Household Migration and Expenditure Decisions

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Abstract

We identify the causal relationship between male head of the household migration and reallocation of decision-making power. Furthermore, we look at the effect of this reallocation on children's welfare. The main econometric model is a difference-in-difference that relies on randomly timed migration spells in order to identify a causal effect. Overall we find that decision-making power over expenditures shifts towards women during their husbands' migration spells, but there is no corresponding change in budget shares. This does not support the commonly cited idea that women invest more in children than do men. These results are relevant to development programs that target female beneficiaries.

Key words: migration, decision-making, bargaining power, household economics, gender

JEL classification: O150, D130, F220, J160

The income benefits of out-migrant work in developing countries are well established. However, much less is known about changes in women's empowerment that might occur during spousal migration spells. Two main questions motivate this research. First, during a spouse's absence, does a woman gain more control over household resources? Second, does this shift in power affect resources allocated to children, indicating that women have different preferences for children's welfare than do their husbands? In this paper we identify causally each step in this sequence using unique panel data from rural Nepal, a country known for high rates of male migrant work.

This research makes contributions to two bodies of literature, the first being the migration literature. The majority of the past migration research has been quite narrow in scope, covering mainly how migrant households use remittances.[1] Consequently, any discussion

on the benefits of migration often centers on remittances with little regard to other potential sources of economic development. This research is a critical addition to the migration literature, as it unlocks more discussion on the effects of migration holding economic resources constant.

Second, this paper contributes to the intersection between intra-household decision-making and international development literature. Many development programs give cash or in-kind transfers to women in poor families, motivated by the common belief that women spend resources in a more pro-child way than do men. This makes sense theoretically: if household members have unique preferences and bargain over control of household resources, then the effectiveness of a transfer depends on the recipient's preferences and bargaining power. The common assumption is that women have less bargaining power and stronger preferences for children's welfare than men. Yet the literature on this issue is not as conclusive as this commonly-held belief would suggest[2]. As development programs continue to choose beneficiaries, more research in this area is certainly warranted in order to maximize effectiveness of transfers. Furthermore, we might expect that outcomes vary with local context, perhaps due to cultural differences. Therefore, evaluating this claim in rural Nepal provides useful policy implications for programs focused in rural South-East Asia.

Using 2014-2016 data from Nepal and a difference-in-difference model, we identify the causal relationship between temporary migration of the male head of the household, intra-household decision-making, and children's well-being. There are several unique benefits to using this dataset. First, the data cover both household expenditures and intra-household decision-making control over various expenditure categories, some of which represent investments in children. These data make up the outcome variables. Second, the panel nature of these data allows for household fixed effects in our first DD model. This is a vital aspect of our identification strategy as fixed effects control for time-constant factors that might cause selection into migration spells, the main threat to causal inference that we face. In

addition, we are able to control for a wide range of control variables such as household composition, income, and education levels. If we assume that there are no unobservable, time-varying omitted variables, we can interpret these estimates as causal. This is not a strong assumption, considering that we limit the sample to households where the husband migrated during at least one time period. In other words, we compare past, current, and future migrant households to each other: making the treatment and control groups similar in their propensity to migrate. Thus the crux of the identification strategy relies on the exogenous timing of migration spells, rather than an exogenous decision to migrate in the first place. Using the same identification strategy, we also specify a different DD model that parses out the "return" and "leave" effects of migration. This allows us to evaluate whether or not the initial changes due to a husband's migration spell persist after the spell is complete.

Results show that women gain substantial decision-making power over expenditures during their husbands' migration spells. However, there is no corresponding change in expenditures allocated to children. This suggests that the link between women's empowerment and children's welfare might be overstated, at least in the rural Nepal context. These results are robust to different sample specifications.

We begin by describing how this research contributes to the current literature, then we outline our data, theoretical model, and empirical strategy. Finally, we discuss our main results and robustness checks before concluding with ideas for future research.

Contribution to the Literature

We seek to answer two main questions. First, does control of expenditures shift from a husband to his wife when he temporarily migrates? Second, if so, what effect does this shift have on resources spent on children? This interdisciplinary thesis contributes to the household decision-making literature, the migration literature, and the small intersection of the two topics.

For over 20 years now, economists have been skeptical of household decision-making models that assume common preferences and unitary action among household members (Doss, 1996). Some empirical questions relevant to policy are an outgrowth of this skepticism. If household members have unique preferences, and if household decisions are a result of a bargaining game, then what determines bargaining power between members? Do income-earning household members pool their income or maintain separate spheres of income in order to pursue their own ends? Do household decisions result in Pareto-optimal outcomes for all members? Economists have developed several household decision-making models in the spirit of answering these questions (Lundberg and Pollak, 1994; Phipps and Burton, 1998; Pollak, 2005; Browning et al., 2010).

Most decision-making models are focused on the causes and effects of intra-household bargaining power. These models assume a game theoretic approach to the way husbands and wives arrive at decisions. In general, a spouse has more bargaining power if they have less to lose upon separation. Thus, wealth, earned wage rate, or productivity often determines the bargaining power distribution between spouses (Pollak, 2005; Friedberg and Webb, 2006; Asim, 2009; Swain and Wallentin, 2009; Aboukhsaiwan, 2014). The prevalence of these models might suggest that the only way to vary household decisions is to vary bargaining power. However, as Ashraf (2009) points out, decision-making might vary with asymmetric information, communication, and other factors that these models often omit or assume away. In other words, these bargaining models are too simple to answer the general question: how do households arrive at decisions and how are members' preferences represented in these decisions?

This question is relevant to international development because many programs give cash or in-kind transfers to poor families. If household members have unique preferences and intra-household decision-making power varies, then the effectiveness of the transfer depends on this variation. Specifically, there is speculation that increasing women's decision-making power in a household results in pro-development outcomes, as some contend that

women have stronger pro-child preferences than men do. This motivates many programs to target female beneficiaries (Duflo, 2012). The literature breaks down into several main camps regarding this assertion. First, some results are consistent with the theory that women tend to invest more in children's well-being (Thomas, 1990; Hoddinott and Haddad, 1995; Handa, 1996; Rubalcava et al., 2002; Pitt et al., 2003; Gitter and Barham, 2008; Bobonis, 2009; Dunbar et al., 2013). Other literature confirms that women and men do indeed spend resources differently, but it is not as simple as a differences in child investments overall: for example, some papers find that women tend to invest more in their daughters than their sons.[3] (Thomas, 1990; Thomas, 1994; Duflo, 2003; Rangel, 2006; Emerson and Souza, 2007; Antman, 2012; Chen, 2012; Antman, 2015). In contrast, some empirical evidence suggests that there is no systematic difference in the way that men and women spend resources on children (Akresh et al., 2012; Braido et al., 2012; Cherchye et al., 2012; Benhassine et al., 2013; Haushofer and Shapiro, 2016). In short, there is no obvious conclusion in the literature about whether or not women invest in children more than men do.[4]

One possible source of variation in household decision-making that few have studied is the temporary migration of a primary household decision-maker. Migrant spells may introduce asymmetric information and imperfect monitoring between decision-makers, therefore affecting household decisions (Chen, 2006; Ashraf, 2009; Chen, 2012; Chen, 2013). Yet the migration literature has primarily centered on the effects of remittances rather than identifying the effects of migration holding income and wealth constant.[5] Most of this literature argues that migration reduces poverty in developing countries or has positive effects on certain development goals such as increasing savings (Adams and Page, 2005; Osili, 2007; Rapoport and Docquier, 2006; Beegle et al., 2011). Specifically, some papers have pointed out that migration-source households tend to spend remittances on education, one particularly important development goal (Edwards and Ureta, 2003; Yang, 2008; Adams and Cuecuecha, 2010). Yet many papers have countered this positive perspective on migration by claiming that migration hurts children's education (Amuedo-Dorantes et al., 2008;

De Brauw and Giles, 2008; Antman, 2011; Grigorian and Melkonyan, 2011; McKenzie and Rapoport, 2011; Cortes, 2015). Cortes (2015) reconciles these contradictory results by expressing children's education as a function of remittances and parental input. She argues that while remittances might positively affect education, the lack of parental input due to migration might negatively affect education. Her model and results confirm that holding remittances constant, there are other interesting effects of migration to study. Perhaps one such effect is shifts in intra-household decision-making.

