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Abortion Legalisation and Adolescent Consequences among Females in the Developing World

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Masaru Nagashima
Waseda Institute for Advanced Study, Waseda University

Waseda Institute for Advanced Study
早稲田大学 高等研究所

1-21-1 Nishi Waseda, Shinjuku-ku, Tokyo 169-0051, Japan
Tel: 03-5286-2460 ; Fax: 03-5286-2470

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Masaru Nagashima[†]

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Abstract

Females in the developing world make major life decisions, such as those regarding marriage and fertility, as early as their teenage years. This study shows that exposure to a liberalised abortion regime significantly affects such choices. The study uses data from low- and middle-income countries covering Africa, Asia, and Latin America and employs a novel approach that combines the regression kink design and difference-in-difference frameworks. The pooled data analysis reveals no strong relationship between longer exposure to extended legal access to abortion and teenage births or pregnancy termination. In contrast, it unveils a robust positive association with educational attainment and a less robust negative link with teenage sexual debut and marriage. These results indicate that females with extended abortion access expect greater returns on education and increase their educational attainment accordingly; at the same time, they delay sexual initiation, which reduces the sexually active population and thus obscures teenage fertility changes. The heterogeneity analysis demonstrates that the findings are broadly consistent across countries. On the other hand, it finds little support for heterogeneity in the birth impact, particularly its increase due to abortion expansion, which can theoretically arise when the reform is exorbitant.

Keywords: Abortion Reform, Teenage Pregnancy, Teenage Marriage, Female Education, Regression Kink Design Difference in Difference, Treatment Effect Heterogeneity.

JEL Classification Code: I12, I15, I18, I25, J12, J13, J18, O15

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[†]Assistant Professor at the Waseda Institute for Advanced Study, Waseda University. Tokyo, Japan. Email: mnagashima@aoni.waseda.jp. Phone: (+81) (0)3 52862120.

1 Introduction

Adolescence is increasingly recognised as a critical period for making life-changing decisions (Cuesta & Leone, 2020). Individual choices have sizeable impacts in developing countries where social safety nets and access to insurance against risks are underdeveloped. The choices made during adolescence play a significant role throughout one's physical development, human capital accumulation, and entry into the labour and marriage markets and can thus have prolonged consequences for one's welfare. The economic literature has shown that various shocks, including those that occur *in utero*, can affect such choices and produce adverse outcomes (Almond & Currie, 2011; Almond, Currie, & Duque, 2018). The design of appropriate public policies for guiding adolescent decisions appears to have attracted less attention in the literature, despite its potential for positive returns (Cuesta & Leone, 2020).

This study demonstrates that legal access to abortion significantly affects the choices of adolescents in low- and middle-income countries. It uses data collected by Demographic and Health Surveys and Multiple Indicator Cluster Surveys from eleven low- and middle-income countries in Africa, Asia, and Latin America. These surveys collect retrospective information on the reproduction, marriage, and education of females aged fifteen to forty-nine years old in a manner that facilitates comparisons across time and space. With a primary focus on adolescent consequences, this study constructs indicators measuring outcomes during teenage years and limits the estimation sample to females aged twenty years or older, thereby circumventing the issue of right censoring.

This study employs an estimation framework based on the regression kink design (RKD) and a difference-in-difference (DID) approach. The framework exploits two sources of plausibly exogenous variation. One is the differential exposure to abortion reforms during the teenage years across age cohorts. Among females who were twenty years or older and surveyed after an abortion reform, some were exposed to the reform in their teens, while others were slightly older than twenty years old when the reform took place and thus effectively constituted the control group. An RKD framework utilises exposure to abortion reform during the teenage years of the two groups of females. The resulting comparison between the younger and older females, however, may include cohort and age effects, the former of which is of more analytical interest, while the latter may arise due to preexisting nonlinearity around the cutoff age. This study thus exploits a second source of exogenous variation, namely, the timing of surveys relative to the abortion reform in each country. Specifically, it estimates the linear RKD model among the females surveyed pre-reform among whom the changes in outcomes, if any, should solely be due to the age effect, as they spent none of their teenage years under the liberalised regime. These two sources of exogenous variation enable the examination of whether abortion reform exposure affects adolescent decisions and which of the two effects makes a more considerable difference in such decisions. Pooling numerous data sets is likely to help credibly identify the causal parameters, as RKD estimation may require a markedly large data set (*e.g.*, Ando, 2016).

The model by Ananat, Gruber, Levine, and Staiger (2006, 2009) provides a theoretical framework for interpreting the empirical results in this study. It shows that a reduction in the cost of an abortion may not affect either pregnancy or abortion if the initial cost is high and the reduction is small. A further reduction can cause an unambiguous increase in pregnancies since it reduces the expected cost of pregnancy before a female becomes pregnant; on the other hand, it may increase abortion among those who do become pregnant. As a result, an abortion cost reduction may not necessarily decrease births and may even increase them depending upon the initial cost and the magnitude of its reduction. These theoretical predictions motivate a heterogeneity analysis that exploits the difference in initial restrictions and reform magnitudes across countries. By relating the estimated birth effects to the pre- and post-reform restrictions, this study examines whether the effects are estimated to be small in countries with modest reforms and whether drastic reform can result in a large and positive birth effect in some countries but potentially a negative effect in others.

The results of this analysis are as follows. The pooled data analysis finds little association between longer exposure to extended legal access to abortion and teenage fertility on both the intensive and extensive margins. Likewise, it does not find a robust association with pregnancy termination.[1] These findings seem inconsistent with past theoretical predictions (Ananat et al., 2006, 2009) and available empirical results (*e.g.*, Levine & Staiger, 2004; Valente, 2014). However, the effect estimates for other outcomes provide a potential interpretation. Specifically, the present anslysis finds a robust increase in education and a less robust decrease in teenage sexual initiation and teenage marriage. Coupled with the evidence from past studies on the negative associations of education with sexual activity and marriage (*e.g.*, Field & Ambrus, 2008; Masuda & Yamauchi, 2018; McCrary & Royer, 2011), the findings in this study suggest that extended abortion access may have reduced the sexually active teen population through an increase in education. Furthermore, the share of teenage females who become pregnant and those who undergo pregnancy termination may have increased *within* the reduced subpopulation of sexually active teenage females. That is, when the decline in the sexually active subpopulation outweighs the birth and pregnancy termination effects, the effect estimates in the whole female population—including both sexually active and inactive ones—can be small and insignificant, as found herein.

This study then conducts a heterogeneity analysis by estimating the RKD-DID model for each country in the pooled data. First, it finds that the estimated effects are consistent across all the included countries despite differences in the degree of reforms and other socioeconomic conditions. For example, countries with a decline in the share of teenage females giving birth (*i.e.*, the extensive margin) are likely to have a decline in the total number of teenage births as well (*i.e.*, the intensive margin). Similarly, countries with an increase in education are likely to have a decrease in teenage intercourse, pregnancy, and marriage. Second, this study finds no significant positive birth effect of abortion cost reduction in countries with a relatively drastic reform. In the analysis, Nepal went through the most remarkable change in legal access to abortion, from an outright ban to ultimate liberalisation. However, the country's estimated effects are negative for teenage birth, sexual initiation, and marriage and positive for education, consistent with the pooled results and those of Valente (2014). Note that these findings do not invalidate the theory discussed above: the birth effect of abortion legalisation can always be negative, but it *can* also be positive if the reform is exorbitant. In fact, this analysis can be considered a check of external validity using the same estimation model, set of covariates, and data sources.

This study contributes to three large bodies of literature. First, it adds new evidence on the impact of legal reforms related to access to birth control methods in developing countries. Past studies have investigated the linkage of safe medical abortion with not only sexual and reproductive decisions but also diverse outcomes of females and their

[1] The measurement of this variable includes not only abortions but also miscarriages and stillbirths.

children (Angrist & Evans, 1999; Gruber, Levine, & Staiger, 1999; Klick & Stratmann, 2003; Levine, Staiger, Kane, & Zimmerman, 1999; Myers, 2017). Common to many of the past studies and to this present study is the use of quasi- and natural experimental settings. For example, the state-level variation in the timing of changes in legal access to abortion services in the US has been extensively exploited, while more granular information has been used in some studies (Joyce, Tan, & Zhang, 2013; Lindo, Myers, Schlosser, & Cunningham, 2020). Another setting frequently analysed is Eastern Europe, where severe restrictions were put in place under the Soviet Union and removed in the late 1980s to early 1990s (*e.g.*, Hjalmarsson, Mitrut, & Pop-Eleches, 2019; Levine & Staiger, 2004; Pop-Eleches, 2006, 2010). Compared to these settings, rigorous evidence from the developing world is relatively scarce, with a few exceptions (*e.g.*, Clarke and Mühlrad 2021 in Mexico, Antón, Ferre, and Triunfo 2018 in Uruguay, Lin, Liu, and Qian 2014 in Taiwan, and Valente 2014 in Nepal). This study makes a unique contribution to this area of the literature by adding evidence from a set of low- and middle-income countries, where the evidence is limited yet the birth rate is high; thus, a more comprehensive understanding of reproductive behaviours is likely still necessary.

Second, this study provides empirical evidence on the determinants of teenage outcomes. Recent studies have shown the multifaceted impacts of teenage pregnancy on various aspects of the life of females, such as education, labour market performance, and marriage (e.g., Buckles, 2008; Daniel, Lacuesta, & Rodríguez-Planas, 2013; Mølland, 2016). The association is also reported in low- and middle-income countries (Ardington, Menendez, & Mutevedzi, 2015; Heath & Mushfiq Mobarak, 2015; Herrera-Almanza & Sahn, 2018; Herrera-Almanza, Sahn, & Villa, 2019). While the consequences are repeatedly analysed, the causes of teenage choices are relatively less well understood. This study attempts to fill this gap with an emphasis on a policy instrument that can impact various aspects of teenage life, particularly that of females.

Third, the current study addresses the difficulty of comparing estimates from different locations and points in time inherent in natural and quasi-experimental research. Heterogeneity in results from different studies can stem from a variety of factors, including differences in socioeconomic or cultural backgrounds across analytical settings or different identification strategies across studies, which can obfuscate the factors that lead to result heterogeneity. This issue may be particularly salient in analysing policies related to access to induced abortion, since the leniency of restrictions pre- and post-reform can be very different across countries. A few studies have employed a unified research design to examine the impact for an international data set (*e.g.*, Bloom, Canning, Fink, & Finlay, 2009; Klick, Neelsen, & Stratmann, 2012), but they do not purposefully analyse the potential heterogeneity in relation to theoretical predictions. This study uses internationally comparable data and the same identification strategy for countries from Africa, Asia, and Latin America and analyses the heterogeneity in the results across different countries.[2] By doing so, this study also addresses the concern known as external validity.

The rest of the paper is structured as follows. Section 2 introduces the data sources and major variables. Section 3 summarises the selection criteria of countries and their abortion reforms. Section 4 describes the identification strategy. Section 5 discusses the results and their interpretations. Section 6 concludes the paper.

[2] In some cases where more than one reform takes place in a country, the impact can also vary across years even within the same country since the second and subsequent reforms have a different state of origin. Although this within-country heterogeneity can be of interest, it is not analysed explicitly in this study, which limits the focus on the first reform in each country. See Section 3 for more discussion.

2 Data and Variables

This study combines data sets from two international surveys. One is the Demographic and Health Survey (DHS) conducted by the United States Agency for International Development, and the other is the Multiple Indicator Cluster Survey (MICS) conducted by the United Nations Children's Fund. The surveys were conducted in a number of low- and middle-income countries: The DHS covers 92 countries, and the MICS covers 118 countries as of June 2021. Both surveys are based on interviews with females aged 15 to 49 years.

This study pools the data from countries that were selected based on the following criteria:

1. At least one statutory change has been made in legitimate access to induced abortion;

2. At least one survey was conducted before and after the statutory change in the country; and

3. There are nonzero females who are '*younger*' and '*older than the cutoff*' (defined in Section 4) in both the pre- and post-reform surveys.

These selection criteria leave eleven countries for analysis: Benin, Burkina Faso, Chad, Colombia, Guinea, Indonesia, Lesotho, Mali, Mozambique, Nepal, and Togo. Table 1 lists the survey years and the number of observations for the pre- and post-reform periods in each country. It shows that the combined data set covers an extended time, a large number of observations in each country, and a comprehensive geographical area—Africa, Asia, and Latin America.

