# Do Age-of-Marriage Laws Work? Evidence from a Large Sample of Developing Countries

**Matthew Collin and Theodore Talbot**

## Abstract

Child marriage is associated with bad outcomes for women and girls. Although many countries have raised the legal age of marriage to deter this practice, the incidence of early marriage remains stubbornly high. We develop a simple model to explain how enforcing minimum age-of-marriage laws creates differences in the share of women getting married at the legal cut-off. We formally test for these discontinuities using multiple rounds of the Demographic and Health Surveys (DHS) in over 60 countries by applying statistical tests derived from the regression discontinuity literature. By this measure, most countries are not enforcing the laws on their books and enforcement is not getting better over time. Separately, we demonstrate that various measures of age-of-marriage discontinuities are systematically related to with existing, widely-accepted measures of rule-of-law and government effectiveness. A key contribution is therefore a simple, tractable way to monitor legal enforcement using survey data. We conclude by arguing that better laws must be accompanied by better enforcement and monitoring in to delay marriage and protect the rights of women and girls.

Keywords: Child marriage, discontinuity tests, rule of law, legal effectiveness

JEL Codes: J12, K42

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# Contents

# 1 Introduction

Today, more than half a billion women were married before the age of eighteen(UNICEF 2014). Described as *early marriage* or *child marriage*, the practice is associated with worse outcomes for women and girls. Girls who get married later have higher levels of education, literacy, better health outcomes, and are less likely to experience domestic violence (Jensen and Thornton 2003; Ambrus and Field 2008; Hicks and Hicks 2015). And although the causal links are less clear, interventions that delay the age at which girls marry deliver a host of benefits (Baird et al. 2010; Baird et al. 2011; Heath and Mobarak 2015). More broadly, there is a global consensus that early and child marriage violates women's and girls' rights.

This paper proposes a new method of assessing a government's effectiveness in enforcing minimum-age-of-marriage laws. Using data from every available round of the Demographic and Health Surveys (DHS), we examine whether or not a country's observed distribution of marriage ages suggests that its government is enforcing laws against underage marriage to deter this behavior.

To do this, we set out a simple model developing intuition provided by Blank, Charles, and Sallee (2009) on how legal enforcement should cause changes in the distributions of ages at which women marry. For example, if the legal cut-off were 18 and this law were effectively enforced, we would expect to see a large 'jump' in the share of women married at 18, compared to those married at 17. Importantly, this is distinct from the *incidence* of early marriage, the share of women married below the legal cut-off as a share of total marriages. We check for such sharp, discontinuous increases in the proportion of women married at the legal age using both the empirical distribution drawn from survey data as well as a formal test of discontinuity developed by McCrary (2008).

The results show that enforcement varies significantly across countries. Using our strictest tests, only a small proportion of countries are enforcing early marriage rules at the margin (close to the legal age). This suggests that renewed efforts to outlaw child marriage may not deter the practice, even where the incidence of early marriage is declining. We also find that the introduction of new laws typically results in a drop in enforcement, suggesting that new laws are rarely complemented by effective enforcement.

In addition, we test whether this distribution-based measure of legal effectiveness is associated with the other, widely-accepted measures of rule-of-law. Countries that are 'better' at deterring early marriage (that is, have larger discontinuities in their distributions of ages of marriage at their legal cut-off) score higher on the Rule of Law and Government Effectiveness components of the World Bank's World Governance Indicators (WGI).

This paper seeks to make two contributions. First, it adds to a growing literature using the presence of 'kinks' in distributions to measure individual responses to (dis)incentives in areas such as tax compliance and labor supply (Saez 2010; Kleven and Waseem 2013;

Bastani and Selin 2014). Second, by using microeconomic data to investigate legal effectiveness, it also contributes to the literature on measures of rule-of-law, supplementing the majority of existing measures defined using expert surveys or other macro-level information.

The paper is structured as follows. Section 3 describes the simple model we use to illustrate how age-of-marriage regimes should affect marriage ages, Section 4 describes the data on marriage outcomes we use and the discontinuity tests we use to detect changes in behavior. Section 5 presents the main results from this exercise. Section 6 then tests whether these results meaningfully measure legal/government effectiveness in enforcing laws on the margin. We conclude with Section 7.

## 2 Global context

Recent survey data indicates that child marriage is declining across the developing world. Part (a) of Figure 1 shows that the proportion of female respondents in Demographic and Health Surveys (DHS) who report being married under the age of eighteen has fallen in most countries over the last 15 years. However, these positive changes are small compared to large cross-country differences in the incidence of child marriage, as well as increasing rates of *illegal marriage*, which we defines as being marriage under a country's minimum legal age.

The lack of progress on reducing child marriage has led policymakers, non-profit organizations and governments to search for effective ways to curb these practices. Many countries have responded by making the practice illegal: according to data provided by the Maternal and Child Health Equity (MACHEquity) project, nearly a quarter of surveyed countries increased their minimum age of marriage with parental consent to 18 or higher between 1995 and 2012. More than a third of countries have minimum-age laws that are set below the age of eighteen, potentially due to a lack of consensus on what a global minimum age should be. Though the 1964 *Convention on Consent to Marriage, Minimum Age for Marriage and Registration of Marriages* mandates that states should set and enforce minimum ages for marriage and enforce them, it does not specify what the minimum age should be. In recent years there have been calls for an outright global ban on the marriage of women under eighteen.[1]

Efforts to reduce the incidence of child marriage through the introduction of new laws may be hampered by the fact that many governments are incapable or unwilling to enforce legislation already on their books. For example, Cammack, Young, and Heaton (1996) find the imposition of a minimum age of marriage in Indonesia had little to no effect on the trend in early marriage rates. Blank, Charles, and Sallee (2009) present evidence that efforts to enforce a minimum-age-of marriage in mid-century US were less successful than official vital statistics suggested. While some studies do find a positive correlation between

[1]https://www.gov.uk/government/speeches/girl-summit-2014-david-camerons-speech

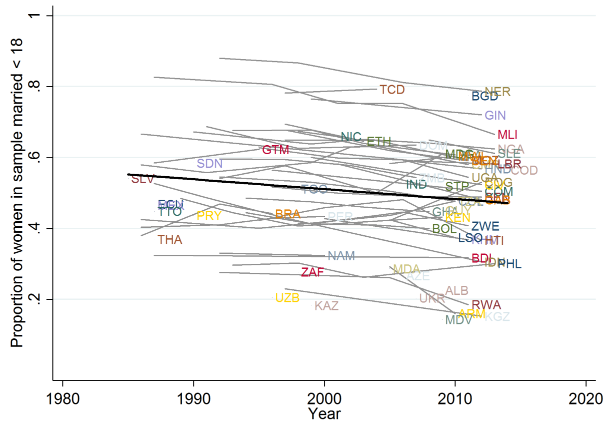
minimum age laws and the incidence of child marriage (Maswikwa, Richter, Kaufman, and Nandi 2015), the cross-sectional nature of this research often limits its ability to causally identify the impact of legislation. Despite the limited evidence around the effectiveness of laws, several studies show that more coherence and stricter enforcement around existing laws can lead to better outcomes for women (Dahl 2010; Bharadwaj 2015; Maswikwa, Richter, Kaufman, and Nandi 2015).

Across the globe, compliance with existing laws appears to be problematic. Parts (b) and (c) of Figure 1 display trends in the proportion of women who are married under the minimum legal age: part (b) uses the minimum legal age without parental consent as the relevant cut-off and part (c) uses the minimum legal age that a woman can be married with parental consent. The graphs reveal that illegal marriage is itself a significant problem for many countries, some of which have recently seen an increases in its incidence. Whether or not the rise of illegal marriage is partially due to higher minimum legal ages, it is clear that many governments are not following up better laws with stricter enforcement.

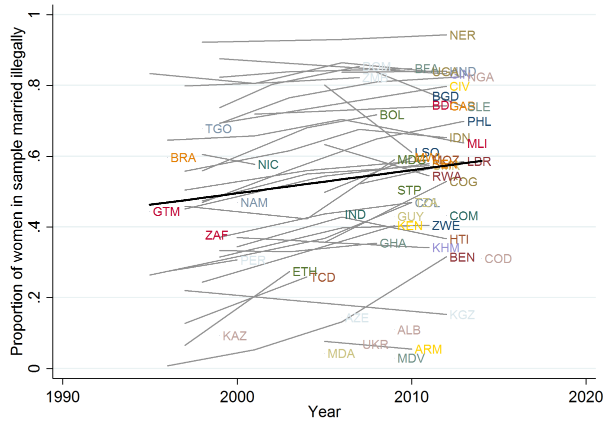
Figure 2 provides further stylised evidence that many governments are ineffective at policing the laws already in place. The chart shows the proportion of women married under the age of eighteen (child marriage) against the proportion of women married under the legal age in that country (illegal marriage). Countries above the forty-five degree line are those with minimum age laws set below the age of eighteen. These are countries for raising the legal age of marriage might seem an attractive way of fighting child marriage. However, consider the mass of countries that are on or below the forty-five degree line. These are countries who have minimum age of marriage laws that are at (on the 45 degree line) or above eighteen (those above the 45 degree line). Even for these countries, there is significant variation in the enforcement, with most having rates of illegal marriage above 40%. The high incidence of illegal marriage in many of these countries calls into question how successful governments would be at enforcing a universal ban, were one to come into force.

Figure 1: Trends in underage marriage over time

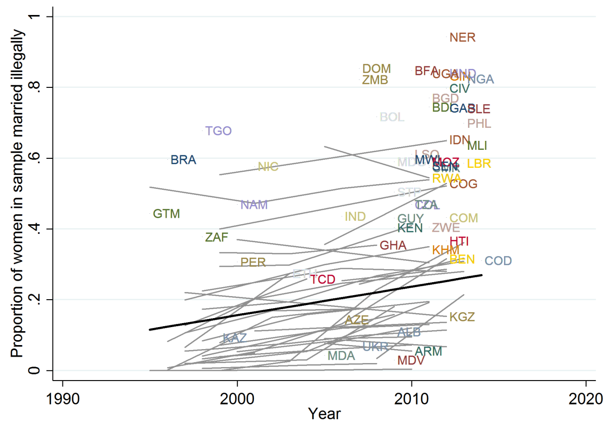
(a) Child marriage (under 18)




(b) Illegal marriage (unless parental consent was given)

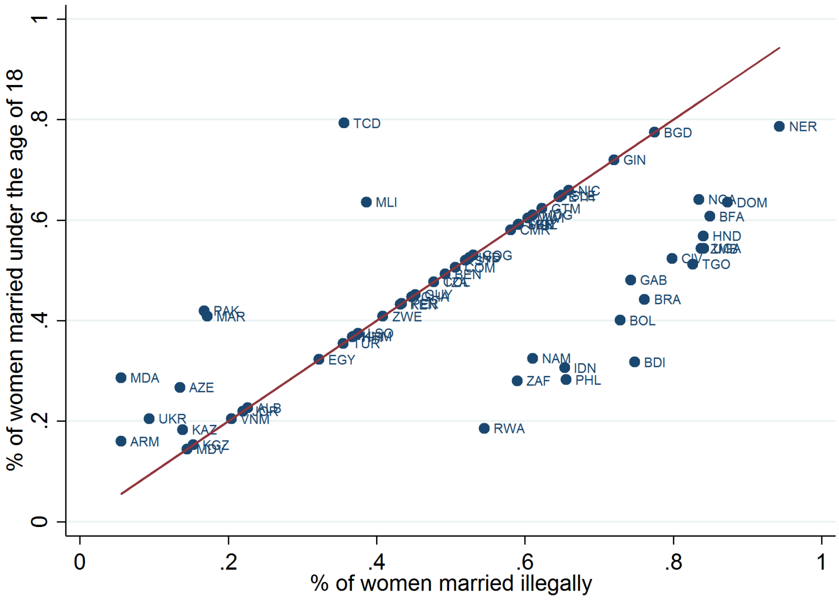



(c) Illegal marriage




**Note:** data taken from every available round of the Demographic and Health Surveys (DHS) and MACHEquity database. Black lines indicate average trend lines taken from averaging across countries with multiple rounds. Part (b) indicates the proportion of women married under the minimum legal age or marriage without parental consent. Part (c) indicates the proportion of women married the minimum legal age of marriage with parental consent.

Figure 2: Child marriage and illegal marriage




**Note:** data taken from most recent round of the Demographic and Health Surveys (DHS) for each country. Illegal marriage is calculated as the share of women married underneath the legal age of marriage without parental consent.

## 3 Conceptual framework

In this section we will formalize our intuition for how legal regimes might affect the distribution of marriage ages we observe in household surveys. An inspiration for our approach is (Blank, Charles, and Sallee 2009), who study enforcement age-of-marriage laws across the US during 1950s. The authors note that if laws are effective in convincing some *share* of the population to delay marriage until the legal age, then there should be a 'spike' in the number of marriages reported at the legal cut-off. They show that while these 'spikes' exist in administrative data on age-of-marriage but not in retrospective census data, suggesting that couples who wanted (or were compelled) to marry early found ways of ducking the law, possibly by lying about their ages or getting married in other states with lower age thresholds. While they test for equality of distributions between administrative and census data, the authors never formalize the argument underlying the existence of these discontinuities, nor apply formal statistical tests to confirm them.

To formalize this intuition, we start by defining a *marriage pair* as two people who wish to get married. Consider a set of marriage pairs indexed by $i$. Normalising the population of marriage pairs to one, we assume that each pair observes an exogenously-set preferred age of marriage (AOM), denoted $a^*$, drawn from a known distribution with a support of minimum and maximum preferred marriage age normalised to $(0, 1)$. The distribution of preferred age of marriage is characterised by some probability density function (PDF) $b(a^*)$, with $\int_0^1 b(a^*)da^* = 1$ as standard.[2] Marriage pairs choose their observed age of marriage, $a$ by maximising payoffs

$$\pi(a) = 1 - |a - a^*| \tag{1}$$

A *legal regime* $c(\hat{a}, a)$ is a cost imposed on choices of $a$ below some minimum age of marriage $\hat{a}$, such that

$$\begin{aligned} c(\hat{a}, a) &= c \quad a < \hat{a} \\ c(\hat{a}, a) &= 0 \quad a \geq \hat{a} \end{aligned}$$

In practice, such a cost might be a punitive measure such as prison time or a fine, levied on either member of the marriage pair or on any who facilitated the marriage. It might also be the unwillingness of the state to recognise marriages below a certain age, denying the marriage pair any of the legal benefits that normally comes with marriage. (The parameter $c$ might not be applied with certainty, in which case it represents the pair's expected cost of choosing some $a < \hat{a}$).

[2]Note that we leave out any description of a marriage market or assortative matching, only that a distribution of preferences exists over ages of marriage.

We assume that $c$ only applies to a share $s \leq 1$ of the population, so that the entire mass of marriage pairs may not be exposed to the legal regime: for example, the legal age of marriage might only be enforced in urban areas. We define an *improvement* in the legal regime to be either an increase in the share of the population exposed to legal sanction (an increase in $s$)or a new legal regime $c^*(\hat{a}, a)$ such that $c^*(\hat{a}, a) > c(\hat{a}, a) \forall a < \hat{a}$.

Taking the costs associated with being married below $\hat{a}$, the proportion $s$ of affected marriage pairs have preferences characterised by

$$\pi(a) = 1 - |a - a^*| - c \tag{2}$$

while the proportion of marriage pairs unaffected by the legal regime $(1 - s)$ have preferences as described in equation (1).

**Proposition 3.1.** *If pairs maximise $\pi(a)$ subject to their exogenous preferences $a^*$, then the distribution of preferred age of marriage $b(a^*)$ and the legal regime $c(\hat{a}, a)$ determine the distribution of observed age of marriage, denoted $d(a)$.*

*Proof.* All marriage pairs with $a^* \geq \hat{a}$ will choose $a = a^*$. Those with $a^* < \hat{a}$ who are part of the share affected by the legal regime will choose $a = \hat{a}$ if $1 - |\hat{a} - a^*| \geq 1 - c$, giving $a^* \geq \hat{a} - c$, otherwise they will choose $a = a^*$ if $a^* < \hat{a} - c$. Those with $a^* < \hat{a}$ who are not part of the share affected by the legal regime will choose $a = a^*$ regardless. Therefore the empirical distribution of age of marriage $d(a)$ is simply

$$d(a) = \begin{cases} b(a^*) & \text{if } a^* < \hat{a} - c, a^* > \hat{a} \\ (1 - s)b(a^*) & \text{if } \hat{a} - c < a^* < \hat{a} \\ b(\hat{a}) + s \int_{\hat{a}-c}^{\hat{a}} b(a^*) da^* & \text{if } a^* = \hat{a} \end{cases}$$

$\square$

It follows that the distribution $d(a)$ is the unique equilibrium distribution of observed age of marriage, as there is no choice $a'$ such that $\pi(a') > \pi(a)$ for a given distribution of preferences $b(a^*)$, legal regime $c(\hat{a}, a)$, and proportion $s$.

We are interested the discontinuity at $\hat{a}$ created by imposing the cost $c$ imposed for choices below this threshold, which we define as the difference in probability of observing the marriage age at the cut-off $\hat{a}$ and at values just below this cut-off. Specifically, we define

$$D = d(\hat{a}) - d(\hat{a} - \epsilon) \tag{3}$$

where $\epsilon$ is small. The intuition is that imposing some cost to choice of $a$ below the threshold $\hat{a}$ generates the discontinuity in the distribution of observed ages of marriage.

**Proposition 3.2.** *The size of the discontinuity in observed marriage ages at the cut-off $\hat{a}$ increases with improvements in the legal regime or in the density of preferences over marriage in the region $(\hat{a} - c, \hat{a})$.*

*Proof.* Substituting the definitions of $d(a)$ into $D$ gives

$$D = \left[ b(\hat{a}) + s \int_{\hat{a}-c}^{\hat{a}} b(a^*)da^* \right] - (1-s)b(\hat{a} - \epsilon)$$

As the above expression is only increasing in $s$, then $\frac{\partial D}{\partial s} > 0$. Increasing $c$ extends the lower bound of the interval $(\hat{a} - c, \hat{a})$, so it follows that $\frac{\partial D}{\partial c} > 0$. Using $A = \int_{\hat{a}-c}^{\hat{a}} b(a^*)da^*$, the size of the discontinuity increases in the density of marriage pairs whose preferred age of marriage falls in this region, $\frac{\partial D}{\partial A} > 0$. □

**Proposition 3.3.** *Though the size of $D$ increases in measures of legal enforcement and preferences for underage marriage below the cut-off $\hat{a}$, an increase in the relative incidence of underage marriage in the population does not imply an increase in the size of the discontinuity.*

*Proof.* The incidence $R$ can be written the share of pairs married below $\hat{a}$ to the share married above the cut-off:

$$\begin{aligned} R &= \frac{\int_0^{\hat{a}} d(a)da}{\int_{\hat{a}}^1 d(a)da} \\ &= \frac{\int_0^{\hat{a}-c} b(a^* da^*) + \int_{\hat{a}-c}^{\hat{a}} d(a)da}{\int_{\hat{a}}^1 b(a^*)da^*} \\ &\equiv \frac{j+k}{l} \end{aligned}$$

This term can increase ($\frac{\partial R}{\partial j} > 0$) or decrease ($\frac{\partial R}{\partial l} < 0$) without implying an increase in the size of the discontinuity. Put differently, an increase in the incidence of early marriage is not a sufficient condition for an increase in the size of the discontinuity at $\hat{a}$. Instead, the discontinuity measures legal enforcement in the neighborhood of the legal cut-off. □

### 3.1 Empirical marriage age distribution when $b(a^*)$ is beta-distributed

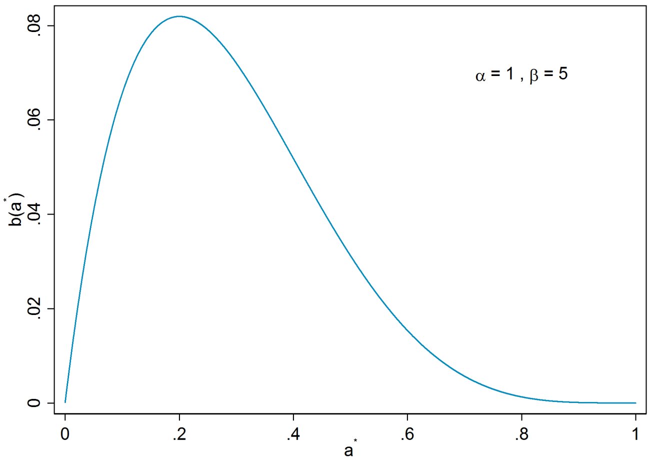
We turn to a concrete example of a preference distribution to clarify how a legal regime would affect the equilibrium age of marriage distribution observed in household survey data. If the distribution of preferences over marriage ages is beta-distributed, then it is described by

$$b(a^*) = a^{\alpha} \times (1-a)^{\beta - 1} \tag{4}$$

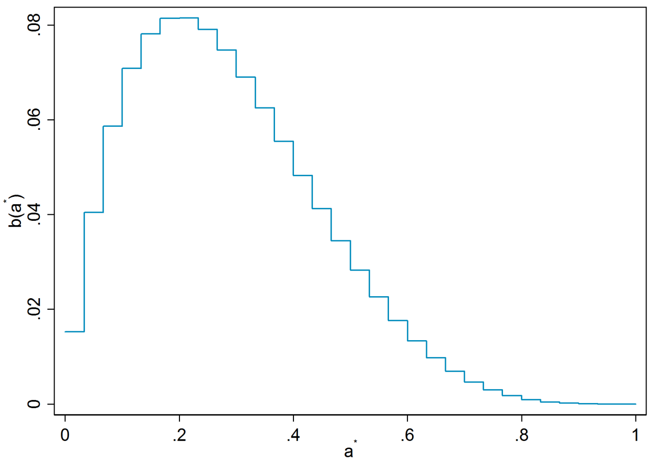
.

