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Discussion Paper no. 2025-17

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JEL Classification: J16, J12, O15

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Beyond the Minimum: The Impact of Indonesia's Marriage Age Law on Child Marriage and Education[§]

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Abstract

Child marriage remains a significant global issue, violating human rights and limiting development outcomes, particularly for girls. This study examines the impact of Indonesia's first minimum marriage age (MMA) law, which in 1975 set the minimum age of marriage for girls at 16 years. The analysis relies on a regression discontinuity design to estimate the effects of the policy on child marriage and girls' education, with specific adjustments to address non-random heaping in reported years of birth. Using data from the 2018–2021 Indonesia National Socio-economic Survey (SUSENAS), we find that the MMA law reduced marriages under the age of 16 by 18% and increased the age at first marriage by about five months. It also had a broader effect by delaying marriages beyond the legal threshold. The effects were stronger in regions with entrenched child-marriage norms and in urban settings. We also find evidence that the MMA policy had positive effects on educational attainment, particularly in obtaining a tertiary degree.

Keywords: Child marriage, Indonesia, non-random heaping, anticipation effect, doughnut regression discontinuity design

JEL Codes: J16, J12, O15

[§]Declaration of competing interest: The authors declare no conflict of interest. Acknowledgement: This research was supported by the Australian Government Research Training Program (RTP) Scholarship.
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1 Introduction

Child marriage is still a global development issue involving human rights violations (OHCHR, 2023). Globally, around 12 million girls per year are married before their 18th birthday (UNICEF, 2020). Previous studies have shown that being in a union or marriage under 18 is linked to disadvantages for women and their offspring, including lower educational attainment, poorer labour market outcomes, domestic violence, reduced household decision-making power, and worse health and educational outcomes for children (Cameron et al., 2022; Dahl, 2010; Delprato et al., 2017; Field and Ambrus, 2008; Roychowdhury and Dhamija, 2021; Chari et al., 2017). These wide-ranging negative consequences make the elimination of child marriage central to women's empowerment and the formation of human capital across generations.

One of the most widely adopted policy measures to curb child marriage is the minimum marriage age (MMA) law. However, the evidence on their effectiveness is mixed, reflecting cross-country differences in the design and enforcement of such laws, as well as the availability of legal exceptions (Wodon et al., 2016). Studies on the effects of MMA reforms in other countries, such as the US (Bharadwaj, 2015), Ethiopia (Garcia-Hombrados, 2021; McGavock, 2021), and a cross-country comparison of low- and middle-income countries (LMICs) (Wilson, 2022), show that raising the legal marriage age can reduce early marriage, with spillovers to education, fertility, labour market outcomes, and even child health. At the same time, other evidence points to more limited or negligible effects. For instance, in Mexico, where informal unions are widespread, raising the legal marriage age reduced formal marriage but left schooling and fertility largely unchanged due to an increase in informal unions (Bellés-Obrero and Lombardi, 2020). Broader cross-country analyses also reinforce this mixed picture, finding that while some countries experienced meaningful declines in child marriage after reform, most showed only modest or insignificant changes (Collin and Talbot, 2023; Batyra and Pesando, 2021). Together, these findings suggest that MMA reforms can be an important policy tool, but their effectiveness remains an open question and appears to vary across institutional settings and cultural contexts.

This paper examines the impact of the first MMA reform on girls in Indonesia, a country that is home to the fourth largest number of child marriages worldwide (The Child Marriage Data Portal, 2025). In 1975, Indonesia implemented its first marriage law, which set the legal minimum age of marriage for girls at 16. While the law also set the minimum age of 19 years for men, child marriage is overwhelmingly concentrated among girls. In Indonesia, for example, 11% of women aged 20–24 had been married before turning 18 in 2018 compared with just 1% of men (UNICEF et al., 2020).

There are several reasons to expect that raising the minimum legal age alters marriage behaviour. The reform discourages early marriage by making under-age unions more costly and uncertain. Parents are required to obtain dispensation from the courts if they wish their child to marry under the legal age, and the outcome of the process is not guaranteed (Katz and Katz, 1978). Indirectly, it also provides a signalling effect, which may influence societal attitudes around early marriage and the value of investing in girls' wellbeing and development (Katz and Katz, 1978). With schooling available as a credible and valuable alternative pathway to early marriage, we may also expect to see an increase in education following the introduction of the MMA reform. In particular, given the opportunity cost of withdrawing girls partway through an education program, we might expect to see that marriage is delayed beyond the age of 16, until after completing high school or tertiary education.

Using nationally representative data from the Indonesian National Socioeconomic Survey (SUSENAS, 2018–2021), we exploit the age discontinuity in exposure to the reform to estimate its causal impact through a regression discontinuity (RD) design. The analysis is conducted under an intention-to-treat framework because despite the new legal minimum age, it was still possible for girls to marry under the age of 16 through dispensations or marriages outside the state system. The estimates therefore capture not only the effect of tighter legal restrictions but also the broader normative signal associated with raising the legal age. To address potential biases, we apply a doughnut-RD approach that excludes cohorts most likely to anticipate the reform and removes cohorts affected by non-random heaping in the reported year-of-birth, which affects the running variable.

Our results show that exposure to the 1975 MMA policy significantly reduced the likelihood of girls marrying before age 16 by 2.2 percentage points (ppt), which is equivalent to an 18% reduction relative to the pre-reform mean. The effect of the reform also extended beyond its legal target by delaying marriages above the minimum age, with the largest reductions observed for marriage before 18 and before 22. Overall, the policy increased the mean age at first marriage for the exposed cohorts by around five months. We also examine potential impacts on women's education and find that the MMA policy marginally increased the probability of completing senior high school (by about 1%) and greatly increased the probability of completing higher (or tertiary) education, with an estimated 29% increase relative to the pre-reform mean. The policy's effects were more pronounced in urban areas and among women from regions with entrenched child-marriage traditions, though the persistence of these effects varied across settings.

This paper makes several contributions. First, to our knowledge, it is the first causal analysis of Indonesia's initial minimum marriage age law, implemented in 1975. Previous work by Cammack et al. (1996) used descriptive comparisons of marriage patterns before and after the law and concluded that it had little direct effect. More recently, an unpublished master's thesis by Yudisthira (2023) applied a fuzzy RD design to study the 2019 reform that raised the minimum age to 19 for girls, finding some reductions in child marriage but with important limitations due to dispensations and cultural practices. Second, the study addresses two potential sources of bias using a doughnut-RD approach, namely potential anticipation effects arising from the long announcement–implementation gap and non-random heaping in reported year of birth that affects the smoothness of the running variable. Despite being a pervasive feature of survey data in developing countries (Stockwell, 1966), non-random heaping is often overlooked as a source of bias in policy evaluations. Finally, we extend the analysis beyond the legal cut-off to assess whether the reform had broader effects that went beyond its legal target age, and whether it resulted in increased educational attainment.

2 Setting

2.1 Child Marriage in Indonesia

Indonesia is the fourth-largest contributor to global child marriages, with more than 32 million women married or in union before the age of 18 (The Child Marriage Data Portal, 2025). Although the prevalence of marriage among girls aged 15–18 has declined markedly over the past five decades (Jones, 2011), child marriage remains a significant concern. In 2018, about 11 per cent of women aged 20–24 had been married before turning 18, compared with only 1 per cent of men (UNICEF et al., 2020). Prevalence also differs sharply by location, ranging from around 4 per cent in the capital city (DKI Jakarta) to more than 19 per cent in the province (West Sulawesi) with the highest rate, and with rural areas consistently showing much higher levels than urban areas. The scale of the problem was far greater in the 1970s, when Indonesia's first minimum marriage age policy was introduced. At that time, nearly 37 per cent of women had married as teenagers (ages 15–19), representing the highest prevalence in Southeast Asia, while the mean age at marriage, about 19 years, was the lowest in the region (Jones, 2011, 2017).

Early marriage in Indonesia was driven by a combination of social expectations and economic reasoning. One of the strongest motives for early marriage was the protection of family honour. Parents feared that daughters might bring disgrace to the family by either failing to maintain premarital purity through sexual relationships outside wedlock or by remaining unmarried beyond the socially acceptable age, thereby facing the stigma of 'old maids' (Jones, 1994). To avoid these risks, it was common for parents to arrange marriages soon after menarche, hoping this would protect family reputation and secure a suitable match while daughters were still considered desirable in the marriage market (Jones, 1994). Economic considerations also reinforced these choices. In contexts where daughters were regarded as having limited economic value compared with sons, marrying daughters early meant reducing the financial responsibility of the natal household and, in some cases, securing support from a son-in-law as an additional labourer (Jones, 1994; Blackburn and Bessell, 1997).

Despite the persistence of child marriage as a social norm, the expansion of female education began to shift patterns towards later marriage by reshaping both parental decisions and expanding opportunities for girls. From the parental perspective, education changed the way daughters were valued within the household: from being seen merely as dependants to being recognised as possible future earners (Jones, 1994). This created a choice between marrying daughters off quickly to reduce immediate expenses or keeping them longer in education with the expectation of later economic contributions. Over time, keeping daughters in school gained wider social acceptance, as education and employment came to be recognised as alternative sources of family pride and status outside marriage. For girls themselves, schooling reshaped both opportunities and aspirations. First, remaining in school increased the likelihood of interaction with boys (Jones, 1994), which strengthened the idea of marriage as a personal choice rather than solely a parental arrangement. Second, education exposed girls to more individualised and 'Westernised' notions of marriage as a form of self-fulfilment, which in turn made them less willing to accept early arranged marriages (Malhotra, 1997). While marriage had traditionally been the primary route for girls to gain social respect and recognition (Jones, 1994; Kok et al., 2023), education and the increased potential to contribute economically created alternative pathways to status and respect outside of marriage. These shifts in norms and aspirations occurred alongside broader movements for women's empowerment, which together formed part of the context for Indonesia's introduction of its first minimum marriage age policy, as discussed in Section 2.2.

2.2 The First Minimum Marriage Age Law

Before the mid-1970s, Indonesia had no single unified marriage law. Instead, marital matters were governed by a mix of different legal systems: the Civil Code of 1847 for Europeans and Chinese Indonesians, the 1933 ordinance for Christians, Islamic law, and diverse customary laws (*adat*) for other groups (Azra, 2003). Minimum marriage ages were only specified under the Civil Code (18 for men and 15 for women) and the Christian ordinance, while neither Islamic nor customary law, which together applied to the vast majority of Indonesians, set any formal age limits (Soewondo, 1977). The coexistence of different legal systems gave rise to debate. Women's movement called for raising the minimum age to protect girls, whereas conservative Muslim groups opposed statutory limits and continued to view puberty or menarche as sufficient maturity for marriage (Blackburn and Bessell, 1997; Katz and Katz, 1975). The country's first minimum marriage law was then the outcome of political compromise, balancing demands for higher ages by the reformists and resistance from conservatives.

Law No. 1/1974, promulgated on 2 January 1974, marked Indonesia's first national marriage law. It brought together the different legal traditions into a single framework that regulated registration, divorce, polygamy, and the minimum marriage age (Katz and Katz, 1978). Under the new law, individuals younger than 21 years must obtain parental consent in order to marry. The law also established minimum ages of 19 years for men and 16 years for women, below which marriage was permitted only if the parents obtained a dispensation from the court.[1] Even though the law did not aim to eradicate under-age marriage altogether, it helped reduce the practice by complicating the process. By requiring parental petitions to a court, the law increased the time and financial costs of arranging an early marriage and introduced uncertainty over whether approval would be granted (Katz and Katz, 1978), which may have discouraged parents from marrying off their under-age children under the legal system.