The DHS and MICS collect information on current socioeconomic characteristics and the history of female pregnancy. The survey questionnaires are designed to be internationally comparable, but are not necessarily comparable with each other. In the analysis below, therefore, this study constructs consistent and comparable variables based on the information collected by the surveys.

The primary outcome indicators of this study include the presence and number of teenage births; teenage pregnancy termination due to miscarriage, stillbirth, or induced abortion; teenage marriage; teenage sexual initiation; and years of education.[3] Teenage outcomes are constructed using the survey responses about the timings of the birth of the interviewed females and their respective life events. Information on the timing of births, pregnancy termination, and marriage is available at monthly intervals for most waves. In contrast, the information regarding the timing of sexual initiation is only available at yearly intervals. However, missing values are prevalent in older surveys, particularly monthly information. In the analysis below, this study mainly uses the year information, which has fewer missing values, and backs this up with a robustness check using the monthly information, which produces an essentially unchanged set of results.

Table 2 presents the summary statistics of the major variables by the survey timing (pre- and post-reform in each country) and age at the time of the interview (older or younger than the cutoff at each survey wave) for females aged 20 years or older within a 10-year bandwidth of the cutoff. The mean values of outcome indicators such as the

[3] One way to measure pregnancy is to take the sum of live births and terminated births. However, this study does not do so, as it may introduce nonclassical measurement errors due to reporting behaviour, which may be a function of abortion reform.

Table 1: Sources of Analysis Data

	Year			No. Obs.	
	Reform	Pre-Reform Surveys	Post-Reform Surveys	Pre	Post
Benin	2003 Jan	1996^{d}, 2001^{d}	2006^{d}, $2011\text{-}2012^{d}$, 2014^{m}, $2017\text{-}2018^{d}$	11710	66669
Burkina Faso	1996 Nov	$1992\text{-}1993^{d}$	$1998\text{-}1999^{d}$, 2003^{d}, 2006^{m}, 2010^{d}, 2014^{d}, $2017\text{-}2018^{d}$	6354	59907
Chad	2002 Mar	$1996\text{-}1997^{d}$, 2000^{m}	2004^{d}, 2010^{m}, $2014\text{-}2015^{d}$	13359	41892
Colombia	2006 May	1986^{d}, 1990^{d}, 1995^{d}, 2000^{d}, $2004\text{-}2005^{d}$	$2009\text{-}2010^{d}$, $2015\text{-}2016^{d}$	78042	92239
Guinea	2000 Jul	1999^{d}	2005^{d}, 2012^{d}, 2016^{m}, 2018^{d}	6753	38215
Indonesia	2009 Sep	1987^{d}, 1991^{d}, 1994^{d}, 1997^{d}, $2002\text{-}2003^{d}$, 2007^{d}	2011^{m}, 2012^{d}, 2017^{d}	154149	101104
Lesotho	2012 Mar	2000^{m}, $2004\text{-}2005^{d}$, $2009\text{-}2010^{d}$	2014^{d}, 2018^{m}	21431	15145
Mali	2002 Jun	1987^{d}, $1995\text{-}1996^{d}$, 2001^{d}	2006^{d}, $2009\text{-}2010^{m}$, $2012\text{-}2013^{d}$, 2015^{d}, 2015^{m}, 2018^{d}	25753	91568
Mozambique	2014 Dec	1997^{d}, $2003\text{-}2004^{d}$, 2009^{d}, 2011^{d}	2015^{d}, 2018^{d}	46154	13933
Nepal	2002 Sep	1996^{d}, 2001^{d}	2006^{d}, 2011^{d}, $2016\text{-}2017^{d}$	17155	36329
Togo	2007 Jan	1988^{d}, 1998^{d}, 2000^{m}, 2006^{m}	2010^{m}, $2013\text{-}2014^{d}$, 2017^{d}, 2017^{m}	22813	28827

Notes: This table shows the list of countries, the years of their first abortion reforms in the Gregorian calendar, the years of surveys, and the number of observations before and after the first reforms. The superscripts ^{d} and ^{m} indicate that the data sources are the DHS and MICS, respectively.

number of pregnancies and terminated pregnancies are likely subject to right censoring since older females are more likely to have ever experienced these life events. By contrast, teenage indicators are not prone to such measurement issues and thus are likely to be comparable across the groups of females. However, the years of education are left unmodified, assuming that the schooling outcome is completed by age twenty among most females in these countries.

Panel A shows the summary statistics of the life event indicators that were ever experienced by the time of the survey response and are thus prone to right censoring. By comparing columns (2) and (5) or (8) and (11), one can see that older females are more likely to have ever given birth, ever terminated a pregnancy, and been married at least once and have given birth more times on average. These patterns are likely partly due to right censoring, which disproportionately affects younger females. Panel B shows the summary for teenage indicators, constructed in a way free from right censoring for females in their twenties or older. One can find that the mean values are largely comparable across the four groups of females. Exceptions include teenage sexual initiation, which can be considered a prerequisite for pregnancy outcomes, and years of education, which is likely to be completed by the age of twenty.

Panel B of Table 2 also suggests possible time effects. For instance, by comparing columns (2) and (8), as well as columns (5) and (11), one can see that the share of females with teenage births decreased for both the older and younger females between the pre- and post-reform surveys, as did the number of teenage births. Although the trend is less clear-cut for other indicators, this finding suggests the importance of appropriately controlling for the possible general time trend.

3 Abortion Reforms in Selected Countries

This section briefly summarises reforms to legal access to induced abortion in each of the eleven countries examined in this study, which are analysed in subsequent sections. The Center for Reproductive Rights (2019a, 2019b) provides a comprehensive summary of the extent to which abortion access is legally permitted in two dimensions. One is an ordinal scale from 1 to 5 representing the following:

1. Abortion is prohibited altogether;
2. Abortion is permitted to save the woman's life;
3. Abortion is permitted to preserve the woman's health;
4. Abortion is permitted on broad social and economic grounds; and
5. Abortion is permitted upon request.

The other scale is categorical and reflective of additional grounds on which abortion is permitted:

R. Abortion is permitted if the pregnancy results from rape;

I. Abortion is permitted if the pregnancy results from incest; and

F. Abortion is permitted if fetal impairment is detected.[4]

Table 2: Descriptive Statistics of Outcome Measures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Pre-Reform						Post-Reform					
	Age > Cutoff			Age ≤ Cutoff			Age > Cutoff			Age ≤ Cutoff		
	N	mean	sd	N	mean	sd	N	mean	sd	N	mean	sd
Panel A. Plain Variables.												
Age	49305	27.56	3.949	44028	24.31	2.539	80584	30.73	4.587	96976	26.39	4.394
Age at Reform	49305	33.08	1.423	44028	27.73	1.669	80584	22.97	1.392	96976	17.67	1.636
Ever Given Birth	49305	0.854	0.352	44028	0.781	0.411	80584	0.894	0.305	96976	0.783	0.409
Total Births	48419	2.298	1.791	43338	1.652	1.373	77231	3.063	2.318	92145	2.279	2.062
Age at 1st Birth	39823	19.93	3.480	33050	19.30	2.862	64387	20.38	3.775	66928	19.46	3.265
Ever Terminated Preg.	39974	0.146	0.361	35448	0.126	0.338	58482	0.160	0.371	68234	0.116	0.329
Age at Termination	5149	23.51	4.595	4058	21.29	3.534	9359	24.53	5.369	7894	22.36	4.592
Ever Married	49304	0.904	0.293	44026	0.843	0.362	76447	0.916	0.272	90660	0.815	0.386
Age at 1st Marriage	43028	18.75	3.780	36522	18.23	3.163	65688	19.20	4.060	68633	18.37	3.510
Ever Had Intercourse	46365	0.978	0.148	42235	0.963	0.186	74270	0.971	0.177	87747	0.918	0.282
Age at 1st Intercourse	45332	17.66	3.410	40742	17.20	2.864	70281	17.66	3.386	78263	17.09	2.881
Years of Education	49274	4.687	4.269	43996	4.911	4.543	80526	4.479	5.176	96883	4.795	5.286
Panel B. Constructed Outcome Indicators.												
Teenage Birth	45530	0.428	0.497	41103	0.438	0.500	72566	0.394	0.490	86925	0.415	0.493
No. Teenage Births	46609	0.584	0.830	42256	0.581	0.812	72743	0.529	0.820	87067	0.564	0.830
Teenage Termination	37228	0.0284	0.166	32443	0.0367	0.189	58426	0.0284	0.166	68191	0.0324	0.178
Teenage Marriage	47040	0.575	0.498	42994	0.571	0.497	71856	0.526	0.499	85214	0.531	0.498
Teenage Intercourse	46360	0.734	0.449	42235	0.774	0.421	72674	0.728	0.445	85909	0.751	0.432

Source: Demographic and Health Surveys and Multiple Indicators Cluster Surveys in selected countries. *Notes*: This table shows the descriptive statistics of outcome measures used in this study for females aged 20 years or older. For the definition of the cutoff, see Section 4.1. Panel A presents the statistics uncorrected for right censoring, while Panel B presents teenage indicators constructed from the survey information. Observations are weighted by the weights provided in the survey data and adjusted so that the sum equals across countries.

Using these scales, a country with '2RI' abortion access, for example, permits no abortion unless it is necessary to save the life of the pregnant woman or the pregnancy results from rape or incest. This study follows the above scale measurement to describe legal access to induced abortion. The text below introduces the first abortion reform in each country after 1994, when the ICPD Plan of Action was signed.

Benin moved from level 2 to level 3RIF in March 2003. Benin had long restricted legal access to abortion since 1877, so females had legal access to it only when their pregnancy was life-threatening. The country's Penal Code, amended on the 3rd of March 2003, explicitly grants legal abortion access if the pregnancy endangers the life and health of the pregnant female, if it results from rape or incest, or if the unborn child is diagnosed with certain severe conditions (Center for Reproductive Rights, n.d.).

Burkina Faso moved from level 1 to level 3RIF in November 1996. Burkina Faso passed the amendment of the Penal Code on the 13th of November 1996 (Burkina Faso, 1997). Before the reform, abortion was prohibited altogether (Center for Reproductive Rights, 2014). Since the reform, it is explicitly permitted if two doctors certify that the pregnancy endangers the health of the pregnant female or if the fetus suffers from a severe illness that is considered incurable. The amendment also added explicit exceptions such that the pregnant woman can ask for an abortion of a pregnancy resulting from rape or incest within the first ten gestational weeks.

Chad moved from level 2 to level 3F in March 2002. Chad restricted legal access to abortion only to pregnancies thought to be life-threatening to pregnant female. It adopted the Law on the promotion of reproductive health on the 20th of March 2002. Article 14 states that therapeutic termination of pregnancy can be authorised when the continuation of the pregnancy endangers the life or health of the mother and when the child in utero is diagnosed with a serious condition (Déby, 2002). Though this information is not utilized in the data analysis below, the country further extended legal access on the 8th of May 2017 to cases where pregnancies result from rape or incest (Droit Afrique, 2017).

Colombia moved from level 1 to level 3RIF in May 2006. Colombia had long imposed an outright ban on abortion, which was changed by the court ruling on the 10th of May 2006 (Corte Constitucional de Colombia, 2006; Women's Link Worldwide, 2007). The court decision, one of the most influential abortion-related rulings in the world, declared that the ban was unconstitutional and explicitly granted legal access when the female's health is endangered, in cases of rape or incest, or when severe fetal impairment is detected (Boland & Katzive, 2008).

Guinea moved from level 3 to level 3RIF in July 2000. Guinea banned abortion access except to save the life of the pregnant female or to protect her health (Boland & Katzive, 2008) until the 10th of July 2000, when it expanded legal access in cases of rape or incest or when the fetus was found to have a severe condition (République de Guinée, 2000).

[4] Some countries grant permission for abortion in a few cases other than rape, incest, and fetal impairment. However, this study does not consider these cases since they are not found in the eleven countries analysed.

Indonesia moved from level 2 to level 2RF in October 2009. Abortion had been illegal in Indonesia, with the only exception being when the life of the pregnant female was at risk (Center for Reproductive Rights, 2014). Law No. 36, adopted on the 13th of October 2009, extended legal access to cases where the life of the fetus is threatened or where the pregnancy results from rape (Article 75, The President of the Republic of Indonesia, 2009).

