The Impact of Legislation on the Hazard of Female Genital Mutilation/Cutting: Regression Discontinuity Evidence from Burkina Faso

Ben Crisman, Sarah Dykstra, Charles Kenny, and Megan O'Donnell

Abstract

In 1996 Burkina Faso enacted legislation banning the practice of female genital mutilation/cutting (FGM/C). Much of the qualitative literature surrounding FGM/C discounts the impact of legal change on what is considered a social/cultural issue. We use data from the Demographic and Health Surveys DHS(VI) in Burkina Faso to test for a discontinuous change in the likelihood of being cut in the year the law was passed. We find robust evidence for a substantial drop in hazard rates in 1996 and investigate the heterogeneous impact of the law by region, religion, and ethnicity. Overall, we roughly estimate that over a ten year period the law averted the genital mutilation/cutting of approximately 237,591 women and girls. We qualify our findings recognizing that Burkina Faso is a special case with a long history of bottom-up and top-down approaches to eliminating the practice.

JEL Codes: A13, I18, J16, J18, K14

Keywords: FGM/C, social norms, legal change, regression discontinuity

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Ben Crisman

Sarah Dykstra

Charles Kenny

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Center for Global Development
2055 L Street NW
Washington, DC 20036

202.416.4000
(f) 202.416.4050

www.cgdev.org

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Contents

1 Introduction

Female genital mutilation/cutting (FGM/C), also referred to as female circumcision,[1] refers to the non-therapeutic, surgical alteration of female genitalia. The reasons behind the perpetuation of the practice are diverse and multi-dimensional. Previous studies on the topic have examined the links of FGM/C to community acceptance and identity, religious and cultural requirements, and socio-economic factors (Coyne and Coyne (2014), Kang'ethe (2013), and Chesnokova and Vaithianathan (2010)).

In spite of its entrenched nature, there is an increasing global consensus that FGM/C is a violation of women's and girls' rights. In addition to the fact that FGM/C procedures are primarily performed on children and adolescents, those who undergo FGM/C face substantial physical and psychological health risks, ranging in severity from urinary tract infections to Post-Traumatic Stress Disorder (PTSD), complications in childbirth, and death (World Health Organization (2000), Reisel and Creighton (2015), and Wagner (2015)). In light of these health and human rights concerns, significant work has been undertaken to eliminate the practice of FGM/C. In 2012, the United Nations General Assembly banned the practice worldwide, following over two decades of local, national, and international campaigns dedicated to its eradication. Many of these national campaigns have led to the passage of anti-FGM/C laws in numerous countries including Ghana in 1994, Côte d'Ivoire in 1998, and our case study, Burkina Faso in 1996 (U.S. Department of State, 2009b).

Burkina Faso saw considerable mobilization before and after the passage of a legal ban to pressure for reductions in FGM/C rates. The goal of this paper is to causally identify, in this context, what impact, if any, the legal change itself had on women and girls' likelihood of being cut. This paper starts from the null hypothesis that laws have no impact and finds that the available data reject this hypothesis with regard to FGM/C in the case studied. We suggest a drop of approximately -0.30 in the log-odds of being cut or a greater than 25% reduction in odds attributable to the passage of the law. Given the legacy of anti-FGM/C activity in the country we stress that our findings should *not* be taken to mean that law

[1] There is significant controversy surrounding these terms. Female circumcision is considered by some to reflect a benignity and therefore misrepresent the brutality of the practice, in its equation of the practice with male circumcision. Female genital mutilation (FGM) was used by early activists in an attempt to reflect the practices severity and harmfulness, but its proponents have been accused of acting as cultural imperialists and failing to understand the emic perspective of those parents who consider FGM/C to be necessary and beneficial for their daughters from cultural and socio-economic perspectives. This paper, in seeking to reflect the severity of cutting procedures while also noting cultural constraints, will employ the hybrid "female genital mutilation/cutting" (FGM/C), also used by UNFPA and UNICEF.

is either necessary or sufficient. We do suggest however that law can be a useful tool in changing attitudes and outcomes and bolstering additional efforts. We discuss potential causal mechanisms and implications for policy in more detail below.

Section 2 provides additional background to the practice of female genital mutilation/cutting and the nature of its prevalence in West Africa and specifically Burkina Faso. We also discuss in brief the formulation of legislative and reform efforts in context. In section 3, we discuss the extant literature on FGM/C, including root causes, current modes of thinking, and existing policy efforts. We also consider similar evaluations of legal changes and their impact on gendered outcomes. Section 4 reviews our data and discusses some descriptive statistics. Section 5 outlays our econometric design, section 6 discusses our findings, and section 7 concludes and presents policy implications and suggestions for future research.

2 FGM/C and Burkina Faso

The term FGM/C encompasses a number of different practices. While the DHS survey does not differentiate between types of FGM/C, it is important to acknowledge the diversity in practice and the underlying heterogeneity that our data capture. According to the World Health Organization (WHO), FGM/C comprises all procedures that partially or completely remove or modify the external female genitalia for non-medical reasons. Four types are recognized; we quote:

> **Clitoridectomy:** "the partial or total removal of the clitoris (a small, sensitive and erectile part of the female genitals), and in very rare cases, only the prepuce (the fold of skin surrounding the clitoris)."
>
> **Excision:** "the partial or total removal of the clitoris and the labia minora (the inner folds of the vulva), with or without excision of the labia majora (the outer folds of skin of the vulva)."
>
> **Infibulation:** "the narrowing of the vaginal opening through the creation of a covering seal…formed by cutting and repositioning the labia minora, or labia majora, sometimes through stitching, with or without removal of the clitoris."
>
> **Other:** "all other harmful procedures to the female genitalia for non-medical purposes, e.g. pricking, piercing, incising, scraping and cauterizing the genital area" (World Health Organization, 2016).

The United Nations Childrens Fund (UNICEF) reports that there are approximately 130 million women and girls living with FGM/C worldwide. The practice primarily occurs within a belt of countries running from West Africa to Yemen and Iraq in the Middle East and to Kenya and Tanzania in East Africa. FGM/C also occurs in parts of South Asia, as well as in Europe, the United States, and Australia, largely as a result of the migration of African and Middle Eastern communities to these areas (Reisel and Creighton, 2015). Across and within countries in West Africa, rates of FGM/C vary considerably. For instance, Multiple Indicator Cluster Surveys (MICS) suggest that Mali, Sierra Leone, the Gambia, Burkina Faso and Mauritania have prevalence rates above 70%, whereas Ghana, Niger and Togo see prevalence at less than 6% (Sipsma et al., 2012).

For our case study, the DHS estimates prevalence of FGM/C in women and girls 15-49 years old in Burkina Faso was 75.8% in 2010, a slight decline from 76.6% in 2003. FGM/C takes place very young; 47.3% of women reported undergoing the procedure during infancy, and 60.4% were cut by the age of 5. There is, however, evidence of a dramatic recent decline in the FGM/C rate in Burkina Faso. Only 14.2% of girls age 0-5 have been cut in 2010 (as reported by mothers), whereas 63.9% of 15-19 year-olds subjected to FGM/C reported being cut by their fifth birthday.

2.1 Legal Reform and Civil Society Efforts

The African Union's Protocol to the African Charter on Human and Peoples Rights on the Rights of Women in Africa, widely known as the Maputo Protocol, came into force in 2005. Article 5 explicitly condemns FGM/C and encourages signatories to prohibit it through legislation with sanctions. Most West African countries have signed and ratified the protocol (Exceptions include Chad, CAR, Sierra Leone, and Niger).The African Union also passed a resolution on July 1, 2011 in support of the 2012 UN General Assembly resolution to ban FGM/C (African Union, 2011). As mentioned above, prior to the resolutions passage, several countries in the region had already outlawed the practice, including Ghana (1994) and Côte d'Ivoire (1998) (NPWJ, 2015). Article 380 of Burkina Fasos Penal Code states:

> [A]ny person who violates or attempts to violate the physical integrity of the female genital organ either in total or ablation, excision, infibulation, desensitization or by any other means will be imprisoned for six months to three years and a fine of 150,000-900,000 francs[2] or by either punishment. If FGM results in death, the punishment shall be imprisonment for 5-10 years (NPWJ, 2015).

[2]Roughly \$265-1590 or 16-95% of PPP per capita

The law also imposes punishments on persons in the medical and paramedical fields taking part in FGM/C or those who have knowledge of a procedure but do not report it (Ras-Work, 2009). There were 94 cases prosecuted under the law, against both parents and excisors, between 1996 and 2005 and 686 cases prosecuted from 2005 to 2009. According to court statistics, the average prison sentence was two to three months (Inter-Parliamentary Union, 2015).

Apart from efforts undertaken by the legislature and law enforcement sector, beginning in 1975, Burkina Faso launched additional awareness-raising and educational initiatives to fight against the practice of FGM/C. In 1990, the government created the National Committee for the Campaign against Excision (CNLPE), which established a national FGM/C hotline and supported a nation-wide awareness-raising campaign surrounding the practice(U.S. Department of State, 2009a).

3 Literature Review

3.1 Root Causes of FGM/C

Previous studies have examined the socio-cultural and economic causes of FGM/C. There is widespread agreement that "a key function of FGM is establishing and strengthening individual and group identity" (Coyne and Coyne, 2014). Shell-Duncan and Hernlund (2000), for example, through a mixed methods study in Senegambia, determined that "being circumcised serves as a signal to other circumcised women that a girl or woman has been trained to respect the authority of her circumcised elders and is worthy of inclusion in their social network. In this manner, FGM/C facilitates the accumulation of social capital by younger women and of power and prestige by elder women" (Shell-Duncan and Hernlund, 2000).