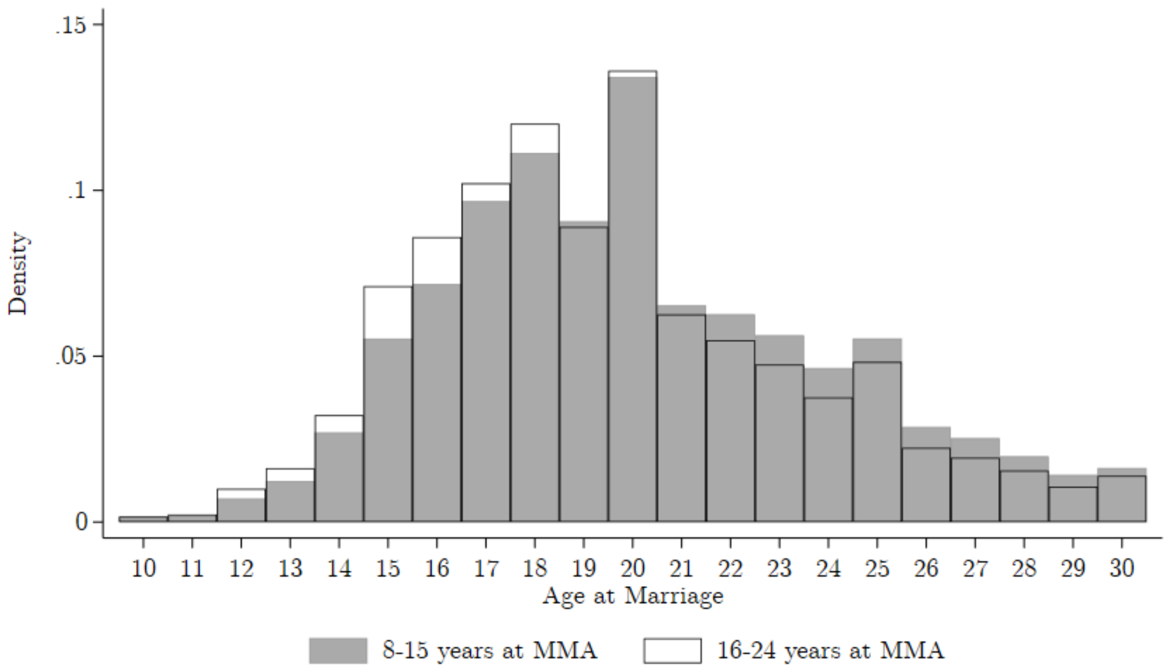
Indonesia's first marriage law was formally enacted on 2 January 1974, but it remained unenforced until implementing regulations were introduced (Katz and Katz, 1975). The implementing regulation (Government Regulation No. 9/1975) was eventually issued in April 1975, with the law becoming effective on 1 October 1975. The gap of around 21 months between promulgation and enforcement created an "announcement period." During this time, families were aware of the impending change but not yet constrained by it, which may have led some to anticipate the new restrictions by arranging marriages for

[1] Article 6(2) of Law No. 1/1974 stipulates that "to enter into a marriage, a person who has not reached the age of 21 years must obtain permission from both parents." Article 7(1) further provides that "marriage is only permitted if the man has reached the age of 19 years and the woman 16 years." Article 7(2) allows for exceptions, whereby "in the case of deviation from paragraph (1), dispensation may be requested from the court or another designated authority by the parents of the man or the woman.

their under-age children before the regulation came into force. We take this anticipation effect into account in our identification strategy, as explained in Section 4.

To illustrate the potential shift in marriage timing associated with the new law, Figure 1 compares the distribution of age at first marriage between women who were exposed to the minimum age restriction (aged 8–15 in 1975, shown in grey) and those who were not (aged 16–24 in 1975, shown in white outline). The figure shows that women unexposed to the reform were more likely to marry before the age of 19, whereas those exposed to the new law had a greater share of marriages taking place in their twenties. The difference is most pronounced around the legal threshold of 16, where the exposed cohort faced restrictions complicating legal marriage. This pattern provides an early indication that the law may have contributed to a shift in marriage behaviour toward later ages, which we examine more formally in Section 5.

Figure 1
Distribution of age at first marriage among women aged 8–15 years (exposed) and 16–24 years (unexposed) at the time the MMA law came into effect in 1975



Source: Authors' visualisation based on Susenas 2018-2021.

3 Data

3.1 Data Sources and Sample

The data in this study comes from the Indonesia National Socio-economic Survey (SUSENAS), a cross-sectional annual large-scale survey conducted by Statistics Indonesia (Badan Pusat Statistik). It collects data on a range of social and demographic indicators from around 300,000 randomly selected households, covering more than 1 million individuals. In this study, we pool four years of cross-sectional SUSENAS conducted in 2018-2021 which provide information about dates of birth (day, month, and year) and the age at first marriage.[2]

We use SUSENAS 2018 to 2021 and restrict the sample to female respondents with complete date-of-birth information who were born between 1951 and 1967. This corresponds to ages 8 to 24 at the policy date of 1 October 1975. At interview, these cohorts were approximately 51 to 70 years old across the four waves.[3] The resulting sample contains 340,388 female observations.

We limit the sample to this birth range (1951 to 1967) to ensure a clean identification strategy and to avoid contamination from overlapping policies. The untreated cohorts start in 1951 to exclude the round-year heaping in 1950, while the sample window already spans the common heaping years of 1955, 1960, and 1965. At the upper end, we stop with the 1967 birth cohort to exclude women born in 1968–1972, who were 2–6 years old in 1974 and thus fully exposed to the INPRES school construction program (Duflo, 2001). This program substantially increased years of schooling among affected cohorts (Duflo, 2001), potentially providing young women more opportunities outside marriage and thereby influencing parental decisions on when to marry their daughters. Beyond schooling, studies show that the INPRES program also shaped marriage market dynamics. For example, (Akresh et al., 2023) document increased assortative mating, while (Zha,

[2]Previous SUSENAS surveys did not provide both full information on birth dates (e.g., only provided month and year of birth) and age at first marriage.

[3]The youngest women in the sample were 8 years old at the time of the MMA policy and were 51 years old when interviewed in 2018, while the oldest were 24 years old at the time of the policy and were 70 years old when interviewed in 2021.

2019) finds that in densely populated areas, where women's secondary schooling declined as teacher resources shifted to primary schools, the program led to earlier marriage and larger spousal age gaps. Together these findings suggest that INPRES influenced both education and marriage outcomes, which could confound estimates of the MMA policy.

3.2 Key Variables

3.2.1 Marriage outcomes

Marriage outcomes are based on women's reported age at first marriage, which is collected for all respondents who have ever married, including those currently married, divorced, or widowed. The survey defines marriage broadly, encompassing both legally registered unions (recognised by the state, tradition, or religion) and informal unions in which couples live together and are regarded as husband and wife by their community (Indonesia, 2012).

From this measure, we construct two main outcome variables. The first is a binary measure of marriage under 16, which equals 1 if the respondent's first marriage occurred before age 16 and 0 if the first marriage occurred at age 16 or older, or if the respondent has never married. This threshold corresponds directly to the minimum marriage age with parental permission for girls under the 1975 Indonesian Marriage Law. The second outcome is the age at first marriage, which has discrete integer values for ever-married women.[4] We also create binary indicators of whether or not the respondent married before specific ages, ranging from 10 to 35 years of age, to explore whether the reform delayed marriage beyond the new legal age of marriage.

3.2.2 Education outcomes

The first education measure is a set of mutually exclusive categories indicating the highest level of schooling completed. Respondents reported the highest education certificate (*ijazah*) they had obtained, which we recode into three groups: junior high school only, senior high school only, and higher (or tertiary) education.

[4]Although this variable is defined only for women who have ever married, the share of women who were never married is small, at round 2.2% of the sample.

The second education measure is total years of schooling, which captures both completed and uncompleted levels of education. For each respondent, we assign years of schooling based on the highest level attended and the grade reached within that level. In Indonesia, primary school (*sekolah dasar*) typically begins at age 6–7 and lasts for six years, followed by three years of junior secondary school (*sekolah menengah pertama*) and three years of senior secondary school (*sekolah menengah atas*). Completion of these stages therefore corresponds to approximately 6, 9, and 12 years of schooling, respectively, while higher education adds further years depending on the degree pursued.

3.3 Summary Statistics

Table 1 shows the summary statistics for women who were unexposed to the MMA policy (those aged 16–24 at the time of implementation) and for those who were exposed (aged 7–15) within our sample. The table also reports mean differences between the two groups, together with the significance of t-tests. For the child marriage outcomes, exposed women have a significantly lower prevalence of first marriage under 16 and a higher mean age at marriage. Specifically, the exposed women have around 2.8 percentage points (ppt) lower prevalence of under-16 marriage and marry on average about eight months later. Both differences are statistically significant at the 1 per cent level.

In terms of education outcomes, women exposed to the policy generally have a higher likelihood of completing all levels of schooling. The largest gap is observed at the senior high level, where exposed women are 6.4 ppt more likely to complete it than unexposed women. By contrast, the smallest gap appears at the junior level, where completion rates differ by exactly 1 ppt. On average, exposed women complete 1.2 more years of schooling than their unexposed counterparts.

Table 1
Summary statistics by exposure to MMA policy

Variables	**Unexposed** *(16–24 years at MMA)* Mean	SD	**Exposed** *(7–15 years at MMA)* Mean	SD	**Mean Difference**
Panel A. Child marriage outcomes					
First marriage under 16	0.127	0.333	0.099	0.298	0.028***
Age at marriage	20.044	4.702	20.701	4.930	-0.657***
Panel B. Education outcomes					
Junior high school only	0.083	0.276	0.094	0.291	-0.010***
Senior high school only	0.075	0.263	0.139	0.346	-0.064***
Higher education	0.045	0.208	0.078	0.268	-0.033***
Years of education	5.454	4.085	6.626	4.606	-1.171***
Number of observations	131,634		208,754		

Notes: This table includes all women in the restricted sample (born 1951–1967 with non-missing dates of birth), including those in the anticipation doughnut. Observations for *Age at marriage* are 128,906 (unexposed) and 203,991 (exposed), since the variable is defined only for ever-married women. Indicators for school completion categories are mutually exclusive. Significance levels: * p<0.1, ** p<0.05, *** p<0.01.

4 Empirical Approach

To estimate the causal impact of Indonesia's 1975 minimum marriage age reform, we employ a regression discontinuity design that compares women just below and just above the legal cut-off of 16 years on 1 October 1975. A valid RD approach, however, requires that assignment around the cut-off is as good as random and that outcomes change smoothly with the running variable. In our setting, two issues may threaten this identification strategy. First, families may have altered marriage timing in anticipation of the reform during the interval between its announcement and enforcement. Second, reported birth information in our data shows non-random heaping, which can create compositional differences across cohorts and lead to biased RD estimates. In what follows, we first examine these threats to identification and provide checks to assess their relevance, before turning to the empirical specification of our RD model, where we also describe how these threats are addressed.

4.1 Threats to Identification

4.1.1 Potential Anticipation effects

One potential threat to identification is the anticipation of the MMA, whereby behavioural changes may have arisen during the long announcement period of the policy. Indonesia's first marriage law was promulgated in January 1974 but did not become enforceable until 1st October 1975. The long delay between the passage of the law and its effective application may have prompted some families to accelerate marriage plans for daughters under the age of 16 in order to avoid the complications of marrying under the new law. The possibility of such anticipatory responses means that cohorts just below the cut-off may not provide a valid counterfactual for those just above it, raising the risk of bias in a standard RD design. We account for this risk in our identification strategy, as discussed in Section 4.2.

4.1.2 Non-random Heaping in the reporting of the Running Variable

A further threat to identification is non-random heaping in the reporting of the running variable. One of the key identifying assumptions of RD is that outcomes vary smoothly along the running variable so that treatment assignment near the cut-off is as good as random. Barreca et al. (2016) show that non-random heaping in the reporting of the running variable can violate this assumption by introducing composition bias, whereby individuals in heaped cohorts differ systematically from those in neighbouring cohorts in characteristics that are also correlated with the outcomes. In such cases, RD estimates may be biased as they capture not only the causal effect of treatment but also differences in composition between heaped and non-heaped cohorts. This concern is especially salient in this study because several primary outcomes are indicators of child marriage, which may also be related to reported birth information. Addressing non-random heaping is therefore essential to preserve the validity of the continuity assumption and keep our RD estimates unbiased.

