

Divorce Causal

Does liberalizing divorce law improve income inequality?

Background: Putative Causal Mechanism

Historically, marriage has been an arrangement in which women have limited agency.

Women still do the majority of housework even in dual income households. In time use studies, researchers have found that only divorced men do an equal share of housework. (*Lockman, D. (2019). All the rage: Mothers, fathers, and the myth of equal partnership. HarperCollins*)

When divorce laws are liberalized, women become empowered to exit marriages. Household structure changes. Men must begin to do housework for themselves, remarry in a changed environment, or pay for services like housekeeping and food preparation.

It is plausible that divorce leads to greater income equality, by requiring that spouses pay for work, which was previously done by their spouse unpaid.

This analysis attempts to identify whether significant changes in income equality (as measured by Gini Index) occurred following the passage of no-fault divorce laws globally.

Causal Diagram (DAG)

U (norms, development)



→ Divorce law liberalization → Divorce / exit option

→ Household structure (separate households)

→ Women stop doing free domestic labor

→ Men must pay for previously free labor

→ Improved income inequality

Treatment Coding

Divorce regimes vary in restrictiveness, in order of most restrictive to most liberal:

1. Illegal
2. Fault-based only (no “irreconcilable differences”)
3. No-fault (lite/mutual): breakdown recognized but may require proof, separation periods, or mutual consent
4. No-fault (unilateral): one party’s assertion suffices, no proof needed

For analysis, I collapse the above four categories into two groups:

Group I ("never treated"): No recognition of “irretrievable breakdown/irreconcilable differences,” or procedures that are substantively one-sided/unfair (**1 & 2 above**)

Group II (treated or not-yet-treated): No-fault permitted, including recognition of irretrievable breakdown/irreconcilable differences (**3 & 4 above**)

Experiment Design

Data is observational. Countries adopt reforms at different calendar years.

Two natural designs for this type of data are:

1. Country-by-Country 2×2 DiD (each country as its own two-period, two-group comparison)
2. Staggered adoption designs (multiple units and periods with different adoption years)

The per-country 2×2 design is not feasible here, as data are national.

Staggered Design - Two Approaches

1. Cohort Approach → Bin countries into cohorts by adoption year decade
2. Event Study → Superimpose different adoption years onto same “event”

Control Assumptions

Never Treated or Not Yet Treated

Identification assumes that, absent reform, treated and control countries would have followed parallel trends in inequality.

- Never-treated countries tend to have lower development levels, raising concerns about heterogeneous trends. Ideally, controls would more closely match treated countries; however, data limitations prevent further restriction.
- Parallel trends are assessed directly using event-study pre-period estimates. The absence of systematic pre-trends supports the identifying assumption.

Computational Methods

For each approach, I used two different computational methods, then compared results to validate. This was probably overkill, but it made me feel better.

Cohort Approach: statsmodels and linearmodels

- smf.ols()
- PanelOLS

Event Study: manual and statsmodels

- Manually ATT per pooled relative year, clustered bootstrap CIs
- Patsy contrasts “Treatment”
 - formula = f"{Y} ~ C(rel_year_es, Treatment(reference=-1)) + C(Country) + C(Year)"

Cohort Approach

Staggered Design: Cohort Approach

I. Cohort Approach

- Group countries into per-decade cohorts, according to the decade in which divorce law reform occurred.
- Define G_i (Treatment Year) as the modal year per decade.
- Binning countries into cohorts leads to mild data contamination on a Country-level
- Given that legal and social changes evolve gradually, limited within-decade contamination may be acceptable, but is clearly less than ideal.
- Event Study design used for proper identification

Viable Cohort Criteria

Decade-cohorts, which did not meet the following criteria were deleted:

1. The cohort contains observations from at least two distinct calendar years (i.e., both pre- and post-periods are represented).
2. The cohort exhibits one of the following treatment patterns:
 - a. At least one treated period and one untreated period (i.e., treatment turns on within the cohort), or
 - b. The cohort is never treated (all observations are untreated).
3. Singleton Countries (with only a single year of data) within a cohort are excluded.

Thin Cohorts, Contaminated Countries

*Gi defined as modal year per cohort, which misaligns with rolling-year reality.
Row-wise data is not contaminated, but Country-wise there is contamination.*

	treated countries	control countries	total countries	n_rows	contaminated_countries
Treatment Cohort					
1960-1969	1	122	123	5701	0
1970-1979	14	109	122	6339	1
1980-1989	10	106	108	5881	8
1990-1999	12	91	98	5637	5
2000-2009	17	81	86	5244	12
2010-2019	7	63	69	4264	1
2020-2029	6	59	62	3844	3

Cohort Results

In some cohorts, standard error estimation was numerically unstable, producing invalid SEs, tvals and pvals. Results should be interpreted with caution.

