Trade Liberalization and Economic Growth: Evidence from WTO Accession

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Abstract This paper examines whether trade openness and World Trade Organization (WTO) accession promote GDP per capita growth using a panel of 141 countries from 1995 to 2025. I estimate five models that progressively address omitted-variable bias and endogeneity: pooled ordinary least squares, fixed effects, random effects, difference-in-differences exploiting the staggered timing of WTO accession, and instrumental variables using a Bartik shift-share built from initial trade exposure and leave-one-out world trade growth. The fixed-effects specification yields a coefficient of 0.0227 ($p < 0.01$): a 10 percentage point increase in trade openness is associated with about 0.227 percentage points of additional annual growth. This estimate is stable across robustness checks, including a log-trade specification, dropping oil exporters and city-states, and restricting the sample to 2000–2015. The difference-in-differences estimate is -1.1439 ($p < 0.05$), suggesting that acceding countries grew about 1.1439 percentage points more slowly per year after joining than original members did over the same years, a pattern most consistent with short-run adjustment costs and selection rather than a negative causal effect of WTO membership. The IV estimate is statistically imprecise: the Bartik first-stage F-statistic is 9.41, just below the Stock–Yogo cutoff of 10, and the estimate cannot reject the fixed-effects coefficient. The Hausman test rejects random effects in favor of fixed effects ($\chi^2(36) = 83.44$, $p < 0.001$). Trade openness is positively related to growth within countries, but WTO accession by itself did not generate an immediate growth dividend over 1995–2025.

Keywords: trade openness; WTO accession; economic growth; panel data; difference-in-differences; Bartik instrument.

JEL Codes: F13, F43, O47, C23.

1 Introduction

Does joining the World Trade Organization make countries richer? This paper investigates whether trade liberalization, specifically through accession to the WTO, promotes GDP per capita growth in acceding countries. The relationship between trade openness and economic growth has been one of the most debated questions in empirical macroeconomics since the 1990s. Sachs and Warner (1995) and Frankel and Romer (1999) report large positive effects of openness, yet Rose (2004) finds no evidence that WTO membership even increases bilateral trade. With more than 20 nations still outside the WTO system, resolving this question carries direct policy stakes: whether membership delivers real growth benefits shapes the decision for countries considering accession and affects the legitimacy of the institution more broadly.

My interest in this question is also personal. I was born in Vietnam in 2005, two years before the country acceded to the WTO in 2007. I grew up in an economy already reshaped by that decision, with manufacturing exports powering rapid growth and a rising middle class that my parents' generation could not have imagined. I wanted to understand whether this transformation was attributable to WTO membership, whether Vietnam would have grown regardless, and whether the same pattern holds across the dozens of other countries that joined during the same era.

Identifying the causal effect of trade on growth is difficult because the two are jointly determined. Reverse causality, omitted variables, and selection into membership all bias a simple regression of growth on trade. The central research question is therefore: to what extent does trade openness causally raise GDP per capita growth, and did the act of joining the WTO produce a measurable short-run growth dividend for acceding economies, once we control for unobserved country characteristics, common global shocks, and the endogeneity of trade?

To answer this question, I estimate a sequence of models that progressively impose more structure and address more sources of bias on a panel of 141 countries from 1995 to 2025 using World Development Indicators (WDI) data. I begin with a pooled OLS regression of growth on trade openness and a standard set of controls. I then exploit the panel structure with a country-and-year fixed-effects specification that absorbs all time-invariant country characteristics and all global shocks, and a random-effects model whose validity I test against fixed effects via the Hausman test. I exploit the staggered timing of WTO accession in a difference-in-differences design comparing the 35 post-1995 acceders to the original GATT/WTO members. Finally, I instrument trade openness with a Bartik shift-share that combines each country's initial trade exposure with leave-one-out world trade growth, addressing residual endogeneity that fixed effects alone cannot remove.

The results show that trade openness is positively and significantly related to growth within countries ($\hat{\beta} = 0.0227, p < 0.01$); that the DiD estimate of WTO accession is negative ($\hat{\beta} = -1.1439, p < 0.05$), most consistent with short-run adjustment costs and selection; and that the Bartik IV estimate is statistically imprecise but does not reject the fixed-effects coefficient. The fixed-effects estimate is robust to alternative functional forms, sample restrictions, and the exclusion of outlier economies. Specification tests support the modeling choices.

The rest of the paper is organized as follows. Section 2 reviews the theoretical and empirical literature on trade, growth, and WTO membership. Section 3 describes the dataset and presents summary statistics. Section 4 sets out the econometric framework and modeling strategy. Section 5 reports the empirical results. Section 6 concludes. Appendix A contains the tables and event-study figure; Appendix B summarizes the replication workflow.