Figure 3: Distribution of $a^*$

(a) Continuous




(b) Discrete




For some choices of the shape parameters $\alpha$ and $\beta$, this provides a plausible distribution of preferences that is not symmetric and with excess mass to the left of the median preferred age of marriage. Figure 3a shows this distribution for an arbitrary choice of shape parameters, and figure 3b is the analogue of this distribution for discrete values of $a$. This is without loss of generality and provides a clear link to the distributions taken from survey data we will examine in the following sections.[3]

In the absence of a legal regime, $c = 0$ or $s = 0$, so the observed distribution of marriage ages will be exactly that of the distribution of preferences: $d(a) = b(a^*)$. Under a legal regime, some density of marriage pairs determined by $s$ and $c$ will shift to $\hat{a}$. Because the parameters $s$ and $c$ determine the density of marriage pairs whose optimal

[3]The *height* of the discontinuity pictured in figure 4b under a legal regime will be a function of length the interval $\hat{a}$ when it is a discrete-valued variable.

choice of $a$ increases to $\hat{a}$, higher values of these parameters imply a larger value of $D(\hat{a})$ and therefore a larger discontinuity. Specifically, the mass

$$s \int_{\hat{a}-c}^{\hat{a}} b(a^*) da^* \tag{5}$$

will shift to $\hat{a}$. Drawing the probability density functions for some legal cut-off demonstrates the same discontinuous increase in the probability being married at a given age $a$ as a function of the legal regime ($\hat{a}$ determines the position of the discontinuity). As shown in Figure 4, this shift generates a discontinuity in the observed distribution of ages of marriage.

### 3.2 Testable hypotheses

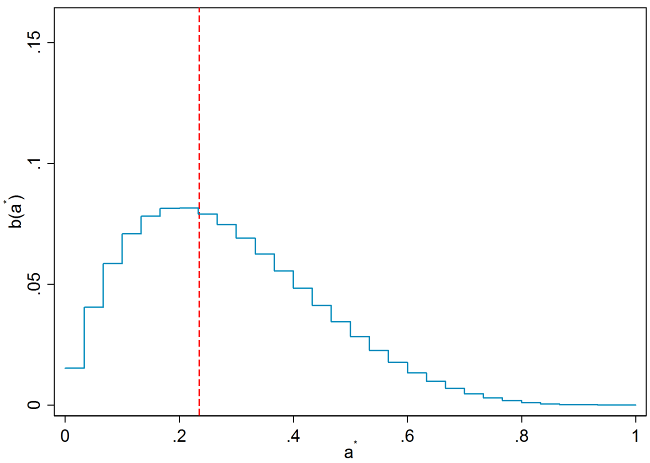
We draw two testable hypotheses from this conceptual framework. The first is that discontinuities should be present wherever a legal regime is binding, but not elsewhere. The second is that tests aimed at estimating the discontinuity as described in equation (3) should provide reasonable estimates as to whether an effective legal regime is present or not. We will discuss how we might construct an empirical test for such a discontinuity in the following section.

It is worth noting that this is just one way to model observed distributions of marriage ages, and not the only one that might generate a substantial difference in density around the legal age cut-off. The fact that this discontinuity also implies that there will be a 'drop' just after the legal age (as is observed in part 4b of Figure 4) *is* a feature the model, driven by the fact that some mass of pairs marry when they can legally do so.

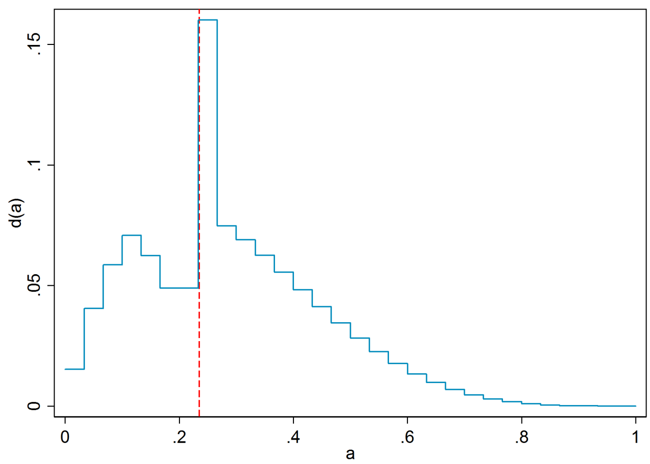
Other models could deliver the same testable predictions. For example, we could imagine a hazard model in which pairs are 'at risk' of getting married every year with a probability that is a function of the costs for early marriage imposed by the state. Since these costs are zero at the legal minimum age, we would expect to see a discontinuous jump in the probability of marriage at that age without a drop after this cut-off. Most of the tests we will go on to use are sensitive to the initial jump, so while we present the above model as a plausible way of thinking about age-of-marriage distributions, the results we find will be unable to distinguish between it and hazard or other models with distinct mechanisms but analogous predictions.

Figure 4: Effect of imposing a legal regime on $a$

(a) Effect of legal cut-off on $a^*$




(b) Effect of legal cut-off on $a$



# 4 Data and discontinuity tests

In the previous section, we discussed how effective legal regimes could be associated with a discontinuity in the distribution of marriage ages. Here, we take our hypotheses to the data using multiple rounds of information from the Demographic and Health Surveys (DHS).

## 4.1 Detecting discontinuities in marriage distributions

There are two reasons why the mere appearance of a discontinuity in the data is not sufficient evidence that enforcement is taking place. First, using the intuition laid out above, the size of discontinuity around the legal cut-off itself can also be informative as to how effective legal enforcement is. Second, because empirical age-of-marriage distributions (including the ones constructed from the DHS) are samples of the underlying population, we will also need to statistically test whether or not a discontinuity is statistically significant, given the variation in the underlying data.

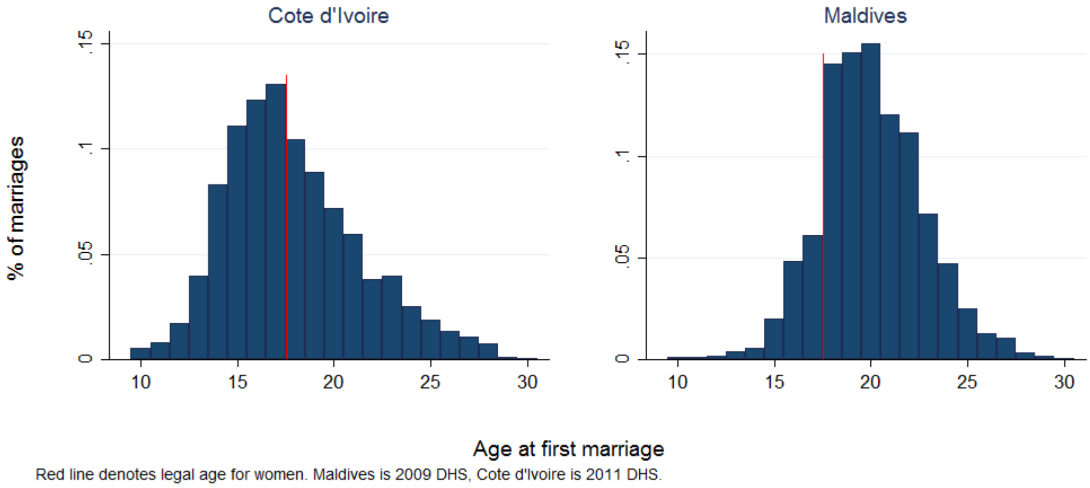
*Looking for discontinuities in age-of-marriage distributions*

There is a growing economic literature that focuses on detecting behavior changes by looking for discontinuities or 'notches' in distributions. For example, Kleven and Waseem (2013) test for notches in the distribution of reported earnings from Pakistan to uncover the underlying effect of tax thresholds on behavsior. Theirs is one of a number of studies using earning distributions to estimate or predict behavioral responses to policy changes (Chetty et al. 2009; Saez 2010; Bastani and Selin 2014). Closer in spirit to this paper is Rani, Belser, Oelz, and Ranjbar (2013), where the authors use the presence of spikes in wage distributions around the minimum wage to argue that minimum wage laws are, in fact, binding in developing countries.

Using the same notation established in Section 3, our aim is to detect the presence of a discontinuity $D$ around a minimum age-of-marriage $\hat{a}$ threshold. As we describe below, obtaining an empirical age-of-marriage distribution is relatively straightforward. Consider in Figure 5 the distribution of age of marriage for women under the age of 30 of women in Côte d'Ivoire and the Maldives, both of which have legal cut-offs $\hat{a} = 18$, indicated by the red line in the graph. The discontinuity for Côte d'Ivoire is small and negative. For the Maldives it appears to be quite large ($D \approx 0.08$). But we cannot be confident that the change in observed marriage behavior around the cut-off is likely to be a true feature of the data and not the result of statistical noise without a formal test that compares the size of $D$ to the variation of underlying data.

We therefore turn to popular methods of detecting distributional discontinuities in applied economics: a density test first proposed in McCrary (2008). The McCrary test

Figure 5: Distribution of age-of-marriage in Cote d'Ivoire and Maldives



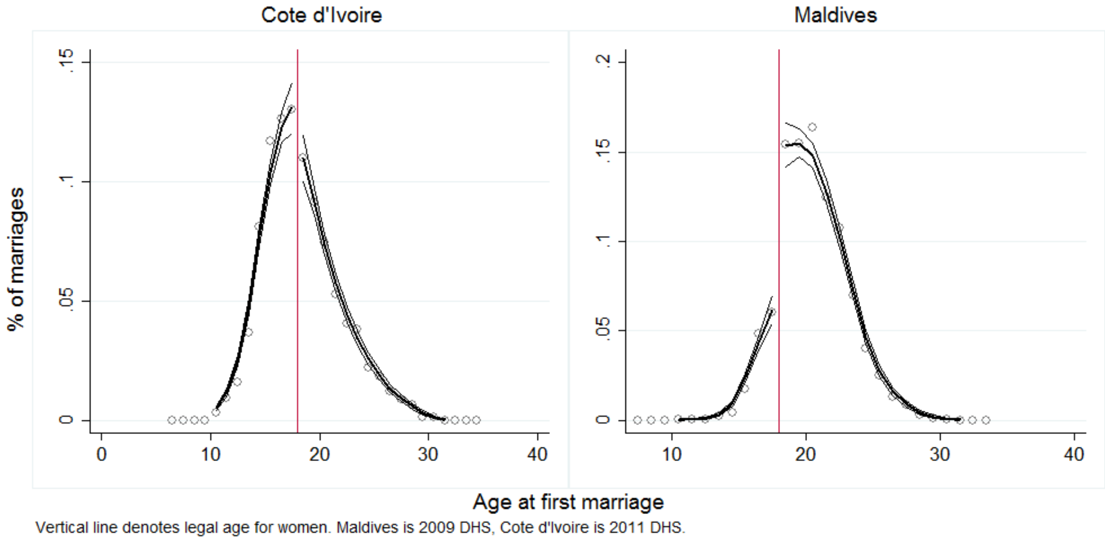

was formulated to detect behavioral responses to discontinuities in treatment. Regression discontinuity designs require that individuals have imprecise control over the running variable (e.g., marks on an exam) around the *threshold* (e.g., a passing score) (Lee and Lemieux 2010). A simple way to check for this is to test if the running variable is smooth around the cut-off. If a large number of agents end up with a score just above or below the threshold, this constitutes evidence of 'manipulation' of the running variable. In the context of exam results, this would appear as a statistically-unlikely mass of students with marginal passes.

In the context of marriage decisions it is precisely this evidence of behavior that we are interested in. If a legal regime creates a discontinuity in the costs of marriage at a given age $\hat{a}$ that leads some people to delay marriage until that age, there will be a jump between the density of the distribution at $\hat{a}$ ($d(\hat{a})$) and some point earlier ($d(\hat{a} - \epsilon)$). The McCrary method tests for such a jump using a two-step approach. First, the continuous distribution of the running variable is divided into bins of width $w$. Then a kernel-weighted linear regression is run on the logarithm of the bin frequencies on either side of the threshold $\hat{a}$.[4] As a demonstration, Figure 6 shows a naïve McCrary test being run on the AOM distributions for both Côte d'Ivoire and the Maldives, revealing the latter to have a positive and significant discontinuity around $\hat{a}$.

However, the McCrary test was designed for large-sample, continuous distributions, and depending on bandwidth selection it has the ability to assess fine degrees of manipulation. In our context, there are two challenges that would make it difficult to detect manipulations at really fine levels (e.g. girls marrying on the precise day they reach the

[4]The bandwidth and bin sizes can be calculated using an automatic procedure or manually. The ratio of bin size and bandwidth approaching zero as the sample grows.

Figure 6: McCrary density tests for discontinuities in Cote d'Ivoire and Maldives




legal age of marriage). The first is the reality these decisions are not likely to be so precise. Marriage pairs might more likely to opt to wait another year for season to marry. Second, as we discuss below, there is potential measurement error in DHS's calculation of age-of-marriage that makes it difficult to reliably calculate the precise age of marriage at the daily or monthly level. Thus, age-of-marriage distributions lend themselves to higher levels of aggregation, which implies wider bin widths. That, in turn, challenges the McCrary test's requirement that bin size be small (relative to bandwidth).

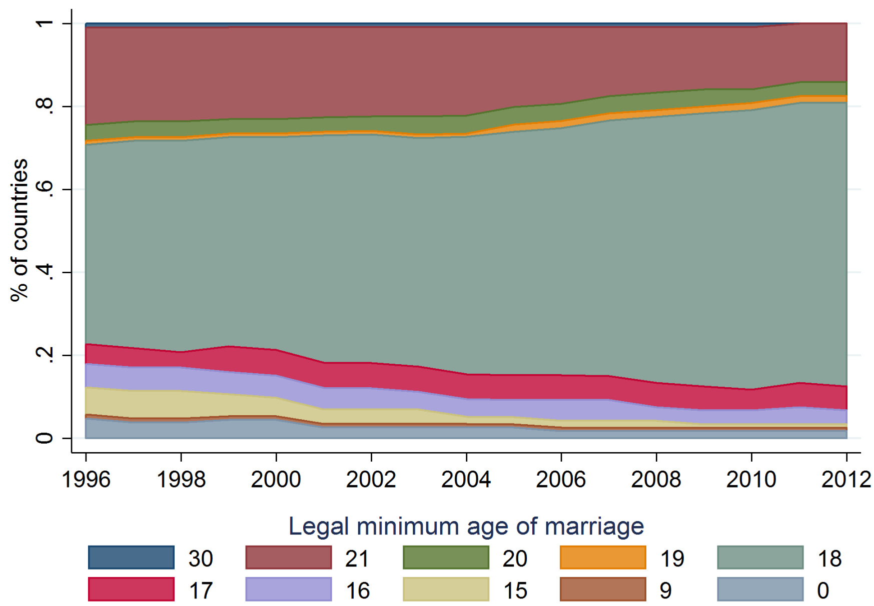
An alternative approach is to use a method of estimating discontinuities developed by Frandsen (2014) for *discrete* distributions. Consider a discrete version of the underlying distribution $d(a)$. If that underlying distribution was smooth around the cut-off $\hat{a}$, then the probability of an observation ending up either in the bin just to the left of $\hat{a}$, at $\hat{a}$ or just to the right of $\hat{a}$ should be equivalent. The Frandsen test's null hypothesis is that across these three bins, $\hat{a}$ has a binomial distribution with $p = \frac{1}{3}$ and is rejected if the observed bins deviate too far.[5]

There is one crucial assumption that must hold for either the McCrary or Frandsen tests to be valid: the response of the underlying variable $a_i$ to the presence of a legal regime should be monotonic, such that $a_i \geq a_i^*$ or $a_i \leq a_i^*$ $\forall i$. That is, the presence of a legal regime cannot both induce some people to delay marriage and others to get married earlier than they would otherwise have done.

In the following analysis, we will investigate two possible measures of an effective legal regime. The first will be a simple estimate of the discontinuity $D$ before any hypothesis tests are run using it. The McCrary test estimates the discontinuity as the (log) difference

[5]The full details of the test are described in Frandsen (2014)

Figure 7: Evolution of minimum age-of-marriage laws over time




**Note:** series covers all countries in MACHEquity database.

in height between the bin immediately to the left of the cutoff and the one immediately to the right of the cutoff: $log(\hat{n}_{\hat{a}}) - log(\hat{n}_{\hat{a}-1})$, where $n_b$ are the number of women who chose to get married in age bin $b$. For the McCrary test, these bin heights are estimates obtained from a kernel-weighted linear regression. For the Fransen test, no such estimation is performed, so in place of $\hat{n}_{\hat{a}}$ we use the actual bin height, $n_{\hat{a}}$.

Though we show results from both the Frandsen and McCrary approach to estimating discontinuities, we consider the McCrary results to be a robustness check because of the discrete nature of our underlying data on ages of marriage. Neither measure informs us of the statistical significance of an observed discontinuity, only the difference in the incidence of marriage at the legal age compared to the previous year. To account for statistical significance, our second measure is an indicator variable which is equal to one if the Frandsen (or McCrary) test returns a positive estimate of $D$ which is also significant at the $p = 0.10$ level or lower. In the next subsection, we will discuss the data we will use to test for discontinuities in age-of-marriage data.

