

Family-Law Reform and Mothers' Employment: Comparative Evidence from Egypt, Jordan, and Morocco

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**Family-Law Reform and Mothers' Employment:
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Abstract

Women's educational gains in the Middle East and North Africa have not translated into commensurate employment gains. This paper examines whether family-law reforms that expanded women's access to marital dissolution were associated with changes in mothers' current employment in Egypt, Jordan, and Morocco. We combine repeated cross-sections from the Demographic and Health Surveys, supplemented by nationally representative household surveys for Morocco, and compare mothers on either side of country-specific child-custody age thresholds before and after reform. The reforms are institutionally distinct: Egypt's khul' and Jordan's iftida' require financial renunciation or compensation, whereas Morocco's 2004 Family Code is a broader package affecting several dimensions of family life. The estimates indicate modest short-window employment differences and divergent longer-window patterns across countries. However, event-study tests reject parallel pre-trends for the married-women samples, and group composition changes across survey waves. We therefore interpret the estimates as comparative, reform-associated evidence rather than causal effects. The findings suggest that formal rights can alter women's options within marriage, but that their translation into paid work remains conditioned by childcare, labor demand, enforcement, social norms, and the economic value of outside options. The paper contributes an institutionally grounded account of how family law, relational autonomy, and access to decent work jointly shape women's economic agency and household welfare.

JEL classification: J12; J22; D13; K36; I31

Keywords: family law; women's employment; mothers; child custody; economic agency; social norms

1 Introduction

Women's labor-force participation in the Middle East and North Africa (MENA) remains exceptionally low despite substantial gains in education. In 2024, female participation was about 20 percent in Morocco and 14 percent in Jordan, illustrating that human-capital accumulation alone does not ensure access to paid work (World Bank, 2024). Family institutions are part of this puzzle because marriage, custody, and divorce rules shape spouses' outside options, the allocation of unpaid care, and the credibility of women's preferences within household decisions.

This paper studies reforms that expanded women's ability to initiate marital dissolution in Egypt, Jordan, and Morocco. The question is not only whether a legal entitlement changes employment. It is whether formal rights become effective capabilities when they interact with family obligations, labor-market opportunities, enforcement, and social norms. This perspective connects household bargaining to a civil-economy concern: economic participation is socially embedded, and the welfare value of legal reform depends on whether institutions enlarge substantive agency without transferring disproportionate care and adjustment costs to women.

The three reforms are analyzed separately because they are not equivalent treatments. Egypt's 2000 *khul'* procedure allows a wife to seek divorce without proving fault but generally requires her to return the dowry and renounce certain financial claims. Jordan's *iftida'* remained politically contested and financially conditional. Morocco's 2004 *Moudawana* was a broad Family Code reform that simultaneously changed divorce procedures, marital guardianship, the minimum marriage age, and restrictions on polygamy. The estimates therefore describe three country-specific institutional episodes; they are not pooled into a single universal effect of 'unilateral divorce.'

The empirical analysis uses repeated cross-sections from the Demographic and Health Surveys (DHS), supplemented by nationally representative Moroccan household surveys. Within each country, it compares mothers whose youngest child is below the legal custody age with mothers whose youngest child is above it, before and after reform. The first group is more strongly protected by custody and associated support provisions, but the second group is not untreated: both groups are subject to the new family law. The design therefore identifies a differential association by exposure intensity, not the effect of legal eligibility versus no eligibility.

The paper makes three contributions. First, it provides a comparative institutional account of family-law change in three MENA countries, highlighting differences in the content, financial accessibility, and political implementation of the reforms. Second, it links these institutions to mothers' current employment over short and longer windows using harmonized microdata. Third, it uses the empirical limitations themselves to discipline interpretation: changing group composition, the close relation between children's ages and maternal employment, and rejected pre-trend tests rule out a strong causal reading of the baseline difference-in-differences coefficients.

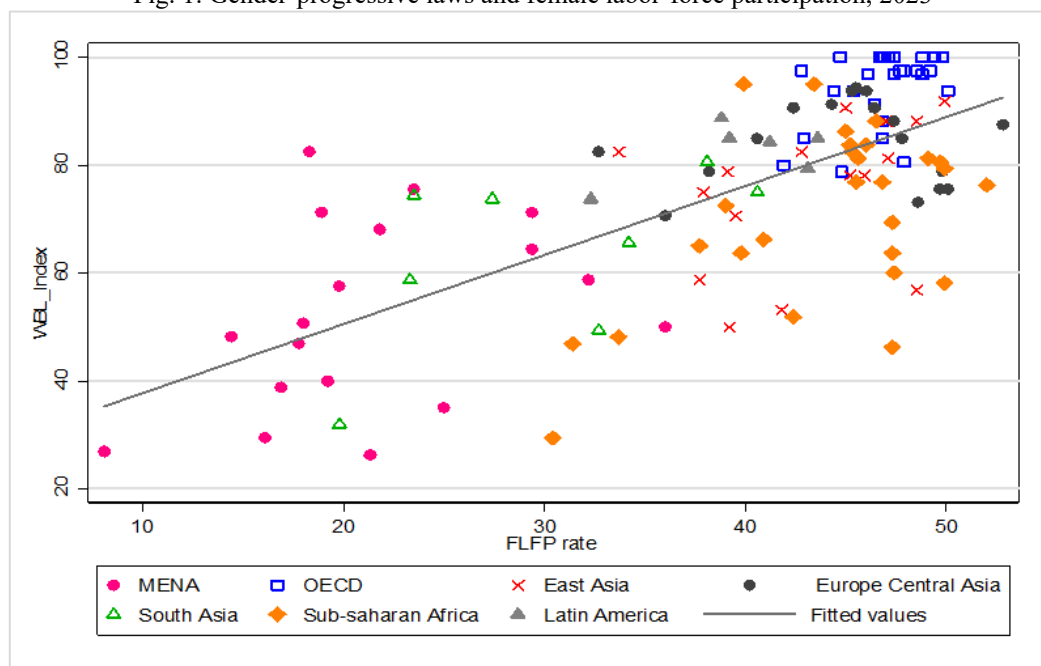
The results show modest positive employment differences in the shorter window and heterogeneous longer-window patterns. Yet the event-study evidence rejects parallel pre-trends for married women in all three countries. We therefore treat the estimates as suggestive comparative evidence. They are consistent with the proposition that legal rights may improve women's position within marriage, but the data cannot separately identify bargaining, anticipation of divorce, or contemporaneous structural change. This distinction is central to the paper's revised contribution.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting and situates the three countries in the regional relationship between gender-progressive law and female employment. Section 3 presents a parsimonious conceptual framework. Section 4 explains the data, samples, and empirical design. Section 5 presents the estimates and their identification limits. Section 6 discusses implications for women's agency, household welfare, and policy.