In addition, women and girls living in areas where FGM/C is prevalent are often restricted in their means to provide for themselves financially, and these individuals thus depend upon marriage for financial stability. Being cut remains tied to eligibility as a marriage partner, as it is considered a mechanism for ensuring that a woman or girl will remain faithful to her spouse, having been deprived of a source of sexual pleasure as a result of the procedure (which in many cases also renders sexual intercourse painful). In this vein, Chesnokova and Vaithianathan (2010) conclude that FGM/C leads to better marital outcomes; cut women live in wealthier households and are more likely to be the first wife in a polygamous household.

For these reasons, parents who choose to have their daughters cut consider their decision to be necessary, if not beneficial, in light of the structural constraints they face. As a result, review studies suggest that simply informing populations that FGM/C is a rights violation is unlikely to decrease prevalence rates (Denison et al., 2009).

In Burkina Faso, previous studies have demonstrated that religion and ethnicity, among other factors, are (univariately) correlated with FGM/C (Karmaker et al., 2011). In rural Tanzania, studies found that the strongest predictors of FGM/C were ethnicity and religion (Protestant or Muslim) with education and marital status influencing likelihood to a lesser extent (Klouman et al., 2005). Evidence from antenatal and family planning clinics in South-West Nigeria found that ethnicity was the most significant social predictor followed by religious affiliation (with Pentecostals seeing a very high rate), as well as age and education. There was also dramatic evidence of long-term decline: three quarters of 45-49 year-olds were cut compared to 15% of 14-19 year-olds (Snow et al., 2002). Because FGM/C is in some regions considered to be a religious or cultural mandate tied to community acceptance, as well as a means of securing a marriage partner and financial stability, it remains culturally ingrained.

3.2 Approaches to Reform

If FGM/C is a culturally entrenched practice, it may suggest a limited role for state institutions and laws in changing behaviors. Coyne and Coyne (2014) assert that "cases of successful changes to FGM practices have typically started at the local level," referring to a 2005 UNICEF report that notes "the most successful [initiatives of change] are participatory in nature and generally guide communities to define problems and solutions themselves" (UNICEF., 2005). Coyne and Coyne (2014) use the examples of The Gambia, Kenya, Tanzania, and Uganda, all of which have experienced decreases in FGM/C prevalence rates "through the local efforts of non-profits and community leaders who are knowledgeable regarding local practices, culture, and perceptions of identity. Local efforts are more likely to appreciate the nuances of identity and the perceived threats to identity from outsiders."

Conversely, criticism has surrounded the use of national-level legislation to combat FGM/C. Previous studies (Shell-Duncan and Hernlund (2000), Coyne and Coyne (2014)) point to the prohibitive enforcement costs of laws criminalizing the practice, because "[w]ithout changes in the underlying identity, members of communities where FGM exists will tend to ignore formal laws, and the practice will continue. In such instances, enforcing the law would involve arresting and imprisoning entire communities."[3] As a result, resources allocated to combatting FGM/C within the international development community have largely targeted community-oriented programs, such as TOSTAN in Senegal and the UK-based Orchid Project and its country partners, recognizing the essential nature of bottom-up change.[4]

However, though Chesnokova and Vaithianathan (2010) find that "ethnic and religious affiliations rather than socioeconomic background" are the greater determinants of FGM/C in Burkina Faso, they note that ethnicity and religious affiliation as determinants of FGM/C do not preclude the decline of the practice. Using a theoretical model they suggest that if returns to those who undergo FGM/C in the marriage market are higher where the practice is more common, even weakly implemented regulation that pushes FGM/C below a tipping point can be effective in reducing rates to zero over the long term.

On the subject of the potential impact of law on the prevalence of FGM/C, Shell-Duncan and Hernlund (2000), examining the impact of Senegal's 1999 law, conclude that legal reform has mixed effects, depending on the degree of attitudinal shifts already occurring within a particular context. They conclude that though "... imposing legal regulations in communities where there is unanimous support for the practice of FGM/C will have little effect," "where there are active debates and divergent opinions about the continuation of FGM/C, legislation can give added strength to those in favor of abandonment."

3.3 Impact of Legal and Institutional Reform

The evidence put forth above suggests that there exist opportunities to continue the decrease in FGM/C prevalence rates and to change social attitudes pertaining to the practice. Can legal and institutional mechanisms contribute to these changes? Beyond studies focused on combating FGM/C, there is a rich literature on the use of legal and institutional reform to alter social attitudes, behavior, and practice. The existing literature suggests that the impact of laws and institutions may be limited in this regard, but has also found evidence of the importance of institutions in the long run exploiting the exogeneity of colonial borders.

Because many African borders were drawn with little regard to ethnic and religious boundaries, communities with similar ethnic, religious, linguistic, and cultural backgrounds

[3] Coyne and Coyne (2014)

[4] In addition to the participation of community leaders and grassroots organizations, UNICEF. (2005) points to the efficacy of approaches that maintain "the socio-cultural aspects of the meta-ritual while making changes to the FGM aspect." Such approaches, which include symbolically pouring milk over the female genitals through a ritual ceremony in place of a cutting procedure, allow for participants to benefit from the identity and community aspects of FGM/C rituals while preventing the adverse consequences of physical and psychological harm.

were artificially divided and became subject to differing policies and institutions, first of colonial powers and then independent African governments. Searching for a discontinuity of outcomes at the border provides one measure of the impact of policies and institutions. Michalopoulos and Papaioannou (2013) use both a matching-type and a regression discontinuity approach to show that differences in countrywide institutional structures across national borders do not explain within-ethnicity differences in economic performance, as captured by satellite light density at night. On the other hand, Lee and Schultz (2012) use a regression discontinuity design to examine development outcomes on either side of the former colonial border between British and French Cameroon. Using the 2004 DHS they find that communities on the British side of the now-defunct border have higher wealth and better access to improved water. Cogneau and Moradi (2014) examine literacy and religious beliefs that diverged between British and French-mandated parts of Togoland as early as the 1920s. Using contemporary survey data they find that border effects that originated during colonial occupation still persist today. Finally, Cogneau et al. (2015) look at the countries bordering Côte d'Ivoire and find significant disparities in welfare across borders, concluding that border discontinuities mirror the differences between country averages with respect to household income, connection to utilities, and education. These results suggest that national institutions and reforms may play some role in social and development outcomes, perhaps extending to rates of FGM/C and attitudes surrounding it.

What about the impact of particular legislative changes? Previous studies suggest that national laws may have limited or perverse effects on actual behaviors. Tougher child labor laws in India may have increased the level of child labor because it reduced earnings, pushing impoverished families to send their children to work for longer hours (Bharadwaj et al., 2013). Similarly Bertrand et al. (2007) find that driving license laws in India appear to play little role in keeping dangerous drivers off the street. Across the world, the link between the official and actual time to complete various business regulatory formalities is low (Hallward-Driemeier and Pritchett, 2011). Collin and Talbot (2015) find that the probability of age at marriage across many countries does not suggest minimum age at marriage laws are affecting actual marriage practices.

On the other hand, Keats (2014) analyzed Uganda's free primary education reform, which eliminated primary school fees beginning in 1997, and found that it caused a discontinuous jump in the trend of average education of almost one year of schooling for women. Regarding laws around gender equality, Klugman et al. (2014) find evidence that new laws are followed by slow changes in attitudes and practices over time, but the evidence base remains limited and perhaps less relevant to the impact of criminalizing a socially-determined behavior.

This paper introduces a new approach to the study of legal change and female genital mutilation/cutting and examines the heterogeneous impact of the legal change across a number of subgroups. By examining the degree of efficacy of legal reforms in combating the entrenched cultural practice of FGM/C, this paper will contribute to the two bodies of literature: that of FGM/C and mechanisms contributing to its eradication, as well as literature on the role of national institutions and legal reform in shaping behavior.

4 Data Overview

All of our data are drawn from the sixth round of the Demographic and Health Survey (DHS) program in Burkina Faso. This 2010 survey collected several measures related to female genital mutilation/cutting including the respondent's own FGM/C status, their daughters' FGM/C status, the respondent or daughter's age at circumcision[5] (in years), who conducted the cutting, whether or not they are aware of the practice, and their attitudes on whether a) FGM/C should be stopped and B) FGM/C is required by religion. Table 1 presents descriptive statistics for these responses broken down by reporting status.

Forty-three percent of our full sample has been cut. In comparing the percentage cut for self-reported (75%) and parental-reported observations (13%) we observe preliminary evidence for a downward trend, however we note that as individuals can be cut at later ages, a number of the daughter observations will likely be cut at a later date. It is important to note that nearly all (99.8%) observations were aware of the practice of female genital mutilation/cutting and that over 90% believe that it should be stopped. The data were gathered following the DHS survey's two-stage sampling design with stratification and are a representative sample of households in Burkina Faso.[6]

Given that our data are drawn primarily from survey sources we must consider some of the valid concerns regarding reporting and social desirability bias, particularly in the case of such a sensitive, and now illegal, practice. Recent work shows that there can be substantial disparities in reporting and observed levels of FGM/C. Elmusharaf et al. (2006) demonstrate that some women who reported being cut *before* the implementation of a ban reported never having been cut in a later survey. Of course, it is difficult to tell which direction this will bias

[5]Here and in subsequent tables directly reflecting DHS data we use the term 'circumcision' as it is employed in the DHS FGM/C module.