If this connection between migration spells and decision-making power shifts exists, there might also be variation in household decisions. Thus, spousal migration may provide useful variation that economists can use to evaluate if and how men and women invest differently in children. There is a small literature on this connection between migration spells, shifts in decision-making power, and child-related outcomes. To our knowledge, Chen (2006) is the first to suggest that migration could change intra-household decision-making power. Chen (2013) goes on to investigate this claim empirically in China. She finds results consistent with an asymmetric information model: when a male migrates, he cannot perfectly monitor his spouse's decisions. Thus, the migrant's wife increases her leisure time and increases her children's participation in household work in order to maintain constant household production. Furthermore, children's caloric intake increases while children's Body Mass Index remains constant. Chen theorizes that this is either so the migrant's wife can hide the increase in child labor or because the couple shares common "child quality/health" preferences. Interestingly, Chen sees no changes in human capital and thus assumes that intra-household bargaining power stayed constant. In short, women appear to adjust along margins that their husbands cannot detect during migration spells and otherwise keep household outcomes constant. This matches the predictions from Chen's household decision-making model where information asymmetries exist between spouses during migration spells.

Antman (2015) adds to Chen's work by looking at spousal migration spells in Mexico and expenditure shares between boys and girls. She finds that before migration, expenditure shares are at parity. During migration of the male head of the household, the shares shift in favor of girls; upon his return, they shift in favor of boys. She also sees an increase in decision-making power over expenditures for the migrants' wives during migration, with a shift back towards the husband when he returns. Unlike Chen, Antman does not rely on a theoretical model of decision-making, nor does she speak to the mechanisms at work behind the shift in decision-making power. It is unclear why decision-making power shifts towards migrant wives–it could be representative of either a shift in bargaining power or wives' non-cooperative behavior due to imperfect monitoring. Furthermore, the evidence for a shift in decision-making power causing expenditure shares to change is only suggestive. Antman's results do not prove the causal chain of events that male migration spells cause wives to increase their control over expenditure decisions, which then causes a change in expenditure shares. Rather, she only proves a "commensurate" change in who makes decisions and what decisions are made. The main conclusion from her research is that mothers tend to favor girls in expenditures and fathers tend to favor boys, so much so that they are willing to overcompensate for any expenditure shift away from boys that happened during the migration spell. Antman argues that the mechanism through which this pattern occurs is shifts in decision-making power.

This research uses Chen's initial insight to look at how decision-making might change during spousal migration spells. Furthermore, it builds on and extends Antman's research on Mexican households, as we look at similar outcome variables in a Nepali context. In this way, we contribute to the small and growing literature on the effects of migration holding income constant. This research also adds to the body of evidence on differences in the way men and women invest in children. Ultimately, this paper yields insight for development policies that target women beneficiaries.

Theory

While past literature suggests that migration spells of a spouse may induce information asymmetries or shifts in decision-making power, there are few formal theories off of which to build in this research (Ashraf, 2009; Chen, 2013; Antman, 2015). Neither Antman (2015) nor Ashraf (2009) include a theoretical model. On the other hand, Chen (2013) uses an intra-household bargaining model to outline the role of information asymmetries in household division of labor and child quality. She shows that the equilibrium might be non-cooperative during migration spells, which introduces asymmetric information between the couple. We use a simpler and broader model to investigate how migration spells affect decision-making power, and consequently, how a shift in decision-making power might affect investments in children.

First consider each spouses' individual utility maximization problem independently. If his or her spouse is uninvolved in resource allocation decisions, an actor will choose how many of the total resources, R, to allocate to either children (C) or other household members (T):

$$\begin{aligned} &\underset{T,C}{\text{maximize}} && U(T,C) \\ &\text{subject to} && T+C=R \end{aligned}$$

The optimal choices of T and C that result are functions of the only parameter in the problem, R:

$$(1) \quad \bar{T} = f(R)$$

$$(2) \quad \bar{C} = g(R)$$

Yet in reality, the spouse's preferences affect the actor's decision. One way to incorporate this into the model is by attaching a price to T and C that captures information about the spouse's preferences and their influence. Let the price be p_i, where $i = T, C$:

$$(3) \quad p_W e = f(D_{jk}, \bar{i}_{jk})$$

The price thus depends on the actor's (j) and spouse's (k) relative decision-making power (D_{jk}) and their relative utility-maximizing choices of either T or C ($\bar{i}_{jk}$). Assuming only k migrates, the relative decision-making power is a function of several variables:

$$(4) \quad D_{jk} = f(B_j(M_k), P_j, B_k(M_k), P_k(M_k))$$

If decision-making power is broadly defined as the amount of influence somebody has over a decision, then we see how it can depend on both bargaining power (B) and physical presence (P) as two distinct factors. On the one hand, bargaining power can increase the amount of influence somebody has over a joint decision. On the other hand, physical presence could affect information or communication availability, thus affecting decision-making power. Both of these factors are functions of the spouse's migration status, M_k. The intra-household bargaining power literature often points towards relative wages being the main source of variation in power between men and women. If this is true, and if a wage premium motivates migration spells, then migration could cause bargaining power to change. It might also be the case that during migration spells of one spouse, the other substitutes time from household production to wage labor, which might also affect bargaining power. Furthermore, migration spells mean that the migrant is physically distant for some amount of time, which could hinder their influence over decisions. For example, migrants might willingly lend decision-making power to their spouses during their absence due to

communication costs. It is also possible that spouses left behind act non-cooperatively and gain decision-making power simply because the migrant cannot monitor their actions.

We are interested in identifying the sign of $\frac{\partial D_{jk}}{\partial M_k}$ in order to answer the first research question: how does migration affect decision-making power? The total differential of Equation 4 with respect to M_k yields:

$$(5) \quad \frac{\partial D_{jk}}{\partial M_k} = \frac{\partial D_{jk}}{\partial B_j}\frac{dB_j}{dM_k} + \frac{\partial D_{jk}}{\partial B_k}\frac{dB_k}{dM_k} + \frac{\partial D_{jk}}{\partial P_k}\frac{dP_k}{dM_k}$$

Thus we see what we can and cannot identify empirically here. For example, if we find that $\frac{\partial D_{jk}}{\partial M_k}$ is positive, then it will be difficult to know why this is the case. It will be unclear whether a change is due to bargaining power or physical presence. However, we will be able to identify directly the net effect of the right-hand side of Equation 5, thus obtaining $\frac{\partial D_{jk}}{\partial M_k}$ in a reduced-form way.

In order to obtain the second testable hypothesis, we demonstrate how the actor maximizes utility by choosing the resource distribution between children and others subject to a resource constraint that incorporates p_i from Equation 3. The relevant utility-maximization problem is:

$$\begin{aligned} &\underset{T,C}{\text{maximize}} && U(T,C) \\ &\text{subject to} && p_T T + p_C C = R \end{aligned}$$

In other words, when the spouse migrates, the actor chooses a resource allocation that maximizes utility and satisfies a resource constraint that includes "prices" assigned to T and C. To solve this optimization problem, we begin by normalizing p_T to one. Also, we assume $U(T,C)$ is quasi-concave.[6] These assumptions simplify the utility maximization problem with no loss of generality. The Lagrangian and first-order conditions (FOC) are

$$\mathscr{L} : U(T,C) + \lambda(R - T - p_C C) \tag{6}$$

$$\begin{aligned} \mathscr{L}_T &: U_T - \lambda = 0 \\ \mathscr{L}_C &: U_C - p_C \lambda = 0 \\ \mathscr{L}_\lambda &: R - T - p_C C = 0 \end{aligned} \tag{7}$$

Because $U(T,C)$ is quasi-concave, the second-order conditions are met[7] and we can solve the FOC for the optimal resource allocation and shadow price in terms of the problem's parameters[8]:

$$\begin{aligned} C^* &= f(p_C, R) \\ T^* &= f(p_C, R) \\ \lambda^* &= f(p_C, R) \end{aligned} \tag{8}$$

Recall that the second research question is: how does the amount of resources allocated to children change due to a shift in decision-making power? Mathematically, this is the comparative static $\frac{\partial C^*}{\partial D_{jk}}$. Recognizing that p_C is a function of D_{jk} and $\bar{C}_{jk}$, we know that:

$$\frac{\partial C^*}{\partial D_{jk}} = \frac{\partial C^*}{\partial p_C} \frac{\partial p_C}{\partial D_{jk}} \tag{9}$$

If we assume that children's resources is a normal good—in other words, that $\frac{\partial C^*}{\partial p_C}$ is negative—then the sign of $\frac{\partial C^*}{\partial D_{jk}}$ relies on the sign of $\frac{\partial p_C}{\partial D_{jk}}$. Thus the theoretical model informs us that signing $\frac{\partial C^*}{\partial D_{jk}}$ boils down to how the "price" of C changes upon a shift in decision-making power. Empirically, if we see that $\frac{\partial C^*}{\partial D_{jk}}$ is positive, then we know that $\frac{\partial p_C}{\partial D_{jk}}$ is negative; if $\frac{\partial C^*}{\partial D_{jk}}$ is negative, then $\frac{\partial p_C}{\partial D_{jk}}$ must be positive. This shows how this research empirically gets at the hypothesis that women allocate resources in a more pro-child way

than men. If we assume that children's resource allocation is a normal good, then how resources change when decision-making power changes depends on women's preferences, which are integrated into p_C. If $\frac{\partial C^*}{\partial D_{jk}} \neq 0$, we accept this hypothesis and if not we reject.

