Women Empowerment and Domestic Abuse:

Theory and Experimental Evidence from Vietnam

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Abstract: Intimate partner violence is an important global health problem that policy makers seek to address by a variety of interventions, including efforts to promote "women empowerment." We use data from an RCT in Vietnam and find that this strategy may backfire: women who participated in a gender and entrepreneurship training suffer more frequent abuse than women in the control group. Guided by theory, we conjecture increased female income is the mechanism linking the training to domestic violence. We also make a methodological contribution, and show that the outcomes of our impact analysis depend on how we measure intimate partner violence.

Keywords: spousal violence, gender training, domestic abuse.

JEL Codes: D10, D74, I10, J12

1. **Introduction**

Violence against women is a widespread and pervasive problem. According to recent estimates, one third of all women have experienced physical or sexual abuse during their lives (Devries et al. 2013, Garcia-Moreno et al. 2005). The most common form of violence against women is intimate partner violence (IPV), which has significant (public) health and social impacts.[1] In addition to reduced agency of women, which is a human rights problem in itself, these impacts include costs due to poor physical and mental health. Fearon and Hoeffler (2014) estimated these costs to exceed the global economic cost of civil war. Not surprisingly, curbing IPV has emerged as an important policy priority. In this paper we will study the effect of an economic and gender empowerment training in peri-urban Vietnam on the incidence of violence against women.

Broadly speaking, two sets of theories try to explain domestic violence. *Expressive* violence theories start from the premise that violence serves the purpose of relieving frustration and anxiety triggered by challenges to traditional gender roles and norms emphasizing male authority.[2] Expressive theories assume violence enters directly in the perpetrator's utility function (e.g. Farmer and Tiefenthaler 1997). In contrast, *instrumental* violence theories are predicated on the assumption that violence helps male household members to assert control over scarce household resources, or to influence the behavior of household members (e.g. Eswaran and Malhotra 2011).

Depending on the underlying cause of IPV in specific settings, policy makers may consider various responses to mitigate the incidence of abuse. In contexts where expressive motives

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[1] some 40% of all homicides of women are committed by an intimate partner
[2] See Leyaro et al. (2017) on the cultural origins of gender roles and within-household inequalities, and how these are shaped by livelihood strategies.

dominate, interventions aiming to change men's attitudes may be appropriate. This may be achieved, for instance, through education programs aiming to alter attitudes and norms about gender inequity, or through engaging groups of men in training and discussion sessions (Jewkes et al. 2015, Elsberg et al. 2014). Economically empowering women in this context may be counterproductive – threatening males in their traditional authoritarian role. If expressive motives are unimportant, however, then perhaps economic interventions aimed at economically empowering women make sense. Such interventions are expected to increase female agency via two channels. Increasing the flow of resources that women bring into the household may alter their position as a bargaining partner directly, and lower the tolerance for abuse (World Bank 2011). Economic independence may also improve "outside options," or post-marriage opportunities in life, which may shift the threat point in cooperative bargaining.[3] This would invite husbands to strategically curb the dishing out of violence so as not to lose their wives (and their incomes). Examples of interventions aimed at promoting economic independence include microfinance interventions, or cash, asset or voucher transfer programs targeting women, but also efforts to promote female entrepreneurship.

The promotion of "outside options" may not be a viable or credible alternative strategy across all cultural contexts. In patriarchal societies, the stigma associated with divorce may be so severe that many women would never choose to terminate a marital relationship – no matter how abusive. Interventions based on instrumental theories of violence may be counterproductive then, as they could inadvertently invite additional violence due to expressive reasons. This is referred to as male backlash (Luke and Munshi 2011).

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[3] In case economic empowerment is achieved via work outside the house, a third channel is relevant: reduced exposure (e.g., Chin 2012a).

The empirical evidence on the effectiveness of anti-IPV interventions is rich, but also ambiguous and heavily skewed towards high-income countries. Early studies established correlations between IPV measures and observational data on employment status, income, credit use, or education (e.g. Hornung et al. 1982, Farmer and Tiefenthaler 1997, Bloch and Rao 2002, Koening et al. 2003, Heath 2014). However, interpretation of such correlations is hampered by simultaneity, omitted variables and (self-)selection. To attenuate endogeneity concerns, instrumental variable estimators may be used, leveraging exogenous variation in supply of, and demand for, female labor (e.g., Aizer 2010, Bhattacharrya et al 2011, Chin 2012a, Lenze and Klasen 2017).

A recent flow of experimental studies goes one step further, and randomly assigns subjects to interventions aimed at mitigating IPV. Several studies took place in developing countries, and especially conditional transfer programs, targeting women as beneficiaries, have drawn considerable attention.[4] Such programs have improved school attendance, health and nutrition outcomes (Duflo 2012), and also appear to have the potential to affect domestic abuse. Angelucci (2008) studies alcohol-related abuse, and finds that the effect varies with the size of the grant. Hidrobo and Fernald (2013) study the impact of cash transfer program in Ecuador, and find that the impact varies with characteristics of the receiving household, such as the (relative) education level of the partners. Hidrobo et al. (2016) use an RCT to compare the effects of cash, vouchers and food transfers on IPV, and document that all transfer modalities are equally effective in curtailing violence – reducing violence from 16% to 10%.

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[4] Another noteworthy, non-experimental study on transfers and IPV is Bobonis et al. (2013), who document that the effect of transfers varies across different measures of violence – physical and emotional abuse.

Other studies evaluate interventions that aim to promote female employment or entrepreneurship. Pronyk et al. (2006) consider a combined microfinance and HIV training program for poor women in South Africa, finding strong and encouraging results – a 55% reduction in IPV during the study period of 12 months. However, a follow-up analysis demonstrates the microfinance intervention itself had little effect on IPV, highlighting the importance of complementary training sessions and suggesting the existence of synergy effects across intervention modules (Kim et al. 2009). This appears consistent with evidence from a pilot RCT in rural Cote d'Ivoire that studies a combination of economic assistance (group savings) and so-called "gender dialogue groups" (Gupta et al. 2013). This study finds large effects of a packaged intervention on physical IPV (but not sexual IPV). Green et al. (2016) study a combination of a training module and financial intervention in Uganda. A one-off cash transfer is combined with a business skills training intervention intended to promote entrepreneurship by developing nonfarm microenterprises. While this intervention resulted in large employment gains for participants (Blattman et al. 2016), it did not mitigate abuse. Taken together, these studies send mixed signals about the scope to leverage economic development interventions to tackle domestic violence in Africa.

In this paper we use an RCT design to empirically explore the impact of a gender and business training on IPV for a sample of poor female entrepreneurs in rural northern Vietnam. The subjects in our sample are female micro-entrepreneurs and members of a microfinance organization (and therefore not necessarily representative for women in Vietnam generally). We randomly assign credit centers of the microfinance organization to either a control or experimental arm, and use both a survey question and List experiment to gauge the prevalence of IPV 6 months after the training.

The contribution of this paper to the literature is threefold. First and foremost, we use an experimental approach to contribute to the experimental literature on the effects of female empowerment on domestic violence, focusing on a novel subsample of subjects. Second, we make a methodological contribution by reporting diverging results for different approaches to measuring IPV. Specifically, we document serious underreporting of IPV when using standard survey-based measures of self-reported domestic violence, obscuring the signal-to-noise ratio that is evident from our preferred measure. Third, we develop a simple theoretical model of instrumental violence that serves as a lens though which we interpret our data. While we do not seek to capture all dimensions relating female empowerment to domestic violence in one encompassing model (e.g. expressive motives do not feature in it), the model is sufficiently rich to predict potentially opposing effects of increased female income and bargaining power on the prevalence of domestic abuse.

Our theory predicts that, for patriarchal contexts where divorced women suffer from extensive stigma (so there is no easy "exit" out of an abusive marriage), an increase in the wife's income results in more extensive abuse. In contrast, an increase in the wife's bargaining position should reduce the prevalence of IPV. These predictions are brought to the data, and receive considerable support. First and foremost, according to our preferred measure of IPV, participating in the training has a large and significant *accentuating* effect on domestic violence. Women in the treatment group are much more likely to suffer from abuse than women in the control group. We also find that participating in the training increases both female income and bargaining power. Additional patterns in our data are consistent with the hypothesis that an increase in female income is the mechanism linking the training to more frequent abuse. Potential mitigating effects via an increase in bargaining power due to the training are too small to offset the income effect. But the

nature of our data does not allow a full mediation analysis of the mechanisms linking the training to domestic violence, and we hasten to add that alternative theories may also have explanatory power.