[6]For an overview, see http://dhsprogram.com/What-We-Do/Survey-Types/DHS-Methodology.cfm

Table 1: Summary Statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Panel A: All Observations					
Respondent's year of birth	1992.42	13.29	1960	2010	34917
Female	1	0	1	1	34917
Respondent circumcised	0.437	0.496	0	1	34917
Age at circumcision (in years)	3.507	3.968	0	25	15199
Ever heard of female circumcision	0.998	0.064	0	9	34917
Belief: FGM/C Required by Religion	0.224	0.729	0	9	34917
Belief: FGM/C Should be Stopped	0.91	0.286	0	1	34726
Panel B: Self-Reported Observations					
Respondent's year of birth	1981.04	9.44	1960	1995	17033
Female	1	0	1	1	17033
Respondent circumcised	0.757	0.429	0	1	17033
Age at circumcision (in years)	3.587	4.184	0	25	12842
Ever heard of female circumcision	0.998	0.048	0	1	17033
Belief: FGM/C Required by Religion	0.17	0.376	0	1	17033
Belief: FGM/C Should be Stopped	0.908	0.29	0	1	17033
Panel C: Daughter (Mother-Reported) Observations					
Respondent's year of birth	2003.26	4.41	1995	2010	17884
Female	1	0	1	1	17884
Respondent circumcised	0.133	0.339	0	1	17884
Age at circumcision (in years)	3.072	2.429	0	15	2357
Ever heard of female circumcision	0.998	0.076	0	9	17884
Belief: FGM/C Required by Religion	0.275	0.948	0	9	17884
Belief: FGM/C Should be Stopped	0.913	0.282	0	1	17693

Note: Belief and knowledge variables for mother-reported sample are the beliefs reported by the mother and not of the daughter.

our results simply because it is difficult to tell in which survey these women were reporting their actual status. It might be that women who were cut now feel that the practice is shameful and want to report that they conform to society's new expectations, or it might be the case that these women were never cut and in the earlier survey were trying to conform to previous societal expectations. It may also be some combination of these two effects. There are also concerns that mothers might be less likely to accurately report their daughter's status compared to self-reporting. We discuss our solutions to these potential biases in the following section.

5 Econometric Strategy

The purpose of this investigation is to causally isolate the impact of legal change on the substantive likelihood of a women or girl in Burkina Faso being cut. In theory, a Regression Discontinuity Design (RDD) allows us to estimate the average likelihood of an individual being cut immediately before and immediately after the law was passed. The difference between these two values estimates the Local Average Treatment Effect (LATE) of the law.[7] For an overview of the use of Regression Discontinuity Designs in economics, see Lee and Lemieux (2010).

The nature of female genital mutilation/cutting, and the data that have been collected on it, makes the traditional Regression Discontinuity approach problematic. If, for example, it was the case that all women and girls were cut at the same age, say at birth, it would be simple to use an individual's date of birth as the running variable and test if those women born immediately *after* the law was implemented had a lower likelihood of being cut compared to those born immediately *before*. In reality, while more than 40% were cut at birth (0), 90% were cut by age 9, and the maximum age at cutting was 25. Thus, it is possible (if unlikely) that a cut individual who was born 24 years prior to the law's passing could still have been cut *after* the law. Furthermore, this approach would involve predicting what the age at cutting would have been for those who were not cut, making additional (unnecessary) structural assumptions. To take the results from such an analysis seriously, we would have to be extremely confident in our specification.

To get around these data features, we take inspiration from Lee et al. (2009) who use an

[7] Note the distinction between a local average treatment effect and the potential long-term impact of the laws. What we capture here is the immediate impact of the law on the rate of hazard and not the cumulative effect of the law over time. In fact it is likely that if enforcement becomes more effective over time, the long run impact of the law will be much greater than the LATE described here.

approach rooted in survival analysis to estimate the discontinuous jump in an individual's propensity to commit a crime immediately following their 18th birthday. Their data is similarly based on events, though theirs are binned at the week level whereas ours, by necessity, are aggregated by year. What this means is that rather than use date of birth as our running variable we approximate the year in which each individual was cut (by adding the age at cutting to the birth year) and use these 'events' to construct hazard rates, or the likelihood that any uncut women or girl within our age limits will be cut in a given year. We can then test if there is a discontinuous drop in that likelihood the year the law was implemented.

Formally, we define the hazard rate as $\hat{h}(t) = n_t/(N_t - \sum_{t_0}^{t-1} n_t)$ where t is the year, n_t is the number of individuals that were cut in time t and the denominator $(N_t - \sum_{t_0}^{t-1} n_t)$ represents the number of individuals who could still be cut in each time period. We also allow individuals to graduate out of the sample if age in year t is greater than 25 (the maximum observed age at cutting). However, there are several concerns regarding the discreteness of our variables. Because we have neither the exact date of birth nor the exact date of cutting it is difficult to say with certainty whether or not an individual event occurs before or after the law was implemented. What we can say, assuming uniform distribution of birth dates, is that the estimated date of cutting for observations born in year t, and cut at age $= 0$, the average date of cutting will be $t + 0.5$. We calculate predicted date of cutting accordingly.

To get the data into the appropriate form, we first calculate the date of cutting from the date of birth and age at cutting variables available in the DHS VI survey (as discussed above). We then convert the cross-sectional data into an unbalanced panel-style data set where individuals have an indicator Y_{it} for being cut in time t. For all years past the implementation of the law ($t > 1996$) we have an indicator variable L equal to 1, else 0. We then estimate the following logit regression.

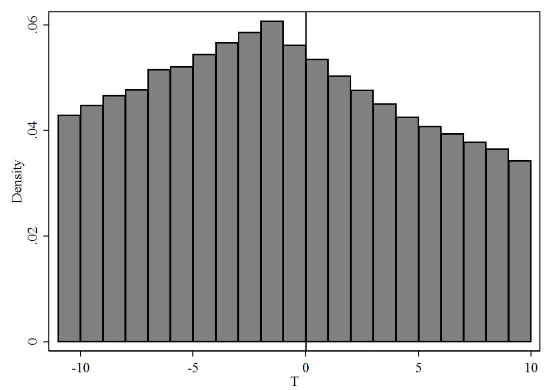
$$P(Y_{it} = 1|T, L_t) = F(T'\alpha + L_t\theta) \tag{1}$$

Where $T \equiv (1, t - t_0, (t - t_0)^2, ..., (t - t_0)^q)$ and $F(x) = e^x/(1 + e^x)$. Thus the estimated value of θ is the predicted value of the discontinuous change in the log-odds of an individual being cut as the law was passed. How the logit regression estimates the above hazard function is described in Efron (1988). All of our regressions are estimated using robust two-stage sampling with stratification clustered standard errors except where otherwise stated.

Underlying our empirical design is the assumption that $F(T')$ is a continuous function. Though our time variable is discrete, recorded time should be a continuous linear function, however, in the case of reported data on an illegal practice, there is a concern that individuals might shift reported age at cutting to fall in line with the legal change. There are a number of ways of testing for this including the McCrary (2008) test.

As our data are discrete, however, we present here a histogram of the density of observations on either side of the threshold and observe no significant change in density immediately around the cutoff. A Frandsen (2013) test (a discretized version of the McCrary (2008) test) finds no evidence for discontinuity in the density of the running variable at the threshold. Additionally, we employ traditional non-parametric Regression Discontinuity Design to test for discontinuous changes in age at cutting. As can be seen in figure 2 we do observe a significant change in the reported age at cutting immediately following the law's passage; it goes up. However, this is the *opposite* effect we would expect to observe if individuals were purposefully shifting their age at cutting to report conforming with the law.[8] Indeed, this change can be seen as preliminary evidence that the law is altering behaviors related to FGM/C in Burkina Faso.

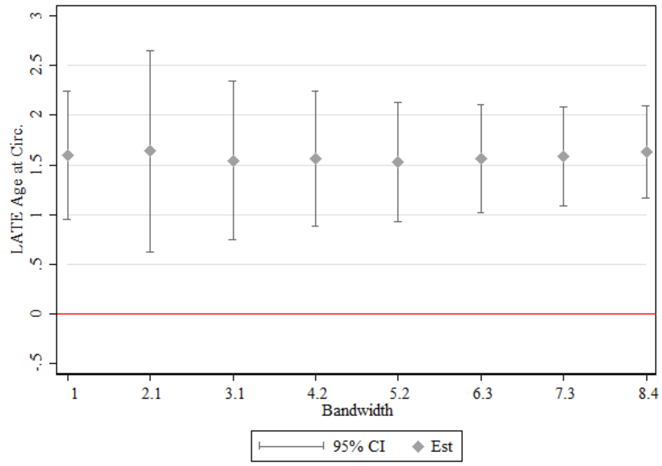
Fig. 1: Density of the Running Variable



The results of each of these tests give us no reason to suspect that survey respondents who report a positive FGM/C status are systematically manipulating their responses to conform to the legal change. However, we stress that these tests cannot disprove systematic manipulation of our primary outcome variable, self-reported FGM/C status.

[8]For example, a cut individual might say they were cut at an earlier age so that the timing of the cutting occurred prior to the law's implementation.

Fig. 2: Effect on Age at Circumcision



Note: The Y-axis reflects the local average treatment effect of the law on the age at cutting. The X-axis is the total window size within which the local-polynomial regressions were estimated. Bandwidth was selected via the Imbens and Kalyanaraman (2011) optimal bandwidth estimator. This graph presents a range of bandwidths from 25% to 200% the size of this optimal bandwidth.

5.1 Overcoming the Discrete Running Variable

As mentioned above, there are several concerns which stem from the discreteness of the running variable. Of primary concern is that without continuous observations immediately around the cutoff, researchers are forced to employ some functional estimator to parametrically extrapolate $E[Y(1) - Y(0)|T - c = 0]$. Imposing a functional form in this manner can induce some level of specification error which can bias the magnitudes of the estimated coefficients. We try to mitigate this error by presenting a broad range of functional forms and bandwidths. However, Lee and Card (2008) present an additional, simple way to account for this potential bias by clustering the errors around the discrete running variable –in our case, year. In practice, it is difficult to implement this approach given the structure of our data. The DHS survey is a nationally representative survey. This makes it a very powerful tool for analysis but it also means that the data are already nested in DHS sampling units which are themselves nested in DHS Regions. This nested data structure consequently requires adjustments to the standard errors.

