# HOW WOMEN'S RIGHTS AFFECT FERTILITY EVIDENCE FROM NIGERIA

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**Abstract**

This paper estimates the impact on fertility of a 1999 reform that reduced litigants' rights for Muslim women in certain states of Nigeria. Using data from Demographic and Health Surveys, I find that, where enforced, the Reform increased the yearly probability of giving birth by 0.026. This effect stems from both a shift of fertility decisions within marriage towards husbands' preferences and an increase in the probability of being married. The change in marital status alone may explain 60% of the total increase in fertility. I also find that the enforced Reform increased women's labour supply.

Equal legal rights for women is a defining feature of developed countries (Doepke et al. 2012). Whether this feature explains part or all of the low fertility that also characterizes developed countries is an open question. In fact, studies show that legal differences across countries may have little observable consequences on development (Herbst 2000, Michalopoulos and Papaioannou 2013), which may result from lack of enforcement of the law (Acemoglu and Johnson 2005) or from the overpowering influence of cultural or ethnic norms (Nunn 2013, Spolaore and Wacziarg 2014).

This paper estimates the impact of a legal reform adopted in Nigeria in 1999 that reduced female litigants' rights. It makes two contributions. First, it examines the role of the justice system on demographic outcomes. It thus addresses the question of the role of the legal system on development, raised in the studies cited above, and complements studies of the effects of other women's legal rights, such as enfranchisement (Miller 2008, Kose et al. 2016). I interpret theoretically the impact of this reform (hereafter capitalized) as a reduction in a woman's utility outside marriage.

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The second contribution of the paper is to assess the effects of this reduction. Unlike determinants of fertility identified in the empirical studies discussed below, the reform has had no impact on the opportunity cost of having a child or preventing a pregnancy.

For most of the 19th century, the northern half of today's Nigeria was a unified Empire, under the rule of a local interpretation of Islamic law. This system was largely kept in place during the British colonisation that lasted from the early 20th century to 1959. When Nigeria became independent in 1960, legal issues were partitioned between criminal and non-criminal matters. Non-criminal matters could still be judged under secular, Islamic, or customary law, according to the choice of the parties involved.[1] Criminal matters were now to be judged by secular courts, under a Penal Code valid for everyone that also included prescriptions from Islamic law for Muslims only.

In 1999, after Nigeria adopted a new Constitution, twelve states that cover Northern Nigeria, hereafter *Reform States*, launched a reform of their legal system in order to reinstate what had been abolished under British rule and subsequent independence. For Muslims only, the Reform brought certain criminal matters once again under the jurisdiction of Islamic courts, as they had been before independence. Even though the Reform was formally adopted by twelve states, it has been little enforced outside of three states, hereafter *High-Enforcement States*. High-Enforcement States include the capital and major cities of the former Empire, which still host administrative and religious authorities.

By moving criminal cases to Islamic courts, the Reform reduced Muslim women's rights in court, or litigants' rights, in cases of illegal sexual behavior, which includes all extra-marital relations. This change led to an increase in convictions for illicit sexual behavior among women. The modification of litigants' rights also abolished the legal protection of women against sexual abuse. Many women convicted of illicit sexual behavior after the Reform are reported to be victims of rape, unable to prove that the relations were not consensual. The Reform thus formally reduced women's public protection from violence. In practice, single women have been disproportionately affected by the Reform.

I study the effects of this Reform in two series of estimations. The first series estimates the impact of the Reform on fertility. The second series of estimations aims at identifying the mechanism that explains this impact. All estimations use data from the Standard *Demographic and Health*

[1]Non-criminal matters encompass family matters, i.e. marriage, divorce, inheritance, etc. as well as commercial matters.

*Surveys* (DHS) for Nigeria. DHS are surveys conducted every few years on samples of men and women. Each survey contains information on the number of children a woman surveyed has had, the date of birth of every child, and other geographic, demographic, and economic information.

To assess the impact of the Reform on fertility, I use the longitudinal form of the data from the survey run in 2008, with one observation by woman and by year, for the years 1993 to 2007, and I define an indicator variable equal to 1 if a woman gave birth in a given year. I then conduct both a graphical analysis and a series of regressions to estimate the fertility change among Muslim women living in High-Enforcement States. These women were the only individuals exposed to the enforced Reform, and I refer to them as the treatment group. All specifications include women fixed effects, as well as interactions between years and women's ages indicator variables, to control for the impact of age on pregnancy.

I estimate a statistically significant increase in the fertility of Muslim women in High-Enforcement States, relative to all other women. Numerically, the Reform increased the probability of giving birth in a given year by around 0.026, which amounts to 1 additional child for every 40 women or to around 10% of the sample's mean probability of giving birth. This increase is robust to controlling for relevant control groups. In addition, there is no change in fertility for Muslim women living in states that adopted but did not enforce the Reform.

These results convey the first contribution of this paper: where enforced, women's legal rights have a causal impact on fertility. The identification assumption is that no change other than that in female litigants' rights affected women in High-Enforcement States after 1999. This assumption is consistent with the historical context of the Reform, which implies that the timing of the legal change was unexpected, and unrelated to any hypothetical change in Muslims' ideology, social norms, religious fervor, etc. The identification assumption is also supported by the graphical representation of the evolution of women's fertility over time for the main demographic groups, and by the results of a series of robustness checks and falsification tests.

What explains the increase in fertility after the Reform? Empirical and experimental studies of developing countries have identified three broad factors that affect fertility: access to modern methods of contraception (Goldin and Katz 2002, Bailey 2006, Ashraf et al. 2014, Miller and Babiarz 2016), electrification and diffusion of household appliances (Bailey and Collins 2011, Lewis 2013, Grimm et al. 2015), and change in education supply or work opportunities (Breierova and Duflo 2004, Jensen 2012, Aaronson et al. 2014, Duflo et al. 2015, Heath and Mobarak 2015, Lavy and Zablotsky 2015).

All of these factors affect either the cost of preventing a birth or the cost of having a child, through access to technology or increase in work opportunities.[2] The Reform studied here is independent of a change in these costs. Its only reported effect is to reduce the public protection of women, especially single women, from extra-marital violence. It is this effect that I interpret as a decrease in women's utility outside of marriage. By contrast, the literature cited above identifies causes of fertility change that may affect the value of being single as one among other variables. For instance, education may affect the opportunity cost of working.

Theoretically, a relative decrease in a woman's utility outside of marriage could affect fertility through two channels. First, under certain conditions on the household decision process, a decrease in the value of being single will in general affect fertility decisions within couples by shifting all decisions – fertility as well as other issues, such as women's leisure – closer to the husband's preferences. Second, if the probability to have a child is null if a woman is not married, a decrease in the value of being single will weakly increase the probability to have a child through an increase in the probability of being married. Appendix A presents a simple model that formalizes these statements. The model assumes no commitment at the time of marriage on subsequent fertility. It follows Rasul (2008)'s main finding that, under lack of commitment, utility outside marriage may affect fertility through its impact on intra-household bargaining power.

The second series of estimations examines these two channels, and compares the effects of the Reform with those of documented determinants of fertility. For all estimations that use a dependent variable other than the yearly probability of birth, I use repeated cross-sections data, where each woman surveyed in any Standard DHS is observed only once, i.e. in the year the survey was conducted.

For the first channel, I use stated fertility preferences to categorize married women according to whether their stated "ideal number of children" is smaller than, equal to, or larger than their husbands'. I then estimate changes in fertility separately for the three subsets of women. Among women who want fewer children than their husbands do, fertility increased for women of the treatment group. Among women who want as many children as their husbands do, fertility did not

[2]The increase in work opportunities often stems from its interaction with "technology" in a general sense. It is the existence of a technology that requires skilled labour which motivates education improvement and increases both the opportunity cost of the time spent rearing children and the cost of educating a child (Becker and Lewis 1973).

change significantly for women of the treatment group. And among women who want more children than their husbands do, fertility decreased for women of the treatment group. In short, the fertility of women in the treatment group moved in the direction of husbands' preferences. Numerically, the marginal increase estimated in the first subsample is comparable in absolute terms to the marginal decrease estimated in the third subsample, but the number of married women who want fewer children than their husbands do is almost twice as large as the number of married women who want more children than their husbands do. On average, the impact of the Reform on fertility decisions within couples led to a net increase in the probability to have a child.

For the second channel, I estimate that the probability to be married in any given year significantly increases after the Reform among Muslim women in High-Enforcement States. The increase in the probability of being married should unambiguously increase fertility.

The Reform thus led to an increase in fertility through both channels. Under certain assumptions, I estimate that the first and the second channel may respectively explain 40 and 60 percent of the total increase of fertility for women of the treatment group.

After examining the two channels, I discuss the difference between the impact of the Reform and the documented impact of a change in the cost of having a child. There is no evidence that the Reform directly or indirectly affected that cost – through, for instance, any decrease in education. In fact, most women in the sample left school (if they ever went) before the Reform. In addition, I estimate that women's labour supply increased after the Reform. This increase stems exclusively from married women, and is consistent with the predictions of the model. It is also the exact opposite of the effect that a decrease in education or in labour demand would induce. I also show no evidence of any change in two other potential causes of a change in fertility: access to family planning and stated fertility preferences.

The rest of the paper is organized as follows. Section 1 describes the historical context and the content of the Reform, and cites anecdotal evidence of its effects. Section 2 presents the data used in this study. Section 3 sets the specification and the identification assumptions. Section 4 presents a graphical analysis of the Reform and the main estimations of its effects on fertility. Section 5 examines the mechanism that can explain the impact of the Reform on fertility and other outcomes, and compares the effects of the Reform to the effects of determinants of fertility change identified in the literature. Section 6 presents additional results and robustness checks. Section 7 concludes.

## 1. The Reform

### 1.1 *The Context of the Reform*

Modern Nigeria is populated mostly by Christians in the South and Muslims in the North.[3] The North corresponds roughly to the territory of the "Sokoto Empire", which was in place for most of the 19th century. This Empire resulted from a military conquest that started from the city of Sokoto, located in the current state of the same name. The central objective of the conquest, in the minds of its leaders, was to impose a certain interpretation of Islamic law throughout the Empire.

The Sokoto Empire was dismantled during the British conquest of contemporary Nigeria, completed in 1901. Under British rule, the legal system of the North remained untouched, except for the abolishment of some types of corporal punishments perceived as barbaric, such as amputation or stoning. In addition, non-Muslims acquired the right to be judged by secular courts.

In 1960 Nigeria became independent and adopted a Constitution that concentrated power in the national government and left little power to local authorities. Criminal law came under the rule of a new Penal Code, which incorporated British law but maintained for Muslims certain prescriptions from Islamic law. The main effect of the new Penal Code was to place criminal cases in the hands of secular courts for all citizens, Muslim or not. The Penal Code also contained rules for Family affairs, even as parties could still choose to be judged by Islamic law or Customary law for these matters.[4]

After independence, some Muslim religious and political authorities, backed by popular support, demanded the reinstatement of the legal system in place before colonization. Yet the centralized nature of the state and a series of military dictatorships deprived states of any substantial authority over their legal system (Laitin 1982).

### 1.2 *The Formal Content of the Reform*

After the elections of 1999, the governors of 12 northern states – the states that approximately cover most of the area of the Sokoto Empire – hereafter *Reform States*, launched the Reform of their legal system that is the study of this paper. Some of these 12 states (e.g. Kano) adopted a new Penal Code, whereas others (e.g. Niger) only added to the pre-Reform Penal Code some

[3]This section is based on the work of historians Falola and Heaton (2008).

[4]Commercial law also continued to be ruled by Islamic law in the North (Peters 2005).

clauses specific to Muslims.[5] In spite of these different approaches, all states re-introduced more or less the same prescriptions.

Formally, the Reform changed the previous legal system in two ways: it reintroduced certain corporal punishments banned under British colonization, and, by placing Criminal cases for Muslims under the jurisdiction of Islamic courts, it reintroduced, for these cases, the trial procedures that were used before independence (and still used for Family affairs). Secular courts remained in place for Non-Muslim parties.

The reintroduced corporal punishments, namely amputation and stoning, follow explicit religious prescriptions regarding the punishment to be delivered to a person convicted of theft, murder, or illicit sexual relations (by which is meant all extra-marital and homosexual relations). These behaviors were already illegal before the Reform, when they were punished by jail, fees, and/or, for the case of illicit sexual relations, flogging.

Regarding trial procedures, the Reform lowered the value of a woman's testimony in court. The value of a woman's testimony depends on the case being judged. In general it is now worth half the value of a man's testimony or is discarded altogether (Ibrahim 2012, Peters 2005). Similarly, to oppose a single male defendant, a woman must provide at least two witnesses.

### 1.3 *Consequences of the Reform*

This section examines the implementation of the Reform. I present anecdotal evidence on the following three topics: the general effects of the Reform, the effect of the Reform on violence against women, and the difference in implementation across states.

*Anecdotal evidence of the general effects of the Reform.* Studies of the Reform stress that its effects have been limited, both formally and in practice. In the words of R. Peters, scholar of Islamic law, 'Sharia has for a long time been accepted as part of the legal systems of the Northern states in the domain of family, civil and commercial law' (Peters 2005, p.173). In addition, the Penal Code already included prescriptions from Islamic law for Muslims only. As the anthropologist M. Last explains: 'Only three domains are seriously affected: women in public [...]; alcohol and non-military music and singing' (Last 2008).

The reintroduction of certain corporal punishments has been described as largely symbolic

[5]The state governorships elections took place in January 1999 and the enactment of the first new Penal Code in January 2000. The imminent implementation of the Reform likely became "publicly known" sometime between these two dates.

because, in practice, convictions are systematically overturned on appeal by upper courts of governors. Legal scholars Ibrahim (2012), Peters (2001) and Weimann (2010) further explain that if a sentence of such corporal punishment were to reach the Supreme Court, which is secular, it would be overturned because the Constitution explicitly bans such cruel punishment.[6]

There is no evidence of any impact (positive or negative) of the Reform on theft or murder or corruption (Last 2008, Campbell 2013, p. 53). There is also no evidence of any additional change in policy for Muslim individuals, men or women. Indeed, the Constitution explicitly bans any public policy (such as welfare) that is religion-based (Peters 2005).

*Anecdotal evidence of the impact of the Reform on violence against women.* The Reform made women more vulnerable to extra-marital violence. According to a report on the effect of the Reform by the NGO Baobab for Women's Rights, which is the main Nigerian women's rights organization, since 1999, 'attacks on women outside of their homes have been common' (Baobab 2003).

