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Assessing the Effects of an Education Policy on Women's Well-being: Evidence from Benin

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Assessing the Effects of an Education Policy on Women's Well-being: Evidence from Benin.

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Abstract

In this paper, we examine the effect of an education policy on women's well-being through the analysis of the impact of a school construction program in Benin. We exploit a sharp increase in school constructions in the 1990s in this country, to assess the causal impact of a primary education program on primary school attendance, age at marriage and tolerance of intimate partner violence (IPV). Using a double difference method, along with a regression kink design, we find that the program increased the probability to attend primary school in rural areas. The policy also increased age at marriage and decreased the probability to find wife beating tolerable. We show that, in this context, the benefits of girls' education have percolated down to women's well-being beyond the initial goal of the policy.

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1 Introduction

The question of whether schooling infrastructure has an impact on women's welfare is gaining scrutiny in a context where it has been established that there is no mechanical link between women's status and economic developement (Duflo (2012)). In the 1990s, policy makers in developing countries have sharply increased school supply to speed up the process of reaching the Millenium Development Goal (MDG) of universal primary education. In developing countries, the study of the impact of this increase in schooling infrastructure availability on beneficiaries has mainly focused on the nexus with educational attainment and labour supply outcomes. Little has been said about the potential consequences of infrastructure on the well-being of its female recipients, in particular on women's well-being within their household. Women's well-being is multi-faceted. Yet, in the context of Western Africa and of Benin, women's well-being within their household is key as there is little safety net but the family. The strength of women's status in their family cell may be complex to grasp but women's age at first marriage and at first child and their tolerance of IPV are proxies for it (Hanmer and Klugman (2016)). Indeed, the literature has shown that the earlier the entry into marriage and motherhood, the worse women's outcomes in terms of health and empowerment (Raj et al. (2009), Nour (2006) and Jensen and Thornton (2003)).

Education has been shown to impact the way women enter marriage in many ways. Access to education is expected to postpone entry into marital life (Breierova and Duflo (2004)). A main empirical challenge for identifying a causal effect is that marriage and education decisions are made simultaneously and probably by a young woman's parents rather than herself. Yet, some papers managed to provide evidence of a causal inference. A large share of the literature highlights that secondary schooling delays marriage (Bharadwaj and Grépin (2015) in Zimbabwe, Ozier (2016) in Kenya and Duflo et al.

(2015) in Ghana). This focus makes sense as the risk to marry is stronger among girls who are old enough to attend secondary school. Yet, the literature has also shown that primary school attendance plays a part in women's exposure to the risk of marriage or of entry in fertile life. Regarding fertility, in Nigeria, Osili and Long (2008) have found that free primary education led to a decrease in fertility for girls who have benefited from the reform. Samarakoon and Parinduri (2015) exploit the lengthening of the school year by six months in Indonesia and show that primary education reduces the number of live births, which is in line with Breierova and Duflo (2004) findings'. Additional evidence from sub-Saharan Africa finds consistent results (Baird et al. (2010)).[1]

Beyond her age at marriage, education could also affect a woman say in the choice of the partner (Banerji (2008)) or the quality of the match, in case of assortative matching (Fafchamps and Quisumbing (2007)). More generally, this higher say and higher education could go with a lower gap in age and in education with the husband, that can translate into a higher bargaining power and therefore a lower tolerance to intimate partner violence (IPV). The relationship between education and acceptance of domestic violence has seldom been studied by the economic literature and the current body of work provides mixed evidence. Mocan and Cannonier (2012) take advantage of the variation in the exposure to a free primary education program and an increase in funding dedicated to primary schools in Sierra Leone in 2001 to show that education reduces women's propensity to approve of wife beating. More recently, Erten and Keskin (2018) exploited a change in compulsory schooling law in Turkey. Using a regression discontinuity design, they demonstrate that increased women's schooling leads to a rise in self-reported psychological violence among rural women but find no impact on tolerance of IPV.

[1]Conversely, Field and Ambrus (2008) study how later marriage increases schooling, using age of menarche as an instrumenting variable for marriage.

The purpose of this paper is to look at the impact of a school construction program, implemented in the 1990s, on women's welfare within the household in Benin. In a first step, we check whether the policy increased attendance rate as the link between school supply and attendance is not straightforward. In a second step, we look beyond educational outcomes and analyze to what extent the school construction policy modified age at marriage, age at first child and tolerance of IPV. Exploiting the Demographic and Health Survey of Benin, we use the quasi-experimental geographical and historical variations in the number of schools built in the 1990s in Benin. The impact of the program is first assessed using a difference in difference and then in a regression kink design framework. In the context of Benin, this type of policy is particularly relevant to track changes in women's outcomes. Indeed, girls turned out to benefit more than boys from the policy as they were less likely to have enrolled to primary school compared to their male counterparts at that time (in 1990 in Benin, the primary school enrollment is of 27% for girls and 52% for boys[2]).

This paper contributes to the literature on primary education and women's welfare in several ways. First, we document the impact of a program of school constructions in Benin that has never been quantitatively evaluated. Second, we offer causal evidence of the link between education and women's status and well-being, relying on geocoded data at a rather granular level. Eventually, we complement the classical approach of the double difference with a strategy inspired from a method rarely used so far in this context: the regression kink design (RKD) [3].

Our findings are threefold. First, we find that the increase in school constructions

[2]These figures come from the World Bank database.

[3]So far, and to the best of our knowledge, the RKD has especially been used in a political economy literature focusing on industrialized countries, using administrative data (Landais (2015), Simonsen et al. (2010) for instance).

led to higher primary school attendance among rural women, while we find no such evidence among urban women. Second, we find that the education program increases the age at marriage, as well as the age at first child. Third, we find that access to primary education decreases tolerance towards domestic violence. Our work documents that a policy that was not initially designed to target women's welfare had beneficial effects on female beneficiaries beyond the initial intended goals of policy makers.

The remainder of the paper proceeds as follows. Section 2 describes the context of the increase in government spending for education in Benin in the 1990s and the data used in the analysis. Section 3 details the identification strategy and section 4 presents the results. Robustness tests are performed in section 5 and section 6 discusses the potential channels that may explain the effects we find. Section 7 concludes.

2 Context and Data

2.1 Education Policies in the 1990's in West Africa

In 1990, 155 countries gathered at the *World Conference for Education for All* in Jomtien (Thailand), and pledged to reach universal primary education for all children by 2015. At the end of the 1990s, this priority was reaffirmed by the international community as one of the eight Millennium Development Goals (MDG's). They were designed by world leaders to frame national policies. These two international milestones kick started large investments in education in developing countries, including in sub-Saharan Africa, in the shape of school constructions or free primary schooling. At the continental level, the Conference of African Ministers of Education (MINEDAF), held in Dakar in 1991, endorsed the program MINEDAF VI which launched the financial efforts needed to achieve universal education in Africa.

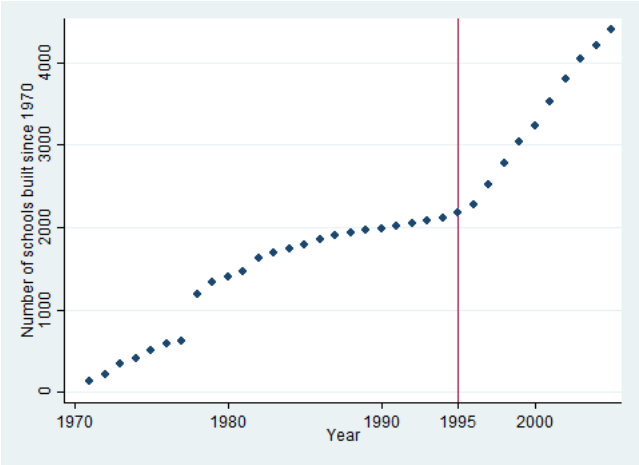
In Benin, since the holding of the Conference for Education in 1991, primary education has been promoted as a priority of the government[4]. A reform of education, whose objective was to improve infrastructure and increase girls' enrollment in primary school, was launched in 1992-1993. Figure 1 shows that, even though there exists a change in trend before 1997, this year sees a sharper change in the rhythm of schools built[5]. Between 1997 and 2003, more than 1500 schools were built by the State or by NGOs, as can be seen in figure 1. This surge reached all districts in Benin, as shown in table A1 and figure 3 in Appendix. During that period, total enrollment increased from around 722000 to 911000 pupils (26.2%) [6]. The surge in the number of pupils is mainly driven by girls enrollment and is consistent with the observed kink in the share of women who went to primary school shown in figure 2. The policy impacted more women than men (at least for primary school attendance), since the school enrollment was lower for women. The rise in school constructions that occured in 1995 is mirrored in the steady increase in the share of women going to primary school starting for those born after 1984. It means that even women aged 12 years old at the time the policy was launched are treated, which is not surprising in a context where children can enter school late, and where age is not well-known. It could also reflect a policy implemented in two steps; first, girls were encouraged to enroll and existing schools were filled, and then new schools were built.

[4] As presented in the National report on the Development of Education prepared for the International Bureau of Education, 2001.

[5] To the best of our knowledge, there are no official document explaining with there is a delay between the announcement of the policy and the change in school constructions that we see in the data. We assume that it is due to potential delays in disbursement of the funds dedicated to the program and to the time required to actually build the schools.

[6] World Bank Country Status Report: "'The Beninese education system, performance and room for improvement for the education policy"', 2002.

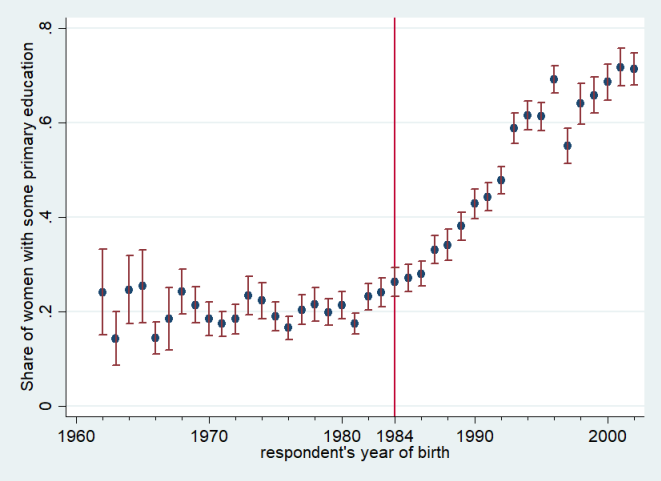
Figure 1: Number of schools built by year in Benin



Note: The figure presents the number of schools built by year in Benin, since 1970.

Source: PASEC data on school construction in Benin.

Figure 2: Share of women attending primary school by cohort in Benin



Note: The figure presents the share of women who attended primary school, by birth cohort in Benin.

Source: DHS Benin 2006, 2011 and 2017. Confidence intervals: 95%.