2 Institutional background

Family law governs marriage, divorce, maintenance, child custody, and related obligations. In societies where paid work, unpaid care, and kinship responsibilities remain strongly gendered, these rules affect more than marital dissolution: they shape credible outside options and everyday bargaining within households. Fig. 1 places Egypt, Jordan, and Morocco in the wider association between gender-progressive laws and female labor-force participation. The cross-country relationship is descriptive, not causal, but it shows why legal equality and effective economic participation must be studied jointly. The three countries occupy a region characterized by relatively low female employment, while differing in the scope and accessibility of their family-law reforms.

Fig. 1. Gender-progressive laws and female labor-force participation, 2023



Source: Authors' calculations using World Bank Women, Business and the Law (2023) and World Development Indicators.

Note: The figure covers 122 economies. The legal index averages eight domains and ranges from 0 to 100; higher values indicate more gender-equal laws. The fitted relationship is descriptive. Egypt, Jordan, and Morocco are highlighted in the country discussion.

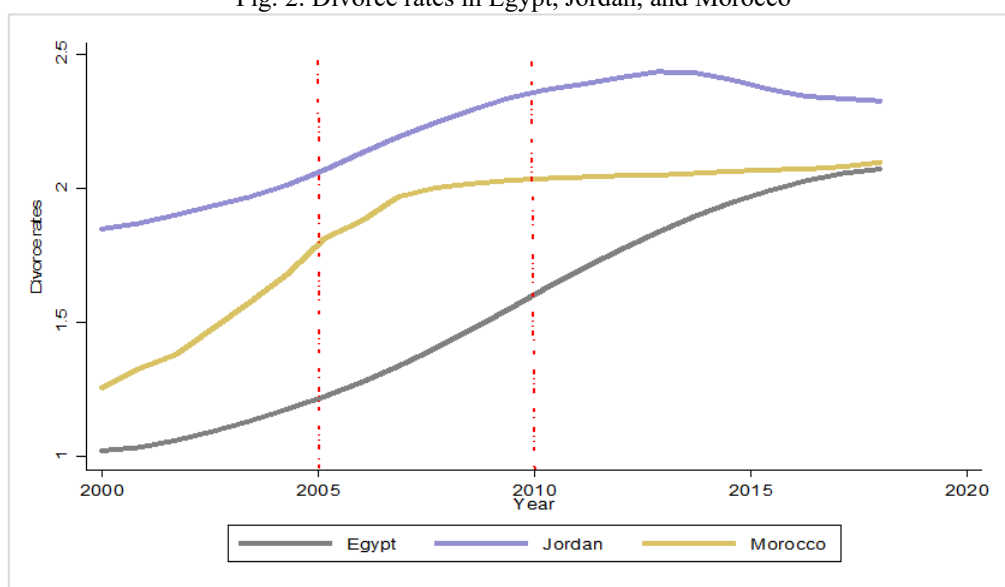
Egypt. Law No. 1 of 2000 established the *khul'* procedure. A wife may seek divorce without proving fault or obtaining her husband's consent, but she generally returns the dowry and relinquishes specified financial claims (Welchman, 2007). This financial condition makes the reform less accessible to women with limited resources. Child-custody provisions initially differed by the child's sex—age 10 for boys and 12 for girls—and a 2005 amendment raised the threshold to 15 for both. During custody, the father remains responsible for child maintenance. The relevant institutional change is therefore a combination of easier legal exit, financial renunciation, and custody-related support, rather than an unconditional right to costless divorce.

Jordan. A 2001 royal decree amended the Personal Status Law, including marriage-age, polygamy, custody, and divorce provisions, but the divorce amendment remained politically

contested and failed to pass parliament in 2004. The 2010 Personal Status Law introduced *iftida'*, under which a wife may obtain dissolution while relinquishing financial rights and paying compensation. Mothers are generally entitled to custody until age 15, while fathers remain legal guardians and responsible for maintenance. Jordan thus represents a financially conditional and institutionally delayed expansion of women's exit options (El Muhtaseb et al., 2016; Engelcke, 2018).

Morocco. The 2004 Moudawana was a comprehensive reform rather than a discrete divorce-law intervention. It removed the general requirement of a matrimonial guardian, set the minimum marriage age at 18, restricted polygamy, articulated reciprocal spousal responsibilities, and expanded judicial routes to divorce, including dissolution for discord (*shiqaq*). Mothers receive priority in custody, subject to legal conditions, and children may choose their custodial parent at age 15. Because these changes occurred together, any observed employment difference cannot be attributed specifically to unilateral divorce. Morocco is best understood as a bundled reform of family governance with potentially broad effects on women's legal status (Charrad, 2012; Sadiqi, 2013; Sonneveld, 2019).

Fig. 2. Divorce rates in Egypt, Jordan, and Morocco



Note: National series cross-checked against UNDP and ESCWA open-data sources. Vertical markers denote the principal family-law reforms and subsequent custody amendments. The series are contextual and are not used to establish identification.

3 Conceptual framework

Family-law reform can change women's economic agency by altering the value and credibility of options inside and outside marriage. In a collective-household framework, legal rules affecting divorce, custody, maintenance, and property claims are distribution factors: they may shift bargaining weights even when a marriage continues (Chiappori et al., 2002; Voena, 2015). Employment can itself strengthen an outside option by generating income, experience, and access to social networks. The relationship can therefore run in both directions, and the present data identify neither a structural bargaining parameter nor a separate anticipation channel.

3.1 Legal rights, outside options, and employment

Two conceptually distinct responses are possible. A bargaining response occurs when stronger legal exit rights allow a married woman's preference for paid work to carry greater weight in household decisions. A precautionary response occurs when a woman seeks employment to reduce the income risk associated with possible separation. Both imply that reform exposure may be associated with higher employment, but current employment and marital status alone cannot distinguish them. The empirical analysis consequently treats these channels as interpretations, not as separately identified mechanisms.

The reforms may also have no employment effect, or an effect that differs across places and time. Formal rights are valuable in themselves, but their conversion into substantive capabilities depends on enforcement, legal and financial accessibility, childcare, mobility, job availability, working conditions, and norms surrounding motherhood and marriage (Jayachandran, 2021; Field et al., 2021). Where suitable jobs are scarce or care remains privately assigned to women, an improved outside option may raise welfare and bargaining power without changing observed employment.

This yields one disciplined empirical expectation: mothers more strongly protected by custody provisions may exhibit a relative change in employment after reform, but the sign and persistence of that change are context-dependent. Country coefficients should not be interpreted as estimates of one common treatment because the legal changes differ substantially. Short- and long-window estimates are descriptive comparisons across institutional episodes; the static framework does not, by itself, predict a particular dynamic path.

3.2 Countervailing institutions and structural conditions

The same legal entitlement can have different labor-market implications because institutions are complementary. Accessible childcare reduces the time cost of market work; labor demand determines whether education can be converted into suitable employment; effective maintenance and custody enforcement determine the economic value of divorce rights; and social norms influence the personal and relational costs of employment. These factors motivate separate country analyses and cautious heterogeneity exercises. They do not supply post hoc causal explanations for every coefficient.

