UNIVERSITY OF GOTHENBURG

SCHOOL OF BUSINESS, ECONOMICS AND LAW

Master degree project in Economics

2016

Does primary education affect intimate partner violence against women?

Evidence from Malawi

SARA ABRAHAMSSON

Supervisor: Annika Lindskog

Does primary education affect intimate partner violence against women?

Evidence from Malawi

Author: Sara Abrahamsson
Supervisor: Associate Senior Lecturer, PhD Annika Lindskog
University: University of Gothenburg, Department of Economics

Abstract

This paper studies the causal effect of educational attainment on the experience of intimate partner violence and attitudes toward intimate partner violence in Malawi. Using data from the Demographic Health Survey, this paper takes advantage of the implementation of the Universal Primary education reform in Malawi in 1994 as a natural experiment. Exploiting differences in program exposure by district and age to determine treatment status, this paper uses a difference-in-difference and instrumental variable approach to model the relationship between educational attainment and the experience of and attitudes toward intimate partner violence. The result suggests that women exposed to the Universal Primary Education reform are more likely to justify intimate partner violence and experience sexual violence, and at the same time they are less likely to experience control behavior from their spouse.

Acknowledgment

I would like to thank my supervisor Annika Lindskog for her valuable feedback and support.

JEL classification: J12, D10, O10

Keywords: Intimate partner violence, attitudes, education, Universal Primary Education Reform, natural experiment, Malawi.

Contents

1. Introduction

Violence against women is a serious abuse of human rights and intimate partner violence (IPV) is one of the most common forms of violence against women. Previous studies have found that between 15-71% of ever-partnered women have experienced physical or sexual violence during their lifetime, and women in developing countries are disproportionally affected (Vyas and Watts, 2009). Violence against women is not only a criminal justice problem but also a serious public health problem with substantial social cost and profound effects on women's mental, physical, sexual and reproductive health. Furthermore, violence against women is a barrier towards development because of the significant economic cost arising from health-related expenditure, reduced productivity, lost income for the woman and her family which negatively impacts human capital formation (Duvvury et al, 2013). The magnitude of IPV and the level of acceptance of IPV vary considerable between countries (Garcia-Moreno et al, 2006). Sub-Saharan African countries have among the highest level of violence against women, and at the same time as the share of the population considering violence against women to be justified is considerably high (Duvvury et al, 2013). 52% of women and 29% of men justify IPV in Sub-Saharan Africa (Cools and Kotsadam, 2015).

Previous literature suggests that education is an important socio-economic variable correlated negatively to IPV (Bates et al, 2004; Heise, 2012; Jewkes et al, 2002; Schlozman et al, 1999). Generally, empirical results show that when women's educational attainment increases, especially to secondary level or higher, women are less likely to report IPV (Kishor and Johnson, 2005; Vyas and Watts, 2009). However, results concerning the direction between IPV and education are not conclusive. Some studies report an inverted U-relationship between education and IPV, where the risk of being exposed to IPV increases for women with low education and first decreases after a certain threshold of education is obtained (Cools and Kotsadam, 2015; Eswaran and Malhotra, 2011; Jewkes et al, 2002; McCloskey et al, 2005; Peterman et al, 2015a). Some scholars suggest that when women's education increases compared to their male partner they challenge the male's status as head of the household, and therefore experience a greater risk of IPV (Atkinson et al, 2005; Flake, 2005). Attitudes justifying IPV has been showed to be positively correlated with the experience of actual abuse (Cools and Kotsadam, 2015), but generally, previous research has paid little attention to how education affects attitudes towards IPV.

A number of empirical issues make previous results concerning education and IPV hard to interpret. First of all, earlier studies are often based on cross-sectional survey data, and at times small number of observations, where causality is hard to prove. Secondly, the

difficulties in reaching conclusive result could partly depend on unobserved heterogeneity as a possible source of endogeneity. Since education is closely related to other socio-economic status variables that simultaneously will affect the experience of IPV and attitudes toward IPV a causal relationship is hard to establish. Thirdly, there is an issue of self-selection into marriage that causes the estimate to be biased. Married couples often belong to the same socio-economic group and have similar educational levels (Borjas, 2016). In addition, women and men who experienced violence as a child are more likely to enter a violent relationship as adults (Heise, 2012). Consequently, cross-sectional studies may overestimate the negative or inverse relationship between education and IPV. Moreover, most empirical evidence is from industrialized countries and it is unclear if these findings can be applied to developing countries where the context might differ substantially. In this paper the aim is to empirically investigate variations in educational attainment following a Universal Primary Education (UPE) reform in Malawi 1994 (see appendix A for a short introduction to the UPE reform) and its effect on adult women's experience of IPV and attitudes toward IPV. The UPE reform aimed at increasing the primary school enrollment rate mainly by eliminating school fees (Kattan, 2006). Following the reform the enrollment rate increased by 47.1% for girls and 54.1% for boys between 1994 and 1995 (The World Bank, 2009). Using data from the 2004 and 2010 Demographic Health Survey (DHS) in Malawi, which is a standardized nationally representative cross-sectional household-based survey, I am able to examine the relationship between the risk of experiencing IPV and attitudes toward IPV for adult women. In order to investigate this relationship I will use a difference in difference (DD) and an instrumental variable (IV) strategy. Understanding the relationship between education and IPV will have policy relevant implications and contribute to the understanding of how to fight gendered based violence (GBV).

This study has three major contributions to the existing literature on education and IPV. First of all, this study adds to the existing literature by using both a DD and IV strategy that will facilitate the interpretation of a causal relationship between education and IPV even with cross-sectional data. Secondly, this study adds to the literature by distinguish between different types of violence (physical, sexual, and emotional violence and control behavior) separately since previous empirical literature do not distinguishing between different IPV types. A third contribution of this study is that I will explore the effect educational attainment has on women's attitudes toward IPV, which previous studies has put little focus on.

Taking advantage of the UPE reform as a natural experiment will reduce the problem of selection bias into education. This study focus on primary education, since only fees

connected to primary education was eliminated following the UPE reform. Secondary education is still subjected to a fee in Malawi and because of this a study focusing on secondary education is more likely to suffer from selection bias, as potentially a less vulnerable and more able part of the population is capable of attending secondary school.

My results suggest that the UPE reform had a substantial impact on increasing female education. My baseline results suggest that the UPE reform did increase the likelihood that women justify IPV but there is no effect on the experience of IPV. However, when controlling for district and year of birth fixed effects, results indicate that women exposed to the UPE reform are more likely to justify IPV and experience sexual violence. At the same time, women exposed to the reform have a lower probability of experience control behavior from their spouse when controlling for fixed effects.

The rest of the paper is structured as followed. Section 2 provides a literature review of previous literature in this area. Section 3 reviews existing theoretical models linking education and the risk to experience IPV and attitudes toward IPV, which leads to the hypothesis to be tested. Section 4 describes the data, the variables of interest and presents some descriptive statistics. Section 5 discusses the empirical strategy and some potential robustness checks. The result is presented in section 6. Finally, section 7 discusses and concludes the result from the study.

2. Literature review

A growing body of empirical literature from different fields has tried to explain how education affects women's risk of IPV (Heise, 2012; Vyas and Watts, 2009). In general, existing empirical evidence concerning education and IPV can be categorized into two groups depending on the direction found between education and IPV: (i) there is a monotonous negative relationship between education and IPV, and (ii) the relationship between education and IPV is non-monotonous where education only is protective against IPV after a certain educational level has been reached.

A common pattern among scholars showing a negative relationship between education and IPV is that they consider the absolute level of education rather than the relative distribution of education within the household or in the context women are living in. Kishor and Johnson (2005) present results of a negative and monotonic relationship between education of women and the experience of IPV in Cambodia, Colombia, India, and Nicaragua by using DHS data. However, the result is only descriptive in nature and do not provide any causal interpretation of the effect education has on IPV. Bowlus and Seitz (2006) study the

behavior and determinants of men and women in abusive relationships through an economic bargaining model with data from Canada by estimating a multinomial logit model. The authors show inter alia that ever-married women who have been abused and men who have abused them have less education compared to non-abused women and non-abusive men.

A few studies investigate the relationship between education, IPV and attitudes towards violence and show that with increased education people are less likely to justify IPV (Friedman et al, 2011; Mocan and Cannonier, 2012). Although, these studies do not show that the actual level of abuse decreases with education, Cools and Kotsadam (2015) provides evidence in their study with pooled data from 30 Sub-Saharan African countries, that attitudes justifying IPV is positively correlated with actual abuse.

However, several scholars show empirical evidence of a non-monotonous relationship between education and IPV. To the best of my knowledge there is only one published study, a working paper done by Peterman et al (2015a) that focuses on the relationship between education and IPV in Malawi. In their study they investigate the effect education has on adult women's experiences of IPV (measured as physical and sexual violence) by using the UPE reform in Malawi and Uganda as a natural experiment. The study focuses on women 22-29 years old and the authors adopt an IV strategy to estimate the effect using data from DHS. The result suggests that education is protective in Uganda but is a risk factor in Malawi. Women with no education or incomplete primary education are more likely to experience IPV in Malawi. Education is only protective against IPV for women with secondary or higher education in Malawi. However, Peterman et al's (2015a) result is limited to the extent that they basically compare the average of IPV for those born after the reform with those born before since they do not control for time effects, for instance by including controls for age or year of birth of the respondent in their regressions. In addition, the specific age group the authors consider is at an elevated risk of experiencing IPV since IPV rates among young women has previously been found to be higher (Peterman et al, 2015b). Hence, the result might not be comparable to other age groups. Furthermore, the authors do not account for different types of violence, suggesting that the causal relationship between education and IPV in Malawi is yet to be established.

The evidence of the threshold when education becomes protective is conflicting. Several studies find that education is protective only after secondary education (Eswaran and Malhotra, 2011; Jewkes, 2002; McCloskey et al, 2005; Peterman et al, 2015a). Cools and Kotsadam (2015) have conducted a comprehensive study on resources and IPV, combining data from DHS for 30 countries in Sub-Saharan Africa using a linear probability model, and it

shows that women with elementary and secondary education are at an increased risk of IPV compared to women lacking formal education. Only women with post-secondary education are significantly less likely to experience IPV.

Several studies show empirical evidence that the asymmetry of education within a household is a source of violence since some men consider it unacceptable that women are more productive or have higher education (Flake, 2005; Hornung et al, 1981). Uthman et al (2009) show, through a multilevel logistic regression with data from DHS covering 17 Sub-Saharan African countries, that the context people live in affects the level of IPV. More disadvantaged communities are more likely to justify IPV and the effect is more pronounced among women. Uthman et al (2009) further show that women being more tolerant against IPV are more likely to experience IPV compared to intolerant women, and people with no education or primary education are more likely to justify IPV compared to those with further education.

The somehow conflicting evidence of the effect education has on IPV makes it harder to interpret existing evidence, not to mention the policy implications. Therefore the objective of this study is to improve the knowledge concerning the causal effect education has on the experience of IPV and attitudes toward IPV.

3. Theoretical framework and hypothesis

An evolving body of economic and sociological theories has tried to explain how the level of education affects women's risk of IPV. Yet there is no existing coherent theoretical framework. Theories concerning education and IPV could, similar to the empirical evidence, be categorized according to: (i) those theories predicting a negative and monotonous relationship between IPV and education, and (ii) those theories predicting a non-monotonous relationship between education and IPV.

3.1 Negative and monotonous relationship between education and IPV

The general intuition behind the negative and monotonous relationship between education and IPV is that when women gain more education, in absolute level, her outside options and bargaining power improves.

Classical economic theory explains domestic violence through bargaining models where the use of violence is modeled as a source of utility for the man and disutility for the woman. First, cooperative bargaining models were developed by economists striving to explain the occurrence of violence, both with and without a common set of preferences within the

household (Becker and Ghez, 1975; Manser and Brown, 1980; McElroy and Horney, 1981). However, in households where domestic violence occurs, cooperative bargaining models cannot adequately explain the occurrence of violence as these models treat the spouses as if they behaved altruistic. Cooperative bargaining models do not adequately account for the fact that domestic violence occurs at large costs both for the victim and the society. In addition, if the female have outside options, like welfare options, shelters etc., there will be a threat point determining the level of violence she will tolerate. Farmer and Tiefenthaler (1997) develop a non-cooperative model of domestic violence and show, through an economic game theoretical approach, that when women's economic opportunities improve, violence decreases as a result of better outside options. Anything that raises women's utility outside marriage will improve her bargaining power and threat point, determining how much violence she tolerates before divorce, which consequently will lower the level of violence if she stays in the relationship.

3.2 A non-monotonous relationship between education and IPV

A common pattern among the second branch of theories, arguing for a non-monotonous relationship between education and IPV, is a focus on the relative distribution of education within the household or in the context where violence occurs.

In economic bargaining models where violence is extractive and conditional on the fact that men use violence to increase their utility, IPV increases when the victim's resources increase since there are more resources to extract. This is especially the case for women with low initial bargaining power if the increase in resources is not sufficient for her to leave a violent relationship (Bloch and Rao, 2002; Tauchen et al, 1991). In these models it is the gain in women's relative position within the household that increases violence, and not whether or not the women have more actual resources than the man. This is in line with the 'backlash theory' from sociology, where an increase in women's individual resources, such as education, increases IPV if men compensate the loss in bargaining power by violence (True, 2012).

Several sociological theories have tried to explain the occurrence of violence. One of the most known is the relative resource theory that stresses the asymmetry in men and women's access to resources to be a source of violence (Heise, 2012). In a related theory, status inconsistency theory predicts that women with more resources, as higher education or income, challenge the male's status as head of the household and because of this they are at more risk of experiencing IPV. According to this theory, violence works as a restorative function for male power and the utility of violence will increase if women become

breadwinners in a society that favors male breadwinning (Anderson, 1997). Relative resource theory and status inconsistency theory have been criticized by gendered resource theory, also originated from sociology, for not taking into account gender ideologies and the cultural context. If men hold more egalitarian gender views, women will not be at increased risk of IPV as their resources increase since these men do not feel their status to be threatened by female empowerment (Atkinson et al, 2005).

A new theoretical approach have been proposed by Cools and Kotsadam (2015), two economists, where they try to incorporate both the relative distribution of resources and the contextual setting on a macro level to understand IPV. The theory is an extension of the gendered resource theory, called contextual gendered resource theory. The authors hypothesize that in settings where it is socially accepted to beat women but not socially accepted for women to work, there will be a positive relationship between female employment and IPV, while in settings less tolerant against female abuse employment will reduce violence. The authors stresses the importance of considering the distribution of resources between household members but also the distribution of resources at the macro level, as well as in interaction across these levels. If one apply this to education, the contextual gendered resource theory then suggest that in a cultural context where women ought not to educate themselves, in particular if women's education is not as common and if violence against women is socially accepted, educational increase will be a risk factor. This could explain the contradictions in empirical findings, where increased education in the US (supposedly less tolerant against female abuse) reduces violence, while in Malawi (supposedly more tolerant to female abuse) increased education increase violence.

3.3 Hypothesis

Based upon the above discussion, two hypotheses will be tested. My null hypothesis is that the relationship between education and the experience of IPV and attitudes toward IPV is monotonous. If the relationship between education and violence is monotonous, women exposed to the UPE reform should experience less IPV and attitudes justifying IPV should decline. My alternative hypothesis is that the relationship between education and the experience of IPV and attitudes toward IPV is non-monotonous. If the relationship is non-monotonous women exposed to the UPE reform might experience an increase in IPV and be more likely to justify IPV.

4. Data

The data for this paper comes from the 2004 and 2010 rounds of DHS for Malawi, collected by Inner City Fund (ICF) international. In the 2004 DHS round for Malawi, 13,664 households were interviewed, involving 11,698 female and 3261 male respondents. In the 2010 round, 24,825 households were interviewed, involving 23,020 female and 7,175 male respondents. All women aged 15-59 years and men aged 15-54 years were eligible for interviews. All respondents are asked questions about their attitudes towards IPV and when IPV is justified. In a random subsample of eligible households one woman was randomly selected and asked questions about domestic violence. As such, I will have two different samples in my analysis, hereafter referred to as the "justify IPV sample" and the "domestic violence sample". In the domestic violence sample, married women are asked if their current spouse has exposed them to IPV while formerly married women (divorced or widowed) are asked questions whether their last spouse had exposed them to IPV. Only ever-married women are selected for the domestic violence module, while both never-married and ever-married women are asked questions regarding attitudes toward IPV. Questions regarding violence are classified in modules of physical, emotional, and sexual. In addition, one module ask the women questions about control behavior of the spouse (Measure Demographic Health Survey/Inner City Fund International, 2013). The questions are detailed and multiple for each type of violence, control behavior and attitudes, making them less cultural bound and give multiple alternatives to report violence and attitudes toward violence (Kishor and Johnson, 2005). The response rate for the domestic violence module is 98.64 percent, suggesting that the validity of the data is not adversely affected by non-responses. The response rate for justify IPV is also high, 98.9 percent.