The paper is structured as follows. In section 2 we sketch a theoretical model linking female income and bargaining power to intra-household violence, providing the lens through which we interpret our data. In section 3 we discuss IPV in Vietnam and the intervention. In section 4 we introduce our data and identification strategy, paying special attention to the measurement of IPV. In section 5 we present reduced-form regression results based on two types of measures of IPV, and seek to shed light on the mechanism linking the training to abuse. The conclusions and discussion ensue.

2. **The model**

Marital partners may pool part of their income in a common pot used to finance household public goods, and spend their remaining funds in accordance with their own preferences. Similarly, partners contribute part of their time and other resources to the production of household goods. A critical question is the decision what share of the income or resources to pool. In patriarchal cultures with strong gender roles, such as the one we study below, it is realistic to assume that the husband decides about the appropriate contribution share (Malapit 2012). In such "limited autonomy models" women have little or no say in the level of the "family tax." We assume both partners contribute equal shares of their endowment to the common pot, but similar results can be obtained when we relax this assumption, and instead assume that the husband can unilaterally

decide about separate contribution shares of himself and his wife (in which case the husband may want the wife to contribute more than she does).[5]

When the compulsory contribution level exceeds the wife's preferred contribution level, a possible response for her would be to resist contributions in excess of preferred levels. A mismatch between partners in terms of preferred contribution levels seems a natural source of tension within the household. In what follows we adopt a utilitarian perspective on domestic abuse and assume that such tensions are an explanation for violence within the household – husbands may use violence to force their preferences with respect to contributions upon their unwilling wives.

As a motivational example we use money (income) as the key resource that has to be allocated across joint or private accounts (but the allocation of time would be an equally valid problem). Consider the case of a patriarchal society where the husband decides about the share of income $\theta \in [0,1]$ that marital partners should contribute to the joint account. Assuming spousal income is given (and observable), we assume he chooses the contribution level that maximizes his private payoffs, or the sum of benefits from consuming the household good G and his discretionary income after taxation. Hence:

$$\pi_h = \delta_h \mu G(\cdot) + (1 - \delta_h)(1 - \theta_h)Y_h \tag{1}$$

where π_h denotes the husband's payoffs, δ_h is a parameter ($0<\delta<1$) capturing the husband's preferences for the household good $G(\cdot)$ financed via the common pot, the parameter μ (with $0<\mu<1$) is a parameter describing how much the husband captures of the household good (or the

extent to which this good reflects his preferences—as opposed to his wife's), and Y_h is the husband's income. In what follows, we interpret μ as a proxy of bargaining power, which can perhaps be affected by empowerment trainings. In the theoretical model we assume μ is fixed and invariant with respect to choices made by the spouses in the game (see Basu (2006) for a richer theoretical model where bargaining power affects choices, and choices affect future bargaining power). The first term of (1) represents the husband's utility from the household good, and the second describes utility of his residual income that can be freely spent on the consumption of private goods.

Assume that cooperating couples can produce household goods more effectively than singles. One extreme approach to modelling this is by assuming that production satisfies the following restrictions: $G(\theta_h Y_h, \theta_w Y_w) \geq 0; G(0, \theta_w Y_w) = G(\theta_h Y_h, 0) = 0$, or that household goods can only be produced by couples. In this specification, Y_w denotes the wife's income. The Cobb-Douglas production function satisfies these restrictions, and will be used in what follows:

$$G(\theta_h Y_h, \theta_w Y_w) = A(\theta_h Y_h)^\alpha (\theta_w Y_w)^\beta ,$$
(2)

where A is a production parameter. We also assume $\alpha + \beta < 1$, or decreasing returns to scale in the production of household goods.[6] Taking his own income and that of his wife as given, the optimal contribution share from the husband's perspective is simply:

$$\theta_h^* = \left[\frac{(\alpha+\beta)\delta_h \mu A Y_w^\beta}{(1-\delta_h)Y_h^{1-\alpha}} \right]^{1/(1-\alpha-\beta)} .$$
(3)

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[6] While the Cobb-Douglas specification makes sense when we think about pooling resources such as the husband's and his wife's time, it may be less appropriate for the case of pooling income (the money of both spouses are perfect substitutes). This is unimportant for the theoretical model, however, as the full analysis can be re-done (without much loss) for the simpler production function $G=A(\theta Y_h + \theta Y_w)^\alpha$, with $\alpha<1$.

Not surprisingly, the optimal contribution level chosen by the husband is increasing in his bargaining power and his wife's income. Greater bargaining power enables the husband to enjoy a greater share of the household good, and a higher income for his wife leverages his ability to "free ride" on his partner in terms of funding the household good (also see Malapit 2012).

Next, assume the wife's objective function is similar, but her level of bargaining power equals $(1-\mu)$, and she may have a different taste parameter δ_w:

$$\pi_w = \delta_w(1-\mu)G(\cdot) + (1-\delta_w)(1-\theta)Y_w. \tag{4}$$

If the wife were free to choose the contribution level, she would choose:

$$\theta_w^* = \left[\frac{(\alpha+\beta)\delta_w(1-\mu)AY_h^\alpha}{(1-\delta_w)Y_w^{1-\beta}}\right]^{1/(1-\alpha-\beta)}. \tag{5}$$

Upon comparing (3) and (5), it is easy to see why conflicts about the contribution level might emerge in households. Even in the absence of diverging preferences with respect to consumption of the household good, $\delta_h=\delta_w$, conflicts materialize if the spouses earn different incomes, $Y_h \neq Y_w$, or when the bargaining power is unequal, $\mu \neq 1/2$. Specifically, the husband will demand a higher contribution than his wife prefers to pay when the following condition holds:

$$\frac{\mu}{1-\mu} > \frac{Y_h}{Y_w}\frac{\beta\delta_w(1-\delta_h)}{\delta_h(1-\delta_w)}\left(\frac{\alpha^\alpha}{\alpha+\beta}\right)^{1/(1-\alpha)} \tag{6}$$

In what follows, we assume that condition (6) is satisfied for some households in the sample, or that the bargaining power of (some) husbands is sufficiently high that they demand a greater contribution from their wives than these women would voluntarily pay.

Assume that husbands have the option to use violence to discipline their unwilling wives, forcing them to contribute the husbands' preferred share to the joint account. To simplify the exposition, but without much loss of generality, we assume the utility loss for abused women is proportional to the intensity of violence, or to the gap in contribution levels demanded by the husband and preferred by the wife: $\gamma(\theta_h^* - \theta_w^*)$.

With these building blocks in place we can use backward induction to solve the intra-household violence problem. In the final stage of the problem the wife has to decide whether to abandon her husband or stay in the marriage. The feasibility of divorce is context and culture specific. In societies where divorced women are social outcasts, the reservation level of utility is close to zero, implying she has few options but to stay married (and possibly endure extensive abuse in marriage, as in Bloch and Rao 2002). In other settings, women are free to abandon their husband and possibly find a new partner. If, upon divorce, a woman loses access to the jointly produced household good, but is free to spend her own income on private goods, her participation constraint can be written as follows:

$$\delta_w(1-\mu)G(\cdot) + (1-\delta_w)(1-\theta)Y_w - \gamma(\theta_h^* - \theta_w^*(\cdot)) \geq (1-\delta_w)Y_w - S \qquad (7)$$

In (7), the constant term S captures the (society-specific) utility loss that may be caused by stigmatization in conservative societies. In the absence of stigmatization, or when $S=0$, it is never optimal for women to stay married if they lack any bargaining power ($\mu=1$) but are forced to contribute to the joint account ($\theta > 0$). The participation constraint can be solved for the critical contribution level $\hat{\theta}$ where the wife is indifferent between staying in the marriage (and suffer exposure to violence defined as $\gamma(\hat{\theta} - \theta_w^*)$), or divorce. Upon rearranging terms, the participation constraint simplifies to:

$$\left(\delta_w(1-\mu)AY_h^\alpha Y_w^\beta\right)\theta^{\alpha+\beta} \geq \left((1-\delta_w)Y_w+\gamma\right)\theta - \gamma\theta_w^*(\cdot) - S. \tag{8}$$

