Essays on Development Economics

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Abstract

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This dissertation is a collection of essays that study women's labor market decisions and outcomes in developing countries.

Chapter 1 studies how fear of sexual assault limits women's labor supply. Numerous criminology survey studies show that women, much more than men, are afraid of being a victim of sexual assault. In response to this fear, women adjust their lives and their behavior in a way to minimize the risk of sexual victimization. I exploit the variation created by the sampling framework of Bangladesh's Quarterly Labor Force Survey (QLFS) to see how individuals in the labor force react to news reports on sexual violence within a small time frame of their survey interview. My results show that women decrease their labor hours and the probability of working on a particular day if there is a high level of reporting on rape that day or one day ago. Women decrease the hours they work 0.0539 hours and 0.0625 hours on a day of and the day after both newspapers have articles on sexual violence, respectively. There is a 0.4 percentage point (PP) reduction in the likelihood a woman works at all on the day of and a 1 PP decrease on the day after both newspapers

report on sexual assault. I find that these effects are largely driven by women who work outside the home. Women who must commute to work decrease hours worked by 0.0975 hours and 0.1025 hours on the day of and day after. They decrease the likelihood of working at all by 1.5PPs and 1.76PPs the day of and day after, respectively. Women who work from home marginally increase their hours. Women begin to increase their hours two days after the headlines are released. However, in aggregate, the total effect on hours is still negative.

Interestingly, I also find effects on men. Men increase their labor supply, in terms of hours and the probability of working that day, in response to a news report shock. It may be that men compensate for the reduced work from women. However, men increase their hours and likelihood of working even on two and three days after reports when women are also increasing their hours. The increase in men's labor supply, both in hours and likelihood of working, does not strictly coincide with women's decreasing their supply. This does not seem to be driven by men in households where women work outside of their home.

Chapter 2 presents joint work with Aneesh Mannava, Elizaveta Perova and Alana Teaman and explores the effects of women participating in a Laotian public work program (PWP) on their exposure to gender based violence (GBV) and intimate partner violence (IPV). We find that treatment increases women's employment in wage work and their household earnings. Their increase in earnings account for nearly 83- 86% of household earnings. We do not find that participation in the program changes their exposure to GBV or IPV but does increase her performance on a decision making index by 1.06 to 1.24 standard deviations. This is a particularly interesting result, since although increasing women's economic opportunities have shown to improve their human capital accumulation, reduce early marriage, delay fertility and generally increase female empowerment, the effect on GBV, and more specifically, intimate

partner violence (IPV) is unclear. To our knowledge, we are the only study exploring the causal relationship between workfare programs and gender-based violence.

Chapter 3 is joint work with Aneesh Mannava, Elizaveta Perova and Alana Teaman and it evaluates the impacts of the same Laotian work-fare program on household income, investments and expenditure. Despite significant increases in a woman's own earned income and household income we do not find any evidence of increased investments, savings or large purchases. This suggests that much of this extra income was used to for consumption. Despite the success of the program in increasing employment and income, it is unlikely that this program will increase income in the long term.

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ACKNOWLEDGEMENTS

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DEDICATION

to Amu for being my inspiration and role model.

Chapter 1:

News Reports on Sexual Assault and Labor Supply: Evidence From Bangladesh

1.1 Introduction

Fear of gender-based violence affects women's economic outcomes. In the past several years, violent sexual assault cases in South Asia have garnered national and international attention. The most infamous case is the 2012 Delhi sexual assault and murder of a 23 year old woman in India. More recently the 2018 assault and murder of a young woman in Mymensingh, Bangladesh made national news. Both victims were attacked on a moving bus after other passengers had disembarked. The assaults and subsequent news coverage led to mass protest and discussion about women's safety in public spaces. Eight-four percent of Bangladeshi women in Dhaka experience staring, deliberate touching, groping, and sexual comments while travelling (Action Aid, 2016). The last national survey report in 2015 regarding violence against women showed 21% of women seeing vehicles/roads/streets as a place where sexual violence occurs (Bangladesh Bureau of Statistics, 2015).

Numerous criminology survey studies show that women, much more than men, are afraid of sexual victimization (Warr 1984; Ferraro 1996; May 2001; Fisher and Sloan 2003; Lane and Meeker 2003; Wilcox et al. 2006; Hilinski 2009; Lane et al. 2009; Cook and Fox 2012). In response to this fear, women adjust their lives and their behavior in a way to minimize the risk of sexual victimization. The 'shadow of sexual assault hypothesis' posits that the fear of being a victim of sexual violence casts a shadow over women's fears of being victims of other crimes, even crimes that statistically affect men more (Ferraro, 1996; Hirtenlehner and Farrall 2014). Fear of sexual assault can greatly limit women's mobility and economic outcomes. In Bangladesh, there has been a steady increase in women's labor force participation in the past 30 years (International Labour Organization). Among employed women, 69% worked outside of

their homes in 2015-2016. Anxiety about being a victim of sexual assault can greatly limit their mobility, negatively affecting income and job security. Further, this can potentially have large impacts on industries that rely on women's labor.

I exploit the variation created by the sampling framework of Bangladesh's Quarterly Labor Force Survey (QLFS) to see how individuals in the labor force react to news reports on sexual violence within a small time frame of their survey interview. Since the timing of the interview is uncorrelated with the timing of news reports of sexual assault, reports on sexual assault can act as an exogenous shock to the saliency of sexual violence. This will give a unique way to study the effect of gender based violence on women's labor market outcomes. I focus on sexual assault for three reasons: (1) sexual assault is a crime that victimizes women more, relative to men, (2) the costs of victimization are large and relatively well understood and (3) newspapers reports on sexual assault occur more than reports on other types of gender-based violence.[1] I argue that these reports proxy non-partner sexual violence.

There has been evidence that suggests that people's perception of crime rates are more closely related to media reports than official reports (Heath, 1984; Romer et al., 2003). This may be exacerbated in countries where official statistics are unreliable or inaccessible[2]. Ferraro (1996) finds that women's fears of sexual assault increase their fears over becoming victims of other crimes. Moreover, there is evidence that crime rates and news coverage on crimes may be unrelated (Davis 1954; Graber 1969).

[1] I find three reports on domestic violence and sexual harassment in Prothom Alo and 14 reports in The Daily Star. Six of the reports found in the Daily Star were about incidences not in Bangladesh.

[2] I have not found official statistics on the incidence of rape in Bangladesh. In fact, human rights organizations Ain O Salish Kendra(ASK) and Odihikar compile statistics on rape using reports from newspapers. ASK estimates 864 in 2015, 724 in 2016; Odihikar estimates 789 rapes in 2015, and 757 in 2016. This is in stark contrast to the figures cited by officials: Home Minister Asaduzzaman Khan Kamal has said that more than 17,000 rape cases were registered between 2014-2017 in an address to Parliament.

Fear of harassment and sexual violence in public have lead women to choosing lower ranked schools in Delhi (Borker 2018) and make them less likely to participate in the labor force in India (Chakraborty et al 2018, Siddique 2018). Chakraborty et al. (2018) finds that the perception of there being more crime against women decreases the likelihood of a woman being in the labor force and can partially explain the trend of decreasing female labor force participation in India. The authors measure perception directly at the household level and use aggregate neighborhood level perception to estimate effects. Siddique (2018), which was developed concurrently to this study, also studies the effects of media reports of assault and finds consistent findings. Higher reporting on assault in one's own area reduces the probability that a married urban woman is employed outside the home in India. These studies show that fear of sexual assault affects women's labor supply on the extensive margin. Increasing fears of sexual assault could explain declining labor participation by women in India. Unlike India, however, Bangladesh has had a steady increase in female labor force participation.

This study differs from the preceding two in several ways. I have the ability to look at changes in labor at the intensive margin, i.e. the numbers of hours worked and whether someone worked on a given day, conditional on being in the labor force– and proxy a shock to perception using the timing of news reports. Chakraborty et al. (2018) and Siddique (2018) find that fear of sexual assault and harassment may be limiting women joining the labor force but my study finds that it also affects women who are already in the labor force. There is evidence that shows that women in India who participate in the labor market typically have access to few resources and are poorer (Klasen and Pieters 2012). In Bangladesh, there is also a positive link between the severity of poverty and the probability of women's labor force participation (Bridges et al. 2011). Women in the labor force can be more economically constrained than women who are not.

Secondly, I can compare the effects on men and women. Men provide an interesting comparison group since they do not have as salient a fear of sexual violence but may have to adjust their own behavior to the behavioral changes of women. Furthermore, I am able to identify saliency shocks within a very small time frame, that is unaffected by changing trends in newspaper reporting.[3] I am also able exploit a retrospective panel to control for unobserved heterogeneity and speak to average effects across a diverse population.

My results show that women decrease their labor hours and the probability of working on a particular day if there is a high level of reporting on rape that day or one day ago. Women decrease the hours they work by 0.0539 hours and 0.0625 hours on a day both newspapers have articles on sexual violence and the day after, respectively. There is a 0.4 percentage point (PP) reduction in the likelihood a woman working at all on the day of and a 1 PP decrease on the day after both newspapers report on sexual assault. However, women increase their hours and the likelihood of working two days after reports. In aggregate, the total effect on hours is negative. I find that these effects are largely driven by women who work outside the home. Women who must commute to work decrease hours worked by 0.0975 hours and 0.1025 hours on the day of and day after. They decrease the likelihood of working at all by 1.5PPs and 1.76PPs the day of and day after, respectively. Women who work from home marginally increase their hours. While these effects are relatively small, they represent the average effect across a large population. In aggregate these effects are potentially substantial.

Interestingly, I also find effects on men. Men increase their labor supply, in terms of hours and the probability of working on a given day, in response to a news report shock. It may

[3]Several women's rights organizations in Bangladesh have used newspaper reports on rape and sexual assault to claim that sexual assaults are on the rise- particularly since the 2012 Delhi case. However, it is unclear if the rise in news coverage is due to an increase in these crimes or, greater coverage, more public interest and less stigma in reporting.

be that men may be compensating for the reduced work from women however, men increase their hours and likelihood of working even two and three days after reports when women also increase their hours. The increase in men's labor supply does not strictly coincide with women's decreased supply. This does not seem to be driven by men in household where women work outside of their home either.

The structure of the paper is as follows: Section 1.2 will explain the context of sexual violence in Bangladesh. Section 1.3 will describe the data sources for the empirical analysis. Section 1.4 will give the empirical specification. Sections 1.5 and 1.6 gives the main results and heterogeneous effects, respectively. Section 1.7 address threats to the identification strategy and robustness checks. Section 1.8 concludes.

1.2 Context

According to the Report on Violence Against Women (VAW) Survey 2015 in Bangladesh, nearly three quarters of women who were ever married experienced some form of violence in their lifetime and 54.7% experienced violence during last 12 months.[4] Despite the high rate of violence, most women have never reported this violence to anyone, largely because they do not feel it is necessary. More than a quarter (27.8%) of women reported experiencing physical violence by someone other than their husband (non-partner) and 6.2% reported experiencing such violence during the last year. Three percent of the women experienced non-partner sexual violence during their lifetime and 2.5% during last 12 months. Rates of non-

[4] These statistics is for any form of violence, including physical, sexual, emotional and, controlling behavior. 27.2% of ever married women experienced sexual violence from their partner and 13.3% experienced it in the last 12 months. The literature analyzing the relationship between gender based violence and women's labor market outcomes has provided evidence on how women's labor market opportunities affect the incidence of domestic and intimate partner violence. But this research often has conflicting results. Aizer (2010) finds that wage shocks that positively affect women's relative wages decrease the incidence of domestic violence, while a shock that favors men's relative wages increase the incidence of domestic violence in California. While giving women job offers in Ethiopia increased their likelihood of experiencing domestic violence (Hjort and Villanger, 2011). Heath (2014) finds that a woman's initial level of bargaining power can explain this heterogeneity; better labor market options for women decrease the likelihood of experiencing domestic violence for women who have higher baseline bargaining power but increase the likelihood of women with lower baseline bargaining power in Bangladesh. Chowdhury et. al (2020) finds no change in the exposure to domestic violence or intimate partner violence from participating in a workfare program in Lao PDR.

partner sexual violence were highest among those aged 20-24 years (4.3% during their lifetime and 3.7% in last 12 months), followed by adolescents aged 15-19 and those aged 35-39. Women indicated that the most likely location of sexual violence was in their husband's home but the second most likely was a public location.[5]

Although it seems that intimate partner sexual violence is more prevalent, the costs of non-partner sexual violence are similar in scope and in extent of trauma. Both have health costs from physical injuries and psychological trauma. The World Health Organization finds that sexual violence can lead to intrauterine hemorrhage, nutritional deficiency, abdominal pain, contraction of STIs and STDs, in addition to an increased risk of PTSD, depression, anxiety and suicidal thoughts. It is difficult to assess if the physical and psychological effects are different. However, one might argue that the social cost of non-partner sexual violence is higher than intimate partner violence due to social norms. For example, marital rape is not illegal in Bangladesh. Moreover, the legal process is cumbersome in prosecuting rape. In the past 10 years, less than 1% of rape trials in Dhaka courts have ended in convictions. 49% of cases brought to trial were dismissed because witnesses and victims refused to testify, at times due to intimidation by the defendants (Islam and Islam, 2017).

1.3 Data

1.3.1 Newspaper Reports

I construct a data set on newspaper articles on sexual assault by scraping the text and metadata of online websites of two major Bangladeshi newspapers, Prothom Alo (PA) and The Daily Star (DS). The two papers together have a combined daily circulation of over 500,000 papers (Bangladesh Ministry of Information). PA is produced in both Bengali and English while the Daily Star is produced in English. PA is the second most widely read paper in Bangladesh.

[5] This included their workplace, buses and public streets.

DS is the most popular English language paper in Bangladesh. I limited the time frame to June 2015 to July 2016 to overlap with the retrospective data from the QLFS.

I scraped the English language websites of both newspapers to compile information on the reporting of sexual assault, sexual harassment, and domestic violence. I limit the analysis to just sexual assaults. The web scraping captures the text of the article, the title, by-line information and meta-data from the web page. Articles were coded as being about sexual assault if the keywords were found in the title of the article and text. The number of articles returned by keyword can be found in the appendix, in figure 1A:1. The program was able to remove special characters, catch different tenses, i.e. “assault” vs “assaulted” vs “assaulting”, and parse through the text without catching keywords that were embedded in other words. Figure 1.1 shows the distribution of the number of headlines (titles of articles) on sexual assault and weekly average number of headlines and how often a headline appeared in both papers. In additional, through scraping the text and headlines of the articles, I extracted meta-data on the articles from the HTML code of the webpage. The HTML included information on the section where the article appeared and, for DS articles, whether the article was on the frontpage, interior or back-page of the paper.

Approximately 33% of days had at least one headline on sexual assault so there is relatively frequent reporting on rape. Table 1.1 details how often these newspapers report on sexual assault. Across the two papers, 4.18 articles on sexual assault were published on average in a day. The articles are most frequently, 71%, news pieces about incidences in Bangladesh. On average there is one article headline on sexual assault a day and on only 15% of days were there articles on sexual assault in both papers.

Table 1.1: Summary Statistics of Scraped Newspapers

	(1)		(2)		(3)	
	Both		Prothom Alo		Daily Star	
	Mean	SD	Mean	SD	Mean	SD
Articles	180.08	24.95	52.45	9.09	130.95	17.87
Articles on sexual assault[6]	4.18	2.34	1.86	1.15	3.49	1.72
News piece[7]	0.71	0.46	0.59	0.49	0.74	0.44
Opinion	0.16	0.36	0.10	0.31	0.17	0.38
International	0.14	0.34	0.30	0.46	0.09	0.29
Front-page					0.11	0.31
Both Papers	0.15	0.36				
Headlines on sexual assault	1.02	1.01	0.49	0.63	0.82	0.86
N	1422		301		1084	

Meta-data on whether articles were on the front page is only available for DS. Both Papers is a binary indicator for whether PA and DS had headlines on sexual assault on a given day.

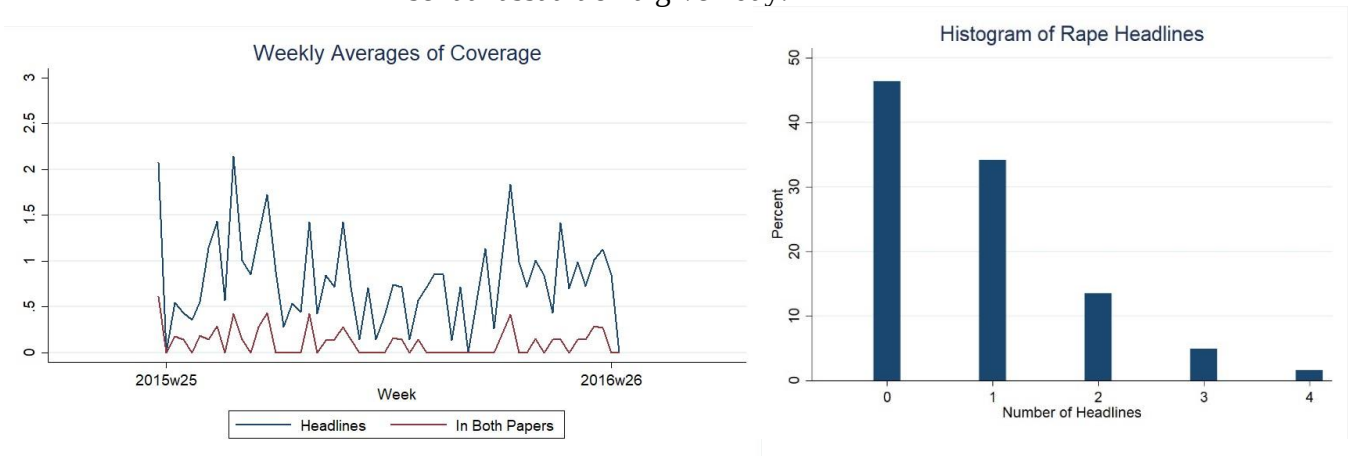


Figure 1.1: Distribution of articles on sexual assault

[6] Based on body of article and/or the title of the article,
[7] News articles about incidences in Bangladesh.

1.3.2 Bangladesh Quarterly Labor Force Survey

I also use information from the Bangladesh Quarterly Labor Force Survey (QLFS). The 2015- 2016 QLFS marks Bangladesh's transition to collecting labor force surveys on a quarterly basis. The QLFS is carried out by the Bangladesh Bureau of Statistics (BBS) with assistance from the World Bank and is representative at the national and district level. The survey collected basic household information, including general education information for all persons aged 5 and older and vocational training for those age 15 and older. Household members age 15 and older were also given questionnaires on labor market activities that asked, but were not limited to, employment status, main activities in their first and second job and own-use production. I limited the sample to those in the labor force, employed and unemployed, to focus on the short run changes due to news reports. Given this, these results may not be generalizable to the Bangladeshi population at large but is telling about the behavior of those who have selected into the labor force.

The survey followed a two stage sampling framework and rotational panel strategy, that will eventually be used to resample some households in future survey periods. The first stage sample frame of the QLFS was based on Enumeration Areas (EAs) from the 2011 Census. Some of the original EAs were too small to support the rotational panel strategy and were joined to neighboring EAs in order to create 146,576 primary sampling units (PSU), of which 1,284 PSUs were used for the survey. All households in each PSU were randomly divided into blocks and surveyed, 4 blocks each quarter. Each block was visited for two consecutive quarters, left to rest for the next two quarters, and then revisited for another two quarters. This survey design makes the data set especially useful for studying the current problem. Since the PSUs were consistently sampled throughout the year and interviews were carried out uniformly throughout the year, I

can focus on the time series variation rather than cross-sectional variation. This will allow me to see the changes in labor hours as a result of sexual assault events and analyze the effect on the intensive margin dynamically. Figure 1.2 shows the distribution of the interview dates of individuals in the sample used for analysis. The distribution, while not uniform, does show that the sample for analysis did not deviate from the intended sampling structure and there exists variation in interview timing.

Figure 1.2: Density of households interviewed by date

Table 1.2 shows the summary statistics for the sample by gender. Thirty percent of the sample is female, with an average age of 37 years, slightly lower for women at 35 years. While men and women are similarly literate, men tend to have a secondary education while the women in the sample did not attain any formal education. Men are either married (80%) or unmarried (19%), while married and unmarried women only account for about 90% of the sample, implying that the remainder of the sample is likely divorced, widowed or separated. The majority of the sample is rural. Men and women most often employed in agriculture. Approximately half (53%) of the women in sample are employed in agriculture compared to 25% of men in the sample. The

second most common industry where women are employed is in the ready-made garment industry while for men it is the retail industry. Women on average work 13 hours less per week than men.

Table 1.2: QLFS Summary Statistics

	Full Sample		Men		Women	
	Mean	SD	Mean	SD	Mean	SD
Female	0.30	.456	-	-	-	-
Age	37.12	13.6	37.93	14	35.20	12.2
HH Size	4.69	1.98	4.78	2	4.49	1.91
Unmarried	0.16	.371	0.19	.396	0.09	.291
Married	0.80	.402	0.80	.404	0.80	.398
Education						
None	0.29	.454	0.27	.444	0.34	.474
Primary	0.25	.434	0.26	.436	0.24	.429
Secondary	0.31	.463	0.31	.464	0.30	.459
Higher Secondary	0.08	.264	0.08	.274	0.06	.238
Tertiary	0.07	.255	0.08	.266	0.05	.224
Literate	0.68	.466	0.70	.459	0.63	.482
Muslim	0.88	.321	0.89	.318	0.88	.329
Barisal	0.08	.268	0.08	.272	0.07	.258
Chittagong	0.17	.374	0.16	.368	0.18	.387
Dhaka	0.26	.438	0.27	.444	0.23	.423
Khulna	0.14	.348	0.14	.342	0.16	.363
Rajshahi	0.15	.354	0.14	.343	0.17	.375
Rangpur	0.13	.334	0.13	.332	0.13	.34
Sylhet	0.08	.27	0.09	.287	0.05	.223
Rural	0.53	.499	0.51	.5	0.57	.495
Work outside home	0.89	.315	0.97	.166	0.69	.462
Industry BSIC2						
Agriculture	0.33	.471	0.25	.432	0.53	.499
Retail	0.11	.317	0.15	.357	0.02	.156
Transportation	0.07	.262	0.10	.304	0.01	.0743
RMG Manu.	0.05	.217	0.04	.189	0.08	.27

Education	0.04	.198	0.03	.181	0.06	.232
Construction	0.04	.193	0.05	.224	0.00	.066
Domestic Services	0.02	.138	0.01	.0754	0.05	.223
Other Personal Services	0.03	.167	0.02	.138	0.05	.22
Weekly hours worked in both job	49.82	16.5	53.73	15.2	40.11	15.8
Monthly Earnings (Taka)	13,004.58	11,119	13,249.03	11,674	12,175.86	8,935
Observations	185745		130871		54874	

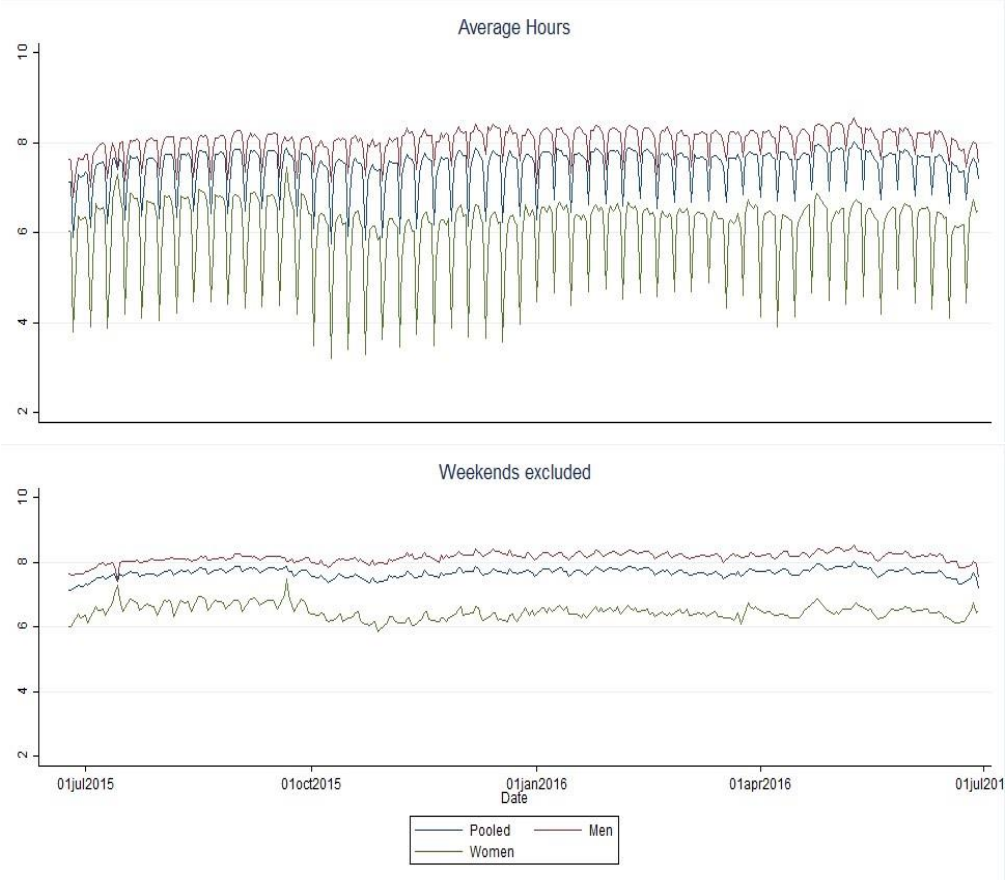


Figure 1.3: Average labor hours by date

Figure 1.3 shows the hours worked by date; men generally work more hours than women and women adjust their hours more on weekends than men do. There is also seasonality to daily hours; the variability in daily hours is lowest from April to May, the harvest season.

