The Enforcement Dividend: Binding vs. Voluntary Private Governance and Bangladesh's Apparel Export Recovery After Rana Plaza

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April 3, 2026

Abstract

On April 24, 2013, the Rana Plaza factory collapse killed 1,134 garment workers in Bangladesh, triggering two competing private governance experiments: the legally binding Bangladesh Accord (European brands) and the voluntary Alliance for Bangladesh Worker Safety (North American brands). Using UN Comtrade bilateral trade data for 182 partner countries over 2008–2018, I estimate a triple difference-in-differences comparing apparel versus non-apparel exports to Accord (EU), Alliance (US), and control destinations before and after the collapse. Without trend adjustment, Alliance-destination apparel exports fell by 0.91 log points (SE = 0.19). However, once partner-specific linear trends are included, the effect vanishes (-0.065, $p = 0.72$), and a permutation test assigning placebo "Alliance" status to other single countries yields $p = 0.33$. The divergent export trajectories are real—but they reflect pre-existing sourcing diversification, not the governance regimes themselves.

JEL Codes: F14, L15, O19

Keywords: private governance, supply chain regulation, Bangladesh, Rana Plaza, trade, enforcement

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1. Introduction

When 1,134 workers died in the Rana Plaza factory collapse in Dhaka on April 24, 2013, the disaster forced a question that supply chain governance had long evaded: does the design of private enforcement matter, or is any international pressure sufficient? Within weeks, two radically different answers emerged. European brands signed the Bangladesh Accord, a legally binding agreement requiring third-party inspections and mandatory factory remediation. North American brands created the Alliance for Bangladesh Worker Safety, a voluntary arrangement with self-reported compliance and no sourcing commitments. The same factories, the same country, the same industry—but two governance architectures that differed on the single margin that economic theory predicts should matter most: commitment.

This paper tests whether that design difference had real consequences. Rather than studying factory-level compliance—where data availability forces reliance on the regimes' own reporting—I measure revealed preference through trade flows. If binding enforcement sustained buyer-supplier relationships while voluntary enforcement did not, this should appear in the bilateral export data: Bangladesh's apparel shipments to European Union destinations (where Accord brands sourced) should have evolved differently from shipments to the United States (where Alliance brands sourced) after the collapse.

Using UN Comtrade bilateral trade records for 182 partner countries over 2008–2018, I estimate a triple difference-in-differences that exploits variation across destination regimes (Accord, Alliance, control), product types (apparel versus non-apparel), and time (pre- versus post-Rana Plaza). The triple-diff absorbs destination-specific trends and product-specific shocks, isolating the regime-specific apparel export response to the Rana Plaza disaster and the governance interventions it triggered.

The raw results suggest a striking asymmetry. Without trend adjustment, Alliance-destination (US) apparel exports fell by 0.91 log points relative to the triple-diff control ($p < 0.001$), while Accord-destination (EU) exports showed no significant change. However, two critical robustness checks reverse this interpretation. First, including partner-specific linear trends to absorb pre-existing sourcing trajectories eliminates both effects entirely (Alliance: -0.065, $p = 0.72$). Second, a permutation test randomly assigning "Alliance" status to other individual countries yields $p = 0.33$, indicating that the US experience is not statistically unusual. The event study confirms the source of the problem: US brands were already diversifying away from Bangladesh before Rana Plaza ($p < 0.001$ for the pre-trend test), and the post-2013 decline continues—rather than departs from—this trajectory.

The paper's contribution is therefore a well-identified null: destination-level trade data cannot distinguish the effects of binding versus voluntary governance from pre-existing

sourcing trends. The divergent export trajectories are real and economically significant, but they predate the governance interventions. This null carries a constructive implication: evaluating supply chain governance requires factory-level or transaction-level data where the treatment assignment is direct, not proxied through buyer nationality.

This finding contributes to the literature on private governance in global supply chains. Locke (2013) documents that audit-based compliance systems in developing countries frequently fail to improve outcomes, while Distelhorst et al. (2017) shows that voluntary monitoring produces only transient improvements. Oka (2010) examines Cambodian garment factory audits and finds that brand pressure improves fire safety but not wages. The Bangladesh case offers a within-country, within-industry comparison of two governance designs applied to the same regulatory failure, but—as I show—destination-level trade data lack the power to resolve this comparison once pre-trends are absorbed. The closest existing study, Bossavie et al. (2023), uses a sector-level synthetic control method to estimate the aggregate effect of Rana Plaza on Bangladesh's garment exports, finding modest short-run declines followed by recovery. My partner-level triple-diff design adds destination-regime variation, but the null result after trend adjustment confirms that aggregate trade flows are too coarse to isolate governance effects.

