Women Empowerment and Domestic Violence: Evidence from a Multi-Dimensional Policy in India*

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Abstract

A facet of gender inequality in developing countries is increasing dependence of women on men for financial, social, political and familial support. A consequence of this dependence is marital and domestic violence faced by women due to potential lack of relative bargaining power in terms of household decision making and poor outside options due to suboptimal human capital endowments. In this paper, we study a multidimensional women empowerment program from India to estimate its effects on domestic violence towards their wives. Using exogenous geographical variation in the policy rollout coupled with cohort-variation generated by eligibility rules, we employ a reduced form difference-in-difference design to find lesser emotional and physical violence on women as a result of the intervention. We also find that husbands' private level of satisfaction with the marriage and overall attitude towards domestic violence improve in response to the program.

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1 Introduction

World Bank estimates suggest that the recent male labor force participation rate worldwide is around 75% whereas that for females is roughly 48%, providing a striking example of widespread gender inequality.[1] While gender inequality is not restricted to only developing countries, an important feature of such inequality in the developing world is the increased dependence of women on men for social, political, familial, moral and sometimes, sadly enough, *existential* support. Such societies are usually characterized by low female earnings, early child bearing and early marriage, poor human capital endowments and high fertility rates among women (Jayachandran 2015). While this could just be an equilibrium response to having a comparative biological advantage in child-bearing (Becker 1991), the facts that women are deprived of the same opportunities as men (Doepke, Tertilt and Voenna 2012) and are subjected to gender-based physical and sexual violence (Banerjee, La Ferrara and Orozco 2019) suggest that these inequalities are actually the result of discrimination. As a result, policymakers and social scientists have diverted a lot of attention to women empowerment measures in such countries.

In this paper, we study a large scale multidimensional women empowerment program from India known as the *Rajiv Gandhi Scheme for Empowerment of Adolescent Girls (SABLA)* to estimate its impacts on gender based violence faced by women from their partners. Although the program did not directly target reducing domestic violence faced by women, our hypothesis is that the tenets of the policy such as better human capital accumulation, higher nutritional intake, awareness about safe-sex practices etc should lead to an overall improvement in the women's relative bargaining power as well as self confidence which may lead to a reduction in domestic violence.

The empirical evidence of impacts of programs targeted at empowering adolescent girls on women-centric outcomes is mixed. Most studies usually look at specific aspects of women empowerment as policies and *lab-in-field* experiments are mostly targeted to address a particular issue and administered at a chosen location. For instance, evidence on school based

[1] See the statistics for workers in the age group of 15+ years here: `https://data.worldbank.org/topic/gender`

experimental interventions suggests that teaching negotiation skills lead to better long term education outcomes (Ashraf et al 2018) and informative discussions about gender equality can address issues of inequality (Dhar, Jain and Jayachandran 2018). On the other hand, it has been found that improving soft-skills for community college girls have no significant impacts on employment or optimism about future prospects (Groh et al, 2016). However, Adhvaryu, Kala and Nyshadham (2018) find that on-the-job soft skills training cam improve outcomes of female workers. Duflo, Dupas and Kremer (2015) find that education subsidy programs help reduce adolescent girls' dropout rates alongwith incidence of marriage and pregnancy. Again, Field, Jayachandran and Pande (2010) find that traditional social and religious institutions may constrain the effectiveness of policies in developing countries due to stereotypical beliefs.

India's SABLA policy combines many of these dimensions into one comprehensive program. Not only does it directly attempt to improve the vocational skills of women but also focusses on developing soft skills and creating awareness about reproductive health and labor market opportunities associated with delayed pregnancy and marriage. It is worth noting that the SABLA program closely resembles the empowerment activities undertaken by an NGO known as BRAC, originally based in Bangladesh and now expanding activities to African nations, under the ambit of their Empowerment and Livelihood for Adolescents (ELA) program. Bandiera et al. (2019) conduct a randomized evaluation with BRAC in Uganda and find that a multidimensional intervention, that combines improving soft skills with vocational skills in adolescents, has several positive impacts on women. They find that vocational training coupled with information dissemination on reproductive health leads to better employment outcomes and reductions in teen pregnancies.

Programs like ELA and SABLA, which are not unidimensional, are becoming increasingly significant in terms of efficacy in achieving stated objectives and putting in proper checks to ever growing gender inequality. This is particularly important because gender inequality in developing countries often permeates through physical, sexual and mental harassment of women subjected to gender-based violence from the opposite sex. While one potential reason

for this would be prevalence of social or religious stigmas and ignorance regarding available outside options, another reason would be that the outside options are mostly poor because of lack of adequate human capital endowments. While a unidimensional policy may address one of the two issues, only a multifaceted program can allow addressing both these issues simultaneously.

There may be two reasons why a multidimensional program like SABLA can lead to women empowerment which in turn may lead to lower incidence of gender based violence. First, the potential of increased human capital at an adolescent age implies better labor market prospects in the future. Consequently, better opportunities in the labor market may lead to delaying of marriage and child bearing (Jensen 2012). De Walque (2014) also suggests that targeted interventions such as through enforced legislation, taxes and subsidies help reduce risky behaviors. This may lead to higher relative bargaining power for women in the household leading to a reduction in violence faced by them from men. Overall, this maybe achieved by the direct vocational training and provision of health supplements and improved nutrition dimensions of SABLA. Second, empirical evidence documents that information dissemination may lead to better outcomes for women in terms of reproductive health (Kearney and Levine 2015). Recent work by Banerjee, La Ferrara and Orozco (2019) provides more relevant motivation for our study as they find direct impacts of communication/information on incidence of domestic violence. The development of soft skills and awareness creation dimension of SABLA may leverage this channel of impact of the multidimensional intervention on gender based violence.

Interestingly, Bandiera et al. (2019) in their study in Uganda exploiting a similar intervention find a reduction in non-consensual sex among women. This provides suggestive evidence that the ELA initiative had led to improvements in women's control over their body. We intend to test whether similar effects exist for the SABLA program with the objective of understanding whether reduction in gender based violence can be a natural consequence of women empowerment programs like these. To the best of our knowledge, the empirical links between multidimensional women empowerment policies and reduction in domestic violence

have not been convincingly established in the literature. We attempt to bridge this gap by exploiting the exogeneous variation generated on account of institutional features of the SABLA program. The Indian government introduced this program in 2010 in 205 districts (out of a total 640 as per the 2011 census) of the country chosen using a weighted index of district level indicators such as female dropout rates, female literacy rates, incidence of early marriages and labor force participation rates of females. The policy document claims to have divided the districts into high, moderate and poor performing districts based on the composite index and the 205 chosen for the SABLA intervention was intended to be representative of the population. As a result, this seems like a stratified random sample chosen to receive the intervention.

However, comparing the outcomes of the 205 treated districts to the untreated ones would require making an assumption about the randomization. The policy briefs are not clear about whether these selected districts were randomly drawn from the population after allowing for the ranking based on the weighted index. As a result, we propose a quasi-experimental design to estimate the impacts of the program. Since the program targeted adolescent girls belonging to a particular age group, we exploit the additional cohort variation in exposure to the program[2]. The identification strategy relies on the difference in outcomes for the affected cohorts including only adolescent girls from girls in other age groups for the 205 SABLA districts compared to the untreated districts after controlling for observables. We use data from the National Family Health Survey (NFHS) - Round 4 conducted in 2015-16 covering information on almost 700 thousand women from India. Since the dataset in use corresponds to a considerable time lag since the policy was implemented, it allows us to use both discrete cohort variation (*ever vs never affected*) as well as continuous treatment assignment using actual years of exposure to the policy interacted with district level treatment status in our identification strategy.

We find that exposure to SABLA leads to a significant decline in domestic violence faced by women. Specifically, we find decline in emotional and physical violence faced by women

[2]However, we do report the results for the case where the districts were randomly assigned in Appendix Table A.1.

as a result of exposure to the policy. Due to unavailability of district identifiers we are unable to use earlier rounds of the NFHS given our empirical strategy. We stand to lose out on a couple of things as a result. First, we could have potentially tracked changes and exploited the timing of the reform to get more robust identification. Currently, we have to depend on a difference-in-difference design based on two cross-sectional sources of variation. Although, it is econometrically not any different in terms of execution or implication, we stand to lose out on some statistical power and other potentially interesting outcomes such as changing notions about domestic violence (Banerjee, La Ferrara and Orozco 2019). Second, we are unable to provide support for the counterfactual identification assumption using common pre-trends in outcomes for treated and untreated units using the same survey.

While there is no obvious way to get around the first problem, we use alternate data sources to try and address the second issue.[3] We collect administrative data from the National Crime Records Bureau (NCRB) of India which provides aggregate district level women related crime data over years. Comparing the aggregate crime rates for women related crimes in SABLA districts with other districts over the years we do not find any significant differential trends providing support to the identification design.

It is important to point out here, that even though SABLA resembles the ELA program, the two initiatives are not exactly identical. First, the sheer scale of operation of SABLA is massive compared to ELA. As per official releases, SABLA affects 10 million adolescent girls per annum whereas BRAC reaches around 1 million girls overall (Bandiera et al. 2019).[4] Second, the SABLA was implemented by the union government in India and was a large scale national public policy aimed at improving women's conditions in the economy. The ELA on the other hand was part of the activities of BRAC which is an NGO. Third, features of the two programs were pretty different. SABLA had components like provision of health supplements, enhanced nutrition through cooked meals provided to the girls etc. Fourth, SABLA had sub-categories of the intervention so as to offer specific treatments to specific

[3]The reason for not being able to use any other dataset but NFHS for the main analysis have been described in detail in the Data section that follows.

[4]See `http://pib.nic.in/newsite/PrintRelease.aspx?relid=133064`

age groups. Finally, on average SABLA catered to a relatively younger cohort compared to the ELA and age verification was strictly institutionalized.

These nuances make SABLA an interesting case to study. Given that the literature is pretty passive on evidence of women empowerment policies leading to a reduction in domestic violence, our paper mainly contributes to the literature by filling this void. Recent research suggests that RCTs conducted with NGO partners may be susceptible to a *NGO effect* which might lead to overestimating some of the impacts (Usmani, Jeuland and Pattanayak 2019). As a result external validity concerns for impact research get accentuated further (Peters, Langbein and Roberts 2018). Estimates from SABLA therefore contribute to the generalizability of some of the experimental findings in this domain. Additionally, we find that this multidimensional program leads to changes in husbands private level of satisfaction from the marriage as well as attitude towards domestic violence, suggesting potential general equilibrium effects of the program. This suggests that the impact of SABLA was not restricted to the targeted women alone but spilled over to other family members which leads us to speculate that this is an indication of changes in the intra-household resource allocation patterns. We do not know of any other study which has estimated the impacts of women empowerment policies on husband satisfaction level and attitude towards domestic violence. Our final contribution is adding to the literature on the overall effects of multidimensional programs affecting adolescent girls (Acevedo et al. 2017; Buchmann et al. 2017, Bandiera et al 2018; Bandiera et al, 2019).

