

Promoting Firm Exports: Identifying Causal Effects on Market Entry and Growth

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November 2025

Abstract

Export promotion programs (EPPs) are widely used policy instruments, yet causal evidence on their effectiveness remains limited due to self-selection bias. This paper evaluates South Korea's export promotion programs using the methodology of Freyaldenhoven et al. (2019) to address selection on unobservables. Exploiting R&D expenditure as a proxy for unobserved productivity and growth potential, we find that EPPs significantly increase export market entry (5–10 percentage points) and destination expansion (0.1–0.2 countries), with effects persisting at least for six years. However, impacts on the intensive margin are modest: sales growth is only 2–4% and value added shows no significant increase. In contrast, exporting itself increases sales and value added by 10%, implying that while EPPs effectively reduce entry barriers, they provide limited support for firm scaling.

JEL Classification: F13, F14, H25, L25, O24

Keywords: export promotion programs, self-selection, pre-trend, difference-in-differences, firm dynamics

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1 Introduction

Export promotion programs (EPPs) are implemented by governments worldwide to help firms overcome barriers to entering foreign markets, resting on the premise that exports generate positive externalities and facilitate firm growth (Lederman et al., 2010; Reed, 2024). Despite widespread use and policy importance, robust empirical evidence on the causal effects of EPPs remains surprisingly limited. The fundamental challenge lies in identification: firms that self-select into these programs are systematically different from non-participants, making it difficult to isolate the true causal impact of policy intervention from pre-existing firm characteristics.

The identification problem is particularly severe because the decision to seek export assistance is endogenous to firms' growth prospects and export potential. Firms with higher expected profits from exporting or superior production capabilities are more likely both to seek government support and to succeed in international markets, regardless of policy intervention. This self-selection process creates pre-trends in outcome variables between treated and control groups, violating the parallel trends assumption crucial for standard difference-in-differences estimation. Consequently, naive comparisons between program participants and non-participants can severely overestimate policy effectiveness.

Previous research on export promotion has followed two approaches with distinct strengths and limitations. Randomized controlled trials provide clean identification but face practical constraints in evaluating large-scale programs (Atkin et al., 2017; Cusolito et al., 2023). Observational studies using quasi-experimental methods can analyze real programs at scale but struggle to address self-selection on unobservables—the fundamental challenge when firms' private information about growth prospects drives both program participation and export outcomes (Broocks and Van Biesebroeck, 2017; Lederman et al., 2016; Munch and Schaur, 2018; Volpe Martincus and Carballo, 2008).¹

This paper contributes to the literature by employing the methodology of Freyaldenhoven et al. (2019) (hereafter, FHS) to assess the effectiveness of South Korea's export promotion programs. The FHS approach offers a novel solution to the pre-trends problem by exploiting the time-series properties of confounding factors. The key insight is that if we can identify an observable covariate whose trend mimics that of the unobservable confounder but is itself unaffected by the policy, we can use this covariate's lead values as instrumental variables to purge the bias from our estimates. In our context, we argue that R&D expenditure serves as such a proxy: it correlates strongly with firms' expected profitability and productivity—the main drivers of both program participation and export success—yet is not directly caused by receiving export subsidies, except through improved growth prospects.

¹A recent contribution, Buus et al. (2025), leverages embassy caseworkers' quasi-random outreach as an instrument, providing a novel identification strategy in this literature that directly targets selection on unobservables.

Our empirical analysis uses comprehensive firm-level data from South Korean manufacturing firms spanning 2005–2022, combined with detailed administrative records on export promotion program participation from 2010–2015. We carefully construct treatment and control groups by focusing on firms that (i) survived throughout the entire sample period, (ii) operated exclusively in the domestic market from 2005–2010, and (iii) first received policy support between 2011–2015. We then employ propensity score matching on observable characteristics to construct a control group that resembles the treatment group. Despite this careful matching, we document significant pre-trends in both export participation and sales, confirming the presence of unobserved confounding factors.

Applying the FHS methodology with R&D intensity as the proxy variable and its lead as the instrumental variable, we find that export promotion programs have statistically significant but economically modest effects. On the extensive margin, program participation increases the probability of exporting by 5–10 percentage points and expands the number of export destinations by 0.1–0.2 countries. These effects are persistent, lasting at least six years after initial program participation. However, on the intensive margin, the effects are considerably weaker: sales growth attributable to the program is only about 2–4%, and we find no statistically significant effect on value added. This pattern is consistent with dynamic trade models in which post-entry investments (e.g., distribution networks, customer accumulation) drive gradual export growth and backloaded profits; in such settings, policies that solely reduce entry costs may have muted effects on intensive-margin scaling when post-entry capability constraints bind (see the discussion in Alessandria et al. (2021)).

To address the concern that exports themselves may not facilitate firm growth, we conduct a parallel analysis treating export market entry (rather than program participation) as the event of interest. Using the same FHS methodology to address self-selection into exporting, we find that export participation per se increases both sales and value added by approximately 10% over the medium to long run. This confirms that exports have causal effects on firm growth through several mechanisms, such as learning-by-exporting (De Loecker, 2013; Hahn, 2012), demand accumulation and market penetration (Arkolakis, 2010; Fitzgerald et al., 2024), information learning and sequential market expansion (Albornoz et al., 2012), quality upgrading and organizational improvements (Atkin et al., 2017; Garcia-Marin and Voigtlander, 2019). The contrast between these results and our main findings implies that the modest impact of EPPs stems not from the inefficacy of exports themselves, but rather from the limited ability of these programs to help firms compete and scale in global markets beyond initial entry.

This disconnect may reflect the breakdown of complementarities between exporting and innovation. Aw et al. (2011) document that R&D and exporting are joint strategic decisions in Taiwanese electronics firms, with firms investing in R&D before export market entry and successful exporters subsequently investing more in innovation. However, this complementarity may only operate when firms possess sufficient competitive capabilities. Export promo-

tion programs that reduce entry costs without addressing innovation capabilities may fail to trigger this reinforcing cycle: if subsidized entrants lack the capacity to compete effectively internationally, the expected returns to R&D remain low even after export entry. Consistent with this interpretation, we find that R&D intensity among supported firms stagnates or even declines after receiving program benefits (relative to control firms).

This paper makes several contributions. First, we are the first to apply the Freyaldenhoven et al. (2019) (FHS) methodology to evaluate export promotion programs, demonstrating its usefulness for addressing selection on unobservables in policy evaluation—where valid instruments are rarely available. Using firms’ R&D expenditure as a proxy for unobserved productivity and growth potential, we illustrate how readily available firm-level variables can mitigate selection bias, providing a practical template for assessing business support programs facing similar identification challenges. To test robustness to imperfect instrument exogeneity, we further implement the Conley et al. (2012) approach and show that our estimates remain stable under substantial relaxations of the exclusion restriction.²

Second, we offer rigorous causal evidence on large-scale export promotion programs in South Korea, a major exporting economy with a long tradition of industrial policy. By separating extensive and intensive margin effects, we show that while programs effectively lower entry barriers and facilitate market entry, their impact on post-entry expansion is limited.

Third, by contrasting program participation with actual exporting, we examine whether EPPs merely facilitate entry or also foster subsequent firm growth—a key dimension of their effectiveness. The observed stagnation in R&D intensity after participation points to binding capability constraints that hinder post-entry growth. These findings suggest that while export promotion programs succeed in bringing new firms into international markets, additional support for technological upgrading and capability accumulation is needed to sustain growth and value creation.

The remainder of this paper is organized as follows. Section 2 describes the institutional background of South Korea’s export promotion programs, and Section 3 outlines the data sources and sample construction. Section 4 presents the empirical framework, introducing the FHS methodology and identification strategy. Section 5 reports the main empirical results on program impacts across extensive and intensive margins and includes robustness checks, such as inference with plausibly exogenous instruments following Conley et al. (2012). Section 6 compares program-induced exports with the effects of actual export participation to assess whether export promotion translates into firm growth, and Section 7 discusses policy implications and concludes.

²We also address recent critiques of staggered-adoption Difference-in-Differences designs by employing robust estimators (Callaway and Sant’Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021). The results—available upon request—are materially unchanged.

2 Institutional Background: Export Promotion Programs in Korea

South Korea's export promotion programs represent a comprehensive suite of government-funded initiatives designed primarily to offset export fixed costs faced by firms, particularly small and medium-sized enterprises. These programs are administered by multiple government agencies including the Korea Trade-Investment Promotion Agency (KOTRA), the Korea SMEs and Startups Agency, the Korea Agro-Fisheries & Food Trade Corporation, the Korea International Trade Association, and local governments.

The programs encompass diverse forms of support aimed at reducing various barriers to export market entry and expansion. Core program components include: (i) export and investment consulting services, including export capability assessments for domestic firms; (ii) overseas market research and marketing support; (iii) business trip assistance for trade fairs, exhibitions, and trade missions; (iv) export incubator services providing overseas office space and overseas branch office services acting as surrogate branches for firms; (v) shared logistics centers at overseas locations; and (vi) the increasingly popular export voucher program, which offers firms flexible subsidies covering the above services.

These programs align with the theoretical framework of fixed export cost subsidies. Rather than directly reducing variable trade costs (such as tariffs) or enhancing production efficiency (such as R&D support), EPPs focus specifically on lowering the one-time or periodic fixed costs associated with entering and maintaining presence in foreign markets—precisely the margin emphasized in models of firm export dynamics ([Das et al., 2007](#); [Melitz, 2003](#); [Roberts and Tybout, 1997](#)).

Importantly, Korea's EPPs operate largely independently from R&D and innovation support programs. While the government provides substantial R&D subsidies through separate channels, export promotion agencies do not typically include direct R&D funding in their program portfolios. This institutional separation allows us to empirically distinguish between the effects of reducing entry barriers (through EPPs) and building production capabilities (through innovation policies), though it also raises questions about potential complementarities between these policy domains.

In terms of policy reach and targeting, these programs exhibit substantial heterogeneity in both coverage and intensity. While some firms receive support only once, many participate in multiple programs across several years, reflecting both the availability of diverse program options and the government's practice of providing repeated assistance to previous beneficiaries. This pattern of repeated participation, documented in our data and discussed in the following Section, plays an important role in our empirical strategy for measuring treatment intensity and identifying causal effects.

3 Data and Sample Construction

3.1 Data Sources

Our empirical analysis combines three main data sources covering South Korean manufacturing firms from 2005 to 2022. First, we obtain comprehensive firm-level production data from KoDATA, a commercial corporate information database that standardizes audited financial statements and firm characteristics (e.g., sales, value added, fixed assets, labor costs, and R&D outlays). Second, we acquire data on firms' export activities from the Korea Trade Statistics Promotion Institute, including annual export status and the number of destination countries for each firm. Third, and most crucially for our identification strategy, we obtained administrative records on export promotion program participation from the Ministry of Strategy and Finance. These records identify which firms received program benefits and in which years during the 2010–2015 period.

The export promotion programs in our data encompass various government-funded initiatives designed to assist firms' export activities, including export subsidies, market research support, trade missions, and marketing assistance. A key feature of these programs is that firms can receive support multiple times, both within the same year (from different programs) and across multiple years. In fact, more than half of the supported firms in our sample received benefits over multiple years, and many received support from multiple programs within the same year.

3.2 Sample Selection and Treatment Group Construction

A fundamental challenge in evaluating export promotion programs is distinguishing the causal effect of policy intervention from pre-existing differences between participating and non-participating firms. To address this challenge and construct a clean treatment group, we impose three strict sample selection criteria that, while reducing sample size, substantially improve the credibility of our identification strategy.