## 4.2 Data sources

### 4.2.1 Age-of-marriage laws

Information on the minimum age-of-marriage comes from a database maintained by the Maternal and Child Heath Equity Project (MACHEquity).[6] The data covers the minimum age of marriage for girls between 1995 and 2012 for 121 countries across the globe. The MACHEquity data is compiled from existing legal databases as well as original government documents.[7]

The MACHEquity data includes the minimum age of marriage both with ($\hat{a}_{pc}$) and without ($\hat{a}$) parental consent. This presents a potential problem for the analysis: in environments where parents support marriage under the age of $\hat{a}$ but not under the minimum age with parental consent $\hat{a}_{pc}$, discontinuity estimates using $\hat{a}$ are likely to be biased towards zero. However, if parents do not in general support marriage under the age of $\hat{a}$, then discontinuity estimates using $\hat{a}$ will be unaffected, but those using $\hat{a}_{pc}$ would then be biased towards zero. We remain agnostic as to which cut-off is more likely to be enforced and report both in our final analysis, although our results do suggest that $\hat{a}_{pc}$ is a more meaningful cut-off.

Figure 8 displays the evolution of the legal minimum age-of-marriage (without parental consent) for the entire period covered by MACHEquity. Over time, a larger proportion of countries have adopted eighteen as the legal cut-off. Interestingly, we do not see continued progress: the share of countries with minimum legal ages *above* eighteen has declined recently.

### 4.2.2 Demographic and Health Surveys (DHS)

We use from the Demographic and Health Surveys (DHS), a multi-country series of population-representative surveys covering over 73 developing countries. Specifically, the DHS gathers detailed information on all women in a household, including their marital status and, if married, the age at which they were married.[8]

We choose retrospective age-of-marriage for women in each DHS round rather than current marital status for two reasons. First, constructing a 'cross-sectional' measure of the age-of-marriage distribution using responses by teenagers in the sample, the number of observations in each round would be very low. Second, if marrying below a given age $\hat{a}$ is illegal in a country, respondents who are *currently* married and underage would be less likely to report their marriage, leading to underreporting of marriage for girls under the

[6] http://machequity.com/

[7] These include Convention on the Elimination of All Forms of Discrimination against Women (CEDAW) and Convention on the Rights of the Child (CRC) reports, the World Bank's Women, Business and the Law database, the World Legal Information Institute and the legal database Lexadin.

[8] We use the variable `v511` which records the age in years of the woman at first marriage or cohabitation, as well as `v509`, which records the date she entered he first marriage or cohabitation. We regard cohabitation as strong, albeit imperfect, proxy for marriage. This is corroborated by the fact that for most countries in our sample, the age-of-consent is either equal to or above the age-of-marriage.

age of $\hat{a}$. Using retrospective age-of-marriage reports for older women does not eliminate the possibility of underreporting, but should mitigate it.

In most instances, the DHS provides information on a woman's age of first marriage in enough detail to conduct the analysis at the monthly level. This would make a typical McCrary test more feasible, as the age of marriage distribution would then begin approaching a continuous distribution. However, women and girls in the DHS show an incredible tendency to get married within the same month as their birthdate. This tendency is illustrated in Figure 12 in the Appendix, which is a heat map of the relative probability that a woman answers that she was born in a given month and that she was married in a given month.

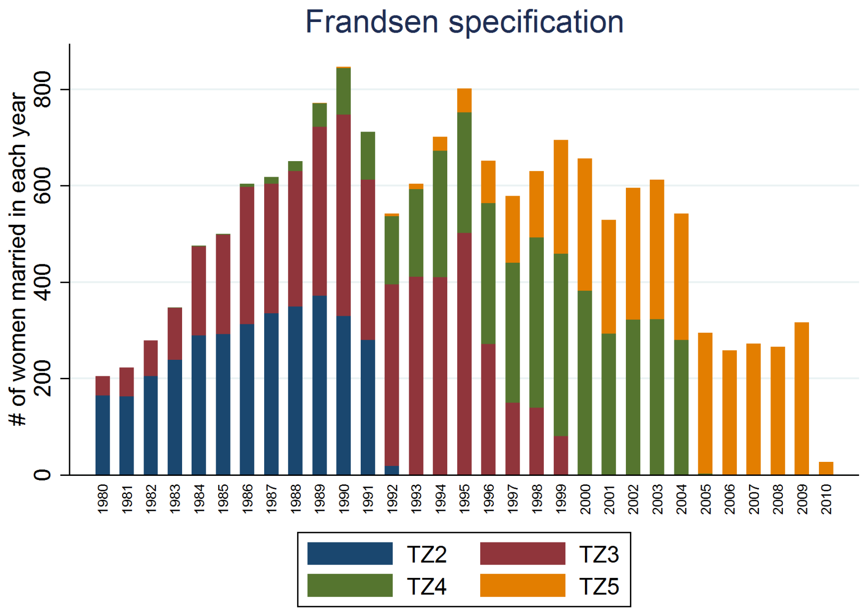
The higher frequency along the heat map's diagonal indicates that women are especially likely to report that they were married in the same month as their birthday. This could be due to either systematic reporting errors in the dates of birth and marriage or a cultural predisposition to being married close to one's birthday. The mechanism is interesting but unimportant: the implication is that any age-of-marriage distribution defined over a woman's age in months will show peaks as as the woman reaches a new age, as Figure 13 illustrates. In this context, a discontinuity test would pick up jumps around any changes in whole-integer years (for example turning 20), regardless of the legal situation. We proceed with age-of-marriage defined over years to account for these measurement issues.

### 4.3 Sample construction and estimation

It would be straightforward to test for discontinuities in each DHS country-round combination separately. However, a given country-round of the DHS comprises a sample of women of different ages. These women may have been exposed to different legal regimes when they chose to get married. To match women in the DHS with the minimum legal age of marriage they faced, we assign women to the year they were married, then assign each woman a minimum age of marriage corresponding to the precise year she was married. We then group those years into 'blocks' to increase number of observations for each age category. For maximum transparency, we do this for every possible year block divisible within the eighteen year period 1995-2012 (the period we have complete minimum legal age data for), giving us one, two, three and six year block samples. Before implementing formal statistical tests, we re-centre the age-of-marriage around the cut-off, so it takes on a value of -1 for women one year too young to be married, 0 for those who are precisely at the legal age, and so on.

By doing so, we are able to apply tests over samples that include women exposed to a specific legal regimes in given time period. We then apply these tests on each country-year-block (e.g. Zambia - 1995 to 1994), dropping country-blocks with too small

Figure 8: Construction of retrospective panels




a sample.[9] The output from this approach is a panel of test results, which we transform into a measure which takes the value of one if there is a positive discontinuity that is also significant at the $p \leq 10\%$ level for a given country-year block.

This approach allows us to answer a specific question about the presence of discontinuities, for example: *For women married between 1995 and 1998, is there evidence of marriage-age discontinuity around* $\hat{a}$? Figure 8 displays an example using real data from Tanzania of how different DHS rounds contribute to different country-blocks.

One risk to applying this data structure is that women in the DHS who were married in a given year may not be representative of women who were married in that year. Put differently, the representativeness of the DHS must hold for retrospective questions. This assumption would fail if, for example, women who were married at a given age were systematically more or less likely to be observed in the DHS. A plausible mechanism that could generate this bias is that women who were married earlier could suffer from the negative consequences of early marriage discussed above. Higher rates of poverty or teenage pregnancy might lead to higher rates of mortality or other forms of attrition, removing them from the DHS sample.

While this attrition should not vary discontinuously around a legal age of marriage, our use of yearly bins means that women who are married one year before the limit might still be less likely to be present in the DHS sample than those who are married at the legal cut-off. That would bias tests towards finding a positive, significant discontinuity.

[9] We drop all country blocks with less than 100 observations.

In contrast, the next section shows that we rarely observe significant and positive results. This attrition bias, to the extent it exists, does not appear to be driving results.

# 5 Results

We calculate two outcomes:

1. The observed jump in marriage, measured as the log difference between the number of women married exactly at the legal age and a year before the cut-off.

2. The test results, indicating that the discontinuity is positive and significant at the 10% level as reported by the Frandsen and McCrary tests.

We calculate these outcomes for every combination of possible country-year blocks (1,2,3 and 6-year blocks), for legal cutoffs both with and without parental consent. In this section, we report results at the country level and investigate how those results change over time and how they change when a country raises the legal age of marriage.

## 5.1 Country-level results

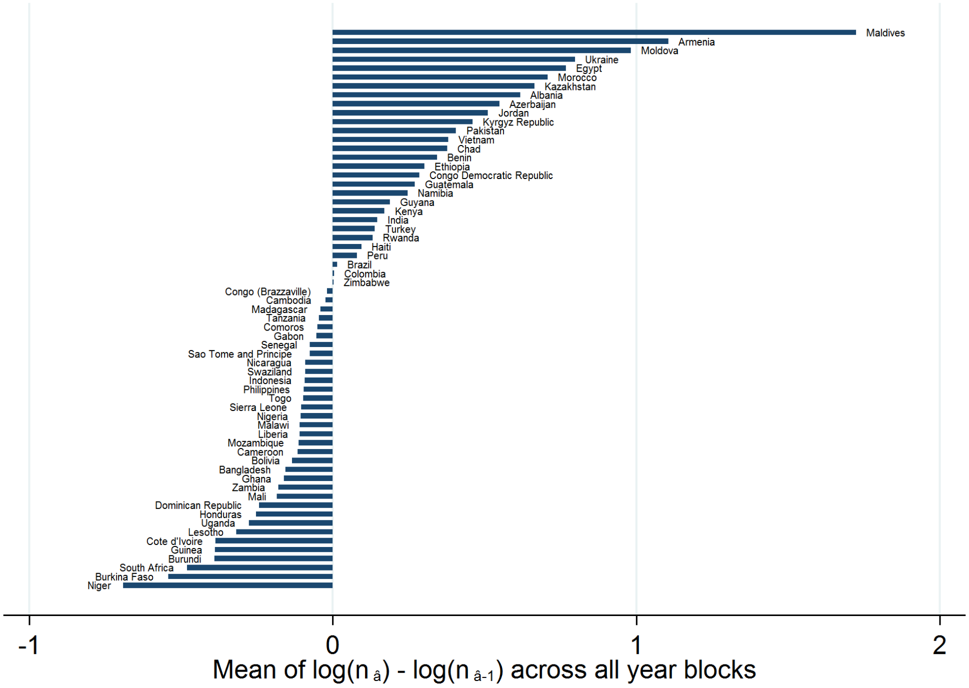
In this section we focus on the observed differences in marriage ages at the cutoff: $(log(n_{\hat{a}}) - log(n_{\hat{a}-1}) = log(\frac{n_{\hat{a}}}{n_{\hat{a}-1}}))$. Figures 9 and 10 display the average estimate of these differences for countries in our sample, ranking them from highest to lowest, for both the minimum legal age and for minimum age with parental consent. The results are sobering. Only 44% of countries in the DHS sample have even positive discontinuities around the legal age of marriage (although the number grows to 83% when we use the minimum legal age *with parental consent* as the cut-off).

In figure 11 we show the 'hit rate' - how often countries have positive and significant discontinuity per year assessed. The results are similar, but not identical. The rank correlation between the discontinuity estimates and the test results using one year bins is 0.44 and 0.02 for the cut-offs without and with parental consent, respectively.

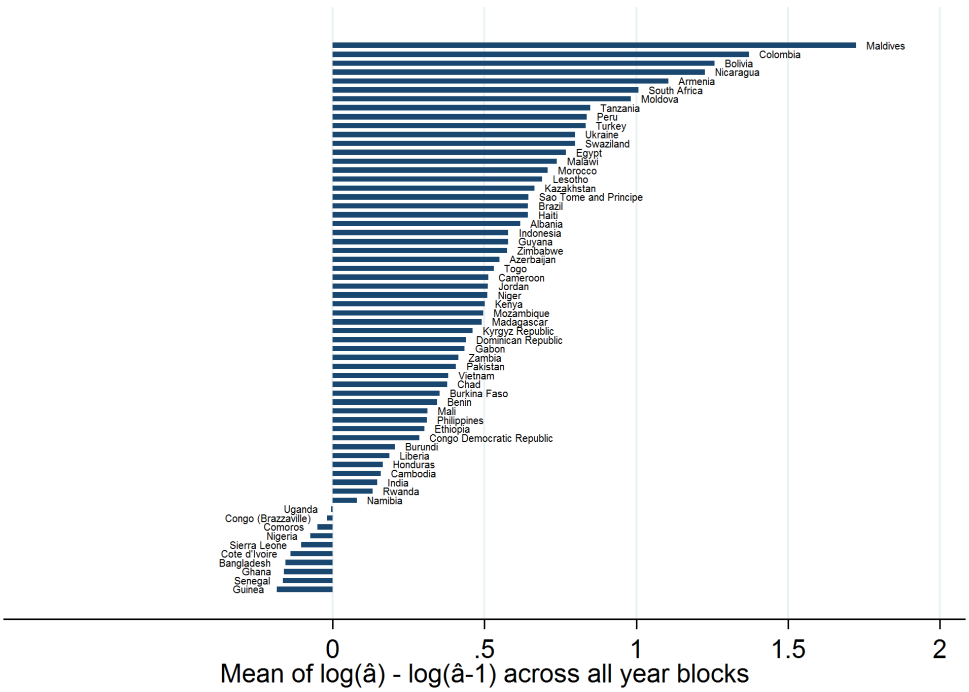
There are two reasons these correlations are low. First, the test is 'passed' only if the discontinuity test returns both a significant *and* positive result, so all information for countries with negative discontinuities is discarded. Second, while the probability that the test results in a positive outcome is increasing in the size of a discontinuity, there are other factors that vary across country-rounds and influence the test particularly the sample size in the area around the legal cut-off.

Figure 9: Countries ranked by average discontinuity (1995-2012)

(a) Threshold = minimum age ($\hat{a}$)



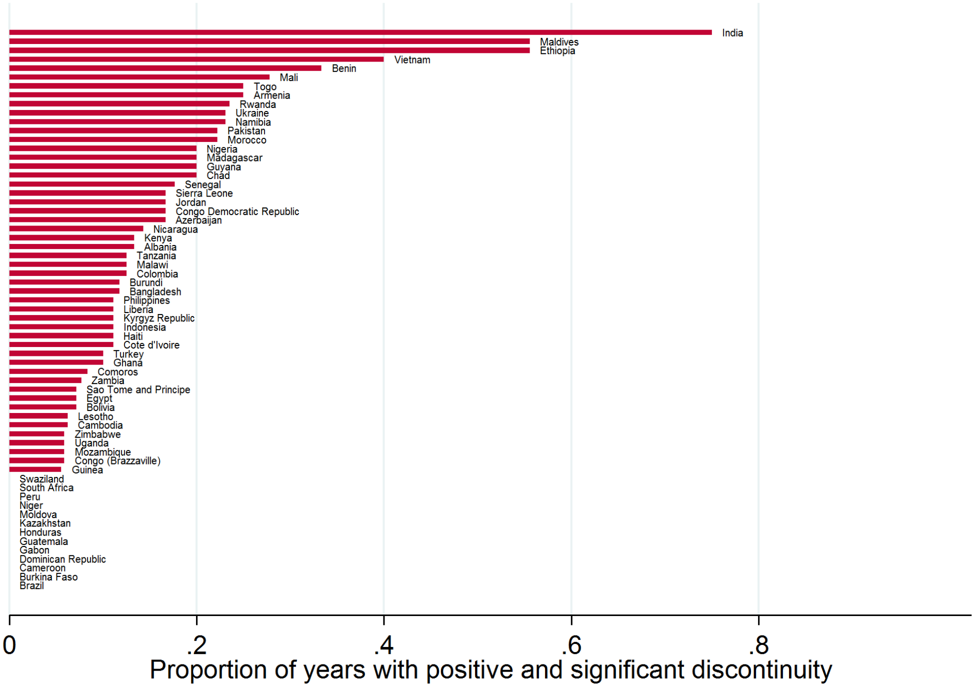

(b) Threshold = minimum age with parental consent ($\hat{a}_{pc}$)



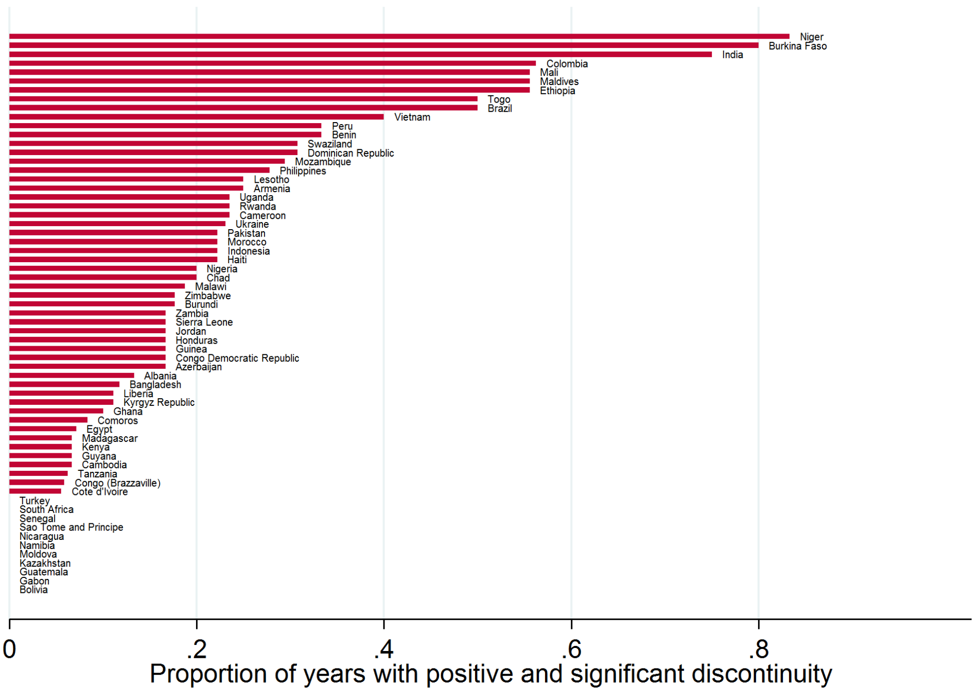

Notes: Ranking shows the ranking of log difference in marriage rates at and one year before the cutoff ($Log(n_{\hat{a}}) - Log(n_{\hat{a}-1})$) averaged across all year blocks, for both the minimum legal and and the minimum with parental consent.

Figure 10: Countries ranked by average Frandsen test pass rate (1995-2012)

(a) Threshold = minimum age ($\hat{a}$)




(b) Threshold = minimum age with parental consent ($\hat{a}_{pc}$)




Notes: Ranking shows the relative position of the average Frandsen pass rate averaged across all year blocks, for both the minimum legal age with or without parental consent. Pass rate is not adjusted for multiple hypothesis tests.

Though we display actual test results here, we caution against emphasising country-level rankings: failure to detect a *statistically-significant* discontinuity in a country-year is not evidence that the country was not enforcing the laws on its books. At a minimum, before any country-level results are used to evaluate whether a state enforces age limits, we recommend applying more conservative hurdle than $p \leq 0.10$.

Finally, the rates at which countries pass these tests are also a function of random change. As the number of tests grows larger, the chance of a significant result increases.[10] When investigating whether test pass rates are correlated with country-characteristics in the following sections, we control for the sample size used in the test.

To capture whether legal enforcement is improving, we calculate both the average discontinuity estimate and the average pass rate for each panel (both two and four year blocks) using the minimum legal ages of marriage with and without parental consent. The results are displayed in Figure 11. For both types of blocking, there appears to be no consistent trend in pass rates: roughly 30% of countries show signs of positive, significant discontinuities around the minimum age of marriage with parental consent, but in most periods, less than 20% of countries pass the test when we use the minimum age of marriage without parental consent.[11] For the average discontinuity estimate, the results are similarly inconclusive when considered across the various specifications.