4.1 Outcome variables

According to Kelly and Johnson (2008) it is important to distinguish between different types of violence in order to accurately describe partner violence and its consequences and design more efficient actions against IPV[1]. This paper will consider physical, sexual, and emotional violence, control behavior, and a combination of these (hereafter referred to as "any type of IPV"), and attitudes toward IPV as separate outcomes. The questions regarding physical, sexual and emotional violence and control behavior are designed to investigate whether or not the respondent's spouse has exposed the respondent to violence or control behavior. The six

[1] Previous literature typically combines physical and sexual violence as a measure of IPV (see for instance Cools and Kostadam, 2015; Peterman et al, 2015).

different outcomes are measured by a binary indicator variable (yes/no). If one of the questions in each violence, behavior or attitude module is answered with a "yes", the variable equals one and zero otherwise. For instance, questions regarding physical violence ask the respondent questions whether or not the husband has physically hurt the respondent and questions regarding control behavior ask the respondent questions if the husband has tried to limit the respondents contact with family and friends. Attitudes are measured through asking the respondent different questions when wife beating is justified, for instance if the wife refuses to have sex with him or if she neglects the children. In appendix B I have included all questions for each violence indicator and how the violence indicator has been coded.

4.2 Independent variables

The key independent variable is women's years of schooling measured as a continuous variable.

I include controls for the respondent's year of birth to control for changes over time and a survey dummy since I append two rounds of DHS data together. The year of birth and the survey dummy will capture age effects. I include a control variable for net enrollment rate in 1992/93 by district to control for mean reversion. The data for net enrollment rate in 1992/93 comes from the Malawi Social Indicator Survey (MSIS) 1995 and is merged together with the DHS data (Ministry of Economic Planning and Development, 1996). Further, I include control variables for the number of siblings to control for family background such as differences along respondents with many or few siblings. I use four dummy variables to control for the number of siblings: (1) whether the respondent has 0-2 siblings or not, (2) 3-5 siblings, (3) 6-8 siblings and (4) more than 8 siblings. I control for ethnicity and religion since different ethnic and religious groups may have different opinions about gender norms, have different access to schooling due to political, socioeconomic and geographic reasons. I control for the largest ethnic and religious group by dummy variables, i.e. those groups that more than 10 percent of the sample belongs to. These ethnic groups are Chewa, Lomwe, Yoa, Ngoni and other being the left out-group. Religion is represented by the dummy variables Catholic, Christian and Muslim that are the three major religious groups in Malawi, and other being the left out-group.

The UPE reform has most likely affected several socio-economic outcomes, but since any post UPE reform variables are endogenous to the model I am unable to control for them. It is possible that women with more years of schooling following the reform might have a

higher wealth if increased education has for instance improved labor market opportunities or improved marriage matches.

4.3 Descriptive statistics

Descriptive statistics are reported both for the domestic violence and justify IPV sample, as well as broken down by the two cohorts that will be used in the analysis; those born between 1966-1975 and 1981-1990. The empirical strategy is explained further in section five, but the cohort born 1981-1990 will be assigned "post UPE cohort" status since respondents who were 13 years (born 1981) or younger when the UPE reform was implemented in 1994 should have benefitted from the UPE reform. The respondents born 1966-1975 are the "pre UPE cohort". As shown in table 1, the average length of education is 4.47 years for the domestic violence sample. Those individuals born between 1981-1990 (post UPE cohort) has an average length of 5.22 years of schooling and those individuals born between 1966-1975 (pre UPE cohort) 3.41 years. On average, 45 percent of the respondents have been exposed to any type of IPV, 21 percent to physical violence, 15 percent to sexual violence, 18 percent to emotional violence and 23 percent to control behavior. The violence variables are almost identical along the post UPE cohort and the pre UPE cohort, except small differences.

Table 1. Summary statistics domestic violence sample

	Full sample		Post UPE cohort		Pre UPE cohort	
	Mean	Std dev	Mean	Std dev	Mean	Std dev
IPV	0.45	0.50	0.45	0.50	0.46	0.50
Physical violence	0.21	0.41	0.21	0.41	0.21	0.41
Sexual violence	0.15	0.36	0.15	0.36	0.15	0.36
Emotional violence	0.18	0.39	0.18	0.38	0.19	0.39
Control behavior	0.23	0.42	0.22	0.41	0.24	0.43
Years of education	4.47	3.51	5.22	3.32	3.41	3.49
Year of birth						
Born 1966-1975	0.42	0.49			1	0
Born 1981-1990	0.58	0.49	1	0		
Number of siblings						
Siblings 0-1	0.10	0.31	0.10	0.30	0.11	0.31
Siblings 3-5	0.36	0.48	0.37	0.48	0.35	0.48
Siblings 6-8	0.39	0.49	0.40	0.49	0.38	0.48
Siblings >8	0.15	0.35	0.13	0.34	0.17	0.38
Religion and ethnicity						
Catholic	0.21	0.41	0.21	0.41	0.21	0.41
Christian	0.63	0.48	0.64	0.48	0.63	0.48
Muslim	0.15	0.36	0.14	0.35	0.16	0.36
Other religion	0.01	0.09	0.01	0.09	0.01	0.09
Chewa	0.32	0.47	0.32	0.47	0.32	0.47
Lomwe	0.19	0.39	0.18	0.39	0.19	0.39
Yao	0.14	0.35	0.14	0.35	0.15	0.36
Ngoni	0.11	0.31	0.10	0.31	0.11	0.31
Other religion	0.25	0.43	0.25	0.44	0.23	0.42
Sample size	8.422		4.941		3.481	

The sample size is larger for the justify IPV sample since all respondents in the DHS data are asked questions whether or nor they justify IPV. The justify IPV sample contains 20.473 respondents and the average length of education is 5.2 years, 6.01 years for the post UPE cohort born 1981-1990 and 3.65 years for the pre UPE cohort born 1966-1975. On average, 18 percent justify IPV.

Table 2. Summary statistics for the justify IPV sample

	Full sample		Post UPE cohort		Pre UPE cohort	
	Mean	Std dev	Mean	Std dev	Mean	Std dev
Justify IPV	0.18	0.39	0.19	0.39	0.17	0.38
Years of education	5.18	3.72	6.01	3.52	3.65	3.60
Year of birth						
Born 1966-1975	0.35	0.48			1	0
Born 1981-1990	0.65	0.48	1	0		
Number of siblings						
Siblings 0-1	0.09	0.29	0.09	0.29	0.10	0.29
Siblings 3-5	0.35	0.48	0.36	0.48	0.32	0.47
Siblings 6-8	0.40	0.49	0.41	0.49	0.39	0.49
Siblings >8	0.16	0.37	0.14	0.35	0.19	0.40
Religion and ethnicity						
Catholic	0.21	0.41	0.21	0.41	0.20	0.40
Christian	0.66	0.47	0.66	0.47	0.65	0.48
Muslim	0.13	0.33	0.12	0.33	0.14	0.34
Other religion	0.01	0.09	0.01	0.08	0.01	0.09
Chewa	0.31	0.46	0.31	0.46	0.31	0.46
Lomwe	0.17	0.38	0.17	0.38	0.17	0.38
Yao	0.12	0.33	0.12	0.33	0.13	0.33
Ngoni	0.12	0.33	0.12	0.33	0.12	0.32
Other religion	0.28	0.45	0.28	0.45	0.28	0.45
Sample size	20.473		13.293		7.180	

The descriptive statistics for the main outcome variables are graphically depicted in figure 1-2 in appendix C. In the graphs we see that the post UPE cohort has more years of schooling on average for both samples used and have a lower experience of violence but a higher mean value of justify IPV. Overall, the descriptive statistics suggest that women's experiences of violence and attitudes justifying wife beating are common in Malawi.

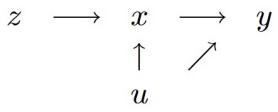
5. Empirical strategy

In order to test the hypotheses and the causal effect the UPE reform had on education, the experience of IPV and attitudes toward IPV, this paper will apply two different empirical strategies taking advantages of the UPE reform as a natural experiment. Both strategies employ the idea that the UPE reform could be treated as an exogenous event that caused discontinuities in educational attainment depending on the individual's year of birth and birth district. By using two strategies I can increase the validity and crosscheck my results. The DD

strategy gives the intention to treat (ITT) effect, since all individuals assigned treatment status will be analyzed, regardless if they actually did "participate" in the reform or not. The advantage with the ITT is that it will avoid overestimating the effect of the UPE reform since the randomization and assignment to treatment will include both compliers and non-compliers (Gupta, 2011). Because of this the ITT will be smaller than the average treatment on the treated (ATT), which measure the average treatment among the treated.

The IV approach provides a way of estimating consistent parameters when a regressor is endogenous. As noted in the literature review, results concerning the direction between education and violence are not clear. Education might be affected by unmeasured individual, household or district characteristics that would bias the result if ordinary least square (OLS) were used. The respondent's year of education is closely related to other socio-economic status variables, like ability and motivation, and districts level of development that will cause the estimates to be biased since these factors might be important determinants for the experience of IPV and attitudes toward IPV.

A valid instrument for years of schooling is an instrument, *z*, that is associated with changes in education but do not cause a change in the outcome variable, *y*, aside from the indirect effect via education as shown in the path diagram:



For *z* to be valid it must be uncorrelated with the error term *u* and correlated with the regressor *x* (years of education). Compared to the DD strategy the IV strategy deals with the problem of non-perfect compliance to primary education. The IV strategy will measure the average causal effect of compliers, which is known as the local average treatment effect (LATE). As such, one should expect the coefficients in the IV model to be larger than the coefficient in the DD strategy.

5.1 Empirical strategy 1: LPM and Probit regression

Before estimating the DD and the IV model I will start my analysis by estimating years of schooling upon the different violence outcomes by linear probability model (LPM) and probit regression. These results might be biased since the model will ignore the possibility that the variable years of schooling might be endogenous or suffer from selection bias. The LPM and the probit baseline equation estimated take the following form:

$$Pr(Violence_{icd} = 1 \mid X) = F(\beta_1 Years\ of\ schooling_{icd} + \boldsymbol{X}'_{icd}\beta_2 + \varepsilon_{icd}) \quad (1)$$

where *Violence* is the different violence outcomes, *i* indexes individuals, *c* indexes cohort and *d* indexes district. $\boldsymbol{X'}$ *is* a vector of predetermined control variables and ε is the error term. In the LPM the function *F* is the identity function and in the probit model *F* represent the probit cumulative density function (CDF) (most often expressed by ϕ instead of *F*).

Since all violence outcome variables are binary they are estimated using LPM and probit model. Marginal effects are presented when the probit model are estimates since the coefficient in a probit model is hard to interpret. It should be remembered that the LPM model has several drawbacks when the dependent variable is binary. It assumes that the error term is normally distributed, which is not the case with a binary outcome variable. It requires the error term to be homoscedastic and LPM might estimate probabilities outside the range of $[0,1]$. However, the advantage with the LPM model is that the coefficient can be interpreted directly and I can compare the result with the probit model.

After including all control variables explained in section 4.1 there might still be unobserved differences at both district level and the year of birth of the respondent. As a robustness check of my baseline equation (1) I control for fixed effect by the inclusion of dummy variables for district and year of birth, both separately and jointly. In the above equation this implies that the term μ_d is included when controlling for district fixed effects and η_c when controlling for year of birth fixed effects. When jointly controlling for district and year of birth fixed effects both μ_d and η_c are included. This gives the equation:

$$Pr(Violence_{icd} = 1 \mid X) = F(\beta_1 Years\ of\ schooling_{icd} + \boldsymbol{X}'_{icd}\beta_2 + \mu_d + \eta_c + \varepsilon_{icd}) \quad (2)$$

This fixed effect model allows me to control for time-invariant and unobserved district and year of birth characteristics. These unobserved district characteristics could be initial differences in the quality of education, distance to schools and the number of schools, and differences in socio-economic development by district. In addition, there could have been other programs running alongside the UPE reform that encouraged girls to enter school that I am unable to control for. Including fixed effect and dummies for year of birth allows me to control for other policies and campaigns that might have taken place at the same time as the UPE reform that encouraged girls to enter school.

5.2 Empirical strategy 2: Difference in difference

After estimating the LPM and probit model I estimate the DD model. The key assumption in a DD strategy is the parallel trend assumption, i.e. that the post-treatment and pre-treatment group follow the same time trend in absence of the treatment (Stock and Watson, 2011).

The UPE reform could be treated as an exogenous event that caused discontinuities in educational attainment depending on the individual's year of birth. As a first difference I will use the individual's year of birth as a determinant of the exposure to the UPE reform. Primary school starts at the age of six for children in Malawi. Since entry was allowed at any primary grade level when the reform was implemented in 1994 those born no later then 1981 should have been exposed to the reform. Individuals born between 1981-1990 will be assigned post UPE cohort status.

A substantial proportion of children do not start school at the recommended age and both overage and underage enrollment is common in Malawi. Overage and underage enrollment varies by district and region but on average only 51 percent of all 6 years olds in Malawi entered primary school at the correct age in 1992/93. The Northern region have a tradition of more schooling and have a higher proportion of children who enter at the correct age, 57 percent, compared to 50.6 percent in the Central region and 50.4 percent in the Southern region (Ministry of Economic Planning and Development, 1996). Because of the problem of overage enrollment the reform might have affected individuals born before 1981. Instead of having to assume that all individuals are in the right grade according to their age I use those individuals born between 1966-1975 as a control group since this cohort is less vulnerable to overage enrollment. A similar strategy has been implemented in Osili and Long's study (2008) concerning schooling and fertility in Nigeria and by Mocan and Cannonier (2012) in their study investigating increased schooling and female empowerment. The first difference will be measured by a dummy variable called "UPE cohort", which is equal to one if the individual belongs to the post UPE cohort born between 1981-1990 and zero if the individual belongs to the pre UPE cohort born 1966-1975.

The second difference will depend on the individual's place of residence. As noted, the UPE had a substantial impact on the net enrolment rate for both girls and boys. Before the reform there were significant differences in net enrollment rate among the 24 districts in Malawi. Many districts had a net enrolment rate below 60 percent in 1992/93 (see table 11 appendix D). The probability and the intensity of treatment thus varied the across the districts. Those individuals born in a district with an initial low net enrollment rate had a higher probability of benefiting from the UPE reform compared to districts where the net enrollment

rate already was high and the probability for improvement in enrollment was lower. As showed in table 11 in appendix D, the percentage point increase in net enrollment rate between 1992/93 and 1995 is much higher for those districts with an initial low net enrollment rate. A continuous variable called "NE increase" (net enrollment increase in percentage point) will measure the second difference[2]. This variable is measured from 0.00 to 1.00 as seen in table 11 when divided by 100. Birth year and state of residence thus jointly determine an individual's treatment status.

The second difference requires the assumption that the woman has been educated in the same district as she has been interviewed in. The DHS data for Malawi has very limited information on migration and unfortunately few previous studies have detailed information of internal migration patterns in Malawi. However, internal migration is more common among men than women in Malawi, and often short-term migration within the same district is more common than long-term (Anglewicz, 2012). The following equation could describe the baseline DD model utilized:

$$S_{icd} = \alpha_0 + \alpha_1 UPE\ cohort_c + \alpha_2 NE\ increase_d + \alpha_3 (UPE\ Cohort_c \times NE\ increase_d) + X'_{icd}\alpha_4 + \varepsilon_{icd} \quad (3)$$

where S is years of schooling for respondent i, belonging to cohort c and born in district d. ε is the error term representing unobserved factors affecting educational attainment. The interaction term is the reduced form of the UPE reform. I expect that the interaction term should be positive since the increase in schooling was larger in district that had an initial lower net enrollment rate. The variable "years of schooling" is a continuous variable and estimated through OLS. $\boldsymbol{X'}$ is a vector of the predetermined control variables explained in section 4.1.