The empirical interpretation therefore follows a hierarchy. First, the paper reports reform-associated differences in current employment. Second, it examines whether the patterns are compatible with stronger household options while recognizing that mechanisms are observationally equivalent. Third, it asks whether country context and education correlate with heterogeneous patterns. Because the data contain no employment histories, direct bargaining measures, time-use outcomes, violence measures, or regionally comparable opportunity indicators, mechanism claims remain hypotheses for future research.

4 Data and empirical strategy

The analysis is based on repeated cross-sections, not an individual panel. Women observed in one survey wave are not followed into later waves. Birth-year cells provide a pseudo-panel organizing device, but the estimating sample remains a sequence of cross-sectional observations. This distinction matters because changes over time may reflect both behavioral responses and changes in the composition of mothers observed on either side of the custody threshold.

4.1 Exposure definition and estimating equation

For each country, mothers are classified by whether their youngest child is below the applicable legal custody age at interview. Mothers below the cutoff are more strongly exposed to custody and associated support protections; mothers above it form the comparison group. The comparison group is not legally untreated, since the reform applies to all eligible women. Moreover, women can move from below to above the threshold as children age, so post-reform comparison groups may contain women previously classified as more exposed. The estimand is therefore a differential post-reform association by current exposure intensity.

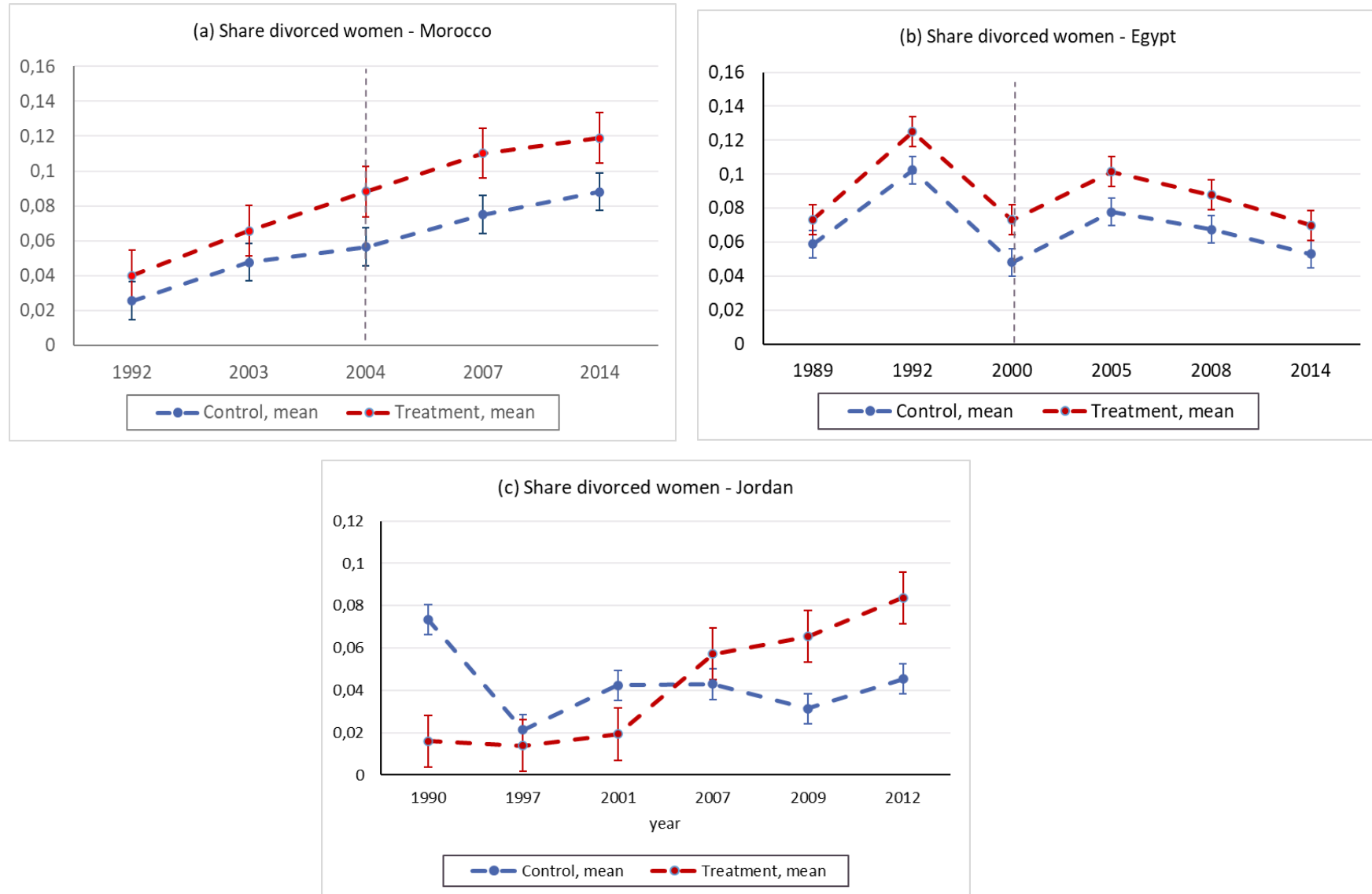
The baseline linear-probability specification relates current employment to post-reform status, the below-cutoff indicator, and their interaction, with survey-wave effects and observed covariates. Specifications add controls for age, age at first marriage, number of children, women's and partners' education, and selected household and macro indicators. Some columns also include cohort-specific linear trends. Because maternal employment follows a nonlinear life-cycle profile and the age of the youngest child directly affects care constraints, estimates that depend on linear cohort trends are treated as specification-sensitive rather than preferred causal estimates. Wild-bootstrap p-values are reported to address inference with few aggregate time periods (Cameron et al., 2008).

$$Y_{it} = \alpha_i + \theta_t + \delta_j(\text{post}_t, \text{treated}_i) + \beta X_{it} + \tau_i \cdot t + \varepsilon_{it}$$

The interaction coefficient compares the change in current employment for mothers below and above the custody cutoff. A causal interpretation would require, among other conditions, stable group composition and parallel counterfactual trends. Neither condition is assured here. The event studies reported in Section 5.6 formally reject equal pre-trends for the married-women samples. Accordingly, the coefficient is labeled a reform-associated employment difference throughout the revised paper.

Figs. 3 and 4 present raw divorce and employment patterns for the two exposure groups. The figures reveal baseline differences and changing gaps, including patterns that are inconsistent with a simple parallel-trends design. They are shown as diagnostic evidence rather than validation of identification.