Benin's efforts in terms of infrastructure came on top of an already existing legal framework that made primary schooling compulsory as soon as 1975. Indeed, the revolutionary regime in place at the time already considered education as a priority. Yet in 1990, at the time of a regime change and in a context where countries were pledging their commitment to a larger access to education, Benin reasserted that primary schooling was mandatory by enshrining it in the Constitution.

2.2 Data

DHS Dataset

This study uses the Demographic and Health Surveys (DHS) for Benin (2011 and 2017). The DHS collects information on women aged 15-49 years old in an harmonized manner across countries. Information collected in all surveys includes women's marital status, age at first marriage and age at first birth among other variables.

The DHS also collect data on women's tolerance of IPV. The respondents are asked whether they find it justified for a husband to beat his wife in a series of five scenarii; whether a woman goes out without telling her husband, if she neglects the children, argues with him, refuses to have sex or burns the food. These variables will be used as our main outcome of interest. Though there exists a module collecting data on women's actual experience of violence, Benin did not include such questions in its 2011 survey.

The data is geocoded so that we can locate the DHS survey clusters. In order to maintain confidentiality, the DHS Program randomly displaces the latitude and longitude of the clusters. They are moved by 0 to 2 kilometers in urban areas and rural clusters are displaced by 0 to 5 kilometers, with 1% of them moved by up to 10 kilometers. Because of this random displacement rule, we build a buffer of no less than 10 kilometers radius around the DHS clusters to have a measure of exposure to primary schooling

that is granular enough but that also limits the error in measurement induced by the displacement. It is also worth noting that the DHS Program randomly displaces clusters but sees to keeping the clusters within their actual municipality.

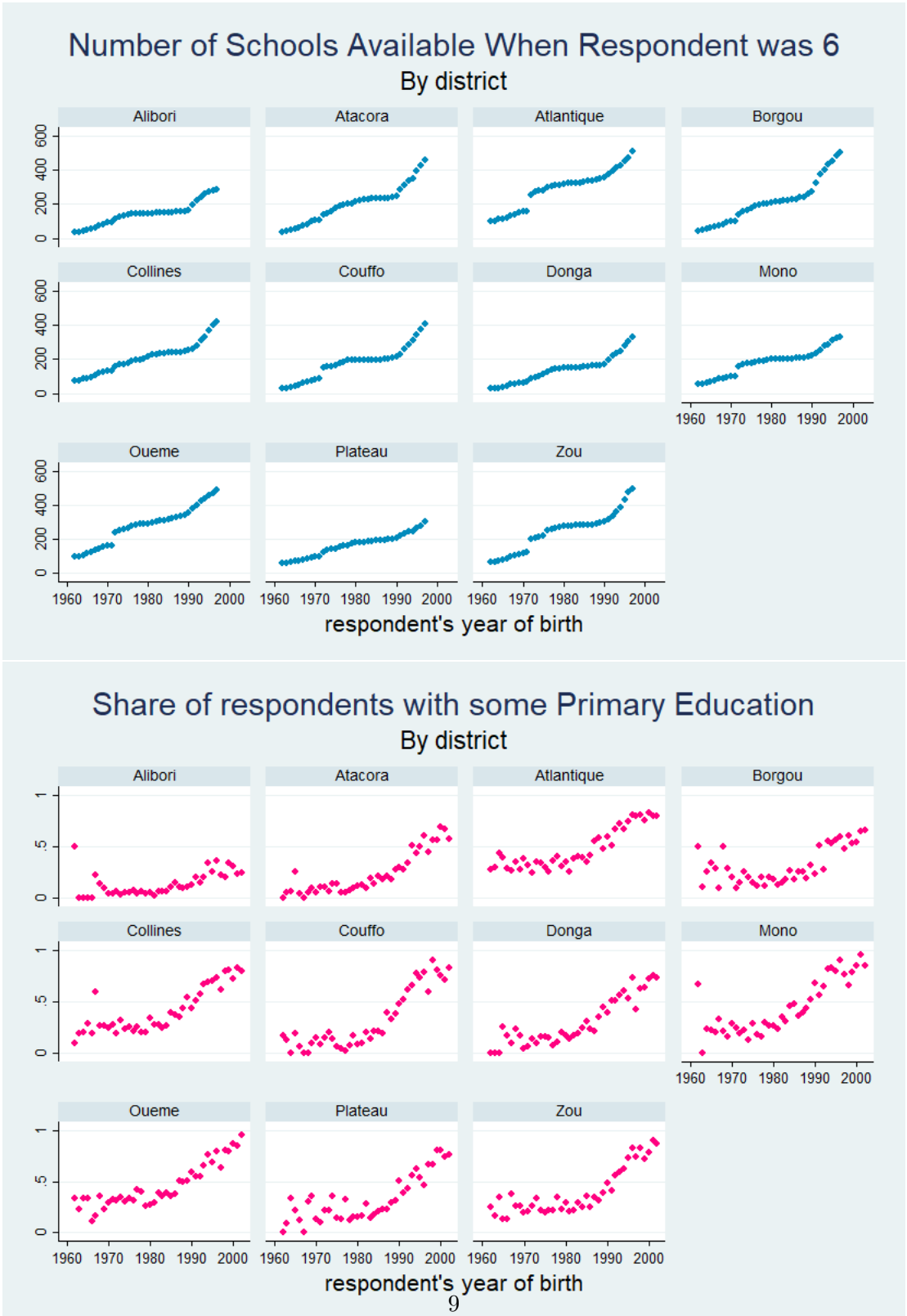
Schools constructions Dataset

In addition to the DHS, we use an administrative database of schools constructions in Benin. It provides the number of school built per year in each of the 12 districts and 76 municipalities of the country between 1970 and 2005. The original school construction dataset contained the town area where the schools were built. We geocoded the data based on the school name, which allowed us to have an even more precise location of the schools. The districts were matched with the 2011 and 2017 DHS datasets for Benin, thanks to DHS's own geolocalization of its clusters. The resulting dataset allows us to know how many schools were built in a buffer around the DHS cluster of a respondent when she was of schooling age. Figure 3 presents the number of schools available when women were of schooling age and primary education by district and cohorts in Benin. It shows that even though the intensity of the treatment and the potential response to the treatment is different across districts, the policy reached all districts of Benin. The difference in difference strategy allows us to take advantage of the differential treatment intensity within Benin across time and space.

Quality of school constructions data

We benchmark our school constructions data against the numbers provided by the 2009 World Bank Report on Schooling in Benin. Table 1 shows that, despite some measurement error, our data accurately estimate the number of schools built in Benin at the national and department levels.

Figure 3:



Source: Upper part of the Figure: DHS Benin 2011. Lower part of the graph: PASEC data on school constructions in Benin.

Table 1: Assessment of the Quality of School Data

	Number of primary school in Benin in 2005/2006	
	Source	
	WB 2009 Report	Administrative dataset
ATACORA / DONGA	868	848
ATLANTIQUE / LITTORAL	1259	861
BORGOU / ALIBORI	881	832
MONO / COUFFO	894	828
OUEME / PLATEAU	1149	795
ZOU / COLLINES	1091	1010
Total	6142	5174

Note: This table presents the total number of primary schools available by districts, as defined before 2006.
Source: World Bank Country Status Report n165 "Le systeme éducatif béninois : Analyse sectorielle pour une politique éducative plus équilibrée et plus efficace", 2009.

Descriptive Statistics

In Benin, the age at marriage averages 19 years old. The share of women who have been married before fifteen years old is equal to 13%, and is comparable to other countries in the region, such as Senegal and Sierra Leone (table A2 in Appendix). Concerning the acceptance of domestic violence, the homogeneity of the averages between the five different items, within country, is striking. In Benin, 10% of women condone IPV for the tree first items (going out without telling the husband, arguing with the husband, neglecting the children), whereas it is around 7% for the two last items (refusing sex and burning the food). Benin exhibits the lowest level of tolerance of IPV among the countries in the sub-region. The correlation matrix (table A3) in Appendix suggests that the first three items are more strongly correlated, compared to the last two, confirming that, in nature, those items appear different. As a result, we choose to build a dummy variable called "commonly accepted offense", equal to one if the individual answers yes to at least one of the three first items, 0 if she answers yes to none. We also build a variable called "less commonly accepted", that takes the value one if the woman answer

Table 2: Number of schools built

Variables	Urban	Rural	Diff.
Number of schools in the cluster in 1996	2.52	1.96	0.56***
			(0.00)
Number of schools built in the cluster between 1997 and 2003	18.89	12.55	6.33***
			(0.00)
Number of schools built in the cluster between 1997 and 2003 for 1000 children	1.08	0.91	0.16***
			(0.01)
Number of schoolst built between 1997 and 2003/Stock in 1996	0.55	0.67	-0.13***
			(0.00)
Number of clusters	413	719	1132

Note: The table presents the differences in mean of school allocations between 1997 and 2003, according to the status of the cluster (in a rural or urban area). We look at the stock of schools available in 1996 in the cluster, before the policy, and to the number of schools built between 1997 and 2003, in absolute terms and in relation to the already available stock. Sample: DHS clusters. Cotonou is excluded.

Source: DHS Benin 2011 and 2017.

yes to one of the two last items (finding wife beating acceptable if a woman refuses sex or burns the food).

2.3 Same treatment, different recipients

Though both rural and urban areas were impacted by the school construction program, the intensity and determinants of the program appear to have been different in cities and in the countryside.

First, the surge in school constructions relative to the initial stock of schools just before the program begun, was stronger in rural areas than in urban areas. The second to last line of table 2 shows that this difference in the intensity of the program is both economically and statistically significant[7].

Not only was the intensity of the program different but the correlates of school constructions also differ across rural and urban areas. As shown in the last two columns of

[7]In this analysis, we choose to exclude Cotonou because of the specificity of this agglomeration, since Cotonou is the economic capital city.