Given the parallel trend assumption, a DD strategy will causally estimate the effect the UPE reform had on years of schooling by comparing the treatment and the control group. I use the same model to measure how the UPE reform has affected women's experiences of and attitudes toward IPV by replacing *S,* years of schooling, by the six different violence outcome variables explained in section 4.1:

[2] Another option would be to use poverty index by district as a second difference where poor districts could be argued to benefit more from the fee removal and thus having a higher potential for benefitting from the fee removal. I have tried this strategy but the results did not show an impact on years of schooling and because of this I am unable to use this strategy. Another potential DD strategy that I have tried is to look at federal funding per district following the reform to determine intensity of the reform. Unfortunately, this data is only available at regional level and since there are only three regions in Malawi this allows for small variations and the result did not show any effect on years of schooling.

$$\Pr(Violence_{icd} = 1 \mid X) = F(\alpha_1 UPE\ cohort_c + \alpha_2 NE\ increase_d + \alpha_3(UPE\ Cohort_c \times NE\ increase_d) + \boldsymbol{X}'_{icd}\alpha_4 + \varepsilon_{icd}) \quad (4)$$

where *Violence* is the different violence outcome variables for respondent *i*, who belong to cohort *c* and are born in district *d*. ε is the error term. *F* represents the identity function in the LPM and the probit CDF for the probit model. If there is a difference in the experience of violence between women born no later than 1981 in districts with high potential for increase in schooling then this difference could be said to be caused by the effect the UPE reform had on years of schooling. I will control for fixed effect through including the variables μ_d for district fixed effect and η_c for year of birth fixed effect in equation (3) and (4).

I have estimated all violence outcome variables using both LPM and probit. Marginal effects are presented for the probit regression results. However, it is not obvious how marginal effects are to be calculated in non-linear models where the interaction term is the parameter of interest. An article done by Ai and Norton (2003) has received a great deal of attention because the authors argue that the interpretation of interaction terms change in non-linear models. According to Ai and Norton (2003) the interaction term in non-linear model requires the cross-derivatives to be calculated before the coefficient can be evaluated. However, when calculating marginal effects in Stata they are calculated for any of the dependent variables, leading to wrong interpretation of the interaction term according to Ai and North (2003). Puhani (2012) instead argue and show that the treatment effect is given by the difference in two cross differences in a non-linear DD model, which is the incremental effect of the interaction term given by Stata when calculating marginal effects. I will follow Puhani (2012) advice and interpret the marginal effect of the interaction term given by Stata. In addition, when controlling for fixed effect it is not possible to calculate marginal effects according to Ai and Norton (2003) advice since the variable for the first difference, UPE cohort, and the variable for the second difference, NE increase, is omitted when controlling for fixed effects.

5.3 Empirical strategy 3: Instrumental variable analysis

Assuming that the UPE reform had no direct effect on the experience of or attitudes toward IPV, expect the indirect effect it had on educational attainment, I can use the interaction term from the DD analysis as an instrument if it has a positive and significant impact on years of schooling. The baseline IV equation could be described as follows:

$$S_{icd} = \pi_0 + \pi_1 UPE\ Cohort_c \times NE\ increase_d + \boldsymbol{X}'_{icd}\pi_2 + v_{icd} \quad (5)$$

$$\Pr(Violence_{icd} = 1 \mid X) = F(\alpha_1 S_{icd} + \boldsymbol{X}'_{icd}\alpha_2 + \varepsilon_{icd}) \quad (6)$$

where S_{icd} is a measure for years of schooling for individual *i,* belonging to birth cohort *c* and born in district. v is the error term in the first stage ε is the error term in the second stage regression. The first stage equation (5) is instrumented by the interaction term from the DD analysis in equation (6). The IV model will be estimated through both ordinary two-stage least square regression (2sls) and ivprobit. In equation (6) *F* represents the identity function for the linear model and the probit CDF for the ivprobit model. The drawback with 2sls regression is similar to the LPM model when the outcome variable is binary since it can estimate probabilities outside the rage of $[0,1]$. Yet, the advantage is that the coefficients can be interpreted directly and I can compare the result to the ivprobit model. In addition, the linear model provides F-statistics regarding the strength of the instrument. If the F-statistics is larger than 10 the instrument could be considered valid (Stock and Watson, 2011). Marginal effects are presented when using the ivprobit model. Similar to the fixed effect equation explained in section 5.1 I will control for district and year of birth fixed effect in the IV model as well.

Standard errors will be clustered at district level in all models. District clustered standard errors assume independence across districts but allows for any type of correlation within the district. Similar to some former studies using DHS data (Durevall and Lindskog, 2015a; Harling et al, 2010) I do not use weights in my regressions since effects are unclear when subsamples or when data from two rounds are appended together. If weights were to be used they must be adjusted after the number of people living in Malawi and after the correct number of ever-married women (since only ever-married women are selected for interviews in the domestic violence sample) the year the survey was conducted. Since I lack data on the number of ever-married women in Malawi for the year of each survey it is not possible to adjust the weights correctly. I have used Stata version 14.0 for all my analyses.

5.4 Pathways outcome

Similar to Behrman (2015a) I will check for possible pathways through which education might have impacted IPV as a second analysis. This will further provide an opportunity to consider the theories outlined in section 3 further. I will use the same DD and IV strategies explained earlier but substitute the violence outcome variables with the pathway variables. This additional analysis provides a first step into understanding how schooling could have

affected violence. However, as noted by Behrman (2015a) this kind of analysis has several limitations. There are many pathways I am unable to control for due to data limitations, and this pathway analysis could not directly link the pathway variables to the experience or attitudes toward IPV but only link the pathways variables to educational attainment.

Heise (2012) provides a comprehensive study on individual level factors associated with increased risk of IPV, such as alcohol consumption, exposure to violence as a child, outside sexual partners, norms around male dominance, gender inequality, overall level of socio-economic development, economic inequality, and women's access to formal wage employment. The pathway variables I use will be related to factors that Heise (2012) has identified as risk factors for women's experience of IPV and attitudes toward IPV.

The first pathway, hereafter referred to as "sexual behavior", explores the effect schooling has on sexual behavior. I test Heise (2012) finding that risky sexual behavior is linked to IPV through investigating the following variables: (1) a binary variable indicating whether or not the respondent had outside sexual partners during the last 12 months or not, (2) a continuous variable for lifetime sexual partners, and (3) a continuous variable for age at sexual debut[3].

Heise (2012) hypothesizes that early marriage and bad marriage matches are a risk factor since it serves as a marker for society with stringent gender norms. I test the second pathway "marriage matches", through: (1) a binary variable indicating whether or not the respondent is married[4], (2) a continuous variable for the age at first marriage, and (3) a binary variable indicating whether or not the spouse is considerably older (≥ 10 years) than the respondent.

The third pathway, "partner characteristics", explores the effect of schooling on partner characteristics through: (1) a continuous variable of spouse years of schooling, (2) a binary variable indicating whether or not the respondent completed more years of schooling than current spouse, and (3) a binary variable indicating whether or not the spouse drinks alcohol or not. Heise (2012) has identified alcohol consumption among spouses as a risk factor for IPV.

Better female bargaining power could serve a proxy for improved gender-related norms. The effect schooling had on bargaining power of the respondent will be investigated through pathway four, "bargaining power", through: (1) a binary variable indicating whether the

[3] Only respondents who had their sexual debut are analyzed.
[4] Only ever-married women are interviewed in the domestic violence sample. 88 % are married and 12 % are widowed or divorced. Those women being widowed or divorced are counted as having a marital status equal to zero.

respondent works for cash or not, (2) a binary variable indicating whether or not the respondent earns more than the spouse, and (3) a binary indicating whether the respondent participates in household decisions (either alone or together with the husband) or not.

The adult socio-economic status, which is based on a survey specific relative wealth index of the household cumulative living standard calculated from the DHS survey, is the fifth pathway, "socioeconomic status", and is explored through: (1) a binary variable indicating whether or not the respondent belongs to a poor socio-economic group, (2) a binary variable for indicating whether or not the respondent belongs to a medium socio-economic group, and (3) a binary variable indicating whether or not the respondent belongs to a rich socio-economic group. Women belonging to a lower socio-economic group are more likely to both experience more violence and justify wife beating (Heise, 2012; Jewkes, 2002).

The last pathway, "mass media", investigates Uthman et al's (2009) finding that women having access to mass media are less likely to accept violence through (1) a binary variable indicating whether or not the respondent reads newspapers, (2) a binary variable indicating whether or not the respondent listens to the radio, (3) a binary variable indicating whether or not the respondent watches TV.

Peer group effects and social networks most likely influence both the experience of and attitudes toward IPV. Unfortunately, the DHS do not include any variables that could be used to control for these types of social effects.

5.5 Robustness

The most preferred robustness check is to compare two cohorts that have not been affected by the UPE reform. This type of robustness check will allow me to control that the individuals born between 1981-1990 increased years of schooling is not due to any other factors not caused by the UPE reform, for instance that years of schooling already was increasing faster in districts that had a high net enrollment increase between 1992/93-1995 prior to the UPE reform.

Results could be sensitive to the choice of strategy and choice of model. As a robustness check I use an alternative strategy to measure exposure to the UPE reform by district to check my main results. I use a dummy variable strategy to classify those districts in 1992/93 that has a high potential for net enrollment increase called "High potential districts". This dummy variable could then be used as a second difference in the DD analysis. The potential for treatment by geographical region has been used before in for instance Duflo (2001). I classify those districts with less than 60 percent in net enrollment rate in 1992/93 as districts with high

potential for large net enrollment rate increase. I choose 60 percent since this appears as a natural cutoff, since most districts either already had a net enrollment rate over 75 percent or below 60 percent, see table 11 in appendix D.

Another plausible strategy that could be used as a robustness check would be a fuzzy regression discontinuity (RD) approach, where individuals born 1981 and later are assigned treatment status and those born prior to 1981 would act as control group to estimate the LATE. This strategy has been used in for instance Behrman (2015a) and Behrman (2015b) to estimate the effect of the UPE reform in Malawi on adult women's fertility decision and HIV status respectively. I have tried estimating a fuzzy RD with different bandwidths but since the first stage is not significant the results are not presented in this paper[5].

5.6 Limitations

A limitation of a natural experiment at micro level is that the result will measure the impact of education following a particular educational policy in a specific setting and population. The internal validity could thus be considered relatively high. Since similar UPE policies have been implemented in several developing countries this increases the external validity of this study as well. At the same time, the result could act as a compliment to existing non-experimental results.

6. Results: The impact of the UPE reform

6.1 Results using LPM and probit model

I start by examining the impact the UPE reform had on female education through the LPM and probit model with years of schooling as an independent variable.

Table 12-17 in appendix E reports the LPM and probit estimation results. The estimations are limited to women born between 1966-1975 and 1981-1990. Since years of education is used as an independent variable these regression result do not explicitly consider the reform, but utilize all variations in years of schooling, including the variation that depends on individual heterogeneity as well. The result suggests that a one-year increase in education decreases the probability for adult women to justify IPV by 0.1 percentage point when using the probit model and controlling for fixed effects and the LPM model shows a similar result. According to both the LPM and the probit baseline results for the year of birth fixed effect

[5] When I tried estimating a fuzzy RD model my first stage regression is the same as Behrman (2015b) use except from the fact that Behrman (2014) do not include control for time changes in her RD model, which limits the credibility of her strategy. If I remove my controls for time changes in my regression, my first stage results show a significant impact on years of schooling but since this is a not credible strategy the results are not included in this paper.

model, a one-year increase in schooling decreases the probability for adult women to experience control behavior by about 3 percentage points. None of the other outcome variables are significant.

6.2 Results using difference-in-difference analysis

Next, I examine the impact the UPE reform had on female education by the DD strategy outlined in section 5.2. As explained, the sample is limited to compare the cohorts born between 1966-1975, aged 19-28 when the UPE reform was initiated, and the cohort born 1981-1990, aged 4-13 when the UPE reform started. Those respondents aged 4-13 years old should have benefited most from the reform. Since the reform allowed entry at any grade level at primary level the cohorts born 1981-1990 should have at maximum been exposed to the UPE reform for eight years as primary education lasts for eight years in Malawi (between 6-13 years) (The World Bank, 2010). A DD strategy is a type of quasi-experiment, where year of birth, the first difference, is used as a cutoff mark to determine whether or not the respondent belongs to the post UPE cohort or the pre UPE cohort. Since one might expect that districts with a low initial net enrollment rate should have a higher expected growth in net enrollment rate following the reform the second difference depending on the individuals place of residence might seem non-random. In order to deal with the problem of mean reversion I do control for net enrollment rate in 1992/93 by district in all my regressions.

Table 3 shows the DD result of the effect the UPE reform had on years of schooling for my domestic violence sample.

Table 3. Years of education for domestic violence sample – DD analysis

	(1) OLS Baseline	(2) OLS District FE	(3) OLS Year of birth FE	(4) OLS District & year of birth FE
Born 1981-1990*NE increase	1.038**	1.067**	0.997*	0.964*
	(0.493)	(0.479)	(0.494)	(0.484)
Born 1981-1990 dummy	1.052***	1.027***		
	(0.252)	(0.256)		
NE increase by district dummy	6.470***		6.489***	
	(1.855)		(1.816)	
Constant	-66.60*	-64.55*	-5.859***	1.261**
	(35.01)	(34.38)	(1.337)	(0.458)
Observations	8,403	8,403	8,403	8,403
R-squared	0.163	0.193	0.177	0.197

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

As shown in table 3 the interaction term is positive and statistically significant and indicates that the UPE reform had a sizeable impact on years of schooling by increasing education for women born 1981-1990. This implies that the UPE reform increased years of education by 1.04 years for women born 1981-1990 living in a district where net enrollment rate increased from 0.00 to 1.00 (i.e. a 100 percent increase). This is in addition to the extra 1.05 years gained by all women in the post UPE cohort as indicated by the "Born 1981-1990 dummy" coefficient. Given that the average net enrollment rate went from about 65 percent to 84 percent between 1992/93 and 1995. Treatment intensity varies across district (from lowest to highest), but in a district with the average increase, years of education increased by 0.2 years for women born 1981-1990. This representing a 6 percent increase in years of schooling compared to the average education years of education for the post UPE cohort. The result is robust against the inclusion of district and year of birth dummies and the coefficient of the interaction term differ only marginally. Column 4 includes both year of birth and district fixed effect and is my preferred estimate. According to this result the UPE reform increased years of education by 0.96 years for women born 1981-1990 living in a district where net enrollment rate increased from 0.00 to 1.00. This represents a 5 percent increase in schooling compared to the average years of education for the post UPE cohort and a 0.18 years increase in education following the UPE reform for women age 4-13 in 1994. Since the average years of schooling is low in Malawi, 4.47 years for the domestic violence sample, and 3.41 for the pre UPE cohort, this suggests that the UPE reform contributed to a sizeable increase in schooling. However, the estimates of the increase in schooling are averages across the sample and do not consider the fact that the effect most likely is larger among women belonging to low socio-economic status. Since the socio-economic status for the respondent is in itself an outcome it cannot be included in the regression without causing endogeneity.

Table 4 shows results for the justify IPV sample. For this sample the UPE reform contributed to a gain in schooling by 1.37 years of schooling for women born 1981-1990 living in a district where net enrollment rate increased from 0.00 to 1.00 when controlling for both year of birth and district fixed effects. This is in addition to the 0.9 years of schooling gained by all women in the post UPE cohort as indicated by the "Born 1981-1990 dummy" coefficient. This represents a 0.26 gain in years of schooling following the UPE reform, given that the average net enrollment rate went from 65 percent to 84 percent. This is a 7 percent increase in years of schooling for women age 4-13 in 1994 compared to women age 19-28 in 1994. This suggests that the UPE reform had a sizeable and statistically significant impact on years of schooling when the sample size increases as well.

Table 4. Years of education for justify IPV sample – DD analysis

	(1) OLS Baseline	(2) OLS District FE	(3) OLS Year of birth FE	(4) OLS District & year of birth FE
Born 1981-1990*NE increase	1.534***	1.370***	1.526***	1.365***
	(0.471)	(0.451)	(0.465)	(0.447)
Born 1981-1990 dummy	0.928***	0.932***		
	(0.241)	(0.243)		
NE increase by district dummy	6.232***		6.240***	
	(2.148)		(2.149)	
Constant	-144.7***	-139.5***	-6.186***	1.422***
	(28.10)	(28.11)	(1.650)	(0.377)
Observations	20,445	20,445	20,445	20,445
R-squared	0.186	0.217	0.187	0.219

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Next, I examine the effect the UPE reform had on the six different violence outcome variables explained in section 4.1 using the same specification as for years of schooling. The only difference is that I use a LPM and probit model since the violence variables are binary.