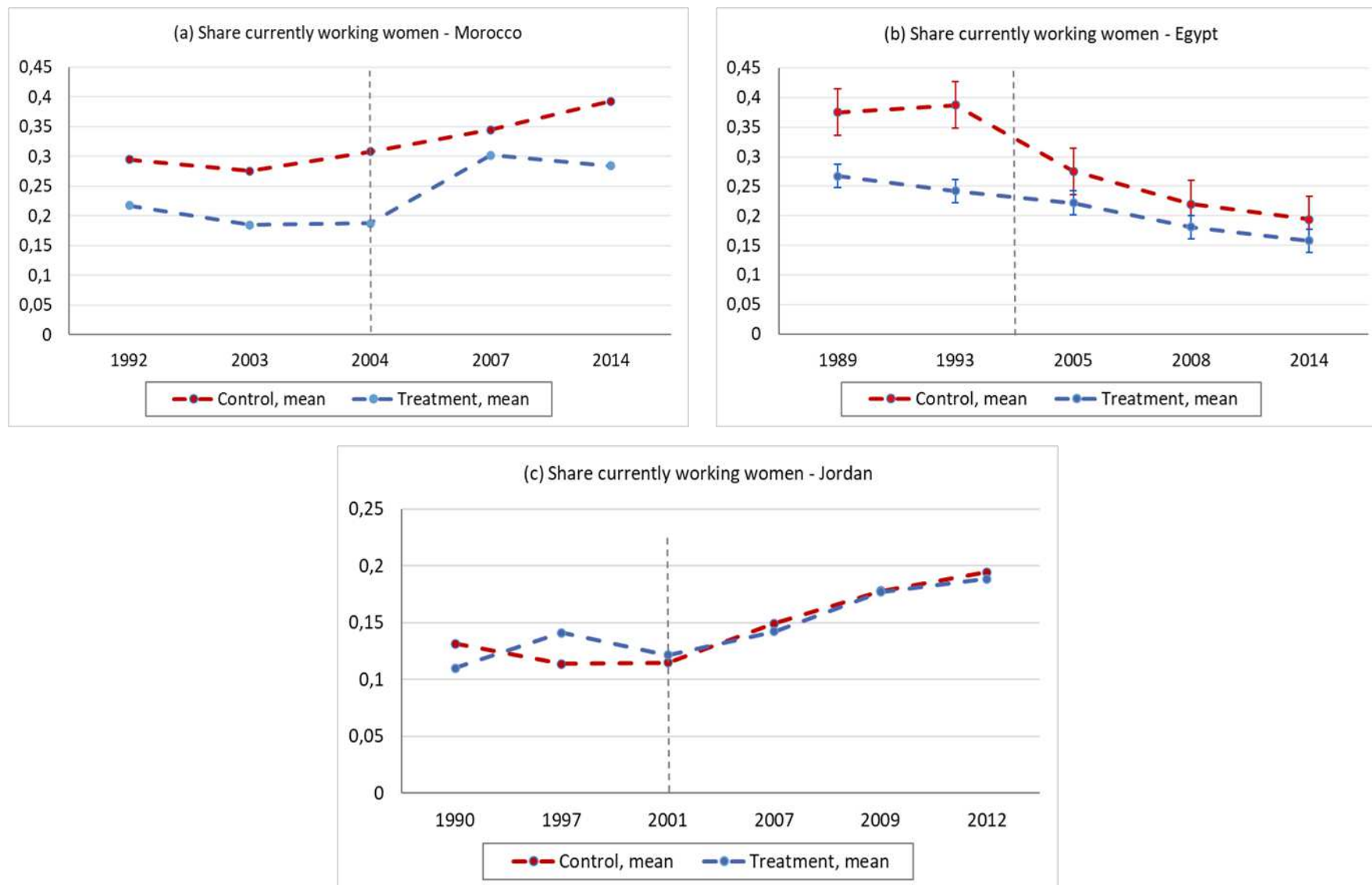
Fig. 3. Raw divorce rates by custody-threshold exposure group



Source: authors' calculations.

Note: Samples include mothers who can be classified relative to the custody cutoff. Red denotes the below-cutoff (more exposed) group and blue the above-cutoff comparison group. Group membership is measured at each survey wave and may change as children age.

Fig. 4. Raw current-employment rates by custody-threshold exposure group



Source: authors' calculations.

Note: Red denotes the below-cutoff (more exposed) group and blue the above-cutoff comparison group. These raw trends show why age-related care constraints and compositional change are central limitations.

4.2 Data, sample construction, and outcome

The main data are nationally representative repeated cross-sections from the Demographic and Health Surveys (DHS), covering 1988–2017. For Morocco, DHS observations are supplemented with the 2007 and 2014 Household Consumption and Expenditure Surveys produced by the High Commission for Planning (HCP). Table 1 lists the waves used. The use of two survey families in Morocco is an additional comparability limitation, despite variable harmonization.

Table 1. Survey waves used by country

Data Sources	Before reform	After reform
Egypt	DHS 1988, 1992, 1996	DHS 2003, 2005, 2008, 2014
Jordan	DHS 1990, 1997	DHS 2002, 2007, 2009, 2012, 2017
Morocco	DHS 1992 and 2003	HCP Household survey 2007 and 2014

The outcome is a binary indicator equal to one when a respondent reports currently working (including the survey-specific recent-work reference where applicable). It measures the extensive margin only. The surveys do not consistently identify full-time versus part-time work, hours, employment quality, or complete employment histories. The analysis therefore cannot assess intensive-margin responses or distinguish women with and without prior work experience.

The analytic sample is restricted to ever-married mothers aged 18–49 who had completed fertility at least one year before the principal reform, so that the comparison focuses on marriages and births already in place. Never-married women cannot be assigned using the youngest-child custody rule and are not a valid placebo within the same design. Polygamous unions are not consistently identifiable across all waves and survey sources; they are therefore not separately excluded, which should be kept in mind when interpreting Morocco and Jordan.

The sample is constructed in four steps: retain women aged 18–49; retain ever-married mothers with the required child-age information; apply the pre-reform fertility restriction; and retain observations within the country-specific analysis window and custody-cutoff bandwidth. Tables 2–4 report full descriptive samples and the below- and above-cutoff working samples. Differences between descriptive and regression sample sizes arise from the additional bandwidth, non-missing-covariate, marital-status, and window restrictions used in each specification. Exact estimation-sample counts are reported in the regression tables.

Table 2. Descriptive statistics: Morocco

	Mean (1)	Pre-reform (2)	Post-reform (3)	Control (post-reform) (4)	Treatment (post-reform) (5)
Marital status					
Married	0.89	0.87	0.90	1	1
Divorced/separated	0.07	0.09	0.04	0	0
Demographics					
Age	35.13	35.01	35.21	36.13	36.17
Age at first marriage	19.52	19.39	20.1	20.56	20
Number of children	2.61	3.31	2.14	1.39	2.53
Number of children under 5	0.85	0.87	0.72	0	0.93
Number of household members	4.79	5.36	4.4	2.8	4.8
Educational attainment					
No Education	0.56	0.65	0.49	0.51	0.48
Primary	0.20	0.15	0.24	0.21	0.26
Secondary	0.17	0.15	0.18	0.17	0.15
Higher	0.05	0.03	0.06	0.07	0.06

D. Outcome variables					
Currently working	0.26	0.21	0.28	0.28	0.26
N	19024	7645	11379	1239	5790

Source: Authors' Calculations – Demographic and Health Survey (DHS): Morocco (1992,2004) and HCP Household survey (2007 and 2014).
Note: This table displays full and working sample means for several variables both pre and post shiqaq divorce reform in Morocco. For the first three columns, we present full sample means pre and post reform. The remaining columns present working sample means for treatment and control groups. While the table shows means values for treatment and control groups post "treatment", the empirical analysis employs a quasi-experimental framework that takes into consideration values for pre and post reform.