A. Imam, the Nigerian executive director of that NGO, points to the devaluing of women's testimony to account for this increased vulnerability to extra-marital violence, especially to rape: 'Reporting rape is thus equivalent to confessing [to unlawful sexual behavior]. In the most probable situation of lack of two witnesses or a confession from the rapist, rape would be hard to prove, and so women would find themselves not only subject to [illicit sexual behavior] punishments, but also liable for false witness in addition. Thus the [Reform] deprive[s] women of protection from rape and sexual assaults' (Imam 2004). Indeed, Weimann (2010) finds that, in a sample of post-Reform trials, many more women than men were convicted of unlawful sexual behavior between a man and a woman. Lawyer Hauwa Ibrahim confirms this point: 'If a woman reports to the police that she was raped, her report can easily be misconstrued as a confession to unlawful intercourse, thus exposing her to the possibility of [...] punishment for [illicit sexual behavior]' (Ibrahim 2012, p.137). Among the hundreds of cases Ibrahim worked on, the majority seem to have involved a woman accused of illicit sexual relations. In almost all of these cases, the male parties were allowed to swear an oath of innocence and were subsequently discharged and acquitted, while the female

[6]There are reports that on a few occasions the sentence was delivered and executed to convicts who did not lodge an appeal. The practice quickly came to a halt. Weimann (2010) states that 'the execution of harsh punishment such as amputation or stoning to death was soon hampered by mounting opposition. The right hands of three convicted thieves were amputated between March and July 2001. No amputation has been reported after 2001' (p.120). In addition, 'no stoning sentence has been carried out' (p.120).

parties were not allowed to swear a similar oath (Ibrahim 2012, p.98).

The formal and actual effects of the Reform were much publicized as soon as it was launched, both by its opponents, as attested by the reports cited above, and by its supporters. For instance, one of the first documented legal cases was the trial of a thirteen-year-old unmarried girl who became pregnant in early 2000 in Zamfara. This girl 'was sentenced to be flogged 180 times for [...] the pregnancy', but also 'for telling lies against the three men she alleged to have raped her'. The governor of Zamfara 'wanted to make an example' of her and therefore pushed for the sentence to be applied (Ibrahim 2012, p.28).

Anecdotal evidence on the Reform shows that single women (whether never married or divorced) were more likely to suffer from the consequences of the Reform. Independent of the Reform, single women are more vulnerable to violence. Izugbara and Ezeh (2010), who interviewed Muslim women from Northern Nigeria, report that 'respondents reported deliberately giving birth to many children in order to prevent men from divorcing them [...]. These women sought marital stability [and] protection.'[7] A decrease in state protection may thus affect single women disproportionately. The fact that they are more vulnerable may result from the help that families provide against crime. In the 2012 Afrobarometer survey for Nigeria, 21.5 percent women answer '[their] own family or friends' to the question 'If a victim of crime, whom to go to first for assistance'.[8]

There is no evidence that any other substantial change affected Muslim women's lives after the Reform.[9]

*Anecdotal evidence on enforcement across states.* The Reform seems to have been little implemented (at least before 2008) except in certain geographic areas.[10] The areas where the Reform

[7] In estimations unreported here, I use Afrobarometer data for Nigeria to estimate that the probability to have been victim of an attack is larger if a woman is the head of the household, controlling for her age. (There is no indication of marital status, so I interpret the fact that a women is the head of her household as evidence that she is not currently married.) Afrobarometer Data for Nigeria, are available on http://www.afrobarometer.org (last accessed: March 2018.)

[8] The greater vulnerability of single women may also stem from the relative lack of exposure that marriage provides.

[9] Some states initially attempted to impose the separation of sexes in public transportation and a dress code for women. After it appeared that these laws affected both Muslims and non-Muslims (mostly women, though men as well), they generated protests and their enforcement was hindered. For instance, after 2002: 'The prohibition on the free mixing of the sexes has drastically slackened so that motorcyclists now carry Muslim women passengers [...]' (cited in Paden 2006, p.164).

[10] Peters (2001) reports: 'We heard many complaints that the changes were not properly introduced. The judges of the new [...] Courts were the same judges who had sat in [the pre-Reform courts], they had not been prepared nor trained to apply the changes in the legal system.'(Peters 2001).

was implemented immediately were for the most part close to the city of Sokoto, the former capital of the 19th century Empire, which still hosts administrative and religious institutions (Peters 2001, Paden 2006, Weimann 2010).[11] A quantitative study of enforcement by Weimann (2010) records cases judged under the new legal system and finds that three states – Sokoto, Zamfara and Katsina, which I call High-Enforcement States – had a distinctly higher enforcement level than other Reform states. Alone, they account for 60 percent of the cases recorded. The timing of the cases reported also shows that the Reform was implemented immediately after the Reform was launched.

Appendix B presents a map of Nigeria that indicates the location of all Reform States and of the High-Enforcement States.

## 2. Data

The source of data for this study is the collection of Standard Demographic and Health Surveys (DHS) of men and women of Nigeria conducted in 1990, 1999, 2003, 2008 and 2013. Each survey contains information on a sample of a few thousand men and women. The survey reports on the current living conditions of respondents. It also contains the date of birth of every child of every woman in the sample and additional information on these children.[12]

To assess the effect of the Reform on yearly probability of giving birth, I use a longitudinal form of the 2008 survey, with one observation by year $t$ and by woman $i$.[13] With information on births, I define an indicator variable equal to 1 if and only if woman $i$ gives birth in year $t$ (excluding non-live births). By definition, this variable is woman $i$'s probability of giving birth in year $t$. I restrict the sample to exclude irrelevant observations in which a woman is younger than 13 years old. DHS 2008 also contains a survey of men that I use to define a variable that measures the difference in fertility preferences between a woman and her husband (if she is married), in the longitudinal survey.[14]

[11]The stronger enforcement of the law in places that hosted pre-colonial political institutions is not specific to Nigeria, but has been documented across Africa (Gennaioli and Rainer 2007 and Michalopoulos and Papaioannou 2014).

[12]The data are publicly available on the following websites: dhsprogram.com/Data/ and www.idhsdata.org/idhs/ (last accessed: 15 March 2018).

[13]Relative to other surveys, DHS 2008 contains many more observations of women who were of childbearing age *both* before and after 2000. DHS 2008 contains roughly four times more observations than the sample of 2003, and roughly twice as many observations as DHS 2013 of women who were old enough to have children before the Reform.

[14]I also use the men's survey of DHS 2008 for the estimation of portential changes in fertility preferences, reported in Appendix.

Almost all variables other than the probability of giving birth are known for the year of the survey only. To assess the changes over time of other outcomes, I append data from the five DHS women's surveys into a repeated cross-sectional dataset. Every woman is observed only once in this dataset, in one of the following years: 1990, 1999, 2003, 2008 or 2013.

Table 1 reports descriptive statistics for the longitudinal and the cross-sectional datasets.[15]

## 3. Specification and Identification Assumption

For a woman $i$ in year $t$, given some dependent variable $Y_{i,t}$, the specification is a variant of:

$$Y_{i,t} = \quad c_i + \alpha\ Post_t + \beta High\ Enforcement_i \times Muslim_i \times Post_t + \gamma X_{i,t} + \epsilon_{i,t} \qquad (1)$$

The main dependent variable of interest $Y_{i,t}$ is the indicator variable equal to 1 if and only if woman $i$ gives birth in year $t$. The main explanatory variables are all indicator variables or interactions of indicator variables. $High\ Enforcement_i$ is equal to 1 if and only if woman $i$ lives in one of the three states that adopted and enforced the Reform. $Muslim_i$ is equal to 1 if and only if woman $i$ is Muslim. $Post_t$ is equal to 1 if and only if $t \geq 2000$. $X_{i,t}$ is a vector of additional control variables.

When using the longitudinal dataset, I estimate the fixed effects model ($c_i$ represents women fixed effects in the equation). In all these estimations, the period of observations comprises the 15 years before the survey, i.e. 1993 to 2007.[16]

When using repeated cross-sections, I exclude women fixed effects from the specification, and include dummy variables for religious affiliations and geographic locations of residence. I estimate the coefficients of the equation in OLS.

In all estimations, standard errors are clustered at the state level. For the main coefficients of interest, I report the p-value of the coefficients of interest from the wild cluster bootstrap-t procedure.

The main coefficient of interest is $\beta$, which measures the change in the dependent variable for Muslim women in High-Enforcement States – the treatment group – relative to other women. The identification assumption is that no factor that is not controlled for and that could influence fertility decisions – or any other dependent variable of interest – changed at the same time specifically for

[15] Statistics for the men's survey of DHS 2008 are not reported.

[16] I exclude years before 1993 to limit measurement error in occurrence of birth and in date of birth long before the survey is conducted, i.e. 2008.

these women.

The historical context described above supports this assumption. The Reform does not stem from a change in fertility preferences but from a sudden legal change that affected women's legal rights. To strengthen identification, the most extensive specifications includes women fixed effects, and covariates to control for the probability of birth in a given year of all women of the same age in that year (the interactions between dummy variables for years and dummy variables for women's ages), as well as to control for changes in fertility over time of women from relevant subsets of the whole sample, primarily Muslim women.

Section 6 reports additional estimations that test the robustness of the main results to changes in infant mortality and to migration, and assess whether the variations in fertility between the treatment group and other women differed *before* 2000.

## 4. Effect of the Reform on Fertility

### 4.1 *Graphical Analysis*

This section presents a graphical analysis of fertility over time for three groups of women: Muslim women in High-Enforcement States, other Muslim women, and non-Muslim women. It uses the longitudinal data from DHS 2008.

Let $S(x, a, g)$ be the set of women from group $g$, who were $a$ years old in year $x$, and let $B(s, x, a, g)$ the set of women in $S(x, a, g)$ who gave birth in year $s$. Given $\underline{s} < \overline{s}$ and $\underline{a} < \overline{a}$, the total number of births in years $\underline{s}$ to $\overline{s}$ for women who are between $\underline{a}$ and $\overline{a}$ years old in year $x$ is:

$$\sum_{s=\underline{s}}^{\overline{s}} \sum_{a=\underline{a}}^{\overline{a}} \#B(s, x, a, g) \tag{2}$$

To limit the visual effect of measurement error in dates and occurrence of births in all figures here, I compute, for any year $x$, any age range $\underline{a}$ to $\overline{a}$, and any group $g$, the empirical yearly probability $m$ of giving birth, on average over the past four years $s \in \{x-3, x-2, x-1, x\}$:[17]

[17] Appendix C presents figures without such smoothing and discusses the sources of measurement errors in the dates or occurrence of birth.

$$m(x;\underline{a},\overline{a},g) \equiv \frac{\sum_{s=x-3}^{x}\sum_{a=\underline{a}}^{\overline{a}} \#B(s,x,a,g)}{4 \times \sum_{a=\underline{a}}^{\overline{a}} \#S(x,a,g)} \tag{3}$$

I represent variations in fertility in two different ways. First, I represent, in Figure 1, how fertility in a given age range evolves over time. All women in the longitudinal data were born between 1958 and 1993. Therefore the largest possible age-range that I can use in every year between, for instance, 1990 and 2007 is 14 to 31 (the oldest women in the sample were 31 in 1990, and the youngest were 14 in 2007). With the previous notations, for any group $g$, $y(x) \equiv m(x; 14, 31, g)$. Figure 1 represents fertility for women in that age range, separately for the treatment group and for the two other groups, as well as standard error bands.[18]

Figure 1 shows that, among Muslim women, the fertility is quite stable for the years 1993 to 1999. After 1999, the fertility of Muslim women in High-Enforcement States increases substantially. The fertility of other Muslim women seems to increase slightly as well, which may capture a small effect of the Reform among Muslim women who were living in Reform States other than High-Enforcement States, and some general increase in fertility in Nigeria, since the fertility of non-Muslim women also seems to increase around the same period. The figure also shows that the fertility of Muslim and non-Muslim women are different, in level and possibly in trend. The latter stresses the need to control for the fertility of Muslim women in the estimations.[19]

Second, I represent, in Figure 2, how the fertility of a fixed subset of women evolves over time. The inclusion of women who are too young or too old to have children will bring the plots of groups closer independent of any actual change in fertility decisions. To limit such visual effect, I keep only women who were born between 1962 and 1972, i.e. who were at least 20 in 1993 and at most 45 if 2007. With the previous notations, $y(x) \equiv m(x; x - 1972, x - 1962, g)$. Figure 2 shows an increase in the fertility of women in the treatment group 1999, relative to the fertility of other Muslim women. Since these are the same women whose probability of birth is computed in every

[18]Graphs starting in later years (e.g. 1991, 1992, etc.) could use a larger maximum age (32, 33, etc.) and would show the same increase for Muslim women in High-Enforcement States in 2000.

[19]Fertility also seems to increase slightly for all three groups around 2000, then decrease afterwards. I interpret this as a result of the fact that children born between 2004 and 2008 are declared to be born in the few years before, and that births declared in 1998 may include births actually occurring before. These are two well-documented sources of error in the measurement of birth dates in DHS that I discuss in Appendix C.

year, that increase reflects within-women changes in fertility among women of the treatment group.

In all figures, fertility seems to increase soon after 1999. This quick increase is consistent with the reports that stress the immediate implementation of the Reform in High-Enforcement States, and the publicity that was given to its consequences. The sudden increase may also stem from the specific effect of the Reform on marital status, which I discuss more in Section 5.2.

The figures also suggest that the increase in fertility of women of the treatment group, relative to the fertility of other Muslim women, is around 0.03 child by woman and by year. This order of magnitude is consistent with the estimations of the next section.

### 4.2 *Results of Estimations*

Table 2 reports the fixed effect estimations of the linear probability model defined in Equation 1, where $t = 1993$ to 2007, and $i$ is a woman in the sample of DHS 2008 aged 13 or more at $t$. The dependent variable is equal to 1 if woman $i$ gives birth at $t$, and 0 otherwise.

Column 1 reports the estimation of the specification in which the only covariates are dummy variables for years interacted with dummy variables for ages. The coefficient of the variable High-Enforcement × Muslim is positive, around 0.026 and statistically significant. Among women of the same age in a given year, women exposed to the enforced Reform have experienced a significant increase in the probability of giving birth after the Reform was adopted, relative to all other women.

In Columns 2 and following, the specifications include additional covariates to control for the potential changes in fertility for various groups of women, whose fertility may differ from the rest of the sample independent of the Reform. Column 2 controls for a potential change in fertility affecting all Muslim women after 1999 (variable Muslim × Post). Column 3 controls for a potential change in fertility affecting all Muslim women in Reform states after 1999 (variable Reform × Muslim × Post). Column 4 controls for a potential change in fertility affecting all women in Reform states after 1999 (variable Reform × Post). The sample comprises too few – fewer than fifty – non-Muslim women living in High-Enforcement States and old enough to have had the possibility to give birth both before and after 1999 to control for their specific change in fertility over time (The shares of each group are reported in Table 1).[20] Column 5 includes all these covariates.

In all these columns, the coefficient of the treatment group remains the same and significant.

[20]There are also few non-Muslim women in all Reform States in general (around 4 percent), so that the estimations of the coefficients of the covariates Reform × Post and Reform Linear Trend may be imprecise.

In addition, Columns 2 and 5 show no visible change in fertility among Muslim women who were living in states that adopted but did not enforce the Reform.

