit and period fixed effects plus unit $A's$ treatment effect in period 2, which is β_{A2}. But at $t = 2$, unit B is untreated, so that its outcome evolution depends just on unit and period fixed effects, λ_B and ζ_2, and so on. In this example, equation (3) reduces to:

$$\beta_{fe} = \frac{1}{2}\{DID_{A,B,1,2} + DID_{B,A,2,3}\}$$

with $DID_{A,B,1,2} = (Y_{A2} - Y_{A1}) - (Y_{B2} - Y_{B1})$, where Y denotes the outcome. Here, we compare the outcome-evolution of unit A, that switches from untreated in period 1 to treated in period 2, using unit B that is untreated at both periods as a comparison. Such a comparison is "admissible". On the other hand, $DID_{B,A,2,3} = (Y_{B3} - Y_{B2}) - (Y_{A3} - Y_{A2})$, compares the outcome evolution of unit B (that switches from untreated in period 2 to treated in period 3) using the already treated unit A as the reference category. Such a comparison is "forbidden", because it does not provide a valid counterfactual for unit B, as discussed above. Therefore, the TWFE estimand as a weighted average of all the admissible and forbidden 2×2 difference-in-differences estimates is obtained as:

$$\beta_{fe} = \beta_{A2} + (1/2)\beta_{B3} - (1/2)\beta_{A3}$$

Consequently, under staggered design, TWFE places a negative weight on the late treatment effect of early treated unit A (β_{A3}). Thus, the larger these effects are in the long-run, the smaller is β_{fe} (Borusyak et al. 2024). The negative weights occur because $\widehat{\beta_{fe}}$ leverages comparison of a cohort that switches from being untreated to treated with a cohort that is treated in both periods.

The Goodman-Bacon (2021) decomposition computes β_{fe} as the weighted average of all possible 2×2 difference-in-differences (DiD) estimates. Some DiD estimates compare units treated at a particular time with units untreated during that time, and these are legitimate comparisons. However, other DiD estimates are obtained using already treated units as a comparison for later treated units, which are forbidden comparisons. The weights on the 2×2 DiD's are proportional to group sizes and the variance of the treatment dummy in each group. This decomposition provides a simple assessment of the source of identifying variation, and tells us how much of the overall DiD estimate β_{fe} is driven by forbidden comparisons and how much by legitimate comparisons.

4.3 Heterogeneity robust staggered DiD estimator

To address the potential bias in TWFE model under staggered rollout design with heterogeneous treatment effects, we rely on the difference-in-differences procedure recently proposed by Callaway and Sant'Anna (2021) [9]. This approach accommodates dynamic treatment effects while preventing inaccurate (forbidden) comparisons and associated problems of negative weights. They provide an estimation procedure to identify the disaggregated causal parameters refereed to as *group-time average treatment effects on the treated*, $ATT(g, t)$ - which is the average treatment effect of group g treated by the time period t, where "group" is defined by the time period when a unit first complies with the TRIPS agreement. For example, in our setting, we observe a group of countries that were compliant with the TRIPS agreement at different points in time, such that $g \in (1995, 1996, 1997, ...2008)$ denotes different treatment timing groups [10]. In order to formalize the identification procedure in Callaway and Sant'Anna (2021), consider a case with $\mathcal{T}$ periods, and denote a particular time period by t, where $t = 1, 2, 3.....\mathcal{T}$. Let G_g denote a binary variable if a country is in treatment time group g. In potential outcome notation, $ATT(g, t)$ can be expressed as:

$$ATT(g, t) = \mathbb{E}\left[Y_t(1) - Y_t(0) \mid G_g = 1\right] \tag{4}$$

where $Y_t(1)$ is the observed outcome (IPR-intensive exports in our context) at time t for treated units under TRIPS compliance, and $Y_t(0)$ is the potential outcome for the same unit had they not

[9]For better clarity, we will employ similar notations as those used originally in Callaway and Sant'Anna (2021) throughout this paper.

[10]For example, $g = 1995$ denotes the group of countries that were TRIPS compliant in period 1995, similarly for other treatment timing groups etc.

complied with the agreement. However, in standard DiD settings, $Y_t(0)$ is not observed for periods after g, the counterfactual outcome for the treated unit had it not been treated remains unobserved. This motivates the use of a comparison group of never-treated or not-yet treated units as a proxy, to estimate the outcomes that would have occurred in the absence of treatment, which is often referred to as the parallel trend assumption. The Callaway and Sant'Anna (2021) framework considers four different versions of the parallel trend assumption to impute the missing counterfactual for the treated units. In our case, the family of $ATT(g,t)$ are nonparametrically identified using "unconditional parallel trend assumption based on not-yet treated units as a comparison". The reason is twofold: (i) we observe no significant pre-trends unconditionally, therefore the matching on any pre-treatment covariates is not required [11]; and (ii) conditioning on covariates may lead to *post-treatment bias*, i.e., the bias that may occur if the control variable is itself affected by treatment (see Zeldow and Hatfield 2021 and Roth et al. 2023 for additional discussion). Further, in our case the least-developed countries (LDCs) are not compliant with TRIPS throughout the entire period of sample, and therefore can be used as a part of the comparison group. However, Callaway and Sant'Anna (2021) argue that if "never-complied" units behave differently from other "eventually complied" units, which may be true in our context as well, then one can drop the "never-complied" units from the analysis, and instead proceed with using "not-yet complied" units as a comparison for units that have complied with the agreement earlier. It utilizes more groups as a valid reference category which may improve inference. Therefore, $\forall\ g, s, t = 2, ...\mathcal{T}$, such that $t \geq g, s \geq t$, the "unconditional parallel trend assumption based on not-yet complied as a comparison" can be expressed as:

$$\mathbb{E}\left[Y_t(0) - Y_{t-1}(0) \mid G_g = 1\right] = \mathbb{E}\left[Y_t(0) - Y_{t-1}(0) \mid D_s = 0\right] \tag{5}$$

The assumption states that the average outcome for the group that complied with the TRIPS agreement in period g, and for the group not-yet complied to the agreement by period s would have followed a parallel path in absence of TRIPS.

[11]The Callaway and Sant'Anna (2021) procedure also consider a case when there are covariate specific trends in outcome overtime and the parallel trend assumption holds only after conditioning on covariates. In which case, they use different estimands to sufficiently weight untreated units by inverse probability scores, such that treatment and control groups are similar in pre-treatment characteristics (Heckman et al. 1997, Abadie 2005, Goodman-Bacon and Cunningham 2019, Sant'Anna and Zhao 2020).

Therefore, under Callaway and Sant'Anna (2021) and Marcus and Sant'Anna (2021) framework, the group-time average treatment effects $ATT(g,t)$ in equation (4) using this version of parallel trend assumption can be nonparametrically point identified as:

$$ATT^{ny}_{unc}(g,t) = \mathbb{E}\left[Y_t - Y_{g-1} \mid G_g = 1\right] - \mathbb{E}\left[Y_t - Y_{g-1} \mid D_t = 0, G_g = 0\right] \tag{6}$$

where the first term is the outcome evolution for the TRIPS compliant group, and the second term is the equivalent outcome evolution for the not-yet complied comparison group. The estimator uses the outcome in period $g-1$ as the base period outcome.[12]

4.3.1 Aggregation of group-time average treatment effects

In the context of DiD with many treated periods and/or cohorts, reporting all the $\widehat{ATT}(g,t)$'s may be cumbersome and each one may be imprecisely estimated if there are relatively few observations in each (g,t) cell. Callaway and Sant'Anna (2021) consider several *partial aggregation* schemes to combine these estimates into a more manageable set of parameters.

First, we take the simple average of all the identified $ATT(g,t)$'s together to get an overall effect of complying with the agreement, i.e., we consider:

$$\theta^o_w = \frac{1}{\kappa} \sum_{g \in \mathcal{G}} \sum_{t=2}^{\mathcal{T}} \mathbf{1}\left\{t \geq g\right\} ATT(g,t) P(G = g | G \leq \mathcal{T}) \tag{7}$$

where θ^o_w is the *simple weighted average* of all group-time average treatment effects, but it puts more weight on $ATT(g,t)$'s with larger group size or on groups that participate in the treatment for longer. Alternatively, the Callaway and Sant'Anna (2021) framework suggests partial aggregation procedures that allow us to highlight the treatment effect heterogeneity at cohort-time, event-time, and calendar-time levels.

[12] By replacing the expectations with their sample analogs, equation (6) can alternatively be expressed as:

$$\widehat{ATT}^{ny}_{unc}(g,t) = \frac{1}{N_g} \sum_{i:G_i = g} \left[Y_{i,t} - Y_{i,g-1}\right] - \frac{1}{N_{\mathcal{G}}} \sum_{i:G_i \in \mathcal{G}} \left[Y_{i,t} - Y_{i,g-1}\right]$$

The estimator uses not-yet treated units ($\mathcal{G} = \{g' : g' > t\}$) as a comparison group for units treated in period g (Roth et al. 2023).