Tables 2–4 show substantial changes across survey waves in education, fertility, and household composition. These shifts caution against treating education-specific estimates as fixed-composition comparisons. The outcome mean in Table 2 has been retained as reported in the underlying harmonized data; it should be rechecked against the analysis code before final submission because the referee identified a possible inconsistency.

The empirical analysis uses a shorter post-reform window and the longest available window. These are labeled short-window and long-window comparisons, not immediate and structural effects. With infrequent survey waves, the data cannot recover the precise timing of behavioral adjustment or separate reform dynamics from other contemporaneous changes, including education, childcare, schooling, and labor-market reforms.

Table 3. Descriptive statistics: Egypt

	Mean	Pre-reform	Post-reform	Control (post-reform)	Treatment (post-reform)
	(1)	(2)	(3)	(4)	(5)
Marital status					
Married	0.95	0.94	0.95	1	1
Divorced/separated	0.31	0.04	0.02	0	0
Demographics					
Age	33.64	33.9	33.5	36.16	35.22
Age at first marriage	19.8	18.8	20	20.46	19.71
Number of children	2.93	3.5	2.77	1.80	3.27
Number of children under 5	0.88	1.03	0.84	0	0.71
Number of household members	4.93	5.53	4.76	3.76	5.28
Educational attainment					
No Education	0.29	0.43	0.25	0.28	0.30
Primary	0.16	0.30	0.12	0.14	0.13
Secondary	0.42	0.19	0.48	0.42	0.44
Higher	0.12	0.06	0.13	0.14	0.11
Outcome variable					
Currently working	0.19	0.19	0.19	0.22	0.22
N	54829	12096	42733	7359	16115

Source: Author's calculations – Demographic and Health Survey (DHS) – Egypt (1988, 1992, 1993, 2005, 2008, 2014).

Note: This table displays full and working sample means for several variables both pre and post Khul' divorce reform in Egypt. For the first three columns, we present full sample means pre and post reform. The remaining columns present working sample means for treatment and control groups. While the table shows means values for treatment and control groups post "treatment", the empirical analysis employs a quasi-experimental framework that takes into consideration values for pre and post reform.

Table 4. Descriptive statistics: Jordan

	Mean	Pre-reform	Post-reform	Control (post-reform)	Treatment (post-reform)
	(1)	(2)	(3)	(4)	(5)
Marital status					
Married	0.97	0.98	0.96	1	1
Divorced/separated	0.019	0.018	0.19	0	0
Demographics					
Age	33.9	32.8	34.12	37.88	33.18
Age at first marriage	20.74	19.3	21.02	20.12	22.4
Number of children	3.77	4.66	3.59	4.69	1.14
Number of children under 5	1.18	1.53	1.11	0	0.07
Number of household members	5.77	6.66	5.59	6.64	3.17
Educational attainment					
No Education	0.06	0.17	0.04	0.07	0.05
Primary	0.10	0.17	0.07	0.11	0.08
Secondary	0.54	0.47	0.55	0.54	0.50
Higher	0.29	0.17	0.32	0.26	0.36
Outcome variable					
Currently working	0.15	0.12	15.72	0.14	0.156
N	52070	8695	43375	6088	4719

Source: Authors' calculations - Demographic and Health Survey (DHS): Jordan (1990, 1997, 2002, 2007, 2009, 2012, 2017)

Note: This table displays full and working sample means for several variables both pre and post Ifida' divorce reform in Jordan. For the first three columns, we present full sample means pre and post reform. The remaining columns present working sample means for treatment and control groups. While the table shows means values for treatment and control groups post "treatment", the empirical analysis employs a quasi-experimental framework that takes into consideration values for pre and post reform.

5 Results

Tables 5–7 report country-specific reform-associated differences in mothers' current employment. The coefficients should be read together with the raw trends and event-study diagnostics. They do not identify a common treatment effect across countries, and differences between short and long windows may reflect changing samples, other reforms, and macroeconomic conditions as well as family-law change.

5.1 Short- and long-window comparisons

The first three columns of each country table use the shorter window; the remaining columns use the longest available window. In Morocco, the short-window estimate is positive and statistically distinguishable from zero only in a specification that includes linear cohort trends. Because the result depends on this functional-form choice and pre-trends are rejected, it is not treated as robust causal evidence. The longer-window Moroccan estimates are negative in several specifications. Egypt and Jordan display more positive longer-window coefficients, but the same identification cautions apply.

Table 5. Reform-associated current-employment differences: Morocco

	shiqaq' effect window 1 : 1992 to 2007			shiqaq effect window 2 : 1992 to 2014		
	(1)	(2)	(3)	(4)	(5)	(6)
	Currently working			Currently working		
Post x treated	0.0121 (1.36)	0.0118 (1.35)	0.0379* (2.33)	0.0236** (2.71)	0.0233** (2.70)	-0.0245** (-3.18)
Household Size		-0.00284 (-0.37)	-0.00398 (-0.53)		0.0242*** (3.55)	0.00724 (1.00)
WBLaw Index			-0.00560 (-1.85)			0.00658*** (17.51)
Observations	8184	8184	8184	17012	17012	17012
Mean of outcome variable	0.214	0.214	0.214	0.259	0.259	0.259
WB p-value	0.002	0.002	0.738	0.57	0.742	0.002
N cohorts	44	44	44	54	54	54
Time trends	No	No	Yes	No	No	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Source: Authors' Calculations - Demographic and Health Survey (DHS): Morocco (1992, 2003) and HCP Household survey data for 2007 and 2014). Notes: The dependent variable is a binary indicator for whether the interviewed woman is currently working. Columns 1 to 3 report results for a restricted period representing 15 years around the reform year between 1992 and 2007. Columns 4 to 6 report results for the full covered period between 1992 and 2014. The estimation samples are restricted to married women whose youngest child's age is around 5 years window around the custody cutoff and whom did not have children after the reform. Wild bootstrap (WB) p-value using one thousand replications. *p<0.10, **p<0.05, ***p<0.01.

5.2 Morocco

Morocco's 2004 Moudawana combined divorce reform with changes to guardianship, marriage age, spousal responsibilities, and polygamy. Controls based on the Women, Business and the Law index cannot isolate one component of this package; they summarize broader legal change. The negative longer-window association may reflect labor-market conditions, childcare constraints, survey harmonization, compositional change, or other reforms. It should not be interpreted as evidence that expanded legal rights reduced women's employment.

Taken together, the Moroccan estimates show specification sensitivity rather than a stable dynamic treatment effect. Their main value is to demonstrate that a broad family-law reform and mothers'

employment evolved differently across exposure groups, and that legal change alone did not overcome the structural constraints facing women's access to paid work.