Table 3: Schools allocation between 1997 and 2003

	Number of schools		Number of schools for 1000 children	
	Urban	Rural	Urban	Rural
Number of children in the municipality	0.001***	0.000***	-0.000***	-0.000***
	(0.00)	(0.00)	(0.00)	(0.00)
Female primary attendance average	11.828***	-0.177	0.760***	0.058
	(2.61)	(1.77)	(0.18)	(0.14)
Number of clusters	407.00	713.00	407.00	713.00
r2	0.37	0.02	0.07	0.04
F	130.81	4.38	9.58	15.78

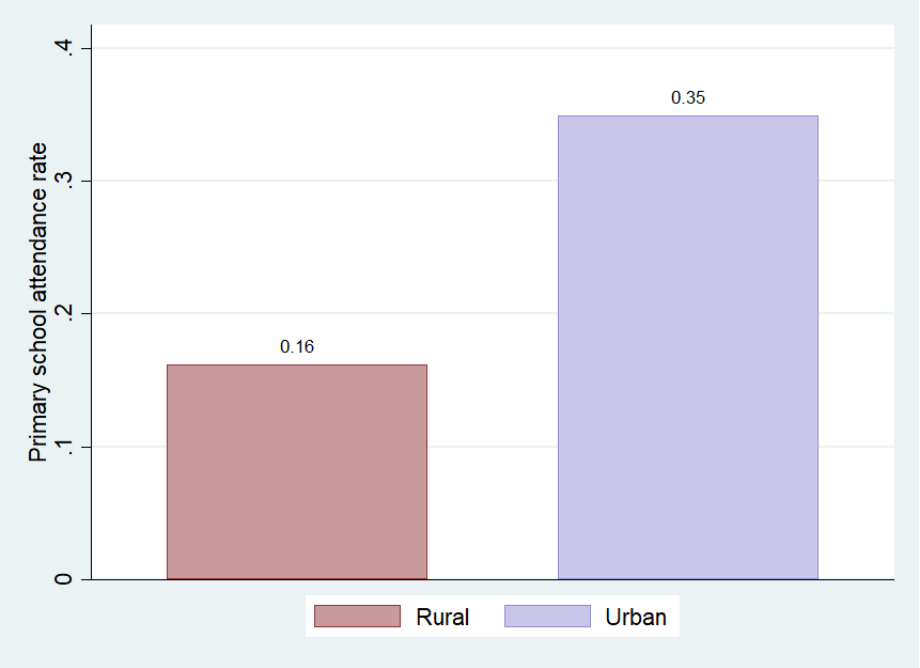
Note: In the first two columns, the dependent variable is the number of school built between 1997 and 2003 in the cluster. In the two last columns, we rescale this number to have the number of schools built on the same period, for 1000 children in the municipality. As explaining variables, we include the number of children in the municipality in 1993 and the share of women having been to primary school among the control group (women born between 1980 and 1985), by clusters. Sample: DHS Benin 2011 clusters. Cotonou is excluded.

Source: DHS Benin 2011 and 2017.

table 3, the correlation between women attendance before the program and the intensity of the treatment is twice as high in urban areas, compared to rural ones. Surprisingly, it seems that in urban areas, clusters were women were already more educated benefited the most from the program. It doesn't seem to be the case in rural areas.

This latter point brings us to the next, which is that the profile of women impacted by the policy differs in rural and urban areas. In urban areas, before the program started, the level of education was more than twice as high compared to rural areas, as shown in figure 4. In addition to this, table 3 showed that that more schools were built in urban clusters where women were more educated. As a result, in rural areas, we expect more women to go from no schooling to some primary schooling. In other words, the policy is more likely to go through an extensive margin effect. Women at the extensive margin in urban areas are probably more peculiar than women at the extensive margin in rural areas.

Figure 4: Primary School Attendance rate among women aged 12 and more in 1997



Source: DHS Benin 2011 and 2017. Women born between 1980 and 1985. Cotonou is excluded.

3 Methodology

3.1 Difference in Difference

Following in the footsteps of Duflo (2001) seminal paper, we first use a difference-in-difference to identify the causal impact of the rise in schools constructions on our outcomes of interest. We exploit the fact that women's exposure to the policy varies according to their birth cohort and municipality of residence. Yet, we go a step further as we take advantage of the geolocalization of the DHS clusters in the 2011 and 2017 DHS survey. We mapped the geocoded DHS clusters and build a 10km radius buffer around them[8]. Using the geocoded school constructions database we built, we count the number of schools that were built between 1997 and 2003, in a 10km radius around a woman's DHS cluster. As a result, exposure to the program varies according to women place of residence and age at the time the program was implemented[9]. Age at entry into primary school is set to 6 years old in Benin but it is not rare that children enter and/or stay in school beyond the official age. With that in mind, in our first specification, we define the exposed cohort as women aged 4 to 8 years old in 1997 and the untreated cohort as women aged 12 to 17 years old when the program began. This choice of cohort may lead to an attenuation bias as some women in the control cohort may be exposed to the education program because of late entry at school. We estimate the following model:

$$y_{imc} = a_0 + \beta_c + \theta * N_g + \delta * N_g * TREAT_i + \alpha_m + \eta X_i + \gamma Z_{mc} + \varepsilon_{imc} \qquad (1)$$

where y_{imc} is the outcome of interest for individual i, residing in municipality m and born in year c, a_0 is a constant and α_m is a municipality of residence fixed-effect[10]. β_c is

[8]The choice of the buffer's size is justified in Section 2

[9] We proxy the place of birth of women by their place of residence. We discuss the implications of this approximation in Section 5 .

[10]We do not apply a DHS cluster fixed-effect because the number of observations in each cluster ranges from 7 to 42 with an average of 23, which we deem to be too few.

a cohort of birth fixed-effect, N_g is the number of schools built between 1997 and 2003 in a 10km-radius around a woman's DHS cluster of residence. It can be read more broadly as the intensity of the program in cluster g. $TREAT_i$ is a dummy variable equal to 1 if the individual was born between 1989 and 1993, equal to 0 if she was born between 1980 and 1985[11]. We also add a set of individual controls X_i including religion and ethnicity. Finally, Z_{mc} is a municipality-specific year effect of the density of children of schooling age before the program begun. This particular control is added because we believe that, should the density of children of schooling age play a role in the implementation of schools on the Beninese territory, the impact of the initial density overtime may vary according to the municipality. Since the intensity of the treatment is correlated to the initial attendance rate, we also control for a municipality-specific year effect of the initial attendance rate. As suggested by Duflo (2001), it allows us to make sure our estimates do not capture a simple reversal to the mean of the primary attendance rate (and therefore a differential in pre-trends). We control also for age effect, introducing the age and the age squared in the specification. The identification assumption would also be violated if the school constructions are correlated with other governmental programs initiated at the same moment. To our knowledge, no other program was implemented at the same time. This is not surprising, since education had been identified as a priority and fundings were limited. Furthermore, since we are exploiting the geographical variation in the schools construction at a very granular level, it seems highly unlikely that we are capturing the impact of another campaign.

When presenting our results, we also provide placebo tests using model 1. In those placebo tests, $TREAT_i$ is a dummy variable equal to 1 if the individual was born between 1980 and 1984, equal to 0 if she was born between 1974 and 1979. In other words, our placebo compares women aged 13 to 17 years old in 1997 to women aged 18 to 24

[11] Because of late entry in primary school, some of the women born in 1985 could be exposed to the treatment. If it were the case, it would bias our estimates downwards.

years old in 1997. Those women are not expected to have benefited from the education program. If the education has started to increase in treated regions before the surge in schools construction, the coefficient δ will be positive and significant.

In Section 4, we also present the effect of the policy on the outcomes of interest per age at the time the policy was implemented which is tantamount to studying the effect of the policy on a given cohort. The results shown are yielded by the following specification:

$$y_{imc} = +\beta_c + \theta * N_g + \sum_{a=2}^{21} \delta_a * (N_g * v_{ia}) + \ + \eta X_i + \gamma Z_{mc} + \varepsilon_{imc} \tag{2}$$

where y_{imc} is the outcome of interest for individual i, residing in muncipality m and born in year c, a_0 is a constant, α_m is a municipality of residence fixed-effect, β_c is a cohort of birth fixed-effect. N_g is the number of schools built between 1997 and 2003 in a 10km-radius around a woman DHS cluster of residence g, v_{ia} is a dummy indicating whether individual i was age a in 1997, X_i includes religion and ethnicity of the individual, and Z_{mc} is a municipality-specific year effect of the density of children of schooling age before the program begun. Standard errors are clustered at the DHS cluster level.

The literature on the impact of education on age at marriage has to tackle several sources of endogeneity. First, there could be an omitted variable bias: some unobservable characteristics, such as the socioeconomic characteristics of parents, can explain both education and child marriage. We are confident that the double difference takes care of this bias.

Second, there is a simultaneity bias. Indeed, parents decide who and when their daughter marry, especially at ages when girls attend primary school. The decision to

have her marry or to keep her in school is made simultaneously, not sequentially. Rosenzweig and Wolpin (2000) have shown that even when using natural experiment, this simultaneity bias prevents researchers from pinning down causal estimates. Translated to our context, when studying the impact of the education policy on child marriage, instrumenting education with exposure to school constructions would violate the exclusion restriction. As a result, we believe it is illusory to try and instrument education with exposure to school constructions when education and marriage are decisions taken by the same person at the same moment. Finally, increase in school constructions can impact the probability to marry as a child, because the education policy also spurs changes in the norms of age at marriage or tolerance to domestic violence without going through a girl's own education. In this case also, the exclusion restriction is violated. In this paper, we suggest a way around these potential sources of bias. Instead of looking at the impact of education on women's welfare outcome, we choose to remain agnostic about the channels through which the education policy impacts these outcomes and we treat primary education as an outcome. Consequently, we look only at reduced forms. It is also more cautious in a context where the educational program could have had an impact on the quality of education at the same time[12]

3.2 Regression Kink Design

Emulating Duflo (2001) using a difference-in-difference has become a classical method used in the development literature to assess the impact of a shock whose effect varies with time and place. Yet, given that the increase in school constructions in Benin in the 1990's follows a linear trend and that our current specification absorbs a good share of this variation with the cohort of birth fixed effects, we choose to apply another strategy as well, inspired from the regression kink design (RKD). With this "kink-in-difference" design, we exploit the geographical variation and more of the time variation available in

[12]The negative impact of school expansion programs on the quality of education is well documented in many contexts (Duraisamy et al. (1998), Deininger (2003)).

the data.

Originally, the RKD exploits a change in slope of the likelihood of being treated at a kink point. If the outcome also exhibits a kink at the same point, then the causal impact is found by dividing the change in slope for the outcome by the change in slope for the treatment. This method has often been used in public economics (Simonsen et al. (2010), Landais (2015), Card et al. (2012) and Card et al. (2015)). It allows us to use the information included in the slope of the treatment, continuously for every individuals born around the kink. Since the program triggered a change in trend in the number of schools built continuously over time, this approach seems also adapted to the setting.

Here, we draw inspiration from the RKD and we exploit the change in the trend in the exposure to schooling defined as the number of schools built between 1997 and 2003 in a 10 kilometers radius around a given cluster and according to the birth cohort.