First restriction: Excluding early program participants. We exclude firms that received program support in 2010, even though our administrative data begins in that year. The rationale is straightforward: since we do not observe program participation before 2010, firms receiving support in 2010 may have been receiving support in earlier years as well. Including such firms would contaminate our treatment group with firms whose initial treatment timing is mismeasured. Under the assumption that firms receive support consecutively once they start (an assumption supported by the high persistence in Figure 1), we restrict the treatment group to firms whose first program participation occurred between 2011 and 2015.

Second restriction: Pure domestic firms before treatment. Even if a firm did not receive support in 2010, it may have received support before that period, and such pre-2010 support

could have persistent effects on firm behavior. This concern is particularly acute given that one-time fixed cost subsidies can generate hysteresis in the extensive margin. To mitigate this concern, we restrict our sample to firms that had no export activity from 2005 (when our sample period begins) through 2010. Firms that did not export for six consecutive years are highly unlikely to have applied for export support programs during or before that period.³

Third restriction: Surviving firms throughout the sample period. Our core interest lies in understanding how export promotion affects firms' export market entry, exit, and subsequent growth through exports. To isolate these export dynamics from the confounding effects of firm entry and exit in the domestic market, we restrict our sample to firms that survived and maintained production activities throughout the entire 2005–2022 period. While this restriction further reduces sample size, it allows us to focus exclusively on export market dynamics without worrying about selection into or out of the domestic market.

Combining these three restrictions, our treatment group consists of firms that: (i) are among the 25,273 manufacturing firms observed continuously from 2005 to 2022; (ii) operated exclusively in the domestic market from 2005 to 2010; and (iii) first received export promotion program benefits between 2011 and 2015. A total of 1,433 firms satisfy all three conditions. Basic descriptive statistics for the treatment group are provided in the Appendix.

Figure 1 illustrates the temporal pattern of program participation among treatment group firms. Each row represents a firm, sorted by the year of initial program receipt, and the color intensity indicates the number of programs received in each year. The figure reveals substantial heterogeneity in treatment intensity: while some firms received support only once, many received benefits from multiple programs within the same year and across multiple years. This heterogeneity poses challenges for estimation, which we address by measuring treatment intensity as the average number of programs received per year for each firm.

3.3 Control Group Construction and Propensity Score Matching

The ideal control group would consist of firms that satisfy the same three sample restrictions as the treatment group but never received program benefits. We begin by identifying all manufacturing firms observed from 2005 to 2022 that had no export activity from 2005 to 2010 and did not receive any program benefits during the 2011–2015 window when we can observe program participation.

A data limitation we must acknowledge is that, similar to the pre-2010 period, we cannot observe program participation after 2015. Therefore, some firms in our control group may have received support after 2015. We address this concern in two ways. First, for our main analysis, we use the full sample through 2022 under the assumption that control group firms did not receive benefits after 2015. Second, as a robustness check (presented in Section 5), we

³Even if a firm exported before 2005, a six-year gap without exporting effectively depreciates any prior export experience to zero, making it reasonable to classify such firms as pure domestic firms.

re-estimate our models using only data through 2015, which guarantees clean control group status but reduces the post-treatment time series available for analyzing dynamic effects.

Even among firms satisfying the basic sample restrictions, substantial heterogeneity remains in firm characteristics. Simply comparing all treated firms to all eligible control firms would yield biased estimates due to differences in observable characteristics. To construct a more comparable control group, we employ propensity score matching (Heckman et al., 1998). Specifically, for each treated firm, we calculate a propensity score based on characteristics measured in the year immediately before the firm first received program benefits,⁴ and match it to the single nearest neighbor in the control group based on propensity score distance (nearest neighbor matching without replacement).

Our propensity score model includes six matching variables: (i) industry classification at the 2-digit level; (ii) firm age; (iii) average sales growth rate over the previous five years; (iv) log(labor costs); (v) log(tangible assets); and (vi) exporter status in the matching year. These variables capture the key determinants of export dynamics discussed in Alessandria et al. (2021): industry, firm size, past growth trajectory, and export experience.⁵

Figure 2 demonstrates the effectiveness of our matching procedure. Panel (a) shows that before matching, treatment and control firms differ substantially across all six dimensions. Control firms are on average nearly twice as large (in terms of both labor costs and tangible assets), have higher past sales growth, and are more likely to be exporters. Interestingly, despite these advantages, treatment group firms are older on average, suggesting slower historical growth—consistent with the notion that these are firms struggling to scale up. These large pre-match differences make the full unmatched sample unsuitable for causal analysis.

Panel (b) shows that after propensity score matching, the two groups are statistically balanced across all six matching variables. None of the mean differences is statistically significant at conventional levels, and the confidence intervals all include zero. This balance on observables provides a necessary foundation for our analysis, though as we show in Section 5, it is not sufficient—significant pre-trends remain even in the matched sample, motivating our use of the FHS methodology.

3.4 Key Variables

Our main outcome variables capture both the extensive and intensive margins of export activity. First, export participation is a binary indicator equal to 1 if the firm exported in year t , and 0 otherwise. This measures the extensive margin of export activity—whether firms enter or exit international markets. Second, the number of export destinations counts the countries

⁴For example, firms first receiving support in 2011 have their propensity scores calculated using 2010 characteristics.

⁵For industry classification, the sample size is insufficient to perform exact matching within narrow industry strata, so we treat the 2-digit classification code as a continuous variable in the propensity score model. This approach relaxes the constraint while still encouraging matches between firms in similar industries.

to which the firm exported in year t . We apply the inverse hyperbolic sine transformation, $\text{arcsinh}(x) = \ln(x + \sqrt{x^2 + 1})$, to this variable (Bellemare and Wichman, 2020). This transformation is appropriate for count variables with many zeros and no upper bound, and has become standard in the trade literature. The transformation approximates $\ln(x)$ for large x and approximates x for small x , making coefficients interpretable as approximate percentage changes. Third, sales measures total annual sales (in logs), capturing overall firm growth including both domestic and export sales. This represents the intensive margin of firm performance. Finally, value added is annual value added (in logs), calculated as sales minus intermediate input costs. This measures the firm's contribution to economic output and is less susceptible to mechanical increases from input substitution.

Our treatment variable is measured in two ways. In our baseline specification, D_{it} is a binary indicator equal to 1 from the first year the firm receives program benefits onward (a staggered adoption design). In our preferred specification that accounts for treatment intensity, D_{it} equals the firm's average number of program benefits received per year, calculated as the total number of programs received divided by the number of years in which the firm received support. For example, a firm receiving 2 programs in 2012 and 3 programs in 2014 has an average intensity of 2.5 ($=5 \div 2$), which is assigned to 2012 and all subsequent years. This measure ranges from 1 to 11 in our sample, with a mean of 1.53.

Our key proxy variable for unobserved confounders is R&D intensity, defined as R&D expenditure divided by sales. As discussed in the methodology section below, we argue that R&D intensity correlates with the unobserved factors (expected profitability and productivity) that drive both program participation and export success, but is not directly caused by program participation except through those same confounding factors. We use R&D intensity rather than R&D levels because our outcome variables include binary indicators, making the ratio more appropriate for the linear probability model framework.

4 Identification Strategy

4.1 Event Study Framework and the Pre-Trends Problem

Our baseline empirical approach employs an event study model that traces out the dynamic effects of program participation over time. The specification is:

$$y_{it} = \alpha_i + \lambda_t + \sum_{m=-\underline{M}}^{\bar{M}} \beta_m D_{it}^m + \eta_{it} + \varepsilon_{it} \quad (1)$$

where y_{it} is the outcome variable for firm i in year t . The term α_i represents firm fixed effects, which absorb all time-invariant firm characteristics such as initial productivity, managerial quality, or location advantages. The term λ_t represents year fixed effects, controlling

for aggregate time trends affecting all firms, such as macroeconomic conditions, exchange rate movements, or changes in global demand. These fixed effects ensure that our identification comes from within-firm variation over time, comparing each firm to itself before and after program participation, while controlling for common time trends.⁶

The key term is $\sum_{m=-M}^{\bar{M}} \beta_m D_{it}^m$, which captures the dynamic treatment effect. Here, D_{it}^m is an indicator variable equal to 1 if firm i is m periods away from first program participation at time t , and 0 otherwise. The coefficient β_m thus represents the effect of program participation m periods after (if $m > 0$) or before (if $m < 0$) the event. This flexible specification allows treatment effects to evolve over time, accommodating anticipation effects (firms may adjust behavior before formally receiving support) and dynamic learning or adjustment processes (effects may grow, fade, or persist over time).

The pre-treatment coefficients $\{\beta_m\}_{m<0}$ serve a dual purpose. First, they provide a visual diagnostic for the parallel trends assumption: if treated and control firms had parallel trends absent treatment, these coefficients should be statistically indistinguishable from zero. Second, we can formally test the null hypothesis $H_0 : \beta_m = 0$ for all $m < 0$ using a joint Wald test (Roth et al., 2023). Rejection of this null indicates violation of parallel trends. We report both conventional 95% confidence intervals for each β_m and uniform confidence bands (Sup-t bands) that maintain 95% coverage probability across all periods simultaneously, providing more conservative inference about the overall pattern of effects.

The term η_{it} represents unobserved time-varying confounders that may correlate with program participation and outcomes. In our context, η_{it} captures firms' private information about their expected profitability from exporting, productivity shocks, or emerging market opportunities. Firms with increasing η_{it} are more likely to apply for export support and, independently, more likely to succeed in international markets. This generates endogeneity: $E[D_{it} \cdot \eta_{it}] \neq 0$, violating the key identifying assumption of standard event studies. Finally, ε_{it} captures idiosyncratic shocks that are orthogonal to treatment and confounders.

The challenge is that propensity score matching, while balancing observable characteristics between treated and control groups, cannot address selection on unobservables (η_{it}). Figure 3 provides stark visual evidence of this problem. Panel (a) plots event study estimates for export participation using our matched sample. Even 6 years before program participation, treated firms show 5–10 percentage points lower export rates than controls, and this gap narrows monotonically as the event approaches. Panel (b) shows similar patterns for log(sales): treated firms are smaller before participation, with the gap closing over time. A joint Wald test strongly rejects the null of no pre-trends in both cases (p-value < 0.01).

This pattern is precisely what we would expect from self-selection on unobserved growth potential. Treated firms are those experiencing rising η_{it} —they anticipate improving export

⁶Additional control variables such as industry-year fixed effects could be included to allow for differential trends across industries. We examine this in robustness checks and find qualitatively similar results.

prospects and apply for support when their expected profitability crosses a threshold. The matching process ensures treated and control firms look similar at baseline, but cannot eliminate the differential trajectory of η_{it} that drives both program application and export success. Naive estimation of Equation (1) using this matched sample would conflate program effects with the pre-existing upward trend in outcomes, severely overstating program effectiveness.

4.2 The Freyaldenhoven-Hansen-Shapiro (FHS) Approach

4.2.1 Methodology Overview

To address pre-trends arising from selection on unobservables, we employ the methodology developed by [Freyaldenhoven et al. \(2019\)](#) (hereafter FHS). The key insight is that if we can identify an observable covariate that tracks the trend of the unobserved confounder but is itself unaffected by treatment, we can use this proxy to purge the confounding bias from our estimates. The intuition is straightforward: the proxy variable reveals the trajectory of the unobservable factor, allowing us to separate true treatment effects from spurious correlation.

To formalize this idea, consider a simplified static version of Equation (1), after applying within-transformation to remove fixed effects:⁷

$$y_{it} = \beta D_{it} + \gamma \eta_{it} + \varepsilon_{it} \quad (2)$$

Here, β represents the (static) treatment effect, γ captures how the unobserved confounder affects outcomes, and ε_{it} is an idiosyncratic error term. The fundamental identification problem is that η_{it} is unobserved yet correlated with D_{it} , generating omitted variable bias: $\hat{\beta}_{OLS} \xrightarrow{p} \beta + \gamma \cdot \text{Cov}(D_{it}, \eta_{it})/\text{Var}(D_{it})$.