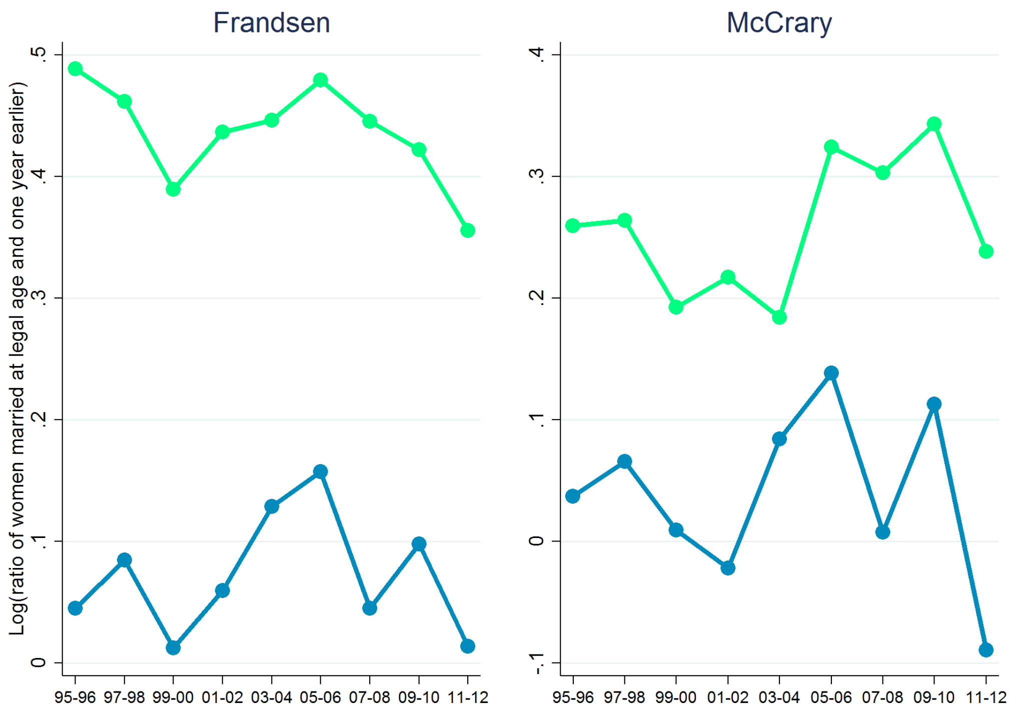
In Section 6 we consider whether or not these test results are correlated with widely-accepted measures of a state's ability to enforce laws. We show that these test results are associated with other, established measures of national capacity and rule of law. We interpret this as evidence that measures of discontinuity are useful measures of enforcement, rather systematic measurement error or idiosyncratic features of age distributions.

[10]Conservative Bonferroni adjustments cut the yearly pass rate by about one-in-four.

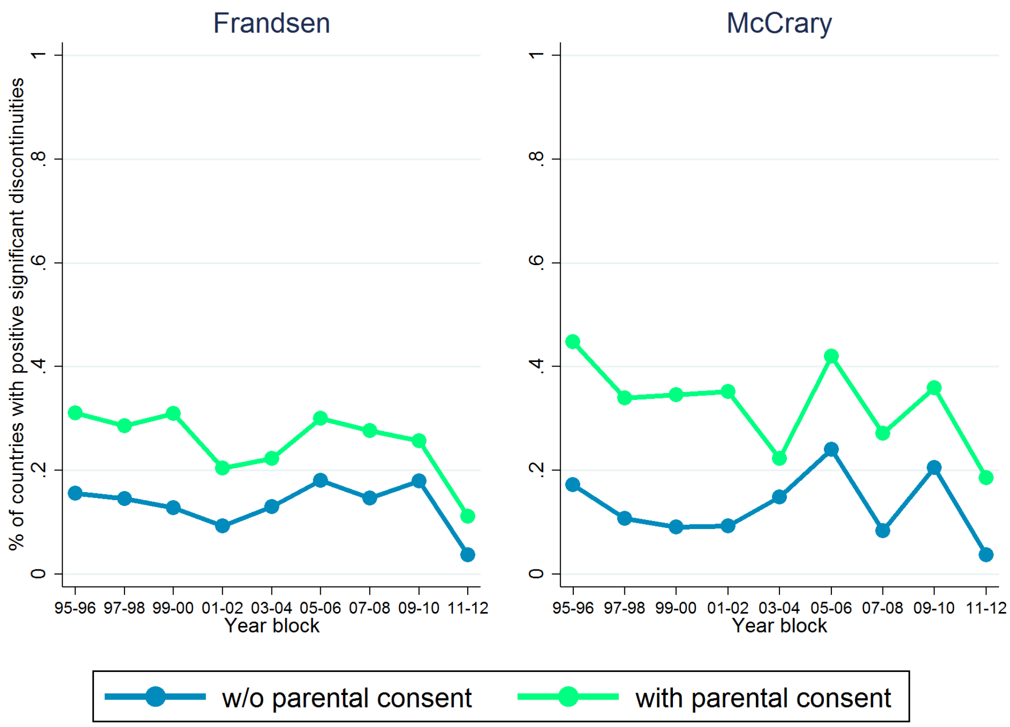
[11]Given that we have not adjusted these results for multiple hypothesis tests, a pass rate of approximately 20% is consistent with a world in which we run a one-sided test at the 10% level on samples for which the data generating process does not actually exhibit any real discontinuities. Even so, while pass rates are between 20-60% lower, on average, when we apply a conservative Bonferroni correction, the trends seen in Figure 11 remain unchanged.

Figure 11: Discontinuity trends over time

(a) Average discontinuity estimate $Log(\frac{\hat{a}}{\hat{a}-1})^*$




(b) Average test past rate




Notes: Two year blocked panel

### 5.2 The impact of changes in the legal age on discontinuity measures

The time series of discontinuity estimates and test results in Figure 11 suggest that countries are not getting much better at enforcing age-of-marriage legislation. In some instances, countries are getting worse.[12] To investigate what happens when a country *changes* its laws, we examine how these measures change when a country raises its legal minimum age using the following specification:

$$D_{it} = \theta L_{it} + \mathbf{X}_{it}^{'}\beta + \delta_t + \lambda i + \epsilon_{it} \tag{6}$$

where $D_{it}$ is the discontinuity outcome of interest (the log difference or the test outcome, for both Frandsen and McCrary tests), $X_{it}$ is a set of time-varying country (or test) characteristics, $\lambda i$ are country fixed-effects and $\delta_t$ are year/period fixed effects.

For each country, $L_{it}$ is an indicator equal to one in a period when a country has adopted the highest minimum legal age it has ever adopted between 1995 and 2012, and zero otherwise.[13] Equation (6) is a fixed effects regression that controls for common shocks and country-level characteristics which are time-invariant. Ideally, $\theta$ will pick up changes in the discontinuity measure when a country raises its minimum legal age. Part of this effect will be mechanical, as the test is now being applied to a different part of the distribution, but the same underlying concept is being tested.[14] $L_{it}$ sheds light on whether or not enforcement improves, stays the same, or weakens when a country adopts new laws.

Table 1 shows the results from estimating (6) using the overall incidence of illegal marriage (columns 1-2), the basic estimate of the discontinuity $Ln(\frac{n_{\hat{a}}}{n_{\hat{a}-1}})$ (columns 3-6) and the Frandsen test result (columns 7-10) for both types of legal cut-offs. The results in columns (1) and (2) indicate that the incidence of marriage under both the legal age and the legal age with parental consent increases by approximately 20 percentage points when a country adopts a higher legal age. Enforcement, as measured by the log difference in the height of bins at the cut-off, also falls: columns (3) and (5) indicate the average estimated discontinuity is roughly 50-55% smaller in periods when a country has adopted a higher legal cutoff, indicating a drop in possible effectiveness.

Because changes in the legal limit might have larger effects on the marriage age distri-

[12]While this might be due to an erosion of rule of law across time, part of this trend might be due to a perverse side-effect of progressive legislation: in an environment where the enforcement of *new* legislation is as bad or worse, compliance may fall in the short term after a reform. This might be the case when a government raises the legal age of marriage, but does not increase policing, enforcement, or knowledge of the change, or when the old minimum age had successfully set norms around the old cut off (or where that age was chosen to match cultural mores).

[13]Roughly 17 countries raise their minimum ages of marriage during this observed period. None reduce their legal ages.

[14]Before a change in the minimum age, distributional tests use $\hat{a}_{old}$ as the relevant cutoff, where after a change a new cutoff, $\hat{a}_{new}$, will be used, implying a different part of the underlying distribution $d(a)$ is being tested.

Table 1: The effect of raising the bar on discontinuity outcomes

| | Incidence | | Discontinuity, $Ln(\frac{n_{\hat{a}}}{n_{\hat{a}-1}})$ | | | | Test pass, Frandsen | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| | $\hat{a}$ | $\hat{a}$ | $\hat{a}$ | $\hat{a}$ | $\hat{a}_{pc}$ | $\hat{a}_{pc}$ | $\hat{a}$ | $\hat{a}$ | $\hat{a}_{pc}$ | $\hat{a}_{pc}$ |
| Country raises legal age | 0.224*** | 0.195*** | -0.500*** | -0.743*** | -0.555*** | -0.931*** | -0.0883** | -0.249*** | -0.0164 | -0.303*** |
| | (0.0269) | (0.0297) | (0.0644) | (0.104) | (0.0674) | (0.0927) | (0.0424) | (0.0765) | (0.0429) | (0.0490) |
| Log(sample size) | -0.0112 | -0.0174 | -0.0355 | -0.0129 | -0.0479 | -0.00352 | -0.00229 | -0.0182 | 0.0232 | 0.0307 |
| | (0.0143) | (0.0193) | (0.0587) | (0.0499) | (0.0678) | (0.0548) | (0.0265) | (0.0341) | (0.0269) | (0.0500) |
| Log(GDP) | -0.0308 | -0.0451 | 0.0907 | 0.184** | 0.0743 | 0.0485 | 0.188** | 0.227** | 0.172 | 0.105 |
| | (0.0294) | (0.0276) | (0.0914) | (0.0918) | (0.118) | (0.108) | (0.0804) | (0.0909) | (0.108) | (0.107) |
| Adjacent bin controls | No | No | No | Yes | No | Yes | No | Yes | No | Yes |
| Country fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.539 | 0.462 | 0.0931 | 0.351 | 0.0935 | 0.359 | 0.0162 | 0.207 | 0.00946 | 0.170 |
| # countries | 63 | 63 | 63 | 63 | 62 | 61 | 63 | 63 | 63 | 61 |
| Obs | 848 | 848 | 841 | 816 | 823 | 766 | 848 | 816 | 845 | 764 |

$\hat{a}$ = minimum legal age without parental consent, $\hat{a}_{pc}$ = minimum age with parental consent. Country raises legal age = dummy which takes on the value of one during a period where a country has adopted its highest-ever minimum legal age. *Incidence* indicates the outcome is the proportion of women/girls married under the given cutoff, *Discont* indicates the outcome is the constructed discontinuity measure and *Test* indicates the outcome is a positive, significant result on the Frandsen test at the 10% level. For the latter two outcomes, results are shown both without and with discontinuity estimates for adjacent bins $Ln(\frac{n_{\hat{a}-1}}{n_{\hat{a}-2}})$ and $Ln(\frac{n_{\hat{a}+1}}{n_{\hat{a}}})$ as controls. Panel uses single year-bins. Standard errors clustered at the country level, $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$

bution, in columns (4) and (6) we also control for the *slope* of the distribution just before and after the discontinuity.[15] We find the impact of raising the legal age to be even stronger, between a 75% and 93% reduction in the log difference, depending on whether the cutoff without or with parental consent is being considered.

We observe similar results when testing whether a country 'passes' the Frandsen test (that is, has a positive and significant discontinuity). Countries that raise the legal age without parental consent see a 8-30 percentage point decrease in the predicted probability of passing that test; the results are less robust for the cutoff with parental consent. Using single year age bins, these results are broadly similar when using McCrary tests, although only the discontinuity estimates are robust to all specification choices and age bins. (These robustness checks are available in Table 5).

These results offer a compact explanation for the lackluster improvement in effectiveness over time. Several countries in our sample have adopted more demanding minimum age requirements, but these new laws were not accompanied by increased enforcement of the updated age rules. This reinforces skepticism that legal reform alone can improve outcomes.

# 6 Age-of-marriage discontinuities as a measure of governance

Our simple theoretical models includes two determinants of discontinuities in age of marriage distributions that states control: the share of people affected by the legal regime ($s$), and the expected strength of punishment for getting married too early ($c$). In this section, we show that other measures of these two factors predict our discontinuity-based measures.

The Worldwide Governance Indicators (WGI) summarise six dimensions of the quality of governance for more than two hundred countries. These scores address six dimensions of state capacity, set out with their descriptions in Table 7 in the appendix.Kaufmann, Kraay, and Mastruzzi (2014) provide a full description of how these measures are constructed.[16]

The WGIs are reductive by construction. As such, they are subject to reasonable critique Thomas (2009). Nevertheless, there is general consensus that both the WGI and CPIA are correlated with the underlying constructs they try to measure. As such, they provide a useful, minimal test of the extent to which our discontinuity-based measures capture meaningful aspects of governance and the rule of law.

[15]That is, including controls for $Ln(\frac{n_{\hat{a}-1}}{n_{\hat{a}-2}})$ and $Ln(\frac{n_{\hat{a}+1}}{n_{\hat{a}}})$.

[16]Essentially, they assert a link between a large number of quantitative data sources and given dimensions of state behavior, applying a data-driven weighting to aggregate these data into a single indicator.

### 6.0.1 Empirical specification, covariates and placebo test

As set out in Section 5, we use direct measures of discontinuity taken from samples aggregated at the block level, as well as the results of discontinuity tests (that is, whether the discontinuities are positive and statistically significant). To test whether these indicators are correlated with 'governance', we combine panel data on discontinuity tests with all available data on WGI scores.[17]

We estimate a simple empirical model:

$$D_{it} = \alpha + \gamma \times G_{it} + \mathbf{X}_{it}^{'}\beta + \delta_t + \epsilon_{it} \tag{7}$$

where $G_{it}$ is a governance measure taken from the WGI for country $i$ averaged over blocked-period $t$. $\mathbf{X}_{it}^{'}$ is a vector of characteristics to the country-year block in question. In $\mathbf{X}_{it}^{'}$ we include the sample size of the test (for test outcomes), dummies for the minimum legal age of marriage that is being used as a threshold, year dummies, the natural log of GDP, the Polity IV project's measure measure of institutionalized democracy and region fixed effects. $\delta_t$ is a year block dummy and $\epsilon_{it}$ is the unobserved country-block error term. In some specifications we will control for all WGI measures, and in others we will control for the incidence of underage marriage.

As in previous specifications, $D_{it}$ measures either our direct estimate of the discontinuity or a dummy indicator equal to one if the discontinuity test performed on the sample of women married in country $i$ during block $t$ is both significant at the 10% level and has a positive coefficient. For our main specification, we use both the minimum age and the minimum age with parental consent. We wish to observe whether there is a positive, significant partial correlation between our measure $D_{it}$ and governance indicators $G_{it}$.

All measures of $G_{it}$ are standardized within the regression sample. For the discontinuity difference outcomes, the coefficient $\gamma$ displays the change in log difference (increase in the predicted probability of a positive test result) following from a one standard deviation increase in $G_{it}$. For the test statistic outcomes, $\gamma$ indicates the increase in predicted probability of a positive test result following from a one standard deviation increase in $G_{it}$.

An immediate critique is that any observed partial correlations may be the result of some unspecified mechanical relationship between our test and governance measures. We propose a simple falsification test: we run the same discontinuity tests on placebo age-of-marriage cut-off which is set as two years after the oldest minimum legal age of marriage for a given country block. For example, if in Tanzania the minimum age of marriage with parental consent in 15 and the minimum age without parental consent is 18, we set the

[17] Governance indicators are averaged over each block (e.g. 1995-1996 averages are taken and merged to the test results panel).

placebo as 20.[18]

Finally, we run specification (8) as a pooled OLS, with standard errors clustered at the country level. While there is both time variation in both $D_{it}$ and $G_{it}$ that we could exploit in a fixed-effects framework, we are more confident that both these measures are more accurate at picking up time-invariant determinants of legal enforcement than time-varying determinants. The World Governance Indicators, for example, are highly stable over time within countries, leading to concerns that year-to-year or period-to-period changes reflect error in the measurement process instead of actual changes in underlying characteristics.

For each WGI, we will run four regressions: a simple bivariate regression between $D_{it}$ and $G_{it}$, one in which we include all the covariates in $\mathbf{X}_{it}^{'}$, a third in which we include both $\mathbf{X}_{it}^{'}$ and all other WGI indicators as controls, and a forth in which we include $\mathbf{X}_{it}^{'}$ as well as controls for the slope of bins adjacent to our cutoff of interest.

### 6.0.2 Partial correlations with World Governance Indicators

Tables 2 and 3 show the results of running specification (8) using two-block panels using both discontinuity estimates and test results as outcomes, respectively. In each table we display only $\gamma$. Each cell indicates a separate specification. Columns (1)-(3) use the minimum legal age of marriage without parental consent, with parental consent, and the placebo cutoff as the threshold. The rows are divided by the WGI measure used, and within each subset of rows, the first row displays the simple bivariate relationship, with subsequent rows introducing progressively greater levels of controls.

The most consistent results are for the WGI measures of *rule of law* and *government effectiveness* and their correlation with outcomes based on the minimum legal age with parental consent. Depending on the specification used, a one standard deviation increase in the rule of law measure is associated with a 6-11% increased in the log difference around the minimum age with parental consent. A one standard deviation increase in government effectiveness shows similar results. These partial correlations are largely borne out in McCrary tests and in the three year blocked panel, but are less robust in the one and six year blocked panels (see Tables 8, 9 and 10 in the appendix).

The placebo outcome in Table 2 is rarely significant and never consistently positive. This suggests that our main results are not being driven by a 'chance' mechanical correlation. We find similar, but less robust correlations with test results in Table 3 and Tables 11, 12 and 15 in the appendix. When we do observe positive and significant partial correlations, they are almost entirely for the rule of law and government effectiveness WGI measures.

As an extra robustness test, focusing solely on the rule of law and government effectiveness measures, we obtained data from all available rounds of the multiple-indicator cluster

[18] The placebo cutoff should not be set closer than two bins from a real cutoff, otherwise the results of the two tests will be negatively correlated by construction.

