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Do Free Trade Agreements Strengthen or Erode Women's Civil Liberties?

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Abstract

Does deeper trade integration advance or undermine women's civil liberties, their freedom of movement, property rights, freedom from forced labor, and access to justice? Trade can raise women's civil standing by expanding female employment and bargaining power, by importing legal commitments through labor and dispute-settlement chapters, and through rule-of-law spillovers. However, it can erode that standing through coercive export-sector labor practices, competitive deregulation, and the displacement of women from protected sectors. We estimate the effect of entering a free trade agreement with the United States on women's civil liberties for all the countries with bilateral agreements drawing on an annual panel of 182 countries observed from 1970 to 2025. When applying a staggered difference-in-differences estimator with common support we find a decline of roughly five percent of a cross-country standard deviation, which is robust to a broad battery of empirical tests. Our results caution against the optimistic view that trade agreements diffuse women's rights.

Keywords: free trade agreements, women's civil liberties, gender and trade, staggered DiD

JEL Codes: F13, F14, J16, K38

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1. Introduction

Whether economic integration improves or worsens the position of women is among the oldest questions in the political economy of globalization. The empirical literature has answered it almost entirely on the terrain of wages and employment, where the weight of evidence tends to be positive, as trade liberalization appears to raise female labor demand and compress the gender wage gap, especially where it expands female-intensive export sectors or sharpens product-market competition (Black and Brainerd, 2004; Aguayo-Téllez et al., 2013; Juhn et al., 2014). However, far less is known about whether trade integration moves the civil dimension of women's lives, namely their freedom to move, to own, to be free of coerced labor, and to seek redress in court. Studying it is important for three reasons. First, civil liberties are not an automatic by-product of employment. A woman may gain a factory wage while losing autonomy over her movement, and the two need not move together. Second, the civil dimension is precisely where modern trade agreements, with their labor chapters, forced-labor prohibitions, and enforcement machinery, are most often claimed to do good, so it is the natural place to test those claims. Third, almost none of the existing evidence on trade and the civil or political standing of women is causally identified; it relies on continuous trade-openness measures and correlational panels rather than discrete, datable agreements and a credible counterfactual (Richards and Gelleny, 2007; Neumayer and de Soysa, 2011). We take a step toward filling that gap.

Our approach exploits free trade agreements (FTAs) with the United States as a discrete, staggered treatment, applied to women's civil liberties and estimated with the Callaway and Sant'Anna (2021) group-time estimator, which is robust to the heterogeneous-timing biases that may contaminate two-way fixed-effects estimators under staggered adoption. Using the United States as the treating partner has clear design advantages, in particular a discrete legal event rather than a continuous flow, a partner whose own regime type is held fixed at "mature democracy", and content-rich agreements, at the cost of a modest number of treated units, a cost we confront directly in our inference.

We articulate why the sign of the trade effect on women's civil liberties is theoretically ambiguous, identifying channels that should raise it and channels that should lower it, so that the empirical estimate adjudicates between competing mechanisms rather than confirming a prior. To recover a credible estimate, we restrict the comparison group to a common support of pre-treatment income: because treated and never-treated countries differ in income, comparing structurally dissimilar countries would induce a differential pre-trend, and the common-support restriction removes it. A further contribution is a comprehensive battery of robustness tests. The bottom line is a directionally robust decline in women's civil liberties following a bilateral free trade agreement with the United States, at about five percent of a cross-country standard deviation.

The rest of the paper is organized as follows. Section 2 lays out the theoretical channels through which trade integration may raise or lower women's civil liberties and situates them in the literature. Section 3 describes the data and empirical strategy. Section 4 presents results, and Section 5 concludes.

2. Trade Integration and Women's Civil Liberties

We define the outcome variable of our study, women's civil-liberties, as composed of four concrete freedoms: freedom of domestic movement, the right to private property, freedom from forced labor, and access to justice (Coppedge, 2023). The question is whether integration with the United States should raise or lower these. Both directions have serious theoretical backing and laying them out is essential to interpreting the estimate.