2 Literature Review

2.1 Cross-Country Evidence on Trade and Growth

On the theoretical side, Grossman and Helpman (1991) develop endogenous-growth models in which trade raises long-run income through faster technology diffusion and access to a larger variety of intermediate inputs, motivating the empirical literature that follows. Frankel and Romer (1999) ask whether trade openness causes higher income. Using a cross-section of about 150 countries, they instrument bilateral trade flows with gravity-model predictions based on geography, population, area, and shared borders. Their IV estimates suggest that a one-percentage-point increase in the trade-to-GDP ratio raises per-capita income by roughly 2 percent, substantially larger than OLS. Like this paper, they use an IV strategy to address the endogeneity of trade. However, their cross-sectional design cannot control for unobserved country characteristics, and their geographic instruments are time invariant, so they capture long-run differences rather than policy changes. This paper improves on that design by using panel data with country fixed effects to absorb unobserved country-level factors and by using a time-varying Bartik instrument that responds to year-by-year world-trade fluctuations.

Sachs and Warner (1995) ask whether countries with open trade policies grow faster than closed economies. They classify countries as “open” or “closed” based on five criteria including tariff levels, non-tariff barriers, and black-market exchange rate premiums, and estimate cross-country growth regressions over 1970–1989. They find that open economies grew about 2.4 percentage points per year faster than closed ones. As in this paper, they study the relationship between openness and growth across a broad set of countries. However, their openness measure is a binary index that has been criticized for mixing trade policy with broader market-oriented reforms, and their single cross-section cannot account for time trends or country-specific unobserved factors. This paper addresses these issues by using a continuous trade-to-GDP ratio rather than a binary index and by exploiting an observable policy event, WTO accession, in a panel with country and year fixed effects.

2.2 The WTO and Trade Volumes

Rose (2004) asks whether WTO and GATT membership actually increase bilateral trade. Using a gravity model spanning 50 years and 175 countries, he finds no significant evidence that membership boosts trade volumes. Like this paper, Rose directly examines the effects of WTO membership rather than openness in general. However, his outcome variable is bilateral trade, so he can detect whether the WTO increases trade but not whether it affects broader economic outcomes. This paper addresses that limitation by using GDP per capita growth as the outcome, which captures all the channels through which WTO accession might affect the economy, including but not limited to trade volumes. I also use multiple estimation strategies–FE, DiD, and IV–rather than relying on a single gravity model.

Subramanian and Wei (2007) ask whether Rose’s null finding survives once one accounts for differences in how actively countries participated in WTO negotiations. Using an augmented gravity model, they find that WTO membership significantly increased imports for countries that actively negotiated reciprocal tariff reductions, but that the effect was negligible for developing countries that received special and differential treatment. Their estimates suggest membership roughly doubled trade for actively participating members. Like this paper, they focus on WTO accession effects specifically. However, like Rose, they study only trade volumes rather than growth. This paper examines GDP per capita growth as the outcome and uses a difference-in-differences design with country and year fixed effects. The 35 post-1995 acceders in my sample all underwent rigorous negotiations requiring real liberalization commitments, which, based on Subramanian and Wei's logic, should be the group most likely to show effects.

2.3 Contribution

A recurring methodological concern across this literature is the endogeneity of trade openness. Cross-sectional designs cannot control for unobserved country characteristics, gravity-based instruments are time invariant and absorbed by country fixed effects in panel data, and tariff-based instruments are themselves endogenous to political-economy considerations. Bartik-style shift-share instruments, formalized by Goldsmith-Pinkham et al. (2020), have emerged as a credible alternative because they combine a plausibly exogenous time-varying shock, world trade growth, with cross-sectional variation, initial exposure, that fixed effects do not absorb. This paper applies that approach by constructing a Bartik instrument from each country's 1995–1997 initial trade exposure and contemporaneous leave-one-out world trade growth, complementing the panel and DiD designs. Collectively, this combination of identification strategies allows me to triangulate the effect of trade and WTO accession on growth more carefully than any single design alone.

3 Data and Descriptive Statistics

3.1 Data Source and Sample

The data come from the World Development Indicators maintained by the World Bank, the standard cross-country panel used in the growth literature. I download annual observations on GDP per capita growth, trade-to-GDP, applied tariff rates, gross capital formation, government consumption, the GDP-deflator inflation rate, population growth, secondary school enrollment, and FDI net inflows for 1995 through 2025. Variable definitions appear in Table 1.

After dropping WDI regional and income-group aggregates, dropping observations with missing GDP per capita growth, and winsorizing GDP per capita growth and inflation at the 1st and 99th percentiles to limit the influence of extreme values, the working estimation sample contains 2,706 country-year observations across 141 countries. The panel is unbalanced because the WDI does not report every series for every country in every year. I declare the data as a panel with country and year as the two dimensions and use country-clustered standard errors throughout the panel and IV regressions to allow for arbitrary heteroskedasticity across countries and arbitrary serial correlation within countries.

For the difference-in-differences design, I exploit the staggered timing of WTO accession. Thirty-five countries acceded between 1996 and 2016. Because the panel begins in 1995, every acceding country has at least one pre-treatment year, although the length varies: Ecuador and Bulgaria, which joined in 1996, have only one pre-treatment year, while Vietnam has 12 years before its 2007 accession and 18 years after, China has 6 years before its 2001 accession and 24 years after, and Kazakhstan has 20 years before its 2015 accession and 10 years after. Most acceding countries joined between 2000 and 2016, providing at least five years of pre-treatment data. The 125 original GATT/WTO founding members serve as the control group. About 11.4 percent of country-year observations fall in the post-accession treated bin.