Second, we consider the average effect of TRIPS compliance separately for each group, which we call group-specific (or cohort-specific) effects. This sort of heterogeneity may be intriguing in our case if the impact of any trade policy instrument, including TRIPS, varies across groups that complied with the agreement earlier, versus those that complied later (Maskus and Yang 2018, Baier et al. 2019, Larch and Yotov 2024, Egger et al. 2023, Nagengast and Yotov 2023). Therefore, the *cohort-specific* aggregation can be expressed as:

$$\theta_{sel}(\tilde{g}) = \frac{1}{\mathcal{T} - \tilde{g} + 1} \sum_{t=\tilde{g}}^{\mathcal{T}} ATT(\tilde{g}, t) \tag{8}$$

where $\theta_{sel}(\tilde{g})$ is the average treatment effect among countries that first complied with the TRIPS agreement in period $\tilde{g}$, across all their post-treatment periods. Further, $\theta_{sel}(\tilde{g})$ can be combined by averaging across all groups to get an easy to interpret overall impact of an agreement, i.e.,

$$\theta^{o}_{sel} = \sum_{g \in \mathcal{G}} \theta_{sel}(g) P(G = g | G \leq \mathcal{T}) \tag{9}$$

Therefore, $\forall\ t \geq g$, we first compute the average treatment effect for each group across all time periods and then average these effects together across all groups. [13]

Third, in DiD setup with multiple time periods and variation in treatment timings, often the interest lies in comparing the outcome trajectories over different lengths of event horizon. As outlined in Subsection 4.2, IPR-intensive exports may exhibit a delayed response to the initiation of the TRIPS agreement, and the estimated effects may increase/decrease with elapsed treatment time. Therefore, the *event-study* aggregation of $ATT(g,t)$'s is of particular interest to us. Furthermore, pseudo-ATT's in Callaway and Sant'Anna (2021) event-study design are useful to test the validity of the parallel trend assumption. Let $e = t - g$ denote the time elapsed since the agreement was

[13] For example, consider a cohort that complied to agreement in 1995 ($g = 1995$), then $\forall\ t \geq g$, $ATT(1995, 1995)$ corresponds to instantaneous treatment effect for this group; $ATT(1995, 1996)$ is the treatment effect one year after the agreement; $ATT(1995, 1997)$ is the effects two years after, and so on. Based on equation (8), we average all these $ATTs$ to get an average effect for $g = 1995$, we follow the same procedure for other cohorts, and then (based on equation 9), we again average across all cohorts to obtain an overall summary parameter.

adopted. Then aggregation of $ATT(g,t)$'s with respect to e is:

$$\theta_{es}(e) = \sum_{g \in \mathcal{G}} \mathbf{1}\left\{g + e \leq \mathcal{T}\right\} P(G = g | G + e \leq \mathcal{T}) ATT(g, g + e) \tag{10}$$

where $\theta_{es}(e)$ is the average treatment effect e time periods after the agreement were adopted across all cohorts. We can further define an overall treatment effect parameter by averaging across all event times, such that: [14]

$$\theta_{es}^{o} = \frac{1}{\mathcal{T} - 1} \sum_{e=0}^{\mathcal{T}-2} \theta_{es}(e) \tag{11}$$

Fourth, in the presence of heterogeneous treatment effects across different time periods, the estimation procedure in Callaway and Sant'Anna (2021) also permits the formulation of an aggregated target parameter with respect to *calendar-time* t, and is given by:

$$\theta_c(t) = \sum_{g \in \mathcal{G}} \mathbf{1}\left\{t \geq g\right\} P(G = g | G \leq t) ATT(g, t) \tag{12}$$

where $\theta_c(t)$ is the effect of compliance to the agreement in time period t across all cohorts that have adopted the agreement by period t. By analogy, we can aggregate $\theta_c(t)$ across all time periods, i.e.,

$$\theta_c^{o} = \frac{1}{\mathcal{T} - 1} \sum_{t=2}^{\mathcal{T}} \theta_c(t) \tag{13}$$

which gives the average impact of TRIPS across all time periods.

5 Data

Our dataset on nominal export flows is retrieved from United Nations Commodity Trade Statistics database (UNCOMTRADE 2024). The majority of countries in our sample have complied with the TRIPS agreement by 2010, while the least-developed countries remained non-compliant for the entire period of analysis. Since, in our case, least-developed countries are not the part of the

[14]For example., for a cohort that complied with TRIPS in 2001, $e = 2$ corresponds to 2003. Similarly, for a cohort that complied with the agreement in 2002, $e = 2$ corresponds to 2004. We first aggregate these ATT's at $e = 2$ across all cohorts to obtain the average effect two years after an agreement. Working by analogy, we again aggregate across all event-times to obtain an overall treatment effect parameter.

comparison group, they are excluded from the analysis. The result is a country-level panel that describes export flows at detailed product categories across 123 countries spanning 1990-2010 period (see Table 1). Therefore, all the countries in our sample are TRIPS compliant, though the dates of compliance are different. We identify the family of group-time average treatment effects on treated *ATT(g,t)*'s using not-yet-complied units as a valid comparison for units that complied to the agreement earlier.

We use the classification criteria of Delgado et al. (2013), which divides product groups from Standard International Trade Classification (SITC), Revision 3 into three distinct categories of IPR-intensity: high-IP group, high-IP clusters and low-IP intensive products. This categorization, detailed in a recent report by the Economics and Statistics Administration (ESA) and the United States Patent and Trademark Office (USPTO) (U.S. Department of Commerce 2012), identifies IPR-intensive industries as ones with above-average IP intensity in the United States (based on patents, copyrights, and trademarks). Moreover, Delgado et al. (2013) employ a cluster mapping approach by including a particular sub-category of products from high-IP group into high-IP clusters. The product categories within the same cluster exhibit closer similarities in terms of skills, input demand and other linkages compared to the categories in other clusters (see Delgado et al. 2016). This approach facilitates a more refined grouping of product into meaningful high-IP intensity clusters. The detailed sectoral classification is provided in Table 2 in appendix.

In this study, we are particularly interested to examine the impact of TRIPS on IPR-intensive export flows, because these sectors involve significant investment in R&D, and IPRs are considered an effective mechanism for appropriating the returns to innovation (Cohen et al. 2000). Furthermore, TRIPS agreement witnessed a disproportionate strengthening of patent rights within these sectors. Therefore, for each exporter country, we aggregate the export flows in high-IP group across all importers, to obtain the total (nominal) value of IPR-intensive exports of country i in year t, which is our outcome variable. The advantage of examining aggregate export flows is, first, we do not have to worry about large number of observations with zero trade values, second, aggregating exports over all partners ensures that partner countries time-varying multilateral resistance do not interfere with our results (Ivus 2010). In the robustness analysis, we look at the separate impact of TRIPS across different IPR-intensive industry clusters.

Finally, we use multiple approaches including the WTOs original compliance schedule to identify each country's date of adherence to TRIPS agreement. The details on the classification criteria of TRIPS compliance are already discussed in Section 2.

6 Major results and analysis

This section outlines our primary findings regarding the impact of TRIPS on IPR-intensive export flows. We develop the analysis in four steps. First, we examine and analyze the results based on the conventional regression-based TWFE-OLS estimator (Subsection 6.1). Second, we demonstrate the limitation of the TWFE estimator, using Goodman-Bacon decomposition in our context (Subsection 6.2). Third, in our baseline model, we present our main findings based on staggered difference-in-differences procedure proposed by Callaway and Sant'Anna (2021), that allows us to highlight treatment effect heterogeneity across different dimensions (Subsection 6.3). Fourth, we explore the robustness of our baseline model by looking at the impact of TRIPS at more disaggregated IPR-intensive industry clusters (Subsection 6.4).

6.1 Results based on TWFE-OLS estimator

Table 3 displays the TWFE estimates on regression of lnX_{it} on country fixed effects, year fixed effects, and an indicator for TRIPS agreement D_{it}, which equals one if a given country has signed the agreement in year t and equals zero otherwise. The estimates in Table 3 reveal no significant (average) impact of participating in the TRIPS agreement on IPR-intensive export

However, in a standard event study setup, often the interest lies in capturing the treatment effect dynamics for l periods relative to the signing of the TRIPS agreement. Therefore, we estimate the fully dynamic TWFE regression specification (equation 1) by incorporating appropriate "leads" and "lags" of TRIPS indicator to the inception of treatment. To visually test for the validity of parallel trend assumption, we display the coefficients on event leads ($l \leq -2$) in Figure 1. Conditional on passing this test, the coefficients of the treatment lags for periods $l \geq 0$ appear in Table 4, and provide the dynamic effect l periods later.

We do not observe any significant differences in outcome (exports) between treated and control units prior to the adoption of an agreement. The increase in exports in treated countries only change after TRIPS rollout. Therefore, we conclude that there is no evidence of differential pre-trends in our setting and proceed to obtain the dynamic effects following the treatment. The results in Table 4 suggest that compliance with TRIPS agreement led to a significant increase in IPR-intensive export flows, although discernible effects were observed to occur with a delay. Specifically, the coefficient of 0.15 at $e = 4$ indicates that after removing unit and period specific confounders, the level of IPR-intensive exports increased by approximately 15 log points five years after the agreement in countries that are compliant with the TRIPS agreement than in non-compliant countries. This pattern continues with an increase of 16.2 log points six years after, and 12.2 log points seven years after the agreement's initiation and so forth. These effects persist and grow over time with the full effect of TRIPS on IPR-intensive export flows occurring ten years following the adoption of an agreement.

Our initial results based on dynamic TWFE event-study design confirm the earlier findings in Ivus (2010), Delgado et al. (2013) and Ivus and Park (2019), that rely on similar regression based TWFE estimator to examine the impact of TRIPS or more general national patent reforms on the direction and composition of trade in knowledge-intensive sectors.

