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Descriptive Finding

Decriminalization of adultery likely changed women's views on divorce following spousal infidelity in South Korea

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Descriptive Finding

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Abstract

BACKGROUND
Laws imposing criminal penalties for extramarital affairs stir intense debates in several countries, highlighted by recent repeals in countries like India, South Korea, and Taiwan. However, we currently lack empirical studies of their societal impacts.

OBJECTIVE
This study examines the impact of decriminalizing extramarital affairs on women's attitudes toward divorce in cases of potential spousal infidelity in South Korea.

METHODS
We analyze the first five waves of the Korean Longitudinal Survey of Women and Families, which provides a nationally representative sample of adult women in South Korea. The fifth wave coincides with the public announcement of the adultery law's repeal. For causal identification, we leverage the potentially exogenous timing of the Constitutional Court's decision in a difference-in-differences analysis.

RESULTS
Our findings reveal a notable shift in attitudes after the repeal: The decriminalization of extramarital affairs has led women to be less likely to consider a husband's infidelity as justifying divorce on its own.

CONCLUSIONS
Decriminalizing adultery laws can affect societal views, shifting how women perceive spousal infidelity as a basis for divorce.

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CONTRIBUTION

While discussions about adultery laws frequently rely on empirical arguments, significant research gaps remain on their societal effects. This study provides what is, to our knowledge, the first empirical and potentially causal analysis of the consequences of decriminalizing adultery, an issue that sparks considerable debate in many countries. It establishes an empirical basis for further exploration, encouraging continued research and more informed public discussions. Future research directions are also briefly discussed.

1. Introduction

Adultery laws criminalizing extramarital sexual conduct still provoke significant debate in many parts of the world. Reflecting a global shift (Frank, Camp, and Boutcher 2010), several countries have recently repealed these laws. Taiwan's Constitutional Court, on May 29, 2020, annulled Article 239 of its Criminal Code, which had criminalized adultery for 85 years. Similarly, in 2018, India's Supreme Court unanimously struck down Section 497, a colonial-era law that penalized men for extramarital affairs. However, adultery laws persist in many countries, especially those under Islamic legislation and in some Asian countries, such as the Philippines and Cambodia. In a striking move, Indonesia's Parliament, in 2022, broadened its adultery definition to include all extramarital sexual activity, affecting even unmarried and cohabiting individuals. In the United States, though seldom enforced, adultery remains a criminal offense in 17 states, with some imposing imprisonment (Echols 2022).

Proponents of abolishing adultery laws argue that they violate fundamental constitutional rights, such as personal autonomy, privacy, and sexual self-determination, without effectively preventing family dissolutions. Opponents claim these laws uphold moral standards and marital sanctity and provide legal remedies for wronged partners, which is especially crucial in societies with strong patriarchal norms. There are also concerns that abolishing these laws could increase divorce and adultery rates (Chung and Liu 2018; Rhode 2016).

The debate over adultery laws encompasses not just legal and moral considerations but also empirical questions about their societal impact. Understanding the real-world consequences of repealing these laws is thus important, especially in contexts where this is a recent development or actively debated. We investigate the consequences of South Korea's (Korea hereafter) adultery decriminalization in 2015, presenting, to our knowledge, the first empirical and potentially causal analysis of its societal effects. Leveraging the plausibly exogenous timing of the Constitutional Court's decision in a difference-in-differences analysis, we observe a notable change: Post-decriminalization, Korean women are less likely to view a husband's infidelity as justifying divorce on its own.

2. Institutional background

The legal prohibition of adultery in Korea began with the 1905 Criminal Code of the Great Korean Empire, which penalized adulterous women and their partners but not men. This bias continued under Japanese colonial rule until the 1953 Criminal Code of the Republic of Korea, notably Article 241, criminalized adultery for both genders, allowing for prison terms of up to two years (see Lee 2016). Since then, Korea's adultery law has led to charges against approximately 53,000 individuals, predominantly men, though female charges have risen over time (Chung and Liu 2018; The Guardian 2015, February 26). Over the years, the frequency of these charges and resultant imprisonments declined sharply, with only 22 cases resulting in imprisonment post-2008 (The Guardian 2015, February 26).