Since we use both historical and the geographical variations, we will look at the following reduced form:

$$\begin{aligned} SchoolAttendance_i = a_0 + \alpha_m + \beta * (BirthCohort_i - 1984) + \gamma * (BirthCohort_i - 1984) * Post + \\ \lambda * (BirthCohort_i - 1984) * Post * N_g + \\ \mu * X_i + \varepsilon_i \end{aligned} \tag{3}$$

where N_g is the number of schools built between 1997 and 2003 in a 10km-radius buffer around individual i's DHS cluster of residence g. α_m is a municipality of residence fixed-effect. The coefficient of interest is λ, which measures the change in the slope of school attendance, by municipality, once the policy has been implemented. We also add municipality fixed effects and individual controls (religion and ethnicity), as well as a

differenciated trend according to the initial enrolment in the municipality and the initial number of children of schooling age. Instead of cohort of birth fixed-effect, we include a time-trend control. Since the increase in exposure to schooling is linear, this strategy is more flexible that difference-in-difference specification with birth cohort fixed-effects and provides more statistical power. Standard errors are clustered at the DHS cluster level. We show in Section 4 that the two strategies yield consistent estimates.

3.3 Duration Model of Entry into Marriage or Motherhood

We identify the effect of primary education on women's well-being on a sample of women aged 18 to 32 years old at the time of the survey. Yet the median age at marriage in Benin is nearly 18 years old. As a result, there is a non negligible share of right-censored observations when we study marital and motherhood outcomes (age at first marriage, age at first child, birth spacing between the first and second child). As a consequence, the difference-in-difference strategy or the RKD for these outcomes yield estimates that are biased by women who entered their marital or fertile life earlier than the average Beninese women. To circumvent this selection issue, we use a duration model of entry into marriage, into motherhood and a model of the interval between the first and second birth. The duration models are able to deal with right-censored observations in ways the usual regression models cannot. Such models have been used in the literature to pin down socio-economic correlated to birth spacing in sub-Saharan Africa (Ghilagaber and Elisa (2014)) or to study son preference through birth spacing (Lambert and Rossi (2016), Rossi and Rouanet (2015)).

We use a discrete time duration model to test whether being exposed to more primary schooling is related to a delay in marital and fertile life. Our variable of interest is t, the duration between birth of a respondent and the age at which she cohabited for the first time with a partner or the age at which she had her first child. Though it is rather

common in the literature to use a proportional hazard (PH) model or Cox model, we choose the discrete time duration model for two main reasons. First, in models such as the Cox model, time is strictly continuous. There cannot be simultaneous events. A duration t_i that led to the studied event should be associated with one observation i if the clock used to measure the duration is precise enough. Yet in the DHS as in many other household surveys, data are collected with a discretized time. As a result there are many simultaneous events (woman born the same year entering their first union at the same age for instance) which violates a necessary condition of the Cox model[13]. Second, the Cox model relies on the assumption of PH, which in our case translates into the ratio of the risk of experiencing the event is constant between treated and untreated women at every moment of the duration studied[14]. Yet, we could imagine that for women from older cohorts, the risk of experiencing the event of interest (getting married for the first time or having a child) intensifies at an earlier moment of the duration studied than for treated women[15]. The discrete time model allows us to circumvent this potential issue since time is introduced as a covariate. The risk is modeled as a conditional probability and the estimation relies on the maximization of a binomial-type likelihood. The most commonly used function is the logistic regression[16]:

$$log(\frac{p}{1-p}) = a_0 + \sum_p a_p * t + \sum_p a_p * t^2 + \sum_k a_k * X_k \tag{4}$$

In the double-difference approach, X_k includes N_g the number of schools built between 1997 and 2003 in a 10km-radius buffer around an individual DHS cluster, $TREAT$

[13]This constraint can be alleviated by correcting the partial likelihood function with simultaneity using the "Breslow" method, which is the method used by most statistical software or programming language like STATA or R respectively.

[14]It is important to remember that the duration studied corresponds to the years between the birth of the respondent and her first union/child.

[15]The log-log plot test seems to suggest otherwise though, which points to the respect of the PH assumption.

[16]The model is applied to a database reshaped according to the principle: one line is one observation-at time t. For instance, if a woman experiences the event at at time 2, she will appear as two lines in the reshaped database

the binary exposure to treatment according to the cohort of birth, $N_g * TREAT$, a municipality of residence fixed-effects, individual controls (religion and ethnicity) as well as cohort of birth fixed-effects and municipality specific time effect of the initial attendance and of density of children of schooling age.

For the RKD inspired specification, X_k includes N_g, the number of schools built between 1997 and 2003 in a 10km-radius buffer around an individual DHS cluster of residence, the cohort of birth centered at the kink $BirthCohort_i - 1984$, the latter interacted with $Post$, $(BirthCohort_i - 1984) * Post * N_g$. X_k also includes a municipality fixed effects and individual controls (religion and ethnicity).

4 Results

4.1 Double Difference

All the tables of this section are split between *Panel A* that shows the results of the regressions of interest, and *Panel B* that displays the results of placebo regressions. The placebo difference in difference regressions rely on the comparison of cohorts that are supposed to be unaffected by the policy intervention. In *Panel A* of table 4, we provide evidence that the education policy increased attendance of primary school among girls of schooling age in 1997. On average, one school built in a 10km radius around a cluster for 1000 children in a municipality, rises the probability to have enrolled to primary school by 4.1 percentage points in rural areas[17]. The effect is robust to controlling for enrollment and density of children of schooling age in the municipality before the program started. The placebo test in *Panel B* shows that earlier cohorts were, as expected, unaffected by the policy. The test also hints at the fact that there was no pre-existing change in trend in primary education that may be confounded with the effect of the schooling program. Figure 5 allows to single out the cohorts that were more affected by the program. It

[17]For a child born in 1982, at ten years old, they were on average 1.2 schools per thousand children. For a child born in 1992, they were on average 1.9 schools per thousand children

shows the coefficients identified by equation 2. The figure provides visual confirmation that women aged 4 to 11 in 1997 in rural areas benefited from the program and suggests that the younger in 1997, the more intense the effect of school constructions on primary school attendance. Mistakes in the declaration of age in household surveys are a well-know phenomenon in many countries of sub-Saharan Africa. One of its manifestation is a preference for round numbers or age heaping. This age heaping brings noise to our estimation as can be seen in figure 5, yet it does not prevent us from identifying a clear change in the trend for beneficiaries of the school constructions program. Because we are interested in improvements in women's well-being induced by education, from now on, our analysis will focus on women currently living in rural areas.We show that our results on education are robust to the addition of younger cohorts in table A4 in the Appendix. [18]. The fact that we only capture effects in rural areas and no effects in urban areas where growth may be higher is a hint that it is unlikely that a change in growth trend drives our results.

Table A5 in Appendix puts forward that the benefits of the intervention on primary school attendance do not seem to extend to secondary school attendance, as we find no significant effect of the program on such variable.

[18]The fact that the effect is not significant and of negative sign in urban area could also be driven by the approximation that we do using the number of schools built in the current place of residence. There is much more migrant women in urban areas (among the treated women 30% versus 16% in rural areas), and this high level of migration could introduce a complex bias in the results. This is an additional reason why we are more confident about the causal impact of the treatment for rural women.

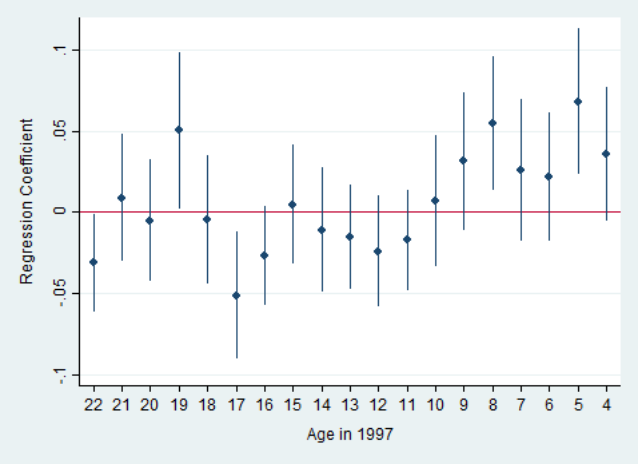
Table 4: Probability of primary school attendance

	All	Urban	Rural
Panel A: Interest Experiment: Individuals aged 4 to 8 or 12 to 17 in 1997			
Number of school built between 1997 and 2003 * Treat	0.018	-0.004	0.041***
	(0.01)	(0.02)	(0.01)
Controls Individual	Yes	Yes	Yes
Mean Dep. Var.	0.35	0.47	0.27
N	10026	3785	6241
r2	0.25	0.27	0.21
F	24.20	13.27	13.93
Panel B: Placebo Experiment: Individuals aged 13 to 17 or 18 to 24 in 1997			
Number of school built between 1997 and 2003 * Placebo	-0.016	-0.011	-0.016
	(0.01)	(0.02)	(0.01)
Controls Individual	Yes	Yes	Yes
Mean Dep. Var.	0.22	0.35	0.15
N	9372	3345	6027
r2	0.22	0.25	0.12
F	9.57	6.12	3.82

Note: The dependent variable is having attended primary school. All specifications include municipality dummies, year of birth dummies and interactions between the year of birth dummies and the number of children in the municipality of residence in 1993. We control for ethnicity, religion, age, age squared and for the interaction between year of birth dummies and the attendance rate in 1993 in the municipality of residence. Sample: Eligible women aged 15-49 years old.

Source: DHS Benin 2011 and 2017.

Figure 5: Effect of the treatment on school attendance by birth cohort



Note: The figure presents the coefficients of the interaction of respondent's age in 1997 and the number of schools built between 1997 and 2003 in the region of residence in equation (2). The dependant variable is having attended primary school. Sample: Rural women aged 15-49 years old.

Source: DHS Benin 2011 and 2017.

We then investigate changes in the age at first marriage, in the probability to be married as a child, as well as changes in age at first birth or birth spacing between the first and second child[19].

Table A6 in the Appendix shows that, with the difference-in-difference model estimated with OLS, we find no effect of the education policy on marital outcomes nor on entry into motherhood or birth spacing. Yet, as mentionned earlier, this model does not take into account the right-censored nature of the data, unlike duration models. Table 5 presents the results with the discrete time duration model. It provides evidence that the education program delayed entry into marital life and motherhood in rural area for the treated cohort. For instance, for age at marriage, building one school per 1000 children decreased the probability of experiencing the event by 12.5 percentage points relative to the older cohort. We find similar results using a Cox model (table A7 in Appendix). However, it does not decrease the probability to be married as a child [20].

[19]The demographic literature has expressed concern about the measurement errors for the age at first marriage due to recall issues. The DHS's interviewer manual (ICF (2017)) states that age at first union is collected by asking women the month and year when they started cohabiting for the first time with a partner. If they do not know the year, the interviewer has to probe the year of first cohabitation. They are advised to do so based on the year of the first birth collected earlier in the survey and by asking how long after the beginning of the union the respondent gave birth to her first child. If the interviewer is unable to have an answer for the year of the first cohabitation, he asks women at which age she started cohabiting with a man for the first time. Like for the age at the time of the survey, if the interviewer does not get an answer, she probes the age following the procedure described earlier.