6.2 DiD decomposition and the pitfalls of TWFE-OLS estimator

The plethora of recent literature has criticized the conventional practice of implementing the event study framework via TWFE estimator, highlighting the potential biases in estimated coefficients in presence of treatment effect heterogeneity (De Chaisemartin and d'Haultfoeuille 2020, Goodman-Bacon 2021, Callaway and Sant'Anna 2021, Sun and Abraham 2021, Wooldridge 2021, Borusyak et al. 2024). With staggered rollout and treatment effect heterogeneity, the results based on TWFE estimator may be contaminated by series of DiD coefficients using "forbidden comparisons" (see Section 4.2).

In this section, we use the decomposition theorem in Goodman-Bacon (2021) to decompose the overall ATT into series of 2×2 estimates to illustrate the sources of identifying variation. Table 5 reports the results from DiD decomposition utilizing a balanced panel [15]. The TWFE DiD estimate of 0.095 suggests that IPR-intensive exports flows increased by almost 9.5 log points in TRIPS-compliant than in non-compliant countries, yielding conclusions similar to those obtained from the full dataset (section 6.1). Figure 2 plots each 2×2 DiD estimate from the decomposition exercise against their weights. The overall DiD estimate of 0.095 is the weighted average of each 2×2 DiD estimates on y-axis, where the weights correspond to the values on the x-axis. However, the estimate is biased and contaminated by series of DiD coefficients using forbidden comparisons (red circles).

Table 5 reports the average DiD estimates against their weights for each DiD comparison. The *early vs late* comparisons (blue triangles in Figure 2) are admissible whereas *late vs early* (red circles in Figure 2) denote the forbidden contrasts. The overall DiD estimate of 0.095 can be expressed as a weighted average of both admissible and forbidden contrasts, such that:

$$0.095 = (-0.111 \times 0.379) + (0.222 \times 0.621)$$

If we only consider the admissible DiD coefficients (*early vs late*) and compute the corresponding TWFE estimate using rescaled weights, we get an estimate of -0.042 (-0.111×0.379), indicating that TRIPS actually led to reduction in IPR-intensive exports in treatment relative to comparison group. Furthermore, the average DiD estimate obtained from forbidden comparisons is 0.222 with a weight of 0.621. This implies that under staggered treatment timing, the TWFE estimator assigns greater weight on impermissible contrasts, and is clearly pulling up the average into positive zone, diluting the overall estimation of β_{fe}. Thus, the 2×2 estimates obtained from using forbidden comparisons introduce biases, even when variance weighted parallel trend assumption holds true (Cunningham 2021).

[15] The Goodman-Bacon decomposition does not work in case of unbalanced panel and requires data to be strongly balanced.

6.3 Results based on heterogeneity-robust DiD estimator

In this section, we discuss our main estimates based on Callaway and Sant'Anna (2021) (denoted C&S hereafter). Table 6 reports the aggregate treatment effects obtained by averaging across all groups, event-times and calendar-times, respectively. The results based on 'simple weighted average' of all $ATT(g,t)$'s shows that that IPR-intensive exports flows in TRIPS compliant countries were 23% lower, relative to the comparison group. Similar conclusion follow with respect to cohort and calendar specific aggregations. However, the results based on event-study aggregation, although negative are statistically insignificant.

The results starkly differ from those obtained in TWFE-OLS design reported above, and in other previous studies such as Ivus (2010), Delgado et al. (2013) and Ivus and Park (2019). Interestingly, the overall negative impact of TRIPS on IPR-intensive export flows complements the findings in Maskus and Ridley (2016) and other related studies. One plausible explanation could be that the the C&S framework uses the appropriate set of comparisons and bias-corrected weighting scheme to circumvent the problem associated with TWFE model under staggered rollout design and treatment effects heterogeneity. In fact, the estimated effects under C&S method are in consonance with the TWFE design when using only admissible comparisons as outlined in section 6.2.

The theoretical mechanism underlining the negative impact of TRIPS on trade in IPR-intensive category is well-documented in previous literature. The cost of establishing a legal system for the protection of intellectual property, as mandated in TRIPS, have been over the odds to most of the middle-income and low-income countries, potentially constraining their exports (Schneider 2005), even though the agreement in principle required minimum standards of protection. Furthermore, developing countries are positioned away from the technological frontier and may be producing less sophisticated products in R&D-intensive category compared to developed world. In that case, stringent IPR protection in developed countries may act as a deterrent to developing countries exports. This is especially true if products from developing countries are perceived as too infringing or imitative under the IPR regime or former, thereby preventing legal entry into those markets (Auriol and Biancini 2009, Odagiri et al. 2010). Additionally, incumbent firms in North often resort to IPR barriers to deter exports from LDCs that may pose a threat to their domestic market. Shin et al. (2016) demonstrated that stronger IPR protection in developed countries can significantly impede exports from LDCs and rapidly emerging economies that are in the process of catching-up in terms of their level of technology. Moreover, the difficulty of would-be-exporters to obtain a patent in destination market, along with their apprehension of facing an infringement suit in those market, would curtail their exports, ceteris paribus (Palangkaraya et al. 2017).

6.3.1 Partial aggregation of group-time average treatment effects

The Callaway and Sant'Anna (2021) procedure proposes several partial aggregation schemes to highlight treatment effect heterogeneity across different dimensions. In this section, we complement the average TRIPS estimates from Table 6 with a series of disaggregated results. Our results reveal several interesting dynamics on treatment effect heterogeneity across different groups (g), at different points in time (t), and across different lengths of treatment exposure ($e = t - g$).

First, we expect the causal effect of TRIPS agreement to vary depending on the years relative to the treatment period g. To assess the credibility of the identification strategy, we begin by testing the validity of the parallel trend assumption. The coefficients on treatment leads, interpreted as a measure of "pre-trends", along with post-treatment dynamic effects are reported in Figure 3. We do not find any evidence of differential pre-TRIPS trends; the pseudo-ATTs are not significantly different from zero. Figure 3 shows that IPR-intensive exports have a U-shaped response to TRIPS compliance, falling for about ten years and then rising again. The post-TRIPS dynamic effects reported in Table 7 (see row "Event-time") reveal significant heterogeneity across different lengths of exposure to the treatment. Specifically, in the first year of agreement compliance, IPR-intensive exports decrease by 16.5%, in the second year by 18.6%, in the third year by 30.2% etc. These effects persist up to lag eight from the commencement of TRIPS complinance. Thus, the significant positive impact of TRIPS-compliance on technology-intensive trade flows kicks in with a substantial lag, suggesting that the implementation of agreement post-signing needs substantial time for the realization of its full potential. In particular, the IPR-intensive export flows eleven years after the agreements are 41% higher in TRIPS-compliant countries relative to the comparison group, by 40% higher in twelfth year, these effects persist until fourteen years after the agreement's inception.

The stronger patent protection can encourage inward technological transfer through trade, FDI, and licensing. As a result, firms in developing countries may learn from the foreign technology embodied in imports, become sufficiently productive over time, and eventually begin exporting new products (Maskus and Yang 2018). However, this adjustment process - *learning by doing from inward FDI and imitation* - may take time and the market for exports will emerge gradually over time. Maskus and Yang (2018) find that R&D-intensive exports increased with the implementation of TRIPS in early years. Contrary to that, our findings indicate that compliance to the TRIPS agreement initially led to a decline in IPR-intensive exports in treatment relative to comparison group, and the significant positive impact kicks in with delay, which seem more plausible than the earlier findings in Maskus and Yang (2018).

Second, we find that TRIPS led to almost 36% increase in IPR-intensive exports for early complied cohorts ($g = 1996$), while the agreement cause almost 38% reduction in exports for later complied cohort ($g = 2000$). The results are consistent with Egger et al. (2023), who examined a scenario with sequential trade liberalization and solve for the gravity equation to explicitly account for the incumbency effects. They found that, ceteris paribus, countries trade more if they liberalized trade earlier, and less if they liberalize later. Interestingly, cohort 1996 comprises a group of high-income countries first-complied in 1996, while cohort 2000 includes countries self-designated as developing, and that complied with the agreement in 2000. These findings compliment with Maskus and Yang (2018), who observed that policy shift towards stronger IPR protection had economically smaller effects on exports in upper-middle-income and lower-middle-income countries compared to the developed countries. Therefore, it follows that TRIPS led to a significant increase in IPR-intensive exports for high-income countries, whereas we do not observe similar gains for medium-income countries, consistent with earlier findings (Briggs and Park 2014; Maskus and Ridley 2016; Maskus and Yang 2018). Moreover, the purported benefits of IPRs are higher in countries with strong skill base and reasonable openness (Maskus 2000). The average treatment effects for other treatment cohorts are insignificant.

Third, although TRIPS-compliance reveals a significant negative impact on export flows with respect to calendar year, in our analysis, we are not particularly interested in treatment effects heterogeneity with respect to calendar year.

6.4 Robustness exercise

Evidence from knowledge-intensive sectors

In this section, we discuss the robustness of baseline model by looking at the impact of TRIPS in specific industrial clusters, demonstrating increased sensitivity to intellectual property. Table 8 presents the findings on the overall impact of complying with the TRIPS agreement. The aggregate summary parameter based on simple weighted average indicate a 48.5% decrease in biopharmaceutical exports, and the cumulative impact of TRIPS adherence across all cohorts that complied with the agreement resulted in a 48.6% reduction in these exports. Similar patterns are observed in information and communications technology (ICT) and production technology. We did not find any significant impact on trade in analytical instruments and chemicals. By considering an event-study type aggregation, TRIPS agreements, however, resulted in an approximately 63.6% increase in the export of medical devices.