If we equate the LHS to the RHS of (8) we can solve for $\hat{\theta}$, or the critical contribution level where the wife is indifferent between marriage and divorce. If husbands demand a contribution level exceeding threshold $\hat{\theta}$, the wife is better of abandoning him. Using the Mean Value Theorem it can be shown that this equilibrium is unique (for $\alpha+\beta<1$). This is also intuitive, and can easily be shown graphically. Imagine a figure with contribution share θ on the horizontal axis. The term on the LHS of (8) is a concave function starting from the origin, and the sum of terms on the RHS is a straight line with negative intercept $(-\gamma\theta_w^* - S)$ and positive slope $\left((1-\delta_w)Y_w+\gamma\right)$. These lines will cross once. Note we also know the following:[7]

$$\frac{d\hat{\theta}}{d\mu} < 0, \text{ and} \tag{9a}$$

$$\frac{d\hat{\theta}}{dY_w} >< 0. \tag{9b}$$

The sign of (9a) is due to two reinforcing effects: increasing the bargaining position of women (or lowering μ) implies (i) the concave function rotates to the left (upwards) and (ii) the intercept of the straight line shifts down (as $\frac{d\theta_w^*}{d\mu} < 0$, from (5)). The first effect captures that women enjoy a greater share of the household good, and the second effect captures that women are willing to pay a larger share of their income (so that the intensity of violence decreases). Both effects imply women are better off, which enables the husband to increase the contribution share so as to bring his wife back to her reservation utility.

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[7] Write the participation constraint as $v\hat{\theta}^{\alpha+\beta} + \sigma\hat{\theta} + \tau = 0$, with $v>0$, $\sigma<0$ and $\tau>0$. Taking total differentials of this condition immediately yields: $\frac{d\hat{\theta}}{da} < 0, \frac{d\hat{\theta}}{d\sigma} < 0$ and $\frac{d\hat{\theta}}{d\tau} < 0$. From these conditions follow (9a) and (9b).

Comparative statics with respect to the woman's income, $\frac{d\hat{\theta}}{dY_w}$, are more complex. Three

effects work in different directions: (i) higher female income increases the supply of household

goods, but (ii) the woman's preferred contribution level shifts down and (iii) the reservation utility

level of the wife increases. In terms of the participation constraint; the concave LHS of (8) rotates

upwards, and the RHS both shifts up and rotates upwards. The net effect on the critical threshold

$\hat{\theta}$ is ambiguous.

We can now characterize the solution to the intra-household sharing and violence model.

Since we are especially interested in the impact of women empowerment on domestic violence,

we consider how female income and female bargaining power affect levels of domestic abuse in

equilibrium. We assume female bargaining power affects control over household resources but

does not necessarily translate into an improved ability to reduce exposure to IPV – couples do not

bargain directly over the level of abuse.

Two possible outcomes are relevant for near-marginal changes in female empowerment.

Case 1: $\theta_h^* < \hat{\theta}$. The husband's preferred contribution level is below the critical

contribution level defined by the wife's participation constraint. He therefore sets his preferred

rate as defined by (3), implying the following testable hypotheses:

Hypothesis 1a: For interior solution $\theta = \theta_h^* < \hat{\theta}$, if the wife's bargaining power increases,

the contribution level set by the husband goes down, and so does the level of IPV. $\left(\frac{d\theta_h^*}{d\mu} > 0\right)$.[8]

(Recall that (1-μ) measures the wife's bargaining power.)

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[8] $\frac{\partial \theta^*}{\partial \mu} = \frac{1}{1-\alpha-\beta}\left[\frac{(\alpha+\beta)\delta\mu AY_w^{\beta}}{(1-\delta)Y_h^{1-\alpha}}\right]^{\frac{\alpha+\beta}{1-\alpha-\beta}}\left[\frac{(\alpha+\beta)\delta AY_w^{\beta}}{(1-\delta)Y_h^{1-\alpha}}\right] = \frac{Y_w}{\beta\mu}\frac{\partial\theta}{\partial Y_w} > 0.$

Hypothesis 1b: For interior solution $\theta = \theta_h^* < \hat{\theta}$, if the wife's income increases, the contribution level set by the husband goes up, and so does the level of IPV. $\left(\frac{d\theta_h^*}{dY_w} > 0\right)$.[9]

Observe that bargaining power and female income are both important dimensions of female empowerment, and that interventions aimed at curtailing IPV often seek to promote both of them simultaneously. However, the theoretical model predicts that increasing female income will have the opposite impact on IPV as increasing female bargaining power, so lumping these dimensions together is not helpful and may obscure important effects.

Observe that if the husband's optimal initial contribution level is sufficiently close to the threshold, it is possibly that a small increase in the contribution level (because of an increase in his wife's income, say), may cause his optimal level to exceed the threshold. In that case, the household enters a different region with qualitatively different results:

Case 2: $\theta_h^* > \hat{\theta}$. The husband's preferred contribution level exceeds the threshold as defined by the participation constraint. Assuming limited or uncertain opportunities for re-marriage, the husband prefers to stay married to his wife: he can always earn the payoffs he would obtain under divorce by setting $\theta = 0$, and does strictly better as a married man than as a divorced man for positive contribution levels ($\theta > 0$).[10] His optimal response, therefore, is to set $\theta = \hat{\theta}$, and

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[9] Observe that the following holds: $\frac{\partial \theta^*}{\partial Y_w} = \frac{1}{1-\alpha-\beta}\left[\frac{(\alpha+\beta)\delta\mu AY_w^\beta}{(1-\delta)Y_h^{1-\alpha}}\right]^{\frac{\alpha+\beta}{1-\alpha-\beta}}\left[\frac{(\alpha+\beta)\beta\delta\mu AY_w^{\beta-1}}{(1-\delta)Y_h^{1-\alpha}}\right] > 0.$

[10] In other words, we assume the husband prefers to remain married. The situation is obviously more complicated if we allow for heterogeneity in match quality. In that case the husband may wish to terminate the relationship and find a better partner. If the expected value of his outside option is better than the utility he obtains from the corner solution studied in Case 2, he would set a contribution share θ^* exceeding the critical value $\hat{\theta}$ (i.e. no longer satisfying the wife's participation constraint) and the marriage would be terminated.

to accept payoffs associated with the corner solution. This implies the following testable hypotheses:

Hypothesis 2a: For corner solution $\theta = \hat{\theta}$, if the wife's bargaining power increases, the critical contribution level shifts up (9a), and so does the level of IPV. $\left(\frac{d\hat{\theta}}{d\mu} < 0\right)$

Hypothesis 2b: For corner solution $\theta = \hat{\theta}$, if the wife's income increases, the critical contribution level is ambiguously affected (9b), and so is the level of violence.

3. **Female empowerment in Vietnam**

As shown, predictions with respect to the effect of female empowerment on levels of domestic violence are fundamentally ambiguous, even in the absence of expressive reasons for violence. An important consideration shaping the dynamics of abuse is the cultural context which affects the salience of the wife's participation constraint. In societies with very conservative values, women will be deterred from abandoning their husband because of the stigmatization associated with divorce and the enduring discrimination that follows. In terms of the model above, countries with large *S* values are likely to be captured by case 1 in equilibrium (rather than case 2).

This is also true for the context of (rural) Vietnam, studied below. Rural Vietnam is a strongly patriarchal society, with Confucian traditions placing high value on female obedience to men. According to a recent article in the Financial Times (2011), divorced women in Vietnam are likely to stay alone for the rest of her life due to strong stigma effects. While the divorce rate (o the percentage of marriages ending in divorce) is rising, it is still only 1.7%. Filing for divorce is primarily a matter for well-educated young women in urban areas – not the women in our sample. The reputational damage for women in rural areas is so severe that few women consider divorce a

realistic alternative. In terms of the model above, for the great majority of our respondents the participation constraint is unlikely to bind so that the solution of the intra-household violence problem is typically an interior solution (as opposed to a corner). Hence hypotheses 1a and 1b are the most appropriate predictions following from the theory.

IPV occurs commonly in rural Vietnam, and its prevalence appears consistent with estimates for other low or middle income countries. For example, Krause et al. (2016) estimate that 31% of all women in Vietnam have experienced physical abuse during their lifetime, and Vung et al. (2008) find that the prevalence during the past year alone was nearly 10% (see also Vung et al. 2009). Among the Vietnamese population, especially in rural areas, attitudes about gender equality are weakly developed, and the tolerance for IPV is considerable. Tran et al. (2016) indicate that in Vietnam almost 40% of the respondents respond it is acceptable for men to use violence against their spouse.