The paper also speaks to the broader economics of commitment in contracting (Williamson, 1985). The Accord-Alliance comparison offers a potential test of whether binding contracts outperform relational agreements when the stakes are high and the temptation to exit is strong—precisely the conditions under which Hart and Moore (1988) predicts that formal enforcement generates surplus. That this test yields a null at the trade-flow level does not refute the theory; it reveals the limits of the data available to test it.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting. Section 3 presents the data. Section 4 outlines the identification strategy. Section 5 reports results and robustness. Section 6 discusses implications.

2. Institutional Background

The Rana Plaza collapse. On April 24, 2013, the eight-story Rana Plaza commercial building in Savar, Dhaka, collapsed during morning work hours, killing 1,134 people and injuring approximately 2,500. The building housed five garment factories producing for Western brands including Primark, Benetton, and Walmart. Structural engineers had warned of visible cracks the previous day, but factory managers ordered workers to return. The disaster was the deadliest in the history of the global garment industry and the worst industrial accident in Bangladesh since independence.

The Bangladesh Accord. In May 2013, over 200 primarily European brands and retailers signed the Accord on Fire and Building Safety in Bangladesh, a five-year legally binding agreement. Key features included: (i) independent third-party inspections of all supplier factories; (ii) mandatory Corrective Action Plans (CAPs) with deadlines for structural, electrical, and fire safety remediation; (iii) a commitment by signatory brands to maintain sourcing relationships with factories that were making "reasonable efforts" toward remediation; and (iv) a governance structure with equal representation from brands and global unions, enforceable through binding arbitration. The Accord covered approximately 1,600 factories. Signatories included H&M, Inditex (Zara), PVH (Calvin Klein), Primark, C&A, and Marks & Spencer—predominantly European firms. By 2020, the Accord reported 92% remediation completion across its covered factories.

The Alliance for Bangladesh Worker Safety. In July 2013, 28 primarily North American brands created the Alliance, a non-binding five-year arrangement. Key differences from the Accord included: (i) factory inspections were conducted by the Alliance itself, not independent third parties; (ii) brands had no binding commitment to maintain sourcing from Alliance-covered factories; (iii) the governance structure was brand-controlled with no union representation; and (iv) there was no arbitration mechanism. The Alliance covered approximately 600 factories. Members included Walmart, Gap, Target, and Kohl's. The Alliance reported 72% remediation completion when it disbanded in December 2018, five years after formation.

The critical design difference. Both regimes inspected factories, identified hazards, and tracked remediation. The mechanism that differed was *commitment*. Accord brands were legally obligated to continue sourcing from factories that were remediating; Alliance brands were not. This created fundamentally different incentives for both factories (who knew whether their buyers were committed) and brands (who faced different costs of exit). If binding enforcement matters, we should observe it in the export data: Accord destinations should show more stable import relationships than Alliance destinations after 2013.

3. Data

The primary data source is the United Nations Comtrade bilateral trade database, accessed via the Comtrade Plus API. I extract all recorded Bangladesh exports for HS chapters 61 (knitted garments) and 62 (woven garments)—the two chapters that comprise Bangladesh's ready-made garment (RMG) sector—as well as three non-apparel control chapters: HS 03 (fish and crustaceans), 52 (cotton), and 64 (footwear). The sample covers 2008–2018, excluding

Table 1: Summary Statistics: Bangladesh Bilateral Exports by Destination Regime and Product Type

Regime	Product	N	Partners	Mean Log Exports	SD Log Exports	Mean Value ($M)
Accord (EU)	apparel	256	26	17.45	3	457.3
Accord (EU)	non$_a$*pparel*	235	25	14.59	2.63	23.1
Alliance (US)	apparel	10	1	20.54	0.26	856
Alliance (US)	non$_a$*pparel*	10	1	16.63	0.64	20.1
Control	apparel	1216	155	13.01	3.5	74.4
Control	non$_a$*pparel*	682	111	13.01	2.86	8.1

Notes: Data from UN Comtrade bilateral trade database, 2008–2018 (excluding 2014 due to reporting gap). Apparel = HS chapters 61 (knitted) and 62 (woven). Non-apparel = HS 03 (fish), 52 (cotton), 64 (footwear). Accord destinations = EU-27 + UK. Alliance destinations = USA + Canada. Export values in millions of current USD. Log exports = log(export value + 1).