As outlined in section 4.2, the dynamic effects of TRIPS may become manifest with a lag, and more so in IPR-intensive industry clusters (Delgado et al. 2013). We begin by presenting the event studies for each industry cluster in Figure 5. The event study plots reveal that IPR-intensive export flows within each cluster exhibit a U-shaped response to TRIPS compliance. Consistent with our identifying assumption, none of the plots provide evidence of differential pre-TRIPS trends. Export flows in cohorts compliant with TRIPS across each industry cluster only change after the implementation of the agreement. Table 9 reports the post-TRIPS dynamic effects. A negative initial response of TRIPS on export flows is observed across all industry clusters. Any positive effects become manifest ten years after the agreement's initiation. For example., the instantaneous effect of TRIPS is 41.8% lower exports in analytical instruments (AI), and one year after the agreement, export flows in treated countries decreased by 40% relative to untreated countries in this sector. However, twelve years after the agreements, AI exports are almost 110% higher in

TRIPS compliant countries relative to the reference group. These substantial lagged effects of TRIPS on export flows across high-IP industry clusters are consistent with the findings of Delgado et al. (2013). Similar conclusions apply to other industry clusters.

The positive impact of TRIPS on export flows in technology-intensive industry clusters may depend on complementary investments and carefully tailored policies. Key initiatives include programs to build human capital and technical skills, ensure factor market flexibility, liberalize restrictions on international trade and investment (Maskus 2000). However, it may have taken several years for such reforms to occur in developing countries, therefore export responses in IPR-intensive industry clusters following TRIPS agreement may be observed with lag. Finally, exporters may have need time to understand the potential for exports in the foreign market. Moreover, Maskus and Yang (2018) provide evidence of a lagged impact of stronger patent protection on trade, and find that R&D-intensive exports grew between 2000 and 2005, i.e., roughly after 5-10 years of TRIPS agreements.

We now examine the group (or cohort) specific heterogeneity in treatment effects across different industry cluster. The estimates for each cohort across each industry are provided in Panel A of Table 10. We find that, the cohorts, which became TRIPS-compliant in 1995 and 1996 ($g = 1995$ and $g = 1996$), predominantly the high-income countries, exhibit a significant positive increase in the exports of biopharmaceuticals, chemicals, ICT, medical devices, and production technology. On the contrary, countries that self-designated as 'developing' and hence became TRIPS-compliant in later year, exhibit a decline in exports (see also Delgado et al. 2013, Briggs and Park 2014 Maskus and Ridley 2016, Maskus and Yang 2018, Egger et al. 2023). Second, developing country exports of medical devices increase substantially across each treatment cohort, in contrast to Delgado et al. (2013). Third, surprisingly, the average treatment effects for the last treatment cohort in our sample ($g = 2008$) are positive across all industries. However, these specific results should be interpreted with caution, for the treatment effects obtained for cohort 2008 use the not-yet treated cohort 2013 as comparison, which may not offer a valid counterfactual because of small group size and compositional changes, which can complicate their interpretation due to difference in weights (see Callaway and Sant'Anna 2021, Nagengast and Yotov 2023 for further details on this aspect).

Panel B of Table 10 reports the aggregated target parameters with respect to calendar time t. This is particularly relevant, if the interest lies in studying the heterogeneity of treatment effects over the business cycle. In the case of analytical instruments, the effect of TRIPS compliance across all cohorts that have complied with the agreement by 1998 is to lower exports by 53.9%. Likewise, TRIPS led to almost 62.9% reduction in exports of analytical instruments across all compliant cohorts by 1999, and this effect, continued through 2003. Similar trends were observed for other IPR-intensive clusters. However, in case of biopharmaceuticals, adherence to agreement resulted in 13.7% increase in exports across all treatment groups that had complied with the agreement by 1996. The average treatment effects of the policy across all groups until time $\tilde{t}$ follows a similar interpretation.

7 Conclusion

The impact of TRIPS induced IPR reforms on the composition and geography of trade has garnered significant attention in the previous literature. The theoretical and empirical analyses have not conclusively established whether stronger IPR protection facilitates trade. Extensive empirical studies on the relationship between trade and IPR protection, employing different econometric approaches and data generating processes, have yielded ambiguous findings. This left ample room for improvement, and our study has attempted to plug that gap.

In this study, we conjecture that the effect of IPR reform following the TRIPS agreement likely had a heterogeneous impact across several dimensions: (i) the average treatment effects may-increase/decrease with respect to event time, and (ii) groups complying with the agreement relatively earlier may have higher/lower treatment effects than groups that comply later, suggesting the importance of compliance timing. Recent econometric papers have demonstrated that, if the treatment effects are heterogeneous, and intervention across groups occurs at different point in time, the estimates derived from TWFE estimator are biased and not causally interpretable. We exploit the variation occurring from differential timing of TRIPS rollout across 123 countries that eventually complied with the TRIPS agreement during the period of 1990-2010, to examine its impact on the level of IPR-intensive export flows. Our estimation approach employs Callaway and

Sant'Anna (2021) difference-in-differences procedure, which yields unbiased estimates in settings when treatment effects are heterogeneous and there is variation in treatment timing.

Computing a Goodman-Bacon (2021) decomposition of the DiD effects of TRIPS induced IPR reform, we demonstrate that the two-way fixed effects estimates derived from the conventional DiD estimation in the received literature, are contaminated by a series of 'forbidden' or inappropriate comparisons. If we consider only the 'admissible' comparisons, and compute the corresponding TWFE estimate using rescaled weights, we find a negative relationship between developing countries exports and IPR reform. To overcome the limitations of TWFE, we employ the heterogeneity-robust DiD estimator proposed by Callaway and Sant'Anna (2021), our estimates reveal significant negative response of TRIPS compliance on IPR-intensive exports. These results should not come as a surprise in line of the predictions made in Callaway and Sant'Anna (2021). Further, the partially aggregated estimates, based on event-study type aggregation, suggest that IPR-intensive exports exhibit a U-shaped response to TRIPS compliance, declining for about ten years before experiencing an increase. Moreover, compliance with the TRIPS agreement is associated with a significant increase in export flows of IPR-intensive sectors in the early complying cohorts, and a significant decline in the exports of later complying cohorts. These conclusions based on aggregate trade flows remain valid when examining the impact of TRIPS compliance on disaggregated industry clusters.

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Table 1 List of sample countries

High-Income		Developing (DC)		Least Developed (LDC)
Innovative	Other High-Inc.	Upper-middle Inc.	Lower-middle Inc.	LDC's
Australia	*Antigua & Barbuda*	Algeria	Albani	Bangladesh
Austria	*Bahrain*	*Argentina*	Armenia	Benin
Belgium	*Barbados*	Belarus	Azerbaijan	Bhutan
Canada	*Brunei*	Bosnia & Herzigovina	*Belize*	Burkina Faso
Hong Kong	Croatia	*Botswana*	*Bolivia*	Burundi
Denmark	*Cyprus*	*Brazil*	*Cameroon*	Cambodia
Finland	Czech Republic	Bulgaria	*China*	Cape Verde
France	*Estonia*	*Chile*	*Congo, Rep.*	Central African Rep
Germany	Greece	*Colombia*	*Cote d'Ivoire*	Comoros
Israel	Hungary	*Costa Rica*	Ecuador	Ethiopia
Italy	Iceland	*Dominica*	*Egypt, Arab Rep.*	Gambia
Japan	Ireland	*Dominican Rep.*	*El Salvador*	Guinea
Netherlands	*Kuwait*	*Fiji*	*Ghana*	Lesotho
Korea, Rep.	Luxembourg	*Gabon*	Georgia	Madagascar
Norway	*Malta*	*Grenada*	*Guatemala*	Malawi
Spain	New Zealand	*Jamaica*	*Guyana*	Maldives
Sweden	Oman	Kazakhstan	*Honduras*	Mali
Switzerland	Portugal	Latvia	*India*	Mauritania
United Kingdom	*Qatar*	Lebanon	*Indonesia*	Mozambique
United States	Saudi Arabia	Lithuania	Iran, Islamic Rep.	Myanmar
	Singapore	*Malaysia*	Jordan	Nepal
	Slovak Republic	*Mauritius*	*Kenya*	Niger
	Slovenia	*Mexico*	Kyrgyz Rep.	Rwanda
	Trinidad & Tobago	*Namibia*	Mongolia	Samoa
	United Arab Emirates	Panama	*Morocco*	Sao Tome & Principe
		Peru	*Nicaragua*	Senegal
		Poland	*Nigeria*	Sierra Leone
		Romania	*Pakistan*	Toga
		Russian Fed.	*Papua New Guinea*	Uganda
		St. Kitts and Nevis	*Paraguay*	Tanzania
		St. Lucia	*Philippines*	Yemen
		St. Vincent & Grenadines	Moldova	
		South Africa	*Sri Lanka*	
		Suriname	Tajikistan	
		North Macedonia	*Thailand*	
		Turkey	Tonga	
		Uruguay	*Tunisia*	
			Ukraine	
			Vietnam	
			Zimbabwe	

Note: The classification criteria for countries in High-income, Developing and Least-developed groups, respectively is based on World Bank classification. The top 20 innovative countries in the high-income group are ranked based on USPTO patents as of 1993. Countries in *italics* are those that self-designated as developing when they joined WTO.