[20]Among women married as children (before 15 years old), we find a positive but not significant impact of the school constructions program on primary school attendance. Though it may be because of a lack of power, it could mean that those women are "non compliers" to this education policy. See table A8 in Appendix.

Table 5: Marital Outcomes - Discrete Time Duration Model

	First union	First child	First child Married women
Panel A: Interest Experiment: Individuals aged 4 to 8 or 12 to 17 in 1997			
Number of school built between 1997 and 2003 * Treat	-0.125***	-0.129***	-0.070*
	(0.04)	(0.04)	(0.04)
Controls Individual	Yes	Yes	Yes
Mean Dep. Var.	0.05	0.05	0.05
N	114545	120594	107035
r2_p	0.26	0.30	0.31
chi2	6072.17	5861.35	5580.94
Panel B: Placebo Experiment: Individuals aged 13 to 17 18 to 24 in 1997			
Number of school built between 1997 and 2003 * Placebo	0.019	0.024	0.020
	(0.03)	(0.03)	(0.03)
Controls Individual	Yes	Yes	Yes
Mean Dep. Var.	0.05	0.05	0.05
N	113657	119915	118039
r2_p	0.24	0.28	0.28
chi2	6571.37	6554.33	6586.37

Note: The dependent variable is in the following order: time before marriage, time before first child and time between first and second child. The table presents the coefficient beta, and not the log odd-ratio. The number of observations changes between the different outcomes, since not all women have faced such events at the time of survey. The number of observations is also higher than in the OLS estimates, because data are reshaped: one observation corresponds to one year for woman. All specifications include municipality dummies, year of birth dummies and interactions between the year of birth dummies and the number of children in the municipality of residence in 1993. We control also for ethnicity, religion, age, age squared and for the interaction between year of birth dummies and the attendance rate in 1993 in the municipality of residence. Sample: Rural women aged 15-49. In the third column, the sample corresponds to women already married at the time of the survey. In the fourth column, the sample is all women who have already given birth.

Source: DHS Benin 2011 and 2017.

Eventually, we shed light on changes in women's mindset regarding tolerance of IPV. Table 6 Panel A provides evidence that, on average, one school built for 1000 children significantly decreases the probability to condone wife beating for going out without telling husband, neglecting the children, refusing sex and burning the food by roughly 2 percentage points. The effects are significant at the 10% level for burning the food and 5% for the other three motives mentioned. For the last two items, it represents nearly one third of the baseline level of tolerance of IPV for those motives. We suggest an alternative measure of IPV using indexes built with a PCA as outcome variables. It allows us to aggregate information according to the type of violence (commonly accepted and less commonly accepted). Using theses indexes as outcomes variables, the difference-in-difference trategy yields that the decrease in tolerance of IPV caused by the school construction policy was acute for severe offense and less commonly accepted motives for violence (table 7) .

The effect of the treatment on each cohort displayed in Figure 6 illustrates that there is a change in trend occurring for the younger cohorts.

Table 6: Tolerance to IPV

	Goes out without telling husband	Neglects the children	Argues with husband	Refuses sex	Burns the food
Panel A: Interest Experiment: Individuals aged 4 to 8 or 12 to 17 in 1997					
Number of school built between 1997 and 2003 * Treat	-0.023**	-0.030**	-0.010	-0.020**	-0.014*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Controls Individual	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var.	0.17	0.19	0.18	0.11	0.12
N	6241	6241	6241	6241	6241
r2	0.16	0.13	0.11	0.08	0.08
F	4.05	5.70	3.09	2.69	2.98
Panel B: Placebo Experiment: Individuals aged 13 to 17 or 18 to 24 in 1997					
Number of school built between 1997 and 2003 * Placebo	0.013	0.024**	0.015	0.016*	0.003
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Controls Individual	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var.	0.16	0.17	0.18	0.11	0.11
N	6027	6027	6027	6027	6027
r2	0.14	0.12	0.10	0.07	0.09
F	4.23	4.78	3.23	2.75	2.84

Note: The dependent variable is in the following order a dummy taking the value 1 if the woman finds wife beating acceptable if a woman goes out without telling her partner, argues with him, neglects the children, refuses sex and burns the food. All specifications include municipality dummies, year of birth dummies and interactions between the year of birth dummies and the number of children in the municipality of residence in 1993. We control also for ethnicity, religion, age, age squared and for the interaction between year of birth dummies and the attendance rate in 1993 in the municipality of residence. Sample: Rural women aged 15-49 years old.

Source: DHS Benin 2011 and 2017.

Table 7: Tolerance to IPV - Indexes

	Index tolerance to all violence	Index tolerance to commonly accepted violence	Index tolerance to less commonly accepted violence
Panel A: Interest Experiment: Individuals aged 4 to 8 or 12 to 17 in 1997			
Number of school built between 1997 and 2003 * Treat	-0.121**	-0.096**	-0.076**
	(0.05)	(0.04)	(0.03)
Controls Individual	Yes	Yes	Yes
Mean Dep. Var.	0.02	0.02	0.02
N	6241	6241	6241
r2	0.14	0.16	0.09
F	4.62	4.70	3.25
Panel B: Placebo Experiment: Individuals aged 13 to 17 or 18 to 24 in 1997			
Number of school built between 1997 and 2003 * Placebo	0.089*	0.080*	0.042
	(0.05)	(0.04)	(0.04)
Controls Individual	Yes	Yes	Yes
Mean Dep. Var.	-0.02	-0.01	-0.01
N	6027	6027	6027
r2	0.13	0.14	0.09
F	4.40	4.67	3.07

Note: In the first column, the dependent variable is an index for every items of tolerance to domestic violence, built through a PCA. In the second column, the index only covers the first three items: the woman finds violence justified if she goes out without telling the husband, if she argues with him or neglects the children. In the last column, the index covers the two last items: if she refuses sex or burns the food. All specifications include municipality dummies, year of birth dummies and interactions between the year of birth dummies and the number of children in the municipality of residence in 1993. We control also for ethnicity, religion, age, age squared and for the interaction between year of birth dummies and the attendance rate in 1993 in the municipality of residence. Sample: Rural women aged 15-49 years old.

Source: DHS Benin 2011 and 2017.

Figure 6: Effect of the treatment on the index for tolerance of severe violence

Note: The figure presents the coefficients of the interaction of respondent's age in 1997 and the number of schools built between 1997 and 2003 in the region of residence in equation (2). The dependent variable is an index built through a PCA on the two last items of tolerance to violence: in case of sex refusal, or burning the food.Sample: Rural women aged 15-49 years old.

Source: DHS Benin 2011 and 2017.

Table 8: Probability of primary school attendance - Regression Kink Design

	(1)
Normalized birth year* post kink* Number of schools built	0.006***
	(0.00)
Controls Individual	Yes
Mean Dep. Var.	0.25
N	11178
r2	0.20
F	49.85

Note: The dependent variable is having attended primary school. We control by the ethnicity and the religion of the woman. Sample: Rural women aged 15-49 years old.

Source: DHS Benin 2011 and 2017.

4.2 Regression Kink Design

There are several identifying assumptions to check to ensure the validity of the method. First, there must exist a kink in the probability to be treated (so in the exposutre to the number of schools built) according to the running variable, which is the year of birth. It is highligted in figure 1. Second, the treatment assignment has to be "as good as random" at the threshold, ie the kink point. Following the literature, we do that by checking whether covariates are smooth at the kink. As highlighted in the table A12 in Appendix, covariates exhibit no kink at the threshold. Eventually, we have to check that there is no manipulation of the running variable at the kink, using a density test. It is worth noting that, in our opinion, it seems unlikely, in our specific case, that women intentionally manipulate the running variable, as it is their age. In addition to that, at the time of the survey, women have no interest nor reason to declare they were born before or after 1984. Despite that, we still perform a McCrary test and find there is no change in the density at the kink, as highlighted in the table A13 in Appendix.

The RKD strategy yields results that are in line with the one found with the double difference strategy, as shown in table 8. A back of the enveloppe calculation based on the 8 years gap between the kink and the median of the treatment group shows that the RKD strategy yields similar estimates than the one of the double difference for attending primary school ($8 * 0.006 = 0.048$, comparatively to the 0.041, found with the double difference).

As with the double difference, we find significant results on marital outcomes with the RKD as shown in table 9. This is also true when we compare the magnitude of the results ($8 * 0.019 = 0.15$, comparatively to the 0.125, found with the double difference). Using a Cox model, results are similar (table A9 in the appendix). We present the results from the OLS estimation in table A10 in the Appendix. We do not find any significant impact like with the double-difference. For tolerance of domestic violence, we find that the estimates, displayed in table 10, are not significant and lower than those found with the double-difference strategy. Yet, the direction remains consistent with the double difference estimates. As developed in Section 3, part of the differences between the estimates yielded by the two strategies are likely to be driven by a more flexible control for time effects in the RKD. In addition the RKD exploits more of the variation coming from the linearity of the treatment than the difference-in-difference.

5 Robustness Checks

5.1 Are the results driven by the increase in men's education?

The results presented in Section 4 make the case for an increase in women's outcomes driven by their increased access to education. Yet, our results would also be consistent with alternative scenarii. It may be that what matters for age at marriage or IPV is not (only) a woman's own education but the education of her partner. So if the husbands of the women in our sample are also impacted by the reform, the effect captured with

Table 9: Marital Outcomes - Discrete Time Duration Model - Regression Kink Design

	First union	First child	First child Married women
Normalized birth year* post kink*	-0.019***	-0.014***	0.008*
Number of schools built	(0.00)	(0.00)	(0.00)
Controls Individual	Yes	Yes	Yes
Mean Dep. Var.	0.05	0.05	0.05
N	206146	217190	194910
r2_p	0.26	0.29	0.30
chi2	10931.16	10732.89	10310.69

Note: The dependent variable is in the following order: time before marriage, time before first child and time between first and second child. The table presents the coefficient beta, and not the log odd-ratio. The number of observations changes between the different outcomes, since not all women have faced such events at the time of survey. The number of observations is also higher than in the OLS estimates, because data are reshaped: one observation corresponds to one year for woman. We control also for ethnicity, religion, age and age squared. Sample: Rural women aged 15-49 years old.

Source: DHS Benin 2011 and 2017.

Table 10: Tolerance to intimate Partner Violence - Regression Kink Design

	Goes out without telling husband	Neglects the children	Argues with husband	Refuses sex	Burns the food
Normalized birth year* post kink*	-0.00021	-0.00113	0.00000	-0.00048	-0.00120
Number of schools built	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Controls Individual	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var.	0.16	0.18	0.18	0.11	0.11
N	11178	11178	11178	11178	11178
r2	0.14	0.11	0.09	0.06	0.07
F	7.18	10.48	5.35	4.71	5.99

Note: The dependent variable is in the following order a dummy taking the value 1 if the woman finds wife beating acceptable if a woman goes out without telling her partner, argues with him, neglects the children, refuses sex and burns the food. We control also for ethnicity, religion, age and age squared. Sample: Rural women aged 15-49 years old.