3.2 Summary Statistics and Group Comparisons

Table 2 reports descriptive statistics for the variables used in the analysis, computed on the same listwise-complete estimation sample of 2,706 country-year observations across 141 countries used in the Section 5 regressions, so that N is uniform across all variables. The applied tariff rate is not included in this table because it is used only as a diagnostic instrument and would otherwise have a different N. Average GDP per capita growth in the estimation sample is 2.27 percent, with a standard deviation of 4.11 and a range from -14.55 to 16.90. Average trade openness is 89.6 percent of GDP and varies considerably across countries; small open economies often trade well above 200 percent of GDP. The control variables behave as expected: gross capital formation averages 24.0 percent of GDP, government consumption 16.1 percent, inflation 5.93 percent, and population growth 1.27 percent. Because the estimation sample is restricted to WTO members and post-1995 acceders, about 95 percent of country-year observations are WTO members and 18.4 percent fall in the post-accession DiD treated bin.

Table 3 compares means across the post-accession treated bin and the control bin of original-member country-years using two-sample t-tests with unequal variances. Acceders post-accession grow noticeably faster on average than original members, are more open to trade, invest a larger share of GDP, have higher secondary enrollment, have lower inflation, and have markedly slower population growth. Their applied tariffs are slightly lower on average and FDI inflows slightly smaller, but those differences are smaller in magnitude. These raw mean differences make clear that the treated and control groups are not identical along observable dimensions, which motivates the panel and DiD designs in Section 4 that absorb time-invariant country characteristics rather than relying on simple group comparisons.

4 Econometric Model

4.1 Goal and Main Variables

The goal of this paper is to estimate the effect of trade openness, and separately of WTO accession, on GDP per capita growth. I use a panel of countries indexed by $i = 1, \dots, N$ and years indexed by $t = 1995, \dots, 2025$. The dependent variable is annual GDP per capita growth, $growth_{it}$. The key explanatory variable for the FE, RE, and IV models is trade openness, $trade_{it}$, measured as the sum of exports and imports of goods and services divided by GDP. For the DiD model the key regressor is the interaction $accgroup_i \times post_{it}$, which equals one in the years on or after a post-1995 acceder's accession year and zero otherwise. The standard set of time-varying controls X_{it} includes gross capital formation, government consumption, inflation, population growth, secondary school enrollment, and FDI inflows.

4.2 Pooled OLS Baseline

I begin with a pooled OLS regression of growth on trade openness, the standard controls, and year fixed effects:

$$growth_{it} = \alpha + \beta_1 trade_{it} + X_{it}'\gamma + \delta_t + u_{it}. \tag{M1}$$

Here δ_t is a year fixed effect that absorbs global shocks common to all countries, and u_{it} is the error term. Heteroskedasticity-robust standard errors are reported. The pooled OLS coefficient β_1 is likely biased because u_{it} contains time-invariant country characteristics, such as institutions, geography, and factor endowments, that are correlated with both trade and growth. Model 1 serves as a baseline rather than as a credible causal estimate.

4.3 Fixed Effects and Random Effects

To address bias from time-invariant unobservables, I add a country fixed effect α_i that absorbs all permanent country characteristics:

$$growth_{it} = \alpha_i + \delta_t + \beta_1 trade_{it} + X_{it}'\gamma + \varepsilon_{it}. \tag{M2}$$

The fixed-effects estimator identifies β_1 from within-country variation: it asks how growth in a given country changes when its own trade openness changes, after netting out global shocks and country averages. Standard errors are clustered at the country level. For comparison I also estimate a random-effects version of (M2) in which the country-specific component is treated as a random draw uncorrelated with the regressors. Random effects is more efficient than fixed effects if its assumption holds, but inconsistent if it does not. I use the Hausman test with the `sigmamore` option to decide between fixed and random effects; the standard Hausman test can give negative χ^2 values in unbalanced panels with clustered standard errors, and `sigmamore` corrects for this.

4.4 Difference-in-Differences and Event Study

To exploit the discrete event of WTO accession, I estimate a staggered difference-in-differences specification:

$$growth_{it} = \alpha_i + \delta_t + \beta_2(accgroup_i \times post_{it}) + X_{it}'\gamma + \varepsilon_{it}. \tag{M4}$$

The coefficient β_2 measures the differential change in growth for acceding countries relative to original members, before versus after each acceder's specific accession year. The identifying assumption is parallel trends: absent accession, the two groups would have followed similar growth paths, conditional on the controls and fixed effects. I assess this assumption with an event-study specification that replaces the single post-accession dummy with leads and lags relative to each country's accession year, omitting the year prior to accession, event time -1, as the reference. The joint F-test on the lead coefficients tests whether acceders were already on different growth paths before joining.