There are several channels that are expected to raise women's civil liberties. The first is economic independence and bargaining power. When an agreement expands female-intensive export sectors, it raises women's employment and earnings (Black and Brainerd, 2004; Juhn et al., 2014). A large literature argues that women's control over income raises their bargaining power within the household and their standing in society, which in turn supports autonomy over movement, the accumulation and control of property, and the willingness to invoke legal protections, so that trade-induced employment should translate into stronger civil liberties (Duflo, 2012; Doepke and Tertilt, 2019). Another channel is imported legal commitments. Modern U.S. agreements are not bare tariff schedules; they embed enforceable labor chapters, prohibitions on forced and compulsory labor, non-discrimination language, and dispute-settlement mechanisms that a powerful partner can use to extract domestic reform.[1] To the extent these provisions bind, they should directly strengthen the forced-labor and access-to-justice components of the index (Hafner-Burton, 2005). An additional channel is rule-of-law spillovers. Agreements that require credible courts and impartial enforcement, such as for intellectual property, investor protection, and regulatory transparency, can strengthen the broader legal apparatus on which women's access to justice depends. A judiciary built to adjudicate commercial disputes is, in part, a judiciary available to adjudicate rights claims (International Development Law Organization, 2014).

On the other hand, there are also several channels where FTAs may reduce women's civil liberties. Women may encounter export-sector labor coercion, as the same export expansion that raises female employment often concentrates women in low-wage manufacturing and agriculture, frequently within export-processing zones where labor protections are weak by design and where employer control over workers' movement, contracts, and bargaining can be acute (Seguino, 2000; Fontana, 2009). This may erode civil

[1] The rapid-response labor mechanism in the USMCA (USA, Canada and Mexico) being the most prominent recent example.

liberties, including freedom of movement and freedom from coerced labor, even as it raises measured employment. In addition, competitive deregulation may also play a role. In order to attract and retain export investment, governments may face pressure to hold down labor and regulatory standards, a dynamic documented as a partial "race to the bottom" in labor rights under trade and investment competition (Davies and Vadlamannati, 2013; Olney, 2013). It is known that weakened enforcement of labor and civil protections tends to fall disproportionately on women in the tradable sector (Greenhill et al., 2009). Furthermore, where import competition falls on female-intensive sectors that had offered relatively protected, formal employment, trade may push women into informal work with fewer civil and legal protections. Finally, rapid economic and social change may provoke conservative reaction that tightens restrictions on women's autonomy, which may negatively impact civil liberties (International Development Law Organization, 2014).

As seen above, the net overall sign is a purely empirical question. Theory alone cannot determine it. The bargaining-power, imported-commitment, and rule-of-law channels push up; the labor-coercion, competitive-deregulation, displacement, and backlash channels push down. The contribution of a credible causal estimate is precisely to reveal which family of mechanisms dominates over the horizon we can observe.

3. Data

Our key outcome variable is a Women's Civil Liberties Index from V-Dem, which runs from 0 to 1 and aggregates expert-coded assessments of women's freedom of domestic movement, freedom from forced labor, property rights, and access to justice.[2] In our sample it has a mean near 0.61, a cross-country (pooled) standard deviation of about 0.27, and a within-country standard deviation of about 0.13; an estimated effect of 0.014 is therefore roughly five percent of a cross-country, and eleven percent of a within-country, standard deviation.

Our treatment is the entry into force of a country's free trade agreement with the United States.[3] The resulting sample is an annual panel that goes from 1970 to 2025, comprising 182 countries with non-missing values on the outcome. The baseline comparison group is the 162 countries that never signed an FTA with the United States.

[2] All the variables employed in this paper come from the well-known V-Dem dataset and is publicly available at www.v-dem.net/data/the-v-dem-dataset/

[3] Bilateral U.S. Free Trade Agreement partner countries by entry-into-force year in parenthesis: Israel (1985); Canada (1989); Mexico (1994); Jordan (2001); Chile (2004); Singapore (2004); Australia (2005); El Salvador (2006); Honduras (2006); Nicaragua (2006); Guatemala (2006); Morocco (2006); Bahrain (2006); Dominican Republic (2007); Peru (2009); Costa Rica (2009); Oman (2009); Colombia (2012); South Korea (2012); Panama (2012). Notice that Canada and Mexico are dated to the Canada–U.S. FTA (1989) and NAFTA (1994), and the CAFTA-DR partners to their individual entry-into-force years (Baggio and Chong, 2020).