The FHS approach requires two key assumptions. The first is an *orthogonality condition*:

$$E[\varepsilon_{it} | \eta_{it}, \{D_{is}\}_{s=t_0}^T] = 0 \quad (3)$$

This condition states that the error term ε_{it} is conditionally independent of both the confounder η_{it} and the entire path of treatment $\{D_{is}\}_{s=t_0}^T$. Intuitively, this requires that once we account for the systematic confounding factor (η_{it}) and treatment itself, there are no additional unobserved factors jointly affecting treatment assignment and outcomes. In our context, this assumes that firms' decisions to apply for export programs are driven by their expected export profitability (η_{it}), not by idiosyncratic shocks (ε_{it}) such as random administrative delays or program officer discretion.

⁷We present the static model for expositional clarity. The methodology extends naturally to settings with dynamic treatment effects, anticipation effects, and staggered adoption—all features present in our application.

The second assumption is a *rank condition* on an observable covariate x_{it} :

$$E[x_{it}|\eta_{it}, \{D_{is}\}_{s=t_0}^T] = \delta\eta_{it} \quad (4)$$

This is equivalent to the structural relationship:

$$x_{it} = \delta\eta_{it} + \nu_{it}, \quad E[\nu_{it}|\eta_{it}, \{D_{is}\}_{s=t_0}^T] = 0 \quad (5)$$

where ν_{it} is a residual component of x_{it} orthogonal to both the confounder and treatment path. This condition embeds two critical requirements. First, x_{it} must co-move with η_{it} : the proxy should track the confounder linearly with scaling factor δ . Second, conditional on η_{it} , the proxy must be independent of treatment: x_{it} can affect treatment only through its correlation with the confounder, not directly. This rules out scenarios where treatment causes the proxy (reverse causality) or where omitted factors jointly determine both treatment and the proxy.

A natural question arises: why not simply control for x_{it} directly in the regression? The answer is that doing so would introduce a new bias. Including x_{it} as a regressor controls for $\delta\eta_{it}$ (eliminating confounding bias) but also introduces the residual ν_{it} into the error term. If ν_{it} correlates with treatment conditional on outcomes—which is likely if, for example, program administrators observe x_{it} when making funding decisions—then controlling for x_{it} creates bad control bias.⁸

The solution exploits the time-series structure of panel data. If η_{it} exhibits persistence (e.g., follows an AR(1) process), and if treatment changes occur when η_{it} crosses a threshold, then future values of the proxy—say x_{it+1} —will correlate with current η_{it} (through persistence) but remain orthogonal to current ν_{it} (as they are separated in time). Formally, leads of the proxy serve as instrumental variables: $E[\nu_{it}|x_{it+k}] = 0$ for $k \geq 1$, while $\text{Cov}(x_{it+k}, \eta_{it}) \neq 0$ due to serial correlation in η_{it} . This delivers valid instruments for the portion of x_{it} that tracks the confounder.

Implementation proceeds via two-stage least squares (2SLS). In the first stage, we regress the proxy x_{it} on its own leads $\{x_{it+k}\}_{k=1}^K$ (and other exogenous variables), obtaining predicted values \hat{x}_{it} . This predicted value isolates the persistent component of x_{it} driven by η_{it} , purging the residual ν_{it} . In the second stage, we estimate Equation (2) including \hat{x}_{it} as a control:

$$y_{it} = \beta D_{it} + \gamma \hat{x}_{it} + \tilde{\varepsilon}_{it} \quad (6)$$

Under the two FHS conditions, this 2SLS estimator $\hat{\beta}_{2SLS}$ consistently estimates the causal effect β even in the presence of pre-trends. Importantly, the methodology extends seamlessly

⁸For instance, suppose R&D spending affects both sales (through innovation) and treatment (through signaling firm quality to program administrators). Controlling for R&D would absorb some true treatment effect. See [Angrist and Pischke \(2009\)](#) for discussion of bad controls.

to dynamic event studies: Equation (1) can be estimated by 2SLS using leads of the proxy to instrument for the proxy itself, yielding consistent estimates of the entire path $\{\beta_m\}_{m=-\underline{M}}^{\bar{M}}$.

4.2.2 Choice of Proxy Variable: R&D Expenditure

The credibility of the FHS approach hinges on the validity of the proxy variable. We use R&D intensity (R&D expenditure divided by sales) as our proxy for the unobserved confounder η_{it} . We argue that R&D intensity satisfies both FHS conditions in our setting.

First, R&D intensity plausibly tracks firms' expected export profitability and productivity—the key unobservables driving program participation. Standard models of endogenous R&D investment predict that firms invest in R&D when they anticipate high returns from innovation, which correlate with productivity and profitability (Doraszelski and Jaumandreu, 2013). Firms expecting to expand into export markets—whether or not they receive subsidies—will invest in R&D to develop products, improve quality, or reduce costs to compete internationally. Conversely, firms with stagnant domestic prospects and no export potential have little incentive for R&D. Thus, R&D intensity should co-move with the profitability and productivity shocks (η_{it}) that drive both export program applications and export success, satisfying the first part of Equation (5).

Second, R&D decisions are determined primarily by technological opportunities and market considerations, not directly by export subsidies. Export promotion programs in South Korea provide matching grants for export-related activities (trade fairs, market research, export marketing) but do not directly subsidize R&D. While firms that become successful exporters may later increase R&D due to higher revenues (learning-by-exporting or other mechanisms), this is precisely the growth channel that the confounder η_{it} represents—not a violation of the exclusion restriction. The key assumption is that receiving export subsidies per se does not cause firms to change R&D intensity except through the mediating channel of improved growth prospects. We find this plausible because: (i) export programs are small in magnitude (average subsidy is 0.5% of annual sales) and targeted at entry costs, not innovation; (ii) R&D is a long-term investment driven by technological trajectories largely orthogonal to short-term export opportunities; (iii) as documented below, R&D intensity actually stagnates or declines after program participation, inconsistent with programs directly stimulating innovation.

Several empirical patterns support the validity of our proxy. As we show in Section 5, R&D intensity tracks the pre-trends in export participation almost perfectly: both variables exhibit the same monotonic convergence pattern between treated and control firms in the 3–4 years before program participation. This tight correlation is exactly what Equation (4) requires. Moreover, the scaling appears linear—there is no evident nonlinearity or structural break—consistent with the linear proxy model in Equation (5).

We address potential threats to identification through extensive robustness checks. First, if firms increase R&D specifically in anticipation of receiving export subsidies (e.g., to signal

worthiness or develop export-oriented products), this would violate the exclusion restriction. To test this, we examine whether R&D intensity spikes immediately before program participation. Our analysis shows that R&D increases smoothly over 3–4 years before participation, not sharply in the year immediately before—consistent with strategic manipulation. Second, we verify instrument relevance: first-stage F-statistics exceed 20 in all specifications, well above conventional thresholds. Third, we conduct sensitivity analysis following Conley et al. (2012) to allow for plausibly exogenous instruments, relaxing the strict exclusion restriction and finding that results remain robust to substantial violations.

5 Empirical Results

5.1 Extensive Margin Effects

We begin by examining program effects on the extensive margin of exporting: whether firms enter export markets and how many destination countries they serve. These outcomes capture the primary mechanism through which export promotion programs are designed to operate—reducing fixed costs of market entry.

5.1.1 Export Market Entry

We estimate Equation (1) via 2SLS with export status (a dummy variable equal to 1 if the firm exports, 0 otherwise) as the dependent variable. This linear probability model uses R&D intensity (R&D expenditure/sales) as the proxy variable x_{it} , with the one-period lead x_{it+1} serving as the instrumental variable. The treatment variable D_{it} is initially specified as a binary indicator taking value 0 before first program participation and 1 from the year of first participation onward, capturing a staggered adoption design where different firms enter at different times.⁹

Figure 4 presents our main results for export participation. Panel (a) provides a key diagnostic: we plot event study coefficients for both the outcome variable (export status) and the proxy variable (R&D intensity), rescaling the proxy to overlap with the outcome in the pre-treatment period. This visualization demonstrates the validity of our identification strategy. The two series track each other closely in the six years before program participation, exhibiting nearly identical patterns: both start with treated firms showing lower values than controls (about 5–10 percentage points lower for export status, 2–3 percentage points lower for R&D

⁹Recent methodological literature has raised concerns about two-way fixed effects estimators with staggered treatment timing and heterogeneous treatment effects (See de Chaisemartin and D'Haultfœuille (2023) for the literature survey). These papers show that conventional TWFE estimators can produce misleading estimates when treatment effects vary across cohorts or over time. We address these concerns in several ways: (i) we implement robust estimators and find qualitatively similar results, (ii) we show results are robust to cohort-specific trends, and (iii) we restrict the sample to one-time participants only, eliminating concerns about dynamic selection.

intensity), and both gaps narrow monotonically as the event approaches. This tight correlation is precisely what the FHS rank condition requires—the observable proxy reveals the trajectory of the unobserved confounder driving outcomes.

This pattern reveals important selection dynamics: treated firms' R&D intensity increases sharply in the two years before program participation, consistent with firms applying when expected profitability crosses a threshold. Notably, after program participation, R&D intensity stagnates relative to controls, suggesting supported firms do not continue building technological capabilities after receiving subsidies—a finding with important policy implications.

Panel (b) presents our main causal estimates. After removing confounding trends via 2SLS, pre-treatment coefficients are statistically indistinguishable from zero (joint Wald test $p = 0.35$), contrasting sharply with the severe pre-trends in panel (a). This validates our identification strategy.

Post-treatment effects are positive, significant, and persistent. The green horizontal line in panel (b) represents a static (constant) treatment effect specification, which provides a parsimonious summary of program impacts. A likelihood ratio test ($p = 0.10$) fails to reject the null hypothesis of constant treatment effects, supporting this specification. The static estimate indicates that export promotion programs increase the probability that a firm exports by 14 percentage points, with effects persisting for at least six years.

5.1.2 Accounting for Treatment Intensity and Misclassification

An important limitation of the estimates in Figure 4 is that they treat all program participants identically, regardless of how many times they received support. In practice, as documented in Section 3, there is substantial heterogeneity in treatment intensity: some firms receive support only once, while others participate in multiple programs over several years. The average firm in our treatment group received support 1.53 times, but the distribution ranges from 1 to 11 programs.

This heterogeneity raises two concerns. First, the binary treatment indicator confounds the effect of initial program participation with the effects of subsequent repeated support. If repeated participation has different (likely larger) effects than one-time support, our estimate of 14pp represents a weighted average across the intensity distribution, not the effect of marginal (first-time) program participation. Second, from a policy perspective, understanding the dose-response relationship is crucial: does providing multiple subsidies to the same firm generate proportionally larger effects, or do returns diminish?

To address this, we exploit the fact that the FHS methodology applies equally to continuous or multi-valued treatment variables, not just binary indicators. We redefine the treatment variable D_{it} to reflect intensity: for each treated firm, we calculate the average number of programs received per year (total number of programs divided by number of years with support),

and assign this value to D_{it} from the first year of participation onward.¹⁰ This specification allows us to estimate the marginal effect of an additional program, holding constant baseline program participation.

Figure 5 presents results using the intensity-adjusted treatment variable. Panel (a) again confirms that R&D intensity tracks outcome pre-trends very closely, validating the proxy even with continuous treatment. Panel (b) shows the dynamic treatment effects alongside the static estimate (green horizontal line). The static specification assumes constant marginal effects over time, which is not rejected by the data (p -value = 0.18). The static estimate indicates that each additional program a firm participates in increases its export probability by 8.3 percentage points.