Source: DHS Benin 2011 and 2017.

the double difference may overestimate the effect of the reform going through women's own outcomes.

Elements specific to the context studied tend to go against the assumption that our effect is solely driven by husbands. First, in Benin, the mean difference in age between partners is 8 years. It means that the average husband was not impacted by the reform because he was too old to have benefited from it, except, perhaps, for the youngest women in our sample. Only 5% of husbands of treated women are born in 1989 or after. We still perform a test to show that the reform did not have an impact for men. We use the same double difference strategy than for women, explaining primary school attendance. Table 11 displays the results: boys are not significantly impacted by the reform. Table 12 presents the results for the husbands: there is no significant effect of the reform as well. The absence of change in the trend of boys' education can be explained by their already greater access to schooling before (and even after) the reform. A 2002 World Bank report[21] estimated the difference in access to primary school between boys and girls to 22 percentage points in rural areas (86% for boys versus 64% for girls).

The policy studied does not seem to have a significant impact on men's education. Still, we also show in table A11 in the appendix that results on tolerance to domestic violence remain unchanged when we remove women married with husbands potentially affected by the reform. Let it be clear that we are not claiming that matching has no part at all in the effect identified. It is plausible that women who are more educated thanks to the policy tend to wed more educated husbands. In this case, the education of the husband is a channel especially when it comes to domestic violence, but a channel activated by the increased education of women.

[21] World Bank Country Status Report: "'The Beninese education system, performance and room for improvement for the education policy"', 2002.

Table 11: Probability of primary school attendance for men

	Primary school attendance
Panel A: Interest Experiment: Individuals aged 4 to 8 or 12 to 17 in 1997	
Number of school built between 1997 and 2003 * Treat	0.013
	(0.03)
Controls Individual	Yes
Mean Dep. Var.	0.57
N	2273.00
r2	0.32
F	6.71
Panel B: Placebo Experiment: Individuals aged 13 to 17 or 18 to 24 in 1997	
Number of school built between 1997 and 2003 * Placebo	-0.036
	(0.03)
Controls Individual	Yes
Mean Dep. Var.	0.42
N	1808.00
r2	0.21
F	2.74

Note: The dependent variable is having attended primary school. All specifications include municipality dummies, year of birth dummies and interactions between the year of birth dummy and the number of children in the district of birth in 1993. We control also for ethnicity, religion, age and age squared. Sample: Rural men aged 15-49 years old.

Source: DHS Benin 2011 and 2017.

Table 12: Probability of primary school attendance for husbands

	School attendance of the husband
Panel A: Interest Experiment: Individuals aged 2 to 6 or 12 to 17 in 1997	
Number of school built between 1997 and 2003 * Treat	-0.016
	(0.07)
Controls Individual	Yes
Mean Dep. Var.	0.81
N	5440
r2	0.11
F	3.48
Panel B: Placebo Experiment: Individuals aged 13 to 17 or 18 to 24 in 1997	
Number of school built between 1997 and 2003 * Placebo	0.046
	(0.08)
Controls Individual	Yes
Mean Dep. Var.	0.76
N	5793
r2	0.09
F	2.24

Note: The dependent variable is having a husband who has attended primary school. All specifications include municipality dummies, year of birth dummies and interactions between the year of birth dummy and the number of children in the district of birth in 1993. We control also for the ethnicity and the religion. Sample: Rural women aged 15-49 years old.

Source: DHS Benin 2011 and 2017.

5.2 Migration

One caveat of our work is that we use the number of schools built in the current residence of the respondent, and not in the residence of birth. Unfortunately, DHS data does not include information on the respondent's municipality of birth. To measure the extent of the approximation we make using the place of residence, we use the Beninese census and compute statistics on migration. We find that 18% of women born between 1989 and 1993 have migrated between municipalities. Ideally, we would have liked to match the location of the schools with the place of birth of womens and check whether we find consistent results on primary school attendance with the census data. But the data on the Beninese census are unfortunately not precisely geolocalised [22]. However, since our main analysis is conducted on women living in rural area, we do not think that this approximation is a major threat to our identification strategy. First, there are far more migrant among women currently living in urban areas than in rural area. According to the census, only 16.8% of women who actually reside in rural areas are migrant while this rate is as high as 30.9% in urban areas. Furthermore, migrant women currently living in urban areas are much more educated than non-migrant women who are currently living in rural areas (71.9% vs 32.7%). It hints at the fact that rural-born women who migrate to urban areas are on average more educated than the ones who stay in rural areas. As a result, if anything, it would bias our estimates downward.

5.3 Correcting for multiple hypothesis testing

Because we are investigating the effect of school constructions on several outcomes, we test whether our results are robust to correcting for multiple hypothesis testing following Simes (1986). Table 13 provides evidence that they remain statistically significant at

[22] We are nevertheless aware that by doing so, we would have introduced another measurement error as women did not necessarily attend primary school in the municipality where they were born. Furthermore, both data are not comparable. For instance, the share of women attending primary school is 10 percentage points lower in the census compared to DHS.

Table 13: P-values of IPV estimates adjusted for Multiple Hypothesis Testing

Condoning IPV for	p-value	Adjusted p-value	Rejection of the null
refusing sex	0.0105361	0.03412326	1
goes out w/o telling husband	0.013974	0.03412326	1
neglects the children	0.020474	0.03412326	1
burning the food	0.0764574	0.09557177	1
argues with husband	0.3451392	0.34513918	0

Note: P-values adjusted following Simes (1986) using the STATA package *qqvalue* with the option *method(simes)*.

Source: DHS Benin 2011 and 2017.

the 5% level for tolerating IPV for refusing sex, going out without telling the husband and neglecting the children, and 10% level for tolerating IPV for burning the food.

6 Channels

We can expect that the key pathway explaining the results in terms of acceptance of intimate partner violence in case of sex refusal is going through the relationship between husband and wife, rather than through parental choice. In the robustness section of the paper, we checked that husbands or potential husbands had not been affected by the reform. Therefore, we can interpret our results as the consequence of an increase in women's education instead of a consequence of improved men's education. Men's education is here a pure channel, and not a confounding effect. Yet, it is not enough to conclude that they do not play a part in the chain of mechanism leading to our results on women's well-being. Indeed, even though men are not affected on average, more educated women potentially have access to more educated men on the marriage market, making a better match. This could explain what we see in terms of domestic violence. Ideally, we would have liked to have data on all potential (unrealized) matches. Nevertheless, our data allows us to look at some characteristics of the realized matches for married women at the time of the survey. However, it is worth noting that the reform being relatively recent, treated women are not all married at the time of the survey. $20,3\%$ of

the treated women have never been in union at the time of the survey, when it is the case for only 1.45% of the women in the control group. As mentioned earlier, already married women are likely to be selected in a particular way and so are their husbands. Our analysis on the characteristics of the husbands is therefore likely to be biased.

Another point to keep in mind is that we conduct our main analysis on both married women and unmarried women. However, the tolerance of IPV is unlikely to have the same meaning for women who never lived with a partner and for those who have an experience of marital life. With that in mind, we first check whether the results on IPV are driven by married or unmarried women, before looking at the characteristics of the husbands. We investigate the results according to the marital status in the table 14. Results on tolerance of domestic violence are driven by married women. It is important to keep in mind that, among the treated, married women are likely to exhibit some vulnerability compared to their unmarried counterparts. It could mean our results are biased downwards. Alternatively, we may believe that there is more room for improvement among those more vulnerable women: in this case, the direction of the bias is unclear. This feature urges us to be modest on the interpretation of the impact of the education policy on tolerance of IPV[23].

We first look at the age gap with the partner. The sign of the coefficient of interest is negative but not significant for all married women (table 15). It hints at the fact that, in our specific case, improvements in women's mindset regarding tolerance of physical abuse is not driven by a change in their partner's age profile. Second, we look at the difference in education with the husband (table 15). We find that the education policy did not decrease the education gap with the partner. This result comes partly from the fact that not every women of the treatment group are married at the time of the survey, which entails, as underlined earlier, issues related to selecting peculiar women among the married.

[23]It would be useful to look at the same impact with posterior data, in order to see whether the effect holds when every women get married, and in the long term

Table 14: Interaction with marital status

	Goes out without telling husband	Neglects the children	Argues with husband	Refuses sex	Burns the food
Panel A: Interest Experiment: Individuals aged 4 to 8 or 12 to 17 in 1997					
Number of school built between 1997 and 2003 * Treat	0.018	0.009	0.043*	-0.004	-0.007
	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)
Number of school built between 1997 and 2003* Ever married*Treat	-0.048**	-0.045**	-0.054**	-0.018	-0.007
	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)
Controls Individual	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var.	0.17	0.19	0.18	0.11	0.12
N	6241	6241	6241	6241	6241
r2	0.16	0.13	0.12	0.08	0.08
F	4.04	5.81	3.27	2.93	3.06

Note: The dependent variable is in the following order a dummy taking the value 1 if the woman finds wife beating acceptable if a woman goes out without telling her partner, argues with him, neglects the children, refuses sex and burns the food. All specifications include municipality dummies, year of birth dummies and interactions between the year of birth dummy and the number of children in the district of birth in 1993. We control also for ethnicity, religion, age and age squared. Sample: Married rural women aged between 15 and 49.

Source: DHS Benin 2011 and 2017.

Table 15: Age and education gap with the husband

	Age difference with husband	Education difference with husband
Panel A: Interest Experiment: Individuals aged 4 to 8 or 12 to 17 in 1997		
Number of school built between 1997 and 2003 * Treat	-0.270	-0.001
	(0.25)	(0.02)
Controls Individual	Yes	Yes
Mean Dep. Var.	7.97	0.29
N	5333	5333
r2	0.05	0.11
F	1.91	2.26
Panel B: Placebo Experiment: Individuals aged 13 to 17 or 18 to 24 in 1997		
Number of school built between 1997 and 2003 * Placebo	0.283	0.030*
	(0.26)	(0.02)
Controls Individual	Yes	Yes
Mean Dep. Var.	8.22	0.29
N	5625	5625
r2	0.06	0.12
F	2.25	2.67

Note: The dependent variable is the the difference between the age of the husband and the age of the bride, for the first column, and the difference in attendance to primary school for the second column. All specifications include municipality dummies, year of birth dummies and interactions between the year of birth dummy and the number of children in the district of birth in 1993. We control also for ethnicity, religion, age and age squared. Sample: Married rural women aged between 15 and 49.

Source: DHS Benin 2011 and 2017.