5.3 Egypt

In the shorter window, coefficients for mothers below the custody threshold are small and generally imprecise. In the longer window, several specifications are positive. Egypt's 2005 custody amendment and the financial conditions attached to khul' complicate interpretation: exposure and the content of the legal option changed during the observation period. The estimates are compatible with a gradual association between stronger family-law options and employment, but do not establish the mechanism or timing.

Table 6. Reform-associated current-employment differences: Egypt

	Khul' effect window 1 : 1992 to 2005			Khul' effect window 2 : 1992 to 2014		
	(1)	(2)	(3)	(4)	(5)	(6)
		Currently working			Currently working	
Post x treated	0.0604*** (6.82)	0.0603*** (6.80)	0.0177 (1.43)	0.0479*** (8.50)	0.0479*** (8.48)	0.0271** (3.04)
Household Size		-0.00755 (-0.99)	-0.00566 (-0.78)		-0.00119 (-0.21)	-0.000743 (-0.13)
WB Law Index			0.0701*** (6.12)			0.00470 (0.61)
Observations	18838	18838	18838	29589	29589	29589
Mean of outcome variable	0.23	0.23	0.23	0.22	0.22	0.22
WB p-value	0.002	0.002	0.396	0.006	0.0140	0.006
N cohorts	45	45	45	58	58	58
Time trends	No	No	Yes	No	No	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Source: Authors' calculations – Demographic and Health Survey (DHS) – Egypt 1992, 1993, 2005, 2008, 2014.

Notes: The dependent variable is a binary indicator for whether the interviewed woman is currently working. Columns 1 to 3 report results for a restricted period representing 13 years around the reform year between 1992 and 2005. Columns 4 to 6 report results for the full covered period between 1992 and 2014. The estimation samples are restricted to married women whose youngest child's age is around 5 years window around the custody cutoff and whom did not have children after the reform. The custody cutoffs are set to 10 for the first window (1992 to 2005), and to 15 for the second window to account for the custody. Wild bootstrap (WB) p-value using one thousand replications. *p<0.10, **p<0.05, ***p<0.01.

The longer coverage distinguishes this analysis from Hassani-Nezhad and Sjögren (2014), but it also increases exposure to concurrent structural change. The Egyptian results should therefore be read as a country-specific institutional trajectory, not as a clean estimate of khul' alone.

5.4 Jordan

Jordan displays positive coefficients in several short- and long-window specifications. Yet the legal episode was staggered and contested: the 2001 decree, subsequent parliamentary resistance, and the 2010 codification of iftida' do not constitute a single treatment date. The estimates may capture gradual institutional change alongside evolving labor demand and social norms. They are most informative as evidence that legal implementation and political legitimacy matter for whether formal entitlements become usable options.

Table 7. Reform-associated current-employment differences: Jordan

	Iftida' effect window 1: 1997 to 2007			Iftida' effect window 2: 1992 to 2017		
	(1)	(2)	(3)	(4)	(5)	(6)
	Currently working			Currently working		
Post x treated	-0.00134 (0.1077)	-0.00123 (-0.11)	0.166 (0.0125)	0.0000315 (0.00)	0.0000466 (0.00)	0.0195* (1.75)
Household Size		0.00193 (0.23)	-0.0033 (0.0087)		0.000380 (0.06)	-0.00167 (-0.27)
WB Law Index			0.0534** (0.0174)			0.0189** (6.40)
Observations	10124	10124	10124	19295	19295	19295
Mean of outcome variable	0.135	0.135	0.135	0.137	0.137	0.137
WB p-value	0.368	0.336	0.314	0.372	0.398	0.0940
N cohorts	42	42	42	59	59	59
Time trends	No	No	Yes	No	No	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Source: Authors' Calculations - Demographic and Health Survey (DHS): Jordan (1990, 1997, 2002, 2007, 2009, 2012).

Notes: The dependent variable is a binary indicator for whether the interviewed woman is currently working. Columns 1 to 3 report results for a restricted period representing 10 years around the reform year between 1997 and 2007. Columns 4 to 6 report results for the full covered period between 1992 and 2017. The estimation samples are restricted to married women whose youngest child's age is around 5 years window around the custody cutoff and whom did not have children after the reform. Wild bootstrap (WB) p-value using one thousand replications. *p<0.10, **p<0.05, ***p<0.01.

The Jordanian case illustrates the gap between law on the books and law in use. Financial renunciation, legal process, and social acceptability can limit access even after formal enactment (El Muhtaseb et al., 2016; Engelcke, 2018). The observed employment pattern cannot determine which of these channels dominates.

5.5 Marital-status decomposition

Table 8. Marital-status decomposition and sensitivity checks

	Iftida' effect in Jordan				Khul' effect in Egypt				shiqaq' effect in Morocco			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Window 1: 1997 to 2007		Window 2: 1992 to 2012		Window 1: 1992 to 2005		Window 2: 1992 to 2014		Window 1: 1992 to 2007		Window 2: 1992 to 2014	
	Married	Divorced	Married	Divorced	Married	Divorced	Married	Divorced	Married	Divorced	Married	Divorced
Treatment x child age cutoff	0.166 (0.0125)	0.0477 (1.12)	0.0195* (1.75)	0.0460 (1.50)	0.0177 (1.43)	-0.0325 (-0.61)	0.0271** (3.04)	-0.00516 (-0.18)	0.0379* (2.33)	0.0584 (0.87)	-0.0245** (-3.18)	0.0460 (1.50)
Household Size	-0.0033 (0.0087)	-0.00583 (-0.20)	-0.00167 (-0.27)	0.0131 (0.59)	-0.00566 (-0.78)	-0.0199 (-0.79)	-0.000743 (-0.13)	-0.0431** (-2.92)	-0.00398 (-0.53)	0.00253 (0.12)	0.00724 (1.00)	0.0131 (0.59)
World Bank Law Index	0.0534** (0.0174)	0.0262 (0.46)	0.0189** (6.40)	0.00523 (0.65)	0.0701*** (6.12)	0.0228 (0.17)	0.00470 (0.61)	0.0362 (1.17)	-0.00560 (-1.85)	-0.0162 (-1.27)	0.00658*** (17.51)	0.00523 (0.65)
Observations	10124	290	19295	670	18838	743	29589	1333	8184	548	17012	670
Mean of outcome variable	0.135	0.127	0.137	0.155	0.23	0.32	0.22	0.30	0.214	0.47	0.259	0.53
WB p-value	0.314	0.608	0.0940	0.262	0.396	0.460	0.006	0.260	0.738	0.158	0.002	0.262
N clusters	42	28	59	45	45	40	58	43	44	34	54	43
Time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Source: Authors' calculations. Notes: The dependent variable is a binary indicator for whether the interviewed woman is currently working. The estimation samples are restricted mothers whose youngest child's age is around 5 years window around the custody cutoff and whom did not have children after the reform. Wild Bootstrap p-value for 1000 reps. *p<0.10, **p<0.05, ***p<0.01.