This marginal effect of 8.3 percentage points per program is lower than the 5.7pp average treatment effect from Figure 4, as expected. The average treated firm participated in 1.53 programs, so the predicted effect at average intensity is $8.3 \times 1.53 \approx 12.7$ pp, which is lower than the binary treatment effect of 14pp. The difference likely reflects both diminishing returns (second and third programs may be less effective than the first, as firms have already overcome entry barriers) and composition effects (firms receiving multiple programs may be positively selected). However, even the marginal effect remains economically substantial.

An alternative way to address concerns about repeated treatment is to restrict the sample to firms that received support exactly once during 2011–2015 (46.2% of treated firms). This eliminates any confounding from multiple treatments and allows us to cleanly estimate the effect of a single program participation. Figure 6 panel (a) shows results are similar in magnitude to our main estimates: the static effect is 7pp, similar to the main estimate of 8.3pp. However, due to the substantially reduced sample size ($N=638$ treated firms vs. 1,433 in the main sample), confidence intervals widen considerably. We interpret this as consistent with our main findings—one-time participation generates meaningful effects.

We also address concerns about potential misclassification of treatment status. Our main analysis uses data through 2022, but we only observe program participation through 2015. If some control firms received support after 2015 (but we misclassify them as untreated), this would attenuate our estimates toward zero. To address this, Figure 6 panel (a) restricts all data to 2005–2015, the period when treatment status is fully observed. This specification limits the post-treatment horizon to 4 years but eliminates contamination risk. Results are qualitatively similar: the static effect is 6.7pp (vs. 8.3pp in the main sample), and effects remain statistically significant. The slightly smaller magnitude likely reflects the shorter horizon (effects may still be growing at year 4). We therefore conclude that our main results are not driven by misclassification of post-2015 participants.

¹⁰For example, if firm A received support from 2 programs in 2012 and 3 programs in 2014, it received 5 programs over 2 years of participation, yielding average annual intensity of 2.5. We set $D_{it} = 0$ for $t < 2012$ and $D_{it} = 2.5$ for $t \geq 2012$. This specification assumes persistent effects and abstracts from timing of multiple treatments, which is reasonable if fixed cost reductions (the program's mechanism) generate long-lasting entry.

To bracket our estimates, we interpret the findings as follows: export promotion programs increase export market entry by 5–10 *percentage points* for first-time participants, with effects potentially reaching 10–15 *percentage points* for firms receiving repeated support over multiple years. Given the modest cost of these programs, even the lower bound represents a cost-effective intervention for expanding the extensive margin of trade.

5.1.3 Number of Export Destinations

As a complementary extensive margin outcome, we examine whether programs help firms expand to multiple export markets. Geographic diversification is often viewed as a sign of export success, as it reduces dependence on any single market and may indicate broader product appeal.

Figure 7 presents results using $\text{arcsinh}(\text{number of destinations})$ as the outcome.¹¹ The results show that programs increase the number of export destinations by an increase of 0.12–0.18 countries (marginal effect for average intensity). Given that new exporters in our sample typically start with 1.2 countries on average and expand to 1.8 countries by year 4, an increase of 0.12–0.18 countries represents roughly 10–15% growth in geographic diversification—a meaningful though not transformative effect.

Taken together, the extensive margin results demonstrate that export promotion programs successfully help firms overcome entry barriers: both the probability of exporting and geographic diversification increase significantly and persistently. We now turn to examining whether these entry effects translate into sustained firm growth.

5.2 Intensive Margin Effects

Having established that export promotion programs successfully increase both export market entry and geographic diversification, we now examine their effects on firm growth and scaling up—the intensive margin. If programs not only help firms enter export markets but also enable them to grow substantially through exports, we should observe significant increases in sales and value added. Conversely, if programs merely facilitate initial market access without supporting subsequent competitive performance, intensive margin effects will be limited.

5.2.1 Effects on Sales

We begin by examining total firm sales, which encompasses both domestic and export revenues. While we lack firm-level export value data for our full sample, sales growth is a

¹¹We apply the inverse hyperbolic sine transformation: $\text{arcsinh}(x) = \ln(x + \sqrt{x^2 + 1})$. This transformation handles zeros gracefully (many non-exporters have zero destinations) while approximating log behavior for large values, making coefficients interpretable as semi-elasticities.

necessary condition for successful scaling up. We estimate Equation (1) with $\log(\text{sales})$ as the outcome variable, using the intensity-adjusted treatment variable (average programs per year) and $\log(\text{R&D expenditure})$ as the proxy. Using $\log(\text{R&D expenditure})$ rather than R&D intensity is more appropriate here because both the outcome and proxy are in logarithmic form, making coefficients interpretable as elasticities.

Figure 8 presents results for sales. Panel (a) provides the now-familiar diagnostic: $\log(\text{R&D expenditure})$ tracks the pre-trends in $\log(\text{sales})$ remarkably closely over the six years before program participation. Both series show treated firms starting 8–10% below control firms and the gap narrowing to 3–5% just before treatment. This tight correlation validates using R&D as the proxy for unobserved productivity and profitability shocks driving sales growth.

Interestingly, the post-treatment dynamics of R&D expenditure reinforce our earlier finding from export participation: R&D growth among treated firms plateaus after program participation. While treated firms maintain higher R&D levels than controls throughout (they are more R&D-intensive firms), the gap stops widening post-treatment. This stagnation in innovative investment may constrain firms' ability to compete in international markets, as we discuss below.

Panel (b) shows 2SLS event study estimates, displaying both dynamic treatment effects (point estimates for each year) and the static effect (green horizontal line representing a constant treatment effect). The effects of export promotion programs translate into sales growth over time, becoming statistically significant around year 4. Combined with our extensive margin results showing that programs increase export market entry probability by 5–10 percentage points (or expand the number of export destinations by 0.1–0.2 countries), the direction and timing of sales growth in panel (b) align well with these patterns.

However, the magnitude of sales growth is modest. Even at its peak in year 5, the growth rate remains below 5%, and subsequently declines. The static estimate of 2.4% (shown as the green horizontal line) provides a summary of the average effect across all post-treatment periods. This small effect suggests that while programs successfully facilitate export market entry, they do not trigger sustained high-growth trajectories.

5.2.2 Effects on Value Added

Value added—the difference between sales and intermediate input costs—provides a more meaningful measure of firm performance than sales alone. A firm can increase sales by purchasing more inputs without creating value; true growth requires higher productivity, quality, or moving up value chains.

Figure 9 shows striking results. Panel (a) confirms R&D expenditure tracks pre-trends in value added. Panel (b) shows 2SLS event study estimates (dynamic effects) along with the static effect. The static estimate is 1.2%, which is statistically insignificant ($p > 0.10$). We cannot reject the null hypothesis of no effect on value added.

The contrast between sales and value added effects is revealing. Sales increase modestly (2.4% on average), but value added does not. This pattern implies that supported firms expand sales primarily by increasing intermediate inputs rather than by creating more value. Mechanically, if a firm increases exports by purchasing more components and assembling them for foreign markets—without improving productivity or moving into higher-margin activities—sales will rise but value added will not. Our results suggest programs help firms access export markets but do not enable them to compete at higher value-added levels or capture greater margins in those markets.

This interpretation aligns with the R&D stagnation documented earlier. If supported firms do not continue investing in innovation and capability building after receiving entry subsidies, they may compete primarily on price in low-margin export segments, achieving volume growth (higher sales) without value creation (higher value added). The absence of value-added effects thus points to a fundamental limitation: export promotion programs reduce entry barriers but do not address the capability constraints and competitive challenges that determine whether firms can successfully scale up in international markets.

Taken together, the intensive margin results reveal a consistent pattern: export promotion programs generate modest sales increases (2–4%) but no significant value-added growth. This stands in sharp contrast to the substantial and persistent effects on the extensive margin documented in Section 5.1. Before drawing policy conclusions from this asymmetry, we examine the robustness of our findings to identification assumptions.

5.3 Robustness to Imperfect Instruments

While we argue that R&D intensity satisfies the exclusion restriction—affecting outcomes only through the unobserved confounder η_{it} —the assumption of strict exogeneity may be too strong. For instance, if program administrators partially observe firms’ R&D investments when making allocation decisions, or if R&D responds weakly to export subsidies through channels other than growth expectations, the instrument would be “plausibly exogenous” rather than perfectly exogenous. Following Conley et al. (2012), we conduct inference that allows for small violations of the exclusion restriction.

The Conley et al. approach specifies a set of plausible values for the direct effect of the instrument on the outcome, γ , in the model $y_{it} = \beta D_{it} + \gamma z_{it} + \varepsilon_{it}$, where z_{it} is our instrument (lead of R&D intensity). For each value of γ in this plausible set, we can compute the implied treatment effect β and construct confidence intervals that account for uncertainty about both β and γ . This yields a union of confidence intervals across all plausible γ values, providing inference robust to modest violations of exogeneity.

We implement this approach by considering a range of direct effects: $\gamma \in [-\delta_j, +\delta_j]$, where δ_j represents the maximum plausible direct effect of R&D leads on outcome j . Crucially, we calibrate δ_j separately for each outcome based on its own reduced-form correlation with the

instrument. Specifically, we compute $\delta_j = \rho \times |\text{RF}_j|$, where RF_j is the reduced-form coefficient from regressing outcome j on the instrument (with fixed effects and controls), and ρ represents the proportion of this reduced-form effect we allow as a direct violation. Our baseline specification uses $\rho = 0.25$ (25% of each outcome's reduced-form), allowing for substantial direct effects that vary naturally across outcomes. This outcome-specific calibration is important because different outcomes exhibit different levels of correlation with R&D intensity—export participation shows a reduced-form coefficient of 0.034, while the number of destinations shows 0.085, and sales and value added show 0.082 and 0.072 respectively. By scaling the plausible violation to each outcome's empirical relationship with the instrument, we ensure the sensitivity analysis is appropriately calibrated to the specific context of each dependent variable. We also examine more conservative ($\rho = 0.125$) and more liberal ($\rho = 0.50$) specifications to assess robustness to the choice of plausible range.

Table 1 presents results using the `plausexog` Stata package (Conley et al., 2012), which implements sensitivity analysis across three specifications. The table's first row reports the absolute value of the reduced-form coefficient ($|\text{RF}_j|$) for each outcome—the direct effect of the instrument (lead R&D intensity) on the outcome before controlling for treatment. These coefficients provide the benchmark for calibrating plausible violations. We test three levels of maximum direct effects: 12.5%, 25%, and 50% of each outcome's reduced-form coefficient. For each specification, the table reports the union of confidence intervals (UCI) for the treatment effect—the range of estimates compatible with the data across all γ values in the plausible set.

The results demonstrate remarkable robustness of our findings across all three specifications. Even when allowing direct instrument effects up to 50% of the reduced-form correlation, the UCI excludes zero for all four outcomes. Export participation shows particularly robust effects: under the baseline specification (25% of RF), the treatment effect confidence interval ranges from 0.108 to 0.226, entirely above zero despite allowing for substantial instrument violations. The number of export destinations exhibits even stronger robustness, with a UCI of [0.191, 0.387] under baseline assumptions. Intensive margin effects on sales (UCI: [2.758, 4.319]) and value added (UCI: [2.629, 4.021]) also remain statistically significant across all specifications, though with wider bounds reflecting greater uncertainty and the log scale of these outcomes. The conservative specification (12.5% of RF) produces narrower UCI bounds while maintaining significance, confirming that even minimal tolerance for exclusion restriction violations preserves our core findings. This pervasive robustness—with all outcomes significant even under liberal assumptions allowing 50% violations—provides strong reassurance that our results are not artifacts of overly restrictive identifying assumptions.