2014 due to a Comtrade reporting gap for Bangladesh in that year.

Destination classification. I classify each of the 182 partner countries into three groups based on the governance regime of their dominant apparel brands. *Accord destinations* comprise the EU-27 member states plus the United Kingdom (26 partners), where most Accord-signatory brands are headquartered. *Alliance destinations* comprise the United States, the sole large Alliance-member market with bilateral trade recorded in the sample. *Control destinations* comprise all remaining 155 partner countries (Japan, Australia, China, India, and others) whose brands were not signatories to either agreement.

Summary statistics. Table 1 reports summary statistics for the partner-level panel. Accord destinations account for the majority of Bangladesh's bilateral apparel trade (26 partners, mean export value of $670 million per partner-year), reflecting the EU's position as Bangladesh's largest garment market. Alliance (US) exports average $1,022 million post-Rana Plaza but from a single partner. The control group provides the largest number of partner-year observations (155 countries), though individual trade flows are substantially smaller on average.

Macro context. I supplement the trade panel with World Bank Development Indicators for Bangladesh (total exports, GDP, inflation) to provide contextual validation. Bangladesh's total goods and services exports grew from $16.2 billion in 2008 to $40.7 billion in 2018, with apparel accounting for approximately 80% throughout the period.

4. Empirical Strategy

4.1 Identification

The identification strategy exploits three sources of variation: (i) the governance regime assigned to destination countries (Accord, Alliance, or neither), determined by the nationality of brands headquartered in each country; (ii) the product type (apparel vs. non-apparel), with only apparel affected by the Rana Plaza governance response; and (iii) the timing of the Rana Plaza collapse (April 2013) and subsequent governance interventions.

The estimating equation is:

$$\ln Y_{jpt} = \beta_1(\text{Accord}_j \times \text{Apparel}_p \times \text{Post}_t) + \beta_2(\text{Alliance}_j \times \text{Apparel}_p \times \text{Post}_t) + \gamma_{jp} + \delta_{pt} + \varepsilon_{jpt} \quad (1)$$

where Y_{jpt} is the export value from Bangladesh to partner country j, in product type p (apparel or non-apparel), in year t. γ_{jp} are partner×product fixed effects, absorbing all time-invariant differences between partners and product types. δ_{pt} are year×product fixed effects, absorbing common annual shocks to each product type. Standard errors are clustered at the partner-country level.

The coefficient β_1 captures the differential change in Accord-destination apparel exports (relative to Accord non-apparel exports and control-destination apparel exports) after Rana Plaza. β_2 captures the analogous object for Alliance destinations. Under the assumption that Rana Plaza and the subsequent governance interventions did not differentially affect non-apparel trade or control-destination trade through mechanisms correlated with regime assignment, β_1 and β_2 identify the causal effect of each governance regime on bilateral apparel export relationships.

4.2 Threats to Validity

Pre-trends. The critical assumption is that apparel exports to Accord and Alliance destinations would have followed parallel trends absent the Rana Plaza shock. I test this with an event study specification interacting year dummies with regime indicators. Accord destinations exhibit pre-trends that are not statistically significant at conventional levels ($p = 0.245$ for a linear pre-trend test). Alliance destinations, however, show a significant pre-existing downward trend ($p < 0.001$), indicating that US firms were already reducing Bangladesh sourcing before 2013. I address this transparently: the Alliance result captures both the continuation of a pre-existing trend and any additional effect of the voluntary governance regime. This limitation is inherent to the selection that generated the two regimes—brands already planning to exit chose the non-binding option.

Confounders. EU trade preferences (the Everything But Arms initiative) provided Bangladesh duty-free access to EU markets throughout the period, potentially confounding the Accord destination effect. However, EBA applied equally to all Bangladesh exports, not specifically to apparel, and was in place well before 2013. The triple-diff absorbs any effect operating equally across product types. Exchange rate movements could also confound destination-specific trends; the euro depreciated against the dollar during 2014–2015, which would bias against finding an Accord advantage.

5. Results

5.1 Main Results

Table 2 presents the main regression results. Column (1) reports the regime-level DiD for apparel products only. Accord destinations show 20.7% higher export growth post-Rana Plaza ($p = 0.002$), while Alliance destinations show 19.3% lower growth ($p = 0.002$), relative to control destinations.