Table 2 Definition of product categories according to IPR-intensity

A. Definition of High-IP Group	
High-patent products (most of which are also high-trademark)	
Crude fertilizer	Metalworking machinery
Organic & inorganic chemicals	General machinery
Dyeing material	Office machines
Medicinal & pharmaceutical products	Telecommunications
Essential oils & perfume materials	Electrical machinery
Chemical materials & products	Professional apparatus
Rubber manufactures	Photographic apparatus
Power-generating machinery	Miscellaneous manufacturing
Machinery for industries	
High-trademark products (with low-patent/copyrights)	
Dairy products & beverages	Manufactures of metals
Crude rubber	Road vehicles
Pulp & waste paper	Furniture
Plastics	Footwear
Paper & related articles	
High-copyright products (most of which are also high-trademark)	
Cinematographic films & beverages	Printed matter & recoded media
B. Definition of High-IP Clusters	
Biopharmaceuticals	**Medical Devices**
Medicinal & pharmaceutical products	Diagnostic substances
	Medical equipment & supplies
Analytical Instruments	**Chemicals**
Optical instruments	Organic chemicals
Laboratory instruments	Chemically based ingridients
Process instruments	Dyeing & packaged chemicals
ICT	**Production Technology**
Office machines	Materials & tools
Computer & peripherals	Process & metalworking machinery
Communication equipments	General industrial machinery
Electric & electronic components	
C. Definition of Low-IP Group	
Food & live animals	Manufactures of leather, cork, wood, minerals & metals
Crude materials, inedible, except fuel	
Mineral fuels, lubricants & related material	Prefabricated buildings, travel goods, and apparel & accessories
Animal & vegetable oils, fats & waxes	

Notes: From Delgado et al. (2013), based on ESA-USPTO reports (U.S. Department of Commerce 2012)

Table 3 TRIPS rollout aggregated treatment effect estimates based on TWFE-OLS

ATT	0.020
	(0.0486)
Country FE's	Yes
Year FE's	Yes
N	2098

Notes: The table reports overall treatment effect estimates based on regression based TWFE design in equation (2). Dependent variable is the logarithm of nominal IPR-intensive export flows. The specification included country and year fixed effects, estimates of these fixed effects are not reported for brevity. Standard errors in parentheses are clustered by country. *** p<0.01, ** p<0.05, * p<0.1 denote statistical significance at 1%, 5%, and 10% levels, respectively.

Table 4 Event study treatment effect estimates based on TWFE-OLS

$e = 0$	0.122*
	(0.0679)
$e = 1$	0.0323
	(0.0658)
$e = 2$	0.0854
	(0.0682)
$e = 3$	0.0860
	(0.0727)
$e = 4$	0.152**
	(0.0702)
$e = 5$	0.156**
	(0.0672)
$e = 6$	0.162**
	(0.0661)
$e = 7$	0.122*
	(0.0671)
$e = 8$	0.141*
	(0.0716)
$e = 9$	0.157**
	(0.0650)
$e = 10$	0.173**
	(0.0751)
Country FE's	Yes
Year FE's	Yes
N	2098

Notes: The table reports post-treatment effects of TRIPS compliance at different lengths of event horizon based on dynamic TWFE difference-in-differences design in equation (1). Dependent variable is the logarithm of nominal IPR-intensive export flows. The specification included country and year fixed effects, estimates of these fixed effects are not reported for brevity. Standard errors in parentheses are clustered by country. *, **, *** denotes $p < 0.10$, $p < 0.05$, $p < 0.01$, respectively

Table 5 Goodman-Bacon decomposition of TWFE estimates using balanced panel

(1) DD comparisons	(2) Weight	(3) Average DD estimate
Earlier (T) vs Later (C)	0.379	-0.111
Later (T) vs Earlier (C)	0.621	0.222
ATT		0.095**
		(0.0456)

T= Treatment; C = Comparison

Notes: The table reports the estimates of Goodman-Bacon decomposition across 7 timing groups using a balanced panel. The column (1) indicated the DiD comparisons with their weights in column(2) and column (3) denotes the average DiD estimate of corresponding comparison. Standard errors in parentheses are clustered by country, *, **, *** denotes $p < 0.10$, $p < 0.05$, $p < 0.01$, respectively

Table 6 Aggregate treatment effects based on Callaway and Sant'Anna (2021)

Simple weighted average(θ_w^0)	-0.229**
	(0.100)
Cohort average(θ_{sel}^0)	-0.230**
	(0.104)
Event average($\theta_{es}^0, \forall\ e \geq 0$)	-0.0831
	(0.0993)
Calendar average(θ_c^0)	-0.192**
	(0.0821)

Notes: The table reports overall treatment effects based on Callaway and Sant'Anna (2021) obtained by averaging across all event-time, cohort-time and calendar-time dimensions respectively. The results are obtained using unconditional parallel trend with not-yet complied as a comparison group. The asymptotic standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1 denote statistical significance at 1%, 5%, and 10% levels, respectively.

Table 7 TRIPS rollout partially aggregated treatment effect estimates

Unconditional parallel trends Comparison: Not-yet complied	Partially aggregated															
Event-time	e=0	e=1	e=2	e=3	e=4	e=5	e=6	e=7	e=8	e=9	e=10	e=11	e=12	e=13	e=14	
	-0.014	-0.165**	-0.186**	-0.302***	-0.252**	-0.367**	-0.266**	-0.361*	-0.332*	-0.308	-0.211	0.414*	0.397*	0.272	0.434**	
	(0.080)	(0.076)	(0.091)	(0.110)	(0.117)	(0.185)	(0.121)	(0.194)	(0.193)	(0.227)	(0.156)	(0.226)	(0.234)	(0.218)	(0.220)	
Cohort-time	g=1995	g=1996	g=1997	g=1999	g=2000	g=2001	g=2002	g=2003	g=2005	g=2006	g=2008					
	-0.062	0.361***	-0.208	0.0197	-0.381***	-0.852	-0.020	-0.177	0.0257	0.228	0.0002					
	(0.083)	(0.138)	(0.138)	(0.330)	(0.141)	(0.665)	(0.301)	(0.168)	(0.218)	(0.347)	(0.153)					
Calendar-time	t=1995	t=1996	t=1997	t=1998	t=1999	t=2000	t=2001	t=2002	t=2003	t=2004	t=2005	t=2006	t=2007	t=2008	t=2009	t=2010
	0.033	-0.0002	-0.159*	-0.160	-0.040	0.088	-0.151	-0.094	-0.276**	-0.299**	-0.507***	-0.315**	-0.306**	-0.333	-0.298	-0.261
	(0.035)	(0.067)	(0.095)	(0.109)	(0.095)	(0.145)	(0.112)	(0.134)	(0.122)	(0.136)	(0.191)	(0.134)	(0.143)	(0.246)	(0.324)	(0.348)

Note: The table reports the partially aggregated estimates based on Callaway and Sant'Anna (2021). The results are obtained using unconditional parallel trend with not-yet complied as a comparison group. The asymptotic standard errors are reported in parentheses. The row 'cohort-time' reports the average treatment effects by the timing of TRIPS roll-out; here g indexes the year a country first complied to the agreement. The row 'Event-time' reports average treatment effects since agreements initiation ; here e indexes event-time(years passed since agreement). Finally, the row 'Calendar-time' denotes the estimates based on calendar year of the treatment. *** p<0.01, ** p<0.05, * p<0.1 denote statistical significance at 1%, 5%, and 10% levels, respectively.

Table 8 Aggregate treatment effects across IPR-intensive industry clusters

	(1) Analytical inst.	(2) Biopharmaceuticals	(3) Chemicals	(4) ICT	(5) Medical devices	(6) Production tech.
Simple weighted avergage	-0.320	-0.485*	-0.440	-0.500*	0.058	-0.491*
	(0.209)	(0.283)	(0.305)	(0.267)	(0.162)	(0.273)
Event average	0.018	-0.128	0.349	-0.223	0.636***	-0.197
	(0.188)	(0.242)	(0.300)	(0.251)	(0.142)	(0.211)
Cohort average	-0.305	-0.486*	-0.457	-0.459*	0.070	-0.483*
	(0.203)	(0.273)	(0.279)	(0.256)	(0.151)	(0.268)
Calendar average	-0.293	-0.362*	-0.343	-0.409*	0.029	-0.378*
	(0.179)	(0.217)	(0.247)	(0.217)	(0.139)	(0.225)
N	1633	1703	1776	1759	1658	1799

Note: The table reports overall treatment effects across IPR-intensive industry clusters based on Callaway and Sant'Anna (2021). The results are obtained using unconditional parallel trend with not-yet complied as a comparison group. The asymptotic standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1 denote statistical significance at 1%, 5%, and 10% levels, respectively.