4.5 Instrumental Variables

To address residual endogeneity of trade openness that fixed effects alone cannot remove, I instrument $trade_{it}$ with a Bartik shift-share. The instrument is constructed as

$$Z_{it} = \overline{trade}_{i,1995\text{–}1997} \times g_t^{-i},$$

where $\overline{trade}_{i,1995\text{–}1997}$ is country i's average trade-to-GDP ratio over 1995–1997, its initial exposure to trade, and g_t^{-i} is the leave-one-out average annual growth rate of trade openness across all other countries in year t. Intuitively, countries that started out trading more in the mid-1990s are more exposed to subsequent global trade shocks, so the same world shock raises their trade openness more than that of less-exposed economies. The two-stage least squares system is

$$trade_{it} = \alpha_i + \delta_t + \pi Z_{it} + X_{it}'\rho + v_{it}, \tag{M5-FS}$$
$$growth_{it} = \alpha_i + \delta_t + \theta_1 \widehat{trade}_{it} + X_{it}'\gamma + \varepsilon_{it}. \tag{M5}$$

The exclusion restriction requires that Z_{it} affects growth only through trade, conditional on the controls and fixed effects. I assess instrument relevance using the first-stage F-statistic against the Stock and Yogo (2005) critical value of 10. I also include the weighted-mean applied tariff rate as a diagnostic instrument; its first-stage F-statistic is well below 1, so I rely on the Bartik in the main IV specification and report the tariff first-stage as a diagnostic only. I assess endogeneity using the Durbin–Wu–Hausman test reported by `estat endogenous` in Stata; Stock and Watson (2019) provide a textbook treatment of 2SLS and the associated diagnostic tests.

I expect heteroskedasticity in this panel because countries differ greatly in growth volatility and trade openness is itself a noisy regressor at country-year frequency. I therefore use heteroskedasticity-robust standard errors in (M1) and country-clustered standard errors in (M2)–(M5). As robustness checks I (i) replace trade openness with its natural log, (ii) drop nine outlier economies whose extreme openness or scale would otherwise dominate the within-country variation–oil exporters Qatar, Kuwait, the United Arab Emirates, Bahrain, and Brunei, and city-state economies Singapore, Hong Kong SAR, Luxembourg, and Macao SAR–and (iii) restrict the sample to the 2000–2015 window during which most accessions occurred and the panel is most balanced.

5 Results

5.1 Pooled OLS Baseline

Table 4, column 1, reports the pooled OLS specification (M1). The coefficient on trade openness is 0.0027 ($p < 0.05$): a 10 percentage point increase in trade as a share of GDP is associated with about 0.027 percentage points of additional annual growth. The control variables behave sensibly. Gross capital formation enters with a positive and highly significant coefficient; government consumption with a negative and highly significant coefficient; and population growth with a negative and highly significant coefficient. The pooled OLS coefficient on trade is small and likely biased downward because the specification fails to absorb permanent country characteristics correlated with both trade and growth, so I treat (M1) as a baseline rather than a credible causal estimate.

5.2 Panel Data: Fixed and Random Effects

Column 2 of Table 4 reports the fixed-effects specification (M2), my preferred panel model. The coefficient on trade openness rises to 0.0227 ($p < 0.01$). A 10 percentage point increase in trade openness within a country is associated with about 0.227 percentage points of additional annual GDP per capita growth, after netting out global shocks, permanent country characteristics, and the standard time-varying controls. Average growth in the sample is 2.273 percent, so 0.227 percentage points is roughly 10 percent of typical annual growth. The effect is economically meaningful, not just statistically significant. Standard errors are clustered at the country level. The within R^2 is 0.402.

The fixed-effects coefficient is broadly in line with prior cross-country evidence. Sachs and Warner reported that open economies grew about 2.4 percentage points per year faster than closed ones; my coefficient of 0.0227 implies that the roughly 100-percentage-point gap in trade openness separating a relatively closed economy from a highly open one in my sample would generate a 2.27 percentage-point growth differential, similar in magnitude but identified from within-country variation rather than a binary cross-country classification. Frankel and Romer's IV estimate of a 2 percent income gain from a 1-percentage-point increase in trade-to-GDP is a long-run level effect on income rather than an annual growth effect, so it is not directly comparable in magnitude, but the positive sign agrees with my result.

Column 3 reports the random-effects specification (M3). The coefficient on trade openness is 0.0054

($p < 0.10$), much smaller than the FE estimate. Whether (M3) is consistent depends on whether country effects are uncorrelated with the regressors. The Hausman test reported in Table 5 yields $\chi^2(36) = 83.44$ with $p < 0.001$, strongly rejecting random effects in favor of fixed effects. I therefore treat (M2) as my preferred specification. The modified Wald test for groupwise heteroskedasticity in the FE residuals gives $\chi^2(141) = 14{,}157.40$ with $p < 0.001$, decisively rejecting homoskedasticity. The Breusch–Pagan/Cook–Weisberg test on (M1) similarly rejects homoskedasticity. These results justify the use of robust and country-clustered standard errors. The joint F-test on the time-varying controls in (M2) gives $F(6, 140) = 22.36$ with $p < 0.001$, confirming that the controls are jointly relevant.

