Journal of International Business Studies
https://doi.org/10.1057/s41267-026-00868-5

EDITORIAL

Heterogeneous treatment effects in international business research: potential issues and practical recommendations

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Over the past decade, the domain of international business (IB) has witnessed growing efforts to enhance the rigorousness of causal identification in observational IB studies in which samples are not randomized. While such efforts have improved substantially the empirical robustness of IB studies, it is worth noting that these efforts have mostly focused on combating endogeneities as the primary issue that would bias causal identification. In contrast, *heterogeneous treatment effects* (*HTEs*), another major source of identification biases long recognized by econometrics scholars, have been largely overlooked in current IB studies. For instance, when combing through the extant IB research, we discovered that while 107 out of the 197 observational studies (54.3%) published in the *Journal of International Business Studies* (*JIBS*) since 2020 diligently adopted at least one advanced endogeneity-alleviating estimator (e.g., DID designs, IV designs, Heckman selection model, and dynamic panel regression), only one study took concrete precautions to gauge the presence of HTEs and account for the potential biases they may cause.

This is a nontrivial omission that warrants immediate attention of IB scholars who primarily adopt observational research designs. Observational studies strive to identify causal effects from nonrandomized secondary data to provide implications for broader populations or settings (Frake, Gibbs, Goldfarb, Hiraiwa, Starr, & Yamaguchi, 2025). For such extrapolation to be valid for a given treatment effect, a central assumption is that this treatment effect ought to remain homogenous for all samples. However, this assumption is often unmet, as the treatment effects generally vary across different observations due to the inherent individual and temporal specificities in practice. As cautioned by recent econometrics works (e.g., Callaway & Sant'Anna, 2021; de Chaisemartin & D'Haultfœuille, 2018), such HTEs diminish the rigorousness of most identification strategies adopted in the extant IB research, such as two-way fixed effects (TWFE) designs and instrumental variable (IV) regressions, as these estimators essentially recover the average treatment effects (ATEs) across all observed samples and extrapolate the findings into generalizable prescriptions based on the assumption of treatment homogeneity.

More importantly, the presence of HTEs tends to be particularly prevalent in IB research. Compared with other business and management contexts, nonrandomized observational samples in IB contexts are inherently characterized by more complicated specificities at multiple levels, such as the locational specificities across countries and regions, the industrial specificities across value chain sectors, and the

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Published online: 29 April 2026

organizational specificities in the inter- and intrafirm operations of multinational enterprises (MNEs; Jiang, Luo, Xia, Hitt, & Shen, 2023). These specificities may render a treatment effect significantly varying across different samples for two primary reasons. First, in practice, a seemingly universal treatment may manifest in idiosyncratic ways for samples in different locations or industries in terms of the treatment's intensity, frequency, timing, duration, and so on. For example, the contents and intensities of national law enactments on mandatory auditing or foreign M&As, which are widely studied by IB scholars as treatment effects (Cheng, Sun, Ye, & Zhang, 2020; Glendening, Khurana, & Wang, 2016), can vary substantially in different countries. Even for treatments based on iconic shocks (e.g., 9/11, the COVID pandemic, and the 2008 Global Economic Crisis), the manifestations and mechanisms of their influences can vary across samples with multilevel specificities. For example, the 2018 Sino–U.S. trade war strengthened the economic dependence between the low-tech industrial sectors in these two countries but attenuated such interdependence in high-tech sectors (e.g., Fan et al., 2024). As previously discussed, such heterogeneities in treatment effects in IB contexts can largely bias the identification rigorousness of observational IB studies and diminish the validity of theoretical prescriptions based on the extrapolation of these estimations.

Against this back, we strive to unveil the mechanisms underlying HTE biases and their implications for IB studies, showcase such biases in representative IB settings, and offer practical guides to combat such biases in the spirit of enhancing the rigorousness of observational IB studies.

Heterogeneous treatment effects and estimation biases

In theory, the individual effect of a treatment, D, on a sample i, τ_i, can be stated as follows:

$$\tau_i = Y_{1i} - Y_{0i},$$

where τ_i refers to the treatment received by i, Y_{1i} is the predicted effect of i after being treated, and Y_{0i} is that of i without receiving that treatment[1]. The average treatment effect across all samples i (i.e., $\tau = E\left(Y_{1i} - Y_{0i}\right)$), offers the unbiased treatment estimator. Notably, in observational studies, for a sample i, Y_{1i} and Y_{0i} cannot be observed at the same time, rendering it impractical to directly obtain the above estimator due to the lack of counterfactual. Instead, scholars generally adopt the potential outcome framework in which the potential outcome of sample i, Y_i, is specified as follows:

$$Y_i = Y_{1i}D_i + Y_{0i}\left(1 - D_i\right),$$

where Y_{1i} is the outcome of i if treated, Y_{0i} is the outcome of i if not treated, and D_i indicates whether i receives the treatment. Accordingly, the ATE is estimated as follows:

$$\hat{\tau} = E\left(Y_i|D_i = 1\right) - E\left(Y_i|D_i = 0\right) = E\left(Y_1|D = 1\right) - E\left(Y_0|D = 0\right),$$

where $D = 1$ indicates the treatment group, and $D = 0$ indicates the control group. If the treatment is randomly assigned across all samples and is homogeneous across all groups, the potential outcome of the treatment group $\left(Y_i|D_i = 1\right)$ and the control group $\left(Y_i|D_i = 0\right)$ ought to be comparable under the same treatment condition, that is $E\left(Y_1|D = 1\right) = E\left(Y_1|D = 0\right)$, and $E\left(Y_0|D = 1\right) = E\left(Y_0|D = 0\right)$. As such, the control group offers valid counterfactuals, making $\hat{\tau}$ the unbiased estimation of the ATE.