We conduct a series of checks to assess whether non-random heaping could bias our RD estimates. First, we visually inspect the distribution of observations along the running variable. Figure A1 plots the number of women by year of birth between 1949 and 1971 across Susenas survey years. Plotting separately by survey year shows that the heaping pattern is not confined to a particular survey round, and that the problem lies in the reporting of year of birth rather than in age statements.[5] Visual inspection reveals clear spikes in cohorts born in years ending in 0, with smaller excess mass in those ending in 5, accompanied by dips in years ending in 1 and 9. This pattern is consistent with classic digit-preference behaviour documented in demographic surveys, which is the overstatement of digits ending—from most to least preferred—0, 5, 2, and 8, and understatement of digits flanking the most preferred digits, such as 1 and 9 (Siegel and Swanson, 2004). Second, we formally assess digit preference for 0 and 5 using Myers' blended index, calculated from year-of-birth data for cohorts 1951–1970.[6] The index compares the observed frequency of the last digit of year of birth with the expected uniform distribution, where a perfectly even pattern would allocate 10 per cent to each digit (Siegel and Swanson, 2004). Table A1 reports the share of observations for each last digit and its deviation from the 10% benchmark, yielding a Myers' index value of 4.33.[7] Relative to the 10% benchmark, digits ending in 0 are most overrepresented, followed by those ending in 5, while adjacent digits 1 and 6 are most underrepresented. These results reinforce the visual spikes in Figure A1 and provide formal evidence of systematic digit preference in reported year of birth.

Third, we apply the McCrary (2008) density test using the same bandwidth and specification as in the main model (see Section 4.2). The test is widely used in RD applications to assess whether the distribution of the running variable is continuous at the cut-off. Although the McCrary test was not originally developed to identify heaping, it can nevertheless capture discontinuities in the running variable due to non-random heaping, especially if the heaping coincides with the treatment threshold (Barreca et al.,

[5]Across all Susenas survey rounds, the question on date of birth was asked prior to the question on age. In addition, the questionnaire guidelines included a tabulation for surveyors to estimate ages conditional on birth month and year, ensuring that the observed heaping reflects misstatement of year of birth rather than age.

[6]We extend the calculation window beyond our main sample because the Myers' index requires a range of years in multiples of ten.

[7]The index ranges from 0, indicating no heaping, to 90, where all observations are concentrated on a single digit.

2016). The McCrary density test strongly rejects the null of a smooth running variable distribution at the cut-off (p-value<0.01), which in our setting is consistent with the presence of systematic heaping in reported years of birth rather than strategic manipulation of the running variable. Lastly, we examine whether cohorts born in heaped years are structurally different from nearby cohorts. Table A2 reports a simple global comparison of outcomes between women born in heaped versus non-heaped years within our sample (1951–1967). The estimates indicate that those born in years ending in 0 or 5 have significantly lower educational attainment, are more likely to have never attended school, and are less likely to reside in urban areas, which may also correlate to their higher likelihood of being married as a child. To further assess whether these differences reflect local discontinuities rather than broad cohort trends, we follow Barreca et al. (2016) in conducting local heap diagnostics. Figure A2 plots the estimated jumps in observable characteristics at each heap year within the sample (1955, 1960, 1965), relative to surrounding non-heaped years within a two-year bandwidth.[8] In all cases, the estimated jumps are statistically significant, indicating that women born in heaped years differ systematically in observable characteristics from those born in adjacent non-heaped years. Taken together, the four detection strategies provide consistent evidence of non-random heaping in the running variable. Individuals reporting birth years ending in 0 or 5 differ systematically from their adjacent cohorts, violating the continuity assumption required for RD identification. To address this concern, we implement a doughnut regression discontinuity design that excludes observations born in heaped years, as detailed in Section 4.2.

We detected non-random heaping in reported dates of birth, with disproportionate clustering on 1 January, 1 July, and 31 December. To ensure smoothness in the running variables, we redistributed excess observations on these dates randomly across other days within the same birth year. The procedure and supporting evidence are described in Appendix A.1.

[8]We use a two-year bandwidth to ensure that the estimation windows remain fully within the 1951–1967 analytic sample.

4.2 Identification Strategy

The minimum marriage age (MMA) policy in Indonesia provides a natural experiment for studying the impact of legal restrictions on child marriage. The implementing regulation that took effect on 1 October 1975 prevented women younger than 16 from having legal marriages without a court-granted dispensation. A regression discontinuity (RD) design can therefore be used to estimate the intention-to-treat (ITT) effect of the MMA policy by comparing outcomes for girls who were just below the age of 16 at the time of implementation (exposed to the policy) with those just above the cut-off (unexposed). The RD setup is as follows:

$$Y_i = \beta_0 + \beta_1 \text{Exposed}_i + \beta_2 (A_i - 16) + \beta_3 \big(\text{Exposed}_i \times (A_i - 16)\big) + \beta_4 X_i + u_i \quad (1)$$

where Y_i denotes the outcome of interest for woman i. Exposed_i is a binary indicator equal to 1 if woman i was younger than 16 years on 1 October 1975, and 0 otherwise. $(A_i - 16)$ is the running variable, defined as age on 1 October 1975 centred at the cut-off of 16 years and measured in weeks. u_i is the error term. The vector X_i includes pre-determined covariates measured before marriage. These are month-of-birth, birth-region, and survey-year fixed effects. Month-of-birth fixed effects are included to control for differences in outcomes for people born in different months (seasonality).[9] Regional indicators are included because patterns of child marriage vary considerably across Indonesia's regions and provinces (Jones and Gubhaju, 2008). We include five broad birth-region indicators (Sumatera, Kalimantan, Sulawesi, Bali & Nusa Tenggara, and Maluku & Papua), with Java, the most developed region, serving as the baseline. Survey-year fixed effects are added to absorb the potential unobservable trend in the data over the year that may be correlated with the outcomes.

The analysis is conducted under an ITT framework, as exposure to the MMA policy did not fully determine marriage behaviour. The regulation did not eliminate marriage

[9] For instance, Buckles and Hungerman (2013) discovered that differences in later outcomes are influenced by maternal characteristics that influence preferences for sex for certain seasons/weather, seasonal preference in wantedness of birth, and preferences for expected conditions at the time of birth.

under 16, as parents could still obtain a court dispensation for a legal union, and marriages could also occur outside the state system under religious or customary law. Nevertheless, the reform made legal marriage under 16 substantially more difficult and costly, while signalling that marriage for girls should take place only from age 16 onwards (Katz and Katz, 1978). The RD estimates should therefore be understood as capturing the causal effect of being subject to the reformed legal environment, rather than the effect of abolishing child marriage altogether. In this sense, the results represent a reduced-form impact of the policy, reflecting both the immediate behavioural response to stricter legal restrictions and the broader influence of the law as a signal of acceptable marriage age.

As explained in Section 4.1, analysing the impact of Indonesia's 1975 MMA policy may face challenges arising from the potential anticipation effects and the heaping in the running variable. For this reason, we apply a doughnut-RD approach to draw causal effects of the MMA policy. To account for the potential anticipation effects, we exclude observations aged 15 to just under 16 years old at MMA. We assume that women closest to the cut-off were more likely to anticipate the law implementation, while the likelihood may decline as the cohorts get younger (e.g., a 15-year woman were more likely to rush her marriage to avoid the hassle as she was more ready into the marriage compared to a 12-year woman who were biologically less ready to be married and to have a partner for marriage). However, as there is no certain cut-off at which age this anticipation effect would stop, we also check whether the results are sensitive to extending the anticipation doughnut hole to match the 21-month period between the announcement and the implementing law.

The presence of heaping in year of birth motivates us to exclude observations with years ending in 0 and 5. This follows the approach of Barreca et al. (2016), who demonstrate that non-random heaping in the running variable can bias RD estimates not only when heaps occur in close proximity to the cut-off but also when they appear elsewhere within the bandwidth. They therefore recommend excluding all observations at heap points within the estimation window regardless of their distance from the cut-off to produce unbiased results. Consistent with this guidance, we remove all cohorts born in years ending in 0 and 5 within the bandwidth in both our main and robustness analyses.

The preferred specification of the model uses uniform kernel and a linear function of the running variable. The first-order polynomial is adopted to avoid the issue of overfitting and inconsistency around the cut-off of the high-order polynomials (Cattaneo and Titiunik, 2022). However, we also check the robustness of our findings to using triangular kernel and a quadratic running variable. Due to the discrete nature of our running variable, we follow Kolesár and Rothe (2018) in using Eicker–Huber–White (EHW) heteroskedasticity-robust standard errors, as clustering by the running variable (CRV) has been shown to perform poorly in such settings.

The baseline specification is estimated using a 42-month bandwidth around the cut-off. Due to the anticipation doughnut hole that excludes women who were 15 to under 16 years old at MMA, the 42-month span extends above the cut-off for the unexposed group and begins after the anticipation doughnut for the exposed group, which helps ensure that both sides of the cut-off contain comparable numbers of observations once the doughnut cohorts are excluded. This particular arbitrary bandwidth size is selected for two reasons. First, data-driven selectors, such as the the mean squared error (MSE) optimal procedure, often yield bandwidth sizes that can vary substantially across outcomes and are sensitive to the exclusion of doughnut observations. Therefore, applying a common arbitrary window provides a consistent basis for comparison across outcomes. Second, among alternative arbitrary windows, 42 months provides a pragmatic balance between bias and precision. As shown in Appendix Figure B1, widening the window beyond 42 months does not materially reduce confidence intervals and instead risks including heaping cohorts such as those born in 1955 or 1965, whereas narrower windows produce less precise estimates with more volatile coefficients.

5 Results

5.1 Graphical Analysis

Before reporting the regression results of equation 1, we first examine the visual evidence on the discontinuities on child marriage and educational attainment around the 16-year-old cut-off of the 1975 MMA policy. Figure 2 depicts the mean of child marriage and education outcomes by age at the time of the MMA policy (in monthly bins). The vertical red dashed line marks the minimum marriage age cut-off at 16, with cohorts to the left (younger at the time of the policy) exposed to the reform and those to the right (older) unexposed. Grey dots indicate observations excluded under the anticipation doughnut (ages 15 to under 16 at MMA) and the heaping doughnut (years of birth ending in 0 or 5). For child marriage outcomes, panels (a) and (b) suggest a discontinuity at the minimum legal marriage age for first marriage under 16 and age at marriage. For education outcomes, we focus on showing the visual patterns for senior high school and higher-education attainment, since completion of these levels falls above the minimum marriage age cut-off and is therefore more likely to reveal policy effects. Panel (d) suggests a possible discontinuity at the cut-off for higher-education attainment, whereas Panel (c) shows little evidence of a sharp change in the share of senior high school graduates. We formally examine these visual patterns in the regression analysis presented in the following subsections.

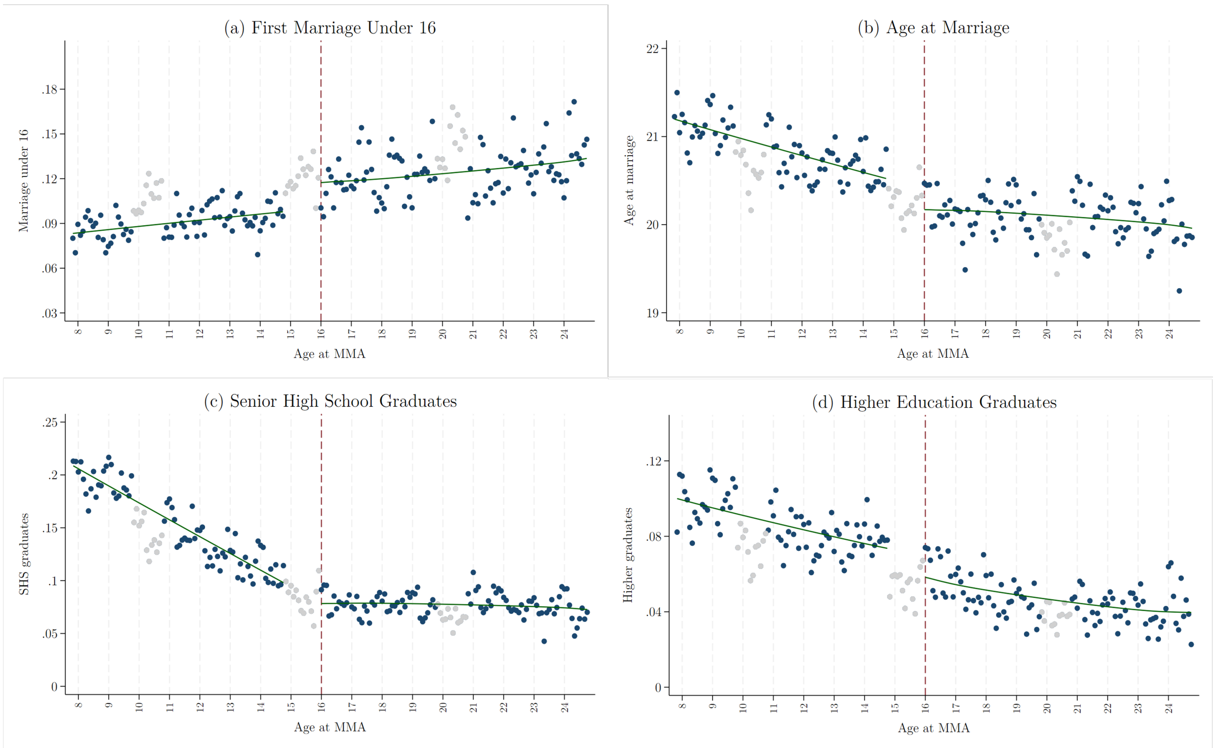
5.2 The Impact of MMA Policy on Child Marriage

Table 2 presents the effect of the minimum marriage age policy on the likelihood of marrying before 16 and on mean age at marriage. Consistent with the visual evidence presented earlier, the estimates indicate that exposure to the MMA reform reduced child marriages and raised average marriage age. Column (1) shows that girls who were younger than 16 at the time of the MMA reform were 2.2 percentage points (ppt) less likely to marry before turning 16. Relative to the pre-reform mean of 12.2%, this represents an 18% reduction. Column (2) indicates that exposure to the reform increased the mean age at first marriage by 0.44 years, or about five months. Given a pre-reform mean of 20.1 years, this corresponds to an increase of roughly 2.2%.