5.3 Difference-in-Differences and Event Study

Column 4 of Table 4 reports the DiD estimate (M4). The coefficient on the accession treatment is -1.1439 ($p < 0.05$): acceding countries grew about 1.1439 percentage points more slowly per year after accession than original members did over the same years, conditional on country fixed effects, year fixed effects, and the standard controls. The 1.1439 coefficient is roughly half of average annual growth in the sample. A magnitude that large is hard to read as a clean causal effect of accession alone, which supports the interpretation of adjustment costs and selection. The control coefficients in (M4) are nearly identical to those in (M2), suggesting that the negative treatment estimate is driven by the accession event rather than by changes in the controls.

I interpret the negative DiD result not as evidence that WTO membership reduces growth but as a combination of two factors. First, the post-1995 acceders are largely transition economies and large developing economies undergoing simultaneous structural changes. Privatization, financial-sector liberalization, and currency-regime transitions all affected these countries during the same period in which they joined the WTO. Second, WTO accession itself imposes adjustment costs in the short run: tariff bindings, intellectual-property enforcement, services-market openings, and customs harmonization can reduce protected-sector output before reallocation generates offsetting gains. The DiD estimate captures the net of these forces over the immediate post-accession years, not a causal effect of WTO membership in isolation.

Table 6 reports the event-study specification. The lead coefficients–years -4 through -2, with -1 omitted as the reference–are individually small and not statistically significant. The joint F-test on the leads is $F(3, 140) = 2.644$ with a p-value of 0.0517, which fails to reject parallel pre-trends at the 5 percent level but is close enough that the assumption should be treated cautiously. Figure 1 plots the lead and lag coefficients with 95 percent confidence intervals constructed from country-clustered standard errors. The pre-period coefficients hover around zero, while the post-period coefficients are flat or slightly negative, with no visible growth dividend that builds up over time. Given the borderline pre-trend test, I interpret the DiD coefficient as suggestive rather than as a clean causal estimate.

5.4 Instrumental Variables

Column 5 of Table 4 reports the IV-2SLS estimate (M5) using the Bartik shift-share. The first-stage F-statistic reported in Table 5 is 9.41 ($p = 0.0026$), just below the Stock and Yogo (2005) critical value of 10 and therefore indicating a borderline weak instrument. The original tariff instrument has a first-stage F-statistic of only 0.42, indicating effectively no predictive power once country and year fixed effects are absorbed. I therefore report it only as a diagnostic and do not use it in the main IV specification.

The second-stage coefficient on instrumented trade openness is 0.0014 with a clustered standard error of 0.0565. The IV point estimate is much smaller than the FE estimate, but the standard error is also nearly ten times larger, so the IV confidence interval comfortably contains both zero and the FE point estimate. The Durbin–Wu–Hausman endogeneity test gives $F(1, 132) = 0.100452$ with $p = 0.7518$, which fails to reject the null that trade openness is exogenous, but with a borderline first stage this test itself is imprecise. I interpret (M5) as imprecise rather than as evidence against (M2). The IV estimate is best read as a robustness check that does not contradict the fixed-effects coefficient, rather than as a clearly preferred causal estimate.

5.5 Robustness Checks

Table 7 reports three robustness variations of the fixed-effects specification. Column 1 replaces trade openness with its natural log; the coefficient on log trade is 2.7515 ($p < 0.01$), which translates to a comparable level-equivalent effect at the sample mean and confirms that the positive trade-growth relationship is not an artifact of the linear functional form. Column 2 drops the nine oil-exporter and city-state economies described in Section 4; the FE coefficient on trade is 0.0254 ($p < 0.01$), almost identical to the baseline 0.0227. Column 3 restricts the sample to 2000–2015, the window in which most accessions occurred and the panel is most balanced; the FE coefficient is 0.0239 ($p < 0.01$), again close to the baseline. All three estimates lie within a tight range of the main FE result and remain statistically significant, so the positive trade-growth relationship is not driven by unusual economies, extreme observations, or any particular subperiod.

6 Conclusion

This paper examines whether trade openness and WTO accession promote GDP per capita growth using a panel of 141 countries from 1995 to 2025 drawn from the World Development Indicators. In the preferred fixed-effects specification, the coefficient on trade openness is 0.0227 ($p < 0.01$), implying that a 10 percentage point increase in trade as a share of GDP within a country is associated with about 0.227 percentage points of additional annual growth. This estimate is essentially unchanged in three robustness variations: a log-trade specification, a sample that drops nine oil-exporter and city-state outliers, and a sample restricted to 2000–2015. The Hausman test strongly rejects random effects in favor of fixed effects, the modified Wald test rejects homoskedasticity, and the joint F-test on the time-varying controls confirms their joint relevance.