By the 1980s, as women's economic and social status improved and awareness of personal freedoms grew, the law's effectiveness and relevance were increasingly debated. It was brought before the Constitutional Court in the years 1990, 1993, 2001, and 2008, culminating in a divided court decision in 2008. Nevertheless, the court upheld the law on each occasion, maintaining that its restrictions on fundamental rights were considered justifiable and proportional.

Public sentiment toward the adultery law was mixed, even close to its repeal; a 2014 survey showed 60% of adults favored maintaining the law, 21% supported eventual but not immediate repeal, and only 19% called for its immediate removal (Park et al. 2014). However, on February 26, 2015, Korea's Constitutional Court struck down the adultery law in a decisive seven-to-two vote, citing violations of sexual autonomy, privacy, and personal freedoms, leading to its immediate nullification and formal repeal through a Criminal Code revision on January 6, 2016. Nonetheless, despite its decriminalization, adultery still affects civil litigation, influencing divorce proceedings in asset division and custody disputes and permitting compensation claims against third parties involved in affairs, which could lead to considerable settlements.

3. Data and measures

3.1 Data and sample

We utilize data from the first five waves of the Korean Longitudinal Survey of Women's Families. The survey, starting in 2007, targets women aged 19 to 64 in noninstitutional households across Korea. Follow-up surveys were conducted biennially, with an additional survey in 2008. Importantly, the fifth wave, from June 30, 2014, to April 1, 2015, coincides with the period the adultery law's repeal was publicized.

From the initial 9,997 respondents, we focus on the 6,152 individuals who participated in all five waves. After excluding 10 respondents for missing outcome data in any wave and an additional 2 for missing covariate data, our final analytical sample comprises 6,140 individuals (30,700 person-waves). Notably, 406 completed their fifth-wave interviews on or after February 26, 2015 – the date the adultery law was repealed.

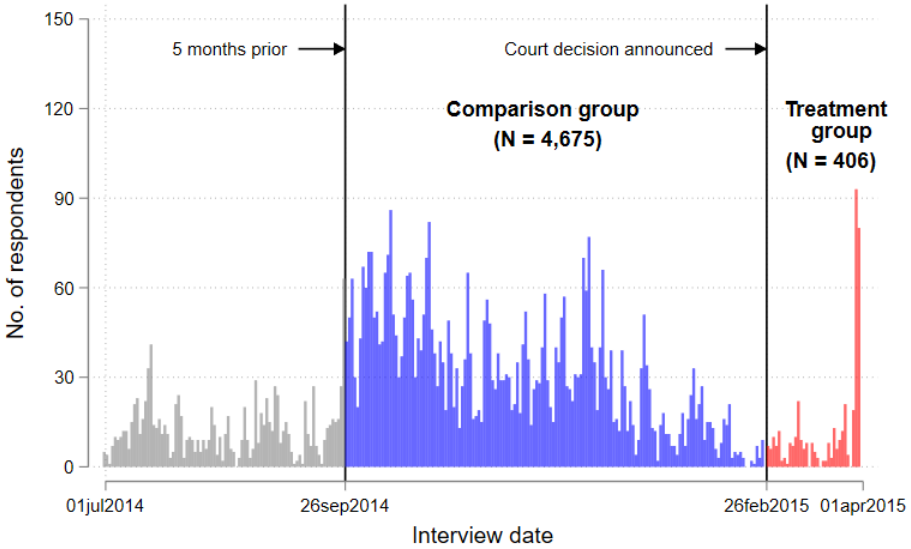
3.2 Measures

Outcome. The question to all respondents in each wave "Please tell us about your opinion on the following: When a husband is involved in an extramarital affair, one should get divorced" serves as our outcome variable. Responses were collected on a four-point Likert scale: 1 "strongly agree," 2 "somewhat agree," 3 "somewhat disagree," and 4 "strongly disagree." For analysis, we treat them as continuous, with reversed coding where higher values indicate a stronger inclination toward divorce following infidelity.

