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The Donor Footprint and Gender Gaps

Maria Perrotta Berlin
Stockholm Institute of Transition Economics (SITE)

Evelina Bonnier
Stockholm Institute of Transition Economics (SITE)
Stockholm School of Economics

Anders Olofsgård
Stockholm Institute of Transition Economics (SITE)
Stockholm School of Economics

Abstract

In this paper, we analyze the impact of foreign aid on female empowerment by matching geo-coded household surveys with the location of aid-projects, thus measuring an average community effect of exposure to aid-financed projects. Given that women's empowerment is a multidimensional concept, we examine the impact on several indicators related to women's relative standing in the household. We find positive effects on women's participation in the labor force, household decision-making and attitudes toward domestic violence, as well as on household consumption and expenditures on children. These effects are generally stronger for gender specific projects. At the same time, we find no or negative effects for other indicators, such as the division of household chores, and children's education. We argue that the variation in outcomes can best be understood by what change would be required from other family members and how this change matches the norms of the community.

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Author Information

Maria Perrotta Berlin
Stockholm Institute of Transition Economics (SITE)

Evelina Bonnier
Stockholm Institute of Transition Economics (SITE)
Stockholm School of Economics

Anders Olofsgård
Stockholm Institute of Transition Economics (SITE)
Stockholm School of Economics

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Contents

1 Introduction

In this paper we look at how individual- and household-level outcomes and attitudes with regards to women's rights and opportunities respond to the presence of aid projects in the geographical neighborhood of the household. We are interested in estimating the impact of any type of aid projects, as well as the particular impact of projects with a specific gender component. Following an emerging literature on geo-spatial impact evaluation, our analysis relies on combining two distinct sets of geo-coded data. First, using new data on the history of location of aid projects in Uganda and Malawi (the only two countries for which there exists a nearly complete recent history of geo-coded aid projects), we create what we refer to as the *donor footprint*, a map of the location of all aid projects dating back to 1999. Second, we collect information on a broad set of outcomes related to women's rights and opportunities from geo-coded household-level survey data. We match these datasets by drawing a 15-kilometer (km) radius around each household, and checking for the presence of aid financed projects in this immediate vicinity of the household. This creates two groups of households, those with at least one aid project in their vicinity and those without. Through this approach we can capture a community level effect of the presence of foreign aid.

To derive results we match treated and control observations on a broad set of exogenous and predetermined variables selected through machine learning, and a blocking estimator to get more robust point estimates. We find rather mixed results. Proximity to aid projects is associated with an increased likelihood of women working outside the household, and a decreased likelihood of women working on the household farm, especially when exposed to gender-specific aid projects. The estimated impact is not trivial. Our results suggest that going from no aid exposure to average aid exposure increases the likelihood of women working outside the household by 40%. Proximity to aid projects, in particular to gender-specific aid projects, is also associated with increased monthly consumption and an increased share of total expenditures spent on children's education and health.

Furthermore, we find that women in proximity to aid projects are more likely to participate in household decision-making (decisions on large purchases, how to spend earnings, their own health care and family visits). Once again, this effect is particularly strong with proximity to gender-specific aid projects. The final encouraging results are that presence of aid, and in particular gender-specific aid, is associated with less positive attitudes toward domestic violence (in particular, but not exclusively, among women). A lower rate of experienced emotional violence is also found if the exposure to aid comes from gender-specific projects.

In terms of children's education, however, we find, if anything, a negative impact (in particular for boys), and in terms of time use, we mostly find that women spend more time doing household chores relative to their husbands if they live in the proximity of aid projects. Mixed results are also found for questions related to women's sexual freedom, where there is a leaning towards positive effects on attitudes toward contraception, but negative effects on the freedom to refuse sex.[1]

The mixed results complicate the interpretation and raise some concerns. As discussed more in depth

[1] Note that these results are not necessarily for the same set of households and reflect an average effect at the community level.

in the paper, project-level data has certain limitations and our empirical strategy cannot completely rule out all endogeneity concerns from aid-project placement. It is however difficult to identify a consistent story based on selection that would generate the set of results that we find.

Instead we argue, though cautiously, that the best way to make sense of our results is to take into consideration that the potential for aid to make a difference at the community level depends on the existing constraints against changing any particular dimension of female empowerment. Using this lens on our results suggests that aid may indeed have a moderate positive impact on dimensions of female empowerment that are relatively easier to influence in a short to medium term. We also find that this is particularly true for projects with a specific gender component. This has implications for the relative merits of direct gender targeting vis-à-vis *mainstreaming* in aid policy, as we will discuss later.

With this paper, we offer several contributions to the existing literature. First, we provide new evidence on a question that has received surprisingly little attention in the academic literature, beyond the evaluation of specific interventions: the impact of foreign aid on female empowerment. The situation of women and girls has been highlighted by international donors at least since the United Nations (UN) declared the UN Decade for Women in 1975. This initiative has been followed by numerous others, most recently the inclusion of an explicit gender equality goal in the Sustainable Development Goals (SDG), also known as the 2030 Agenda. With this in mind, it is surprising that so little quantitative analysis beyond individual project impact evaluations has been undertaken to assess the effectiveness of aid in fostering female empowerment.[2] Studies have shown that foreign aid is partly invested in public goods and services that have been known to particularly benefit women in different ways, such as health spending on maternity care, and investments in water and sanitation (Agénor et al., 2014; Seguino, 2008). However, whether this aggregates up to a measurable positive effect of foreign aid at the macro level has been left mostly unexplored. Pickbourn and Ndikumana (2016) made a recent attempt to bridge this gap, using cross-country data to estimate the correlations between foreign aid inflows and the United Nations Development Program's (UNDP) Gender Inequality Index (GII). The study finds no robust evidence of a correlation.[3] The authors do, however, find some positive impact on maternal mortality rates and youth gender literacy gaps from aid targeted towards the health and education sector respectively.

Second, female empowerment can take many forms and is not easily simplified into a single index. As discussed above, and more so in the next section, it is also likely that aid has a greater potential to change some outcomes relative to others in a short to medium time frame. Understanding this variation may have a value in itself, and may also give a more correct picture of what aid has accomplished (and realistically can be expected to accomplish). We have therefore chosen to look at many different outcomes associated with female empowerment and the welfare of women and girls that also align with the priorities emphasized in Goal 5 of the SDG 2030 agenda.[4] We are, however, limited by the data availability in the geo-coded household surveys we have access to. Moreover, as we are focusing on individuals and households, some indicators commonly used, such as female representation in national legislative bodies, are not applicable.

[2] This is particularly true when comparing with the rather large literature on aid and economic growth (e.g. Burnside and Dollar, 2000; Rajan and Subramanian, 2008; Galiani et al., 2017).

[3] This paper was published as part of a special issue devoted to foreign aid and gender published by the Journal of International Development, Volume 28, 2016.

[4] The targets of Goal 5 of the SDP can be found at `https://www.un.org/sustainabledevelopment/gender-equality/`.

Third, we differentiate between the impacts of aid in general and the impacts of aid with a specific gender component. The donor community already in the Beijing Platform of Action in 1995 agreed to gender mainstreaming, i.e. that gender concerns should be integrated into all policy and program cycles, and that governments should engage in a dialogue on gender and development. For instance, even though only 6% of interventions funded by the Swedish International Development Cooperation Agency (Sida) had gender equality as the prime objective in 2008, as many as 71% had gender equality listed as a significant objective (Nanivazo and Scott, 2012). Gender mainstreaming thus implies that also projects and programs not primarily targeting gender equality are often designed with an eye to it, since many donors have this as an overall explicit objective. This ambition is also shown in the development of a Gender Equality Policy Marker by the Organisation for Economic co-operation and Development's (OECD) Development Assistance Committee (DAC), an indicator that measures how strongly aid-financed projects support gender, and the inclusion of a specific gender goal in the SDG 2030 agenda that donors have committed to.

The approach of gender mainstreaming is not without problems, though.[5] It has been argued that stated commitments are not met by necessary increases in human capacity, financing, and resources towards monitoring and evaluation. Moreover, mainstreaming may even detract resources from real focused investments in gender projects and programs, and the sector division of aid management means that personnel would need skills in both the sector at hand (say infrastructure) and gender, which is rarely the case (Grown et al., 2016). This suggests that even though aid in general has as an objective to better the situation for women, gender-specific projects may be more likely to generate a measurable impact. On the other hand, just as there is a link between economic growth and poverty alleviation, there is a link between economic development and female empowerment in many (but not all) dimensions (see e.g. Duflo, 2012). It follows that the indirect effect of general aid projects may be as important as targeted gender projects if the former more effectively contribute to economic development. Either way, the theoretical ambiguity suggests that it could be valuable to differentiate between the effects of aid in general and the effects of gender-specific aid.

Fourth, studies of aid effectiveness tend to either focus on the very micro level – randomized controlled trials (RCT) investigating the impact of a specific project on direct participants – or the very macro level – the effect of total aid on GDP growth or some other country-level aggregate. The first approach is typically more geared towards understanding the impact of a particular type of intervention or mechanism, rather than the impact of aid as such, and it is well known that the approach faces problems of external validity due to for example site selection bias (Allcott, 2015), and that positive or negative community-level externalities are often neglected. The second approach, in turn, faces challenges with internal validity and measurement, giving rise to a much divided literature (see e.g. Rajan and Subramanian, 2008; Galiani et al., 2017). Recently, due to new data on aid-project placement within countries, an intermediate approach has emerged, sometimes referred to as geo-spatial impact evaluation, focusing on the average impact on communities at a more disaggregated level.[6] Relative to cross-country analysis, geo-spatial

[5] A recent study of gender mainstreaming at the Swedish International Development Cooperation Agency (Sida) finds that the plan for integration of gender equality has diffuse goals and few concrete guidelines, and that staff, despite showing high levels of support for gender equality, are often unaware of the existence of the plan (Bjarnegård and Uggla, 2017).

[6] Dreher and Lohmann (2015) study the regional impact of World Bank projects on nighttime light as a proxy for economic growth, finding a positive correlation but no convincing support for a causal impact. Findley et al. (2011), Wood and Sullivan (2015), and Strandow et al. (2014) find that aid project location is correlated with more local violence and conflict. De and Becker (2015) find that health and education aid have positive impact on some health and education indicators in Malawi. Finally, some studies have

impact evaluations have the advantage that they can control for potential confounding factors at finer geographic levels, facilitating the creation of a control group with more similar conditions in terms of institutional arrangements, culture, climate, etc., relative to the treatment group. Moreover, many projects target local development and cannot be expected to yield results large enough to be possible to isolate and detect at the national level (see e.g. Dreher and Lohmann, 2015; Findley et al., 2011).

Relative to RCTs of individual projects, geo-spatial impact evaluations estimate the impact of many projects rather than one. These projects are typically spread out geographically and implemented by different organizations and financed by different donors. In this sense, this method provides an estimate of the average return across these different dimensions, which reduces concerns with regards to external validity and site selection bias. With geo-spatial impact evaluations, it is also possible to estimate more long-term effects, and no involvement prior to project implementation is required. Hence, this type of evaluation requires substantially less resources both financially and time-wise and can be applied retrospectively making use of existing data. The literature is still developing, though, and data is not complete, restricting us to focus on two recipient countries. Another limitation is that not all aid can be geographically placed below the country level, think for instance of general budget support.

Finally, a concern in the geo-spatial impact literature is selection problems in the placement of aid projects. To reduce these concerns, we apply machine learning and matching techniques, following the most recent contributions in the econometric literature as summarized by Imbens (2015). The aim is to create more comparable treatment and control units and derive more robust treatment effects. The details of the method are presented in Section 4. But first, in Section 2, we discuss how aid can contribute to female empowerment, and what are the constraints faced. In Section 3 we present the data used in our analysis. After the empirical strategy and specification, Section 5 reports our results. We conclude in Section 6.

2 Foreign Aid and Female Empowerment

One possible reason why so few studies, beyond individual project evaluations, have focused on foreign aid and female empowerment can be the complexity and multidimensionality of the concept. Female empowerment spans from areas of education and health over to areas of political influence and career opportunities, and covers indicators best measured at varying degrees of aggregation. In this study, we focus on individuals at the community level, and aim to take a broad perspective. While we are constrained by indicators for which we have geo-coded survey data, we can include most of the dimensions emphasized in Goal 5 of the SDG 2030 agenda, an agenda that donor countries are committed to support through their foreign aid priorities. We have data on access to education and workforce participation, attitudes toward and experiences of different forms of violence, attitudes and experiences around sexual rights, as well as data on influence over major household decisions and household expenditures (more details in the data section). One area that we do not have data on is political influence and participation.

Developing a complete model of drivers of female empowerment and how foreign aid can contribute to

focused on Chinese aid. Isaksson and Kotsadam (2016) find that perceptions of corruption are higher in the vicinity of Chinese aid projects, whereas BenYishay and Mobarak (2015) find that the impact of Chinese-funded infrastructure projects on forest loss depends on the quality of domestic environmental governance.

improvements is beyond the scope of this paper. However, we do want to highlight some key drivers of empowerment and some of the constraints that exist and discuss what they may suggest for the likelihood that foreign aid can make a measurable impact at the individual level in a short to medium term. Our discussion revolves around societal constraints and family constraints, and how different opportunities for women and girls may also affect other family members.

At a societal level, female empowerment largely relies on formal and informal institutions. Legal discrimination is a big constraint in many countries. In 28 countries there are more than 10 legal differences between men and women, ranging from restrictions on owning or using property to requiring the husband's consent to work (World Bank, 2014). However, legislation is also an area where change in principle can happen quite quickly, and where foreign aid can be part of facilitating change through both financing, technical assistance and conditionality. In 1976 only one out of 100 countries included in a study by the World Bank (2013) had legislation against domestic violence. In 2013 that number was 76, and women's acceptance of domestic violence was lower in these countries.

In principle, aid for legislation at the national level should not give raise to local variation, but laws are only effective as long as they are implemented and as long as women and girls are aware of their rights. Implementation and awareness is often a local issue, and foreign aid is often targeted toward local projects to provide low-cost legal assistance, legal literacy training and information to women, and to train and educate legal personnel (World Bank, 2014). Another area where legislation has made a change at the local level is mandated political representation of women in legislative bodies, such as the Indian law to reserve one-third of village governments to women. Not only did males' prejudice against female leaders end and women get elected even after reservations ended in local communities with mandated female leaders, but also parents' aspirations for their daughters in general increased (Beaman et al., 2009, 2012). The reform also increased the reporting of crimes against women and police responsiveness to such crimes, suggesting a change in attitudes toward what is acceptable both among women and men (Iyer et al., 2012). This is an example of how legal reforms can change also more slow-moving institutions such as social norms and expectations.

Unfortunately, there are also many examples of the opposite. Even though child marriage has been illegal in Bangladesh for almost 100 years, still as much as 75% of girls are married before the age of 18 (World Bank, 2014). Informal institutions and norms are thus just as important as the legal code, and changing such slow-moving institutions requires more patience.

Nevertheless, foreign aid has the potential to slowly change these informal institutions through for instance targeted information drives and discussions groups (Kim et al., 2007; Pulerwitz et al., 2015). Munshi and Myaux (2006) emphasize how norms with regards to contraception changed through networks based on religious affiliation in the Matlab region of Bangladesh. Women who came into contact with the relatively well-educated health workers of the Matlab program were affected by new norms and information around sexuality and contraception, and these women, in turn, transmitted information through their networks. This illustrates how whole communities, beyond direct beneficiaries, can be influenced by aid-financed development projects. Information can also be transmitted, and norms and preferences change, through aid-financed edutainment channels, as illustrated in a recent survey by La Ferrara (2016).

Aid can change behavior and attitudes of women by offering economic opportunities. For instance, a study on sexual risk-taking intentions among AIDS-orphaned students in rural Uganda (Ssewamala et al., 2010) found that having access to economic assets reduced adolescents' sexual risk-taking. At the very general level, research shows that gender relations across most (but not all) dimensions tend to become more equal with economic development and that women tend to gain more than men (Duflo, 2012). Hence, to the extent that aid contributes to economic development, it may also, over time, contribute to slowly empowering women and reduce gender inequality.

Beyond the societal context, female empowerment also depends on what happens within the specific household. Models of intra-household bargaining typically emphasize how bargaining power reflects outside options as given by societal norms or individual skill sets (see e.g. Thomas, 1990). Individual income and education are associated with a stronger position of women in the household. For instance, female education has been found to be negatively correlated with fertility (McCrary and Royer, 2011), and educated women have been found to be more likely to have influence over decisions when it comes to their children's education and health (Gakidou et al., 2010). By contributing to girls' education, foreign aid could thus also strengthen women's bargaining power within the household. Beyond direct investments in the education sector, aid projects can target conditional cash transfers to mothers (under the assumption that they care relatively more about girls' education), provide information about economic returns to girls' education and indirectly by contributing to reduced poverty and changing social norms (Duflo, 2012; Benhassine et al., 2015).