The difference-in-differences estimate of -1.1439 ($p < 0.05$) implies that acceding countries grew about 1.1439 percentage points more slowly per year after joining the WTO than original members over the same years. I interpret this not as a negative causal effect of membership but as a combination of short-run adjustment costs–tariff bindings, services-market openings, and customs harmonization–and selection: the post-1995 acceders are largely transition economies and large developing economies that were undergoing simultaneous structural changes around the time they joined. The event-study test for parallel pre-trends fails to reject the null at the 5 percent level but is borderline, so the DiD coefficient is suggestive rather than a clean causal estimate. The instrumental-variables estimate using a Bartik shift-share is statistically imprecise. The first-stage F-statistic of 9.41 is just below the weak-instrument threshold, and the second-stage confidence interval is wide enough to cover both zero and the FE point estimate. The IV result therefore neither confirms nor rejects the FE coefficient and is best read as a robustness check that does not overturn it.

The analysis has three main limitations. First, the panel is unbalanced, and the working sample is reduced to 2,706 country-year observations after dropping observations with missing controls. Second, the DiD design relies on a parallel-trends assumption that the lead test only marginally satisfies. Third, the Bartik instrument is borderline weak, so the IV results should not be used to overturn the FE or DiD estimates. For Vietnam specifically, which acceded in 2007, the results are a useful reminder that WTO accession was part of a much broader reform program and that attributing the country's subsequent growth to that single event would be premature.

Two extensions would further sharpen the causal interpretation. First, a stronger instrumental variable could raise the first-stage F-statistic above the weak-instrument threshold and, if overidentified, allow a Sargan or Hansen test of instrument validity that the just-identified Bartik design does not permit. Second, the staggered nature of accession and the heterogeneity of acceding economies invite a heterogeneous-treatment-effects analysis that traces out short-run versus longer-run effects across cohorts of acceders, for example by separating transition economies from non-transition acceders. The central finding remains: trade openness is positively and robustly related to growth within countries, while WTO accession by itself did not produce an immediate growth dividend over 1995–2025.

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A Tables and Figure

Table 1: Description of Variables

Variable	Definition	Units	Source	Role
gdp_pc_growth	GDP per capita growth, annual percent, winsorized at the 1st and 99th percentiles	Percent	WDI	Dependent variable
trade_pct_gdp	Exports plus imports of goods and services as a share of GDP	Percent of GDP	WDI	Key regressor
ln_trade	Natural log of trade openness	Log percent	Constructed	Robustness
tariff_rate	Applied weighted-mean tariff rate	Percent	WDI	Diagnostic IV
gfcf_pct_gdp	Gross capital formation as a share of GDP	Percent of GDP	WDI	Control
govt_consumption	General government consumption as a share of GDP	Percent of GDP	WDI	Control
inflation_deflator	Inflation, GDP deflator, annual percent, winsorized at the 1st and 99th percentiles	Percent	WDI	Control
pop_growth	Population growth	Percent	WDI	Control
sec_enrollment	Gross secondary school enrollment ratio	Percent	WDI	Control
fdi_pct_gdp	FDI net inflows as a share of GDP	Percent of GDP	WDI	Control
wto_member	Indicator equal to one if a country is a WTO member in year *t*	0/1	WTO	Indicator
accession_group	Indicator equal to one if a country acceded after January 1, 1995	0/1	WTO	DiD treated
post_accession	Indicator equal to one in years on or after a country's accession year	0/1	WTO	DiD post
wto_accession_did	accession_group × post_accession	0/1	Constructed	DiD treatment
bartik_trade_iv	Initial trade exposure, 1995–1997, multiplied by leave-one-out world trade growth	Index	Constructed	Bartik IV

Sources: World Bank, World Development Indicators; World Trade Organization; and author's Stata constructions.

Table 2: Descriptive Statistics

Variable	Mean	SD	Min	Max	N
GDP per capita growth (%)	2.273	4.109	-14.55	16.90	2,706
Trade (% of GDP)	89.64	54.12	21.38	442.6	2,706
Gross capital formation (% GDP)	23.95	6.767	1.429	60.16	2,706
Government consumption (% GDP)	16.07	5.432	4.354	46.26	2,706
Inflation, GDP deflator (%)	5.926	9.157	-11.68	112.1	2,706
Population growth (%)	1.271	1.552	-10.93	21.70	2,706
Secondary enrollment (%)	82.42	30.02	5.044	164.1	2,706
FDI, net inflows (% GDP)	6.231	27.92	-391.6	452.2	2,706
WTO member	0.951	0.216	0	1	2,706
DiD treatment	0.184	0.387	0	1	2,706

Source: World Development Indicators, World Bank, 1995–2025. The sample contains 2,706 country-year observations across 141 countries and is the listwise-complete estimation sample used in the main regressions. GDP per capita growth and inflation are winsorized at the 1st and 99th percentiles. The applied tariff rate is excluded because it is used only as a diagnostic instrument and has a non-uniform N.