2 Rajiv Gandhi Scheme for Empowerment of Adolescent Girls or *SABLA*

The Rajiv Gandhi Scheme for Empowerment of Adolescent Girls (or SABLA) is a Government of India initiative which aims at empowerment and development of adolescent girls who are in the age group of 11 to 18 years. The scheme is implemented under the aegis of Ministry of Women and Child Development (GOI) and was introduced near the end of year

2010. The main objectives that the intervention seeks to achieve include improved health, delayed pregnancy and reduction in maternal and infant mortality, for the enrolled girls. It also seeks to create productive individuals who are educated, aware and economically empowered.

Due to the holistic nature of the goals that the programme strives to achieve, the scheme also uses a multifaceted policy program to target the developmental needs of the adolescent girls. Under the programme, the girls falling in the above-mentioned age group are provided nutrition and health education, life skills training, vocational training, information related to accessing public services, and guidance on family welfare, reproductive and sexual health, child care practices and home management. The girls enrolled under the scheme are also eligible for nutrition provision in the form of 'Take Home Ration' or a hot cooked meal, along with iron and folic acid supplementation and health check-up services. All the activities under the scheme are selected and modelled based on the theory of life-course approach and meant to facilitate an easier transition to womanhood for the girls[5].

The Scheme was introduced in 205 districts spread throughout the country[6]. The districts were chosen based on a composite weighted index for four different criterions related to adolescent girls, which included, drop out rate of females from school (50%), Female literacy rate (20%), Girls married before the age of 18 years (20%) and female work participation (10%). The districts selected were a combination of good, moderate and poor performing districts based on the above based methodology. This was done primarily to ensure the successful implementation of the scheme in the pilot districts before its full-scale expansion.

The scheme also differentiates between adolescent girls in different age groups, with regards to benefits provided under the scheme. For instance, the nutritional component of the scheme in the form of 'Take Home Ration' or hot cooked meal was provided to all the girls in the age group of 14-18 years whereas to only out of school girls in the age group of 11-14 years. Similarly, to address age specific concerns and provide age appropriate attention to girls, health and personal hygiene related modules were planned for age groups 11-14 years

[5]See `http://wcd.nic.in/sites/default/files/1-SablaEVAReportver5.1_0.pdf`

[6]In 2017-18, the programme's covergae was expanded to include 303 additional districts

and 15-18 years. Also, only girls in the age group of 16-18 years were eligible for receiving vocational training.

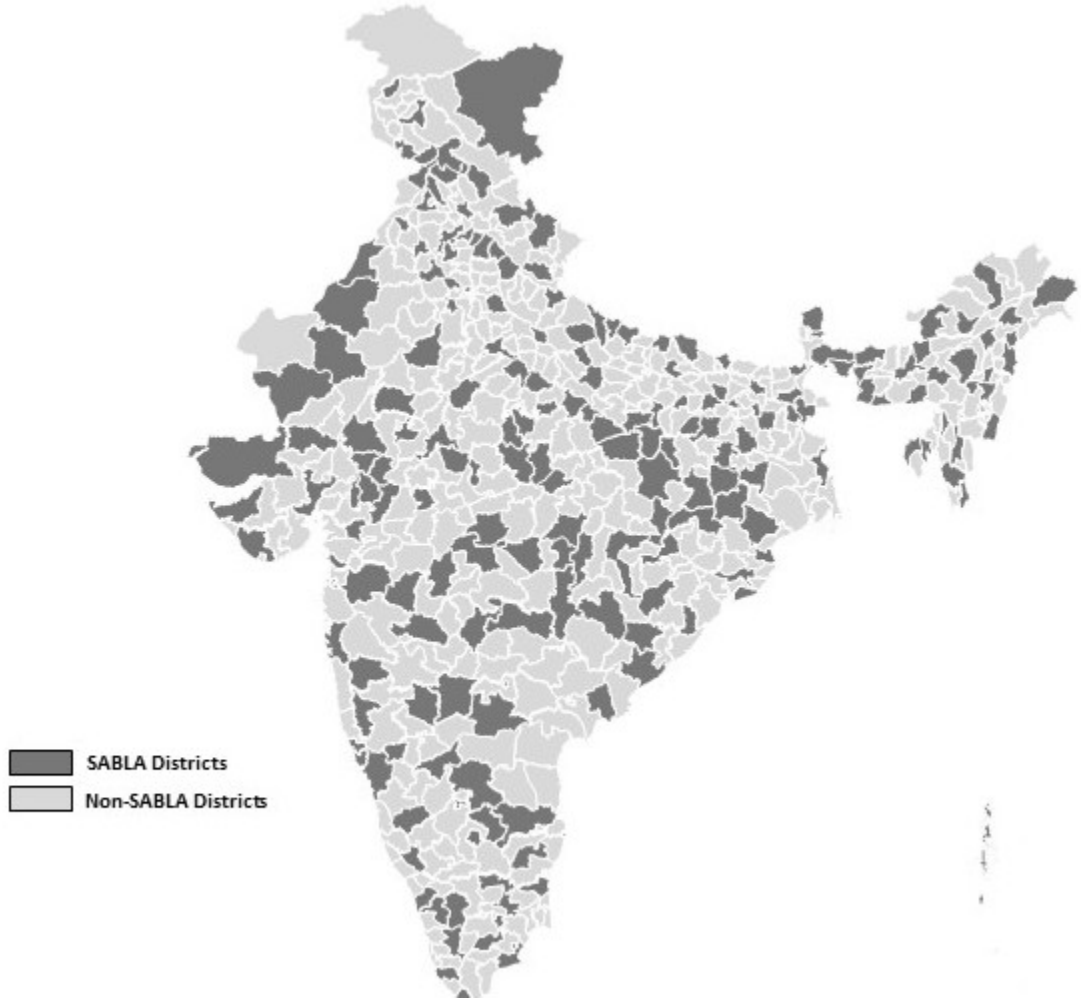
The scheme is implemented using the infrastructure provided by Integrated Child Development Services Scheme through Anganwadi Centres (AWCs)[7]. In cases where existing infrastructure is considered inadequate, alternative arrangements are made in schools or panchayat community halls to deliver the services under the programme. Under the scheme, at each AWC a group of 15-25 girls is formed, known as *Kishori Samooh*, within which the activities related to the programme are performed. Each group is headed by a *Sakhi* assisted by two *Saheli's*, selected from within the group on a rotational basis. Both the *Sakhi* and the *Saheli's* play an important leadership role within the group and facilitate peer to peer learning.

As discussed earlier, the SABLA program closely resembles the ELA intervention carried out by the NGO named BRAC. However, there are some striking dissimilarities which makes the case of SABLA particularly interesting for us and unique from a policy perspective. We discuss some of the main differences here. SABLA targeted girls in the age group of 11-18 years old whereas as per our understanding, ELA was given to girls aged 14-20 which makes the average cohort in SABLA younger compared to the ones in the ELA. Bandiera et al (2019) acknowledge the *practical difficulty* in enforcing age compliance in the ELA program due to high demand for vocational training. However, SABLA being a government initiative strictly enforced and verified age documents. Although, it is still possible that SABLA recipients misreport ages institutionally or through fake certification, the probability of compliance is higher for SABLA. The SABLA program provides for health and nutrition supplements such as iron, folic acid and cooked meals to the girls. We do not know of such features in the ELA. Moreover, SABLA provides health and education modules designed specifically for the age cohorts being addressed. For instance, the module for 11-14 year olds would be different from the modules for 15-18 year olds. Also, vocational training is only provided at

[7]Anganwadi Centres are an important part of public healthcare system in India and are responsible to provide basic healthcare services inside a village. Apart from their role as providers of basic healthcare services, they also serve as child care centres in rural India

a later stage for recipients of SABLA on attainment of age 16. Therefore, unlike ELA, it is a comprehensive tracking program over a 7 year span when the girl is growing up. To get rid of deep rooted problems like domestic violence, one would perhaps require such an extensive program over the years to not only change the perception of the growing women but also make them independent and strong.[8]

Figure 1: Map of India with SABLA districts



In Table 1 we summarize the SABLA program in a snapshot. Since this was a multi-dimensional program, we provide details of how each of the components were incorporated in terms of frequencies of delivery, target groups etc. The program reached out to both

[8]The term *sabla* from Indian languages literally translates to 'strong independent woman'.

Table 1: Components of SABLA program

	Frequency	Age group
Out of school girls		
Take home ration or hot cooked meal (atleast 600 calories and 18 gms protein)	300 days/ year	11-18
Iron Folic Acid Supplements	2-3 times/week	11-18
Health Check-up	2-3 times/ week	11-18
Nutrition and Health Education	2-3 times/ week	11-18
Counseling on family welfare, reproductive and sexual health, and child care practices	2-3 times/ week	11-18
Life Skills Education	2-3 times/ week	11-18
Vocational training	2-3 times/ week	16-18
In-school girls		
Take home ration or hot cooked meal (atleast 600 calories and 18 gms protein)	300 days/ year	14-18
Nutrition and Health Education	2 times/ month	11-18
Counseling on family welfare, reproductive and sexual health, and child care practices	2 times/ month	11-18
Life Skill Education	2 times/ month	11-18

Notes: Policy components as per the implementation guidelines provided to state governments and union territory administrators. For more information see: `https://wcd.nic.in/sites/default/files/1-SABLAscheme_0.pdf`.

in-school and out of school girls. As is clear from the table, there are certain variations in age limits for receiving the components and these also differ by in-school status.

3 Empirical Framework

3.1 Identification Strategy

To understand the impact of women empowerment on our outcomes of interest, as mentioned above, we need to understand the following functional relation:

$$Y = f(Women\_Empowerment, X_{obs}, X_{unobs})$$

This suggests that our outcome variable Y is a function of women empowerment and other observed and unobserved individual as well as household characteristics. The empirical estimation of the above functional relation using OLS is, however, problematic on account of several reasons. The primary reason is the presence of unobservables (X_{unobs}) as women who are empowered might be characteristically different from the ones who are not which might be causing differential outcomes in terms of domestic violence faced by women. Another potential concern is that of reverse causality as absence domestic violence faced might itself be responsible for making women feel empowered. Under such circumstances, usage of OLS is likely to yield us a biased estimate for the impact of women empowerment on domestic violence.

The ideal way to overcome the above issues would be a randomized assignment of women in treatment and control groups where the treated women have access to some support that potentially empowers them. With the control group lacking access to such a support, in the presence of a randomized assignment, a pre and post means comparison of the treatment and control group would ideally give us the causal impact of women empowerment on our outcomes of interest. However, using Randomized Control Trials (RCT) can be prohibitively costly in such cases. Under such circumstances, we make use of quasi-experimental design which exploits the exogenous variation generated on account of a multifaceted women

empowerment policy i.e. SABLA.

Table 2: Tracking Cohorts

Age in Survey Year	15	16	17	18	19	20	21	22	23 and above
Interviewed in 2015									
Age in Policy Year	11	12	13	14	15	16	17	18	19
Years of Exposure	4	4	4	4	3	2	1	0	0
Interviewed in 2016									
Age in Policy Year	10	11	12	13	14	15	16	17	18
Years of Exposure	4	5	5	5	4	3	2	1	0

Notes: Since the intervention was introduced near the end of the year 2010, the policy year has been taken as 2011.