1.4 Empirical Strategy

Using the data from the newspapers, I constructed two variables to proxy a shock to saliency of sexual assault on a particular day. The first was the total number of headlines on a given day from both papers. This variable will show how individuals react to any reporting on sexual assault. The second indicator is an indicator for whether a headline on sexual assault appeared in both newspapers on that day. Since the treatment variable may be working through two possible channels, saliency and information, it is plausible that these effects are correlated with unobservable individual characteristics. If we believe the news reports provide information on the risk of sexual assault, this treatment may have a differential effect on those who have access to other sources of information and those who do not. Plausibly this access could be somewhat accounted for by controlling for literacy. However, this information may be spread to individuals regardless of their literacy through other channels, such as their social networks. Alternatively, if the effects are driven by increased saliency to the costs of victimization, individual specific characteristics may cause different levels of disutility from becoming a victim of sexual assault. Additionally, since shocks are strictly time varying in this setup, I choose to present fixed effects estimations to abstract from heterogeneity within the population. I provide heterogeneous effects across several demographic characteristics in section 6 and with respect to commuting.

$$y_{imdt} = \alpha_i + \lambda_m + \gamma_d + \delta_{mdt} PublicHoliday + \sum_{j=0}^{3} \beta_{1,j} L_{t-j} + \sum_{j=0}^{3} \beta_{2,j} L_{t-j} * Female_i + \varepsilon_{imdt} \quad (1)$$

Equation 1 estimates the effect of sexual assault reporting on labor supply. The dependent variables are hours worked on a given day and a dummy for whether or not the person worked at all that day(i.e. hours > 0). α_i are individual fixed effects, λ_m are month fixed effects, γ_d is day of week fixed effects and there is a control for public holidays. L_{t-j} is the measure of rape reporting, lagged by *j* time periods and the interaction term shows the additional effect on women. Figure 1.3 shows that hours vary depending on the day of the week and month. Newspaper coverage on sexual assault may also be time varying; assaults may be more frequent at different times of the year or even days of the week that could effect when they are reported on or newspapers may have their preferences on reporting on these types of events. Appendix table 1A shows whether day of the week, month or public holidays have predictive power of the proxy variables. There is a statistically significant association between certain months and the proxies. The month, day of week fixed effects and the control for public holiday will control for this variation. The standard errors are clustered at the PSU level given the sample design (Abadie et al., 2017). Since we are including an interaction term between the reporting variable and gender, we can interpret $\beta_{1,j}$'s as the effect on men. $\beta_{1,j} + \beta_{2,j}$ will give us the effect on women. If this term is negative for any given *j*, that implies that women reduce their labor supply in reaction to reporting on rape. It would be reasonable to expect $\beta_{1,j}$ to be zero since sexual assault is a greater concern for women and should not affect men's labor supply. However, $\beta_{1,j}$ may be positive if there is some household readjustment to compensate for the decrease in women's labor supply.

Additionally, there may be a larger labor market effect where men are compensating for the decrease in women's labor supply. For example, employers may increase the hours of male employees to complete the tasks usually done by female employees. Here we would expect to see

men increase their hours when women decrease their hours. Alternatively, if the coefficients are negative, it implies that men also decrease their labor supply in reaction to news reports on rape. This could also be due to the household readjusting to the shock. For example, male household male members may adjust their hours to accompany female household members to work.

1.4.1 Commuting

Women, generally are very sensitive to commuting costs. Longer and more costly commutes are associated with lower labor force participation and labor supply (Abe and Kawabata 2018; Madden, 1981; McLafferty and Preston, 1997; Hjorthol, 2000, Chakerborty 2018, Siddique 2019). Moreover, fear of sexual assault limits women's use of the public space (Valentine 1989, Pain 1997). The news reports will proxy the violence women may face outside of the home. This can increase the cost of working outside the home.

Equation 2 estimates the heterogeneous effects on women who work outside the home. The effect on women who work outside the home will be If $\Sigma_{d=1}^{4}\beta_{d,j}$ for any given lag period. If $\Sigma_{d=1}^{4}\beta_{d,j}$ is more negative than $\beta_{1,j} + \beta_{2,j}$ the women working outside the home will be more affected by the reports than women inside the home. $\beta_{1,j}$ will shows the effect on men and $\beta_{1,j} + \beta_{3,j}$ will show the effect on men who work outside the home. If men are unaffected by these reports we should expect both of the effects to be zero. If reports do not differentially affect men and women working outside the home, we should expect $\beta_{4,j}$ to be zero.

$$y_{imdt} = \alpha_i + \lambda_m + \gamma_d + \delta_{mdt} PublicHoliday + \sum_{j=0}^{3} \beta_{1,j} L_{t-j} + \sum_{j=0}^{3} \beta_{2,j} L_{t-j} * Female_i + \sum_{j=0}^{3} \beta_{3,j} L_{t-j} * Outside_i + \sum_{j=0}^{3} \beta_{4,j} L_{t-j} * Female_i * Outside_i + \varepsilon_{imdt} \quad (2)$$

1.5 Results

Table 1.3 shows the results from equation 1 and table 1.4 provides the marginal effects on women. When using headlines as the proxy, we see there is an overall effect on men, as they increase their labor hours and increase the likelihood of working. Women, however, are more sensitive to when the headline occurred. If they experienced a headline on that day or a day prior, they do not change their hours. But if the headline is in the more distant past, they increase their overall hours, but, not in as great of a magnitude as men. Headlines also increase women's likelihood of working. However, this occurs to a lesser extent than men, unless the headlines were 3 days prior. These results seem counter intuitive as it seems like reports have either no effect on women or small positive effects. But it is plausible that respondents do not react to all reporting equally. Since there is a headline on rape relatively frequently, individuals, specifically women, may not react. However, when there is an unusually large amount of reporting the effect may be more acute.

Table 1.3: Fixed effects estimate of reporting on labor supply

Proxy	Headlines		Both Papers	
Dep. Var	Hours worked	Worked at al1	Hours worked	Worked at all
L_0	0.040	0.006	0.045	0.006
	(0.007)***	(0.001)***	(0.020)**	(0.002)***
Female*L_0	-0.034	-0.001	-0.099	-0.011
	(0.009)***	(0.001)	(0.027)***	(0.004)***
L_1	0.058	0.008	0.059	0.008
	(0.007)***	(0.001)***	(0.020)***	(0.002)***
Female*L_1	-0.055	-0.005	-0.122	-0.018
	(0.009)***	(0.001)***	(0.028)***	(0.004)***
L_2	0.032	0.005	0.127	0.019
	(0.006)***	(0.001)***	(0.019)***	(0.002)***
Female*L_2	-0.013	0.001	-0.100	-0.008

	(0.009)	(0.001)	(0.027)***	(0.004)**
L_3	0.029	0.004	0.064	0.010
	(0.006)***	(0.001)***	(0.018)***	(0.002)***
Female*L_3	-0.006	0.003	0.036	0.019
	(0.009)	(0.001)***	(0.025)	(0.003)***
R^2	0.30	0.28	0.29	0.28
N	743,077	743,077	743,668	743,668

*p<0.1; ** p<0.05; *** p<0.01 Standard errors clustered at PSU. Retroactive panel constructed from the information on the seven days prior to interview. I include individual level fixed effects, day of week fixed effects, month fixed effects and public holiday fixed effects. Columns 3 and 5 show results for OLS linear prediction models.

The alternative proxy is a dummy for whether a headline appeared in both papers. This proxy can indicate whether there is an unusual level of reporting. If both papers have a headline on rape on a given day, then there have to be at minimum two headlines that day. This may be a significant difference as the average number of headlines in this time frame was .81 and there were only headlines in both papers on 15% of days. Thus, having headlines in both papers on the same day can be thought of as a fairly large shock of news reports.

Table 1.4: Marginal effects on women

Proxy	Headlines		Both Papers	
Dep. Var	Hours worked	Worked at all	Hours worked	Worked at all
L_0	.00052	.0053	-.0539	-.004
	[-.112, .022]	[.003, .008]	[-.106, -.006]	[-.011, .002]
L_1	.0025	.003	-.0625	-.01
	[-.013, .018]	[.001, .005]	[-.111, -.015]	[-.017, -.003]
L_2	.0191	.0066	.0273	.011
	[.004, .034]	[.004, .009]	[-.017, .073]	[.004, .018]
L_3	.0231	.0072	.101	.03
	[.008, .038]	[.005, .009]	[.06, .141]	[.024, .035]

95% confidence intervals in brackets.

A headline appearing in both papers decreases women's labor supply on the day of reporting and the following day. However, we see that women compensated as the reports became more distant. Reports one day before have the most negative effect, decreasing hours by .0625 and the likelihood of working that day by 1%. Interestingly, women overcompensate for the original decrease in hours. This might imply that the channel this is working through is salience instead of information as we should not expect the information effect to disappear so quickly. Information would lead to a more permanent changing of expectations of the future, while a salience effect might fade.

Figure 1.4 shows the results for equation 2. I run this estimation only for whether a headline appeared in both papers. Given that these reports are more likely a proxy for sexual violence in public areas we should see different effects for those who work outside the home than inside the home. Figure 1.4 shows the point estimates and 95% confidence intervals for headlines in both papers on hours worked and whether someone worked. Table 1.5 give the marginal effects on women who work outside the home.

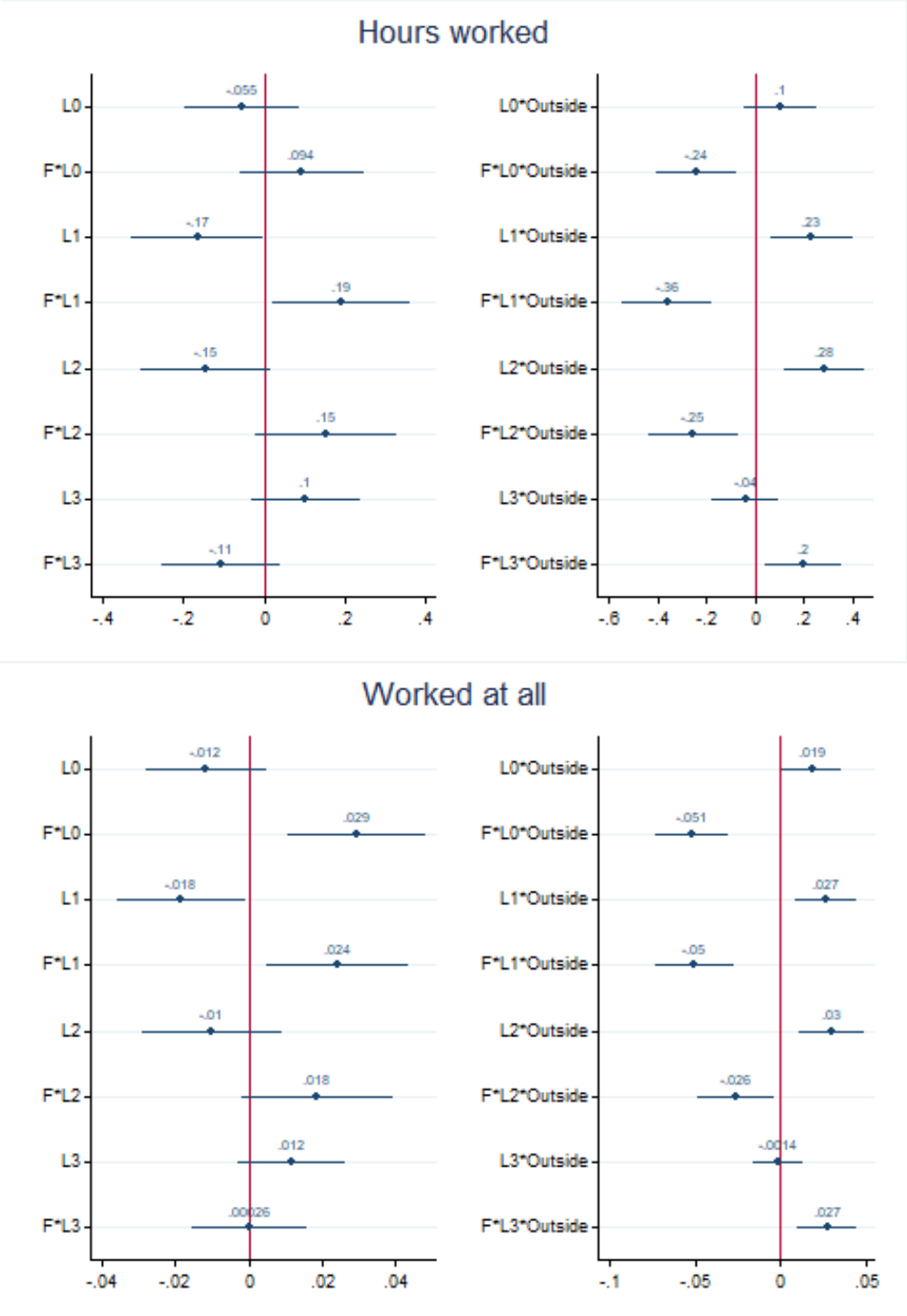


Figure 1.4: Fixed effects results on labor supply by whether individuals commute. Point estimates with 95% CI. Standard errors clustered at PSU. Retroactive panel constructed from the information on the seven days prior to interview. I include individual level fixed effects, day of week fixed effects, month fixed effects and public holiday fixed effects. Lower panel shows results for OLS linear prediction models.

Table 1.5: Marginal effects on women who commute

Proxy	Both Papers	
Dep. Var	Hours worked	Worked at all
L_0	-.0975	-.015
	[-.16, -.035]	[-.024 -.007]
L_1	-.1025	-.0176
	[-.165, -.04]	[-.028, –.008]
L_2	.0363	.012
	[-.023, .096]	[.003, .021]
L_3	.1511	.0381
	[.1, .202]	[.031, .045]

95% confidence intervals in brackets.

If a headline on sexual assault appears in both papers, it decreases the hours worked by women who work outside the home by .0975 hours. However, there is no effect on men or women overall. If the reports were from a day ago, women working outside the home decreased their hours by .1025 hours. Women working inside the home however, increased their hours. There is a positive effect on the hours of men who work outside the home a day and two days before. Women who work outside the home also try to overcompensate for an initial decrease in hours. Reports decrease the likelihood of working by 1.5% and 1.76% on the day of the reports and the day after, respectively. However, reports two days and three days prior increase the probability of working more than the decrease in the prior two days; these effects are largely driven by women who work outside the home.

Men who work outside the home also react to the reports, and 97% of men in the sample work outside the home. We see that the reports on the day of, one day ago and two days ago

increase their labor supply.[8] This effect could potentially be driven by men adjusting their supply to account for women in their household decreasing their supply.

1.5.1 Are men readjusting for women in their households?

To test whether the change in men's hours is driven by households readjusting their labor supply to compensate for decreased labor supply of female household members I run the following estimation:

$$y_{imdt} = \alpha_i + \lambda_m + \gamma_d + \delta_{mdt} PublicHoliday + \sum_{j=0}^{3} \beta_{1,j} L_{t-j} + \sum_{j=0}^{3} \beta_{2,j} L_{t-j} * FemaleLaborers + \lambda T + \varepsilon_{imdt} \quad (3)$$

I limit the sample to just men. If the effect we see on men is driven by households compensating to keep total hours the same, we should expect $\beta_{1,j}$'s to be zero and $\beta_{2,j}$'s to be positive. However, it also possible that the effect on the household may take on readjustments that would decrease men's supply. For example, men may have to accompany female household members on their commutes. In this case we would expect $\beta_{2,j}$ to be negative. I test the effect on the total number of female laborers in the household and a dummy for if any female household member works. Men from households without women in the labor force then would be the ones who increased their hours. The results are shown in table 1.6.

Interestingly, men in households where female household members work do not react differently than men in households where female household members do not work. Men increase their labor hours overall. The probability of working when there is a headline on rape in both papers also increases. The coefficient on the interaction terms are positive implying that

[8] The effect of reports on the day of is not significant at the 5% significance level.

households with female laborers increase their hours more but the effects are insignificant. There may be some underlying general labor market forces at play since they are likely not compensating for lost income.

Since labor supply reductions are largely driven by women who work outside the home, it is possible that households with female laborers who work outside the home are the ones readjusting. To test this, I run the following estimation.

.

$$y_{imdt} = \alpha_i + \lambda_m + \gamma_d + \delta_{mdt} PublicHoliday + \sum_{j=0}^{3} \beta_{1,j} L_{t-j} + \sum_{j=0}^{3} \beta_{2,j} L_{t-j} * FemaleLaborersOutside + \lambda T + \varepsilon_{imdt} \quad (4)$$

If this hypothesis is correct, we should see $\beta_{1,j}$ to be zero and $\beta_{2,j}$ to be positive. The results for this estimation are shown in table 1.7.

Table 1.6: Are households readjusting? Fixed effects regression on men's labor supply.

Dep. Var.	Hours worked	Worked at all	Hours worked	Worked at all
L_0	0.039	0.006	0.033	0.006
	(0.023)*	(0.003)**	(0.024)	(0.003)**
L_1	0.051	0.006	0.052	0.006
	(0.024)**	(0.003)**	(0.025)**	(0.003)**
L_2	0.138	0.019	0.139	0.019
	(0.022)***	(0.003)***	(0.023)***	(0.003)***
L_3	0.053	0.010	0.048	0.009
	(0.022)**	(0.002)***	(0.022)**	(0.002)***
L_0*Total female laborers	0.013	0.002		
	(0.022)	(0.002)		
L_1*Total female laborers	0.012	0.002		
	(0.023)	(0.003)		

L_2*Total female laborers	-0.019	-0.001		
	(0.022)	(0.003)		
L_3*Total female laborers	0.024	0.002		
	(0.021)	(0.002)		
L_0*Any female laborer			0.028	0.002
			(0.030)	(0.003)
L_1*Any female laborer			0.013	0.001
			(0.031)	(0.004)
L_2*Any female laborer			-0.025	-0.002
			(0.030)	(0.004)
L_3*Any female laborer			0.041	0.004
			(0.028)	(0.003)
R^2	0.27	0.28	0.27	0.28
N	523,484	523,484	523,484	523,484

*p<0.1; ** p<0.05; *** p<0.01 Sample only includes men. Standard errors clustered at PSU. Retroactive panel constructed from the information on the seven days prior to interview. I include individual level fixed effects, day of week fixed effects, month fixed effects and public holiday fixed effects. Columns 3 and 5 show panel results for OLS linear prediction models.

Table 1.7: Are households readjusting? Fixed effects regression on men's labor supply, households with female commuters.

Dep. Var.	Hours worked	Worked at all	Hours worked	Worked at all
L_0	0.042	0.007	0.045	0.007
	(0.022)*	(0.002)***	(0.022)**	(0.002)***
L_1	0.051	0.007	0.054	0.007
	(0.023)**	(0.003)***	(0.023)**	(0.003)***
L_2	0.133	0.019	0.136	0.019
	(0.022)***	(0.003)***	(0.022)***	(0.003)***
L_3	0.059	0.010	0.062	0.010
	(0.020)***	(0.002)***	(0.020)***	(0.002)***
L_0*Total female laborers out	0.009	-0.001		
	(0.032)	(0.004)		
L_1*Total female laborers out	0.022	-0.001		
	(0.035)	(0.004)		
L_2*Total female laborers out	-0.016	-0.002		
	(0.034)	(0.004)		
L_3*Total female laborers out	0.022	0.003		
	(0.031)	(0.003)		
L_0*Any female laborers out			-0.000	-0.002
			(0.032)	(0.004)
L_1*Any female laborers out			0.011	-0.002
			(0.034)	(0.004)
L_2*Any female laborers out			-0.025	-0.002
			(0.033)	(0.004)
L_3*Any female laborers out			0.013	0.002

			(0.031)	(0.003)
R^2	0.27	0.28	0.27	0.28
N	523,484	523,484	523,484	523,484

*p<0.1; ** p<0.05; *** p<0.01 Sample only includes men. Standard errors clustered at PSU. Retroactive panel constructed from the information on the seven days prior to interview. I include individual level fixed effects, day of week fixed effects, month fixed effects and public holiday fixed effects. Columns 3 and 5 show panel results for OLS linear prediction model.

Again, the results are similar to the results of estimation 3. There are no additional effects on men in households where women work outside the home. Men increase their labor hours and have a higher probability of working on the day of and up to three days after headlines on sexual assault appearing in both papers. The effects are consistent with the estimated effects on men's labor supply shown in table 1.3.

1.6 Heterogeneous Effects

The results show treatment effects across a heterogeneous population. However, it is reasonable to think that the conditional treatment effects may vary greatly across demographic characteristics. One dimension of heterogeneity that is important is whether the individual works outside the home. Women who worked from home did not adjust their hours and saw a small increase in the likelihood of working on a given day. However, women who worked outside the home decreased their overall labor hours and likelihood of working on days of high levels of rape reporting and the day after. High levels of sexual assault reporting may have different levels of costliness for women of different demographics.

1.6.1 Age

Younger women, particularly unmarried women, may already have greater limits to their mobility due to cultural and social norms of being “virtuous" and the “obedient daughter" (Uteng, 2012) and the reports may cause them to avoid situations they perceive as dangerous themselves or perceived as dangerous by authority figures like their fathers. They may also have a higher cost to victimization due to social norms around sex before marriage. I explore these different effects by age cohort, marital status, and literacy. I limit the sample to just women and interact the demographic variable with the indicator for headlines on sexual assault.

Tables 1.8 shows the conditional treatment effects by age cohort. Younger women react the most to a high level of sexual assault news both in terms of labor hours and likelihood of working. However, as time passes women across all ages react similarly. If there was a news report a day ago all ages decrease the likelihood of working by 1PP and increase the likelihood of working after 2 days. Younger women may be choosing to decrease their labor supply in reaction to the news because they may perceive they are more likely to be victimized. They also have fewer opportunities to make investments to avoid dangerous situations. However, since younger women already face greater restrictions to their mobility, this may be due to other family or household members’ perceptions.