In short, this research attempts to identify empirically the effect of male migration on decision-making power ($\frac{\partial D_{jk}}{\partial M_k}$) and any subsequent effect on children's welfare ($\frac{\partial C^*}{\partial D_{jk}}$). This section outlined the possible factors at work behind these changes. Also, this theoretical framework is helpful in outlining precisely what we can identify empirically given the data and empirical strategy, which we discuss in the following two sections.

Data

Data Source

These data are from a Randomized Control Trial evaluating Heifer International's livestock transfer program's impact on development in Nepal and make up a two-period panel (2014-2016). The entire sample includes nearly 3,000 households, and respondents were almost always female decision-makers. After dropping households that only surveyed at baseline and those without a married female respondent in both time periods, our full sample is about 2,400 households.[9]

The data contain a variety of household-level information, including household income, expenditures, exposure to idiosyncratic shocks, exposure to the 2015 earthquake and embargo shocks[10], and members' ages, sex, residency, and education.

This dataset also contains rich information on intra-household distribution of control. In particular, there are data on intra-household ownership of productive assets and decision-making regarding expenditures and livestock. Furthermore, there are data on children's welfare. Regarding education, there is individual-level information on each child's enrollment and recent attendance. The dataset also includes dietary diversity information, an indicator of children's health.

Tables 1-2 describe household characteristics. First, we summarize these statistics for the full sample. Next, we break these statistics down by whether or not the respondent's husband is migrating.[11] we also include summary statistics on decision-making over expenditure categories and corresponding expenditures broken down in the same way. Specifically, Table 3 describes the fraction of expenditures of a particular category over which the spouse (potential migrant) and the respondent (his wife) have input. For example, we can see that for households where the husband is currently migrating, the respondent solely makes expenditure decisions for a quarter of temptation goods. Table 4 covers annual budget shares and expenditure levels (in Rupees) across the same expenditure categories. In order to get a sense for how different these households are, we include a column indicating the difference and its significance level.

In general, we see that migrant households have slightly higher incomes, smaller households with more children, are more likely to live in a joint household with parent-in-laws, less likely to live with their daughter-in-law[12], and are more educated on average. Also, the spouse tends to have more control over expenditures in households where he is not currently migrating. This is true for all expenditure categories. Finally, we see that non-migrant households tend to spend less on temptation goods and formal health, while they spend more on educationa and children's clothing. Most of the variation here comes from budget shares rather than expenditure levels.

2015 Earthquake and Embargo

Given that our data are from the years 2014 and 2016, two historical events are relevant. First, a 7.8-magnitude earthquake hit Nepal on April 25, 2015 (Barry, 2015). This devastating natural disaster and its violent aftershocks heavily impacted two of the districts in our sample—Dhading and Nuwakot. Furthermore, a new, controversial constitution was implemented in September 2015. Many protests erupted through Nepal as a result, includ-

ing a Indian fuel blockade that took place September 2015-February 2016. Allegedly, a specific ethnic minority from India felt underrepresented in the new constitution, and in protest, blocked off the Indian supply of fuel to Nepal. This caused severe shortages in fuel and as a result, fuel prices rose and became an economic crisis (Haviland, 2015; Pokharel, 2015).

Fortunately, the data contain a variety of measures for both shocks. For example, at the household level, there are data on how the house was physically damaged in the earthquake, if at all. There are also data on how the house coped with any negative effects of the embargo. Furthermore, the data include information from a ward survey that was administered to a prominent community member in each ward. The survey asked respondents to recall prices on a variety of common household goods before, during, and after the embargo. Finally, there is a VDC-level measure of the Mercalli Scale from an outside data source, the USGS Earthquake Hazards Program. While these data are at a more aggregate level, they may be more accurate since they assign Mercalli values to areas based on a ShakeMap rather than peoples' recall testimonies.[13]

While the household-level data are more precise insofar as the data capture between-household variation, the ward- and VDC-level data might give more precise measures of the shocks. Coming up with a measure for magnitude at the household-level data would require creating some sort of index based on either categorical or binary data. Thus we use what we believe is most accurate, then we check that results are not driven by this particular measure of the earthquake.

Empirical Strategy

First consider the ideal experiment. Ideally, we would randomly assign non-migrant male heads of households to migrant work. Given the random assignment and assuming no shocks occur unevenly across the sample, we would measure the causal effect of male migrant work on their spouses' decision-making power and resources allocated to children

by simply comparing means between the treatment and control households. This estimate would measure the effect of a first-time migrant's absence on these outcomes.

However, since our data include households at various stages of their migration spells who may or may not have migrated before the panel began, the causal estimate that we obtain speaks to a slightly different effect. In order to motivate this, consider a modified ideal experiment where we would randomly assign migration spells starting and ending at random times to male heads of the household. Data collection after random assignment would catch some households in the middle of and some after their migration spells. Comparing their means to the control group's would yield estimates for the causal effects of a migrant's absence and his return.

This discussion motivates the naive regression. In order to compare households at various stages of their migration spells in a difference-in-difference setting, the specification would look like:

$$(10) \quad Y_{it} = \beta_0 + \beta_1 CurrentMigrant_{it} + \beta_2 y2016 + \overrightarrow{X}'_{it}\alpha + \delta_W e + \varepsilon_{it}$$

where Y_{it} is either women's decision-making power in a household (W_{it}) or resources assigned to children (C_{it}) for household i in period t. $CurrentMigrant_{it}$ is a dummy for if the woman's spouse in household i is migrating in time t, and $y2016$ is a binary variable for being in the second time period (t=2). $\overrightarrow{X}_{it}$ is a vector of time-varying controls, and δ_i represents household fixed effects. The control group here is any i where the respondent's husband is not migrating in time t ($CurrentMigrant_{it}$=0), while the treatment group is any i during a migration spell. Econometrically, these groups are represented by β_0 and β_1, respectively. In other words, β_0 is the average Y for for all households where the husband is not currently migrating; β_1 is the change in Y between households where the husband is currently migrating and those where he is not, regardless of the order in which this happens between time periods.[14] The difference between the differences of these two groups in Y is

β_1. Note that this is lumping both households who never experience migration and households who are not *currently* experiencing migration into the same control group. Likewise, the treatment group includes households with migration spells only in 2014, only in 2016, and in both time periods. Thus, β_1 averages over those who migrated in just 2014 or just 2016 in comparing their differences over time to the control group.[15] Unlike the modified ideal experiment, this naive regression averages over the magnitude of the return and absence effects.

Equation 10 is naive because male migration was not randomly assigned to Nepali households. In fact, husbands select into migrant work based on a variety of factors. Their reasons for making certain decisions can be anything from risk preferences (time-constant) to a response to an economic shock (time-varying). Luckily, household fixed effects will control for any time constant factors. Yet if any time-varying factors are also related to either outcome of interest and are unobserved in the data, then a simple difference-in-difference effect estimate is endogenous. For example, non-migrant households could be prone to more (or less resiliant to) economic shocks than migrant households. If these shocks affect the decision to migrate and also women's decision-making power or resources allocated to children, then we cannot interpret the results as causal. This warrants a more robust identification strategy.

In the next section we lay out two different empirical strategies that will better identify a causal effect than equation 10. In general, both approaches maintain the intuition behind a difference-in-difference model and control for all time-constant sources of endogeneity.

Specification 1: FE Model

The equation in the first DD approach is the same as Equation 10. The difference is that we limit the sample to only households where the husband migrated during either time period, thus reducing the identifying assumptions required to obtain a causal effect. By limiting the sample in this way, the model now relies on assuming the exogenous timing of migration

spells. This circumvents assuming that the household's decision for the husband to migrate at all is exogenous. Also, it ensures more similarities between treatment and control households: presumably households that migrate at some point are similar in their propensity to migrate, only varying in the timing of their migration spells. Recall that the variable of interest is $CurrentMigrant_{it}$, a dummy variable for whether or not the respondent's husband in household i is currently migrating during time t. The equation is:

$$(11) \quad Y_{it} = \beta_0 + \beta_1 CurrentMigrant_{it} + \beta_2 y2016_t + \overrightarrow{X}'_{it}\alpha + \delta_W e + \varepsilon_{it}$$

where Y_{it} is either W_{it} or C_{it}. Note that $y2016_t$ is a dummy variable for time, and t equals one for year 2014 and two for year 2016. $\overrightarrow{X}_{it}$ is a vector of time-varying controls, and δ_i represent household fixed effects. Controls include variables for household composition[16], the natural log of total household income (including remittances)[17], the number of shocks that the household encountered over the past two years[18], the respondent's and husband's years of completed education, the household's treatment status from the randomized control trial, a dummy for if the survey respondents were different between the first and second time period[19], the respondent's age, the Modified Mercalli Scale measure of earthquake intensity,[20] and the log of oil, sugar, and salt prices as a measure of embargo intensity.[21] Thus Equation 11 is immune to observed time-varying bias and all time-constant bias, specifically bias that affects households where the husband migrates in at least one time period. As alluded to earlier, the control group here is any household i where the respondent's husband is not migrating in time t ($CurrentMigrant_{it}$=0). Thus β_0 is still the average Y for for all households where the husband is not currently migrating, but it is limited to households where the husband does migrate at some point. Also similar to the niave regression, β_1 is the change in Y between households where the husband is currently migrating and those where he is not, regardless of the order in which this happens between time periods.