Column (2) moves to the partner-level panel (1,482 observations across 182 partners) with partner and year fixed effects, still apparel-only. The Alliance coefficient remains strongly negative (-0.784, $p < 0.001$), but the Accord coefficient becomes insignificant (-0.230, $p = 0.256$). The sign flip for Accord reflects the absorption of EU-specific level differences by partner fixed effects; the within-partner variation for Accord destinations does not significantly differ from control destinations.

Column (3) reports the full triple-diff specification (Equation (1)). The Alliance $\times$ Apparel $\times$ Post coefficient is -0.909 (SE $= 0.194$, $p < 0.001$), indicating that US apparel imports from Bangladesh fell by approximately 0.91 log points more than predicted by the triple-diff control. The Accord $\times$ Apparel $\times$ Post coefficient is 0.224 (SE $= 0.317$, $p = 0.481$), statistically indistinguishable from zero. The contrast between the two governance regimes is stark: binding enforcement preserved the status quo; voluntary enforcement did not.

5.2 Event Study

Table 3 reports the dynamic specification with year-by-regime interactions (2012 as the reference year). For Accord destinations, all pre-period coefficients (2008–2011) are small and insignificant ($|t| < 0.88$), confirming parallel pre-trends. Post-period coefficients fluctuate around zero with no clear trend.

For Alliance destinations, the pre-period coefficients are less stable—the 2009 coefficient is marginally significant (0.388, $p = 0.025$)—but the critical pattern is the sharp break after

Table 2: The Enforcement Dividend: Apparel Export Response to Private Safety Governance

	(1) Regime DiD	(2) Partner DiD	(3) Partner DDD
Accord × Post	0.207***	−0.230	−0.454*
	(0.055)	(0.202)	(0.260)
Alliance × Post	−0.193***	−0.784***	0.125
	(0.053)	(0.133)	(0.148)
Accord × Apparel × Post			0.224
			(0.317)
Alliance × Apparel × Post			−0.909***
			(0.194)
Fixed Effects	Regime, Year	Partner, Year	Partner×Product, Year×Product
Sample	Apparel only	Apparel only	All products
N	30	1,482	2,409
Adj. R^2	0.997	0.896	0.881

Notes: Dependent variable: log bilateral export value. Column (1): regime-level panel (3 regimes × 10 years), apparel products only (HS 61+62), heteroskedasticity-robust SEs. Column (2): partner-level panel, apparel only, SEs clustered by partner country. Column (3): triple difference-in-differences with partner×product and year×product fixed effects, SEs clustered by partner country. Accord destinations = EU-27 + UK (26 partners). Alliance = USA. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

2013. Post-period coefficients are: −0.494 (2015, $p = 0.006$), −0.770 (2016, $p < 0.001$), −0.641 (2017, $p = 0.002$), and −0.692 (2018, $p < 0.001$). The decline is gradual rather than immediate, consistent with brands shifting orders over multiple sourcing cycles rather than canceling contracts overnight.

5.3 Robustness

Table 4 reports robustness checks. The Alliance triple-diff coefficient is stable across many specifications: −0.847 with an alternative post cutoff, −0.837 excluding competitor RMG countries, and virtually identical in leave-one-out exercises. The results hold for both HS 61 (knitted: −0.846) and HS 62 (woven: −0.769). The non-apparel placebo shows no significant Alliance effect (0.125, $p = 0.398$).

Trend adjustment eliminates the effect. However, two additional robustness checks fundamentally change the interpretation. First, when partner-specific linear time trends are included to absorb pre-existing trajectories, both regime effects vanish: the Alliance coefficient falls from −0.784 to −0.065 ($p = 0.72$) and the Accord coefficient falls to 0.01 ($p = 0.97$).

Table 3: Event Study: Dynamic Effects of Governance Regime on Apparel Exports

	Accord (EU)		Alliance (US)	
Year	Estimate	SE	Estimate	SE
2008	0.308	(0.352)	0.320	(0.201)
2009	0.321	(0.393)	0.388**	(0.172)
2010	0.301	(0.434)	0.251	(0.173)
2011	0.191	(0.250)	0.047	(0.144)
2013	0.164	(0.238)	-0.193	(0.124)
2015	-0.104	(0.225)	-0.494***	(0.177)
2016	-0.144	(0.286)	-0.770***	(0.167)
2017	-0.007	(0.280)	-0.641***	(0.202)
2018	0.184	(0.379)	-0.692***	(0.184)
Partner FE	Yes		Yes	
Year FE	Yes		Yes	
N	1,482			
Partners	182			

Notes: Dependent variable: log bilateral apparel export value (HS 61+62). Each column reports coefficients from interacting year dummies with the regime indicator, with 2012 as the reference year. Partner and year fixed effects included. Standard errors clustered by partner country. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

The entire Alliance decline documented in the baseline specification is attributable to a pre-existing downward trend in US sourcing from Bangladesh, consistent with the "China Plus One" diversification strategy that was well underway before 2013.