We overcome this by generating non-parametric estimates of our hazard rates using discrete survival analysis which takes into account the two-stage cluster sampling with stratification and weighting survey design employed by the Demographic and Health Surveys. To start, we estimate the following equation:

$$\operatorname{logit} h(T_j) = \alpha_1 T_1 + \alpha_2 T_2 + \ldots + \alpha_n T_n \tag{2}$$

Where T_j is simply a dummy variable for each year present in the sample. The alpha parameters (α_j) describe the survey-weighted population log-odds of being cut in year j. We can then use a Weighted Least Squares (WLS) estimator on the estimated log-odds and cluster the standard errors over each discrete year following equation.3[9] We weigh each estimated hazard rate by the inverse of the standard error of the alpha terms as discussed in Lewis and Linzer (2005) and King (2013). This generates an estimate of the discontinuity that is robust to both the sampling design and potential specification error.

$$\alpha_j = \gamma + \theta' L_j + T'\beta + \varepsilon_j \tag{3}$$

There are several drawbacks to this approach, and for these reasons we offer this as a robustness check rather than the primary analytical approach. Primarily, our unit of analysis with this technique is the discrete year level. This means that we lose the ability to differentiate between individual characteristics such as religion, ethnicity, and region. For calculating the local average treatment effect of the legal change, this is fine. Given that our aim is also to identify the heterogeneous impact of legal change, however, this approach falls flat.

Throughout our analyses we also present a number of other robustness checks. These include varying the bandwidth of our local-linear regressions, altering the specification by including increasing orders of polynomials, including only self-reported observations in our RD, and a falsification exercise.

Following this series of initial robustness tests we also investigate potential heterogeneity in the impact of the law across a variety of subgroups. From a policy perspective, identifying sub-populations for whom legal change has a very strong effect, and conversely those for whom there is no observable effect, has immediate implications for refining eradication efforts. For this reason, we explore heterogeneous treatment effects by ethnicity and religion. Furthermore, it is possible that impact of legal change may be a function of geography. Regions at the periphery might suffer from lessened enforcement or lower media access. Additionally, geographically isolated communities will house different social-norm environ-

[9] We also include interaction effects to allow the slope of the line or higher order derivatives on either side of the cutoff to vary.

ments. The differential composition of norms and preferences regarding FGM/C in these communities suggests varied 'pay-off' functions in the decision to have one's daughter cut or not. Whether legal change tips the scale in favor of *not* performing this action will depend on the community in question.[10]

To account for this differential impact we add dummy variables and interaction terms between the subpopulation and our legal change indicator L for each Region, Religion, and Ethnicity variables in turn. This allows both the intercepts, slopes, and discontinuity to reflect the underlying differences in each subgroup. To calculate the group-specific LATE we estimate the marginal effect of the law within each subgroup when $T = 0$.

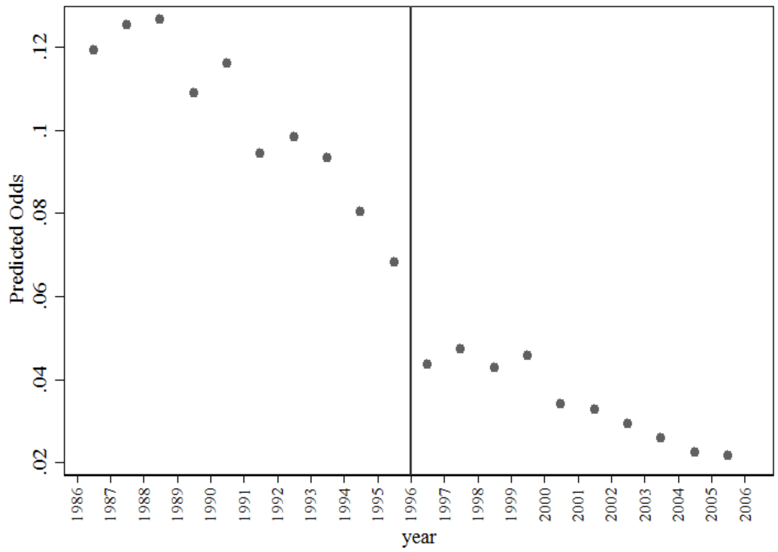
6 Results

With these considerations in mind, we return to our initial research question; did the passing of legislation prohibiting the practice of female genital mutilation/cutting significantly reduce the likelihood that at-risk women and girls would be cut in a given year? We first present non-parametric estimates of hazard rates as estimated by equation 2. In general, the likelihood of women and girls being cut in a given year has been decreasing over time. We observe a fairly continuous decrease with several observable drops in rates of hazard (see figure 3). In 1989 there appears a large drop, however, this is followed by a reversal of trend in the following year. In 1991, the risk diminishes once again and remains low. Some of these might be explained by other anti-FGM/C actions. For example, in 1990, the National Committee to fight the Practice of Excision was founded and might explain some of drop off in 1991. Furthermore, as can be seen in figure 6, these differences fall within the 95% confidence bounds of the adjacent year's estimated rate of hazard. We also note that the single largest year-to-year change in the likelihood of being cut was between 1995 and 1996, the year the law was passed. In this section we rigorously test the magnitude and significance of this drop.

Figure 4 graphically demonstrates our initial findings. The Y-axis reflects the likelihood of an individual being cut (the exponentiated log-odds, or just 'odds') and the X-axis is simply years. Across a variety of specifications which include additional higher order polynomials, we observe a statistically significant discontinuous drop in the hazard of being cut in the year the law was passed. Variable L in table 2 is our estimated value of θ (change in log-odds) across specifications. A polynomial of degree zero simply reflects the change in the average

[10] For the intuition behind this argument, see Benabou and Tirole (2011)

FIG. 3: Predicted Odds of Being Circumcised by Year



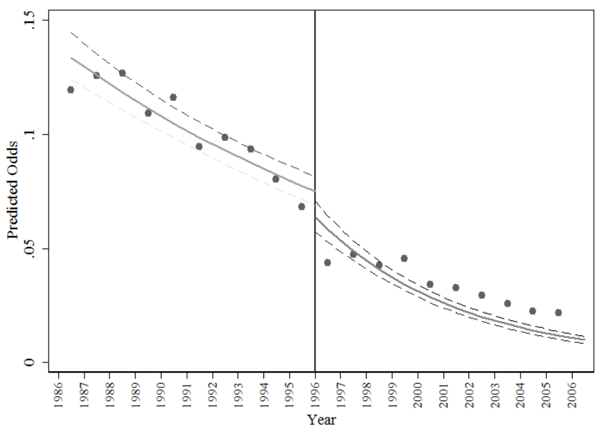
hazard rates before and after the law was passed. Excluding this column, our estimates range from a drop of -0.3015 to -0.3911 in the log-odds of being cut. This means for the average eligible woman, the effect of the law was to reduce her odds of being cut by between 26% and 32%.[11] When we allow the derivatives on either side of the cutoff to be independent (by including interaction terms between legal change and time) we get very consistent estimates of θ hovering around -0.31, or a 27% reduction in the odds. Even with our most flexible specification (3rd degree polynomial with interaction terms) we observe a significant drop of -0.3911 in the log-odds, or a 32% reduction.

To put the potential impact of this legislation into perspective, we perform a quick, back-of-the-envelope calculation of the change in the cumulative hazard as wel as the total number of FGM/Cs averted as a result of the law. Acknowledging the *major* caveat that RDD results are only valid in the region of the cutoff, we can exploit the nature of our parametric estimation technique to predict the rate of hazard in the counterfactual scenario in which the legal change never took place. We can then estimate the cumulative predicted odds of an individual being cut in the counterfactual absence of the law ($\sum_{t=0} \frac{\hat{p}_t^{L=0}}{1-\hat{p}_t^{L=0}}$) and

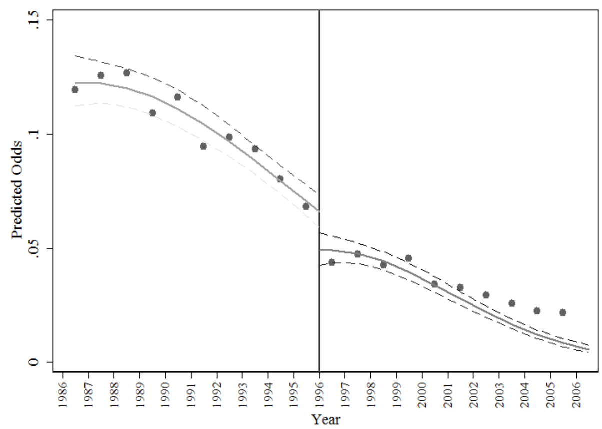
[11]To calculate this change we first exponentiate the log-odds to get the odds ratio (e^{θ}). Given that the odds ratio is less than one, we invert this value ($\frac{1}{e^{\theta}}$) to calculate the odds of those born immediately before the law relative to those born after. Thus, given an estimated change in log-odds of -0.30, those born immediately before the law were $\frac{1}{e^{-0.30}}$ or 1.35 times more likely to be cut. To calculate the relative change for those born after the law we simply subtract 1 from that estimate and divide by it again, $\frac{1.35-1}{1.35} \approx 0.26$. Thus a decrease in the log-odds of 0.30 is equivalent to a 26% reduction in the odds of being cut.