4. **The intervention**

We evaluate the impact of a gender and business training on domestic violence for a sample of female members of the largest microfinance organization in northern Vietnam; the TYM fund. TYM provides the training because the organization seeks to empower its female members – both by helping them to increase their incomes (promoting economic independence) and by raising awareness of gender equality. For full details of the intervention, we refer to Bulte et al (2017) who study the impact of this training on a range of business outcomes (including profits), and to Huis et al. (2017) who focus on training's impact on intra-household bargaining. While the latter study considers "intra-household frictions", neither of them considers domestic violence.

The training intervention took place between February and October 2012 in two districts; Hanoi and Vinh Phuc. The program is based on the Gender and Entrepreneurship Together (GET) Ahead for Women in Enterprise Training Package and Resource Kit, of the International Labor Organization (ILO), and contains nine modules lasting 45 minutes each. The first three training modules are purely devoted to gender issues, and are intended to promote gender equality and to improve the bargaining position of women in the household. The remaining six modules cover standard business subjects, including marketing, financial literacy, cash management, and cost calculations. These modules should help women to turn their business activities into successful endeavors, increasing the flow of income they earn from these activities. Details of the training content are provided in Appendix A. Trainings were delivered during monthly meetings at TYM credit centers, and weekly refresher meetings. Participating in the trainings was voluntarily and for free.

Our sample contains 187 credit centres. We stratify randomization by lending branch, and initially assigned centres randomly to one of three experimental arms: (i) 31 credit centres to a treatment arm where only female clients were invited, (ii) 70 credit centres to a treatment arm where male partners were also invited to join the training, and (iii) 86 credit centres were assigned to the control group (and did not receive training). We purposefully assigned many centres to the treatment arms where husbands were invited to participate because we feared extensive non-compliance among husbands. By increasing the number of centres we attempted to attenuate power problems due to low attendance of husbands (unsuccessfully, as will become clear). We randomly selected 23 members per centre for the interviews, and for those centres with less than 23 clients we interviewed all members.

While one of the underlying ideas of this study was to explore how the participation of men in the trainings affects the outcomes of the training intervention, our data do not allow us to explore this question in a satisfactorily way. The reason is extensive non-compliance among husbands, who most of the time did not show up at the training sessions. As summarized by Bulte et al. (2017), the participation rate of husbands declined from nearly 40% in the first session to less than 10% in later ones.[11] Nearly half of the invited husbands did not attend any training session, and only 2% of them attended all sessions. Due to male non-compliance the differences between the two treatment arms are very small, and outcomes for the two subsamples are statistically very similar. To increase statistical power we pool the data from the treatment arms and consider only two experimental groups: 101 credit centres receiving training, and 86 credit centres from the control group not receiving training. But we will also demonstrate our results are robust with respect to distinguishing between the two initial treatment arms.

More than 80% of the invited women attended all training modules. Approximately 90% of the women in the sample belong to the Kinh ethnicity (the dominant ethnic group in Vietnam). The remaining 10% belongs to a mix of ethnic minorities from a range of geophysical backgrounds, with (potentially) quite different gender norms.[12] As is evident from the opposing comparative static effects summarized in Cases 1 and 2 above, lumping different ethnicities (cultures) together may obscure important results for individual ethnicities. This is why the main empirical analysis focuses on the Kinh only.

5. **Data and identification**

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[11] Attendance was relatively high in the first session because we financially incentivized attendance then.

[12] for discussions on how traditional livelihoods agriculture affect gender roles and the balance of power within households, refer to Alesina et al. (2013) and BenYishay et al. (2017). Refer to Michalopoulos et al. (2016) and Leyaro et al. (2017) for explorations of the link between traditional livelihoods and (the justification of) violence.

This analysis is based on baseline data collected before the intervention (in November 2011) and two waves of data collected after the intervention. We used two alternative approaches to collect data on IPV six months after completion of the intervention. Our main analysis is based on a so-called List experiment, also known as the item count technique. List experiments seek to elicit truthful answering by using an indirect questioning method, and are particularly helpful to deal with 'sensitive issues' such as domestic abuse. Since subjects may give 'socially desirable answers', direct questions about IPV possibly cause underreporting of the true prevalence of violence. The advantage of List experiments is that subjects have no incentive to lie as they do not have to admit directly to being the victim of IPV. The main drawback is that while List experiments enable identification of effects at the level of experimental groups, they (typically) do not enable the analyst to identify with certainty which individual women are victims of abuse.

A standard List experiment works as follows. A sample of respondents is randomly assigned to one of two groups: a "control group" receiving a list of J non-sensitive (or neutral) statements, and a "treatment group" receiving the same statements plus one sensitive item – the one about abuse in our case. Individuals from both groups are asked to report with how many of the statements they agree, but not with which particular statements they agree. Due to the random assignment of the "sensitive statement" to one of the sub-samples, the difference-in-means between the two groups gives an unbiased estimate of the proportion of the sample agreeing with the sensitive issue.

Our main purpose is to evaluate whether the gender and business training reduces the incidence of IPV – we are primarily interested in prevalence heterogeneity due to the training. Our design therefore involves stratified randomization, and we stratify by experimental arms in the RCT (being invited to the training, or not). We randomly divide our two sub-samples in two

groups: one group receiving questionnaire A (with five statements) and another group receiving questionnaire B (with four statements). The four non-sensitive statements that are common to questionnaire A and B are:

1.   "*I have money in a saving account*";

2.   "*I prefer local fruits over Chinese fruits*";

3.   "*My household does not have a television*";

4.   "*I usually buy pears*".

The additional statement only included to questionnaire A reads as follows: "*I am regularly hit by my spouse.*" The question that followed both lists of statements was as follows: "*with how many of the statements in the list above do you agree?*"

We also used a conventional survey approach to gauging IPV. Specifically, we asked the following question: "*How often did your husband push, slap, beat or hit you during the last 6 months?*" We provided the following categories for answering: 0 = Never; 1 = Rarely; 2 = Sometimes; 3 = Often; 4 = Very often; 99 = Don't know; and 88 = Refused to answer. Observe this question is intentionally not phrased exactly the same as the sensitive issue included in the List experiment. We hope this attenuates concerns about one response leading the other.

The List experiment was part of the survey, and the order in which the List experiment and direct IPV question were presented was fixed: first the List experiment, and later in the survey the direct question. We have no reason to believe this fixed order affects the choices of the respondents in the two experimental arms differently.

Since centers were randomly assigned to the training intervention in the RCT, and subjects were also randomly assigned to arms in the List experiment, treatment status is orthogonal to

subject characteristics and we do not need to control for additional variables. However, to increase the precision of our estimates we not only control for branch dummies, but also for the subject's age and marital status, and the size of her household in some of the models.

Next, turn to the models we will estimate to assess the impact of the training on IPV. Our main analysis is based on a simple linear reduced-form model:

$$Y_i = \alpha + \beta_1 Training_i + \beta_2 ListA_i + \beta_3 Training_i * ListA_i + \varepsilon_i \tag{10}$$

In (10), Y_i captures the number of statements that respondent i agrees with, *Training* is a dummy variable taking the value of 1 when the respondent was invited to participate in the gender and business training (and 0 otherwise), and *ListA* is a dummy taking the value of 1 when she received the questionnaire with five statements (and 0 otherwise). Interpretation of the outcomes of the linear model is straightforward. The coefficient of interest is β_3, which takes a negative value if the training reduces exposure to violence and which takes a positive value if the training increases exposure to violence. As robustness tests we will estimate various variants of (10). In all these models we will cluster standard errors at the training center level to tackle within-center correlation in outcomes.