Lesotho moved from level 2 to level 3RIF in March 2012. Lesotho had banned abortion unless performed to save the life of the pregnant female (United Nations, 2011) until the 9th of March 2012, when it legalised access to abortion intended to prevent harm to the health of the pregnant female, to prevent the birth of a child with handicap, or to terminate pregnancies resulting from incest or rape (Government of Lesotho, 2012).

Mali moved from level 2 to level 2RI in June 2002. In Mali, abortion was inaccessible unless performed to save the life of the pregnant female (Boland & Katzive, 2008). The restriction was lifted on the 24th of June 2002 for pregnancies resulting from sexual assault or incestuous acts (Présidence de la République du Mali, 2002).

Mozambique moved from level 3 to level 5 in December 2014. Abortion was legally accessible in Mozambique only when the continuation of the pregnancy endangered the life or health of the pregnant female until the Penal Code act was signed into law on the 18th of December 2014, when it became legal on any grounds (República De Moçambique, 2014). This signing was likely a surprise at the time: the bill was returned by the President to the Parliament in November due to the potential inadequate protection of the rights of women and girls (e.g., Equity Now, 2014).

Nepal moved from level 1 to level 5 in March 2002. Nepal went through the most significant change in abortion regulations, from an outright ban to full liberalisation. After several years of debate, a bill liberalising access to abortion was finally passed by the House of Representatives and signed by the king on the 27th of September 2002 (Thapa, 2004). The reform helped significantly reduce maternal mortality (Guttmacher Institute, 2017; Valente, 2014).

Togo moved from level 2 to level 3RIF in January 2007. Togo had a stringent restriction on abortion access, which was only allowed to save the life of the pregnant female. It was extended on the 10th of January 2007 to cases where abortion is deemed necessary to protect the pregnant female's health, where the pregnancy is the result of rape or incest, or when the unborn child is diagnosed with a severe condition (Republique Togolaise, 2007a).

4 Estimation

4.1 Regression Specification

This study attempts to estimate the impact of abortion reforms on the teenage outcomes of young females, focusing on aspects related to fertility, marriage, and education. It has to address the issue of right censoring since some of the outcome indicators are likely to still be unobserved, and disproportionately so for younger females. This study restricts the estimation sample to females aged 20 years or older and constructs teenage indicators since the teenage outcomes of these individuals have already been completed, and right censoring should thus not affect the data analysis. In addition, for teenage indicators to be affected, the sample females should have been exposed to abortion reform during their teenage years. Therefore, this study relates adolescent outcomes to abortion reforms during the teenage years and estimates whether more years of exposure induce a larger change in the outcomes.

The regression equation is specified as follows:

$$\begin{aligned} y_{ijt} = & \alpha_0 + \alpha_1(c_{jt} - a_{it}) + \alpha_2 \boldsymbol{I}\{c_{jt} \geq a_{it}\}(c_{jt} - a_{it}) \\ & + \boldsymbol{I}\{t \geq r_j\}[\beta_0 + \beta_1(c_{jt} - a_{it}) + \beta_2 \boldsymbol{I}\{c_{jt} \geq a_{it}\}(c_{jt} - a_{it})] \\ & + f_j(t - a_{it}) + \phi_{tj} + u_{ijt} \end{aligned} \tag{1}$$

where c_{jt} denotes the cutoff age of country j conducted in survey year t defined below, a_{it} denotes the age of female i in survey year t, and r_j denotes the year of abortion reform in country j. $\boldsymbol{I}\{\cdot\}$ represents an indicator function that equals unity if the condition in the brackets holds and zero otherwise, f_j is a country-specific polynomial function of the year of birth of female i, written as $t - a_{it}$, and ϕ_{jt} the survey fixed effects in country j in year t. This equation contains two kinked linear functions of the age of females relative to the cutoff, which splits them into 'younger' and 'older' groups. The one for the pre-reform period is characterised by α's, and the other for the post-reform period is characterised by β's. If this equation is estimated using only the post-reform data and omitting terms involving αs, model (1) reduces to a parametric analogue of the regression kink design (RKD, Card, Lee, Pei, and Weber 2015). The use of the pre-reform data and the inclusion of a separate kinked linear function follow the spirit of difference-in-difference (DID) estimation and allow to exploit the additional variation created by the survey timing and control for any effects spuriously correlated with age relative to the cutoff. This study estimates the RKD-DID model in Equation (1) for females with their relative age, $c_{jt} - a_{it}$, within a bandwidth of 5 years, in the main results. It then conducts a robustness check using the three-year or ten-year bandwidth to find that the results are insensitive to the choice of the bandwidth.[5]

The cutoff age in country j in year t, c_{jt}, is defined as

$$c_{jt} \equiv 20 + t - r_j + s \times \boldsymbol{I}\{t \leq r_j\} \tag{2}$$

Figure 1 is prepared to visually aid the understanding of Equation (2). In post-reform surveys (e.g., those with $t = t_1$ and t_2), $20 + t - r_j$ is the age of those females in survey year t who turn twenty years old in year r_j, country j's abortion reform year. In pre-reform surveys (e.g., those with $t = t_3$ and t_4), $20 + r_j - t$ becomes smaller than 20. In this case, a non-negative value $s > 0$ is added so that the sample of females older than 20 years in the pre-reform surveys includes a decent number of females below and above the cutoff. The cutoff age thus defined varies across countries and survey waves. The choice of s should not bias the estimation since it is constant for all countries and thus uncorrelated with the timing of abortion reforms in any country. This study sets $s = 10$ to obtain the main results and conducts a robustness check with $s = 20$, barely affecting the conclusions.

[5]A bandwidth equal to or less than half of the period between pre-reform and post-reform cutoffs ensures that no birth cohorts are categorised in more than one of the following groups of females: pre-reform older, pre-reform younger, post-reform older, or post-reform younger.

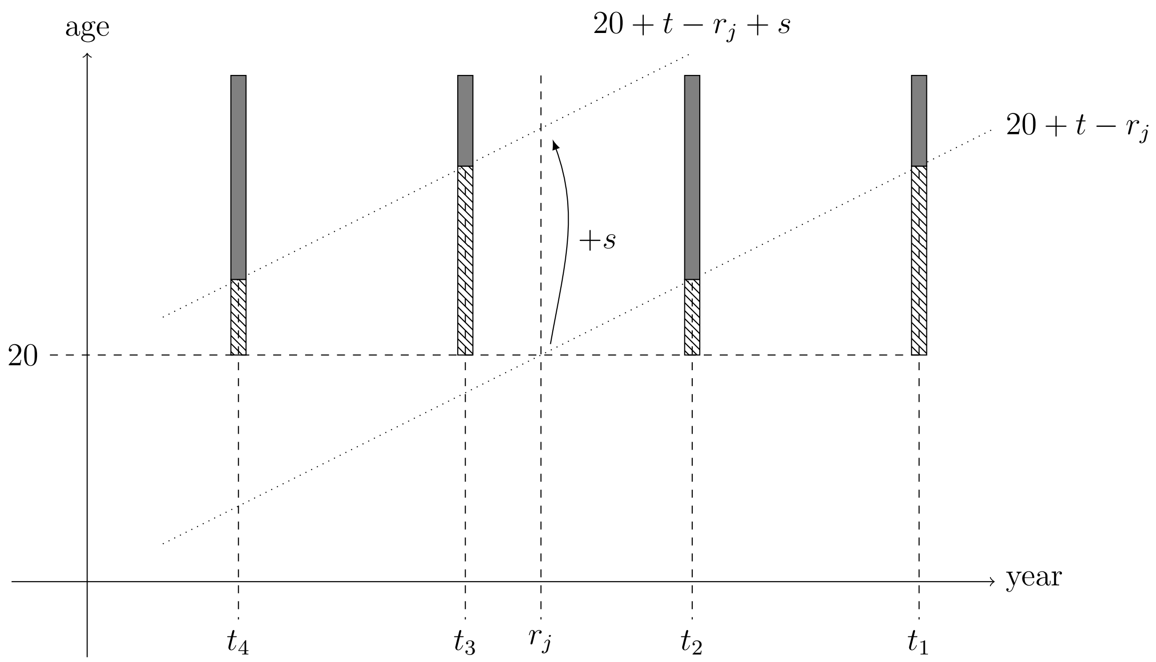


Figure 1: Illustration of the Cutoff and the Groups of Females

The females whose ages are below the cutoff (*i.e.* , ages of individuals in the shaded areas in Figure 1) are referred to as those *younger than the cutoff* or simply *younger* females. Similarly, those whose ages are above the cutoff (*i.e.* , ages of painted areas) are referred to as those *older than the cutoff* or simply *older* females. The females aged 20 years or older in the data thus fall into one of the four groups: pre-reform older, pre-reform younger, post-reform older, or post-reform younger. Out of the four groups, only the post-reform younger females were exposed to the liberalised abortion regime during their teenage years; thus, their duration of exposure is longer the younger they are, whereas females in other groups had zero exposure during their teenage years. The post-reform older females are comparable to the post-reform younger females in the sense that they were exposed to the same socio-economic environment as the younger females. Nevertheless, they differ in the sense that the older females were marginally unexposed to the liberalised abortion regime *during teenage*. Females surveyed pre-reform were never exposed to the reform, but using their data helps control for the age effects, if any, which may create a spurious kinked relationship around the cutoff.

4.2 Parameters of Interest

On estimating the model in Equation (1), this study computes the first-order derivative of the age relative to the cutoff for the three groups of females:

$$\delta|_{pre,young} \equiv \alpha_1 + \alpha_2 \tag{3a}$$

$$\delta|_{post,old} \equiv \alpha_1 + \beta_1 \tag{3b}$$

$$\delta|_{post,young} \equiv \alpha_1 + \alpha_2 + \beta_1 + \beta_2 \tag{3c}$$

where the subscripts $_{pre}$ and $_{post}$ indicate the pre-reform and post-reform observations, and $_{old}$ and $_{young}$ indicate the older- or younger-than-cutoff observations, respectively.

Using the derivative estimators in Equations (3a) to (3c), this study then calculates the differences in derivatives:

$$\Delta^{post}|_{young} = \delta|_{post,young} - \delta|_{pre,young} = \beta_1 + \beta_2 \tag{4a}$$

$$\Delta^{young}|_{post} = \delta|_{post,young} - \delta|_{post,old} \quad = \alpha_2 + \beta_2 \tag{4b}$$

$\Delta^{post}|_{young}$ in Equation (4a) measures the difference in the derivatives between the post-reform females and pre-reform females, who are both younger than the cutoff. This parameter captures the net effect of exposure duration based on the variation in birth cohorts after taking account of differences in ages between younger and older females (*i.e.*, age effects). The parameter is, however, estimated by comparing slightly more distant birth cohorts. On the other hand, $\Delta^{young}|_{post}$ in Equation (4b) provides the change in the derivative for the post-reform younger females compared to that for post-reform older females. It thus identifies the net effect of exposure duration based on the variation in ages after taking account of differences in birth cohorts between the females surveyed pre- and post-reform (*i.e.*, cohort effects). It may, however, include the age effect that may be pre-existing due to an underlying nonlinear trend. This study presents estimates for both of these parameters.