No significant results identified. Marginal result in 1970's with unstable SE.

cohort	coef	se	tval	pval
1960-1969	-0.016503	0.038256	-0.431376	0.666195
1970-1979	0.233481	0.125114	1.866153	0.062020
1980-1989	0.096794	0.062074	1.559327	0.118919
1990-1999	0.133092	0.190947	0.697011	0.485796
2000-2009	0.346534	0.283137	1.223909	0.220987
2010-2019	-0.640045	1.203258	-0.531927	0.594777
2020-2029	-0.158826	0.224135	-0.708619	0.478561

Cohort Approach

Staggered Design - Event Study

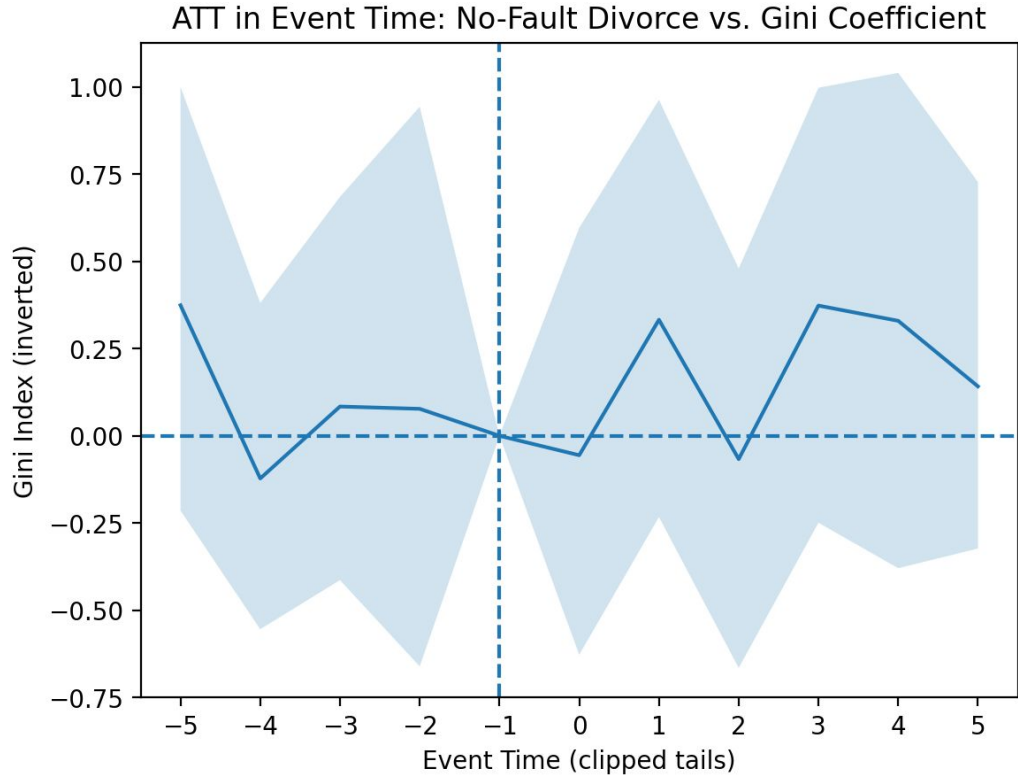
- Re-index time relative to adoption: $e = t - G$, using event-time bins (e.g., -5, -4, -3, -2, -1, 0, 1, 2, 3, 4, 5+).
- For each treated country and event time e , compute an ATT as a difference-in-differences:
 - Treated country's change from baseline vs. the control pool's change from baseline in the same calendar year.
 - Average country-level ATTs across treated countries within each event-time bin to obtain a pooled event-time ATT.
- This avoids contamination of controls/treatment inherent in Cohort Approach
- While avoiding contamination, the event study approach requires assumptions that strain credulity, i.e. parallel trends hold for 1975 and 1995

Event Study Results: No Significant Results

Parallel Trends assumption looks sound.

Cannot reject zero on any post-treatment horizon (alpha=0.05)

No statistically detectable effect of divorce law liberalization on the Gini coefficient in this sample.



Haunting Questions

Counterfactual validity: Are never-treated and not-yet-treated countries plausible counterfactuals for treated countries (i.e., do they exhibit similar pre-treatment trends)?

Treatment definition: Is my treatment indicator too coarse? Would effects be more visible if “treatment” were defined as a strong reform (e.g., full no-fault adoption or broader liberalization), rather than any reform?

Heterogeneity: Does the effect vary by baseline development, region, or initial inequality—so that pooling masks subgroup effects?

Timing: Are the true effects delayed or gradual (legal change → behavioral change → labor market change), making the event window too short or misaligned?

Next Steps

- Stratify and Segment:
 - Explore weighting by country properties like population, GDP, or OECD
- Experiment with different treatment coding:
 - No Fault (strongest) vs. all other variants
- Try wider cohorts.
- Try another target variable, which might pick up on theorized mechanism:
 - Number of unipersonal households
 - Human Development Index
- Find or research richer data with county or state level laws

Links

GitHub Repo:

<https://github.com/LittleBiggler/Divorce-Causal>

Slide Deck:

<https://docs.google.com/presentation/d/1tKbl3vgDQC28jvYPhcNPnpUbmyYaBl9kMYIFa5Yk1zI/edit?usp=sharing>