In addition to applying the difference-in-means estimator we control for boundedness of the response variable by using a nonlinear least squares estimator due to Imai (2011) as an additional robustness analysis:

$$Y_i = f(X_i, \gamma) + ListA_i f(X_i, \partial) + \varepsilon_i \tag{11}$$

Where $E(\varepsilon_i | X_i, ListA_i) = 0$, (γ, ∂) is a vector of unknown parameters, and X is a vector of observed covariates, including the intercept and the binary indicator *Training*. As before,

Training=1 again implies the respondent belongs to the (treatment) group invited to participate in the training, and *Training*=0 indicates subjects from the control group. Next, $f(x,\gamma)$ and $g(x,\partial)$, where $x \in X$, are regression models (logit models) for the conditional expectations of control and sensitive statements, given the covariates. The nonlinear least squares estimator uses a two-step procedure in which $f(x,\gamma)$ is first fitted to the group receiving four statements and next $g(x,\partial)$ is fitted to the group receiving five statements. Heteroscedasticity-consistent robust standard errors are obtained via method of moments (as in Imai 2011). This model implies the linear multivariate regression model with treatment-by-covariate interaction terms (as in Holbrook and Krosnick 2010) if the two sub-models are assumed to be linear. That is;

$$Y_i = X_i^T \gamma + ListA_i X_i^T \partial + \varepsilon_i \qquad (12)$$

Finally, and as outlined below, we also seek to open the "black box" and document whether additional patterns in the data are consistent with our theory about the mechanisms linking the training intervention to domestic violence, or not.

6. **Empirical results**

6.1 *Evaluating the design*

A condition for unbiased measurement with List experiments is the absence of so-called design effects – answers to control statements should not be affected by the inclusion of the sensitive statement. Blair and Imai (2012) develop a likelihood ratio test to examine whether the no-design effect assumption is violated. This test uses the assumption that the addition of a sensitive statement should not change the sum of affirmative answers to the control statements,

and that (cumulative) proportions of different respondent types should be non-negative.[13] If one of the proportions is negative, the no-design effect assumption is violated. The test statistic indicates whether negative values arise by chance, in which case we do not reject the null hypothesis of no-design effects.

We calculate the proportions of each respondent type, assuming no-design effects and no-lying, as follows:

$$\pi_{y1} = \Pr\left(Y_i \leq y \middle| T_i = 0\right) - \Pr(Y_i \leq y \middle| T_i = 1),$$
$$\pi_{y0} = \Pr\left(Y_i \leq y \middle| T_i = 1\right) - \Pr(Y_i \leq y - 1 \middle| T_i = 0)$$

(13)

This amounts to subtracting the cumulative proportions of the observed data from the same rows for π_{y1} and cumulative proportions of consecutive rows for π_{yo}. For our data, outcomes are reported in Table 1.

<< Insert Table 1 about here >>

For one respondent type the proportion is negative. Following Blain and Imai (2012), we use the *list* package in R to test for design effects. The *p*-value of the test equals 0.64, so we cannot reject the null of no-design effects.

We also need to consider the possibility that respondents might lie about the sensitive statement. Lying is particularly likely to occur in case of *ceiling* or *floor* effects. A ceiling effect occurs if subjects agree with all control statements, and a floor effect occurs if subjects agree with none of the control statements. In either case, affirmative responses to the sensitive statement are easy to identify and it is difficult to protect the privacy of respondents. However, for our List experiment the potential of *floor* or *ceiling* effects is small. In our control sample of 1922

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[13] Note that this refers to the two groups randomly determined in terms of the list experiment; it does not refer to the groups randomly separated in terms of the training.

respondents, only 1.98% agreed with zero statements, and only 0.16% agreed with all control statements. The modal response was approximately two, suggesting the preferred negative correlation between control statements.

6.2 Balancing tests and attrition

Before presenting our regression results we test whether there is balance across the groups receiving and not receiving the sensitive statement. As our main aim is to test the impact of a business and gender training on affirmative answers regarding the sensitive question, we conduct balancing tests for respondents invited to participate in the training and respondents from the control group separately. We consider our exogenous control variables: age of the female TYM member (*Age*); marital status (*Married*); household size (*Members hh*), and a regional dummy for Vinh Phuc (*Region*).

In addition, we consider whether at baseline the following variables were balanced: number of loans from TYM (*Credit access TYM*); intra-household bargaining (*Bargain*); a dummy for agricultural activities (*Agr. activity*); land size in agriculture in acres (*Land size*): an index for business knowledge (*Knowledge*); our observational measure of IPV (*IPV-Survey*); and inverted hyperbolic profits in the previous month (*Profit*).

<< Insert Tables 2a, 2b, 2c and 3 about here >>

Results are given in Tables 2a and 2b below, based on regressing baseline values on the *ListA* dummy (and a constant). These tables suggest balance across the two subgroups as the mean for the group with the sensitive statement does not significantly differ from the mean of the group responding to only non-sensitive statements – *ListA* is insignificant in all regressions.

In Table 2c we present balance tests for the training as such. The table shows that baseline means for the treatment group (female that will follow the training) are not significantly different from baseline means of the control group.

In Table 3 we report the outcomes of a probit model explaining the determinants of attrition – subjects dropping from our sample after the baseline. This is the case for 13% of our baseline respondents. To explain attrition we use the same variables as considered in the balance tests summarized in Tables 2a and 2b. As is evident, attrition is not purely random. Instead, it seems negatively correlated with age and marital status, and (not surprisingly) the number of loans with TYM.[14] To reduce bias due to non-random attrition of observables we also provide estimates based on attrition probability weighting, showing that the main results are not affected.

6.3 Women empowerment and domestic abuse

We now report our main reduced form regression results. Column (1) of Table 4 summarizes the results of the linear regression model explaining variation in the number of affirmative answers to the statements.[15] Surprisingly, the training has a significant positive effect on the probability of answering affirmatively to the question about domestic abuse. Women participating in the training are more likely to report being the victim of regular violence.

<< Insert Table 4 about here >>

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[14] We also interacted all observables with the treatment dummy (followed the training) to check for non-random attrition across treatment arms. It appears that only one variable (the interaction with household size) is significant (with a negative sign). After interaction, *age* becomes insignificant. The results can be obtained on request

[15] In alternative regressions we added *Age, Married, Members hh and Region* to the different linear models. This this not affect the results (neither the coefficients, nor the standard errors, of our variables of interest changed). These regression results can be obtained on request.

The results of the linear model are relatively easy to interpret. On average, respondents from the control group agreed with 1.89 statements from the set of control statements. Respondents from the treatment group agreed with slightly fewer control statements, namely 1.75 (or 1.89–0.14). This divergence in outcomes is not really surprising, as earlier analyses found that participating in the training encourages investments in economic activities and therefore affects the probability that households have (residual) money in a saving account (Bulte et al. 2017).

More interesting are the coefficients associated with the sensitive statement. From the control group, 11% of the members agree with the statement that they are regularly hit by their husband. For women from the treatment group, this share increases to 21%: the training induced additional violence. The effect is not only statistically significant; it is also meaningful: women participating in the training are 10 percentage points more likely to report being the victim of regular abuse, amounting to an increase of 90% compared to the control group.

How do these numbers compare to those in the existing literature? As mentioned earlier; estimates suggest that slightly more than 30% of the Vietnamese women have been exposed to IPV during their lives. Comparing these outcomes with our estimates is complicated by the fact that we consider the issue or regular abuse (as opposed to, possibly, incidental abuse) and focus on a non-representative sample of entrepreneurial women who have joined a microfinance organization and have their own business. Nevertheless, it appears as if our numbers are in the right ballpark.

It is possible that the intervention affects the accuracy of measurement, which would introduce non-classical measurement error. If the training content raises women's knowledge about issues related to gender equality, as intended, then this may affect the way they respond to

the statement about abuse.[16] Specifically, this would occur if trained women are better able to recognize domestic abuse in case it occurs. This concern is attenuated by the fact that the training did not contain any content focusing on violence, so trained women do not have additional knowledge about this issue, nor are they "primed" to respond in a certain way. Also observe that exposure to physical abuse is obvious for the victim, and unlikely to go unnoticed. Another potential concern is experimenter demand effects, which would bias our estimates of impact downward if trained women are inclined to report fewer cases of abuse (so that our results are a lower bound of the causal effect of the training on the prevalence of IPV). There would be an upward bias if trained women are more inclined to report abuse. Without dismissing the importance of this caveat entirely, we speculate these biases are more relevant for the case of our survey-based measure of IPV, discussed below. The survey-based measure enables the analyst to link responses to individual respondents, and is therefore more likely to suffer from biases.