The only outcome variable showing significant results across all probit specification is "justifying IPV". I found that for women born 1981-1990 living in a district where net enrollment rate increased from 0.00 to 1.00 the UPE reform increased the probability that adult women justify IPV by about 7 percentages points as displayed in table 5. In relation to the average net enrollment in 1992/93 and 1995 the UPE reform caused a 1.4 percentage points increase in justify IPV for women born 1981-1990, given that the average net enrollment rate went from 65 percent to 84 percent. The result is robust for the inclusion of district and birth year dummies. This result is contrary to Friedman et al's (2010) and Mocan and Cannonier's (2012) findings where attitudes justifying IPV decrease when educational attainment increases. Since this result is contrary to previous result I have checked if a specific question among the five "justify IPV" questions (see appendix B) are driving the result. When dividing the five "justify IPV" questions into separate outcomes the result suggest that the "refuse sex" and "burns the food" questions are driving the result[6]. The other questions are insignificant. Men's right to sex is often seen as a normal part of marriage in countries such as Malawi (Heise, 2012). In addition, Malawi is one of the world poorest countries and in a context where poverty is widespread burning the food might be considered as quite severe.

[6] Results available upon request.

The LPM results show no significant impact but the magnitude of the coefficient is similar to the coefficients in the probit model.

Table 5. Justify IPV – DD analysis

	(1) LPM Baseline	(2) LPM District FE	(3) LPM Year of birth FE	(4) LPM District & year of birth FE	(5) Probit Baseline	(6) Probit District FE	(7) Probit Year of birth FE	(8) Probit District & year of birth FE
Born 1981-1990*NE increase	0.0663 (0.0392)	0.0652 (0.0404)	0.0652 (0.0399)	0.0644 (0.0411)	0.0737** (0.0376)	0.0720* (0.0388)	0.0721* (0.0386)	0.0705* (0.0394)
Born 1981-1990 dummy	-0.0267 (0.0160)	-0.0225 (0.0157)			-0.0284* (0.0147)	-0.0260* (0.0148)		
NE increase by district dummy	-0.231 (0.158)		-0.230 (0.158)		-0.224 (0.160)		-0.223 (0.160)	
Constant	-4.489*** (1.425)	-3.999** (1.432)	0.242* (0.130)	0.244*** (0.0430)				
Observations	20,445	20,445	20,445	20,445	20,445	20,445	20,445	20,445
R-squared	0.084	0.105	0.085	0.106				
Pseudo R^2					0.087	0.109	0.088	0.110

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Sexual violence shows a statistically significant result when controlling for fixed effects as shown in table 6, but is not significant in the probit baseline model or in the linear result. As shown in table 6, column 8, for women born 1981-1990 living in a district where net enrollment rate increased from 0.00 to 1.00 the UPE reform increased the probability that adult women experienced sexual violence by 6.4 percentage points. Given that the average net enrollment rate went from 65 percent to 84 percent, this is a 1.2 percentage points increase in the experience of sexual violence for women born 1981-1990 caused by the UPE reform. The linear model shows no effect but similar size of the coefficients.

Table 6. Sexual violence – DD analysis

	(1) LPM Baseline	(2) LPM District FE	(3) LPM Year of birth FE	(4) LPM District & year of birth FE	(5) Probit Baseline	(6) Probit District FE	(7) Probit Year of birth FE	(8) Probit District & year of birth FE
Born 1981-1990*NE increase	0.0575 (0.0428)	0.0544 (0.0419)	0.0617 (0.0419)	0.0585 (0.0410)	0.0586 (0.0404)	0.0559 (0.0386)	0.0668* (0.0393)	0.0638* (0.0371)
Born 1981-1990 dummy	0.000205 (0.0278)	0.00130 (0.0277)			-0.0001 (0.0265)	0.000503 (0.0258)		
NE increase by district dummy	-0.0202 (0.154)		-0.0217 (0.153)		-0.0142 (0.147)		-0.0191 (0.147)	
Constant	2.087 (3.483)	2.128 (3.387)	2.658 (3.067)	2.742 (2.966)				
Observations	8,434	8,434	8,434	8,434	8,434	8,434	8,434	8,434
R-squared	0.015	0.030	0.017	0.032				
Pseudo R^2					0.018	0.035	0.021	0.038

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

At the same time the UPE reform decreased the probability for women to experience control behavior as adults by almost 8 percentage points, as displayed in table 7 when controlling for district and year of birth fixed effect when net enrollment rate increased from 0.00 to 1.00 for women born 1981-1990. Given that the average net enrollment rate went from 65 percent to 84 percent, this is a 1.5 percentage points decrease in the experience of control behavior for women born 1981-1990 caused by the UPE reform. The linear model predicts the same effect.

Table 7. Control behavior – DD analysis

	(1) LPM Baseline	(2) LPM District FE	(3) LPM Year of birth FE	(4) LPM District & year of birth FE	(5) Probit Baseline	(6) Probit District FE	(7) Probit Year of birth FE	(8) Probit District & year of birth FE
Born 1981-1990*NE increase	-0.0616	-0.0712*	-0.0618	-0.0706*	-0.0659	-0.0779**	-0.0666	-0.0783*
	(0.0417)	(0.0388)	(0.0427)	(0.0399)	(0.0414)	(0.0396)	(0.0418)	(0.0400)
Born 1981-1990 dummy	0.0640**	0.0670***			0.0664***	0.0699***		
	(0.0242)	(0.0234)			(0.0235)	(0.0226)		
Net enrollment increase by district	-0.134		-0.132		-0.126		-0.124	
	(0.202)		(0.204)		(0.195)		(0.196)	
Constant	7.106*	6.994**	2.202	2.142				
	(3.462)	(3.378)	(3.355)	(3.306)				
Observations	8,411	8,411	8,411	8,411	8,411	8,411	8,411	8,411
R-squared	0.035	0.052	0.038	0.055				
Pseudo R^2					0.033	0.049	0.037	0.053

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Both sexual violence and control behavior only turn significant when controlling for fixed effects. This suggests that there potentially could be omitted variable bias in the form of unobserved characteristics across district and year of birth that the fixed effect model eliminates. It might seem counterintuitive that the UPE reform should have increased the probability of women justifying IPV and experience sexual violence at the same time as they experience less control behavior. One possible explanation is that these two different violence forms are *different* and that women are empowered by schooling to the extent that they experience less control behavior at the same time as men compensate the loss of bargaining power following an increase in female education by the use of another type of violence, i.e. sexual violence.

I find no statistically significant effect on the other outcome variables: any type of IPV, physical violence, and emotional violence (see appendix F table 18-20). The results using the DD strategy suggest that the UPE reform increased the probability that adult women justify IPV and experience sexual violence at the same time as the reform decreased the probability for women born 1981-1990 to experience control behavior from their spouse. Compared to the LPM and probit estimation in section 6.1 the DD analysis estimates coefficients that are opposite in their direction, most likely because of the endogeneity problem with years of schooling in the LPM and probit model.

6.3 Possible pathways using DD analysis

As a second analysis I explore the pathways outlined in section 5.4 through which schooling might have affected the results given the DD analysis. I use the same DD strategy as above, controlling for district and year of birth, separately and simultaneously. The summary statistics for the pathway variables are available in appendix G, table 21-22, and is divided between the domestic violence sample and justify IPV sample. I only provide tables showing significant results for the different pathways. The number of observations varies between 1,134-8,587 for the pathway variables in the domestic violence sample, and between 3,936-20,693 in the justify IPV sample because of missing observations.

For the justify IPV sample, I found evidence for women's sexual behavior, partner characteristics and mass media, as displayed in table 23-26 in appendix G. The UPE reform have increased the number of outside partners women had the last 12 months, which is evidence in favor of a risky sexual behavior. Previous studies have found that a risky sexual behavior is more common among women who have experienced IPV (Andersson et al, 2007; Durevall and Lindskog, 2015b). The experience of IPV has previously been linked to a higher probability to justify IPV (Cools and Kotsadam, 2015)[7]. A risky sexual behavior might also trigger attitudes justifying IPV. Though causality is unclear, if violence is used as an instrument to control women, one could expect that risky sexual behavior should decrease with violence. However, it is possible that women with higher education are more likely to travel and work more (not necessarily wage labor), which further could increase the number of outside partners because of more interacting with other men. Another possibility is that attitudes justifying IPV and risky sex are due to lack of self-esteem.

At the same time the pathway results suggest that the UPE reform has decreased the total lifetime sexual partners for women in the justify IPV sample, which is evidence against a risky sexual behavior. One possible explanation is that when girls are kept in school, they tend to have fewer sexual partners. However, these two results for risky sexual behavior are counterintuitive. The increase in outside partners could be linked to risky sexual behavior and the increase in women justifying IPV in accordance with previous studies and to the backlash theory explained in section 3.2. One possible explanation for this counterintuitive finding is peer effects. The UPE reform caused a large inflow of students following the abolition of school fees, and many students were most likely from lower socio-economic groups. The acceptance of IPV has previously been showed to be larger among poorer socio-economic groups (Heise, 2012). This could have caused the "quality" of the pool of peers to decrease,

[7] Justify IPV are stronger among women who have experienced violence in my sample as well. Results provided upon request.

causing the UPE reform to increase women's risky sexual behavior by increasing the number of outside sexual partners at the same time as the increase in schooling contributes to a decline in the total lifetime partners when girls are kept longer in school. However, it is not possible to test peer effects with the data I have and it is also possible that peer effects did not decline at average for compliers. The quality of the pool of peers might have declined for compliers who had access to primary education even without the fee removal, possible because these individuals belong to a higher socio-economic group, when these individuals became mixed with students from lower socio-economic groups who benefitted from the fee removal. However, the *average* quality of the pool might have increased for compliers after the fee removal since many students from lower socio-economic groups were mixed with students from higher socio-economic groups, which on average might have caused a quality improvement in the pool of peers.

The result for the pathway partner characteristics and mass media is counterintuitive with the result that women exposed to the UPE reform are more likely to justify IPV since the result indicates that the UPE reform has increased women's bargaining power, and increased women's access to mass media through TV. However, when controlling for fixed effect women's access to mass media through TV is no longer significant.

For the domestic violence sample I found evidence only in favor of sexual behavior and the bargaining power pathway, as displayed in table 27-28 in appendix G. The result suggests that the UPE reform has delayed age at first sex for the domestic violence sample. Prolonged education delay girls' age at first sex seems logical. This is evidence in favor of a less risky sexual behavior, which could be linked to the decrease in the experience of control behavior for women exposed to the UPE reform. However, the result is not significant when controlling for district and year of birth fixed effects.

In addition, women exposed to the UPE reform are more likely to earn more money than their spouse and this could explain why women experience more sexual violence. This is in line with status inconsistency theory and contextual gendered resource theory where women with higher income challenge male's status as head of household and because of this experience more violence. According to the contextual gendered resource theory this effect will be particularly pronounced in a context where it is both socially accepted to use violence at the same time as the prevailing norms and values consider women's wage labor as non accepted. However, the variable whether or not the respondent earns more than the spouse contains rather few observations compared to my other estimates, 1,133 observations. Hence, this result should be interpreted with a degree of caution.

None of the other pathways were significant, neither in the baseline model nor when controlling for fixed effects.

6.4 Robustness check using DD analysis

As a first robustness check I check whether or not other cohorts benefitted from the UPE reform. If the UPE reform actually caused an increase in years of schooling there should be a smaller or no effect on years of schooling for cohorts born before the UPE reform was initiated. I compare the cohort born between 1956-1965 and the cohort born 1966-1975, which both should have been unaffected by the UPE reform. I conduct this type of robustness check for the district and year of birth fixed effect model. Since I lack data on net enrollment rate prior to 1992/93 I cannot conduct this robustness check for my baseline model since this is one of my control variables. However, when controlling for district fixed effects this variable is omitted from the analysis due to collinearity. The result is displayed in table 29 in appendix H and none of the coefficients show a significant result (except the coefficient for years of schooling for the justify IPV sample who is negative). This is evidence in favor of the result that the UPE reform actually caused an increase in years of schooling for respondent aged 4-13 when the UPE reform was initiated, and that the increase in schooling actually caused women to be more likely to justify IPV, experience sexual violence and experience less control behavior.

As a second robustness check I use an alternative measure of district treatment by utilizing a dummy variable strategy for districts with high potential for net enrollment rate increase as explained in section 5.5. The results are displayed in appendix H, table 30-37. Even if a dummy variable allows for much less variation compared to when a continuous variable is used to identify the second difference the result still shows that the UPE reform has increased years of education for women born 1981-1990 in districts with a high potential for net enrollment rate increase, both for the domestic violence sample and the justify IPV sample. For the domestic violence the UPE reform increased years of education by 0.36 years, representing a 0.1 standard deviation increase, and for the justify IPV sample by 0.48 years, representing a 0.21 standard deviation increase when controlling for district and year of birth fixed effects as displayed in table 30-31. Furthermore, the result still suggests that the UPE reform increased the probability that women justify IPV by about 3 percentage points as displayed in table 32. Since this strategy shows a smaller effect of the UPE reform on years of schooling one would expect the coefficients to be smaller for "justify IPV" as well. Sexual violence and control behavior are no longer significant, most likely due to the smaller effect

on the interaction term when assigning a threshold value to the second difference as shown in table 35 and 37 in appendix H. According to table 33-34 and 36 in appendix H the UPE reform did not impact women's experiences of any type of IPV, physical and emotional violence, which is consistent with the DD analysis using a continuous variable.

6.5 Results using instrumental variable analysis

In this section I will examine the impact the UPE reform had on the different violence outcome variables through the IV model. In table 3 and 4 we saw that the interaction term in the DD analysis had a statistically significant impact on educational attainment. I will use the interaction term as an instrument for years of schooling. Similar to the previous regression, all IV regressions include control variables for year of birth of the respondent, a survey dummy, net enrollment rate in 1992/93 by district, number of siblings, religion and ethnicity.

The IV regression result suggests that a one-year increase in schooling increases the probability that females justify IPV by 16.9 percentage points when controlling for district and year of birth fixed effects, as shown in table 8. A one-year increase in years of schooling increases the probability that women will experience sexual violence by 21.2 percentage points and reduces the likelihood of the experience of control behavior of their spouse by 20.9 percentage points as displayed in table 9-10 respectively. This result suggest that the coefficients for years of schooling in the LPM and the probit estimation in table 12-17 in appendix E is estimated with the wrong sign on the coefficients the effect educational attainment has on attitudes toward violence and the experience of control behavior. However, these coefficients are large, to large to be credible, and the F-statistics from the linear regression suggest that when including district and year of birth fixed effects the power of the instrument is reduced and is precisely below 10 for the justify IPV estimates and below 5 for both the sexual violence and control behavior estimates. This could indicate that the coefficient is imprecisely estimated and may overestimate the impact education has on attitudes toward violence and the experience of violence. Even though these coefficients are significant, robust conclusions could then be hard to draw. The F-statistics for the baseline model, the district fixed effect and birth year fixed effect model in all the linear regression models are strong and above 10. The results for all outcome variables in the baseline model suggest that a one-year increase in education did not impact attitudes toward IPV or the experience of IPV for adult women as displayed in table 8-10 and table 38-40 in appendix I. The direction of the coefficients is the same in the 2sls model but the magnitude of the coefficients is smaller and the coefficients are not statistically significant. The large difference

between the linear 2sls model and the ivprobit model for all results most likely depends on the fact that the 2sls models do not provide good estimates of marginal effects when the independent variable is binary (Stock and Watson, 2011). Pseudo R-square is not included in the tables for the ivprobit model since Stata does not report this kind of statistics.