We use a strategy similar to Antman's to show the causal chain of migration on intra-household allocation of control and ownership, and its effect on resources allocated to children (2015). That is, we use Equation 11 for both outcome variables. If β_1 is statistically different from zero for both W and C, then like Antman, we can suggest that male migrant work changes W which changes C.[22] Yet this is only suggestive; while we can directly identify $\frac{\partial D_{jk}}{\partial M_k}$ using W as the outcome variable, we cannot directly identify $\frac{\partial C^*}{\partial D_{jk}}$. Instead, when we use C as an outcome variable, we are identifying $\frac{\partial C^*}{\partial M_k}$. If we are holding all other variables constant that could be related to migration and resources allocated to children, such as total household resources, then a coefficient for $\frac{\partial C^*}{\partial M_k}$ is just as informative as $\frac{\partial C^*}{\partial D_{jk}}$.[23] Thus, not only are controls essential for omitted variable bias concerns, but also to ensure that variation in C is working through the causal chain outlined in the theoretical model.

One main problem with this FE model motivates the second empirical strategy. This specification averages over households with a husband migrating in both periods and in only one period (2014 or 2016). This may overlook interesting effects of migration. For example, if migration spells have a permanent effect on women's decision-making power, we would not see an opposite change in decision-making power for the migrant group that comes home by 2016. This model will overlook any such pattern. Thus, we explore a second identification strategy and empirical model in the proceeding section.

Specification 2: FD Model

In this approach, we also compare migrant households to other migrant households, and again we rely on the randomness in the timing of migration spells in order to interpret our estimates as causal. The variables of interest here are $Migrant2014_i$ and $Migrant2016_i$, dummy variables for whether or not the respondent's husband in household i migrated during (only) 2014 or (only) 2016. The equation is

$$(12) \quad \Delta Y_i = \beta_0 + \beta_1 Migrant2014_i + \beta_2 Migrant2016_i + \beta_3 MigrantNeither_i + \Delta \overrightarrow{X}_i' \pi + u_i$$

where Y_i is either W_i or C_i, and $\Delta\overrightarrow{X}_i$ is a vector of controls that vary over time for households. Note that $MigrantNeither_i$ is a dummy variable equal to one if the respondent's husband in household i migrates in neither 2014 nor 2016. As in Specification 1, we use both W_i and C_i as outcome variables in order to identify the causal chain of migration on intra-household allocation of control and ownership, and subsequently, its effect on children's well-being.[24]

Like the FE model, this specification identifies a causal effect by assuming the timing of migration spells is random as it compares households with migrant husbands to each other. It does so without limiting the sample, and it has the benefit of parsing out the effect of a migrant's return and his leaving. Specifically, this FD model compares differences over time for returned-migrant households ($Migrant2014_i$) and sent-migrant households ($Migrant2016_i$) to households where the migrant was gone in both 2014 and 2016, the dummy variable base-group (β_0). Thus, β_1 represents the change in Y over time for the $Migrant2014$ group relative to the change in Y over time for households where the husband migrated in both time periods. Likewise, β_2 represents the change in Y over time for the $Migrant2016$ group relative to the same base group. The model also controls for households where the husband does not migrate in either period ($MigrantNeither_i$). Presumably, households that migrate in both or either time period are similar to each other—the only difference being the timing of their migration spells. Of course, if they are similar in ways that are time-constant, this model makes no difference; the naive FE model would have sufficiently controlled for all time-constant factors. However, in light of the discussion on shocks, perhaps these groups will be similar in their exposure to unobserved shocks. In other words, perhaps the control group here is a better counterfactual than the control group in the naive FE model, which is in both cases represented by β_0.

While the FD model is more robust to potential sources of bias, the naive FE model has the advantage of answering a broad question: if we randomly assign male migration to households, do power structures in a household change? If so, do resources allocated to

children also change? In contrast, the FD model (like the FE model) narrows the population in question to migrant households. Then the question becomes: if we randomly assign male migration spells to migrant households, how do power structures and children's share of resources change? Unfortunately, this is less relevant to migration policy questions. It also narrows the external validity to migrant households. Furthermore, the model assumes that the difference in outcomes over time for households where the husband migrated in both periods is approximately zero in order for any observed effect to be interpreted with ease. For example, women might gain more decision-making power over expenditures over time rather than instantly as a binary response to a change in their husband's migration status. If this is the case, then interpreting β_1 and β_2 *relative to the base group,* those who are migrating in both periods, becomes especially important. In this example, a small, positive change in decision-making power for the $Migrant2016_i$ would result in a negative β_2. While this would not invalidate the identification strategy, it would make interpretation more difficult. Moreover, it would show that the outcome variables might not change in a binary way with migration status, and that our empirical strategy fails to investigate this interesting variation. Unfortunately, we do not have data on the length of migrant terms and therefore cannot explore this possibility further. Finally, while it is interesting to parse out the "return" and "leave" effects of a migrant's presence here–the magnitudes might be different–it is mostly exploratory. This is because the year 2014 may not be comparable to 2016, so it could be fallacious to interpret differences in magnitudes as demonstrating permanent changes in W or C. Nevertheless, we report results from the naive FE, FE, and FD models in the next section.

Main Results

Recall that outcome variables include both women's ownership or decision-making power in a household (W) and children's share of household resources (C). We begin by presenting

the first part of the causal chain: the effect of migration on W. Next we discuss the effect of migration on C. In the following section we discuss robustness checks.

We present results from the naive FE (Eq. 10), FE (Eq. 11), and FD models (Eq. 12) for each outcome variable. (Refer to the previous section for a discussion on the differences between each model.) Before going through the results, it is instructive to look at a table with example results. We can interpret a positive ${\beta_1}^{FE}$ and ${\beta_2}^{FD}$ as in Table **??** as a positive change in the outcome variable attributable to migration spells; after a husband's migration spell, the outcome variable increased. Specifically, ${\beta_1}^{FE}$ is the difference-in-difference between households with and without a husband who is currently migrating. Similarly, ${\beta_2}^{FD}$ is the change over time for households where the husband was home in 2014 and left by 2016 relative to the change for households where the husband migrated in both 2014 and 2016. We refer to this as the "leave effect." On the other hand, ${\beta_1}^{FD}$ is the "return effect." It is the difference over time in the outcome variable for households where the husband was migrating during 2014 but returned by 2016 relative to the difference seen by households where the husband was migrating in both time periods.

In general we find that migration spells cause shifts in power towards women with no clear accompanying shift in children's share of resources. Statistically significant results on resources allocated to children are sparse and sensitive to model specification.

Women's Decision-making Power

We have data on the spouse's (the potential migrant) and respondent's (the potential migrant's wife) degree of involvement in decisions regarding expenditures on temptation goods (alcohol, tobacco, and pipe-smoking), formal health care, items for ceremonies or celebrations, children's education, children's clothing, and adult women's clothing. We specify W as the fraction of decisions made where either (1) the spouse alone makes the decision, (2) the spouse and respondent make the decision jointly, (3) the respondent alone makes the decision, or (4) the respondent is involved in the decision with somebody other

than her spouse.[25] we aggregated the decision-making indicators in this way in order to observe all interesting variation: upon migration, do decisions made solely by the spouse become joint, or does the respondent gain full responsibility? If joint, does the respondent share power with the migrant spouse or with somebody else in the households? Or do decisions move from joint between the spouse and respondent to solely in the hands of the respondent? Separating the columns like this captures all such changes.