Table 9 Event study estimates across IPR-intensive industry clusters

	(1) Analytical inst.	(2) Biopharmaceuticals	(3) Chemicals	(4) ICT	(5) Medical devices	(6) Production tech.
$e = 0$	-0.418***	-0.327**	-0.354*	-0.123	0.084	-0.094
	(0.158)	(0.166)	(0.209)	(0.224)	(0.119)	(0.174)
$e = 1$	-0.406*	-0.417*	-0.531**	-0.399	-0.079	-0.398*
	(0.217)	(0.227)	(0.237)	(0.251)	(0.171)	(0.222)
$e = 2$	-0.447*	-0.429	-0.463	-0.438*	0.109	-0.509**
	(0.261)	(0.269)	(0.291)	(0.265)	(0.207)	(0.252)
$e = 3$	-0.595**	-0.510	-0.789*	-0.829**	0.036	-0.690**
	(0.261)	(0.341)	(0.403)	(0.338)	(0.218)	(0.289)
$e = 4$	-0.424	-0.571	-0.554	-0.639*	-0.151	-0.612**
	(0.288)	(0.368)	(0.400)	(0.344)	(0.224)	(0.296)
$e = 5$	-0.547*	-1.022	-0.976	-1.081**	-0.045	-0.940**
	(0.289)	(0.650)	(0.613)	(0.505)	(0.278)	(0.422)
$e = 6$	-0.162	-0.646**	-1.375*	-0.727	-0.025	-0.767
	(0.270)	(0.255)	(0.733)	(0.505)	(0.295)	(0.519)
$e = 7$	-0.363	-0.543**	-1.103	-0.959*	-0.125	-0.886
	(0.374)	(0.258)	(0.719)	(0.583)	(0.307)	(0.622)
$e = 8$	-0.343	-0.559	-0.0915	-0.234	-0.134	-0.661
	(0.392)	(0.377)	(0.279)	(0.332)	(0.201)	(0.548)
$e = 9$	-0.450	-0.398	0.300	-0.389	-0.022	-0.310
	(0.357)	(0.375)	(0.211)	(0.397)	(0.259)	(0.472)
$e = 10$	0.289	-0.448	0.225	0.017	0.202	0.130
	(0.325)	(0.513)	(0.210)	(0.324)	(0.286)	(0.214)
$e = 11$	0.743	0.568	0.717	0.979***	1.506***	0.464**
	(0.474)	(0.450)	(0.544)	(0.370)	(0.358)	(0.197)
$e = 12$	1.109***	0.821**	2.594***	0.550	1.914***	0.119
	(0.268)	(0.335)	(0.789)	(0.855)	(0.299)	(0.285)
$e = 13$	1.098***	1.039***	3.002***	0.119	2.845***	1.004***
	(0.314)	(0.317)	(0.844)	(0.627)	(0.280)	(0.325)
$e = 14$	1.188***	1.526***	4.639***	0.814*	3.430***	1.197***
	(0.432)	(0.216)	(0.779)	(0.482)	(0.501)	(0.204)
N	1633	1703	1776	1759	1658	1799

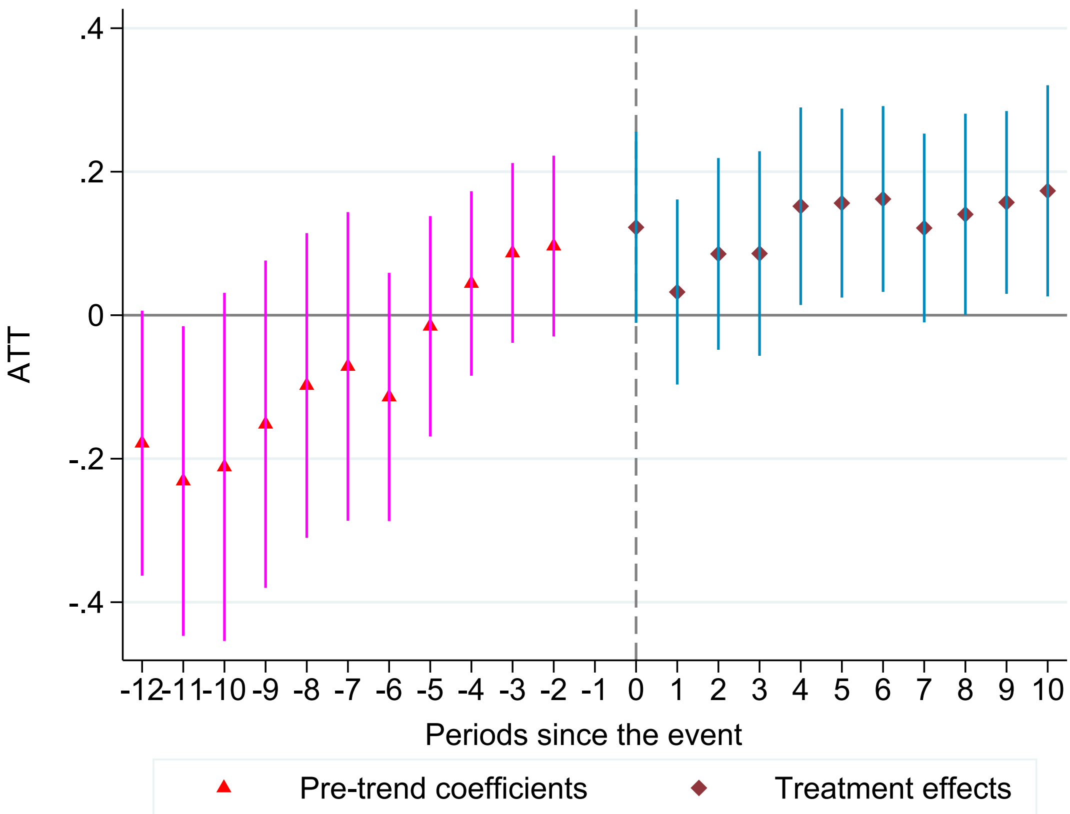
Note: The table reports the treatment effect estimates by the length of exposure to the treatment across IPR-intensive industry clusters based on Callaway and Sant'Anna (2021); here e indexes event-time(years passed since TRIPS). The results are obtained using unconditional parallel trend with not-yet complied as a comparison group. The asymptotic standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1 denote statistical significance at 1%, 5%, and 10% levels, respectively.

Table 10 Cohort-time specific and Calendar-time specific effects

	(1) Analytical inst.	(2) Biopharmaceuticals	(3) Chemicals	(4) ICT	(5) Medical devices	(6) Production tech.
			(A) Cohort-time			
$g = 1995$	-0.232	-0.019	0.294***	-0.209	-0.270	-0.178
	(0.154)	(0.196)	(0.113)	(0.160)	(0.206)	(0.123)
$g = 1996$	-0.018	0.562**	1.734**	0.638**	0.927**	0.316**
	(0.247)	(0.250)	(0.732)	(0.277)	(0.380)	(0.150)
$g = 1997$	0.175	-0.896**	-0.346	-1.206	0.858**	0.221
	(0.362)	(0.390)	(0.447)	(0.808)	(0.424)	(0.293)
$g = 1999$	-0.072	-0.618	-0.848	0.090	0.729***	-0.403
	(0.768)	(0.704)	(0.703)	(0.770)	(0.262)	(0.534)
$g = 2000$	-0.426	-1.006**	-1.234**	-1.034*	-0.124	-0.778*
	(0.349)	(0.486)	(0.578)	(0.530)	(0.246)	(0.428)
$g = 2001$	-1.370*	-0.206	-0.163	-0.507	-0.416	-1.908
	(0.819)	(0.154)	(0.657)	(0.906)	(0.545)	(1.684)
$g = 2002$	-0.292	-0.183	-0.472	0.286	0.452*	0.369
	(0.467)	(0.303)	(0.497)	(0.479)	(0.233)	(0.378)
$g = 2003$	0.524	-0.383	-1.141**	-0.524***	1.216**	0.058
	(0.530)	(0.457)	(0.535)	(0.172)	(0.604)	(0.285)
$g = 2005$	0.512	-0.366	-0.573	0.849**	0.386*	0.116
	(0.358)	(0.259)	(0.689)	(0.402)	(0.219)	(0.211)
$g = 2006$	-0.861	-0.281	0.413	1.439*	1.028	0.272
	(1.567)	(0.303)	(0.473)	(0.795)	(0.967)	(0.250)
$g = 2008$	0.464*	0.228**	1.475	0.159*	0.760***	0.295**
	(0.259)	(0.113)	(0.899)	(0.085)	(0.178)	(0.129)
			(B) Calendar-time			
$t = 1995$	0.017	-0.011	-0.055	-0.076	-0.121	0.033
	(0.137)	(0.080)	(0.058)	(0.064)	(0.137)	(0.072)
$t = 1996$	-0.025	0.137*	-0.056	0.003	-0.153	0.201
	(0.194)	(0.077)	(0.112)	(0.110)	(0.152)	(0.139)
$t = 1997$	-0.236	0.014	0.090	-0.071	-0.230	-0.060
	(0.184)	(0.086)	(0.166)	(0.143)	(0.280)	(0.114)
$t = 1998$	-0.539***	0.043	0.155	-0.371*	-0.070	-0.162
	(0.188)	(0.101)	(0.178)	(0.199)	(0.216)	(0.115)
$t = 1999$	-0.629***	0.310*	0.107	-0.161	-0.145	-0.006
	(0.195)	(0.173)	(0.287)	(0.227)	(0.225)	(0.189)
$t = 2000$	-0.454*	-0.428*	-0.030	-0.305	0.289	-0.042
	(0.235)	(0.252)	(0.325)	(0.318)	(0.201)	(0.296)
$t = 2001$	-0.570*	-0.515	-0.419	-0.475	-0.248	-0.531*
	(0.298)	(0.314)	(0.331)	(0.291)	(0.292)	(0.296)
$t = 2002$	-0.498	-0.503	-0.170	-0.482	0.223	-0.565
	(0.054)	(0.363)	(0.376)	(0.307)	(0.369)	(0.358)
$t = 2003$	-0.744***	-0.621	-0.624	-0.904***	-0.063	-0.619**
	(0.278)	(0.455)	(0.434)	(0.348)	(0.282)	(0.300)
$t = 2004$	-0.520	-0.601	-0.478	-0.922***	-0.208	-0.916***
	(0.358)	(0.509)	(0.476)	(0.344)	(0.197)	(0.337)
$t = 2005$	-0.491	-1.247	-0.929	-1.093*	-0.283	-1.159***
	(0.368)	(0.957)	(0.717)	(0.592)	(0.353)	(0.432)
$t = 2006$	0.193	-0.633**	-2.184**	-0.665	-0.096	-0.962
	(0.282)	(0.317)	(1.001)	(0.617)	(0.331)	(0.650)
$t = 2007$	-0.0878	-0.613*	-1.729**	-1.057	-0.300	-0.895
	(0.341)	(0.322)	(0.874)	(0.661)	(0.308)	(0.651)
$t = 2008$	0.008	-0.624	-0.128	0.286	0.253	-0.605
	(0.474)	(0.508)	(0.385)	(0.338)	(0.223)	(0.671)
$t = 2009$	-0.083	-0.381	0.346	-0.326	0.718**	-0.032
	(0.430)	(0.473)	(0.331)	(0.463)	(0.315)	(0.520)
$t = 2010$	-0.028	-0.116	0.613**	0.076	0.904***	0.272
	(0.483)	(0.479)	(0.243)	(0.474)	(0.253)	(0.445)
N	1633	1703	1776	1759	1658	1799