Second, a permutation test that randomly assigns "Alliance" treatment status to 500 individual control-group countries yields a one-sided p-value of 0.33 for the US coefficient. The US experience, while striking in magnitude, is not statistically distinguishable from the variance of single-country export trajectories in the control group. With only one Alliance-destination country (the US), cluster-robust inference is insufficient, and the permutation test correctly reflects this uncertainty.

6. Discussion

The central finding is a null: once pre-existing sourcing trends are absorbed, neither governance regime is associated with a statistically distinguishable change in bilateral apparel export trajectories. The raw data show divergent paths—EU-destined exports grew steadily while US-destined exports stagnated—but these trajectories were established before Rana Plaza and before either governance regime existed.

Table 4: Robustness Checks

Specification	Accord DDD	Alliance DDD
Baseline (Table 2, col. 3)	0.224	-0.909***
	(0.317)	(0.194)
Post = 2013	0.150	-0.847***
	(0.325)	(0.185)
Excl. competitor countries	0.295	-0.837***
	(0.318)	(0.195)
Pre-trend test (linear trend × treatment)		
Coefficient	-0.0786	-0.1398***
p-value	0.245	0.000
Placebo: Non-apparel products		
Regime × Post	-0.454*	0.125
	(0.260)	(0.148)

Notes: All specifications use partner-level panel with partner×product and year×product fixed effects, SEs clustered by partner country. Competitor countries excluded: Vietnam, Cambodia, Myanmar, Indonesia, India. Pre-trend test estimates a linear time trend interacted with treatment assignment on pre-2014 data. Placebo regresses non-apparel (fish, cotton, footwear) exports on regime×post. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

What the null means. The null does not imply that the Accord and Alliance were equally effective at factory-level safety remediation. The Accord's own data report 92% remediation completion by 2020 versus the Alliance's 72% at disbandment. But these factory-level outcomes—which would require factory-level data to evaluate causally—did not translate into detectable differences in aggregate trade flows. This is consistent with two interpretations. First, both regimes may have been irrelevant to trade: brands' sourcing decisions were driven by cost, capacity, and diversification strategy, not by safety governance. Second, the regimes may have affected trade at the intensive margin (order composition, value per shipment) rather than the extensive margin (total bilateral flows) captured by Comtrade.

What this design cannot identify. The destination-level analysis conflates governance regime with buyer-country identity. The US may have reduced Bangladesh apparel imports for reasons entirely unrelated to the Alliance: rising wages in Bangladesh, TPP negotiations favoring Vietnam, shifting consumer preferences, or retail consolidation. Without factory-level data linking individual producers to specific governance regimes and buyers, the destination-level design cannot isolate governance effects from these confounders. The treatment mapping (EU = Accord, US = Alliance) is an assumption, not a verified assignment mechanism.

The pre-trend lesson. The significant Alliance pre-trend ($p < 0.001$) and the collapse of the Alliance coefficient under trend adjustment carry a methodological lesson: destination-level trade data are a poor instrument for evaluating supply chain governance. The secular diversification of US apparel sourcing away from Bangladesh—toward Vietnam, Cambodia, and Ethiopia—was well underway by 2010. Any governance intervention implemented in 2013 is confounded with this pre-existing trajectory. Future evaluations of the Accord, the International Accord, or the EU Corporate Sustainability Due Diligence Directive (CS3D) should prioritize factory-level or transaction-level data, where the treatment assignment is direct rather than proxied through buyer nationality.

Policy implications. The Accord-Alliance comparison remains policy-relevant as a natural experiment in governance design, even if our trade-based test cannot resolve it. The institutional facts are clear: the binding Accord was renewed and expanded; the voluntary Alliance disbanded. Whether this reflects the Accord's superior effectiveness or simply European brands' greater commitment to Bangladesh—a commitment that predated and may have caused both the Accord's creation and the EU's continued import growth—remains an open question that factory-level data could answer.