The institutional features of the SABLA program provides an interesting source of geographic and cohort variation which is exogenous in nature. We exploit these features by using a difference-in-difference (DID) design to estimate our underlying model for the impact of women empowerment on domestic violence. Specifically, we employ a cross-sectional DID design as opposed to the more popular and generic time based DID design as the former helps us to exploit the key features of the policy for identification. The first dimension of our DID framework comes from the geographic variation in the implementation of the scheme. As discussed in the background section, the SABLA program was initially introduced only in 205 pilot districts and did not have a national roll-out, thereby giving us a classification of 'SABLA Districts' i.e. districts where the policy was introduced and 'Non-SABLA Districts' for where it wasn't. The second dimension of our DID framework comes from the cohort variation generated on account of policy's age eligibility rules. Again, as mentioned earlier, girls only in the age group of 11-18 are eligible for the policy support. With the policy introduced at the end of the year 2010 (or beginning 2011) and the survey years for our data being 2015 and 2016 (more on this in the data section), tracking girls exposed to the policy takes the highest current age of the affected cohort to 22 years. This can be understood more clearly through Table 2.

In Table 2, we map the survey year age of the girls to their age when the policy came into action in order to determine their eligibility for the policy. Since SABLA is a multi-dimensional program spread over the years, we also report the number of years a girl was potentially enrolled under the program. We see that, girls aged 22 in the survey year 2016 belonged to the first cohort that was exposed to the policy for at least an year. Girls aged 23 and above remained unexposed to the policy. So for the purpose of our analysis, we take girls in the age group of 23-28 as the unaffected cohort who serve as the potential control group for our study.

We then compare the cohort based differences in the outcomes of the 'Exposed' and 'Unexposed' women in the districts where the SABLA program was implemented or 'SABLA districts' to cohort based differences in outcomes of the 'Exposed' and 'Unexposed' women in the districts where it was not or the 'Non-SABLA districts'. The usage of such a framework is appealing as it also helps us to exploit the variation in the number of years of exposure to the policy which is also an important feature of the SABLA program.

Table 3: Identification Strategy

	Non-SABLA District	SABLA District
Unexposed Cohort (23-28 Years)	Not Eligible	Not Eligible
Exposed Cohort (15-22 Years)	Not Eligible	Eligible

The identifying assumption for our double difference framework is that the difference in the means of the outcomes for 'Exposed' cohort in a SABLA and Non-SABLA district would be no different from the difference in the same mean of outcomes for 'Non-Exposed' cohort in a SABLA and Non-SABLA district, in the absence of the SABLA policy program. While trying to estimate the causal impact of women empowerment on our outcomes of interest, we also take care of several other observable and unobservable differences between these cross-sections, so as to ensure that any effect picked up by the double difference estimation design

is on account of exposure to SABLA program. Table 3 clearly represents our identification strategy and shows that only women in the age cohort of 15-22 years residing in the SABLA districts were eligible for the program. The exogeneous nature of the policy coupled with the geographic and cohort variation in the implementation of the program helps us to assign treatment status for the purpose of our identification strategy.

Since women belonging to the 'Exposed' age cohort in the SABLA districts were eligible to participate in the program, we propose to study the impact of the program on domestic violence faced by women using an intent to treat analysis. For this we run the following regression equation of individual i belonging to household h from district d:

$$Y_{i,h,d} = \alpha_d + \beta_1 \cdot SABLA + \beta_2 \cdot (Exposed\_Cohort_i) + \gamma_1 \cdot X_i + \gamma_2 \cdot X_h + \epsilon \quad (1)$$

where , α_d is a dummy for district fixed effects. We use $Exposed\_Cohort_i$ as a dummy variable which takes the value 1 for the 'Exposed Cohort'. We use this discrete treatment status approach i.e. exposed vs. never exposed for our main specification as we believe that this helps to provide us a much clearer and simpler measure to understand impact of the exposure to the policy for the 'Exposed Cohort' [9]. We also generate a dummy $SABLA\_District_d$ which takes the value 1 for SABLA District. However, we do not use it in the final specification as it gets subsumed by α_d. The variable $SABLA$ captures the interaction of these two cross sectional dummy variables i.e. $Exposed\_Cohort_i$. $SABLA\_District_d$. Our coefficient of interest under the above specification is β_1 which captures the effect of exposure to SABLA policy program on our outcomes of interest i.e. Y. We include the controls for education, age, religion, number of children in the household, size of the household, age of the household head, sex of the household head, type of residence and wealth index which are represented by X. The controls at the individual and household level are captured by X_i and X_h, respectively.

[9]However, the variation generated on account of differential years of exposure for the 'Exposed Cohort' is also a source for useful information which we exploit subsequently for the purpose of our secondary identification strategy.

Using the identification strategy outlined above, we try to understand the impact of exposure to SABLA program on our outcomes of interest. We look at pure reduced form effects of the program as estimated in an ITT framework. Considering that take-up or non-compliance for the intervention can be a potential concern, we believe that the ITT design is suitable as it will provide us a conservative estimate of the effects.

We supplement the identification strategy outlined by Equation 1 with an additional identification strategy which also exploits the 'years of exposure to the policy' for an adolescent girl. As mentioned before, years of exposure provide us with a source of exogeneous variation as the enrolled girls were exposed to the benefits of the program for periods of different length. For instance, as given in Table 2 the maximum number of years a girl could be exposed to the policy was 5 years with the minimum being 0 years i.e. not exposed at all. We exploit this exogeneous variation generated on account of policy features to come up with our secondary identification strategy as represented in Equation 2:

$$Y_{i,h,d} = \alpha_d + \beta_1 \cdot (SABLA_d \cdot Years\_Exposed_i) + \beta_2 \cdot (Years\_Exposed_i) + \gamma_1 \cdot X_i + \gamma_2 \cdot X_h + \epsilon \tag{2}$$

where , α_d is a dummy for district fixed effects. The variable $Years\_Exposed_i$ is a continuous variable measuring the number of years an adolescent girl was exposed to the policy. The maximum value this valriable can take is 5 with the minimum being 0. For this identification strategy too, we generate a dummy $SABLA\_District_d$ which takes the value 1 for SABLA District. However, as before we omit it from the final specification as it gets subsumed by α_d. The variable $SABLA\ .\ Years\_Exposed$, which is an interaction of $Years\_Exposed_i$ and $SABLA\_District_d$, provides us with our coefficient of interest i.e. β_1. The coefficient gives us the impact of 'Continuous Treatment' or the years of exposure to the SABLA policy program. We use the same control variables as employed in Equation 1.

3.2 Data

The main objective of our paper is to understand the impact of exposure to SABLA on domestic violence experienced by women. The ideal data set in such a case would be one which provides information on domestic violence for a large number of women. Such data can potentially be collected by conducting a primary survey as it would give allow us to customize the survey questions according to our needs. However, such a survey comes at a prohibitive cost coupled with administrative issues in running the same. Hence, we shift our attention to secondary sources of data. There is no secondary data repository which focuses solely on providing dis-aggregated information at an individual level on domestic violence experienced by women at home. The National Crime Records Bureau (NCRB) provides information on some measures related to crimes committed against women at the district level. However, as per our identification strategy we need dis-aggregated data for women at an individual level coupled with information on their age. The data-set therefore does not meet the requirements of our identification design. The India Human Development Survey (IHDS) provides information on a national representative set of households over a wide variety of topics. The survey is conducted both at the individual and the household level and provides the advantage of a panel study design as the households can be tracked. But the survey does not cover any specific questions related to domestic violence faced by the women themselves even though some questions related to community norms have been included. An even bigger issue with using IHDS arises when we observe that the survey year is very close to the year of introduction of the policy. IHDS-Round 2, which we could potentially have served our needs, was collected during 2011-12 while the SABLA program was introduced near the end of year 2010. This leaves us not even one full year of policy exposure to see its impact.

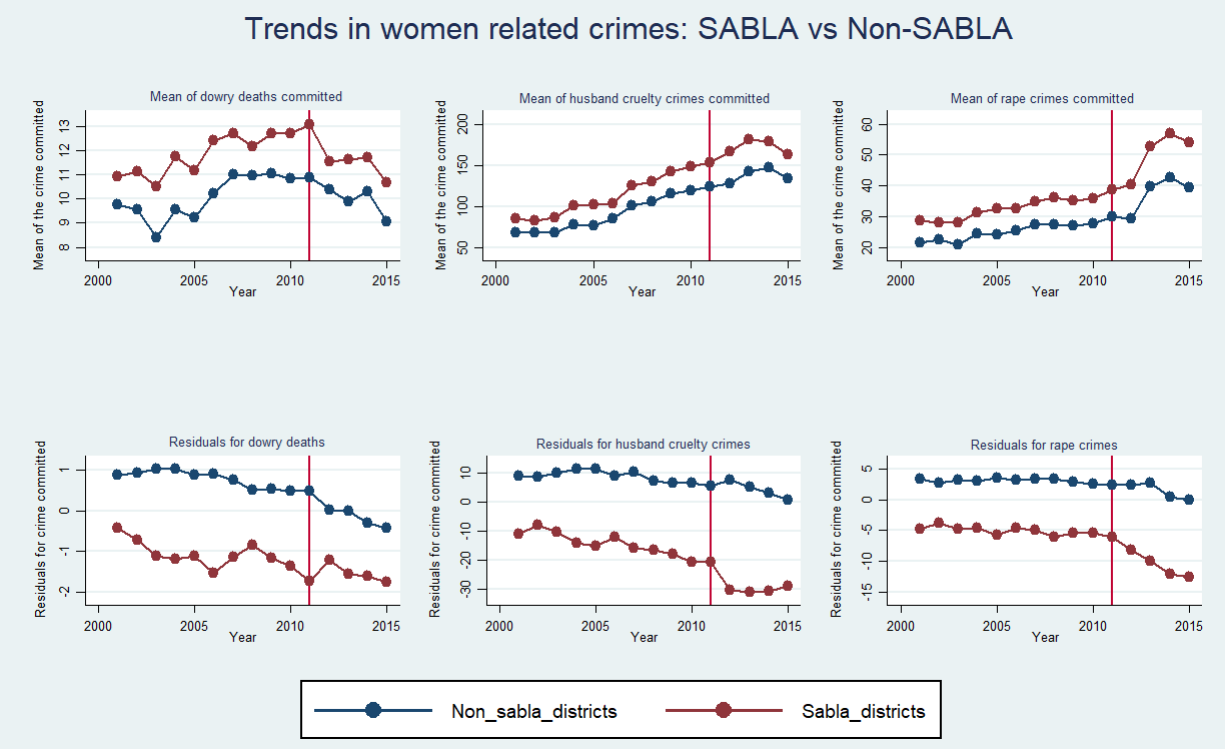
Under such constrains of data availability, we find that only National Family Health Survey (NFHS) meets the requirement of our study design. The NFHS is a multi-round survey which provides nationally representative information over a large sample of population. For instance, NFHS-Round 4 conducted in 2015-16 covers 601,509 households across India cou-

pled with individual level information on 699,686 women. This compares favorably to IHDS (2011-12) which provides information on 42,152 households coupled with individual level information on 39,253 women. Furthermore, NFHS provides information on our variable of interest i.e. domestic violence while also meeting the needs of our identification strategy in terms of providing information on the age and residential districts of the respondents. The main issue that we face with this survey is that the NFHS is not a panel survey and does not track households. Also, the NFHS Round 3 conducted in pre-policy year (2005-06), does not provide information on residential districts. Even though our identification strategy does not rely on pre-period data as we use a cross-sectional DID design, this still makes testing for parallel trends in outcomes difficult for us. We, therefore, rely on usage of NCRB data to get around this problem. Plotting the district level crime rates specifically for 'women related crimes' such as dowry deaths, husband cruelty and rapes in SABLA and Non-SABLA districts using the NCRB data helps to provide us with some evidence for the existence of parallel trends in pre-policy years. This comparative trend in 'women related crimes' is depicted in Figure 2.