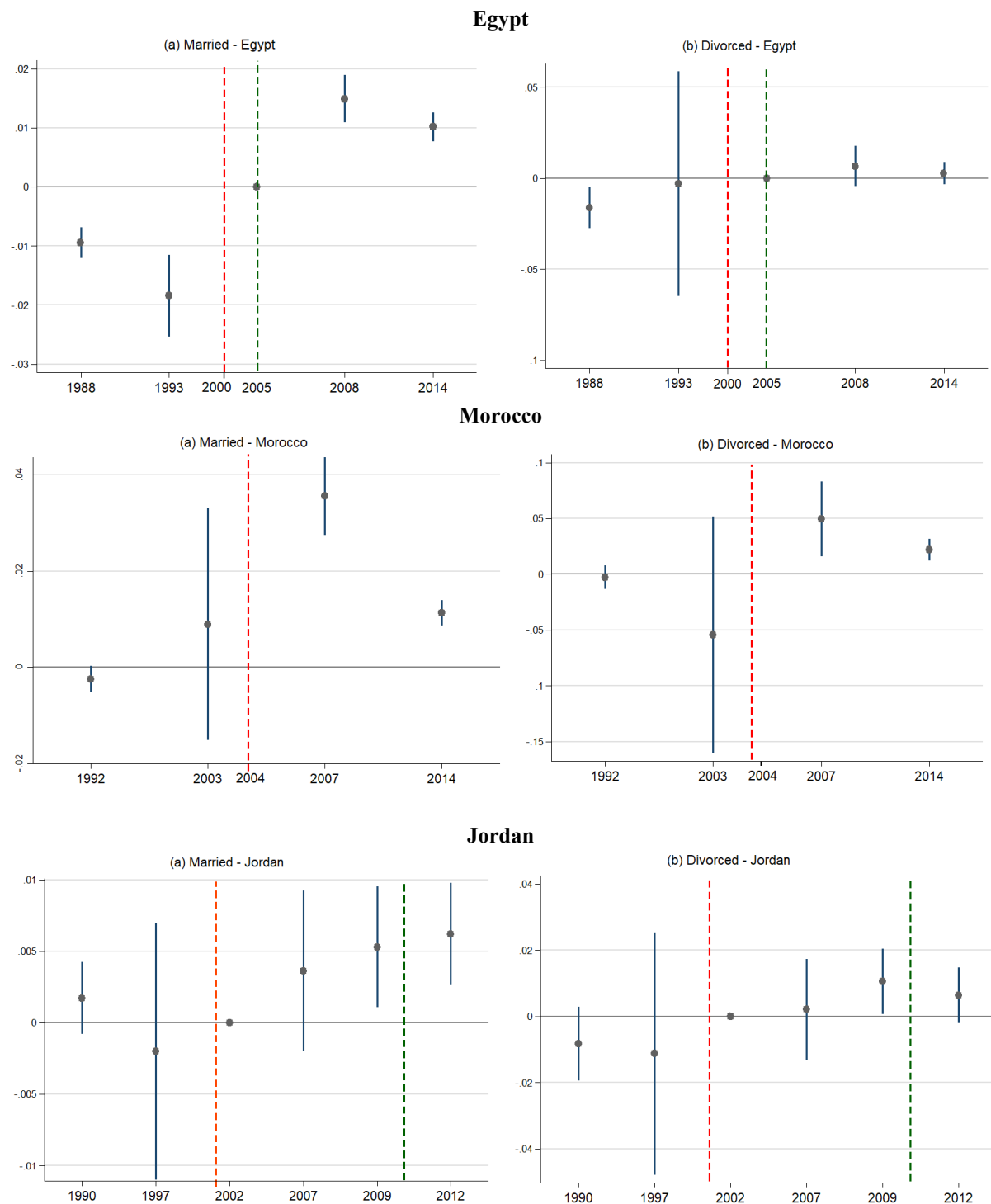
5.6 Pre-trend diagnostics and identification limits

The event-study specification examines whether employment gaps between exposure groups were already changing before reform. For married women, the joint tests of pre-reform coefficients yield p-values reported as 0.000 in all three countries. This is a formal rejection of the parallel-trends condition required for a causal difference-in-differences interpretation. The event-study estimates are therefore diagnostic: they show that the baseline coefficients combine reform timing with pre-existing differential trajectories.

$$Y_{it} = \sum_{\tau=-L}^{-1} \beta_{\tau} (1\{H(t) = \tau\} \times post_i) + \sum_{\tau=1}^K \beta_{\tau} (1\{H(t) = \tau\} \times post_i) + \alpha_i + \theta_t + \varepsilon_{it} \quad (2)$$

Several features plausibly generate these trajectories. The age of the youngest child is itself a strong determinant of maternal employment; group composition changes as children age; post-reform comparison groups can contain previously more-exposed mothers; and cohort-specific linear trends impose an unjustified approximation to nonlinear life-cycle employment. No available specification fully resolves these problems. The paper consequently avoids causal verbs and does not interpret coefficient differences as proof of bargaining or anticipatory mechanisms.

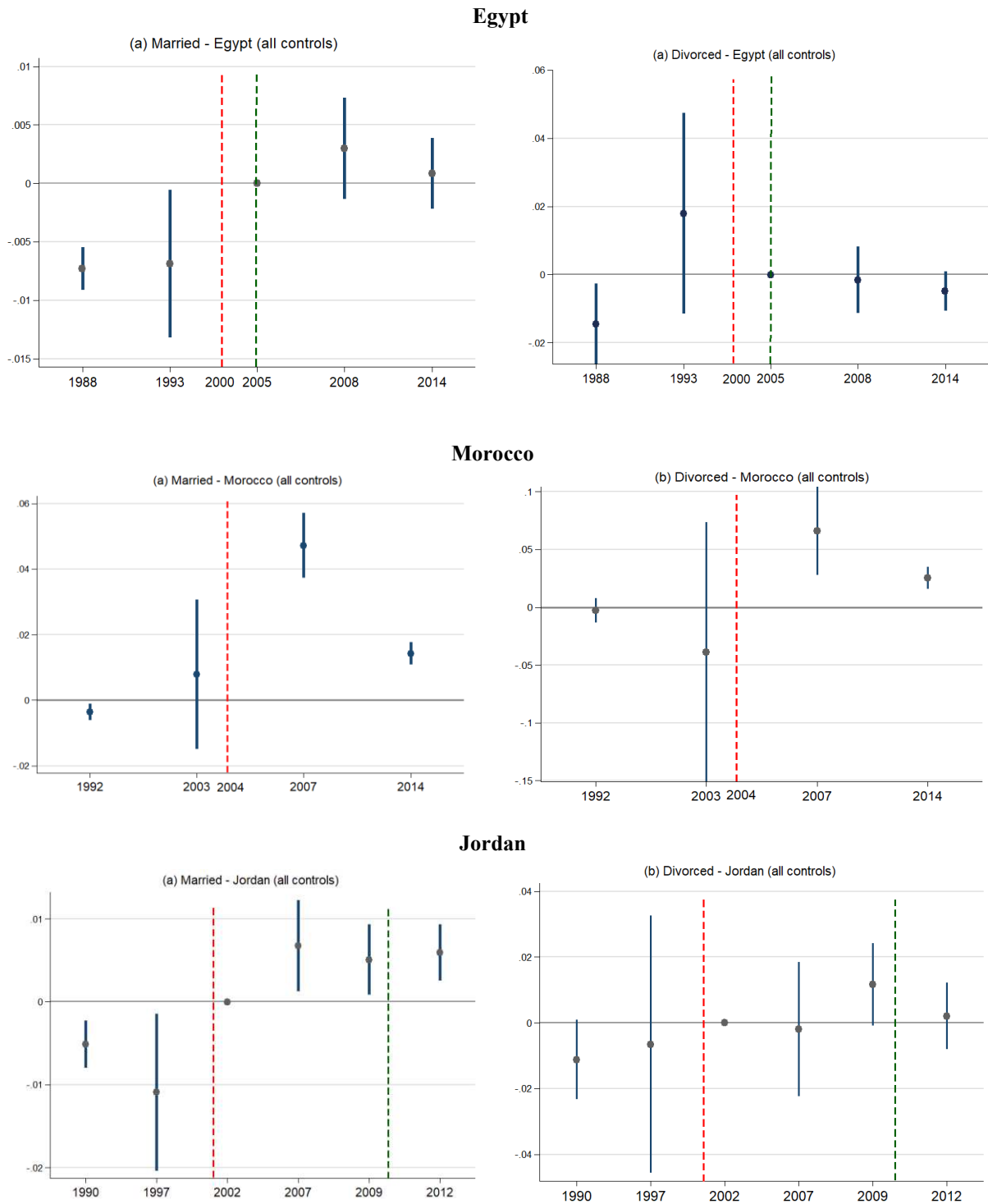
Fig. 5. Event-study diagnostics with baseline controls