Treatment. Participants interviewed after the repeal's public announcement on February 26, 2015, during the fifth wave, are considered the treatment group, where "treatment" indicates being surveyed after the announcement was made. Conversely, those interviewed from September 26, 2014, to February 25, 2015 – the five months leading up to the announcement – in the same wave form the comparison group. Figure 1 illustrates this treatment and comparison group classification. The selection of a five-month threshold, while somewhat arbitrary, balances between identification and statistical power. Extending this interval increases precision but risks the identification assumption, while shorter periods strengthen identification but add estimate uncertainty. This five-month interval is our benchmark, and we explore the consequences of modifying this interval in our results.

Covariates. Age, education, employment status, religion, residence type at age 15, and self-rated health are considered as covariates, with measurements taken across the waves. Marital status was considered but excluded due to its limited variability; only 6.7% of respondents in our analytical sample were never married in the initial wave. Given their minimal overtime variation, we consider these covariates time-constant. Consequently, we use measurement from the fourth wave for these variables, except for the residence at age 15, which is based on the first wave.

Figure 1: Illustration of the research design



Notes: The horizontal axis indicates the interview dates during the fifth survey wave, spanning June 30, 2014, to April 1, 2015. The vertical axis records the daily interview counts. The two vertical solid lines mark the date of the court's decision announcement and the exact date five months prior, establishing the bounds of our comparison group.

3.3 Empirical approach

Denoting individuals and survey waves by i and w, respectively, D_i is a binary variable, assigned one for the treatment group and zero for the comparison group. P_w is one for the posttreatment period (wave 5) and zero for the pretreatment periods (waves 1–4). Our treatment indicator $A_{i,w} = D_i \cdot P_w$ equals one for treatment group members during the posttreatment period and zero otherwise.

We aim to estimate the average treatment effect on the treated (ATT), defined as

$$\mathbb{E}[Y_{i,5}(1) - Y_{i,5}(0)|A_{i,w} = 1], \tag{1}$$

where $Y_{i,w}(1), Y_{i,w}(0)$ are the potential outcomes with and without treatment, respectively (Rubin 1974). This parameter quantifies the impact of knowing about the adultery law's repeal on the treatment group during the fifth survey wave.

To estimate the parameter, we use a difference-in-differences model (Wooldridge 2021):

$$Y_{i,w} = \alpha + \delta^{(u)} A_{i,w} + \gamma D_i + \delta P_w + \epsilon_{i,w}. \quad (2)$$

Here, $\delta^{(u)}$ is the key parameter representing a simple comparison of change-in-means – the differences in the mean outcome change between the pretreatment (waves 1–4) and posttreatment (wave 5) periods across the treatment/comparison groups. It represents the ATT under the assumption of (1) no anticipation and (2) parallel trends. The first assumption stipulates the treatment had no effect prior to its enactment, particularly relevant for the comparison group surveyed close to the repeal, who may have adjusted their opinions in expectation of the change. Any deviations would suggest that our estimates are conservative, rendering them lower-bound estimates.

Figure 2 displays the search trends in Korea from January 2014 to January 2016 for "adultery" (*gantong*), "adultery law" (*gantongbeob*), and "abolition of adultery law" (*gantongbeob pyeji*), using Google Trends data normalized to a peak value of 100 for each term. Search interest for all terms spiked on the repeal date, with minor peaks around the court decision day, particularly for "abolition of adultery law." This pattern, by and large, suggests that the court's decision might not have been prominently in the minds of the comparison group respondents at the time of their survey.