The contrast between Columns (1) and (2) reflects the fact that the policy directly targeted marriages below the legal minimum, while the overall effect on the average age at

Figure 2

Outcome against running variable: Women aged 8-24 at MMA



Notes: This figure plots the means of outcome variables against the age at the time of MMA policy implementation (in monthly bins). The vertical red dashed line marks the reform cut-off, corresponding to 16 years of age on 1 October 1975. Grey dots indicate observations within the anticipation doughnut (ages 15 to under 16 at MMA) and the heaping doughnut (years of birth ending in 0 or 5). The fitted lines are estimated while excluding these doughnuts, using a 42-month bandwidth and a uniform kernel.

marriage was more modest. By preventing marriages at ages younger than 16, the reform produced a sizeable reduction in the probability of marrying under 16. The magnitude of this effect, a decline of about 18% relative to the pre-reform mean, is comparable to the historical decline reported by Jones (2011), who noted that the share of girls marrying below 16 fell from around 20% in 1974 to about 16% in 1984. Since most women in the sample would have married well above the 16-year cut-off, around age 20, the overall increase in mean age at first marriage was limited. Although the estimated rise of 0.44 years may appear modest in absolute terms, it is sizeable when viewed against long run trends. Historical estimates indicate that the singulate[10] mean age at marriage rose by only about three years over four decades, from 19.3 in 1970 to 22.2 in 2010 (Jones, 2017).

[10] The singulate mean age at marriage is derived from the proportion single in each age group (15–54) to estimate the average age at which people enter marriage (Jones, 2017).

Against this backdrop, the 1975 reform accounts for roughly 15% of the total increase in marriage age that otherwise unfolded only gradually over four decades.

These results are robust to a wide range of alternative specifications. The estimates remain significant when applying narrower and wider bandwidth windows (see Figure B1). In Table B1, we also check further whether our main model is robust across various specifications. To account for potential non-linearity in the relationship between the running variable and the outcomes, we estimate models with a second-order polynomial, and the results remain significant (Panel (a)). Since we detected mass points in the running variable, we also check the sensitivity of the estimates to using a triangular kernel, and the results remain significant with similar magnitudes (Panel (b)). As the period between the announcement of the reform in January 1974 and its implementation in October 1975 spanned 21 months, we check the sensitivity of the results by extending the anticipation doughnut to cover this full window. The estimates remain significant (Panel (c)), although the magnitudes are smaller, potentially due to the exclusion of observations close to the cut-off that were most directly affected by the reform, which weakens the treatment contrast and reduces statistical power. Finally, since we use an imputation method to correct heaping in some dates of birth, namely 1 January, 1 July, and 31 December, we check whether the models are sensitive to using reported dates of birth. The results are very similar to those of the main specification and remain statistically significant (Panel (d)). A placebo test using a hypothetical reform in 1969 in Table B3 show that the effects are much smaller than the main effects and statistically insignificant.

5.3 MMA Policy Impacts on Proportion Married Below Specific Ages

Figure 3 shows the estimated effects of the 1975 MMA policy on the probability of marrying below different age thresholds. Each point estimate represents the change in the likelihood of being married below 10 to 35 as a result of the reform. The horizontal bars

Table 2
The effects of MMA policy on child marriage

	First marriage under 16 (1)	Age at marriage (2)
Exposure to MMA policy	-0.022***	0.439***
	(0.004)	(0.071)
Pre-reform mean	0.122	20.109
Observations	138,270	135,284

Notes: This table presents regression discontinuity estimates from models using a linear function of the running variable, a uniform kernel, and a bandwidth of 42 months before the cut-off for the untreated (older) group and after the anticipation doughnut for the treated (younger) group. Observations within the anticipation doughnuts (15 to under 16 at MMA) and the heaping years (years or birth ending 0 and 5) inside the bandwidth are excluded from the estimation model. Control variables include survey year, month of birth, and region of birth. The symbols ***, **, and * denote statistical significance at the 1, 5, and 10 per cent levels, respectively. Robust standard errors are in parentheses.

denote 90% confidence intervals and the red dash line shows the minimum marriage age of 16 years old set by the policy. The pattern reveals that the policy not only reduced the probability of marriage under 16, the explicit legal target, but also delayed marriage into later ages. The strongest impacts are concentrated at the age of 18 and 22, with around 4 ppt reduction (see Table A4). These two ages coincide with key educational milestones in Indonesia, with most girls completing senior high school around age 18 and undergraduate degree around age 22. The results therefore suggest that the policy may have contributed to postponing marriage until after these stages of schooling were completed, which is consistent with the estimated impacts on educational attainment observed in Table 3.

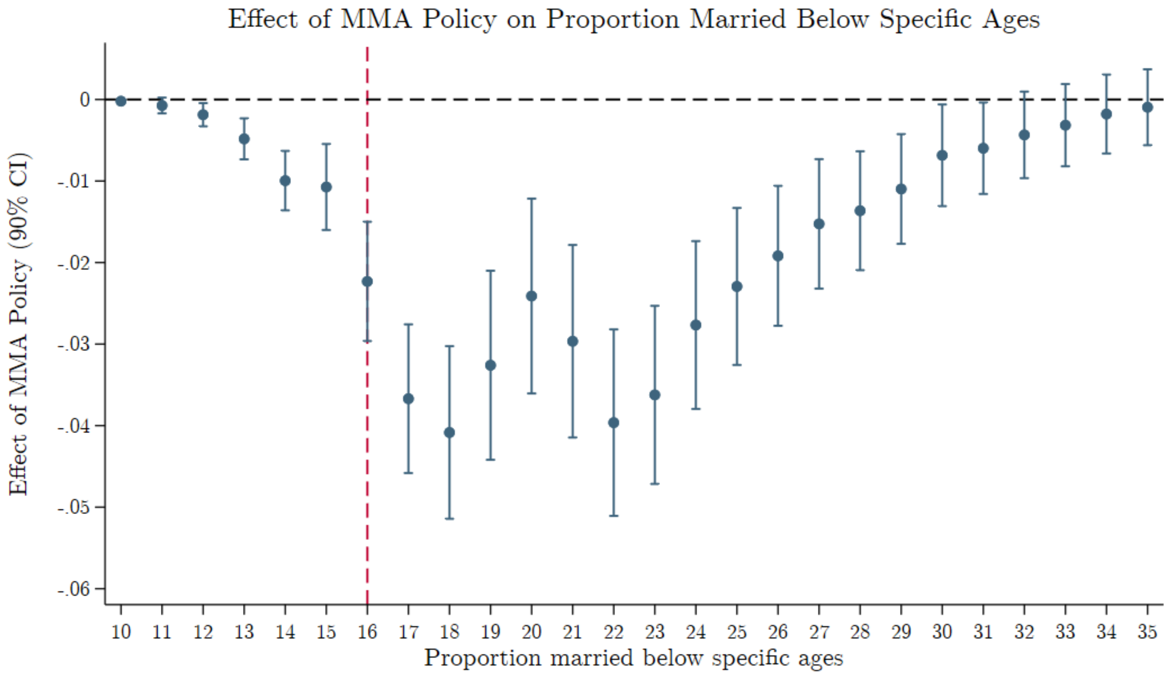
At thresholds below the legal minimum, the effects are smaller as the age cut-off gets lower, which is in line with the lower probability of getting married under the younger age. For instance, the probability of getting married under 10 years old among the sample is near zero (0.006%), so that the estimated effect is also very small. For the cut-off getting closer to the minimum age, such as 14 and 15, the effects become more pronounced, indicating that the reform also reduced teen marriages just short of the legal boundary. Beyond age 22, the policy's effects gradually diminish until converge to zero by marriage under 32. While the reform shifted the timing of marriage to ages above the intended threshold, it did not seem to alter the overall probability of marriage. This is further confirmed by near-zero and insignificant effect on the overall rate of ever being married in Table A3.

Overall, Figure 3 demonstrates that the 1975 MMA policy had broader effects than its initial target of reducing marriages under 16. This suggests that the policy not only imposed a legal restriction but also acted as a social signal, encouraging later marriage as a more desirable norm. The strongest impacts occur at ages 18 and 22, which coincide with the completion of senior secondary school and a bachelor's degree. This suggests that parents may have delayed their daughters' marriages until after these milestones rather than interrupting their education. This pattern further indicates that the MMA policy may have reinforced the role of education as a viable alternative to early marriage. This interpretation is consistent with the gains in educational attainment documented in Table 3, where exposure to the reform is associated with higher completion rates at both the secondary and tertiary levels.

5.4 Estimated Effect on Educational Attainment

Table 3 presents the estimated effects of the minimum marriage age reform on educational attainment. We classify outcomes into three mutually exclusive categories: junior high, senior high, and higher education. Based on the Indonesian school system, the estimated ages at completion are 15 years for junior high, 18 years for senior high, and 22 years for higher education. In addition, we use total years of education as a discrete measure that captures both completed and uncompleted schooling levels, ranging from 0 to 23 years of education. The estimates suggest improvements at upper ends of the schooling distribution, with increases in the probability of completing senior high school and higher education. Column (1) indicates that exposure to the MMA reform increased the likelihood of completing senior high school by 0.8 ppt, but this is only significant at the 10% level. Column (3) points to a 1.5 ppt rise in higher education completion, which represents a substantial increase of about 29% relative to the pre-reform mean. In contrast, there is

Figure 3
The effect of 1975 MMA policy on proportion married below specific ages



Notes: This figure plots the coefficients and 90% confidence intervals from the regression-discontinuity estimates. All model specifications use linear model, uniform kernel, a bandwidth of 42 months before the cut-off for the untreated (older) group and after the anticipation doughnut for the treated (younger) group, and control variables of survey year, month of birth, and region of birth. Observations within the anticipation doughnuts (15 to under 16 at MMA) and the heaping years (years or birth ending 0 and 5) inside the bandwidth are excluded from the estimation models. The vertical red dashed line marks the reform cut-off, corresponding to marriage under 16 years old.

no meaningful change at the junior high level (column 3). Finally, as shown in column 6, average years of schooling increased by about 0.23 years, or roughly 4% of the pre-reform mean, consistent with gains at the upper end of the schooling distribution.

To assess the robustness of the education estimates, we applied the same checks as for the child marriage outcomes. Table B2 presents the robustness checks across changes in the model specification. Using second-degree polynomials (Panel (a)) and triangular kernels (Panel (b)) yields results broadly in line with the baseline, though statistical significance is weaker in the polynomial case. The results are quite sensitive to the extension of the anticipation doughnut to 21 months (Panel (c)), as the effects on most education outcomes have weaker power or even losing significances, while one on junior secondary became significant with negative signs. However, this volatility may be partly attributable to the loss of statistical power from excluding a larger set of observations close to the cut-off. The effects on junior and senior high school attainment are also sensitive to using reported dates of births (Panel (d)). Estimation results using narrower and wider sizes of bandwidth, as seen in Figure B1, shows that the results on junior high and senior high school graduates are sensitive to the selection of observation windows. By contrast, the increase in higher education completion appear more stable across both alternative specifications and bandwidths. The gains in years of education are generally positive but attenuate with wider bandwidths.