Column 6 is the most extensive specification used in the next sections. It includes the covariates to control for the change after 1999 and the linear time trend in fertility for each of the groups.

The conclusion from all estimations reported in the table is that there was a significant increase in fertility among women of the treatment group. Numerically, the yearly probability of birth increased by 0.026 among these women, which is around 10 percent of their average yearly probability of giving birth. The magnitude of this increase is unaffected by the inclusion of covariates that control for changes in fertility among various groups that may share similarities with those women but were not themselves subject to the enforced Reform. They also show no significant change in fertility among these control groups over time, relative to all other women.

## 5. Two Channels Through which the Reform Operates

### 5.1 *Impact on Fertility within Couples*

To assess whether the Reform influenced decisions within couples, an ideal experiment would be to "observe" fertility preferences among married couples before the Reform was announced and estimate Equation 1 on subsamples of married women defined by their and their husbands' statements. There is no such variable in the data, but DHS 2008 indicate for every adult surveyed (man or woman) their "ideal number of children".[21] After matching married women with their husbands, these "ideal numbers" induce a partition of the sample of married women into three subsets, depending on whether a woman's ideal number of children is larger than, equal to, or smaller than her husband's.[22][23]

Table 3 reports the results of the estimation of Equation 1 separately for each of these three subsets, for the least and most extensive specifications used in the main fertility results. Among

[21]The questionnaire aims at measuring "intrinsic" fertility preferences. The respondent is asked to state: "the ideal number of children that the respondent would have liked to have in her [or his] whole life, irrespective of the number she [or he] already has." (*Description of the Demographic and Health Surveys - Individual Recode - Data File - DHS III* p. 64). www.dhsprogram.com/pubs/pdf/DHSG4/Recode2DHS.pdf (last accessed: 15 March 2018).

[22]By definition, these three subsamples all exclude women who are not married at the time of the survey.

[23]Whenever a woman's answer is non-numeric, I assume that she wants more children than her husband does (unless he provides the same answer, in which case I assume that they have equal ideal number of children). The main conclusion from the estimations are unchanged if these observations are removed from the sample.

women whose ideal number of children is smaller than their husbands' (Columns 1 and 2), the fertility of women in the treatment group increased after 1999. Among women whose ideal number of children is equal to their husbands' (Columns 3 and 4), the fertility of women in the treatment group did not change after 1999. Among women whose ideal number of children is larger than their husbands' (Columns 5 and 6), the fertility of women in the treatment group decreased after 1999.

This last effect is not statistically significant, possibly because the high probability of pregnancy among younger women may dampen any potential negative effect of the Reform. Since almost all men want at least one child, the younger women of the sample are likely to get married for the first time and have a child after 1999, in the first years of their marriage, even if their ideal number of children is higher than their husbands'. Any potential impact of the Reform on household fertility decisions may thus be more likely to affect older women. To test that claim, I estimate the same regressions as before, after restricting the three subsamples to women born before 1981.[24] The results of these estimations, reported in Table 4, are the same as before, except that the decrease in probability of giving birth among women whose ideal number of children exceeds their husbands' is now stronger and statistically significant.

Theoretically, the results do not mean that, in all couples, the husband decides on fertility, consumption, and leisure for both spouses (which is the decision-making process considered in the model in Appendix A), but that, in some couples, this may be the case.

Numerically, the differences in size across the subsamples used in Tables 3, reported at the bottom of each column, reveal large differences in preferences between men and women. In general, women want fewer children than men. Since the magnitudes of the negative and positive impacts on fertility in the first and third subsamples are comparable, the imbalance in fertility preferences should cause an increase in fertility on average.

Remark. Since all these estimations rely on marital status recorded in 2008, some of the women in these subsamples may thus have gotten or remained married because of the Reform. I cannot estimate the specific change in fertility of women who would have remained married independent of the Reform. I discuss this point further in the next section.[25]

[24] The choice of 1981 results from the tradeoff between keeping as many observations as possible for the estimations and excluding as many women as possible in the sample who may have had a child after 2000.

[25] Another potential problem here is that stated fertility preferences in 2008 may be a noisy proxy for actual fertility preferences before the Reform. This noise could lead to imprecise estimations,

The results of this section contribute to the literature on the role of intra-household bargaining on fertility (Doepke and Kindermann 2014). The model shows that the most documented effect of the Reform – the relative decrease in utility for single women due to a relative increased vulnerability – is enough to affect fertility within couples, and the estimations identify the effects of changes in household fertility decisions *within* women.

### 5.2 *Impact on Marital Status*

This section examines the effect of the Reform on the marital status of women. To do so, I use the repeated cross-sections data formed by the five waves of DHS, conducted in 1990, 1999, 2003, 2008 and 2013. Because each woman appears only once, I do not include women fixed effects in the specification. All the estimations are in OLS.

The specification includes interactions between year of observation and age, as well as interactions between the Muslim indicator variable, dummy variables for the 11 main ethnic groups of Nigeria, and dummy variables for the 22 states of residence, according to the 1990 administrative division of the country. Between 1990 and 2013, the administrative division of the country changed twice, in 1991 and 1996: each time, new states were established. Almost every new state was created from a small part of a single pre-existing state. With minor approximations, the states of 1990 aggregate the states of 1996 and later years, and can be partitioned into High-Enforcement, Low-Enforcement, and non-Reform States.

Table 5 reports the results of these estimations. In Columns 1 and 2, the dependent variable is equal to 1 if and only a woman is married at the time of observation. The results show a significant increase in the probability that women in the treatment group are married. In Columns 3 and 4, the dependent variable is restricted to 1 if and only if the woman surveyed is married and living

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and may bias the estimated coefficients if the difference in the number of ideal children between a wife in the treatment group and her husband were disproportionately more likely to change over time in one direction or another. There is no obvious driver of such differentiated change, except perhaps anecdotal evidence on the cases of parents who align their answers with their actual number of children. For a couple in which at least one parent declares that his/her ideal number of children is similar to his/her number of children, the difference in stated ideal numbers of children in 2008 between husband and wife should be smaller than the difference in actual ideal numbers of children in 1999. Unless the stated difference becomes null, the subsamples are similar to what they would have been if we were able to observe the actual fertility preferences. If the stated difference becomes null, that couple will be wrongly put in the "equal to" subsample, which may affect the sign of the estimation of the coefficient of interest in that group. It may also reinforce or reduce the magnitude of the coefficient of the actual subsample it should belong to, but should not affect its sign.

with her husband. The results show a similar increase, which means that it is not the incentive to be legally married that changed, but the incentive to be actually living with one's husband.

In Columns 5 and 6, the dependent variable is now equal to 1 if and only a woman is currently married or was ever married. These results show no significant change after the Reform for women of the treatment group. This result is not surprising, given the high probability to get married young (Even before 2000, all Muslim women from High-Enforcement States who are 25 or older in the sample are married, divorced or widowed.) Because widowhood and divorce are common, the probability to be married *in an given year* is much smaller than the probability to have ever been married.

Because the Reform did not change family law, divorce laws remained unchanged. Yet, as the model shows, a decrease in the relative value of being single for women in the treatment group implies greater incentive to be married after the Reform. Empirically, it is the probability of remaining married that significantly increased (or of remarrying after divorce or widowhood). This effect may partly explain the quick change in fertility mentioned in Section 4.1. It may be easier to immediately react to a Reform by delaying a divorce (or not considering it), than to get married for the first time.

Numerically, around 6 percent of women who would have been single (whether divorced, widowed, or never married) without the Reform's enforcement are married in a given year after the Reform. The fertility of these women is unknown. Before 2000, 87 percent Muslim women in High-Enforcement States were married, and among these women the average probability of giving birth in the years 1993 to 1999 was 0.24. If women who are single do not give birth, a rough estimation of the average yearly probability of giving birth conditionally on being married is 0.24/0.87. Under the assumption that the additional 6 percent of women who are or remain married because of the Reform give birth with the same probability as other married women exposed to the enforced Reform – an assumption I examine in Appendix D – the additional number of births due to the increased fertility of this 6 percent, in any given year after 1999, is $0.06 \times 0.24/0.87 = 0.015$. This means that 60 (0.015/0.026) percent of the fertility change estimated above may be explained by women remarrying or remaining married, and hence having more children that they would otherwise have had.

These results are related to the few papers (Aaronson et al. 2014 and Baudin et al. 2015) that study childlessness. Between lifetime childlessness and changes in the number of children within marriage, the results here suggest the existence of a form of "temporary childlessness" through divorce or not remarrying after divorce or widowhood. Empirically, the estimations suggest that marital status may be a substantial way for women to regulate their fertility.

To test the validity of the identification assumptions for the estimations of this section, I examine pre-Reform changes in marital status. The results, reported in Appendix E, show no change in any of the dependent variables considered here, specific to women of the treatment group, between 1990 and 1999.

### 5.3 *Impact on Other Outcomes and Comparison with Identified Sources of Fertility Changes*

#### 5.3.1 *Comparison with identified sources of fertility changes*

The main estimations show a decrease in yearly probability of giving birth of 2.6 points of percentage. How does this effect compare to other determinants of fertility?

A randomized experiment in Matlab, Bangladesh, The Maternal and Child Health and Family Planning program, launched in 1977, provided married women in treatment villages with contraceptives. The exogenous provision of contraceptives led to a reduction of lifetime fertility of around 20 percent (Sinha 2005, Joshi and Schultz 2013). Because of its scope and its methodology, the Matlab randomized experiment has been extensively studied. Its impact is in the same range as other studies of access to contraceptives in developing countries (see Miller and Babiarz 2016's review). In comparison with these studies, the 0.026 increase due to the Reform amounts to around 10 percent of the average yearly probability of birth for women of the treatment group before 2000. If that number reflects the average increase in births over all fertile years of a woman, it also corresponds to the increase in lifetime fertility.

A critique of these studies is that the effect of family planning per se is hard to distinguish from the effect of potential improvement in health, since family planning is usually provided in health facilities or by health practitioners. The experiment by Ashraf et al. (2014) does not suffer from these problems. It shows that access to family planning in the form of a voucher for family planning services does decrease birth after two years, and significantly more so if the voucher is given to the woman of the couple. However, quantitatively, there is no straightforward way to assess how much private access to family planning would decrease the yearly probability of birth over a longer period. In fact, studies of access to contraceptives in the US (Goldin and Katz 2002, Bailey 2006) show that private access may affect "only" the timing of first birth and not extend beyond young adulthood.

Studies of access to education usually find small effects on lifetime pregnancy. Studies that use school expansions (Breierova and Duflo 2004, Osili and Long 2008), or compulsory schooling laws (Black et al. 2008) usually find some negative impact on early pregnancy, but are silent or find little to no effect on pregnancy at older ages, or on average probability of giving birth over time. There are two exceptions. Lavy and Zablotsky (2015) estimate a large negative impact on fertility of the increase in education due to a lift in travel bans that affected Arab women in Israel in 1963: they find that this policy change decreased completed fertility by 0.47-0.61 child, i.e. roughly 10 percent of completed fertility in the relevant group of women. Aaronson et al. (2014) find that a school-expansion program that increased education in the American South between 1913 and 1932 led to a decrease in early pregnancy (before age 22) among women who gained access to school thanks to that program. They also find that the program partly explains the fertility of women whose children could benefit from it, since women above 25 increased their probability to have a child but decreased their total number of children. Studies that do not exploit natural experiments find more contrasted results: McCrary and Royer (2011) find little impact of early schooling due to a woman's month of birth on early pregnancy in the US, Duflo et al. (2015) find that a school subsidy in Kenya decreased early pregnancy, though that effect was reduced for women who were also exposed to an HIV-prevention program.

Regarding access to work, Jensen (2012) shows experimentally that a change in women's job opportunities in India reduces the probability of marriage and childbearing for women aged 15 to 21 over three years. He finds that women exposed to these opportunities reduce their probability to give birth by 5-6 percentage points over 3 years, or 2.5-3 percentage point per year, which is comparable in absolute terms with the results of the Reform studied here. Unlike the Reform here, however, these effects may not have the same magnitude for older women. Similarly, Heath and Mobarak (2015), who document the impact of the multiplication of garment factories, find a small decrease of 0.23 percentage points over 6.4 years of the probability to have a first birth.[26]

Apart from the literature on development, studies of the baby boom and the baby bust in the US also inform determinants of fertility. Diffusion of home appliances (Greenwood et al. 2004,

[26]There are a few studies that identify electrification and diffusion of household appliances as sources of fertility change. Lewis (2013) find a small and negative impact on fertility, whereas Grimm et al. (2015) find a large effect. Given the heterogeneity of these results and the few papers, I do not attempt to compare their results with mine.

Bailey and Collins 2011), decline in maternal mortality (Albanesi and Olivetti 2014), and increase in labour supply and demand (Doepke et al. 2015, Bellou and Cardia 2016) had an overall negative impact on fertility (possibly with heterogenous effects). There is no consensus on which of the three is the main determinant of these fertility changes, occurring as they do in a context – the aftermath of the Great Depression and World War II – different from that of developing countries. What is interesting here, however, is that these papers find that an increase in women's labour participation and fertility are negatively correlated. I find here a mutual increase in labour supply and fertility, in line with two recent papers that examine the causal impact of fertility on women's labour supply (Aaronson et al. 2017, Heath 2017).

#### 5.3.2 *Impact on other outcomes*

This section estimates the impact of the Reform on outcomes other than fertility. The specification and estimation method are the same as in Section 5.2.

*Labour supply.* Table 6 presents the effect of the Reform on the labour supply of women. Columns 1 to 3 estimate the effect of the Reform on the binary variable equal to 1 if and only if the woman surveyed is working at the time of the survey. (There is no information on the number of hours worked.) I find that women in the treatment group significantly increased their probability to work. In Columns 4 to 6 (resp. 7 to 9), the dependent variable is equal to 1 if and only if the woman surveyed currently works and is married (resp. lives with her husband). By construction, the coefficients in these columns should be weakly smaller than the previous ones, and strictly smaller if the Reform increases the labour supply of single women. Instead, the coefficients are as large as before. The increase in labour supply comes entirely from married women.

These results are consistent with the predictions of the model in Appendix A: if the value of being a single woman decreases, the allocation of leisure is more likely to favor husbands, so married women are likely to work more. If, in addition, the number of children increases, the need to increase a family's income to provide for a child may be another source of that increase in women's labour supply.

Numerically, the increase in the probability to work is larger than the increase in the probability of being married, which suggests that labour supply might have increased among women who would have remained married independent of the Reform. In fact, all coefficients for the treatment group in Table 6 are large. To investigate some possible bias, I assess how the probability to work changed before the Reform. In Appendix E, I report the results of estimations that show no specific change in that probability for women of the treatment group. In Appendix F, a figure shows a large increase in the probability to be working for women of the treatment group, which does not follow from the previous changes in that probability.

These estimations are consistent with the lack of anecdotal evidence of any labour-related ban, formally or in practice, after the Reform.