Tables 5-10 show that the respondent's decision-making involvement increases during her husband's migration spells while his involvement decreases. This general pattern is remarkably consistent in all expenditure categories. For example, in the fixed effects model, the fraction of temptation goods expenditure decisions where the respondent alone makes decisions increases by 0.17 when her spouse migrates; this is about 81% of the mean. Effect sizes of the respondent's gain in decision-making power across categories range from approximately 32% to 81%. If we assume away unobserved time-varying omitted variables, we can interpret these changes as causal. Indeed, when men migrate, their wives gain a significant amount of decision-making power in the household. The FD model, which parses out the "return" and "leave" effects, confirms this pattern with minor loss of significance. As discussed previously, this model has an advantage over the FE model by revealing that the change in decision-making power appears to be a temporary shift: when the husband returns, women lose some decision-making power. While there are some differences in magnitude for the "return" and "leave" effects, the p-value for the F-Test $|\beta_1{}^{FD}| = |\beta_2{}^{FD}|$ indicates that the difference is not statistically significant. Thus, any effects of migration on decision-making are likely temporary.

Most of the movement between columns appears to be between spouse-respondent joint and respondent alone decision-making–except for temptation goods. Interestingly, temptation goods also appear to be the category where the spouse has the most decision-making power: the average fraction of temptation goods expenditures for which the spouse alone makes decisions is 53%. This is about three-times as large as every other expenditure

category's average. However, the sample size for temptation goods expenditures is also the smallest–with a sample size about half of the other categories'. This means that more households claimed to "not make decisions" at all regarding temptation goods, in which case there is no decision-making power to distribute between household members.[26] Regardless, out of the population that did declare some distribution of decision-making power, temptation goods appears to be a category where women have a large "deficit" of involvement. Thus, the decision-making power gain for women in this category is especially significant.

Children's Budget Shares

The data include an important measure of resource allocation to children (C): expenditure amounts as percentages of a yearly budget.[27] Household expenditures on children relative to entire budget is one representation of resource distribution within households. Thus our outcome variables in Tables 11 and 12 are the amount of money spent on different expenditure categories as a percentage of a yearly budget.[28] Interestingly, the budget share for alcohol, tobacco, and smoking decreases by 0.03 in the naive FE model (Table 11). While this seems like a small magnitude, it is 16% of the mean. However, this effect disappears with the FE and FD models. Given that the estimate was only significant at the 10% level and that it disappeared in the stronger models, we interpret this result with caution. We see a similar, slightly stronger effect with children's clothing budget allocations (Table 12), where expenditures allocated to children increases with an effect size of around 15% in both FE models. Yet the effect goes to zero in the FD model. Thus, while we saw that a husband's migration spell increases his wife's decision-making power, there are no clear corresponding significant effects on budget allocations.

A Note on Robustness and Extensions

Not only is the change in decision-making power attributable to spousal migration spells remarkably robust across expenditure categories, the results also pass a few robustness checks. First we drop earthquake-affected districts in order to ensure that the earthquake is not driving any of the results. We do this two different ways–both by dropping all households in Dhading and Nuwakot, two of the most affected districts, and by dropping households living in VDC's with a Mercalli Scale measure of the earthquake in the 75th percentile or higher. The results remain robust, if not stronger. In fact, the share of the budget dedicated to temptation goods appears to decrease during a husband's migration term at a statistically significant level in all three models. However, the statistically significant result on children's clothing is not robust to these tests. These results confirm that we can be

confident in a causal effect of a husband's migration on his wife's decision-making power and perhaps a corresponding change in temptation goods (decrease); yet we can only be skeptical of a corresponding change in children's clothing (increase) expenditures.

One concern for interpretting these estimates as causal is that we are running many regressions and therefore more likely to find false results. In light of this, we specify outcome variables for decision-making power that aggregate across all expenditure groups. The results echo the overall pattern between expenditure groups: during their husbands' migration spells, women temporarily gain sole decision-making power over expenditures. Interestingly, most of the movement comes from joint decisions rather than spouse-alone decisions, with effect sizes for the FE model at about 48% and 26%, respectively. This is informative: on average, a husband's migration spell causes a larger shift away from spouse-respondent joint decisions than from sole decisions made by the spouse. Furthermore, this shift is temporary–with the difference between the magnitude of the coefficients on 2014 and 2016 migration spells being the same statistically speaking. Thus any gains in decision-making power do not appear to be strong, in either a permanence or magnitude sense.

One final robustness check is to test the results on temptation goods and children's clothing in terms of expenditure levels instead of budget shares. Econometrically, the coefficient on the dummy variable for migration should give the same information: holding income constant, both the budget share and the level of corresponding expenditures will be correlated and therefore the regressions should produce the same results. Thus it is an important exercise to check that the budget shares results are not sensitive to technical specification. These regressions produce no statistically significant results, except that the level of spending on children's clothing decreases by roughly 15 USD in the FD model upon a husband's return (significant at the 10% level). This shows that the result on temptation goods is sensitive to data specification and hence unreliable.

We also check the sample attrition for non-randomness. The attrition in these data is most likely because households migrated or enumerators were not able to set up an interview with the household. It is probable that these households are systematically different from the households that remained in the sample. This is only problematic if these differences cause both a change in the husband's migration status and in decision-making power or resources allocated to children. To investigate this, we run regressions of 2014 household characteristics on a dummy equal to one if the household leaves the sample. We find that attrited households have less females, smaller households, and are less likely to have in-laws present. This is most likely not problematic, however, since there is no statistically significant difference in the propensity for the husband to migrate.

Conclusion

Using data from rural Nepal on migration, we identify the causal relationship between migration spells and intra-household decision-making on expenditures. Subsequently, we identify the relationship between that shift and a change in expenditures allocated to children. We find a robust positive effect of male migration spells on women's decision-making power in all expenditure categories; however, there is no clear corresponding change in expenditures, aside from a weak shift away from temptation goods and towards children's clothing.

We used two difference-in-difference models to get at these relationships, one that parses out the effect of a migrant's leaving and returning separately. In order for the estimates to be interpreted as causal, we must assume that there are no unobserved, time-varying differences between households where the husbands migrate at some point in the sample. This is because the DD models identify an effect by relying on the quasi-randomness in the timing of migration spells, thus avoiding the assumption that households that experience migration are similar to those who do not.

Results from both models make it clear that women do indeed gain more control over expenditure decision-making in a household when their husbands migrate. The second DD model demonstrates that this gain only lasts as long as the husband's migration spell: there are no persistent changes in decision-making. These results are remarkably robust across expenditure categories and against mutliple robustness checks. However, there is only weak evidence for a corresponding shift in expenditures away from temptation goods and towards children's clothing during migration spells. These results do not hold consistently throughout the robustness checks. Thus it is inconclusive at best whether or not a causal effect on resources allocated to children truly exists. This suggests that development programs that target women beneficiaries could be misguided about the returns, at least in a rural, Southeast Asian context.[29]

This paper builds on a small literature that looks at the effect of migration on decision-making. This literature would benefit from more research, as it is clear that husband migration spells do indeed shift decision-making power. Since this is the case, it may provide a good natural experiment for investigating the claim that women allocate resources towards children more than men in different contexts. The difficultly here will always be controlling for selection into migration in order to obtain a causal estimate. Another difficultly is identifying the specific mechanisms at work. If decision-making power does change during migration spells—as we find here—then future research ought to identify whether this is due to bargaining power or some other mechanism at work. This would benefit theoretical models on decision-making and bargaining power, and it would also give policy-makers a stronger understanding of how to increase women's decision-making power if that is their goal.

Notes

[1]Clemens and Ogden (2014) give a comprehensive review of this issue in the migration literature. We also cover this issue in the "Contribution to the Literature" section.

[2]In a literature review of the evidence on cash transfers, Evans and Popova (2014) highlight this inconclusiveness. We expand on this in the "Contribution to the Literature" section and highlight other evidence that speaks to the link between women's empowerment and children's welfare.

[3]It is possible that prior to investment, girls are at a disadvantage relative to boys. If this is true, then women might be investing in children in a way that reduces disparities.

[4]These papers rely on evidence from a variety of countries, and their empirical methods are just as varied. It is no wonder that the literature on this issue is inconclusive.

[5]Clemens and Ogden (2014) point out that the literature lacks breadth as a result of its hyper-focus on remittances.

[6]In other words, assume

$$U(\mathbf{x}^t) \geq min[U(\mathbf{x}^1), U(\mathbf{x}^2)] \text{ for all t} \in [0,1]$$

where $\mathbf{x}$ is a vector of the choice variables T and C in the function's domain.

[7]In general, meeting the second-order conditions implies that the implicit function theorem is met, which says an explicit solution exists to this system of equations.

[8]The shadow price here represents the marginal change in utility with respect to a change in total resources, R.

[9]We will compare the dropped sample to the final sample in the paper that will come out of this thesis to ensure that survey attrition was effectively random.

[10]See the following subsection for a brief discussion of these events.