In column (2) of Table 4 we introduce the additional treatment dummy *T2* to distinguish between the two sub-samples of women whose husbands were and were not invited to participate in the training. Importantly, *Training* still takes the value 1 for respondents from all centres assigned to treatment (with and without invitations extended to husbands), so *T2* captures the additional effect of inviting husbands. Treatment dummy (*Training*) remains significant when interacted with the *ListA* variable. Reflecting the extensive non-compliance among invited husbands, we find the dummy variable associated with one of the sub-groups is small and not significantly different from zero, and neither is the interaction of *T2* and *ListA*. Also observe the coefficient of the latter interaction term is very small. Estimating regression models with separate

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[16] This is akin to the familiar question whether business trainings affect the accuracy of reporting sales and profits (McKenzie and Woodruff 2014). The empirical literature is divided on the relevance of this concern (e.g. De Mel et al. 2012, Drexler et al. 2014, and Berge et al. 2014).

dummies for the two subgroups produces the outcome that the associated coefficients are not significantly different from each other (according to a Wald test).

Our regression results are also robust with respect to alternative modifications. For example, in column (3) we present the results of a model based on attrition probability weighting. In column (4) we report the regression outcomes of the simplest specification for the full sample of female respondents – including the non-Kinh women with potentially different norms of gender equality. As a result, the coefficient of interest takes a slightly smaller value, and is only significant at the 15% level. This is consistent with attenuation bias introduced by respondents with different gender norms (levels of stigma) and, hence, diverging responses to the training. In column (5) we report the results of the non-linear model for the sample of Kinh women. These results are qualitatively consistent with the earlier findings.

We next analyze the survey data on domestic violence. Interestingly, not a single client from either the treatment or control group now reports to be the victim of abuse "often" or "very often" during the past six months. Instead, 92% (or 96%) of the respondents from the control (or treatment) group now indicates "never" to be "pushed, slapped, beat or hit" by their husband during the past 6 months. No respondents were unwilling (or unable) to answer the question, and nobody answered they "did not know" the answer.

Hence, while 11% (or 21%) of the women from the control (or treatment) group admits to regular victimization in the List experiment, none of our subjects does the same in response to a normal survey question. Recall: these are the same individuals, responding to the survey question and participating in the List experiment. Of course the results of the experiment are much more consistent with alternative estimates of the prevalence of IPV in rural Vietnam, which we believe

supports the conjecture that respondents are prone to lie about sensitive issues in surveys and are more likely to reveal outcomes truthfully when their anonymity is guaranteed.

Not surprisingly, the patterns in the survey-based data are also quite different from the ones found in the experimental data. This is evident from Table 5: according to both the parsimonious regression model as well as the model that includes control variables we now find that trained women appear to be abused *less* frequently by their husbands than women from the control group. This is the exact opposite of the results of the List experiment. However, in light of the fact that the survey data deviate considerably from population data obtained in earlier studies we believe these survey-based results on IPV cannot be trusted, and presumably are explained by experimenter demand effects.

<< Insert Table 5 about here >>

6.4 "Mediation analysis": The mechanisms linking the training to IPV

We next probe the mechanisms linking participation in the training and the prevalence of IPV. Full-fledged mediation analysis, as outlined by Barron and Kenny (1986), is rare in economics, and Deaton (2010) observes that treatments in most economic analyses are best interpreted as a "black box." One reason why mediation analysis is controversial in economics is that it is difficult to assess the plausibility of the so-called "sequential ignorability assumption" – the assumption that there is no confounder affecting both the outcome (IPV) and mediator variables (income and bargaining power). We therefore refrain from a full mediation analysis and causal claims with respect to the mechanism. However, we will describe patterns in our data, and argue these are at least consistent with our theory.

The mediation variables of interested are female income and bargaining power. We may of course treat these mediating variables as dependent variables, and explain variation in income and bargaining power by the training treatment (as this was randomly assigned). We therefore first estimate the following two models:

$$Profit_i = \alpha + \beta Training_i + \gamma X_i + \varepsilon_i, \text{ and} \tag{13a}$$

$$Bargaining\ power_i = \alpha + \beta Training_i + \gamma X_i + \varepsilon_i. \tag{13b}$$

We used the same survey instrument (and the same sample) to collect information on the two mediating variables; *profits* and *bargaining power*. Our measure of profits (or income derived from the business activity undertaken by the wife) is based on a direct estimate of last month's total profits. To deal with outliers and zero-responses, we use the inverted hyperbolic sine transformation of this variable. As a measure of bargaining power we construct an index capturing how often the wife is involved in decisions regarding certain expenditures (such as consumer durables, financial assistance to family members, and so on). Observe this variable captures the key dimension of our bargaining power parameter μ in the model above. Please refer to the Appendix for additional details about these variables, or refer to Bulte et al. (2017) for information about *profits*, and Huis et al. (2017) for information about *bargaining power*. As before, *Training* is a dummy indicating whether the respondent was invited to participate in the training intervention, *X* is a vector of controls, and we cluster standard errors at the center level.

Regression results for the two mediating variables are reported in Table 6.

<< Insert Tables 6 and 7 about here >>

The main thing to observe is that – as intended – the training increases both female income and bargaining power. The magnitude of these causal effects is meaningful, consistent with the theory, and in line with earlier results reported in Bulte et al. (2017) and Huis et al (2017). Specifically, we find that participating in the training causes a 0.33 standard deviation increase in profits, and a 0.17 standard deviation increase in our index of bargaining power. Importantly, the standardized effect size on profits is twice as large as the effect size on bargaining power. This reflects that while gender norms may be malleable, they are more resistant to change than economic variables such as income.[17] For details on the impact of the training on economic and gender outcomes, refer to Bulte et al. (2017) and Huis et al. (2017).

Next we move to more treacherous academic terrain, and consider the second link in our theory: increases in income and women bargaining power are expected to invite, respectively, higher and lower levels of IPV. We do not have random variation in the mediating variables, so this part of the analysis is not experimental but rather based on observational data. We also cannot assume that the training is a valid instrument for either mediating variable, as the exclusion restriction is likely violated. Therefore, we simply document the association between our mediating variables and the prevalence of IPV, and examine whether these associations are consistent with predictions. Using data from the List experiment we again estimate linear and non-linear models explaining variation in the number of statements that the respondent agrees with. Regression results are reported in Table 7. Increasing female income is associated with more

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[17] De Hoop et al. (2014) analyze the impact of women self-help groups on female autonomy – a concept related to how we operationalize empowerment. They find that self-help groups can increase autonomy, further suggesting that women's bargaining position is malleable to interventions. However, and as a word of caution, they also find that this does not automatically translate into improved subjective well-being – the reverse appears true in conservative societies with strong gender norms that may be transgressed through increased female autonomy. In such contexts, social sanctions (perhaps including violence – see Koenig et al. 2003) as well as identity-related factors may undermine any gains in well-being following from female empowerment.

domestic abuse: the interaction between *Profits* and *ListA* is positive and statistically significant. Women who bring more money in the household are more likely to report being abused by their husband. As mentioned, unobserved heterogeneity imply we cannot interpret this relationship as causal. However, we do note that the correlation is consistent with the theoretical prediction of increased intra-household conflict about the allocation of money.

The evidence for bargaining power as an important mediating variable is much weaker. While the coefficient of the training variable has the correct (negative) sign, it is not statistically significant at conventional levels.

7. **Discussion and conclusion**

In this study we analyze the complex relationship between female empowerment and domestic violence. Intimate partner is a widespread and persistent problem adversely affecting the lives of hundreds of millions of women, and posing a first-order challenge for policy makers worldwide. Since violence against women is often regarded as a symptom of lack of gender equality, it seems natural to assume that efforts to promote female empowerment will help to reduce intimate partner violence. Indeed, many governments and NGOs pursue such an agenda by both promoting economic independence of women and increasing their bargaining power.

In this paper we demonstrate that such strategies may be counterproductive, even in a simple context without expressive motives for violence against women (but with strong stigma associated with divorce). We develop a theoretical model where tensions between spouses might emerge because of disagreement about sharing norms within the household ("the family tax"), and demonstrate that two common dimensions of female empowerment – economic independence and bargaining power – may have complex and opposite effects on the prevalence of domestic

violence. Specifically, while increasing the bargaining power of women within the household, or their say in how to spend household resources, tends to attenuate IPV, the reverse may be true when the income increases that women bring into the household. The model predicts that increasing the bargaining position of women will reduce tensions over the compulsory contribution level, and that increasing female income has the opposite effect. The latter effect seems counterintuitive, but is in line with a review by Vyas and Watts (2009). In reality the situation is arguably even more complex, as income and bargaining power are likely to be inter-linked in the household model.