*Contraception.* Table 7 shows no robust decrease in the use, knowledge or availability of contraception after the Reform. In general, the probability to have access to or to use modern contraception was small *before* 2000. It is therefore unlikely that any hypothetical impact of the Reform on contraception that these estimations would not capture could explain the fertility changes estimated above.

## 6. Robustness Checks and Falsification Tests

*Births of children who survived infancy.* In Columns 1 to 4 in Table 8, I report the results of the four main regressions – namely the regression on the whole sample of women and the three regressions on the subsamples of women partitioned by stated fertility preferences – on a dependent variable equal to 1 if and only if woman $i$ gives birth to a child who survives infancy, with the most extensive specification (Column 6 in Table 2). The results are similar to the significant estimations of Section 4.

*Migration.* To estimate the impact of the Reform on fertility, one must know in which state each woman was living when the Reform was launched, i.e. in 1999. Since this information is unavailable, I have used instead the variable $Reform$, which is equal to 1 if and only if a woman lives in a Reform state at the time of the survey, i.e. 2008 in most estimations of this paper. The measurement error stemming from using this variable might bias the main results. To examine this question, I conduct the same estimations as before, after excluding women who have moved since 2000. Columns 5 to 9 in Table 8 report the results of these regressions. The results are similar to the significant estimations of Section 4. They are consistent with reports that find no impact of the Reform on migration (e.g. Harnischfeger 2004).[27]

*Comparison with Muslim women of adjacent states.* In Table 9, I report the estimation of the main

[27] Harnischfeger 2004 explains that some non-Muslim residents of Reform States temporarily moved to non-Reform States immediately after 1998 and went back to their original location of residence soon after 2000.

regressions on the subsample of Muslim women who reside either in High-Enforcement States or in one of the five adjacent states, i.e. Jigawa, Kaduna, Kano, Kebbi or Niger (See Map in Appendix B). By construction, all women are Muslim and live in Reform States, so that there is no other group of control than women who are not in High-Enforcement States, and interactions of years and ages dummy variables control for the time variations in fertility of that group. The estimations show that the values of the main coefficient of interest are very close to its value in the general estimations.[28]

*Falsification tests and pre-2000 trends.* In Columns 1 to 6 of Table 10, I report the estimation of a change in fertility after any arbitrary year before 2000, restricting the sample of the longitudinally transformed DHS 2008 to observations from 1993 to 1999. In every column, the variable *Post* is equal to 1 if and only if the year of observation is larger or equal to the year indicated at the bottom of the column (on the *Fictitious Reform year* row). I find no evidence of any change before the Reform, which supports the main identification assumption that no significant jump in fertility occurred before the Reform, except perhaps when the fictitious Reform year is 1999. In Columns 7 and 8 of Table 10, I estimate the linear trend in fertility for Muslim women in High-Enforcement States relative to women of the other relevant groups in the years *before* 2000. These columns show no significant difference in the trend of that group, with or without covariates associated with control groups.

What explains the significant dip in Column 6? Figures reported in Appendix C show that this difference is likely due to the fact that some births that occurred in 1999 (or 1997) are reported to have happened in 1998, and more so in High-Enforcement States. This effect induces no bias in the main estimations. In fact, by definition, Column 5 shows that, on average over 1998 and 1999, there is no significant change in fertility for women of the treatment group; i.e. any birth wrongly reported in 1998 may have actually occurred in 1999. To examine this point further, I run the same falsification tests on the subsample of observations that exclude women who report being either 40 or 35 years old in 2008, i.e. born either in 1968 or in 1973. Fewer than 700 women are reported being born in any given year 1967, 1969, 1972 and 1974, whereas around 1400 are reported being born in 1968 and 1973. Removing all women born in these years from this sample may therefore remove women for whom reported ages (theirs and their children's) are more likely to be wrong, rounded to the closest multiple of 5. The estimations, reported in Table 11 show no

[28] There is no significant change in fertility for Muslim women in the subsets of Reform states/non-Reform states that share a border with a non-Reform state/Reform state.

significant change in fertility for women subject to the enforced Reform.

I also run two additional robustness tests. The first test addresses the concern that births for women in High-Enforcement States in 1999 might be disproportionately more likely to be undeclared, which could induce a positive bias on the main regressions above. To assess the size of such potential bias, I run the same regressions on the subsample that excludes all observations for the year 1999. The results, reported in Columns 1 to 4 of Table 12, are identical to the results of the main estimations. The second test addresses the concern that women who report being born in 1968 and 1973 may have had a disproportionate share on the estimated change of fertility in the main estimations. To show that that was not the case, Columns 5 to 8 of Table 12 report the main estimations excluding these women from the sample. The estimations are again identical to the results of the main estimations. (Even if all treated women, i.e. not only those who declare being born in 1968 or 1973, were more likely to declare births that really occurred in 1997 or 1999, as in 1998, that should not in itself induce any bias, since it does not affect the underlying trends in fertility or the difference in the average fertility in the years 2000 to 2007, with respect to the years 1999 and before.)

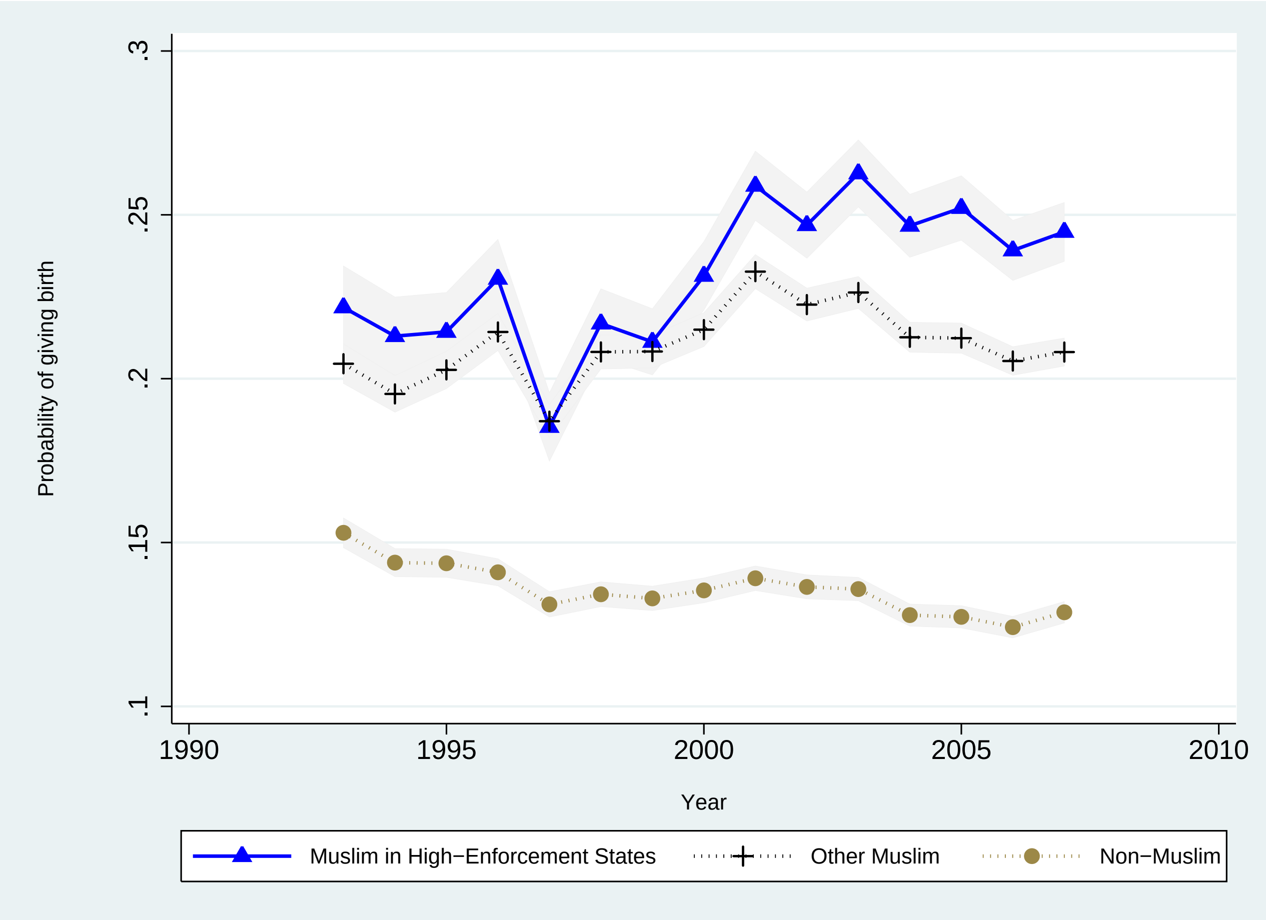
## 7. Conclusion

This paper shows that a reform that reduced certain women's legal rights led to an increase in their probability of giving birth. This increase was due both to an increase in the probability of being married, which weakly increases the probability of having a child, and a shift in married couples' fertility decisions in favor of husbands' preferences.

These results complement studies that identify sources of fertility changes, usually technological improvements or changes in labour supply incentives. These studies stress the role of non-institutional factors in women's empowerment. In fact, Doepke and Tertilt (2009) argue that certain legal rights in England and the US – such as women's right to vote – may themselves be a consequence of some technological progress that had preceded them. The paper here shows a decrease in women's empowerment that can be linked exclusively to a reduction in women's legal rights. This paper goes some way towards addressing the question of whether the source of development is legal or technological change.

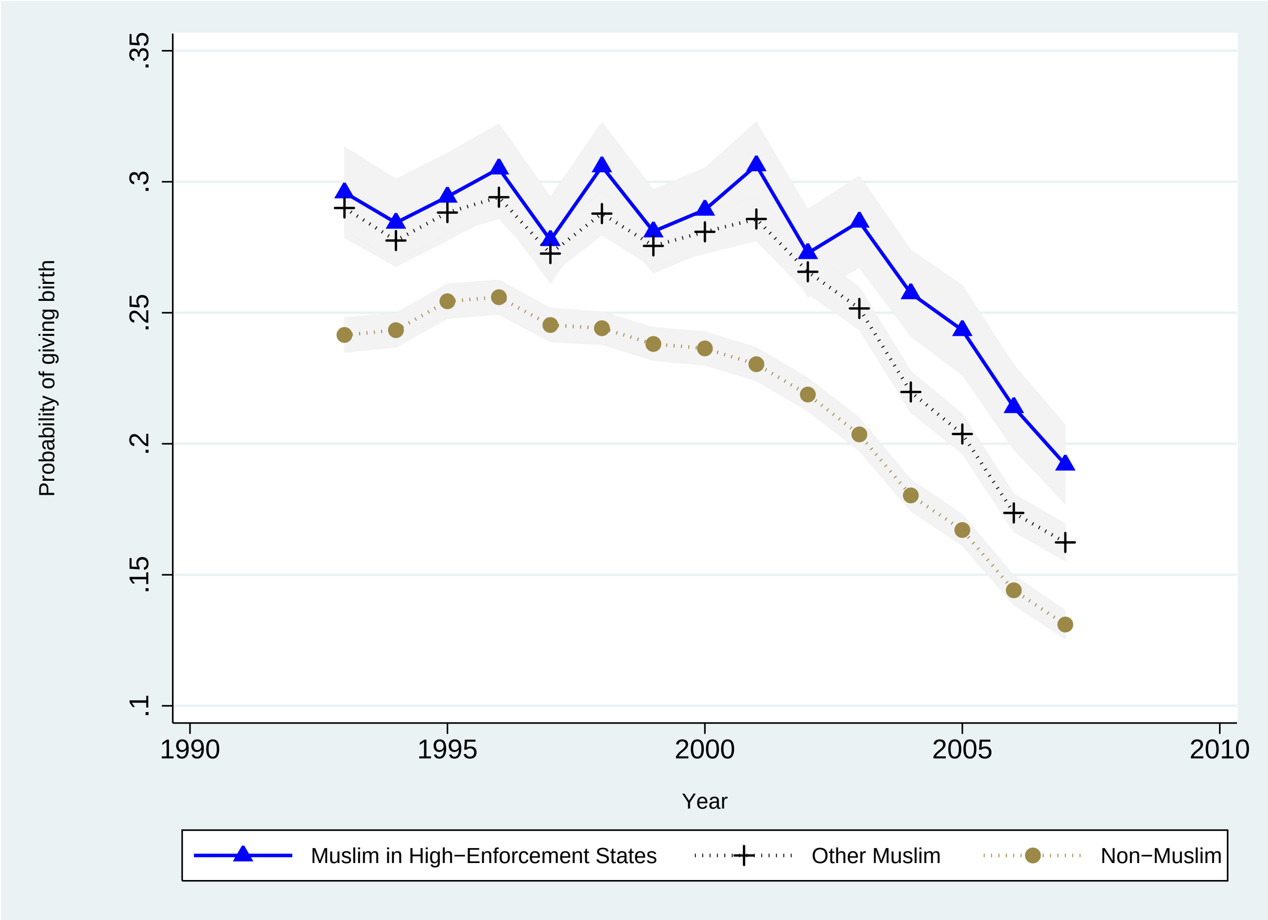
## 8. Tables and Figures

Figure 1: *Empirical Probability of Giving Birth for Women 14 to 31 Years Old*




*Notes.* For any year $x$, $y$ represents the yearly probability of giving birth, on average over the four years $\{x-3, x-2, x-1, x\}$, for all women who are between 14 and 31 years old in year $x$. See Section 4.1.

Figure 2: *Empirical Probability of Giving Birth for Women Born between 1962 and 1972*




*Notes.* For any year $x$, $y$ represents the yearly probability of giving birth, on average over the four years $\{x-3, x-2, x-1, x\}$, for all women born between 1962 and 1972. See Section 4.1.