Table 1.8: Marginal effects by age cohort

Age Cohort	15-24		25-34	
Dep. Var	Hours worked	Worked at all	Hours worked	Worked at all
L_0	-.13	-.02	-.05	-.01
	[-.22, -.03]	[-.03, -.004]	[-.11,.01]	[-.02, .002]

L_1	-.1 [-.19, -.008]	-.01 [-.02, .002]	-.03 [-.12, .02]	-.01 [-.02, .002]
L_2	.03 [-.06, .11]	.01 [-.003, .02]	.03 [-.04,.1]	.01 [.005, .02]
L_3	.12 [.04, .2]	.03 [.02, .04]	.11 [.05, .2]	.03 [.02, .04]

Table 1.8 cont.: Marginal effects by age cohort

Age Cohort	35-44		44+	
Dep. Var	Hours worked	Worked at all	Hours worked	Worked at all
L_0	-.02 [-.09, .05]	-.001 [-.01, .008]	-.04 [-.11,.03]	.-.002 [-.01, .007]
L_1	-.03 [-.1, .05]	-.01 [-.02, .002]	-.06 [-.13, .01]	-.01 [-.02, .004]
L_2	.02 [-.06, .1]	.01 [.00004, .02]	.02 [-.05, .01]	.01 [.004, .02]
L_3	.13 [.06, .19]	.03 [.02, .03]	.03 [-.03, .09]	.03 [.02, .03]

95% confidence intervals in brackets

1.6.2 Marital Status

Table 1.9 shows the effects on currently married women, previously married women and, never married women. We see that women who were never married are 2PPs less likely to work on the day of reporting but do not adjust their hours and we see an increase to the likelihood of working and an increase to their hours if reports happened 3 days ago. Women who have ever been married decrease their hours and the likelihood of working, on the day after reports. Similarly, to

never married women, ever married women increase their labor supply in both respects 3 days after reports.

Table 1.9: Marginal effects by marital status

	Currently or Previously Married		Never Married	
Dep. Var	Hours worked	Worked at all	Hours worked	Worked at all
L_0	-.049	.02	-.12	-.02
	[-.1, -.001]	[-.01, .002]	[-.24 .008]	[-.4,-.01]
L_1	-.07	-.008	.03	.005
	[-.12, -.017]	[-.02, -.001]	[-.1, .15]	[-.01,.02]
L_2	.02	.01	.04	.005
	[-.02, .07]	[.006, .02]	[-.08, .16]	[-.01,.02]
L_3	.09	.03	.13	.015
	[.05, .1]	[.02, .04]	[.009, .26]	[.002,.02]

95% confidence intervals in brackets.

1.6.3 Literacy

Literacy can potentially tell us about the intensity of treatment. News reports will reflect information and discussions in the public sphere. It is not necessary that women need to be literate to receive this information. However, it may impact the speed at which they gain this information and the intensity. Non-literate women hear of these reports from other channels such as social networks and broadcast news but consuming the information directly from newspapers is unlikely.

Table 1.10 shows the marginal effects on literate and non-literate women. We see that literate women decrease their hours and likelihood of working on the day of reporting, while non-literate women do not adjust on either dimension. We see decreases in labor supply of non-literate women the day after but the effects are insignificant. All of the women in the sample begin to increase their labor supply after 2 days.

Table 1.10: Marginal effects by literacy

	Non-Literate		Literate	
Dep. Var	Hours worked	Worked at all	Hours worked	Worked at all
L_0	.004	.0002	-.1	-.01
	[-.06, .06]	[-.008, .009]	[-.15 -.03]	[-.02, -.002]
L_1	-.007	-.005	-.087	-.008
	[-.07, .06]	[-.015, .005]	[-.13 -.03]	[-.02, -.0003]
L_2	-.008	.01	.04	.01
	[-.07, ,05]	[.0007, .018]	[-.01, .1]	[.005, .02]
L_3	.07	.03	.11	.03
	[.02, .12]	[.02, .03]	[.06, .16]	[.02,.04]

95% confidence intervals in brackets.

1.7 Threats to Identification and Robustness

1.7.1 Changing Sample Across Time

The identification strategy of this paper relies on time variation and abstracts away from spatial and demographic variation. Figure 1.2 shows that the distribution of when households were sampled was relatively dispersed and closely followed the intended sampling structure of carrying out interviews weekly. The density of observations is relatively consistent across the time frame of interest. However, one may be concerned that despite the sampling framework, the samples by week of interview are different across space and demographics. If the sample differs significantly across time, the control group (those who did not experience shocks to salience of sexual violence), and the treated group may not be comparable. I analyze several spatial and demographics characteristics of the sample, including district, gender, marital status, religion, education level and industry of employment.

Figures 1.5-1.8 show the mean level of those characteristics in the weekly samples. Table 1.11 shows the average difference of the sample characteristics from the mean across the time frame.[9] The average difference in the sample characteristics are very small and close to zero. The sample is relatively similar across time by observable characteristics. Therefore, those who did not experience newspaper headlines of sexual assault during their interview periods are similar demographically and spatially as those who did.

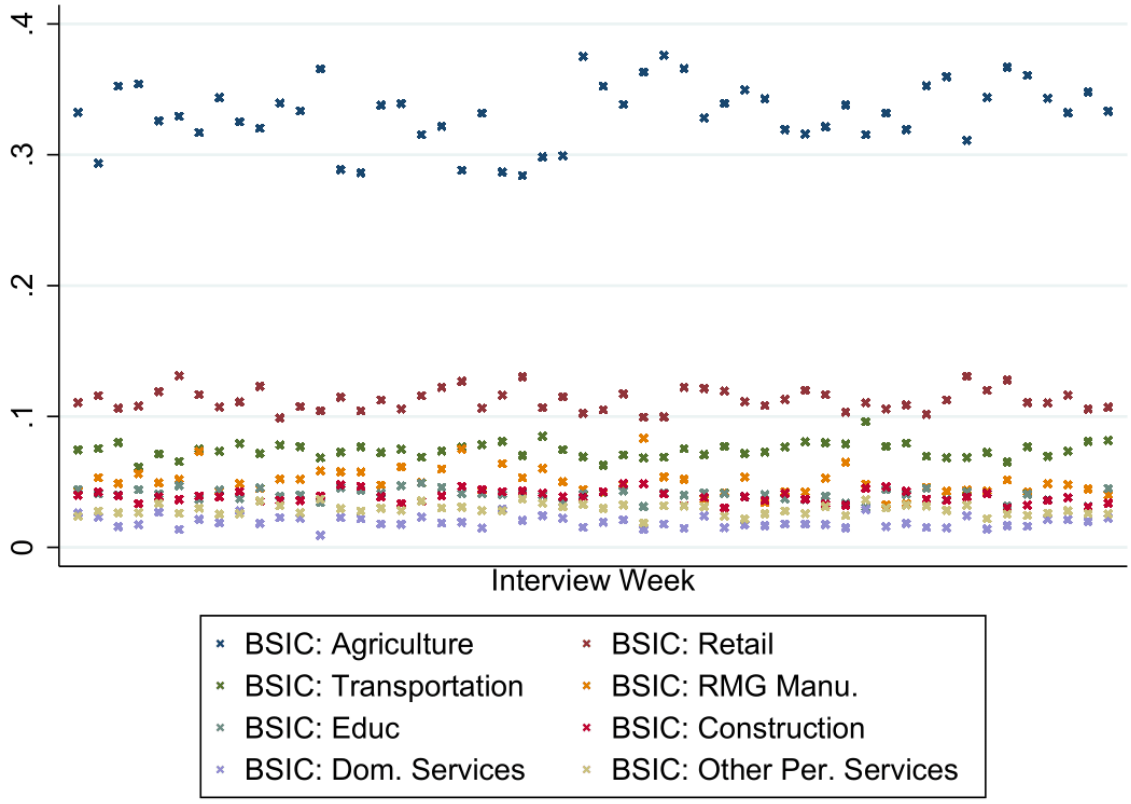


Figure 1.5: Portion of from different industries by interview week

[9] Appendix table 1A: shows the summary statistics of the weekly sample characteristics.

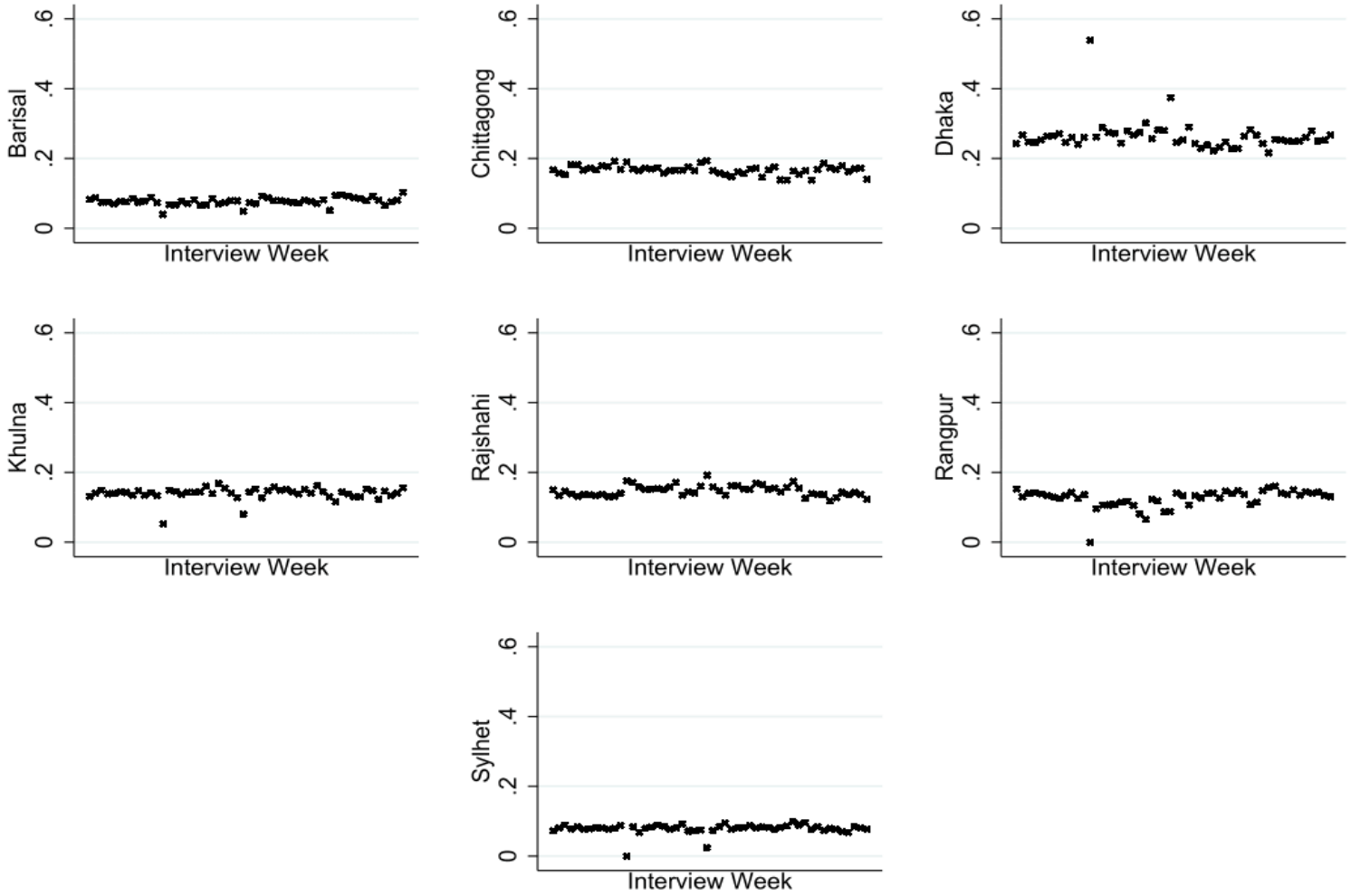


Figure 1.6: Portion of sample from each district by interview week

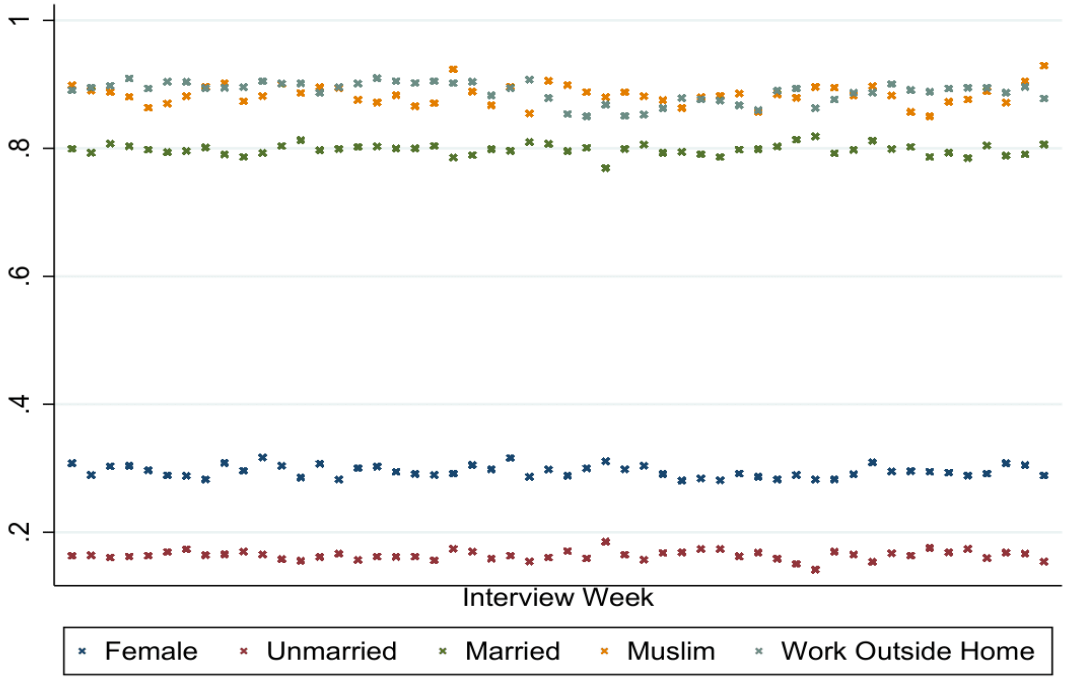


Figure 1.7: Demographic characteristic in sample by interview week

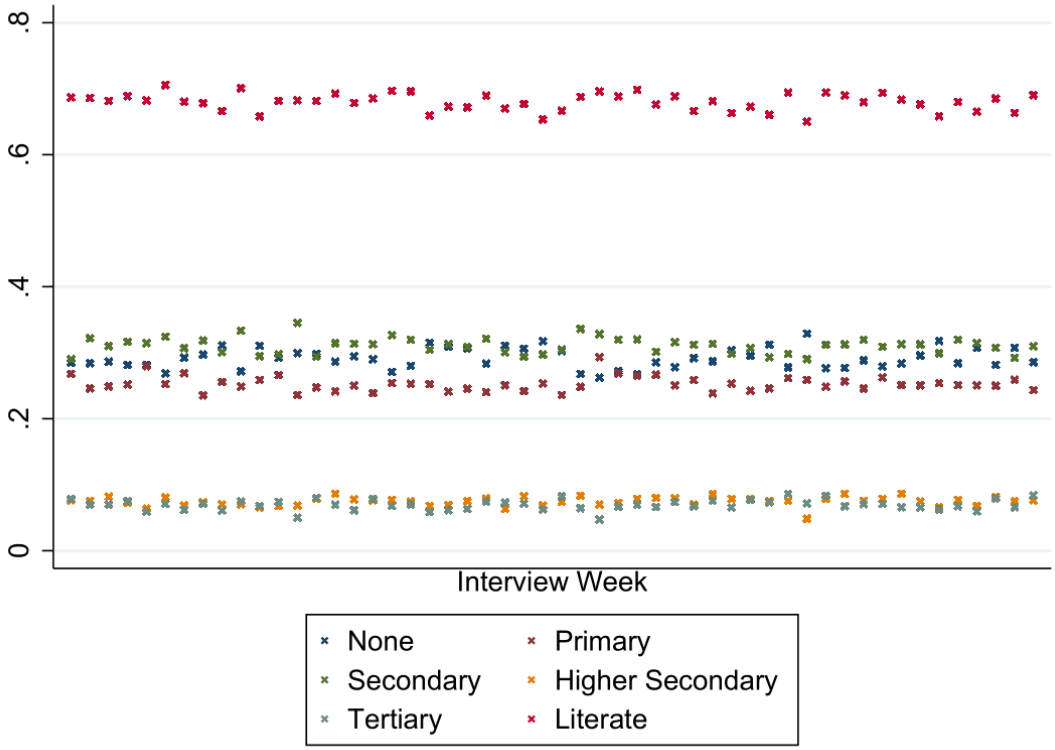


Figure 1.8: Education characteristics of sample by interview week

Table 1.11 Average differences in weekly sample characteristics

	Mean	SD	Min	Max
District				
Barisal	6.68e-11	.009428	-.0380292	.0252707
Chittagong	4.55e-11	.0120973	-.0294075	.0262923
Dhaka	2.56e-10	.0315503	-.0425468	.2806377
Khulna	2.69e-11	.0138621	-.0888775	.0268115
Rajshahi	1.25e-11	.0139401	-.0285452	.0452797
Rangpur	-1.27e-11	.0217744	-.128234	.0319428
Sylhet	4.33e-11	.0109093	-.0793176	.019876
Female	7.54e-11	.0092911	-.0147972	.0215863
Unmarried	9.20e-11	.0061517	-.0228295	.0206809
Married	8.25e-11	.0071464	-.0283624	.021177
Muslim	-1.43e-10	.0156766	-.0331081	.0461348
Works Outside Home	1.05e-10	.0161548	-.0385639	.0210389
Education				
None	-5.88e-11	.014317	-.0289961	.038017
Primary	5.13e-11	.0098655	-.0167758	.0410789
Secondary	-5.64e-11	.0109955	-.0201836	.0348294
Higher Secondary	-1.33e-11	.0063039	-.0268281	.0112108
Tertiary	1.00e-10	.0067947	-.0224502	.0163581
Literate	-6.23e-11	.0124195	-.0299829	.0256395
BSIC Industry				
Agriculture	1.44e-10	.0232997	-.0468642	.045121
Retail	-8.50e-12	.0081928	-.0144316	.0177637
Transportation	2.53e-11	.0053626	-.0125539	.0219846
RMG Manu.	-3.64e-11	.0090791	-.0170279	.03383
Education	-2.55e-11	.0039269	-.0100504	.0082829
Construction	8.60e-12	.0045978	-.0084714	.0101563
Dom. Services	-1.68e-11	.0039332	-.0103394	.0096048
Other Per. Services	-3.28e-11	.0036596	-.0101462	.0081542

1.7.2 Measurement Error

The independent variable of interest used in all specifications, x, a binary indicator of whether both newspapers had headlines on sexual assault on a given day, can be measure inaccurately. It may be that the newspaper shock might be underestimating the actual number of newspaper headlines on sexual assault. Suppose we call the true value, X. Then the variable we use can be related to X in the following way: $X = x + u$.

X	x	u
1	1	0
0	0	0
1	**0**	**1**
0	1	-1

In the case of a binary variable, the proxy may be mis-measured in two ways, (1) classifying days that have shocks as not having shocks with some probability p and, (2) days that were classified as having shocks that did not with probability,q. I focus on the former here and do not consider over-counting.[10] There is potential for measurement error in measuring the shocks to the sexual violence saliency through newspaper reports. The predominant language spoken in Bangladesh is not English. Using information from English language websites may lead to under counting of incidences and misclassifying days as non-shocks when they did in fact have shocks. However, since the Daily Star is exclusively produced in English it may mitigate some of the measurement issues. [11] It is unlikely however, that I observe a shock in the English language papers that is not in the Bengali version. There could also be undercounting due to the limited set

[10] A strict set of key-words were used so it is unlikely that there is over counting from scraping and it is unlikely that an article about sexual assault in Bangladesh is published in only the English language version of these papers.
[11] Yet, since the Daily Star is only produced in English its audience may be significantly different than this sample of workers from the QLFS. It is likely that this audience may be people who live outside of Bangladesh, they may be more educated on average and wealthier. In Bangladesh, there is a negative link between wealth and women's labor force participation.

of keywords used in the web-scraping. Unlike classical measurement error, where attenuation bias would bias the point estimates toward zero, if there is systematic under-counting the point estimate can be biased in either direction and lead to improper conclusions.

I simulate the effect on the point estimate from equation (1) varying p, the probability of misclassifying a shock day as a non-shock day from 10%, 25%, 50%, 75% and 90%. Figure 1.9 shows the distribution of βs. The results suggest that potential measurement error could be biasing the estimate on the contemporaneous effect on men's labor supply of sexual assault headline in both papers. It may be that men are actually reducing their labor supply on the day of a report. However, the simulations for the lagged periods are directionally consistent with the effects on men's labor supply with the point estimates from model (1). The simulation result also shows that the estimated effects on women's labor are also directionally consistent with the results of the analysis. For effect on women of L_1 I predict a negative effect but the distribution of βs suggest the total effect on women is close to zero when measurement error becomes increasingly severe.

1.7.3 Future Periods

In order to test the extent to which shocks were unexpected, I also regress my main outcomes on the leads of the shock to see if there is any anticipatory behavior observed in the data. Based on table 1.12, we can see that there is a weakly significant effect on the lead of the shock on female hours worked. All other effects on either outcome are not significant. When we control for the lags of the shock (as in table 1.13), we see that the effect of the leads are no longer significant. This gives evidence for the idea that there was no anticipatory change in labor behavior and that the shock was plausibly exogenous.

Table 1.12 The effect of future headlines in both papers on labor supply

	Hours worked	Worked at all
F_1	0.0003	0.0001
	(0.014)	(0.08)
F_1 ∗Female	0.037	0.004
	(0.019)*	(1.57)
R^2	0.30	0.29
N	1,114,470	1,114,470

* p<0.1; ** p<0.05; *** p<0.01 Standard errors clustered at PSU. Retroactive panel constructed from the information on the seven days prior to interview. I include individual level fixed effects, day of week fixed effects, month fixed effects and an indicator for public holidays.

Table 1.13 The effect of future and contemporaneous headlines in both papers on labor supply

	Hours worked	Worked at all
L_0	0.049	0.005
	(0.015)***	(2.89)***
L_0 ∗Female	-0.083	-0.007
	(0.020)***	(2.61)***
F_1	0.007	0.001
	(0.014)	(0.50)
F_1 ∗Female	0.026	0.003
	(0.019)	(1.16)
R^2	0.30	0.29
N	1,114,470	1,114,470

* p<0.1; ** p<0.05; *** p<0.01 Standard errors clustered at PSU. Retroactive panel constructed from the information on the seven days prior to interview. I include individual level fixed effects, day of week fixed effects, month fixed effects and an indicator for public holidays.

1.7.4 Sensitively to different number of lags

One plausible critique might be the choice of lags of the shock. I posit that labor market behavioral changes should dissipate and that longer lags will not be informative. This is due to the nature of how the shock changes behavior; observing or hearing about sexual assault (as proxied by news reports) works through a fear-based mechanism. This causes a short-term shock that then settles back to some labor supply equilibrium. Furthermore, labor supply although flexible in the short term is constrained by minimum consumption needs by the

household and cannot be reduced for too long. As such, higher level lags will only tend to pick up autocorrelation, and not the signal coming from the proxy, itself. There is an acute trade-off in the number of lags to include; too many lags may cause issues arising from collinearity that may lead to biased and unreliable estimates and greater variance however, with too few lags I may miss important dynamic effects of the shock (Wooldridge 2012).

Tables 1.14 and 1.15 show the effects from including lower and higher order lags of the shock to the estimation strategy. Due to individual fixed effects, it is only possible to have up to a fifth-order lag. We can see that including the fourth lag leads to effects that are not significant. When we include the fifth lag, there is significance, however, the point estimate vary greatly from the pervious models suggesting that degree of multi-collinearity has been sufficiently exacerbated.