Table 1. Summary Statistics

Variable	Obs.	Mean	Std. Dev.	Min	Max
Women's Civil Liberties Index	9528	0.605	0.273	0.000	0.981
Ever signs a U.S. FTA (country-level indicator)	9528	0.118	0.322	0.000	1.000
Treated × post (=1 in years from FTA entry)	9528	0.045	0.208	0.000	1.000
GDP per capita	8261	26.62	31.53	0.70	263.05
Log GDP per capita	8261	2.650	1.159	-0.352	5.572
Baseline (1990) log GDP per capita	9063	2.240	1.016	0.457	4.919

Notes: The estimation panel is annual and comprises 9528 country-year observations on 182 countries over 1970–2025; 20 countries are ever-treated (enter a U.S. free trade agreement) and 162 are never-treated. The women's civil-liberties index is bounded on [0,1]. For the outcome, the overall standard deviation of 0.273 decomposes into a between-country standard deviation of 0.243 and a within-country standard deviation of 0.129; reported effect sizes are scaled by these. GDP per capita is expressed in thousands; "log GDP per capita" is its natural log, and "baseline (1990) log GDP per capita" is each country's 1990 value held constant, used to define the common-support comparison group. The primary specification employs no control variables; the income variables enter only sample construction and robustness. Geographic region is used as the stratifying variable in the matched-controls specification. At baseline (the last pre-treatment year), treated countries are more empowered and richer than never-treated countries on the outcome (0.717 vs. 0.601) and on log GDP per capita (3.37 vs. 2.58), motivating the design.

We emphasize that the counterfactual is "no U.S. FTA", not "no trade agreement", as many comparison countries have agreements with other partners, so the estimate is the effect of a U.S. agreement relative to the comparison countries' own trade policy. It should be highlighted that U.S.-FTA partners are markedly more empowered than never-treated countries at baseline, as in the last pre-treatment year, treated countries average about 0.72 on the civil-liberties index against 0.60 for never-treated countries, and they are richer. This positive selection is exactly why a design that differences out fixed levels is necessary, and as described below, shows why differencing out levels alone is not sufficient.

We estimate group-time average treatment effects ATT(g,t), the effect for the cohort first treated in year g, evaluated in year t, using Callaway and Sant'Anna (2021), with doubly-robust estimation of the underlying 2×2 comparisons. Each ATT(g,t) contrasts the change in women's civil liberties from g−1 to t for cohort g against the contemporaneous change for comparison units, so identification rests on a parallel-trends assumption. In order to test for the latter, we perform an event study and in order to make sure that this exercise is fully comparable, we restrict the comparison group to never-treated countries whose pre-treatment (baseline) income lies within the range spanned by the treated countries, retaining 95 countries in total. This common-support restriction helps reduce potential differential pre-trends.

4. Results

Since treated countries are richer and growing, one might suspect any finding might simply be due to income. We distinguish two possibilities with a direct test. Conditioning on deep pre-treatment income (each country's income in 1990, well before any modern agreement) leaves the estimate negative and significant

(−0.014, t = 2.4): the effect is not a pre-existing income-level confound. By contrast, conditioning on income as it evolves around the agreement absorbs the effect and flips its sign. The natural reading is that income is a mediator. Trade agreements raise income, and income is one pathway through which they act, so controlling for the contemporaneous income path is a bad control that nets out part of the very effect we wish to measure. We therefore do not condition on contemporaneous income in the primary specification, and we report the deep-pre-treatment-income result as a robustness check.[4]

Table 2 reports the primary estimate, the effect of a US FTA on women's civil liberties. Entering a free trade agreement with the United States reduces women's civil liberties by 0.0144 on the 0–1 index over the first ten post-agreement years or about 0.05 of a cross-country and 0.11 of a within-country, standard deviation. The effect is statistically significant under analytic standard errors (t = 2.09) and its country-clustered bootstrap confidence interval, [−0.028, −0.001], excludes zero. Crucially, this estimate is delivered by a specification with clean pre-trends (zero of ten significant pre-coefficients). By design, these estimates use no control variables at all, as in Callaway and Sant'Anna (2021) identification comes from the difference-in-differences structure plus the choice of comparison group, not from a vector of regressors. Country fixed levels are differenced out and time is handled by comparing each treated cohort to the contemporaneous change in the comparison group, period by period. The comparison group is a common-support of never-treated countries, which uses 1990 log GDP per capita as a sample restriction. We kept the headline covariate-free deliberately: conditioning on slow-moving variables the FTA may itself move (income, democracy) risks bad control, so covariate adjustment is relegated to robustness tests.