Table 8. Justify IPV – Instrumental variable estimates

	(1) 2sls Baseline	(2) 2sls District FE	(3) 2sls Year of birth FE	(4) 2sls District & year of birth FE	(5) ivprobit Baseline	(6) ivprobit District FE	(7) ivprobit Year of birth FE	(8) ivprobit District & year of birth FE
Years of education	-0.00831	0.0168	-0.00835	0.0471	-0.0242	0.0761	-0.0203	0.169**
	(0.0140)	(0.0122)	(0.0190)	(0.0335)	(0.0570)	(0.0473)	(0.0740)	(0.0710)
Constant	-5.731	1.678	0.153**	9.693**				
	(4.529)	(3.678)	(0.0732)	(3.820)				
Observations	20,445	20,445	20,445	20,445	20,445	20,445	20,445	20,445
R-squared	0.089	0.062	0.090	-0.118				
1st F-statistics	23.73	22.99	15.13	9.345				

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Table 9. Sexual violence – Instrumental variable estimates

	(1) 2sls Baseline	(2) 2sls District FE	(3) 2sls Year of birth FE	(4) 2sls District & year of birth FE	(5) ivprobit Baseline	(6) ivprobit District FE	(7) ivprobit Year of birth FE	(8) ivprobit District & year of birth FE
Years of education	0.0131	0.0247	0.0154	0.0583	0.0588	0.105	0.0733	0.212**
	(0.0105)	(0.0217)	(0.0124)	(0.0525)	(0.0437)	(0.0796)	(0.0471)	(0.0962)
Constant	3.272	5.936	0.0128	-0.0800				
	(2.431)	(5.198)	(0.0885)	(0.189)				
Observations	8,434	8,434	8,434	8,434	8,434	8,434	8,434	8,434
R-squared	0.003	-0.015	0.001	-0.224				
1st F-statistics	30.45	14.03	18.41	4.481				

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Table 10. Control behavior – Instrumental variable estimates

	(1) 2sls Baseline	(2) 2sls District FE	(3) 2sls Year of birth FE	(4) 2sls District & year of birth FE	(5) ivprobit Baseline	(6) ivprobit District FE	(7) ivprobit Year of birth FE	(8) ivprobit District & year of birth FE
Years of education	-0.0104	0.00198	-0.0326	-0.0715	-0.0353	-0.00180	-0.106	-0.209**
	(0.0167)	(0.0229)	(0.0240)	(0.0557)	(0.0566)	(0.0813)	(0.0682)	(0.0893)
Constant	-1.721	1.047	0.268***	0.399**				
	(3.690)	(5.179)	(0.0776)	(0.172)				
Observations	8,411	8,411	8,411	8,411	8,411	8,411	8,411	8,411
R-squared	0.031	0.050	-0.013	-0.215				
1st F-statistics	30.11	13.71	18.18	4.239				

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Comparing my IV results to Peterman et al's (2015a) recent study on the impact the UPE reform has on adult women's experiences of IPV in Malawi suggests that their result is overestimating the positive impact that the UPE reform had on violence. In their IV strategy they only use the variation in year of birth as an instrument. Yet, a shortcoming of their model is that they do not include controls for time effects. Peterman et al (2015a) define their outcome variable as a combination of physical and sexual violence. I have used this combination of physical/sexual violence as an outcome variable and estimate it using both my DD and IV model and the results shows no statistically significant impact of educational attainment on physical/sexual violence using my baseline specification.

Since the IV estimates show no robust significant results I do not conduct a pathway analysis using the IV method.

6.6 Robustness check using instrumental variable analysis

As a first robustness check I investigate whether or not years of schooling has affected the different violence outcomes by comparing two unaffected cohorts. The cohorts I use are the same as in the DD robustness check, those born 1956-1965 against those born 1966-1975. The coefficients show no significant result as displayed in appendix J, table 41, except for emotional violence. The coefficient for emotional violence suggests that a one-year increase in schooling decrease emotional violence by almost 20 percent. However, similar to the above IV results this coefficient is too large to be credible.

As a second robustness check, I use the binary interaction term as an instrument. Results are presented in appendix J, table 42-47. Similar to the IV analysis above a one-year

increase in schooling increase the probability that adult women justify IPV by 17.9 percentage point and decrease the probability of the experience of control behavior by 17.4 percentage point when controlling for district and birth year effects. In addition, any type of IPV turns significant and the result suggests that a one-year increase in schooling decreases the probability that women experience any type of IPV by 16.7 percentage points. However, the linear regression result suggests that the validity of the instrument is low when including district and year of birth fixed effects (F-statistics <10). Again, similar to the above IV analysis the significant coefficients are large. The linear model and the baseline result from both the linear and the non-linear model shows no significant impact for none of the outcome variables, which is consistent with my main IV results.

7. Discussion and conclusion

In this paper I have investigated the causal impact of educational attainment on adult women's experiences of IPV and their attitudes toward IPV, using the UPE reform in Malawi in 1994 as a natural experiment. The reform had a statistically significant and sizable impact on years of schooling. The result from the DD analysis further suggest that the UPE reform has increased the probability that women born between 1981-1990 justify IPV by 7 percentage points when net enrollment rate increase from 0.00 to 1.00. The IV analysis suggests that a one-year increase in education increase the probability that women justify IPV by 16.8 percentage points. Whether or not women justify IPV is important, because if women perceive IPV to be accepted, a natural part of men's supremacy and a normal part of marriage, it is unlikely that women will divorce, which often already is stigmatized, or to report the experience of violence to the judicial system. The success of many violence intervention programs will depend to a large extent on attitudes and norms around the acceptance of IPV against women.

The result that the UPE reform caused women to be more likely to justify IPV is robust across different specifications. The result is driven by the questions whether or not wife beating is justified if the wife refuse sex and if she burns the food. Especially, the result that IPV is justified when the wife refuse sex is in line with the result that women experience more sexual violence following the change in education caused by the UPE reform. It has previously been showed that women are more likely to justify IPV when they experience actual abuse. One possibility is that there has been a shift in how severe the different stated reason for justifying wife beating are. The increase in attitudes justifying IPV is however counterintuitive with previous findings where attitudes toward IPV decreased with education

(Friedman et al, 2011; Mocan and Cannonier, 2012). One possible explanation for this result is peer effects and the changing composition of peer groups caused by the reform. It is likely that girls and boys from different socio-economic groups had more contact with each other after the abolishment of school fees than in the old system. Thus, one could expect a decline in the "quality" of the pool of peer among students. Peer effects could also be a possible explanation to the increase in risky sexual behavior, as indicated by the result of increased outside sexual partners, found among the justify IPV sample. I cannot rule out the possibility of peer effects, but if peer effect is an explanation to the results found in this paper, they must have been large and had long lasting effects to serve as a valid explanation.

Furthermore, the result suggests that women exposed to the UPE reform are more likely to experience sexual violence when controlling for fixed effects, both according to the DD and IV analysis. This finding could be related to economic bargaining models where violence is extractive and used by men to increase their utility and to control the distribution of resources within the household, and to backlash theory where men compensate the loss of bargaining power, caused by an increase in female education, with more violence. Another likely scenario explaining this result is that women who have been empowered by education are more likely to deny sex. In a social context where men are considered to have the right to sex within marriage, men could use violence to force sex. In addition, the pathway analysis suggested that women in the domestic violence sample are more likely to earn more than their spouse. This result is more in line with gendered resource theory, status inconsistency theory and contextual gendered resource theory where men use violence when their status as head of household is challenged by increased female education and income. Regardless of explanation this result is in line with the finding of an increase in the acceptance of IPV since women who experience more violence are more likely to justify IPV.

At the same time the result suggests that women who were exposed to the UPE reform are less likely to experience control behavior from their spouse when controlling for district and year of birth fixed effects, both in the DD and IV analysis. This result is more in line with non-cooperative bargaining models where women's outside options increase with education and because of this they are less likely to experience more violence. The pathway analysis did not suggest that women were more likely to work for cash, but working for cash is only one of many outside options, which I was able to control for with the data I have.

The result from this paper suggests that the relationship between education and the experience of and attitudes toward IPV is non-monotonous. However, the findings in this paper might seem counterintuitive. One possible explanation for women exposed to the UPE

reform being more likely to justify IPV and experience sexual violence at the same time as they are at lower risk of experiencing control behavior is because these are *different* types of violence that are affected differently by female empowerment. It is possible that women with more years of schooling are empowered to the extent that they are less likely to experience control behavior but as a consequence of the increase in female empowerment in a society with conservative gender norms, men compensate the loss in bargaining power with sexual violence. The UPE reform did not only increase education for women but also for men[8]. It is possible that with higher education norms and values are changed regarding men's control behavior but not on men's right sex within marriage.

The pathway analysis suggests that women exposed to the UPE reform have a higher bargaining power, which could be linked to less experience of control behavior and non-cooperative bargaining models. In addition, women exposed to the UPE reform in the justify IPV sample are more likely to have a more risky sexual behavior due to more outside partners at the same time the number of lifetime partners has decreased among women exposed to the UPE reform. Keeping girls in school might decrease the lifetime number of sexual partners. Since previous studies have showed that women with more outside partners are more likely to experience violence this could possible have caused the increase in women justifying IPV following the UPE reform.

My interpretation of the results relies on the assumption that the only channel through the reform affects my outcome variables are through the increase in educational attainment. However, the magnitude and the coefficients from the IV analysis when controlling for district and year of birth fixed effects is large for justify IPV, sexual violence and control behavior, which suggests that, the coefficient might be imprecisely estimated. Because of this it is complicated to draw robust conclusions, even though the regression results are significant. The DD and the IV model measure different treatment effects, where the DD model measure the effect of the UPE reform for both compliers and non-compliers while the IV model measure the average causal effect of compliers. It is possible that there is a difference between compliers that cause the magnitude of the coefficient to differ in the two models. However, it is likely that the large coefficients in the IV analysis are caused by the fact that the UPE reform is a non-perfect experimental setting to be analyzed. In a perfect experimental setting predetermined control variable should not affect the effect the UPE reform have on the different outcome variables. However, if the UPE reform is a non-perfect

[8] I have conducted all my results for men as well. However, since the interaction term is not significant for years of education in the DD analysis and is not a good instrument for years of schooling for men (the F-statistics is less the 0.01), these results have not been discussed in this paper.

experimental setting to be analyzed it is possible that the results suffer from omitted variable bias. Two important variables that probably affect the outcome variables is the respondent's socio-economic status as child and if the respondent grew up in a rural or urban area since violence is often higher in rural areas. However, because the DHS data not includes retrospective information of the respondent's socio-economic status or residence area as a child I am unable to control for this without causing endogeneity problems.

In my analysis I have implicitly assumed that when the quantity of schooling increased, quality has been constant. However, the sudden abolishment of school fees in Malawi lead to a large increase in net enrollment rate and inexperienced teachers were employed, which most likely caused the quality of education to decrease. If there was a decline in the quality of education, years of schooling would be diluted and the relationship between educational attainment and the outcome variables considered would be underestimated. Unfortunately, this is not possible to test with the data I have.

As with all observational studies, this study has its limitations. As emphasized by many of the theories in the theoretical framework, the context is important to consider. I have only been able to make assumptions about the context when explaining my results. In addition, one drawback is that I do not have information about the severity of violence. With panel data and information of the socio-economic status of the respondent as a child I would have been able to estimate the impact of years of schooling and the probability for the experience of IPV and attitudes toward IPV more accurately.

For policy makers this result is important since it provides evidence of the importance of considering the fact that education might impact violence differently depending on the type of violence. Countries with a high percentage of individuals justifying IPV and high percentage of women experiencing IPV provide an excellent opportunity for future research. Important topics for further research are to precisely estimate the impact primary education has on adult women's experiences of violence and their attitudes toward violence, through collecting and analyzing accurate data, preferably panel data. This would not only be important for policy makers but most importantly it is central for the protection of human rights and the expansion of women's freedom.

References

Ai, C. and Norton, E. C. 2003. Interaction Terms in Logit and Probit Models. *Economics Letters,* 80**,** 123-129.

Anderson, K. L. 1997. Gender, Status, and Domestic Violence: An Integration of Feminist and Family Violence Approaches. *Journal of Marriage and Family,* 59**,** 655-669.

Andersson, N., Ho-Foster, A., Mitchell, S., Scheepers, E. and Goldstein, S. 2007. Risk Factors for Domestic Physical Violence: National Cross-Sectional Household Surveys in Eight Southern African Countries. *BMC women's health,* 7**,** 11-11.

Anglewicz, P. 2012. Migration, Marital Change, and Hiv Infection in Malawi. *Demography,* 49**,** 239-265.

Atkinson, M. P., Greenstein, T. N. and Lang, M. M. 2005. For Women, Breadwinning Can Be Dangerous: Gendered Resource Theory and Wife Abuse. *Journal of Marriage and Family,* 67**,** 1137-1148.

Bates, L. M., Schuler, S. R. and Islam, F. 2004. Socioeconomic Factors and Processes Associated with Domestic Violence in Rural Bangladesh. *International Family Planning Perspectives,* 30**,** 190-199.

Becker, G. S. and Ghez, G. R. 1975. *The Allocation of Time and Goods over the Life Cycle,* New York, Columbia Univ. Press for the National Bureau of Economic Research.

Behrman, J. A. 2015a. Does Schooling Affect Women's Desired Fertility? Evidence from Malawi, Uganda, and Ethiopia. *Demography,* 52**,** 787.

Behrman, J. A. 2015b. The Effect of Increased Primary Schooling on Adult Women's Hiv Status in Malawi and Uganda: Universal Primary Education as a Natural Experiment. *Social science & medicine (1982),* 127**,** 108-115.

Bloch, F. and Rao, V. 2002. Terror as a Bargaining Instrument: A Case Study of Dowry Violence in Rural India. *The American Economic Review,* 92**,** 1029-1043.

Borjas, G. J. 2016. *Labor Economics,* New York;McGraw-Hill Eduation;.

Bowlus, A. J. and Seitz, S. 2006. Domestic Violence, Employment, and Divorce. *International Economic Review,* 47**,** 1113-1149.

Cools, S. and Kotsadam, A. 2015. Resources and Intimate Partner Violence in Sub-Saharan Africa. *University of Oslo***,** 1-47.

Duflo, E. 2001. Schooling and Labor Market Consequences of School Construction in Indonesia: Evidence from an Unusual Policy Experiment. *The American Economic Review,* 91**,** 795-813.

Durevall, D. and Lindskog, A. 2015a. Intimate Partner Violence and Hiv in Ten Sub-Saharan African Countries: What Do the Demographic and Health Surveys Tell Us? *The Lancet. Global health,* 3**,** e34-e43.

Durevall, D. and Lindskog, A. 2015b. Intimate Partner Violence and Hiv Infection in Sub-Saharan Africa. *World Development,* 72**,** 27-42.

Duvvury, N., Callan, A., Carney, P. and Raghavendra, S. 2013. Intimate Partner Violence: Economic Costs and Implications for Growth and Development. *Women's voice, agency, and participation research series, World Bank***,** 1-94.

Eswaran, M. and Malhotra, N. 2011. Domestic Violence and Women's Autonomy in Developing Countries: Theory and Evidence. *The Canadian Journal of Economics / Revue canadienne d'Economique,* 44**,** 1222-1263.

Farmer, A. and Tiefenthaler, J. 1997. An Economic Analysis of Domestic Violence. *Review of Social Economy,* 55**,** 337-358.

Flake, D. F. 2005. Individual, Family, and Community Risk Markers for Domestic Violence in Peru. *Violence Against Women,* 11**,** 353-373.

Friedman, W., Kremer, M., Miguel, E. and Thornton, R. 2011. Education as Liberation? : National Bureau of Economic Research.

Garcia-Moreno, C., Jansen, H., A. F. M, Ellsberg, M., Heise, L. and Watts, C. H. 2006. Prevalence of Intimate Partner Violence: Findings from the Who Multi-Country Study on Women's Health and Domestic Violence. *The Lancet,* 368**,** 1260-1269.

Gupta, S. K. 2011. Intention-to-Treat Concept: A Review. *Perspectives in Clinical Research,* 2**,** 109-112.

Harling, G., Msisha, W. and Subramanian, S. V. 2010. No Association between Hiv and Intimate Partner Violence among Women in 10 Developing Countries. *PloS one,* 5**,** e14257.

Heise, L. L. 2012. *Determinants of Partner Violence in Low and Middle-Income Countries: Exploring Variation in Individual and Population-Level Risk*, London School of Hygiene and Tropical Medicine, University of London.

Hornung, C. A., Mccullough, B. C. and Sugimoto, T. 1981. Status Relationships in Marriage: Risk Factors in Spouse Abuse. *Journal of Marriage and Family,* 43**,** 675-692.

Jewkes, R. 2002. Intimate Partner Violence: Causes and Prevention. *The Lancet,* 359**,** 1423-1429.