Fig. 4: Treatment Effect with Different Specifications

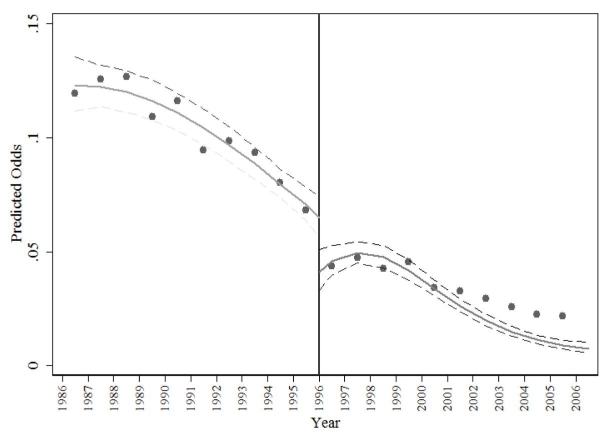
(a) Polynomial of Degree 1



(b) Polynomial of Degree 2



(c) Polynomial of Degree 3



Note: Includes interaction effects between Law (L) and Year ($T, T2...$) polynomials.

the presence of the law ($\sum_{t=0} \frac{\hat{p}_t^{L=1}}{1-\hat{p}_t^{L=1}}$).[12] The difference between these two values represents the change in cumulative odds of an individual being cut between the year of the law's passage and the year of the DHS survey. For simplification, we employ the specifications that restrict the first order derivative to being the same on either side of the cutoff (without interaction effects). In figure 5, the dashed line indicates the predicted counterfactual odds and the shaded area the cumulative hazard that the law averted. Calculated with the first degree polynomial, the cumulative hazard drops from approximately 0.47, to 0.33 - a net difference of approximately 0.14.

Following a similar logic, we can calculate the number of FGM/Cs averted. In each year we take the number of predicted FGM/C events in both the counterfactual and realized scenarios by multiplying the probability $\hat{p}_t$ by the risk set n_t. We then sum these values across the ten year period 1996—2006 to get the total number of averted FGM/Cs in our sample (2,128).[13] Scaling this value by the factor $111.65 \approx \frac{8{,}180{,}108}{73{,}265}$, the ratio of population size to sample size,[14] we very roughly estimate the total number of FGM/Cs averted in Burkina Faso in the years following the passage of the law. Applying this methodology to the first order polynomial, we estimate that over a ten year period the law averted the genital mutilation/cutting of approximately 237,591 women and girls. While some might view this as a modest decrease, these findings cast serious doubt on the claim that legal change will have *no* impact on the status of FGM/C.

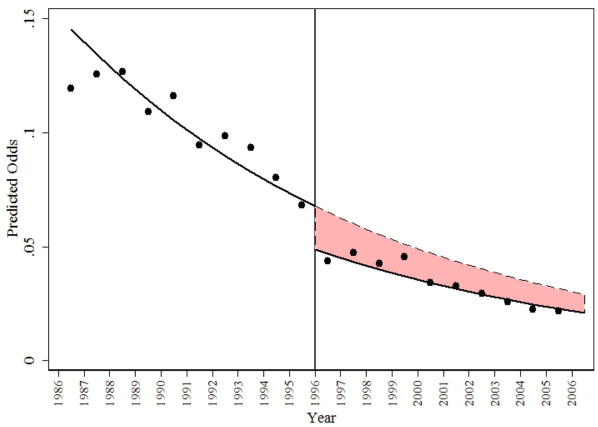
[12]We implement this calculation in Stata 14 by predicting expected hazard rates for each year after the cutoff and the predicting the same again setting the value of L to 0. We then use the -integ- command on the predicted odds in both scenarios.

[13]Or $\sum_{t=0} \hat{p}_t^{L=0} \cdot n_t - \hat{p}_t^{L=1} \cdot n_t$

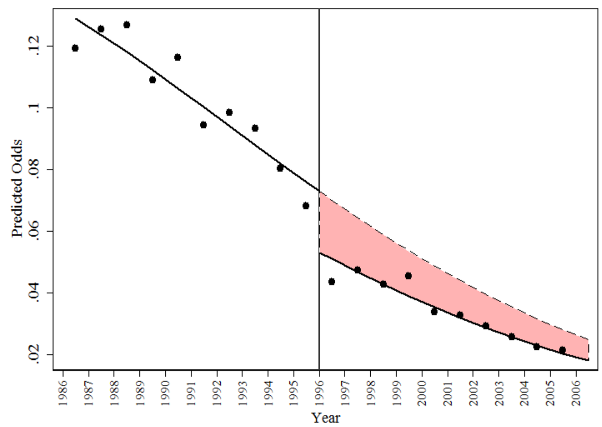
[14]Numerator is the total population of Burkina Faso in 2010 multiplied by the .52 (the percentage of population that is female) from FASO (2012). Denominator is the 17,087 female observations plus 10 56,178 daughter observations.

Fig. 5: Treatment Effect with Different Specifications

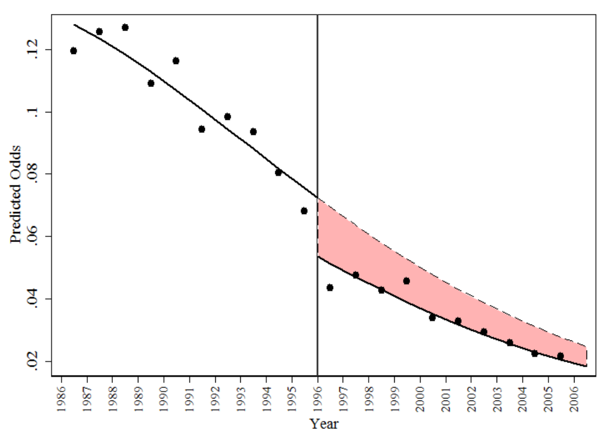
(a) Polynomial of Degree 1



(b) Polynomial of Degree 2



(c) Polynomial of Degree 3



Note: Do not include interaction effects between Law (L) and Year ($T, T2...$) polynomials. Dashed line indicates the counterfactual continuation of the pre-law trends.

Table 2: Treatment Effects Under Different Specifications

	Polynomial of Degree:						
	Zero	One	One	Two	Two	Three	Three
Y							
L	-1.1926***	-0.3255***	-0.3132***	-0.3172***	-0.3135***	-0.3015***	-0.3911***
	(0.038)	(0.055)	(0.053)	(0.053)	(0.086)	(0.072)	(0.121)
T		-0.0804***	-0.0620***	-0.0802***	-0.1370***	-0.0836***	-0.1461**
		(0.004)	(0.006)	(0.005)	(0.026)	(0.011)	(0.065)
LxT			-0.0380***		0.1067***		0.2056**
			(0.010)		(0.035)		(0.094)
T2				-0.0021***	-0.0075***	-0.0022***	-0.0098
				(0.000)	(0.002)	(0.000)	(0.015)
LxT2					0.0011		-0.0171
					(0.003)		(0.021)
T3						0.0000	-0.0002
						(0.000)	(0.001)
LxT3							0.0014
							(0.001)
Interaction Effects			✓		✓		✓
N	158579	158579	158579	158579	158579	158579	158579

Note: Uses full sample (both mother and daughter observations). Interaction Effects indicates that the regression allows the derivatives of T polynomials to change on either side. Survey weighting with clustered standard errors.

6.1 Robustness Checks

Particularly in the case of parametric estimation, there is a concern that the bandwidth of our regressions might induce bias. In the absence of a continuous time variable, we can test for this bias simply by varying the bandwidth of our regressions. As local linear regressions describe most closely the intuition behind the traditional RDD we specify our regressions with first degree polynomials and allow the slope on either side of the cutoff to vary. We then gradually reduce the bandwidth (the window within which the regressions are run) to observe if this changes our estimates. Looking at table 3, we see magnitudes of remarkable consistency across each bandwidth, ranging from -0.3311 to -0.4141. We also see a large

Table 3: Treatment Effects Under Different Bandwidths

	Window Size:				
	[-10,+10]	[-7,+7]	[-5,+5]	[-3,+3]	[-2,+2]
Y					
L	-0.3363***	-0.3311***	-0.3589***	-0.3314***	-0.4141***
	(0.055)	(0.066)	(0.079)	(0.118)	(0.152)
T	-0.0616***	-0.0769***	-0.0846***	-0.1559***	-0.1585**
	(0.006)	(0.011)	(0.018)	(0.040)	(0.079)
LxT	-0.0322***	-0.0011	0.0302	0.1429**	0.2439*
	(0.010)	(0.017)	(0.028)	(0.059)	(0.129)
N	143347	94238	65158	38238	25278

Note: Uses full sample. Interaction effects indicates allowing derivatives to change on either side. Survey weighting with clustered standard errors. Each regression includes year (T) polynomials of degree 1 and interaction effects between year (T) and legal change (L).

degree of significance across all specifications. Furthermore, there appears no systematic direction of bias as we exclude additional years, though the specification with the narrowest bandwidth also finds evidence for the largest discontinuity. See figure 7 in the appendix.

We also test the robustness of our results to potential bias from differential reporting of FGM/C status by self-reported observations versus those which were reported by mothers. We remove those observations that were reported by the mother and observe no major differences. As can be seen if table 4, our results remain significant and are of a similar magnitude. The specification with a polynomial of degree 1 with interaction effects offers the least significant (though still significant at a 95% confidence level) and lowest magnitude estimate of a -0.1652 change in the log-odds whereas the most flexible specification with third order polynomials with interaction effects again produces the largest estimates (a change in the log-odds of -0.4564).

As a falsification exercise, we also test for discontinuous changes in hazard rates at other points. This analysis is analogous to a placebo test in medical trials. If we were to consistently observe discontinuities at other points, it would cast doubt on the validity of the findings we present here.