Table 2. U.S. FTA Entry on Women's Civil Liberties

	Estimate	Std. Error	t / p
ATT on women's civil liberties (0–10 window)	−0.0144	0.0069	t = 2.09
95% confidence interval	[−0.028, −0.001]		
In cross-country standard deviation units	−0.053		
In within-country standard deviation units	−0.112		
Significant pre-trend coefficients	0 of 10		
Country-clustered wild bootstrap (999 reps)	[−0.028, −0.001]		excl. 0

Notes: Callaway and Sant'Anna (2021) group-time ATTs, comparable (common-support) comparison group, doubly-robust estimation, averaged over event times 0–10. SD units use pooled and within-country standard deviations of the index. Randomization inference uses 60 placebo re-assignments of treatment timing among never-treated countries (two-sided).

Figure 1 plots the corresponding event study. All the pre-entry coefficients are small and statistically indistinguishable from zero; the effect is essentially zero at the year of entry and then drifts steadily negative

[4] Twenty treated clusters (see Footnote 3) may be few, and asymptotic cluster-robust standard errors may overstate precision. We therefore test both analytic standard errors and a country-clustered wild (multiplier) bootstrap with 999 replications and both reject zero.

at about −0.012 at year +1, −0.018 at +4, and −0.027 at +6, before stabilizing. The dynamics are coherent with an effect that accumulates as integration deepens over roughly six years, rather than a discontinuous jump at entry.[5]

Figure 1. FTA Entry on Women's Civil Liberties

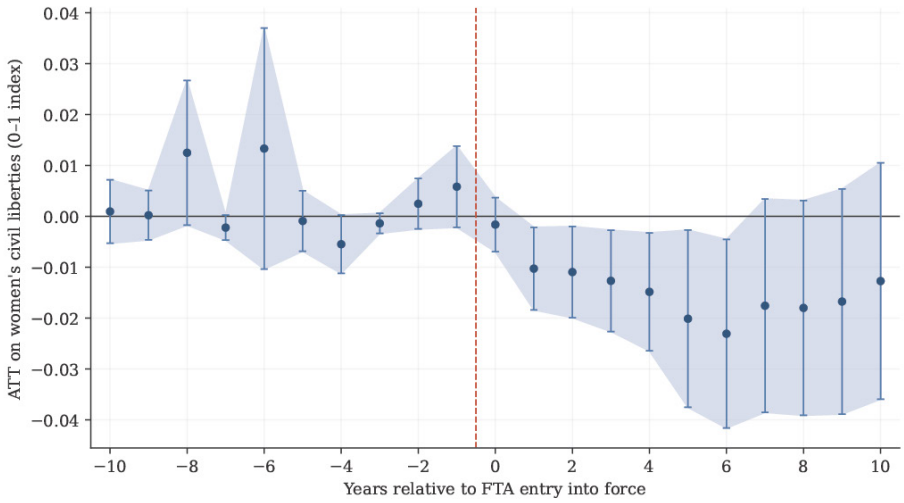

Figure 1. Event-study estimates of U.S. FTA entry on women's civil liberties. Points are Callaway and Sant'Anna (2021) group-time ATTs aggregated by years since entry; whiskers and bands are 95% confidence intervals. The figure uses the common-support comparison group (zero of ten significant). The dashed line marks the year before entry.

Table 3 presents a robustness battery of tests. The estimates are strikingly stable. Replacing the comparison group with not-yet-treated units leaves it at −0.015; matching treated to comparison countries on baseline income and region gives −0.012; conditioning on deep pre-treatment income gives −0.014. The three estimation methods within the Callaway and Sant'Anna (2021) family namely, doubly robust, outcome-regression, and inverse-probability-weighting, agree to the third decimal (−0.0144). Shortening the window to 0–8 or 0–5 years preserves significance; leaving out each of the ten treatment cohorts in turn moves the estimate only within [−0.017, −0.011]. Combining the common-support restriction with pre-treatment-income conditioning yields −0.0145 with, again, clean pre-trends.

[5] A naive implementation including all 162 never-treated countries as the comparison group yields a statistically significant and negative overall effect (ATT = −0.017) but yields three of the ten pre-treatment event-study coefficients as individually statistically significant. However, this differential trend is due to a composition problem, as the never-treated spans from high-income to low-income countries where civil liberties trajectories may differ systematically with respect to those of the middle-income, mostly Latin American countries, that signed bilateral FTAs with the United States. By construction, comparing treated countries to this dissimilar control pool will produce divergent trends. The observed pre-trend issue in the naïve implementation is thus an issue of whom the treated were compared against, not of the treatment effect itself.