The placebo analysis using a false 1969 cut-off in Table B4 shows no significant effect on most education outcomes. The exception is junior high school completion, which displays a positive and significant effect. We are less certain about this significant result on junior high school graduates, and it is possible that other educational policies happening around 1969 drove this significant outcome, such as the implementation of 1968 curriculum. [11] The overall robustness checks show that the most stable effects appear at the upper ends of the schooling distribution, namely increasing higher education completion. By contrast, the impacts on junior and senior high school attainment are more volatile.

For completeness, we also reports the coefficients for the remaining attainment categories, namely less than primary education and primary education attainment, in Appendix A5. The results show no significant change at the primary level, but a significant reduction in the probability of completing less than primary. We interpret this coefficient with caution. A plausible pathway is that, by constraining marriage under 16, the reform allowed additional time for adolescents just below the legal threshold to re-enter schooling via Indonesia's non-formal system. Programs such as *Paket A* (primary equivalency) enabled school leavers to obtain a certificate, with some potentially progressing into further levels (Dilts, 1982). In this setting, some who would otherwise have remained below primary may have shifted into primary or beyond, reducing the share of never finishing primary without a corresponding rise at the primary level when part of this shift continued to higher levels.

[11] The 1968 curriculum by the New Order replaced the Old Order curriculum of 1965. It focused not only in increasing primary graduates (as opposed to the previous 1965 curriculum) but also in encouraging the junior school graduates to continue to high school education and above (Simatupang et al., 2019). The new curriculum also intended to increase the number of vocational school students, therefore introducing vocational subjects particularly to junior school students (Forman, 1977). This may have contributed to the higher rate of junior school graduates after the 1969 placebo policy.

The shift in the education distribution can be seen as the policy influencing outcomes at the highest ends of attainment. Most girls would already have completed lower levels of schooling before reaching this age, so the policy was unlikely to influence those outcomes. By contrast, the cut-off lies within the senior high school years and before the entry into higher education, making it plausible that the reform affected attainment at these stages. The pattern is consistent with the idea that when early marriage is made more difficult, education emerges as a credible outside option. With marriage under 16 no longer an easy option, the law reduced parents' incentive to withdraw daughters from school early, making continued education the default path. While results at the very bottom of the distribution may partly reflect re-entry through non-formal pathways, the most robust effects are clearly observed at the upper end. As girls remained in school longer, parents may have gradually come to recognise the benefits of continued education. Advancing through higher levels of schooling became more widely accepted as both a source of pride and a pathway to future income (see Section 2), which reduced the appeal of early marriage. This helps explain why the policy shifted more girls into senior and tertiary attainment.

Table 3

The effects of MMA policy on education completion

	Junior high school graduates (1)	Senior high school graduates (2)	Higher education graduates (3)	Years of education (4)
Exposure to real MMA policy in 1975	-0.006	0.008*	0.015***	0.229***
	(0.004)	(0.005)	(0.004)	(0.066)
Pre-reform mean	0.087	0.076	0.051	5.634
Observations	138,270	138,270	138,270	138,270

Notes: This table presents regression discontinuity estimates from models using a linear function of the running variable, a uniform kernel, and a bandwidth of 42 months before the cut-off for the untreated (older) group and after the anticipation doughnut for the treated (younger) group. Observations within the anticipation doughnuts (15 to under 16 at MMA) and the heaping years (years of birth ending in 0 and 5) inside the bandwidth are excluded from the estimation model. Control variables include survey year, month of birth, and region of birth. The symbols ***, **, and * denote statistical significance at the 1, 5, and 10 per cent levels, respectively. Robust standard errors are in parentheses.

5.5 Heterogeneous Analysis

Previous results have shown that Indonesia's first MMA policy significantly reduced child marriage, increased age at marriage, and may have also affected educational attainment. This section investigates whether the impact of being exposed to the MMA cut-off at 16 years varied across subgroups, namely by urban residence and by areas with entrenched child-marriage traditions. For each case, we estimated a saturated interaction model by interacting the subgroup indicator with the binary policy exposure variable.

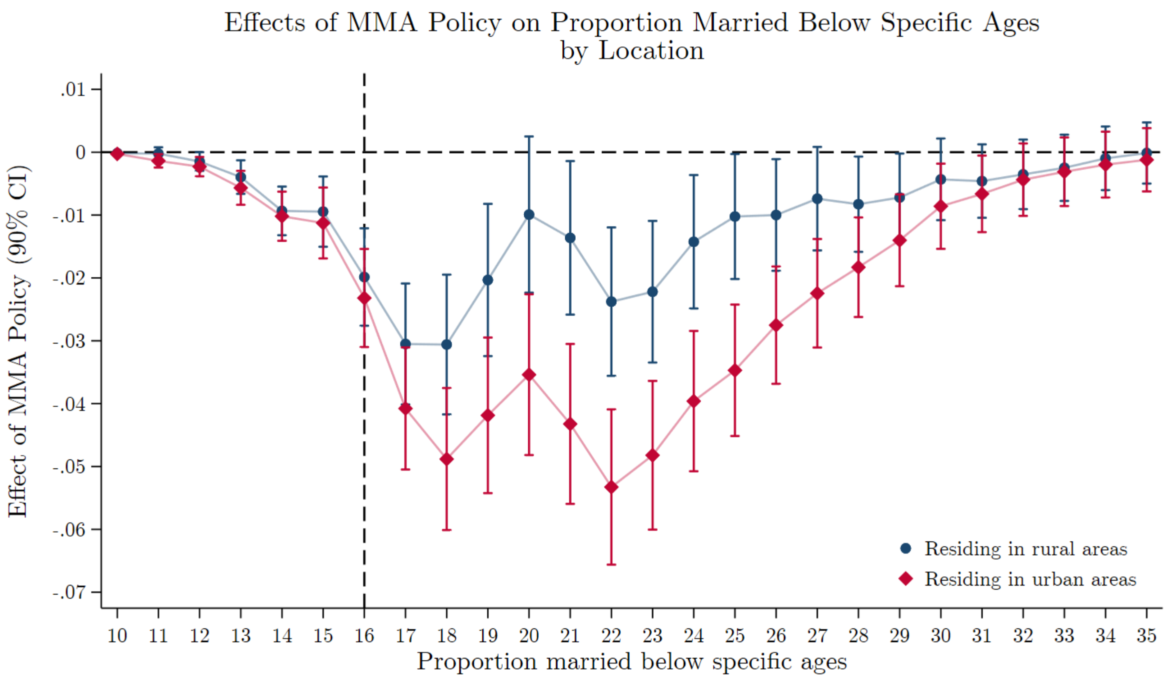
5.5.1 Heterogeneity by urbanicity

Based on Jones and Gubhaju (2011), who documented distinct marriage patterns between rural and urban Indonesia, we examine heterogeneous policy effects across these two settings. Since location of birth is not available, we use current residence as a proxy for urban or rural origin. Figure 4 presents regression discontinuity estimates of child marriage under different age thresholds for women residing in urban and rural areas. The vertical dashed line represents the 16-year-old minimum marriage age, while the red and blue data points show estimates and 90% confidence intervals for urban and rural areas, respectively. The figure shows that the policy effects are broadly similar across urban and rural groups up to the legal cut-off, but diverge thereafter. Above age 16, the effects are consistently larger for urban women and statistically significant until around age 31, compared to age 25 for rural women. The sharpest reductions in urban areas occur around the age of transition to higher education, whereas in rural areas they are concentrated at senior high school ages. Overall, the results suggest that the policy reinforced a tendency toward later marriage well beyond the intended threshold in urban settings, while in rural areas its effects were more immediate and short-lived. This pattern is consistent with the generally lower prevalence of child marriage and higher baseline age at marriage in urban settings (see Panel (a) of Table A6).

The stronger effects of the policy in urban areas can be explained by several factors. Women's movements and reformist debates about delaying marriage were more prominent in urban centres, making women more exposed to the idea and more receptive to the law's

Figure 4

Heterogenous Effects by Location



Notes: This figure plots the coefficients and 90% confidence intervals from the regression-discontinuity estimates. All model specifications use linear model, uniform kernel, a bandwidth of 42 months before the cut-off for the untreated (older) group and after the anticipation doughnut for the treated (younger) group, and control variables of survey year, month of birth, and region of birth. Observations within the anticipation doughnuts (15 to under 16 at MMA) and the heaping years (years or birth ending 0 and 5) inside the bandwidth are excluded from the estimation models.

new minimum age (Blackburn and Bessell, 1997). Information campaigns were also more effectively disseminated in cities, where media access and state presence were greater, and enforcement was generally stronger given closer proximity to higher levels of government. In addition, urban women had better access to education than their rural counterparts, which not only provided socially legitimate alternatives to marriage but also helped spread more individualised ideas of marriage as a form of self-fulfilment rather than a family obligation (Malhotra, 1997). In contrast, traditional views of marriage as a family duty and strong parental authority that encourages early unions were more entrenched in rural areas, making them more resistant to reforms aimed at raising the marriage age (Jones and Gubhaju, 2011; Malhotra, 1997).

The education results in Appendix Table A7 (Panel (a)) reinforce this view. In urban areas, the policy raised completion of senior secondary (around age 18) and higher education (around age 22), while in rural areas the estimates are small and not statistically significant. This suggests that a policy designed to reduce marriage under 16 also encouraged education to serve as a viable alternative to early marriage in urban settings, whereas in rural settings the effect was more limited. These contrasts are in line with Figure 4, where urban women postpone marriage well into later ages while rural women delay only slightly beyond the cut-off.

5.5.2 Heterogeneity by strength of child-marriage tradition

The effect of the MMA reform may differ by the strength of child-marriage norms. To examine this, we divide women into two groups; those with strong child-marriage traditions and those without. Javanese and Maduranese women had strong social norms favouring early marriage and the lowest ages at marriage for cohorts born in the 1950s–1960s (Jones, 2001). In the absence of direct ethnicity information, we proxy these groups by women born in West Java province and Madura Island.[12] Figure 5 presents regression discontinuity estimates of child marriage under different age thresholds and estimated years of

[12] West Java is defined to include all districts in the province at the time of interview, excluding the suburbanising zones surrounding Jakarta (Tangerang, Bekasi, Bogor, and Depok). Banten province is also included, as it was part of West Java until 2000 and shares similar language and traditions. Madura Island is proxied by Bangkalan, Sampang, Pamekasan, and Sumenep districts.

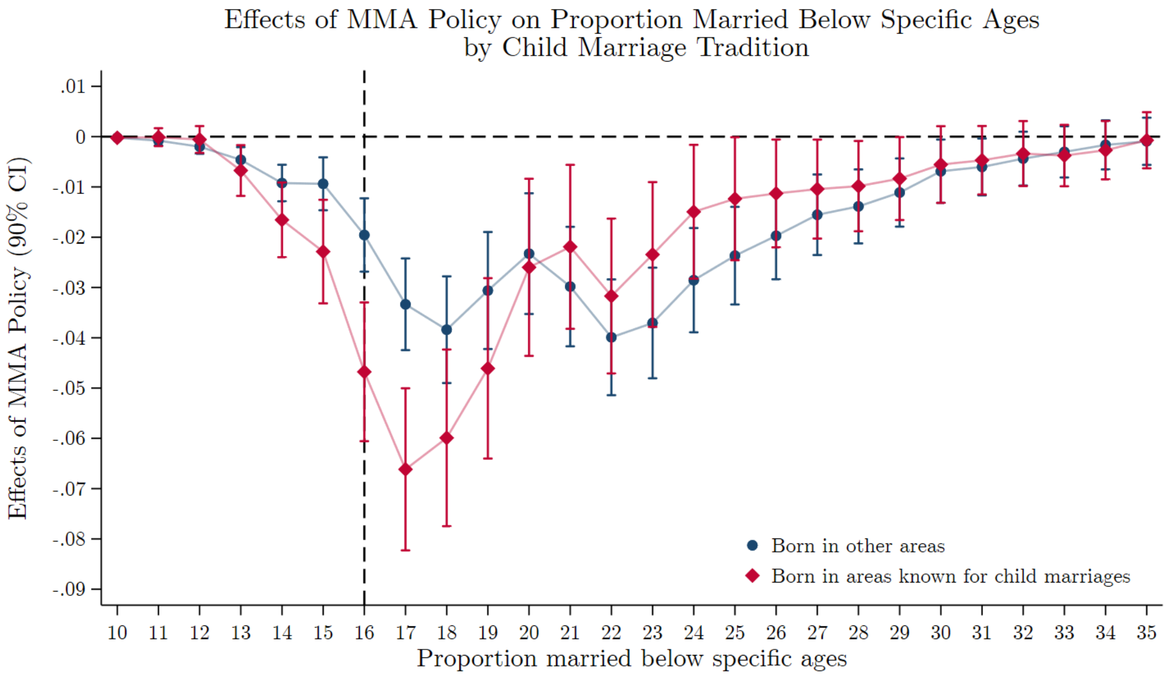
schooling for women born in these and other areas.