However, if the individual treatment effects τ_i are systematically heterogeneous across the samples or groups with different features, the estimand $\hat{\tau}$ would be potentially biased. Specifically:

$$\begin{aligned}\hat{\tau} &= E\left(Y_1|D = 1\right) - E\left(Y_0|D = 0\right)\\ &= \left(E\left(Y_1|D = 1\right) - E\left(Y_0|D = 1\right)\right) + \left(E\left(Y_0|D = 0\right) - E\left(Y_0|D = 1\right)\right)\\ &= \tau + \left(E\left(Y_0|D = 0\right) - E\left(Y_0|D = 1\right)\right).\end{aligned}$$

In this case, $E\left(Y_0|D = 0\right) - E\left(Y_0|D = 1\right)$ represents the systematic heterogeneities across the potential outcomes of the treatment group when not treated and that of the control group.

In practice, HTEs can arise from two key sources, i.e., the variation in samples' compliance conditions and the cross-sample idiosyncrasies. First, samples in an observational study may fall into four mutually exclusive groups based on their compliance conditions, i.e., *compliers* that take the treatment during the study period *t1* ($D_{i,t0} = 0, D_{i,t1} = 1$), *always-takers* that constantly take the treatment ($D_{i,t0} = D_{i,t1} = 1$), *never-takers* that constantly refuse the treatment ($D_{i,t0} = D_{i,t1} = 0$), and *defiers* that take the treatment before the study period but refuse it during the study period ($D_{i,t0} = 1, D_{i,t1} = 0$). In each group, the specific executions of the treatment received by samples can also vary in terms of strength and content. Second, samples with different ascribed specificities may respond to a treatment in different ways: Not only can some samples be less responsive or counteractive to this treatment, but their responses to the treatment may also be driven by idiosyncratic mechanisms.

We can illustrate such a bias caused by HTEs with the following OLS breakdown:

[1] While we refer the treatment as dichotomous to facilitate illustration, all mechanisms in our illustrations shall apply to any ordered (scale or continuous) treatment, for which the ATE is estimated based on the difference between different levels of treatments.

$$Y_i = \alpha + \tau D_i + \beta X_i + \gamma \left(D_i \times X_i \right) + \epsilon_{i,t},$$

where τ is the part of treatment effect that remains homogeneous across i, and X_i refers to covariates that capture the heterogeneities of i. The heterogeneous treatment effect, τ_i manifests as $\tau + \gamma X_i$. As such, if we ignore such HTEs and conduct causal estimation under the (unmet) homogeneous treatment effect assumption as follows (assuming all covariates X_i are appropriately controlled):

$$Y_i = \alpha + \tau D_i + \beta X_i + \varepsilon_{i,t}$$

then the estimated ATE, $\hat{\tau}$, would manifest as follows:

$$\hat{\tau} = \tau + \gamma \frac{\text{Cov}\left(D_i, X_i \right)}{Var\left(D_i \right)}.$$

In this case, heterogeneities in the treatment effects exist when γ is nonzero and $D_i$and X_i are not fully independent. Notably, when occurring in experimental studies, such HTEs can be effectively subsumed into the error variance through randomization, making $\hat{\tau}$ an unbiased estimand of ATEs. However, in observational studies wherein samples cannot be randomized, $\hat{\tau}$ would inherently fail to provide unbiased estimation for the authentic treatment effect in the presence of such HTEs.

Take a classic debate in IB and international economics research, i.e., the treatment effects of tariffs on domestic firms' R&D investments, as an example. While such treatment effects have been widely documented as positive, they are also characterized by industrial heterogeneities. For instance, less developed domestic industries can benefit more from the protective effects of tariffs in terms of increasing R&D investments (Lederman, 2010; Reitzes, 1991). We thus assume γ to be -0.5 in line with the industry-level HTEs, τ to be 1, the covariance between tariffs and the level of development in a local industry i (X_i) to be -0.2 (as higher tariffs are more likely to be applied to protect less developed local industries), and the variance of tariffs across industries to be 0.2. Consequently, the estimated ATE, $\hat{\tau}$, equals to $1 + (-0.5)(-0.2/0.2) = 1.5$, significantly deviates from the true ATE.

Notably, in the above illustration, the HTE and the consequent biases echo the endogeneity biases caused by nonrandomized treatment assignments. In practice, IB scholars generally adopt two representative identification strategies to combat such endogeneity (i.e., TWFE estimators, also referred to as TWFE DID design) and IV regression. However, the presence of HTEs would also bias the estimations of these two identification designs. In the next section, we follow prior studies (e.g., Bertrand, Duflo, & Mullainathan, 2004; Baker, Larcker, & Wang, 2022) and conduct two *Monte Carlo* simulations (see Appendix for the full codes) to illustrate the potential estimation biases in TWFE and IV estimators in the presence of HTEs in IB contexts. Note that our simulations do not strive to replicate the findings of prior studies, as we do not have access to their original data. Instead, following the practice of prior studies of similar nature, we base our simulations on hypothetical databases that have been generated to simulate classic IB settings, and gauge the basic simulation parameters based on the findings of prior studies on similar treatment effects and in comparable settings. Doing so allows us to clearly illustrate the HTE-related biases in representative identification strategies in IB studies without losing the generalizability and practical relevance of our conclusions. Please also refer to the online appendices for the econometrics theorization of such biases.

HTE biases in IB contexts: simulated illustrations

Illustration 1: TWFE estimations of tariff increase and R&D intensity

The first illustration focuses on an industry-level treatment used in several IB studies using TWFE designs, i.e., tariff increase. We simulate the treatment effects of upward tariff shocks on domestic firms' R&D spending. Echoing the insights and findings of prior IB studies, we designate the enhanced tariff barriers in an industry to increase domestic firms' R&D investment in our simulation. That is, such increases in tariffs protect domestic firms from the competition brought on by imported foreign goods and alleviate their immediate competitive pressures, allowing them to be more long-term oriented and encouraging them to invest more on R&D activities in the spirit of enhancing competitive advantages in the long run. To ensure both simplification and real-world pertinence, we build our dataset based on the list of U.S. public firms in 2017 and 2019—a period that straddled across the outbreak of Sino–U.S. trade war in 2018 when many domestic industries and firms in the U.S. experienced enhanced tariff protections. The list is extracted from *Compustat*. In line with such underpinning, we perform the following data generation process:

$$RDI_{i,t} = \tau \times TariffShock_{i,t} + \beta_i + \mu_t + \varepsilon_{i,t},$$

where $RDI_{i,t}$ is the simulated R&D investments made by firm i in year t, and $TariffShock_{i,t}$ indicates the treatment condition of the industry of firm i was increased at year t. Echoing the specification of the TWFE estimator, β_i and μ_t represent the firm and year fixed effects, respectively. Additionally, $\varepsilon_{i,t}$ is the random error term following a normal distribution. The sampled firms belonged to 90 industries at the SIC 2-digit level. We set the tariff shock as exogenous by randomly designating half of the 90 sample industries as the treatment group experiencing tariff increase and the rest as the control group without such tariff shocks. Based on this setup, we compose two hypothetical treatment effects of tariff increases to illustrate the TWFE estimation biases caused by HTEs of different forms. Using each hypothetical treatment, we use Monte Carlo simulations to create 1,000 simulated datasets. For each simulated dataset, we use the TWFE design to estimate the ATE of tariff increases on treated firms' RDI.

In Simulation 1–1, we construct a simplified hypothetical treatment effect to highlight how even modest levels of HTEs can induce bias in pooled OLS and regular TWFE estimators. The treatment, $\textit{TariffShock}_{i,t}$, is set to be a binary variable that is valued as 0 for all firms in year 2017, and 1 for firms in the 45 treated industries in year 2019. In line with our theoretical underpinning, we set the marginal effect of tariff increases on R&D investment, τ, to be 0.2 for all complier firms that are subject to the influences of such tariff shocks (i.e., the ATT). This value (0.2) is comparable with the average of the marginal effects of tariffs on domestic R&D in prior studies. We manipulate the HTEs in the simulation by randomly assigning 90% of firms into the treated industries and 10% of firms into the control industries as compliers. This setup, which is based on firms' compliance conditions, reflects the heterogeneities in the implications of tariff shocks in practices. That is, some firms residing in a treated industry may not be as responsive to the increased tariffs due to the lack of foreign competition in their particular product or regional markets. Likewise, the increased tariff protections in treated industries and their implications for firms' R&D may encourage firms in control industries to engage in extra R&D due to the spillover through value chains. As such, this simulation proxies the modest levels of HTEs based on only the treatment rates across groups.

In Simulation 1–2, we further introduce industrial specificities as additional sources of HTEs on top of the heterogeneous compliance conditions. Specifically, on the basis of the above setup in Simulation 1–1, we randomly assign different levels of responsiveness to the treatment of tariff shocks for different industries. For each of the 45 treated industries, we randomly draw the tariff responsiveness for all complier firms in the industry (i.e., the 90% randomly selected firms in the industry) from a uniform distribution between 0.5 and 1. For each of the 45 control industries, the tariff shock responsiveness for complier firms (i.e., the 10% randomly selected of the firms in the industry) is drawn from a uniform distribution between 0 and 0.2. This setup further intensifies the degree of HTEs by introducing extra industry-specific heterogeneities in terms of responsiveness (or susceptibility) to the influences of tariffs in addition to variances in firms' compliance conditions.

We report the statistics of the estimands of two representative estimators, pooled OLS and TWFE, based on the 1,000 simulated datasets of Simulations 1–1 and 1–2. As can be seen in Table 1a, which demonstrates the results for Simulation 1–1, the mean of the 1,000 pooled OLS estimands is 0.231 (sd = 0.189), substantially deviating from the true ATE (0.2) by 15.45%. While the TWFE estimator recovers less biased estimands than OLS across the 1,000 simulations (mean = 0.185, sd = 0.336), the TWFE estimands still significantly deviate from the true ATE by 7.60%. Such biases become even more pronounced as the HTEs become more intensive. As shown in Table 1b, in Simulation 1–2, the pooled OLS estimators average to 0.247 (sd = 0.269), which deviates from the true ATE by 23.40%. Furthermore, the TWFE estimators (mean = 0.182, sd = 0.478), while more robust than the OLS estimator, substantially biased against the true ATE by nearly 10 percent (9.05%). These results together show that even the modest levels of HTEs based on sheer variance in samples' compliance would cause major estimation bias for TWFE estimators regardless of their relatively higher robustness than regular OLS estimations (see Fig. 1 for visualization).

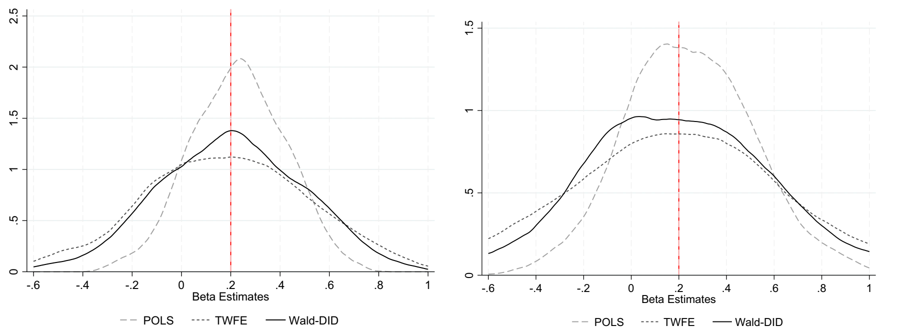
Illustration 2: IV designs of exports and firm performance

In the second illustration, we focus on another classic topic that has been widely studied in the IB literature, i.e., the treatment effects of exports for firms' performance. Prior IB studies have widely documented the positive relationships between firms' export activities and performance. That is, exporting to a foreign market not only enlarges firms' sales and revenue, but also facilitates the spillover of advanced technologies and institutions embedded in foreign markets, all of which would contribute to firms' performance. However, it is also noted that firms with better performance are better able to afford the uncertainties and coordination costs

Table 1 Results of Illustration 1

Estimator	Pooled OLS	TWFE	Fuzzy DID (Wald DID)
1a. Simulation 1–1			
True ATE	0.2	0.2	0.2
Mean of simulated estimand	0.2309	0.1848	0.2013
Bias relative to the true ATE	15.45%	− 7.60%	0.65%
SD	0.1893	0.3360	0.2897
1b. Simulation 1–2			
True ATE	0.2	0.2	0.2
Mean of simulated estimand	0.2468	0.1819	0.2003
Bias relative to the true ATE	23.40%	− 9.05%	0.15%
SD	0.2692	0.4781	0.3918

Figure 1 Visualization of Illustration 1



a Density Curves of Simulation 1-1 b Density Curves of Simulation 1-2

arising from foreign markets and thus have more export activities. In this regard, estimating the treatment effects of exports on firm performance with OLS model would be biased by endogeneity caused by such reverse causality.