Table 2: Partial correlations between estimates of $D$ and World Governance Indicators (2 year blocked panel)

| | Frandsen[a] | | | McCrary[b] | | |
|---|---|---|---|---|---|---|
| | (1) Min age | (2) Min age (pc) | (3) Placebo | (4) Min age | (5) Min age (pc) | (6) Placebo |
| **Rule of law** - no controls | 0.084* | 0.11** | 0.022 | 0.056* | 0.086** | -0.0073 |
| | (0.045) | (0.045) | (0.021) | (0.032) | (0.035) | (0.014) |
| with basic controls | 0.040 | 0.059* | -0.017 | 0.061** | 0.077*** | -0.025 |
| | (0.026) | (0.033) | (0.018) | (0.023) | (0.026) | (0.023) |
| + other WGIs | 0.022 | 0.11* | 0.026 | 0.062 | 0.10 | -0.0027 |
| | (0.050) | (0.062) | (0.031) | (0.041) | (0.067) | (0.044) |
| + adjacent bin controls | 0.037 | 0.065* | -0.020 | 0.048** | 0.075** | -0.032 |
| | (0.026) | (0.034) | (0.018) | (0.020) | (0.032) | (0.020) |
| **Gov't effectiveness** - no controls | 0.087** | 0.13*** | 0.021 | 0.051 | 0.080** | -0.016 |
| | (0.043) | (0.044) | (0.019) | (0.031) | (0.035) | (0.012) |
| with basic controls | 0.045 | 0.065** | -0.028 | 0.044* | 0.081*** | -0.035 |
| | (0.029) | (0.031) | (0.018) | (0.025) | (0.030) | (0.021) |
| + other WGIs | 0.040 | 0.11** | -0.038 | -0.0077 | 0.10* | -0.051 |
| | (0.039) | (0.054) | (0.032) | (0.035) | (0.057) | (0.031) |
| + adjacent bin controls | 0.044 | 0.053* | -0.018 | 0.040* | 0.060** | -0.031 |
| | (0.029) | (0.031) | (0.019) | (0.021) | (0.029) | (0.019) |
| **Regulatory quality** - no controls | 0.078* | 0.13*** | 0.028 | 0.053* | 0.065** | 0.010 |
| | (0.042) | (0.042) | (0.018) | (0.027) | (0.030) | (0.015) |
| with basic controls | 0.043* | 0.022 | -0.018 | 0.058*** | 0.038 | -0.0064 |
| | (0.023) | (0.031) | (0.020) | (0.021) | (0.024) | (0.021) |
| + other WGIs | 0.017 | -0.082* | 0.00036 | 0.037 | -0.073* | 0.046 |
| | (0.034) | (0.041) | (0.025) | (0.030) | (0.037) | (0.030) |
| + adjacent bin controls | 0.037 | 0.036 | -0.015 | 0.040** | 0.031 | -0.0077 |
| | (0.024) | (0.030) | (0.017) | (0.019) | (0.030) | (0.016) |
| **Voice & accountability** - no controls | -0.023 | 0.061 | -0.0071 | -0.018 | 0.050 | -0.024 |
| | (0.033) | (0.042) | (0.020) | (0.024) | (0.032) | (0.017) |
| with basic controls | 0.0054 | 0.019 | 0.00065 | 0.019 | 0.047 | -0.015 |
| | (0.023) | (0.036) | (0.020) | (0.029) | (0.032) | (0.026) |
| + other WGIs | -0.064 | -0.054 | 0.049* | -0.063 | -0.038 | 0.025 |
| | (0.050) | (0.050) | (0.028) | (0.049) | (0.050) | (0.030) |
| + adjacent bin controls | 0.011 | 0.014 | -0.012 | 0.010 | 0.023 | -0.028 |
| | (0.023) | (0.036) | (0.019) | (0.025) | (0.034) | (0.024) |
| **Political stability** - no controls | 0.051 | 0.10** | -0.00048 | 0.031 | 0.091*** | -0.014 |
| | (0.046) | (0.041) | (0.021) | (0.032) | (0.027) | (0.014) |
| with basic controls | 0.016 | 0.0038 | -0.019 | 0.029 | 0.025 | -0.014 |
| | (0.021) | (0.026) | (0.014) | (0.020) | (0.022) | (0.012) |
| + other WGIs | 0.0100 | -0.026 | -0.028 | 0.010 | -0.014 | -0.010 |
| | (0.026) | (0.035) | (0.019) | (0.023) | (0.031) | (0.022) |
| + adjacent bin controls | 0.012 | 0.016 | -0.023 | 0.020 | 0.024 | -0.020 |
| | (0.021) | (0.023) | (0.016) | (0.018) | (0.021) | (0.012) |
| **Control of corruption** - no controls | 0.0058 | 0.070* | -0.016 | 0.0045 | 0.048 | -0.015 |
| | (0.039) | (0.040) | (0.020) | (0.026) | (0.033) | (0.017) |
| with basic controls | 0.035 | 0.031 | -0.031** | 0.044 | 0.051* | -0.039** |
| | (0.038) | (0.030) | (0.015) | (0.033) | (0.028) | (0.015) |
| + other WGIs | 0.0032 | -0.037 | -0.031 | 0.0091 | -0.027 | -0.032 |
| | (0.047) | (0.046) | (0.024) | (0.041) | (0.048) | (0.039) |
| + adjacent bin controls | 0.039 | 0.021 | -0.016 | 0.043 | 0.029 | -0.034** |
| | (0.037) | (0.030) | (0.018) | (0.026) | (0.027) | (0.015) |
| Observations | 415 | 398 | 421 | 415 | 393 | 418 |
| Countries | 61 | 59 | 61 | 61 | 59 | 61 |

Each cell indicates an estimate of the partial correlation between a World Governance Indicator and an outcome. 'Min age', 'min age (pc)' and 'placebo' indicate that the cut-off used in the outcome construction was the minimum age without parental consent, with parental consent, and the placebo cut-off, respectively. The WGI measure in bold indicates the WGI measure used in the regression. No controls indicate as a bivariate regression. Basic controls are the log of per capita GDP, the log of the test sample size, the Polity IV measure of democracy, region fixed effects and year block fixed effects. '+ other WGIs' indicates that all other WGIs are also included as controls. '+ adjacent bin controls' indicates controls for the slopes of bins immediately to the left and right of the cut-off Standard errors clustered at the country level, $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$

Table 3: Partial correlations between positive, significant $D$ and World Governance Indicators (2 year blocked panel)

| | Frandsen[a] | | | McCrary[b] | | |
|---|---|---|---|---|---|---|
| | (1) Min age | (2) Min age (pc) | (3) Placebo | (4) Min age | (5) Min age (pc) | (6) Placebo |
| **Rule of law** - no controls | 0.034 | 0.025 | 0.0052 | 0.038 | 0.063* | -0.0040 |
| | (0.026) | (0.029) | (0.024) | (0.029) | (0.037) | (0.018) |
| with basic controls | 0.057* | 0.076* | -0.0099 | 0.014 | 0.072** | -0.0055 |
| | (0.032) | (0.045) | (0.020) | (0.022) | (0.034) | (0.016) |
| + other WGIs | 0.062 | 0.067 | 0.019 | 0.045 | 0.10 | 0.0051 |
| | (0.061) | (0.074) | (0.031) | (0.052) | (0.072) | (0.034) |
| + adjacent bin controls | 0.051 | 0.084** | -0.013 | 0.012 | 0.070* | -0.0080 |
| | (0.033) | (0.040) | (0.020) | (0.024) | (0.040) | (0.015) |
| **Gov't effectiveness** - no controls | 0.041 | 0.0018 | 0.0023 | 0.027 | 0.045 | -0.013 |
| | (0.025) | (0.027) | (0.020) | (0.025) | (0.032) | (0.017) |
| with basic controls | 0.067** | 0.035 | -0.0094 | 0.0038 | 0.059* | -0.013 |
| | (0.030) | (0.034) | (0.020) | (0.017) | (0.031) | (0.016) |
| + other WGIs | 0.099*** | -0.069 | 0.013 | 0.015 | 0.078 | -0.040 |
| | (0.036) | (0.063) | (0.028) | (0.028) | (0.062) | (0.026) |
| + adjacent bin controls | 0.067** | 0.045 | -0.0012 | 0.00093 | 0.051 | -0.012 |
| | (0.030) | (0.030) | (0.020) | (0.018) | (0.035) | (0.016) |
| **Regulatory quality** - no controls | 0.0079 | 0.014 | -0.0056 | 0.014 | 0.026 | -0.0036 |
| | (0.025) | (0.026) | (0.019) | (0.023) | (0.038) | (0.018) |
| with basic controls | 0.032 | 0.056 | -0.012 | -0.0068 | 0.011 | 0.0015 |
| | (0.029) | (0.039) | (0.021) | (0.016) | (0.040) | (0.017) |
| + other WGIs | -0.054 | 0.033 | -0.011 | -0.036 | -0.11* | 0.025 |
| | (0.043) | (0.054) | (0.031) | (0.027) | (0.059) | (0.024) |
| + adjacent bin controls | 0.031 | 0.050 | -0.0068 | -0.010 | 0.0016 | 0.0015 |
| | (0.030) | (0.038) | (0.020) | (0.017) | (0.043) | (0.016) |
| **Voice & accountability** - no controls | -0.023 | -0.026 | -0.025 | -0.012 | 0.040 | -0.021 |
| | (0.028) | (0.030) | (0.018) | (0.023) | (0.032) | (0.017) |
| with basic controls | 0.016 | 0.051 | -0.017 | -0.0043 | 0.064* | -0.0046 |
| | (0.029) | (0.044) | (0.024) | (0.019) | (0.034) | (0.019) |
| + other WGIs | -0.071 | -0.036 | -0.013 | -0.035 | -0.0064 | 0.011 |
| | (0.056) | (0.061) | (0.034) | (0.030) | (0.053) | (0.025) |
| + adjacent bin controls | 0.0068 | 0.041 | -0.030 | -0.0035 | 0.054 | -0.0079 |
| | (0.031) | (0.039) | (0.024) | (0.021) | (0.037) | (0.019) |
| **Political stability** - no controls | -0.0021 | 0.017 | -0.0033 | 0.0054 | 0.069** | -0.011 |
| | (0.025) | (0.031) | (0.018) | (0.026) | (0.029) | (0.017) |
| with basic controls | 0.015 | 0.078** | -0.0091 | 0.017 | 0.066** | -0.0095 |
| | (0.023) | (0.032) | (0.016) | (0.019) | (0.028) | (0.011) |
| + other WGIs | -0.0017 | 0.058 | -0.0044 | 0.019 | 0.046 | -0.014 |
| | (0.030) | (0.037) | (0.026) | (0.026) | (0.038) | (0.017) |
| + adjacent bin controls | 0.0085 | 0.060* | -0.014 | 0.017 | 0.063** | -0.012 |
| | (0.024) | (0.034) | (0.015) | (0.020) | (0.030) | (0.012) |
| **Control of corruption** - no controls | 0.011 | 0.024 | -0.013 | -0.011 | 0.017 | 0.00075 |
| | (0.025) | (0.032) | (0.017) | (0.019) | (0.031) | (0.018) |
| with basic controls | 0.047 | 0.062 | -0.019 | 0.0024 | 0.042 | -0.0049 |
| | (0.040) | (0.039) | (0.017) | (0.021) | (0.032) | (0.011) |
| + other WGIs | -0.0070 | 0.029 | -0.028 | -0.013 | -0.042 | 0.0076 |
| | (0.045) | (0.053) | (0.022) | (0.031) | (0.055) | (0.028) |
| + adjacent bin controls | 0.054 | 0.063* | -0.0076 | 0.0085 | 0.033 | 0.00014 |
| | (0.038) | (0.035) | (0.017) | (0.022) | (0.036) | (0.012) |
| Observations | 414 | 398 | 421 | 415 | 398 | 421 |
| Countries | 61 | 59 | 61 | 61 | 59 | 61 |

Each cell indicates an estimate of the partial correlation between a World Governance Indicator and an outcome. 'Min age', 'min age (pc)' and 'placebo' indicate that the cut-off used in the outcome construction was the minimum age without parental consent, with parental consent, and the placebo cut-off, respectively. The WGI measure in bold indicates the WGI measure used in the regression. No controls indicate as a bivariate regression. Basic controls are the log of per capita GDP, the log of the test sample size, the Polity IV measure of democracy, region fixed effects and year block fixed effects. '+ other WGIs' indicates that all other WGIs are also included as controls '+ adjacent bin controls' indicates controls for the slopes of bins immediately to the left and right of the cut-off Standard errors clustered at the country level, $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$

survey (MICS), a DHS-like nationally-representative surveys coordinated by UNICEF.[19] Combining the MICS data with DHS data increases our country coverage to 86. Tables 14 and 15 in the Appendix display the results from re-running the above specification with both DHS and MICS data on the rule of law and government effectiveness outcomes.[20] Here, we find our results on the size of the discontinuity are even stronger, although the performance of the 'placebo' cut-off is weaker. In many situations we would be unable to reject the null that these partial correlations are different. The DHS/MICS combined results do not find consistent effects on the test result outcome.

In summary, we find modest evidence that our discontinuity measure (and much more weakly, our test outcomes) are positively correlated with both rule of law and government effectiveness, even after controlling for a number of country-level characteristics. Countries with stronger rule of law and more effective governments are more likely to enforce the laws on their books and ensure they cover a larger share of their population.[21]

## 6.1 Rural and urban differences

Finally, we might expect enforcement to be lower in rural communities. To test this, we investigate how our measures of legal enforcement differ across rural and urban subsamples of the DHS by dividing the sample drawn from DHS into girls and women located in rural and urban communities and applying Frandsen and McCrary tests to the resulting subsamples. We then create a panel, include both subsamples, and apply the following specification:

$$D_{rit} = \alpha + \gamma rural_{ri} + \mathbf{X}'_{rit}\beta + \delta_{it} + \epsilon_{rit} \tag{8}$$

Where $rural_{rit}$ is equal to one when sub-sample $r$ in country $i$ is rural and zero otherwise. The coefficient $\gamma$ here picks up the average differences in the value of $D$ between rural and urban sub-samples, within the same country, within the same year-block. All within country-block variation is picked up by the fixed effect $\delta_{it}$. Any variation other variation across sub-samples that we wish to control for (such as sample size or the slope of nearby bins) will be included in $\mathbf{X}_{rit}$.

Rural communities see a higher incidence of illegal marriage: between 7-11% more women and girls are married beneath the legal age, depending on the cutoff used. We see a similar pattern when looking at our discontinuity estimates: the log difference in the discontinuity is between 8 and 20 percent smaller in rural communities, even when

[19] Because national governments implement the MICS, the data quality is sometimes suspect.

[20] Other World Governance Indicators Voice and Accountability, Political Stability, Absence of Violence, Regulatory Quality, and Control of Corruption are not consistently positive or significant in the DHS and MICS combined sample.

[21] While this is suggestive evidence that our measures are picking up some meaningful aspect of these institutional characteristics, our results are partial correlations, not causal estimates.

controlling for the slope of nearby bins (the trends in the neighbourhood of the test). Table 4 displays the results using six-year blocks. For comparison, the first two columns (1) and (2) indicate the difference in the incidence of illegal marriage using both cut-offs.

In table 6 in the Appendix, we find that the results on discontinuities are robust across both Frandsen and McCrary specifications and all possible year-block constructions for the lower cut-off (with parental consent) only, although the higher cut-off (without parental consent) is robustly negative for all variations of the Frandsen test.

The discontinuity measures here are consistent with a narrative of worse (less) enforcement in rural areas. Of course, there are many unobservable factors *other* than local rural of law and enforcement that may be driving these rural/urban differences.

## 7 Discussion and Conclusion

This paper derives a measurement of the enforcement of laws against early marriage based on the empirical distribution of ages of marriage. It is motivated by a simple distributional model that shows how the ages of marriage in microdata reflect trade-offs between underlying (unobserved) preferences and the deterrent effects of breaking the law. Applying this test to a large number of countries and rounds of the Demographic and Health Surveys shows that the vast majority of countries fail to 'pass.' That is, their distributions of the age at which women or girls marry do not show a discontinuity around the legal cut-off that we would expect to see if laws banning early marriage actually deterred the practice.

We turn to partial correlations between the resulting (binary) test outcome and World Governance Indicators that capture state capacity to establish whether this survey-based measure is a parsimonious indicator of governments' effectiveness. Fitting a simple statistical model to these data suggests that our survey-based measure is meaningfully associated with rule of law and government effectiveness. In short: countries that pass the statistical test are better at enforcing the laws that are on their books, consistent with the simple model associating preferences with marriage ages.

As a result, we believe that the paper makes two contributions. First, it demonstrates that new laws will not, on their own, be enough to prevent early marriage. Second, it provides a methodological contribution, showing how a narrow dimension of microdata from household surveys can generate a meaningful, national-level measurement of legal enforcement. This suggests a useful way to measure how countries with the 'right' legislation on the books actually perform when it comes to enforcing them.

Measures of the rule of law are generally based on national-level data, and try to capture enforcement in-the-round, rather than focusing on particular groups or interests. The relationship between those measures and outcomes that are of particular relevance for women and girls is unclear. Interest in this narrow dimension of government effectiveness is topical: an international push for laws banning early marriage has gained renewed

Table 4: Rural-urban differences in test results, 6 year bins

| | Incidence | | Discontinuity, $Ln(\frac{n_{\hat{a}}}{n_{\hat{a}-1}})$ | | | | Test pass, Frandsen | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| | $\hat{a}$ | $\hat{a}_{pc}$ | $\hat{a}$ | $\hat{a}$ | $\hat{a}_{pc}$ | $\hat{a}_{pc}$ | $\hat{a}$ | $\hat{a}$ | $\hat{a}_{pc}$ | $\hat{a}_{pc}$ |
| Rural sample | 0.119*** | 0.0703*** | -0.0961*** | -0.216*** | -0.0844*** | -0.152*** | -0.0680 | -0.102* | 0.0191 | -0.0180 |
| | (0.00875) | (0.00814) | (0.0290) | (0.0279) | (0.0301) | (0.0317) | (0.0465) | (0.0520) | (0.0429) | (0.0486) |
| Log(sample size) | | | | | | | 0.0665 | 0.0197 | 0.114** | 0.121* |
| | | | | | | | (0.0640) | (0.0710) | (0.0492) | (0.0668) |
| Country-year fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Controls for adjacent bins | | | | Yes | | Yes | | Yes | | Yes |
| Adjusted $R^2$ | 0.694 | 0.509 | 0.0686 | 0.469 | 0.0567 | 0.190 | 0.0119 | 0.105 | 0.0301 | 0.0859 |
| # countries | 162 | 162 | 160 | 157 | 157 | 149 | 162 | 157 | 162 | 149 |
| Obs | 324 | 324 | 319 | 311 | 310 | 292 | 324 | 311 | 324 | 292 |

$\hat{a}$ = minimum legal age without parental consent, $\hat{a}_{pc}$ = minimum age with parental consent. Country raises legal age = dummy which takes on the value of one during a period where a country has adopted its highest-ever minimum legal age. Standard errors clustered at the country level, $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$

momentum, and well-regarded work in empirical economics suggests that early marriage imposes life-long costs.

We posit two clear implications of this research for policy. First, the laudable goal of legislation banning early marriage must be accompanied by capacity-building and resourcing for more legal enforcement. Second, monitoring the efficacy of deterrence, including through exploiting cheap and plentiful microeconomic data as we do here, is essential to test and improve the link from laws to ages of marriage, the outcome targeted by policy and the one that matters for women and girls.