5 Results

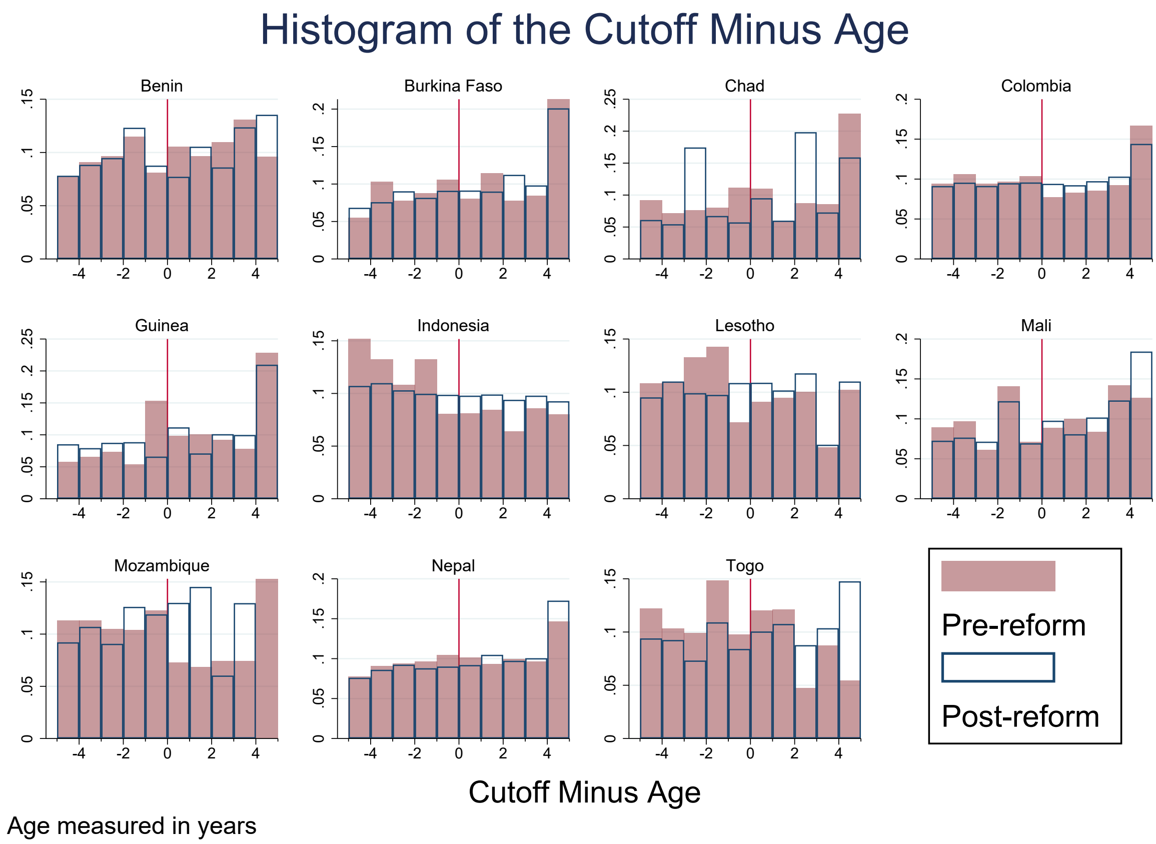
5.1 Identification Assumptions

Card et al. (2015) summarises the testable implications for RKD estimation. The methodology taken in this present study can be considered an extension of the sharp design. The two main testable implications are the smoothness of the density of the running variable and that of predetermined covariates. Although it is not easy to gather information and construct consistent measures of predetermined covariates, this study can examine the density of the running variable.

Figure 2 shows the density of age relative to the cutoff for each country. The histogram bins fail to reveal a systematic heaping or dipping around the cutoff for many countries. In countries where female education is particularly low—such as Chad and Guinea—there appear to be heaps every two, five, or ten years. This is likely due to the rounding when respondents have inaccurate information about their exact year of birth (see, for instance, Cappelli & Baten, 2021). The histogram bins appear smooth in countries such as Colombia and Nepal, where female education is relatively higher. This finding supports the interpretation that the fluctuations are indeed unrelated to exposure to abortion reforms in respective countries. The graphical analysis thus seems to suggest that either the density of the cutoff minus age is smooth in countries with greater average education or that heapings are unrelated to reform exposure.

More formal statistical tests are also employed based on methods proposed by Cattaneo, Jansson, and Ma (2018, CJM below) and Frandsen (2017). Table 3 presents the p-values from the two tests, where columns (1) through (3) show the results from the CJM test,[6] and columns (4) through (6) show those from Frandsen's test. The first row shows the results when pooling all countries, while the rest shows the results separately for each country. The table shows that the null hypothesis of the absence of systematic sorting is rejected more frequently than would be expected by random chance. However,

[6]The test is adjusted to the presence of mass points of the year measurement.



Source: Demographic and Health Surveys and Multiple Indicators Cluster Surveys. *Notes*: This figure shows the histogram of the running variable within the five-year bandwidth by each of the countries used in the main analysis. The running variable is the cutoff year minus age, where the cutoff is the year of birth of cohorts that turn twenty years old when the first abortion reform took place in their countries.

Figure 2: Histogram of the Cutoff minus Age in Years

it is evident in the table that the null is more frequently rejected in the *pre-reform* data and in countries with a relatively lower level of development. This pattern seems contradictory to a systematic sorting—which is likely to arise in the *post-reform* data—but indicative of the measurement error of the year of birth since it is likely more rampant in poorer countries and older surveys, even in relatively richer countries. Density sorting around the cutoff is unlikely to emerge in the present analysis since the cutoff is defined only to divide females by whether they were twenty years or older when abortion reforms took place; that is, the older females surveyed post-reform were not denied legal access to abortion services after the age of twenty, and none of the females surveyed pre-reform spent time under the liberalised regime on either side of the cutoff. Therefore, although the results may not be completely clear, a systematic sorting of the running variable related to abortion liberalisation is unlikely to be present.

Table 3: p-Values from Two Density Tests of the Cutoff Minus Age in Years.

	(1)	(2)	(3)	(4)	(5)	(6)
	CJM (2018)			Frandsen (2017)		
	Pre+Post	Pre	Post	Pre+Post	Pre	Post
All Countries	0.000	0.000	0.000	0.000	0.114	0.000
Benin	0.000	0.000	0.000	0.000	0.004	0.000
Burkina Faso	0.004	0.903	0.002	0.264	0.000	0.750
Chad	0.000	0.000	0.000	0.000	0.000	0.000
Colombia	0.733	0.506	0.326	0.028	0.000	0.989
Guinea	0.492	0.000	0.000	0.000	0.001	0.000
Indonesia	0.000	0.000	0.839	0.458	0.512	0.687
Lesotho	0.004	0.000	0.216	0.065	0.053	0.521
Mali	0.000	0.000	0.000	0.000	0.398	0.000
Mozambique	0.026	0.000	0.001	0.000	0.000	0.214
Nepal	0.347	0.148	0.952	0.317	0.663	0.115
Togo	0.000	0.000	0.000	0.000	0.000	0.217

Source: Demographic and Health Sruveys and Multiple Indicators Cluster Surveys.
Notes: This table reports the p-values from the density tests proposed by Cattaneo et al. (2018) and Frandsen (2017) for the cutoff minus age.

Given the absence of strong evidence that invalidates a causal interpretation, section 5.3 discusses the estimation results with the pooled data of the eleven countries, and section 5.4 presents the analysis that allows the results to be heterogeneous across countries.

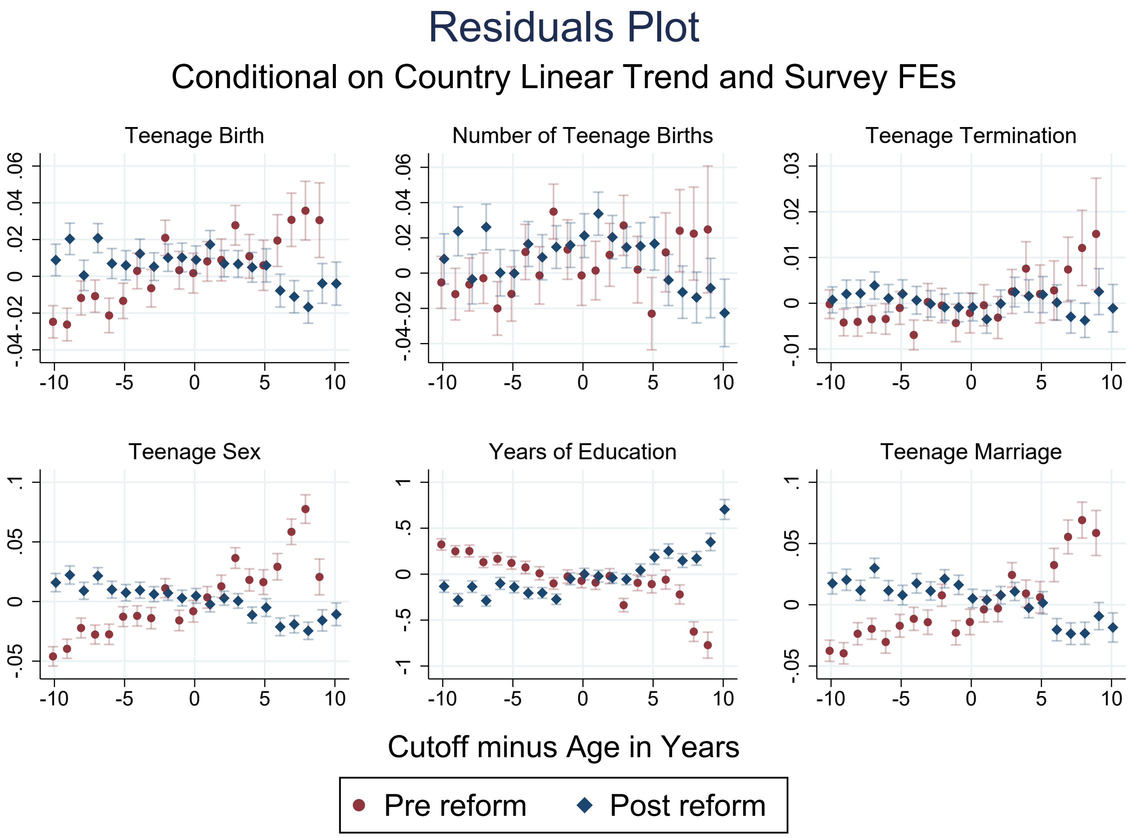
5.2 Functional Form Assumptions

Before moving on to the main results, it is worthwhile to discuss the functional form assumptions in Equation (1). This equation assumes that the impact of exposure to the liberalised abortion regime is linear conditional on country-specific trends of the year of birth and survey fixed effects. Additionally, the order of the country-specific birthyear polynomial functions needs to be decided prior to the main analysis. For these reasons, this study estimates the following equation:

$$y_{ijt} = f_j(t - a_{it}) + \phi_{jt} + u_{ijt}. \tag{5}$$

This study then obtains residuals, $\hat{u}_{ijt}$, and plots them on the cutoff minus age separately for the pre- and post-reform data to examine whether the linearity assumptions are reasonable for the impact of treatment exposure and the country-specific trends.

Figure 3 shows the plots for the six outcome indicators. Residuals from teenage birth outcomes conditional on country-specific polynomial functions of the year of birth and survey fixed effects fail to present a strong kinked relationship below and above the cutoff. However, residuals from other indicators show a slope change below and above the cutoff. For instance, residualised teenage termination shows an increasing trend only for females younger than the cutoff (*i.e.* cutoff minus age greater than zero) but no such trend either for the younger or older females in the post-reform data. Likewise, residualised teenage marriage and teenage sexual initiation show a stable, slightly increasing trend for the pre-reform older females, which intensifies for the pre-reform younger females, but there seems no such trend for females surveyed post-reform. Residualised years of education data show a similar pattern but in reverse: an increasingly upward trend exists in the post-reform data but does not exist in the pre-reform data. These residual plots suggest that a kinked linear function is likely to provide a good fit for the remaining variation in the outcome indicators, which supports the parametric RKD-DID specification assumption in Equation (1).



Source: Demographic and Health Surveys and Multiple Indicators Cluster Surveys. *Notes*: This figure shows the mean residuals at each value of the cutoff minus age separately for pre-reform and post-reform survey data for six outcome indicators. The markers indicate the mean residual value at the cutoff minus age, while the capped bars indicate the 95% confidence intervals. The red circle markers indicate the residuals for females surveyed pre-reform, while the navy diamonds indicate those for females surveyed post-reform. Observations are weighted by the weights provided in the survey data.

Figure 3: Residuals Plot on the Cutoff Minus Age.

On the other hand, the country-specific trends may be nonlinear in the year of birth.

Hence, the linearity assumption for the f_j function may not provide an adequately smooth approximation. To see this, different polynomial orders are examined. As one such example, Appendix Figure A.1 shows the residual plot using a quartic birth-year trend function for each country. The graphical results are strikingly similar to those in Figure 3. From these results, this study uses the linear specification for the country-specific time trends in the main results.

5.3 Pooled Results

Table 4 shows the selected coefficient estimates from Equation (1) using the pooled data of the eleven countries, along with the estimates for parameters in Equations (3a) through (4b). The observations are weighted using the sample weights provided in the survey data, and standard errors are clustered at the sampling unit level. In what follows, this study mainly interprets estimates for the difference-in-derivatives parameters ($\hat{\Delta}^{post}|_{young}$ and $\hat{\Delta}^{young}|_{post}$).