Guided by our theoretical model we analyze the impact of a gender and business training for a sample of female entrepreneurs in rural Vietnam. We developed an RCT in which a random subsample of women benefitted from a training intervention that aimed to promote gender equality and help them to scale up and professionalize their micro-enterprises. Consistent with the stated objectives and theory of change of the implementing organization we find that participating in the training indeed resulted in higher incomes and a greater say in major spending decisions of the household. However, and consistent with the theory we develop, we also find that the implications for domestic violence are complex. Specifically, we obtain the robust reduced form result that participating in the training invited additional abuse. We conjecture this is due to the additional tensions associated with income pooling within the household. Theoretically, increased bargaining power may attenuate such tensions, but our empirical results suggest this offsetting effect is too weak to balance the accentuating effect of higher female income.

These results should be interpreted with care. For example, we do not have a placebo treatment for women in the control group, who consequently were able to spend more time at home – working in the house or firm. We also certainly do not wish to convey the message that increasing

female income will always invite more intense levels of abuse. This depends on the context, including the viability of the "exit option" for married women. In particular, this depends on stigma associated with divorce. In conservative societies, such as in rural Vietnam, divorce hardly occurs and women typically cannot credibly threaten to abandon their husband. This is the context where promoting women empowerment through economic independence appears especially risky. It is interesting to observe that Hidrobo et al. (2016) obtain results, based on Peruvian data, that appear opposite to ours. In Peru divorce rates are also very low (perhaps because of the influence of the Catholic church), yet Hidrobo et al. (2016) find that transfers to women reduce IPV. However, it is important to observe that while the divorce rate may be low in Peru, the *separation rate* is high (6.5%). From the perspective of our model the distinction between divorce and separation is moot, hence it is possible that while Vietnam is a Case I country (in terms of our model), Peru is a Case II country.

Outcomes probably also depend on employment and education of husbands. Heise (2011) in a survey of the literature on IPV argues that improving economic empowerment of women may invite more IPV if the man is unable to fulfil his traditional role as a "bread winner." This suggests it may be important to explicitly engage boys and men in the prevention of IPV. Unfortunately our data do not enable us to test this issue in detail.

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Table 1: proportions of respondents types

Y-value	Control counts	Control Proportion (cumul)	Treatment Counts	Treatment Proportion (cumul)	π_{yo}	SE (π_{yo})	π_{y1}	SE (π_{y1})
		Observed data			Estimated proportion of respondent types			
0	27	1.59 (1.59)	30	1.79 (1.79)	0.0179	0.0032	-0.0022	0.0044
1	479	28.19 (29.78)	364	21.77 (23.56)	0.2197	0.0108	0.0622	0.0152
2	980	53.68 (87.46)	976	58.37 (81.94)	0.5216	0.0145	0.0522	0.0124
3	210	11.36 (99,82)	213	12.74 (94.68)	0.0722	0.0097	0.0540	0.0056
4	3	0.18 (100)	88	5.26 (99.94)	0.0017	0.0012	0.0001	0.0006
5			1	0.06 (100)				
Total	1699		1672		0.8331		0.1669	

Notes: *Y-value*: indicates amount of answers respondent agrees with; *Control counts*: how many people in control group agree with *Y-value* answers, where control group refers to group of female confronted with four statements; *Control proportion (Cumul)*: the proportion (cumulative proportion) of female in control group that agrees with *Y-value* answers; *Treatment counts:* how many people in treatment group agree with *Y-value* answers, where *treatment* group refers to group of female confronted with five statements; *Treatment proportion (Cumul)*: the proportion (cumulative proportion) of female in treatment group that agrees with *Y-value* answers.

$$\pi_{y1} = \Pr\left(Y_i \le y \middle| T_i = 0\right) - \Pr(Y_i \le y | T_i = 1),$$
$$\pi_{y0} = \Pr\left(Y_i \le y \middle| T_i = 1\right) - \Pr(Y_i \le y - 1 | T_i = 0)$$

Where *T=1* refers to treatment (five statements) and *T=0* refers to control (4 statements). Thus π_{yo} equals *Treatment Proportion (cumul) Y-value- Control Proportion (cumul) Y-value$_{-1}$* and π_{y1} equals *Control Proportion (cumul) Y-value- Treatment Proportion (cumul) Y-value*. *SE* refers to standard error.

Table 2a: Balance tests control group (not invited to the training)

VARIABLES	(1) Age	(2) Married	(3) Members hh	(4) Region	(5) Credit access TYM	(6) Bargaining power	(7) Agr. activity	(8) Land size	(9) Knowledge	(10) Observational IPV	(11) Profit
ListA	-0.86	-0.02	0.02	-0.02	-0.02	-0.02	0.001	-64.59	-0.03	-0.00	0.55
	(0.55)	(0.02)	(0.08)	(0.02)	(0.03)	(0.07)	(0.02)	(58.47)	(0.08)	(0.02)	(0.38)
Constant	44.55	0.87	4.76	0.29	1.18	4.22	0.82	1,539.60	9.11	0.08	2.26
	(0.47)***	(0.01)***	(0.07)***	(0.05)***	(0.03)***	(0.06)***	(0.03)***	(70.96)***	(0.13)***	(0.02)***	(0.51)***
Observations	1,557	1,557	1,557	1,557	1,556	1,557	1,556	1,557	1,557	1,548	1,557
R-squared	0.002	0.001	0.000	0.001	0.000	0.000	0.000	0.001	0.000	0.000	0.001

Cluster robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. *List* refers to a dummy indicating groups with (1) and without (0) sensitive questions. *Age*: Age of the female TYM member; *Married*: marital status; *Members hh*: household size; *Region*:a regional dummy for Vinh Phuc; *Credit access TYM*: number of loans from TYM; *Bargain:* intra-household bargaining; *Agr. Activity:* a dummy for agricultural activities; Land size: land size in agriculture in acres; *Knowledge*: an index for business knowledge; *Observational IPV:* our observational measure of IPV; *Profit:*inverted hyperbolic profits in the previous month.

Table 2b: Balance tests treatment group (invited to the training)

VARIABLES	(1) Age	(2) Married	(3) Members hh	(4) Region	(5) Credit access TYM	(6) Bargaining power	(7) Agr. Activity	(8) Land size	(9) Knowledge	(10) Observational IPV	(11) Profit
List	-0.01	-0.02	0.01	-0.02	0.01	0.08	0.01	10.97	-0.03	-0.00	-0.10
	(0.45)	(0.02)	(0.08)	(0.02)	(0.03)	(0.06)	(0.02)	(52.43)	(0.08)	(0.01)	(0.34)
Constant	43.79	0.87	4.76	0.28	1.14	4.13	0.79	1,506.78	8.98	0.06	2.78
	(0.49)***	(0.01)***	(0.06)***	(0.05)***	(0.03)***	(0.06)***	(0.03)***	(66.01)***	(0.11)***	(0.01)***	(0.44)***
Observations	1,817	1,817	1,817	1,817	1,817	1,817	1,817	1,817	1,817	1,813	1,817
R-squared	0.000	0.001	0.000	0.000	0.000	0.001	0.000	0.000	0.000	0.000	0.000

Cluster robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. *List* refers to a dummy indicating groups with (1) and without (0) sensitive questions. *Age*: Age of the female TYM member; *Married*: marital status; *Members hh*: household size; *Region*:a regional dummy for Vinh Phuc; *Credit access TYM*: number of loans from TYM; *Bargain:* intra-household bargaining; *Agr. Activity:* a dummy for agricultural activities; Land size: land size in agriculture in acres; *Knowledge*: an index for business knowledge; *Observational IPV:* our observational measure of IPV; *Profit:*inverted hyperbolic profits in the previous month.