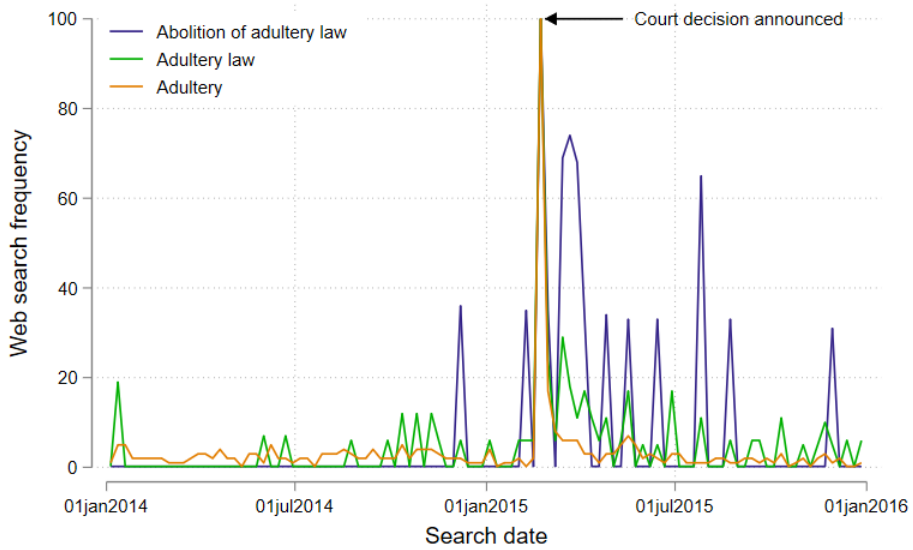
The parallel trends assumption asserts that, absent the treatment, outcome trajectories would have been the same for both treatment and comparison groups between the pre- and posttreatment periods. We do not expect the legal amendment itself to have influenced the interview schedules in ways that would violate this assumption. Nevertheless, our research design places the treatment group on later interview dates, potentially introducing differences from the comparison group that could impact outcomes. Our extended model partially addresses these concerns by incorporating covariates:

$$Y_{i,w} = \alpha + \delta^{(c)} A_{i,w} + (A_{i,w} \cdot \bar{\mathbf{X}}_i)\boldsymbol{\phi} + \mathbf{X}_i\boldsymbol{\zeta} + \gamma D_i + (D_i \cdot \mathbf{X}_i)\boldsymbol{\lambda} + \eta P_w + (P_w \cdot \mathbf{X}_i)\boldsymbol{\xi} + \epsilon_{i,w}. \quad (3)$$

Here, $\bar{\mathbf{X}}_i$ represents the covariates centered at their treatment-group means. This adjustment identifies the ATT under slightly weaker conditions: The two main identification assumptions hold within the levels of the covariates (Wooldridge 2021).

Table 1 shows the covariate balance, assessed through t-tests and χ^2-tests, with most variables balanced except for religious affiliation. Perhaps more importantly, this observed difference may indicate unmeasured differences. Our difference-in-differences approach remains robust to such unobserved variables, assuming their effects on the outcome and imbalance across the two groups remain constant in pre- and posttreatment periods. We address this further in the next section. All analyses cluster standard errors at the individual level (Bertrand, Duflo, and Mullainathan 2004) and apply longitudinal sampling weights to account for sampling error and nonrandom panel attrition.

Figure 2: Search trends in Korea for "adultery," "adultery law," and "abolition of adultery law" on Google from January 2014 to January 2016



Notes: Search frequencies for each term are normalized, with the maximum frequency set to 100. The figure illustrates peaks in the public interest that coincide with the announcement of the court's decision.

4. Findings

The ATT estimates are shown in Table 2. Within Panel A, the baseline model, which uses a five-month criterion to define the comparison group, is outlined in the first column. The ATT is estimated at –0.23 on a four-point scale, representing about one-third of a standard deviation (standard error 0.11; $p = 0.036$). This indicates a noticeable shift among Korean women away from the view that a husband's infidelity automatically justifies divorce. For context, this effect's magnitude is comparable to two-thirds of the observed educational difference (high school or less vs. some college education or more) during the pretreatment periods, approximately 0.35, and three-fourths of the age difference between individuals under 40 and those 50 and older.

Subsequent analyses test the stability of the ATT across different comparison group definitions. The baseline ATT remains unchanged when the comparison group includes all nontreated respondents (second column). The stability of the estimates persists in the third and fourth columns, where the threshold is reduced to four or three months. However, the precision of these estimates decreases with smaller sample sizes. When the

comparison group is restricted to those surveyed two months or fewer before the treatment, the ATT in the final two columns shows less certainty, mainly due to diminished statistical power as sample sizes shrink to less than a third of the original benchmark. Panel B, incorporating covariates, shows almost identical results with modest efficiency gains, reflected in lower standard errors and p-values.