Similarly, bargaining power can also be strengthened by offering income-generating opportunities for women outside the household and by targeting transfers, such as micro-credits and old-age pensions, to women (Chakravarty et al., 2013; Attanasio and Lechene, 2002). Many studies also suggest a negative correlation between female earnings and domestic violence (see e.g. Aizer, 2010; Bobonis et al., 2013; Anderberg et al., 2016). Aid could thus also have an indirect positive impact on attitudes around domestic violence and female sexual empowerment, and reduce female exposure to domestic violence.

A strengthened bargaining position of women could in turn strengthen their influence over the household budget. Women have traditionally been thought of as making spending decisions more in line with the welfare of the family, not least the children (Thomas, 1990; Case and Deaton, 1998; Lundberg et al., 1997). This has, however, been challenged to some extent in recent studies using randomization to identify causal effects (Benhassine et al., 2015; Akresh et al., 2016). These later results are also confirmed in lab experiments by Ringdal and Hoem Sjursen (2017), who find that more bargaining power in the hands of mothers does not increase spending on children's education; if anything, it is the other way around. Ringdal and Hoem Sjursen also emphasize that other characteristics, such as time preferences, are more important than gender in determining the share of a given budget allocated to children's education.

Finally, bargaining power can also influence women's control over their own time, in particular the share of time devoted to household chores such as fetching water and collecting firewood. Time-use data suggest that these two activities account for the biggest gap between men and women in time spent on household activities (Blackden and Wodon, 2006). Research from the United States suggests that increasing female relative wages reduces time spent on household chores, but the effects are quite limited in size and smaller for couples with children (Friedberg and Webb, 2005). On the other hand, a gender-

targeted conditional cash transfer program in Pakistan reduced time spent by women on children's needs by an average of 100 minutes per day, but also increased the time spent on housework by 120 minutes, suggesting a net reduction in time for work or leisure (Hasan et al., 2010). Generally, research suggests that time use is highly dependent on norms (Maxwell and Wozny, 2017), and men fetching water and collecting firewood are very visible ways of breeching such norms, which few might be ready to do.

3 Data

As mentioned earlier, our approach to understanding how gender outcomes and attitudes co-vary with the presence of aid projects relies on the possibility of linking the precise location of aid projects to geo-coded household-level data as well as predetermined covariates from the same locations. Given that we are interested in understanding the average effect of any type of aid project from any type of donor, we focus on the countries for which a mapping of at least 80% of total aid flows in recent years is available. This leaves us with two suitable countries: Uganda and Malawi. These two countries will therefore be the focus of this study.

3.1 Treatment Variables

The newly released datasets on Uganda and Malawi from the AidData consortium (2016) provide geo-coded information on all aid projects reported to these countries' respective Aid Information Management Systems (AIMS).[7] More precisely, we use the Uganda AIMS, level 1, version 1.4.1, which includes all geo-coded projects from Uganda's Aid Management Platform (AMP; Peratsakis et al., 2012), corresponding to a total of 2,426 project locations over the years 1988–2014. For Malawi, we use the Geocoded Activity-Level Data from the Government of Malawi's AMP. This dataset includes 80% of all externally funded projects initiated in the period 1996–2011, which corresponds to a total of 2,523 project locations.

The project locations are provided with different levels of precision. We limit our sample to only include projects where the geographic coordinates correspond to the exact location (precision code 1) or a location known to be within 25 km of the reported coordinates (precision code 2). In other words, we exclude projects formally allocated to the whole administrative area of the district or above. Our rationale for doing so is that we believe that it is the projects that are precisely geo-coded that affect nearby households on the margin, while we assume that the influence of projects with larger coverage is uniformly spread across households in the corresponding administrative unit. Moreover, in order to match these project locations to both predetermined covariates and post-intervention outcomes, we further restrict the sample to projects that were initiated in 1999–2011. Applying these restrictions leaves us with a dataset including

[7] The AIMS was developed and introduced by the State Committee on Investments and State Property Management (SCISPM) with technical support from the UNDP and funded by Department for International Development (DFID). It is a web-based Aid Management Platform (AMP) that allows governments of developing countries and their donors to share and analyze aid information. The data and information collected include the number of implemented projects and agreements, their cost, terms, and duration, and executing and implementing agencies.

518 project locations from the Uganda AIMS and 1,005 project locations from Malawi's AMP.[8] However, we will use the information on the number of project-years at precision level 3 (district level) as a control variable in our empirical estimation.

Our first treatment, the presence of at least one aid project in the immediate vicinity of a household, is constructed by drawing a circle with a 15 km radius around each household's location and checking whether any type of aid project was implemented there during the relevant time period. Our second treatment is the presence of at least one "gender project", which is constructed in the same way as our first treatment variable but only focusing on projects with a specific gender component. More precisely, we define a project as a gender project if its title, project description, or activities list include any of the following words: *women*, *girl*, *bride*, *maternal*, *gender*, *genital*, and *child*. There are 73 projects that meet this definition in Uganda and 277 in Malawi.

The appropriate radius to define the treatment is debatable. On the one hand, there are purely statistical considerations such as the size of the treatment and control groups and the noise implied by potential misclassification of individual units. On the other hand, the very idea of impact of a development project is a question of how far the presence of a facility (school, clinic, other service delivery point, and infrastructure) is noticeable and impacting the individual's conditions, and how far is too far. Obviously this will depend on the type of project. To give an idea, the 2009 Uganda Malaria Indicator Survey found that 96% of respondents lived within 9 km of a health-care facility, and the 2009 Uganda National Household Survey reported that the average distance of a household from a government hospital was 20 km. Less than 2% of the respondents in the Malawi Demographic and Health Survey (DHS) 2004 lived farther away than 15 km from a health facility. In half of the school districts in Malawi the average distance to a school is more than 4 km, which is considered too far by parents, according to a survey cited in Ravishankar et al. (2016). Kabunga et al. (2016) report that the median distance to a primary school is 2 km in Uganda, and twice as far to a secondary school. The average distance to a water source is 1 km and to an all-weather road 2 km. We try to strike a balance between these very different acceptable distances and take into account the substantial variation in the nature of the aid projects we study. We have also tried a 10 km radius and the results are very similar.

3.2 Outcome Variables

The World Bank's Living Standards Measurement Survey (LSMS) program serves as a convenient starting point for retrieving some of the outcome variables of interest. The LSMS program collects nationally representative surveys of households with information on their GPS-based locations, and links these locations to geo-spatial variables such as distances to other features, climatology, and terrain from various other databases. The latter are important for our empirical strategy, as will be described further in Sub-

[8] Most of the excluded projects are dropped because their level of geographic precision is above prevision level 2. For Uganda, 1,588 project locations have a precision level above 2, of which 1,300 are at the district level (precision level 3), 33 at the province level (precision level 4), 5 at the level of larger geographic features such as rivers or national parks (precision level 5), and 255 at the country level (precision level 6 and 8). Another 239 project locations are dropped because their implementation year was prior to 1999 or after 2011. For Malawi, 1,516 observations are dropped because their precision level is above 2. Of these, 1,061 project locations have precision level 3, 34 have precision level 4, 25 have precision level 5, and 396 have precision level 6 or 8. Another six project locations are dropped because their implementation year was either before 1999 or after 2011.

section 3.3 and Section 4.

From the LSMS program, we use the Uganda National Panel Survey (UNPS) from 2013/2014 and the Malawi Integrated Household Panel Survey (IHPS) from 2013. The UNPS 2013/2014 was carried out on 3,119 households from 317 enumeration areas (EA), and the IHPS 2013 comprises 4,000 households from 204 EAs. Based on information from these two datasets, we construct outcome variables related to workforce participation, household expenditures, education, and time spent on household chores.

In terms of *workforce participation*, we look at whether women (above 18 years old) have worked outside the home and worked on the household farm in the past 12 months. In terms of *household expenditures*, we look at total monthly consumption in the household (in ppp-adjusted USD) and the share of total household expenditures on children's health and education over the past month. In terms of *education*, we look at two different outcomes for children (respondents 18 years or younger): namely, the share of girls and boys in the relevant cohorts who (i) were currently attending primary school, and (ii) were currently attending secondary school. Finally, in terms of *time spent on household chores*, we look at the share of total time that women, men, girls, and boys within the same household respectively spent on (i) collecting firewood and (ii) fetching water in the past 7 days.

The outcomes related to household decision-making, domestic violence, and women's sexual rights are retrieved from Uganda's and Malawi's most recent Standard DHS. These datasets include, similarly to the LSMS data, geo-coded information on the location of the household clusters that participated in the surveys. In 2017, the DHS program made available a set of geo-spatial variables that can be linked to these household clusters. These variables will be used in our identification strategy. The Uganda DHS was collected in 2011 and includes information on 11,055 individuals (8,774 women in the age range 15–49 and 2,281 men in the age range 15–54) from 400 different clusters. The Malawi DHS was carried out in 2010 and includes information on 29,050 individuals (22,153 women in the age range 15–49 and 6,897 men in the age range 15–54) from 827 clusters.

Based on the DHS data, we construct five indicator variables for women's participation in *household decision-making*. The first variable takes the value 1 if the woman has responded that she, by herself or jointly with her husband/partner, decides how to spend her and/or her husband's/partner's earnings. The second variable takes the value 1 if the woman, by herself or jointly with her husband/partner, makes decisions about large household purchases. The third variable takes the value 1 if the woman, by herself or jointly with her husband/partner, makes decisions about her own health care. The fourth variable takes the value 1 if the woman, by herself or jointly with her husband/partner, makes decisions about visits to her family and relatives. Lastly, we combine these variables into a measure that takes the value 1 if the woman does not participate in making any of the above household decisions.

In terms of *domestic violence*, we are interested in two sets of outcomes. First, we are interested in the share of women who have experienced domestic violence in the past 12 months, separating between three forms of violence: emotional, physical, and sexual violence. Thus, three indicator variables are constructed, one for each form of violence, to take the value 1 if the woman has experienced that form of violence in the past 12 months, and 0 otherwise. This set of questions was only asked to a subset of the women and hence this sample is smaller than for the other DHS variables. The second set of outcomes

of interest is women's and men's attitudes toward domestic violence. We construct a measure for women and men, separately, taking the value 1 if the respondent do not deem it justified for a husband to beat his wife for any of the following reasons: wife burns the food; wife neglects the children; wife goes out without telling the husband; wife argues with the husband; and wife refuses to have sex with husband.

Lastly, we construct two measures reflecting women's views on their own *sexual rights* and three measures of men's views on women's sexual rights. The questions are phrased slightly differently for men and women. For a woman, the two questions are whether she feel that she can (i) say to her husband if she does not want to have sexual intercourse, and (ii) ask her husband to use a condom if she wanted him to. Our measures will take the value 1 if the woman agrees to the statement, and 0 otherwise. For men, the questions are whether they agree that (i) a woman is justified to refuse sex when she knows her husband has sex with women other than his wives; and (ii) a woman is justified to to ask her husband that they use a condom if she knows her husband has a sexually transmitted infection (STI). The two variables based on these questions are constructed as indicator variables taking the value 1 if the man agrees with the statement, and 0 otherwise.

3.3 Matching Variables

Our empirical strategy, which we will describe in detail in Section 4, is based on matching on observables. These predetermined observables are retrieved from the LSMS and DHS datasets, which both provide a wide set of geo-spatial variables for each household location (LSMS) or survey cluster (DHS), such as average rainfall, temperature and distance to closest border.[9]

In addition to the geo-spatial variables, we construct predetermined socio-economic and needs indicators, such as district averages in educational attainment, women's employment rate, and access to piped water, based on information from the Uganda 2002 census and the Malawi 1998 census.[10] For the outcomes measured at the individual level, we include additional variables such as ethnicity, religion, and parent's education that are retrieved from the relevant outcome dataset (LSMS or DHS). Notice, in passing, that not all of these variables are actually used as matching factors. The specification of the matching equation is left to a data-driven algorithm that will be described in detail in Section 4, which will select, among all of these (arguably) exogenous and predetermined variables, the ones with the best predicting power for our treatment.[11]

[9] The complete list of variables available in the LSMS datasets and their sources can be found in the LSMS information material; for Malawi at `\\http://siteresources.worldbank.org/INTLSMS/Resources/3358986-1233781970982/5800988-1271185595871/6964312-1404828635943/IHPS.Geovariables.Description.pdf`, and for Uganda at `\\http://siteresources.worldbank.org/INTLSMS/Resources/3358986-1233781970982/5800988-1265043582346/UNPS\_2009\_10\_BID\_rev\_2014.pdf`. A list of the geo-spatial covariates and their sources linked to the DHS data is available at `https://spatialdata.dhsprogram.com/methodology/##GEOSPATIALCOVARIATES`.

[10] We use a 10% sample of the Uganda 2002 census and the Malawi 1998 censes, provided from the Integrated Public Use Microdata Series (IPUMS), International.

[11] Notice that gender-specific aid projects are included in the count of general aid projects.

4 Method

The main methodological challenge lies in the identification of the causal effect of aid. It is possible that donors seek out areas where the local population have certain attitudes or are more responsive to new opportunities, either deliberately to increase the chances of a successful project or because this correlates with other factors that go into project placement decisions. The opposite may be true if donors are targeting particularly poor or isolated communities.

Our proposed strategy draws on recent developments in the literature on matching (Imbens and Rubin, 2015) to offer an estimate of the treatment effect as robust as possible, although based on a conditional independence assumption. This strategy has four key ingredients. First, we exploit the rich set of geospatial variables in the LSMS and DHS, respectively, and add several predetermined "need" indicators from the 2002 and 1998 censuses, described in the previous section, in order to provide a large set of presumably exogenous covariates. Second, we carefully assess the overlap in the distribution of covariates between treated and control units and improve it as much as possible through propensity score matching and iterative trimming of the sample. Third, we let the data determine which covariates provide the best model for the conditional expectations through a stepwise regression algorithm (Imbens and Rubin, 2015). Fourth, we choose a blocking estimator based on averaging the Ordinary Least Squares (OLS) coefficients separately estimated in a number of data-defined homogeneous subsamples, or *blocks*. The within-block estimate does not rely as much on extrapolation as it would in the full sample since the covariates are well balanced within the block. This whole process reduces one type of bias in the OLS estimation, related to functional form assumptions and extrapolation across potentially very different covariate distributions. The resulting estimator has also better small-sample properties and is robust to the difference in covariates distribution by treatment status (Imbens, 2015).

The first part of the analysis only uses the treatment indicators and the covariates. The covariates to be used in the matching will be determined by a data-driven algorithm.[12] We therefore start off, as mentioned above, using all the geographic and climate-related variables from the LSMS and DHS in the corrsponding dataset, and some predetermined "need" indicators from the 2002 and 1998 census for, respectively, Uganda and Malawi. These are the variables shown in Tables A1 and A3. We also add some contemporaneous covariates from the LSMS that we think might affect the outcomes, but are not very likely to be affected by the treatment in the short run, such as the education level of the mother and father in the household. As we see, the difference is significant for a substantial number of covariates, indicating that the overlap between treated and control households is not very good.

Trimming the sample is a way to improve the overlap between the covariates distribution for the treated and control units, i.e. to make the two groups more similar. A trimming rule that works well with multiple covariates, and which is not sensitive to outliers, is based on propensity scores. This trimming rule suggests the exclusion of observations with extreme values, i.e. close to 0 or 1 (Imbens, 2015). This requires the estimation of the propensity scores. As mentioned above, we take the most agnostic approach and

[12] This stage has no structural or causal interpretation. Moreover, as already emphasized, data on outcomes are not used at this point. Specification search is therefore acceptable, as noted also in Imbens and Rubin (2015), as "the role of these covariates is purely mechanical in balancing the two samples and approximate accurately the conditional expectation". Potential role for theory at this stage is purely supportive, if the researcher has relevant information guiding the choice of factors.

let the data determine which covariates provide the best model for the conditional expectations. The selection of covariates to be included in the estimation of the propensity scores is left to a subset selection algorithm.[13] The list of chosen factors and the corresponding parameter estimates for the propensity score equation are presented in Tables A5 and A6.

Propensity scores are estimated with two alternative linking functions. The correlation is very close to 1. The probit model gives a lower log-likelihood value and slightly less density at the extremes of the propensity scores distribution compared to the logit model, so we proceed with this model. To improve the overlap even further, we proceed with a one-to-one caliper matching, identifying the closest pairs of treatment and control observations. We then restrict the sample to these pairs of households/clusters. The plots in Figures A1 and A2 show the density of propensity scores by treatment status before and after trimming the sample for the LSMS and DHS samples, respectively. The probability of success is clearly lower for non-treated observations, and the interval of overlap between the two samples is substantial after trimming, for both the LSMS and DHS samples. Tables A2 and A3 show that covariate averages remain to a large extent significantly different even after trimming, but for much fewer variables, in particular in the DHS sample (see Column 5 in both tables; Column 6 reports as a reference the p-values for the untrimmed data).