Table2c: Balance tests training

VARIABLES	(1) Age	(2) Married	(3) Members hh	(4) Region	(5) Credit access TYM	(6) Bargaining power	(7) Agr. activity	(8) Land size	(9) Knowledge	(10) Observational IPV	(11) Profit
Treatment	-0.38	-0.00	-0.05	-0.01	-0.02	-0.04	-0.01	5.75	-0.10	-0.03	0.07
	(0.54)	(0.01)	(0.08)	(0.07)	(0.04)	(0.08)	(0.04)	(88.36)	(0.16)	(0.02)	(0.59)
Constant	44.04	0.86	4.77	0.27	1.15	4.20	0.82	1,498.83	9.08	0.08	2.64
	(0.36)***	(0.01)***	(0.06)***	(0.05)***	(0.03)***	(0.06)***	(0.03)***	(63.17)***	(0.12)***	(0.01)***	(0.45)***
Observations	3,804	3,805	3,804	3,805	3,804	3,805	3,804	3,805	3,805	3,791	3,805
R-squared	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.001	0.002	0.000

Robust standard errors in parenthese; *** p<0.01, ** p<0.05, * p<0.1. *List* refers to a dummy indicating groups with (1) and without (0) sensitive questions. *Age*: Age of the female TYM member; *Married*: marital status; *Members hh*: household size; *Region*:a regional dummy for Vinh Phuc; *Credit access TYM*: number of loans from TYM; *Bargain:* intra-household bargaining; *Agr. Activity:* a dummy for agricultural activities; Land size: land size in agriculture in acres; *Knowledge*: an index for business knowledge; *Observational IPV:* our observational measure of IPV; *Profit:*inverted hyperbolic profits in the previous month.

Table 3: Determinants of attrition

VARIABLES	(1) Attrition
Training	-0.01
	(0.080)
Age	-0.01
	(0.003)*
Married	-0.16
	(0.090)*
Members hh	-0.03
	(0.020)
Region	-0.19
	(0.124)
Credit access TYM	-0.18
	(0.039)***
Bargaining power	-0.02
	(0.023)
Agr. Activity	-0.04
	(0.082)
Land size	0.00
	(0.000)
Knowledge	0.01
	(0.019)
Observational IPV	0.12
	(0.079)
Profit	0.00
	(0.005)
Constant	-0.44
	(0.250)*
Observations	3,787

Cluster robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. *Training* refers to a dummy indicating groups with (1) and without (0) being invited to the training. *Age*: Age of the female TYM member; *Married*: marital status; *Members hh*: household size; *Region*:a regional dummy for Vinh Phuc; *Credit access TYM*: number of loans from TYM; *Bargaining power:* intra-household bargaining; *Agr. Activity:* a dummy for agricultural activities; Land size: land size in agriculture in acres; *Knowledge*: an index for business knowledge; *Observational IPV:* our observational measure of IPV; *Profit:*inverted hyperbolic profits in the previous month.

Table 4: Gender and business trainings and IPV (List experiment)

VARIABLES	(1) Linear model	(2) Linear model (including extra treatment dummy)	(3) Linear model (attrition probability weighting)	(4) Linear model (including non-Kinh sample)	(5) Non-linear model
	Y	Y	Y	Y	Y
ListA	0.11	0.11	0.12	0.13	-0.21
	(0.040)***	(0.040)***	(0.039)***	(0.040)***	(0.37)***
Training	-0.14	-0.11	-0.14	-0.15	-0.15
	(0.078)*	(0.088)	(0.033)***	(0.074)**	(0.03)***
*Training * ListA*	0.10	0.11	0.09	0.08^+	0.784
	(0.059)*	(0.067)*	(0.051)*	(0.056)	(0.43)*
T2		-0.12			
		(0.094)			
*T2 * ListA*		-0.03			
		(0.090)			
Constant	1.89	1.89	1.88	1.88	-0.11
	(0.060)***	(0.060)***	(0.025)***	(0.059)***	(0.03)***
Observations	3,371	3,371	3,356	3,822	3,371
R-squared	0.018	0.022	0.018	0.020	

Cluster robust standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$, $^+$ $p<0.15$. *Training:* a zero-one dummy indicating who has been invited to follow the training; T2: a zero-one dummy indicating the group of female who has been invited to follow the training without husbands. *ListA:* a dummy indicating which women are confronted with the sensitive statement.

Table 5: Gender and business trainings and IPV (observational data)

VARIABLES	(1) IPV	(2) IPV
Training	-0.04	-0.04
	(0.022)	(0.021)
Age		-0.00
		(0.001)
Married		0.02
		(0.014)
Members hh		-0.00
		(0.002)
Region		-0.06
		(0.015)
Constant	0.08	0.15
	(0.019)	(0.036)
Observations	3,325	3,325
R-squared	0.006	0.022

Cluster robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. *Training* refers to a dummy indicating groups with (1) and without (0) being invited to the training. *Age*: Age of the female TYM member; *Married*: marital status; *Members hh*: household size; *Region*:a regional dummy for Vinh Phuc;

Table 6: Impact of the training on profits and bargaining power

VARIABLES	(1) *Profits*	(2) *Bargaining power*
Training	2.06	0.25
	(0.502)***	(0.103)**
Age	-0.05	0.01
	(0.012)***	(0.003)**
Members hh	-0.09	-0.07
	(0.092)	(0.019)***
Married	0.77	-1.73
	(0.363)**	(0.132)***
Branch dummies	Yes	Yes
Constant	5.40	5.18
	(0.910)***	(0.205)***
Observations	3,368	3,341
R-squared	0.062	0.196

Cluster robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. *Training* refers to a dummy indicating groups with (1) and without (0) being invited to the training. *Age*: Age of the female TYM member; *Married*: marital status; *Members hh*: household size; *Region*: a regional dummy for Vinh Phuc; Branch dummies refer to branch dummies.

Table 7: Profits and IPV

VARIABLES	Non-linear model Y	Linear model Y
ListA	-1.60	0.18
	(1.14)	(0.143)
Profits	0.00942	0.01
	(0.0028)***	(0.004)**
Bargaining power	0.01531	0.02
	(0.01161)	(0.015)
Profits * ListA	0.078	0.01
	(0.043)*	(0.004)**
Bargaining * ListA	-0.0941	-0.02
	(0.12102)	(0.015)
Controls	Yes	Yes
Controls * ListA	Yes	Yes
Constant	-0.40	1.61
	(0.098)***	(0.108)***
Observations	3329	3,329
R-squared		0.030

Cluster robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1; *Bargaining power:* intra-household bargaining; *Profit:* inverted hyperbolic profits in the previous month. *ListA:* a dummy indicating which women are confronted with the sensitive statement.

Appendix A

The training program consists of nine training modules: see Table A1 for an overview. Three modules focus on gender issues: (1) gender beliefs, roles, prejudice, and gender equality; (2) female entrepreneurs' business skills, confidence, and identifying successful business goals and (3) difficulties and challenges for women in doing business and setting up a cooperation. The six remaining modules focused on business-related topics: (4) identifying and selecting business ideas and opportunities; (5) the importance of product, price, promotion, and place in marketing and business sales; (6) calculating interest rates and the possibility of saving; (7) opening and managing cash books; (8) managing account receivable and account payable books and (9) calculating purchasing and production costs and costs of sold goods.

Table A1. Modules of the GET Ahead Training package

Module 1: Gender and gender equality
Module 2: The business woman and her self-confidence
Module 3: The business woman and her environment
Module 4: The business project: Business ideas
Module 5: The business project: Marketing and how to sell with success
Module 6: The business project-finance: Calculations and how to calculate interest rate
Module 7: The business project-finance: Managing cash
Module 8: The business project-finance: How to record accounts receivable and accounts payable
Module 9: The business project: How to calculate costs of production and cost of goods sold

Appendix B

Intermediate variables used in the analysis:

   1) <u>Bargaining power:</u>

The bargaining power index is taken from Huis et al. (2017). They constructed a bargaining power index for decision making on large and small expenditures. We took the index for large expenditures, which is constructed by summing scores on 7 questions (answered by women):

  Who makes most decisions about asking for a loan?

  Who makes most decisions about consumer durable items? (TV, Fridge, Tape recorder etc.)

  Who makes most decisions about what health expenditures to make?

  Who makes most decisions about saving for business and for household?

  Who makes most decisions about expenses for home purchase, improvement or repair?

  Who makes decisions about where to invest surplus money?

  Who makes decisions about how to assist family members?

Huis et al. (2017) assigned 0 points for each decision made by the husband, 0.5 points for each decision made by the couple together, and 1 point for each decision made by the wife, then summed these points for all relevant items.

   2) <u>Profits</u>

The indicator for profits is taken from Bulte et al. (2017), who distinguish between various indicators for profits. We used the inverse hyperbolic sine transformation of last month total profits, which attenuates the importance of outliers and accommodates zero-responses (for details, see Bulte et al. 2017).