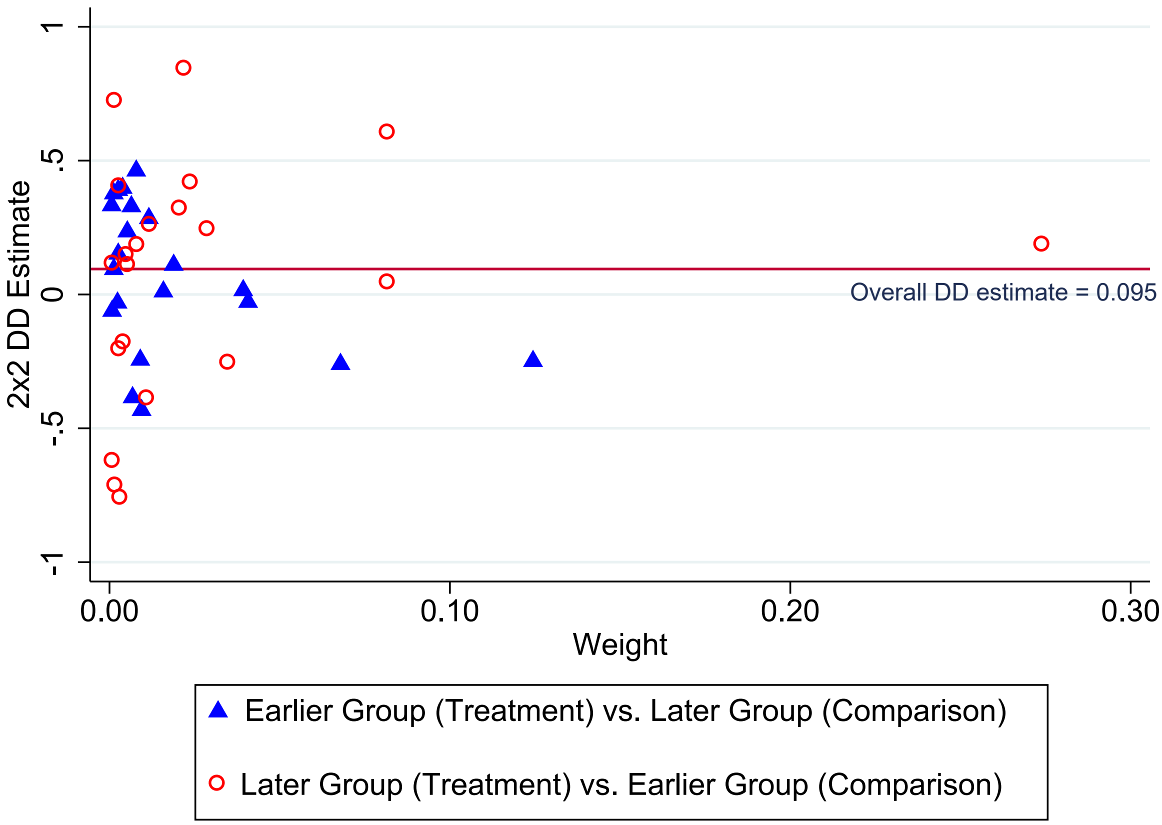
Note: The table reports the partially aggregated cohort and calendar time specific estimates across different IPR-intensive industry clusters based on Callaway and Sant'Anna (2021). The results are obtained using unconditional parallel trend with not-yet complied as a comparison group. The asymptotic standard errors are reported in parentheses. The results in panel (A) report cohort-specific $ATT's$; here g indexes the year a country first complied to the agreement, whereas the results in panel (B) reports the estimates based on calendar year of the treatment denoted by t. *** p<0.01, ** p<0.05, * p<0.1 denote statistical significance at 1%, 5%, and 10% levels, respectively.

Figure 1 Event study estimates based on dynamic TWFE design.



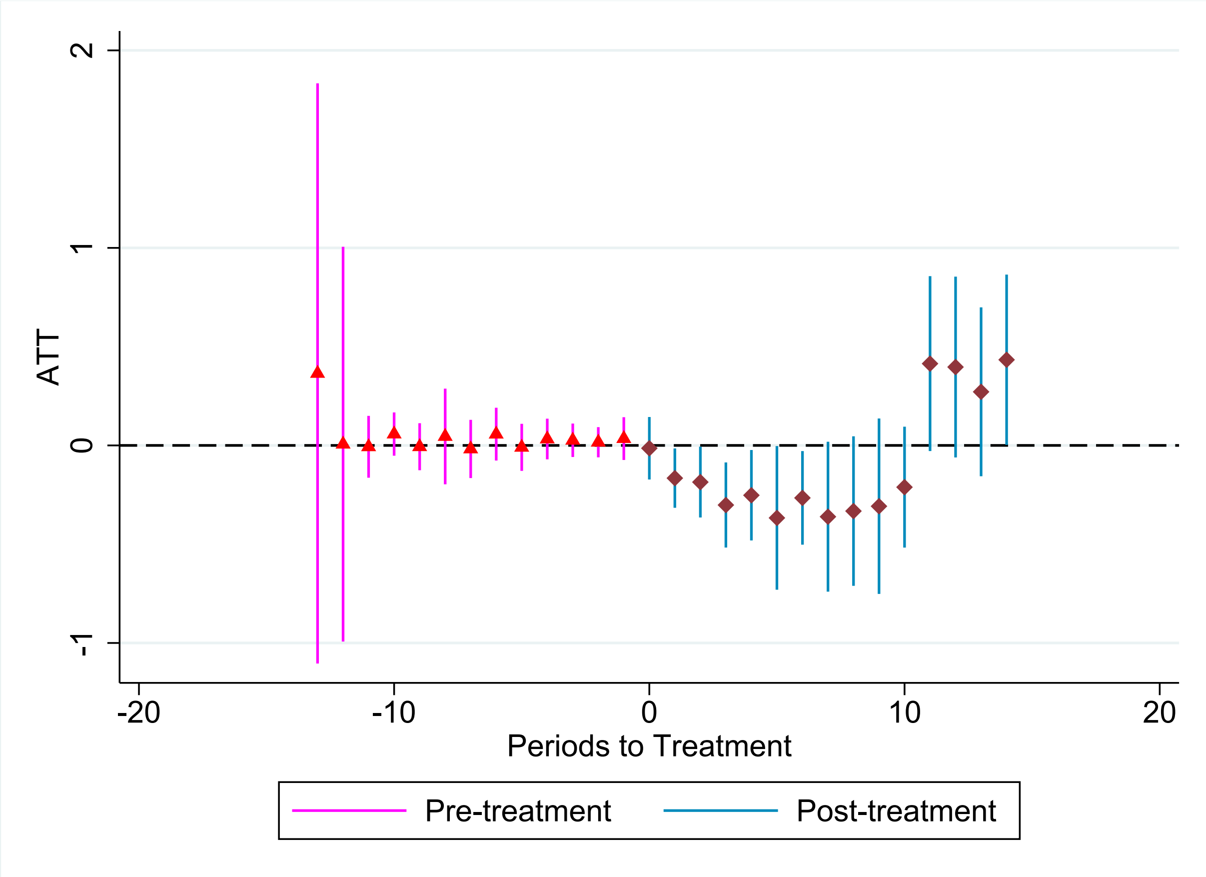
Note: The figure shows the event study estimates based on dynamic TWFE design from equation (1). The outcome variable is the logarithm of nominal IPR-intensive export flows. The first-difference placebo estimates are denoted by red triangles, maroon diamonds denote post-treatment effects and whiskers denote 95% confidence interval. The estimates are obtained by clustering standard errors at country level.

Figure 2 Goodman-Bacon decomposition of TWFE coefficients



Note: The figure plots the series of possible 2×2 difference-in-differences (DiD) estimates against their weights from the decomposition theorem in Goodman-Bacon (2021). The blue triangles indicate the DiD estimates obtained using legitimate comparisons (early vs. late), whereas open circles in red indicate the estimates obtained using forbidden comparisons (late vs. early). The overall ATT of 0.095 is obtained as averages of y-axis values weighted by their x-axis values.