6 Do Exports Themselves Matter for Firm Growth?

Our finding of weak intensive margin effects for export promotion programs raises a critical interpretive question: are exports themselves ineffective at promoting firm growth, or do programs simply fail to help firms fully exploit export opportunities? This distinction has profound implications. If exports inherently provide little value for firm development, then even well-designed programs cannot be expected to generate substantial growth effects. Conversely, if exports do matter but programs provide insufficient support, this suggests opportunities for policy improvement through complementary interventions targeting capability building and competitive positioning.

To investigate this question, we treat export market entry itself, rather than program participation, as the "treatment" and estimate its causal effect on firm outcomes using the same FHS methodology. This analysis serves two purposes. First, it provides direct evidence on whether exporting promotes firm growth in our Korean manufacturing context. A large literature documents various mechanisms through which exporting can foster firm growth: learning-by-exporting and productivity improvements (De Loecker, 2013; Garcia-Marin and Voigtlander, 2019; Hahn, 2012), demand accumulation and market penetration (Arkolakis, 2010; Fitzgerald et al., 2024), information learning and sequential expansion (Albornoz et al., 2012), and quality upgrading through exposure to foreign competition and standards (Atkin et al., 2017).¹² Second, by comparing the effects of export participation (the activity programs aim to facilitate) with the effects of program participation (the policy intervention), we can assess whether programs successfully channel firms toward growth-enhancing activities or merely help marginal firms enter without enabling them to benefit from export opportunities.

6.1 Empirical Approach

We apply the identical FHS methodology used for program evaluation, but now define the "event" as a firm's first year of export participation (transition from non-exporter to exporter). The sample includes all 25,273 surviving manufacturing firms observed from 2005 to 2022, not just those potentially eligible for programs. This broader sample allows us to identify the average effect of exporting across the full distribution of Korean manufacturing firms, providing a benchmark for interpreting program effects.

The identification challenge is similar to program evaluation: firms self-select into exporting based on unobserved productivity and growth prospects, generating severe pre-trends that violate parallel trends assumptions. We again use R&D intensity as the proxy for unobserved confounders, instrumenting with its lead value in 2SLS estimation of Equation (1). The key difference is that the treatment variable D_{it} now indicates export status rather than

¹²Our analysis does not distinguish among these mechanisms—we simply test whether export participation generates growth, regardless of the underlying channel.

program participation.

One important caveat: this analysis estimates the effect of exporting among firms that choose to export, not the effect of randomly assigned exporting (which would require an RCT like [Atkin et al. \(2017\)](#)). Our estimates should be interpreted as the effect of export participation for the selected sample of firms whose productivity and profitability cross the export entry threshold. This selected sample likely experiences larger benefits than would a randomly chosen firm forced to export. Nevertheless, comparison with program effects remains valid: both estimate effects for self-selected participants (program participants vs. exporters), making them directly comparable for policy assessment.

6.2 Results: Export Participation Effects

Figure 10 presents 2SLS event study estimates for the effects of export market entry on firm growth, examining both sales (panel a) and value added (panel b). The treatment variable is a firm’s first year of export participation, and the sample includes all 25,273 surviving manufacturing firms observed from 2005 to 2022.

The results stand in contrast to program effects documented earlier. Panel (a) shows that export participation increases sales substantially, with effects becoming evident around year 3 and growing to 10–13% by years 6–9. Panel (b) shows that value added increases by a nearly identical magnitude, reaching 10–12% in the long run. Unlike program participants—who achieve modest sales growth (2.4%) without any significant value-added effects—firms that become exporters experience genuine value creation, not just intermediate input expansion. Both sales and value added grow proportionally, indicating that exporters benefit from accessing international markets through various channels: productivity improvements, scale economies, demand expansion, quality upgrading, or other mechanisms that enhance firm performance.

The magnitude and statistical precision of these estimates far exceed the program effects documented earlier (2.4% sales increase, 1.2% value added increase, latter insignificant). Export participation generates roughly 4–5 times larger sales effects and substantial value-added effects (10%) compared to insignificant effects from programs. This pattern confirms two critical findings:

First, exports matter. The notion that international market participation inherently fails to promote firm growth is decisively rejected. We find that Korean manufacturing firms experience substantial and sustained growth benefits from exporting—approximately 10% increases in both sales and value added. This effect size aligns with prior empirical evidence from various contexts and through various channels: [De Loecker \(2013\)](#) and [Garcia-Marin and Voigtlander \(2019\)](#) find 7–11% productivity gains for Slovenian exporters through learning-by-exporting mechanisms; [Hahn \(2012\)](#) estimates 8–12% TFP gains for Korean manufacturers; [Arkolakis \(2010\)](#) documents demand accumulation effects; and [Atkin et al. \(2017\)](#) shows qual-

ity upgrading from exposure to international standards. Our estimates capture the combined effect of these various mechanisms without distinguishing among them.

Second, program-supported firms underperform self-selecting exporters. The firms that programs help enter export markets experience only 20–25% of the sales growth and essentially none of the value-added growth achieved by firms that enter exports without program support. This performance gap cannot be attributed to differences in sample composition (both groups are drawn from surviving manufacturing firms) or estimation methodology (we use identical FHS specifications). The most plausible interpretation is that programs induce marginal firms to enter export markets—firms that are less prepared, less competitive, or operating in less favorable market segments than self-selecting exporters.

These findings offer clear guidance for the design of export promotion policy. Export subsidies effectively lower entry barriers but do little to strengthen the capabilities needed to sustain growth after entry. The key constraint is not access to foreign markets but firms' limited technological and organizational capacity to compete once they enter. To address this, export promotion programs may (i) target export-ready firms that meet basic capability thresholds, (ii) combine market-access assistance with complementary support for R&D, quality upgrading, and training, and (iii) adopt sequential designs that link continued support to demonstrated performance. More broadly, the contrast between program and export effects highlights a central principle of industrial policy: effective interventions must relax the binding constraints that ultimately limit firms' growth and international competitiveness.

7 Conclusion

This paper provides novel causal evidence on the effects of export promotion programs by addressing a fundamental identification challenge: firms self-select into these programs based on unobserved growth prospects and productivity, violating the parallel trends assumption underlying standard difference-in-differences estimation. We apply the methodology of Freyaldenhoven et al. (2019), exploiting R&D expenditure as a proxy for unobserved confounders and using its lead values as instrumental variables to consistently estimate treatment effects even in the presence of substantial pre-trends. This approach demonstrates practical utility for industrial policy evaluation, where self-selection problems are pervasive but suitable instruments are rarely available. Our findings complement recent evidence on export promotion effectiveness from diverse contexts (Buus et al., 2025; Munch and Schaur, 2018) and contribute to understanding how industrial policies shape export dynamics in developing economies (Reed, 2024).

Our analysis of South Korea's export promotion programs reveals significant heterogeneity across margins. On the extensive margin, program participation increases export market entry probability by 5–10 percentage points and expands the number of export destinations

by 0.1–0.2 countries—effects that persist for at least six years. However, impacts on the intensive margin are considerably weaker: sales growth attributable to the program is only 2–4%, and we find no significant effect on value added. In contrast, our analysis of export participation itself shows that exporting increases both sales and value added by approximately 10%, confirming that the limited intensive margin effects of programs stem from program design rather than exports being inherently ineffective for firm growth.

These findings carry important policy implications. While export promotion programs effectively reduce entry barriers and expand the extensive margin, subsidizing fixed costs alone is insufficient to generate sustained firm growth or productivity gains. Policymakers seeking to maximize aggregate export value or facilitate firm scaling should complement entry subsidies with capability-building programs targeting innovation, quality upgrading, and competitive positioning. The stagnation of R&D investment after program participation further suggests that export promotion should be better integrated with innovation policies to ensure that supported firms continue investing in technological capabilities. More broadly, our results underscore the importance of distinguishing between extensive and intensive margin effects in program evaluation, matching policy instruments to specific objectives, and employing rigorous causal methods that adequately address selection on unobservables.

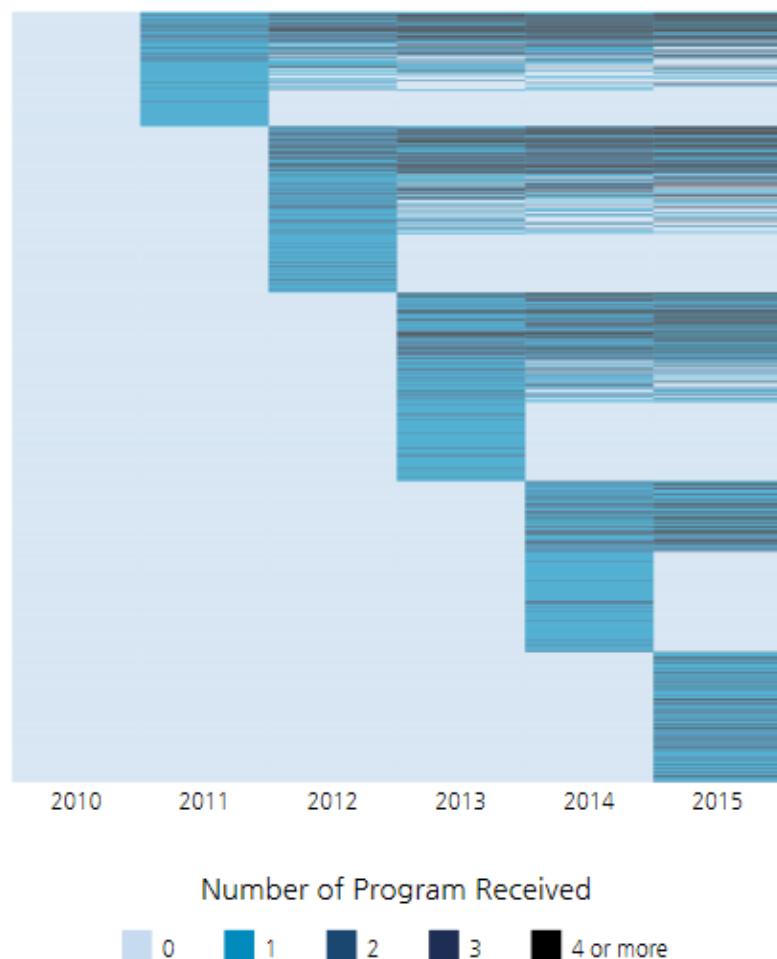
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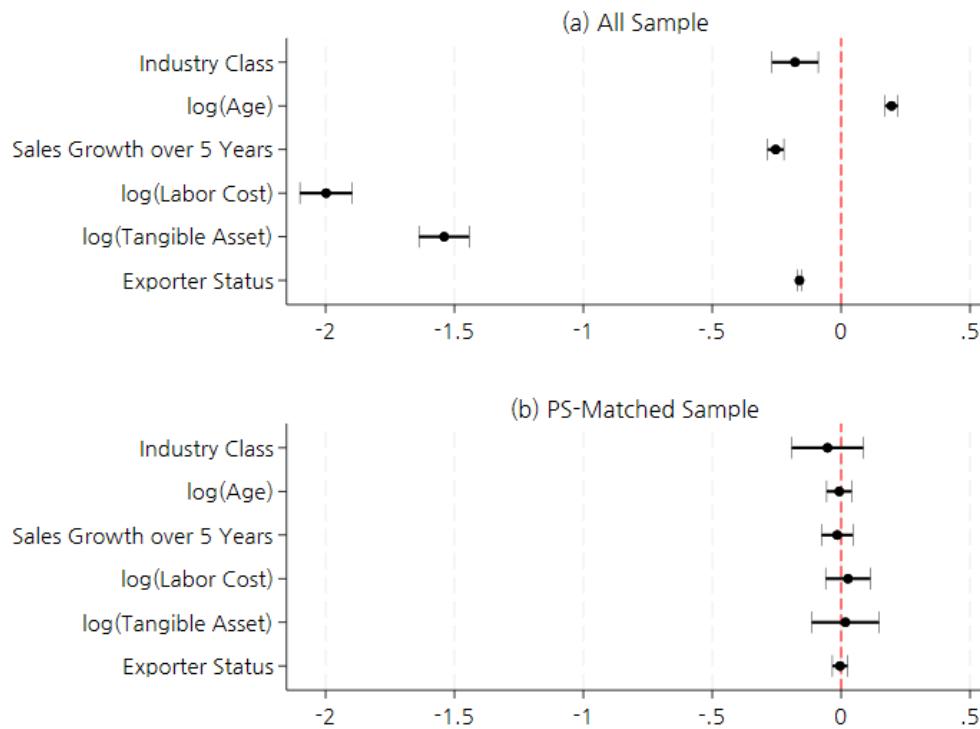
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Figure 1: Temporal Pattern of Program Participation