Table 1: *Summary Statistics*

| *Longitudinal data* | | | |
|---|---|---|---|
| **Variable** | **Mean** | **Std. Dev.** | **N** |
| Year | 2000.93 | 4.21 | 382544 |
| Birth | 0.18 | 0.39 | 382544 |
| Birth (survives infancy) | 0.17 | 0.38 | 382544 |
| High-Enforcement | 0.09 | 0.29 | 382544 |
| High-Enforcement x Muslim | 0.09 | 0.28 | 382544 |
| Reform x Muslim | 0.33 | 0.47 | 382544 |
| Reform | 0.37 | 0.48 | 382544 |
| Muslim | 0.47 | 0.5 | 382544 |
| Non-Reform x Muslim | 0.14 | 0.34 | 382544 |
| Reform x Non-Muslim | 0.04 | 0.2 | 382544 |
| Born before 1981 | 0.65 | 0.48 | 382544 |
| Ideal # children smaller than husband's | 0.53 | 0.5 | 105961 |
| Ideal # children equal than husband's | 0.18 | 0.38 | 105961 |
| Ideal # children large than husband's | 0.29 | 0.46 | 105961 |
| Respondent's year of birth | 1976.12 | 8.70 | 382544 |
| Age | 24.81 | 8.52 | 382544 |
| *Cross-sectional data* | | | |
| **Variable** | **Mean** | **Std. Dev.** | **N** |
| Year | 2007.09 | 6.92 | 98544 |
| High-Enforcement | 0.12 | 0.32 | 98544 |
| High-Enforcement x Muslim | 0.11 | 0.32 | 98544 |
| Reform x Muslim | 0.33 | 0.47 | 98544 |
| Reform | 0.37 | 0.48 | 98544 |
| Muslim | 0.47 | 0.5 | 98544 |
| Non-Reform x Muslim | 0.14 | 0.34 | 98544 |
| Reform x Non-Muslim | 0.04 | 0.19 | 98544 |
| Married | 0.67 | 0.47 | 98543 |
| Married and lives with husband | 0.6 | 0.49 | 98543 |
| Ever married | 0.74 | 0.44 | 98543 |
| Works | 0.58 | 0.49 | 98009 |
| Works and is married | 0.45 | 0.5 | 98544 |
| Works and lives with husband | 0.39 | 0.49 | 98544 |
| Knows method of contraception | 0.72 | 0.45 | 98544 |
| Knows supplier of contraception method | 0.47 | 0.5 | 88174 |
| Ever used contraception | 0.24 | 0.43 | 98544 |
| Age | 28.73 | 9.69 | 98544 |
| Year of birth | 1978.36 | 11.53 | 98544 |

*Notes.* The top panel reports descriptive statistics of the longitudinal dataset variables. The bottom panel reports descriptive statistics of the cross-sectional dataset variables. Sources: Demographic and Health Surveys for Nigeria 1990, 1999, 2003, 2008 and 2013.

Table 2: *Effect of the Reform on the Probability of Giving Birth*

The dependent variable is a binary variable equal to 1 if and only if woman $i$ gives birth at $t$

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| High-Enforcement x Muslim x Post | 0.027*** | 0.025*** | 0.026*** | 0.028*** | 0.026*** | 0.026*** |
| | (0.003) | (0.004) | (0.006) | (0.005) | (0.006) | (0.006) |
| Muslim x Post | | 0.004 | | | 0.005 | 0.007 |
| | | (0.004) | | | (0.004) | (0.007) |
| Reform x Muslim x Post | | | 0.002 | | 0.009 | 0.012 |
| | | | (0.006) | | (0.008) | (0.012) |
| Reform x Post | | | | -0.001 | -0.012* | 0.008 |
| | | | | (0.005) | (0.006) | (0.009) |
| Muslim Linear Trend | | | | | | -0.000 |
| | | | | | | (0.001) |
| Reform x Muslim Linear Trend | | | | | | -0.000 |
| | | | | | | (0.002) |
| Reform Linear Trend | | | | | | -0.003* |
| | | | | | | (0.001) |
| CGM | | | | | | [0.00] |
| Women FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year x Age Interactions | Yes | Yes | Yes | Yes | Yes | Yes |
| # Observations | 382544 | 382544 | 382544 | 382544 | 382544 | 382544 |
| R2 | 0.034 | 0.034 | 0.034 | 0.034 | 0.034 | 0.034 |
| Mean of Dependent Var. | .181 | .181 | .181 | .181 | .181 | .181 |

*Notes.* This table reports the fixed effect estimations of Equation 1, using longitudinal data from DHS 2008. The sample comprises one observation by year $t$, for any year $t$ between 1993 and 2007, and by woman $i$, aged 13 or more at $t$. $High-Enforcement$ (resp. $Muslim$; $Reform$) is an indicator variable equal to 1 if $i$ lives in a High-Enforcement State (resp. is Muslim; lives in a Reform State) in 2008; Post is a dummy variable equal to 1 if $t > 1999$. The only additional covariates are the interactions of years dummy variables and women's ages dummy variables. The row CGM reports in parentheses the p-value from Cameron et al. (2008)'s wild cluster bootstrap-t procedure for the null that the coefficient on $High-Enforcement$ is zero. Nigeria is divided into 36 states and the Federal Capital, hereafter referred to as a state. Standard errors are clustered at the state level, and reported in parentheses. $*p < 0.05, **p < 0.01, ***p < 0.001$.

Table 3: *Effect of the Reform on the Probability of Giving Birth, Using Stated Fertility Preferences*

The dependent variable is a binary variable equal to 1 if and only if woman $i$ gives birth at $t$

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| High-Enforcement x Muslim x Post | 0.025*** | 0.030*** | 0.025* | 0.032 | -0.026* | -0.032* |
| | (0.005) | (0.006) | (0.013) | (0.021) | (0.015) | (0.016) |
| Muslim x Post | | -0.000 | | -0.040* | | 0.038 |
| | | (0.019) | | (0.020) | | (0.024) |
| Reform x Muslim x Post | | 0.051 | | 0.037 | | 0.049 |
| | | (0.030) | | (0.069) | | (0.047) |
| Reform x Post | | 0.005 | | 0.051 | | -0.007 |
| | | (0.021) | | (0.053) | | (0.037) |
| Muslim Linear Trend | | -0.002 | | 0.000 | | -0.004 |
| | | (0.002) | | (0.002) | | (0.003) |
| Reform x Muslim Linear Trend | | -0.004 | | -0.005 | | -0.008 |
| | | (0.003) | | (0.008) | | (0.005) |
| Reform Linear Trend | | -0.004 | | -0.004 | | 0.002 |
| | | (0.002) | | (0.006) | | (0.004) |
| CGM | | [0.01] | | | | |
| Women FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year x Age Interactions | Yes | Yes | Yes | Yes | Yes | Yes |
| # Observations | 55780 | 55780 | 19003 | 19003 | 31178 | 31178 |
| R2 | 0.044 | 0.045 | 0.062 | 0.063 | 0.055 | 0.055 |
| Mean of Dependent Var. | .231 | .231 | .193 | .193 | .218 | .218 |
| Sample: | | | | | | |
| *Ideal # children is ...* | *smaller* | *smaller* | *equal* | *equal* | *larger* | *larger* |
| ... | *than* | *than* | *to* | *to* | *than* | *than* |
| *her husband's* | | | | | | |

*Notes.* This table reports fixed effect estimations of Equation 1, using longitudinal data from DHS 2008. The whole sample comprises one observation by year $t$, for any year $t$ between 1993 and 2007, and by woman $i$, aged 13 or more at $t$. The subsample used here is restricted to women who are married, whose ideal number of children is known, and whose husband's ideal number of children is known as well. This subsample is further partitioned into three subsamples, corresponding to whether a woman's ideal number of children is smaller than (Columns 1 and 2), equal to (Columns 3 and 4) or larger (Column 5 and 6) than her husband's. (See bottom of each column.) The only additional covariates are the interactions of years dummy variables and women's ages dummy variables. Columns 1, 3 and 5 (resp. 2,4 and 6) use the least (resp. most) extensive specification. The row CGM reports in parentheses the p-value from Cameron et al. (2008)'s wild cluster bootstrap-t procedure for the null that the coefficient on $High-Enforcement \times Muslim$ is zero. Standard errors are clustered at the state level. Standard errors in parentheses. $*p < 0.05, **p < 0.01, ***p < 0.001$.

Table 4: *Effect of the Reform on the Probability of Giving Birth, Using Stated Fertility Preferences for Women Born Before 1980.*

The dependent variable is a binary variable equal to 1 if and only if woman $i$ gives birth at $t$

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| High-Enforcement x Muslim x Post | 0.023*** | 0.034*** | 0.013 | 0.036 | -0.055*** | -0.053*** |
| | (0.007) | (0.008) | (0.020) | (0.032) | (0.008) | (0.011) |
| Muslim x Post | | -0.009 | | -0.023 | | 0.056* |
| | | (0.022) | | (0.028) | | (0.032) |
| Reform x Muslim x Post | | 0.071** | | -0.063 | | -0.012 |
| | | (0.033) | | (0.075) | | (0.045) |
| Reform x Post | | -0.019 | | 0.112** | | 0.023 |
| | | (0.024) | | (0.055) | | (0.035) |
| Muslim Linear Trend | | -0.002 | | -0.003 | | -0.007* |
| | | (0.002) | | (0.004) | | (0.003) |
| Reform x Muslim Linear Trend | | -0.005 | | 0.005 | | -0.001 |
| | | (0.003) | | (0.010) | | (0.005) |
| Reform Linear Trend | | -0.002 | | -0.011 | | -0.001 |
| | | (0.002) | | (0.008) | | (0.004) |
| CGM | | [0.00] | | | | [0.06] |
| Women FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year x Age Interactions | Yes | Yes | Yes | Yes | Yes | Yes |
| # Observations | 38490 | 38490 | 12645 | 12645 | 21510 | 21510 |
| R2 | 0.031 | 0.032 | 0.050 | 0.051 | 0.044 | 0.044 |
| Mean of Dependent Var. | .249 | .249 | .209 | .209 | .235 | .235 |
| Sample: | | | | | | |
| *Ideal # children is ...* | *smaller* | *smaller* | *equal* | *equal* | *larger* | *larger* |
| ... | *than* | *than* | *to* | *to* | *than* | *than* |
| *her husband's* | | | | | | |

*Notes.* This table reports fixed effect estimations of Equation 1, using longitudinal data from DHS 2008. The whole sample comprises one observation by year $t$, for any year $t$ between 1993 and 2007, and by woman $i$, aged 13 or more at $t$. The subsample used here is restricted to women who were born in 1980 or before, who are married, whose ideal number of children is known, and whose husband's ideal number of children is known as well. This subsample is further partitioned into three subsamples, corresponding to whether a woman's ideal number of children is smaller than (Columns 1 and 2), equal to (Columns 3 and 4) or larger (Column 5 and 6) than her husband's. (See bottom of each column.) The only additional covariates are the interactions of years dummy variables and women's ages dummy variables. Columns 1, 3 and 5 (resp. 2,4 and 6) use the least (resp. most) extensive specification. The row CGM reports in parentheses the p-value from Cameron et al. (2008)'s wild cluster bootstrap-t procedure for the null that the coefficient on $High-Enforcement \times Muslim$ is zero. Standard errors are clustered at the state level. Standard errors in parentheses. $*p < 0.05, **p < 0.01, ***p < 0.001$.

Table 5: *Effect of the Reform on Marital Situation*

The dependent variable is a binary variable equal to 1 if and only if woman $i$ :

| | is married | | lives w. husband | | ever married | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| High-Enforcement x Muslim x Post | 0.058*** | 0.061*** | 0.044*** | 0.054*** | 0.020 | 0.008 |
| | (0.012) | (0.012) | (0.010) | (0.016) | (0.015) | (0.015) |
| Muslim x Post | | 0.006 | | 0.034 | | -0.001 |
| | | (0.025) | | (0.034) | | (0.019) |
| Reform x Muslim x Post | | -0.003 | | -0.025 | | 0.034 |
| | | (0.070) | | (0.085) | | (0.048) |
| Reform x Post | | 0.024 | | 0.021 | | 0.020 |
| | | (0.065) | | (0.078) | | (0.040) |
| Muslim Linear Trend | | 0.001 | | -0.001 | | 0.001 |
| | | (0.002) | | (0.002) | | (0.001) |
| Reform x Muslim Linear Trend | | 0.003 | | 0.002 | | -0.001 |
| | | (0.005) | | (0.005) | | (0.003) |
| Reform Linear Trend | | -0.005 | | -0.004 | | -0.003 |
| | | (0.003) | | (0.004) | | (0.003) |
| CGM | | [0.02] | | | | |
| Year x Age Interactions | Yes | Yes | Yes | Yes | Yes | Yes |
| State x Muslim x Ethnicity Interactions | Yes | Yes | Yes | Yes | Yes | Yes |
| # Observations | 98141 | 98141 | 98141 | 98141 | 98141 | 98141 |
| R2 | 0.400 | 0.400 | 0.348 | 0.348 | 0.502 | 0.502 |
| Mean of Dependent Var. | .673 | .673 | .604 | .604 | .743 | .743 |

*Notes.* This table reports the OLS estimation of Equation 1 without women fixed effects, using repeated cross-sectional data from DHS 1990, 1999, 2003, 2008 and 2013. The sample comprises one observation by woman, who appears in only one of the following years $1990, 1999, 2003, 2008$ and 2013. The only additional covariates are: interactions between the $Muslim$ indicator variable, dummy variables for ethnic groups, and dummy variables for states in the 1990 administrative division of Nigeria (see Section 5.2), and interactions between dummy variables for years of observations and women's ages. The row CGM reports in parentheses the p-value from Cameron et al. (2008)'s wild cluster bootstrap-t procedure for the null that the coefficient on $High-Enforcement \times Muslim$ is zero. Standard errors are clustered at the state level, and reported in parentheses. $*p < 0.05, **p < 0.01, ***p < 0.001$.

Table 6: *Effect of the Reform on Labour Supply*

The dependent variable is a binary variable equal to 1 if and only if woman $i$ :

| | works | | works & is married | | works & lives w. husband | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| High-Enforcement x Muslim x Post | 0.182*** | 0.121*** | 0.194*** | 0.136*** | 0.183*** | 0.133*** |
| | (0.022) | (0.037) | (0.023) | (0.037) | (0.018) | (0.037) |
| Muslim x Post | | 0.046 | | 0.068 | | 0.082* |
| | | (0.058) | | (0.041) | | (0.044) |
| Reform x Muslim x Post | | -0.021 | | -0.041 | | -0.064 |
| | | (0.075) | | (0.075) | | (0.078) |
| Reform x Post | | 0.089 | | 0.071 | | 0.069 |
| | | (0.087) | | (0.084) | | (0.082) |
| Muslim Linear Trend | | -0.002 | | -0.001 | | -0.002 |
| | | (0.003) | | (0.003) | | (0.003) |
| Reform x Muslim Linear Trend | | -0.001 | | 0.002 | | 0.003 |
| | | (0.005) | | (0.004) | | (0.004) |
| Reform Linear Trend | | 0.001 | | -0.002 | | -0.001 |
| | | (0.004) | | (0.003) | | (0.003) |
| CGM | | [.08] | | | | |
| Year x Age Interactions | Yes | Yes | Yes | Yes | Yes | Yes |
| State x Muslim x Ethnicity Interactions | Yes | Yes | Yes | Yes | Yes | Yes |
| # Observations | 97610 | 97610 | 98142 | 98142 | 98142 | 98142 |
| R2 | 0.245 | 0.246 | 0.244 | 0.244 | 0.200 | 0.201 |
| Mean of Dependent Var. | .580 | .580 | .445 | .445 | .394 | .394 |

*Notes.* This table reports the OLS estimation of Equation 1 without women fixed effects, using repeated cross-sectional data from DHS 1990, 1999, 2003, 2008 and 2013. The sample comprises one observation by woman, who appears in only one of the following years 1990, 1999, 2003, 2008 and 2013. The only additional covariates are: interactions between the $Muslim$ indicator variable, dummy variables for ethnic groups, and dummy variables for states in the 1990 administrative division of Nigeria (see Section 5.2), and interactions between dummy variables for years of observations and women's ages. The row CGM reports in parentheses the p-value from Cameron et al. (2008)'s wild cluster bootstrap-t procedure for the null that the coefficient on $High-Enforcement \times Muslim$ is zero. Standard errors are clustered at the state level, and reported in parentheses. $*p < 0.05, **p < 0.01, ***p < 0.001$.

