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**DO ELECTED COUNCILS IMPROVE
GOVERNANCE? EXPERIMENTAL
EVIDENCE ON LOCAL INSTITUTIONS IN
AFGHANISTAN**

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Abstract

Using data from a field experiment across 500 villages in Afghanistan, we study how electoral accountability of local institutions affects the quality of governance. In villages with newly created elected councils, food aid distributed by local leaders is more likely to reach needy villagers. However, this effect is observed only if the council is mandated to be the entity responsible for managing the distribution. In the absence of such a mandate the presence of elected councils increases embezzlement without improving aid targeting. Thus, while elected councils can improve governance, unclear and overlapping mandates may increase rent-seeking and worsen governance outcomes.

JEL Classification: D7, O1

Keywords: Political Institutions, field experiment, democratization, governance quality

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Do Elected Councils Improve Governance? Experimental Evidence on Local Institutions in Afghanistan¹

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Using data from a field experiment across 500 villages in Afghanistan, we study how electoral accountability of local institutions affects the quality of governance. In villages with newly created elected councils, food aid distributed by local leaders is more likely to reach needy villagers. However, this effect is observed only if the council is mandated to be the entity responsible for managing the distribution. In the absence of such a mandate the presence of elected councils increases embezzlement without improving aid targeting. Thus, while elected councils can improve governance, unclear and overlapping mandates may increase rent-seeking and worsen governance outcomes.

I. Introduction

Over the past two decades, various studies have employed theory, empirics, and case studies to establish the importance of the quality of political institutions in determining economic outcomes (Sokoloff and Engerman, 2000; Acemoglu et. al., 2001; Banerjee and Iyer, 2005; Acemoglu and Robinson, 2013). Such research, however, generally focuses on comparison of institutions in equilibrium. Much less is known about how attempts to reform political institutions affect governance quality and, by extension, economic outcomes (Pande and Udry, 2006), largely because institutional change is rarely exogenous to other political and economic outcomes. Researchers have more recently sought to exploit variation in sub-national institutions to identify the effects of specific institutional reforms (Casey et. al., 2012; Chattopadhyay and Duflo, 2004; Grossman 2014; Olken, 2010). However, with the exception of Martinez-Bravo et. al. (2017), very few studies have identified the effects of how broader institutional change, such as the introduction of democratic elections, affect governance outcomes.

This study exploits the creation of democratically-elected, gender-balanced councils across a randomly-selected set of villages in Afghanistan to examine the effects of sub-national institutional reform on governance outcomes. As customary structures in rural Afghanistan are often hereditary

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and exclude women, the creation of election councils had the potential to change both the composition of and incentives facing the village leadership.

To identify the effects of the creation of elected councils, we organized village-level food aid distributions approximately four years after the creation of these councils. Food aid distributions are a regular service performed by village leaders in rural Afghanistan and have important economic consequences for villagers. Compared to other governance services provided by village leaders, such as conflict resolution or liaisons with other institutions, aid distributions generate quantifiable outcomes that are comparable across villages with different structures of governance.² We utilize data on the outcomes of aid distributions to identify the effects of the creation of councils on aid targeting (i.e. whether food aid reached the neediest households), the incidence of embezzlement and nepotism, and the inclusiveness of the process that determined which households received food aid.

The creation of elected councils may affect local governance through a range of different channels. Elected councils can have a direct effect on governance outcomes if council members assume responsibility for the provision of local governance services and behave differently than customary leaders. However, even if customary leaders retain governance responsibilities, the creation of democratic councils may indirectly affect local governance outcomes by changing the behavior of customary village leaders. Governance outcomes may also be affected by the increased involvement of underrepresented groups. In elected councils half of the council seats in elected councils were reserved for women, which constituted an important change, since in Afghanistan women are traditionally excluded from local governance.