Figure 5 presents regression discontinuity estimates of child marriage under different age thresholds for women born in these and other areas. The effects of the MMA policy were sharper around the legal cut-off for women born in areas known for child-marriage practices (red line) than for those born elsewhere (blue line). The reductions are most pronounced between the thresholds of 14 and 18, centred on the legal minimum age of 16. However, the effects for the child-marriage group taper off more quickly and become not significant by around age 25. By contrast, in other areas the effects are smaller in size near the cut-off but remain evident until around age 29. This pattern is consistent with the results in Panel (b) of Table A6, showing that women born in traditional child-marriage regions were significantly more likely to marry before age 16 and at a younger average age than those from other regions. This suggests that the MMA policy achieved its main aim of preventing child marriages around the legal threshold in areas where the practice was most entrenched, while its influence extended to later ages in other areas.

Entrenched norms of early marriage and strong parental authority in West Java and Madura (Jones, 2001), likely explain both the sharper reductions at younger ages and the quicker fading of effects. With child marriage far more prevalent in these regions, the 1975 law directly constrained a larger share of marriages that would otherwise have occurred below age 16, leading to a stronger immediate drop. However, these same norms also pushed women into marriage soon after the legal minimum age , causing the effects to diminish more quickly than in other areas of Indonesia.

The education results in Appendix Table A7 are consistent with this pattern. Outside the tradition areas, the reform increased senior secondary and higher-education completion, while in the tradition areas the estimates are small and not significant. This suggests that where norms were weaker, education offered a credible alternative to early marriage, whereas in tradition areas the impact was more limited. These contrasts align with Figure 5, which shows longer delays outside the tradition regions but only short extensions beyond the cut-off within them.

Figure 5
Heterogenous Effects by Child Marriage Tradition



Notes: This figure plots the coefficients and 90% confidence intervals from the regression-discontinuity estimates. All model specifications use linear model, uniform kernel, a bandwidth of 42 months before the cut-off for the untreated (older) group and after the anticipation doughnut for the treated (younger) group, and control variables of survey year, month of birth, and region of birth. Observations within the anticipation doughnuts (15 to under 16 at MMA) and the heaping years (years or birth ending 0 and 5) inside the bandwidth are excluded from the estimation models.

6 Conclusion

Child marriage remains a persistent challenge in many developing countries, particularly for girls, with far-reaching consequences for human capital outcomes, such as women's educational attainment and the health of their children. MMA laws are among the most widely adopted policy responses, yet their effectiveness remains contested in the literature. This paper adds to this debate by providing new causal evidence from Indonesia, where the first MMA law was implemented in 1975, setting the minimum age of marriage for girls at 16 years.

This study draws on data from SUSENAS (2018–2021) and applies a regression discontinuity design that accounts for anticipation of the reform and non-random heaping in reported birth dates by adopting a doughnut-RD specification. The estimates indicate that the MMA policy reduced the likelihood of girls under the age of 16 marrying by about 18%. Importantly, the influence of the reform extended beyond its legal minimum, with delays in marriage observed up to age 30, with particularly large impacts found at ages 18 and 22, which correspond to the typical ages of high school and higher education completion. Our analysis of educational attainment indicates that education was a natural alternative pathway to early marriage for girls following the reform. We find that the MMA reform increased the probability of completing a higher education degree. Heterogeneous impacts reveal stronger effects in urban areas and in regions where child marriage had been more culturally entrenched.

Despite strong resistance from conservative Muslim groups in the 1970s who argued that puberty should define marriageability (Blackburn and Bessell, 1997), Indonesia's first minimum marriage age law nonetheless had real effects. The findings of this paper show that, even in this constrained environment, the policy had a measurable impact by delaying marriage both below and beyond the legal threshold. Several factors likely contributed to its success. Grassroots communication by rural officials and private organisations helped spread awareness and encourage compliance, while the national family planning body actively promoted the law as later marriage aligned with its broader fertility-reduction goals (Katz and Katz, 1978).

Nevertheless, the scale of the impacts was limited. The 1975 law was more trend-following than trend-setting, as women's organisations had already been calling for higher ages of marriage and broader social trends pointed toward later unions even before the law was enacted (Katz and Katz, 1978). In this sense, the policy largely formalised and accelerated an ongoing shift rather than creating an entirely new one. Moreover, obtaining a dispensation was often possible in practice since the law did not clearly specify the grounds for approval. At the same time, unions outside the legal system, such as religious or customary marriages, still persisted. These factors help to explain why the estimated effects are modest.

In the decades that followed, the first Indonesia's MMA policy created room for gradual shifts in social norms and further legal change. What began as a politically contested reform, with allowances for dispensations and uneven enforcement, nonetheless marked an important step in shifting expectations around early marriage. This progress eventually culminated in the 2019 amendment (Law no. 16/2019), which raised the minimum age for girls to 19 in line with boys, tightened the dispensation process, and aligned Indonesia's legal definition of child marriage with global standards. The progression from a contested and imperfect reform to gender-equal legislation shows how even limited legal changes can influence behaviour and lay the foundation for stronger commitments over time.

This study is subject to several limitations. First, the SUSENAS data define first marriage broadly, including both legally registered and unregistered unions such as religious or customary marriages. If the policy primarily constrained registered unions, then including unregistered marriages in the analysis would dilute the measured impact, implying that our estimates are more likely attenuated than exaggerated. Second, recall bias may affect reports of age and age at marriage, particularly among older respondents. However, as long as such errors are not systematically correlated with the reform cut-off, they should mainly introduce noise that reduces precision rather than producing spurious discontinuities. A third limitation concerns potential mortality selection. Because the unexposed cohorts are slightly older at the time of interview, women who married very young could be underrepresented among them if early marriage was linked to higher mortality. However, the RD relies on narrow bandwidths around the cut-off, so the mortality gap between exposed and unexposed women is therefore likely to be small and, if present, would attenuate rather than exaggerate the estimated effects. Finally, while we address non-random heaping of birth years through a doughnut RD that excludes affected cohorts, some under-representation remains in adjacent years with unknown true distributions. However, any residual mismeasurement is more likely to reduce precision and attenuate the estimated effects toward zero rather than exaggerate them.

Future work could usefully expand this study in several directions. While our analysis focuses on marriage and education outcomes, further work could investigate the broader consequences of delayed marriage for fertility, women's labour market outcomes, and the development of their offspring, where data availability allows. Linking large-scale surveys with administrative data would also help to clarify mechanisms, including the role of dispensations, unregistered unions, and local enforcement practices. Finally, it would be valuable to compare the effects of Indonesia's 1975 reform with those of the 2019 amendment, recognising that the latter was introduced in a very different context of rapid educational expansion and shifting gender norms.

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Appendices

A Supplementary Tables and Figures

Figure A1
The number of observations with stated year of birth between 1949–1971, Susenas 2018–2021

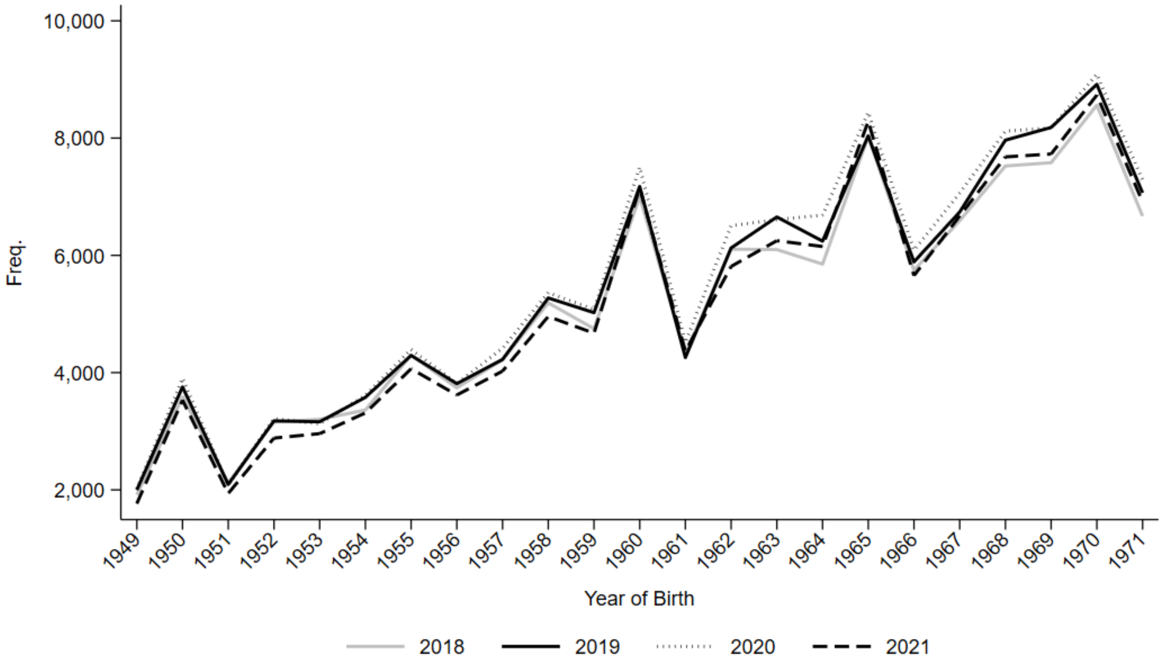


Table A1
Distribution of last digits of year of birth (1951–1970 female cohorts)

Last digit	Freq.	%	Deviation from 10 (%)
0	641,240	12.07	2.07
1	434,269	8.18	-1.82
2	556,472	10.48	0.48
3	531,487	10.01	0.01
4	503,610	9.48	-0.52
5	604,795	11.39	1.39
6	462,506	8.71	-1.29
7	495,244	9.32	-0.68
8	551,564	10.39	0.39
9	529,792	9.98	-0.02

Table A2

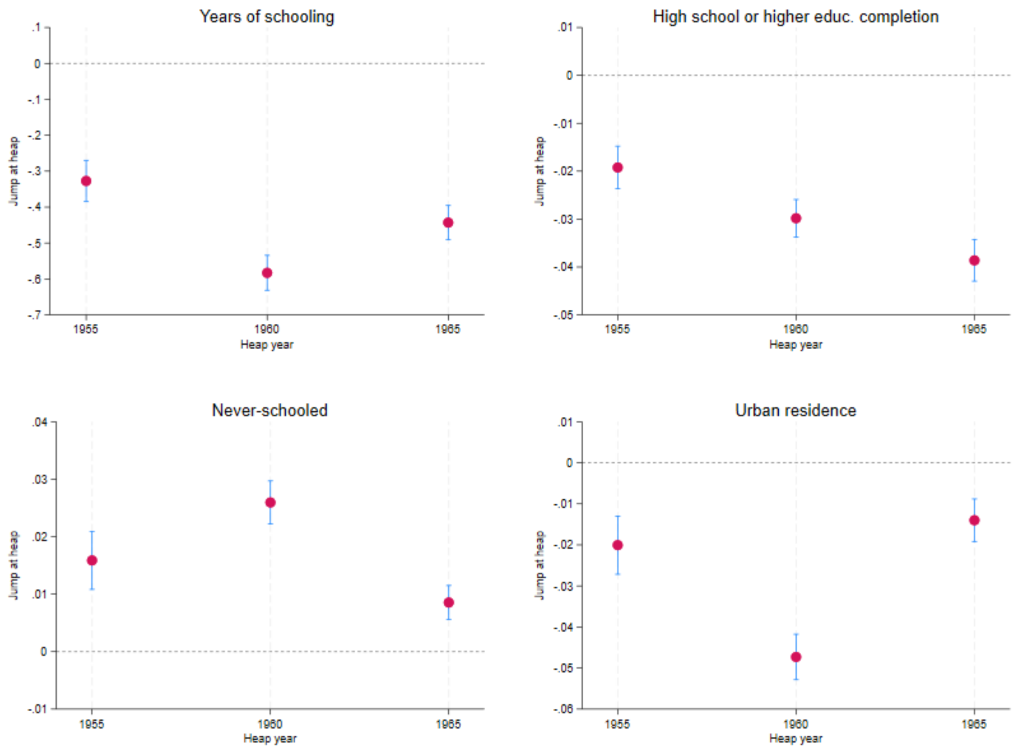
Relationship between heaping in year of birth (ending in 0 and 5) and observable characteristics

	Years of schooling	Never-schooled	At least completing high school	Urban residence
Year of birth ending 0 and 5	-0.375*** (0.018)	0.013*** (0.001)	-0.025*** (0.002)	-0.027*** (0.002)

Notes: This table presents the global regression estimates of the association between year of birth heaping (years ending in 0 and 5) and observable characteristics. Robust standard errors are in parentheses. The symbols ***, **, and * denote significance at the 1, 5, and 10 per cent levels, respectively.