Table 7: *Effect of the Reform on Use, Knowledge and Availability of Family Planning (FP)*

The dependent variable is a binary variable equal to 1 if and only if woman $i$ :

| | ever used FP | | knows FP | | knows FP supplier | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| High-Enforcement x Muslim x Post | -0.063*** | 0.012 | 0.098*** | 0.021 | 0.003 | 0.037 |
| | (0.021) | (0.012) | (0.026) | (0.031) | (0.032) | (0.035) |
| Muslim x Post | | -0.027 | | -0.094 | | -0.071 |
| | | (0.042) | | (0.073) | | (0.113) |
| Reform x Muslim x Post | | 0.150* | | 0.129 | | 0.181 |
| | | (0.079) | | (0.106) | | (0.211) |
| Reform x Post | | -0.162** | | -0.106 | | -0.169 |
| | | (0.074) | | (0.088) | | (0.211) |
| Muslim Linear Trend | | 0.001 | | 0.002 | | 0.002 |
| | | (0.002) | | (0.005) | | (0.004) |
| Reform x Muslim Linear Trend | | -0.015** | | -0.002 | | -0.014 |
| | | (0.007) | | (0.008) | | (0.008) |
| Reform Linear Trend | | 0.010 | | 0.011** | | 0.012 |
| | | (0.006) | | (0.004) | | (0.008) |
| Year x Age Interactions | Yes | Yes | Yes | Yes | Yes | Yes |
| State x Muslim x Ethnicity Interactions | Yes | Yes | Yes | Yes | Yes | Yes |
| # Observations | 98142 | 98142 | 98142 | 98142 | 87921 | 87921 |
| R2 | 0.224 | 0.226 | 0.285 | 0.288 | 0.246 | 0.247 |
| Mean of Dep. Var. | .242 | .242 | .718 | .718 | .471 | .471 |

*Notes.* This table reports the OLS estimation of Equation 1 without women fixed effects, using repeated cross-sectional data from DHS 1990, 1999, 2003, 2008 and 2013. The sample comprises one observation by woman, who appears in only one of the following years 1990, 1999, 2003, 2008 and 2013. The only additional covariates are: interactions between the *Muslim* indicator variable, dummy variables for ethnic groups, and dummy variables for states in the 1990 administrative division of Nigeria (see Section 5.2), and interactions between dummy variables for years of observations and women's ages. Standard errors are clustered at the state level, and reported in parentheses. $*p < 0.05, **p < 0.01, ***p < 0.001$.

Table 8: *Effect of the Reform on the Probability of Giving Birth - Robustness Checks*

The dependent variable is a binary variable equal to 1 if and only if woman $i$ gives birth at $t$

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| High-Enforcement x Muslim x Post | 0.030*** | 0.042*** | 0.060* | -0.055*** | 0.028*** | 0.034*** | 0.029 | -0.050*** |
| | (0.005) | (0.008) | (0.031) | (0.009) | (0.007) | (0.009) | (0.036) | (0.015) |
| Muslim x Post | 0.009 | 0.001 | -0.030 | 0.053* | 0.010 | -0.006 | -0.020 | 0.038 |
| | (0.006) | (0.024) | (0.030) | (0.031) | (0.010) | (0.025) | (0.037) | (0.038) |
| Reform x Muslim x Post | 0.002 | 0.043 | -0.059 | -0.017 | 0.013 | 0.063 | -0.042 | 0.011 |
| | (0.011) | (0.038) | (0.073) | (0.049) | (0.014) | (0.039) | (0.080) | (0.047) |
| Reform x Post | 0.014 | -0.003 | 0.111** | 0.034 | 0.008 | -0.033 | 0.084 | -0.001 |
| | (0.009) | (0.028) | (0.053) | (0.036) | (0.007) | (0.030) | (0.059) | (0.036) |
| Muslim Linear Trend | -0.000 | -0.002 | -0.002 | -0.006* | -0.000 | -0.001 | -0.006 | -0.005 |
| | (0.001) | (0.003) | (0.004) | (0.003) | (0.001) | (0.003) | (0.005) | (0.004) |
| Reform x Muslim Linear Trend | 0.001 | -0.003 | 0.005 | -0.001 | -0.001 | -0.004 | 0.007 | -0.001 |
| | (0.001) | (0.004) | (0.010) | (0.005) | (0.002) | (0.004) | (0.012) | (0.005) |
| Reform Linear Trend | -0.003** | -0.004 | -0.011 | -0.002 | -0.002** | -0.000 | -0.009 | -0.001 |
| | (0.001) | (0.003) | (0.007) | (0.004) | (0.001) | (0.003) | (0.010) | (0.004) |
| Women FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year x Age Inter. | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| # Observations | 382544 | 38490 | 12645 | 21510 | 271249 | 31290 | 8445 | 16050 |
| R2 | 0.033 | 0.030 | 0.051 | 0.042 | 0.034 | 0.034 | 0.061 | 0.050 |
| Mean of Dep. Var. | .173 | .173 | .173 | .173 | .199 | .199 | .199 | .199 |
| Sample: | | | | | | | | |
| *Ideal # children ...* | | *smaller* | *equal* | *larger* | | *smaller* | *equal* | *larger* |
| *... her husband's* | | *than* | *to* | *than* | | *than* | *to* | *than* |
| Modifications | [M1] | [M1] | [M1] | [M1] | [M2] | [M2] | [M2] | [M2] |

*Notes.* This table reports fixed effect estimations of Equation 1, using longitudinal data from DHS 2008, except for the following modifications. [M1]: the dependent variable is equal to 1 if woman $i$ gives birth at $t$ to a child who survives infancy, and 0 otherwise. [M2]: the sample excludes women who moved in 2000 or later. In Columns 1 and 5, the estimations use observations from all women (as in Column 6 of Table 2). In Columns 2 to 4, and 6 to 8, the estimations use observations from married women who were born in 1980 or before, partitioned by fertility preferences (as in Columns 2, 4 and 6 of Table 4). Standard errors are clustered at the state level. Standard errors in parentheses. $*p < 0.05, **p < 0.01, ***p < 0.001$.

Table 9: *Effect of the Reform on the Probability of Giving Birth, for the Subsample of Muslim Women in High-Enforcement and Adjacent States.*

The dependent variable is a binary variable equal to 1 if and only if woman $i$ gives birth at $t$

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| High-Enforcement x Muslim x Post | 0.020** | 0.034** | 0.002 | -0.060*** |
| | (0.008) | (0.012) | (0.045) | (0.015) |
| Women FE | Yes | Yes | Yes | Yes |
| Year x Age Interactions | Yes | Yes | Yes | Yes |
| # Observations | 85121 | 10500 | 2340 | 4875 |
| R2 | 0.049 | 0.055 | 0.153 | 0.097 |
| Mean of Dependent Var. | .239 | .239 | .239 | .239 |
| Sample: | | | | |
| *Ideal # children ...* | | *smaller* | *equal* | *larger* |
| ... | | *than* | *to* | *than* |
| *her husband's* | | | | |

*Notes.* This table reports fixed effect estimations of Equation 1, using longitudinal data from DHS 2008 on the subsample of Muslim women living either in High-Enforcement or in adjacent states (Kaduna, Kano, Kebbi, Jigawa, Niger). The sample used in Column 1 comprises one observation by year $t$, for any year $t$ between 1993 and 2007, and by woman $i$, aged 13 or more at $t$. The samples used in Column 2 to 4 are further restricted to include only married women who were born in 1980 or before, and whose ideal number of children is smaller than their husband's (Column 2), equal to their husband's (Column 3), or larger than their husband's (Column 4). The only additional covariates are the interactions of years dummy variables and women's ages dummy variables. Standard errors are clustered at the state level. Standard errors in parentheses. $*p < 0.05, **p < 0.01, ***p < 0.001$.

Table 10: *Falsification Tests and Pre-Reform Trends in Probability of Giving Birth*

The dependent variable is a binary variable equal to 1 if and only if woman $i$ gives birth at $t$

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| High-Enforcement x Muslim x Post | -0.012 | -0.006 | -0.006 | -0.008 | -0.002 | -0.030** | | |
| | (0.010) | (0.013) | (0.008) | (0.011) | (0.005) | (0.012) | | |
| High-Enforcement x Muslim Linear Trend | | | | | | | -0.000 | -0.003 |
| | | | | | | | (0.002) | (0.002) |
| Muslim x Post | -0.008 | 0.010 | -0.015 | 0.007 | 0.013 | -0.009 | | |
| | (0.010) | (0.012) | (0.013) | (0.008) | (0.012) | (0.012) | | |
| Reform x Muslim x Post | -0.008 | -0.020 | 0.001 | 0.008 | 0.029* | 0.005 | | |
| | (0.019) | (0.018) | (0.023) | (0.019) | (0.017) | (0.019) | | |
| Reform x Post | 0.009 | 0.008 | 0.031 | -0.036* | 0.031** | -0.038*** | | |
| | (0.021) | (0.014) | (0.019) | (0.021) | (0.015) | (0.012) | | |
| Muslim Linear Trend | 0.003* | 0.000 | 0.005 | 0.000 | -0.000 | 0.003** | | 0.002* |
| | (0.002) | (0.003) | (0.003) | (0.002) | (0.002) | (0.001) | | (0.001) |
| Reform x Muslim Linear Trend | 0.004** | 0.007* | 0.003 | 0.002 | -0.002 | 0.004 | | 0.004** |
| | (0.002) | (0.004) | (0.005) | (0.005) | (0.003) | (0.003) | | (0.002) |
| Reform Linear Trend | -0.003* | -0.004 | -0.009** | 0.006 | -0.008*** | 0.002 | | -0.002 |
| | (0.002) | (0.003) | (0.004) | (0.005) | (0.003) | (0.002) | | (0.002) |
| Women FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year x Age Interactions | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| # Observations | 142990 | 142990 | 142990 | 142990 | 142990 | 142990 | 142990 | 142990 |
| R2 | 0.015 | 0.015 | 0.015 | 0.015 | 0.016 | 0.016 | 0.015 | 0.015 |
| Fictitious year of Reform | 1994 | 1995 | 1996 | 1997 | 1998 | 1999 | N.A. | N.A. |

*Notes.* This table reports fixed effect estimations of Equation 1, using longitudinal data from DHS 2008. The whole sample comprises one observation by year $t$, for any year $t$ between 1993 and 2007, and by woman $i$, aged 13 or more at $t$. The subsample used here is restricted to observations from 1993 to 1999. Column 1 to 6 estimate the change in fertility after a fictitious year of reform. In these columns, the binary variable *Post* is defined to be equal to 1 if and only if the year of observation comes in or after the year indicated at the bottom of the column (the "fictitious year of reform"). Columns 7 and 8 test for the presence of a significant linear time trend specific to Muslim women in High-Enforcement States before 2000. In all columns, the only additional covariates are the interactions of years dummy variables and women's ages dummy variables. Standard errors are clustered at the state level. Standard errors in parentheses. $*p < 0.05, **p < 0.01, ***p < 0.001$.

Table 11: *Falsification Tests and Pre-Reform Trends in Probability of Giving Birth - Correction for Bunching of Children's Dates of Births*

The dependent variable is a binary variable equal to 1 if and only if woman $i$ gives birth at $t$

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| High-Enforcement x Muslim x Post | -0.013 | -0.008 | -0.008 | -0.010 | -0.005 | -0.022 | | |
| | (0.011) | (0.012) | (0.008) | (0.008) | (0.005) | (0.013) | | |
| High-Enforcement x Muslim Linear Trend | | | | | | | 0.002 | -0.003 |
| | | | | | | | (0.002) | (0.002) |
| Muslim x Post | -0.000 | 0.013 | -0.012 | -0.001 | 0.011 | -0.012 | | |
| | (0.010) | (0.011) | (0.013) | (0.009) | (0.012) | (0.011) | | |
| Reform x Muslim x Post | -0.022 | -0.019 | -0.015 | 0.012 | 0.030* | 0.021 | | |
| | (0.022) | (0.015) | (0.023) | (0.020) | (0.016) | (0.018) | | |
| Reform x Post | 0.023 | 0.006 | 0.052*** | -0.032 | 0.024 | -0.059*** | | |
| | (0.024) | (0.013) | (0.014) | (0.023) | (0.014) | (0.009) | | |
| Muslim Linear Trend | 0.002 | 0.000 | 0.005 | 0.003 | 0.000 | 0.004** | | 0.002* |
| | (0.002) | (0.002) | (0.003) | (0.003) | (0.002) | (0.002) | | (0.001) |
| Reform x Muslim Linear Trend | 0.007** | 0.008* | 0.008 | 0.002 | -0.001 | 0.003 | | 0.005** |
| | (0.003) | (0.004) | (0.006) | (0.006) | (0.003) | (0.003) | | (0.002) |
| Reform Linear Trend | -0.003 | -0.002 | -0.012*** | 0.006 | -0.005** | 0.006** | | -0.001 |
| | (0.002) | (0.003) | (0.004) | (0.006) | (0.003) | (0.003) | | (0.002) |
| Women FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year x Age Interactions | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| # Observations | 122697 | 122697 | 122697 | 122697 | 122697 | 122697 | 122697 | 122697 |
| R2 | 0.017 | 0.017 | 0.017 | 0.017 | 0.018 | 0.018 | 0.017 | 0.017 |
| Fictitious year of Reform | 1994 | 1995 | 1996 | 1997 | 1998 | 1999 | N.A. | N.A. |

*Notes.* This table reports fixed effect estimations of Equation 1, using longitudinal data from DHS 2008. The whole sample comprises one observation by year $t$, for any year $t$ between 1993 and 2007, and by woman $i$, aged 13 or more at $t$. The subsample used here is restricted to observations from 1993 to 1999, and excludes observations for women born either in 1968 or 1973. Column 1 to 6 estimate the change in fertility after a fictitious year of reform. In these columns, the binary variable *Post* is defined to be equal to 1 if and only if the year of observation comes in or after the year indicated at the bottom of the column (the "fictitious year of reform"). Columns 7 and 8 test for the presence of a significant linear time trend specific to Muslim women in High-Enforcement States before 2000. In all columns, the only additional covariates are the interactions of years dummy variables and women's ages dummy variables. Standard errors are clustered at the state level. Standard errors in parentheses. $*p < 0.05, **p < 0.01, ***p < 0.001$.