Column (1) of Table 4 shows that the derivative estimates are positive but small and statistically insignificant. The difference-in-derivatives estimates are negative, small and statistically insignificant, suggesting that the duration of abortion reform exposure is either negatively associated with teenage birth or not associated at all. Column (2) presents the results for the number of teenage births, which is zero for females with no birth history. Similar to the results for teenage births, the derivative estimates here are positive but small and statistically insignificant. The difference-in-derivatives estimate for the post-reform indicator among younger females is small but positive and statistically significant, suggesting that females with longer exposure are likely to have a greater number of teenage births than similarly younger females in the pre-reform data. This increase is estimated within the younger females and thus free from the age effect. Combined with the null effect on the occurrence of teenage births (Column 1), this finding suggests that the cohort effects may have increased teenage births among those who experienced any teen birth. In contrast, the difference-in-derivatives estimate for the younger females within post-reform data is estimated to be negative but small in magnitude and statistically insignificant. Although conflicting results such as these can arise with some random chance, it may be safe to summarise these findings as inconclusive.

Column (3) presents the results for having ever terminated a pregnancy as a teenager. The derivative estimates are small but positive, and some are statistically significant, while their difference estimates are small and statistically insignificant. One should note that the variable includes any termination of pregnancy, including miscarriage or stillbirth. Although the changes in miscarriage and stillbirth rates around the cutoff are likely to be controlled for by the pre-reform kinked linear functions, country-specific birth-year trends, and survey FEs, the remaining variation in this variable may still include pregnancy termination other than abortion. If one is willing to assume that the miscarriage and stillbirth trends are well controlled for, the estimated impacts reflect a change in abortion cases or reporting behaviours. Further assuming that abortion legalisation results in a non-negative change in reporting, the estimates suggest that abortion rates may not have increased and have possibly even decreased.

These results may seem inconsistent with Ananat et al. (2006, 2009) but can be consistently interpreted in conjunction with the estimated change in teenage sexual initiation. Column (4) shows the negative derivative estimates, suggesting that the younger the females are, the less likely they are to have had sexual intercourse during their teenage

Table 4: Pooled Estimation Results of the Effect of Teenage Exposure to Abortion Reform.

	(1) Teenage Birth	(2) No. Teenage Births	(3) Teenage Termination	(4) Teenage Intercourse	(5) Years of Education	(6) Teenage Marriage
Cutoff-age	0.0155*	0.0201	0.0057**	-0.0195**	0.4660***	-0.0094
	(0.0088)	(0.0132)	(0.0028)	(0.0088)	(0.1245)	(0.0091)
(Cutoff-age≥0)×(Cutoff-age)	-0.0038	-0.0073*	0.0013	0.0011	0.0069	0.0045*
	(0.0028)	(0.0042)	(0.0012)	(0.0023)	(0.0192)	(0.0026)
Post reform×	-0.0031*	0.0045	-0.0015**	-0.0070***	0.0643***	-0.0027
(Cutoff-age)	(0.0018)	(0.0029)	(0.0008)	(0.0016)	(0.0139)	(0.0018)
Post reform×	0.0026	0.0027	0.0000	-0.0011	0.0440*	-0.0058*
(Cutoff-age≥0)×(Cutoff-age)	(0.0034)	(0.0052)	(0.0014)	(0.0028)	(0.0245)	(0.0032)
$\delta\|_{pre,young}$	0.0117	0.0129	0.0070**	-0.0184**	0.4730***	-0.0050
$\delta\|_{post,old}$	0.0124	0.0247*	0.0042	-0.0265***	0.5303***	-0.0121
$\delta\|_{post,young}$	0.0112	0.0201	0.0055*	-0.0265***	0.5813***	-0.0134
$\Delta^{post}\|_{young}$	-0.0005	0.0072**	-0.0015	-0.0081***	0.1083***	-0.0084***
$\Delta^{young}\|_{post}$	-0.0012	-0.0046	0.0013	0.0000	0.0510***	-0.0013
Adj. R^2	0.078	0.122	0.007	0.159	0.426	0.124
No. Clusters	40115	39287	33256	39884	42393	40277
No. Obs.	246124	248675	196288	247178	270679	247104

Source: Demographic and Health Surveys and Multiple Indicators Cluster Surveys. *Notes*: This table shows the estimated parameters based on the RKD-DID specification in equation (1). The estimation sample consists of females whose age was 20 or above and within the 5-year bandwidth of the cutoff. The regression includes country-specific linear birth year trend and survey fixed effects. Standard errors are reported in parentheses and clustered at the sampling cluster level of each survey. Statistical significance is denoted by *** if p<0.01, ** if p<0.05, and * if p<0.1. Observations are weighted by the weights provided in the survey data.

years. The estimates also show that the pattern has particularly intensified for post-reform younger females relative to pre-reform younger females. This finding suggests that among younger females, those exposed to abortion reforms were statistically significantly less likely to first have intercourse during their teenage years. This decline may naturally lead to a lesser chance of conception and eventually to a reduction in pregnancy termination if the effect of the decline in sexual initiation outweighed the rise in abortion conditional on pregnancy that is theoretically predicted within the population of sexually active females. Similarly, no change in the birth rate in the whole teenage female population and a reduction in the sexually active subpopulation may imply a rise in the birth rate among sexually active ones. In other words, the framework by Ananat et al. (2006, 2009) is concerned with females who are sexually active and thus subject to pregnancy hazards, while the findings in this study may point to the possibility that abortion reforms may endogenously alter the population of sexually active teenage females.

Then follows a question on why legal reforms that reduce the abortion cost may delay sexual initiation. The results regarding the impacts on education and marriage suggest one possibility. Column (5) shows that the females with longer reform exposure are more educated than younger females surveyed pre-reform. The education impact is also significantly estimated when comparing older and younger females surveyed post-reform. Column (6) shows that females with longer teenage exposure to abortion reforms are less likely to marry by the age of twenty than slightly older females and, to a larger extent, than those who are younger and surveyed pre-reform. These results suggest that younger females surveyed post-reform had higher educational attainment and delayed both their first sexual intercourse and their first pregnancy, which then led to a decrease in pregnancy termination and early marriage. Among the possible, but not the only, scenarios consistent with these findings are that abortion legalisation reduced the expected cost of pregnancy and the perceived hazard of (so-called shotgun) marriage, which can increase the perceived labour market returns on education and thus the investment in education, resulting in a delay in sexual initiation and first marriage, or that the increase in expected returns to education may increase females' expected outside option and thus their bargaining power, in which case, females' desire for delaying pregnancy, if any, is more likely to be reflected in the decision making of future couples. Another important potential mechanism is the interaction of males and females in the marriage market, which can change in the presence of abortion cost reduction. Without additional assumptions or exogenous variations, it is not easy to pin down the precise causal relationships among these outcomes. The exploration of underlying mechanisms is left for future research.

Another possibility is that in low-income settings, insecurity due to political unrest or conflict may lead to the sexual assault of young girls. If recovery from such a state of insecurity coincides with granting legal access to abortion services, girls may have a lower risk of unwanted sexual initiation at young ages and pregnancy termination during their teenage years. Similarly, even in the absence of such insecurity, when a national government can implement a reform that grants enlarged legal access to abortion, it may also implement other reforms related to family planning and female living conditions. In other words, the institutional environment and other policies could confound the impact of abortion reform exposure. This confounding scenario is unlikely since it seems inconsistent with the obtained results. In particular, fewer sexual assaults are unlikely to result in fewer marriages, as conflict-related assaults are unlikely to result in marriage. An exploration of recent history has failed to find conflicts or armed battles that came to an end around abortion reforms in most of the eleven countries.

Robustness Checks. A few additional analyses are conducted to ensure that the above results are robust. First examined is the possibility that the changes in outcomes are driven not only by the abortion reforms but also by other reforms that took place at or around the same time. Panels A through D of Appendix Table B.4 present the potentially related reforms and major events that took place within ten years of the cutoff in each of the eleven countries. In particular, Panel A shows that a legal ban on female genital mutilation was passed on the same day as the abortion reforms in Benin, Burkina Faso, and Chad. If these bans confound the main results, the analysis excluding these countries will likely yield a different set of results. Appendix Table B.5 shows that although less precise, the main findings are robust in that the abortion reforms decreased teenage pregnancy while increasing both pregnancy termination among teenagers and female educational attainment.

Panel B of Appendix Table B.4 shows that five countries out of eleven analysed in this study—namely, Benin, Burkina Faso, Chad, Lesotho, and Togo—each revised the legal age for marriage around the time of their abortion reform. The new legal age for marriage is eighteen years, and so this factor is thus unlikely to contaminate the analysis of this study, which investigates a kink for those aged twenty in the abortion reform year. Although the findings are based on an identification strategy that is unlikely to cause strong bias in this study, Wilson (2021) finds that child marriage reform significantly affects female education and labour market outcomes. A further robustness check excluding these five countries produces the estimated effects of the abortion reform that are slightly less precise than but much the same in point estimates as the main results (Appendix Table B.6).

Panel C of Appendix Table B.4 shows that Benin liberalized access to contraceptives, which may have reduced teen pregnancies and increased education. However, it seems inconsistent with the reduction in teenage sexual initiation, as the lower cost of contraceptives may have increased sexual activity through a decrease in the risk of pregnancy. Indonesia, on the contrary, made it explicit that contraceptives were accessible only for *married* individuals, which implies a higher cost of contraceptives for most teenage females and is thus unlikely to explain the overall decline in teenage pregnancies. Panel D indicates that Colombia prohibited sexual acts with children fourteen years or younger in 2000, six years before the abortion reform. The youngest age cohort affected by this ban on sexual acts with minors coincides with those turning twenty in the same year as the abortion reform in Colombia. However, this ban seems inconsistent with the main findings regarding the increase in pregnancy termination in Table 4. For these reasons, it is unlikely that these confounding reforms entirely drive the main results.

Then sensitivity checks are conducted. The timing information used for the above analysis is at the year level, in which case the functional form assumption may be inappropriate and thus bias the estimates. Some but not all surveys collected monthly information for some of the outcomes and the birth of each female. The month information is not used for the main analysis since the missing values are more frequent and are thus likely to cause potentially endogenous sample selection. It is unknown *a priori* which is more problematic: the functional form assumption with the year measurement or the sample selection with the month measurement. Appendix Table B.1 presents the results when the month, rather than the year, of birth is used to compute reform exposure.[7] The table shows that the numbers of observations used for estimation are smaller,

[7] Years of education and the timing of the first marriage are measured only at the year level and are thus not included in this table.

but the estimated parameters are virtually unchanged from the main results in Table 4. This finding suggests that although this cannot rule out bias from sample selection, the main results are unlikely to be sensitive to the difference in the measurement precision.

The potential sensitivities of the results to the bandwidth and cutoff selection criteria are then examined. The main analysis is based on the sample of females aged twenty years or above whose year of birth is within five-year bandwidths of the cutoff where the cutoff for the pre-reform surveys is ten years above that for the post-reform surveys (*i.e.* setting $s = 10$ in Figure 1). These choices are arguably arbitrary and thus need to be tested for sensitivity. Appendix Table B.2 presents results when the bandwidth is three years and the pre-reform cutoff is six years above the post-reform cutoff (*i.e.* $s = 6$). Appendix Table B.3 presents the results when the bandwidth is ten years and the pre-reform cutoff is twenty years above the post-reform cutoff (*i.e.* $s = 20$). Most of the estimated parameters are unstable and statistically insignificant with a smaller bandwidth, likely due to the substantially smaller estimation sample (almost half that of the main analysis). By contrast, a larger bandwidth produces much the same results as the main analysis. The estimates are highly statistically significant, with almost twice the size of the estimation sample in the main analysis. A slight difference from the main results is found here for teenage pregnancy termination, where the increase for younger females in the post-reform data is tiny and statistically insignificant.[8] Although the results with the narrower bandwidth seem underpowered, those with the wider bandwidth suggest that the main results are likely to be robust to the potential bias arising from the underlying unobservable heterogeneity.