Although with might be concerned that treated and control locations are still not homogeneous, this concern is partly mitigated by our choice of estimator. For the estimation of the treatment effect, we use a blocking estimator proposed by Imbens (2015). This involves, in essence, of partitioning the data into smaller subsamples, or *blocks*, that are more homogeneous in terms of the covariates distribution compared to the whole sample. Observations in each estimating block are selected and grouped to be more similar in terms of covariates than what the full-sample Tables A2 and A3 show. The treatment effect is estimated using OLS within each of these blocks, and then averaging the coefficients across blocks with weights taking into account the block composition in terms of treated and control units. The division into blocks is completely data-driven, according to an algorithm developed in Imbens and Rubin (2015).[14] The linear regression in each of the blocks does not rely as much on extrapolation as it would in the full sample, since the covariates are well balanced within the block. In fact, the larger the number of blocks, the more we can infer that the sample is heterogenous, and the larger the bias in the OLS coefficient.

When it comes to the second treatment, i.e. exposure to gender-specific aid, we use the subsample that has been exposed to at least one gender-specific aid project. The comparison group for this treatment is the same, namely households/clusters not exposed to any aid projects at all. The fact that there are far fewer gender-specific projects and that these were implemented relatively more recently reduces the number of observations for these estimations.

[13] Subset selection is a technique to select among a large number of potential predictors. Other related methods are ridge regression and lasso (Tibshirani, 1996; Belloni et al., 2012). Starting with a large number p of predictors, for each $k < p$ we fit all k models that contain exactly k predictors, and pick the best model, i.e. the one with the smallest RSS. Among the k thus selected, we identify a single best model using cross-validated prediction error. This means we randomly split the data into ten subsets, estimate each model on nine of these subsets, obtain the out-of-sample prediction for the left-out subset of locations, and calculate the mean squared error in prediction. We repeat this over all the k alternatives and choose the one that minimizes the mean squared error in the out-of-sample prediction. We therefore let the data determine the best predictors for aid placement, only assuming that the functional form $f(\cdot)$ is linear.

[14] Researchers have often used five subsamples with an equal number of units. This rule was, however, developed for the case of a single normally distributed covariate (Cochrane, 1968) and should not be considered generally applicable.

While the propensity score, and hence the division into blocks, is based on a binary definition of the treatment, namely exposure, or lack thereof, to any number of (gender-specific) aid projects, we know that the number and duration of these projects varies substantially in the sample. In order to capture the variation in the intensity of treatment, we estimate the following equation within each block:

$$Y = \alpha + \beta * AidPrYr + \mathbf{M}'\gamma + \delta * \overline{PrYr}_d + \epsilon \quad (1)$$

where $AidPrYr$ is the variable of interest, the number of (gender) aid project-years that the household or individual is exposed to. The estimated effect β can thus be interpreted as the impact of the marginal project-year. Tables B1–B12 report, along with β, the impact size evaluated at the mean project-years of exposure for the treated units. The regressions also include and control for all the matching factors (the matrix **M** in equation 1) plus the number of (gender) project-years implemented at the district level, the household size, a dummy for urban setting and a country dummy. The gender-aid equations also include a control for the total number of project-years in the vicinity of the household/cluster. Individual-level outcome equations include further controls for age and a Muslim dummy.

Given our broad approach, looking for impact in a multifaceted definition of "gender gaps" through many outcomes, we face an issue of multiple hypothesis testing. When testing separately a large number of hypotheses, there is some probability of observing a few significant results just by chance even if all of the tests are in fact not significant. A standard, and quite straightforward, solution to this issue is the Bonferroni method, which tests the significance of individual coefficients viewed as part of a family of hypotheses, by simply using a critical value for statistical significance of α/n rather than α, where n is the number of hypotheses. This correction assumes, however, that the outcomes are independent. Since many of our outcomes are very much correlated or even interdependent, we follow Sankoh et al. (1997) and adjust the Bonferroni correction for correlation.[15] The families of outcomes we adjust in this way are the following: labor force participation and consumption; education; household chores; household decision-making; attitudes toward domestic violence; experience of domestic violence; and attitudes on women's sexual freedom.

5 Results

In this section, we illustrate the marginal effects of aid-financed project-years with a visual presentation. All the corresponding tables can be found in Appendix B.

As already mentioned, we consider the effect of exposure to any type of aid project within 15 km of the household implemented during 1999–2011, as well as the particular effect of exposure to gender-targeted aid projects within the same geographical and temporal range. The control group is the same in both cases; namely households that had no aid-financed projects in their neighborhood during this time period. In the analysis with gender-targeted projects, we control for the number of aid-financed project-

[15] The confidence intervals reported in the figures and tables do not reflect this correction; however, the significance level reported in the plots and the p-values in the tables are, for the relevant variables, the corrected ones.

years so that we get an estimate of the additional marginal effect of another year of a gender-targeted aid project.

For every outcome, the figures report the estimated effect and confidence intervals evaluated in relation to the mean of the control group, for both the blocking and OLS estimators, as well as the number of observations, which varies across outcomes according to the survey response rate. Since our main results are obtained through the blocking estimator, defined in the previous section, the figures also report the number of blocks used in each estimation. The stars indicate the significance level of the blocking estimator. The p-values are adjusted for the multiple hypotheses problem. All this information, together with the mean in the outcome variables for the control group and the "raw" point estimates (i.e., not evaluated at the mean), can also be found in Tables B1 to B12. In these tables, we also report the impact at the mean (gender) project-years.

The results reported in Figure 1 suggest that general aid presence is positive for women's labor force participation. Women who are exposed to aid are more likely to report having worked outside the home and less likely to report having worked on the household farm in the last 12 months compared to women with no exposure to aid projects. This effect appears to be even larger if the aid projects had a specific gender component, as suggested by the positive point estimate in Figure 2, which gives the additional marginal effect of another gender-specific project-year controlling for exposure to general-aid project-years.

The increase in female labor force participation associated with more exposure to general aid projects also translates into an increase in monthly consumption and an increased share of total expenditure spent on children's health and education. Moreover, when a household is exposed to more gender-specific aid projects, monthly consumption as well as the share of total expenditures spent on children increase even further, suggesting that women's relative standing may be particularly improved by more exposure to gender-specific aid.

The results for children's primary and secondary schooling do not, however, align very well with this. Figures 3 and 4 report the results of exposure to general aid, and the additional effect of exposure of gender-specific aid, on children's school attendance. Girls in households more exposed to general aid projects are on average less likely to attend primary school than girls in households with less or no exposure to aid. Boys are, on the other hand, more likely to attend primary school if they live in an area with more exposure to general aid, but if this exposure comes from aid projects with a specific gender component, the effect is the opposite. In terms of secondary schooling, attendance appear to be unrelated to aid presence for girls and negative for boys.

From Figures 5 and 6, it seems as if there is a reallocation of time use within families following more exposure to aid. Time spent on fetching water and collecting firewood is shifted from men and girls to women and boysin areas with more exposure to general aid. The exposure to gender-specific aid projects shifts the burden of collecting firewood from adults to children, and fetching water from men to everyone else in the family. Summing up, only men seem to be consistently benefited by aid in this respect.

Perhaps as a result of the increased female labor force participation, women that are more exposed to

general aid and, in particular gender-specific aid, are more likely to report that they at least jointly with their husband participate in household decision-making, and they are less likely to report that they do not participate in any household decision-making (Figures 7–8). Women are also found to be less tolerant of wife beating and, if exposed to gender-specific projects, less likely to have experienced emotional violence in the past 12 months (Figures 11–12). Men's attitudes toward wife beating is also only affected by exposure to gender-specific aid projects. In terms of women's sexual freedom, on the other hand, the impact on both men's and women's attitudes are more mixed (Figures 9–10).

All in all, our results suggest that more exposure to general aid, and in particular to aid projects with a specific gender component, may help in changing women's relative standing in the household and related attitudes. However, as seems clear from our results on time use and women's sexual freedom, certain areas may be harder or take a longer time to change. Although our results may still suffer from selection and/or omitted variable bias, notwithstanding the measures we take to control for selection of project location and other confounding variables, the pattern in the results is not completely consistent with a pure story of selection either. The results on work outside the household and on the farm may reflect an urban bias that we do not manage to completely eliminate through matching and controlling for confounding factors. This interpretation, though, would be difficult to square with the finding that women in proximity of aid projects spend more time fetching water and collecting firewood. As anticipated in the Introduction, we feel that the best way to make sense of our results is to take into consideration that the potential for aid to make a difference at the community level depends on the existing constraints against changing any particular dimension of female empowerment. In areas where norms strongly favor different roles for men and women, where no progressive legislation exists, and where men perceive little personal gain, change may be particularly slow. The division of household chores may be one such area. At the other end of the spectrum are areas where norms are more fluid, where legislation favors equality, and where also men can see a benefit to themselves or at least their children. One such area may be income opportunities for women, and, in this area, one may expect changes to be faster. Lastly, one should keep in mind that it may be easier to affect reported attitudes than actual attitudes or behavior. Detecting a change in reported attitudes toward domestic violence or sexual empowerment may not necessarily mean that behavior has also changed. Using this lens on our results suggests that aid may indeed have a moderate positive impact on dimensions of female empowerment that are relatively easier to influence in a short to medium term. We also find that this is particularly true for projects with a specific gender component, putting into question the effectiveness, or commitment to, gender mainstreaming.

6 Conclusions

Until recently, studies of the impact of foreign aid have typically taken one of two forms: on the one hand, cross-country comparisons of the impact of aggregate aid inflows on countrywide indicators such as growth, savings, or level of democracy; on the other, micro-level impact evaluations of the effect on immediate beneficiaries from a particular aid-financed intervention. The results from these studies are often contradictory, as suggested by the so-called micro-macro paradox. The newly emerging geo-coded data on the history of aid project locations create an opportunity to find a middle ground: looking at community-level effects in the vicinity of aid projects, including both direct beneficiaries and others who

may, or may not, be affected indirectly. In this paper, we have taken this route to examine the impact of foreign-aid-financed activities on female empowerment, an important objective of most Western donors that, beyond impact evaluations of individual aid-financed interventions, has received surprisingly little attention in the economics literature.

Our results suggests a mixed impact of foreign aid, with improvements in the sphere of economic independence, household decision-making, and domestic violence, but unclear, or even clearly negative, in terms of time use, children's education, and attitudes around women's sexual freedoms. At the present, we do not have a unifying model to make sense of these results. What we propose is the idea that different areas of gender equality face different intensity of resistance to change. At the same time, even if we take measures to reduce selection and omitted variable bias, we cannot rule out completely that our results may still suffer from such biases.

In this first attempt, we paint with a big brush, trying to capture a broad spectrum of dimensions related to the idea of female empowerment, at the cost of missing the details. Each of the areas we strive to include is complex in itself, and to really understand the non-trivial interactions with aid interventions, a deeper analysis of channels and unpackaging of mechanisms is needed. We plan to do this in forthcoming work. However, we still think this approach is valuable as a first overall picture, and we hope that it can pave the way for future work in this area.

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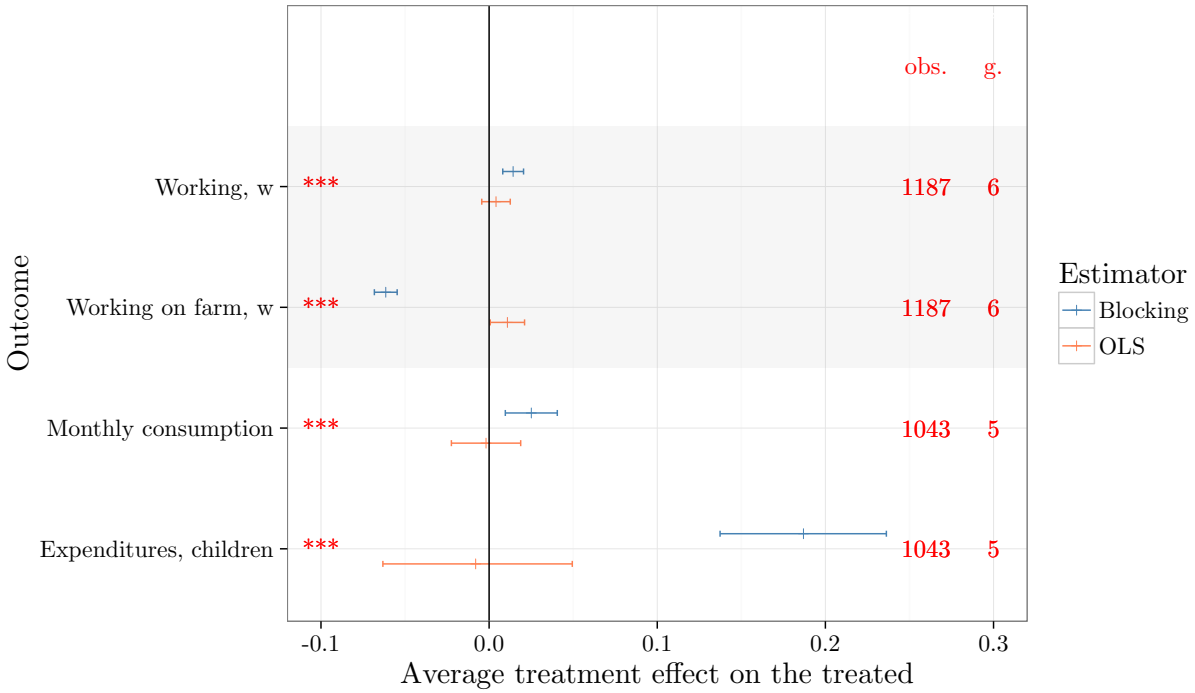
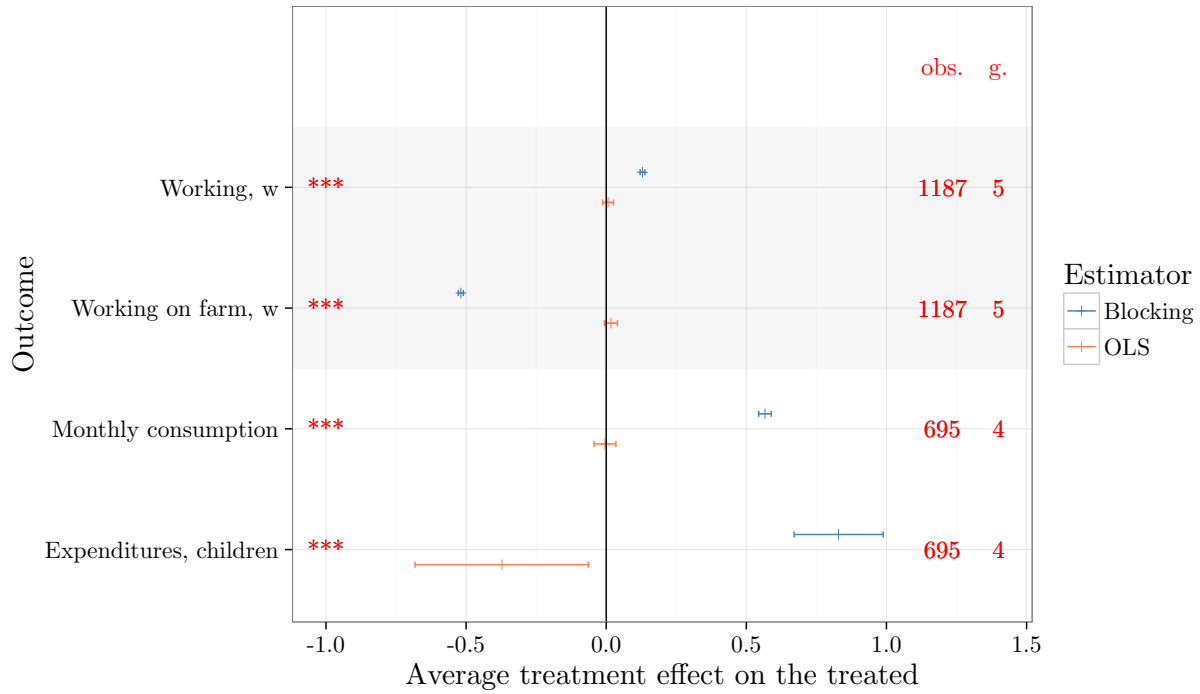
Figures and Tables

Figure 1: Labor force participation and consumption – Marginal effect of aid project-years