The survey questions in NFHS (2015-16) provides detailed responses on our outcomes of interest. The survey contains categorical variables which capture intensity of different forms of domestic violence. For example, emotional violence includes instances of being humiliated, threatened or insulted by husband. Similarly, sexual violence includes instances of being physcially forced into unwanted sex, forced into unwanted sexual acts, and physically forced to perform unwanted sexual acts. It also covers information on physical violence which includes instances such as those of being pushed, slapped, kicked, arm-twisted, punched, strangled or threatened with a weapon by the husband[10]. For the purpose of our analysis, we construct two aggreagte measures for each type of violence along with domestic violence as a whole to understand the impact of the SABLA program. The first is, 'Faced a particular type of Domestic Violence' which is a dummy variable taking value 1 if the woman faced a particluar type of domestic violence. For instance, in case of emotional violence the variable

[10]Further details about exact questions asked as a part of the survey can be found in the Appendix.

Figure 2: Trends in women related crimes: SABLA vs Non-SABLA districts



'Faced Emotional Violence' will take value 1 if a woman has experienced any of the instances related to humiliation, threatening and insulting on part of husband. The variable takes value zero if a woman have not experienced any of the three instances. The variables for sexual and physical violence have been created in a similar manner. The second aggreagte measure that we create is the 'Index of a particular type of violence' which is a continuous variable measuring the number of questions on a particular type of violence answered in 'Yes'. For instance, the 'Emotional Violence Index' variable takes the value 2 if a woman was threatened and insulted but not humiliated by husband. Again, the variables for sexual and physical violence have been created in a similar manner. We also construct these two aggregate measure for Domestic Violence as a whole which includes answers on all the three types of Domestic Violence. So, if a woman answers 'yes' to being humiliated by husband, the dummy variable 'Faced Domestic Violence' would take value 1. Similarly, if the woman answers 'yes' to being 'insulted' and 'pushed' by the husband, the continuous variable 'Domestic Violence Index' takes value 2.

The reasons for presenting our analysis in this aggregated form are two fold. First, any measure of a particular type of violence is actually a representation of that type of violence. Also, sometimes there might be a subtle difference between any two measures. For instance, being humiliated or being insulted are much closer to each other in terms of similarity rather than dissimilarity. So disaggregated measures in this context are likely to provide a much more hazy picture as compared to the aggregate measures. Another reason behind using aggregate measures to understand different types of domestic violence is that it is a sensitive issue to tackle for the society as a whole. A victim of domestic violence as well as the society in general, is unlikely to be concerned about whether the the victim was being pushed or kicked by the husband. Any act of violence in itself is horrendous and derogatory and thus getting into details and studying different measures of a particular type of violence does not hold much rationale.

As mentioned earlier,we also have information on residing districts of the individual coupled with their current ages which helps us to track the cohort to the policy year in order to calculate the years of exposure to the policy. We also make use of several other variables such as education of the individual, religion of the individual, size of the household as controls as they may potentially bias our estimates.

Table 4 provides summary statistics for our outcome variables of interest i.e. domestic violence. We see that nearly 30% of women experienced some form of domestic violence which would constitute exposure to either emotional, sexual or physical violence. Nearly 26% of women have experienced some form of physical violence which is higher than the proportion of women who experienced emotional violence i.e. 12%. Around 7% of women were also exposed to sexual violence of some kind. These high rates of domestic violence experienced in India are much in line with those experienced in the developing world (Bobonis et al. 2013, Hidrobo et al. 2013 and Hidrobo et al. 2016). Coming to the Index variables, we see that women in the sample were likely to answer atleast one question on domestic violence experience in affirmative as given by the average value of 0.908 for Domestic Violence Index. For physical violence, we observe that on an average around 0.6 questions were answered

Table 4: Sample Means : Outcome Variables

		All (1)	Eligible (2)	Ineligible (3)	Ineligible (4)	Ineligible (5)
Faced Domestic Violence	No=0 Yes=1	0.299 (0.457)	0.261 (0.439)	0.272 (0.445)	0.306 (0.461)	0.309 (0.462)
Domestic Violence Index	Count of Questions Answered Yes	0.908 (1.950)	0.738 (1.729)	0.839 (1.888)	0.953 (2.011)	0.933 (1.968)
Faced Emotional Violence	No=0 Yes=1	0.118 (0.323)	0.101 (0.302)	0.113 (0.317)	0.126 (0.332)	0.118 (0.323)
Emotional Violence Index	Count of Questions Answered Yes	0.195 (0.594)	0.160 (0.530)	0.185 (0.577)	0.215 (0.631)	0.194 (0.591)
Faced Sexual Violence	No=0 Yes=1	0.066 (0.249)	0.064 (0.245)	0.067 (0.250)	0.069 (0.255)	0.065 (0.248)
Sexual Violence Index	Count of Questions Answered Yes	0.113 (0.470)	0.097 (0.415)	0.110 (0.455)	0.116 (0.473)	0.114 (0.480)
Faced Physical Violence	No=0 Yes=1	0.263 (0.440)	0.216 (0.412)	0.237 (0.425)	0.269 (0.443)	0.275 (0.446)
Physical Violence Index	Count of Questions Answered Yes	0.599 (1.268)	0.480 (1.140)	0.543 (1.217)	0.621 (1.301)	0.623 (1.284)
Observations		22,168	1,593	3,740	5,245	11,590
SABLA Age Eligibility			Yes	Yes	No	No
SABLA District			Yes	No	Yes	No

Notes: All columns represent data based on women's questionnaire of the NFHS-IV dataset. Only women selected for the domestic violence module by NFHS have been included in the sample for all the subsequent analysis. 'Eligible' consists of women who were in the eligible cohort for the policy and were also residing in a district chosen for SABLA implementation. 'Ineligible' consists of women in three categories: those who were not a part of the eligible cohort even though they resided in a SABLA district, those who were part of the eligible cohort but didn't reside in a SABLA district and those who were neither a part of the eligible cohort nor resided in a SABLA district.

Table 5: Sample Means : Control Variables

	Description	All (1)	Eligible (2)	Ineligible (3)	Ineligible (4)	Ineligible (5)
Place of Residence	Urban=1, Rural=2	1.716 (0.450)	1.695 (0.460)	1.742 (0.437)	1.674 (0.468)	1.723 (0.447)
Education in Single Years	Continuous Variable	8.128 (4.747)	8.744 (3.848)	8.586 (3.954)	7.861 (5.256)	7.654 (5.300)
Household Size	Continuous Variable	5.299 (2.206)	5.326 (2.170)	5.407 (2.243)	5.163 (2.185)	5.264 (2.194)
Children below 5years in household	Continuous Variable	0.793 (0.931)	0.409 (0.725)	0.434 (0.751)	1.060 (0.957)	1.104 (0.963)
Wealth Index	1=Poorest, 2=Poorer 3=Middle, 4=Richer 5=Richest	2.917 (1.356)	2.890 (1.385)	2.821 (1.333)	3.022 (1.392)	2.956 (1.365)
Age of household head	Continuous Variable	46.362 (15.092)	48.336 (13.839)	48.493 (13.588)	44.667 (15.790)	44.682 (16.032)
Sex of household head	1=Male 2=Female	1.145 (0.352)	1.156 (0.362)	1.150 (0.357)	1.143 (0.350)	1.138 (0.345)
Current Age	Continuous Variable	22.285 (3.981)	18.459 (2.214)	18.484 (2.217)	25.413 (1.865)	25.342 (1.865)
Religion	1=Hindu 0=Other Religion	0.732 (0.442)	0.731 (0.443)	0.720 (0.448)	0.735 (0.441)	0.741 (0.437)
Observations		35,012	4,812	10,843	6,070	13,287
SABLA Age Eligibility			Yes	Yes	No	No
SABLA District			Yes	No	Yes	No

Notes: All columns represent data based on women's questionnaire of the NFHS-IV dataset. Only women selected for the domestic violence module by NFHS have been included in the sample for all the subsequent analysis.'Eligible' consists of women who were in the eligible cohort for the policy and were also residing in a district chosen for SABLA implementation. 'Ineligible' consists of women in three categories: those who were not a part of the eligible cohort even though they resided in a SABLA district, those who were part of the eligible cohort but didn't reside in a SABLA district and those who were neither a part of the eligible cohort nor resided in a SABLA district.

in affirmative among the sampled women. The mean number of questions answered in affirmative for emotional and sexual violence were around 0.2 questions and 0.1 questions, respectively. We also report the subsample characteristcs for our outcome variables. on observation we see that the eligible women experienced lesser domestic violence across each category and in general, as compared to the Ineligible women. However, as we can't rely on a simple means comparison analysis of eligible and ineligible groups due to endogeneity issues, these results only serve as a precusor to our actual findings.

We also report sample means for individual and household control variables used in our analysis in Table 5. We see that nearly 71% of our respondents reside in rural areas. They were likely to have 8 years of education on an average. Nearly 73% of respondents belonged to Hindu religion. The current age of sampled women repondents stood at around 22 years. With respect to household characteristics we observe that respondents resided in an average household size of 5.3 members. They were likely to have around 1 child who was less than 5 years, at their house. Only a small proportion of households (around 14%) had a female head with the average age of a household head standing at 46.3 years. Women surveyed in our sample belonged to a middle income household on an average. On observation we see that the subsample characteristics are not very different across eligible and ineligible households. Some differences, however, can be observed in 'Current age' and 'Children below five years of age' categories which are only on account of identification strategy and do not present a threat for the purpose of our analysis.

4 Results

We report the results from our regressions in this section. The results for both 'discrete treatment' and 'continuous treatment' as outined in our section on identification strategy are reported. First we analyse the results for incidence of domestic violence, in general. We then report and analyse results for different categories of domestic violence, i.e. emotional violence, sexual violence and physical violence. We then follow our main findings with a

subsection on treatment effects based on schooling status. This is important as the benefits provided under SABLA program varied significantly based on the schooling status of women (or adolescent girls). The results in this section present estimates of β_1 from equation 1 and equation 2 using distict and state fixed effects coupled with robust standard errors clustered at district level.

4.1 Main Findings

Table 6 reports the results for domestic violence of any type. Column 1 and Column 2 show the impact of SABLA on the likelihood of facing domestic violence using district and state fixed effects, respectively. We find that coefficient estimate for SABLA is negative although imprecise in both the cases implying that the policy did not impact the likelihood of facing domestic violence, in general. Column 3 and Column 4 show the impact of SABLA on Domestic violence Index using district and state fixed effects, respectively. The results are significant for both discrete and continuous treatment cases. For discrete treatment, the point estimate is around -0.17 which translates into an effect of 18%. This means that women exposed to SABLA were likely to answer 18% lesser questions on domestic violence in affirmative. This tells us that SABLA policy programme helped reduce domestic violence. When we look at the continuous treatment specification we find the point estimate to reduce to -0.045, although still significant. Under this specification, we find that exposure to SABLA reduced the number of questions answered in affirmative by 4%. All in all, we find some evidence for the positive impact of SABLA in reducing domestic violence. The results, however, become more clear as we delve into the analysis of different types of domestic violence.