Table 1: Descriptive statistics by treatment status

	Mean		Min/Max		p-value
	Untreated group	Treated group	Min	Max	χ^2/t-test
Outcome variable					
Pretreatment periods (waves 1-4)	2.52	2.63	1	4	0.11
	(0.57)	(0.52)			
Posttreatment period (wave 5)	2.53	2.41	1	4	0.07
	(0.79)	(0.73)			
Age					
Below 40	0.40	0.44	0	1	0.36
40–49	0.24	0.27	0	1	
50 and above	0.37	0.29	0	1	
Education					
High school or less	0.60	0.56	0	1	0.56
Some college or above	0.40	0.44	0	1	
Employment status					
Not in labor force/unemployed	0.49	0.44	0	1	0.48
Employed	0.51	0.56	0	1	
Religion					
None/other	0.47	0.52	0	1	0.04
Protestant	0.22	0.29	0	1	
Catholic	0.08	0.06	0	1	
Buddhist	0.23	0.13	0	1	
Type of residence at age 15					
Metropolitan/abroad	0.38	0.34	0	1	0.67
Mid-sized city	0.22	0.26	0	1	
Rural	0.40	0.39	0	1	
Self-rated health	3.60	3.49	1	5	0.16
	(0.88)	(0.77)			
No. of individuals	4,675	406	-	-	

Notes: The descriptive statistics are based on individual respondents, not respondent-time observations and the five-month comparison group threshold. All covariates are measured during the fourth survey wave, except for the residence at age 15, which is captured in the initial wave. Differences in means or distributions between the groups are examined using χ^2 tests for categorical variables and t-tests for interval variables.

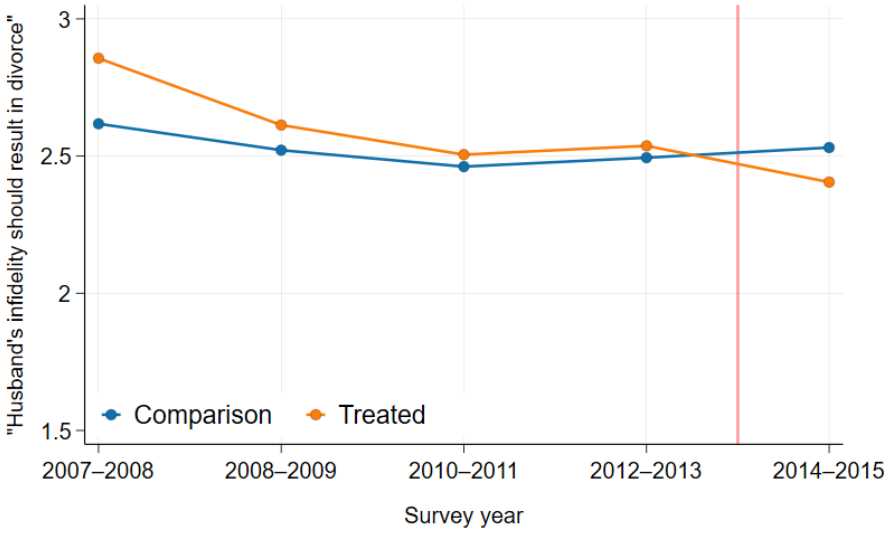
Table 2: Effects of treatment on outcome variable

	Comparison group threshold					
	5 Months (1)	All Prior (2)	4 Months (3)	3 Months (4)	2 Months (5)	1 Month (6)
Panel A. Baseline model						
$\hat{\delta}^{(u)}$: ATT Estimate	–0.23	–0.23	–0.20	–0.19	–0.07	–0.11
(s.e.)	(0.11)	(0.11)	(0.11)	(0.11)	(0.12)	(0.13)
p-value	0.04	0.03	0.08	0.09	0.56	0.39
Panel B. Covariates adjusted						
$\hat{\delta}^{(c)}$: ATT estimate	–0.23	–0.23	–0.20	–0.19	–0.09	–0.14
(s.e.)	(0.11)	(0.11)	(0.10)	(0.11)	(0.10)	(0.11)
p-value	0.03	0.03	0.06	0.07	0.35	0.20
Observations (person-waves)	25,405	30,700	17,625	12,330	6,870	3,650

Notes: This table shows the estimated ATT $\hat{\delta}^{(u)}$ and $\hat{\delta}^{(c)}$ for Models (1) and (2), respectively. Standard errors (s.e.) are provided in parentheses. *p*-values represent the statistical significance of the ATT estimates.