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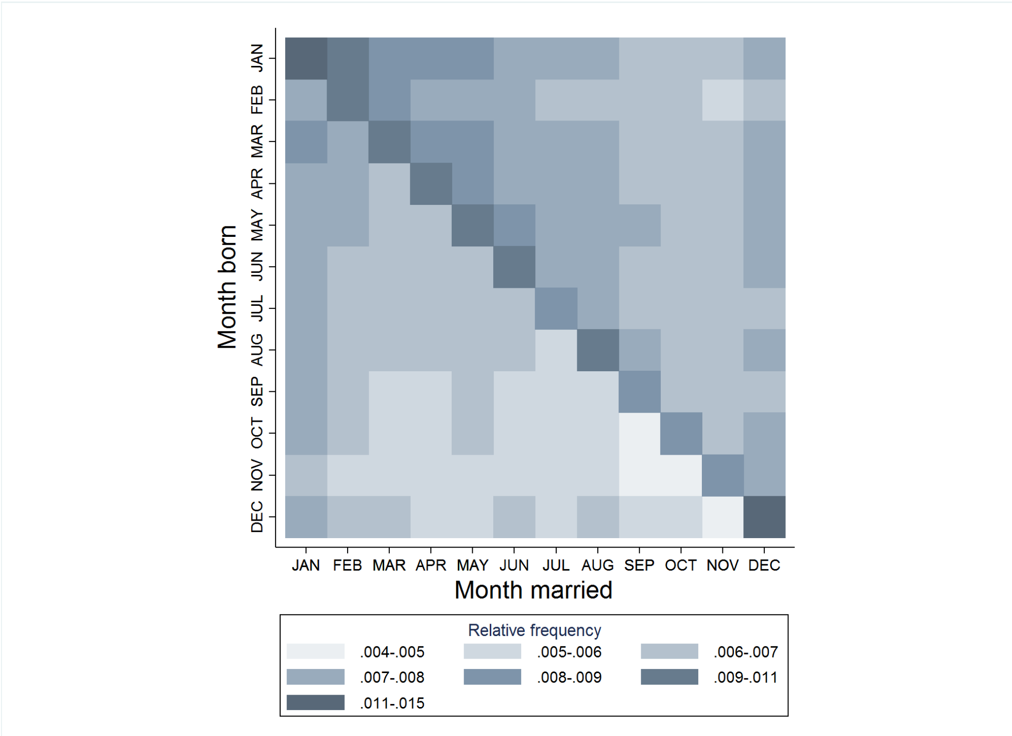
# A Appendix

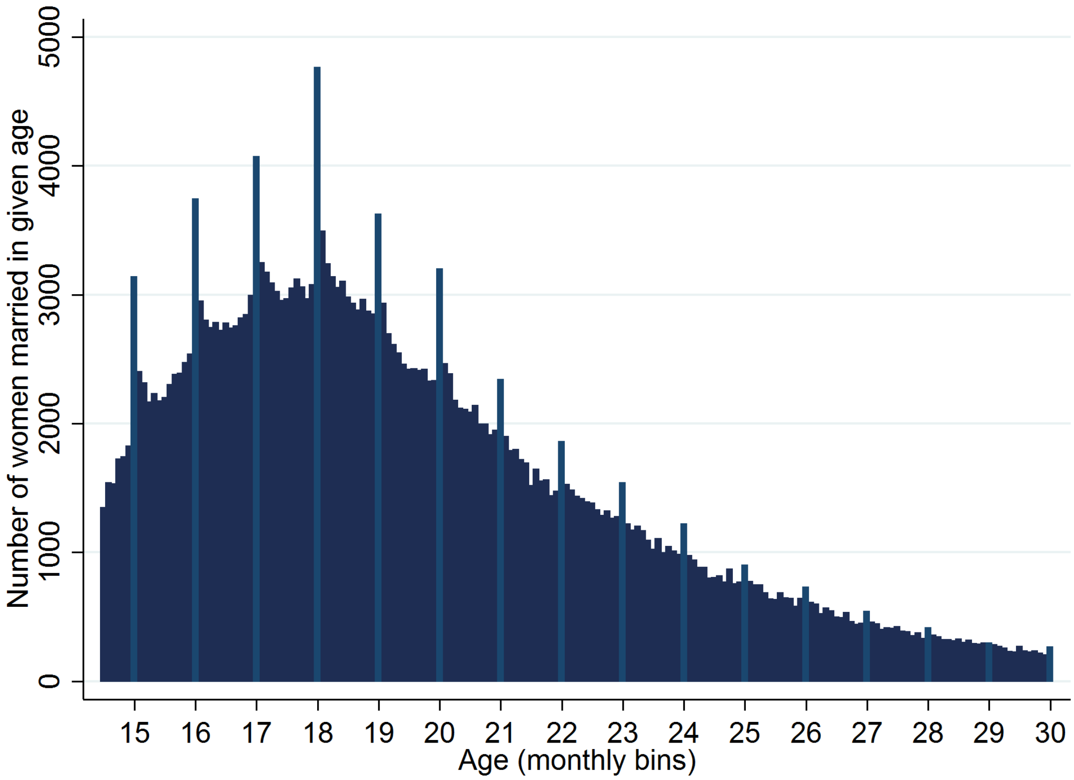
## A.1 Additional figures

Figure 12: Relative frequency of reporting a given month of birth and month of marriage




**Note:** data comprises all married women in all DHS rounds.

Figure 13: Women are most likely to get married in their birthday month




**Note:** sample restricted to all women in DHS born in a country where legal minimum age of marriage is 18. Lighter-shaded bins indicate first month of a given age.

# B Appendix: additional tables

Table 5: The effect of raising the bar: alternative specifications/tests

| | | Frandsen | | | | McCrary | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Incidence | *Discont* 1 | *Discont* 2 | *Test* 1 | *Test* 2 | *Discont* 1 | *Discont* 2 | *Test* 1 | *Test* 2 |
| *1 year blocks:* | | | | | | | | | |
| Legal cutoff | 0.22*** | -0.50*** | -0.74*** | -0.088** | -0.25*** | -0.13* | -0.74*** | -0.18** | -0.24*** |
| | (0.027) | (0.064) | (0.10) | (0.042) | (0.077) | (0.073) | (0.10) | (0.074) | (0.083) |
| Legal cutoff /w parental consent | 0.19*** | -0.56*** | -0.93*** | -0.016 | -0.30*** | -0.22*** | -0.93*** | -0.21*** | -0.39*** |
| | (0.030) | (0.067) | (0.093) | (0.043) | (0.049) | (0.072) | (0.093) | (0.054) | (0.069) |
| *2 year blocks:* | | | | | | | | | |
| Legal cutoff | 0.21*** | -0.44*** | -0.57*** | -0.060 | -0.18* | -0.017 | -0.57*** | -0.051 | -0.10 |
| | (0.027) | (0.059) | (0.098) | (0.084) | (0.100) | (0.11) | (0.098) | (0.090) | (0.10) |
| Legal cutoff /w parental consent | 0.18*** | -0.48*** | -0.64*** | 0.048 | -0.24*** | -0.069 | -0.64*** | -0.11 | -0.29*** |
| | (0.032) | (0.072) | (0.089) | (0.086) | (0.088) | (0.10) | (0.089) | (0.081) | (0.090) |
| *3 year blocks:* | | | | | | | | | |
| Legal cutoff | 0.18*** | -0.41*** | -0.49*** | -0.14 | -0.29* | -0.097 | -0.49*** | 0.022 | -0.012 |
| | (0.018) | (0.062) | (0.082) | (0.16) | (0.17) | (0.067) | (0.082) | (0.13) | (0.13) |
| Legal cutoff /w parental consent | 0.16*** | -0.43*** | -0.54*** | 0.054 | -0.34*** | -0.14 | -0.54*** | -0.065 | -0.22 |
| | (0.027) | (0.092) | (0.12) | (0.12) | (0.13) | (0.088) | (0.12) | (0.14) | (0.17) |
| *6 year blocks:* | | | | | | | | | |
| Legal cutoff | 0.16*** | -0.29*** | -0.33*** | 0.041 | -0.10 | -0.055 | -0.33*** | -0.22 | -0.29 |
| | (0.022) | (0.070) | (0.084) | (0.18) | (0.17) | (0.076) | (0.084) | (0.22) | (0.22) |
| Legal cutoff /w parental consent | 0.16*** | -0.39*** | -0.42*** | 0.18 | -0.26* | -0.16* | -0.42*** | -0.36* | -0.65*** |
| | (0.033) | (0.091) | (0.13) | (0.15) | (0.15) | (0.093) | (0.13) | (0.18) | (0.20) |

This table displays robustness checks for Table 1 in the main text. Each cell displays the coefficient of X from equation Y (the effect of a country switching a higher legal age) for a different combination of year blocks, type of legal cutoff, and outcome. *Incidence* indicates the proportion of women/girls married under the give cutoff, *Discont* indicates the outcome is the constructed discontinuity measure and *Test* indicates the outcome is a positive, significant result on the discontinuity test (McCrary or Frandsen) at the 10% level. For every outcome, the results are shown without the discontinuity estimates for adjacent bins s ($Ln(\frac{\hat{a}-1}{\hat{a}-2})$ and $Ln(\frac{\hat{a}1}{\hat{a}})$ ) as controls (columns (1)) and including them as a control (columns (2)). All specifications include log(GDP) log(sample size), country and year blocked fixed effects as controls. Standard errors clustered at the country level, $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$

Table 6: Rural/urban differences, all specifications

| | | Frandsen | | | | McCrary | | | |
|---|---|---|---|---|---|---|---|---|---|
| | *Incidence* | *Discont* 1 | *Discont* 2 | *Test* 1 | *Test* 2 | *Discont* 1 | *Discont* 2 | *Test* 1 | *Test* 2 |
| *1 year blocks:* | | | | | | | | | |
| Legal cutoff | 0.12*** | -0.097*** | -0.23*** | -0.0085 | -0.034** | -0.057 | -0.16** | 0.011 | 0.00066 |
| | (0.0087) | (0.023) | (0.026) | (0.013) | (0.015) | (0.060) | (0.061) | (0.014) | (0.016) |
| Legal cutoff /w parental consent | 0.072*** | -0.12*** | -0.20*** | 0.0072 | -0.031 | -0.048 | -0.13 | 0.0075 | -0.018 |
| | (0.0079) | (0.026) | (0.030) | (0.019) | (0.026) | (0.066) | (0.077) | (0.026) | (0.033) |
| *2 year blocks:* | | | | | | | | | |
| Legal cutoff | 0.12*** | -0.11*** | -0.22*** | -0.022 | -0.059*** | 0.0071 | -0.099*** | 0.0072 | -0.0080 |
| | (0.0087) | (0.027) | (0.027) | (0.022) | (0.022) | (0.034) | (0.026) | (0.026) | (0.024) |
| Legal cutoff /w parental consent | 0.072*** | -0.10*** | -0.20*** | 0.043* | -0.015 | 0.0062 | -0.099*** | 0.037 | -0.0045 |
| | (0.0080) | (0.032) | (0.029) | (0.024) | (0.025) | (0.041) | (0.035) | (0.039) | (0.039) |
| *3 year blocks:* | | | | | | | | | |
| Legal cutoff | 0.12*** | -0.11*** | -0.21*** | -0.041 | -0.053* | 0.0030 | -0.087*** | 0.044 | 0.030 |
| | (0.0094) | (0.025) | (0.027) | (0.027) | (0.030) | (0.031) | (0.027) | (0.029) | (0.032) |
| Legal cutoff /w parental consent | 0.071*** | -0.10*** | -0.17*** | 0.027 | -0.0034 | 0.33*** | 0.27*** | 0.26*** | 0.26*** |
| | (0.0080) | (0.029) | (0.031) | (0.029) | (0.032) | (0.040) | (0.036) | (0.040) | (0.048) |
| *6 year blocks:* | | | | | | | | | |
| Legal cutoff | 0.12*** | -0.096*** | -0.22*** | -0.068 | -0.10* | 0.024 | -0.13*** | 0.026 | -0.042 |
| | (0.0087) | (0.029) | (0.028) | (0.046) | (0.052) | (0.032) | (0.029) | (0.040) | (0.044) |
| Legal cutoff /w parental consent | 0.070*** | -0.084*** | -0.15*** | 0.019 | -0.018 | 0.10** | 0.0067 | 0.12** | 0.048 |
| | (0.0081) | (0.030) | (0.032) | (0.043) | (0.049) | (0.040) | (0.042) | (0.056) | (0.071) |

This table displays all specifications for Table 4 in the main text. Each cell displays the coefficient of a dummy variable = 1 when the sample is rural for a different combination of year blocks, type of legal cutoff, and outcome. *Incidence* indicates the percentage of women born underneath the legal cutoff. *Discont* indicates the outcome is the constructed discontinuity measure and *Test* indicates the outcome is a positive, significant result on the discontinuity test (McCrary or Frandsen) at the 10% level. For all discontinuity outcomes, the results are shown with country-block fixed effects. Columns listed with 1 include the log of the sample size used in the test for test outcomes. Columns listed with a 2 include controls for the size of the discontinuity one year above and below the cutoff being considered. Standard errors clustered at the country level, $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$

Table 7: WGI Indicators

| **WGI** | **Description** |
|---|---|
| Rule of Law | Perceptions of the extent to which agents have confidence in and abide by the rules of society, and in particular the quality of contract enforcement, property rights, the police, and the courts, as well as the likelihood of crime and violence. |
| Voice and Accountability | Perceptions of the extent to which a country's citizens are able to participate in selecting their government, as well as freedom of expression, freedom of association, and a free media. |
| Regulatory Quality | Perceptions of the ability of the government to formulate and implement sound policies and regulations that permit and promote private sector development. |
| Political Stability and Absence of Violence | Political Stability and Absence of Violence/Terrorism measures perceptions of the likelihood of political instability and/or politically- motivated violence, including terrorism. |
| Government Effectiveness | Perceptions of the quality of public services, the quality of the civil service and the degree of its independence from political pressures, the quality of policy formulation and implementation, and the credibility of the government's commitment to such policies. |
| Control of Corruption | Perceptions of the extent to which public power is exercised for private gain, including both petty and grand forms of corruption, as well as "capture" of the state by elites and private interests. |

Table 8: Partial correlations between estimates of $D$ and World Governance Indicators (1 year blocked panel)

| | Frandsen[a] | | | McCrary[b] | | |
|---|---|---|---|---|---|---|
| | (1) Min age | (2) Min age (pc) | (3) Placebo | (4) Min age | (5) Min age (pc) | (6) Placebo |
| **Rule of law** - no controls | 0.079 | 0.10** | 0.018 | 0.046 | 0.062 | 0.016 |
| | (0.051) | (0.051) | (0.021) | (0.039) | (0.045) | (0.014) |
| with basic controls | 0.029 | 0.057 | -0.032* | 0.055* | 0.072** | 0.0030 |
| | (0.029) | (0.036) | (0.019) | (0.030) | (0.036) | (0.034) |
| + other WGIs | 0.0086 | 0.12** | 0.0058 | 0.050 | 0.054 | 0.066 |
| | (0.047) | (0.054) | (0.040) | (0.049) | (0.061) | (0.072) |
| + adjacent bin controls | 0.025 | 0.057 | -0.035* | 0.036 | 0.067* | -0.0034 |
| | (0.030) | (0.042) | (0.020) | (0.027) | (0.038) | (0.034) |
| **Gov't effectiveness** - no controls | 0.086* | 0.12** | 0.018 | 0.043 | 0.068* | -0.0060 |
| | (0.048) | (0.048) | (0.020) | (0.037) | (0.037) | (0.013) |
| with basic controls | 0.032 | 0.052 | -0.038* | 0.033 | 0.065** | -0.021 |
| | (0.030) | (0.032) | (0.020) | (0.027) | (0.033) | (0.029) |
| + other WGIs | 0.038 | 0.067 | -0.035 | -0.026 | 0.038 | -0.033 |
| | (0.041) | (0.049) | (0.033) | (0.038) | (0.055) | (0.046) |
| + adjacent bin controls | 0.033 | 0.048 | -0.028 | 0.024 | 0.050 | -0.027 |
| | (0.030) | (0.035) | (0.021) | (0.025) | (0.031) | (0.025) |
| **Regulatory quality** - no controls | 0.086* | 0.12** | 0.033* | 0.058 | 0.071* | 0.028* |
| | (0.048) | (0.049) | (0.019) | (0.036) | (0.042) | (0.015) |
| with basic controls | 0.033 | 0.020 | -0.023 | 0.059** | 0.034 | 0.015 |
| | (0.026) | (0.034) | (0.021) | (0.027) | (0.034) | (0.026) |
| + other WGIs | 0.017 | -0.073 | 0.013 | 0.060* | -0.058 | 0.052* |
| | (0.034) | (0.045) | (0.028) | (0.035) | (0.046) | (0.030) |
| + adjacent bin controls | 0.029 | 0.023 | -0.019 | 0.043* | 0.029 | 0.0057 |
| | (0.028) | (0.039) | (0.018) | (0.025) | (0.034) | (0.022) |
| **Voice & accountability** - no controls | -0.052 | 0.022 | -0.0076 | -0.048* | -0.011 | -0.0045 |
| | (0.035) | (0.041) | (0.020) | (0.027) | (0.039) | (0.018) |
| with basic controls | -0.0082 | 0.0071 | -0.012 | 0.014 | 0.041 | 0.0082 |
| | (0.028) | (0.036) | (0.024) | (0.032) | (0.035) | (0.034) |
| + other WGIs | -0.083* | -0.077 | 0.044 | -0.073 | -0.053 | 0.021 |
| | (0.047) | (0.050) | (0.034) | (0.047) | (0.053) | (0.037) |
| + adjacent bin controls | -0.0035 | 0.011 | -0.035 | 0.011 | 0.042 | -0.020 |
| | (0.029) | (0.042) | (0.022) | (0.029) | (0.038) | (0.029) |
| **Political stability** - no controls | 0.052 | 0.084* | 0.0020 | 0.029 | 0.055 | 0.014 |
| | (0.054) | (0.049) | (0.020) | (0.040) | (0.047) | (0.018) |
| with basic controls | 0.027 | 0.0023 | -0.024 | 0.041* | 0.025 | 0.010 |
| | (0.025) | (0.027) | (0.017) | (0.023) | (0.024) | (0.022) |
| + other WGIs | 0.038 | -0.025 | -0.023 | 0.034 | -0.016 | 0.0016 |
| | (0.028) | (0.034) | (0.023) | (0.027) | (0.033) | (0.031) |
| + adjacent bin controls | 0.016 | 0.0064 | -0.034* | 0.024 | 0.027 | -0.0053 |
| | (0.025) | (0.030) | (0.018) | (0.023) | (0.028) | (0.021) |
| **Control of corruption** - no controls | -0.0019 | 0.055 | -0.013 | -0.013 | 0.045 | -0.013 |
| | (0.037) | (0.038) | (0.019) | (0.027) | (0.034) | (0.017) |
| with basic controls | 0.025 | 0.039 | -0.045** | 0.034 | 0.092** | -0.060** |
| | (0.038) | (0.033) | (0.019) | (0.032) | (0.036) | (0.023) |
| + other WGIs | 0.0014 | -0.0034 | -0.041 | 0.0022 | 0.097 | -0.12*** |
| | (0.046) | (0.046) | (0.029) | (0.035) | (0.060) | (0.041) |
| + adjacent bin controls | 0.039 | 0.034 | -0.023 | 0.037 | 0.082** | -0.037* |
| | (0.038) | (0.036) | (0.022) | (0.027) | (0.034) | (0.021) |
| Observations | 590 | 558 | 601 | 585 | 546 | 597 |
| Countries | 60 | 59 | 60 | 60 | 59 | 60 |

Each cell indicates an estimate of the partial correlation between a World Governance Indicator and an outcome. 'Min age', 'min age (pc)' and 'placebo' indicate that the cutoff used in the outcome construction was the minimum age without parental consent, with parental consent, and the placebo cutoff, respectively. The WGI measure in bold indicates the WGI measure used in the regression. No controls indicate as a bivariate regression. Basic controls are the log of per capita GDP, the log of the test sample size, the Polity IV measure of democracy, region fixed effects and year block fixed effects. '+ other WGIs' indicates that all other WGIs are also included as controls. '+ adjacent bin controls' indicates controls for the slopes of bins immediately to the left and right of the cutoff. Standard errors clustered at the country level, $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$