Table 12: *Effect of the Reform on the Probability of Giving Birth - Correction for Bunching of Children's Dates of Births*

The dependent variable is a binary variable equal to 1 if and only if woman $i$ gives birth at $t$

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| High-Enforcement x Muslim x Post | 0.021*** | 0.026** | 0.014 | -0.045*** | 0.029*** | 0.037*** | 0.044 | -0.036*** |
| | (0.006) | (0.010) | (0.036) | (0.015) | (0.006) | (0.009) | (0.034) | (0.012) |
| Muslim x Post | 0.005 | -0.015 | 0.013 | 0.037 | 0.008 | -0.019 | -0.019 | 0.037 |
| | (0.007) | (0.025) | (0.026) | (0.035) | (0.007) | (0.026) | (0.032) | (0.033) |
| Reform x Muslim x Post | 0.016 | 0.090** | -0.125* | 0.004 | 0.013 | 0.098** | -0.075 | 0.017 |
| | (0.011) | (0.036) | (0.069) | (0.054) | (0.013) | (0.042) | (0.085) | (0.054) |
| Reform x Post | -0.006 | -0.053** | 0.161*** | -0.001 | 0.007 | -0.040 | 0.100 | 0.011 |
| | (0.010) | (0.025) | (0.051) | (0.039) | (0.010) | (0.031) | (0.070) | (0.044) |
| Muslim Linear Trend | -0.000 | -0.001 | -0.006* | -0.005 | -0.000 | -0.001 | -0.002 | -0.005 |
| | (0.001) | (0.003) | (0.003) | (0.004) | (0.001) | (0.003) | (0.004) | (0.003) |
| Reform x Muslim Linear Trend | -0.001 | -0.007 | 0.011 | -0.003 | -0.001 | -0.010** | 0.007 | -0.005 |
| | (0.002) | (0.004) | (0.009) | (0.005) | (0.002) | (0.004) | (0.011) | (0.005) |
| Reform Linear Trend | -0.002 | 0.001 | -0.015* | 0.001 | -0.002 | 0.002 | -0.010 | 0.000 |
| | (0.002) | (0.003) | (0.007) | (0.004) | (0.001) | (0.003) | (0.009) | (0.004) |
| Women FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year x Age Inter. | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| # Observations | 358446 | 35924 | 11802 | 20076 | 339059 | 31155 | 10695 | 17880 |
| R2 | 0.035 | 0.033 | 0.053 | 0.046 | 0.037 | 0.034 | 0.056 | 0.049 |
| Mean of Dep. Var. | .182 | .182 | .182 | .182 | .172 | .172 | .172 | .172 |
| Sample: | | | | | | | | |
| *Ideal # children ...* | | *smaller* | *equal* | *larger* | | *smaller* | *equal* | *larger* |
| *... her husband's* | | *than* | *to* | *than* | | *than* | *to* | *than* |
| Modifications | [M3] | [M3] | [M3] | [M3] | [M4] | [M4] | [M4] | [M4] |

*Notes.* This table reports fixed effect estimations of Equation 1, using longitudinal data from DHS 2008, except for the following modifications. [M3]: the sample excludes observations for the year 1999. [M4]: the sample excludes women born either in 1968 or 1973. In Columns 1 and 5, the estimations use observations from all women (as in Column 6 of Table 2). In Columns 2 to 4, and 6 to 8, the estimations use observations from married women who were born in 1980 or before, partitioned by fertility preferences (as in Columns 2, 4 and 6 of Table 4). Standard errors are clustered at the state level. Standard errors in parentheses. $*p < 0.05, **p < 0.01, ***p < 0.001$.

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### Appendix A. Model

This model formalizes the impact on fertility of a decrease in the value for women of being single. A man $M$ and a woman $F$ who have been married decide whether to remain married or not, and, if they remain married, they choose whether to have a child (possibly in addition to the children they already have) and how to allocate consumption and leisure. The assumption that the couple is married and renegotiates the conditions of their marriage implies that there was no commitment regarding these conditions at the time of their marriage. Although I retain the assumption of lack of commitment and show how fertility depends on that threat point, the model differs from Rasul (2008). In Rasul (2008), the threat point without commitment will affect the number of children only if the utility outside marriage depends on that number; I make no such assumption here.

Let $u_i$ be the utility function of person $i = M, F$. I assume that $u_i(c_i, \ell_i, \delta) = \log c_i + \log \ell_i + 2\delta \log g_i$ if $M$ and $F$ are married, where $c_i$ is $i$'s consumption, and $\ell_i$ is $i$'s leisure, and $\delta \in \{0, 1\}$ is a binary variable equal to 1 if they have another child. Child-related expenses are denoted $2b$. If they are not married, $M$ and $F$ have no (other) child and their utilities are $2 \log v_M$ and $2 \log v_F$ respectively.[29] For simplicity, the unit price of the consumption good and the hourly wages are assumed to be equal to 1.

If $g_M$ is larger than $g_F$, it means that, everything else equal, $M$ derives more utility from having a child than $F$ does. Conversely, if $g_M$ is smaller than $g_F$, $F$ derives more utility from having a child than $M$.

Here, I assume that, conditionally on being married, $M$ makes all decisions and maximizes his utility under the budget constraint and the incentive compatibility constraints for both spouses to remain married. He will thus solve:

[29] The parameters $v_i$ and $g_i$ are strictly positive so that $2 \log g_i$ and $2 \log v_M$ can take any real value. $2\delta \log g_i$ is a reduced form of the net gain, which may be negative, of having an additional child.

$$\begin{aligned}
\underset{c_F,\ell_F,c_M,\ell_M,\delta}{\text{Max}} \quad & \log c_M + \log \ell_M + 2\delta \log g_M \\
s.t. : \; & c_M + c_F + 2b\delta = 1 - \ell_M + 1 - \ell_F \\
& c_F, c_M, \ell_F, \ell_M > 0 \\
& \ell_F, \ell_M \leq 1 \\
& \log c_F + \log \ell_F + 2\delta \log g_F \geq 2 \log v_F \\
& \log c_M + \log \ell_M + 2\delta \log g_M \geq 2 \log v_M
\end{aligned}$$

*Effect on labour supply and fertility within marriage.* $M$ can increase his consumption and leisure if $F$'s incentive compatibility constraint is not met, so that this constraint is binding at the optimum. Conditionally on being married, consumption and leisure must satisfy $c_F^* = \ell_F^* = (v_F)/(g_F^\delta)$. For a given value of $\delta$, $F'$s labour supply $(1 - \ell_F)$ increases if $v_F$ decreases.

The budget constraint implies that $M$'s consumption and leisure must satisfy $c_M^* = \ell_M^* = 1 - \delta b - (v_F)/(g_F^\delta)$.

Substituting consumption and leisure in the objective function, the couple will have another child if and only if the difference between the objective function with $\delta = 1$ is larger than with $\delta = 0$, i.e. if:

$$\Delta(v_F) \equiv \Big[(1-b)g_M - 1\Big] g_F - v_F(g_M - g_F) > 0$$

This term is decreasing in $v_F$ if $g_M > g_F$, and increasing in $v_F$ if $g_M < g_F$. It implies that for an initial value $v_F^0$ of $v_F$:

- if $g_M > g_F$ and $\Delta(v_F^0) < 0$, $\delta^*$ weakly increases if $v_F$ decreases,
- if $g_M < g_F$ and $\Delta(v_F^0) > 0$, $\delta^*$ weakly decreases if $v_F$ decreases,
- if $g_M > g_F$ and $\Delta(v_F^0) > 0$, or if $g_M < g_F$ and $\Delta(v_F^0) < 0$ a decrease in $v_F$ has no impact on $\delta^*$.

In summary, a decrease in $v_F$ will weakly increase (resp. decrease) fertility if, everything else equal, $M$'s marginal utility from having a child is larger (resp. smaller) than $F$'s.

Remarks. (1) The following example proves that, for certain values of the parameters, the first or the second points do occur, i.e. fertility will indeed change, if $v_F$ decreases. $g_M = 3$, $g_F = 0.5$, $b = 0.5$ and $v_F = 0.1 \times (1 + \alpha)$, with $|\alpha| < 1$. We have $c_F = \ell_F = 2^{\delta} \times \big[0.1 \times (1 + \alpha)\big] > 0$ and $c_M = \ell_M = 1 - 0.5 \times \delta - 2^{\delta} \times \big[0.1 \times (1 + \alpha)\big] > 0$.

In addition, $g_M > g_F$ and $\Delta(v_F) = \log(9 - 6\alpha)/(9 - \alpha)$ moves from being negative to being positive as $\alpha$, i.e. $v_F$, decreases.

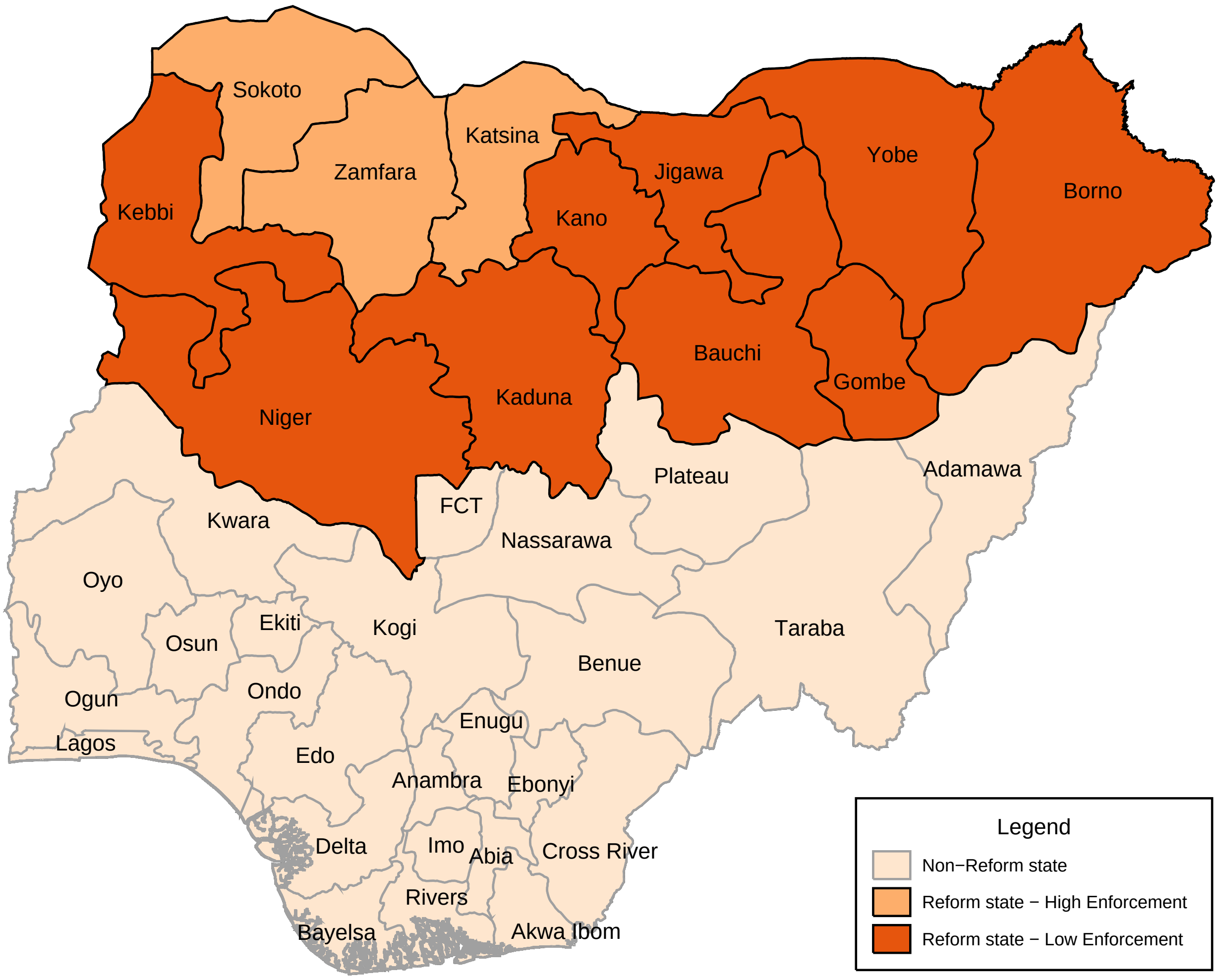
(2) The model assumes that it is not possible to transfer utility from one individual to another, but possible to transfer money through the budget constraint. Under this assumption, but assuming that household decisions result from Nash-bargaining, a decrease in $F$'s outside value will in general have no impact on fertility, but may decrease $F$'s leisure. Under the assumption that it is possible to transfer utility from one individual to another, regardless of how the surplus is shared, the outside value will in general have no impact on fertility decisions or on labour supply.

*Effect on marital status and on fertility through marital status.* The maximum utility $M$ obtains if married is $\text{Max}_{\delta \in \{0,1\}} 2 \log \left[1 - \delta b - (v_F)/(g_F^{\delta})\right] + 2\delta \log g_M$. This term is continuous and decreasing in $v_F$, and $M$ and $F$ are married if and only if that term is smaller than $2 \log v_M$, so that a decrease in $v_F$ will weakly increase the probability to remain married, and weakly increase the probability to have a child, assuming that the couple will have no child if they separate.

Remark. Without any assumption on the joint distribution of $(v_M, v_F, g_M, g_F)$, the average fertility of the marginal couples who remain married because $v_F$ decreases may be smaller, equal, or larger than the average fertility of couples who would have remained married regardless.

**Appendix B. Map**

Figure B1: *Legal Reform in Nigerian States and Federal Capital Territory (FCT)*




NOTE. Definition of High and Low−Enforcement States is based on Weimann 2010

## Appendix C. Additional Figures and Sources of Measurement Error

This Appendix shows additional figures that represent the empirical probability of giving birth for the three groups (Muslim women in High-Enforcement States, other Muslim women, non-Muslim women) and discusses the sources of errors in the measurement of fertility. Unlike the figures of Section 4.1, a point here represents the probability of giving birth in that year only, not on average over four years. In Figure C1, $y(x, g)$ is the probability of giving birth in year $x$, for all women of group $g$ who are 14 to 31 in that year.[30] In Figure C2, $y(x, g)$ represents the probability of giving birth in year $x$ for women born between 1962 and 1972.[31]

The figures show that the probability of giving birth in the treatment group is lower than in Muslim women in Reform States before 2000, and almost systematically higher after 1999. There are, however, two main sources of variations in the figures that are independent of any change in the actual number of births over time and create artificially high and low points on the figure (See Pullum 2006, Pullum et al. 2013).

(1) Measurement error in the year of birth. Two factors contribute to incorrect years of birth for some children. The first factor is that, during the data-gathering interview, children's ages are often rounded to the closest multiple of 5. Such error creates artificial peaks of births in certain years. This is the case for instance in 1998 (i.e. 10 years before the survey), in which there seems to have been a large increase, surrounded by large decreases in births in 1997 and 1999; This peak results from the fact that children who were either 9 or 11 in 2008 were declared to be 10. The second factor is that, children who are born within 5 years of the survey are often declared to be slightly older than 5, so that the interviewer and the interviewee do not have to fill a lengthy questionnaire on children under 5. This behavior artificially inflates the number of births before 2004 and deflates it after that.