5.4 Cross-Country Heterogeneity

The previous section analyses the pooled data from eleven countries, but these countries can differ from each other in terms of the degree of abortion reforms and other potential determinants of the outcomes of interest. Disentangling the potential heterogeneity in the impact of abortion reforms per se can be beneficial for economic research since differing degrees of a change in abortion cost can theoretically have both positive and negative impacts on giving birth (Ananat et al., 2006, 2009). It can also allow for examining the external validity of the results by repeating the estimation exercise with the same specification and the same data sources. This strategy may be a way to address the difficulty in comparing estimates from separate studies that employ different estimation methods or different data sets, which can also increase heterogeneity in the results.

Caution is warranted when relating the estimated effects to the degree of abortion reforms. As noted in Section 3, the reforms expanded legal access to induced abortion in two major dimensions: increased levels and the addition of special exemptions. This makes the comparison complex since, for instance, the legal accesses of '3' (allowed to protect the health of the mother) and '2RIF' (allowed to protect the life of the mother,

[8]Under the assumption that the OLS estimation of Equation (1) produces the unbiased estimators of the parameters of interest, the mean squared error (MSE) is the same as the variance of the obtained estimates. This breaks down the bias-variance tradeoff in the optimal bandwidth selection, and the largest available bandwidth achieves the smallest variance and hence the MSE. From this view, the results with the ten-year bandwidth may be preferred over the main results. Nonetheless, the results with the five-year bandwidth are presented as the main results, since at least qualitatively, the larger the bandwidth is, the more distant the birth cohorts that the estimation compares, which may bring unobserved heterogeneity that cannot be adequately controlled for by country-specific time trends and survey fixed effects.

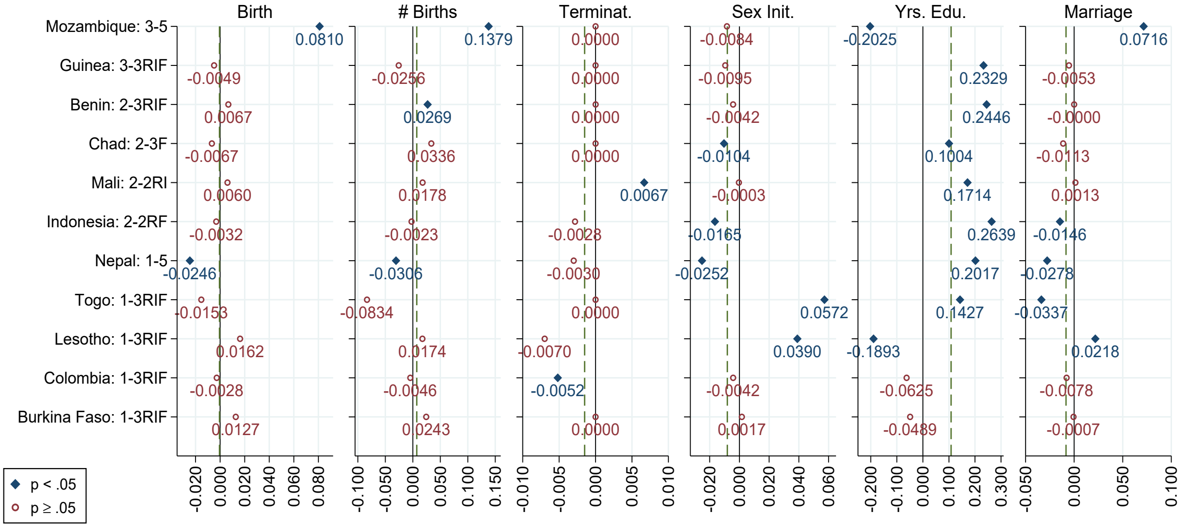
as well as in cases of rape, incest, or foetal impairment) are not directly comparable. One way to proceed is to compare only countries with the same initial or terminal state. For example, while Burkina Faso, Colombia, Lesotho, and Togo moved from level '1' to '3RIF,' Nepal went through a much larger change from level '1' to the unambiguously higher level of '5.' Similarly, among the countries with the post-reform level of '3RIF,' Togo, Lesotho, Colombia, and Burkina Faso moved from level '1,' thus going through a larger change than Benin and Guinea, which had initial levels of '2' and '3,' respectively. The analysis below thus limits its efforts to comparing countries with the same pre- or post-reform levels and attempts to relate the potential heterogeneity in estimation results to the different post-reform (pre-reform) conditions among the countries.

Figure 4 shows the estimated difference in derivatives between post-reform and pre-reform younger females in Panel (a) and the estimated difference in derivatives between younger and older females in post-reform data in Panel (b) for each of the eleven countries. The solid navy diamonds represent statistically significant estimates at the 95% level, while hollow maroon circles represent statistically insignificant estimates. Solid black vertical lines represent zero, and dashed green vertical lines represent the pooled estimates shown in Table 4. Countries are sorted in the pre-reform level, post-reform level, and then the alphabetical order from the bottom.

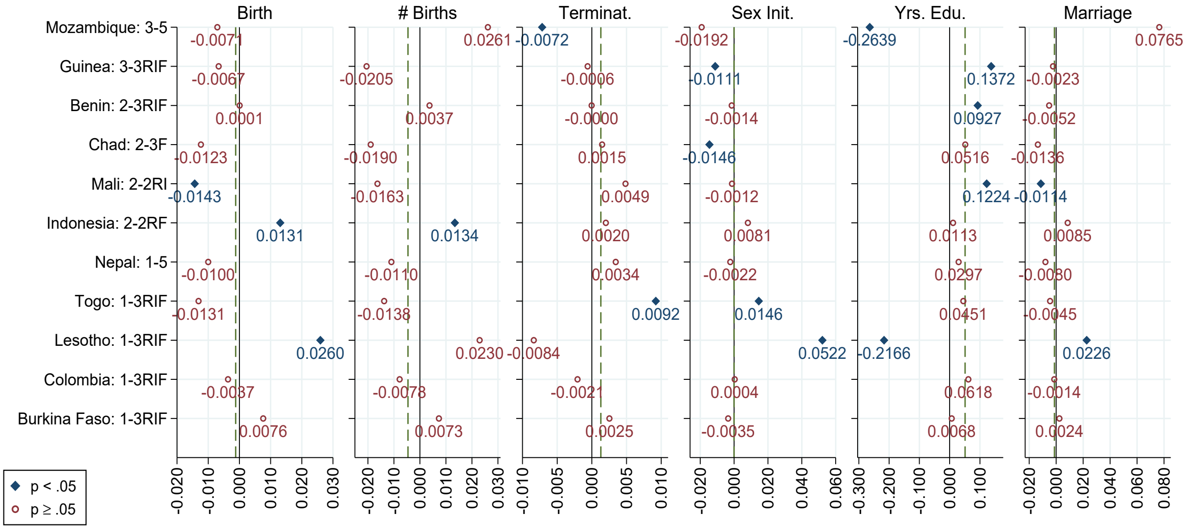
Overall, the country-specific parameter estimates fluctuate around the pooled estimates and lose statistical significance. One reason for this is the smaller estimation samples. As is evident in the robustness check and Appendix Table B.2, even a sample size reduction of half can result in unstable parameter estimates and large standard errors. Qualitative evidence for this possibility is the seemingly larger deviations of estimates for Mozambique and Lesotho, and for Togo to a lesser extent, and these are the countries that have relatively small sample sizes in the post-reform data (see Table 1).

Nevertheless, interesting patterns of heterogeneity emerge. First, Panel (a) shows that countries with a negative estimate of $\Delta^{post}|_{young}$ for the occurrence of teenage births (the *extensive margin*) tend to have a negative estimate for the number of teenage births as well (the *intensive margin*). However, the results fail to show the theoretical prediction that abortion reform can boost birth rates when the abortion cost reduction is so large in magnitude that the resulting increase in pregnancy exceeds the rise in termination conditional on becoming pregnant. In Panel (a), statistically significant positive birth effects are found for Mozambique and Benin. Among these two, estimates for Mozambique are volatile, likely due to the small sample sizes in the country. For Benin, the reform was from level '2' to '3RIF' and not necessarily a large change compared to other countries. Similarly, Panel (b) shows significant positive effects for Indonesia and Lesotho, the former of which changed from level '2' to only '2RF,' and the latter of which is likely prone to large sampling uncertainty due to its small sample sizes. It must be emphasized that these results are still compatible with those of Ananat et al. (2006, 2009), as they predict that the birth response to abortion cost reduction can always be negative. The theoretical model predicts that a positive birth response can arise only when the abortion cost reduction is sufficiently large.

Second, the estimated impacts on the six outcome indicators show similar patterns across the eleven countries for both $\Delta^{post}|_{young}$ in Panel (a) and $\Delta^{young}|_{post}$ in Panel (b). For example, positive impacts on education are likely to coincide with negative impacts on teenage sexual initiation, birth, and marriage. To quantify such patterns of estimated impacts, this study constructs correlation matrices of the difference-in-derivatives estimates across countries. The results shown in Table 5 indeed show a strong correla-



(a) Heterogeneous Estimates of the Difference in Derivatives between Pre- and Post-Reform Younger Females.



(b) Heterogeneous Estimates of the Difference in Derivatives between Older and Younger Females in Post-Reform Data.

Source: Demographic and Health Surveys and Multiple Indicators Cluster Surveys. *Notes*: These figures show the estimated parameters from separate regressions for each country based on the RKD-DID specification in equation (1). The estimation sample consists of females whose age was 20 or above and within the 5-year bandwidth of the cutoff. The regression includes the linear birth year trend and survey fixed effects. Observations are weighted by the weights provided in the survey data.

Figure 4: Heterogeneous Estimates for the Difference-in-Derivatives Parameters.

tion between the impact on education and those on teenage births and sexual initiation (negative), pregnancy termination (positive), and teenage marriage (negative). Strong correlations among estimated impacts sporadically emerge for other combinations of outcome indicators. Nevertheless, they are relatively less robust (*i.e.,* , found for one of the difference-in-derivatives parameters but not for the other) and may thus be due to sampling variability.

Third, additional information may be required to more rigorously interpret the heterogeneity results in relation to the magnitude of abortion reforms. In particular, the present analysis fails to consider potential barriers to abortion other than legal access. For instance, factors such as the financial costs to obtain an abortion, the accessibility and availability of abortion clinics, and social stigma are likely to affect the overall abortion cost but are left out of this analytical framework. If these factors keep the *de facto* cost of abortion access high, extending legal access alone may be insufficient for inducing a more noticeable change in teenage birth rates. Indeed, physical distance to abortion clinics is shown to be a significant predictor according to data from Nepal (Valente, 2014) and the US (Joyce et al., 2013; Lindo et al., 2020). Future studies may thus need to incorporate other dimensions of overall abortion cost.

6 Conclusion

Teenage pregnancy, marriage, and education can have a persistent influence on the economic welfare of females. Past studies have indicated that legal access to abortion services at an early stage of life can affect decisions about them. However, available evidence has been concentrated in developed countries, even though teenage pregnancies can greatly impact females in developing countries. Additionally, using different exogenous variations and data sources has made it difficult to directly compare results from separate studies. This study is one of the first attempts to analyse the effects of legal access to induced abortion on the early life outcomes of females in developing countries. In so doing, the study pays special attention to obtaining results that are likely directly comparable across countries by exploiting the unified RKD-DID framework and internationally comparable data sets across time and space.

This study little effect of exposure to abortion reform on teenage birth in both the extensive and intensive margins and pregnancy termination during teenage years. These results may seem inconsistent with the theoretical predictions by Ananat et al. (2006, 2009). However, additional investigation reveals that longer reform exposure is associated with greater educational attainments and lower likelihoods of sexual initiation and marriage in adolescence. This study hypothesizes that the increase in female education correlates with a decline in sexual debut and marriage during the teenage years, which reduces the share of sexually active teen females and thus obscures the birth and pregnancy termination impacts. It then explores potential heterogeneity since past theory predicts that the impact of reforms on births can be positive, depending on both the pre-reform conditions and the magnitude of the reform (Ananat et al., 2006, 2009), and the effects on the other outcomes are specific to the context of each country. While the study fails to find a positive birth effect in countries with relatively more drastic reforms, the country-specific impacts exhibit consistent patterns across the sample countries.

These findings have important implications for the literature on abortion reform. The obtained results point to the possibility that the framework needs to incorporate whether

Table 5: Correlation Matrices of Difference-in-Derivatives Estimates.