Table 9: Partial correlations between estimates of $D$ and World Governance Indicators (3 year blocked panel)

| | Frandsen[a] | | | McCrary[b] | | |
|---|---|---|---|---|---|---|
| | (1) Min age | (2) Min age (pc) | (3) Placebo | (4) Min age | (5) Min age (pc) | (6) Placebo |
| **Rule of law** - no controls | 0.076* | 0.10** | 0.013 | 0.039 | 0.078* | -0.014 |
| | (0.045) | (0.044) | (0.022) | (0.033) | (0.040) | (0.013) |
| with basic controls | 0.032 | 0.057* | -0.029 | 0.029 | 0.066* | -0.037** |
| | (0.022) | (0.033) | (0.020) | (0.020) | (0.037) | (0.017) |
| + other WGIs | 0.033 | 0.12* | 0.0064 | 0.040 | 0.10 | 0.036 |
| | (0.047) | (0.066) | (0.033) | (0.040) | (0.078) | (0.034) |
| + adjacent bin controls | 0.032 | 0.060* | -0.025 | 0.032* | 0.064** | -0.036** |
| | (0.023) | (0.031) | (0.021) | (0.019) | (0.032) | (0.015) |
| **Gov't effectiveness** - no controls | 0.077* | 0.12** | 0.011 | 0.038 | 0.077* | -0.037** |
| | (0.044) | (0.044) | (0.020) | (0.032) | (0.039) | (0.014) |
| with basic controls | 0.031 | 0.060* | -0.037* | 0.020 | 0.074* | -0.065*** |
| | (0.025) | (0.031) | (0.022) | (0.022) | (0.038) | (0.020) |
| + other WGIs | 0.038 | 0.094 | -0.042 | 0.017 | 0.12* | -0.090*** |
| | (0.034) | (0.057) | (0.034) | (0.026) | (0.066) | (0.032) |
| + adjacent bin controls | 0.035 | 0.043 | -0.023 | 0.029 | 0.046 | -0.054*** |
| | (0.025) | (0.028) | (0.021) | (0.019) | (0.030) | (0.017) |
| **Regulatory quality** - no controls | 0.072* | 0.12*** | 0.017 | 0.049 | 0.066* | -0.012 |
| | (0.043) | (0.041) | (0.018) | (0.032) | (0.036) | (0.016) |
| with basic controls | 0.023 | 0.023 | -0.032 | 0.030 | 0.027 | -0.029 |
| | (0.019) | (0.028) | (0.021) | (0.019) | (0.032) | (0.019) |
| + other WGIs | -0.0059 | -0.073 | -0.015 | 0.025 | -0.086* | 0.032 |
| | (0.029) | (0.047) | (0.026) | (0.026) | (0.050) | (0.027) |
| + adjacent bin controls | 0.022 | 0.032 | -0.019 | 0.029* | 0.029 | -0.018 |
| | (0.020) | (0.025) | (0.019) | (0.017) | (0.029) | (0.015) |
| **Voice & accountability** - no controls | -0.032 | 0.057 | -0.012 | -0.027 | 0.054 | -0.034** |
| | (0.034) | (0.043) | (0.022) | (0.022) | (0.035) | (0.017) |
| with basic controls | -0.0076 | 0.020 | 0.0057 | -0.016 | 0.031 | -0.032 |
| | (0.024) | (0.036) | (0.022) | (0.024) | (0.038) | (0.022) |
| + other WGIs | -0.082* | -0.049 | 0.079** | -0.092** | -0.065 | 0.024 |
| | (0.046) | (0.052) | (0.037) | (0.042) | (0.056) | (0.036) |
| + adjacent bin controls | 0.00045 | 0.016 | -0.0066 | 0.00091 | 0.019 | -0.040* |
| | (0.023) | (0.035) | (0.022) | (0.022) | (0.033) | (0.022) |
| **Political stability** - no controls | 0.049 | 0.093** | -0.0058 | 0.029 | 0.092*** | -0.016 |
| | (0.046) | (0.042) | (0.021) | (0.034) | (0.032) | (0.015) |
| with basic controls | 0.021 | -0.00077 | -0.019 | 0.022 | 0.024 | -0.016 |
| | (0.021) | (0.026) | (0.017) | (0.019) | (0.030) | (0.015) |
| + other WGIs | 0.024 | -0.037 | -0.025 | 0.028 | 0.00062 | -0.0099 |
| | (0.029) | (0.034) | (0.019) | (0.027) | (0.041) | (0.020) |
| + adjacent bin controls | 0.021 | 0.016 | -0.020 | 0.024 | 0.029 | -0.020 |
| | (0.021) | (0.023) | (0.018) | (0.019) | (0.022) | (0.014) |
| **Control of corruption** - no controls | -0.0053 | 0.055 | -0.025 | -0.018 | 0.032 | -0.030 |
| | (0.039) | (0.038) | (0.020) | (0.023) | (0.036) | (0.018) |
| with basic controls | 0.027 | 0.027 | -0.036* | 0.013 | 0.039 | -0.061*** |
| | (0.032) | (0.032) | (0.019) | (0.025) | (0.036) | (0.015) |
| + other WGIs | 0.0032 | -0.042 | -0.023 | -0.015 | -0.041 | -0.044 |
| | (0.041) | (0.049) | (0.026) | (0.030) | (0.051) | (0.029) |
| + adjacent bin controls | 0.035 | 0.011 | -0.018 | 0.028 | 0.012 | -0.048*** |
| | (0.032) | (0.029) | (0.020) | (0.023) | (0.029) | (0.015) |
| Observations | 286 | 275 | 290 | 286 | 271 | 289 |
| Countries | 61 | 59 | 61 | 61 | 58 | 61 |

Each cell indicates an estimate of the partial correlation between a World Governance Indicator and an outcome. 'Min age', 'min age (pc)' and 'placebo' indicate that the cutoff used in the outcome construction was the minimum age without parental consent, with parental consent, and the placebo cutoff, respectively. The WGI measure in bold indicates the WGI measure used in the regression. No controls indicate as a bivariate regression. Basic controls are the log of per capita GDP, the log of the test sample size, the Polity IV measure of democracy, region fixed effects and year block fixed effects. '+ other WGIs' indicates that all other WGIs are also included as controls. '+ adjacent bin controls' indicates controls for the slopes of bins immediately to the left and right of the cutoff. Standard errors clustered at the country level, $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$

Table 10: Partial correlations between estimates of $D$ and World Governance Indicators (6 year blocked panel)

| | Frandsen[a] | | | McCrary[b] | | |
|---|---|---|---|---|---|---|
| | (1) Min age | (2) Min age (pc) | (3) Placebo | (4) Min age | (5) Min age (pc) | (6) Placebo |
| **Rule of law** - no controls | 0.079* | 0.10** | 0.017 | 0.028 | 0.070* | -0.0079 |
| | (0.045) | (0.046) | (0.022) | (0.038) | (0.041) | (0.011) |
| with basic controls | 0.032 | 0.038 | -0.026 | 0.024 | 0.050 | -0.028* |
| | (0.024) | (0.035) | (0.020) | (0.024) | (0.036) | (0.017) |
| + other WGIs | 0.015 | 0.068 | 0.021 | 0.035 | 0.050 | 0.043 |
| | (0.055) | (0.081) | (0.038) | (0.044) | (0.085) | (0.037) |
| + adjacent bin controls | 0.031 | 0.048 | -0.020 | 0.038* | 0.067* | -0.026 |
| | (0.023) | (0.032) | (0.021) | (0.022) | (0.034) | (0.015) |
| **Gov't effectiveness** - no controls | 0.086* | 0.12*** | 0.016 | 0.033 | 0.083* | -0.022* |
| | (0.045) | (0.046) | (0.019) | (0.039) | (0.042) | (0.011) |
| with basic controls | 0.034 | 0.052 | -0.030 | 0.015 | 0.065* | -0.041** |
| | (0.025) | (0.034) | (0.022) | (0.024) | (0.038) | (0.017) |
| + other WGIs | 0.037 | 0.11 | -0.032 | 0.017 | 0.15** | -0.029 |
| | (0.036) | (0.072) | (0.042) | (0.026) | (0.070) | (0.036) |
| + adjacent bin controls | 0.034 | 0.030 | -0.014 | 0.034 | 0.054 | -0.031* |
| | (0.024) | (0.029) | (0.023) | (0.021) | (0.033) | (0.017) |
| **Regulatory quality** - no controls | 0.075* | 0.11*** | 0.022 | 0.042 | 0.068* | -0.0052 |
| | (0.042) | (0.043) | (0.020) | (0.033) | (0.037) | (0.013) |
| with basic controls | 0.026 | 0.0083 | -0.032 | 0.032 | 0.018 | -0.030 |
| | (0.023) | (0.034) | (0.024) | (0.020) | (0.035) | (0.020) |
| + other WGIs | -0.0048 | -0.087 | -0.023 | 0.035 | -0.092* | -0.0069 |
| | (0.035) | (0.060) | (0.038) | (0.030) | (0.054) | (0.037) |
| + adjacent bin controls | 0.023 | 0.023 | -0.020 | 0.036* | 0.030 | -0.021 |
| | (0.022) | (0.028) | (0.020) | (0.019) | (0.030) | (0.015) |
| **Voice & accountability** - no controls | -0.025 | 0.091* | -0.019 | -0.031 | 0.080** | -0.025 |
| | (0.034) | (0.050) | (0.024) | (0.027) | (0.038) | (0.016) |
| with basic controls | 0.0099 | 0.013 | 0.0023 | -0.0065 | 0.031 | -0.011 |
| | (0.025) | (0.042) | (0.028) | (0.028) | (0.041) | (0.025) |
| + other WGIs | -0.053 | -0.050 | 0.077 | -0.075* | -0.065 | 0.043 |
| | (0.051) | (0.065) | (0.052) | (0.041) | (0.066) | (0.043) |
| + adjacent bin controls | 0.015 | 0.0046 | 0.000039 | 0.019 | 0.024 | -0.016 |
| | (0.024) | (0.038) | (0.025) | (0.025) | (0.037) | (0.022) |
| **Political stability** - no controls | 0.050 | 0.11** | -0.0079 | 0.019 | 0.10*** | -0.014 |
| | (0.047) | (0.044) | (0.024) | (0.041) | (0.033) | (0.017) |
| with basic controls | 0.019 | -0.0081 | -0.027 | 0.023 | 0.030 | -0.015 |
| | (0.023) | (0.031) | (0.017) | (0.024) | (0.031) | (0.015) |
| + other WGIs | 0.019 | -0.028 | -0.042* | 0.032 | 0.029 | -0.018 |
| | (0.030) | (0.042) | (0.021) | (0.033) | (0.047) | (0.022) |
| + adjacent bin controls | 0.017 | 0.020 | -0.031 | 0.031 | 0.043* | -0.019 |
| | (0.023) | (0.022) | (0.018) | (0.023) | (0.024) | (0.015) |
| **Control of corruption** - no controls | 0.0020 | 0.067 | -0.025 | -0.035 | 0.039 | -0.031* |
| | (0.039) | (0.043) | (0.022) | (0.030) | (0.037) | (0.017) |
| with basic controls | 0.033 | 0.034 | -0.034 | 0.0030 | 0.038 | -0.057*** |
| | (0.032) | (0.036) | (0.024) | (0.029) | (0.038) | (0.016) |
| + other WGIs | 0.0090 | -0.0088 | -0.024 | -0.040 | -0.038 | -0.072** |
| | (0.046) | (0.069) | (0.038) | (0.036) | (0.069) | (0.031) |
| + adjacent bin controls | 0.036 | 0.014 | -0.013 | 0.028 | 0.029 | -0.043*** |
| | (0.032) | (0.033) | (0.023) | (0.026) | (0.033) | (0.015) |
| Observations | 153 | 147 | 155 | 153 | 145 | 155 |
| Countries | 61 | 59 | 61 | 61 | 58 | 61 |

Each cell indicates an estimate of the partial correlation between a World Governance Indicator and an outcome. 'Min age', 'min age (pc)' and 'placebo' indicate that the cutoff used in the outcome construction was the minimum age without parental consent, with parental consent, and the placebo cutoff, respectively. The WGI measure in bold indicates the WGI measure used in the regression. No controls indicate as a bivariate regression. Basic controls are the log of per capita GDP, the log of the test sample size, the Polity IV measure of democracy, region fixed effects and year block fixed effects. '+ other WGIs' indicates that all other WGIs are also included as controls. '+ adjacent bin controls' indicates controls for the slopes of bins immediately to the left and right of the cutoff. Standard errors clustered at the country level, $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$

Table 11: Partial correlations between positive, significant $D$ and World Governance Indicators (1 year blocked panel)

| | Frandsen[a] | | | McCrary[b] | | |
|---|---|---|---|---|---|---|
| | (1) Min age | (2) Min age (pc) | (3) Placebo | (4) Min age | (5) Min age (pc) | (6) Placebo |
| **Rule of law** - no controls | 0.038* | 0.036* | 0.0047 | 0.025 | 0.047* | -0.0038 |
| | (0.019) | (0.021) | (0.014) | (0.021) | (0.026) | (0.013) |
| with basic controls | 0.052** | 0.071** | -0.020 | 0.012 | 0.047 | -0.022 |
| | (0.024) | (0.028) | (0.016) | (0.020) | (0.029) | (0.014) |
| + other WGIs | 0.049 | 0.024 | 0.025 | 0.0061 | 0.041 | -0.022 |
| | (0.042) | (0.060) | (0.037) | (0.037) | (0.054) | (0.031) |
| + adjacent bin controls | 0.047** | 0.082*** | -0.019 | 0.0087 | 0.047 | -0.024* |
| | (0.024) | (0.025) | (0.017) | (0.019) | (0.032) | (0.014) |
| **Gov't effectiveness** - no controls | 0.029* | 0.027 | 0.0030 | 0.018 | 0.037* | -0.0064 |
| | (0.017) | (0.019) | (0.014) | (0.019) | (0.022) | (0.014) |
| with basic controls | 0.037* | 0.061** | -0.015 | 0.0064 | 0.053** | -0.020 |
| | (0.020) | (0.024) | (0.014) | (0.015) | (0.024) | (0.013) |
| + other WGIs | -0.0099 | -0.0058 | 0.023 | -0.0070 | 0.077** | -0.010 |
| | (0.032) | (0.043) | (0.033) | (0.022) | (0.037) | (0.024) |
| + adjacent bin controls | 0.033 | 0.064*** | -0.0075 | 0.0018 | 0.049* | -0.020 |
| | (0.020) | (0.021) | (0.015) | (0.015) | (0.026) | (0.013) |
| **Regulatory quality** - no controls | 0.023 | 0.034** | -0.0016 | 0.014 | 0.022 | -0.0017 |
| | (0.015) | (0.017) | (0.014) | (0.019) | (0.025) | (0.014) |
| with basic controls | 0.042** | 0.066*** | -0.021 | 0.0068 | 0.013 | -0.011 |
| | (0.018) | (0.023) | (0.017) | (0.017) | (0.029) | (0.012) |
| + other WGIs | 0.013 | 0.035 | -0.022 | -0.0019 | -0.079* | 0.018 |
| | (0.028) | (0.037) | (0.032) | (0.021) | (0.041) | (0.018) |
| + adjacent bin controls | 0.039** | 0.055** | -0.016 | 0.0070 | 0.013 | -0.0099 |
| | (0.019) | (0.022) | (0.016) | (0.017) | (0.032) | (0.011) |
| **Voice & accountability** - no controls | 0.0095 | -0.0091 | -0.0048 | -0.019 | 0.018 | -0.0057 |
| | (0.019) | (0.022) | (0.016) | (0.016) | (0.021) | (0.015) |
| with basic controls | 0.044** | 0.051* | -0.024 | 0.021 | 0.048* | -0.023 |
| | (0.021) | (0.029) | (0.021) | (0.019) | (0.028) | (0.016) |
| + other WGIs | 0.0075 | -0.032 | -0.0067 | 0.021 | 0.0077 | -0.014 |
| | (0.035) | (0.041) | (0.035) | (0.022) | (0.041) | (0.027) |
| + adjacent bin controls | 0.039* | 0.051** | -0.035 | 0.017 | 0.033 | -0.027 |
| | (0.021) | (0.024) | (0.023) | (0.019) | (0.031) | (0.018) |
| **Political stability** - no controls | 0.011 | 0.0063 | -0.0019 | 0.00033 | 0.042 | -0.0013 |
| | (0.017) | (0.021) | (0.015) | (0.026) | (0.026) | (0.013) |
| with basic controls | 0.022 | 0.043* | -0.023 | 0.011 | 0.023 | -0.0028 |
| | (0.019) | (0.021) | (0.017) | (0.019) | (0.027) | (0.0097) |
| + other WGIs | -0.0072 | 0.0094 | -0.018 | 0.0029 | -0.0031 | 0.014 |
| | (0.022) | (0.028) | (0.023) | (0.024) | (0.033) | (0.014) |
| + adjacent bin controls | 0.0055 | 0.035 | -0.025 | 0.0086 | 0.018 | -0.0059 |
| | (0.018) | (0.021) | (0.018) | (0.019) | (0.031) | (0.011) |
| **Control of corruption** - no controls | 0.013 | 0.032 | -0.0093 | -0.012 | 0.017 | -0.0050 |
| | (0.015) | (0.024) | (0.011) | (0.015) | (0.021) | (0.013) |
| with basic controls | 0.035 | 0.073*** | -0.031* | 0.0069 | 0.044* | -0.020 |
| | (0.022) | (0.025) | (0.018) | (0.015) | (0.022) | (0.014) |
| + other WGIs | 0.00035 | 0.051 | -0.043* | -0.00069 | 0.0047 | -0.0069 |
| | (0.028) | (0.038) | (0.025) | (0.020) | (0.032) | (0.021) |
| + adjacent bin controls | 0.044** | 0.074*** | -0.021 | 0.010 | 0.042* | -0.017 |
| | (0.020) | (0.023) | (0.018) | (0.014) | (0.025) | (0.014) |
| Observations | 590 | 557 | 601 | 590 | 558 | 601 |
| Countries | 60 | 59 | 60 | 60 | 59 | 60 |

Each cell indicates an estimate of the partial correlation between a World Governance Indicator and an outcome. 'Min age', 'min age (pc)' and 'placebo' indicate that the cutoff used in the outcome construction was the minimum age without parental consent, with parental consent, and the placebo cutoff, respectively. The WGI measure in bold indicates the WGI measure used in the regression. No controls indicate as a bivariate regression. Basic controls are the log of per capita GDP, the log of the test sample size, the Polity IV measure of democracy, region fixed effects and year block fixed effects. '+ other WGIs' indicates that all other WGIs are also included as controls. '+ adjacent bin controls' indicates controls for the slopes of bins immediately to the left and right of the cutoff. Standard errors clustered at the country level, $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$