It should be mentioned that income enters only through the doubly robust estimator, as a covariate in the propensity-score and outcome-regression models of each 2×2 cell. Conditioning on deep pre-treatment income (log GDP per capita around 1990, time-invariant) leaves the estimate negative (−0.0137 to −0.0145), whereas conditioning on near-treatment income (the base period g−1, which reflects FTA-driven income growth) flips it to a null (+0.003). This contrast, deep income preserves the effect, near-treatment income absorbs it, is the confounder-versus-mediator test mentioned above.

Table 3. Robustness

Specification	ATT	S.E.	T-Stat
Primary: comparable controls	−0.0144	0.0069	2.09
Full control group (no trimming)	−0.0168	0.0068	2.46
Not-yet-treated comparison group	−0.0149	0.0068	2.19
Matched controls (1:3 nearest neighbor)	−0.0119	0.0072	1.66
Condition on pre-treatment (1990) income	−0.0137	0.0058	2.36
Comparable controls + pre-treatment income	−0.0145	0.0058	2.49
Outcome-regression estimator	−0.0144	0.0070	2.07
Inverse-probability-weighting estimator	−0.0144	0.0070	2.07
Event window 0–8 years	−0.0144	0.0062	2.32
Event window 0–5 years	−0.0118	0.0047	2.51
Drop pre-2000 cohorts	−0.0088	0.0056	1.56
Allow one-year anticipation	−0.0086	0.0090	0.95
BJS imputation estimator	0.0032	0.0258	0.12
Leave-one-cohort-out (range over 10 cohorts)	[−0.017, −0.011]		

Notes: Each row is a separate Callaway and Sant'Anna (2021) estimation of the overall ATT on women's civil liberties over event times 0–10 (except where a different window is stated). The primary specification is highlighted. The BJS refers to the Borusyak–Jaravel–Spiess estimator. Standard errors for the leave-one-out row are not directly comparable. It is summarized as a range.

Some of the tests performed merit some further comments. Dropping the pre-2000 cohorts removes the three earliest and most data-rich treated countries, cutting power, and thus shrinks the estimate to −0.009 (t = 1.6). Allowing one year of anticipation, which is reasonable since agreements are signed and ratified before entry, attenuates it to −0.009 by shifting the baseline and absorbing the immediate post-entry dynamics. Notice that in neither case does the sign change. The one estimator that returns a point estimate near zero, the Borusyak–Jaravel–Spiess (BJS) imputation estimator (0.003), is far too imprecise to be informative, as its standard error of roughly 0.026 yields a confidence interval that comfortably contains the primary estimate, so it neither confirms nor contradicts the result.

In the context of our review of the literature, a small negative effect points toward the export-sector labor-coercion and competitive-deregulation channels and away from the imported-commitment and bargaining-power channels, at least over the one-to-two-decade horizon we observe and, on the perception-based measure we use. The finding is consistent with a literature that has long argued trade can disadvantage women by drawing them into weakly protected export work and by pressuring governments to hold standards down (Seguino, 2000; Fontana, 2009; Davies and Vadlamannati, 2013). It sits against the optimistic case that the labor and rights provisions of modern agreements translate into measurable civil-liberties gains; whatever those provisions accomplish, they do not, on this evidence, move the aggregate index upward.

Two caveats are in order. First, since the treatment is bundled, our estimate is the effect of a composite package relative to the comparison countries' trade policy, and it cannot attribute the decline to any single FTA-related component. In addition, the outcome is perception-based and as such any agreement could in principle shift coded perceptions in either direction independently of women's civil standing.

5. Conclusion

We have asked whether trade integration with a trade partner strengthens or erodes women's civil liberties, a question whose answer theory leaves open and whose existing evidence is very scarce and if so, correlational. Using U.S. free trade agreements as a staggered treatment and a heterogeneity-robust estimator and enforcing common support in the comparison group to minimize differential trends bias, we find a directionally robust decline in women's civil liberties about five percent of a cross-country standard deviation that emerges over the first six post-agreement years and survives an extensive robustness battery. The sign is stable, and while quantitatively relatively small, it points toward a mechanism by which trade may disadvantage women and goes against the optimistic view that trade agreements diffuse women's rights.

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