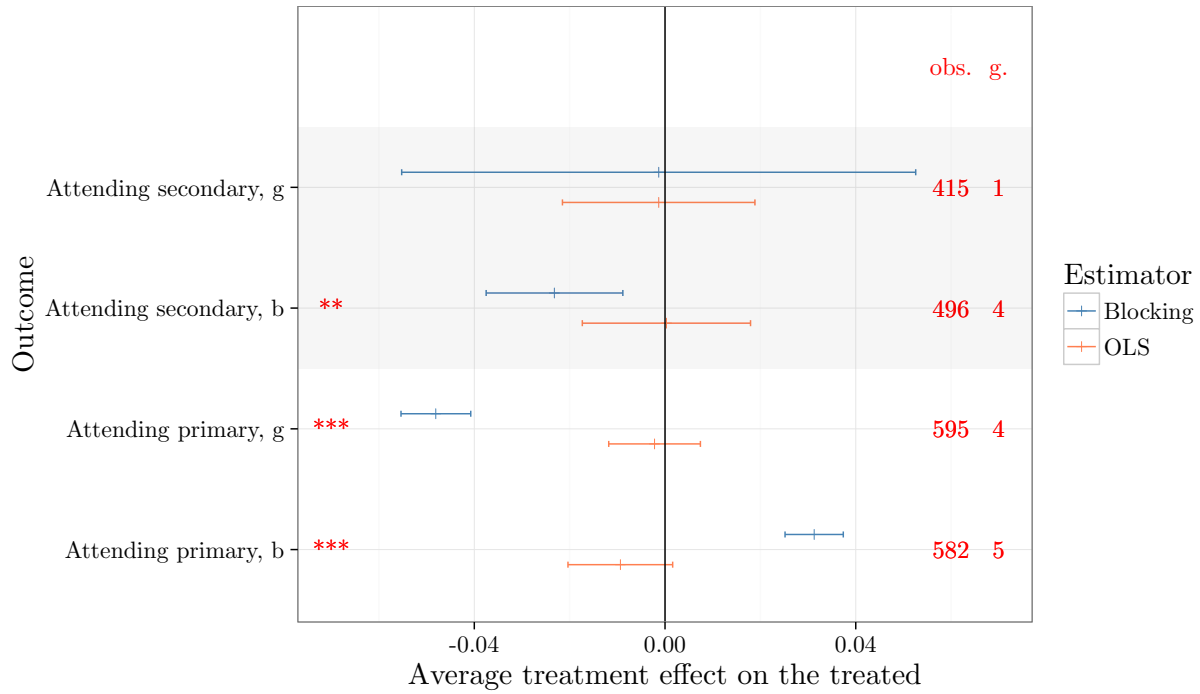
Notes: Data on labor force participation and consumption are retrieved from Ugandan National Panel Survey (2013/2014) and Malawi Integrated Household Panel Survey (2013). **Working, w** is share of women in the household that has worked outside the home in the past 12 months. **Working on farm, w** is the share of women who has worked in the field in the past 12 months. **Monthly consumption** is monthly nominal household expenditures. **Expenditures, children** is the share of total household expenditures spent on health and education for children in the past 12 months. The explanatory variable of interest, **Aid Project-Years**, is the number gender aid project years within a 15 km radius of the household. All regressions control for all the matching factors, the number of aid-project years at district level, household size, a dummy for urban setting, and a dummy for country. For each outcome, we present two coefficient estimates evaluated at the mean in the control group. Blue represents the blocking estimates and confidence intervals, orange represents the ordinary least squares (OLS) estimates and confidence intervals. **obs.** is the number of observations. **g.** is the number of blocks used in the blocking estimation. Stars indicate the significance level after adjusting for the multiple hypothesis problem using the Bonferroni method adjusted for correlation, with *p<0.05, **p<0.01, ***p<0.005.

Figure 2: Labor force participation and consumption – Marginal effect of gender project-years



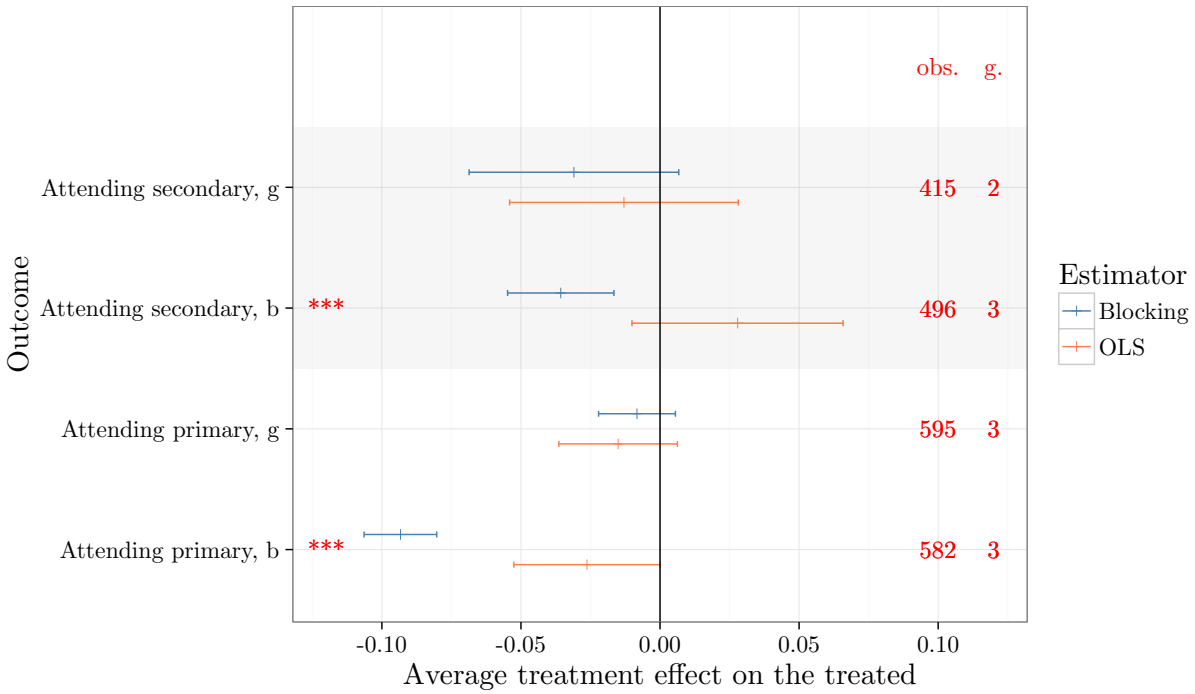
Notes: Data on labor force participation and consumption are retrieved from Ugandan National Panel Survey (2013/2014) and Malawi Integrated Household Panel Survey (2013). **Working, w** is share of women in the household that has worked outside the home in the past 12 months. **Working on farm, w** is the share of women who has worked in the field in the past 12 months. **Monthly consumption** is monthly nominal household expenditures. **Expenditures, children** is the share of total household expenditures spent on health and education for children in the past 12 months. The explanatory variable of interest, **Gender Aid Project-Years**, is the number gender-targeted aid project years within a 15 km radius of the household. All regressions control for all the matching factors, the number of aid-project years at district level, household size, a dummy for urban setting, a dummy for country, and the total number of general aid project-years within a 15 km radius of the household. For each outcome, we present two coefficient estimates evaluated at the mean in the control group. Blue represents the blocking estimates, orange represents the ordinary least squares (OLS) estimates. **obs.** is the number of observations. **g.** is the number of blocks used in the blocking estimation. Stars indicate the significance level after adjusting for the multiple hypothesis problem using the Bonferroni method adjusted for correlation, with *p<0.05, **p<0.01, ***p<0.005.

Figure 3: Education – Marginal effect of aid project-years



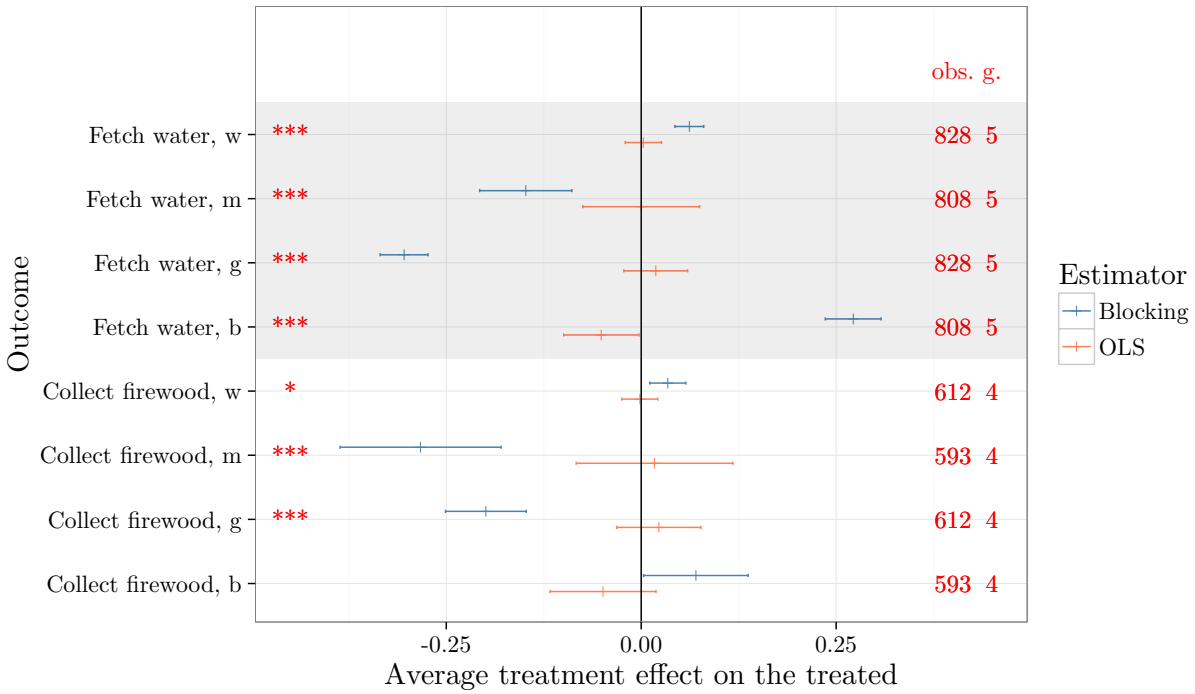
Notes: Data on school attendance are retrieved from Ugandan National Panel Survey (2013/2014) and Malawi Integrated Household Panel Survey (2013). **Attending secondary, g (b)** is a dummy taking the value 1 if girls (boys) in the relevant cohort at the time of the survey was currently attending secondary school. **Attending primary, g (b)** is a dummy taking the value 1 if girls (boys) in the relevant cohort at the time of the survey was currently attending primary school. The explanatory variable of interest, **Aid Project-Years**, is the number aid project years within a 15 km radius of the household. All regressions control for all the matching factors, the number of aid-project years at district level, household size, a dummy for urban setting, and a dummy for country. For each outcome, we present two coefficient estimates evaluated at the mean in the control group. Blue represents the blocking estimates and confidence intervals, orange represents the ordinary least squares (OLS) estimates and confidence intervals. **obs.** is the number of observations. **g.** is the number of blocks used in the blocking estimation. Stars indicate the significance level after adjusting for the multiple hypothesis problem using the Bonferroni method adjusted for correlation, with *p<0.05, **p<0.01, ***p<0.005.

Figure 4: Education – Marginal effect of gender project-years



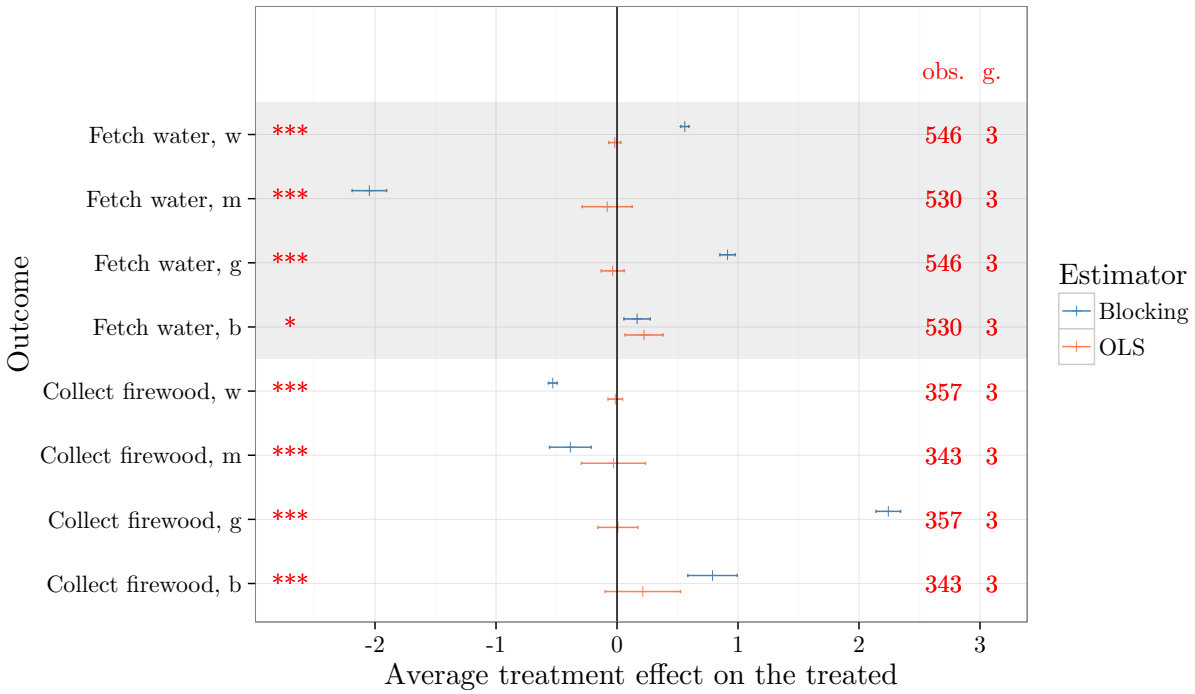
Notes: Data on school attendance are retrieved from Ugandan National Panel Survey (2013/2014) and Malawi Integrated Household Panel Survey (2013). **Attending secondary, g (b)** is a dummy taking the value 1 if girls (boys) in the relevant cohort at the time of the survey was currently attending secondary school. **Attending primary, g (b)** is a dummy taking the value 1 if girls (boys) in the relevant cohort at the time of the survey was currently attending primary school. The explanatory variable of interest, **Gender Aid Project-Years**, is the number gender-targeted aid project years within a 15 km radius of the household. All regressions control for all the matching factors, the number of aid-project years at district level, household size, a dummy for urban setting, a dummy for country, and the total number of general aid project-years within a 15 km radius of the household. For each outcome, we present two coefficient estimates evaluated at the mean in the control group. Blue represents the blocking estimates and confidence intervals, orange represents the ordinary least squares (OLS) estimates and confidence intervals. **obs.** is the number of observations. **g.** is the number of blocks used in the blocking estimation. Stars indicate the significance level after adjusting for the multiple hypothesis problem using the Bonferroni method adjusted for correlation, with *p<0.05, **p<0.01, ***p<0.005.

Figure 5: Time use – Marginal effect of aid project-years



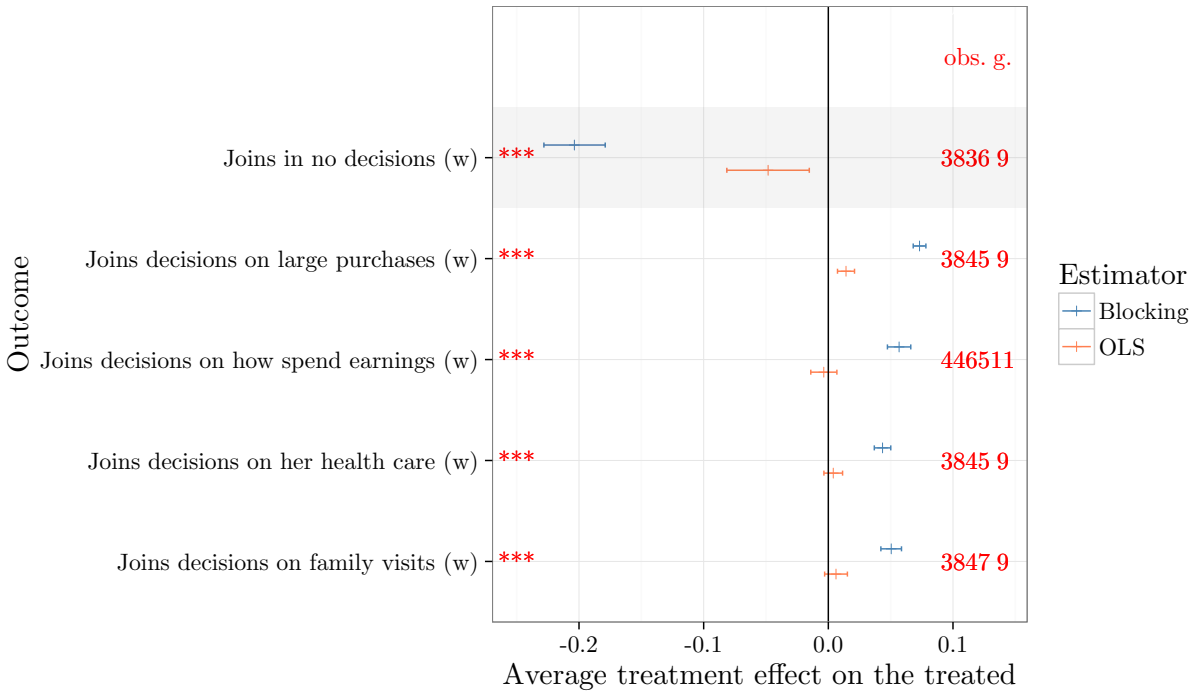
Notes: Data on time use are retrieved from Ugandan National Panel Survey (2013/2014) and Malawi Integrated Household Panel Survey (2013). **Fetch water** is the share of a household's total time spent on fetching water carried out by the women, men, girls and boys in the household, respectively. **Collect firewood** is the share of a household's total time spent on collecting firewood carried out by the women, men, girls and boys in the household, respectively. The explanatory variable of interest, **Aid Project-Years**, is the number gender aid project years within a 15 km radius of the household. All regressions control for all the matching factors, the number of aid-project years at district level, household size, a dummy for urban setting, and a dummy for country. For each outcome, we present two coefficient estimates evaluated at the mean in the control group. Blue represents the blocking estimates and confidence intervals, orange represents the ordinary least squares (OLS) estimates and confidence intervals. **obs.** is the number of observations. **g.** is the number of blocks used in the blocking estimation. Stars indicate the significance level after adjusting for the multiple hypothesis problem using the Bonferroni method adjusted for correlation, with *p<0.05, **p<0.01, ***p<0.005.