We simulate such treatment effects of exports on performance by constructing a dataset using *Compustat* data from 2010 to 2019. We designate the exports of firm i in year t, $E_{i,t}$, as follows:

$$E_{i,t} = 0.1 \times ROA_{i,t-1} + 0.3 \times \eta_{j,t} + e_{i,t},$$

where $\eta_{j,t}$ is the overseas dependence of industry j in which firm i resides in year t, $ROA_{i,t-1}$ is the real-world ROA (extracted from *Compustat*) of firm i in year $t-1$, and $e_{i,t}$ is the random error term. We set the effect size of ROA on export to be comparable with the marginal effects estimated in prior studies. We randomly choose 60 of the 75 industries at 2-digit SIC level in the sample to set $\eta_{j,t}$ as:

$$\eta_{j,t} = 0.1 \times \sigma_{ROA_j} + e_{j,t},$$

where $\sigma_{ROA_{j,t-1}}$ is standard deviation across real-world ROA of all firms in industry j. For the other 15 industries, $\eta_{j,t}$ is set as 0. Based on this definition, $E_{i,t}$ is set to be hinged on firm's past performance, and $\eta_{j,t}$ is a relevant and exogenous industry-level IV for $E_{i,t}$ with a modest portion of never-takers (20%). Such HTE turns the two-stage estimators based on this instrument to LATE.

Based on above setup of exports, we then simulate the ROA of firm i in year t as follows:

$$\widetilde{ROA_{i,t}} = 0.8 \times ROA_{i,t-1} + 0.5 \times E_{i,t} + \beta_i + \mu_t + \varepsilon_{i,t},$$

where β_i is the firm-fixed effect, μ_t is the year-fixed effect, and $\varepsilon_{i,t}$ is the residual. This model setup designates the treatment effects of exports on performance to be 0.5. However, as $\widetilde{ROA_{i,t}}$ and $E_{i,t}$ are both determined by $ROA_{i,t-1}$, OLS estimation based on this setup will be biased by endogeneities. Based on the above setup, we use Monte Carlo simulations to create 1,000 simulated datasets. For each simulated dataset, we estimate the ATE of exports on firms' ROA using three representative models: pooled OLS, fixed-effect regression, and two-stage regression. We construct the IV as:

$$Trad\_IV_{i,t} = \frac{1}{M_j-1} \sum_{i,k \in j, k \neq i} E_{k,t},$$

where peer firm k belongs to industry j, and M_j is the total number of firms in industry j. This IV design, widely adopted in prior IB studies, calculates the average export volume of all peer firms in the same industry as the focal firm. The rationale is to instrument the focal firm's export decisions with industry-specific trends, which aligns with our simulation design.

Table 2, which reports the estimands of the three estimators, reveals that OLS and FE estimators utterly overestimate the true ATE (0.5) of exports on ROA due to endogeneities. In particular, for OLS, the mean of the 1000 estimands is 0.913 (sd = 0.413), which is biased against the true ATE by 82.56%. For the FE estimator, the mean of estimands is 0.908 (sd = 0.408), which is biased against the true ATE by 81.56%. More importantly, while the two-stage estimator based on the above IV design significantly mitigates the endogeneity biases of OLS and FE models (mean = 0.533, sd = 0.01), the estimator remains significantly biased against the true ATE by 6.50%. These results corroborate our deduction above and reveal that, even modest levels of HTEs can render estimators based on IV designs significantly biased (see also Fig. 2 for visualization).

Practical recommendations for IB scholars: a checklist for HTE

Enhancing the rigorousness of treatment effect estimation has become a crucial trend in the development of IB research over the past decade. However, in spite of this trend, little attention has been paid to the issue of HTEs. As highlighted above, the multilevel specificities inherent in many (if not most) IB research settings make the existence of HTEs a prevalent issue in IB studies. Such existence, which violates the assumption of homogeneous treatment effects underlying

Table 2 Results of Illustration 2

Estimator	Pooled OLS	TWFE	Traditional IV	Bartik IV
True ATE	0.5	0.5	0.5	0.5
Mean of simulated estimand	0.9128	0.9078	0.5325	0.5033
Bias relative to the true ATE	82.56%	81.56%	6.50%	0.66%
SD	0.0010	0.0010	0.0100	0.0031

mainstream identification strategies, may potentially bias the estimations of extant IB research. By conducting theoretical deductions and simulation illustrations situated in classic IB contexts, we demonstrate that HTEs, even at modest levels, could drastically bias the estimations of representative causal estimators widely adopted in recent IB studies, such as TWFE design and IV design. These findings urge IB scholars to take rigorous precautions to detect and combat HTE biases.