Table 16: Tolerance to IPV of husbands

	Goes out without telling husband	Neglects the children	Argues with husband	Refuses sex	Burns the food
Panel A: Interest Experiment: Individuals aged 4 to 8 or 12 to 17 in 1997					
Number of school built between 1997 and 2003 * Treat	-0.007	-0.012	-0.039*	-0.029*	-0.024*
	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)
Controls Individual	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var.	0.08	0.08	0.10	0.07	0.06
N	1727	1736	1732	1733	1733
r2	0.15	0.14	0.20	0.12	0.17
F	1.09	1.22	1.79	0.86	1.38
Panel B: Placebo Experiment: Individuals aged 13 to 17 or 18 to 24 in 1997					
Number of school built between 1997 and 2003 * Placebo	0.017	0.019	0.031*	0.020*	0.013
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Controls Individual	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var.	0.08	0.07	0.10	0.06	0.05
N	1729	1739	1741	1738	1740
r2	0.19	0.14	0.20	0.15	0.17
F	1.06	1.16	1.62	1.16	0.94

Note: The dependent variable is in the following order a dummy taking the value 1 if the man finds wife beating acceptable if a woman goes out without telling her partner, argues with him, neglects the children, refuses sex and burns the food. All specifications include municipality dummies, year of birth dummies and interactions between the year of birth dummies and the number of children in the municipality of residence in 1993 for the wife. We control also for ethnicity, religion, age, age squared and for the interaction between year of birth dummies and the attendance rate in 1993 in the municipality of residence of the wife. Sample: men married with a sub-sample of women aged between 15 and 49.

Source: DHS Benin 2011 and 2017.

For a subsample of married women, we are able to analyse the tolerance of IPV of their husbands. We find that the policy decreases their tendency to condone domestic violence, as shown in Table 16. This is interesting since these men were not directly treated by the policy (because they were too old). It either means that the policy has created a more general change in gender norms, or that they could have been influenced by their wives. [24]

It stems from this analysis that the impact of the program on the tolerance of IPV seems not driven by a change in the relative characteristics of women comparatively to their partner. An improvement in women's own opinion (and the one of their husband) of the way they should be treated and increased bargaining power due to schooling are likely to be the main driver of our results.

[24] For men treated by the policy (who are in general not married with the first cohort of women treated), there is also a reduction in the tolerance to domestic violence. Table available on demand.

6.1 Discussing tolerance of IPV as a proxy for women's empowerment and well-being

We argue, with Hanmer and Klugman (2016), that using the tolerance of IPV is a meaningful proxy to capture women's own sense of empowerment and well-being. Despite the success of the notion of women's empowerment, there are no clear and consensual definition of the concept. This is why, finding inspiration in the literature reflecting on the concept of women's empowerment in development studies (Kabeer (2005), see Mosedale (2005) for a review of it) we combined several approaches to propose our own definition of it. We define empowerment as the process by which women become aware of and challenge the gender norms that curtail the realm of possibilities available to them, compared to men, as well as their ability to choose and act, individually and collectively, to pursue their own strategic interests. This definition, though close to the one of Kabeer (2005) reintroduces an explicit mention to the psychological process of awareness necessary to challenge power relations betweens sexes and in that is closer to Stromquist (1999).

Starting from this definition, studying tolerance of IPV is a matter of studying women's individual support to a norm that allows a man, here the partner or husband, to exercise his physical power (physical violence) to police a woman's behavior. The DHS questions actually relate to two things: first, it supposes that the behavior described in the scenario to justify wife beating transgresses gender norms. In the present case, the literature on Western African societies has largely documented that the behaviors mentioned refer to what is indeed expected from women (though it is less clear for the "burning the food" item). Second, if the behavior mentioned is considered transgressive, finding acceptable for a husband to beat his wife in those circumstances actually means that a woman recognizes that it falls to husbands to police women's behavior, which suggests that she acknowledges that partners have an authority to sanction wives' be-

havior, that they have power over them and that this power legitimately impedes their right to physical integrity. It could be argued that some women may answer "yes" to this question because they know that should violence in the household happen, they wouldn't be the one experiencing it because they are the oldest wive for instance or because they live with their son and his wife and answers the question with the situation of her daughter-in-law in mind. In both these examples, the respondents would derive their protection from their seniority, not from being a woman. The definition of empowerment we use, as the one mentioned in Mosedale (2005), is not incompatible with women deriving power from their age, ethnicity, wealth or position in the family. But this power would not be derived from being a woman, which still says something about their own sense of women's empowerment.

Additionally, the literature has shown that, in some context, women's individual and collective tolerance of IPV was positively associated with the risk of experiencing IPV (Boyle et al. (2009) in India). In Jewkes (2002), the author relies on a cross-cultural analysis to demonstrate that the occurence of IPV is stronger in contexts where physical violence against women is condoned for certain motives.

Eventually, we may wonder what a change in tolerance of IPV over time means. As mentioned before, the DHS questions to assess tolerance of IPV rely on the fact that (i) the behavior in the scenario transgresses gender norms, (ii) that a woman believes it falls on the husband to chastise her using violence. So the effect of primary school we capture may mean that (1) either education modified gender norms and relaxed the expectation around women's behavior in the household; or (2) that increased education changed women's perception of husbands' alleged right to use physical violence to police their behavior or that both phenomena happened simultaneously. Either way, both these scenarii are testimony of improvement of different dimensions of women's empowerment.

The former would mean that the education policy relaxed the constraint on women's expected behavior in their household. As a result, it would mean that education can foster a process of awareness of a gender norm that curtails women's ability to choose, for instance, when to have intercourse with their partner. If access to primary school only impacted women's opinion of the use of violence to police wives' behavior, it would also be a matter of improving women's empowerment through challenging another norm ie husbands' right to use violence to chastise wives. Either way, both channels are synonymous of improving women's empowerment and well-being and are likely to be at play.

7 Conclusion

Using a double difference strategy along a method inspired by a regression kink design (RKD), we show that a rise in school constructions in the nineties in Benin, designed to reach the MDG's, increases primary school attendance of women living in rural areas. We find evidence that the policy intervention decreased the justification of wife-beating for diverse scenarii. We also find that the education program delays entry into marital life and motherhood. Investigating the pathways of our effect, we tried to identify channels through which the policy impacts women's wellbeing. As for condoning physical abuse in case of sex refusal, our results hint at the fact that the effect is not driven by an evolving profile of women's partner in terms of age difference or education, but rather by a change in women's own outcomes. This education policy, which targets essentially the supply side of education, appears to have been successful beyond its initial agenda. This work provides evidence that the benefits of girls' education percolate down to women's well-being.

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Appendix

Table A1: School Construction by District since the 1980's

Region level					
	Stock 1979	*1980-1989*	*1990-1999*	*2000-2005*	*Stock 2005*
Mean	175.17	53.42	89.42	113.25	431.17
min	99	23	56	27	299
max	284	84	180	170	562
median	162	54	84.5	125	454.5
N	12	12	12	12	12

Source: PASEC data on school constructions in Benin.

Table A2: Descriptive Statistics for Benin, Senegal, Guinea and Sierra Leone

	Benin			Senegal		
	Mean	SD	Observations	Mean	SD	Observations
	Marital and Domestical Violence outcome					
age at first cohabitation	18.88	4.77	12768	18.08	4.36	30306
Married before 15 years old	0.13	0.33	16599	0.13	0.33	41663
age of respondent at 1st birth	19.81	4.47	12522	19.48	4.05	27941
beating justified if wife goes out without telling husband	0.08	0.27	16094	0.49	0.50	41488
beating justified if wife neglects the children	0.09	0.29	16315	0.49	0.50	41500
beating justified if wife argues with husband	0.11	0.31	16346	0.52	0.50	41482
beating justified if wife refuses to have sex with husband	0.07	0.25	16286	0.51	0.50	41142
beating justified if wife burns the food	0.06	0.24	16360	0.26	0.44	41519
	Education and Birth cohort					
Enrolled to Primary School	0.37	0.48	16599	0.41	0.49	41663
respondent's year of birth	1982.20	9.01	16599	1984.37	9.40	41663
	Covariates					
urban	0.43	0.49	16599	0.39	0.49	41663
Main Ethnic Group	0.44	0.50	16599	0.34	0.47	41663
	Guinee			Sierra Leone		
	Mean	SD	Observations	Mean	SD	Observations
	Marital and Domestical Violence outcome					
age at first cohabitation	16.66	3.62	7144	17.80	4.28	11747
Married before 15 years old	0.23	0.42	9142	0.13	0.33	16658
age of respondent at 1st birth	18.20	3.73	6950	18.72	3.95	12352
beating justified if wife goes out without telling husband	0.83	0.38	9101	0.54	0.50	16002
beating justified if wife neglects the children	0.82	0.39	9119	0.54	0.50	16017
beating justified if wife argues with husband	0.78	0.41	9105	0.49	0.50	16009
beating justified if wife refuses to have sex with husband	0.71	0.45	9058	0.27	0.44	15708
beating justified if wife burns the food	0.47	0.50	9097	0.19	0.39	15894
	Education and Birth cohort					
Enrolled to Primary School	0.33	0.47	9141	0.45	0.50	16658
respondent's year of birth	1983.28	9.59	9142	1984.55	9.65	16658
	Covariates					
urban	0.39	0.49	9142	0.41	0.49	16658
Main Ethnic Group	0.39	0.49	9142	0.34	0.47	16658

Note: The table reports mean, standard deviation and number of observations for a certain number of characteristics. "Urban" means living in an urban milieu at the time of the study. Sample: Women aged 15-49 years old.

Table A3: Correlation matrix of tolerance of IPV items

	Goes out w/o telling	Neglects the children	Argues	Refuses sex	Burns the food
Goes out w/o telling	1				
Neglects the children	0.7031*	1			
Argues	0.6561*	0.6568*	1		
Refuses sex	0.4966*	0.4890*	0.5348*	1	
Burns the food	0.5250*	0.5555*	0.5447*	0.4913*	1
N	15906	15906	15906	15906	15906

Source: DHS 2011 and 2017 Benin.

Table A4: Probability of primary school attendance : variation of the treated cohorts

	1980-1985 and 1989-1993	1975-1985 and 1989-1993	1980-1988 and 1989-1993	1980-1985 and 1989-1996	1975-1988 and 1989-1996
Number of school built between 1997 and 2003 * Treat	0.041***	0.038***	0.031**	0.042***	0.042***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Controls Individual	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var.	0.27	0.23	0.27	0.36	0.29
N	6241	8489	7571	7600	12130
r2	0.21	0.21	0.19	0.26	0.24
F	13.93	14.03	12.82	25.39	24.08

Note: The dependent variable is having attended primary school. All specifications include municipality dummies, year of birth dummies and interactions between the year of birth dummies and the number of children in the municipality of residence in 1993. They include also controls for ethnicity, religion, age, age squared and for the interaction between year of birth dummies and the attendance rate in 1993 in the municipality of residence. Sample: Rural women aged 15-49 years old.

Source: DHS Benin 2011 and 2017.