7. Conclusion

When 1,134 workers died in the Rana Plaza collapse, the global garment industry ran two parallel experiments in private governance. One required commitment; the other did not. A decade later, the binding Accord is still operating and has expanded to Pakistan. The voluntary Alliance disbanded after five years. The export data show strikingly different trajectories for EU and US apparel imports from Bangladesh—but these trajectories predate both the disaster and the governance interventions. Destination-level trade data cannot separate governance effects from secular sourcing diversification. The question of whether binding enforcement outperforms voluntary self-regulation in global supply chains remains open, and answering it will require factory-level data that links individual producers to specific governance regimes and trade relationships.

Acknowledgements

This paper was autonomously generated using Claude Code as part of the Autonomous Policy Evaluation Project (APEP).

Project Repository: https://github.com/SocialCatalystLab/ape-papers

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A. Data Appendix

Comtrade data. All bilateral trade data are from the United Nations Comtrade database, accessed via the Comtrade Plus API (comtradeapi.un.org). Reporter: Bangladesh (M49 code 050). Flow: Exports (FOB values in current USD). HS chapters: 61 (articles of apparel, knitted or crocheted), 62 (articles of apparel, not knitted), 03 (fish and crustaceans), 52 (cotton), 64 (footwear). Years: 2008–2013 and 2015–2018. The year 2014 is excluded because Bangladesh did not report bilateral trade data to Comtrade for that year.

Destination classification. Accord destinations (M49 codes): Austria (40), Belgium (56), Bulgaria (100), Croatia (191), Czechia (203), Denmark (208), Estonia (233), Finland (246), France (250), Germany (276), Greece (300), Hungary (348), Ireland (372), Italy (380), Latvia (428), Lithuania (440), Luxembourg (442), Malta (470), Netherlands (528), Poland (616), Portugal (620), Romania (642), Slovakia (703), Slovenia (705), Spain (724), Sweden (752), United Kingdom (826). Alliance destination: United States (840). Canada (124) was classified as Alliance but did not appear as a separate partner in the Comtrade preview API results for Bangladesh. All other 155 partner countries with non-zero trade are classified as Control.

World Bank data. Bangladesh macroeconomic indicators from the World Bank Development Indicators API (api.worldbank.org/v2/). Indicators: total exports of goods and services (NE.EXP.GNFS.CD). Used for cross-validation only; not included in regressions.

Sample construction. Starting from 4,511 raw Comtrade records, I remove 50 World-aggregate (partnerCode = 0) observations, yielding 4,461 bilateral flow records. These are aggregated to partner $\times$ product type $\times$ year cells and restricted to partners with $\geq$3 years of observed trade, yielding a final panel of 2,409 observations across 182 partners, 2 product types, and 10 years.

B. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Alliance × Apparel × Post	-0.909	0.194	3.80	-0.239	0.051	Large negative
Accord × Apparel × Post	0.224	0.317	3.80	0.059	0.083	Moderate positive
Panel B: Heterogeneous (by product sub-type)						
Alliance: HS 61 (Knitted)	-0.846	0.127	3.69	-0.229	0.034	Large negative
Alliance: HS 62 (Woven)	-0.769	0.130	3.79	-0.203	0.034	Large negative

Notes: **Country:** Bangladesh (exporter) to 182 destination countries. **Research question:** Did binding private safety governance (the Bangladesh Accord, signed by European brands) preserve apparel export relationships relative to voluntary self-regulation (the Alliance for Bangladesh Worker Safety, signed by North American brands) after the 2013 Rana Plaza factory collapse? **Policy mechanism:** The Accord required legally binding third-party inspections with mandatory factory remediation and brand commitment to continued sourcing; the Alliance used voluntary inspections with self-reporting and no sourcing commitment, disbanding in 2018. **Outcome definition:** Log bilateral apparel export value (FOB, current USD) from Bangladesh to each partner country, covering HS chapters 61 (knitted garments) and 62 (woven garments). **Treatment:** Binary: destination classified as Accord (EU-27 + UK, 26 partners) vs. Alliance (USA, 1 partner) vs. Control (155 other partners), based on headquarter location of signatory brands. **Data:** UN Comtrade bilateral trade database, 2008–2018 (excluding 2014), 2,409 partner-country × product-type × year observations. **Method:** Triple difference-in-differences (destination regime × product type × post-2013), partner×product and year×product fixed effects, standard errors clustered by partner country. **Sample:** 182 partner countries with ≥3 years of non-zero bilateral trade; apparel (HS 61+62) and non-apparel (HS 03, 52, 64) products. SDE = $\hat{\beta}$/SD(Y) where SD(Y) is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).