Table 7 reports the results for the impact of SABLA on emotional violence. Column 1 and Column 2 show that the policy had a positive impact on the likelihood of having ever faced emotional violence. Under discrete treatment case, we find that, women exposed to SABLA were less likely to have faced emotional violence when compared to women who were not exposed to the program. The point estimate for the same is around -0.02 which

Table 6: Effect on Domestic Violence of any type

	Faced Domestic Violence		Domestic Violence Index	
	(1)	(2)	(3)	(4)
Discrete Treatment				
SABLA	-0.019	-0.022	-0.177***	-0.172***
	(0.015)	(0.015)	(0.066)	(0.065)
R^2	0.13	0.09	0.11	0.07
Observations	22,168	22,168	22,168	22,168
District FE	Yes	No	Yes	No
State FE	No	Yes	No	Yes
Continuous Treatment				
SABLA	-0.008	-0.008	-0.045*	-0.043*
	(0.005)	(0.005)	(0.024)	(0.024)
R^2	0.13	0.09	0.11	0.07
Observations	22,168	22,168	22,168	22,168
District FE	Yes	No	Yes	No
State FE	No	Yes	No	Yes

Notes: All columns represent different regressions based on women's questionnaire of the NFHS-IV dataset. The point estimates for the coefficient of interest, $SABLA$, is the estimated β_1 from equation 1 and equation 2. It represents the impact of exposure to SABLA on outcomes of interest. The outcome variable 'Faced Domestic Violence' is a dummy measuring whether the respondent ever faced domestic violence and is constructed using the answers of the domestic violence module. The outcome variable 'Domestic Violence Index' is measured as the count of the domestic violence questions answered 'yes' by the respondent. The questions included for creating these variables are recorded in the Appendix. All regressions results use district-fixed effects as well as state fixed effects and control for baseline dummy variable $Exposed\_Cohort_i$. Additional variables in the regression model include both individual and household level controls such as age, education in single years, religion, age of household head, sex of household head, family size, number of children below or equal to 5 years of age in household, wealth index and type of place of residence. Robust standard errors clustered at the district level are reported in parentheses. *** p<0.01 **p<0.05 *p<0.1

translated into an effect of 17%. The point estimates for the continuous treatment case are also negative although imprecise. Analysing Emotional Violence Index results, we find that women who benefitted from the SABLA program were likely to answer lesser number of questions on emotional violence in affirmative than those who didn't benefit from the program. For discrete treatment case the point estimate is -0.05 which translates into an effect of around 26%. The point estimate for continuous treatment case is smaller at -0.01 translating into an effect of 5%, although still significant. The above results suggest that SABLA had a positive impact on reducing emotional violence which is robust to alternative specifications as well as alternate measures of emotional violence.

We now move on to Table 8 which reports the results for the impact of SABLA on sexual violence faced by women. For discrete treatment case, Column 1 and Column 2 show that women impacted by the policywere no more likelier to have faced lesser sexual violence than women who were not exposed to the policy. The point estimates for the same, though insignificant, point in the negative direction. The results do not change for the continuous treatment case. For Sexual Violence Index too, we find that women exposed to SABLA were no more likelier to have answered lesser questions in affirmative on sexual violence than those who were not exposed to the policy. As in the earlier case, the point estimate is negative though insignificant across both discrete and continuous treatment cases. We, therefore, do not find any significant impact of SABLA program on sexual violence.

Table 9 reports the results for the impact of SABLA on physical violence faced by women. Column 1 and Column 2 show that the policy had a positive impact in reducing the likelihood of ever facing physical viiolence for women who benfefitted from SABLA program when compared to those who didn't. The point estimate for the discrete treatment case is around -0.03 which translates into an effect of 11%. The point estimate for continuous treatment case is lower at -0.01 with an effect size of 3%. The point estimates for both the cases are, however, significant. Next we analyse the results for Physical violence Index reported in Column 3 and Column 4. The results show a positive impact of program on reducing the number of questions on physical violence answered in affirmative. This implies that women exposed

Table 7: Effect on Emotional Violence

	Faced Emotional Violence		Emotional Violence Index	
	(1)	(2)	(3)	(4)
Discrete Treatment				
$SABLA$	-0.022*	-0.024**	-0.055***	-0.053***
	(0.011)	(0.011)	(0.020)	(0.020)
R^2	0.07	0.03	0.06	0.03
Observations	22,168	22,168	22,168	22,168
District FE	Yes	No	Yes	No
State FE	No	Yes	No	Yes
Continuous Treatment				
$SABLA$	-0.004	-0.006	-0.013*	-0.013*
	(0.004)	(0.004)	(0.007)	(0.007)
R^2	0.07	0.03	0.06	0.03
Observations	22,168	22,168	22,168	22,168
District FE	Yes	No	Yes	No
State FE	No	Yes	No	Yes

Notes: All columns represent different regressions based on women's questionnaire of the NFHS-IV dataset. The point estimates for the coefficient of interest, $SABLA$, is the estimated β_1 from equation 1 and equation 2. It represents the impact of exposure to SABLA on outcomes of interest. The outcome variable 'Faced Emotional Violence' is a dummy measuring whether the respondent ever faced emotional violence and is constructed using the answers of the emotional violence module. The outcome variable 'Emotional Violence Index' is measured as the count of the emotional violence questions answered 'yes' by the respondent. The questions included for creating these variables are recorded in the Appendix. All regressions results use district-fixed effects as well as state fixed effects and control for baseline dummy variable $Exposed\_Cohort_i$. Additional variables in the regression model include both individual and household level controls such as age, education in single years, religion, age of household head, sex of household head, family size, number of children below or equal to 5 years of age in household, wealth index and type of place of residence. Robust standard errors clustered at the district level are reported in parentheses. *** p<0.01 **p<0.05 *p<0.1

Table 8: Effect on Sexual Violence

	Faced Sexual Violence		Sexual Violence Index	
	(1)	(2)	(3)	(4)
Discrete Treatment				
$SABLA$	-0.010	-0.010	-0.021	-0.021
	(0.008)	(0.008)	(0.015)	(0.020)
R^2	0.07	0.03	0.06	0.02
Observations	22,168	22,168	22,168	22,168
District FE	Yes	No	Yes	No
State FE	No	Yes	No	Yes
Continuous Treatment				
$SABLA$	-0.003	-0.003	-0.006	-0.006
	(0.003)	(0.003)	(0.005)	(0.005)
R^2	0.07	0.03	0.06	0.02
Observations	22,168	22,168	22,168	22,168
District FE	Yes	No	Yes	No
State FE	No	Yes	No	Yes

Notes: All columns represent different regressions based on women's questionnaire of the NFHS-IV dataset. The point estimates for the coefficient of interest, $SABLA$, is the estimated β_1 from equation 1 and equation 2. It represents the impact of exposure to SABLA on outcomes of interest. The outcome variable 'Faced Sexual Violence' is a dummy measuring whether the respondent ever faced sexual violence and is constructed using the answers of the sexual violence module. The outcome variable 'Sexual Violence Index' is measured as the count of the sexual violence questions answered 'yes' by the respondent. The questions included for creating these variables are recorded in the Appendix. All regressions results use district-fixed effects as well as state fixed effects and control for baseline dummy variable $Exposed\_Cohort_i$. Additional variables in the regression model include both individual and household level controls such as age, education in single years, religion, age of household head, sex of household head, family size, number of children below or equal to 5 years of age in household, wealth index and type of place of residence. Robust standard errors clustered at the district level are reported in parentheses. *** p<0.01 **p<0.05 *p<0.1

Table 9: Effect on Physical Violence

	Faced Physical Violence		Physical Violence Index	
	(1)	(2)	(3)	(4)
Discrete Treatment				
SABLA	-0.028*	-0.029*	-0.101**	-0.097**
	(0.015)	(0.015)	(0.042)	(0.041)
R^2	0.14	0.09	0.12	0.08
Observations	22,168	22,168	22,168	22,168
District FE	Yes	No	Yes	No
State FE	No	Yes	No	Yes
Continuous Treatment				
SABLA	-0.010*	-0.009*	-0.026*	-0.023
	(0.005)	(0.005)	(0.015)	(0.014)
R^2	0.14	0.09	0.12	0.08
Observations	22,168	22,168	22,168	22,168
District FE	Yes	No	Yes	No
State FE	No	Yes	No	Yes

Notes: All columns represent different regressions based on women's questionnaire of the NFHS-IV dataset. The point estimates for the coefficient of interest, $SABLA$, is the estimated β_1 from equation 1 and equation 2. It represents the impact of exposure to SABLA on outcomes of interest. The outcome variable 'Faced Physical Violence' is a dummy measuring whether the respondent ever faced physical violence and is constructed using the answers of the physical violence module. The outcome variable 'Physical Violence Index' is measured as the count of the physical violence questions answered 'yes' by the respondent. The questions included for creating these variables are recorded in the Appendix. All regressions results use district-fixed effects as well as state fixed effects and control for baseline dummy variable $Exposed\_Cohort_i$. Additional variables in the regression model include both individual and household level controls such as age, education in single years, religion, age of household head, sex of household head, family size, number of children below or equal to 5 years of age in household, wealth index and type of place of residence. Robust standard errors clustered at the district level are reported in parentheses. *** p<0.01 **p<0.05 *p<0.1

to SABLA were likely to answer lesser number of physical violence questions in affirmative than the women who were not exposed to the SABLA policy program. The point estimate for Physical Violence Index in case of discrete treatment is around-0.1 which translates into an effect size of around 17%. The point estimate for continuous treatment is again smaller at -0.02, an effect size of 3% , but significant for district fixed effects specification. The results suggest a positive impact of SABLA on reducing physical violence which is remains largely robust to alternative specifications and alternate measures of physical violence.

4.2 Treatment Effects based on Schooling Status

Existing literature has shown mixed effects of education on domestic violence. While several studies report a positive impact of education on reducing domestic violence (Farmer and Tiefenthaler 1997, Ackerson et al. 2008, Vyas and Watts 2009, Eswaran and Malhotra 2011, Anderberg et al. 2016), some recent studies (Erten and Keskin 2018) also show adverse impact of education on reducing domestic violence. The reduction in domestic violence with an improvement in education level has been seen mainly on account of improvement in 'outside options' due to better labour market outcomes. The argument for increase in domestic violence due to improvement in education resides on an increased usage of threat for domestic violence by husbands in wake of higher labor market earnings of the women who have a higher level of schooling.