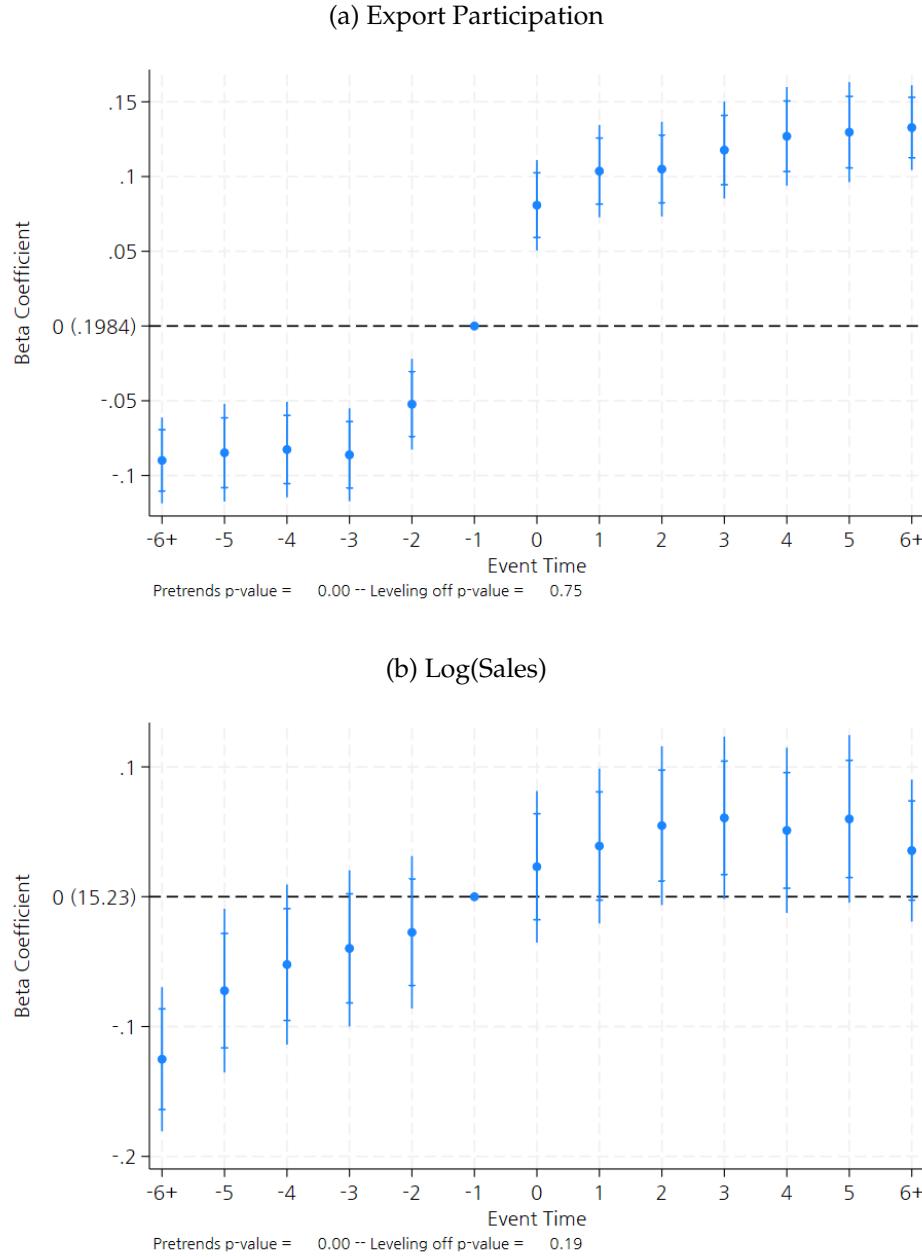
Notes: This figure illustrates the temporal pattern of export promotion program participation among treatment group firms ($N=1,433$). Each row represents a firm, sorted by the year of first program receipt. Color intensity indicates the number of programs received in each year, with darker colors representing more programs.

Figure 2: Propensity Score Matching: Covariate Balance



Notes: This figure demonstrates the effectiveness of propensity score matching. Panel (a) shows mean differences between treatment and control groups before matching for six covariates: industry (2-digit code), firm age, average sales growth (past 5 years), $\log(\text{labor costs})$, $\log(\text{tangible assets})$, and exporter status. Before matching, treatment and control firms differ substantially across all dimensions. Panel (b) shows that after 1:1 nearest neighbor matching, the two groups are statistically balanced across all six variables. None of the mean differences is statistically significant at conventional levels, and confidence intervals all include zero. Error bars represent 95% confidence intervals.

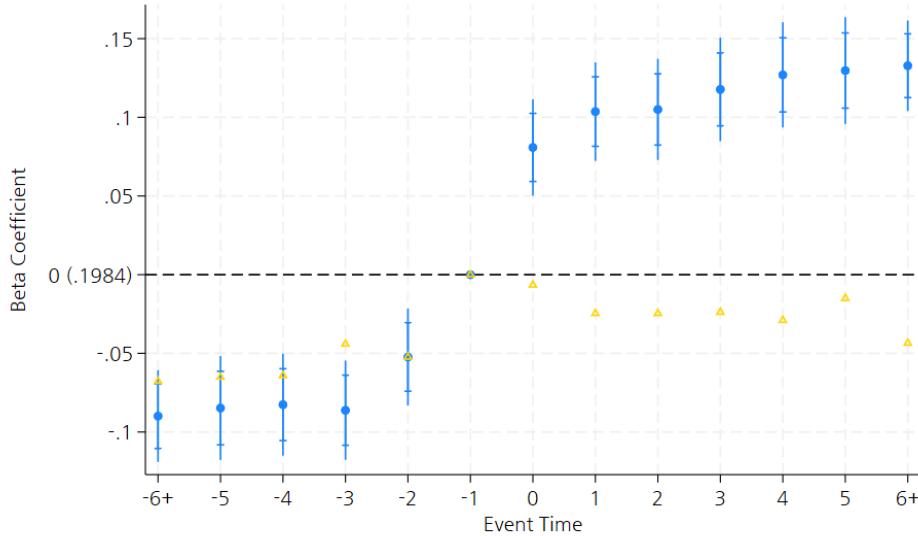
Figure 3: Pre-Trends in Matched Sample: OLS Event Study Estimates



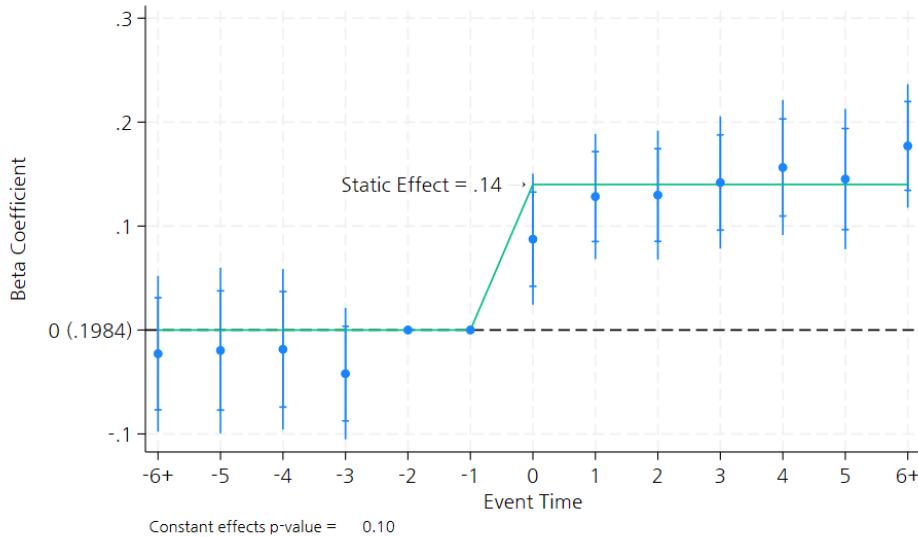
Notes: OLS event study estimates from propensity-score matched sample. Horizontal dashed line marks $t - 1$ outcome level. Vertical inner bars show 95% pointwise confidence intervals; outer lines show sup-t uniform bands. Joint Wald test rejects no pre-trends ($p < 0.01$), indicating selection on unobservables.

Figure 4: FHS Estimates: Effect on Export Participation

(a) Proxy Validation: Outcome and Proxy Overlay



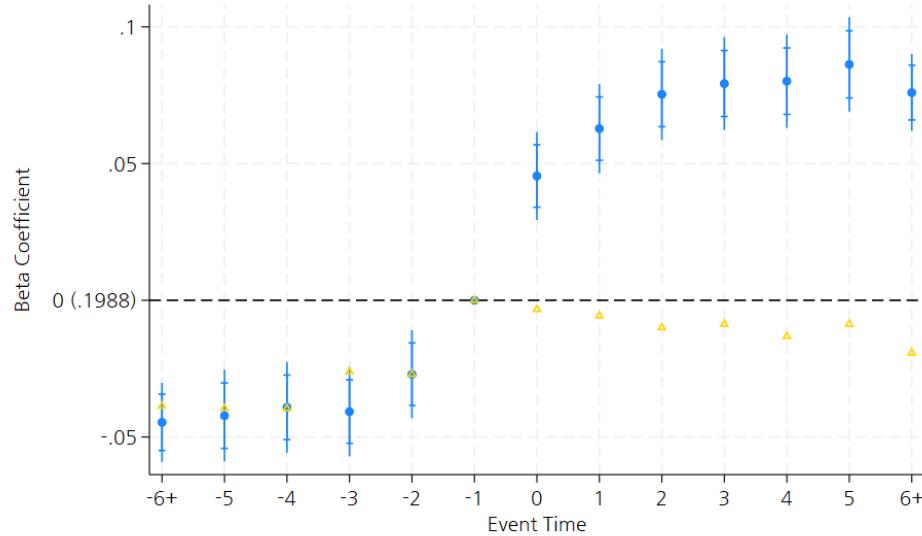
(b) 2SLS Estimates



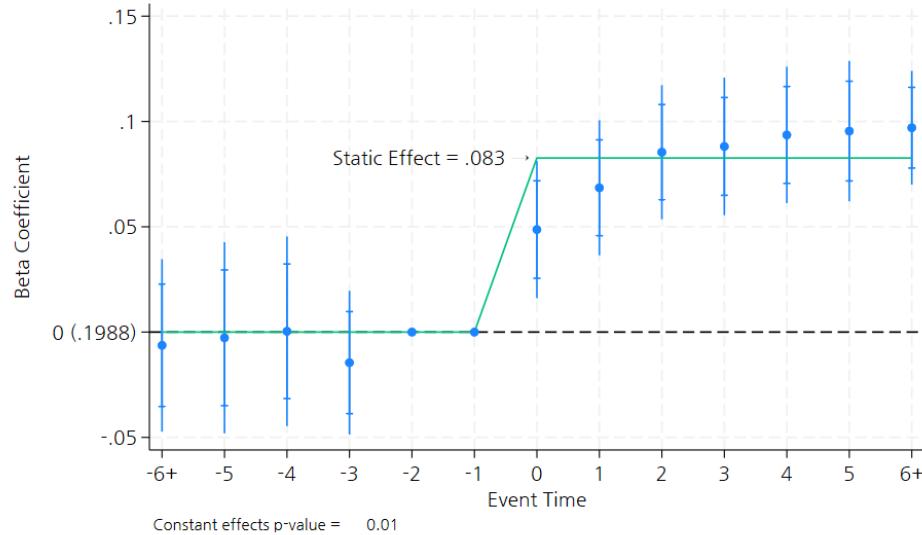
Notes: Panel (a) overlays outcome and proxy pre-trends to validate FHS identification. Panel (b) shows 2SLS event study estimates (dynamic effects). Vertical inner bars show 95% pointwise confidence intervals; outer lines show sup-t uniform bands. The green horizontal line represents the static estimate (0.14), which assumes constant treatment effects over time; this assumption is not rejected by the data ($p=0.10$).