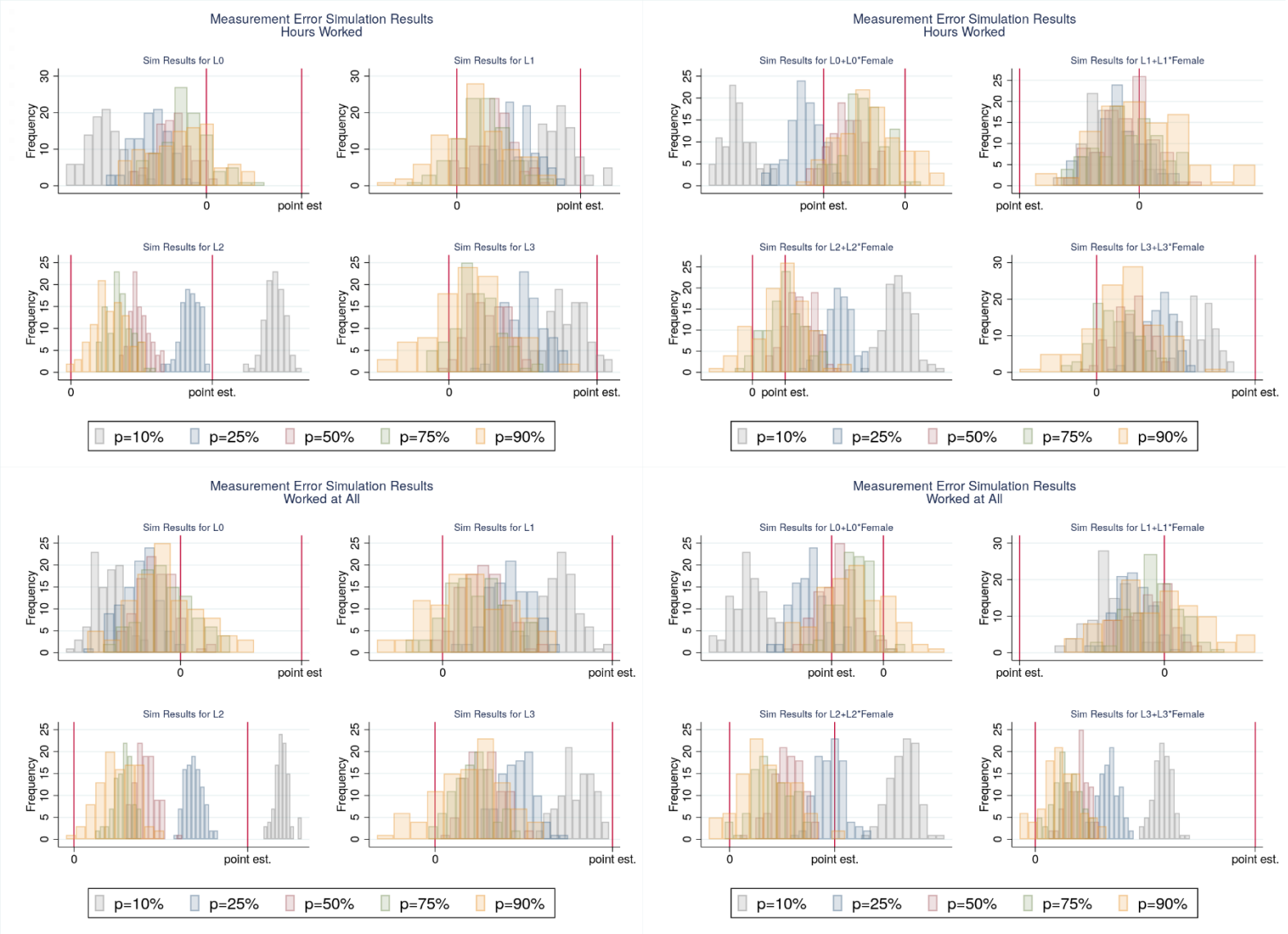


Figure 1.9: Simulated results from measurement error due to undercounting sexual assault newspaper headlines

Table 1.14 Changing number of lags in specification on hours worked

	Hours worked	Hours worked	Hours worked	Hours worked	Hours worked	Hours worked
L_0	0.005	0.034	0.045	0.044	0.042	0.040
	(0.032)	(0.023)	(0.020)**	(0.019)**	(0.017)**	(0.014)***
L_0 ∗Female	-0.086	-0.074	-0.099	-0.106	-0.097	-0.081
	(0.044)*	(0.031)**	(0.027)***	(0.025)***	(0.022)***	(0.019)***
L_1	0.003	0.047	0.059	0.060	0.040	
	(0.037)	(0.024)*	(0.020)***	(0.018)***	(0.016)**	
L_1 ∗Female	-0.062	-0.093	-0.122	-0.120	-0.105	
	(0.054)	(0.034)***	(0.028)***	(0.024)***	(0.021)***	
L_2	0.051	0.106	0.128	0.104		
	(0.038)	(0.024)***	(0.019)***	(0.016)***		
L_2 ∗Female	0.019	-0.070	-0.100	-0.087		
	(0.057)	(0.034)**	(0.027)***	(0.022)***		
L_3	0.001	0.068	0.064			
	(0.038)	(0.023)***	(0.018)***			
L_3 ∗Female	0.128	0.051	0.036			
	(0.056)**	(0.034)	(0.025)			
L_4	-0.099	0.020				
	(0.039)**	(0.025)				
L_4 ∗Female	0.102	0.044				
	(0.057)*	(0.033)				
L_5	-0.106					
	(0.031)***					
L_5 ∗Female	-0.033					
	(0.045)					
R^2	0.26	0.29	0.29	0.30	0.30	0.30
N	371,490	557,235	742,980	928,725	1,114,470	1,300,215

* p<0.1; ** p<0.05; *** p<0.01 Standard errors clustered at PSU. Retroactive panel constructed from the information on the seven days prior to interview. I include individual level fixed effects, day of week fixed effects, month fixed effects and an indicator for public holidays.

Table 1.15 Changing number of lags in specification on working at all

	Worked at all	Worked at all	Worked at all	Worked at all	Worked at all	Worked at all
L_0	-0.00007	0.004	0.006	0.006	0.005	0.004
	(0.003)	(0.003)*	(0.002)***	(0.002)***	(0.002)***	(0.002)***
L_0 ∗Female	-0.013	-0.010	-0.011	-0.013	-0.011	-0.008
	(0.005)**	(0.004)***	(0.004)***	(0.003)***	(0.003)***	(0.002)***
L_1	-0.00002	0.006	0.008	0.008	0.005	
	(0.004)	(0.003)**	(0.002)***	(0.002)***	(0.002)***	
L_1 ∗Female	-0.019	-0.016	-0.018	-0.018	-0.016	
	(0.006)***	(0.005)***	(0.004)***	(0.004)***	(0.003)***	
L_2	0.010	0.017	0.019	0.016		
	(0.004)**	(0.003)***	(0.002)***	(0.002)***		
L_2 ∗Female	-0.00005	-0.006	-0.008	-0.010		
	(0.007)	(0.004)	(0.004)**	(0.003)***		
L_3	0.004	0.011	0.010			
	(0.004)	(0.003)***	(0.002)***			
L_3 ∗Female	0.019	0.020	0.019			
	(0.007)***	(0.004)***	(0.003)***			
L_4	-0.011	0.002				
	(0.004)**	(0.003)				
L_4 ∗Female	-0.001	0.004				
	(0.007)	(0.004)				
L_5	-0.014					
	(0.003)***					
L_5 ∗Female	-0.017					
	(0.005)***					
R^2	0.24	0.27	0.28	0.29	0.29	0.29
N	371,490	557,235	742,980	928,725	1,114,470	1,300,215

* p<0.1; ** p<0.05; *** p<0.01 Standard errors clustered at PSU. Retroactive panel constructed from the information on the seven days prior to interview. I include individual level fixed effects, day of week fixed effects, month fixed effects and an indicator for public holidays. Estimates for OLS linear probability model

1.8 Conclusions

By using the timing structure in the Bangladesh QLFS, I exploit plausibly exogenous variation in the salience to sexual assault risks. By scraping the websites of two popular newspapers in Bangladesh, I construct proxies that measured a shock to the salience of sexual violence. My results add to the evidence that fear of gender-based violence affects women's economic outcomes. I estimate the average effect on short term labor supply of a large heterogenous population. Women decrease their hours and decrease the likelihood of working in response to news headlines about sexual assault. Women decrease hours they work by 0.0539 hours and 0.0625 hours on a day both newspapers have articles on sexual violence and the day after, respectively. There is a 0.4PP reduction in the likelihood that a woman works at all on the day of and a 1PP decrease on the day after both newspapers report on sexual assault.

The effects are concentrated among women who work outside the household. Women who must commute to work decrease hours worked by 0.0975 hours and 0.1025 hours on the day of and day after. They decrease the likelihood of working at all by 1.5PPs and 1.7PPs the day of and day after, respectively. Women who work from home modestly increase their labor supply. These results are largely robust to changes to specification and accounting for measurement error.

This paper contributes to the growing literature connecting women's labor market outcomes and fear of sexual violence by exploring intensive margin effects on labor supply. Previous work has focused on the extensive margin such as whether women participate in the labor market or have work outside of their home. However, in Bangladesh and many other emerging economies, women's labor force participation is positively correlated with poverty status. Women who are in the labor force are more economically constrained than those who are

not, so reducing labor supply is potentially more costly for them. Furthermore, this paper finds evidence that men also react to headlines on sexual assault. Men increase their labor supply when both newspapers have headlines on sexual violence. They continue to increase their labor supply even when women begin to increase their hours and begin to increase the probability of working at all. These effects are also robust to whether women in their households are in the labor force or work outside the home.

Women's labor force participation has been increasing consistently in Bangladesh for the past 30 years and as of 2015, sixty-nine percent of women in labor force work outside of their home. Fear of sexual violence limits their mobility and use of public spaces. Limiting women's mobility has negative economic impacts on the individual level but with an increasingly feminized work force, there are potentially negative effects of this limitation for the larger economy.[12]

The policy implications of this study show that to maintain and increase women participation in the labor force, investments need to be made to increase the sense of safety women feel in public. This may include creating more transportation options for women, like female carriages in trains or buses, destigmatizing being a victim of sexual violence, greater prosecution of perpetrators and more protection for victims.

[12] Back of the envelope calculation suggest this could lead to an aggregate loss of 3,593 hours in L_0 by women who work outside of their home.

Chapter 2:

Public Work Programs and Gender Based Violence: Evidence from a Road Maintenance Program in Laos PDR

2.1 Introduction

Public workfare programs (PWPs) can be used both to provide economic opportunities for at-needs groups, such as poor households, and to provide a public good through infrastructure improvements or other services completed by program members. Unlike cash transfers, which are also often used by policymakers to improve welfare of poor households, PWPs require participants to work, often in low skilled jobs, to receive transfers. The target population of workfare programs like PWPs traditionally lack access to the labor market for various reasons such as limited supply of wage jobs and low level skills to be competitive. Since PWPs are typically limited term or seasonal, they can be viewed as temporary labor-intensive low skilled jobs.

In this paper we investigate the effects of temporary employment in a Laotian PWP that targeted poor women to do routine maintenance work on ancillary rural roads. Exploiting the randomized control trial design of the Lao PDR Road Management Group (RMG) program, we are able to provide evidence on the causal relationship between women's temporary employment opportunities and their exposure to GBV. The RMG program targeted women from the poorest households in villages in rural Laos where few wage opportunities exist. We find that treatment increases women's employment in wage work and their household earnings. Their increase in earnings account for nearly 83- 86% of household earnings. However, we do not find that participation in the program changes their exposure to GBV. Our results hold when we look at specific forms of IPV and domestic violence such as controlling behavior, emotional violence, physical violence or physical violence from other household members. Despite finding no

change to GBV in the household, we find that treatment improved a women's well-being in the household as measured through decision-making and increases in her willingness to speak publicly in the community on different issues.

Our study is similar to the experiment of Kotsadam and Villanger (2020) that identifies the effects of employment on IPV by randomly assigning manufacturing jobs to equally qualified female applicants in Ethiopia. Our sample has fewer outside options than that of Kotsadam and Villanger (2020); households live in an area with few wage opportunities and qualified to be assigned a job solely based on poverty level. The authors find no average effect of being offered a job on experiencing abuse in the previous 3 months. Kotsadam and Villanger (2020) also find that treatment does not impact mediators such as gender attitudes, acceptance of GBV or female empowerment, measured by decision making power. We, however, find that treatment increases a woman's decision making power by 1.06 to 1.24 standard deviations.

This is a particularly interesting result, since although increasing women's economic opportunities have shown to improve their human capital accumulation, reduce early marriage, delay fertility and generally increase female empowerment, the effect on GBV, and more specifically, intimate partner violence (IPV) is unclear. Theories of Nash-bargaining (Manser and Brown, 1980) suggest that improving women's outside options also improves their bargaining power, and is likely to improve their outcomes within marriage, including exposure to gender-based violence (GBV). However, instrumental violence theory, which states that violence can be used for extractive purposes (Block and Rao 2006) suggests a possibility of increasing GBV. A subset of instrumental violence theory models predict that increasing a woman's outside options may increase or decrease her exposure to domestic violence depending on her initial bargaining power. (Tauchen 1991; Eswaran and Malhotra 2011)

Empirical studies on spousal violence and job opportunities similarly find conflicting results. Aizer (2010) finds that wage shocks that positively affect women's relative wages decreases the incidence of domestic violence, while a shock that favors men's relative wages increases the incidence of domestic violence, in California. Giving women job offers in Ethiopia increased their likelihood of experiencing domestic violence (Hjort and Villanger, 2011). Heath (2014) finds empirical evidence that a woman's initial level of bargaining power can explain heterogeneity; better labor market options for women decrease the likelihood of experiencing domestic violence for women who have higher baseline bargaining power, but increase the likelihood for women with lower baseline bargaining power in Bangladesh.

Empirical work on the relationship between cash-transfers and GBV likewise shows mixed results. Buller et al. (2018) conducted a review of cash transfer programs and IPV in LMICs, and found that out of 14 studies, IPV decreased in 11, two studies registered no impact, and one showed that IPV increased and in another, it decreased. Male partners have used violence to extort cash transfer funds from female recipients (Bobonis et al., 2013; Buller et al, 2018). Some studies have found cash transfer programs to be effective at reducing IPV (Bobonis, González-Brenes, and Castro, 2013; Hidrobo and Fernald, 2013; Haushofer and Shapiro, 2013; Perova and Vakis, 2013). There are reductions in physical violence (Bobonis et al., 2013; Hidrobo et al 2012), sexual violence (Hidrobo et al., 2012), and controlling behavior (Perova, 2010; Hidrobo et al, 2012; Hidrobo and Fernald, 2013). But there is also considerable heterogeneity in impacts. For instance, Perova (2010) finds that physical and emotional abuse fell primarily to women with cash-paying jobs and fewer children, indicating that the change in the incidence of violence varies depending on the woman's options outside of marriage. Similarly, Hidrobo and Fernald (2013) find larger reductions in emotional abuse among women

who have more than 6 years of schooling. CCT programs have also had opposite effects on different types of violence: Mexico's Oportunidades conditional cash transfer program, led to a 5-7 percentage point lower likelihood of facing physical violence, but a 3-5 percentage point increase to the likelihood of facing emotional violence and threats of physical attack (Bobonis et al., 2013). Transfers from PWPs my induce different effects than unearned income from cash-transfers. With the RMG program, women take on a new role as providers for the household, challenging social and gender norms. However, working outside the home may create a greater sense of autonomy, allowing them to build more social connections and become more integrated with the larger community.

We contribute to the existing literature on GBV and women's economic opportunities by evaluating the effects of a PWP. To our knowledge, we are the only study exploring the causal relationship between workfare programs and gender based violence. PWPs and cash-transfer programs are often both used as tools for social protection for vulnerable populations in low income countries. However, PWPs may be substantially different in nature than unconditional cash transfers and conditional cash transfers that have non-labor requirements. Workfare programs often target a relatively vulnerable population that is typically poorer, less educated and have less access to the traditional job market. For women, these characteristics are often correlated with lower empowerment and bargaining power.[13] Instrumental violence theory and empirical work implies that this group may be particularly vulnerable to an increased exposure of GBV.

The remainder of this paper is organized as follows. Section 2.2 gives an overview of the RMG program, the randomization procedure and experimental design. Section 2.3 describes the

[13] Doss (2017) reviews the literature on proxies for bargaining power.

data. Section 2.4 reviews our methodology and estimation strategy. In section 2.5, we present and discuss results. Section 2.6 presents and discusses heterogeneity in outcomes by bargaining power at baseline. Section 2.7 presents results on other empowerment indices- decision making in the household and voice in the community. Section 2.8 concludes.

2.2 Road Management Group Program and Experiment Design

Lao PDR has averaged a 7.7% GDP growth rate over the past decade, making it among the fastest growing economies in the South East Asia region. As of 2011, Lao PDR has graduated to lower-middle income status. However, this economic growth has not been uniform across the country. Poverty remains highest in rural areas in the southeast and the central mountainous areas (along the border with Vietnam) as well as the northern midlands and highlands. The Lao Poverty Reduction Fund (PRF) is the largest multi-sector community-driven development project in the country. [14] The PRF's aim is to reduce poverty and eradicate mass poverty by 2020; with specific goals of making infrastructure improvements in sectors such as education, drinking water, irrigation, health and transportation at the village level within the country's poorest communities.

The RMG intervention is a part of the broader PRF program. A process evaluation of PRF's projects carried out in 2015 highlighted that they had been successful in improving infrastructure but that the post-completion sustainability of road projects was relatively poor: road quality deteriorated quickly. Under pressure from harsh rainy seasons and vehicular traffic, the access roads typically fell into extreme disrepair within a couple years and often became unusable. Aiming to provide a part-time supplementary income-earning opportunity to at-need households

[14] Unless otherwise specified, the information in this section is drawn from the PRF III Operations manual document available here: http://prflaos.org/sites/default/files/Library%20Items/files/1080/eng//prf_operationsmanual_Eng.pdf

and to extend the lives of the access roads, the Poverty Reduction Fund (PRF) established a pilot Road Maintenance Group (RMG) program, where villagers would be trained and formed into work units to provide basic road maintenance services. PRF is housed under the Government Office of Lao PDR. Its mission is to reduce poverty in Laos through community driven development to improve infrastructure and access to services and resources, with a focus on rural populations. PRF targeted women from poor households living in these villages, where wage-earning opportunities were limited.

These women were organized into RMGs and tasked with carrying out routine road maintenance, such as clearing roads of vegetation, clearing the drainage system and making small repairs to the road surface. They were provided with simple training and basic hand tools and were paid a fixed-daily rate set slightly below the prevailing market wage in each village. Payments were made monthly or quarterly, depending on the payment preference of each RMG, corresponding to the number of days worked. Typically, this was a few days each month, with monthly variation based on maintenance needs.[15] There may have been deductions to wage payments in the case of poor performance. To enforce this, PRF carried out road quality audits each month.[16]

The road maintenance activities under this cycle started in June 2018, soon after the road improvements under PRF III were completed. The maintenance contracts ran for 18 months, from October 2018 up to the January 2020, covering 2 full rainy seasons and providing an average of 75 days of employment for each RMG member. This equates to just over 4 days of

[15] Generally, the RMG members work more days in the rainy season months (e.g. 6-7) and fewer days in the dry season (2-3).
[16] In the pilot, the RMGs were paid for 50 person-days of work per km per year, divided into equal monthly payments. These were paid in full each month, unless PRF inspections found the performance of the RMG to be poor and a deduction was applied.

work per month. The RMG members were paid wages of around LAK 60,000/day (US$7.3/day), resulting in a total average income of around $550 per RMG member over the implementation period. Since this is a part-time activity, the expectation was that it provided supplementary income for these women.

PRF identified 71 road segments covering 344 km of rural roads in 7 provinces and 24 districts for the RMG intervention in this cycle. Usually, each road segment was maintained by one RMG consisting of 3-5 members from one village.[17] RMG members were selected from each village prior to the intervention. The Village Head played an important role in the selection process by informing and then identifying eligible candidates. There were two eligibility criteria:

I. Candidates must belong to a poor household, and up to one woman from each such household could participate in the selection.
II. Candidates must be between 18 and 50 years old, though the upper and lower age limits were not strictly enforced.

The number of eligible and interested women exceeded the number of available RMG jobs in every village. Therefore, a lottery was carried out to select RMG group members. Information on the RMG program was disseminated by the village chief and by local PRF staff. Interested women registered with their name and age. Using PRF poverty rankings from 2016, the data collection team determined which of the interested women were eligible for the lucky draw. [18] [19]

[17] The size of the groups was set based on the length of the road segment, with approx. one RMG member per km.
[18] Households were ranked among four poverty levels: poorest, poor, middle-income and better-off.
[19] The village head reviewed and updated the poverty rankings to account for changes since 2016.

Preference was given to women from the poorest households and female-headed households.[20] These are critical components in the implementation of PWPs – evaluations of the NREGS in India for example have found that its effectiveness was constrained by poor targeting performance and substantial leakage in some states (Dutta 2012, Banerjee 2016). Interventions that reduced these leakages were found to have substantial welfare effects (Muralidharan et al. 2018).

The lottery was carried out manually, with women drawing numbered balls from an urn.[21] Women who drew numbers 1 through n, where n was the number of spots in the RMG group, became RMG members. Women who pulled numbers starting from $n + 1$ joined the waitlist (WL). For the waitlist, the number also determined their position on the waitlist. Our final sample includes 339 RMG members and 843 waitlist women across 85 villages in 7 provinces. [22] The women in the RMG groups formed our treatment group and the women on the waitlist formed our control group.

One point worth noting is that the members of the waitlist could be used to replace RMG members who dropped out or did not take up the job, leading to imperfect compliance with our treatment assignment.

[20] The eligibility based on poverty-ranking was carried out to give preference to women from poorer households. It was implemented in the following way: if there were enough interested women from "Poorest" ranked households to fill the village's RMG and waitlist spots, the lucky draw was restricted to women from these households only. If there were not enough women from "Poorest" households, the lucky draw was opened up to households with the next poverty rank – "Poor" – households, and so on.

[21] The women did not necessarily need to be at the drawing; they could be registered by another household member. But women who were selected for the RMG program had to be available for a training session in the following two days after the drawing.

[22] Although most RMGs were intended to be drawn from only one village, some were drawn from two. This is why we ended up with a higher number of villages than road-segments. Furthermore, we had targeted 87 villages when planning data collection but later learned that four villages were merged. Therefore, there were only 85 villages in our sample and not 87.

The women selected through random lottery were registered as the intended worker. However, households were allowed to send in a substitute for the registered woman if she was sick or otherwise unavailable. The substitute worker could be any other household member, including male household members. It was not recorded when another household member temporarily filled in for the registered woman. But it *was* recorded in the endline survey if another woman in the household permanently took over the duties. Payments could only be made to the treatment and control households so households could not formally give away jobs to a woman in another household permanently. The road maintenance work started in June of 2018.

2.3 Data

Baseline data was collected between September and October 2018, before any salary payments were made. Women knew whether they were selected to be a part of a road maintenance group at the time of baseline data collection. We were able to interview 333 treatment households and women, and 813 control households and women.[23] Six treated households and 34 control households were not interviewed at baseline. We acknowledge that this may bias our estimation, but we argue that attrition is sufficiently small as compared to our sample size, limiting any potential bias.[24] The baseline data allowed us to verify that treatment and control groups were largely balanced on observable characteristics prior to the intervention, supporting our understanding that the lotteries were conducted fairly and without bias. Endline data collection took place in December 2019 and January 2020, 18 months after the program started.

[23] The waitlist group was intended to be about 1.5 times the size of the RMG, rounded up to the nearest whole number.