Table 12: Partial correlations between positive, significant $D$ and World Governance Indicators (3 year blocked panel)

| | Frandsen[a] | | | McCrary[b] | | |
|---|---|---|---|---|---|---|
| | (1) Min age | (2) Min age (pc) | (3) Placebo | (4) Min age | (5) Min age (pc) | (6) Placebo |
| **Rule of law** - no controls | 0.027 | 0.022 | -0.016 | 0.031 | 0.065 | 0.0035 |
| | (0.028) | (0.032) | (0.027) | (0.031) | (0.048) | (0.018) |
| with basic controls | 0.056 | 0.092** | -0.050** | 0.039 | 0.11** | -0.015 |
| | (0.035) | (0.045) | (0.024) | (0.027) | (0.045) | (0.016) |
| + other WGIs | 0.086 | 0.10 | -0.044 | 0.036 | 0.12 | -0.043 |
| | (0.063) | (0.080) | (0.034) | (0.065) | (0.094) | (0.047) |
| + adjacent bin controls | 0.052 | 0.11*** | -0.048** | 0.036 | 0.11** | -0.017 |
| | (0.036) | (0.039) | (0.024) | (0.026) | (0.047) | (0.017) |
| **Gov't effectiveness** - no controls | 0.025 | -0.0028 | -0.014 | 0.025 | 0.050 | -0.0089 |
| | (0.026) | (0.029) | (0.025) | (0.027) | (0.041) | (0.018) |
| with basic controls | 0.057* | 0.036 | -0.038* | 0.032 | 0.10** | -0.021 |
| | (0.030) | (0.037) | (0.022) | (0.022) | (0.042) | (0.016) |
| + other WGIs | 0.083* | -0.079 | 0.011 | 0.024 | 0.096 | -0.054 |
| | (0.049) | (0.077) | (0.028) | (0.036) | (0.086) | (0.035) |
| + adjacent bin controls | 0.059* | 0.058* | -0.029 | 0.036 | 0.094** | -0.018 |
| | (0.031) | (0.032) | (0.023) | (0.022) | (0.043) | (0.016) |
| **Regulatory quality** - no controls | -0.0017 | 0.024 | -0.0075 | 0.022 | 0.025 | 0.0051 |
| | (0.028) | (0.031) | (0.027) | (0.027) | (0.049) | (0.017) |
| with basic controls | 0.018 | 0.059 | -0.032 | 0.033 | 0.050 | 0.0030 |
| | (0.029) | (0.040) | (0.026) | (0.024) | (0.057) | (0.014) |
| + other WGIs | -0.078 | 0.017 | 0.012 | 0.0080 | -0.11 | 0.051 |
| | (0.050) | (0.061) | (0.031) | (0.041) | (0.087) | (0.032) |
| + adjacent bin controls | 0.022 | 0.052 | -0.022 | 0.034 | 0.040 | 0.0058 |
| | (0.030) | (0.036) | (0.025) | (0.024) | (0.057) | (0.015) |
| **Voice & accountability** - no controls | -0.013 | -0.0069 | -0.034 | -0.013 | 0.045 | -0.013 |
| | (0.030) | (0.034) | (0.022) | (0.029) | (0.041) | (0.017) |
| with basic controls | 0.030 | 0.081 | -0.036 | 0.019 | 0.092* | -0.014 |
| | (0.035) | (0.052) | (0.032) | (0.031) | (0.051) | (0.025) |
| + other WGIs | -0.039 | 0.0076 | 0.011 | -0.039 | -0.034 | -0.0040 |
| | (0.049) | (0.067) | (0.038) | (0.041) | (0.078) | (0.039) |
| + adjacent bin controls | 0.034 | 0.085** | -0.050 | 0.024 | 0.088 | -0.017 |
| | (0.035) | (0.042) | (0.034) | (0.030) | (0.053) | (0.027) |
| **Political stability** - no controls | 0.0046 | 0.030 | -0.013 | 0.0018 | 0.063 | 0.0044 |
| | (0.032) | (0.037) | (0.027) | (0.031) | (0.040) | (0.018) |
| with basic controls | 0.019 | 0.10*** | -0.021 | 0.029 | 0.083* | 0.0026 |
| | (0.028) | (0.034) | (0.022) | (0.025) | (0.043) | (0.012) |
| + other WGIs | -0.0039 | 0.074* | 0.0048 | 0.022 | 0.040 | 0.015 |
| | (0.034) | (0.041) | (0.027) | (0.036) | (0.062) | (0.019) |
| + adjacent bin controls | 0.0044 | 0.080** | -0.021 | 0.024 | 0.088* | 0.0020 |
| | (0.029) | (0.035) | (0.021) | (0.025) | (0.044) | (0.013) |
| **Control of corruption** - no controls | -0.012 | -0.0078 | -0.038 | -0.022 | 0.0092 | 0.0056 |
| | (0.022) | (0.032) | (0.023) | (0.022) | (0.041) | (0.019) |
| with basic controls | 0.039 | 0.054 | -0.054** | 0.019 | 0.085* | -0.0015 |
| | (0.032) | (0.040) | (0.024) | (0.027) | (0.043) | (0.016) |
| + other WGIs | -0.019 | -0.021 | -0.045** | -0.020 | -0.015 | 0.034 |
| | (0.040) | (0.061) | (0.021) | (0.041) | (0.071) | (0.037) |
| + adjacent bin controls | 0.050 | 0.061* | -0.044* | 0.023 | 0.071 | 0.0015 |
| | (0.032) | (0.034) | (0.023) | (0.028) | (0.044) | (0.015) |
| Observations | 286 | 274 | 289 | 286 | 275 | 290 |
| Countries | 61 | 59 | 61 | 61 | 59 | 61 |

Each cell indicates an estimate of the partial correlation between a World Governance Indicator and an outcome. 'Min age', 'min age (pc)' and 'placebo' indicate that the cutoff used in the outcome construction was the minimum age without parental consent, with parental consent, and the placebo cutoff, respectively. The WGI measure in bold indicates the WGI measure used in the regression. No controls indicate as a bivariate regression. Basic controls are the log of per capita GDP, the log of the test sample size, the Polity IV measure of democracy, region fixed effects and year block fixed effects. '+ other WGIs' indicates that all other WGIs are also included as controls. '+ adjacent bin controls' indicates controls for the slopes of bins immediately to the left and right of the cutoff. Standard errors clustered at the country level, $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$

Table 13: Partial correlations between positive, significant $D$ and World Governance Indicators (6 year blocked panel)

| | Frandsen[a] | | | McCrary[b] | | |
|---|---|---|---|---|---|---|
| | (1) Min age | (2) Min age (pc) | (3) Placebo | (4) Min age | (5) Min age (pc) | (6) Placebo |
| **Rule of law** - no controls | 0.031 | 0.027 | -0.0089 | 0.016 | 0.049 | 0.0035 |
| | (0.034) | (0.046) | (0.033) | (0.041) | (0.059) | (0.032) |
| with basic controls | 0.085* | 0.093 | -0.037 | 0.040 | 0.10** | -0.033 |
| | (0.047) | (0.060) | (0.034) | (0.040) | (0.048) | (0.030) |
| + other WGIs | 0.14 | 0.18 | 0.014 | 0.0031 | 0.096 | 0.062 |
| | (0.089) | (0.12) | (0.048) | (0.072) | (0.11) | (0.062) |
| + adjacent bin controls | 0.086* | 0.14*** | -0.034 | 0.038 | 0.10** | -0.034 |
| | (0.047) | (0.050) | (0.033) | (0.038) | (0.051) | (0.030) |
| **Gov't effectiveness** - no controls | 0.041 | 0.010 | -0.016 | 0.0073 | 0.029 | -0.012 |
| | (0.031) | (0.044) | (0.032) | (0.038) | (0.054) | (0.031) |
| with basic controls | 0.080* | 0.038 | -0.036 | 0.038 | 0.082* | -0.047* |
| | (0.041) | (0.055) | (0.033) | (0.039) | (0.046) | (0.028) |
| + other WGIs | 0.071 | -0.096 | -0.0071 | 0.025 | 0.079 | -0.028 |
| | (0.069) | (0.12) | (0.046) | (0.073) | (0.11) | (0.037) |
| + adjacent bin controls | 0.090** | 0.082* | -0.022 | 0.041 | 0.087* | -0.041 |
| | (0.040) | (0.043) | (0.032) | (0.040) | (0.049) | (0.029) |
| **Regulatory quality** - no controls | 0.019 | 0.028 | -0.025 | 0.0064 | 0.017 | -0.017 |
| | (0.031) | (0.041) | (0.033) | (0.039) | (0.055) | (0.032) |
| with basic controls | 0.052 | 0.042 | -0.050 | 0.049 | 0.056 | -0.060** |
| | (0.038) | (0.051) | (0.034) | (0.038) | (0.056) | (0.027) |
| + other WGIs | -0.055 | -0.034 | -0.052 | 0.045 | -0.061 | -0.069* |
| | (0.065) | (0.085) | (0.042) | (0.054) | (0.093) | (0.035) |
| + adjacent bin controls | 0.066* | 0.049 | -0.042 | 0.044 | 0.042 | -0.055** |
| | (0.038) | (0.042) | (0.032) | (0.038) | (0.057) | (0.026) |
| **Voice & accountability** - no controls | -0.026 | -0.00070 | -0.036 | -0.052 | 0.040 | -0.012 |
| | (0.038) | (0.047) | (0.031) | (0.037) | (0.047) | (0.029) |
| with basic controls | 0.041 | 0.11 | -0.016 | -0.0053 | 0.069 | -0.043 |
| | (0.062) | (0.071) | (0.048) | (0.040) | (0.057) | (0.045) |
| + other WGIs | -0.054 | 0.092 | 0.053 | -0.11 | -0.092 | -0.0032 |
| | (0.10) | (0.13) | (0.055) | (0.067) | (0.11) | (0.055) |
| + adjacent bin controls | 0.024 | 0.10* | -0.019 | 0.0074 | 0.076 | -0.048 |
| | (0.060) | (0.054) | (0.047) | (0.039) | (0.056) | (0.045) |
| **Political stability** - no controls | -0.018 | 0.012 | -0.019 | -0.010 | 0.083* | -0.016 |
| | (0.035) | (0.041) | (0.034) | (0.044) | (0.049) | (0.033) |
| with basic controls | 0.00029 | 0.065 | -0.030 | 0.033 | 0.095* | -0.024 |
| | (0.045) | (0.049) | (0.026) | (0.035) | (0.049) | (0.030) |
| + other WGIs | -0.048 | -0.0094 | -0.032 | 0.046 | 0.082 | -0.020 |
| | (0.053) | (0.074) | (0.035) | (0.042) | (0.076) | (0.041) |
| + adjacent bin controls | -0.0052 | 0.072 | -0.035 | 0.030 | 0.092* | -0.028 |
| | (0.044) | (0.043) | (0.025) | (0.034) | (0.051) | (0.029) |
| **Control of corruption** - no controls | -0.010 | -0.0063 | -0.035 | -0.033 | 0.0030 | -0.0066 |
| | (0.032) | (0.045) | (0.029) | (0.033) | (0.053) | (0.032) |
| with basic controls | 0.066 | 0.053 | -0.038 | 0.041 | 0.080* | -0.035 |
| | (0.053) | (0.051) | (0.028) | (0.049) | (0.045) | (0.026) |
| + other WGIs | -0.024 | -0.038 | -0.020 | 0.017 | -0.018 | -0.0097 |
| | (0.071) | (0.090) | (0.021) | (0.069) | (0.084) | (0.043) |
| + adjacent bin controls | 0.081 | 0.071* | -0.022 | 0.050 | 0.079* | -0.027 |
| | (0.050) | (0.042) | (0.028) | (0.049) | (0.046) | (0.027) |
| Observations | 152 | 147 | 155 | 153 | 147 | 155 |
| Countries | 61 | 59 | 61 | 61 | 59 | 61 |

Each cell indicates an estimate of the partial correlation between a World Governance Indicator and an outcome. 'Min age', 'min age (pc)' and 'placebo' indicate that the cutoff used in the outcome construction was the minimum age without parental consent, with parental consent, and the placebo cutoff, respectively. The WGI measure in bold indicates the WGI measure used in the regression. No controls indicate as a bivariate regression. Basic controls are the log of per capita GDP, the log of the test sample size, the Polity IV measure of democracy, region fixed effects and year block fixed effects. '+ other WGIs' indicates that all other WGIs are also included as controls. '+ adjacent bin controls' indicates controls for the slopes of bins immediately to the left and right of the cutoff. Standard errors clustered at the country level, ${}^{*}p < 0.10, {}^{**}p < 0.05, {}^{***}p < 0.01$

Table 14: Partial correlations between rule of law and discontinuity size estimates using DHS/MICS combined sample

| | Frandsen[a] | | | McCrary[b] | | |
|---|---|---|---|---|---|---|
| | (1) Min age | (2) Min age (pc) | (3) Placebo | (4) Min age | (5) Min age (pc) | (6) Placebo |
| *1 year bins* | | | | | | |
| Bivariate | 0.041 | 0.091*** | 0.034** | 0.024 | 0.063** | -0.010 |
| | (0.032) | (0.031) | (0.017) | (0.025) | (0.026) | (0.017) |
| /w basic controls | 0.071*** | 0.084** | 0.027 | 0.061** | 0.054 | 0.044 |
| | (0.022) | (0.034) | (0.021) | (0.024) | (0.035) | (0.028) |
| + other WGIs | 0.073 | 0.12 | 0.057 | 0.11 | 0.041 | 0.14 |
| | (0.079) | (0.083) | (0.059) | (0.079) | (0.072) | (0.092) |
| + adjacent bin controls | 0.063*** | 0.097*** | 0.029 | 0.065** | 0.079** | 0.047* |
| | (0.022) | (0.031) | (0.021) | (0.025) | (0.032) | (0.027) |
| *2 year bins* | | | | | | |
| Bivariate | 0.041 | 0.091*** | 0.034** | 0.024 | 0.063** | -0.010 |
| | (0.032) | (0.031) | (0.017) | (0.025) | (0.026) | (0.017) |
| /w basic controls | 0.071*** | 0.084** | 0.027 | 0.061** | 0.054 | 0.044 |
| | (0.022) | (0.034) | (0.021) | (0.024) | (0.035) | (0.028) |
| + other WGIs | 0.073 | 0.12 | 0.057 | 0.11 | 0.041 | 0.14 |
| | (0.079) | (0.083) | (0.059) | (0.079) | (0.072) | (0.092) |
| + adjacent bin controls | 0.063*** | 0.097*** | 0.029 | 0.065** | 0.079** | 0.047* |
| | (0.022) | (0.031) | (0.021) | (0.025) | (0.032) | (0.027) |
| *4 year bins* | | | | | | |
| Bivariate | 0.041 | 0.091*** | 0.034** | 0.024 | 0.063** | -0.010 |
| | (0.032) | (0.031) | (0.017) | (0.025) | (0.026) | (0.017) |
| /w basic controls | 0.071*** | 0.084** | 0.027 | 0.061** | 0.054 | 0.044 |
| | (0.022) | (0.034) | (0.021) | (0.024) | (0.035) | (0.028) |
| + other WGIs | 0.073 | 0.12 | 0.057 | 0.11 | 0.041 | 0.14 |
| | (0.079) | (0.083) | (0.059) | (0.079) | (0.072) | (0.092) |
| + adjacent bin controls | 0.063*** | 0.097*** | 0.029 | 0.065** | 0.079** | 0.047* |
| | (0.022) | (0.031) | (0.021) | (0.025) | (0.032) | (0.027) |
| *6 year bins* | | | | | | |
| Bivariate | 0.041 | 0.091*** | 0.034** | 0.024 | 0.063** | -0.010 |
| | (0.032) | (0.031) | (0.017) | (0.025) | (0.026) | (0.017) |
| /w basic controls | 0.071*** | 0.084** | 0.027 | 0.061** | 0.054 | 0.044 |
| | (0.022) | (0.034) | (0.021) | (0.024) | (0.035) | (0.028) |
| + other WGIs | 0.073 | 0.12 | 0.057 | 0.11 | 0.041 | 0.14 |
| | (0.079) | (0.083) | (0.059) | (0.079) | (0.072) | (0.092) |
| + adjacent bin controls | 0.063*** | 0.097*** | 0.029 | 0.065** | 0.079** | 0.047* |
| | (0.022) | (0.031) | (0.021) | (0.025) | (0.032) | (0.027) |
| Observations | 221 | 212 | 223 | 221 | 209 | 223 |
| Countries | 86 | 83 | 86 | 86 | 82 | 86 |

Each cell indicates an estimate of the partial correlation between the WGI Rule of Law and an outcome. ‘Min age’, ‘min age (pc)’ and ‘placebo’ indicate that the cutoff used in the outcome construction was the minimum age without parental consent, with parental consent, and the placebo cutoff, respectively. The WGI measure in bold indicates the WGI measure used in the regression. No controls indicate as a bivariate regression. Basic controls are the log of per capita GDP, the log of the test sample size, the Polity IV measure of democracy, region fixed effects and year block fixed effects. ‘+ other WGIs’ indicates that all other WGIs are also included as controls. ‘+ adjacent bin controls’ indicates controls for the slopes of bins immediately to the left and right of the cutoff. Standard errors clustered at the country level, $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$

Table 15: Partial correlations between government effectiveness and discontinuity size estimates using DHS/MICS combined sample

| | Frandsen[a] | | | McCrary[b] | | |
|---|---|---|---|---|---|---|
| | (1) Min age | (2) Min age (pc) | (3) Placebo | (4) Min age | (5) Min age (pc) | (6) Placebo |
| *1 year bins* | | | | | | |
| Bivariate | 0.051 | 0.11*** | 0.033** | 0.033 | 0.064** | -0.020 |
| | (0.031) | (0.032) | (0.016) | (0.025) | (0.025) | (0.012) |
| /w basic controls | 0.076*** | 0.085*** | 0.024 | 0.063*** | 0.041 | 0.012 |
| | (0.021) | (0.030) | (0.021) | (0.024) | (0.034) | (0.019) |
| + other WGIs | 0.10** | 0.12 | 0.028 | 0.085** | -0.0059 | -0.052 |
| | (0.041) | (0.073) | (0.049) | (0.038) | (0.061) | (0.042) |
| + adjacent bin controls | 0.065*** | 0.067** | 0.027 | 0.068*** | 0.070** | 0.017 |
| | (0.022) | (0.027) | (0.021) | (0.022) | (0.030) | (0.019) |
| *2 year bins* | | | | | | |
| Bivariate | 0.051 | 0.11*** | 0.033** | 0.033 | 0.064** | -0.020 |
| | (0.031) | (0.032) | (0.016) | (0.025) | (0.025) | (0.012) |
| /w basic controls | 0.076*** | 0.085*** | 0.024 | 0.063*** | 0.041 | 0.012 |
| | (0.021) | (0.030) | (0.021) | (0.024) | (0.034) | (0.019) |
| + other WGIs | 0.10** | 0.12 | 0.028 | 0.085** | -0.0059 | -0.052 |
| | (0.041) | (0.073) | (0.049) | (0.038) | (0.061) | (0.042) |
| + adjacent bin controls | 0.065*** | 0.067** | 0.027 | 0.068*** | 0.070** | 0.017 |
| | (0.022) | (0.027) | (0.021) | (0.022) | (0.030) | (0.019) |
| *4 year bins* | | | | | | |
| Bivariate | 0.051 | 0.11*** | 0.033** | 0.033 | 0.064** | -0.020 |
| | (0.031) | (0.032) | (0.016) | (0.025) | (0.025) | (0.012) |
| /w basic controls | 0.076*** | 0.085*** | 0.024 | 0.063*** | 0.041 | 0.012 |
| | (0.021) | (0.030) | (0.021) | (0.024) | (0.034) | (0.019) |
| + other WGIs | 0.10** | 0.12 | 0.028 | 0.085** | -0.0059 | -0.052 |
| | (0.041) | (0.073) | (0.049) | (0.038) | (0.061) | (0.042) |
| + adjacent bin controls | 0.065*** | 0.067** | 0.027 | 0.068*** | 0.070** | 0.017 |
| | (0.022) | (0.027) | (0.021) | (0.022) | (0.030) | (0.019) |
| *6 year bins* | | | | | | |
| Bivariate | 0.051 | 0.11*** | 0.033** | 0.033 | 0.064** | -0.020 |
| | (0.031) | (0.032) | (0.016) | (0.025) | (0.025) | (0.012) |
| /w basic controls | 0.076*** | 0.085*** | 0.024 | 0.063*** | 0.041 | 0.012 |
| | (0.021) | (0.030) | (0.021) | (0.024) | (0.034) | (0.019) |
| + other WGIs | 0.10** | 0.12 | 0.028 | 0.085** | -0.0059 | -0.052 |
| | (0.041) | (0.073) | (0.049) | (0.038) | (0.061) | (0.042) |
| + adjacent bin controls | 0.065*** | 0.067** | 0.027 | 0.068*** | 0.070** | 0.017 |
| | (0.022) | (0.027) | (0.021) | (0.022) | (0.030) | (0.019) |
| Observations | 221 | 212 | 223 | 221 | 209 | 223 |
| Countries | 86 | 83 | 86 | 86 | 82 | 86 |

Each cell indicates an estimate of the partial correlation between the WGI Government Effectiveness and an outcome. 'Min age', 'min age (pc)' and 'placebo' indicate that the cutoff used in the outcome construction was the minimum age without parental consent, with parental consent, and the placebo cutoff, respectively. The WGI measure in bold indicates the WGI measure used in the regression. No controls indicate as a bivariate regression. Basic controls are the log of per capita GDP, the log of the test sample size, the Polity IV measure of democracy, region fixed effects and year block fixed effects. '+ other WGIs' indicates that all other WGIs are also included as controls. '+ adjacent bin controls' indicates controls for the slopes of bins immediately to the left and right of the cutoff. Standard errors clustered at the country level, $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$