Jewkes, R., Levin, J. and Penn-Kekana, L. 2002. Risk Factors for Domestic Violence: Findings from a South African Cross-Sectional Study. *Social Science & Medicine,* 55**,** 1603-1617.

Kattan, B. R. 2006. Implementation of Free Basic Education Policy. *Education working paper seried, World Bank,* 1**,** 1-133.

Kelly, J. B. and Johnson, M. P. 2008. Differentiation among Types of Violence of Intimate Partner Violence: Reseach Updates and Implications for Interventions. *Family Court Review,* 46**,** 476-499.

Kishor, S. and Johnson, K. 2005. Profiling Domestic Violence: A Multi-Country Study. *Studies in Family Planning,* 36**,** 259.

Manser, M. and Brown, M. 1980. Marriage and Household Decision-Making: A Bargaining Analysis. *International Economic Review,* 21**,** 31-44.

Mccloskey, L. A., Williams, C. and Larsen, U. 2005. Gender Inequality and Intimate Partner Violence among Women in Moshi, Tanzania. *International Family Planning Perspectives,* 31**,** 124-130.

Mcelroy, M. B. and Horney, M. J. 1981. Nash-Bargained Household Decisions: Toward a Generalization of the Theory of Demand. *International Economic Review,* 22**,** 333-349.

Measure Demographic Health Survey/Inner City Fund International 2013. Standard Recode Manual for Dhs 6. *Deomographic and Health Surveys - MEASURE DHS***,** 1-144.

Ministry of Economic Planning and Development 1996. Malawi Social Indicators Survey 1995. *National Statistical Office, Centre for Social Researcher*

Mocan, N. H. and Cannonier, C. 2012. Empowering Women through Education: Evidence from Sierra Leone. National Bureau of Economic Research.

Osili, U. O. and Long, B. T. 2008. Does Female Schooling Reduce Fertility? Evidence from Nigeria. *Journal of Development Economics,* 87**,** 57-75.

Peterman, A., Behrman, A. J. and Palermo, T. 2015a. Primary Education and Adult Women's Experience of Intimate Partner Violence: Quasi-Experimental Evidence from Sub-Saharan Africa. *Working paper***,** 1-27.

Peterman, A., Bleck, J. and Palermo, T. 2015b. Age and Intimate Partner Violence: An Analysis of Global Trends among Women Experiencing Victimization in 30 Developing Countries. *The Journal of adolescent health : official publication of the Society for Adolescent Medicine,* 57**,** 624-630.

Puhani, P. A. 2012. The Treatment Effect, the Cross Difference, and the Interaction Term in Nonlinear "Difference-in-Differences" Models. *Economics Letters,* 115**,** 85-87.

Schlozman, K. L., Burns, N. and Verba, S. 1999. "What Happened at Work Today?": A Multistage Model of Gender, Employment, and Political Participation. *The Journal of Politics,* 61**,** 29-53.
Stock, J. H. and Watson, M. W. 2011. *Introduction to Econometrics,* Harlow, Pearson.
Tauchen, H. V., Witte, A. D. and Long, S. K. 1991. Domestic Violence: A Nonrandom Affair. *International Economic Review,* 32**,** 491-511.
The World Bank 2009. Abolishing School Fees in Africa: Lessons Learned from Ethiopia, Ghana, Kenya, and Mozambique. *The World Bank***,** 1-280.
The World Bank 2010. *The Education System in Malawi*, World Bank.
True, J. 2012. *The Political Economy of Violence against Women*, Oxford University Press.
Uthman, O. A., Moradi, T. and Lawoko, S. 2009. The Independent Contribution of Individual-, Neighbourhood-, and Country-Level Socioeconomic Position on Attitudes Towards Intimate Partner Violence against Women in Sub-Saharan Africa: A Multilevel Model of Direct and Moderating Effects. *Social Science & Medicine,* 68**,** 1801-1809.
Vyas, S. and Watts, C. 2009. How Does Economic Empowerment Affect Women's Risk of Intimate Partner Violence in Low and Middle Income Countries? A Systematic Review of Published Evidence. *Journal of International Development,* 21**,** 577-602.

Appendix

A. The UPE reform and schooling in Malawi

In Malawi primary school is eight years and is supposed to be carried out between ages 6 to 13. However, as in many African countries grade repetition is common. Secondary education is four years and is supposed to take place between ages 14 to 17. Following the adoption of the 'World Declaration on Education for All' in 1990, Malawi implemented the first steps toward the UPE policy in 1991. Starting with a sequential approach to eliminate school fees in 1991 that entailed provision of fee waivers for grade one the first year of implementation and for grade two the second year. In 1994 this sequential approach was replaced by a comprehensive approach, where all primary school fees were eliminated with start in September 1994. Furthermore, indirect fees such as the requirement to wear school uniforms were eliminated and learning materials became provided for free from the government. The Ministry of Education launched a big media campaign in July 1994 in order to make everyone aware of the policy. 20,000 new teachers were recruited and mother tongue instructions were implemented for grade 1-4. The year after the implementation there was a 51% increase in primary enrollment rate with a 59% increase in grade 1. Since entry was allowed at any grade the largest increase occurred at later grades with 76% increase at the final grade of schooling. Although the UPE reform has been claimed to be a big success considering the increase in children attending school, shrinking the gender and the socio-economic gap in primary education, an important critique against the policy is that it occurred at the expense of school quality and increase in class size. 90% of the new teachers recruited were considered poorly trained and the enrollment rate increase was not matched by a proportionate increase in expenditure from the government (The World Bank, 2009).

B. IPV questions and coding

Justify IPV

Is wife beating justified if?

1. Goes out without telling him?
2. Neglects the children?
3. If you argue with him?
4. If you refuse to have sex with him?
5. If you burn the food?

If the respondent answers yes to any of the justify IPV question the justify IPV indicator equals 1, and 0 otherwise.

Physical violence

Have your partner/husband or last partner/husband:

1. Ever push, shook or threw something at you?
2. Ever slapped you?
3. Ever punched you with fist or something else harmful?
4. Ever kicked or dragged you?
5. Ever tried to strangle or burn you?
6. Ever threatened you with any kind of weapon?
7. Ever attacked you with any kind of weapon?

If the respondent answers yes to any of the physical violence question the physical violence indicator equals 1, and 0 otherwise.

Sexual violence

Have your partner/husband or last partner/husband:

1. Ever physically forced sex when not wanted?
2. Ever forced or threat you to perform sexual act you did not want to?

If the respondent answers yes to any of the sexual violence question the emotional violence indicator equals 1, and 0 otherwise.

Emotional violence

Have your partner/husband or last partner/husband:

1. Ever humiliated you?
2. Ever threaten to hurt you?

If the respondent answers yes to any of the emotional violence question the emotional violence indicator equals 1, and 0 otherwise.

Control behavior

Have your partner/husband or last partner/husband:

1. Not permitted you to meet female friends?
2. Limited your contact with your family?

If the respondent answers yes to any of the control behavior question the control behavior indicator equals 1, and 0 otherwise.

Any kind of IPV

If the respondent answers yes to any of the physical, sexual, emotional violence question or control behavior the any kind of IPV indicator equals 1, and 0 otherwise.

Source: (Measure Demographic Health Survey/Inner City Fund International, 2013).

C. Graphical presentation of the main outcome variables

Figure 1. Mean years of schooling by birth cohort for the two different samples used in the analysis

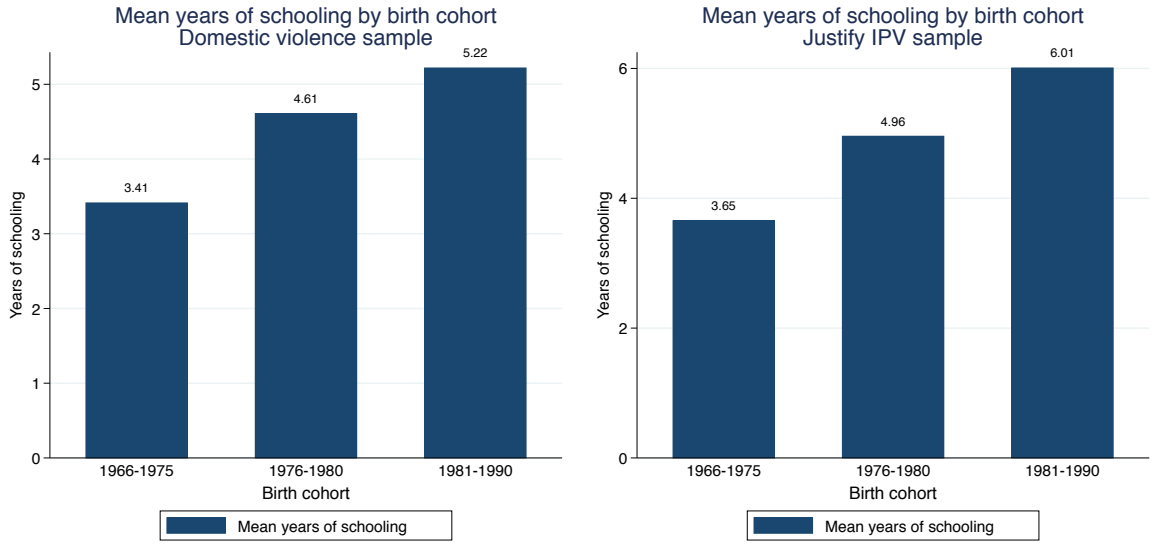
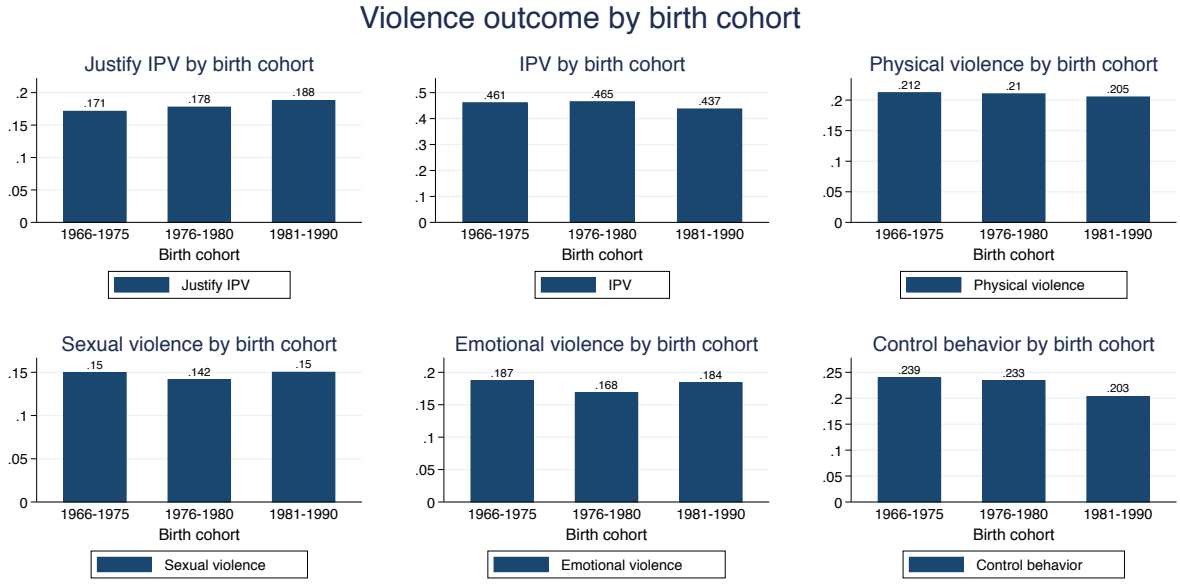


Figure 2. Violence outcome by birth cohort.



D. Net enrollment rate by district

Table 11. Net enrollment rate by district in Malawi 1994/95 and 1995.

District	1992/93	1995	Percentage point increase
Northern region			
Chipita	79.3	89.8	10.5
Karonga	85.5	89.3	3.8
Mzimba	90.7	90.9	0.2
Nkhata bay	76.4	94.3	17.9
Rumphi	91.8	91.9	0.1
Central region			
Dedza	38.1	66.1	28
Dowa	48.7	87.6	38.9
Kasungu	80.6	77.1	-3.5
Lilongwe	53.8	85.1	31.3
Mchinji	54.8	86.1	31.3
Nkhotakota	86.6	82.9	-3.7
Ntcheu	54.5	80.3	25.8
Ntchisi	58.8	73.8	15
Salima	54.3	72.8	18.5
Southern region			
Blantyre	48.5	90.7	42.2
Chikwawa	45.3	74.5	29.2
Chirdazulu	54.4	89.3	34.9
Machinga	47.7	83.4	35.7
Mangochi	49	86.5	37.5
Mulanje	41.9	80.9	39
Mwanza	57.9	64.6	6.7
Nsanje	57.1	83.3	26.2
Thylo	48.5	75	26.5
Zombo	51.5	90.5	39

Source: (Ministry of Economic Planning and Development, 1996).

E. LPM and probit regression results

Table 12. Justify IPV – LPM and Probit results

	(1) LPM Baseline	(2) LPM District FE	(3) LPM Year of birth FE	(4) LPM District & year of birth FE	(5) Probit Baseline	(6) Probit District FE	(7) Probit Year of birth FE	(8) Probit District & year of birth FE
Years of education	-0.00910***	-0.00896***	-0.00912***	-0.00899***	-0.00978***	-0.00980***	-0.00984***	-0.00987***
	(0.00111)	(0.00117)	(0.00112)	(0.00117)	(0.000999)	(0.000929)	(0.00102)	(0.000925)
Constant	-5.941***	-5.733***	0.0830	0.248***				
	(0.772)	(0.763)	(0.0684)	(0.0455)				
Observations	20,438	20,438	20,438	20,438	20,438	20,438	20,438	20,438
R-squared	0.089	0.111	0.090	0.112				
Pseudo R^2					0.093	0.117	0.095	0.1176

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Table 13. IPV – LPM and Probit results

	(1) LPM Baseline	(2) LPM District FE	(3) LPM Year of birth FE	(4) LPM District & year of birth FE	(5) Probit Baseline	(6) Probit District FE	(7) Probit Year of birth FE	(8) Probit District & year of birth FE
Years of education	-0.00269	-0.000992	-0.00300	-0.00131	-0.00271	-0.000979	-0.00304	-0.00133
	(0.00223)	(0.00197)	(0.00220)	(0.00195)	(0.00226)	(0.00202)	(0.00223)	(0.00200)
Constant	0.243	0.660	0.532***	0.467***				
	(1.712)	(1.672)	(0.0979)	(0.0655)				
Observations	8,403	8,403	8,403	8,403	8,403	8,403	8,403	8,403
R-squared	0.017	0.035	0.022	0.040				
Pseudo R^2					0.012	0.025	0.016	0.029

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Table 14. Physical violence – LPM and Probit results

	(1) LPM Baseline	(2) LPM District FE	(3) LPM Year of birth FE	(4) LPM District & year of birth FE	(5) Probit Baseline	(6) Probit District FE	(7) Probit Year of birth FE	(8) Probit District & year of birth FE
Years of education	-0.000942	-0.00109	-0.00113	-0.00132	-0.000904	-0.00105	-0.00108	-0.00125
	(0.00183)	(0.00174)	(0.00187)	(0.00178)	(0.00185)	(0.00176)	(0.00188)	(0.00179)
Constant	0.566	0.588	0.280***	0.257***				
	(1.178)	(1.209)	(0.0552)	(0.0582)				
Observations	8,425	8,425	8,425	8,425	8,425	8,425	8,425	8,425
R-squared	0.008	0.017	0.012	0.021				
Pseudo R^2					0.0085	0.017	0.013	0.021

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Table 15. Sexual violence – LPM and Probit results

	(1) LPM Baseline	(2) LPM District FE	(3) LPM Year of birth FE	(4) LPM District & year of birth FE	(5) Probit Baseline	(6) Probit District FE	(7) Probit Year of birth FE	(8) Probit District & year of birth FE
Years of education	0.00105	0.000589	0.00103	0.000556	0.00124	0.000691	0.00124	0.000689
	(0.00173)	(0.00149)	(0.00177)	(0.00154)	(0.00172)	(0.00150)	(0.00176)	(0.00154)
Constant	0.470	0.357	0.0985	0.185***				
	(1.181)	(1.180)	(0.0753)	(0.0555)				
Observations	8,434	8,434	8,434	8,434	8,434	8,434	8,434	8,434
R-squared	0.015	0.030	0.017	0.032				
Pseudo R^2					0.018	0.035	0.021	0.038