[24] Data on age, treatment status and PRF poverty rank was collected at the time of randomization. Table 2A.2 shows how attrition varied by characteristics at randomization. Attrition from randomization to endline was positively and significantly correlated with treatment status.

There were four instruments at baseline: a household survey and a survey given to the RMG women and waitlisted women (and their households), a survey to a randomly selected group of women, a survey to the village head and a survey of market prices. The endline survey consisted of only the household and women's surveys and the market price survey.

The 85 villages participating in the RMG program were among Lao's poorest and offered few job opportunities. According to the baseline data, only one-fifth of working-age individuals in RMG-eligible households had paid work outside the household. Over 70% of RMG-eligible women worked on the household farm or business, typically in unpaid roles. When compared against a nationally representative sample from Lao Expenditure and Consumption Survey (LECS) V, the RMG-eligible households were poorer across a range of wealth indicators, including housing composition, ownership of durables, and nutrition consumption; wait-listed and RMG women were also less educated on average and less likely to be in the labor force (Worldbank 2019; Ministry of Agriculture and Forestry PRF 2016). [25] [26]

[25] LECS V data were collected in 2013.
[26] Authors' calculations; available upon request.

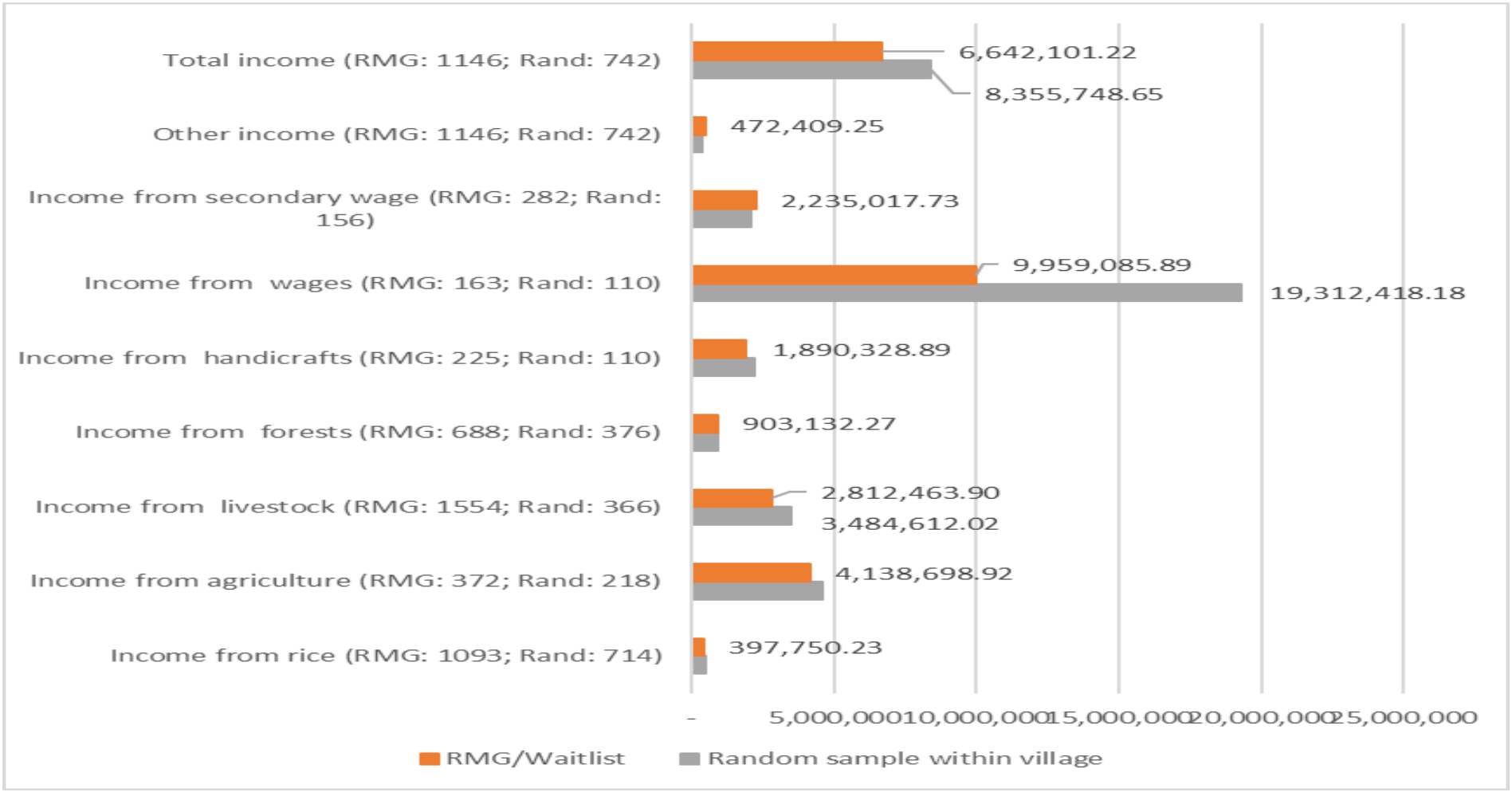


Figure 2.1: Annual household income (LAK) for RMG eligible women and a random sample of women. Values for both groups reported when difference is statistically significant at the 5% level.

We use the data collected from the survey of randomly selected women to see whether the program also targeted poor women within the village well. We compare the characteristics of the broader village population to the characteristics of the treatment and control women. The village heads were surveyed on the criteria they used to target households for the program. Figure 2.1 describes how RMG and waitlist women compared to a random sample of woman from these villages along income. We find that the treatment and control groups come from poorer households and earn substantially less from wages than the women from the random sample. Based on observable characteristics, our sample is particularly vulnerable when compared to the large population of Laos PDR and are among the poorest in these already poor villages. Figure 2.2 shows that not only are RMG-eligible women from poorer households, on average, but that there is a greater density of them at lower levels of income.

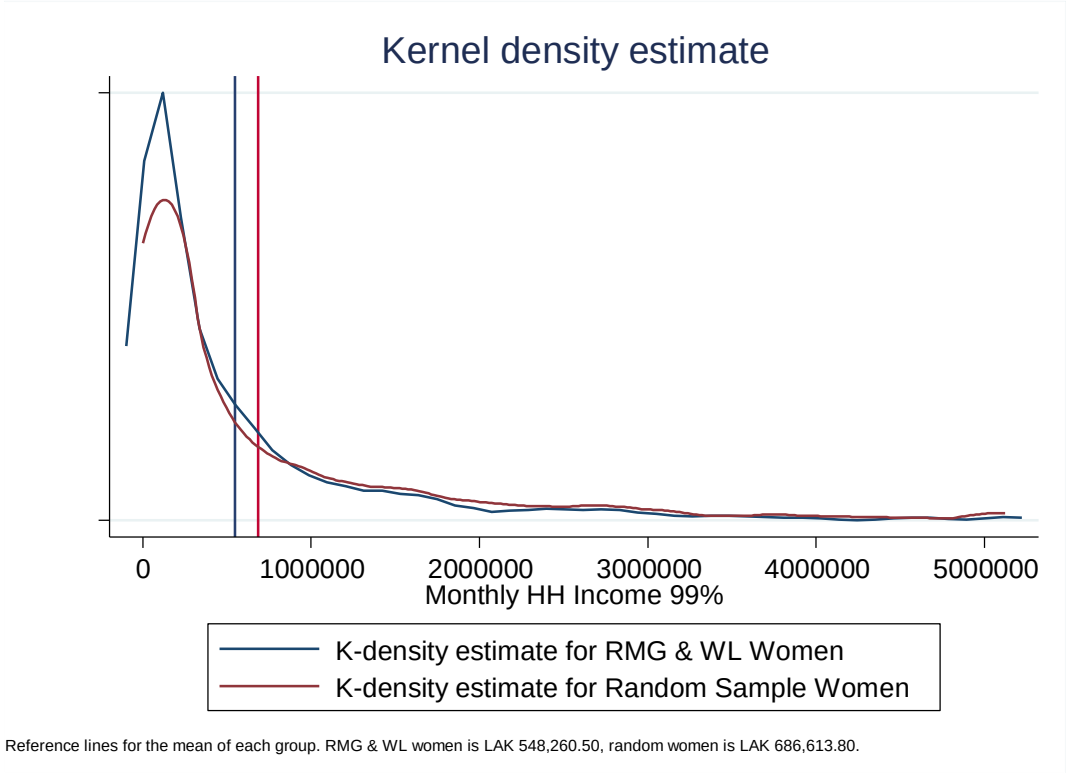


Figure 2.2: Density estimates of RMG eligible women and a random sample of women

To explore whether the sample was balanced, we analyze whether the RMG women were statistically different from waitlisted women across the following characteristics at baseline: age, whether they were currently married, education level, ethnicity, household size and average monthly household income. Table 2.1 summarizes the characteristics of the non-attrition sample.[27] The treated and control samples are balanced across many characteristics, however, there are relatively fewer Hmong-Emien women in the treated sample at baseline. Treated households were also poorer, on average, but this may be due to how the program specifically targeted the poorest households and only relaxed the eligibility criteria to create a sufficiently sized comparison group, causing some villages to have a higher average wealth among eligible women. We control for all of these baseline characteristics in our estimations to account for potential bias from imbalance

Table 2.1: Baseline Characteristics by Treatment Status

	RMG Women Mean	sd	Waitlist Women Mean	sd	Diff.	Obs.
Age	32.90	10.76	33.26	10.46	-0.503	1099
Married	0.89	0.32	0.87	0.33	0.012	1099
Education: None	0.45	0.50	0.41	0.49	0.035	1099
Education: Primary	0.44	0.50	0.48	0.50	-0.032	1099
Education: Lower Sec.	0.09	0.29	0.10	0.31	-0.015	1099
Education: Upper Sec +	0.02	0.15	0.01	0.10	0.011	1099
Lao-Tai	0.22	0.42	0.23	0.42	-0.012	1099
Mon-Khmer	0.61	0.49	0.66	0.48	-0.040	1099
Chinese-Tibet	0.06	0.24	0.04	0.21	0.014	1099
Hmong-Emien	0.10	0.30	0.07	0.25	0.038**	1099
Ethnicity – Other	0.00	0.06	0.00	0.05	0.001	1099
HH Size	6.36	3.83	5.85	2.69	0.511**	1099
Monthly HH Income	484992.00	768128.64	582842.25	871138.21	-95669.62*	1099

* $p < 0.1$ ** $p < .05$, *** $p < 0.01$. Lao-Tai is the national ethnic majority culture in Laos.

[27] Appendix table 2A.1 summarizes baseline characteristics for the original sample.

The endline survey consisted of only the household and women's surveys and the price survey. At endline, we were able to interview 323 treated households and 776 control households. There was attrition of 47 households. The attrition rate was not different between treatment and control groups. However, the women who were not able to follow up were on average older and were more likely to be of the Lao-Tai ethnic group. We present attrition rates by treatment status and baseline characteristics in Table 2.2.

Table 2.2: Difference in attrition rates from baseline to endline by treatment status and baseline characteristics

Difference between baseline and endline Sample	
RMG	-0.0811
	(0.0676)
Age	3.742**
	(1.565)
Married	0.0687
	(0.0494)
None	0.0836
	(0.0736)
Primary	-0.0697
	(0.0743)
Lower Sec.	0.0150
	(0.0446)
Upper Sec +	-0.0289
	(0.0180)
Lao-Tai	0.167***
	(0.0620)
Mon-Khmer	-0.124*
	(0.0712)

Chinese-Tibet	0.0279
	(0.0319)
Hmong-Emien	-0.0734*
	(0.0400)
Eth Other	0.00273
	(0.00762)
HH Size	0.615
	(0.458)
Monthly HH Inc	-23516.3
	(125706.3)
Observations	1146

* p<0.1, ** p<.05, *** p<0.01

At both baseline and endline, questionnaires were administered to the RMG and WL households and to the specific women enrolled as RMG and WL members. The household and individual questionnaires included modules on labor force participation, household farms and businesses, household income, housing conditions, assets, savings, economic shocks, nutrition, education, childcare, time use, social connections, public voice, subjective wellbeing, sense of agency, decision-making, gender-based violence (GBV) norms and experience, and perceptions and experiences regarding RMG utility, membership, and payments.[28]

A special audio-computer self-interviewing (ACASI) module was developed for endline administration to protect participants' privacy while answering sensitive questions on GBV. Otherwise, fully private conversations were not possible. The construction of the participants' homes (frequently wood or bamboo) allowed sound to pass through easily, and family members

[28] Social connections inquired into relationships with the village head at baseline and with other village members at endline. GBV experience was measured only at endline. Subjective wellbeing was included in the household questionnaire (often answered by the household head) at baseline and in the individual questionnaire (answered only by the RMG or WL woman) at endline.

were often home. The participants were also typically illiterate and frequently understood only minority ethnic languages. Our team specifically designed the ACASI module to suit this study population's needs. Questions were recorded on electronic tablets in multiple ethnic languages and participants were taught during survey administration how to select answers by pressing the appropriate color coded symbols (e.g. green star = yes). Participants listened to the questions with headphones and selected their own answers, ensuring confidentiality.

Figure 2.3: RMG and WL women using ACASI

2.4 Methodology

We measure impact of the RMG program on women and their households through ITT and ToT models. ITT allows us to estimate the population-level effectiveness of the intervention where participants may not perfectly comply with the intervention activities, while ToT identifies the results on those who engaged in the intervention and shows the efficacy that can be achieved if participants comply. We estimate the ITT effect on GBV by following estimating equation:

$$y_{i1} = \alpha + \beta Treat_{i0} + \gamma X_{i0} + \epsilon_i$$

where y_{i1} is the value of the outcome of interest at endline, $Treat_{i0}$ is a dummy that takes the value of 1 for the treatment group (RMG) as assigned at baseline, and X_{i0} contains a set of baseline characteristics (pre-intervention).

For decision making outcomes, we were able to include the baseline values. Therefore, we estimate ITT by regressing various outcomes of interest according to the following ANCOVA formula (McKenzie, 2012),

$$y_{i1} = \alpha + \beta Treat_{i0} + \gamma X_{i0} + \theta y_{i0} + \epsilon_i$$

Where y_{i0} is the baseline value of the outcome variable y.

We estimate ToT through: [29]

$$y_{i1} = \alpha + \beta \widehat{T_{i1}} + \gamma X_{i0} + \epsilon_i$$

[29] We include the baseline value of the outcome variable when possible.

where $\widehat{T_{i1}}$ is the predicted value of take-up of the RMG role based on random assignment through the public lottery. Likelihood of take-up is estimated as:

$$T_i = \alpha + \pi Z_i + u_i$$

where Z_i is an indicator that takes the value of 1 for women assigned to the RMG and 0 for women assigned to the waitlist.

We use robust standard errors in all specifications. Since randomization was at the individual level, standard errors are not clustered. However, we also run the estimations with errors clustered by village as a robustness check to account for potential variation in treatment intensity due to the varying number of RMG members per village. The standard errors and p-values for these estimations can be found in Tables 2A.3-2A.5.

Expecting differences across different levels of bargaining power at baseline, we include an interaction term to account for heterogenous effects. We include a term H_i, a binary indicator equal to 1 if the observation has a relatively higher level of bargaining power at baseline.

$$y_{i1} = \alpha + \beta_1 Treat_{i0} + \beta_2 H_i * Treat_{i0}$$
$$+\kappa H_i + \gamma X_{i0} + \epsilon_i$$

Therefore β_1 provides the treatment impact for someone with lower initial bargaining power and $\beta_1 + \beta_2$ provides an estimate of the treatment effect for someone with a higher level of bargaining power.

2.5 Results and Discussion

2.5.1 Employment and Income

The RMG program was meant to directly change labor market activities by giving participants a regularly paying job for 18 months. To see if the program worked as intended, we estimate the direct effect of treatment on whether the women were listed among the household members that regularly contributed wage income, and average monthly wage income contributed over the 12 months preceding the survey. We winsorized average monthly earnings at the 99th percentile to minimize the effects of outliers.

Table 2.3: The effect of treatment on women's LFP and employment.

	LFP	Employed in Paid Work	Regular Earner	Extra Work
		Panel A: Intent to Treat		
RMG	0.0330	0.155	0.774***	-0.0251
	(0.0325)	(0.103)	(0.0230)	(0.0169)
Dep. Var. Control Group Mean	.324	.336	.05	.088
Adj. R-sq	0.0210	0.139	0.637	0.0128
		Panel B: Treatment on the Treated(2SLS)		
RMG	0.0489	0.170	0.939***	-0.0356*
	(0.0385)	(0.104)	(0.0273)	(0.0203)
Dep. Var. Control Group Mean	.312	.322	.055	.089
Adj. R-sq	0.0245	0.147	0.684	0.0121
Obs	1061	124	1099	1099
Baseline Char.	Yes	Yes	Yes	Yes
Baseline Dep. Var.	Yes	Yes	Yes	Yes

Robust standard errors in parentheses. LFP is a binary indicator if the RMG eligible woman is participating in the labor force. LFP=1 if the a woman is currently employed in reference week or attached to job she was absent from in the reference week or if she is unemployed, defined as being without a job and seeking work in the reference week or did not seek work but was available to work. Employed in Paid Work is a binary indicator of whether she was engaged in paid labor in the reference week. Regular earner is a binary outcome that is equal to 1 if the woman was listed as a household member that regularly earned a salary in past 12 months. Extra work is binary indicator equal to 1 if the RMG eligible woman was listed as a household member that took on extra work over the past 12 months that contributed to

household wage income. We control for the following baseline characteristics: age, a binary indictor for married, an indicator for ever attending school, indicators for ethnicity being Lao-Tai, Mon-Khmer, Chinese-Tibet, Hmong-Emien or other, household size and average monthly household income. We also control for the baseline value of the outcome of interest.
* $p < 0.1$, ** $p < .05$, *** $p < 0.01$

We see increases in regular work for RMG members. In Table 2.3, we see that, under the ITT model, RMG members are 77.4% more likely than the WL group to be regular income earners. When looking only at those who engaged in the RMG program, rather than all who were assigned to participate, we see that the effect is even stronger—RMG members are 93.9% more likely to be regular earners. The recognition that comes with being a regular earner may increase the women's value and bargaining power in the household.[30]

We also see that RMG women and their households receive more income in wages and have higher monthly incomes than the WL group. The income variables were all winsorized at the 99th percentile to minimize the effects of outliers. Tables 2.4 and 2.5 show that RMG members earn 175,598.30 LAK ($19.58 USD) more per month than the control group in wage jobs.[31] Monthly income increases by 211,302.60 LAK ($23.56 USD) for RMG households, nearly 50% greater than WL household income, and by 188,721.50 LAK ($21.04 USD) in wage earnings alone, approximately 110% above WL household wage earnings, according to the ITT calculation. Under ToT calculations, RMG member and household wage earnings are even higher, but RMG members are shown to be less likely to engage in additional side work.[32] An

[30] RMG workers are not seen to be significantly more likely to be employed in paid work. However, this is likely a function of two elements in the survey structure and administration. Firstly, the survey was administered after RMG members had been assigned and started work, but before any RMG payments were made. If we exclude baseline data from the analysis, we see a 15% increase in likelihood of paid work among the RMG group. Secondly, the survey gathered job payment information for the 7 days prior to survey administration. If RMG members did not perform paid work, including on the RMG, in the 7 days prior to the survey, they would not be recorded as engaged in paid work. As the RMGs required approximately 4 days of labor per month, RMG members commonly did not report RMG income for the week prior to the survey.
[31] Currency conversion rates as per freecurrencyrates.com on April 29, 2020.
[32] Although wage and monthly incomes increase, earnings for RMG household non-farm businesses decrease. Chowdhury (2020) discusses changes in work and earnings for RMG households in greater depth.

RMG women's increase in income accounts for nearly 83-86% of the increase in overall household income and 93-95% of the increase in a household's wage income

Table 2.4: The effect of treatment on women's income from wage work.

	Average Monthly Earnings	Average Monthly Earnings from Extra Work	Total Monthly Earnings
Panel A: Intent to Treat			
RMG	175598.3***	-468.2	173799.4***
	(7079.0)	(1066.7)	(7313.8)
Dep. Var. Control Group Mean	13886.491	3919.137	20404.424
Adj. R-sq	0.439	0.0150	0.403
Panel B: Treatment on the Treated(2SLS)			
RMG	212685.3***	-854.6	210335.7***
	(7998.6)	(1287.0)	(8231.4)
Dep. Var. Control Group Mean	14004.667	3975	19924.111
Adj. R-sq	0.495	0.0147	0.467
Obs	1099	1099	1099
Baseline Char.	Yes	Yes	Yes
Baseline Dep. Var.	Yes	Yes	Yes

Robust standard errors in parentheses. Outcomes are winsorized at the 99th percentile to minimize the effects of outliers. Earnings refer to wages earned from working outside of the household and household enterprises. We control for the following baseline characteristics: age, a binary indictor for married, an indicator for ever attending school, indicators for ethnicity being Lao-Tai, Mon-Khmer, Chinese-Tibet, Hmong-Emien or other, household size and average monthly household income. We also control for the baseline value of the outcome of interest. All income variables were divided by 12 to construct average monthly earnings. * $p < 0.1$, ** $p < .05$, *** $p < 0.01$

The substantial increase in a woman's relative income could decrease her exposure to GBV from her partner or other household members since it can improve her outside options and

likely reduce psychological stress by easing financial constraints. However, it may increase her exposure to violence since it could challenge the established social norms of men being providers in the households. We may also see an increase if other household members will use violence to extract resources from the woman.

Table 2.5: The effect of treatment on household income in past 12 months.

	HH Income	HH Wage Income
Panel A: Intent to Treat		
RMG	211302.6***	188721.5***
	(42089.3)	(27782.8)
Dep. Var. Control Group Mean	489609.915	166407.968
Adj. R-sq	0.333	0.392
Panel B: Treatment on the Treated(2SLS)		
RMG	247109.5***	221934.7***
	(50386.0)	(33469.8)
Dep. Var. Control Group Mean	505175.337	176662.444
Adj. R-sq	0.332	0.406
Obs	1099	1099
Baseline Char.	Yes	Yes
Baseline Dep. Var.	Yes	Yes

Robust standard errors in parentheses. Outcomes are winsorized at the 99th percentile to minimize the effects of outliers. We control for the following baseline characteristics: age, binary indictor for married, an indicator for ever attending school, indicators for ethnicity being Lao-Tai, Mon-Khmer, Chinese-Tibet, Hmong-Emien or other, and household size. We also control for the baseline value of the outcome of interest. We do not control for average household income at baseline to avoid collinearity with the baseline outcome. All income variables are average monthly values for income from the past 12 months. HH Income is the sum of income from farming and forestry, non-farm enterprises and wages (aggregated for all household members).
* $p < 0.1$, ** $p < .05$, *** $p < 0.01$.

2.5.2 Effects of PWP on GBV

Table 2.6 shows the estimated effect of being in the RMG program on two indices of violence. The first is the z-score over all types of domestic violence (exposure to controlling behavior by their most recent partner, emotional violence by their most recent partner, physical violence from their most recent partner, sexual violence from their most recent partner and physical violence from another household member). The second is a z-score index that relates only to IPV and excludes whether the woman experiences violence from other household members. The z-score indices show whether treat has a "general effect" on violence against women. They improve power to detect an effect on GBV since many marginally insignificant outcomes may aggregate to be significant (Anderson, 2008). The z-score indices are calculated as the sum of z-scores of each component of the index. Z-scores for each component of the index are computed by subtracting control group mean and dividing by the control group SD (as in Kling, Katz and Liebman, 2007). We also estimate the effect on the disaggregated set of measures; these results are presented in Table 2.7.