Table A5: Probability of secondary school attendance

	Secondary school attendance
Panel A: Interest Experiment: Individuals aged 4 to 8 or 12 to 17 in 1997	
Number of school built between 1997 and 2003 * Treat	0.008
	(0.01)
Controls Individual	Yes
Mean Dep. Var.	0.12
N	6241
r2	0.16
F	10.21
Panel B: Placebo Experiment: Individuals aged 13 to 17 or 18 to 24 in 1997	
Number of school built between 1997 and 2003 * Placebo	0.001
	(0.01)
Controls Individual	Yes
Mean Dep. Var.	0.04
N	6027
r2	0.08
F	2.35

Note: The dependent variable is having attended secondary school. All specifications include municipality dummies, year of birth dummies and interactions between the year of birth dummies and the number of children in the municipality of residence in 1993. They include also controls for ethnicity, religion, age, age squared and for the interaction between year of birth dummies and the attendance rate in 1993 in the municipality of residence. Sample: Rural women aged 15-49 years old.

Source: DHS Benin 2011 and 2017.

Table A6: Marital Outcomes - OLS

	Age at marriage	Marriage before 15	Age at first child	Time between marriage and first birth	Time between first and second child
Panel A: Interest Experiment: Individuals aged 4 to 8 or 12 to 17 in 1997					
Number of school built between 1997 and 2003 * Treat	0.129	0.006	0.170	0.362	-1.238**
	(0.16)	(0.01)	(0.12)	(0.83)	(0.62)
Controls Individual	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var.	18.02	0.15	19.01	21.58	34.25
N	5574	6241	5436	4589	4564
r2	0.09	0.05	0.11	0.06	0.04
F	6.33	3.29	9.63	3.98	1.39
Panel B: Placebo Experiment: Individuals aged 13 to 17 or 18 to 24 in 1997					
Number of school built between 1997 and 2003 * Placebo	-0.061	-0.006	-0.102	0.344	-0.122
	(0.15)	(0.01)	(0.12)	(0.85)	(0.65)
Controls Individual	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var.	18.67	0.17	19.64	24.19	36.11
N	5949	6027	5899	4935	5615
r2	0.06	0.04	0.07	0.05	0.04
F	2.73	1.67	3.09	2.13	1.48

Note: The dependent variable is in the following order: age at marriage, marriage before 15 years old, age at first child, time between the marriage and the first birth, and lastly, time between the first and second birth. The number of observations changes slightly between the different outcomes, since not all women have faced such events at the time of survey. All specifications include municipality dummies, year of birth dummies and interactions between the year of birth dummies and the number of children in the municipality of residence in 1993. We control also for the ethnicity, religion, age, age squared and for the interaction between year of birth dummies and the attendance rate in 1993 in the municipality of residence. Sample: Rural women aged 15-49.

Source: DHS Benin 2011 and 2017.

Table A7: Marital Outcomes - Cox Duration Model

	First union	First child	First child Married women	Second child Married with a child
Panel A: Interest Experiment: Individuals aged 4 to 8 or 12 to 17 in 1997				
Number of school built between 1997 and 2003 * Treat	-0.095***	-0.099***	-0.050	0.055
	(0.03)	(0.03)	(0.03)	(0.05)
Controls Individual	Yes	Yes	Yes	Yes
Mean Dep. Var.	18.35	19.32	19.20	34.54
N	6241	6241	5574	5311
r2_p	0.01	0.00	0.01	0.00
chi2	521.42	482.90	594.57	311.92
Panel B: Placebo Experiment: Individuals aged 13 to 17 18 to 24 in 1997				
Number of school built between 1997 and 2003 * Placebo	0.016	0.028	0.024	0.006
	(0.03)	(0.03)	(0.03)	(0.03)
Controls Individual	Yes	Yes	Yes	Yes
Mean Dep. Var.	18.86	19.90	19.84	37.99
N	6027	6027	5949	5877
r2_p	0.00	0.00	0.00	0.00
chi2	383.02	413.92	419.58	307.01

Note: The dependent variable is in the following order: time before marriage, time before first child and time between first and second child. The table presents the coefficient beta, and not the odd-ratio. The number of observations changes between the different outcomes, since not all women have faced such events at the time of survey. All specifications include municipality dummies, year of birth dummies and interactions between the year of birth dummies and the number of children in the municipality of residence in 1993. We control also for the ethnicity, religion, age, age squared and for the interaction between year of birth dummies and the attendance rate in 1993 in the municipality of residence. Sample: Rural women aged 15-49. In the third column, the sample corresponds to women already married at the time of the survey. In the fourth column, the sample is all women who have already given birth.

Source: DHS Benin 2011 and 2017.

Table A8: Probability of primary school attendance - Girls married before 15 years old

	Primary school attendance
Panel A: Interest Experiment: Individuals aged 4 to 8 or 12 to 17 in 1997	
Number of school built between 1997 and 2003 * Treat	0.012
	(0.03)
Controls Individual	Yes
Mean Dep. Var.	0.13
N	940
r2	0.24
F	1.93
Panel B: Placebo Experiment: Individuals aged 13 to 17 or 18 to 24 in 1997	
Number of school built between 1997 and 2003 * Placebo	0.027
	(0.02)
Controls Individual	Yes
Mean Dep. Var.	0.10
N	1009
r2	0.20
F	1.57

Note: The dependent variable is having attended primary school. All specifications include municipality dummies, year of birth dummies and interactions between the year of birth dummies and the number of children in the municipality of residence in 1993. In model (2), we control for urban or rural residence, ethnicity, religion, age, age squared and for the interaction between year of birth dummies and the attendance rate in 1993 in the municipality of residence. Sample: Eligible women aged 15-49 years old, who have been married before 15 years old.

Source: DHS Benin 2011 and 2017.

Table A9: Outcome Mariage - OLS - Regression Kink Design

	Age at marriage	Marriage before 15	Age at first child	Time between marriage and first birth	Time between first and second child
Normalized birth year* post kink* Number of schools built	0.011	0.001	0.014	0.107	-0.202**
	(0.02)	(0.00)	(0.01)	(0.10)	(0.08)
Controls Individual	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var.	18.18	0.16	19.17	22.54	35.09
N	10058	11178	9856	8293	8527
r2	0.07	0.04	0.08	0.05	0.03
F	15.83	6.93	23.71	8.75	3.20

Note: The dependent variable is in the following order: age at marriage, marriage before 15 years old, age at first child, time between the marriage and the first birth, and lastly, time between the first and second birth. We control also by ethnicity, religion, age and age squared. Sample: Rural women aged 15-49 years old.

Source: DHS Benin 2011 and 2017.

Table A10: Marital Outcomes - Duration Cox Model - Regression Kink Design

	First union	First child	First child Married women	Second child Married with a child
Normalized birth year* post kink* Number of schools built	-0.019***	-0.014***	0.004	0.009*
	(0.00)	(0.00)	(0.00)	(0.01)
Controls Individual	Yes	Yes	Yes	Yes
Mean Dep. Var.	18.44	19.43	19.38	35.72
N	11178	11178	10058	9674
r2_p	0.00	0.00	0.00	0.00
chi2	699.66	569.30	592.97	367.90

Note: The dependent variable is in the following order: the time before marriage, the time before first birth and the time between first and second child. The table presents the coefficient beta, and not the odd-ratio. The number of observations changes between the different outcomes, since not all women have faced such events at the time of survey. All specifications include municipality dummies, year of birth dummies and interactions between the year of birth dummies and the number of children in the municipality of residence in 1993. We control also for ethnicity, religion, age, age squared and for the interaction between year of birth dummies and the attendance rate in 1993 in the municipality of residence. Sample: Rural women aged 15-49. In the third column, the sample corresponds to women already married at the time of the survey. In the fourth column, the sample is all women who have already given birth.

Source: DHS Benin 2011 and 2017.

Table A11: Tolerance to IPV - Women whose husband is not affected by the policy

	Goes out without telling husband	Neglects the children	Argues with husband	Refuses sex	Burns the food
Panel A: Interest Experiment: Individuals aged 4 to 8 or 12 to 17 in 1997					
Number of school built between 1997 and 2003 * Treat	-0.025**	-0.031**	-0.011	-0.022***	-0.014*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Controls Individual	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var.	0.17	0.19	0.18	0.11	0.12
N	6095	6095	6095	6095	6095
r2	0.16	0.13	0.12	0.08	0.08
F	3.90	5.61	3.08	2.67	3.11
Panel B: Placebo Experiment: Individuals aged 13 to 17 or 18 to 24 in 1997					
Number of school built between 1997 and 2003 * Placebo	0.012	0.023**	0.014	0.016*	0.001
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Controls Individual	Yes	Yes	Yes	Yes	Yes
Mean Dep. Var.	0.16	0.18	0.18	0.11	0.11
N	5970	5970	5970	5970	5970
r2	0.14	0.12	0.10	0.07	0.09
F	4.18	4.75	3.18	2.75	2.89

Note: The dependent variable is in the following order a dummy taking the value 1 if the woman finds wife beating acceptable if a woman goes out without telling her partner, argues with him, neglects the children, refuses sex and burns the food. All specifications include municipality dummies, year of birth dummies and interactions between the year of birth dummies and the number of children in the municipality of residence in 1993. We control also for ethnicity, religion, age, age squared and for the interaction between year of birth dummies and the attendance rate in 1993 in the municipality of residence. Sample: Rural women aged 15-49 years old whose husband is not in the cohort affected by the policy.

Source: DHS Benin 2011 and 2017.

Table A12: Smoothness of Covariates

	Fon	Traditional	Muslim	Christian
Normalized birth year* post kink* Number of schools built	0.001	0.000	0.001	0.000
	(0.00)	(0.00)	(0.00)	(0.00)
Mean Dep. Var.	0.38	0.15	0.27	0.51
N	11178	11178	11178	11178
r2	0.71	0.22	0.57	0.31
F	2.22	4.56	0.46	5.17

Note: The dependent variable is the number of observations by cohorts. Models (1) represents the simple regression kink design. Models (2) includes also a dummy indicating whether the cohort is younger than the kink. The bandwidth is 10 years (on both sides of the kink). Sample: Eligible women aged 15-49 years old.

Source: DHS Benin 2011 and 2017.

Table A13: McCrary Test for RKD Design - Benin

	Density	
	(1)	(2)
Normalized birth year* post kink* Number of schools built	-0.00	-0.00
	(0.00)	(0.00)
Number of cohorts	7384.00	7384.00
r2	0.03	0.05
F	8.99	41.94

Note: The dependent variable is the number of observations by cohorts. Models (1) represents the simple regression kink design. Models (2) includes also a dummy indicating whether the cohort is younger than the kink. The bandwidth is 10 years (on both sides of the kink). Sample: Eligible women aged 15-49 years old.

Source: DHS Benin 2011 and 2017.