We run similar regressions with first and second degree polynomials within varying the temporal location of a hypothetical legal change. As can be seen in Tables 5 and 6, hypothetical legal changes in the years 1989, 1990, 1993, 1994, 1998, 2001, 2002, and 2003 had no observable, discontinuous effect in hazard rates. These null findings suggest that the results

Table 4: Self-Reported Observations Treatment Effects

	Polynomial of Degree:						
	Zero	One	One	Two	Two	Three	Three
Y							
L	-1.1926***	-0.3255***	-0.1652**	-0.3172***	-0.2941***	-0.3015***	-0.4564***
	(0.038)	(0.055)	(0.066)	(0.053)	(0.103)	(0.072)	(0.149)
T		-0.0804***	-0.0607***	-0.0802***	-0.1336***	-0.0836***	-0.1369**
		(0.004)	(0.006)	(0.005)	(0.025)	(0.011)	(0.064)
LxT			-0.1168***		0.1410***		0.3537***
			(0.014)		(0.047)		(0.123)
T2				-0.0021***	-0.0072***	-0.0022***	-0.0080
				(0.000)	(0.002)	(0.000)	(0.015)
LxT2					-0.0131**		-0.0674**
					(0.005)		(0.030)
T3						0.0000	-0.0001
						(0.000)	(0.001)
LxT3							0.0038**
							(0.002)
Interaction Effects			✓		✓		✓
N	158579	158579	91756	158579	91756	158579	91756

Note: Uses mother-only sample. Interaction effects indicates allowing derivatives to change on either side. Survey weighting and clustered standard errors.

of our main regressions are not simple artifacts of methodology and are capturing a real change in hazard rates immediately around the time of the law's passage. For no year do we find evidence of significant discontinuities in both specifications.

We do, however, observe a positive change in 1992 and 1999 using first degree polynomial specifications, and negative changes in 1991 and 2000 using second degree polynomial specifications. However, each of these vary substantially between specifications and are not robust to additional checks. Additionally, we note that 1991 is the year following the establishment of Burkina Faso's CNLPE (National Committee to Fight the Practice of Excision) and that this year was also characterized by substantial anti-FGM/C action. The results of these falsification exercises lend us substantial confidence in the robustness of our main finding of a significant drop in hazard rates as a result of the law's implementation.

Table 5: Falsification Tests I

	Placebo Legal Change									
	1989	1990	1991	1992	1993	1999	2000	2001	2002	2003
Y										
L	-0.0265	0.1186	0.0299	0.1706**	0.0846	0.2277**	-0.0685	-0.0556	0.0295	0.0329
	(0.066)	(0.064)	(0.068)	(0.064)	(0.070)	(0.081)	(0.079)	(0.077)	(0.076)	(0.075)
T	−0.0385*	−0.0745***	-0.0196	−0.0647***	−0.0653***	−0.1662***	−0.0870***	−0.0546**	−0.0963***	−0.1081***
	(0.017)	(0.017)	(0.017)	(0.017)	(0.016)	(0.020)	(0.022)	(0.021)	(0.020)	(0.020)
LxT	-0.0227	-0.0172	−0.1182***	−0.1059***	−0.1062***	0.0373	-0.0138	−0.0676**	-0.0488	-0.0440
	(0.022)	(0.022)	(0.022)	(0.023)	(0.023)	(0.025)	(0.027)	(0.025)	(0.026)	(0.027)
N	49218	51970	54643	57936	61687	94783	102673	110498	119130	128134

Note: Uses full sample. The table presents local linear regressions within a 10 year window which allow derivatives to change either side of each simulated cut-off.

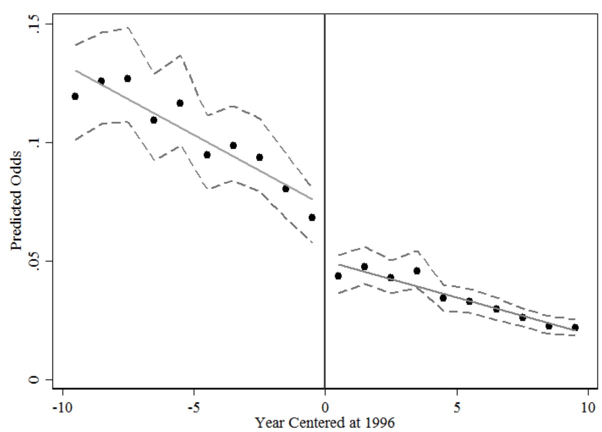
Table 6: Falsification Tests II

	Placebo Legal Change									
	1989	1990	1991	1992	1993	1999	2000	2001	2002	2003
Y										
L	-0.0739	-0.0210	−0.2231*	0.1728	0.1559	0.0874	−0.3376**	0.0960	0.0128	0.0094
	(0.105)	(0.101)	(0.113)	(0.113)	(0.107)	(0.124)	(0.125)	(0.125)	(0.131)	(0.130)
T	-0.0401	0.0232	-0.0663	−0.1454*	-0.0285	0.0501	0.2138*	−0.2613**	−0.1825*	-0.1457
	(0.066)	(0.073)	(0.072)	(0.074)	(0.077)	(0.090)	(0.091)	(0.089)	(0.093)	(0.087)
LxT	0.0271	-0.0588	0.2245*	0.0449	−0.2503**	−0.2270*	−0.2987**	0.1630	0.1303	0.0515
	(0.085)	(0.094)	(0.090)	(0.090)	(0.094)	(0.112)	(0.111)	(0.110)	(0.108)	(0.112)
T2	-0.0003	0.0194	-0.0094	-0.0160	0.0073	0.0426*	0.0599***	−0.0417*	−0.0172	-0.0075
	(0.013)	(0.014)	(0.014)	(0.014)	(0.015)	(0.017)	(0.018)	(0.018)	(0.018)	(0.017)
LxT2	-0.0077	-0.0288	−0.0410*	0.0040	0.0110	-0.0345	−0.0626**	0.0377	0.0013	-0.0025
	(0.016)	(0.017)	(0.017)	(0.019)	(0.018)	(0.020)	(0.020)	(0.021)	(0.022)	(0.021)
N	49218	51970	54643	57936	61687	94783	102673	110498	119130	128134

Note: Uses full sample. The table presents local linear regressions within a 10 year window which allow derivatives to change either side of each simulated cut-off.

Table 7 presents the result of the mean-cell version of our RD. We find qualitatively similar results to our main regressions; we observe comparable magnitudes of effect across specifications and similar levels of significance. In each of our specifications we reject the null hypothesis of no discontinuity in the rate of hazard immediately following the legal change. These specifications take into account survey-weighting and are robust to potential specification error. Figure 6 presents the estimated hazard rate for each year, 95% confidence bounds, and a linear regression fit either side of the law. Magnitudes are, again, broadly similar and range between -0.2690 and -0.3563.

FIG. 6: "Mean'-Cell Local-Linear Regressions with Population Confidence Bounds



6.2 Heterogeneous Treatment Effects

We now turn to potential heterogeneity in treatment effects by religion, ethnicity, and region. There are a number of reasons to look beyond the average effect of the law. The primary reasons include 1) to identify where and for whom legal change had the largest impact, 2) to identify patterns which can inform the construction of future policy and 3) to inform the targeting of those policy and advocacy efforts. Figures 8 through 10 in the appendix present the estimated odds of being cut within each subgroup with and without the law ($L = 1$ and $L = 0$ respectively) at time T=0. Tables 8 through 10 present the change in odds. We also note that here we estimate each group interactions separately. It is likely that there is some correlation between ethnicity and region or ethnicity and religion, for example.

We observe substantial disparities in initial hazard as well as the estimated impact of the law for these subgroups. Fortunately, and perhaps as one might expect, subpopulations that

Table 7: Treatment Effects and Confidence with “Mean’-Cell Version

	Polynomial of Degree:					
	One	One	Two	Two	Three	Three
L	-0.0067***	-0.0065***	-0.0060***	-0.0014	-0.0021	-0.0078**
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)
T	-0.0015***	-0.0013***	-0.0016***	-0.0034***	-0.0025***	0.0013
	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.002)
LxT		-0.0006		0.0009		-0.0016
		(0.000)		(0.001)		(0.003)
T2			-0.0000*	-0.0002**	-0.0000	0.0008*
			(0.000)	(0.000)	(0.000)	(0.000)
LxT2				0.0003*		-0.0014*
				(0.000)		(0.001)
T3					0.0000**	0.0001**
					(0.000)	(0.000)
LxT3						-0.0000
						(0.000)
Interaction Effects		✓		✓		✓
N	20	20	20	20	20	20

Note: Interaction effects denotes regressions which allow derivatives to change on either side. We cluster over discrete values of T to account for specification error.

have the highest initial rate see the largest drop in rates of hazard, though many of these populations also still hold the highest risk for women and girls. In religion, for example, while individuals who identified as Muslim had one of the highest initial odds of being cut, they also exhibited one of the largest drops in odds. In contrast, for those who identified as Traditional/Animist the law had no observable impact. We also observe considerable heterogeneity when we compare across ethnicities. Notably, individuals who report their ethnicity as Touareg/Bella, Dagara, or Gourmatch appear to be least affected by the law. However, Touareg/Bella also had the lowest baseline odds of being cut and the estimated treatment effect is not significantly different from zero. For those identifying as Dioula we find a slightly larger magnitude but insignificant change in odds of being cut.

Interestingly, there does appear to be significant differences in effect by administrative region. At first glance, however, these appear uncorrelated with distance from the national capital in Ouagadougou or their remoteness. For example, in Boucle de Mouhoun the law appeared to have minimal impact; however in regions further removed from the capital such as the Cascades, Hauts-Basins, and the Sud-Ouest the law seems to have had a substantial impact. It should be noted, however, that there does appear to be some clustering of magnitude around the regions Plateau-Central, Centre, Centre-Est, and Centre-Nord. This initially suggests that perhaps regions immediately proximal to the legislative center receive a boost in efficacy, but that outside of a certain range other factors play a role. Further analysis is necessary, perhaps controlling for indicators of government capacity or media coverage.