Figure 6: Time use – Marginal effect of gender project-years



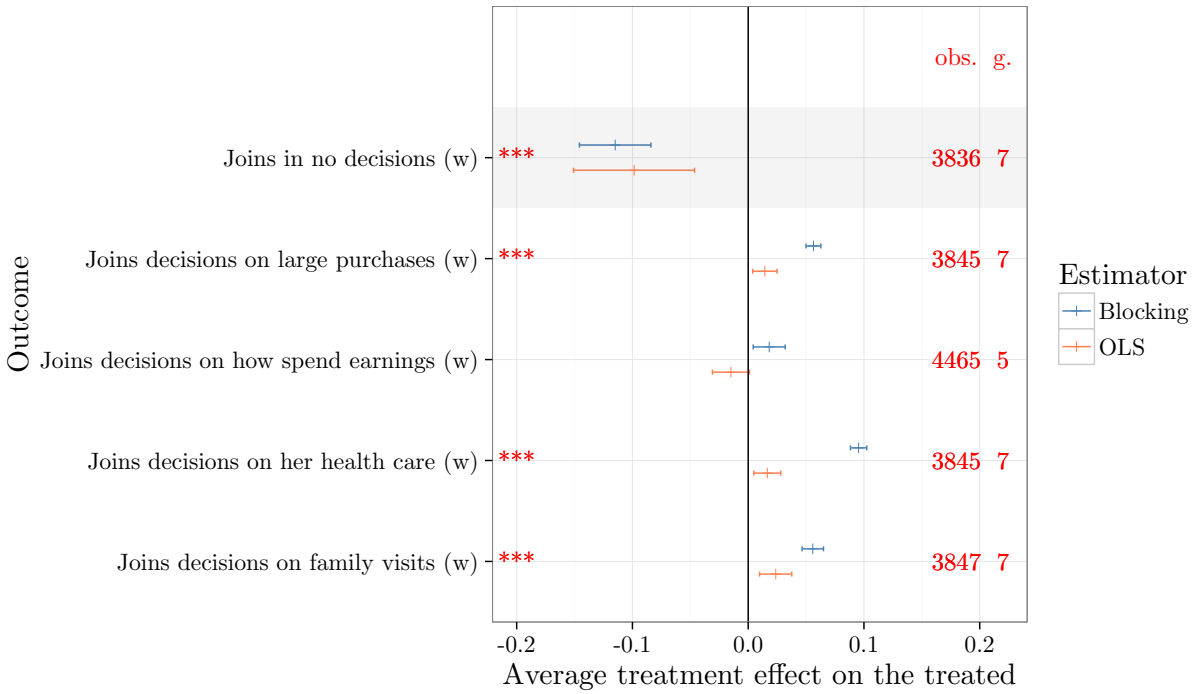
Notes: Data on time use are retrieved from Ugandan National Panel Survey (2013/2014) and Malawi Integrated Household Panel Survey (2013). **Fetch water** is the share of a household's total time spent on fetching water carried out by the women, men, girls and boys in the household, respectively. **Collect firewood** is the share of a household's total time spent on collecting firewood carried out by the women, men, girls and boys in the household, respectively. The explanatory variable of interest, **Gender Aid Project-Years**, is the number gender-targeted aid project years within a 15 km radius of the household. All regressions control for all the matching factors, the number of aid-project years at district level, household size, a dummy for urban setting, a dummy for country, and the total number of general aid project-years within a 15 km radius of the household. For each outcome, we present two coefficient estimates evaluated at the mean in the control group. Blue represents the blocking estimates, orange represents the ordinary least squares (OLS) estimates. **obs.** is the number of observations. **g.** is the number of blocks used in the blocking estimation. Stars indicate the significance level after adjusting for the multiple hypothesis problem using the Bonferroni method adjusted for correlation, with *p<0.05, **p<0.01, ***p<0.005.

Figure 7: Household decision-making – Marginal effect of aid project-years



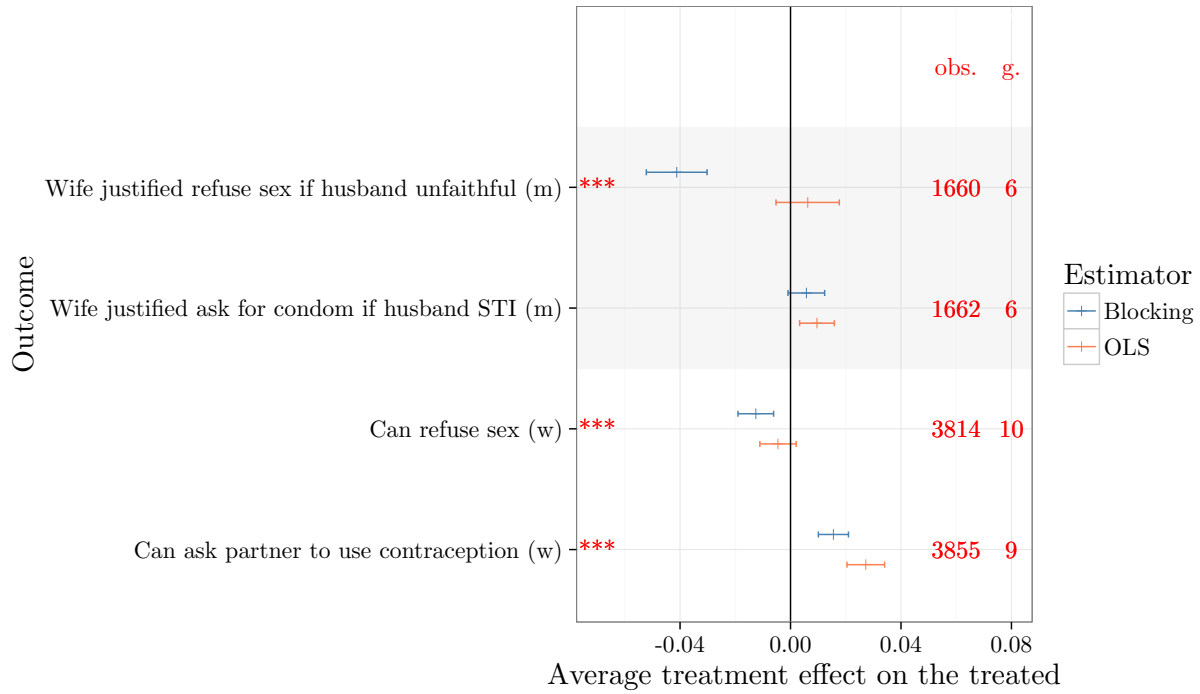
Notes: Data on participation in household decision-making are retrieved from Uganda DHS (2011) and Malawi DHS (2010). **Joins decisions on large purchases (w)** is a dummy taking the value 1 if wife, on her own or jointly with her husband, makes decisions about major household purchases. **Joins decisions on earnings (w)** is a dummy taking the value 1 if wife, on her own or jointly with her husband, makes decisions on how to spend her own and her husband's earnings. **Joins decisions on her health care (w)** is a dummy taking the value 1 if wife, on her own or jointly with her husband, makes decisions about her own health care. **Joins decisions on family visits (w)** is a dummy taking the value 1 if wife, on her own or jointly with her husband, makes decisions on visits to her family or relatives. **Joins in no decisions (w)** is a dummy taking the value 1 if wife do not participate in making any of the previous decisions. The explanatory variable of interest, **Aid Project-Years**, is the number gender aid project years within a 15 km radius of the household. All regressions control for all the matching factors, the number of aid-project years at district level, household size, a dummy for urban setting, and a dummy for country. For each outcome, we present two coefficient estimates evaluated at the mean in the control group. Blue represents the blocking estimates and confidence intervals, orange represents the ordinary least squares (OLS) estimates and confidence intervals. **obs.** is the number of observations. **g.** is the number of blocks used in the blocking estimation. Stars indicate the significance level after adjusting for the multiple hypothesis problem using the Bonferroni method adjusted for correlation, with *p<0.05, **p<0.01, ***p<0.005.

Figure 8: Household decision-making – Marginal effect of gender project-years



Notes: Data on domestic violence are retrieved from Uganda DHS (2011) and Malawi DHS (2010). **Joins decisions on large purchases (w)** is a dummy taking the value 1 if wife, on her own or jointly with her husband, makes decisions about major household purchases. **Joins decisions on earnings (w)** is a dummy taking the value 1 if wife, on her own or jointly with her husband, makes decisions on how to spend her own and her husband's earnings. **Joins decisions on her health care (w)** is a dummy taking the value 1 if wife, on her own or jointly with her husband, makes decisions about her own health care. **Joins decisions on family visits (w)** is a dummy taking the value 1 if wife, on her own or jointly with her husband, makes decisions on visits to her family or relatives. **Joins in no decisions (w)** is a dummy taking the value 1 if wife do not participate in making any of the previous decisions. The explanatory variable of interest, **Gender Aid Project-Years**, is the number gender-targeted aid project years within a 15 km radius of the household. All regressions control for all the matching factors, the number of aid-project years at district level, household size, a dummy for urban setting, a dummy for country, the total number of general aid project-years within a 15 km radius of the household, age, and a dummy for being Muslim. For each outcome, we present two coefficient estimates evaluated at the mean in the control group. Blue represents the blocking estimates, orange represents the ordinary least squares (OLS) estimates. **obs.** is the number of observations. **g.** is the number of blocks used in the blocking estimation. Stars indicate the significance level after adjusting for the multiple hypothesis problem using the Bonferroni method adjusted for correlation, with *p<0.05, **p<0.01, ***p<0.005.

Figure 9: Women's sexual freedom – Marginal effect of aid project-years

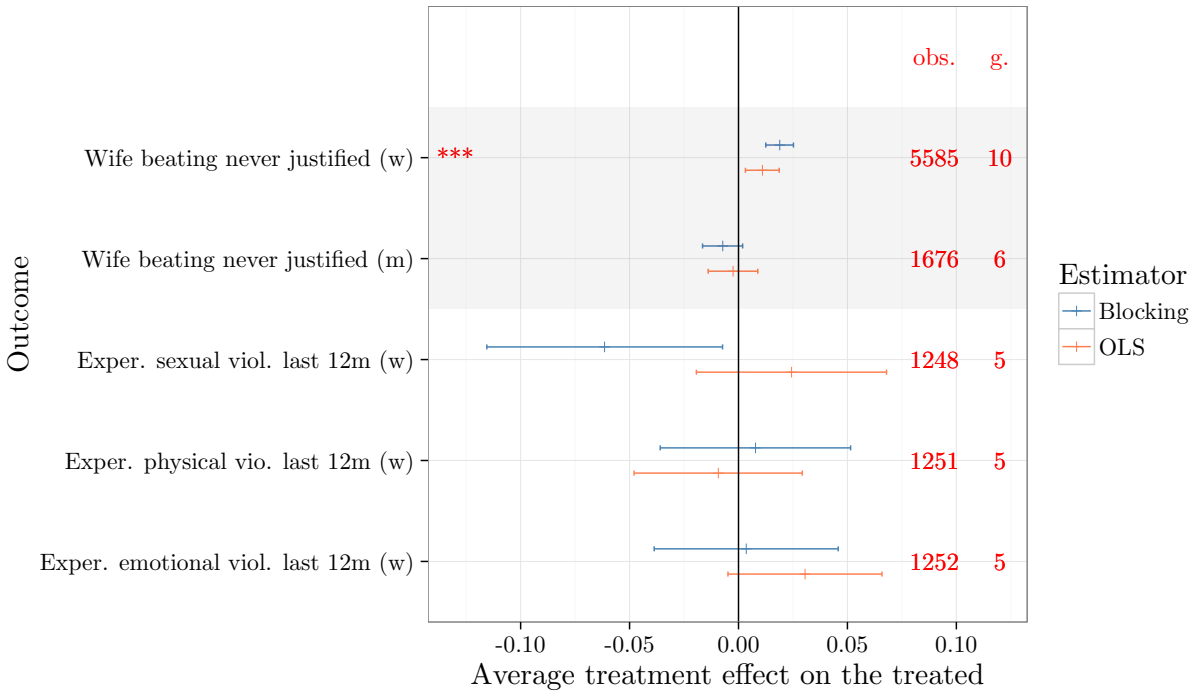


Notes: Data on women's sexual freedom are retrieved from Uganda DHS (2011) and Malawi DHS (2010). **Can refuse sex if m unfaithful (m)** is a dummy taking the value 1 if husband deem it justified for a wife to refuse to have sex with her husband when she knows her husband has sex with women other than his wives. **Can refuse sex (w)** is a dummy taking the value 1 if wife agrees that she can say to her husband if she does not want to have sexual intercourse. **Can ask for condom if m STI (m)** is a dummy taking the value 1 if husband deem it justified for a wife to ask that they use a condom if she knows her husband has a sexually transmitted infection (STI). **Can ask for condom (w)** is a dummy taking the value 1 if a wife agrees that she can ask her husband to use a condom if she wanted him to. The explanatory variable of interest, **Aid Project-Years**, is the number gender aid project years within a 15 km radius of the household. All regressions control for all the matching factors, the number of aid-project years at district level, household size, a dummy for urban setting, and a dummy for country. For each outcome, we present two coefficient estimates evaluated at the mean in the control group. Blue represents the blocking estimates and confidence intervals, orange represents the ordinary least squares (OLS) estimates and confidence intervals. **obs.** is the number of observations. **g.** is the number of blocks used in the blocking estimation. Stars indicate the significance level after adjusting for the multiple hypothesis problem using the Bonferroni method adjusted for correlation, with *p<0.05, **p<0.01, ***p<0.005.