Figure A2

Jumps in observables at heap years



Notes: Points show estimated discontinuities ("jump at heap") at years 1955, 1960, and 1965 following Barreca et al. (2016). Vertical lines are 90% confidence intervals.

Table A3

The effects of MMA policy on the probability of ever married

	Ever married
Exposure to MMA policy	-0.000
	(0.002)
Observations	138,270

Notes: This table presents regression discontinuity estimates from models using a linear function of the running variable, a uniform kernel, and a bandwidth of 42 months before the cut-off for the untreated (older) group and after the anticipation doughnut for the treated (younger) group. Observations within the anticipation doughnuts (15 to under 16 at MMA) and the heaping years (years or birth ending 0 and 5) inside the bandwidth are excluded from the estimation model. Control variables include survey year, month of birth, and region of birth. The symbols ***, **, and * denote statistical significance at the 1, 5, and 10 per cent levels, respectively. Robust standard errors are in parentheses.

Table A4
The effects of MMA policy on the proportion married below specific ages

Age	Exposure to MMA policy		Pre-reform mean
	Coeff.	SE	
10	-0.000**	(0.000)	0.000
11	-0.001	(0.001)	0.001
12	-0.002**	(0.001)	0.004
13	-0.005***	(0.002)	0.013
14	-0.010***	(0.002)	0.028
15	-0.011***	(0.003)	0.058
16	-0.022***	(0.004)	0.122
17	-0.037***	(0.006)	0.201
18	-0.041***	(0.006)	0.297
19	-0.033***	(0.007)	0.409
20	-0.024***	(0.007)	0.495
21	-0.030***	(0.007)	0.626
22	-0.040***	(0.007)	0.690
23	-0.036***	(0.007)	0.744
24	-0.028***	(0.006)	0.790
25	-0.023***	(0.006)	0.826
26	-0.019***	(0.005)	0.871
27	-0.015***	(0.005)	0.892
28	-0.014***	(0.004)	0.911
29	-0.011***	(0.004)	0.926
30	-0.007*	(0.004)	0.937
31	-0.006*	(0.003)	0.949
32	-0.004	(0.003)	0.955
33	-0.003	(0.003)	0.960
34	-0.002	(0.003)	0.963
35	-0.001	(0.003)	0.966

Notes: The number of observations is 138,270. This table presents regression discontinuity estimates from models using a linear function of the running variable, a uniform kernel, and a bandwidth of 42 months before the cut-off for the untreated (older) group and after the anticipation doughnut for the treated (younger) group. Observations within the anticipation doughnuts (15 to under 16 at MMA) and the heaping years (years of birth ending in 0 and 5) inside the bandwidth are excluded from the estimation model. Control variables include survey year, month of birth, and region of birth. The symbols ***, **, and * denote statistical significance at the 1, 5, and 10 per cent levels, respectively. Robust standard errors are in parentheses.

Table A5
MMA policy and lower-education completion

	Less than primary	Primary school graduates
	(1)	(2)
Exposure to real MMA policy in 1975	-0.025***	0.008
	(0.007)	(0.007)
Pre-reform mean	0.458	0.328
Observations	138,270	138,270

Notes: This table presents regression discontinuity estimates from models using a linear function of the running variable, a uniform kernel, and a bandwidth of 42 months before the cut-off for the untreated (older) group and after the anticipation doughnut for the treated (younger) group. Observations within the anticipation doughnuts (15 to under 16 at MMA) and the heaping years (years of birth ending in 0 and 5) inside the bandwidth are excluded from the estimation model. Control variables include survey year, month of birth, and region of birth. The symbols ***, **, and * denote statistical significance at the 1, 5, and 10 per cent levels, respectively. Robust standard errors are in parentheses.

Table A6
Heterogeneous effects on child marriage outcomes

	First marriage under 16 years old (1)	Age at first marriage (2)
(a) By location		
Exposure to MMA policy × living in an urban area	-0.023*** (0.005)	0.581*** (0.076)
Exposure to MMA policy × living in a rural area	-0.020*** (0.005)	0.271*** (0.073)
Living in an urban area	-0.044*** (0.003)	1.124*** (0.037)
Observations	138,270	135,284
(b) By strength of child-marriage tradition		
Exposure to MMA policy × born in the areas	-0.047*** (0.008)	0.456*** (0.098)
Exposure to MMA policy × born in other areas	-0.020*** (0.004)	0.430*** (0.071)
Born in areas known for practising child marriages	0.078*** (0.006)	-1.067*** (0.057)
Observations	138,270	135,284

Notes: This table presents regression discontinuity estimates from models using a linear function of the running variable, a uniform kernel, and a bandwidth of 42 months before the cut-off for the untreated (older) group and after the anticipation doughnut for the treated (younger) group. Observations within the anticipation doughnuts (15 to under 16 at MMA) and the heaping years (years of birth ending in 0 and 5) inside the bandwidth are excluded from the estimation model. Control variables include survey year, month of birth, and region of birth. The symbols ***, **, and * denote statistical significance at the 1, 5, and 10 per cent levels, respectively. Robust standard errors are in parentheses.

Table A7
Heterogeneous effects on education outcomes

	Junior high school graduates (1)	Senior high school graduates (2)	Higher education graduates (3)	Years of education (4)
(a) By location				
Exposure to MMA policy × living in an urban area	-0.016***	0.016***	0.031***	0.377***
	(0.005)	(0.005)	(0.004)	(0.070)
Exposure to MMA policy × living in a rural area	-0.001	-0.003	0.000	0.005
	(0.004)	(0.004)	(0.004)	(0.065)
Living in an urban area	0.068***	0.112***	0.065***	2.523***
	(0.002)	(0.002)	(0.002)	(0.032)
Observations	138,270	138,270	138,270	138,270
(b) By whether born in areas known for traditionally practising child marriages				
Exposure to MMA policy × born in the areas	0.001	-0.001	0.009*	0.073
	(0.006)	(0.006)	(0.005)	(0.096)
Exposure to MMA policy × born in other areas	-0.007*	0.009*	0.016***	0.242***
	(0.004)	(0.005)	(0.004)	(0.067)
Born in areas known for practising child marriages	-0.028***	-0.010***	-0.004	-0.252***
	(0.003)	(0.003)	(0.003)	(0.055)
Observations	138,270	138,270	138,270	138,270

Notes: This table presents regression discontinuity estimates from models using a linear function of the running variable, a uniform kernel, and a bandwidth of 42 months before the cut-off for the untreated (older) group and after the anticipation doughnut for the treated (younger) group. Observations within the anticipation doughnuts (15 to under 16 at MMA) and the heaping years (years of birth ending in 0 and 5) inside the bandwidth are excluded from the estimation model. Control variables include survey year, month of birth, and region of birth. The symbols ***, **, and * denote statistical significance at the 1, 5, and 10 per cent levels, respectively. Robust standard errors are in parentheses.

A.1 Heaping in certain dates of birth

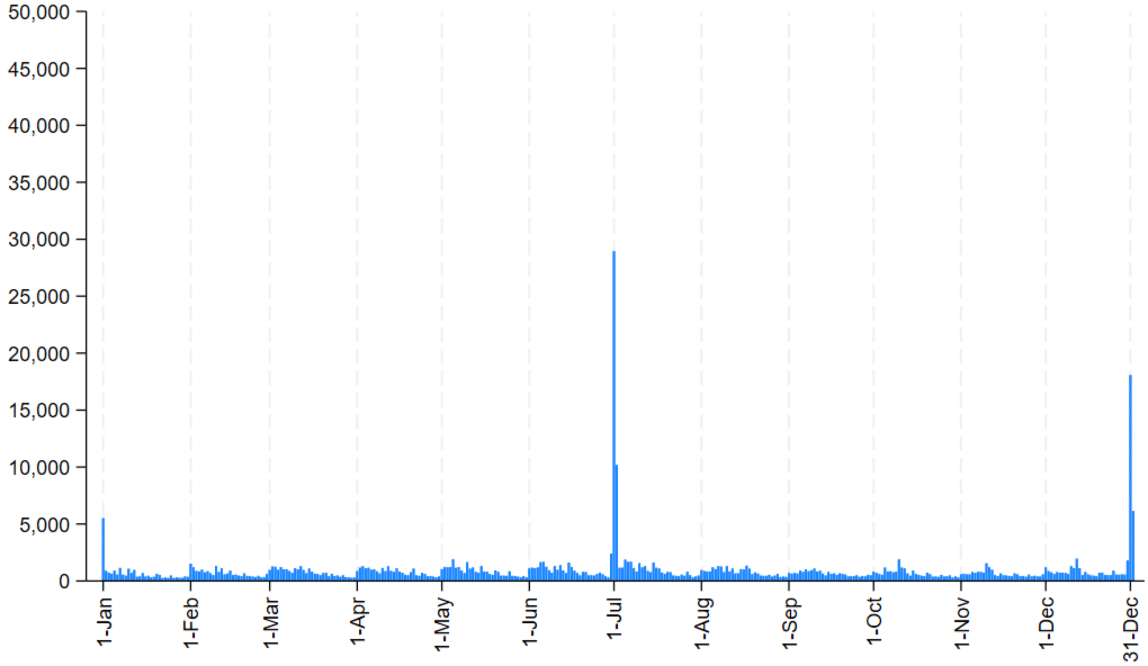
In our data, reported dates of birth are not uniformly distributed across the calendar year. Instead, certain days appear with disproportionate frequency, reflecting non-random heaping. This type of heaping is a common feature in survey and administrative records where exact birth dates may be unknown or substituted with convenient defaults. Figure A3 illustrates this pattern for females born between 1951 and 1967. Three dates, namely 1 January, 1 July, and 31 December, stand out with strikingly high frequencies, accounting for 1.62%, 11.00%, and 6.93% of all sample, respectively.

In the Indonesian context, such patterns largely reflect civil registration practices. From the early 1970s until 2004, individuals with unknown birthdays were commonly assigned "31 December." Since 2004, the default has been "1 July," a practice later formalised under Minister of Home Affairs Regulation No. 19/2010. While there is no official policy for "1 January," it is plausible that parents preferred to record an easy-to-remember date such as New Year. This aligns with previous findings that parents often choose symbolic or memorable dates for their children's birthdays, either due to administrative convenience in earlier decades or, more recently, through medical scheduling (Pelham et al., 2021).

To correct for this heaping and ensure smoothness in the running variables, we randomly reassigned excess cases from each heap date across other days within the same year. For example, if the number of observations on 1 January exceeded the expected average for that year, the surplus was redistributed evenly across days 2 January and 31 December, excluding the other two heaped dates (e.g., excluding 1 July and 31 December). A similar procedure was applied for 1 July and 31 December. Figure A4 illustrates the distribution before and after correction, showing that the shares at these dates converge towards the expected daily probability of $1/365 \approx 0.27\%$.