[11]Note that we include two different measures of household income. Due to entry error, several households declared their annual income to be zero. (These are probably mistakes and not true zero's; when comparing zero-income households' assets to nonzero-income households', asset ownership looks roughly the same.) This constituted about 17% of the sample. We imputed the median of all-nonzero values to replace the zero's. Even still, the data included several outliers. This is why we include the logged annual household income, which we ultimately use as a control.

[12]In Nepal, most women join their husband's household upon marriage. It is unclear when and why families move out and transition to independent households. This is important because joint households could have unique power structures that affect decision-making. It looks like women with migrant husbands tend to live with their in-laws, while women with husbands residing at home have started independent households where their son(s) might have married recently.

[13]A ShakeMap measures ground motion that occurs during an earthquake. It has the advantage of better capturing earthquake damage than simple magnitude and distance-from-epicenter measures (See the ShakeMap Manual: http://usgs.github.io/shakemap/introduction.html).

[14]Note that due to the household fixed effects, the variation captured here is that which occurs within each household above its own average over time.

[15]The difference in the difference for 2014 migrant households is $-\beta_1$ and for 2016 migrant households is β_1.

[16]These variables describe the age and gender breakdown in a household: counts of females and males who are ages 0-5, 6-13, 14-17, 18-64, and 65 or older. (When we look at children's education outcomes, we adjust these age groups to have cut-offs aligned with ages where kids are eligible for school.) Also, because joint households (where young couples live with the husband's parents) are common in Nepal, we include dummy variables for if a mother-, father-, or daughter-in-law lives in the household. Qualitative research revealed that often times joint households are headed first by the father-in-law, his wife, their son, and finally, the son's wife. This is an important control in case households transition from joint to nuclear (or vice-versa) as migration status changes. Also, note that enumerators prioritized interviewing female household decision-makers. This may mean either a mother-in-law or daughter-in-law in a household.

[17]Although other migration literature treats remittances as a special source of income (claiming it is windfall income), we treat remittances as a return on an investment in line with more recent migration literature (Clemens and Ogden, 2014). There is nothing distinct between remittances and other sources of income, and accordingly we control for all income as one variable.

[18]Types of shocks include non-earthquake natural disasters, serious illnesses, deaths of household members, falling agricultural prices, decreases in income, and loss of employment.

[19]At endline enumerators interviewed households with the same sampling protocol described in Footnote 16, prioritizing above all the baseline respondent. Sometimes interviewing the baseline respondent was infeasible, so they chose a different female decision-maker. We limit the sample to respondents who are married females in both time periods. This means that if the midline respondent is different from the baseline respondent, she is most likely either the mother-in-law or daughter-in-law of the baseline respondent. As we

mentioned previously, we control for household composition, therefore fully accounting for any changes in survey responses due to differing respondents over time.

[20]These data are from the USGS Earthquake Hazards Program and are at the VDC level.

[21]These data came from a separate survey given to a prominent community leader within each ward, as discussed in the Data section. Thus these values vary at the ward level. It is also important to note that some wards were missing data on the prices. Specifically, there were six missing data points for sugar and oil prices. This is not surprising since these were recall data collected in 2016. For these cases, we imputed the Village Development Committee (VDC) mean to the missing wards.

[22]A second way to identify this causal chain would be to create a system of equations where $W = f(migration)$ and $C = g(W)$. Then, much like Acemoglue et al. (2001), we could instrument for W with something that is related to migration and exogenous to children's well-being. One common instrument in the migration literature is migrant networks, for which we do have data. However, these data only cover half of the sample. Furthermore, the instrument is not likely to be exogenous (i.e. people choose their networks) or excludable (i.e. networks affect many things). Thus we do not pursue this strategy.

[23]We cannot run a regression where resources allocated to children is a function of decision-making power because we would need to ensure that variation in decision-making power was exogenous and related to migration spells.

[24]As in Footnote 22, we could also approach this specification using a system of equations.

[25]It is important to note that we are not commenting on what is "better" for women in these households. For example, without knowing their preferences, we cannot tell if a woman prefers to be making joint decisions with her spouse (Column 2) or on her own

(Column 3). Yet we do not need to know what women prefer; we only need to know which columns consist of more "power." This is so we can evaluate whether or not women are gaining more decision-making power upon their husbands' migration spells. Certainly a woman has more decision-making power if she makes decisions alone than if she does jointly–whether or not she prefers this situation is a different question that we will not attempt to answer.

[26]Respondents answered the question about household decision-making regardless of whether or not they spent money on the good.

[27]We also have data on household division of labor, distribution of nutrition, children's enrollment rates, and school absences. However, these results are largely uninteresting. Furthermore, the data do not include decision-making over labor, nutrition, or education, so explicitly linking these results to the causal chain discussed in the theory section would be difficult.

[28]Note that the recall period on certain expenditure items only covered one or three months. We extrapolated those items to a 12-month period by multiplying the expenditure amounts by 12 and four. Also, the expenditures module is by no means comprehensive (for example, it does not ask how much a household spends on food). Thus, we use the term "budget" loosely.

[29]Of course, if gender equality is a main goal of these programs, then these results provide little insight.

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Tables

Table 1. Household Characteristics: Full Sample

	Mean	Standard Deviation	Minimum	Maximum	Count
Logged Annual Household Income (Rupees)	11.31	1.25	4.57	17.22	4942.00
Annual Household Income (Rupees)	184978.17	739687.29	97.00	30159996.00	4942.00
Number of Residing Males (14+ Years)	0.72	0.85	0.00	6.00	4942.00
Number of Residing Females (14+ Years)	1.92	0.97	0.00	7.00	4942.00
Number of Residing Boys (<14 Years)	0.45	0.67	0.00	4.00	4942.00
Number of Residing Girls (<14 Years)	0.48	0.75	0.00	4.00	4942.00
Number of Residing Household Members	4.31	1.90	1.00	14.00	4942.00
If Mother-in-law Resides in Household	0.25	0.43	0.00	1.00	4942.00
If Father-in-law Resides in Household	0.19	0.39	0.00	1.00	4942.00
If Daughter-in-law Resides in Household	0.17	0.38	0.00	1.00	4942.00
Husband's Education (years)	5.12	4.28	0.00	22.00	4942.00
Respondent's Education (years)	2.59	3.87	0.00	18.00	4942.00
Number of Migrants in Household	1.39	1.82	0.00	17.00	4942.00
If Respondent's Husband is Migrating	0.26	0.44	0.00	1.00	4942.00

Table 2. Household Characteristics: Migrant vs. Non-migrant Households

	Not Currently Migrating		Currently Migrating		
	Mean	**SD**	**Mean**	**SD**	**Diff.**
Logged Annual Household Income (Rupees)	11.21	1.23	11.61	1.26	-0.40***
Annual Household Income (Rupees)	171444	770517	224357	640260	-52912*
Number of Residing Males (14+ Years)	0.73	0.85	0.69	0.83	0.04
Number of Residing Females (14+ Years)	1.93	0.97	1.91	0.96	0.02
Number of Residing Boys (<14 Years)	0.42	0.67	0.54	0.67	-0.12***
Number of Residing Girls (<14 Years)	0.44	0.75	0.57	0.75	-0.13***
Number of Residing Household Members	4.51	1.91	3.71	1.76	0.80***
If Mother-in-law Resides in Household	0.21	0.41	0.38	0.49	-0.17***
If Father-in-law Resides in Household	0.15	0.36	0.29	0.45	-0.14***
If Daughter-in-law Resides in Household	0.22	0.41	0.04	0.21	0.17***
Husband's Education (years)	4.59	4.33	6.67	3.70	-2.08***
Respondent's Education (years)	1.99	3.58	4.32	4.13	-2.33***
Number of Migrants in Household	1.27	1.92	1.74	1.45	-0.47***
If Respondent's Husband is Migrating	0.00	0.00	1.00	0.00	–

Table 3. Average Fraction of Expenditures with Varying Decision-Input Levels

	Not Currently Migrating	Currently Migrating	Difference	Count
Temptation Goods				
Spouse Alone	0.63	0.49	0.14***	2000
Spouse Involved	0.20	0.11	0.09***	2000
Respondent Alone	0.10	0.25	-0.15***	2000
Respondent Involved	0.06	0.15	-0.09***	2000
Formal Health				
Spouse Alone	0.28	0.17	0.11***	4350
Spouse Involved	0.49	0.27	0.22***	4350
Respondent Alone	0.09	0.39	-0.30***	4350
Respondent Involved	0.15	0.18	-0.03*	4350
Ceremonial				
Spouse Alone	0.23	0.14	0.09***	4520
Spouse Involved	0.50	0.33	0.17***	4520
Respondent Alone	0.12	0.35	-0.23***	4520
Respondent Involved	0.15	0.17	-0.03*	4520
Education				
Spouse Alone	0.29	0.13	0.16***	3184
Spouse Involved	0.46	0.27	0.19***	3184
Respondent Alone	0.10	0.46	-0.36***	3184
Respondent Involved	0.15	0.14	0.01	3184
Children's Clothing				
Spouse Alone	0.26	0.14	0.12***	3174
Spouse Involved	0.48	0.26	0.22***	3174
Respondent Alone	0.12	0.46	-0.34***	3174
Respondent Involved	0.15	0.14	0.01	3174
Adult Women's Clothing				
Spouse Alone	0.21	0.13	0.07***	4652
Spouse Involved	0.43	0.22	0.21***	4652
Respondent Alone	0.21	0.47	-0.26***	4652
Respondent Involved	0.15	0.17	-0.02	4652