Table 3: Comparison of Means by Treatment Group

Variable	Treated mean	Control mean	Difference	p-value
GDP per capita growth (%)	3.010	1.875	1.135	< 0.001
Trade (% of GDP)	98.017	84.098	13.919	< 0.001
Tariff rate (%)	5.591	6.598	-1.006	0.211
Gross capital formation (% GDP)	27.960	22.617	5.343	< 0.001
Government consumption (% GDP)	16.138	15.820	0.318	0.140
Inflation, GDP deflator (%)	6.049	7.456	-1.407	< 0.001
Population growth (%)	0.767	1.506	-0.739	< 0.001
Secondary enrollment (%)	89.927	80.632	9.295	< 0.001
FDI, net inflows (% GDP)	5.011	6.874	-1.862	0.006

Two-sample t-tests use unequal variances. The sample is restricted to country-years with WTO membership. Treated denotes post-accession years for the 35 post-1995 acceders. Control denotes original GATT/WTO members. Difference is treated mean minus control mean.

Table 4: Main Regression Results: GDP per Capita Growth

Variable	(1) M1 Pooled OLS	(2) M2 Fixed effects	(3) M3 Random effects	(4) M4 DiD-FE	(5) M5 IV-2SLS
Trade (% of GDP)	0.0027**	0.0227***	0.0054*		0.0014
	(0.0012)	(0.0059)	(0.0030)		(0.0565)
	[0.020]	[0.000]	[0.073]		[0.980]
DiD treatment				-1.1439**	
				(0.5734)	
				[0.048]	
Gross capital formation	0.1271***	0.1240***	0.1272***	0.1283***	0.1263***
	(0.0114)	(0.0203)	(0.0172)	(0.0206)	(0.0206)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Government consumption	-0.1493***	-0.2952***	-0.1897***	-0.3058***	-0.3125***
	(0.0146)	(0.0568)	(0.0323)	(0.0580)	(0.0543)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Inflation	-0.0195	-0.0469**	-0.0338**	-0.0452**	-0.0385*
	(0.0137)	(0.0185)	(0.0155)	(0.0182)	(0.0204)
	[0.157]	[0.012]	[0.030]	[0.014]	[0.060]
Population growth	-0.7355***	-0.8084***	-0.7598***	-0.7914***	-0.7895***
	(0.0583)	(0.1258)	(0.0921)	(0.1216)	(0.1261)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Secondary enrollment	-0.0091***	0.0036	-0.0068	-0.0004	0.0071
	(0.0030)	(0.0106)	(0.0054)	(0.0100)	(0.0120)
	[0.002]	[0.735]	[0.204]	[0.968]	[0.557]
FDI inflows	0.0050**	0.0017	0.0025	0.0015	0.0014
	(0.0024)	(0.0026)	(0.0023)	(0.0025)	(0.0024)
	[0.039]	[0.520]	[0.275]	[0.557]	[0.559]
Constant	2.4831***	3.0947**	3.0377***	5.2846***	4.4710
	(0.8114)	(1.4873)	(1.0204)	(1.3810)	(5.2005)
	[0.002]	[0.039]	[0.003]	[0.000]	[0.390]
Observations	2,706	2,706	2,706	2,706	2,604
R^2 / within R^2	0.398	0.402	0.396	0.396	0.497
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Country fixed effects	No	Yes	RE	Yes	Yes
Instrument	–	–	–	–	Bartik

Robust standard errors are reported in column 1; country-clustered standard errors are reported in columns 2–5. Standard errors are in parentheses and p-values are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 5: Specification and Diagnostic Tests

Test	Statistic	p-value
Hausman test, FE vs. RE (`sigmamore`)	$\chi^2(36) = 83.44$	< 0.001
Joint F-test on time-varying controls (FE)	$F(6, 140) = 22.36$	< 0.001
Breusch–Pagan/Cook–Weisberg (pooled OLS)	$\chi^2(36) = 178.57$	< 0.001
Modified Wald, groupwise heteroskedasticity (FE)	$\chi^2(141) = 14{,}157.40$	< 0.001
First-stage F, Bartik IV	$F(1, 132) = 9.41$	0.0026
First-stage F, tariff IV (diagnostic only)	$F(1, 137) = 0.42$	0.5159
Pre-trend joint test, event-study leads	$F(3, 140) = 2.644$	0.0517
Durbin–Wu–Hausman endogeneity, IV vs. FE	$F(1, 132) = 0.100452$	0.7518

p-values are from Stata output. The Bartik first-stage F-statistic is just below the Stock–Yogo weak-instrument rule-of-thumb value of 10.

Table 6: Event Study Around WTO Accession

Event-time indicator	Coefficient	SE / p-value	
4+ years before accession	1.0530	(0.7864)	[0.183]
3 years before accession	-1.9919	(1.3699)	[0.148]
2 years before accession	-0.1763	(0.8391)	[0.834]
Accession year	-0.6171	(0.8537)	[0.471]
1 year after accession	0.0902	(1.4002)	[0.949]
2 years after accession	-0.4795	(1.1436)	[0.676]
3+ years after accession	-1.0814	(0.8258)	[0.193]
Observations	2,706		
Number of countries	141		
Pre-trend joint F	2.644		
Pre-trend joint p-value	0.0517		

Estimated by fixed effects with country-clustered standard errors. Standard time-varying controls are included. The omitted reference event year is -1.