(2) Measurement error in the occurrence of birth. Some births may not be declared, primarily the births of children who died very young. This is well-documented for births that occurred long before the date of a survey (recall bias), and for births that occurred in the five years before a

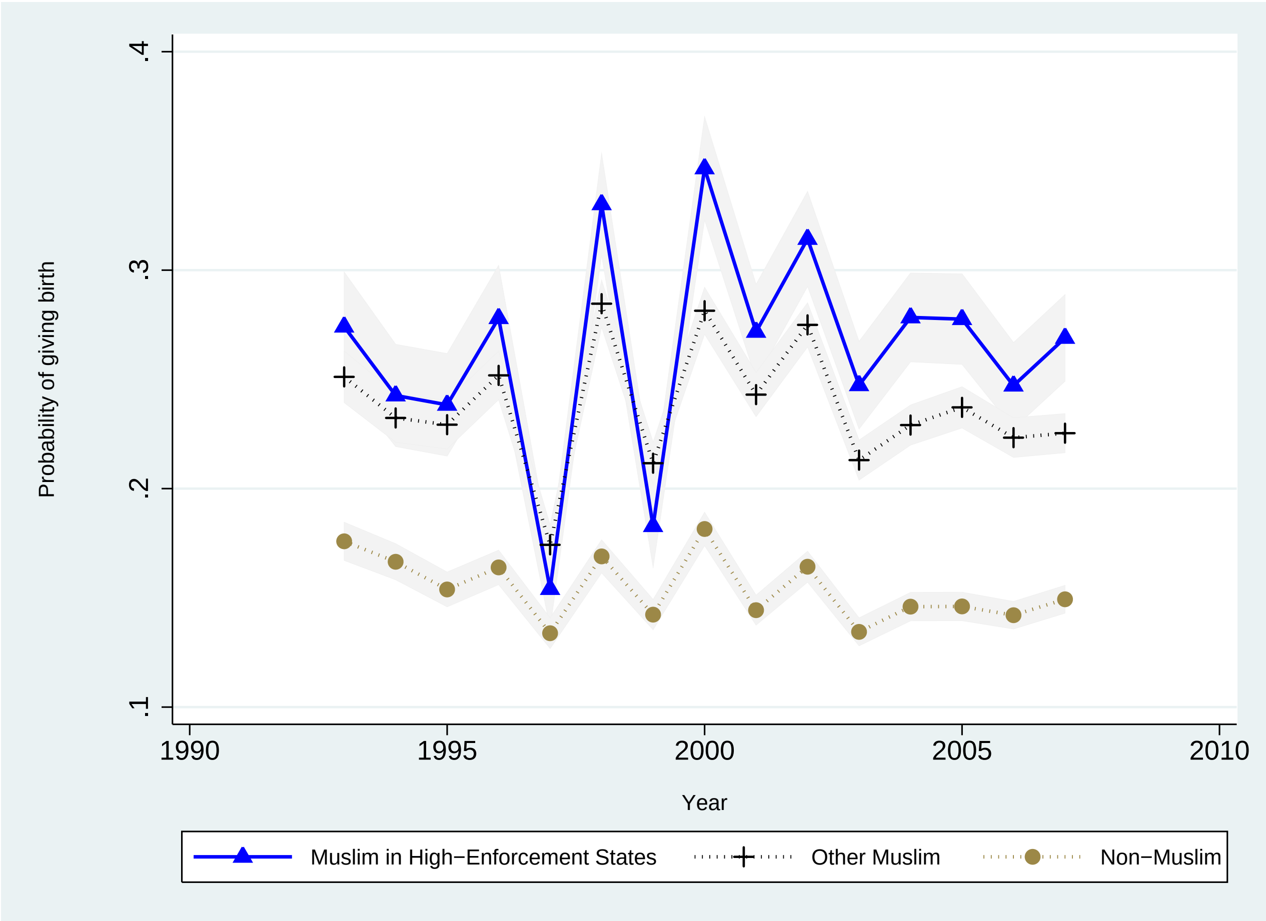
[30] With the notations of Section 4.1, $y(x, g) \equiv \left[\sum_{a=14}^{31} \#B(x, x, a, g)\right] / \left[\sum_{a=14}^{31} \#S(x, a, g)\right]$.

[31] With the notations of Section 4.1, $y(x, g) \equiv \left[\sum_{a=x-1972}^{x-1962} \#B(x, x, a, g)\right] / \left[\sum_{a=x-1972}^{x-1962} \#S(x, a, g)\right]$.

survey specifically.

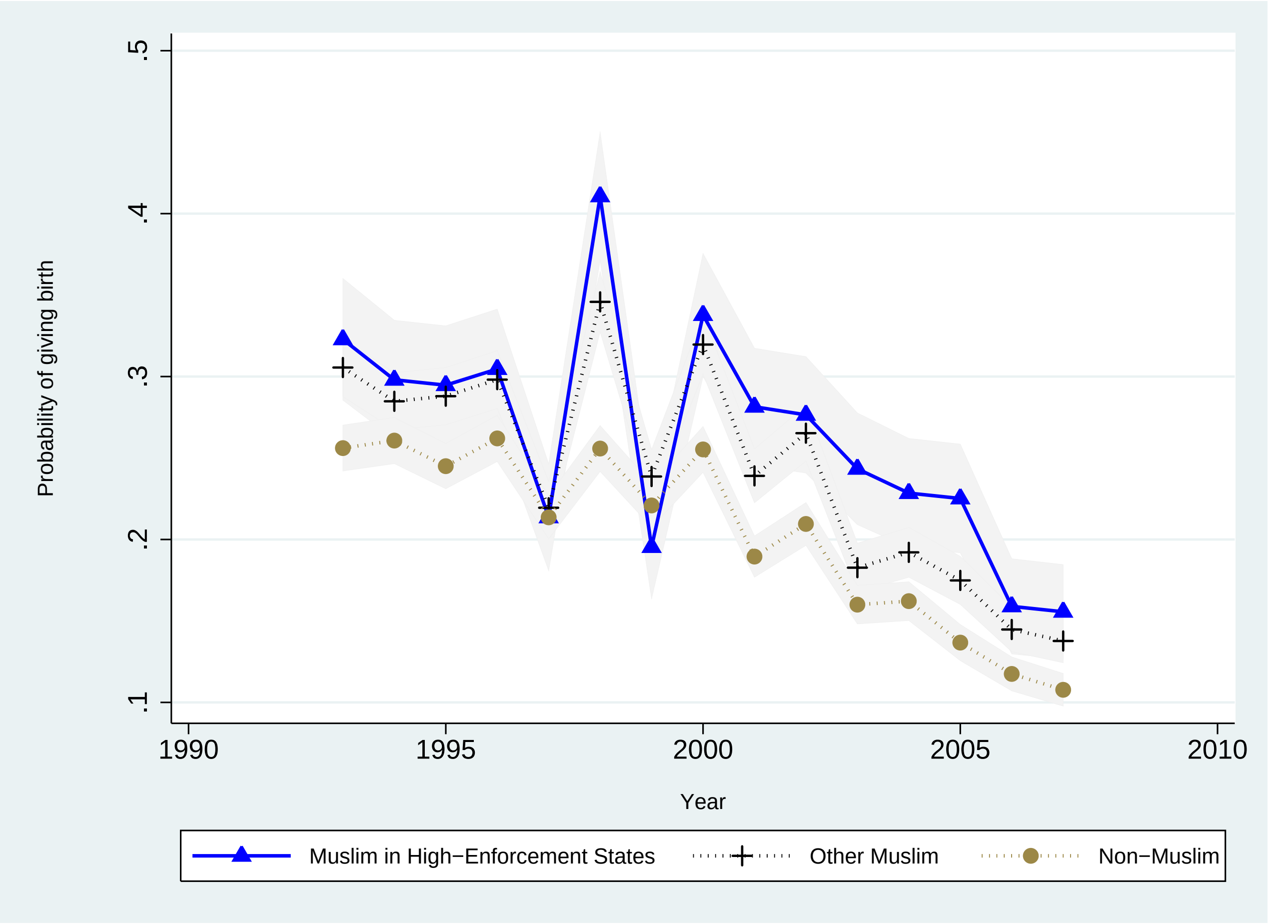
These two sources do not vary differentially *both* across groups and across time periods (i.e. before or after 2000). Thus, the measurement error they induce will not bias the main estimations of Section 4.2, since they include interaction between year dummy variables and age dummy variables.

Figure C1: *Probability of Giving Birth - Women 14 to 31 Years Old*




*Notes.* For any year $x$, $y$ represents the empirical probability of giving birth in year $x$ for all women who are between 14 and 31 years old in year $x$. See Section 4.1.

Figure C2: *Probability of Giving Birth - Women Born Between 1960 and 1972*




*Notes.* For any year $x$, $y$ represents the empirical probability of giving birth in year $x$ for all women born between 1960 and 1972. See Section 4.1.

### Appendix D: Potential Effect on Fertility Preferences

This appendix examines whether fertility preferences changed after the Reform; a change in fertility preferences would not have invalidate the identification assumption, since the Reform could have itself affected these preferences, but would inform the channels through which the Reform operates. Here, I compare the stated ideal numbers of children of women and men before and after the Reform, in two ways. To do so, I use men and women surveys of DHS 2008, which are the data used in the estimations reported in Tables 3 and 4, to construct a sample of observations based on the date of first marriage of individuals. The sample appends the subsample of women (resp. men) who were married for the first time before the Reform, to the subsample of women (resp. men) who are married at the time of the survey (some of whom were also married before 2000, and thus appear twice in the sample constructed here). I define the variable Post to be 0 for the former subsample, and 1 for the latter. The specification is the same as in Section 5.2, except that interactions between the variable Post and Age dummy variables replace interactions between Year and Age dummy variables.[32] Since there are only two periods in this specification, there are no linear time trends variables.

Table D1 reports the results of these estimations. In every column, the dependent variable is an individual's ideal number of children. Columns 1 and 2 run the estimations on the subsample of women, and Columns 3 and 4 on the subsample of men. None of these regressions shows any significant change in the fertility preferences of men or women of the treatment group. These results mean that the Reform did not induce individuals with certain preferences to be more likely to marry, remain married, or remarry after the Reform. The results also mean that fertility preferences did not change differently for individuals who were married at some point before the Reform, and individuals who are married after the Reform starts.

[32] In addition, states of residence are post-1996 states. (See Section 5.2.)

Table D1: *Effect of the Reform on Stated Fertility Preferences, Using DHS 2008*

The dependent variable is a binary variable equal to 1 if and only if :

| | Ideal # Children | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| High-Enforcement x Muslim x Post | 0.120 | 0.041 | -0.150 | 0.253 |
| | (0.080) | (0.098) | (0.187) | (0.230) |
| Muslim x Post | | 0.099* | | -0.210 |
| | | (0.054) | | (0.195) |
| Reform x Muslim x Post | | -0.056 | | -0.457 |
| | | (0.110) | | (0.284) |
| Reform x Post | | 0.110 | | 0.028 |
| | | (0.113) | | (0.292) |
| Post x Age Interactions | Yes | Yes | Yes | Yes |
| State x Muslim x Ethnicity Interactions | Yes | Yes | Yes | Yes |
| # Observations | 33552 | 33552 | 11675 | 11675 |
| R2 | 0.282 | 0.282 | 0.305 | 0.305 |
| Mean of Dep. Var. | 7.07 | 7.07 | 10.0 | 10.0 |
| *Sample* | *Women* | *Women* | *Men* | *Men* |

*Notes.* This table reports the OLS estimation of Equation 1 without women fixed effects. It uses the subsample of women (resp. men) married for the first time before 2000 appended to the subsample of women (resp. men) married at the time of the survey (some of whom were married before 2000, and hence appear twice in the sample). The variable Post is equal to 1 if and only an individual belongs to the latter subsample; individuals married before 2000 and still married in 1999 thus appear twice in the whole sample of observations. The only additional covariates are: interactions between the *Muslim* indicator variable, dummy variables for ethnic groups, and dummy variables for states of residence, and interactions between the binary variable *Post* and dummy variables for women's (resp. men's) ages. Standard errors are clustered at the state level. Standard errors in parentheses. $*p < 0.05, **p < 0.01, ***p < 0.001$.

## Appendix E: Pre-Reform Changes in Marital Status and Labour Supply

Table E1 reports the OLS estimations of the difference in marital status and labour supply between observations of DHS 1990 and DHS 1999, the only two surveys conducted before the Reform. The specification is the same as in Section 5.2, except that the variable *Post* is now equal to 1 if and only if the year of observation is 1999. Since there are only two periods in this specification, there are no linear time trends variables. All columns shows no change over time specific to women of the treatment group for any of the dependent variable.

Table E1: *Pre-Reform Changes in Marital Status and Labour Supply*

The dependent variable is a binary variable equal to 1 if and only if woman $i$ :

| | is married | lives with husband | ever married | works | works & is married | works & lives w. husband |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| High-Enforcement x Muslim x Post | 0.125 | 0.121 | 0.034 | 0.039 | 0.062 | 0.064 |
| | (0.120) | (0.120) | (0.033) | (0.075) | (0.078) | (0.077) |
| Muslim x Post | -0.014 | 0.001 | 0.047*** | 0.017 | -0.030 | -0.024 |
| | (0.037) | (0.044) | (0.013) | (0.037) | (0.036) | (0.037) |
| Reform x Muslim x Post | 0.091 | 0.047 | 0.020 | -0.186** | -0.087 | -0.096 |
| | (0.070) | (0.092) | (0.048) | (0.070) | (0.068) | (0.070) |
| Reform x Post | -0.131** | -0.125 | -0.043 | -0.016 | -0.092 | -0.096 |
| | (0.058) | (0.074) | (0.044) | (0.109) | (0.106) | (0.108) |
| Year x Age Interactions | Yes | Yes | Yes | Yes | Yes | Yes |
| State x Muslim x Ethnicity | Yes | Yes | Yes | Yes | Yes | Yes |
| # Observations | 18429 | 18429 | 18429 | 18378 | 18429 | 18429 |
| R2 | 0.425 | 0.380 | 0.586 | 0.334 | 0.283 | 0.234 |
| Mean of Dependent Var. | .616 | .554 | .710 | .497 | .365 | .319 |

*Notes.* This table reports the OLS estimation of Equation 1 without women fixed effects, using repeated cross-sectional data from DHS 1990 and 1999. The sample comprises one observation by woman, who appears in only one of the following years 1990, 1999. In this table only, the variable *Post* is equal to 1 if and only if the year of observation is 1999. The only additional covariates are: interactions between the *Muslim* indicator variable, dummy variables for ethnic groups, and dummy variables for states in the 1990 administrative division of Nigeria (see Section 5.2), and interactions between dummy variables for years of observations and women's ages. Standard errors are clustered at the state level, and reported in parentheses. $*p < 0.05, **p < 0.01, ***p < 0.001$.