Figure 5: FHS Estimates: Effect on Export Participation (Treatment Intensity)

(a) Proxy Validation: Outcome and Proxy Overlay



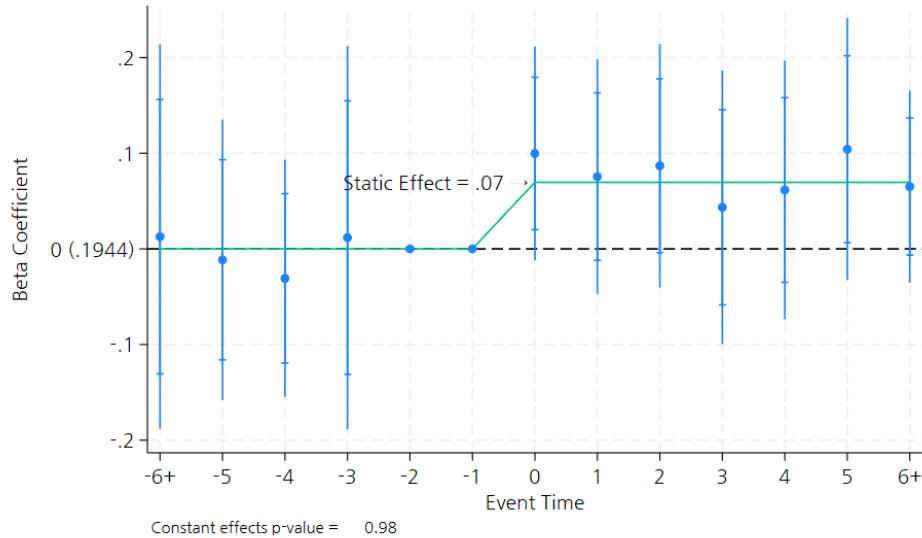
(b) 2SLS Estimates



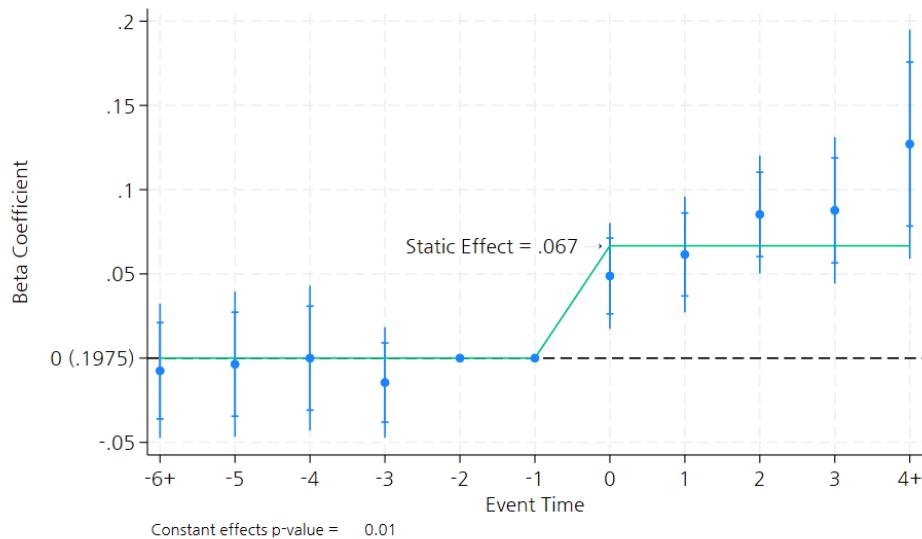
Notes: Panel (a) overlays outcome and proxy pre-trends. Panel (b) shows 2SLS event study estimates (dynamic effects) with continuous treatment intensity. Vertical inner bars show 95% pointwise confidence intervals; outer lines show sup-t uniform bands. The green horizontal line represents the static estimate (0.083), which assumes constant marginal effects over time; this assumption is not rejected by the data ($p=0.18$).

Figure 6: FHS Estimates: Effect on Export Participation (Alternative Samples)

(a) Sample of One-time Participants Only



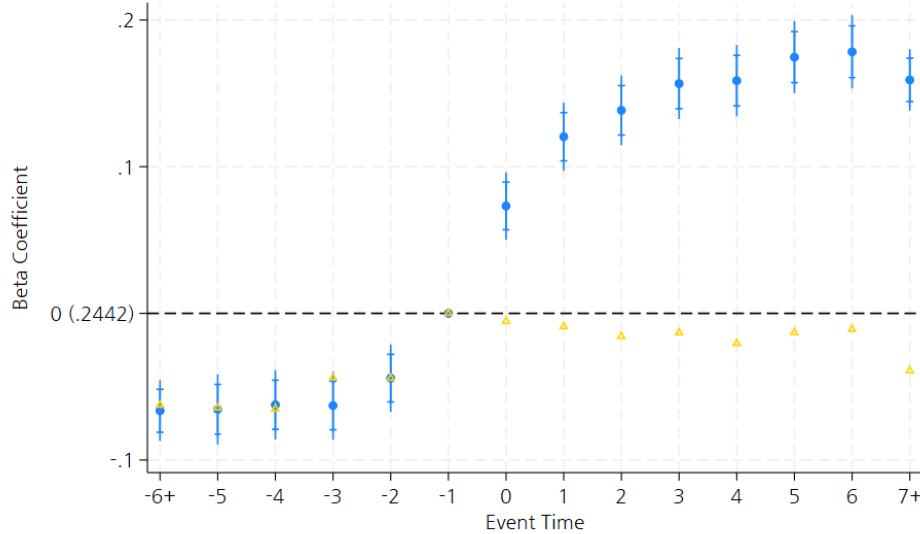
(b) Sample Period Restricted to 2005–2015



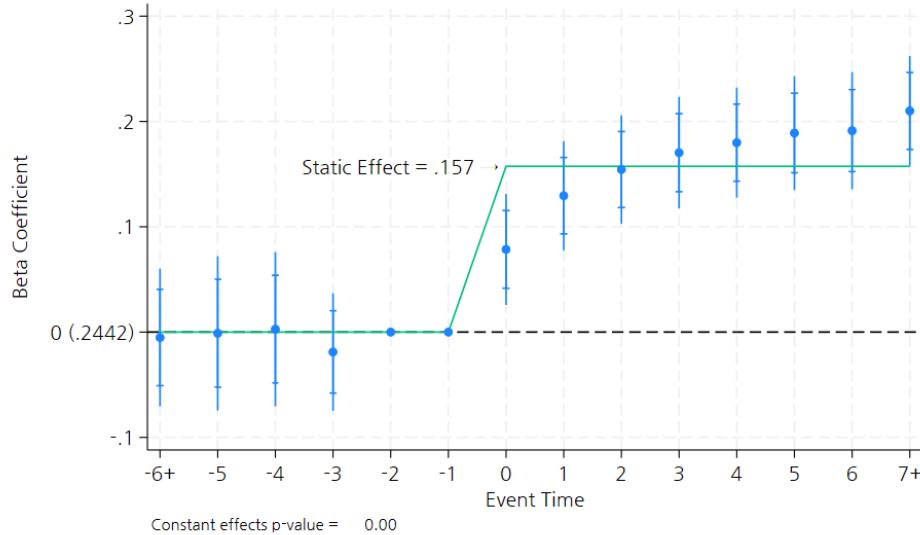
Notes: Both panels show 2SLS event study estimates (dynamic effects) with alternative samples: (a) One-time participants only (number of treated firms=638), (b) sample period restricted to 2005–2015. Vertical inner bars show 95% pointwise confidence intervals; outer lines show sup-t uniform bands. The green horizontal line represents the static estimate (0.07 in (a), 0.067 in (b)), which assumes constant marginal effects over time; this assumption is not rejected in (a), but rejected in (b).

Figure 7: FHS Estimates: Effect on Number of Export Destinations

(a) Proxy Validation: Outcome and Proxy Overlay



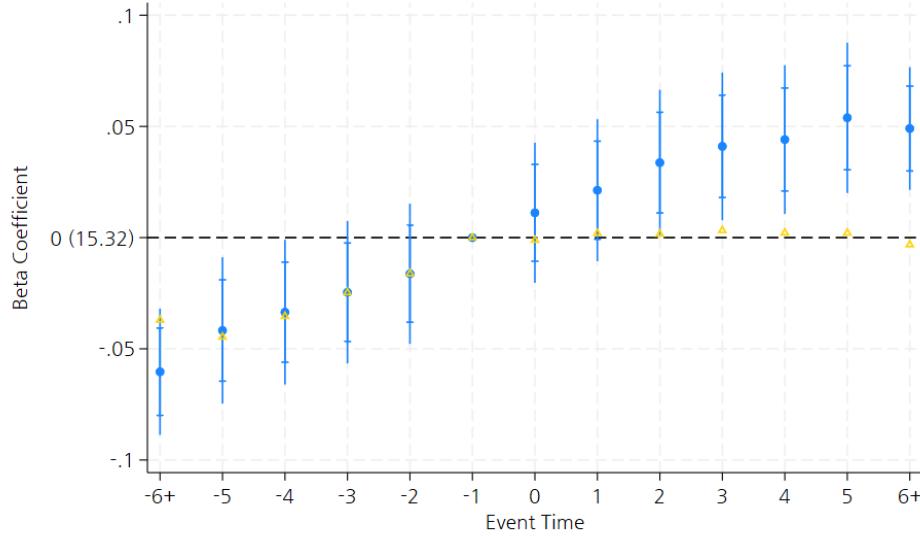
(b) 2SLS Estimates



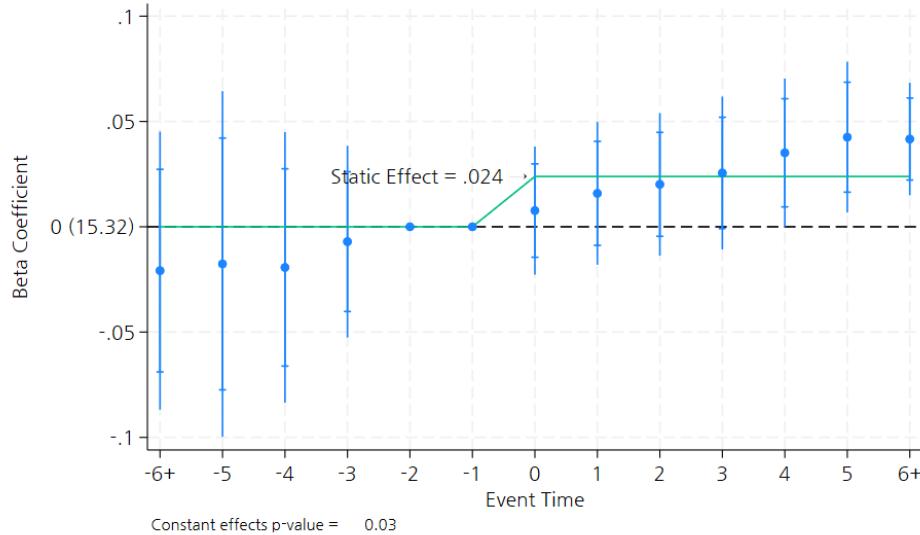
Notes: Panel (a) overlays outcome and proxy pre-trends. Panel (b) shows 2SLS event study estimates (dynamic effects) for arcsinh(destinations). Vertical inner bars show 95% pointwise confidence intervals; outer lines show sup-t uniform bands. The green horizontal line represents the static estimate (0.157), which assumes constant treatment effects over time; this assumption is rejected by the data ($p=0.00$).