Table 4. Average Expenditure Amounts

	Not Currently Migrating	Currently Migrating	Difference	Count
Temptation Goods				
Total Amount Spent	20882.94	18616.15	2266.79	2000
Budget Share	0.19	0.16	0.03**	2000
Formal Health				
Total Amount Spent	56572.26	40953.99	15618.27	4922
Budget Share	0.24	0.21	0.03***	4922
Ceremonial				
Total Amount Spent	37525.76	39052.43	-1526.67	4520
Budget Share	0.25	0.26	-0.01	4520
Education				
Total Amount Spent	30864.31	26840.16	4024.15	3182
Budget Share	0.19	0.20	-0.01*	3182
Children's Clothing				
Total Amount Spent	4302.56	5307.00	-1004.45**	4922
Budget Share	0.04	0.06	-0.02***	4922
Adult Women's Clothing				
Total Amount Spent	7787.23	7494.16	293.07	4922
Budget Share	0.08	0.09	-0.00	4922

Table 5. Decisions on Temptation Goods Expenditures

	Spouse Alone			Spouse and Respondent Jointly			Respondent Alone			Respondent and Other Jointly		
	Naive FE	FE	FD	Naive FE	FE	FD	Naive FE	FE	FD	Naive FE	FE	FD
Husband Currently Migrant	-0.123**	-0.154**		-0.066*	-0.061		0.173***	0.166***		0.016	0.049	
	(0.018)	(0.014)		(0.063)	(0.105)		(0.000)	(0.000)		(0.663)	(0.240)	
Year is 2016	-0.148***	-0.154*		0.178***	0.222**		0.038	-0.024		-0.068	-0.045	
	(0.008)	(0.092)		(0.001)	(0.011)		(0.411)	(0.745)		(0.117)	(0.552)	
Husband Migrated Only in 2014 (Return)			0.211*			0.032			-0.136*			-0.107
			(0.062)			(0.655)			(0.065)			(0.173)
Husband Migrated Only in 2016 (Leave)			-0.026			-0.129*			0.223**			-0.068
			(0.815)			(0.077)			(0.026)			(0.426)
Husband Home 2014 & 2016			0.084			-0.001			0.024			-0.106*
			(0.226)			(0.986)			(0.690)			(0.084)
Household Fixed Effects?	YES	YES	–	YES	YES	–	YES	YES	–	YES	YES	–
Mean of Dependent Variable	0.610	0.527	-0.103	0.185	0.138	0.105	0.126	0.204	0.0214	0.0789	0.130	-0.0230
R-squared	0.0734	0.200	0.0455	0.0824	0.202	0.0401	0.0823	0.253	0.0802	0.0543	0.100	0.0569
P-Value 2014 vs. 2016	–	–	0.353	–	–	0.446	–	–	0.566	–	–	0.223
N	1998	496	999	1998	496	999	1998	496	999	1998	496	999

Robust standard errors are clustered at the VDC level. All estimates reported are coefficients on binary variables. P-values in parentheses.
Significance level denoted * p<0.10, ** p<0.05, *** p<0.01.

Table 6. Decisions on Formal Healthcare Expenditures

	Spouse Alone			Spouse and Respondent Jointly			Respondent Alone			Respondent and Other Jointly		
	Naive FE	FE	FD	Naive FE	FE	FD	Naive FE	FE	FD	Naive FE	FE	FD
Husband Currently Migrant	-0.042	-0.035		-0.156***	-0.163***		0.204***	0.203***		-0.006	-0.005	
	(0.181)	(0.241)		(0.000)	(0.000)		(0.000)	(0.000)		(0.783)	(0.816)	
Year is 2016	-0.100*	-0.182***		0.113**	0.123**		0.066**	0.098*		-0.079**	-0.039	
	(0.074)	(0.001)		(0.034)	(0.042)		(0.034)	(0.074)		(0.015)	(0.374)	
Husband Migrated Only in 2014 (Return)			0.044			0.178***			-0.227***			0.005
			(0.382)			(0.004)			(0.000)			(0.881)
Husband Migrated Only in 2016 (Leave)			-0.033			-0.144**			0.183***			-0.006
			(0.441)			(0.016)			(0.002)			(0.900)
Husband Home 2014 & 2016			-0.021			0.065**			-0.037			-0.007
			(0.544)			(0.035)			(0.305)			(0.825)
Household Fixed Effects?	YES	YES	–	YES	YES	–	YES	YES	–	YES	YES	–
Mean of Dependent Variable	0.248	0.186	-0.156	0.434	0.329	0.139	0.164	0.315	0.0322	0.154	0.170	-0.0149
R-squared	0.101	0.135	0.0245	0.0878	0.0955	0.0438	0.0826	0.125	0.0786	0.0313	0.0402	0.0303
P-Value 2014 vs. 2016	–	–	0.880	–	–	0.685	–	–	0.592	–	–	0.986
N	4350	1534	2175	4350	1534	2175	4350	1534	2175	4350	1534	2175

Robust standard errors are clustered at the VDC level. All estimates reported are coefficients on binary variables. P-values in parentheses.
Significance level denoted * p<0.10, ** p<0.05, *** p<0.01.

Table 7. Decisions on Ceremonies and Celebrations Expenditures

	Spouse Alone			Spouse and Respondent Jointly			Respondent Alone			Respondent and Other Jointly		
	Naive FE	FE	FD	Naive FE	FE	FD	Naive FE	FE	FD	Naive FE	FE	FD
Husband Currently Migrant	-0.018	-0.011		-0.142***	-0.150***		0.157***	0.167***		0.003	-0.006	
	(0.410)	(0.619)		(0.000)	(0.000)		(0.000)	(0.000)		(0.877)	(0.772)	
Year is 2016	0.011	-0.051		0.113*	0.107		-0.022	0.006		-0.101***	-0.062	
	(0.861)	(0.415)		(0.086)	(0.152)		(0.562)	(0.925)		(0.009)	(0.253)	
Husband Migrated Only in 2014 (Return)			0.044			0.181***			-0.186***			-0.039
			(0.296)			(0.002)			(0.000)			(0.244)
Husband Migrated Only in 2016 (Leave)			0.018			-0.117**			0.141**			-0.042
			(0.678)			(0.047)			(0.011)			(0.301)
Husband Home 2014 & 2016			0.003			0.090**			-0.078**			-0.015
			(0.916)			(0.015)			(0.042)			(0.591)
Household Fixed Effects?	YES	YES	–	YES	YES	–	YES	YES	–	YES	YES	–
Mean of Dependent Variable	0.210	0.152	-0.121	0.458	0.381	0.125	0.177	0.299	0.0186	0.155	0.168	-0.0232
R-squared	0.0908	0.122	0.0420	0.0800	0.102	0.0472	0.0633	0.102	0.0664	0.0363	0.0645	0.0345
P-Value 2014 vs. 2016	–	–	0.400	–	–	0.461	–	–	0.566	–	–	0.189
N	4520	1578	2260	4520	1578	2260	4520	1578	2260	4520	1578	2260

Robust standard errors are clustered at the VDC level. All estimates reported are coefficients on binary variables. P-values in parentheses.
Significance level denoted * p<0.10, ** p<0.05, *** p<0.01.