Table 7: Robustness Checks: Fixed-Effects Specification

	(1) Log Trade	(2) Drop Oil/City-States	(3) 2000–2015
ln(Trade % of GDP)	2.7515***		
	(0.6099)		
	[0.000]		
Trade (% of GDP)		0.0254***	0.0239***
		(0.0066)	(0.0084)
		[0.000]	[0.005]
Gross capital formation	0.1203***	0.1421***	0.1732***
	(0.0201)	(0.0203)	(0.0279)
	[0.000]	[0.000]	[0.000]
Government consumption	-0.3076***	-0.2658***	-0.2364***
	(0.0574)	(0.0466)	(0.0543)
	[0.000]	[0.000]	[0.000]
Inflation	-0.0487***	-0.0477**	-0.0135
	(0.0186)	(0.0194)	(0.0229)
	[0.010]	[0.015]	[0.558]
Population growth	-0.8061***	-0.9929***	-0.9800***
	(0.1257)	(0.1206)	(0.1131)
	[0.000]	[0.000]	[0.000]
Secondary enrollment	0.0037	-0.0014	0.0337*
	(0.0104)	(0.0106)	(0.0179)
	[0.722]	[0.892]	[0.063]
FDI inflows	0.0015	-0.0005	-0.0080**
	(0.0023)	(0.0014)	(0.0037)
	[0.516]	[0.707]	[0.031]
Observations	2,706	2,529	1,653
Within R^2	0.406	0.417	0.336
Number of countries	141	132	136

Country and year fixed effects are included in all columns. Country-clustered standard errors are in parentheses and p-values are in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

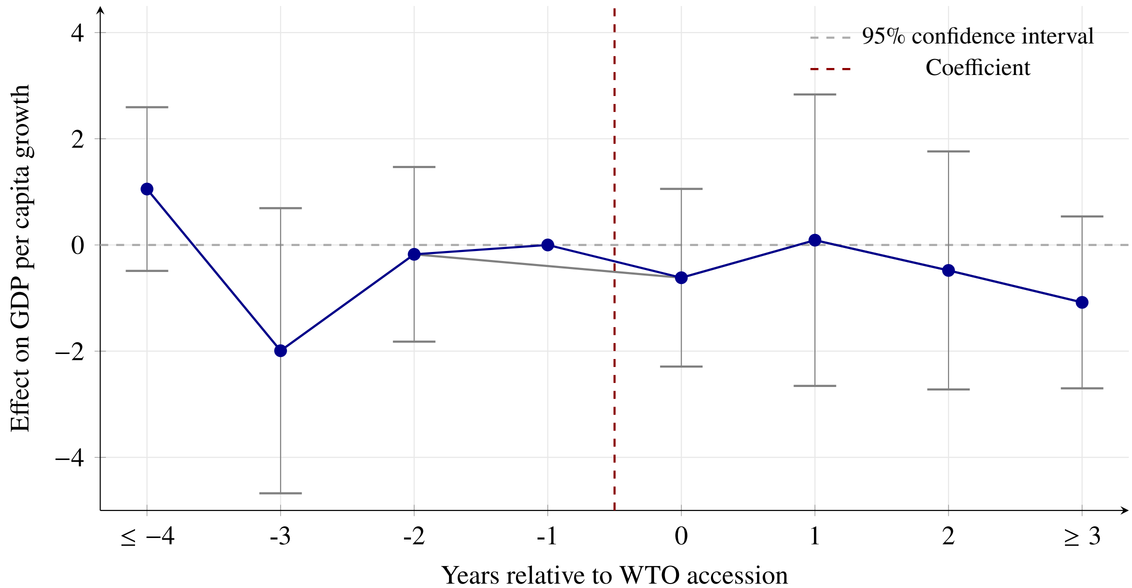

Figure 1: Event-Study Coefficients Around WTO Accession

Notes: Each dot is the fixed-effects coefficient on an event-time indicator. Confidence intervals use country-clustered standard errors. Year −1 is the omitted reference category. The pre-trend joint test is $F(3, 140) = 2.644$, $p = 0.0517$.

B Replication Workflow

The replication workflow imports annual WDI data, reshapes the country-series-year file into a country-year panel, drops non-country aggregate rows, winsorizes GDP per capita growth and GDP-deflator inflation at the 1st and 99th percentiles, and constructs the log-trade robustness variable and Bartik shift-share instrument. WTO accession timing is coded for the 35 post-1995 acceders, while original GATT/WTO members serve as the comparison group. The script then estimates the pooled OLS, fixed-effects, random-effects, DiD, and IV specifications reported in Table 4; runs the diagnostic tests in Table 5; produces the robustness specifications in Table 7; and generates the event-study estimates and Figure 1. All panel regressions use country-clustered standard errors, and the pooled OLS baseline uses heteroskedasticity-robust standard errors.