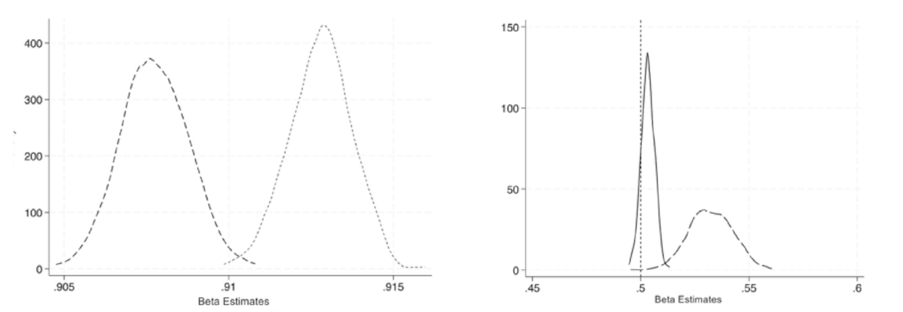
Notably, such omission of HTE issues is not specific in IB research, but prevalent across all domains in business and management research. In fact, business scholars have only recently started to focus on the issue of HTEs. The author team systematically reviewed all empirical studies published on the 50 leading business journals identified by *Financial Times* since 2000. This review indicates that, in line with our review of *JIBS* studies, only a small number of economics studies published after 2020 have specifically noted the HTE issues in their research designs. As the sources and manifestations of HTEs vary significantly across different studies, the empirical measures taken by these recent studies to account for potential HTE biases also vary significantly. Therefore, there lacks a universal guidebook for business and management researchers aiming to navigate HTE-related issues. Against this background, we strive to synthesize our findings and the insights of the extant econometrics works on HTEs to offer a brief checklist for the reference of future IB scholars in the spirit of better discerning, gauging, and remediating HTE-related biases and further improving the empirical rigorousness of IB research. Table 3 summarizes the key points of this checklist.

Step 1: Discern primary sources of HTEs

As much as we emphasize the major biases that can be caused by HTEs, it is impractical for IB scholars to completely eliminate HTEs in observational studies, which are ubiquitously inherent in numerous sources due to the intrinsic multilevel specificities in the IB contexts. Instead, the first step to account for HTE issues is to diligently delineate the primary sources of HTEs that are of particular pertinence to the empirics and theorization in their studies. Doing so requires IB scholars to accomplish two crucial tasks, namely, (1) *check for the idiosyncratic manifestations of a treatment*, and (2) *explore the potential idiosyncrasies in samples' responsiveness to the treatment.*

First, as discussed, a given treatment can often manifest on samples in different industries, countries, or regions in idiosyncratic ways, which can bring about HTEs in terms of the compliance conditions and treatment executions across these samples. A recent example that embodies such idiosyncrasies is the heterogeneities in the tariffs imposed by the U.S. administration during the 2025 global trade war. For example, different tariff rates were applied to different sectors and countries. Moreover, the tariffs on certain products were even lowered amid the systematic tariff raises. Another case of particular pertinence in light of the surging geopolitical conflicts nowadays is the legislation of governmental interventions on foreign investments for national security concerns, the contents and stages of which are known to vary substantially across countries (Dikova, Sahib, & van Witteloostuijn, 2010). In both examples, simply examining the global trade wars in 2025 or the legislation of FDI interventions as the uniform treatments for different samples will

Figure 2 Visualization of Illustration 2



a Density Curves of OLS and TWFE b Density Curves of Traditional and Bartik IV

Table 3 Checklist for HTEs in IB Research

1. Discern primary sources of HTEs	*1. Discern primary sources of HTEs* a. Track possible variances in the manifestations of a given treatment for different samples. – Different compliance conditions: Treatment time and cross-sectional dynamics – Different treatment executions (e.g., contents, intensity, and treatment rates) b. Highlight potential variances in the responsiveness of different samples to the treatment. – Different levels of responsiveness (potential boundary conditions/moderators) – Different responsive mechanisms (potential mediation paths)
2. Account for HTEs in analyses	*2. Account for HTEs in analyses* a. Gauge the face validity of the measure for the treatment. – Adopt measures that could afford more variances (e.g. using continuous and non-staggered measures for quasi-experimental shocks in TWFE designs). – Adopt and combine multiple measures that could tackle the potential HTEs from different angles (e.g., using multiple instruments in IV designs). b. Control for variables that could serve as theoretically sound boundary conditions, even if those are not actually theorized as moderators. c. Diligently control for fixed effects to account for specifies at locational, industrial, time, or organizational levels, which could be the ultimate sources of HTEs in IB studies.
3. Use advanced estimation tools	*3. Use advanced estimation tools* a. Try and use quasi-parametric approaches (e.g., causal forest analyses) to better detect HTEs. b. Try and adopt advanced alternative estimators (e.g., Fuzzy DID and Bartik IV). c. Always choose robust identification strategies (e.g., TWFE and IV designs) over sheer OLS or FE regressions, as they are less susceptible to potential HTE biases.

inevitably create HTE biases in causal inference. Such biases essentially embody a unique form of measurement error, which cannot be directly resolved by regular identification strategies (Imai & Yamamoto, 2010). As such, proactively teasing out the heterogeneities in treatment manifestations is crucial to ensure the face validity of the empirical operations of the treatments in observational IB studies.

Likewise, IB scholars should also diligently check for the potential idiosyncrasies in samples' responsiveness to a given treatment in terms of the intensities and reasons of their responses. Let us consider once again the treatment effects of the Sino–U.S. trade war as an example. While U.S. firms in the treated industrial sectors would all respond to this grand geopolitical shock, it stands to reason that different firms would respond to such a shock in different intensities and for different strategic concerns. For example, for U.S. firms whose sourcing from China only focuses on the tariffed products, a major concern would be to countervail the exogenous uptick in their material costs. In contrast, those firms who also had Chinese supplies for products that were not covered by the trade war tariffs were also required to preempt the potential geopolitical risks in other Chinese supply chains. Such varying concerns would create potential HTE issues when scholars estimate the treatment effects of this trade war on U.S. firms as a uniform industry-level shock.

Such heterogeneities in responsiveness essentially embody two primary cues for IB scholars to detect the potential existence of HTEs in observational studies: *boundary conditions* and *alternative explanations*. On the one hand, HTE sourcing from varying response intensities entails unobserved boundary conditions for the treatment effects. Inherent in the locational-, industrial-, or firm-level idiosyncrasies, these boundary conditions can render the treatment effects systematically more or less pronounced for certain samples than others. In practice, such contingent variances of treatment effects can often be discrete, uneven, or nonmonotonic across different levels of the given boundary conditions, thus making the HTE biases particularly prominent in traditional parametric estimations. On the other hand, HTEs rooted in different response mechanisms signify the existence of alternative explanations for the underlying treatment mechanisms theorized in a study. Such distinct mechanisms not only drive different samples to respond to the treatment for varying reasons but also render such heterogeneous responses covarying with confounds in different ways for various samples, thereby leading to HTE biases in the casual identification.