Figure A3

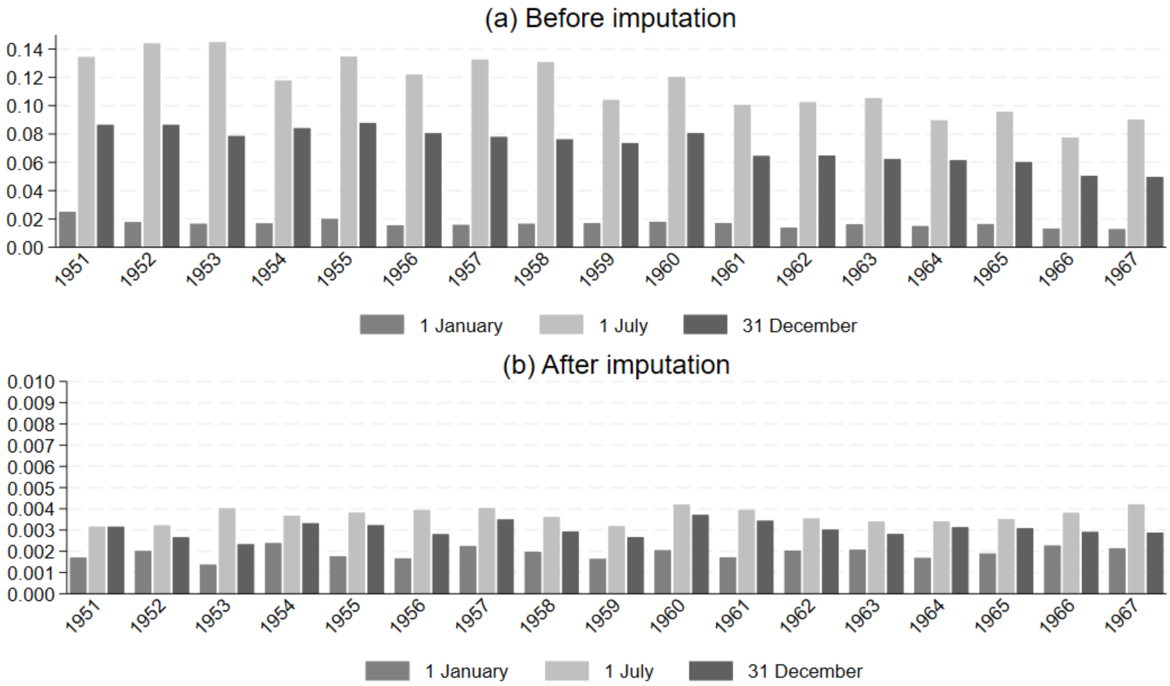
The number of observations by stated dates of birth, 1951–1967



Notes: This figure plots the frequency of reported dates of birth for females in the sample (born between 1951-1967). The three largest spikes correspond to 1 January, 1 July, and 31 December, reflecting preferences for certain dates of birth.

Figure A4

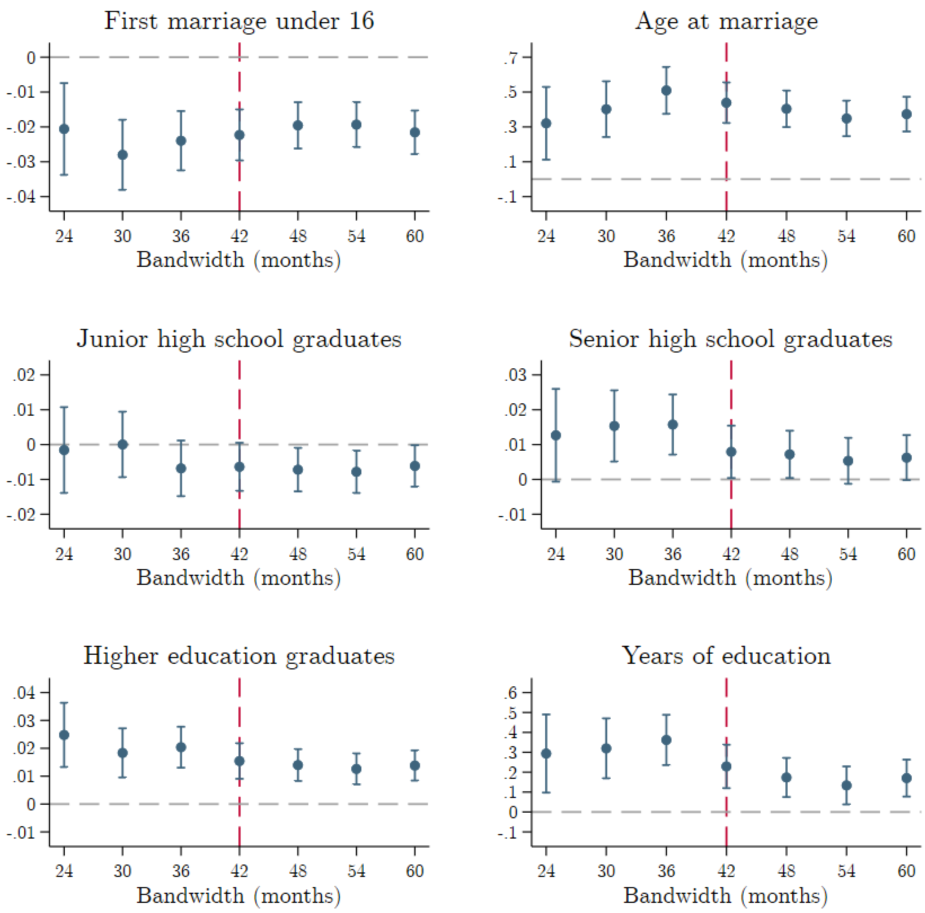
The proportion of certain dates of birth per year before (a) and after (b) redistribution



Notes: The observations show the yearly proportions of reported dates of birth on 1 January, 1 July, and 31 December from females born between 1951 and 1967. Panels (a) and (b) show such proportions before and after redistribution, respectively.

B Robustness Checks

Figure B1
RD estimates using various bandwidth sizes



Notes: This figure plots the coefficients and 90% confidence intervals from the regression-discontinuity estimates. Bandwidths of 24, 30, 36, 42, 48, 54, and 60 months before the cut-off for the untreated (older) group and after the anticipation doughnut for the treated (younger) group are used. The red dashed line shows the preferred bandwidth size in the main analysis. All model specifications use linear model, uniform kernel, and control variables of survey year, month of birth, and region of birth. Observations within the anticipation doughnuts (15 to under 16 at MMA) and the heaping years (years or birth ending 0 and 5) inside the bandwidth are excluded from the estimation models.

Table B1
Robustness of RD estimates for child marriage outcomes

	First marriage under 16 (1)	Age at first marriage (2)
(a) Second-degree polynomial		
Exposure to MMA policy	-0.023** (0.010)	0.361** (0.156)
Observations	138,270	135,284
(b) Triangular Kernel		
Exposure to MMA policy	-0.022*** (0.005)	0.417*** (0.080)
Observations	137,104	134,148
(c) Extended anticipation doughnut to 21 months		
Exposure to MMA policy	-0.018*** (0.005)	0.288*** (0.074)
Observations	147,472	144,279
(d) Using stated dates of birth		
Exposure to MMA policy	-0.021*** (0.004)	0.417*** (0.071)
Observations	137,473	134,529

Notes: This table presents regression discontinuity estimates from models using alternative specifications that are modified from the baseline model. Observations within the anticipation doughnuts (15 to under 16 at MMA) and the heaping years (years of birth ending in 0 and 5) inside the bandwidth are excluded from the estimation model. Control variables include survey year, month of birth, and region of birth. The symbols ***, **, and * denote statistical significance at the 1, 5, and 10 per cent levels, respectively. Robust standard errors are in parentheses.

Table B2
Robustness of RD estimates for education outcomes

	Junior high school graduates (1)	Senior high school graduates (2)	Higher education graduates (3)	Years of education (4)
(a) Second-degree polynomial				
Exposure to MMA policy	0.000 (0.009)	0.018* (0.010)	0.017** (0.008)	0.330** (0.145)
Observations	138,270	138,270	138,270	138,270
(b) Triangular Kernel				
Exposure to MMA policy	-0.005 (0.005)	0.011** (0.005)	0.015*** (0.004)	0.240*** (0.074)
Observations	137,104	137,104	137,104	137,104
(c) Extended anticipation doughnut to 21 months				
Exposure to MMA policy	-0.009** (0.004)	0.000 (0.005)	0.007* (0.004)	0.022 (0.069)
Observations	147,472	147,472	147,472	147,472
(d) Using stated dates of birth				
Exposure to MMA policy	-0.008* (0.004)	0.007 (0.005)	0.014*** (0.004)	0.195*** (0.067)
Observations	137,473	137,473	137,473	137,473

Notes: This table presents regression discontinuity estimates from models using alternative specifications that are modified from the baseline model. Observations within the anticipation doughnuts (15 to under 16 at MMA) and the heaping years (years of birth ending in 0 and 5) inside the bandwidth are excluded from the estimation model. Control variables include survey year, month of birth, and region of birth. The symbols ***, **, and * denote statistical significance at the 1, 5, and 10 per cent levels, respectively. Robust standard errors are in parentheses.

B.1 Placebo Analysis

In this section, we perform a placebo exercise to test whether the estimated effects of the 1975 minimum marriage age policy are genuine. Specifically, we use 1 January 1969 as a placebo cut-off, six years before the policy's implementation, and examine whether any discontinuities are observed. To ensure that the placebo analysis does not include observations within the bandwidth of the main analysis and does not extend beyond the sample range of women born between 1951 and 1967, we use a placebo policy in 1969 and using 30-month bandwidth before the cut-off for the untreated (older) group and after the anticipation doughnut for the treated (younger) group. For comparability, the main results using a 30-month bandwidth are also presented. Table B3 and Table B4 report the placebo estimates (Panel (a)) together with the main policy estimates (Panel (b)) using 30-month bandwidth.

Table B3

Placebo and main policy effects on child marriage (30-month bandwidth)

	First marriage under 16 (1)	Age at first marriage (2)
(a) Placebo MMA 1969 with 30-month bandwidth		
Exposure to MMA policy	-0.009 (0.010)	0.018 (0.142)
Observations	50,508	49,478
(b) Main policy 1975 with 30-month bandwidth		
Exposure to MMA policy	-0.028*** (0.006)	0.402*** (0.097)
Observations	97,613	95,491

Notes: This table presents regression discontinuity estimates from models using a linear function of the running variable, a uniform kernel, and a bandwidth of 30 months before the cut-off for the untreated (older) group and after the anticipation doughnut for the treated (younger) group. Observations within the anticipation doughnuts (15 to under 16 at MMA) and the heaping years (years or birth ending 0 and 5) inside the bandwidth are excluded from the estimation model. Control variables include survey year, month of birth, and region of birth. The symbols ***, **, and * denote statistical significance at the 1, 5, and 10 per cent levels, respectively. Robust standard errors are in parentheses.

Table B4

Placebo and main policy effects on education completion (30-month bandwidth)

	Junior high school graduates (1)	Senior high school graduates (2)	Higher education graduates (3)	Years of education (4)
(a) Placebo MMA 1969 with 30-month bandwidth				
Exposure to MMA policy	0.026*** (0.009)	0.005 (0.009)	-0.001 (0.006)	0.175 (0.125)
Observations	50,508	50,508	50,508	50,508
(b) Main policy 1975 with 30-month bandwidth				
Exposure to MMA policy	0.000 (0.006)	0.015** (0.006)	0.018*** (0.005)	0.320*** (0.091)
Observations	97,613	97,613	97,613	97,613

Notes: This table presents regression discontinuity estimates from models using a linear function of the running variable, a uniform kernel, and a bandwidth of 30 months before the cut-off for the untreated (older) group and after the anticipation doughnut for the treated (younger) group. Observations within the anticipation doughnuts (15 to under 16 at MMA) and the heaping years (years of birth ending in 0 and 5) inside the bandwidth are excluded from the estimation model. Control variables include survey year, month of birth, and region of birth. The symbols ***, **, and * denote statistical significance at the 1, 5, and 10 per cent levels, respectively. Robust standard errors are in parentheses.