The benefits obtained under the SABLA programe, as mentioned in Table 1, differ significantly based on the schooling status of an eligible adolescent girl. For instance, iron and folic acid supplements, health check up and referral services and, vocational training under the program is only available to out of school girls and not to in-school girls. Similarly, the out of school girls recieved policy benefits more frequently when compared to in-school girls. The out of school girls received nutrition and health education, counseling on famiy welfare and, life skill education 2-3 times a week when comared to in-school girls who received the same benefits 2 times a month. The difference in policy benefits can therefore result in differences in outcomes related to domestic violence for treated women. We there perfrom

Table 10: Treatment Effects based on Schooling Status

	Domestic Violence				Emotional Violence				Sexual Violence				Physical Violence			
	Faced DV		DV Index		Faced EV		EV Index		Faced SV		SV Index		Faced PV		PV Index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Out of School Girls																
SABLA	-0.003	0.001	-0.090	-0.014	-0.009	-0.005	-0.045	-0.013	0.015	0.008	0.025	0.014	-0.023	-0.003	-0.070	-0.016
	(0.029)	(0.010)	(0.141)	(0.049)	(0.022)	(0.007)	(0.043)	(0.015)	(0.018)	(0.006)	(0.035)	(0.013)	(0.029)	(0.010)	(0.090)	(0.030)
R^2	0.14	0.14	0.14	0.13	0.12	0.11	0.11	0.11	0.11	0.11	0.11	0.11	0.14	0.14	0.14	0.14
Observations	8,139	8,139	8,139	8,139	8,139	8,139	8,139	8,139	8,139	8,139	8,139	8,139	8,139	8,139	8,139	8,139
Discrete Treatment	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Continuous Treatment	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
In-School Girls																
SABLA	-0.026	-0.012*	-0.262***	-0.071**	-0.029**	-0.004	-0.065**	-0.014	-0.032***	-0.011***	-0.061***	-0.021***	-0.030	-0.013*	-0.135***	-0.035*
	(0.020)	(0.007)	(0.083)	(0.030)	(0.014)	(0.005)	(0.026)	(0.009)	(0.011)	(0.006)	(0.035)	(0.013)	(0.019)	(0.006)	(0.051)	(0.018)
R^2	0.14	0.14	0.12	0.12	0.09	0.09	0.08	0.08	0.08	0.08	0.08	0.08	0.14	0.14	0.13	0.13
Observations	11,664	11,664	11,664	11,664	11,664	11,664	11,664	11,664	11,664	11,664	11,664	11,664	11,664	11,664	11,664	11,664
Discrete Treatment	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Continuous Treatment	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Notes: All columns represent different regressions based on women's questionnaire of the NFHS-IV dataset. The point estimates for the coefficient of interest, *SABLA*, is the estimated β_1 from equation 1 and equation 2. It represents the impact of exposure to SABLA on outcomes of interest. For the purpose of this table, DV stands for Domestic Violence, EV stands for Emotional Violence, SE stands for Sexual Violence and PE stands for Physical Violence. All regressions results use district-fixed effects and control for baseline dummy variable $Exposed\_Cohort_i$. Additional variables in the regression model include both individual and household level controls such as age, education in single years, religion, age of household head, sex of household head, family size, number of children below or equal to 5 years of age in household, wealth index and type of place of residence. Robust standard errors clustered at the district level are reported in parentheses. *** p<0.01 **p<0.05 *p<0.1

sub-sample analysis to understand whether differential schooling status had any impact on outcomes experienced with regard to domestic violence.

As evident from Table 10, the positive impact of SABLA on domestic violence was largely restricted to girls who were in school at the time of the policy. This is true both for domestic violence as well as its various types such as emotional violence, sexual violence and physical violence. The effects for these in-school girls are robust to alternate specifications and measures of recording domestic violence. We find no impact of SABLA on domestic violence faced by out of school girls. The result is much in line with Hidrobo and Fernald (2013) who find that the Ecuador's cash tranfer program reduced physchological violence only for women whose education levels were higher that primary standard.

A possible mechanism which might cause the impact of SABLA to vary depending on the schooling status of the adolescent girls is that it provides credibility to 'outside' options (Hidrobo and Fernald, 2013). A more educated woman is more likely to exercise her labour market options credibly. This extra credibility earned on account of education might be the reason that girls impacted by SABLA experience lesser domestic violence. Another possible reason is that schooling might generate complematarities for SABLA as in-school girls will be able to gather more and relate to the health and nutritional education, life skill education and family welfare education imparted as part of SABLA. This would make them much more knowledgeable about their rights and when coupled with credibility of education is likely to result in lower incidence of domestic violence.

5 Potential Mechanisms

The preceeding section provides us with a clear reduced-form result: a multifaceted women empowerment policy helps to reduce domestic violence faced by women. The possible mechanism for this result, however, still remains unclear. Improved labor market opportunities on account of vocational training coupled with better health and educational endownment as a result of benefits under SABLA can be one of the potential channels. This is due to

the improved 'outside' options which might provide improved bargaining power to women in the household, thus leading to reduction in domestic violence faced by her (Tauchen et al. 1991, Farmer and Tiefenthaler 1996). However, we do not find any evidence for the same in our analysis[11].

Bloch and Rao (2002), for the Indian context, show that a husbands' level of satisfaction with the marriage acts as an important indicator of domestic violence. The authors show that dissatisfied husbands have a higher incentive to engage in domestic violence than satisfied husbands. Furthermore, an important manisfestation of this level of satisfaction with the marriage is that the satisfied husbands are more likely to maintain ties with the wife's family than the dissatisfied husbands, especially when they do not receive any additional financial transfers from her family[12].

With the authors' context being similar to ours, it is possible that the positive effects of SABLA on women in the form of better knowledge and information about family welfare practices, child care practices, reproductive and sexual health practices and life skills help to increase the husbands' private level of satisfaction with the marriage thereby causing them to engage in less domestic violence[13]. If that's the case then these husbands would be likely to demand less financial transfer payements from the brides' family after the marriage and would also be less likely to severe ties from them. We, however, do not have information on dowry or financial payments in our dataset but we do observe some information availability in respect of husbands attitude towards his in-laws.

Table 11 reports the effect of SABLA on the controlling behaviour expressed by the husband. We find that husbands' of the women who were positively impacted by SABLA were less likely to limit their contact with their family (around 16%). This implies that the grooms' of the affected women had a more accepting attitude towards their in-laws as compared to the grooms' of the women who were not affected by the policy. Coupled with the above result, we also find evidence that the husbands' of the affected women were more

[11] Results available upon request.

[12] Bloch and Rao (2002) model domestic violence as a bargaining instrument used by the groom in India to extract a higher level of dowry and additional financial payments from the bride's family.

[13] Bloch and Rao (2002) show that the husbands' private level of satisfaction with the marriage is only revealed post marriage.

Table 11: Effect on Husband's Controlling Behavior

	Discrete Treatment		Continuous Treatment	
	(1)	(2)	(3)	(4)
Control Behavior Index	-0.074	-0.088*	-0.036**	-0.039**
	(0.047)	(0.047)	(0.018)	(0.018)
Husband jealous when talk to men	-0.002	-0.012	-0.003	-0.007
	(0.015)	(0.015)	(0.005)	(0.005)
Husband accuses of unfaithfulness	-0.009	-0.013	-0.002	-0.003
	(0.010)	(0.010)	(0.003)	(0.003)
Husband does not allow meeting female friends	-0.002	-0.002	-0.005	-0.004
	(0.012)	(0.012)	(0.004)	(0.004)
Husband limits contact with family	-0.025*	-0.026**	-0.008*	-0.008*
	(0.013)	(0.012)	(0.004)	(0.004)
Husband wants to know location	-0.003	-0.007	-0.002	-0.004
	(0.013)	(0.013)	(0.005)	(0.005)
Husband doesn't trust with money	-0.032**	-0.027*	-0.014***	-0.011**
	(0.014)	(0.014)	(0.005)	(0.005)
District FE	Yes	No	Yes	No
State FE	No	Yes	No	Yes

Notes: All columns represent different regressions based on the women's questionnaire of the NFHS-IV dataset. The point estimates for the coefficient of interest, $SABLA$, is the estimated β_1 from equation 1 and equation 2. It represents the impact of exposure to SABLA on outcomes of interest. The outcome variable 'Control Behavior Index' is measured as the count of the Control Behavior questions answered 'yes' by the respondent. The other outcome variables include the questions used for creating this variable. All regressions results use district-fixed effects as well as state fixed effects and control for baseline dummy variable $Exposed\_Cohort_i$. Additional variables in the regression model include both individual and household level controls such as age, education in single years, religion, age of household head, sex of household head, family size, number of children below or equal to 5 years of age in household, wealth index and type of place of residence. Robust standard errors clustered at the district level are reported in parentheses. *** p<0.01 **p<0.05 *p<0.1

Table 12: Effect on Help-seeking Behaviour

	Own Family		In-Law's		No Help sought	
	(1)	(2)	(3)	(4)	(5)	(6)
Discrete Treatment						
$SABLA$	0.024*	0.021	0.0001	0.005	-0.026	-0.026
	(0.014)	(0.014)	(0.008)	(0.008)	(0.017)	(0.016)
R^2	0.10	0.01	0.09	0.01	0.10	0.01
Observations	8,874	8,874	8,874	8,874	8,874	8,874
District FE	Yes	No	Yes	No	Yes	No
State FE	No	Yes	No	Yes	No	Yes
Continuous Treatment						
$SABLA$	0.006	0.005	0.001	0.002	-0.007	-0.006
	(0.004)	(0.004)	(0.002)	(0.002)	(0.005)	(0.005)
R^2	0.10	0.01	0.09	0.01	0.10	0.01
Observations	8,874	8,874	8,874	8,874	8,874	8,874
District FE	Yes	No	Yes	No	Yes	No
State FE	No	Yes	No	Yes	No	Yes

Notes: All columns represent different regressions based on women's questionnaire of the NFHS-IV dataset. The point estimates for the coefficient of interest, $SABLA$, is the estimated β_1 from equation 1 and equation 2. It represents the impact of exposure to SABLA on outcomes of interest. The outcome variable 'Own Family' is a dummy measuring whether the respondent went to her own family for help to stop violence. The outcome variable 'Own Family' is a dummy measuring whether the respondent went to her own family for help to stop violence. The outcome variable 'In-Law's' is a dummy measuring whether the respondent went to her in-law's for help to stop violence. All regressions results use district-fixed effects as well as state fixed effects and control for baseline dummy variable $Exposed\_Cohort_i$. Additional variables in the regression model include both individual and household level controls such as age , education in single years, religion, age of household head, sex of household head, family size, number of children below or equal to 5 years of age in household, wealth index and type of place of residence. Robust standard errors clustered at the district level are reported in parentheses. *** p<0.01 **p<0.05 *p<0.1

likely to trust them with the money matters. This gives us some sense that money is less likely to be a bone of contention among couples where women were affected by SABLA in their adolescence. The overall results represented by 'Control Behavior Index' also show some evidence of reduced husband control on affected women when compared with unaffected women.

We also try to understand if the help seeking behavior of women, when faced with violence, change in reponse to exposure to SABLA. Table 12 reports the results for help-seeking behavior of women. Although most of the results turn out to be imprecise, we still find that the point estimates suggest improved help seeking behavior among affected women. One particular interesting result suggests that the affected women were more likely to approach their own families for help regarding domestic violence than unaffected women. This also helps to reiterate our earlier point that husbands of women affected by SABLA were more likely to allow them to keep contact with their families due to a higher level of satisfaction with their marriage.