Table 8: Heterogeneous Treatment Effects by Religion

	Religion:				
	Catholic	Muslim	Protestant	Traditional	None
Δ Odds	-0.0242***	-0.0427***	-0.0187***	-0.0130**	-0.0218*
	(0.004)	(0.006)	(0.005)	(0.006)	(0.012)
Baseline Odds	0.0497	0.0982	0.0397	0.0545	0.0474
N	158084	158084	158084	158084	158084

Note: Δ Odds is the discontinuous change in the odds (note, this is varies from our other presentations which demonstrate the change in log-odds). Standard Errors calculated using Delta-Method.

Table 9: Heterogeneous Treatment Effects by Region

	Region:						
	B. Mouhoun	Cascades	Centre	Centre-Est	Centre-Nord	Centre-Ouest	Centre-Sud
Δ Odds	-0.0127*	-0.0494***	-0.0292***	-0.0646***	-0.0590***	-0.0147***	-0.0215***
	(0.008)	(0.009)	(0.005)	(0.015)	(0.012)	(0.004)	(0.006)
Baseline Odds	0.0522	0.0890	0.0519	0.1277	0.1110	0.0363	0.0492
N	158579	158579	158579	158579	158579	158579	158579
	Region (Continued):						
	Est	Hauts Basins	Nord	Plat. Central	Sahel	Sud-Ouest	
Δ Odds	-0.0178***	-0.0581***	-0.0503***	-0.0654***	-0.0369	-0.0299***	
	(0.006)	(0.013)	(0.014)	(0.011)	(0.024)	(0.009)	
Baseline Odds	0.0505	0.1214	0.1211	0.1169	0.1192	0.0775	
N	158579	158579	158579	158579	158579	158579	

Note: Δ Odds is the discontinuous change in the odds (note, this is varies from our other presentations which demonstrate the change in log-odds). Standard Errors calculated using Delta-Method.

Table 10: Heterogeneous Treatment Effects by Ethnicity

	Ethnicity:					
	Bissa	Bobo	Dagara	Dioula	Fulfuld/Peulh	Gourmatch
Δ Odds	-0.0361***	-0.0284**	-0.0140*	-0.0311	-0.0437***	-0.0134**
	(0.014)	(0.011)	(0.008)	(0.021)	(0.016)	(0.006)
Baseline Odds	0.0917	0.0590	0.0519	0.0635	0.1309	0.0418
N	157200	157200	157200	157200	157200	157200
	Ethnicity (Continued):					
	Gourounsi	Lobi	Mossi	Other	Senoufo	Touareg/Bella
Δ Odds	-0.0258***	-0.0339***	-0.0385***	-0.0341***	-0.0579***	-0.0060
	(0.006)	(0.011)	(0.005)	(0.011)	(0.015)	(0.004)
Baseline Odds	0.0466	0.0813	0.0817	0.0885	0.1205	0.0103
N	157200	157200	157200	157200	157200	157200

Note: Δ Odds is the discontinuous change in the odds (note, this is varies from our other presentations which demonstrate the change in log-odds). Standard Errors calculated using Delta-Method.

7 Conclusion

Our analysis demonstrates that in Burkina Faso, the passage of a law against FGM/C resulted in a decrease in the likelihood that the average woman or girl would be cut. We find that the impact of the legal change is varied across religious and ethnic subgroups as well as by administrative region. However, we stress again that legislation should not be understood to be operating in isolation, absent wider shifts in attitudes, social practices, and government and civil society efforts to alter each of them. As mentioned, Burkina Faso's legislation was preceded by decades of advocacy efforts led by the national government as well as civil society organizations beginning in 1975. Through radio announcements, broader public awareness campaigns, and the establishment of a hotline to assist those at risk for being cut or those who had undergone the procedure and experienced complications, actors within Burkina Faso likely created favorable social and political conditions helping to ensure that the law would be effective. Were an anti-FGM/C law to be passed in another country context that lacked such advocacy and support service establishment efforts, its efficacy may be less pronounced.

Furthermore, though we do our best to attenuate the majority of data issues, there remain a few concerns. First, we have no means of determining the veracity of self-reporting and there is some evidence that individuals have altered their responses in similar surveys. Second, our cutting events are estimated at the year level. Though we believe that our results are econometrically robust to this data feature, it does pose some issues for causal inference. If, as we assume, the decision to have one's self or one's daughter cut exists within a system of social, economic, and legal norms, we cannot exclude the possibility that contemporaneous large changes in social norms occurred the same year of the law and were the primary drivers of change. This is one of the central problems in estimating the impact of laws on FGM/C and other related practices such as child and early marriage or inheritance. However, in comparison with the existing empirical literature which often employs difference-in-difference designs across many years, our approach greatly reduces the potential impact of such bias.

Policy Implications

Our research has a variety of public policy implications. Within Burkina Faso, we indicate several subpopulations for whom additional and alternative methods will be required to achieve change. At a more general level, there are two key take-aways from the analysis in this study:

1) Do not discount the role of legal change: Alongside more comprehensive action and reform, legislative action can be a useful tool for improving harmful and entrenched practices.

2) Do not rely solely on legal change: Even in what many take to be a robust example of change there remain regions and communities where the law had no observable impact.

To clarify and refine these findings, future research should unpack potential causal mechanisms. For example, through which mechanism(s) does national legal reform have an effect? Does the law heighten the costs of engaging in the practice of FGM/C through its (potential) imposition of fines and prison sentences on its participants, thereby offsetting the potential social and identity costs of abstaining from the practice? Or does the presence of a national law shift the social desirability or necessity of FGM/C itself?

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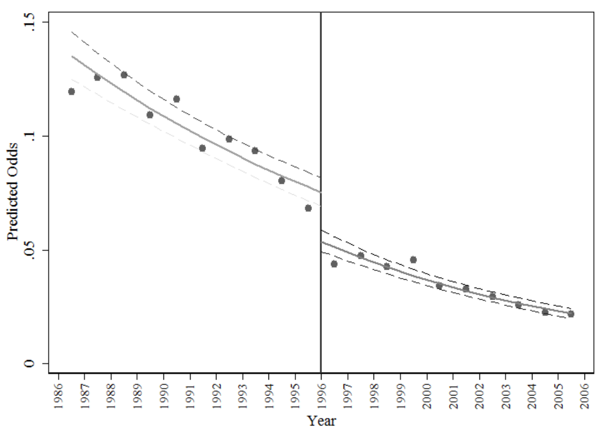
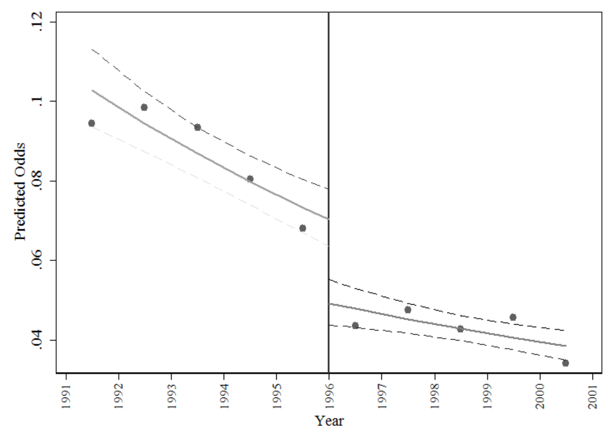
Appendix

Fig. 7: Treatment Effect with Varying Window Sizes

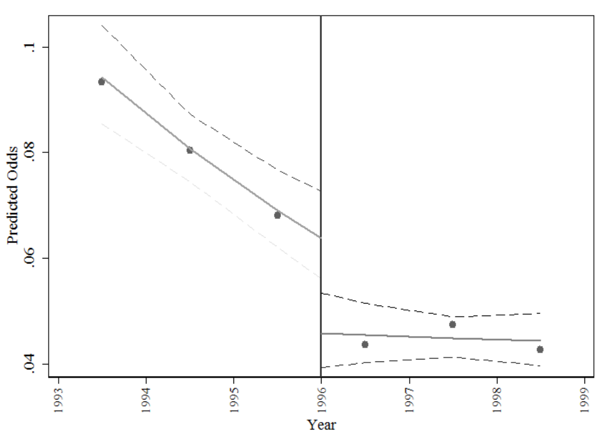
(a) Window = ±10



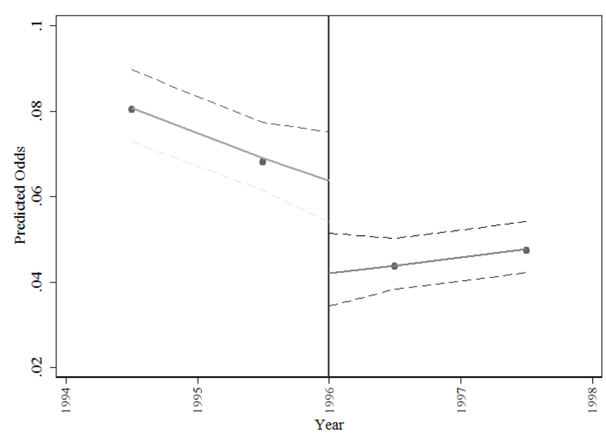
(b) Window = ±5



(c) Window = ±3



(d) Window = ±2



Note: Logit regression includes only linear specification of year. Includes interaction effects between Law (L) and Year (T), LxT.