Figure 8: FHS Estimates: Effect on Sales

(a) Proxy Validation: Outcome and Proxy Overlay



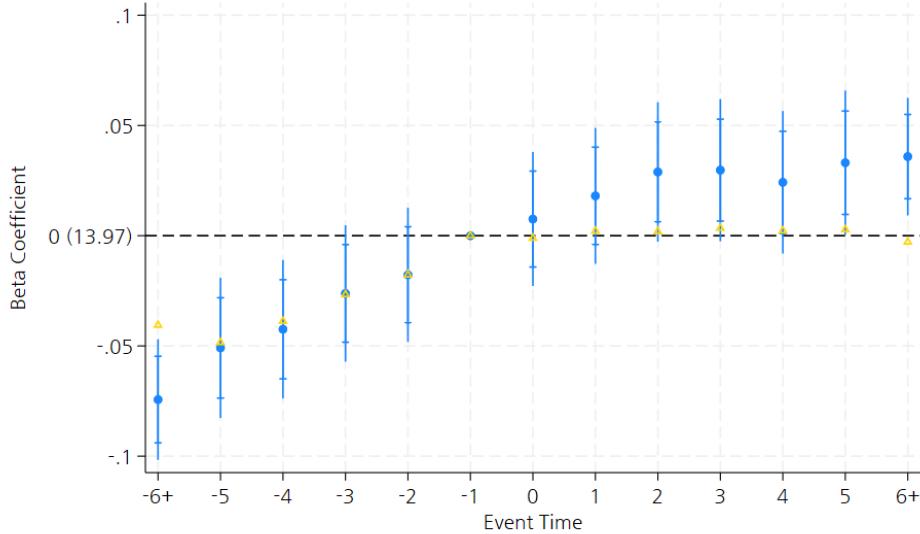
(b) 2SLS Estimates



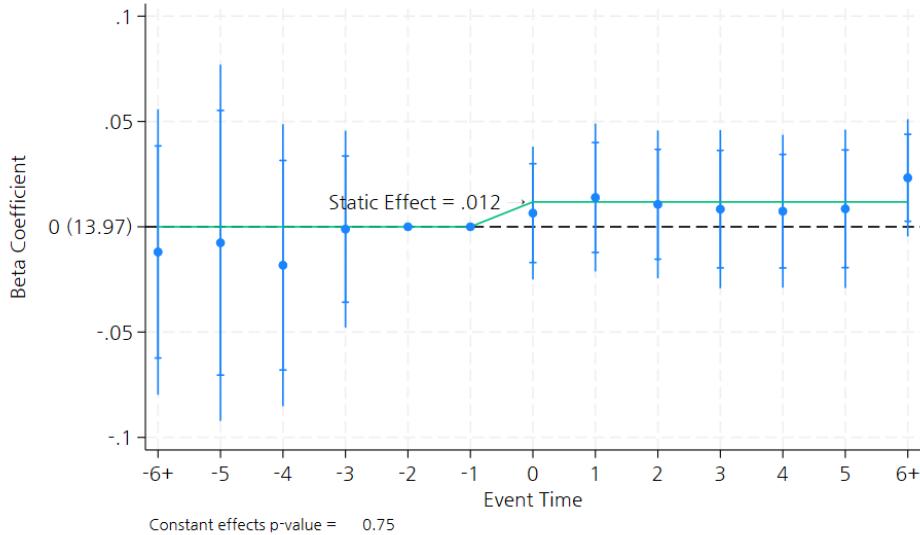
Notes: Panel (a) overlays log(sales) and log(R&D expenditure) pre-trends. Panel (b) shows 2SLS event study estimates (dynamic effects). Vertical inner bars show 95% pointwise confidence intervals; outer lines show sup-t uniform bands. The green horizontal line represents the static estimate (0.024), which assumes constant treatment effects over time; this assumption is rejected by the data ($p=0.03$).

Figure 9: FHS Estimates: Effect on Value Added

(a) Proxy Validation: Outcome and Proxy Overlay

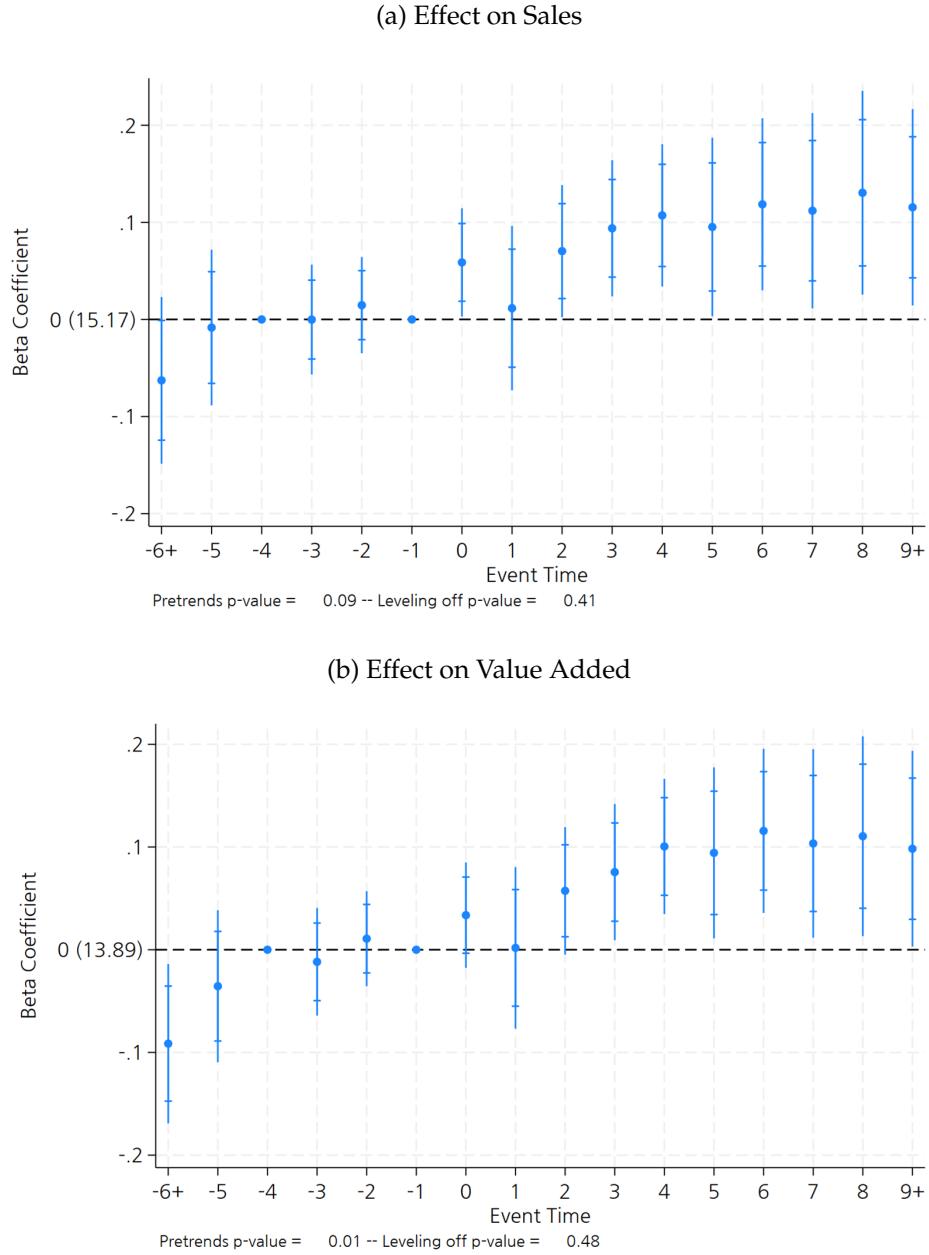


(b) 2SLS Estimates



Notes: Panel (a) overlays log(value added) and log(R&D expenditure) pre-trends. Panel (b) shows 2SLS event study estimates (dynamic effects). Vertical inner bars show 95% pointwise confidence intervals; outer lines show sup-t uniform bands. The green horizontal line represents the static estimate (0.012), which assumes constant treatment effects over time; this assumption is not rejected by the data ($p=0.75$).

Figure 10: FHS Estimates: Effects of Export Participation on Firm Growth



Notes: Treatment is export market entry (full sample of 25,273 firms, not restricted to program participants). Both panels show 2SLS event study estimates of dynamic treatment effects. Vertical inner bars show 95% pointwise confidence intervals; outer lines show sup-t uniform bands. Panel (a) shows export participation increases sales, with effects growing from near zero at entry to approximately 0.10–0.13 (10–13%) by years 6–9. Panel (b) shows similar patterns for value added, with effects reaching approximately 0.10–0.12 (10–12%) in the long run.

Table 1: Sensitivity Analysis of Imperfect Instruments

| Violation of Exclusion Restriction | Outcome Variable | | | |
|---------------------------------------|--------------------------------|----------------------------------|-------------------|----------------------------|
| | Export Participation (1) | Number of Destinations (2) | Log(Sales) (3) | Log(Value Added) (4) |
| | | | | |
| Estimated $ RF_j $ | 0.034 | 0.085 | 0.082 | 0.072 |
| UCIs for 12.5% of RF | [0.113, 0.221] | [0.203, 0.374] | [2.886, 4.154] | [2.738, 3.881] |
| UCIs for 25% of RF | [0.108, 0.226] | [0.191, 0.387] | [2.758, 4.319] | [2.629, 4.021] |
| UCIs for 50% of RF | [0.099, 0.236] | [0.167, 0.411] | [2.502, 4.649] | [2.410, 4.302] |

Notes: This table reports results from the Conley et al. (2012) approach for inference with plausibly exogenous instruments (R&D intensity or expenditure). The first row reports the absolute value of reduced-form coefficient ($|RF_j|$) for each outcome (the effect of the instrument on the outcome). We test three levels of maximum direct effects: 12.5%, 25%, and 50% of each outcome's reduced-form coefficient. For each outcome, we allow $\gamma_j \in [\pm \rho \times |RF_j|]$ where $\rho \in \{0.125, 0.25, 0.50\}$. Intervals in brackets represent the union of confidence intervals (UCIs) across all γ values in each plausible range (the union of 95% CIs for the treatment effect across all assumed direct instrument effects).