Another possible reason for reduced domestic violence faced by 'SABLA women' could be a change in their attitude towards such violence as a result of exposure to the policy. This is possible as better knowledge and information services provided under SABLA might have given women a better understanding about their rights thereby resulting in their transformed attitude towards domestic violence. These women would then be much less likely to have an accepting attitude towards domestic violence. If this happens, then there is a possibility that such women experience less domestic violence simply on account of their changed views (Dasgupta 2019). Existing literature has looked at *updated* women attitudes as a potential mechanism in impacting domestic violence (Erten and Keskin 2018, Haushofer et al. 2018). We, however, do not find any evidence for the same in our analysis[14].

Other than the attitudes of women, mens' (or husbands') attitudes about domestic violence and gender roles can also have an impact on violence inflicted by them on their wives. As the perpetrator of violence, if the attitude of husband towards domestic violence changes

[14]Results available upon request.

Table 13: Effect on Husband's Attitude towards Domestic Violence

	Discrete Treatment		Continuous Treatment	
	(1)	(2)	(3)	(4)
Domestic Violence Attitude Index	-0.035	-0.037	-0.016*	-0.017*
	(0.023)	(0.024)	(0.008)	(0.008)
Domestic Violence justified if wife unfaithful	-0.014	-0.018	-0.006	-0.007*
	(0.013)	(0.012)	(0.004)	(0.004)
Domestic violence justified if wife disrespects	-0.018	-0.017	-0.009*	-0.009*
	(0.013)	(0.014)	(0.005)	(0.005)
District FE	Yes	No	Yes	No
State FE	No	Yes	No	Yes

Notes: All columns represent different regressions based on the men's questionnaire of the NFHS-IV dataset. The point estimates for the coefficient of interest, $SABLA$, is the estimated β_1 from equation 1 and equation 2. It represents the impact of exposure to SABLA on outcomes of interest. The outcome variable 'Domestic Violence Attitude Index' is measured as the count of the domestic violence attitude questions answered 'yes' by the respondent. The other outcome variables include the questions used for creating this variable. All regressions results use district-fixed effects as well as state fixed effects and control for baseline dummy variable $Exposed\_Cohort_i$. Additional variables in the regression model include both individual and household level controls such as age, education in single years, religion, age of household head, sex of household head, family size, number of children below or equal to 5 years of age in household, wealth index and type of place of residence. Robust standard errors clustered at the district level are reported in parentheses. *** p<0.01 **p<0.05 *p<0.1

then he is likely to inflict a lower level of domestic violence on his wife (Green et al. 2017, Dhar et al. 2018 and Haushofer et al. 2018). We, therefore, try to understand if SABLA had any impact on attitudes of husbands' towards domestic violence. Table 13 reports these results. We find some evidence that husbands' whose wives were potentially affected by SABLA at an adolescent age were less likely to have an accepting attitude towards domestic violence. We observe that these husbands' were less likely to justify domestic violence on account of infidelity or disrespecting attitude of their wives. Again, this improved attitude of husbands' towards domestic violence is not a direct impact of policy as men were never the intended beneficiaries of SABLA. However, improved private level of satisfaction with the marriage on account of *well informed* wives might be the reason behind this attitude.

6 Robustness Check

In this section, we try to find additional evidence to ensure that the reduced form results generated by our identification strategy actually represent the true causal effect of SABLA. This is important in order to make sure that our identification strategy represented by an interaction of SABLA cohort and SABLA district actually makes sense and is not giving us a spurious result. In order to generate this additional evidence we perform a test for random simulation of treatment status or *exact randomization* (Bharadwaj et al. 2014, Chatterjee and Poddar 2019).

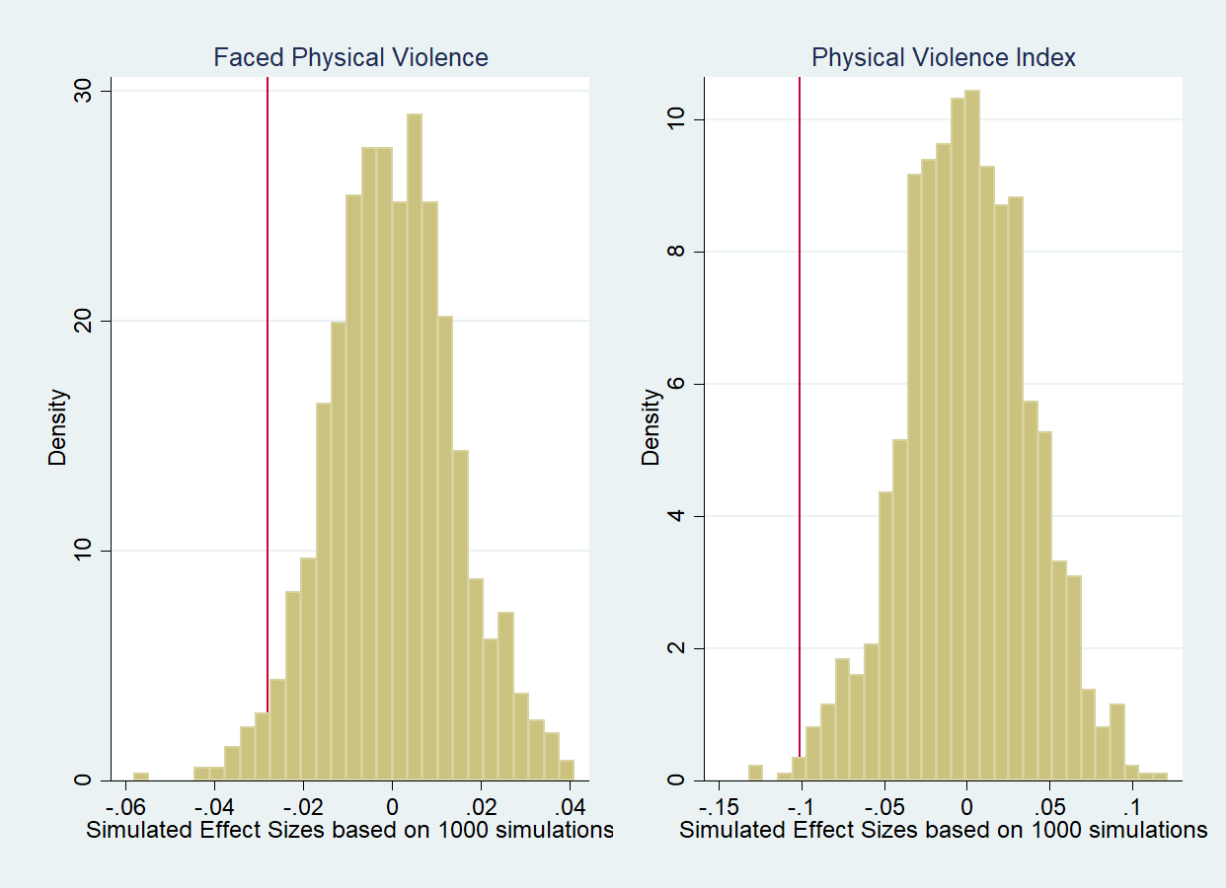
To perform the test we run two specific simulations. For the first simulation, we randomly assign districts a SABLA and Non-SABLA status instead of using the SABLA districts as mentioned under the policy. We then run the regression as stated in equation 1 for one of our primary outcome variables, for instance, physical violence[15]. We then repeat this exercise 1000 times and record the results. If the critical identifying assumption of our empirical analysis holds i.e. apart from the effect of SABLA, the difference between eligible age cohort and non-eligible age cohort should not be different in SABLA - Non SABLA districts, then most of the results from this randomization of districts into SABLA and Non-SABLA category would yield us insignificant or imprecise results. Coupled with that, the effect sizes (even for imprecise estimates) are likely to be smaller in magnitude. This is because these estimates do not represent true effect of SABLA and merely serve as a placebo experiment.

When we calculate the results for the above simulation we find that for our outcome variable *Faced Physical Violence* only 5.8% and 10.9% results come out to be significant at 95% and 90% confidence intervals, respectively. This is only a small as well as expected proportion and gives us some confidence that our identification design is actually picking up the true causal effect and not giving us a spurious result. We further observe the distribution of coefficients from this exercise as given in Figure 3 and find that most of the coefiicients are centered around zero and are much smaller in magnitude compared to our estimated

[15]We perform this exercise for our other outcome variables too with a similar effect. Results for the same are available on request.

coefficient of -0.028 (represented by a solid red line). For our other outcome variable for physical violence, *Physical Violence Index*, we again observe similar results. We find that only 5.4% and 9.8% results come out to be significant at 95% and 90% confidence intervals, respectively. This is again on expected lines and does not caste any doubt on our identification strategy. The distribution of coefficents again show that our true causal estimate, -0.101, is much larger and in the right direction when compared to the simulated coefficients which are mainly centered around zero.

Figure 3: Physical Violence: District Randomization



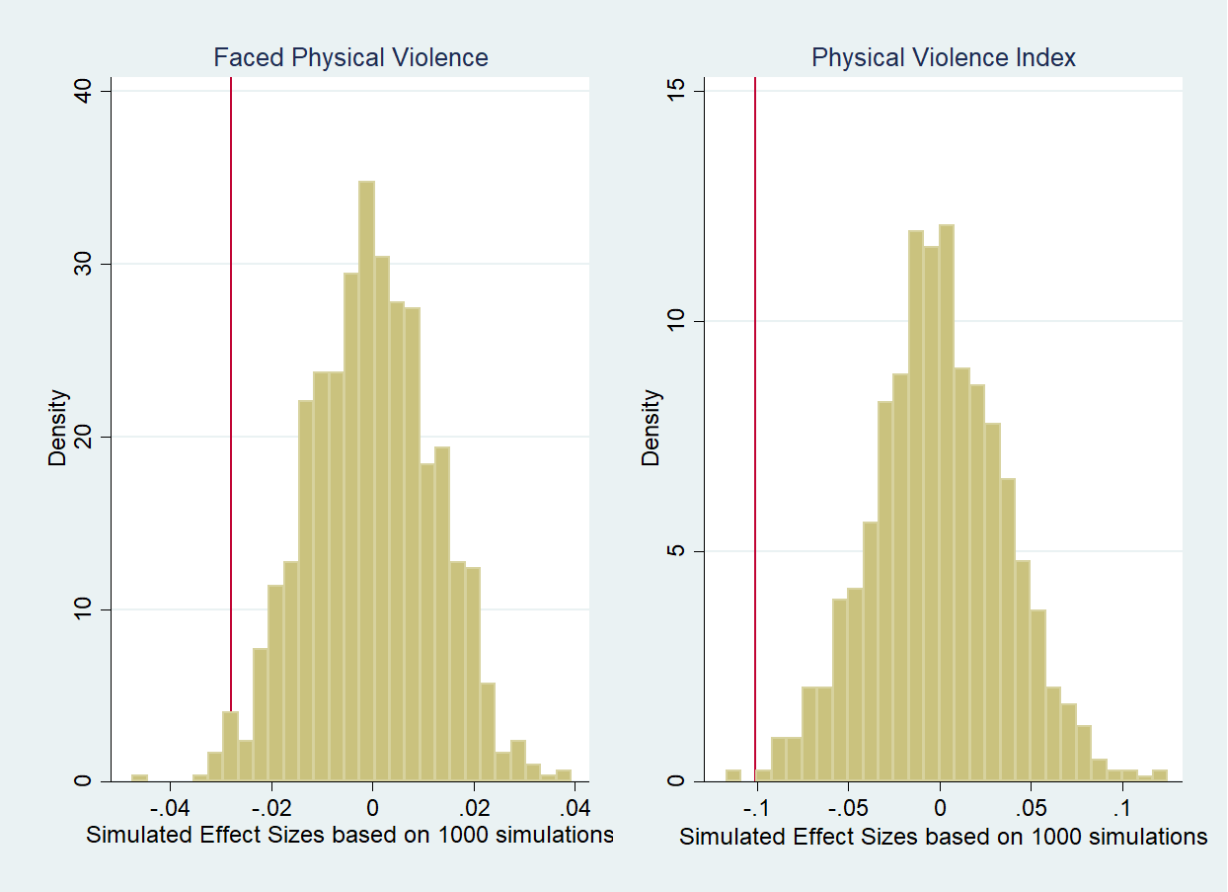
Notes: The figure reports the distribution of coefficients from the regressions based on 1000 simulations which randomly assign SABLA and Non-SABLA status to the districts. We see that most of the coefficients are centered around zero and also have a smaller magnitude when compared to the true causal estimate (marked with red solid line). The above exercise gives us confidence that our identification startegy is picking up the true effects rather than giving us a spurious result. The point estimates above have been calculated using regression Equation 1 with robust standard errors clustered at the district level coupled with district fixed effects. Additional variables in the regression model include both individual and household level controls such as age, education in single years, religion, age of household head, sex of household head, family size, number of children below or equal to 5 years of age in household, wealth index and type of place of residence.