Figure 10: Women's sexual freedom – Marginal effect of gender project-years

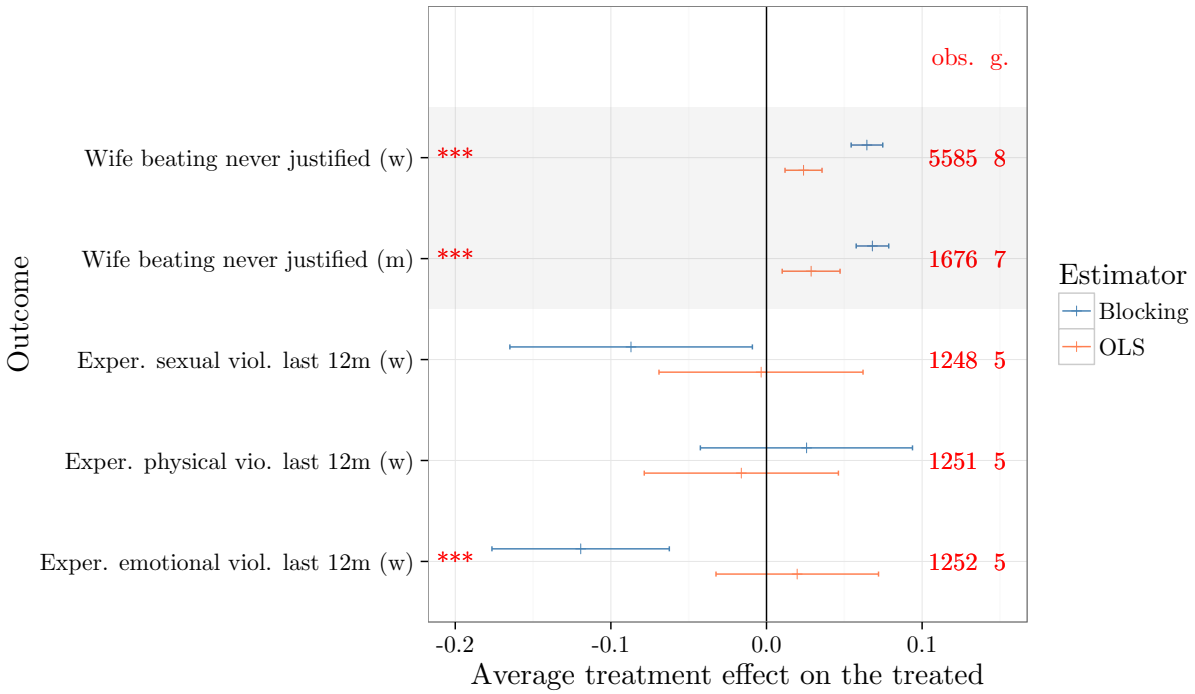
Notes: Data on women's sexual freedom are retrieved from Uganda DHS (2011) and Malawi DHS (2010). **Can refuse sex if m unfaithful (m)** is a dummy taking the value 1 if husband deem it justified for a wife to refuse to have sex with her husband when she knows her husband has sex with women other than his wives. **Can refuse sex (w)** is a dummy taking the value 1 if wife agrees that she can say to her husband if she does not want to have sexual intercourse. **Can ask for condom if m STI (m)** is a dummy taking the value 1 if husband deem it justified for a wife to ask that they use a condom if she knows her husband has a sexually transmitted infection (STI). **Can ask for condom (w)** is a dummy taking the value 1 if a wife agrees that she can ask her husband to use a condom if she wanted him to. The explanatory variable of interest, **Gender Aid Project-Years**, is the number gender-targeted aid project years within a 15 km radius of the household. All regressions control for all the matching factors, the number of aid-project years at district level, household size, a dummy for urban setting, a dummy for country, the total number of general aid project-years within a 15 km radius of the household, age, and a dummy for being Muslim. For each outcome, we present two coefficient estimates evaluated at the mean in the control group. Blue represents the blocking estimates, orange represents the ordinary least squares (OLS) estimates. **obs.** is the number of observations. **g.** is the number of blocks used in the blocking estimation. Stars indicate the significance level after adjusting for the multiple hypothesis problem using the Bonferroni method adjusted for correlation, with *p<0.05, **p<0.01, ***p<0.005.

Figure 11: Domestic violence – Marginal effect of aid project-years



Notes: Data on domestic violence are retrieved from Uganda DHS (2011) and Malawi DHS (2010). **Wife beating never justified (w/m)** is a dummy taking the value 1 if wife/husband agrees do not agree that husband is justified in beating his wife for any of the following reasons: burns the food; argues with him; goes out without telling him; neglects the children; or refuses to have sexual intercourse with him. **Exper. sexual viol. last 12m (w)** is a dummy taking the value 1 if wife has experienced sexual violence in the last 12 months preceding the survey. **Exper. physical viol. last 12m (w)** is a dummy taking the value 1 if wife has experienced physical violence in the last 12 months preceding the survey. **Exper. emotional viol. last 12m (w)** is a dummy taking the value 1 if wife has experienced emotional violence in the last 12 months preceding the survey. The explanatory variable of interest, **Aid Project-Years**, is the number gender aid project years within a 15 km radius of the household. All regressions control for all the matching factors, the number of aid-project years at district level, household size, a dummy for urban setting, and a dummy for country. For each outcome, we present two coefficient estimates evaluated at the mean in the control group. Blue represents the blocking estimates and confidence intervals, orange represents the ordinary least squares (OLS) estimates and confidence intervals. **obs.** is the number of observations. **g.** is the number of blocks used in the blocking estimation. Stars indicate the significance level after adjusting for the multiple hypothesis problem using the Bonferroni method adjusted for correlation, with *p<0.05, **p<0.01, ***p<0.005.

Figure 12: Domestic violence – Marginal effect of gender project-years



Notes: Data on participation in household decision-making are retrieved from Uganda DHS (2011) and Malawi DHS (2010). **Wife beating never justified (w/m)** is a dummy taking the value 1 if wife/husband agrees do not agree that husband is justified in beating his wife for any of the following reasons: burns the food; argues with him; goes out without telling him; neglects the children; or refuses to have sexual intercourse with him. **Exper. sexual viol. last 12m (w)** is a dummy taking the value 1 if wife has experienced sexual violence in the last 12 months preceding the survey. **Exper. physical viol. last 12m (w)** is a dummy taking the value 1 if wife has experienced physical violence in the last 12 months preceding the survey. **Exper. emotional viol. last 12m (w)** is a dummy taking the value 1 if wife has experienced emotional violence in the last 12 months preceding the survey. The explanatory variable of interest, **Gender Aid Project-Years**, is the number gender-targeted aid project years within a 15 km radius of the household. All regressions control for all the matching factors, the number of aid-project years at district level, household size, a dummy for urban setting, a dummy for country, the total number of general aid project-years within a 15 km radius of the household, age, and a dummy for being Muslim. For each outcome, we present two coefficient estimates evaluated at the mean in the control group. Blue represents the blocking estimates, orange represents the ordinary least squares (OLS) estimates. **obs.** is the number of observations. **g.** is the number of blocks used in the blocking estimation. Stars indicate the significance level after adjusting for the multiple hypothesis problem using the Bonferroni method adjusted for correlation, with *p<0.05, **p<0.01, ***p<0.005.

Appendix

A Summary Statistics and Propensity Score Estimates

Table A1: Summary statistics – LSMS

	Treat, N=4231		Control, N=543		t-test
	mean	(s.d.)	mean	(s.d.)	p-value
Distance to major road	7.33	8.67	12.40	11.17	0.00
Distance to pop center	27.24	20.86	36.16	18.40	0.00
Distance to borderpost	66.11	37.51	72.26	48.21	0.00
Annual temperature	213.91	18.80	217.52	17.73	0.00
Temperature wettest q	226.25	18.66	221.33	15.86	0.00
Annual rainfall	1100.36	242.42	1138.40	190.42	0.00
Rainfall wettest m	232.32	49.29	207.23	48.51	0.00
Rainfall wettest q	627.37	126.92	551.62	142.49	0.00
Percentage under agriculture within 1 km	30.98	20.31	40.70	20.55	0.00
Landcover within 1 km	5.82	4.25	4.11	3.08	0.00
Agro-ecological zones	15.73	2.01	17.11	2.00	0.00
Rainfall July-June	897.75	157.58	1024.10	168.80	0.00
Rainfall wettest q, July-June	592.72	94.59	547.36	132.83	0.00
Start of wettest q	14.85	3.91	11.82	5.65	0.00
Change in greenness, district avg.	63.08	12.90	63.89	19.08	0.20
Onset of greenness, district avg.	141.70	30.10	115.58	38.43	0.00
Onset of greenness decrease, district avg.	227.91	40.18	202.08	55.57	0.00
Peak of greenness, district avg.	0.50	0.04	0.51	0.05	0.01
Urban dummy	0.37	0.48	0.52	0.50	0.00
Father's education (months)	5.20	15.02	12.62	25.85	0.00
Mother's education (months)	7.27	19.53	21.29	33.79	0.00
Months of school, 2002 district avg.	19.82	1.42	20.81	1.16	0.00
Educational attainment, 2002 district avg.	1.03	0.12	1.05	0.07	0.00
Never been in school, 2002 district share	0.31	0.13	0.27	0.11	0.00
Access to grid, 2002 district share	0.05	0.09	0.03	0.02	0.00
Access to piped water, 2002 district share	0.18	0.15	0.11	0.09	0.00
Malawi dummy	0.80	0.40	0.46	0.50	0.00
Distance to market	21.97	16.47	36.40	14.08	0.00
Distance to adm center	22.13	20.77	39.47	25.52	0.00
Elevation	6.16	3.19	6.52	3.26	0.01
Slope	269.72	515.52	638.56	600.83	0.00

Notes: This table presents the summary statistics of all variables used in the machine learning process for the LSMS samples. The treatment group consists of all household locations with at least one aid-project within a 15 km radius, and the control group consists of all household locations without. Before the matching process, there are 4231 household locations in the treatment group and 543 household locations in the control group. The column furthest to the right reports the p-value from a t-test testing for equality of means in the treatment and control groups. The definition and source of all variables can be found at `\\http://siteresources.worldbank.org/INTLSMS/Resources/3358986-1233781970982/5800988-1271185595871/6964312-1404828635943/IHPS.Geovariables.Description.pdf` for Malawi, and at `\\http://siteresources.worldbank.org/INTLSMS/Resources/3358986-1233781970982/5800988-1265043582346/UNPS\_2009\_10\_BID\_rev\_2014.pdf` for Uganda.

Table A2: Summary statistics in the trimmed sample – LSMS

	Treat, N=524		Control, N=524		t-test (p-value)	
	mean	(s.d.)	mean	(s.d.)	Trim	Untrim
Distance to major road	6.25	7.02	12.24	11.22	0.00	0.00
Distance to pop center	36.15	24.32	36.40	18.54	0.85	0.00
Distance to borderpost	68.13	42.41	72.56	48.81	0.12	0.00
Annual temperature	225.99	19.57	217.52	17.94	0.00	0.00
Temperature wettest q	232.48	17.50	220.92	15.85	0.00	0.00
Annual rainfall	1223.29	286.82	1136.00	189.63	0.00	0.00
Rainfall wettest m	257.68	87.11	205.59	47.94	0.00	0.00
Rainfall wettest q	646.06	187.78	546.50	140.43	0.00	0.00
Percentage under agriculture within 1 km	41.63	16.77	41.18	20.50	0.70	0.00
Landcover within 1 km	3.68	2.60	3.98	2.98	0.09	0.00
Agro-ecological zones	16.38	1.92	17.18	1.99	0.00	0.00
Rainfall July-June	1035.01	142.20	1030.29	166.68	0.62	0.00
Rainfall wettest q, July-June	630.62	126.85	545.83	134.25	0.00	0.00
Start of wettest q	14.31	5.30	11.70	5.68	0.00	0.00
Change in greenness, district avg.	57.94	10.03	63.92	19.32	0.00	0.20
Onset of greenness, district avg.	134.63	38.75	114.63	38.47	0.00	0.00
Onset of greenness decrease, district avg.	211.21	43.91	201.04	55.94	0.00	0.00
Peak of greenness, district avg.	0.51	0.04	0.51	0.05	0.21	0.01
Urban dummy	0.34	0.47	0.53	0.50	0.00	0.00
Father's education (months)	7.66	20.14	12.93	26.12	0.00	0.00
Mother's education (months)	11.81	25.89	21.96	34.13	0.00	0.00
Months of school, 2002 district avg.	20.84	0.99	20.85	1.14	0.90	0.00
Educational attainment, 2002 district avg.	1.08	0.05	1.05	0.07	0.00	0.00
Never been in school, 2002 district share	0.20	0.06	0.26	0.11	0.00	0.00
Access to grid, 2002 district share	0.03	0.03	0.03	0.02	0.01	0.00
Access to piped water, 2002 district share	0.19	0.11	0.11	0.09	0.00	0.00
Malawi dummy	0.70	0.46	0.45	0.50	0.00	0.00
Distance to market	29.83	20.29	36.66	14.12	0.00	0.00
Distance to adm center	44.12	35.21	39.82	25.74	0.02	0.00
Elevation	5.75	3.33	6.62	3.25	0.00	0.01
Slope	348.35	520.63	655.19	600.01	0.00	0.00

Notes: This table presents the summary statistics of the matching variables for the household locations in the matched treatment and control groups, respectively. Both groups consist of 524 households. The two columns furthest to the right report the p-values from t-tests testing for equality of means in the treatment and control groups for the trimmed and untrimmed samples, respectively. The definition and source of all variables can be found at `\\http://siteresources.worldbank.org/INTLSMS/Resources/3358986-1233781970982/5800988-1271185595871/6964312-1404828635943/IHPS.Geovariables.Description.pdf` for Malawi, and at `\\http://siteresources.worldbank.org/INTLSMS/Resources/3358986-1233781970982/5800988-1265043582346/UNPS\_2009\_10\_BID\_rev\_2014.pdf` for Uganda.

Table A3: Summary statistics – DHS

	Treat, N=940		Control, N=131		t-test
	mean	(s.d.)	mean	(s.d.)	p-value
Uganda Dummy	0.30	0.46	0.59	0.49	0.00
Avg aridity index, 1960–90	7109.87	1572.94	6736.33	1518.49	0.01
Avg build-up index, 2000	0.09	0.21	0.01	0.01	0.00
Share Christians, pre-tre. district avg.	0.82	0.15	0.82	0.19	0.73
Avg dependency ratio, 2000	0.92	0.63	1.35	0.70	0.00
Avg drought episodes, 1980–2000	6.37	2.02	4.36	2.92	0.00
Electricity, pre-tr. district avg.	0.07	0.13	0.03	0.03	0.00
Enhanced vegetation index, 2000	3211.42	577.77	3360.79	629.97	0.01
Avg global human footprint index	38.63	17.26	29.55	8.51	0.00
Growing season length, 1961–91	9.09	2.32	9.76	2.40	0.00
Homeowner, pre-tr. district share	0.84	0.15	0.89	0.05	0.00
Avg share illit., 2000	0.38	0.15	0.39	0.19	0.80
ITN coverage, 2005	0.19	0.13	0.10	0.12	0.00
Avg share with malaria, 2000	0.43	0.12	0.46	0.12	0.01
Potential evapotranspiration, 2009	1604.71	115.99	1654.72	121.86	0.00
Dist nearest border post (m)	49706.02	40701.15	48066.16	47743.99	0.67
Dist nearest protected area (m)	54721.84	47190.73	49714.57	40907.97	0.25
Dist nearest water body (m)	44455.83	35484.00	61896.70	45849.58	0.00
Avg Rainfall, 2000	1093.69	214.63	1050.94	202.71	0.03
Male ratio, pre-tr. district avg.	0.49	0.01	0.49	0.01	0.03
Slope	1.76	1.69	1.58	1.36	0.24
Urban	1.11	0.92	0.40	0.51	0.00
Avg temp (1970–2000), Apr	21.90	1.81	22.20	1.92	0.08
Avg temp (1970–2000), Aug	19.58	2.04	20.47	2.18	0.00
Avg temp (1970–2000), Dec	23.07	1.99	22.67	1.99	0.03
Avg temp (1970–2000), Feb	22.85	1.92	23.05	2.19	0.28
Avg temp (1970–2000), Jan	22.86	1.90	22.90	2.13	0.85
Avg temp (1970–2000), Jul	18.63	2.32	19.83	2.59	0.00
Avg temp (1970–2000), Jun	18.93	2.38	20.25	2.72	0.00
Avg temp (1970–2000), Mar	22.61	1.92	22.91	2.18	0.11
Avg temp (1970–2000), May	20.47	1.93	21.28	2.30	0.00
Avg temp (1970–2000), Nov	23.74	2.23	23.13	2.08	0.00
Avg temp (1970–2000), Oct	23.32	2.14	22.65	1.86	0.00
Avg temp (1970–2000), Sep	21.54	1.78	21.74	1.78	0.24
Travel time to nearest city, 2000	169.00	122.01	264.65	131.64	0.00
Avg pop (UN), 2000	53989.83	67721.25	30455.01	22012.56	0.00
Avg pop density (UN), 2000	840.54	1683.82	122.89	126.08	0.00
Access to piped water, pre-tr. district share	0.21	0.20	0.10	0.10	0.00
Months of school, pre-tr. district avg	19.81	1.52	20.16	1.81	0.02

Notes: This table presents the summary statistics for all variables used in the machine learning process for the DHS samples. The treatment group consists of all survey cluster locations with at least one aid-project within a 15 km radius, and the control group consists of all survey cluster locations without. Before the matching process, there are 940 survey clusters in the treatment group and 131 survey clusters in the control group. The column furthest to the right reports the p-value from a t-test testing for equality of means in the treatment and control groups. The definition and source of all variables can be found at `https://spatialdata.dhsprogram.com/methodology/#GEOSPATIALCOVARIATES`.