Further support for our findings appears in Figure 3, which displays the time-specific outcome averages for both groups based on the five-month benchmark, providing a visual assessment of our identification assumption. The data broadly suggest no significant trends despite a small initial level difference (estimated at 0.24) between the groups at the start of the first wave (with a standard error of 0.14 and $p = 0.09$). These differences, especially when comparing averages across all pretreatment periods to those posttreatment, do not, in itself, compromise our approach; the parallel trends assumption relies fundamentally on the consistent counterfactual trend slopes rather than initial level differences. By and large, the alignment of trend lines and levels across groups before treatment adds credence to the assumption of parallel trends.

Figure 3: Outcome averages by survey wave and treatment group status



Notes: This figure displays the average outcomes over time for both treatment and comparison groups, defined using the five-month benchmark. Across each time period, variations between the groups remain minimal, with the most significant discrepancy occurring in the first wave, measured at 0.24 ($p = 0.09$).

5. Conclusions and discussion

In many countries, adultery laws have historically shaped norms around marital fidelity. Yet, the recent repeal of these laws in several countries reflects increasing doubts about their relevance and prompts debates in regions where they remain. Despite frequent reliance on empirical arguments, a notable research gap exists regarding their societal impacts. Our study examined the impact of adultery law abolition in Korea on women's perceptions of divorce following spousal infidelity. We found that decriminalizing adultery has made women less likely to consider a husband's infidelity as grounds for divorce, indicating a shift in how infidelity and divorce are linked – at least in the abstract. We consider some potential explanations for this change.

One possible explanation involves rational decision-making (Becker 1993), particularly in reassessing the costs and benefits of divorce following the repeal. The repeal altered the civil dispute landscape, disadvantaging the spouse who did not commit adultery in asset and custody disputes since criminal charges can no longer be leveraged against unfaithful partners. Historical court data show that adultery-related claims pre-

viously resulted in higher alimony payments in Korea, with unfaithful partners paying an average of 14% more in compensation (Oh and Park 2015). Consequently, the repeal may have reduced women's divorce incentives, considering the personal, familial, and financial stakes involved.

Another view is that while their effectiveness in preserving marital stability was debatable, the removal of adultery laws might be seen by women as the loss of a symbolic protective measure for marriage. In many Asian cultures with patriarchal norms, these laws long symbolized a defense of women's rights and marital harmony. Their removal could heighten women's concerns about marital stability, leading them to defend their marriages more actively. The observed shift may also reflect a broader cultural and cognitive recalibration (Cherlin 2020). Previously, infidelity could have been seen as a stronger basis for divorce because of its legal association with criminal behavior, which carried additional social stigma. With the removal of the criminal label, the cognitive link between infidelity and a legitimate reason for divorce may have weakened, suggesting that past perceptions were shaped as much by attitudes toward criminality as by infidelity itself. In this view, this shift represents a societal and attitudinal adaptation to the Constitutional Court's revised legal stance on adultery.

Despite these insights, we acknowledge limitations. Our findings primarily reflect short-term attitudinal changes, which we interpret as potential precursors to more concrete outcomes. However, these early signals may not reliably predict enduring and tangible effects, such as changes in rates of adultery or divorce; further research could verify this. The relatively small size of our treatment group limits our ability to explore heterogeneous impacts across different subpopulations. Furthermore, while we believe that the identified effects are substantive, readers should also be mindful that their overall magnitude is relatively modest.

As more countries reconsider adultery laws, our research provides an empirical basis for assessing the societal impacts of these legislative changes. Reforms in countries like India and Taiwan offer additional contexts for examination, enhancing the relevance of our findings across different cultural and legal environments. We advocate for further research into how these changes affect divorce rates, adultery occurrences, impacts on men, and household dynamics, such as shifts in bargaining power and domestic responsibilities, to deepen our understanding of their societal effects.

6. Acknowledgments

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