	(1) Teenage Birth	(2) No. Teenage Births	(3) Teenage Termination	(4) Teenage Intercourse	(5) Years of Education	(6) Teenage Marriage
Panel A. Difference in Derivatives between Pre- and Post-Reform Younger Females.						
Teenage Birth	1.000	.	.	.	.	.
Number of Teenage Births	0.890	1.000	.	.	.	.
Teenage Termination	0.093	0.121	1.000	.	.	.
Teenage Intercourse	-0.037	-0.362	-0.128	1.000	.	.
Years of Education	-0.671	-0.556	0.378	-0.297	1.000	.
Teenage Marriage	0.973	0.902	0.015	-0.077	-0.696	1.000
Panel B. Difference in Derivatives between Post-Reform Older and Younger Females.						
Teenage Birth	1.000	.	.	.	.	.
Number of Teenage Births	0.719	1.000	.	.	.	.
Teenage Termination	-0.534	-0.666	1.000	.	.	.
Teenage Intercourse	0.705	0.355	-0.203	1.000	.	.
Years of Education	-0.477	-0.842	0.713	-0.333	1.000	.
Teenage Marriage	0.243	0.782	-0.685	-0.031	-0.873	1.000

Source: Demographic and Health Surveys and Multiple Indicators Cluster Surveys. *Notes*: This table shows the correlation coefficients among the estimated parameters from separate regressions for each country based on the RKD-DID specification in equation (1). The estimation sample consists of females whose age was 20 or above and within the 5-year bandwidth of the cutoff. The regressions include linear birth year trend and survey fixed effects. Observations are weighted by the weights provided in the survey data.

to become sexually active as a response to abortion cost changes. Undesired pregnancies and their abortions are likely more relevant to the younger individuals, particularly teenagers. It thus seems to be a natural next step to incorporate the endogenous change in the sexually active subpopulation into the analytical framework.

Nevertheless, this study is not free of limitations. First, although the data at hand come from a large sample and are comparable across countries and years, more accurate measures for aborted pregnancies and sexual activities would allow a more comprehensive analysis of total fertility and reproductive behaviours. Similarly, data for males and their partners can help enrich the analyses on more diverse topics such as changes in the mating market. Second, methodological advances are necessary to incorporate factors that determine the cost of abortions in terms of more than just legal access, such as the availability of abortion clinics and social norms. Addressing these issues is thus left for future studies that aim to deepen the understanding of abortion reform impacts and teenage behaviours.

Despite its limitations, the results of this study shed light on the significant role of public policy in helping youth lay the foundation for their lives. Specifically, greater exposure to liberalised abortion regimes is likely to boost female education, increase the age of sexual initiation, and reduce teenage marriage rates. Past literature suggests that these changes are likely to facilitate human capital accumulation and enhance future welfare (Cuesta & Leone, 2020). Given the low cost involved, at least from the governmental budget perspective, the expansion of legal access to abortion may provide an alternative and inexpensive policy tool to encourage youth to improve their living conditions through the choices they make during their adolescence.[9] This implies that relaxing legal restrictions may be a policy tool for facilitating human capital accumulation in resource-constrained settings.

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[9]However, one should note that the actual implementation of such reforms may involve other costs, such as reconciling divergent views in society.

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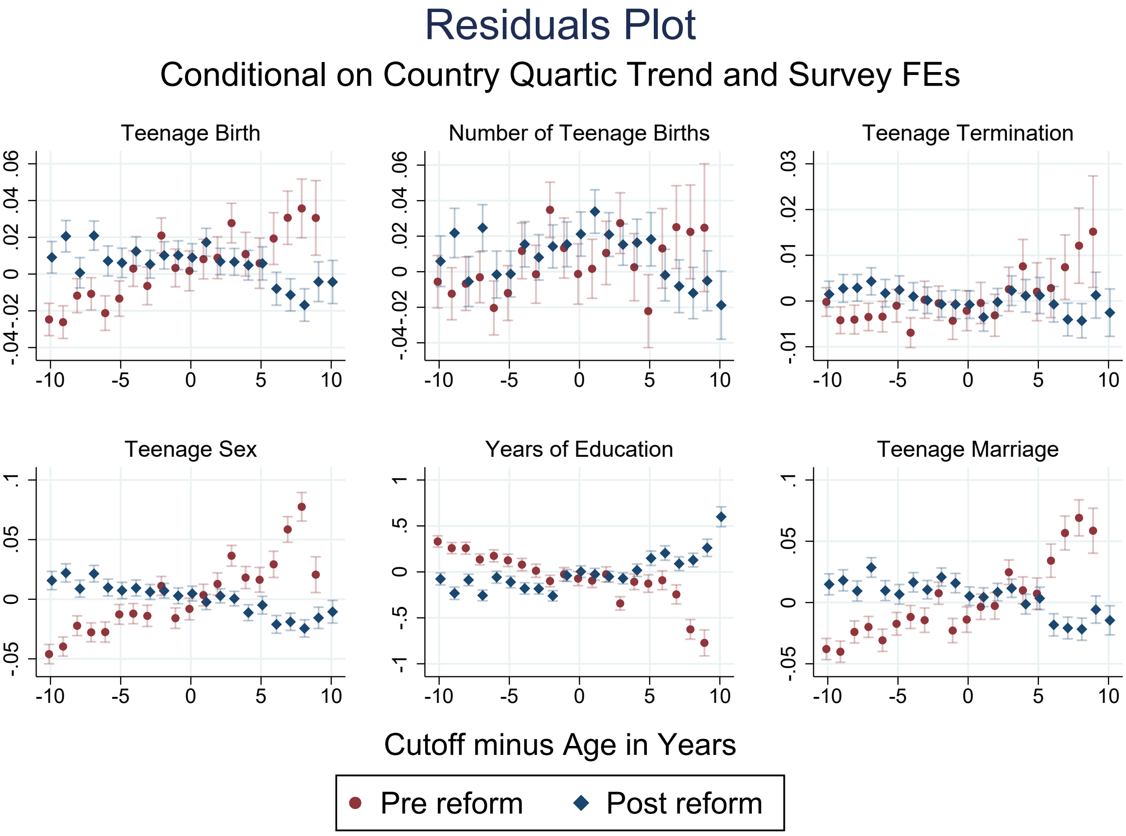
Appendix A Appendix Figures.



Source: Demographic and Health Surveys and Multiple Indicators Cluster Surveys. *Notes*: This figure shows the mean residuals at each value of the cutoff minus age separately for pre-reform and post-reform survey data for six outcome indicators. The markers indicate the mean residual value at the cutoff minus age, while the capped bars indicate the 95% confidence intervals. The red circle markers indicate the residuals for females surveyed pre-reform, while the navy diamonds indicate those for females surveyed post-reform. The trends in the year of birth are approximated by country-specific quartic functions. Observations are weighted by the weights provided in the survey data.

Figure A.1: Residuals Plot on the Cutoff Minus Age with Country-Specific Quartic Trends.

Appendix B Appendix Tables.

Table B.1: Robustness Check of the Main Results with Reform Exposure Measured at the Monthly Level.

	(1) Teenage Birth	(2) No. Teenage Births	(3) Teenage Termination	(4) Teenage Intercourse
Cutoff-age	-0.0135	-0.0219	0.0018	-0.0252***
	(0.0093)	(0.0158)	(0.0035)	(0.0097)
(Cutoff-age≥0)×(Cutoff-age)	0.0007	-0.0028	0.0023*	0.0066**
	(0.0032)	(0.0052)	(0.0014)	(0.0030)
Post reform×	-0.0004	0.0096***	-0.0007	-0.0026
(Cutoff-age)	(0.0022)	(0.0035)	(0.0008)	(0.0021)
Post reform×	-0.0031	-0.0055	-0.0007	-0.0072*
(Cutoff-age≥0)×(Cutoff-age)	(0.0040)	(0.0064)	(0.0016)	(0.0038)
$\delta\|_{pre,young}$	-0.0128	-0.0247	0.0041	-0.0186*
$\delta\|_{post,old}$	-0.0139	-0.0123	0.0011	-0.0278***
$\delta\|_{post,young}$	-0.0164*	-0.0205	0.0026	-0.0284***
$\Delta^{post}\|_{young}$	-0.0036	0.0042	-0.0014	-0.0098***
$\Delta^{young}\|_{post}$	-0.0024	-0.0083**	0.0016*	-0.0007
Adj. R^2	0.079	0.134	0.008	0.130
No. Clusters	38663	38119	32134	38162
No. Obs.	215671	222404	175349	213051

Source: Demographic and Health Sruveys and Multiple Indicators Cluster Surveys. *Notes*: This table shows the estimated parameters based on the RKD-DID specification in equation (1). The estimation sample consists of females whose age was 20 or above and within the 5-year bandwidth of the cutoff where the outcomes and relative age are measured at the monthly level. The regression includes country-specific linear birth year trends and survey fixed effects. Standard errors are clustered at the sampling cluster level of each survey and reported in parentheses. Statistical significance is denoted by *** if p<0.01, ** if p<0.05, and * if p<0.1. Observations are weighted by the weights provided in the survey data.

Table B.2: Robustness Check of the Main Results with a Three-Year Bandwidth for the Estimation Sample.

	(1) Teenage Birth	(2) No. Teenage Births	(3) Teenage Termination	(4) Teenage Intercourse	(5) Years of Education	(6) Teenage Marriage
Cutoff-age	0.0076	0.0175	0.0040	-0.0219**	0.4171***	-0.0054
	(0.0119)	(0.0170)	(0.0040)	(0.0104)	(0.1454)	(0.0108)
(Cutoff-age≥0)×(Cutoff-age)	0.0053	0.0003	-0.0015	0.0046	0.1155***	0.0073
	(0.0058)	(0.0088)	(0.0031)	(0.0051)	(0.0404)	(0.0056)
Post reform×	-0.0008	0.0083	-0.0043**	-0.0114***	0.1568***	-0.0146***
(Cutoff-age)	(0.0038)	(0.0058)	(0.0018)	(0.0033)	(0.0277)	(0.0037)
Post reform×	-0.0074	-0.0063	0.0033	-0.0021	-0.1614***	-0.0028
(Cutoff-age≥0)×(Cutoff-age)	(0.0068)	(0.0104)	(0.0034)	(0.0060)	(0.0496)	(0.0067)
$\delta\|_{pre,young}$	0.0130	0.0179	0.0025	-0.0174	0.5325***	0.0019
$\delta\|_{post,old}$	0.0068	0.0258	-0.0003	-0.0333***	0.5739***	-0.0200*
$\delta\|_{post,young}$	0.0048	0.0199	0.0015	-0.0308***	0.5279***	-0.0155
$\Delta^{post}\|_{young}$	-0.0082**	0.0020	-0.0011	-0.0135***	-0.0046	-0.0174***
$\Delta^{young}\|_{post}$	-0.0021	-0.0059	0.0018	0.0025	-0.0459	0.0044
Adj. R^2	0.084	0.127	0.007	0.167	0.443	0.133
No. Clusters	34600	33915	28146	34517	36570	34776
No. Obs.	150592	150907	119866	150846	164995	150179

Source: Demographic and Health Surveys and Multiple Indicators Cluster Surveys in selected countries. *Notes*: This table shows the estimated parameters based on the RKD-DID specification in equation (1). The estimation sample consists of females whose age was 20 or above and within the 3-year bandwidth of the cutoff. The regression includes country-specific linear birth year trends and survey fixed effects. Standard errors are clustered at the sampling cluster level of each survey and reported in parentheses. Statistical significance is denoted by *** if p<0.01, ** if p<0.05, and * if p<0.1. Observations are weighted by the weights provided in the survey data.

Table B.3: Robustness Check of the Main Results with a Ten-Year Bandwidth for the Estimation Sample.