Source: Authors' calculations.

Note: Event-study coefficients for current employment. Left panels show married women and right panels divorced women. The joint pre-reform tests reject equal trends for married women in all countries; confidence intervals use the inference procedure described in Section 4.

Fig. 6. Event-study diagnostics with extended controls



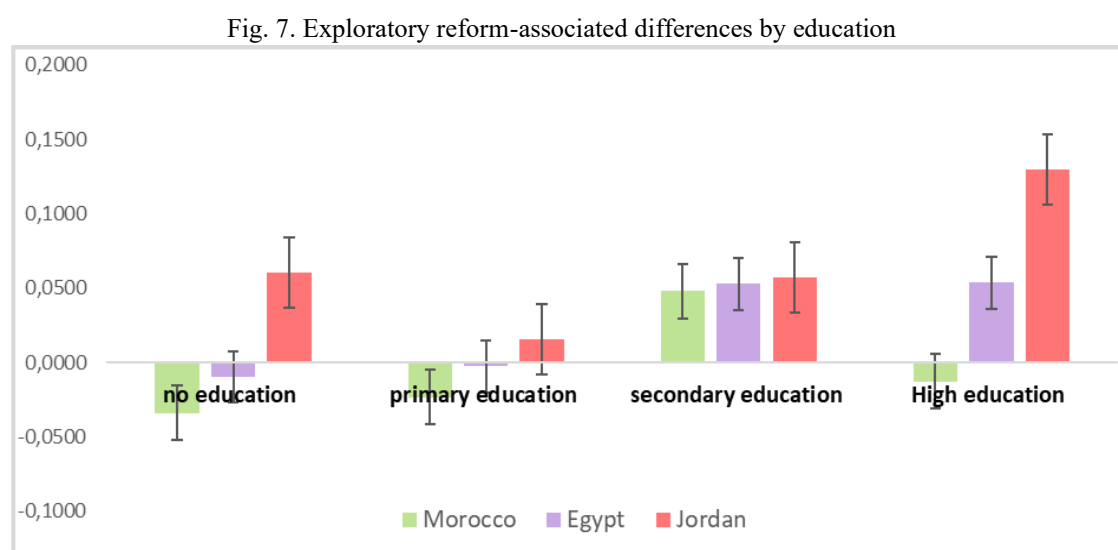
Source: Authors' calculations.

Note: Extended controls do not restore a credible parallel-trends pattern. The figures are presented to make the design's limitations transparent, not to support causal interpretation.

5.7 Education heterogeneity

Fig. 7 reports exploratory estimates by education. Educational attainment rises across survey waves, so these subgroups are not compositionally stable. The estimates may reflect changes in who reaches each educational category as well as heterogeneous reform associations. They should not be interpreted as causal subgroup effects.

Across countries, coefficients vary by education, with some more positive estimates among women with secondary education. This pattern is consistent with the importance of matching between education and available jobs, but the surveys do not provide a sufficiently comparable demand-side or occupational history measure to test that explanation.



Source: Authors' calculations using DHS and HCP data. Note: Samples are restricted as described in Section 4. Education categories change in composition over time; estimates are exploratory.

The education results reinforce a limited conclusion: legal rights operate within labor markets that differ in their capacity to offer suitable and decent work. Establishing whether occupational mismatch mediates the relationship requires linked labor-demand data or repeated employment histories, which are beyond the present study.

6 Discussion and conclusion

This paper asks how reforms of family governance relate to mothers' paid employment in Egypt, Jordan, and Morocco. Its central finding is not a universal employment effect of unilateral divorce. Rather, it is that formal rights, custody protections, and employment evolved in heterogeneous ways across distinct institutional settings. Short-window differences are generally modest; longer-window patterns are more positive in Egypt and Jordan and negative in Morocco in several specifications. These contrasts cannot be given a causal interpretation because pre-trends are rejected and exposure groups change composition.

The revised interpretation separates legal value from labor-market response. A right to exit an unequal or harmful marriage can improve dignity, security, and bargaining power even when current employment does not change. Conversely, employment is not automatically emancipatory when work is insecure, poorly paid, or incompatible with care responsibilities. A human-centred assessment of family-law reform should therefore consider substantive capabilities: whether women can use legal rights, obtain maintenance and custody decisions in practice, access childcare and safe transport, and find decent work that makes economic autonomy feasible.

Policy should treat family law, social protection, care services, and labor-market institutions as complements. Priorities include affordable childcare, enforceable maintenance and custody decisions, accessible legal aid, safe working conditions, and labor-demand policies that convert women's education into suitable employment. Better evidence is equally important. Future surveys should link marital histories, employment trajectories, hours and job quality, childcare use, region, and measures of household decision-making. Designs based on narrower eligibility discontinuities or administrative implementation variation would permit stronger causal inference. The present results are best understood as a transparent comparative diagnosis: law can enlarge women's options, but the common good is advanced only when social and economic institutions make those options genuinely usable.

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Ethics approval. Not applicable. The study uses anonymized secondary survey data.

Data and code availability. DHS microdata are available from the DHS Program subject to registration and approval. Moroccan HCP microdata are subject to the provider's access conditions. Harmonization and estimation code can be made available by the corresponding author upon reasonable request, subject to data-use restrictions.

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