Notably, IB scholars often frame such boundary conditions or alternative explanations as moderators (Murphy & Aguinis, 2022). In such a manner, a valuable approach for IB scholars to detect HTEs based on response heterogeneities in their research designs is to *search for covariates that could moderate the given treatment effect in theoretically or practically viable ways.* Even if these covariates are not formally theorized as moderators, they can still provide valuable cues for IB scholars to check for HTEs caused by heterogeneous responsiveness. Scholars can then conduct *post-hoc* analyses to compare the marginal treatment effects across different scales of these covariates to gauge the presence and intensity of HTEs. In this regard, the recent *JIBS* study of Cornelissen, Dustmann, Raute and Schönberg (2018) offers an exemplary case for the reference of IB scholars.

Step 2: Account for potential HTEs in analyses

Based on the primary sources of HTEs identified with the protocols above, the second step is for IB scholars to account for the particular HTEs in their studies with appropriate empirical solutions. Specifically, in response to the HTEs sourcing from the variances in the treatments' manifestations, a valid solution is to *refine the empirical measures for the treatment* to enhance the face validity and reduce measurement errors thereof. For example, a prevalent practice in extant IB studies using TWFE DID designs is to measure treatments with staggered dummy measures. Such measures fall short in separating different compliance conditions (especially late defiers) and the switches over time and in capturing the different executions of the treatments received by different samples. For example, foreign firms can be subject to the Entity List sanctions imposed by the U.S. but removed from the list later. If the sanctions are captured with a staggered dummy valued as 1 after a firm was sanctioned for the first time, the removal of the sanctions, as late defier of the firm-specific treatment, cannot be effectively reflected and thus lead to HTE biases. In this regard, scholars can *adopt time-varying and scaled measures to capture treatments in TWFE estimations in place of staggered dummies*. These time-varying and scaled measures, which afford higher variances, can more effectively depict the heterogeneous manifestations of a treatment in terms of its changing compliance conditions and treatment intensities. At the very least, IB scholars ought to try and adopt those non-dichotomous measures for sensitivity analyses.

Likewise, in their recent work, Mogstad, Torgovitsky, and Walters (2024) highlight the value of *combining multiple IVs in two-stage estimators in light of HTEs in terms of the existence of noncompliers* lacking sufficient correspondence between the endogenous treatments and a single IV. Because two-stage estimators based on multiple IVs would recover the average LATEs across those IVs, as long as the noncompliers of the treatment to different IVs do not overlap perfectly, the estimators can be more precise than those based on any single IV. On a related note, given that such HTEs often reduce the power of instruments, IB scholars can use the presence of weak instrument issues as an indicator to determine whether or not more IVs should be added to account for HTEs.

Another crucial practice for IB scholars to combat HTEs originating from the variances in treatment manifestations is by *diligently controlling for fixed effects at different levels* in their analyses. As discussed above, the multilevel specificities are among the ultimate sources of HTEs in IB contexts. As such, controlling for fixed effects can account for the idiosyncratic manifestations of a treatment in particular industries, regions, or countries, thereby alleviating explicit and tacit HTE biases in estimations (Breuer & DeHaan, 2024). Our simulation illustrations reveal that even simply controlling for the sample- and time-fixed effects still reduces the estimation biases of HTEs.

To account for HTEs inherent in samples' responsiveness to a treatment, it is crucial for IB scholars to *control for covariates that could plausibly moderate the treatment effect*. As highlighted above and also by prior studies (e.g., Nielsen & Raswant, 2018), even if these covariates are not theorized as moderators, it is still helpful to control them in the analyses to account for unobserved boundary conditions or alternative explanations, which could create potential HTEs.

Notably, while we note that plausible moderators can provide valuable cues for the sources of HTEs, merely relying on traditional moderator tests, which estimate the interactions between the moderators and the treatments, would not be sufficient to fully delineate and rule out HTE biases in observational IB studies. These moderator tests recover the parametric estimand of the ATE of the interaction term to capture the treatment effect's shifting trajectory along the distribution of the moderator. In this regard, such parametric tests essentially assume that the shifts of the treatment effect would follow the same functional form for all samples. However, in practice, HTEs could manifest as different functional forms of the treatment effect at different scales of the moderator. Take the reciprocal tariffs imposed by Trump administration in 2025 as an example. Based on the formula disclosed by the *U.S. Trade Representative*, the reciprocal tariff rates were positively related to the trade deficit between U.S. and a given country. Moreover, the positive treatment effects of trade deficits ought to be negatively moderated by the trade volume between U.S. and the deficit country, with larger trade partners subjected to lower tariff rates[2]. However, in practice, the reciprocal tariffs executed by the U.S. government did not follow the uniform functional form along the boundary condition of trade volume. For example, the minimum reciprocal tariff rate was bound to 10% for all countries. Moreover, for many deficit countries with large trade volumes (e.g., China, India, and Vietnam), the reciprocal tariff rates were adjusted up due to nontrade factors. As a result, the treatment effects of trade deficits on the actual reciprocal tariff rates shifted along trade volumes in nonlinear and discrete ways. Such nonuniform shifts of the

[2] https://ustr.gov/sites/default/files/files/Issue_Areas/Presidential%20Tariff%20Action/Reciprocal%20Tariff%20Calculations.pdf.

treatment effect essentially violate the uniform assumption and thus subject the estimand of the interaction term itself to HTE biases.