Table 8. Decisions on Children's Education Expenditures

	Spouse Alone			Spouse and Respondent Jointly			Respondent Alone			Respondent and Other Jointly		
	Naive FE	FE	FD	Naive FE	FE	FD	Naive FE	FE	FD	Naive FE	FE	FD
Husband Currently Migrant	-0.040	-0.055*		-0.126***	-0.131***		0.192***	0.193***		-0.026	-0.006	
	(0.189)	(0.086)		(0.002)	(0.003)		(0.000)	(0.000)		(0.279)	(0.786)	
Year is 2016	-0.109	-0.179***		0.189***	0.231***		0.053	0.064		-0.133***	-0.116**	
	(0.123)	(0.003)		(0.008)	(0.001)		(0.110)	(0.335)		(0.000)	(0.023)	
Husband Migrated Only in 2014 (Return)			-0.006			0.192***			-0.184***			-0.002
			(0.896)			(0.003)			(0.004)			(0.958)
Husband Migrated Only in 2016 (Leave)			-0.095*			-0.060			0.216***			-0.061
			(0.066)			(0.287)			(0.002)			(0.199)
Husband Home 2014 & 2016			-0.027			0.083**			-0.042			-0.014
			(0.479)			(0.023)			(0.368)			(0.654)
Household Fixed Effects?	YES	YES	–	YES	YES	–	YES	YES	–	YES	YES	–
Mean of Dependent Variable	0.244	0.152	-0.138	0.399	0.313	0.118	0.212	0.396	0.0444	0.145	0.139	-0.0242
R-squared	0.0950	0.144	0.0345	0.0847	0.0959	0.0543	0.0843	0.112	0.0795	0.0416	0.0594	0.0392
P-Value 2014 vs. 2016	–	–	0.165	–	–	0.143	–	–	0.766	–	–	0.391
N	3184	1330	1592	3184	1330	1592	3184	1330	1592	3184	1330	1592

Robust standard errors are clustered at the VDC level. All estimates reported are coefficients on binary variables. P-values in parentheses.
Significance level denoted * p<0.10, ** p<0.05, *** p<0.01.

Table 9. Decisions on Children's Clothing Expenditures

	Spouse Alone			Spouse and Respondent Jointly			Respondent Alone			Respondent and Other Jointly		
	Naive FE	FE	FD	Naive FE	FE	FD	Naive FE	FE	FD	Naive FE	FE	FD
Husband Currently Migrant	-0.035	-0.051*		-0.165***	-0.154***		0.199***	0.199***		0.001	0.006	
	(0.240)	(0.076)		(0.000)	(0.001)		(0.000)	(0.000)		(0.966)	(0.834)	
Year is 2016	-0.094	-0.149**		0.131*	0.138*		0.082**	0.148**		-0.120***	-0.138***	
	(0.159)	(0.012)		(0.058)	(0.080)		(0.027)	(0.026)		(0.003)	(0.008)	
Husband Migrated Only in 2014 (Return)			-0.007			0.203***			-0.175***			-0.021
			(0.871)			(0.001)			(0.004)			(0.621)
Husband Migrated Only in 2016 (Leave)			-0.080			-0.120*			0.230***			-0.031
			(0.139)			(0.071)			(0.002)			(0.547)
Husband Home 2014 & 2016			-0.043			0.025			0.003			0.014
			(0.270)			(0.546)			(0.946)			(0.655)
Household Fixed Effects?	YES	YES	–	YES	YES	–	YES	YES	–	YES	YES	–
Mean of Dependent Variable	0.220	0.152	-0.121	0.407	0.314	0.0970	0.228	0.394	0.0460	0.145	0.139	-0.0220
R-squared	0.0813	0.109	0.0386	0.0660	0.0929	0.0465	0.0744	0.112	0.0683	0.0323	0.0693	0.0313
P-Value 2014 vs. 2016	–	–	0.247	–	–	0.387	–	–	0.618	–	–	0.495
N	3176	1364	1588	3176	1364	1588	3176	1364	1588	3176	1364	1588

Robust standard errors are clustered at the VDC level. All estimates reported are coefficients on binary variables. P-values in parentheses.
Significance level denoted * p<0.10, ** p<0.05, *** p<0.01.

Table 10. Decisions on Adult Women's Clothing Expenditures

	Spouse Alone			Spouse and Respondent Jointly			Respondent Alone			Respondent and Other Jointly		
	Naive FE	FE	FD	Naive FE	FE	FD	Naive FE	FE	FD	Naive FE	FE	FD
Husband Currently Migrant	-0.007	-0.005		-0.146***	-0.148***		0.138***	0.133***		0.015	0.021	
	(0.785)	(0.853)		(0.000)	(0.000)		(0.000)	(0.000)		(0.539)	(0.405)	
Year is 2016	-0.040	-0.094		0.149***	0.192***		-0.030	-0.049		-0.079**	-0.049	
	(0.460)	(0.111)		(0.005)	(0.002)		(0.497)	(0.439)		(0.031)	(0.276)	
Husband Migrated Only in 2014 (Return)			-0.023			0.157***			-0.093*			-0.041
			(0.557)			(0.006)			(0.087)			(0.310)
Husband Migrated Only in 2016 (Leave)			-0.036			-0.151***			0.199***			-0.012
			(0.361)			(0.009)			(0.004)			(0.815)
Husband Home 2014 & 2016			-0.038			0.065*			-0.002			-0.025
			(0.169)			(0.055)			(0.961)			(0.413)
Household Fixed Effects?	YES	YES	–	YES	YES	–	YES	YES	–	YES	YES	–
Mean of Dependent Variable	0.187	0.139	-0.127	0.375	0.275	0.0916	0.280	0.417	0.0735	0.158	0.168	-0.0383
R-squared	0.0837	0.105	0.0322	0.0506	0.0852	0.0347	0.0686	0.103	0.0562	0.0393	0.0645	0.0337
P-Value 2014 vs. 2016	–	–	0.318	–	–	0.945	–	–	0.309	–	–	0.487
N	4650	1634	2325	4650	1634	2325	4650	1634	2325	4650	1634	2325

Robust standard errors are clustered at the VDC level. All estimates reported are coefficients on binary variables. P-values in parentheses.
Significance level denoted * p<0.10, ** p<0.05, *** p<0.01.

Table 11. Expenditures as Percentages of Yearly Budget (A)

	Temptation Goods			Formal Healthcare			Items for Ceremonies or Celebrations		
	Naive FE	FE	FD	Naive FE	FE	FD	Naive FE	FE	FD
Husband Currently Migrant	-0.034*	-0.035		0.023	0.014		-0.002	-0.002	
	(0.092)	(0.132)		(0.159)	(0.329)		(0.847)	(0.866)	
Year is 2016	-0.044*	-0.071*		0.007	0.001		-0.008	-0.012	
	(0.063)	(0.072)		(0.753)	(0.973)		(0.631)	(0.677)	
Husband Migrated Only in 2014 (Return)			0.024			-0.023			0.011
			(0.456)			(0.328)			(0.599)
Husband Migrated Only in 2016 (Leave)			-0.047			0.014			0.008
			(0.127)			(0.640)			(0.755)
Husband Home 2014 & 2016			-0.008			0.026			0.005
			(0.629)			(0.103)			(0.759)
Household Fixed Effects?	YES	YES	–	YES	YES	–	YES	YES	–
Mean of Dependent Variable	0.187	0.168	-0.0138	0.229	0.210	0.0350	0.251	0.254	-0.00807
R-squared	0.0675	0.109	0.0645	0.0490	0.0583	0.0394	0.0251	0.0535	0.0244
P-Value 2014 vs. 2016	–	–	0.639	–	–	0.834	–	–	0.632
N	1998	496	999	4920	1720	2460	4520	1578	2260

Robust standard errors are clustered at the VDC level. All estimates reported are coefficients on binary variables. P-values in parentheses.
Significance level denoted * p<0.10, ** p<0.05, *** p<0.01.

Table 12. Expenditures as Percentages of Yearly Budget (B)

	Children's Education			Children's Clothing			Adult Women's Clothing		
	Naive FE	FE	FD	Naive FE	FE	FD	Naive FE	FE	FD
Husband Currently Migrant	0.000	0.003		0.008*	0.009*		0.007	0.006	
	(0.983)	(0.780)		(0.083)	(0.051)		(0.159)	(0.280)	
Year is 2016	0.019	-0.013		-0.002	-0.000		-0.012	-0.010	
	(0.224)	(0.565)		(0.633)	(0.992)		(0.211)	(0.449)	
Husband Migrated Only in 2014 (Return)			0.009			-0.005			-0.006
			(0.691)			(0.421)			(0.507)
Husband Migrated Only in 2016 (Leave)			0.011			0.014			0.011
			(0.596)			(0.107)			(0.302)
Husband Home 2014 & 2016			0.006			-0.005			-0.008
			(0.688)			(0.340)			(0.270)
Household Fixed Effects?	YES	YES	–	YES	YES	–	YES	YES	–
Mean of Dependent Variable	0.194	0.201	-0.00572	0.0482	0.0587	0.00289	0.0849	0.0867	0.00452
R-squared	0.0343	0.0798	0.0338	0.0470	0.0740	0.0480	0.0502	0.0855	0.0503
P-Value 2014 vs. 2016	–	–	0.572	–	–	0.441	–	–	0.736
N	3184	1330	1592	4920	1720	2460	4920	1720	2460

Robust standard errors are clustered at the VDC level. All estimates reported are coefficients on binary variables. P-values in parentheses.
Significance level denoted * p<0.10, ** p<0.05, *** p<0.01.