Table 2.6: The effect of treatment on GBV in the household

	GBV Index	IPV Index
Panel A: Intent to Treat		
RMG	0. 163	0.199
	(0. 235)	(0.198)
Dep. Var. Control Group Mean	.006	-.111
Adj. R-sq	0.0260	0.0257
P-val	.49	.31
Panel B: Treatment on the Treated (2SLS)		
RMG	0.190	0.239
	(0.272)	(0.234)
Dep. Var. Control Group Mean	.016	.024
Adj. R-sq	0.0256	0.0210
P-val	.49	.31
Obs	1025	1025
Baseline Char.	Yes	Yes

Baseline Dep. Var.	No	No

Robust standard errors in parentheses. We control for the following baseline characteristics: age, a binary indictor for married, an indicator for ever attending school, indicators for ethnicity being Lao-Tai, Mon-Khmer, Chinese-Tibet, Hmong-Emien or other, household size and average monthly household income. We also control for the baseline value of the outcome of interest when possible. GBV index is a z-score index of whether the woman was exposed to any form intimate partner violence, or physical violence from other HH members. IPV index is a z-score index if the woman has experienced any more of intimate partner violence in the past 12 months. * $p < 0.1$, ** $p < .05$, *** $p < 0.0$

The ITT and ToT models both estimate a positive effect on both the GBV index and the IPV index. However, the estimated effects are relatively small, .16 and .19 standard deviations on the GBV index and the .2 to .24 standard deviations on the IPV index. However, neither set of effects are statistically different from 0.

When we analyze specific types of violence, we similarly find small and insignificant effects. The point estimates of treatment on experiencing controlling behavior, physical violence from a partner, and sexual violence from their most recent partner are positive. The largest estimated effect is on the probability of experiencing controlling behavior; treatment increases the probability by 4.4-4.8PPs, however even this relatively large effect is not statistically distinguishable from 0. Treatment is estimated to decrease the probability of experiencing emotional violence and experiencing physical violence from other household members, however, these too are not statistically significant. We do not find evidence that supports Nash-bargaining theories, theories of backlash or instrumental violence theories as we do not see either a decrease or increase in GBV and IPV.

Table 2.7: The effect of treatment on GBV in the household (Disaggregated Measures)

	Controlling Behavior	Emotional Violence	Physical Violence	Sexual Violence	Physical Violence from Non-Partner
			*Panel **A**: Intent to Treat*		
RMG	0.0438	-0.00100	0.0159	0.0379	-0.0139
	(0.0346)	(0.0319)	(0.0316)	(0.0280)	(0.0191)
Dep. Var. Control Group Mean	.462	.3	.298	.181	.099
Adj. R-sq	0.0155	0.00640	0.0425	0.0371	0.0219
			Panel B: Treatment on the Treated(2SLS)		
RMG	0.0482	-0.00240	0.0171	0.0436	-0.0160
	(0.0400)	(0.0368)	(0.0364)	(0.0322)	(0.0222)
Dep. Var. Control Group Mean	.493	.316	.313	.186	.093
Adj. R-sq	0.0120	0.00701	0.0424	0.0383	0.0208
Obs	1025	1025	1025	1025	1049
Baseline Char.	Yes	Yes	Yes	Yes	Yes
Baseline Dep. Var.	No	No	No	No	No

Robust standard errors in parentheses. We control for the following baseline characteristics: age, a binary indictor for married, an indicator for ever attending school, indicators for ethnicity being Lao-Tai, Mon-Khmer, Chinese-Tibet, Hmong-Emien or other, household size and monthly household income. Data on gender-based violence was not collected at baseline so we cannot control for exposure before the RMG program. All outcomes are binary outcomes where the outcome equals 1 if she has experienced this type of violence in the past 12 months. * $p <$ 0.1, ** $p < .05$, *** $p < 0.01$.

2.6 Heterogenous Effects by Baseline Level of Empowerment

Our sample is on average poorer, less educated and has less access to the traditional labor market; it would not be unreasonable to expect increases on the average level as we might expected this population to have a lower initial level of bargaining as compared to the larger population. However, we further explore whether there are heterogenous effects depending on different levels of initial bargaining power within our sample.

We test whether there are heterogenous effects by initial bargaining level by using the following proxies: a woman's relative education to her spouse, whether the woman has any formal education, household income being below, at, or above the sample median; and finally a binary indicator if she is below, at, or above the sample median level of decision making in the household. [33]

The above proxies are established within the women's empowerment literature. Education (Malhotra, Schuler, et al. 2005; Quisumbing and Hallman 2003) and relative education (Thomas 1994, Quisumbing, Estudillo and Otsuka 2004, Hidrobo and Fernald 2013) between spouses has been shown to be correlated with bargaining power; women who are more educated and relatively more educated than their husbands tend to have higher levels of bargaining power and therefore may see a reduction in violence relative to their less educated counterparts, if treated.

Higher relative wealth and economic status has been associated with lower levels of violence against women; compared to the poorest socioeconomic group, the highest asset quintile was associated with significantly lower physical violence in Egypt and in Peru (Kishor and Johnson, 2004); higher levels of income and monthly household expenditures were associated with lower violence in India (Rao, 1997; Panda and Agarwal, 2005); and women on Medicare were more like to experience violence from their partner in the US (Azier 2010).

Women with greater bargaining power may have greater decision-making power in the household. Many studies have used self-reported decision-making as proxies for empowerment and bargaining power (Allendorf 2007, Patel et al. 2007, Mabsout et al 2010 , Reggio, 2010). We consider a binary indicator of being above or below the median level of a z-score index of

[33] A z-score index of all decision-making variables.

three key decision- making indicators: being the final decision maker, being the final decision maker on decisions the woman cares about and feeling that she should be the decision-maker on particular decisions and the relative fraction of decisions of the listed indicators.

Table 2.8 and table 2.9 present the results on whether heterogenous effects exist on experiencing GBV or IPV by a woman's baseline level of bargaining power. The ITT results estimate that being selected for the RMG program increases the amount of GBV and IPV violence a less empowered woman faces in all the models except where we use the decision-making level as a proxy for empowerment. Women with less initial decision-making power see a reduction in GBV and IPV but more empowered women see an increase in experiencing violence. But it should be noted that all effects are not statistically significant.

Treatment does not change whether a woman experiences violence in her home and does not show the existence of heterogenous effects by initial bargaining power, when proxied by relative education to her husband, having any formal education, and coming from a relatively wealthier household. We find some weak evidence that women with higher decision making at baseline see an increase in the amount of IPV they face. The ITT model estimates that participating in the RMG program increases the amount of IPV a woman faces by .465 standard deviations and does not have a statistically significant effect on women with lower initial decision making; this effect has a p-value of .09. We however, regard this outcome with caution since we cannot dually reject that the treatment effects are statistically different across these two groups (or that the base effect or interaction effects are statistically different from 0) as well as that the total treatment effect is statistically distinguishable from zero.

Table 2.8: Heterogeneous effect on GBV in the household by baseline level of bargaining power

	GBV Index	GBV Index	GBV Index	GBV Index
		Panel A: Intent to Treat		
RMG	0.222	0.356	0.225	-0.165
	(0.296)	(0.384)	(0.343)	(0.333)
RMG#Wife Edu Higher	-0.882			
	(0.677)			
RMG#Formal Edu		-0.345		
		(0.483)		
RMG#Above Median Inc			-0.126	
			(0.470)	
RMG#Above Median DM				0.598
				(0.467)
Control Group Mean (Proxy=0)	.095	.457	.141	.147
(Proxy=1)	.132	-.326	-.119	-.132
P-val $\beta_1 + \beta_2$=0	.28	.97	.76	.19
Adj. R-sq	0.0295	0.0265	0.0261	0.0275
		Panel B: Treatment on the Treated(2SLS)		
RMG	0.252	0.415	0.251	-0.187
	(0.335)	(0.446)	(0.380)	(0.374)
RMG#Wife Edu Higher	-1.059			
	(0.808)			
RMG#Formal Edu		-0.402		
		(0.557)		
RMG#Above Median Inc			-0.131	
			(0.541)	
RMG#Above Median DM				0.696
				(0.535)
Control Group Mean (Proxy=0)	.092	.438	.169	.125
(Proxy=1)	-.515	-.354	.062	.334

P-val $\beta_1 + \beta_2$=0	.28	.97	.77	.19
Adj. R-sq	0.0303	0.0266	0.0255	0.0258
Obs	803	1025	1025	1025
Baseline Char.	Yes	Yes	Yes	Yes
Baseline Dep. Var.	No	No	No	No

Robust standard errors in parentheses. We control for the following baseline characteristics: age, a binary indictor for married, an indicator for ever attending school, indicators for ethnicity being Lao-Tai, Mon-Khmer, Chinese-Tibet, Hmong-Emien or other, household size and average monthly household income. GBV index is a z-score index of whether the woman was exposed to any form intimate partner violence, or physical violence from other HH members. * $p < 0.1$, ** $p < .05$, *** $p < 0.01$

Table 2.9: Heterogeneous effect on IPV in the household by baseline level of bargaining power

	IPV Index	IPV Index	IPV Index	IPV Index
		Panel A: Intent to Treat		
RMG	0.222	0.241	0.284	-0.127
	(0.251)	(0.314)	(0.282)	(0.285)
RMG#Wife Edu Higher	-0.805			
	(0.565)			
RMG#Formal Edu		-0.0757		
		(0.402)		
RMG#Above Median Inc			-0.175	
			(0.395)	
RMG#Above Median DM				0.592
				(0.394)
Control Group Mean (Proxy=0)	.055	.286	-.049	-.048
(Proxy=1)	.089	-.405	-.181	-.187
P-val $\beta_1 + \beta_2$=0	.25	.51	.69	.09
Adj. R-sq	0.0246	0.0258	0.0260	0.0280
		Panel B: Treatment on the Treated(2SLS)		
RMG	0.311	0.414	0.310	-0.0854
	(0.285)	(0.373)	(0.319)	(0.333)
RMG#Wife Edu Higher	-1.066			

	(0.678)			
RMG#Formal Edu		-0.312		
		(0.475)		
RMG#Above Median Inc			-0.150	
			(0.466)	
RMG#Above Median DM				0.601
				(0.463)
Control Group Mean (Proxy=0)	.087	.373	.114	.121
(Proxy=1)	-.495	-.285	-.006	.296
P-val $\beta_1 + \beta_2$=0	.22	.73	.64	.11
Adj. R-sq	0.0263	0.0216	0.0210	0.0210
Obs	804	1026	1026	1026
Baseline Char.	Yes	Yes	Yes	Yes
Baseline Dep. Var.	No	No	No	No

Robust standard errors in parentheses. We control for the following baseline characteristics: age, a binary indictor for married, an indicator for ever attending school, indicators for ethnicity being Lao-Tai, Mon-Khmer, Chinese-Tibet, Hmong-Emien or other, household size and average monthly household income. IPV index is a z-score index if the woman has experienced any more of intimate partner violence in the past 12 months. * $p < 0.1$, ** $p < .05$, *** $p < .01$.

2.7 Empowerment through Other Channels

We explore if there were secondary effects of treatment on decision making within the household and social behavior within the community. The RMG program increased women's income by significant magnitudes; increasing their share of household income may increase their decision-making power in the household. The program also allows women to work outside the home on a visible community project.

2.7.1 Decision-making in the Household

Despite finding no changes in the exposure to GBV, we find that treatment increases their decision-making index by 1.06 to 1.24 standard deviations. We find similarly strong effects when looking at the individual components of the decision-making index. The survey asked a series of decision-making questions on four household topics: visiting the woman's family and friends, spending income earned by the woman, spending the partner's earned income, and buying expensive items for the household. For each of these topics, the survey asked which household members participated in discussions on the topic, who made the final decision, who the RMG or WL woman thought was best suited to make final decisions, and whether the RMG or WL woman cared about making a decision on these topics. RMG members participate more actively in household decisions and demonstrate increased self-efficacy in being the final decision makers, as well as an increased propensity to actually be the final decision maker. We see that RMG women believe they are most capable of being the final decision maker in 48.1% of cases, compared to 45.7% of cases among the WL, and that this difference is significant. We also see that RMG women are the final decision-makers 33.1% of the time, compared to WL women at 31.7%, and that this difference is also significant. The RMG women are 9.23PPs more likely to be the final decision makers on at least one household decision and 8.97PPs more likely

to be a decision-maker on a decision they care about. The figures presented here are for the ITT model; effects are stronger under ToT.

Table 2.10: The effect of treatment on decision-making index

	Decision Making(DM) Index
Panel A: Intent to Treat	
RMG	1.055***
	(0.315)
Dep. Var. Control Group Mean	.307
Adj. R-sq	0.181
P-val	.008
Panel B: Treatment on the Treated (2SLS)	
RMG	1.238***
	(0.368)
Dep. Var. Control Group Mean	.359
Adj. R-sq	0.178
P-val	.0008
Obs.	994
Baseline Char.	Yes
Baseline Dep. Var.	Yes

Robust standard errors in parentheses. We control for the following baseline characteristics: age, a binary indictor for married, an indicator for ever attending school, indicators for ethnicity being Lao-Tai, Mon-Khmer, Chinese-Tibet, Hmong-Emien or other, household size and average monthly household income. We also control for the baseline value of the outcome of interest when possible. Decision making index is a z-score index of whether the woman is a final decision maker, a final decision maker on decisions she cares about, a final decision maker on a decision she believes she should be the decider on and the fraction of total decisions in the above decisions she is the decision maker on and the number of decisions she cares about. * $p < 0.1$, ** $p < .05$, *** $p < 0.01$

Table 2.11: the effect of treatment on decision-making in the household (Disaggregated Measures)

	Is Final DM (A)	Frac. of Decisions is a DM (B)	Final DM on Decision She Cares About (C)	Frac of Decisions She Cares About is DM (D)	Believes She Should be DM (E)	Frac of Decisions Believes Should be DM (F)
			*Panel **A**: Intent to Treat*			
RMG	0.0923***	0.0466***	0.0897***	0.0406**	0.0667**	0.0517**
	(0.0274)	(0.0169)	(0.0292)	(0.0184)	(0.0277)	(0.0229)
Dep. Var. Control Group Mean	.681	.317	.624	.23	.724	.457
Adj. R-sq	0.139	0.149	0.133	0.0936	0.127	0.173
			Panel B: Treatment on the Treated(2SLS)			
RMG	0.108***	0.0544***	0.105***	0.0474**	0.0780**	0.0604**
	(0.0320)	(0.0197)	(0.0341)	(0.0214)	(0.0322)	(0.0266)
Dep. Var. Control Group Mean	.685	.32	.632	.233	.724	.456
Adj. R-sq	0.135	0.146	0.127	0.0908	0.127	0.173
Obs	1049	1049	1049	995	1049	1049
Baseline Char.	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Dep. Var.	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses. We control for the following baseline characteristics: age, a binary indictor for being married , an indicator for ever attending school, indicators for ethnicity being Lao-Tai, Mon-Khmer, Chinese-Tibet, Hmong-Emien or other, household size and monthly household income. We also control for the baseline value of the outcome of interest. Outcomes A,C and E are binary outcomes. We collected data on four decisions of the household; (1) decisions related to the woman visiting family or relatives, (2) how to spend income the woman earns, (3) how to spend income the husband earns and (4) large purchases. * $p < 0.1$, ** $p < .05$, *** $p < 0.01$

A = whether she is the final decision maker (DM) on at least one household decision

B = fraction of total decisions on which she is the final decision maker

C = whether she is the decision maker on at least one decision she cares about

D = fraction of decisions where she is the final decision maker for a decision she cares about

E = whether she believes she should be the final decision maker on at least one decision

F = fraction of total decisions on which she believes she should be the final decision maker

2.7.2 Voice in the Community

We find that RMG members report being more willing to engage in communal affairs and voice their opinion. Table 2.12 illustrates whether treatment affected a woman's voice in the community. We see that treated women are more likely to express their criticism with authority figures and advocate for themselves. RMG women feel 11.2% more comfortable than WL women with speaking up in public to ensure proper payment of wages for public works and similar programs. RMG women are also 8.5% more likely to be comfortable than WL members with speaking up in public to criticize the misbehavior of authorities or assigned officials. This may be a function of RMG women having more interactions with the village head and other authorities regarding work responsibilities, payment, and other issues related to the RMG roles. Women were also asked if they were willing to volunteer their opinions in village meetings on how to improve village infrastructure and we do not find a change in these dimensions.

Table 2.12: The effect of treatment on voice in the community

	Comfortable Raising Hand	Comfortable speaking up to ensure payment of wages	Comfortable Criticizing Authority
*Panel **A**: Intent to Treat*			
RMG	0.0459	0.112***	0.0846***
	(0.0327)	(0.0331)	(0.0313)
Dep. Var. Control Group Mean	.416	.367	.271
Adj. R-sq	0.0990	0.0946	0.0878
Panel B: Treatment on the Treated(2SLS)			
RMG	0.0538	0.131***	0.0991***
	(0.0382)	(0.0384)	(0.0364)
Dep. Var. Control Group Mean	.419	.363	.273
Adj. R-sq	0.0979	0.0987	0.0868
Obs	1020	1019	1018
Baseline Char.	Yes	Yes	Yes
Baseline Dep. Var.	Yes	Yes	Yes

Robust standard errors in parentheses. We control for the following baseline characteristics: age, a binary indictor for being married, an indicator for ever attending school, indicators for ethnicity being Lao-Tai, Mon-Khmer, Chinese-Tibet, Hmong-Emien or other ,household size and average monthly household income. We also control for the baseline value of the outcome of interest. All outcomes are binary, the outcome equals 1 if the respondent said they answered they "Yes with difficulty" or "Yes without difficulty."* $p < 0.1$, ** $p < .05$, *** $p < 0.01$

2.8 Conclusion

PWPs, like cash transfer programs, are an important tool in creating social protections and increasing the well-being of vulnerable populations that are typically poorer and have less access to the traditional labor market. Unlike cash-transfer programs, public work programs require participants to work, often in labor intensive jobs, to receive a transfer. In many ways, a PWP directly creates job opportunities. However, unlike traditional jobs, gaining employment through a PWP is dependent on economic status rather than relevant skills and qualifications. In many ways, PWPs can be an efficient way to both improve a participant's economic well-being and provide public works. However, for social programs that target women specifically, there is a concern that there may be unintended negative consequences, particularly on the incidence of GBV.

Both theoretical and empirical work have found mixed predictions and results on the relationship between female bargaining power and GBV. This paper exploits the randomized control trial design of the Lao PDR Road Management Group (RMG) program, to provide evidence on the causal relationship between women's employment opportunities and their exposure to GBV. To the best of our knowledge, we are the first to investigate the causal relationship of PWPs that target women, and GBV. PWPs offer jobs to their participants who traditionally lack wage opportunities and are therefore different from the traditional population that is studied when investigating the relationship between women's jobs and violence. We find that treatment increases women's employment in wage work and their household earnings. However, we do not find that participation in the program changes their exposure to GBV. Our results hold when we look at specific forms of IPV and domestic violence such as controlling behavior, emotional violence, physical violence or physical violence from other household

members. Despite finding no change to GBV in the household, we find that treatment improves a woman's well-being in the household as measured through decision-making and increases her willingness to speak publicly in the community on different issues.

Previous work has found that null average level effects may mask heterogenous effects by initial level of empowerment or bargaining power. Women with higher initial empowerment could see a reduction in GBV while less empowered women may see increases. Given that the target population of PWPs are more vulnerable and have few outside options, we might expect average level increases, but do not find evidence of this. Furthermore, we do not find an effect of treatment on the indices of GBV even when we look at heterogeneity by empowerment within our sample.

This is a particularly interesting result, since although increasing women's economic opportunities have shown to improve their human capital accumulation, reduce early marriage, delay fertility and generally increase female empowerment, the effect on GBV, and more specifically, intimate partner violence (IPV) is unclear. We provide evidence that PWPs do not seem to increase or decrease whether a woman experiences violence in her home but do seem to increase her well-being in terms of decision-making in the household and voice in the her community.

Chapter 3:

Expenditure and Investment Impacts of Public Work Programs: Evidence from a Road Maintenance Program in Laos PDR

3.1 Introduction

Public workfare programs (PWPs) generally serve the dual objective of providing employment and creating or maintaining infrastructure projects. They are an important part of the social protection toolkit in many low and middle-income countries around the world. They are also an integral part of the World Bank's own social protection portfolio– in the last ten years alone, the World Bank has supported over 80 such operations in 45 countries.[34]

Despite their importance, the evidence on PWPs is not conclusive, not least of which because the two existing Randomized Control Trials (RCTs) on PWPs find weak support for their effectiveness. At the same time, non-experimental studies suggest that PWPs have been effective in achieving a number of development objectives, including reducing poverty and generating employment. We propose to fill the evidence gap by carrying out a rigorous evaluation of a well targeted public workfare program in Laos that is being implemented by the World Bank supported Lao PDR Poverty Reduction Fund Program (PRF), using an RCT design. We find strong gains in employment and poverty reduction, through greater income, during the period of the program. However, we do not find changes in investments or savings that would imply long term poverty reduction.

We know of two Randomized Controlled Trials of public works programs. The first is the Beegle et al. (2017) study of the Malawi Social Action Fund. Beegle and her co-authors find

[34] www.worldbank.org/safetynets/publicworks

limited impacts of the program on food security and in fact, some evidence of negative spillover effects on non-participants. Varying the timing of the program or frequency of payments seems to have little impact and though the program is much smaller in scale than PNSP or NREGS, the absence of effects does not appear to be driven by low statistical power. The second is the Bertrand et al. (2017) study of a public works program supported by the World Bank in Côte d'Ivoire that offered temporary jobs for 7 months to youth to rehabilitate and clean road infrastructure at minimum wage. The program appears to have had no impact on the levels of employment but did impact earnings during and one year after the program. The magnitude of contemporaneous earnings gains is about 40% of the transfer. The authors argue that one cause for the poor performance of the program is the fact that it relied on self-targeting. In the case of this proposed study, we believe that the program we are studying is stronger on two points of weakness in the Malawi Social Action Fund discussed by Beegle et al. (2017) – targeting and transfer value. While the payments in our study are similarly about 25-50% of monthly food consumption expenditure, the program is nearly three times as long - these wages are offered for 75 days on average, compared to 24 days in Malawi. We also believe that the intervention in our study is better targeted than either the intervention in Malawi or the one in Côte d'Ivoire in Bertrand et al (2017). [35]

This paper focuses on the impacts of the Road Maintenance Group (RMG) Program implemented in rural Laos. The RMG program rigorously targets women from poor households to complete routine road maintenance on feeder roads in remote and rural areas. The RMG program offered a stable source of wage income in areas with a lack of wage opportunities for

[35] Dervisevic et al (2020) finds that the targeting system used in the intervention was successful in identifying poor households in these villages.

both men and women. Interested and eligible women were selected for the program based on lucky draw and the women not selected were on put on a wait-list and made up the control group. We evaluate whether the program was successful in employing women and increasing income. We additionally evaluate whether the program had secondary effects on expenditure and investments, that may speak to whether the program can affect long term income.

Our evaluation finds that the RMG program had strong impacts on income. The intervention sustainability increased women's monthly income from wages and their probability of consistently earning a salary. We also find large gains in terms of income on the household level, with increases to household income by 43-48% relative to average income of control households, and income diversification. Our effects are relatively higher than found in the literature. In Peru, net wage gains from the Trabajar Urbano y Rural program are estimated at 24 percent of the program wage amount (Chacaltana, 2003). In Colombia's Empleo en Acción (Employment in Action), the corresponding figure is close to 39 percent (Departamento Nacional de Planeación, 2004) In the Jefes program in Argentina, the figure is approximately 33 percent (Ravallion and Galasso, 2004). We see substantially larger relative increases on household income from wages of 113%- 125%, much closer to the effect seen of a PWP in Liberia of nearly 93% (Backiny-Yetna, Wodon, and Zampaglione, 2011). The authors of the Liberia study attribute the high impacts observed in that program to the fact that participants had few alternate employment opportunities, which is true for our context as well.

Despite these large increases in the household income, this study, unlike other studies that have found a positive effect on financial safety or investments through increases in savings and acquisition of productive assets (Ravi and Engler, 2015; and Liu, 2013; Berhane et al. 2014; Rosas and Sabarwal, 2016), does not find any effects on investment and savings behavior.