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Table 16. Emotional violence – LPM and Probit results

	(1) LPM Baseline	(2) LPM District FE	(3) LPM Year of birth FE	(4) LPM District & year of birth FE	(5) Probit Baseline	(6) Probit District FE	(7) Probit Year of birth FE	(8) Probit District & year of birth FE
Years of education	-0.000482	0.000617	-0.000477	0.000614	-0.000399	0.000666	-0.000379	0.000679
	(0.00180)	(0.00138)	(0.00184)	(0.00143)	(0.00174)	(0.00138)	(0.00178)	(0.00144)
Constant	4.066**	4.399***	0.111	0.0432				
	(1.607)	(1.545)	(0.0714)	(0.0753)				
Observations	8,429	8,429	8,429	8,429	8,429	8,429	8,429	8,429
R-squared	0.038	0.052	0.042	0.055				
Pseudo R^2					0.040	0.054	0.044	0.058

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Table 17. Control behavior – LPM and Probit results

	(1) LPM Baseline	(2) LPM District FE	(3) LPM Year of birth FE	(4) LPM District & year of birth FE	(5) Probit Baseline	(6) Probit District FE	(7) Probit Year of birth FE	(8) Probit District & year of birth FE
Years of education	-0.00315*	-0.00192	-0.00344**	-0.00224	-0.00315**	-0.00207	-0.00346**	-0.00238
	(0.00152)	(0.00139)	(0.00152)	(0.00150)	(0.00158)	(0.00157)	(0.00156)	(0.00156)
Constant	-0.0473	0.228***	0.251**	0.170***				
	(1.082)	(0.0497)	(0.0946)	(0.0597)				
Observations	8,411	8,411	8,411	8,411	8,411	8,411	8,411	8,411
R-squared	0.034	0.051	0.038	0.055				
Pseudo R^2					0.033	0.049	0.037	0.053

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

F. Difference in difference result

Table 18. Any type of IPV – DD results

	(1) LPM Baseline	(2) LPM District FE	(3) LPM Year of birth FE	(4) LPM District & year of birth FE	(5) Probit Baseline	(6) Probit District FE	(7) Probit Year of birth FE	(8) Probit District & year of birth FE
Born 1981-1990*NE increase	0.00470 (0.0723)	-0.0106 (0.0685)	0.00869 (0.0728)	-0.00646 (0.0686)	0.00467 (0.0735)	-0.0109 (0.0704)	0.00862 (0.0737)	-0.00716 (0.0702)
Born 1981-1990 dummy	0.0403 (0.0288)	0.0463 (0.0276)			0.0413 (0.0293)	0.0487* (0.0286)		
NE increase by district dummy	-0.258 (0.245)		-0.257 (0.247)		-0.260 (0.247)		-0.260 (0.249)	
Constant	6.392 (4.278)	6.547 (4.129)	0.649*** (0.178)	0.391*** (0.0784)				
Observations	8,403	8,403	8,403	8,403	8,403	8,403	8,403	8,403
R-squared	0.018	0.035	0.023	0.039				
Pseudo R^2					0.013	0.0256	0.017	0.029

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Table 19. Physical violence – DD results

	(1) LPM Baseline	(2) LPM District FE	(3) LPM Year of birth FE	(4) LPM District & year of birth FE	(5) Probit Baseline	(6) Probit District FE	(7) Probit Year of birth FE	(8) Probit District & year of birth FE
Born 1981-1990*NE increase	0.0350 (0.0669)	0.0324 (0.0662)	0.0323 (0.0659)	0.0321 (0.0659)	0.0369 (0.0671)	0.0357 (0.0658)	0.0380 (0.0668)	0.0371 (0.0650)
Born 1981-1990 dummy	0.00108 (0.0293)	0.00287 (0.0285)			0.000722 (0.0288)	0.00225 (0.0281)		
NE increase by district dummy	-0.0330 (0.138)		-.0316 (.137)		-0.0341 (0.135)		-0.0328 (0.134)	
Constant	2.052 (2.987)	2.249 (2.939)	-0.125 (3.414)	-0.131 (3.415)				
Observations	8,425	8,425	8,426	8,425	8,425	8,425	8,425	8,425
R-squared	0.008	0.017	0.021	0.021				
Pseudo R^2					0.009	0.017	0.013	0.021

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Table 20. Emotional violence – DD results

	(1) LPM Baseline	(2) LPM District FE	(3) LPM Year of birth FE	(4) LPM District & year of birth FE	(5) Probit Baseline	(6) Probit District FE	(7) Probit Year of birth FE	(8) Probit District & year of birth FE
Born 1981-1990*NE increase	0.0401 (0.0379)	0.0259 (0.0404)	0.0421 (0.0373)	0.0271 (0.0398)	0.0372 (0.0374)	0.0233 (0.0408)	0.0405 (0.0368)	0.0259 (0.0404)
Born 1981-1990 dummy	0.000203 (0.0241)	0.00680 (0.0243)			-0.00428 (0.0233)	0.00238 (0.0237)		
NE increase by district dummy	-0.0510 (0.117)		-0.0502 (0.116)		-0.0441 (0.115)		-0.0440 (0.115)	
Constant	5.503 (3.787)	5.965 (3.731)	0.258** (0.102)	0.178** (0.0661)				
Observations	8,429	8,429	8,429	8,429	8,429	8,429	8,429	8,429
R-squared	0.038	0.052	0.042	0.055				
Pseudo R^2					0.040	0.054	0.044	0.058

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

G. Pathway analysis

Table 21. Summary statistics pathway outcomes for the domestic violence sample

	Full sample		Post UPE cohort		Pre UPE cohort	
	Mean	Std dev	Mean	Std dev	Mean	Std dev
Sexual behavior						
Outside sex partners (0-1)	0.03	0.18	0.03	0.17	0.03	0.18
Sex partners	1.69	1.35	1.62	1.45	1.83	1.11
Age at first sex	16.29	2.36	16.17	2.19	16.47	2.59
Marriage matches						
Marital status (0-1)	0.89	0.32	0.90	0.30	0.86	0.34
Age at first marriage	17.24	3.02	16.87	2.33	17.75	3.71
Spouse 10+ years older (0-1)	0.16	0.37	0.13	0.33	0.21	0.41
Partner characteristics						
Spouse's year of schooling	5.81	4.16	6.36	4.08	5.04	4.16
Spouse more years of schooling (0-1)	0.25	0.43	0.27	0.45	0.21	0.41
Souse drinks alcohol (0-1)	0.36	0.48	0.34	0.47	0.38	0.49
Bargaining power						
Work for cash (0-1)	0.11	0.32	0.12	0.33	0.10	0.30
Earns more than spouse (0-1)	0.10	0.30	0.09	0.29	0.12	0.33
Participate in household decision (0-1)	0.85	0.36	0.87	0.33	0.82	0.39
Socio-economic status						
Poor (0-1)	0.41	0.49	0.44	0.50	0.38	0.49
Middle (0-1)	0.23	0.42	0.24	0.42	0.23	0.42
Rich (0-1)	0.35	0.48	0.33	0.47	0.39	0.49
Mass media						
Reads newspaper (0-1)	0.09	0.29	0.10	0.30	0.08	0.27
Listen to radio (0-1)	0.63	0.48	0.62	0.48	0.64	0.48
Watch TV (0-1)	0.08	0.27	0.08	0.27	0.07	0.25

Table 22. Summary statistics pathway outcomes for the justify IPV sample

	Full sample		Post UPE cohort		Pre UPE cohort	
	Mean	Std dev	Mean	Std dev	Mean	Std dev
Sexual behavior						
Outside sex partners (0-1)	0.06	0.24	0.07	0.26	0.04	0.19
Sex partners	1.66	1.08	1.59	1.08	1.79	1.07
Age at first sex	16.40	2.45	16.35	2.33	16.49	2.67
Marriage matches						
Marital status (0-1)	0.74	0.44	0.71	0.45	0.81	0.39
Age at first marriage	17.33	3.07	17.07	2.49	17.71	3.75
Spouse 10+ years older (0-1)	0.17	0.37	0.13	0.34	0.23	0.42
Partner characteristics						
Spouse's year of schooling	6.17	4.24	6.73	4.14	5.32	4.25
Spouse more years of schooling (0-1)	0.25	0.43	0.28	0.45	0.21	0.41
Souse drinks alcohol (0-1)	0.36	0.48	0.34	0.47	0.38	0.49
Bargaining power						
Work for cash (0-1)	0.12	0.32	0.12	0.32	0.11	0.32
Earns more than spouse (0-1)	0.10	0.30	0.08	0.27	0.13	0.33
Participate in household decision (0-1)	0.41	0.49	0.44	0.50	0.37	0.48
Socio-economic status						
Poor (0-1)	0.39	0.49	0.39	0.49	0.39	0.49
Middle (0-1)	0.21	0.41	0.21	0.41	0.21	0.41
Rich (0-1)	0.40	0.49	0.40	0.49	0.40	0.49
Mass media						
Reads newspaper (0-1)	0.12	0.32	0.14	0.34	0.08	0.27
Listen to radio (0-1)	0.61	0.49	0.62	0.49	0.60	0.49
Watch TV (0-1)	0.11	0.31	0.12	0.33	0.09	0.29

Table 23. Outside partners – DD results for the justify IPV sample

	(1) Probit Baseline	(2) Probit District FE	(3) Probit Year of birth FE	(4) Probit District & year of birth FE
Born 1981-1990*NE increase	0.0619***	0.0606***	0.0613***	0.0597***
	(0.0223)	(0.0209)	(0.0223)	(0.0209)
Born 1981-1990 dummy	-0.0403***	-0.0402***		
	(0.0101)	(0.00960)		
NE increase by district dummy	0.0931*		0.0915*	
	(0.0536)		(0.0535)	
Observations	20,425	20,425	20,425	20,425
Pseudo R^2	0.042	0.053	0.046	0.057

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Table 24. Lifetime number of sexual partners – DD results for the justify IPV sample

	(1) OLS Baseline	(2) OLS District FE	(3) OLS Year of birth FE	(4) OLS District & year of birth FE
Born 1981-1990*NE increase	-0.403**	-0.426**	-0.403**	-0.424**
	(0.158)	(0.155)	(0.156)	(0.153)
Born 1981-1990 dummy	0.233***	0.223***		
	(0.0787)	(0.0788)		
NE increase by district dummy	1.419***		1.423***	
	(0.416)		(0.414)	
Constant	48.44***	47.38***	30.36***	28.57***
	(7.265)	(7.461)	(6.661)	(7.032)
Observations	12,776	12,776	12,776	12,776
R-squared	0.046	0.066	0.048	0.069

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Table 25. Household decision – DD results for the justify IPV sample

	(1) Probit Baseline	(2) Probit District FE	(3) Probit Year of birth FE	(4) Probit District & year of birth FE
Born 1981-1990*NE increase	0.101***	0.100***	0.0985***	0.0981***
	(0.0276)	(0.0294)	(0.0272)	(0.0290)
Born 1981-1990 dummy	-0.0243*	-0.0208		
	(0.0138)	(0.0137)		
NE increase by district dummy	-0.396***		-0.393***	
	(0.126)		(0.125)	
Observations	17,705	17,705	17,705	17,705
Pseudo R^2	0.020	0.033	0.022	0.034

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Table 26. Watch TV – DD results for the justify IPV sample

	(1) Probit Baseline	(2) Probit District FE	(3) Probit Year of birth FE	(4) Probit District & year of birth FE
Born 1981-1990*NE increase	0.0522*	0.0320	0.0515*	0.0314
	(0.0300)	(0.0243)	(0.0298)	(0.0243)
Born 1981-1990 dummy	-0.00810	-0.00713		
	(0.0120)	(0.0118)		
NE increase by district dummy	0.311		0.311	
	(0.213)		(0.213)	
Observations	20,433	20,433	20,433	20,433
Pseudo R^2	0.032	0.077	0.034	0.079

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Table 27. Age at first sex – DD results for the domestic violence sample

	(1) OLS Baseline	(2) OLS District FE	(3) OLS Year of birth FE	(4) OLS District & year of birth FE
Born 1981-1990*NE increase	0.744*	0.718*	0.722*	0.697
	(0.408)	(0.404)	(0.415)	(0.410)
Born 1981-1990 dummy	0.00846	0.0431		
	(0.204)	(0.204)		
NE increase by district dummy	1.105		1.132	
	(1.083)		(1.073)	
Constant	114.9***	119.0***	122.0***	123.5***
	(28.17)	(27.81)	(23.05)	(22.33)
Observations	8,396	8,396	8,396	8,396
R-squared	0.041	0.060	0.047	0.065

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Table 28: Earns more than spouse – DD results for domestic violence sample

	(1) Probit Baseline	(2) Probit District FE	(3) Probit Year of birth FE	(4) Probit District & year of birth FE
Born 1981-1990*NE increase	0.176**	0.206**	0.176**	0.205***
	(0.0834)	(0.0832)	(0.0824)	(0.0785)
Born 1981-1990 dummy	0.0758	0.0670		
	(0.0461)	(0.0490)		
NE increase by district dummy	0.102		0.0857	
	(0.107)		(0.106)	
Observations	1,133	1,133	1,133	1,133
Pseudo R^2	0.045	0.087	0.061	0.107

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

H. Robustness check difference in difference analysis

Table 29. Robustness check of UPE on unaffected cohorts – Cohort 1956-1965 against cohort 1966-1975. DD results for district and year of birth fixed effects

	(1) OLS Years of schooling Justify IPV	(2) Probit Justify IPV	(3) OLS Years of Schooling Domestic violence	(4) Probit IPV	(5) Probit Physical violence	(6) Probit Sexual violence	(7) Probit Emotional violence	(8) Probit Control behavior
Born 1966-1975*NE increase	-1.359*	0.0251	-0.921	0.0262	0.0647	0.0724	0.0952	0.0232
	(0.697)	(0.0461)	(0.623)	(0.0897)	(0.0662)	(0.0656)	(0.0589)	(0.0811)
Constant	-0.171		0.794					
	(0.890)		(0.592)					
Observations	10,568	10,568	5,112	5,112	5,132	5,135	5,133	5,117
R-squared	0.163		0.169					
Pseudo R^2		0.106		0.033	0.031	0.062	0.058	0.043

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. This model includes controls for year of birth, a survey dummy, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Table 30. Years of education for domestic violence sample – DD robustness check using potential increase by district to measure district treatment

	(1) OLS Baseline	(2) OLS District FE	(3) OLS Year of birth FE	(4) OLS District & year of birth FE
Born 1981-1990*Potential increase	0.336*	0.393**	0.302	0.362**
	(0.187)	(0.164)	(0.191)	(0.168)
Born 1981-1990 dummy	1.063***	0.985***		
	(0.252)	(0.240)		
Potential increase by district dummy	0.211		0.243	
	(1.107)		(1.112)	
Constant	-61.54*	-62.46*	-2.317	3.149***
	(35.75)	(34.35)	(2.019)	(0.412)
Observations	8,403	8,403	8,403	8,403
R-squared	0.144	0.193	0.149	0.197

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Table 31. Years of education for justify IPV sample – DD robustness check using potential increase by district to measure district treatment

	(1) OLS Baseline	(2) OLS District FE	(3) OLS Year of birth FE	(4) OLS District & year of birth FE
Born 1981-1990*Potential increase	0.532***	0.484***	0.531***	0.483***
	(0.161)	(0.159)	(0.161)	(0.158)
Born 1981-1990 dummy	0.913***	0.890***		
	(0.231)	(0.236)		
Potential increase by district dummy	0.0297		0.0330	
	(1.035)		(1.035)	
Constant	-139.5***	-138.6***	-1.666	2.590***
	(28.56)	(28.10)	(2.046)	(0.426)
Observations	20,445	20,445	20,445	20,445
R-squared	0.168	0.217	0.169	0.219

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Table 32. Justify IPV – DD robustness check using potential increase by district to measure district treatment