Table 11: Status by Ethnicity

Ethnicity	Respondent Circumcised No	Yes	Total
Bissa	119	553	672
	(17.71%)	(82.9%)	100%
Bobo	214	430	644
	(33.23%)	(66.7%)	100%
Dagara	172	388	560
	(30.71%)	(69.29%)	100%
Dioula	47	108	155
	(30.32%)	(69.68%)	100%
Fulfuld/Peulh	197	1,149	1,346
	(14.64%)	(85.36%)	100%
Gourmatch	395	650	1,045
	(37.8%)	(62.2%)	100%
Gourounsi	349	441	790
	(44.18%)	(55.82%)	100%
Lobi	96	521	617
	(15.56%)	(84.44%)	100%
Mossi	1,960	6,954	8,914
	(21.99%)	(78.01%)	100%
NSP	0	3	3
	(0%)	(100%)	100%
Other	212	804	1,016
	(20.87%)	(79.13%)	100%
Other Fr. Ctry	5	7	12
	(41.67%)	(58.33%)	100%
Other Nationalities	0	2	2
	(0%)	(100%)	100%
Pays CEDEAO	44	23	67
	(65.67%)	(34.33%)	100%
Senoufo	135	771	906
	(14.9%)	(85.1%)	100%
Touareg/Bella	184	70	254
	(72.44%)	(27.56%)	100%
Total	4,129	12,874	17,003
	(24.28%)	(75.72%)	100%

Note: Includes only Self-Reported Observations.

Table 12: Status by Region

Region	Respondent Circumcised No	Respondent Circumcised Yes	Total
Boucle de	404	940	1,344
Mouhoun	(30.06%)	(69.94%)	100%
Cascades	217	889	1,106
	(19.62%)	(80.38%)	100%
Centre	556	1,136	1,692
	(32.86%)	(67.14%)	100%
Centre-Est	127	1,135	1,262
	(10.06%)	(89.94%)	100%
Centre-Nord	165	991	1,156
	(14.27%)	(85.73%)	100%
Centre-Ouest	751	766	1,517
	(49.51%)	(50.49%)	100%
Centre-Sud	368	782	1,150
	(32.%)	(68.%)	100%
Est	429	924	1,353
	(31.71%)	(68.29%)	100%
Hauts Basins	262	1,276	1,538
	(17.04%)	(82.96%)	100%
Nord	185	1,114	1,299
	(14.24%)	(85.76%)	100%
Plateau Central	173	1,080	1,253
	(13.81%)	(86.19%)	100%
Sahel	216	935	1,151
	(18.77%)	(81.23%)	100%
Sud-Ouest	283	929	1,212
	(23.35%)	(76.65%)	100%
Total	4,136	12,897	17,033
	(24.28%)	(75.72%)	100%

Note: Includes only Self-Reported Observations.

Table 13: Status by Religion

Religion	Respondent Circumcised No	Yes	Total
Catholic	1,464	2,702	4,166
	(35.14%)	(64.86%)	100%
Muslim	1,856	8,351	10,207
	(18.18%)	(81.82%)	100%
No religion	52	76	128
	(40.63%)	(59.38%)	100%
Other	0	2	2
	(.%)	(100.%)	100%
Protestant	439	631	1,070
	(41.03%)	(58.97%)	100%
Traditional	315	1,100	1,415
	(22.26%)	(77.74%)	100%
Total	4,126	12,862	16,988
	(24.29%)	(75.71%)	100%

Note: Includes only Self-Reported Observations.

Fig. 8: Differential Impact by Religion

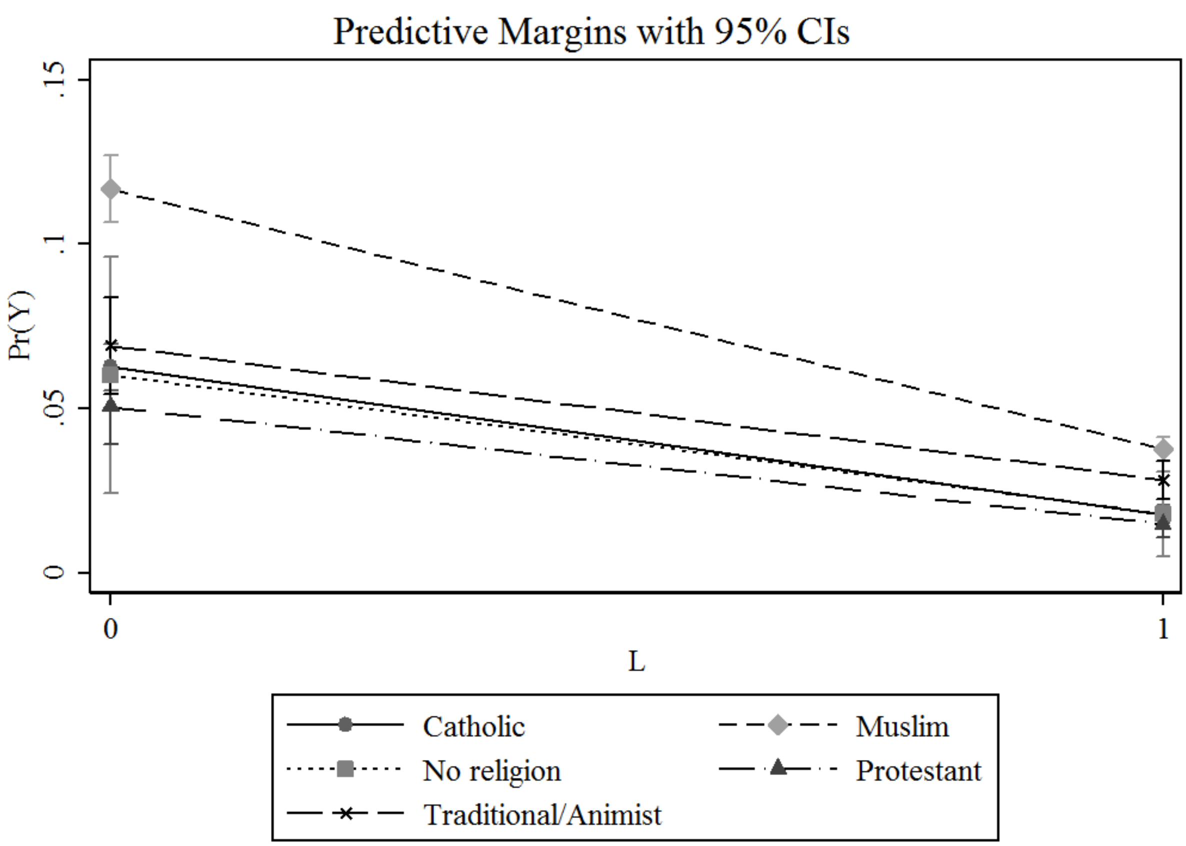


Fig. 9: Differential Impact by Ethnicity

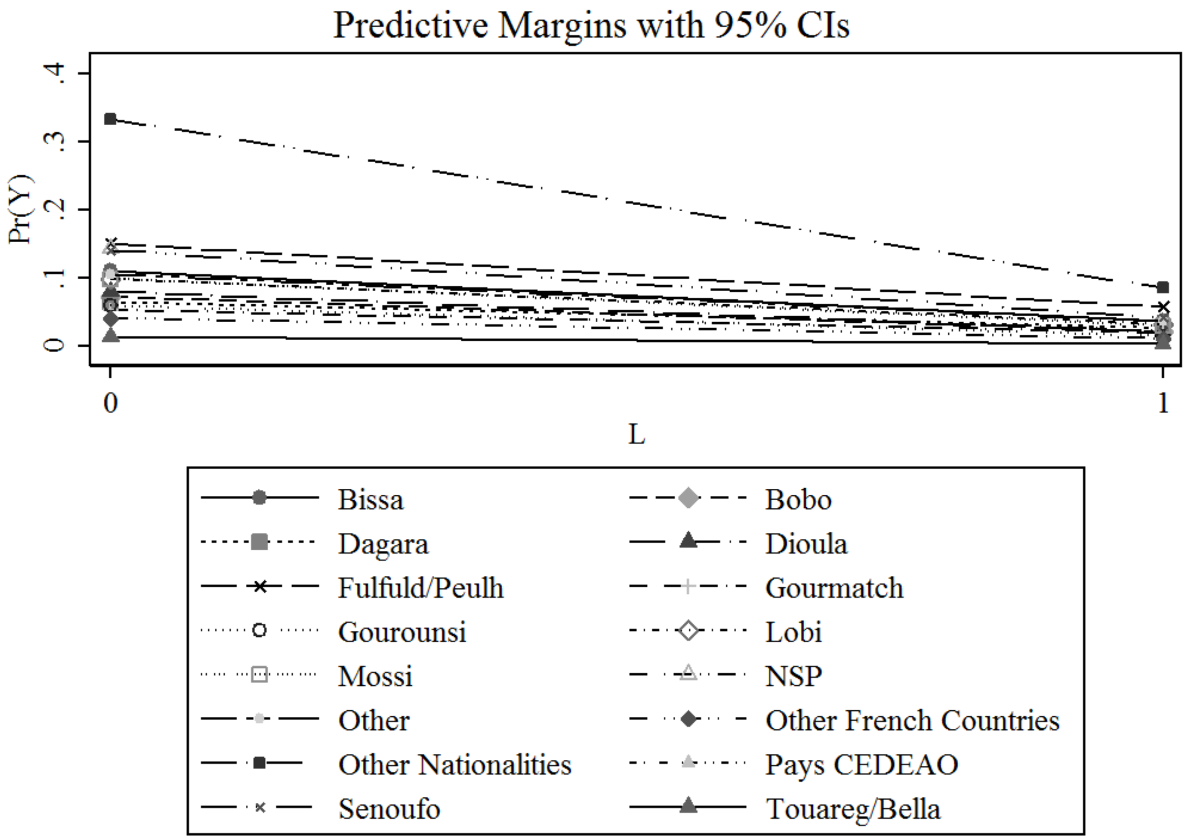


Fig. 10: Differential Impact by Region