Evidence of increased savings and investments could indicate longer term benefits of the program. We cannot say whether the RMG program will have an effect on income beyond the program period. This may suggest that much of the income from the intervention was used for consumption.

We find some evidence that relatively less poor households reap larger benefits from the intervention. However, we find the gains among household that were below median income at baseline to be smaller than of households at median income or above median income at baseline. Relatively wealthier households saw larger increases in the household income, despite similar treatment effect sizes on being regular earners and women's income from wage labor. We also find that relatively wealthier households increase expenditure on children's education while there is no effect on average.

The remainder of this paper is organized as follows. In section 3.2 briefly reviews the program and estimation strategy. In section 3.3, we present and discuss results. Section 3.4 presents and discusses heterogeneity in outcomes by relative poverty at baseline. Section 3.5 concludes.

3.2 RMG Program and Methodology

We evaluate the same intervention as in chapter 2. The RMG program employed women to conduct routine maintenance on ancillary roads in rural Laos. The program specifically targeted women from the poorest households within these villages. The number of eligible and interested women exceeded the number of positions available so participants were randomly assigned women through a lucky draw lottery. The women that drew a ball numbered 1 to n, where n is the number of positions available in a given group, were offered a road maintenance

job. The women that drew a ball n+1 and higher were place on the waitlist and act as our control group.

We estimate ITT by regressing various outcomes of interest according to the following ANCOVA formula (McKenzie, 2012) whenever possible to include baseline measures of the outcomes.

$$y_{i1} = \alpha + \beta Treat_{i0} + \gamma X_{i0} + \theta y_{i0} + \epsilon_i$$

3.3 Results and Discussion

For the following results we present that the ITT estimations and the ToT estimations. While attrition was not differential by treatment status, we do find that attrition differed by other characteristics, so we control for a set of baseline characteristics and estimate the effects based on original treatment status. [36] The ITT estimations will likely underestimate the effects of treatment since it does not account for compliance; for this reason we also present the ToT estimations. (Angrist, Imbens and Rubin 1996) In fact, we do find effect sizes of larger magnitudes among compliers than in the ITT estimations.

3.3.1 Employment and Income

The RMG program was meant to directly change labor market activities by giving participants a regularly paying job for 18 months. To see if the program worked as intended, we estimate the direct effect of treatment on whether the women were listed among the household members that regularly contributed wage income, and average monthly wage income contributed over the 12 months preceding the survey. We winsorized average monthly earnings at the 99th

[36] See table 2.2.

percentile to minimize the effects of outliers. We discuss the results presented in chapter 2 to contextualize the first order effects of the intervention.

Table 2.3 shows the results of treatment on employment and income of RMG women and waitlisted women from the ITT estimation. Our outcomes of interest are whether the woman participated in the labor force in the reference week, was employed in paid work in the reference week, regularly contributed to household wage income in the preceding 12 months and whether she took on extra wage work in the past 12 months. We do not find evidence that treatment increased women's labor force participation or likelihood of being employed in paid work in the reference week. However, the effect of treatment on these outcomes may have been absorbed during the baseline because the baseline survey was conducted after the start of the RMG program but before any payments were made. We run an additional estimation excluding the baseline values for LFP and employment in paid work and we find that treatment increases the likelihood of working in paid work by 15%. Treatment increases the likelihood that a woman was a regular earner for the household in the preceding 12 months. Five percent of the control group women were regular earners for their households, and treatment increased the probability that a woman was regularly earning a salary by 77.4%. We do not find an effect of treatment on whether a woman intermittently took on extra work for wages in the past 12 months in ITT estimations but the ToT estimation shows that compliers were less likely to take on extra work in the past year.

Table 2.4 presents the effects of treatment on a woman's average monthly salary from the preceding 12 months. The RMG program increases treated women's monthly wage income from a job they performed regularly by LAK 175,598.30. This is approximately equal to 3 days of work under the RMG program. There is a similarly sized effect on total monthly income. There

is no evidence that the RMG program affected income from intermittent work. We find that the RMG program was effective in its goals to employ women and provide them with a stable source of supplementary work and income. It should be noted that these effects only speak to income from a salary or wages for work outside the home. Rural, remote Laos PDR has very few wage opportunities in general and this context is important to remember when comparing the effect sizes to the control group mean. We argue that these effects should be compared to household income as it accounts for income from farming, forestry and non-farm enterprises. However, since only wage income is disaggregated to the individual level we cannot construct total income for each individual.

3.3.2 Household Income and Household Income Diversification

The RMG program allowed for flexibility in who completed the road maintenance work. It is possible that the program had larger effects on household income coming from other household members working for the registered woman in her absence. [37] Having a household member in the RMG program may have also induced the household to change the labor and time allocation of other household members either from income generating work to unpaid work or between different types of income generating work.

Table 3.1 shows the ITT and ToT estimations of the effect of treatment on (1) average monthly household income in the past 12 months, (2) average monthly household income for farming and forestry, (3) average monthly income from non-farm enterprises and (4) average monthly household income from wages. The outcome variables were all winsorized at the 99th percentile to minimize the effects of outliers.

[37] 68.71% of women in the treatment group had another household member complete the RMG work in their place.

We find that being selected for the RMG program increases household income by LAK 211,302.60 and household wage income by LAK 188,721.5. The magnitude of the effects are similar to the effect on women's average monthly income over the past 12 months. This suggests that the increase in household wage income and a large portion of the increase in household income come directly from the registered woman doing road maintenance work.

Treatment decreases household income from non-farm enterprises by an effect size of LAK 9,476.20. This effect is statistically significant at the 10% level. The reduction in income is relatively small in magnitude; it is approximately $1/6^{th}$ of daily RMG rate, and there is a positive but insignificant effect on farm income. The RMG work is meant to be a supplementary source of income by design, so this could suggest that when constrained, RMG women are making a trade-off with working in the household business.

Table 3.1: The effect of treatment on household income in past 12 months.

	HH Income	HH Farm & Forestry Income	HH Non Farm Enterprise Income	HH Wage Income
		Panel A: Intent to Treat		
RMG	211302.6***	16102.6	-9467.2*	188721.5***
	(42089.3)	(24621.5)	(5045.9)	(27782.8)
Dep. Var. Control Group Mean	489609.915	215258.941	44546.499	166407.968
Adj. R-sq	0.333	0.194	0.447	0.392
P-val	6.03e-07	.51	.06	1.81e-11
		Panel B: Treatment on the Treated(2SLS)		
RMG	247109.5***	18328.1	-10864.2*	221934.7***
	(50386.0)	(29144.6)	(6017.4)	(33469.8)
Dep. Var. Control Group Mean	505175.337	218686.207	44624.111	176662.444
Adj. R-sq	0.332	0.185	0.467	0.406
P-val	9.37e-07	.53	.07	3.34e-11
Obs	1099	1099	1099	1099
Baseline Char.	Yes	Yes	Yes	Yes
Baseline Dep. Var.	Yes	Yes	Yes	Yes

Robust standard errors in parentheses. . Outcomes are winsorized at the 99th percentile to minimize the effects of outliers. We control for the following baseline characteristics: age, binary indictor for being married, an indicator for ever attending school, indicators for ethnicity being Lao-Tai, Mon-Khmer, Chinese-Tibet, Hmong-Emien or other, and household size. We also control for the baseline value of the outcome of interest. We do not control for average household income at baseline to avoid collinearity with the baseline outcome. All income variables are average monthly values for income from the past 12 months. HH Income is the sum of income from farming and forestry, non-farm enterprises and wages (aggregated for all household members). * $p < 0.1$, ** $p < .05$, *** $p < 0.01$

Table 3.2: The effect on household income diversification in the past 12 months.

	Sources of Income	Non-Farm Income	Frac of total income not from farming
	*Panel **A**: Intent to Treat*		
RMG	0.882***	2244035.3***	0.0654***
	(0.116)	(471768.5)	(0.0150)
Dep. Var. Control Group Mean	3.135	4971356.041	.819
Adj. R-sq	0.163	0.381	0.273
P-val	6.84e-14	2.23e-06	.000001
	Panel B: Treatment on the Treated(2SLS)		
RMG	1.014***	2611154.9***	0.0782***
	(0.139)	(565329.0)	(0.0181)
Dep. Var. Control Group Mean	3.147	5192445.997	.822
Adj. R-sq	0.156	0.391	0.264
P-val	2.76e-13	3.86e-6	.00002
Obs	1099	1099	980
Baseline Char.	Yes	Yes	Yes
Baseline Dep. Var.	Yes	Yes	Yes

Robust standard errors in parentheses. We control for the following baseline characteristics: age, a binary indictor for married, an indicator for ever attending school, indicators for ethnicity being Lao-Tai, Mon-Khmer, Chinese-Tibet, Hmong-Emien or other, household size and average monthly household income. We also control for the baseline value of the outcome of interest. Sources of incomes are the sum of all household member that have a wage job, all harvest crop varieties, forestry goods, aquaculture goods and non-farm enterprises. Non- farm income excludes income from rice and vegetable crop harvest and sales. * $p < 0.1$, ** $p < .05$, *** $p < 0.01$

The RMG program also diversified income sources for the households of treated women. Treatment increased the sources of household income by nearly one source, with the point estimate being 0.882. There was also an increase in the amount of income that did not come from sale of harvest goods and the share of income from non-farming activities.

3.3.3 Household Investments

The effect of the program on women's and their households' incomes roughly equate to the pay of 3 days of RMG work but is nearly equivalent to 35% of the average monthly income of a household of a waitlisted woman. We explore if households use the additional income from the RMG program to invest it into the household farm, non-farm enterprises or make human capital investments into children through education expenditure. If treated households increase investments and savings behavior this may imply that the RMG program could benefit the households beyond the program period. Households could potentially invest in the farm or business to make it more profitable in the future, increase savings to smooth through future shocks or increase human capital investments in children.

Table 3.3 analyzes whether treatment induced households to change farming behavior or make investments on the household farm. We estimate whether treatment affects (1) whether the household invests in the farm, (2) amount invested in the farm, (3) whether the household grows a cash crop, (4) the size of farm land the household owns and (5) an index that measures livestock ownership. Outcomes 1 and 2 were not measured at baseline so we are not able to run an ANCOVA specification using them. The livestock index is constructed using a principal components method that creates an orthogonal transform of the correlated measures. We use only the first principal component as it has the largest variance and accounts for the greatest

degree of variation in underlying components. We do not find evidence that suggests that treatment affects agricultural investments.

We explore whether treatment changes investment behavior related to non-farm enterprises. Table 3.4 shows the estimation results on the effect of being selected in the RMG program on (1) the total number of non-farm businesses the household owns (2) whether the household has opened a new business since baseline (3) whether the household invested in their non-farm business (4) the amount invested in a household non-farm business, winsorized at 99th percentile (5) the number of non-residential buildings a household owns and (6) a binary indicator for whether the household saved for a business. It is relatively rare for households to have a non-farm enterprise. Of the entire sample, only 83 households own a business at endline. However, there is evidence that treatment decreases the number of businesses a household owns by a relatively small magnitude of 0.035. Rural non-farm enterprises are typically not profitable and usually owned by the poorest of households that lack access to other revenue streams (Nagler and Naude 2017).[38] While the evidence is modest, it could suggest that treatment decreases poor households' reliance on their non-farm businesses.

Finally, we explore whether treatment impacts human capital investment in children. Table 3.5 shows the ITT and ToT estimations on the effect of treatment on per-child education expenditure and the per-child education expenditure for children under the age of 12. We look at the outcome for children under the age of 12 because there may be more flexibility in their education decisions; older children may have already completed schooling or dropped out due to

[38] In our sample, non-farm income accounts for approximately 10% of total household income, suggesting that these enterprises are not the main source of income.

financial constraints, jobs or marriage. We do not find evidence that treatment affects human capital investments, measured through education expenditures, on children.

Table 3.3: Agricultural Investment in past 12 months.

	Invested in HH Farm	Amt Invested on Farm (W99%)	Grows Cash Crop	Ag Land (Sq Meters)	Livestock Index
			*Panel **A**: Intent to Treat*		
RMG	0.00385	17240.6	0.009*	-861.4	0.00951
	(0.0328)	(71837.2)	(0.005)	(1912.3)	(0.0498)
Dep. Var. Control Group Mean	.417	209169.303	.981	22037.096	-.188
Adj. R-sq	0.0188	0.0583	0.105	0.0623	0.437
P-val	.91	.81	.07	.97	.84
			Panel B: Treatment on the Treated(2SLS)		
RMG	-0.0146	24940.4	0.0112*	206.3	0.0128
	(0.0388)	(86146.2)	(0.00619)	(2107.7)	(0.0603)
Dep. Var. Control Group Mean	.421	199070.556	.983	18958.563	-.189
Adj. R-sq	0.0220	0.0700	0.141	0.0632	0.447
P-val	.71	.77	.07	.92	.83
Obs.	1084	1084	1099	1099	1099
Baseline Char.	Yes	Yes	Yes	Yes	Yes
Baseline Dep. Var.	No	No	Yes	Yes	Yes

Robust standard errors in parentheses. We control for the following baseline characteristics: age, a binary indictor for married, an indicator for ever attending school, indicators for ethnicity being Lao-Tai, Mon-Khmer, Chinese-Tibet, Hmong-Emien or other, household size and average monthly household income. We also control for the baseline value of the outcome of interest when possible. Livestock index is constructed using a principal components method of the number of cows, buffaloes, goats, pigs, chicken and ducks.* $p < 0.1$, ** $p < .05$, *** $p < 0.01$

Table 3.4: Non-agricultural Enterprise Investments in past 12 months.

	Total # of Businesses	New Business	Invested in HH Business	Amt Invested in Business (W99%)	# of Non-Residential Buildings	Saved for Business
			*Panel **A**: Intent to Treat*			
RMG	-0.0301*	-0.0211	-0.170	-367879.9	0.00177	-0.00146
	(0.0182)	(0.0159)	(0.106)	(278731.5)	(0.00947)	(0.0104)
Dep. Var. Control Group Mean	.097	.084	.277	568984.615	.019	.027
Adj. R-sq	0.0440	0.0392	0.125	0.118	0.0454	0.0172
P-val	.0993	.18	.11	.19	.85	.89
			Panel B: Treatment on the Treated(2SLS)			
RMG	-0.0396*	-0.0303*	-0.216*	-476540.4	0.00187	0.00105
	(0.0212)	(0.0184)	(0.131)	(347857.5)	(0.0109)	(0.0124)
Dep. Var. Control Group Mean	.097	.085	.281	577875	.02	.025
Adj. R-sq	0.0389	0.0338	0.113	0.126	0.0437	0.0160
P-val	.07	.0995	.098	.17	.86	.93
Obs	1099	1099	83	83	1099	1099
Baseline Char.	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Dep. Var.	No	No	No	No	Yes	Yes

Robust standard errors in parentheses. We control for the following baseline characteristics: age, a binary indictor for married, an indicator for ever attending school, indicators for ethnicity being Lao-Tai, Mon-Khmer, Chinese-Tibet, Hmong-Emien or other, household size and average monthly household income. We also control for the baseline value of the outcome of interest when possible. Invested in HH Business, New Business and Saved for Business are binary outcomes.

Table 3.5: Expenditure on education for children

	Per-child Expenditure on Edu	Per-child (under 12) Expenditure on Edu
*Panel **A**: Intent to Treat*		
RMG	28601.5	35722.9
	(23624.6)	(41156.1)
Dep. Var. Control Group Mean	251988.11	368277.293
Adj. R-sq	0.268	0.325
P-val	.22	.38
Panel B: Treatment on the Treated(2SLS)		
RMG	40926.6	41894.5
	(27398.1)	(47114.7)
Dep. Var. Control Group Mean	253744.555	376845.086
Adj. R-sq	0.275	0.323
P-val	.13	.37
Obs		
Baseline Char.	Yes	Yes
Baseline Dep. Var.	Yes	Yes

Robust standard errors in parentheses. We control for the following baseline characteristics: age, a binary indictor for married, an indicator for ever attending school, indicators for ethnicity being Lao-Tai, Mon-Khmer, Chinese-Tibet, Hmong-Emien or other, household size and average monthly household income. We also control for the baseline value of the outcome of interest when possible. The education module was limited to household children between the age of 5-24. Outcomes are winsorized at the 99^{th} percentile. * $p < 0.1$, ** $p < .05$, *** $p < 0.01$

3.3.4 Savings and Financial Behavior

Table 3.6 presents the results of the estimation on whether treatment affected savings and other financial behavior. We look at the following outcomes; (1) a binary indicator for whether any household member saves, (2) a binary indicator for whether any household member has a formal savings account, (3) a binary indicator for whether any household member has a bank account and (4) the total number of bank accounts a household has. We do not find evidence that treatment effects savings behavior. Increased savings may indicate that the program would have longer impacts on household income; we cannot conclude whether the effect on income will persist beyond the program period.

Table 3.6: Household savings behavior

	Any HH Mem. Saves	Any HH Mem. has Formal Savings Account	Any HH Mem. Has Bank Account	Total # of Bank Accounts
*Panel **A**: Intent to Treat*				
RMG	0.0356	0.00765	0.0129	0.0109
	(0.0296)	(0.0129)	(0.0102)	(0.0106)
Dep. Var. Control Group Mean	.271	.04	.026	.03
Adj. R-sq	0.0496	0.0439	0.179	0.175
P-val	.22	.55	.22	.3
Panel B: Treatment on the Treated(2SLS)				
RMG	0.0332	0.00408	0.0188	0.0166
	(0.0351)	(0.0146)	(0.0121)	(0.0125)
Dep. Var. Control Group Mean	.279	.037	.028	.031
Adj. R-sq	0.0497	0.0476	0.183	0.162
P-val	.34	.78	.12	.18
Obs	1099	1099	1099	1099
Baseline Char.	Yes	Yes	Yes	Yes
Baseline Dep. Var.	Yes	Yes	Yes	Yes

Robust standard errors in parentheses. We control for the following baseline characteristics: age, a binary indictor for married, an indicator for ever attending school, indicators for ethnicity being Lao-Tai, Mon-Khmer, Chinese-Tibet, Hmong-Emien or other, household size and average monthly household income. We also control for the baseline value of the outcome of interest. Outcomes 1-3 are binary outcomes. * $p < 0.1$, ** $p < .05$, *** $p < 0.01$

3.4 Heterogeneous Effects by Relative Poverty

The RMG program employed the poorest households of a village. However, even within this sample there are varying levels of poverty and as such, these households may face different constraints or even poverty traps that limit their ability to benefit from the program. Classical models of growth show that all households, no matter their initial level of wealth, eventually converge to some stable level. However, significant poverty traps may exist that limit the effectiveness of interventions. Even in our sample, which has a relatively poor population, there

could be significantly different income generating technologies that the households use pre-intervention. Poorer households are typically more diversified in their income sources to protect themselves from uncertainty. However, their risk aversion can lead them to lose out on gains from specializing in activities with higher returns, where they may have a comparative advantage (Taylor and Lybbert 2015).

3.4.1 Employment and Income

We estimate the ITT for households above the median level of household income at baseline and for households below the median level of household income at baseline separately. Table 3.7 shows the effect on women's labor force participation measured through their LFP in the reference week, whether they were employed in paid work in the reference week, whether they were a regular earner for the household in the past two months and, whether they took on extra work over the past 12 months. We do not find differential effects on employment outcomes. The ITT estimates show that RMG women from household with above median income were 78.1PPs more likely to be regular earners and women from relatively poorer households were 76.6PPs more likely. Treatment did not affect other measures of employment and labor participation.

Table 3.7: Heterogeneous effects on LFP and employment by baseline HH income above and below 50th percentile

	LFP	Employed in Paid Work	Regular Earner	Extra Work
Panel A: ITT estimates for Below Median Income				
RMG	0.0144	0.0796	0.781***	-0.0299
	(0.0463)	(0.166)	(0.0309)	(0.0224)
Dep. Var. Control Group Mean	.314	.33	.042	.079
Adj. R-sq	0.0314	0.132	0.664	0.0178
Obs	523	57	546	546
P-val	.75	.63	2.706e-93	.18
Panel B: ITT estimates for Median or Above Median Income				
RMG	0.0422	0.202	0.766***	-0.0256
	(0.0467)	(0.150)	(0.0347)	(0.0254)
Dep. Var. Control Group Mean	.333	.341	.058	.096
Adj. R-sq	0.0319	0.276	0.619	0.0247
Obs	538	67	553	553
P-val	.37	.18	1.185e-77	.31
H_0 *Treatment effects equal*				
P-val	.669	.543	.758	.899

Robust standard errors in parentheses. LFP is a binary indicator if the RMG eligible woman is participating in the labor force. LFP=1 if the woman is currently employed in reference week or attached to job she was absent from in the reference week or if she is unemployed, defined as being without a job and seeking work in the reference week or did not seek work but was available to work. Employed in Paid Work is a binary indicator for whether she was engaged in paid labor in the reference week. Regular earner is a binary outcome that is equal to 1 if the woman was listed as a household member that regularly earned a salary in past 12 months. Extra work is binary indicator equal to 1 if the RMG eligible woman was listed as a household member that took on extra work over the past 12 months that contributed to household wage income. We control for the following baseline characteristics: age, a binary indictor for married, an indicator for ever attending school, indicators for ethnicity being Lao-Tai, Mon-Khmer, Chinese-Tibet, Hmong-Emien or other, household size and average monthly household income. We also control for the baseline value of the outcome of interest. * $p < 0.1$, ** $p < .05$, *** $p < 0.01$

Table 3.8 shows the results of the estimation on women's wage income. We find that women in households with median income or above see a statistically significant increase of LAK 186,414.9 while women from poorer households see a statistically significant increase of LAK 163,790.40. Women from poorer households see smaller relative gains, despite becoming a

regular earner for the household, similarly to women from relatively wealthier households. This may signal that poorer households do not reallocate women's labor hours as well as wealthier households. There is some suggestive evidence that the women from poorer houses do not see as large of a reduction in income from extra wage work, this could signal a preference to diversify sources of wage income.

Table 3.8: Heterogeneous effects on women's income from wages by baseline HH income above and below 50th percentile

	Average Monthly Earnings	Average Monthly Earnings from Extra Work	Total Monthly Earnings
Panel A: ITT estimates for Below Median Income			
RMG	163790.4***	-248.0	164907.5***
	(8300.4)	(1298.7)	(8391.4)
Dep. Var. Control Group Mean	10218.997	2660.51	13198.329
Adj. R-sq	0.511	0.0128	0.491
Obs	553	553	553
P-val	1.663e-65	.85	7.929e-47
Panel B: ITT estimates for Median or Above Median Income			
RMG	186414.9***	-1102.1	182264.1***
	(10917.3)	(1700.0)	(11489.9)
Dep. Var. Control Group Mean	17387.699	5120.697	27283.795
Adj. R-sq	0.417	0.0280	0.363
Obs	553	553	553
P-val	1.262e-52	.52	4.190e-65
Bonferroni ($\alpha = .05$)	.0167	.0167	.0167
H_0 Treatment effects equal			
P-val	.096	.687	.218

Robust standard errors in parentheses. Outcomes are winsorized at the 99th percentile to minimize the effects of outliers We control for the following baseline characteristics: age, a binary indictor for married, an indicator for ever attending school, indicators for ethnicity being Lao-Tai, Mon-Khmer, Chinese-Tibet, Hmong-Emien or other, household size and average monthly household income. We also control for the baseline value of the outcome of interest. * $p < 0.1$, ** $p < .05$, *** $p < 0.01$

3.4.2 Household Income and Household Income Diversification

Table 3.9 shows the differential effects on household income. The effect of treatment on household income is relatively smaller for poor households by LAK 164,439.1. We also find that the estimates are statistically different from one another at the 5% level. We also find that treatment reduces farm and forest income for poorer households but positively affects farm and forestry income for less poor households by negative LAK 35,911.90 and LAK 75,836.50 respectively. These treatment effects for household that had income below the 50^{th} percentile at baseline is not statistically significant while the effect on those above the 50^{th} percentile is significant at the 10% level. We can reject the hypothesis that these effects are equivalent.