We illustrate such HTE biases in moderator tests with a brief simulation illustration based on the same setting of Illustration 1, i.e., the treatment effects of tariff increase on domestic firms' R&D spending. With the same *Compustat* sample frame, we generate the simulation data as follows:

$$RDI_{i,t} = \tau_1 \times TariffShock_{i,t} + \tau_2 \times TariffShock_{i,t} \times DomesticDependence_{i,t} + \beta_i + \mu_t + \epsilon_{i,t}.$$

As with Illustration 1, $RDI_{i,t}$ is the simulated R&D investments made by firm i in year t, and β_i and μ_t represent the firm and year fixed effects. The treatment, $Tariff\,Shock_{i,t}$, is a binary variable that is valued as 0 for all firms in 2017, and 1 for firms in 45 randomly selected treated industries in 2019. We delineate the treatment effect of $Tariff\,Shock_{i,t}$ into two parts, a homogeneous baseline (τ_1) for all treated firms as 0.2, and a heterogeneous variation on $DomesticDependence_{i,t}$, which indicates the degree to which firm i depends on the U.S. domestic markets. Prior IB research has noted that the tariff effects are more pronounced for firms with higher levels of domestic market dependence. We thus manipulate the HTEs based on monotonically positive but nonuniform functional forms (τ_2): For firms with $DomesticDependence_{i,t}$ at the levels of 20, 40, 60, 80, and 100%, we designate their responsiveness to $Tariff\,Shock_{i,t}$ to increase by 0.2, 0.4, 1, 1.4, and 2, respectively. We then randomly assign 20% of sample firms into each of the five domestic dependence levels. This setup simulates a modest level of HTEs caused by the nonuniform variances in responsiveness across the moderator. Based on this setup, the true ATE of the baseline treatment effect is 0.2, and the true ATE of the moderating effect (or, the contingent variance of treatments on domestic dependence) is 1. We use Monte Carlo simulations to create 1,000 simulated datasets based on the above setup and then use pooled OLS and TWFE design to estimate the ATE of the moderating effect.

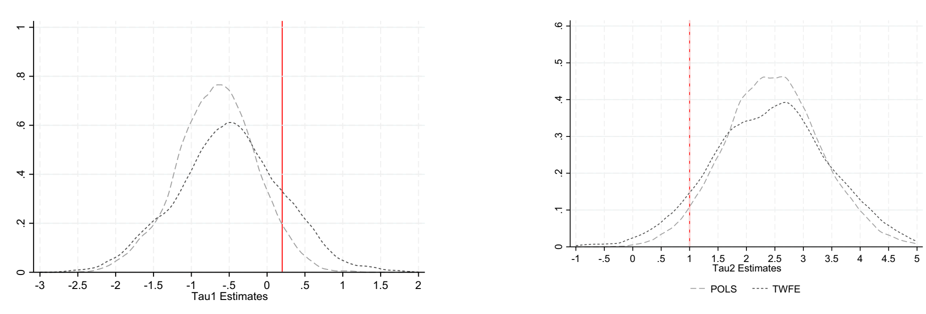
We visualize the results of these two representative estimators in Fig. 3. As can be seen, the average of the 1,000 pooled OLS estimands of τ_1 substantially deviates from the true ATE of the baseline treatment effect (0.2) by − 433.6% (mean = − 0.6671, sd = 0.5182). While the average of the estimated τ_1 recovered by the TWFE estimator is less biased than that for the pooled OLS, it still deviates from the true ATE by − 347.5% (mean = − 0.4950, sd = 0.6855). Likewise, the averages of the estimated τ_2 of the interaction terms of both pooled OLS and TWFE estimators significantly deviate from the true ATE of the moderating effect (for pooled OLS, mean = 2.467, sd = 2.913, deviating by 146.7%; for TWFE, mean = 2.430, sd = 1.008, deviating by 143.0%). Together, these results show that, even in the presence of even modest levels of HTEs based on boundary conditions, traditional moderator tests fail to discern the true sources of HTEs, let alone accurately recover the true ATEs or capture how the treatment effect shifts along different scales of the boundary conditions.

Step 3: Adopt advanced estimation tools

In light of the limitation of traditional designs and estimators in detecting and combating HTE-related biases, IB scholars must explore more advanced and robust estimation tools. In particular, recent methodological advancement in estimation models and algorithms offer better-off solutions to discern and account for HTEs in observational studies.

For example, IB scholars can draw on nuanced data-driven approaches based on supervised machine learning techniques to more effectively detect and discern nonlinear and discrete HTEs embedded in observational data. A representative example is ***causal forest estimation***. Unlike most traditional parametric estimators, causal forest estimation, as a nonparametric estimation, does not estimate the ATEs

Figure 3 Visualization of moderator test simulations



a Density Curves of τ_1 a Density Curves of τ_2

based on the assumption of homogeneous treatment effects. Instead, it strives to identify the optimal distribution of treatment effects that can best suit a given observational dataset through post-hoc iterations. In essence, causal forest estimation partitions a set of covariates into optional scale regions. In each region, the treatment effect is relatively homogeneous. Based on the optimal partition, the conditional average treatment effects (CATEs) i.e., the treatment effects conditional on the value changes of the set of covariates, can thus be estimated.

We use the causal forest estimation to analyze the treatment effects of tariff increase on domestic firms' R&D investments using the simulation setup in Step 2 above. As shown in Table 4, causal forest estimation accurately identifies the 5 scale regions of domestic dependence (20, 40, 60, 80, and 100%, respectively) in which the treatment effects of tariff increase change in nonuniform and discrete ways. Moreover, for each scale region, the conditional average treatment effects recovered by causal forest estimation offer rigorous estimands for the true ATEs (with the maximum deviation lower than 5%). These results together indicate the value of causal forest estimation in detecting and discerning HTEs in observational IB studies. IB scholars can draw on the Python package *EconML* and the *cate* command in STATA to perform causal forest estimations.

By the same token, recent progress in econometrics has promoted several alternative estimators that can help remediate the potential biases caused by HTEs (e.g., CSDID estimator and stacked DID estimator). In particular, *Fuzzy DID design* and *Bartik IV design*, as representative estimators that have attracted growing attention in recent IB studies, have been proven to be effective in helping address potential HTE biases for TWFE designs and IV designs, respectively.