To explore the mechanisms through which council creation affects local governance, randomized variation was induced in how the food aid distribution was managed. Specifically, in villages with elected councils, we varied whether the elected council was explicitly mandated to manage the distribution or whether the distribution was managed by those persons identified as leaders by villagers, without specifying the rules according to which these leaders were selected. In villages without elected councils, we varied whether women were requested to participate in addition to the *de facto* village leadership. By comparing outcomes in these four groups of villages, we are able to

² Similar papers that use behavioral measures applicable to the local context include Casey et. al. 2012; Fearon et. al. 2013; and Humphreys et.al. 2014.

isolate the effects of: (i) the management of the distribution by the elected council; (ii) the existence of elected councils *per se* (without an explicit requirement on who manages the distribution); and (iii) mandating female participation in the distribution.

Results indicate that the creation of elected councils improves the targeting of food aid as measured by objective measures, but does not have a significant effect on any other outcomes. Mandating that elected councils manage the distribution improves aid targeting without any significant effect on embezzlement or the inclusiveness of decision-making. However, the existence of an elected council *per se*, without a clear mandate on who assumes responsibility for the distribution, increases embezzlement, renders decision-making less inclusive, and does not change targeting, relative to outcomes in villages without elected councils. Mandating female involvement in the distribution in villages with customary rather than elected local institutions has a similar effect, with increases in embezzlement and no change in targeting or participation. Overall, the results indicate that democratically-elected councils can improve the quality of governance, but only if there is no ambiguity in institutional accountability and elected councils are explicitly put in charge. If, however, multiple institutions exist in parallel without a clear division of institutional responsibility, local leaders may respond opportunistically by engaging in rent-seeking.

That elected councils, when suitably empowered, can have a positive impact on governance is aligned with recent findings on the effectiveness of representative local institutions in enhancing elite behavior (Platteau and Abraham, 2002; Dasgupta and Beard, 2007; Labonne and Chase, 2009). Specifically, our results are consistent with those of Martinez-Bravo et. al. (2017), who find that elections in Chinese villages improved public goods provision and increased redistribution. They are also in agreement with Grossman (2014), which finds that elected leaders of farmers associations in Uganda exhibit higher levels of responsiveness and accountability than appointed leaders. However, the mechanisms behind our results and these comparable findings appear to be different. While the effect of elections in Chinese villages was driven primarily by an increase in leader incentives (Besley and Case, 1995; Besley and Coate, 2003; Dal Bó and Rossi, 2008; Ferraz and Finan, 2011), and in Ugandan farmer associations by the development of better monitoring capacity among constituents that constrained predatory elite behaviors, the effect in Afghanistan appears to be driven primarily by the ability of villagers to select better candidates (Beath et. al., 2016; Besley, 2006; Besley and Reynal-Querol, 2011).

Consistent with the theoretical literature on the effects of governance structure on corruption (Shleifer and Vishny, 1993; Persson, Roland and Tabellini, 1997), we find that when elected councils are created alongside customary institutions with no clear separation of responsibilities between them, this leads to an increase in the amounts extracted rents. Our paper is also related to the literature on the transplantation of institutions, as it shows that even transplanted democratic institutions can potentially improve governance, but only in tasks where they assume full responsibility (Hayek 1960; Berkowitz et. al. 2003; Acemoglu et al. 2011). Finally, our study contributes to growing research on the effects of community-driven development programs on governance outcomes (e.g. Labonne and Chase, 2008; Olken, 2010; Fearon et. al., 2009, 2011; Casey et. al., 2012; Humphreys et. al., 2012, 2014). The paper also makes a methodological contribution by using outcomes of food aid distribution as a relevant measure of the quality of governance that is comparable across villages with different structures of governance.

This paper is part of an array of experimental works that study a specific country in detail in order to derive general findings and implications. Looking specifically at Afghanistan, field experiments have already been used to study corruption (Callen and Long, 2015), determinants of risk preferences (Callen, Sprenger, Isaqzadeh, and Long, 2014), the effect of school construction (Burde and Linden, 2013), and women's empowerment (Beath, Christia and Enikolopov, 2013). The general caveats on generality and external validity that arise in those works also apply here.