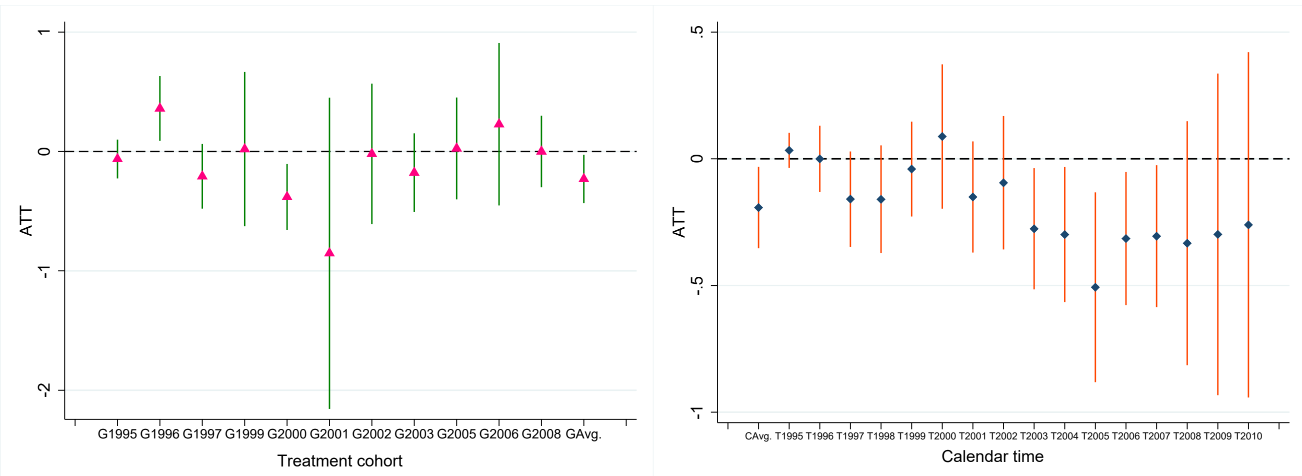
Figure 3 Event study estimates



Note: The figure plots the event study estimates based on Callaway and Sant'Anna (2021) using the aggregation from equation (10). The outcome variable is the logarithm of nominal IPR-intensive export flows and whiskers denote the 95% confidence intervals.

Figure 4 Cohort-specific and Calendar-time specific treatment effects

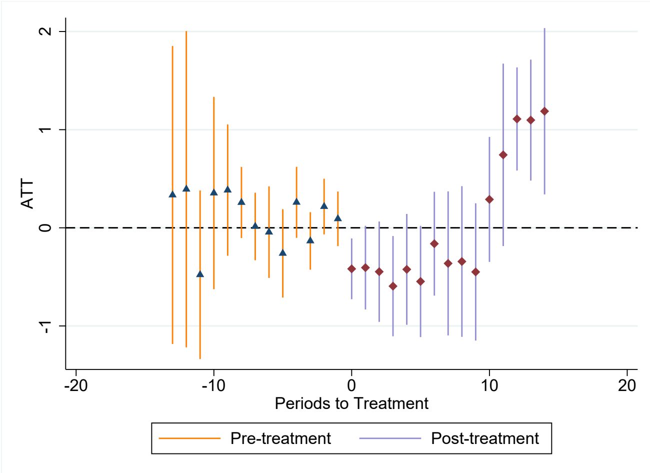
(a) By cohorts (b) By calendar-time



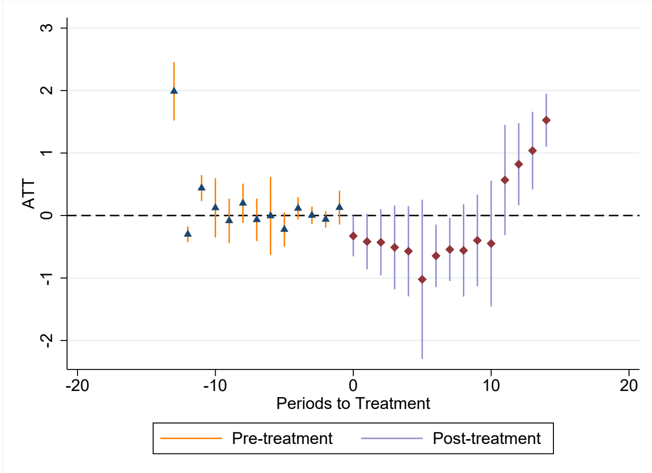
Note: The figure reports different aggregations of cohort-time specific (panel a) and calendar-time specific (panel b) treatment effects from equation (8) and (12), respectively, using the heterogeneity-robust DiD estimator proposed by Callaway and Sant'Anna (2021). The outcome variable is the logarithm of nominal IPR-intensive export flows and whiskers denote the 95% confidence intervals.

Figure 5 Event study estimates across IPR-intensive industry clusters

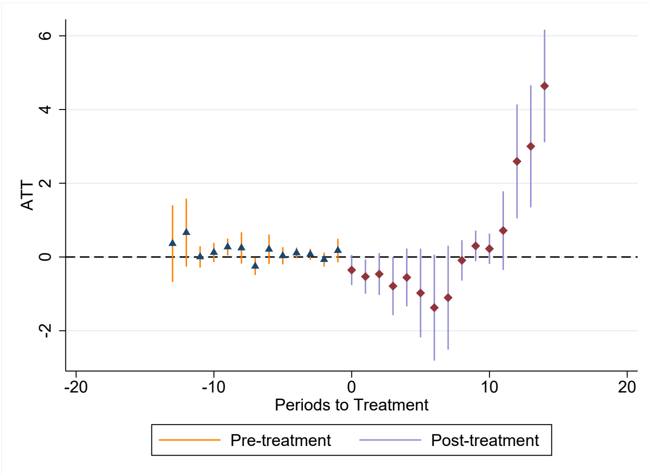
(a) Analytical instruments



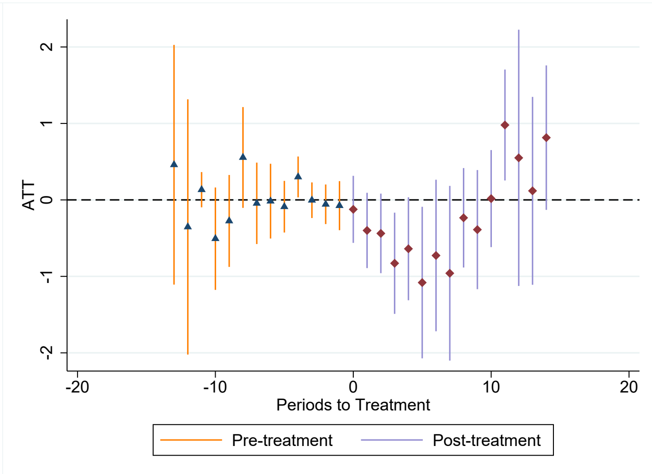
(b) Biopharmaceuticals



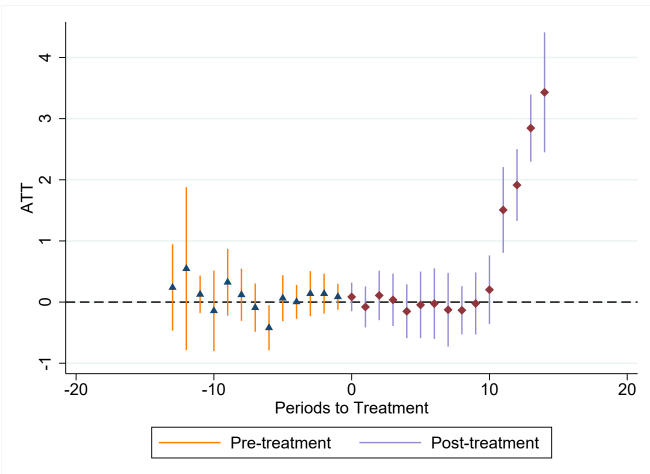
(c) Chemicals



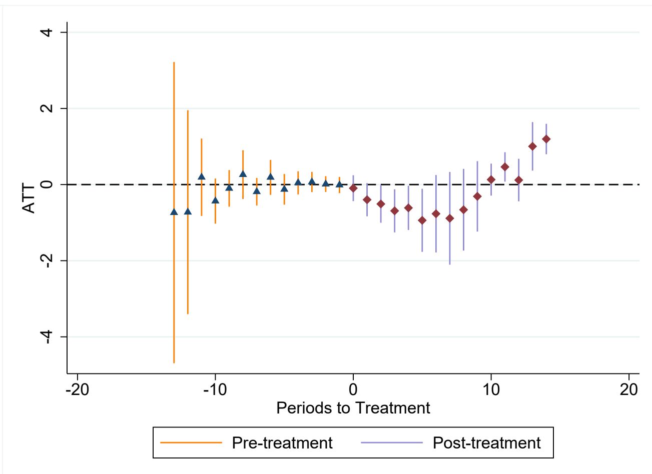
(d) ICT



(e) Medical devices



(f) Production technology



Note: The figure reports the event study estimates across different IPR-intensive industry clusters using the heterogeneity-robust DiD estimator proposed by Callaway and Sant'Anna (2021). The outcome variable is the logarithm of nominal exports in each industry cluster and whiskers denote the 95% confidence intervals.