Table A4: Summary statistics with trimmed sample – DHS

	Treat, N=111		Control, N=111		t-test (p)	
	mean	(s.d.)	mean	(s.d.)	Trim	Untrim
Uganda Dummy	0.23	0.42	0.48	0.50	0.00	0.00
Avg aridity index, 1960–90	6701.40	1409.71	6758.29	1477.52	0.79	0.01
Avg buil-up index, 2000	0.01	0.05	0.01	0.02	0.34	0.00
Share Christians, pre-tre. district avg.	0.86	0.16	0.81	0.22	0.08	0.73
Avg dependency ratio, 2000	0.93	0.79	1.18	0.65	0.02	0.00
Avg drought episodes, 1980–2000	5.82	2.57	4.22	3.00	0.00	0.00
Electricity, pre-tr. district avg.	0.03	0.05	0.03	0.02	0.11	0.00
Enhanced vegetation index, 2000	3242.39	500.23	3310.32	623.31	0.41	0.01
Avg global human footprint index	31.20	8.99	28.80	7.96	0.06	0.00
Growing season length, 1961–91	8.66	1.77	9.24	2.25	0.05	0.00
Homeowner, pre-tr. district share	0.88	0.07	0.89	0.04	0.44	0.00
Avg share illit., 2000	0.38	0.12	0.38	0.17	0.90	0.80
ITN coverage, 2005	0.18	0.11	0.12	0.12	0.00	0.00
Avg share with malaria, 2000	0.46	0.12	0.46	0.13	0.90	0.01
Potential evapotranspiration, 2009	1644.65	142.34	1652.80	132.81	0.69	0.00
Dist nearest border post (m)	38185.12	39882.37	46375.59	48338.52	0.21	0.67
Dist nearest protected area (m)	49299.10	37206.25	48547.99	39448.35	0.89	0.25
Dist nearest water body (m)	53996.94	40553.83	63485.47	44764.12	0.13	0.00
Avg Rainfall, 2000	1055.17	188.28	1045.49	182.17	0.72	0.03
Male ratio, pre-tr. district avg.	0.49	0.01	0.49	0.01	0.57	0.03
Slope	1.61	1.27	1.55	1.28	0.74	0.24
Urban	0.76	0.58	0.44	0.52	0.00	0.00
Avg temp (1970–2000), Apr	22.31	2.19	22.44	2.04	0.66	0.08
Avg temp (1970–2000), Aug	19.91	2.49	20.48	2.43	0.12	0.00
Avg temp (1970–2000), Dec	23.50	2.25	23.09	2.00	0.20	0.03
Avg temp (1970–2000), Feb	23.32	2.44	23.39	2.33	0.84	0.28
Avg temp (1970–2000), Jan	23.30	2.38	23.25	2.24	0.90	0.85
Avg temp (1970–2000), Jul	18.85	2.82	19.76	2.87	0.03	0.00
Avg temp (1970–2000), Jun	19.23	2.94	20.22	3.03	0.03	0.00
Avg temp (1970–2000), Mar	23.09	2.43	23.22	2.34	0.71	0.11
Avg temp (1970–2000), May	20.86	2.53	21.42	2.56	0.14	0.00
Avg temp (1970–2000), Nov	24.35	2.18	23.68	2.00	0.03	0.00
Avg temp (1970–2000), Oct	23.80	2.23	23.04	1.83	0.01	0.24
Avg temp (1970–2000), Sep	22.04	2.13	21.98	1.92	0.84	0.00
Travel time to nearest city, 2000	275.20	174.05	290.21	125.81	0.51	0.00
Avg pop (UN), 2000	28113.50	22916.39	26434.36	18329.95	0.58	0.00
Access to piped water, pre-tr. district share	0.17	0.13	0.12	0.11	0.01	0.00
Years of school, pre-tr. district avg	20.22	1.05	20.21	1.48	0.97	0.02

Notes: This table presents the summary statistics for all variables used in the machine learning process for the DHS sample. The treatment group consists of all survey cluster locations with at least one aid-project within a 15 km radius, and the control group consists of all survey cluster locations without. Before the matching process, there are 940 survey clusters in the treatment group and 131 survey clusters in the control group. The columns furthest to the right report the p-values from t-tests testing for equality of means in treatment and control groups for the trimmed and untrimmed samples, respectively. The definition and source of all variables can be found at`https://spatialdata.dhsprogram.com/methodology/#GEOSPATIALCOVARIATES`.

Table A5: Estimated parameters of propensity score equation, LSMS

		Aid projects		
	Variable	Estimate	s.e.	p-value
1	Intercept	5.162	0.028	0.000
2	Malawi dummy	3.663	0.015	0.000
3	Annual temperature	-0.078	0.000	0.000
4	Annual rainfall	0.001	0.000	0.000
5	Rainfall wettest m	-0.001	0.000	0.000
6	Temperature wettest q	0.087	0.000	0.000
7	Rainfall July-June	0.005	0.000	0.000
8	Distance to adm center	-0.021	0.000	0.000
9	Distance to market	-0.019	0.000	0.000
10	Onset of greenness, district avg.	0.020	0.000	0.000
11	Slope	0.002	0.000	0.000
12	Rainfall wettest q, July-June	-0.015	0.000	0.000
13	Start of wettest q	0.159	0.001	0.000

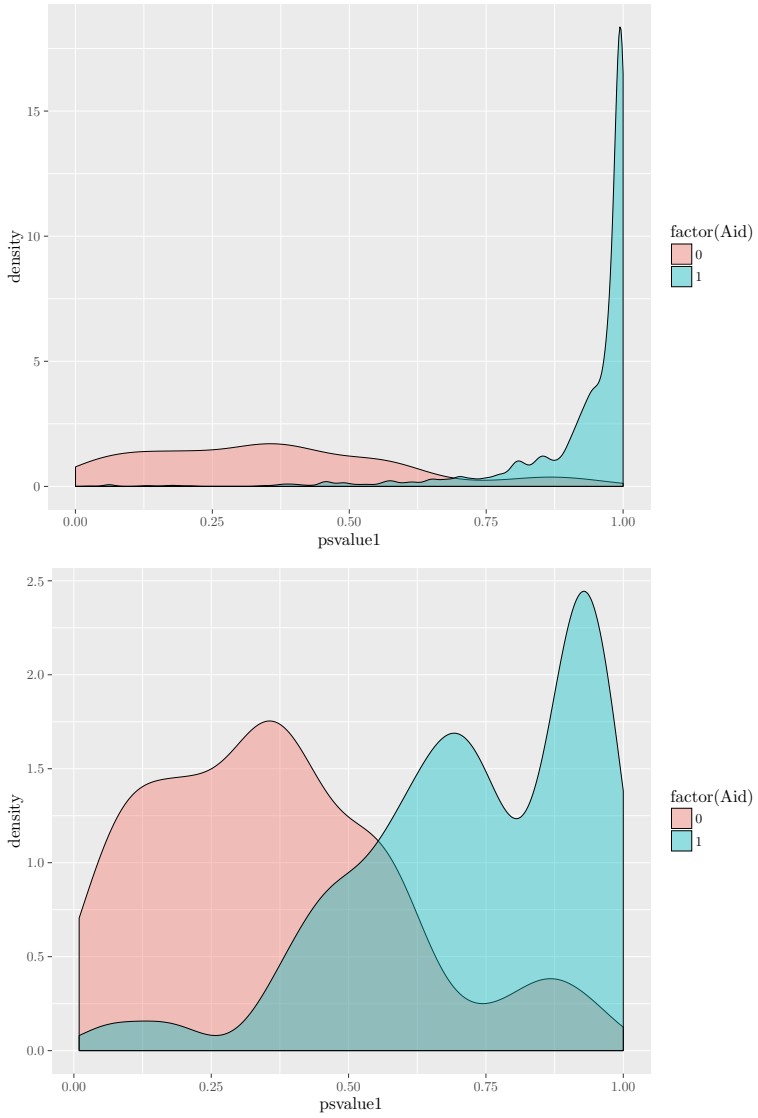
Notes: This table presents the 13 variables that are selected in the machine learning process for the LSMS sample, the estimated parameters of the propensity score equation, their standard errors and p-values. The definition and source of all variables can be found at `\\http://siteresources.worldbank.org/INTLSMS/Resources/3358986-1233781970982/5800988-1271185595871/6964312-1404828635943/IHPS.Geovariables.Description.pdf` for Malawi, and at `\\http://siteresources.worldbank.org/INTLSMS/Resources/3358986-1233781970982/5800988-1265043582346/UNPS\_2009\_10\_BID\_rev\_2014.pdf` for Uganda.

Table A6: Estimated parameters of propensity score equation, DHS

		Aid projects		
	Variable	Estimate	s.e.	p.value
1	Intercept	-3.203	1.492	0.032
2	Avg aridity index, 1960–90	0.000	0.000	0.001
3	Electricity, pre-tr. district avg.	4.100	4.452	0.357
4	Avg global human footprint index	-0.021	0.020	0.295
5	Avg share illit., 2000	4.741	1.069	0.000
6	Nightlights composite, 2015	5.795	1.977	0.003
7	Dist nearest protected area (m)	-0.000	0.000	0.000
8	Avg temp (1970–2000), Jul	-0.232	0.048	0.000
9	Travel time to nearest city, 2000	-0.004	0.001	0.000
10	Avg pop (UN), 2000	0.000	0.000	0.000

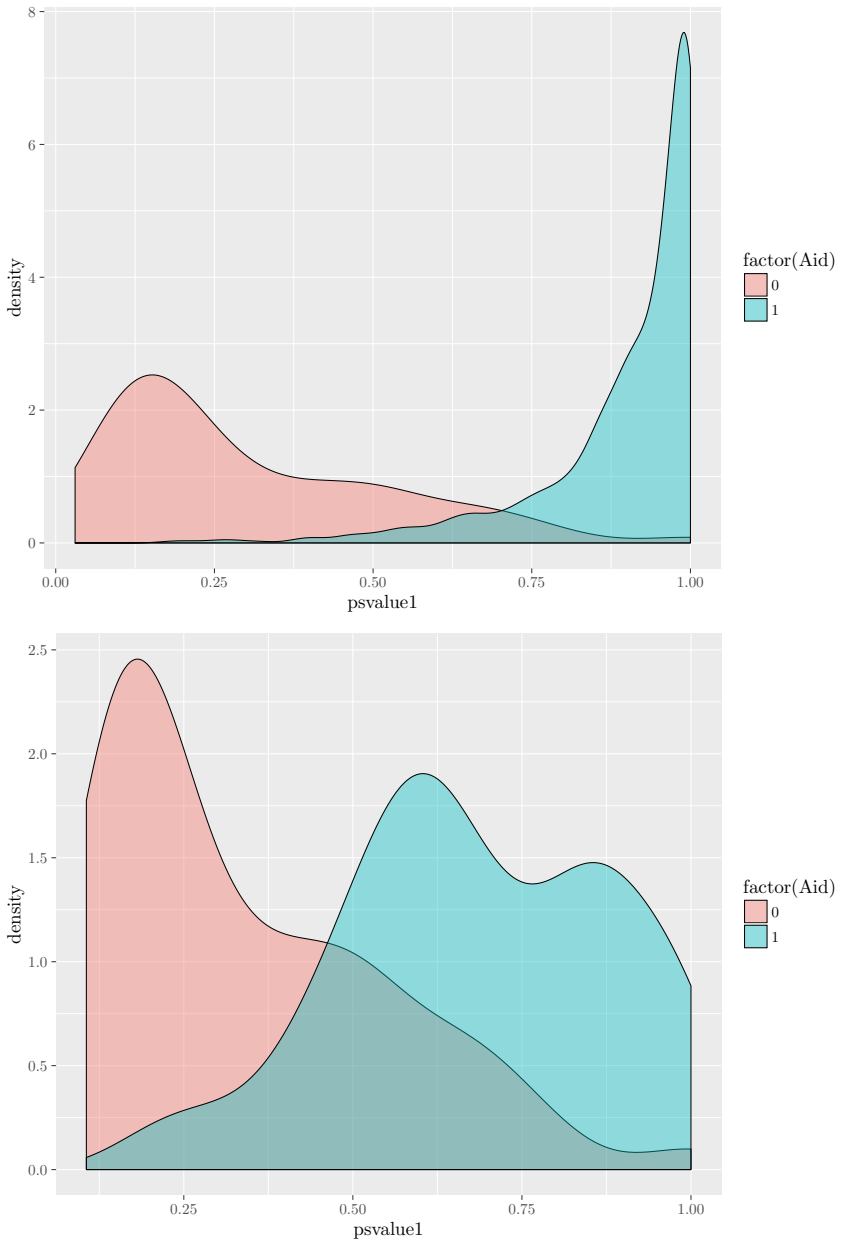
Notes: This table presents the 10 variables selected in the machine learning process for the DHS samples, the estimated parameters of the propensity score equation, their standard errors and p-values. The definition and source of all variables can be found at `https://spatialdata.dhsprogram.com/methodology/#GEOSPATIALCOVARIATES`.

Figure A1: Covariates overlap, LSMS sample



Notes:
The figure shows the distribution of propensity scores (psvalues1) before and after trimming for the LSMS sample.

Figure A2: Covariates overlap, DHS sample



Notes: The figure shows the distribution of propensity scores (psvalues1) before and after trimming for the DHS sample.

Table A7: LSMS sample sizes for unmatched and matched subsamples by treatment status

	Control	Treated	Sum
Unmatched	14	3702	3716
Matched	524	524	1048
Sum	538	4226	4764

Notes: This table reports the number of households in the matched and unmatched subsamples from LSMS after the matching procedure, by treatment status.

Table A8: DHS sample sizes for matched and unmatched subsamples by treatment status

	Control	Treated	Sum
Unmatched	2961	1833	4794
Matched	636	1939	2575
Sum	3597	3772	7369

Notes: This table reports the number of individuals in the matched and unmatched subsamples from DHS after the matching procedure, by treatment status.

B Result Tables

Table B1: Labor force participation and consumption – Marginal effect of aid project-years

Outcome	beta	Confidence interval	p-value	Obs.	Blocks	Mean	Impact @ mean
Working, w	0.013	[0.007; 0.019]	0.000	1187	6	0.913	0.404
Working on farm, w	-0.054	[-0.060; -0.048]	0.000	1187	6	0.878	-1.673
Monthly consumption	11.373	[4.366; 18.380]	0.002	1043	5	453.255	352.688
Expenditures, children	0.007	[0.005; 0.009]	0.000	1043	5	0.036	0.211

Notes: Data on labor force participation and consumption are retrieved from Ugandan National Panel Survey (2013/2014) and Malawi Integrated Household Panel Survey (2013). **Working, w** is share of women in household that has worked outside the home in the past 12 months. **Working on farm, w** is the share of women who has worked in the field in the past 12 months. **Monthly consumption** is monthly nominal household expenditures. **Expenditures, children** is the share of total household expenditures spent on health and education for children in the past 12 months. The explanatory variable of interest, **Aid Project-Years**, is the number of aid project years within a 15 km radius of the household. All regressions control for all the matching factors and the number of aid-project years. beta and confidence intervals are from the blocking estimation. p-values are adjusted for the multiple hypothesis problem using the Bonferroni method adjusted for correlation. Mean is the mean value of the outcome variable for the control group. Blocks is the number of blocks used in the estimation. Impact @ mean is the blocking estimator (beta) multiplied with average aid-project years in the sample.

Table B2: Labor force participation and consumption – Marginal effect of gender project-years

Outcome	beta	Confidence interval	p-value	Obs.	Blocks	Mean	Impact @ mean
Working, w	0.119	[0.112; 0.127]	0	1187	5	0.921	2.500
Working on farm, w	-0.458	[-0.466; -0.451]	0	1187	5	0.884	-9.616
Monthly consumption	236.350	[226.920; 245.781]	0	695	4	417.060	4957.138
Expenditures, children	0.014	[0.012; 0.017]	0	695	4	0.017	0.301

Notes: Data on labor force participation and consumption are retrieved from Ugandan National Panel Survey (2013/2014) and Malawi Integrated Household Panel Survey (2013). **Working, w** is share of women in household that has worked outside the home in the past 12 months. **Working on farm, w** is the share of women who has worked in the field in the past 12 months. **Monthly consumption** is monthly nominal household expenditures. **Expenditures, children** is the share of total household expenditures spent on health and education for children in the past 12 months. **Gender Project-Years** is the explanatory variable of interest and is the number of gender-specific aid project years within a 15 km radius of the household. All regressions control for all the matching factors and the number of aid-project years. beta and confidence intervals are from the blocking estimation. p-values are adjusted for the multiple hypothesis problem using the Bonferroni method adjusted for correlation. Mean is the mean value of the outcome variable for the control group. Blocks is the number of blocks used in the estimation. Impact @ mean is the blocking estimator (beta) multiplied with average gender aid-project years in the sample.

Table B3: Education – Marginal effect of aid project-years

Outcome	beta	Confidence interval	p-value	Obs.	Blocks	Mean	Impact @ mean
Attending primary, b	0.029	[0.024; 0.035]	0.000	582	5	0.933	0.906
Attending primary, g	-0.046	[-0.053; -0.039]	0.000	595	4	0.957	-1.427
Attending secondary, b	-0.020	[-0.032; -0.008]	0.002	496	4	0.848	-0.610
Attending secondary, g	-0.001	[-0.046; 0.044]	0.962	415	1	0.827	-0.034

Notes: Data on school attendance are retrieved from Ugandan National Panel Survey (2013/2014) and Malawi Integrated Household Panel Survey (2013). **Attending secondary, g (b)** is a dummy taking the value 1 if girls (boys) in the relevant cohort at the time of the survey was currently attending secondary school. **Attending primary, g (b)** is a dummy taking the value 1 if girls (boys) in the relevant cohort at the time of the survey was currently attending primary school. The explanatory variable of interest, **Aid Project-Years**, is the number of aid project years within a 15 km radius of the household. All regressions control for all the matching factors. beta and confidence intervals are from the blocking estimation. p-values are adjusted for the multiple hypothesis problem using the Bonferroni method adjusted for correlation. Mean is the mean value of the outcome variable for the control group. Blocks is the number of blocks used in the estimation. Impact @ mean is the blocking estimator (beta) multiplied with average aid-project years in the sample.