This paper is structured as follows: Section II provides background information on the National Solidarity Program, local governance, and food distributions in rural Afghanistan; Section III describes the research design; Section IV outlines the hypotheses for our study as previously outlined in our pre-analysis plan; Section V presents the relevant data sources; Section VI outlines the specifications used to test the proposed hypotheses; Section VII describes the results; Section VIII discusses the results; and Section IX concludes

II. Background Information

II.1 - National Solidarity Programme

This experiment is embedded within the randomized impact evaluation of the National Solidarity Programme (NSP), which created the elected councils under study. Following the ousting of the Taliban in 2001, the Government of Afghanistan developed NSP to build representative and gender-inclusive institutions for local governance and to deliver critical services to its rural

population. Since its inauguration in 2003, NSP has been implemented in over 32,000 villages across all 34 provinces of Afghanistan, making it the largest single development program in the country. The program is executed by the Government of Afghanistan, funded by the World Bank and a consortium of bilateral donors, and implemented by NGOs.

NSP employs the community-driven model of aid delivery, and is structured around two major interventions at the village level: (i) the creation of a Community Development Council (CDC); and (ii) the disbursement of block grants to support project implementation. In order to facilitate the creation of representative institutions for village governance, NSP mandates that CDCs be gender-balanced and created through a secret-ballot, universal suffrage election. Once councils are formed, NSP disburses block grants, valued at \$200 per household up to a village maximum of \$60,000, to support the implementation of projects selected by the council in consultation with the village community. Projects are ordinarily focused on either the construction or rehabilitation of infrastructure, such as drinking water facilities, irrigation canals, roads and bridges, or electrical generators; or the provision of human capital development, such as training and literacy courses.

NSP intends to provide repeat block grants to participating villages, although villages receive no firm guarantees of when – or if – they will receive these. The process for conducting follow-up elections for the council is also uncertain. According to NSP rules, villages are supposed to hold re-elections for council positions every four years in order to be eligible for receiving repeat block grants, but the credibility of this requirement is questionable. By the end of 2011 when this study was conducted, no re-elections had been held in any of the villages in the study, which had elected councils by early 2008. Given this and the general uncertainty, which accompanies planned future development activity in Afghanistan, NSP does not provide strong reelection incentives to council members. As such, it may affect the perceptions of elected members' time horizons and could in turn weaken accountability mechanisms.

II.2 - Customary Local Governance Structures in Afghanistan

Afghanistan's central government has historically lacked the legitimacy and resources necessary to extend institutional structures or provide public goods to the country's far-flung and ethnically diverse rural areas (Barfield, 1984). This had left the Afghan countryside bereft of formal local

governance structures, leading to the evolution of informal yet sophisticated structures of local governance to protect local interests and provide local public goods (Pain and Kantor, 2010).

A tri-partite customary local governance structure is common throughout the country. The foundation of local governance is the local *jirga* or *shura*, a local council that meets on an *ad hoc* basis to resolve a local dispute or other issue (Nojumi et. al. 2004). Council members tend to be the elders of families in the village (Rahmani 2006) and decision-making is by consensus (Boesen 2004). Villages also ordinarily have a headman (termed a *malik*, *arbab*, or *qariyadar*) - usually a large landowner - who represents the village to government (Kakar 2005). The local religious authority or *mullah* is responsible for conducting rites and services and mediating disputes involving family or moral issues (Rahmani 2006).

Qualitative studies suggest that the behavior of local leaders is constrained by the checks and balances that exist between the three entities, resulting in generally high levels of legitimacy and provision of local public goods (Brick, 2008). Pain and Kantor (2010) report that headmen are occasionally deposed due to errant behavior and are often accountable to villagers, although the variation in economic inequality within villages produces substantial variation in local governance, with cases of near-absolute political authority by land-holding local elites.

Across Afghanistan, the role served by women is universally marginal. With the partial exception of regions dominated by the *Hazara* ethnic minority, the principal of *pardab* - which stipulates that women should be hidden from public observation - precludes involvement by women in communal gatherings (Brick, 2008). Local governance is thus a strictly male-dominated activity, with women only being allowed to participate in unusual circumstances, such as when they act on behalf of a deceased husband (Boesen, 2004).