We find that the magnitude of the effect on wage income to also be lower for poorer households, and effects are not statistically different from one another. While poorer households and relatively less poor households both see increases in wage income, our results may imply that poorer households may also see a reduction in farm income and a smaller overall increase in total income. We also find that sources of income increase for both groups by relatively the same size. However, the poorer households see a smaller increase in income not derived from farming suggesting that despite the intervention they are still heavily relying on farming to generate income. These results can be found in table 3.10.

Table 3.9: Heterogeneous effects on household income by baseline HH income above and below 50th percentile

	HH Income	HH Farm & Forestry Income	HH Non Farm Enterprise Income	HH Wage Income
Panel A: ITT estimates for Below Median Income				
RMG	125881.1***	-35911.9	-7068.8	159909.1***
	(40208.5)	(29363.7)	(4915.9)	(17762.5)
Dep. Var. Control Group Mean	244434.148	125874.178	17766.711	70573.439
Adj. R-sq	0.0688	0.0751	0.133	0.120
Obs	546	546	546	546
P-val	.002	.22	.17	1.84e-17
Panel B: ITT estimates for Median or Above Median Income				
RMG	307048.3***	75836.5*	-9557.5	233462.2***
	(75576.2)	(40953.8)	(8943.8)	(53383.3)
Dep. Var. Control Group Mean	723669.4	300590.995	70112.091	257897.355
Adj. R-sq	0.320	0.220	0.485	0.409
Obs	550	553	553	553
P-val	.00005	.098	.23	.0003
H_0 *Treatment effects equal*				
P-val	.033	.025	.805	.187

Robust standard errors in parentheses. We control for the following baseline characteristics: age, a binary indictor for married, an indicator for ever attending school, indicators for ethnicity being Lao-Tai, Mon-Khmer, Chinese-Tibet, Hmong-Emien or other, and household size. We also control for the baseline value of the outcome of interest. All income variables are average monthly values for income from the past 12 months and winsorized at the 99th percentile. * $p < 0.1$, ** $p < .05$, *** $p < 0.01$

Table 3.10: Heterogeneous effects on household income diversification by baseline HH income above and below 50th percentile

	Sources of Income	Non-Farm Income	Frac of total income not from farming
Panel A: ITT estimates for Below Median Income			
RMG	0.826***	1297571.0***	0.0475**
	(0.145)	(425482.0)	(0.0207)
Dep. Var. Control Group Mean	2.689	2527105.9	.878
Adj. R-sq	0.145	0.0778	0.207
Obs	546	546	464
P-val			
Panel B: ITT estimates for Median or Above Median Income			
RMG	1.026***	3327112.4***	0.0837***
	(0.176)	(855768.8)	(0.0223)
Dep. Var. Control Group Mean	3.562	7304783.758	.772
Adj. R-sq	0.154	0.392	0.290
Obs	553	550	516
H_0 *Treatment effects equal*			
P-val	.374	.032	.228

Robust standard errors in parentheses. We control for the following baseline characteristics: age, a binary indictor for married, an indicator for ever attending school, indicators for ethnicity being Lao-Tai, Mon-Khmer, Chinese-Tibet, Hmong-Emien or other, household size and average monthly household income. We also control for the baseline value of the outcome of interest. Sources of incomes are the sum of all household member that have a wage job, all harvest crop varieties, forestry goods, aquaculture goods and non-farm enterprises. Non- farm income excludes income from rice and vegetable crop harvest and sales. * $p < 0.1$, ** $p < .05$, *** $p <$ 0.

3.4.3 Human Capital Investment

Table 3.11 shows the ITT estimation education expenditures on children for women in household with lower than median income vs higher than median income. Unlike the results from the full sample we do find evidence that treatment increases human capital investment. However, the effect is concentrated among less poor households. Per-child education expenditures decrease by LAK 17,281.8 and per-child expenditures for children under the age 12 by LAK 51,947. However, these effects are not statistically significant. Therefore, treated poorer households do not differentially spend on education as compared to poorer households in the control group. But the ITT estimates show that treatment increases per-child education expenditure for children by LAK 114,556.90 and children under the age of 12 by LAK 80,981.9. These effects are statistically significant. These effects are statistically different across income groups with a p-value of 0.03.

Table 3.11: Heterogeneous effects on expenditure on education for children by baseline HH income above and below 50th percentile

	Per-child Expenditure on Edu	Per-child (under 12) Expenditure on Edu
Panel A: ITT estimates for Below Median Income		
RMG	-17281.8	-51947.0
	(23553.4)	(43928.3)
Dep. Var. Control Group Mean	195135.589	286441.328
Adj. R-sq	0.334	0.304
Obs	385	280
P-val	.46	.23
Panel B: ITT estimates for Median or Above Median Income		
RMG	80981.9**	114556.9*
	(39726.3)	(69026.8)
Dep. Var. Control Group Mean	305732.937	452777.685
Adj. R-sq	0.260	0.314
Obs	395	269
P-val	.04	.098
H_0 *Treatment effects equal*		
P-val	.031	.038

Robust standard errors in parentheses. We control for the following baseline characteristics: age, a binary indictor for married, an indicator for ever attending school, indicators for ethnicity being Lao-Tai, Mon-Khmer, Chinese-Tibet, Hmong-Emien or other, household size and average monthly household income. We also control for the baseline value of the outcome of interest when possible. The education module was limited to household children between the age of 5-24. Outcomes are winsorized at the 99th percentile. * $p < 0.1$, ** $p < .05$, *** $p < 0.01$

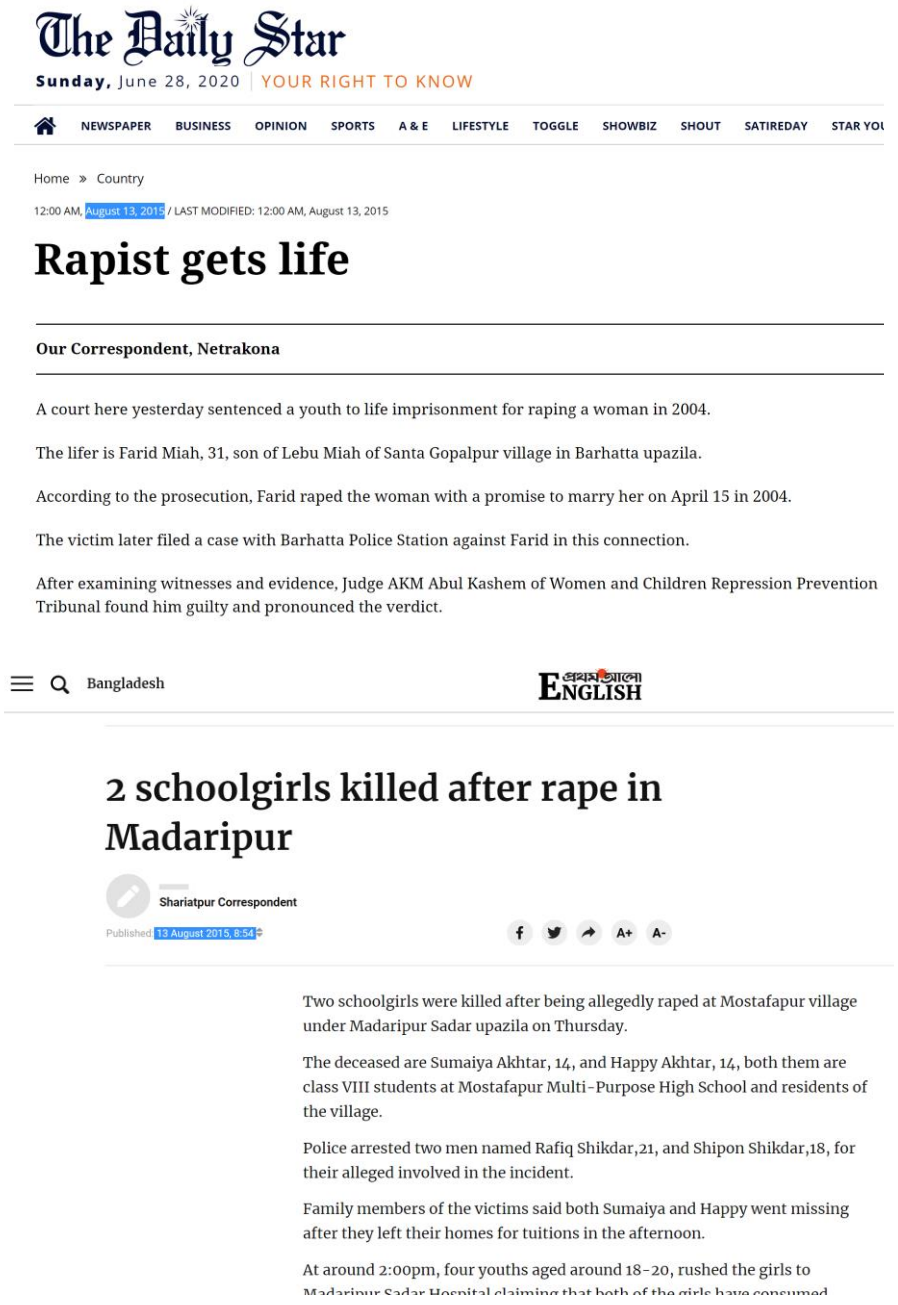
3.5 Conclusion

The current literature on experimental and non-experimental evaluations of PWPs are relatively inconclusive about their success. What is known, however, is that the effectiveness of such programs seem to be highly dependent on successful targeting. Our study evaluates the effects of a PWP that used rigorous targeting methods to identify eligible participants. We find large effects on employment, wage income and household income from participation in the program. Our effects suggest a net gain of household income of 43-48% and a net gain in household income from wages to be 113-125%. Our effects are on the higher end of effects seen

in the PWP literature and they can most likely be attributed to successful targeting. But the wage effects are also buttressed by the fact there is an initial lack of economic opportunity in these villages.

Despite our targeting of the poorest households we do still find heterogeneity in gains by relative poverty. Women from households that were below the median level of income at baseline had a lower effect on their wage income. Their household saw a smaller increase in overall income and farm income. Relatively wealthier households increased expenditure on education for children while there was no effect on education expenditures on poorer households. PWPs may need to still consider that there may be substantial heterogeneity among poor households, in the constraints they face and their abilities to take advantage of interventions.

Appendix A:



The Daily Star

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NEWSPAPER BUSINESS OPINION SPORTS A & E LIFESTYLE TOGGLE SHOWBIZ SHOUT SATIREDAY STAR YOU

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12:00 AM, August 13, 2015 / LAST MODIFIED: 12:00 AM, August 13, 2015

Rapist gets life

Our Correspondent, Netrakona

A court here yesterday sentenced a youth to life imprisonment for raping a woman in 2004.

The lifer is Farid Miah, 31, son of Lebu Miah of Santa Gopalpur village in Barhatta upazila.

According to the prosecution, Farid raped the woman with a promise to marry her on April 15 in 2004.

The victim later filed a case with Barhatta Police Station against Farid in this connection.

After examining witnesses and evidence, Judge AKM Abul Kashem of Women and Children Repression Prevention Tribunal found him guilty and pronounced the verdict.

Bangladesh

প্রথম আলো ENGLISH

2 schoolgirls killed after rape in Madaripur

Shariatpur Correspondent

Published: 13 August 2015, 8:54

A+ A-

Two schoolgirls were killed after being allegedly raped at Mostafapur village under Madaripur Sadar upazila on Thursday.

The deceased are Sumaiya Akhtar, 14, and Happy Akhtar, 14, both them are class VIII students at Mostafapur Multi-Purpose High School and residents of the village.

Police arrested two men named Rafiq Shikdar,21, and Shipon Shikdar,18, for their alleged involved in the incident.

Family members of the victims said both Sumaiya and Happy went missing after they left their homes for tuitions in the afternoon.

At around 2:00pm, four youths aged around 18-20, rushed the girls to Madaripur Sadar Hospital claiming that both of the girls have consumed

Figure 1A.1: Example of scraped headlines on sexual assault. The Daily Star above, Prothom Alo below. Date August 13, 2015.

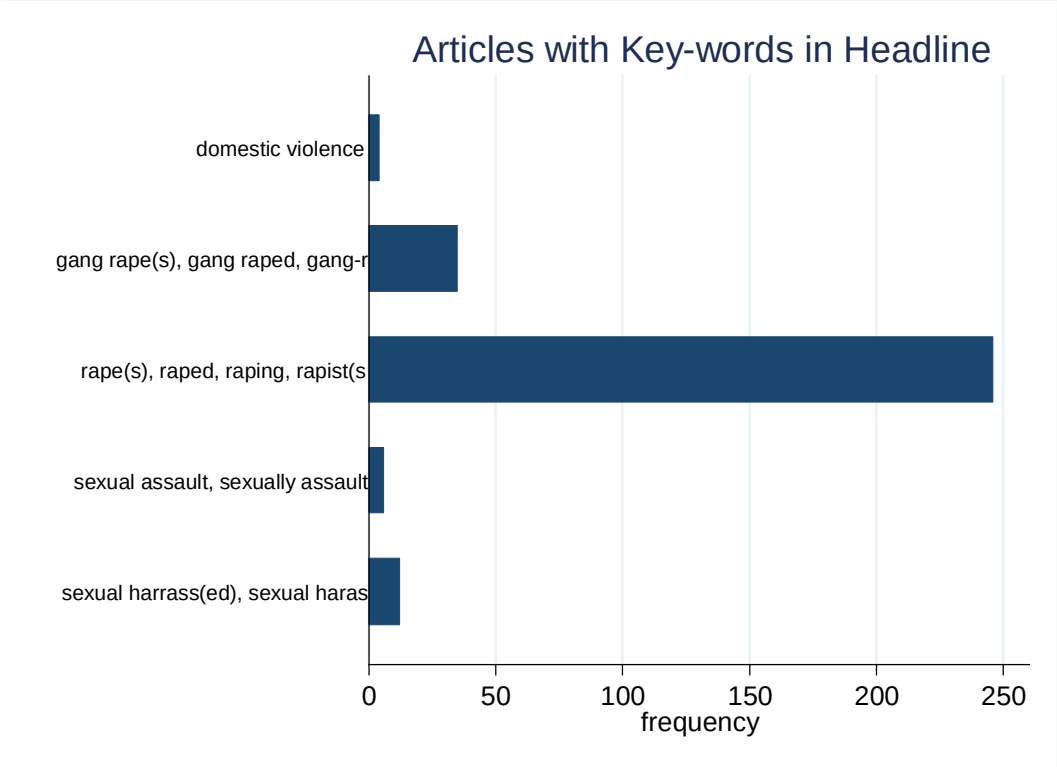

Figure 1A.2: Number of articles with key words in headline

Table 1A.1: Does day of week, month or public holidays predict sexual assault reporting?

	Both Papers	Headlines
Sunday	0.0438	-0.1172
	(0.0592)	(0.139)
Monday	-0.0136	0.0769
	(0.0592)	-0.182
Tuesday	-0.0423	0.346
	(0.0592)	-0.182
Wednesday	0.0181	0.119
	(0.0597)	-0.181
Thursday	0.0181	-0.0697
	(0.0597)	-0.181
Friday	-0.0392	0.232

	(0.0597)	-0.181
Saturday	0.0151	-4.31E-16
	(0.0592)	-0.182
January	0.0371	-0.2670
	(0.0472)	(0.1741)
February	0.0884*	0.00334
	(0.0456)	-0.236
March	0.0658	0.129
	(0.0471)	-0.232
April	-0.0233	0.618**
	(0.0465)	-0.234
May	0.00844	0.323
	(0.0472)	-0.232
June	-0.0520	0.285
	(0.0465)	-0.234
July	-0.0172	0.108
	(0.0479)	-0.23
August	-0.0776*	0.677**
	(0.0471)	-0.232
September	-0.0520	0.452
	(0.0465)	-0.234
October	0.00844	0.0968
	(0.0472)	-0.232
November	-0.0520	0.352
	(0.0465)	-0.234
December	0.0658	-0.0968
	(0.0471)	-0.232
Public Holiday	0.0262	-0.2496
	(0.0367)	(0.2246)
N	367	

Standard errors in parentheses
* $p < 0.1$, ** $p < .05$, *** $p < 0.01$

Table 1A.2: Pair-wise correlation of headlines appearing in both papers across lags

	Both Papers
Both Papers	1.00*
L1.Both Papers	0.12*
L2.Both Papers	0.12*
L3.Both Papers	.08
L4.Both Papers	-0.01
L5.Both Papers	0.04

* p<.05

Table 1A.3: Transition probabilities of headlines appearing in both papers from t-1 to t

	Both Papers	
L1.Both Papers	0	1
0	90.29	9.71
1	78.57	21.43

Table 1A.4: Summary statistics of sample characteristics by interview week

	Mean	SD	Min	Max
District				
Barisal	.0780626	.009428	.0400334	.1033333
Chittagong	.1675808	.0120973	.1381733	.1938731
Dhaka	.2589786	.0315503	.2164318	.5396163
Khulna	.1414213	.0138621	.0525438	.1682328
Rajshahi	.1464052	.0139401	.1178599	.1916849
Rangpur	.128234	.0217744	0	.1601768
Sylhet	.0793176	.0109093	0	.0991936
Female	.2954265	.0092911	.2806294	.3170128
Unmarried	.1644152	.0061517	.1415858	.1850962
Married	.7975932	.0071464	.7692308	.8187702
Muslim	.8831986	.0156766	.8500904	.9293333
Works	.8885784	.0161548	.8500146	.9096173
Outside Home				
Education				
None	.2910153	.014317	.2620192	.3290323
Primary	.2521903	.0098655	.2354145	.2932692
Secondary	.3104583	.0109955	.2902747	.3452877
Higher	.0752152	.0063039	.0483871	.086426
Secondary				
Tertiary	.0697139	.0067947	.0472637	.086072
Literate	.6799829	.0124195	.65	.7056224
BSIC Industry				
Agriculture	.3309214	.0232997	.2840572	.3760425
Retail	.1133436	.0081928	.098912	.1311073
Transportation	.0739831	.0053626	.0614293	.0959677
RMG Manu.	.0495034	.0090791	.0324755	.0833333
Education	.0406956	.0039269	.0306452	.0489784
Construction	.0385744	.0045978	.030103	.0487307
Dom. Services	.0195214	.0039332	.009182	.0291262
Other Per.	.0288029	.0036596	.0186567	.0369571
Services				

Appendix B:

Table 2A.1: Baseline Characteristics by Treatment Status for Full Sample

	RMG Women Mean	sd	Waitlist Women Mean	sd	Diff.	Obs.
Age	32.72	10.68	33.26	10.47	-0.538	1146
Married	0.89	0.31	0.87	0.34	0.021	1146
Education: None	0.45	0.50	0.41	0.49	0.038	1146
Education: Primary	0.44	0.50	0.48	0.50	-0.038	1146
Education: Lower Sec.	0.09	0.29	0.10	0.30	-0.009	1146
Education: Upper Sec +	0.02	0.14	0.01	0.11	0.009	1146
Lao-Tai	0.22	0.42	0.23	0.42	-0.003	1146
Mon-Khmer	0.62	0.49	0.66	0.47	-0.044	1146
Chinese-Tibet	0.06	0.23	0.04	0.21	0.013	1146
Hmoung-Emien	0.10	0.30	0.07	0.25	0.033*	1146
Ethnicity-Other	0.00	0.05	0.00	0.05	0.001	1146
HH Size	6.28	3.81	5.85	2.71	0.436**	1146
Monthly HH Inc	481891.64	762249.10	582842.25	873472.06	-100950.610*	1146

* p<0.1, ** p<.05, *** p<0.01

Table 2A.2: Attrition rate from randomization to endline

	Difference in Attrition Rate
Treatment(RMG=1)	0.255***
	(0.0583)
Age	3.671***
	(1.188)
Poverty Rank	
Poorest	0.00376
	(0.0427)
Poor	0.00160
	(0.0521)
Middle Income	-0.0587
	(0.0586)
Better off	0.0533*
	(0.0284)
N	1186

* $p < 0.1$, ** $p < .05$, *** $p < 0.01$

Table 2A.3: Standard errors and P-values with village level clustering

	ITT		TOT	
Dep Var	Clustered SE	P-value	Clustered SE	P-value
LFP	0.038	0.38852	0.046	0.291328
Employed in Paid Work	0.096	0.112377	0.096	0.077508
Regular Earner	0.028	0	0.030	0
Extra Work	0.016	0.123078	0.020	0.069478
Average Monthly Earnings	9340.229	3.30E-32	10090.490	0
Average Monthly Earnings from Extra Work	1076.046	0.664597	1276.982	0.50333
Total Monthly Earnings	9499.091	2.15E-31	10614.730	0
HH Income	44359.200	7.65E-06	53006.210	3.13E-06
HH Wage Income	29761.180	1.02E-08	35259.940	3.09E-10
GBV Index	0.276	0.555	0.316	0.549
IPV Index	0.220	0.368675	0.264	0.363905
Controlling Behavior	0.037	0.249316	0.044	0.270347
Emotional Violence	0.033	0.950981	0.040	0.951891
Physical Violence	0.032	0.709396	0.039	0.65969
Sexual Violence	0.027	0.185189	0.032	0.176013
Physical Violence from Non-Partner	0.023	0.548433	0.026	0.543363
Decision Making(DM) Index	0.335	0.002255	0.389	0.001474
Is Final DM (A)	0.030	0.003181	0.035	0.002245
Frac. of Decisions is a DM (B)	0.018	0.012181	0.021	0.009537
Final DM on Decision She Cares About (C)	0.029	0.002747	0.034	0.001854
Frac of Decisions She Cares About is DM (D)	0.018	0.024853	0.020	0.020678
Believes She Should be DM (E)	0.032	0.037825	0.037	0.033549
Frac of Decisions Believes Should be DM (F)	0.024	0.037044	0.028	0.033261
Comfortable Raising Hand	0.038	0.235733	0.045	0.226842
Comfortable speaking up to ensure payment of wages	0.035	0.002187	0.041	0.001342
Comfortable Criticizing Authority	0.034	0.013416	0.039	0.010468

Table 2A.4: Standard errors and P-values with village level clustering for heterogeneity results on GBV index

	Clustered SE	P-Value	Clustered SE	P-Value	Clustered SE	P-Value	Clustered SE	P-Value
				ITT				
RMG	0.335	0.509	0.415	0.394	0.384	0.559	0.302	0.586
RMG#Wife Edu Higher	0.715	0.221						
RMG#Formal Edu			0.472	0.467				
RMG#Above Median Inc					0.507	0.804		
RMG#Above Median DM							0.486	0.222
				TOT				
RMG	0.335	0.509	0.479	0.386	0.423	0.552	0.338	0.581
RMG#Wife Edu Higher	0.715	0.221						
RMG#Formal Edu			0.542	0.458				
RMG#Above Median Inc					0.581	0.821		
RMG#Above Median DM							0.553	0.208

Table 2A.5: Standard errors and P-values with village level clustering for heterogeneity results on IPV index

	Clustered SE	P-Value	Clustered SE	P-Value	Clustered SE	P-Value	Clustered SE	P-Value
				ITT				
RMG	0.289	0.445	0.300	0.346	0.336	0.474	0.258	0.624
RMG#Wife Edu Higher	0.622	0.200						
RMG#Formal Edu			0.427	0.684				
RMG#Above Median Inc					0.403	0.851		
RMG#Above Median DM							0.396	0.138
				TOT				
RMG	0.331	0.347	0.401	0.302	0.340	0.363	0.298	0.774
RMG#Wife Edu Higher	0.740	0.149						
RMG#Formal Edu			0.307	0.571				
RMG#Above Median Inc					0.508	0.767		
RMG#Above Median DM							0.458	0.189

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