For the second simulation, we randomize the eligibility for SABLA instead of assigning it in accordance to the administrative norms. So, under the simulation some women who were not originally eligible for the policy would have been assigned a SABLA status. Similarly, some women who were originally eligible for the policy were now assigned a Non-SABLA status. We then run the regression stated in Equation1 for our outcome variable physical violence and again repeat this process 1000 times. Again, if our critical identifying assumption is true then most of the results from the randomisation of women into SABLA eligible and SABLA non-eligible status would give us imprecise results which should also be much smaller in magnitude as compared to our true causal estimate.

When we analyze the results for the outcome variable *Faced Physical Violence* , we find that only 4.6% and 9.1% of the simulated results come out to be significant at 95% and 90% confidence intervals, respectively. This is on expected lines and adds to the confidence in our identification strategy. Also, while observing the distribution of simulated coefficients given in Figure we find that nearly all the coefficients are smaller then the true causal estimate of -0.028. The distribution is heavily centered around zero with simulated coefficients evenly distributed across both sides. We observe similar results for our other outcome variable for physical violence i.e. *Physical Violence Index* where only 5.7% and 10.3% results turn out to be significant at 95% and 90% confidence levels respectively. The true causal estimate is again much larger and in the right direction when compared with the distribution of coefficients given in Figure 4 .

The robustness exercise given above helps us to ensure that our identification design which exploits cohort eligibility and SABLA - Non SABLA status of a district to pick the effects of the policy is actually giving us a true causal estimate and not some spurious result. This is because a random allocation to treated group in form of a treated individual *or* a treated district generates largely insignificant results which are centered around zero coupled with smaller effect sizes.

Figure 4: Physical Violence: Eligibility Randomization



Notes: The figure reports the distribution of coefficients from the regressions based on 1000 simulations which randomly assign eligibility for SABLA among women. We see that most of the coefficients are centered around zero and also have a smaller magnitude when compared to the true causal estimate (marked with red solid line). The above exercise gives us confidence that our identification startegy is picking up the true effects rather than giving us a spurious result. The point estimates above have been calculated using regression Equation 1 with robust standard errors clustered at the district level coupled with district fixed effects. Additional variables in the regression model include both individual and household level controls such as age, education in single years, religion, age of household head, sex of household head, family size, number of children below or equal to 5 years of age in household, wealth index and type of place of residence.

7 Conclusion

The main objective of this paper is to provide evidence of the impact of a multi-dimensional women empowerment policy - SABLA - on domestic violence experienced by women post-marriage in India. Using a difference-in-difference design we find that women who were exposed to the policy as adolescent girls were 17% less likely to experience emotional violence and 11% less likely to experience physical violence. The overall level of domestic violence experienced by these women is also likely to be lower when compared to women who didn't benefit from the program at an adolescent age. We also find evidence that these benefits were likely to be limited to girls who were enrolled in middle school at the time of implementation of the policy, suggesting a complementary role played by schooling in deriving the benefits out of the government programme. Our analysis suggests that a higher private level of satisfaction with the marriage and changed attitude towards domestic violence on part of the husbands' of the women affected by SABLA serve as the potential channels behind these effects. The results are robust to alternative specifications and test of *exact randomization*.

In performing the above analysis we provide the first evidence for the impact of multi-dimensional women empowerment policies on domestic violence. We also add to the limited literature on the effects of multifaceted women empowerment interventions on women welfare (Bandiera et al. 2018; Bandiera et al. 2019). The literature on government programes which have an impact on domestic violence (Bobonis et al. 2013, Hidrobo and Fernald 2013) have traditionally focussed on cash transfers to women in order to increase their bargaining power inside the household. Our analysis suggests that even without the cash transfer, an improvement in the husbands' level of satisfaction with the marriage on account of *well informed* wives' can be a potential channel that helps to reduce domestic violence. This is important as cash transfers are more likely to be *one-shot* which makes it possible that there effect on domestic violence may disappear in long term. On the contrary, benefits of a program like SABLA which helps to augment women with wide variety of information thereby creating better individuals who are likely to provide a higher level of marital satifaction to

their partners is likely to have sustained long term effects on domestic violence.

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A Appendix Tables

Table A.1: Effect of SABLA: Random Assignment of Districts

	Faced Domestic Violence (1)	Domestic Violence Index (2)	Faced Emotional Violence (3)	Emotional Violence Index (4)	Faced Sexual Violence (5)	Sexual Violence Index (6)	Faced Physical Violence (7)	Physical Violence Index (8)
$SABLA$	-0.016	-0.115*	-0.012	-0.027	-0.002	-0.012	-0.025*	-0.074*
	(0.015)	(0.064)	(0.010)	(0.019)	(0.008)	(0.014)	(0.014)	(0.041)
R^2	0.04	0.03	0.02	0.02	0.01	0.01	0.04	0.04
Observations	5,333	5,333	5,333	5,333	5,333	5,333	5,333	5,333

Notes: Robust standard errors clustered at the district level are reported in parentheses. *** p<0.01 **p<0.05 *p<0.1

Table A.2: Survey Questions

	Response
Emotional Violence	
1) Spouse ever humiliated you?	No=0, Yes=1
2) Spouse ever threatened you with harm?	No=0, Yes=1
3) Spouse ever insulted you?	No=0, Yes=1
Sexual Violence	
1) Spouse ever physically forced sex when not wanted?	No=0, Yes=1
2) Spouse ever physically forced to perform sexual acts you didn't want to?	No=0, Yes=1
3) Spouse ever forced other sexual acts when not wanted?	No=0, Yes=1
Physical Violence	
1) Spouse ever pushed, shook or threw something?	No=0, Yes=1
2) Spouse ever slapped?	No=0, Yes=1
3) Spouse ever punched with fist or something harmful?	No=0, Yes=1
4) Spouse ever kicked or dragged?	No=0, Yes=1
5) Spouse ever tried to strangle or burn?	No=0, Yes=1
6) Spouse ever threatened with knife/gun or other weapon?	No=0, Yes=1
7) Spouse ever twisted her arm or pulled her hair?	No=0, Yes=1

Figure A.1: SABLA: Scheme Details

☺ Why are these services being provided:

→ Nutrition provision:

- To improve your health and nutritional status: this nutrition is in addition to what you eat at home.

→ Iron Folic Acid (IFA) tablets:

- These are a must for you to improve:
  - iron content in blood
  - memory
  - physical energy level
  - dark circles under eyes
- Best time to have the IFA tablet is at night with dinner to avoid feelings of nausea, etc. which is experienced at times.

Conclusion: Don't give up consuming these tablets even if you have nausea for a while or excrete black stools **as it is essential for the body**.

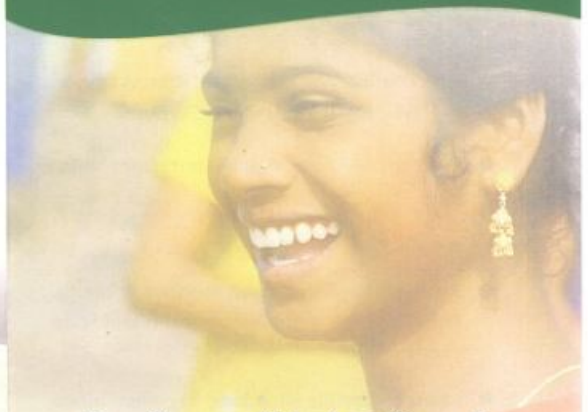
→ Learning to access public services provided by:

- Post Office, Police Station, Bank: This will make you feel confident in using them at any point in the future.
- Government under different Schemes / Programmes / Facilities will empower the Adolescent Girls.

If you are below 14 years, you should be rightfully admitted to a nearby school under Right to Education.

Rajiv Gandhi Scheme
For Empowerment of Adolescent Girls

Sabla



Enroll yourself for benefits under Sabla at the Anganwadi Centre of the village / mohalla if you are between 11 – 18 years.

नए सम्मान की ओर
Towards a new dawn

Ministry of Women & Child Development
Government of India

Figure A.2: SABLA: Scheme Details

Under Sabla you will:

- Get nutrition either as Take Home Ration or Hot Cooked Meal for 300 days in a year, if:
  - You are 11 – 14 years and out of school

  or
  - You are 14 -18 years irrespective of whether you are in school or not.
- Get iron and folic acid tablets on regular basis.
- Have your health check-up done, height, weight, BMI measured.
- Receive knowledge about nutrition & health, family welfare, reproductive health, child care practices, etc.
- Learn to face life situations, to communicate and manage stress, develop self esteem and leadership skills, make healthy and safe choices in life.
- Visit public service facilities like Post Office, Police Station, Bank, etc. and learn about their working for accessing them at any point of time in life.
- Be enrolled in vocational training under National Skill Development Programme if you are between 16 to 18 years.

States / UTs may add message

You will be a part of **Kishori Samooh** if you are out of school:

- Wherein a group of 15 to 25 Adolescent Girls in the village / area of the AWC will be formed.
- 3 Peer Leaders – 1 Sakhi and 2 Sahelis will be selected yearly from amongst you.
- The Sakhi and Saheli will be trained as peer leaders and will be link between Service Providers and the Adolescent Girls.

There will be a **Kit** for your use at the Anganwadi Centre:

- Which will have games, charts, flashcards, etc.
- Which will assist in understanding various health, nutrition, social and legal issues.

Kishori Diwas, the special health day will be held **once** in three months at AWC in which:

- AWW, Medical Officer ANM and ASHA will be present.
- Following activities will be performed:
  - General health check-up of all Adolescent Girls.
  - Filling up of Kishori Card.
  - Health and Nutrition Education.
  - Any other relevant activity.

You can ask questions about growing up, health, personal hygiene, etc.