	(1) Teenage Birth	(2) No. Teenage Births	(3) Teenage Termination	(4) Teenage Intercourse	(5) Years of Education	(6) Teenage Marriage
Cutoff-age	0.0130*	0.0234**	0.0055**	-0.0246***	0.4546***	-0.0111
	(0.0072)	(0.0109)	(0.0022)	(0.0075)	(0.1071)	(0.0077)
(Cutoff-age≥0)×(Cutoff-age)	0.0037***	0.0076***	0.0004	0.0073***	-0.0419***	0.0054***
	(0.0010)	(0.0016)	(0.0004)	(0.0009)	(0.0068)	(0.0009)
Post reform×	0.0021***	0.0095***	0.0004	0.0028***	0.0071	0.0037***
(Cutoff-age)	(0.0007)	(0.0012)	(0.0003)	(0.0007)	(0.0059)	(0.0007)
Post reform×	-0.0066***	-0.0150***	-0.0006	-0.0103***	0.1327***	-0.0097***
(Cutoff-age≥0)×(Cutoff-age)	(0.0013)	(0.0021)	(0.0005)	(0.0011)	(0.0096)	(0.0012)
$\delta\vert_{pre,young}$	0.0167**	0.0310***	0.0059***	-0.0173**	0.4127***	-0.0057
$\delta\vert_{post,old}$	0.0151**	0.0329***	0.0059***	-0.0218***	0.4617***	-0.0074
$\delta\vert_{post,young}$	0.0122*	0.0255**	0.0057***	-0.0248***	0.5526***	-0.0117
$\Delta^{post}\vert_{young}$	-0.0045***	-0.0055***	-0.0002	-0.0075***	0.1398***	-0.0060***
$\Delta^{young}\vert_{post}$	-0.0029***	-0.0074***	-0.0002	-0.0031***	0.0908***	-0.0043***
Adj. R^2	0.062	0.096	0.005	0.136	0.382	0.108
No. Clusters	45920	44851	37605	45523	48025	46022
No. Obs.	482449	487797	384301	483158	524861	483470

Source: Demographic and Health Surveys and Multiple Indicators Cluster Surveys in selected countries. *Notes*: This table shows the estimated parameters based on the RKD-DID specification in equation (1). The estimation sample consists of females whose age was 20 or above and within the 10-year bandwidth of the cutoff. The regression includes country-specific linear birth year trends and survey fixed effects. Standard errors are clustered at the sampling cluster level of each survey and reported in parentheses. Statistical significance is denoted by *** if $p<0.01$, ** if $p<0.05$, and * if $p<0.1$. Observations are weighted by the weights provided in the survey data.

Table B.4: Related Reforms Implemented in Years around Abortion Reforms in Eleven Countries

Country	Events
	Panel A. Female Genital Mutilation (FGM)
Benin	A new law that banned any FGM practice was promulgated on the 3rd of March, 2003, the same day as the law that extended legal access to abortion.[1]
Burkina Faso	The revised Penal Code not only extended access to abortion but also prohibited any practice of FGM.[2]
Chad	The revised Penal Code in March 2002 not only extended access to abortion but also prohibited any practice of FGM.[3]
	Panel B. Marriage
Benin	The revised Penal Code criminalized forced marriage and prostitution. Furthermore, the Code of Persons and Family in 2004 established that those under the age of eighteen cannot marry without the consent of the person with parental authority.[4]
Burkina Faso	The revised Penal Code criminalized forced marriage, though the criminalization is concerned only with the civil marriage.[5]
Chad	Early marriage under the age of eighteen years has been prohibited by law since the Promotion of Reproductive Health Law.[6]
Lesotho	Children's Protection and Welfare Act, published on the 31st of December, 2011, prohibited marrying anyone under the age of eighteen years.[7]
Togo	The newly introduced Children's Code on the 6th of July, 2007, defined the minimum age of marriage as 18 years and prohibited underage marriage.[8]
	Panel C. Contraceptives
Benin	The Penal Code promulgated on the 3rd of March, 2003, not only extended access to abortion but also repealed the 1920 law that had prohibited incitement to abortion and contraceptives and gave everyone the right to be informed and to use the family planning method of their choice.[9]
Indonesia	The 2009 law on population growth and family development (Article 26) strengthend the restriction on access to contraceptives and allowed them to be accessible only to married couples.[10]
	Panel D. Others
Colombia	The Criminal Code published on the 24 of July, 2000, prohibited sexual acts with a minor aged 14 years or younger.[11]

Source: Author. [1] Ras-Work (2009). [2] Burkina Faso (1997). [3] Déby (2002). [4] Cledjo and Tingbe (2018). [5] Burkina Faso (1997). [6] Déby (2002) and Immigration and Refugee Board of Canada (2015). [7] Kingdom of Lesotho (2011). [8] Republique Togolaise (2007b). [9] Center for Reproductive Rights (n.d.). [10] Amnesty International (2013). [11] República de Colombia (2000).

Table B.5: Robustness Check Excluding Countries That Implemented FGM Bans in the Same Year as the Abortion Reforms.

	(1) Teenage Birth	(2) No. Teenage Births	(3) Teenage Termination	(4) Teenage Intercourse	(5) Years of Education	(6) Teenage Marriage
Cutoff-age	0.0170*	0.0209	0.0058**	-0.0186**	0.4724***	-0.0091
	(0.0089)	(0.0132)	(0.0028)	(0.0088)	(0.1246)	(0.0091)
(Cutoff-age≥0)×(Cutoff-age)	-0.0044	-0.0045	0.0013	0.0013	0.0000	0.0052*
	(0.0030)	(0.0044)	(0.0012)	(0.0026)	(0.0213)	(0.0029)
Post reform×	-0.0049**	0.0042	-0.0017**	-0.0093***	0.0568***	-0.0038*
(Cutoff-age)	(0.0021)	(0.0031)	(0.0008)	(0.0018)	(0.0159)	(0.0020)
Post reform×	0.0027	-0.0028	0.0000	0.0004	0.0501*	-0.0054
(Cutoff-age≥0)×(Cutoff-age)	(0.0038)	(0.0056)	(0.0016)	(0.0033)	(0.0285)	(0.0037)
$\delta\vert_{pre,young}$	0.0126	0.0164	0.0071**	-0.0173*	0.4724***	-0.0039
$\delta\vert_{post,old}$	0.0121	0.0251*	0.0041	-0.0278***	0.5292***	-0.0130
$\delta\vert_{post,young}$	0.0104	0.0178	0.0055*	-0.0262***	0.5793***	-0.0131
$\Delta^{post}\vert_{young}$	-0.0022	0.0014	-0.0017	-0.0089***	0.1070***	-0.0092***
$\Delta^{young}\vert_{post}$	-0.0017	-0.0073**	0.0014	0.0017	0.0501***	-0.0002
Adj. R^2	0.078	0.120	0.007	0.176	0.412	0.121
No. Clusters	33671	33089	29165	33750	35499	34056
No. Obs.	188573	193413	159689	192317	208765	192316

Source: Demographic and Health Surveys and Multiple Indicators Cluster Surveys. *Notes*: This table shows the estimated parameters based on the RKD-DID specification in equation (1). The estimation sample consists of females whose age was 20 or above and within the 3-year bandwidth of the cutoff in countries without an FGM ban in the same year as the abortion reform (*i.e.*, Colombia, Guinea, Indonesia, Lesotho, Mali, Mozambique, Nepal, and Togo). The regression includes country-specific linear birth year trends and survey fixed effects. Standard errors are clustered at the sampling cluster level of each survey and reported in parentheses. Statistical significance is denoted by *** if p<0.01, ** if p<0.05, and * if p<0.1. Observations are weighted by the weights provided in the survey data.

Table B.6: Robustness Check Excluding Countries That Implemented Marital Age Reform around the Year of the Abortion Reforms.

	(1) Teenage Birth	(2) No. Teenage Births	(3) Teenage Termination	(4) Teenage Intercourse	(5) Years of Education	(6) Teenage Marriage
Cutoff-age	0.0184**	0.0225*	0.0058**	-0.0177**	0.4756***	-0.0073
	(0.0089)	(0.0132)	(0.0028)	(0.0088)	(0.1248)	(0.0091)
(Cutoff-age≥0)×(Cutoff-age)	-0.0064*	-0.0069	0.0014	0.0003	-0.0105	0.0031
	(0.0033)	(0.0047)	(0.0013)	(0.0027)	(0.0236)	(0.0031)
Post reform×	-0.0065***	0.0028	-0.0016*	-0.0099***	0.0490***	-0.0058***
(Cutoff-age)	(0.0022)	(0.0033)	(0.0009)	(0.0020)	(0.0176)	(0.0022)
Post reform×	0.0045	-0.0013	-0.0002	0.0004	0.0710**	-0.0039
(Cutoff-age≥0)×(Cutoff-age)	(0.0041)	(0.0060)	(0.0017)	(0.0035)	(0.0310)	(0.0040)
$\delta\|_{pre,young}$	0.0120	0.0156	0.0072**	-0.0174**	0.4651***	-0.0042
$\delta\|_{post,old}$	0.0119	0.0253*	0.0042	-0.0276***	0.5247***	-0.0131
$\delta\|_{post,young}$	0.0100	0.0171	0.0054*	-0.0268***	0.5852***	-0.0138
$\Delta^{post}\|_{young}$	-0.0020	0.0016	-0.0019*	-0.0095***	0.1200***	-0.0097***
$\Delta^{young}\|_{post}$	-0.0019	-0.0082**	0.0012	0.0008	0.0605***	-0.0007
Adj. R^2	0.082	0.122	0.007	0.189	0.422	0.126
No. Clusters	30180	30290	27641	30957	31435	30735
No. Obs.	167392	174190	149316	175181	183515	172091

Source: Demographic and Health Surveys and Multiple Indicators Cluster Surveys. *Notes*: This table shows the estimated parameters based on the RKD-DID specification in equation (1). The estimation sample consists of females whose age was 20 or above and within the 5-year bandwidth of the cutoff in countries without a marriageable age reform in the same year as an abortion reform (*i.e.*, Colombia, Guinea, Indonesia, Mali, Mozambique, and Nepal). The regression includes country-specific linear birth year trends and survey fixed effects. Standard errors are clustered at the sampling cluster level of each survey and reported in parentheses. Statistical significance is denoted by *** if p<0.01, ** if p<0.05, and * if p<0.1. Observations are weighted by the weights provided in the survey data.

Table B.7: Robustness Check Using the Weights Whose Sum Is Equal across Countries.

	(1) Teenage Birth	(2) No. Teenage Births	(3) Teenage Termination	(4) Teenage Intercourse	(5) Years of Education	(6) Teenage Marriage
Cutoff-age	0.0110	0.0145	0.0069**	-0.0220**	0.4605***	-0.0123
	(0.0089)	(0.0133)	(0.0028)	(0.0088)	(0.1245)	(0.0091)
(Cutoff-age≥0)×(Cutoff-age)	-0.0008	-0.0050	-0.0016	0.0028	0.0195	0.0069**
	(0.0031)	(0.0048)	(0.0013)	(0.0025)	(0.0195)	(0.0028)
Post reform×	0.0040*	0.0141***	-0.0032***	-0.0029*	0.0728***	0.0016
(Cutoff-age)	(0.0021)	(0.0034)	(0.0009)	(0.0017)	(0.0141)	(0.0020)
Post reform×	-0.0023	-0.0029	0.0038**	-0.0045	0.0254	-0.0092***
(Cutoff-age≥0)×(Cutoff-age)	(0.0037)	(0.0061)	(0.0016)	(0.0030)	(0.0253)	(0.0035)
$\delta\|_{pre,young}$	0.0102	0.0095	0.0053*	-0.0191**	0.4800***	-0.0054
$\delta\|_{post,old}$	0.0150*	0.0287**	0.0038	-0.0248***	0.5333***	-0.0107
$\delta\|_{post,young}$	0.0119	0.0207	0.0060**	-0.0265***	0.5782***	-0.0130
$\Delta^{post}\|_{young}$	0.0017	0.0113***	0.0007	-0.0074***	0.0982***	-0.0076***
$\Delta^{young}\|_{post}$	-0.0031	-0.0079**	0.0022**	-0.0017	0.0449***	-0.0023
Adj. R^2	0.074	0.134	0.007	0.107	0.400	0.126
No. Clusters	40115	39287	33256	39884	42393	40277
No. Obs.	246124	248675	196288	247178	270679	247104

Source: Demographic and Health Sruveys and Multiple Indicators Cluster Surveys. *Notes*: This table shows the estimated parameters based on the RKD-DID specification in equation (1). The estimation sample consists of females whose age was 20 or above and within the 5-year bandwidth of the cutoff. The regression includes country-specific linear birth year trends and survey fixed effects. Standard errors are clustered at the sampling cluster level of each survey and reported in parentheses. Statistical significance is denoted by *** if p<0.01, ** if p<0.05, and * if p<0.1. Observations are weighted by the weights provided in the survey data and adjusted so that the sum equals across countries.