Fuzzy DID

In their seminal work, de Chaisemartin and D'Haultfœuille (2018) developed the fuzzy DID estimator, which is originally designed to suit quasi-experimental settings in which the treatments affect all samples with different treatment rates, with the treatment groups having larger proportions of samples receiving the treatments than the control groups. In such "fuzzy settings" with inherent HTEs (at least based on variances in responsiveness across treatment and control groups), the ATT estimations from the traditional TWFE designs are inherently biased. Instead, the fuzzy DID estimator suits such settings by estimating the LATEs of switchers, i.e., samples whose treatment conditions changed in the observation window) to capture the treatment effect. Building on such rationale, the fuzzy DID design offers three alternative estimands, i.e., Wald DID that is suitable when the treatment remains stable for each group, and time-corrected Wald ratio (Wald TC) and changes-in-changes Wald ratio (Wald CIC) that are suitable in settings in which the treatment shifts over time. As such, fuzzy DID serves as a valid alternative estimator in place of TWFE design in the presence of HTEs caused by variances in responsiveness and manifestations.

To gauge the rigorousness of fuzzy DID estimator in the presence of HTEs, we contrast the estimations of fuzzy DID against those of pooled OLS and TWFE estimator in the two simulations in Illustration 1, respectively. Notably, the setup of the two simulations only has one pre-shock year and one post-shock year, intrinsically ruling out the cross-sectional heterogeneities of treatment effects. As such, the Wald DID estimand of fuzzy DID is identified to be the most suitable. As shown in Table 1 and Fig. 1, in both simulations, the Wald DID estimands obtain much more rigorous estimations of the true ATEs than the pooled OLS and TWFE estimators. In Simulation 1–1, the mean of the 1,000 Wald DID estimands is 0.201, which only deviates from the true ATE by 0.65%. Likewise, in Simulation 1–2, the Wald DID estimators average to 0.200, which only deviates from the true ATE by 0.15%. These findings indicate the merits of fuzzy DID in mitigating HTE biases. IB scholars can draw on the STATA package *fuzzydid* to perform the fuzzy DID analyses.

Bartik IVs

Originating from Bartik's seminal work (1991) on the influence of county-level employment changes on wages, Bartik IVs (also known as "shift-share IVs") estimate causal effects by isolating the plausibly exogenous variances of an endogenous treatment and using such variances as an instrument. For example, Bartik (1991) isolates the exogenous variations in county-level employment changes into two factors: The

Table 4 Conditional ATEs estimated by causal forest estimations

Domestic dependence	20%	40%	60%	80%	100%
Baseline ATE	0.2	0.2	0.2	0.2	0.2
Moderating ATE	0.04	0.16	0.6	1.12	2
Total ATE	0.24	0.36	0.8	1.32	2.2
Mean of simulated estimand	0.2337	0.3433	0.8141	1.3058	2.2030
Deviation from the true ATE	− 2.617%	− 4.638%	1.757%	− 1.072%	0.135%
SD	0.8491	0.8235	0.8321	0.8391	0.8552

"share" part, which captures the exogenous baseline status of county-level employment, is measured with a county's pre-determined industry structure in terms of the predetermined fraction of employees in an industry located in a given county. The "shift" part, which captures the exogenous changes of county-level employment over time, is captured with the national growth rate in employment within an industry. The Bartik IV is then constructed as the product of the shift and the share. As shown by recent works, estimations with a Bartik IV can remain robust when the IV is of relatively low power or when either the shift or the share of this IV is endogenous (Breuer, 2022). Such advantages inherently render Bartik IVs a valuable estimator in the presence of HTEs. As discussed above, in IV designs, HTEs in terms of correspondence to IVs would often lead to issues of weak instrument, which can be effectively mitigated with Bartik IVs. More importantly, when constructing a Bartik IV, IB scholars could choose the shift and share based on the specificities at different levels that could bring about heterogeneities in the treatment of interest, thus effectively incorporating HTEs into the IV.

We compare the two-stage estimations based on Bartik IV against those of other estimators in the simulation in Illustration 2 to showcase the value of Bartik IVs in combating HTEs. Echoing the design of Bartik (1991), we construct the shift-share IV as follows:

$$Bartik\_IV_{i,j,t} = shift_{j,t} \cdot \eta_{j,2009},$$

where the share is designated as the overseas dependence of industry j in which resides firm i in 2009—the baseline year in the simulation setup. The shift captures a random industry-year shock to depict whether industry j witnessed technological advancements in foreign countries in year t. Under this setup, the share and shift are both exogeneous to firm i's decisions in year t and relevant to its annual exports, thus ensuring its validity.

Table 2 reports the means and standard deviations of the two-stage estimators based on the above Bartik IV. As shown in this example, Bartik IV estimands average to 0.503 (sd = 0.003) in the 1,000 simulated datasets, which only deviates from the true ATE by 0.66%. In accordance with these results, the Bartik IV estimator is substantially more rigorous than the estimators based on traditional IVs in our setting (see also the visualization of Fig. 2). In such a manner, this finding corroborates our discussions above and indicates the merits of Bartik IV in combating HTE biases. IB scholars can use the *ssaggregate* command in STATA to perform Bartik IV design.

Summary

While a growing body of recent research has cautioned against the issue of heterogeneous treatment effects (HTEs) and the consequent estimation biases thereof, we realized that this issue has not received much attention in the extant IB research. Hence, in this study, we first unveil the theoretical mechanisms underlying the estimation biases of HTEs and highlight their implications and manifestations in IB contexts. Then, by conducting two simulation illustrations contextualized within classic IB contexts, we demonstrate the intensity of biases that could be caused by HTEs in representative identification strategies in IB research. Finally, we synthesize our findings and the insights of the extant research on HTEs to offer a brief checklist to help future IB scholars better discern and remediate HTE-related biases to enhance the empirical rigorousness in IB research.

Supplementary Information The online version contains supplementary material available at https://doi.org/10.1057/s41267-026-00868-5.

Acknowledgements We thank the JIBS EIC, Prof. Rosalie Tung, and anonymous reviewers for their insightful comments. This editorial was supported in part by the Research Grants Council of Hong Kong (HKUST# 16505124).

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