Table B4: Education – Marginal effect of gender project-years

Outcome	beta	Confidence interval	p-value	Obs.	Blocks	Mean	Impact @ mean
Attending primary, b	-0.088	[-0.010; -0.075]	0.000	582	3	0.938	-1.835
Attending primary, g	-0.008	[-0.021; 0.005]	0.238	595	3	0.959	-0.167
Attending secondary, b	-0.031	[-0.047; -0.014]	0.000	496	3	0.863	-0.646
Attending secondary, g	-0.026	[-0.057; 0.006]	0.107	415	2	0.832	-0.540

Notes: Data on school attendance are retrieved from Ugandan National Panel Survey (2013/2014) and Malawi Integrated Household Panel Survey (2013). **Attending secondary, g (b)** is a dummy taking the value 1 if girls (boys) in the relevant cohort at the time of the survey was currently attending secondary school. **Attending primary, g (b)** is a dummy taking the value 1 if girls (boys) in the relevant cohort at the time of the survey was currently attending primary school. **Gender Project-Years** is the explanatory variable of interest and is the number of gender-specific aid project years within a 15 km radius of the household. All regressions control for all the matching factors and the number of aid-project years. beta and confidence intervals are from the blocking estimation. p-values are adjusted for the multiple hypothesis problem using the Bonferroni method adjusted for correlation. Mean is the mean value of the outcome variable for the control group. Blocks is the number of blocks used in the estimation. Impact @ mean is the blocking estimator (beta) multiplied with average gender aid-project years in the sample.

Table B5: Time use – Marginal effect of aid project-years

Outcome	beta	Confidence interval	p-value	Obs.	Blocks	Mean	Impact @ mean
Fetch water, b	0.056	[0.049; 0.064]	0.000	808	5	0.207	6.097
Fetch water, g	-0.073	[-0.080; -0.065]	0.000	828	5	0.238	-7.847
Fetch water, m	-0.018	[-0.025; -0.011]	0.000	808	5	0.120	-1.926
Fetch water, w	0.030	[0.0213; 0.040]	0.000	828	5	0.493	3.292
Collect firewood, b	0.011	[5e-04; 0.022]	0.041	593	4	0.163	1.240
Collect firewood, g	-0.043	[-0.054; -0.032]	0.000	612	4	0.214	-4.622
Collect firewood, m	-0.026	[-0.036; -0.017]	0.000	593	4	0.093	-2.866
Collect firewood, w	0.020	[0.006; 0.033]	0.004	612	4	0.575	2.120

Notes: Data on time use are retrieved from Ugandan National Panel Survey (2013/2014) and Malawi Integrated Household Panel Survey (2013). **Fetch water** is the share of a household's total time spent on fetching water carried out by the women, men, girls and boys in the household, respectively. **Collect firewood** is the share of a household's total time spent on collecting firewood carried out by the women, men, girls and boys in the household, respectively. The explanatory variable of interest, **Aid Project-Years**, is the number of aid project years within a 15 km radius of the household. All regressions control for all the matching factors. beta and confidence intervals are from the blocking estimation. p-values are adjusted for the multiple hypothesis problem using the Bonferroni method adjusted for correlation. Mean is the mean value of the outcome variable for the control group. Blocks is the number of blocks used in the estimation. Impact @ mean is the blocking estimator (beta) multiplied with average aid-project years in the sample.

Table B6: Time use – Marginal effect of gender project-years

Outcome	beta	Confidence interval	p-value	Obs.	Blocks	Mean	Impact @ mean
Fetch water, b	0.024	[0.008; 0.040]	0.003	530	3	0.144	0.748
Fetch water, g	0.220	[0.205; 0.235]	0.000	546	3	0.241	6.909
Fetch water, m	-0.216	[-0.231; -0.201]	0.000	530	3	0.106	-6.784
Fetch water, w	0.317	[0.298; 0.336]	0.000	546	3	0.567	9.956
Collect firewood, b	0.076	[0.056; 0.095]	0.000	343	3	0.096	2.375
Collect firewood, g	0.436	[0.416; 0.455]	0.000	357	3	0.194	13.671
Collect firewood, m	-0.039	[-0.057; -0.022]	0.000	343	3	0.102	-1.229
Collect firewood, w	-0.351	[-0.375; -0.328]	0.000	357	3	0.661	-11.018

Notes: Data on time use are retrieved from Ugandan National Panel Survey (2013/2014) and Malawi Integrated Household Panel Survey (2013). **Fetch water** is the share of a household's total time spent on fetching water carried out by the women, men, girls and boys in the household, respectively. **Collect firewood** is the share of a household's total time spent on collecting firewood carried out by the women, men, girls and boys in the household, respectively. **Gender Project-Years** is the explanatory variable of interest and is the number of gender-specific aid project years within a 15 km radius of the household. All regressions control for all the matching factors and the number of aid-project years. beta and confidence intervals are from the blocking estimation. p-values are adjusted for the multiple hypothesis problem using the Bonferroni method adjusted for correlation. Mean is the mean value of the outcome variable for the control group. Blocks is the number of blocks used in the estimation. Impact @ mean is the blocking estimator (beta) multiplied with average gender aid-project years in the sample.

Table B7: Household decision-making – Marginal effect of aid project-years

Outcome	beta	Confidence interval	p-value	Obs.	Blocks	Mean	Impact @ mean
Joins in no decisions (w)	-0.024	[-0.027; -0.021]	0.000	3836	9	0.118	-0.738
Joins decisions on...							
... large purchases (w)	0.055	[0.051; 0.059]	0.000	3845	9	0.748	1.681
... how spend earnings (w)	0.031	[0.026; 0.036]	0.000	4465	11	0.540	0.942
... her health care (w)	0.032	[0.027; 0.037]	0.000	3845	9	0.735	0.982
... family visits (w)	0.032	[0.027; 0.038]	0.000	3847	9	0.641	0.994

Notes: Data on participation in household decision-making are retrieved from Uganda DHS (2011) and Malawi DHS (2010). **Joins decisions on large purchases (w)** is a dummy taking the value 1 if wife, on her own or jointly with her husband, makes decisions about major household purchases. **Joins decisions on earnings (w)** is a dummy taking the value 1 if wife, on her own or jointly with her husband, makes decisions on how to spend her own and her husband's earnings. **Joins decisions on her health care (w)** is a dummy taking the value 1 if wife, on her own or jointly with her husband, makes decisions about her own health care. **Joins decisions on family visits (w)** is a dummy taking the value 1 if wife, on her own or jointly with her husband, makes decisions on visits to her family or relatives. **Joins in no decisions (w)** is a dummy taking the value 1 if wife do not participate in making any of the previous decisions. The explanatory variable of interest, **Aid Project-Years**, is the number of aid project years within a 15 km radius of the household. All regressions control for all the matching factors. beta and confidence intervals are from the blocking estimation. p-values are adjusted for the multiple hypothesis problem using the Bonferroni method adjusted for correlation. Mean is the mean value of the outcome variable for the control group. Blocks is the number of blocks used in the estimation. Impact @ mean is the blocking estimator (beta) multiplied with average aid-project years in the sample.

Table B8: Household decision-making – Marginal effect of gender project-years

Outcome	beta	Confidence interval	p-value	Obs.	Blocks	Mean	Impact @ mean
Joins in no decisions (w)	-0.013	[-0.010; -0.017]	0.000	3836	7	0.117	-0.246
Joins decisions on...							
... large purchases (w)	0.042	[0.037; 0.047]	0.000	3845	7	0.747	0.773
... how spend earnings (w)	0.010	[0.002; 0.018]	0.010	4465	5	0.554	0.184
... her health care (w)	0.069	[0.064; 0.074]	0.000	3845	7	0.724	1.269
... family visits (w)	0.036	[0.030; 0.042]	0.000	3847	7	0.650	0.666

Notes: Data on participation in household decision-making are retrieved from Uganda DHS (2011) and Malawi DHS (2010). **Joins decisions on large purchases (w)** is a dummy taking the value 1 if wife, on her own or jointly with her husband, makes decisions about major household purchases. **Joins decisions on earnings (w)** is a dummy taking the value 1 if wife, on her own or jointly with her husband, makes decisions on how to spend her own and her husband's earnings. **Joins decisions on her health care (w)** is a dummy taking the value 1 if wife, on her own or jointly with her husband, makes decisions about her own health care. **Joins decisions on family visits (w)** is a dummy taking the value 1 if wife, on her own or jointly with her husband, makes decisions on visits to her family or relatives. **Joins in no decisions (w)** is a dummy taking the value 1 if wife do not participate in making any of the previous decisions. **Gender Project-Years** is the explanatory variable of interest and is the number of gender-specific aid project years within a 15 km radius of the household. All regressions control for all the matching factors and the number of aid-project years. beta and confidence intervals are from the blocking estimation. p-values are adjusted for the multiple hypothesis problem using the Bonferroni method adjusted for correlation. Mean is the mean value of the outcome variable for the control group. Blocks is the number of blocks used in the estimation. Impact @ mean is the blocking estimator (beta) multiplied with average gender aid-project years in the sample.

Table B9: Women's sexual freedoms – Marginal effect of aid project-years

Outcome	beta	Confidence interval	p-value	Obs.	Blocks	Mean	Impact @ mean
Can refuse sex if m unfaithful (m)	-0.031	[-0.039; -0.022]	0.000	1660	6	0.741	-0.938
Can refuse sex (w)	-0.010	[-0.015; -0.005]	0.000	3814	10	0.783	-0.302
Can ask for condom if m STI (m)	0.005	[-8e-04; 0.011]	0.243	1662	6	0.882	0.156
Can ask for condom (w)	0.011	[0.007; 0.015]	0.000	3855	9	0.734	0.350

Notes: Data on women's sexual freedom are retrieved from Uganda DHS (2011) and Malawi DHS (2010). **Can refuse sex if m unfaithful (m)** is a dummy taking the value 1 if husband deem it justified for a wife to refuse to have sex with her husband when she knows her husband has sex with women other than his wives. **Can refuse sex (w)** is a dummy taking the value 1 if wife agrees that she can say to her husband if she does not want to have sexual intercourse. **Can ask for condom if m STI (m)** is a dummy taking the value 1 if husband deem it justified for a wife to ask that they use a condom if she knows her husband has a sexually transmitted infection (STI). **Can ask for condom (w)** is a dummy taking the value 1 if a wife agrees that she can ask her husband to use a condom if she wanted him to. The explanatory variable of interest, **Aid Project-Years**, is the number of aid project years within a 15 km radius of the household. All regressions control for all the matching factors. beta and confidence intervals are from the blocking estimation. p-values are adjusted for the multiple hypothesis problem using the Bonferroni method adjusted for correlation. Mean is the mean value of the outcome variable for the control group. Blocks is the number of blocks used in the estimation. Impact @ mean is the blocking estimator (beta) multiplied with average aid-project years in the sample.

Table B10: Women's sexual freedoms – Marginal effect of gender project-years

Outcome	beta	Confidence interval	p-value	Obs.	Blocks	Mean	Impact @ mean
Can refuse sex if m unfaithful (m)	-0.747	[-0.756; -0.738]	0.000	1660	7	0.742	-13.714
Can refuse sex (w)	-0.047	[-0.052; -0.042]	0	3814	7	0.785	-0.860
Can ask for condom if m STI (m)	1.004	[0.100; 1.010]	0.000	1662	7	0.893	18.448
Can ask for condom (w)	-0.154	[-0.158; -0.150]	0.000	3855	7	0.784	-2.823

Notes: Data on women's sexual freedom are retrieved from Uganda DHS (2011) and Malawi DHS (2010). **Can refuse sex if m unfaithful (m)** is a dummy taking the value 1 if husband deem it justified for a wife to refuse to have sex with her husband when she knows her husband has sex with women other than his wives. **Can refuse sex (w)** is a dummy taking the value 1 if wife agrees that she can say to her husband if she does not want to have sexual intercourse. **Can ask for condom if m STI (m)** is a dummy taking the value 1 if husband deem it justified for a wife to ask that they use a condom if she knows her husband has a sexually transmitted infection (STI). **Can ask for condom (w)** is a dummy taking the value 1 if a wife agrees that she can ask her husband to use a condom if she wanted him to. **Gender Project-Years** is the explanatory variable of interest and is the number of gender-specific aid project years within a 15 km radius of the household. All regressions control for all the matching factors and the number of aid-project years. beta and confidence intervals are from the blocking estimation. p-values are adjusted for the multiple hypothesis problem using the Bonferroni method adjusted for correlation. Mean is the mean value of the outcome variable for the control group. Blocks is the number of blocks used in the estimation. Impact @ mean is the blocking estimator (beta) multiplied with average gender aid-project years in the sample.

Table B11: Domestic violence – Marginal effect of aid project-years

Outcome	beta	Confidence interval	p-value	Obs.	Blocks	Mean	Impact @ mean
Wife beating never justified (m)	-0.005	[-0.012; 0.001]	0.312	1676	6	0.734	-0.163
Wife beating never justified (w)	0.011	[0.007; 0.014]	0.000	5585	10	0.567	0.330
Experienced ...							
... Sexual violence (w)	-0.012	[-0.022; -0.001]	0.065	1248	5	0.191	-0.361
... Physical violence (w)	0.002	[-0.008; 0.012]	0.970	1251	5	0.225	0.054
... Emotional violence (w)	0.001	[-0.010; 0.012]	0.996	1252	5	0.269	0.029

Notes: Data on participation in household decision-making are retrieved from Uganda DHS (2011) and Malawi DHS (2010). **Wife beating never justified (w/m)** is a dummy taking the value 1 if wife/husband agrees do not agree that husband is justified in beating his wife for any of the following reasons: burns the food; argues with him; goes out without telling him; neglects the children; or refuses to have sexual intercourse with him. **Sexual violence (w)** is a dummy taking the value 1 if wife has experienced sexual violence in the past 12 months preceding the survey. **Physical violence (w)** is a dummy taking the value 1 if wife has experienced physical violence in the past 12 months preceding the survey. **Emotional violence (w)** is a dummy taking the value 1 if wife has experienced emotional violence in the past 12 months preceding the survey. The explanatory variable of interest, **Aid Project-Years**, is the number of aid project years within a 15 km radius of the household. All regressions control for all the matching factors. beta and confidence intervals are from the blocking estimation. p-values are adjusted for the multiple hypothesis problem using the Bonferroni method adjusted for correlation. Mean is the mean value of the outcome variable for the control group. Blocks is the number of blocks used in the estimation. Impact @ mean is the blocking estimator (beta) multiplied with average aid-project years in the sample.

Table B12: Domestic violence – Marginal effect of gender project-years

Outcome	beta	Confidence interval	p-value	Obs.	Blocks	Mean	Impact @ mean
Wife beating never justified (m)	0.047	[0.040; 0.055]	0.000	1676	7	0.694	0.867
Wife beating never justified (w)	0.037	[0.031; 0.043]	0.000	5585	8	0.572	0.677
Experienced ...							
... Sexual violence (w)	-0.017	[-0.033; -0.002]	0.394	1248	5	0.198	-0.317
... Physical violence (w)	0.006	[-0.009; 0.020]	1.000	1251	5	0.216	0.102
... Emotional violence (w)	-0.034	[-0.050; -0.018]	0.000	1252	5	0.283	-0.621

Notes: Data on participation in household decision-making are retrieved from Uganda DHS (2011) and Malawi DHS (2010). **Wife beating never justified (w/m)** is a dummy taking the value 1 if wife/husband agrees do not agree that husband is justified in beating his wife for any of the following reasons: burns the food; argues with him; goes out without telling him; neglects the children; or refuses to have sexual intercourse with him. **Sexual violence (w)** is a dummy taking the value 1 if wife has experienced sexual violence in the past 12 months preceding the survey. **Physical violence (w)** is a dummy taking the value 1 if wife has experienced physical violence in the past 12 months preceding the survey. **Emotional violence (w)** is a dummy taking the value 1 if wife has experienced emotional violence in the past 12 months preceding the survey. The explanatory variable of interest, **Gender Aid Project-Years**, is the number of gender-specific aid project years within a 15 km radius of the household. All regressions control for all the matching factors and the number of aid-project years. beta and confidence intervals are from the blocking estimation. p-values are adjusted for the multiple hypothesis problem using the Bonferroni method adjusted for correlation. Mean is the mean value of the outcome variable for the control group. Blocks is the number of blocks used in the estimation. Impact @ mean is the blocking estimator (beta) multiplied with average gender aid-project years in the sample.