	(1) LPM Baseline	(2) LPM District FE	(3) LPM Year of birth FE	(4) LPM District & year of birth FE	(5) Probit Baseline	(6) Probit District FE	(7) Probit Year of birth FE	(8) Probit District & year of birth FE
Born 1981-1990*Potential increase	0.0284**	0.0266*	0.0280**	0.0262*	0.0303**	0.0282**	0.0298**	0.0277**
	(0.0132)	(0.0134)	(0.0135)	(0.0136)	(0.0119)	(0.0118)	(0.0123)	(0.0121)
Born 1981-1990 dummy	-0.0335**	-0.0273*			-0.0346**	-0.0302**		
	(0.0158)	(0.0153)			(0.0142)	(0.0141)		
Potential increase by district dummy	-0.0765		-0.0769		-0.0719		-0.0723	
	(0.0731)		(0.0725)		(0.0695)		(0.0688)	
Constant	-4.558***	-3.993**	0.236	0.274***				
	(1.509)	(1.430)	(0.179)	(0.0402)				
Observations	20,445	20,445	20,445	20,445	20,445	20,445	20,445	20,445
R-squared	0.083	0.105	0.084	0.106				
Pseudo R^2					0.086	0.109	0.087	0.110

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Table 33. Any type of IPV – DD robustness check using potential increase by district to measure district treatment

	(1) LPM Baseline	(2) LPM District FE	(3) LPM Year of birth FE	(4) LPM District & year of birth FE	(5) Probit Baseline	(6) Probit District FE	(7) Probit Year of birth FE	(8) Probit District & year of birth FE
Born 1981-1990*Potential increase	-0.0227	-0.0289	-0.0210	-0.0271	-0.0231	-0.0296	-0.0215	-0.0279
	(0.0240)	(0.0211)	(0.0232)	(0.0202)	(0.0241)	(0.0214)	(0.0233)	(0.0204)
Born 1981-1990 dummy	0.0582*	0.0665**			0.0595*	0.0695**		
	(0.0325)	(0.0299)			(0.0330)	(0.0309)		
Potential increase by district dummy	-0.0600		-0.0634		-0.0596		-0.0633	
	(0.115)		(0.114)		(0.116)		(0.115)	
Constant	6.331	6.711	0.623**	0.517***				
	(4.336)	(4.152)	(0.258)	(0.0733)				
Observations	8,403	8,403	8,403	8,403	8,403	8,403	8,403	8,403
R-squared	0.017	0.035	0.022	0.040				
Pseudo R^2					0.013	0.026	0.016	0.029

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Table 34. Physical violence – DD robustness check using potential increase by district to measure district treatment

	(1) LPM Baseline	(2) LPM District FE	(3) LPM Year of birth FE	(4) LPM District & year of birth FE	(5) Probit Baseline	(6) Probit District FE	(7) Probit Year of birth FE	(8) Probit District & year of birth FE
Born 1981-1990*Potential increase	0.0169	0.0164	0.0173	0.0169	0.0173	0.0171	0.0182	0.0183
	(0.0210)	(0.0204)	(0.0209)	(0.0201)	(0.0207)	(0.0201)	(0.0205)	(0.0197)
Born 1981-1990 dummy	-0.0033	-0.00190			-0.00354	-0.00234		
	(0.0299)	(0.0286)			(0.0294)	(0.0280)		
Potential increase by district dummy	0.00036		-0.00091		0.00126		-0.0001	
	(0.0515)		(0.0505)		(0.0502)		(0.0493)	
Constant	2.033	2.335	0.194	0.347***				
	(3.012)	(2.930)	(0.140)	(0.0541)				
Observations	8,425	8,425	8,426	8,425	8,425	8,425	8,425	8,425
R-squared	0.008	0.017	0.012	0.021				
Pseudo R^2					0.009	0.017	0.013	0.021

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Table 35. Sexual violence – DD robustness check using potential increase by district to measure district treatment

	(1) LPM Baseline	(2) LPM District FE	(3) LPM Year of birth FE	(4) LPM District & year of birth FE	(5) Probit Baseline	(6) Probit District FE	(7) Probit Year of birth FE	(8) Probit District & year of birth FE
Born 1981-1990*Potential increase	0.00228	0.00289	0.00423	0.00473	0.00275	0.00483	0.00586	0.00779
	(0.0199)	(0.0193)	(0.0196)	(0.0190)	(0.0182)	(0.0175)	(0.0179)	(0.0171)
Born 1981-1990 dummy	0.0120	0.0127			0.0109	0.0102		
	(0.0272)	(0.0272)			(0.0251)	(0.0247)		
Potential increase by district dummy	-0.0780		-0.0792		-0.0658		-0.0676	
	(0.0594)		(0.0592)		(0.0546)		(0.0544)	
Constant	2.253	2.263	0.271	0.286***				
	(3.518)	(3.374)	(0.160)	(0.0588)				
Observations	8,434	8,434	8,434	8,434	8,434	8,434	8,434	8,434
R-squared	0.016	0.030	0.018	0.032				
Pseudo R^2					0.019	0.035	0.022	0.038

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Table 36. Emotional violence – DD robustness check using potential increase by district to measure district treatment

	(1) LPM Baseline	(2) LPM District FE	(3) LPM Year of birth FE	(4) LPM District & year of birth FE	(5) Probit Baseline	(6) Probit District FE	(7) Probit Year of birth FE	(8) Probit District & year of birth FE
Born 1981-1990*Potential increase	0.000751	-0.00642	0.00155	-0.00599	0.000894	-0.00568	0.00180	-0.00502
	(0.0179)	(0.0177)	(0.0175)	(0.0173)	(0.0172)	(0.0176)	(0.0168)	(0.0170)
Born 1981-1990 dummy	0.0104	0.0184			0.00480	0.0126		
	(0.0272)	(0.0267)			(0.0262)	(0.0259)		
Potential increase by district dummy	0.0556		0.0534		0.0499		0.0485	
	(0.0750)		(0.0742)		(0.0687)		(0.0677)	
Constant	5.425	6.028	0.0937	0.210***				
	(3.804)	(3.751)	(0.166)	(0.0592)				
Observations	8,429	8,429	8,430	8,429	8,429	8,429	8,429	8,429
R-squared	0.038	0.052	0.042	0.055				
Pseudo R^2					0.040	0.054	0.044	0.057

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Table 37. Control violence – DD robustness check using potential increase by district to measure district treatment

	(1) LPM Baseline	(2) LPM District FE	(3) LPM Year of birth FE	(4) LPM District & year of birth FE	(5) Probit Baseline	(6) Probit District FE	(7) Probit Year of birth FE	(8) Probit District & year of birth FE
Born 1981-1990*Potential increase	-0.0179	-0.0210	-0.0177	-0.0205	-0.0195	-0.0224	-0.0193	-0.0219
	(0.0159)	(0.0147)	(0.0162)	(0.0151)	(0.0154)	(0.0145)	(0.0157)	(0.0150)
Born 1981-1990 dummy	0.0613**	0.0657**			0.0645**	0.0681***		
	(0.0265)	(0.0253)			(0.0261)	(0.0246)		
Potential increase by district dummy	-0.0854		-0.0865		-0.0794		-0.0810	
	(0.0828)		(0.0824)		(0.0800)		(0.0792)	
Constant	7.098**	7.190**	0.495**	0.415***				
	(3.431)	(3.403)	(0.205)	(0.0517)				
Observations	8,411	8,411	8,411	8,411	8,411	8,411	8,411	8,411
R-squared	0.035	0.051	0.038	0.055				
Pseudo R^2					0.033	0.049	0.037	0.052

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

I. Instrumental variable regression results

Table 38. IPV - Instrumental variable results

	(1) 2sls Baseline	(2) 2sls District FE	(3) 2sls Year of birth FE	(4) 2sls District & year of birth FE	(5) ivprobit Baseline	(6) ivprobit District FE	(7) ivprobit Year of birth FE	(8) ivprobit District & year of birth FE
Years of education	-0.00840	0.0187	-0.0261	-0.00670	-0.0213	0.0495	-0.0657	-0.0187
	(0.0204)	(0.0335)	(0.0285)	(0.0687)	(0.0529)	(0.0858)	(0.0706)	(0.181)
Constant	-1.081	5.227	0.416***	17.63**				
	(4.126)	(7.339)	(0.0839)	(8.745)				
Observations	8,403	8,403	8,403	8,403	8,403	8,403	8,403	8,403
R-squared	0.015	0.019	-0.001	0.038				
1st F-statistics	29.81	13.42	17.87	3.969				

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Table 39. Physical violence - Instrumental variable results

	(1) 2sls Baseline	(2) 2sls District FE	(3) 2sls Year of birth FE	(4) 2sls District & year of birth FE	(5) ivprobit Baseline	(6) ivprobit District FE	(7) ivprobit Year of birth FE	(8) ivprobit District & year of birth FE
Years of education	0.00669	0.0158	0.00654	0.0324	0.0241	0.0580	0.0255	0.120
	(0.0145)	(0.0248)	(0.0192)	(0.0717)	(0.0512)	(0.0846)	(0.0667)	(0.202)
Constant	2.336	4.509	0.219***	11.16				
	(3.309)	(5.572)	(0.0627)	(9.302)				
Observations	8,425	8,425	8,425	8,425	8,425	8,425	8,425	8,425
R-squared	0.004	-0.001	0.009	-0.047				
1st F-statistics	30.24	13.97	18.25	4.376				

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Table 40. Emotional violence - Instrumental variable results

	(1) 2sls Baseline	(2) 2sls District FE	(3) 2sls Year of birth FE	(4) 2sls District & year of birth FE	(5) ivprobit Baseline	(6) ivprobit District FE	(7) ivprobit Year of birth FE	(8) ivprobit District & year of birth FE
Years of education	0.00643	0.0148	0.00664	0.0275	0.0197	0.0451	0.0263	0.0997
	(0.0122)	(0.0208)	(0.0134)	(0.0433)	(0.0488)	(0.0814)	(0.0530)	(0.148)
Constant	5.669*	7.697	0.107	-0.0432				
	(3.161)	(5.185)	(0.0736)	(0.161)				
Observations	8,429	8,429	8,429	8,429	8,429	8,429	8,429	8,429
R-squared	0.034	0.038	0.038	0.008				
1st F-statistics	30.12	13.78	18.05	4.196				

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

J. Robustness check using instrumental variable analysis

Table 41. Robustness check of UPE on unaffected cohorts – Cohort 1956-1965 against cohort 1966-1975. IV results for district and year of birth fixed effects

	(1) ivprobit Justify IPV	(2) ivprobit IPV	(3) ivprobit Physical violence	(4) ivprobit Sexual violence	(5) ivprobit Emotional violence	(6) ivprobit Control behavior
Years of education	-0.0707	-0.0460	-0.141	-0.185	-0.197**	-0.0522
	(0.139)	(0.150)	(0.124)	(0.139)	(0.0872)	(0.167)
Observations	10,568	5,112	5,132	5,135	5,133	5,117

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. This model includes controls for year of birth, a survey dummy, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Table 42. Justify IPV – IV robustness check

	(1) 2sls Baseline	(2) 2sls District FE	(3) 2sls Year of birth FE	(4) 2sls District & year of birth FE	(5) ivprobit Baseline	(6) ivprobit District FE	(7) ivprobit Year of birth FE	(8) ivprobit District & year of birth FE
Years of education	0.00290	0.0184	0.0158	0.0543	0.0297	0.0803*	0.0926	0.179***
	(0.0216)	(0.0115)	(0.0513)	(0.0353)	(0.0802)	(0.0419)	(0.150)	(0.0642)
Constant	-2.422	2.140	0.109	10.45**				
	(6.562)	(3.418)	(0.107)	(4.083)				
Observations	20,445	20,445	20,445	20,445	20,445	20,445	20,445	20,445
R-squared	0.078	0.056	0.042	-0.180				
1st F-statistics	11.56	24.13	3.410	9.355				

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Table 43. IPV – IV robustness check

	(1) 2sls Baseline	(2) 2sls District FE	(3) 2sls Year of birth FE	(4) 2sls District & year of birth FE	(5) ivprobit Baseline	(6) ivprobit District FE	(7) ivprobit Year of birth FE	(8) ivprobit District & year of birth FE
Years of education	-0.0120	-0.000485	-0.0988	-0.0749	-0.0296	0.000442	-0.198**	-0.167*
	(0.0306)	(0.0275)	(0.0743)	(0.0537)	(0.0791)	(0.0732)	(0.0912)	(0.0883)
Constant	-1.905	0.777	0.460***	9.153				
	(6.642)	(5.931)	(0.0905)	(7.087)				
Observations	8,403	8,403	8,403	8,403	8,403	8,403	8,403	8,403
R-squared	0.013	0.035	-0.368	-0.177				
1st F-statistics	10.57	15.05	1.510	4.612				

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Table 44. Physical violence – IV robustness check

	(1) 2sls Baseline	(2) 2sls District FE	(3) 2sls Year of birth FE	(4) 2sls District & year of birth FE	(5) ivprobit Baseline	(6) ivprobit District FE	(7) ivprobit Year of birth FE	(8) ivprobit District & year of birth FE
Years of education	0.0200	0.0189	0.0475	0.0452	0.0694	0.0668	0.152	0.150
	(0.0222)	(0.0218)	(0.0732)	(0.0639)	(0.0732)	(0.0727)	(0.171)	(0.154)
Constant	5.415	5.209	0.199**	0.0473				
	(4.831)	(4.942)	(0.0925)	(0.216)				
Observations	8,425	8,425	8,425	8,425	8,425	8,425	8,425	8,425
R-squared	-0.020	-0.007	-0.137	-0.108				
1st F-statistics	10.62	15.65	1.531	5.176				

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Table 45. Sexual violence – IV robustness check

	(1) 2sls Baseline	(2) 2sls District FE	(3) 2sls Year of birth FE	(4) 2sls District & year of birth FE	(5) ivprobit Baseline	(6) ivprobit District FE	(7) ivprobit Year of birth FE	(8) ivprobit District & year of birth FE
Years of education	-0.00623	0.0101	-0.0377	0.0126	-0.0180	0.0466	-0.0962	0.0890
	(0.0172)	(0.0217)	(0.0629)	(0.0492)	(0.0744)	(0.0916)	(0.194)	(0.180)
Constant	-1.220	2.561	0.0382	0.0659				
	(4.026)	(4.925)	(0.0722)	(0.153)				
Observations	8,434	8,434	8,434	8,434	8,434	8,434	8,434	8,434
R-squared	0.010	0.023	-0.105	0.021				
1st F-statistics	10.64	15.76	1.530	5.243				

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Table 46. Emotional violence – IV robustness check

	(1) 2sls Baseline	(2) 2sls District FE	(3) 2sls Year of birth FE	(4) 2sls District & year of birth FE	(5) ivprobit Baseline	(6) ivprobit District FE	(7) ivprobit Year of birth FE	(8) ivprobit District & year of birth FE
Years of education	0.0168	0.00180	0.0380	-0.0162	0.0510	-0.00170	0.128	-0.0520
	(0.0270)	(0.0201)	(0.0868)	(0.0442)	(0.0965)	(0.0806)	(0.244)	(0.170)
Constant	8.081	4.673	0.0922	14.58***				
	(6.082)	(4.605)	(0.0899)	(5.646)				
Observations	8,429	8,429	8,429	8,429	8,429	8,429	8,429	8,429
R-squared	0.017	0.052	-0.062	0.037				
1st F-statistics	10.60	15.51	1.497	4.906				

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.

Table 47. Control behavior – IV robustness check

	(1) 2sls Baseline	(2) 2sls District FE	(3) 2sls Year of birth FE	(4) 2sls District & year of birth FE	(5) ivprobit Baseline	(6) ivprobit District FE	(7) ivprobit Year of birth FE	(8) ivprobit District & year of birth FE
Years of education	-0.00919	0.00871	-0.104	-0.0559	-0.0296	0.0245	-0.236***	-0.174*
	(0.0286)	(0.0226)	(0.0858)	(0.0470)	(0.0971)	(0.0778)	(0.0867)	(0.0995)
Constant	-1.449	2.606	0.310***	0.347**				
	(6.697)	(5.048)	(0.107)	(0.153)				
Observations	8,411	8,411	8,411	8,411	8,411	8,411	8,411	8,411
R-squared	0.032	0.045	-0.566	-0.107				
1st F-statistics	10.65	15.38	1.530	4.926				

*** Significant at 0.01 level. ** Significant at 0.05 level. * Significant at 0.1 level. Notes: Standard errors are clustered at district level. All models include controls for year of birth, a survey dummy, net enrollment 1992/93 by district, dummy variables for number of siblings, and dummy variables for the largest religious and ethnic groups. District fixed effects refers to the 24 district in Malawi based on the 1990 district boundaries.