II.3 – Elected Councils and Local Governance in Afghanistan

Elected councils created by NSP differ from customary local governance structures in the mode of selection. While elected councils are selected by a secret ballot, universal suffrage election, the position of headman and other customary local governance positions are ordinarily inherited or otherwise derived through land holdings or other forms of economic authority. As elected councils implementing NSP-funded projects have control over what is an unprecedented amount of resources for villages, elected councils assume substantial *de facto* authority.

Evidence from the NSP impact evaluation indicates a substantial overlap between elected councils and pre-existing elites. Specifically, 40 percent of council members and 70 percent of council leaders were previously identified by villagers as *de facto* village leaders, although villagers elected to councils are on average younger and better educated (Beath et. al. 2010). While there is some shift of local governance responsibilities from customary leaders to elected councils, the magnitude of these changes is of limited practical significance (Beath et. al., 2010). Accordingly, elected councils do not appear to fundamentally alter the activities of customary institutions. Councils do, however, improve the access of women to local governance services, as well as women's perception of local governance quality (Beath, et al., 2013).

Existing qualitative studies provide conflicting evidence on whether customary power-holders have captured elected councils and whether the new institution brings about changes in the identity of the village leadership. Various observers provide accounts of customary leaders attempting to subvert council elections (Boesen, 2004; Brick, 2008; and Noelle-Karimi, 2006), indicating that leaders indeed covet council positions but are not constrained by electoral accountability. Barakat (2006) also observes that, while some younger and more educated villagers are elected to councils, customary elites remain influential. Brick (2008) concludes, on the basis of qualitative and non-experimental quantitative research, that council creation degrades provision of local public goods due to a weakening of accountability structures between local elites and villagers.

II.4 - Wheat Distribution in Afghanistan

According to the UN's World Food Program (WFP), between a fourth and a third of the population of Afghanistan are considered food insecure.³ To alleviate food shortages, WFP and other humanitarian agencies regularly deliver food assistance to poor or otherwise vulnerable communities. Due to Afghanistan's varied topography and pockets of insecurity, local governance institutions regularly facilitate distributions for WFP and other agencies.

The clientelistic nature of Afghan politics – and the difficulties of monitoring distributions given poor transportation and telecommunication infrastructure and the low capacity of sub-national institutions – results in alleged diversions of food aid by local governance actors, sub-national government figures, line ministries, and police. Saltmarshe and Medhi (2011) estimate that more

³ In a prior survey of a random sample of villagers in the 500 villages under study, 48 percent of respondents indicated that members of their household were hungry at least one day during the previous week.

than a third of food aid is sold in the local markets instead of being delivered to the intended beneficiaries.

Thus, despite the importance of food aid distributions to the welfare of the rural population, food aid is also a vehicle for predation and diversion. Measures of aid targeting and diversion thus capture outcomes critical to the welfare of vulnerable villagers that are directly under the control of the behavior of village leaders. However, as the responsibility for distributing food aid is not specifically assigned to elected councils, the councils are not expected to have developed specialized expertise in managing food aid distributions. Critically then, wheat distribution does not mimic skills imparted by NSP (because NSP does not do food distributions, which happen fully outside its confines). This provides the study with the ability to identify the effect of elected councils on the behavioral incentives facing different groups of local leaders, rather than inadvertently capturing the effect of specific capacities or skills imparted to individual leaders by NSP.

III. Research Design

III.1 - Sample and Randomization of NSP

Variation in elected councils is provided by the randomized impact evaluation of NSP, a multi-year study designed to assess the effects of the program on a broad range of economic, institutional, and social indicators. Ten large districts with no prior NSP activity and satisfactory security conditions were selected for inclusion in the evaluation. The districts cover all major regions of Afghanistan except the south, which could not be included due to adverse security conditions (see Figure 1). The sample is also representative of Afghanistan's ethno-linguistic diversity, with five predominantly Tajik districts, four predominantly Pashtun districts, one predominantly Hazara district, and two districts with substantial Uzbek and Turkmen minorities.⁴

In each of the ten sample districts, NSP facilitating partners selected 50 villages for inclusion in the study, which were randomly assigned to treatment and control groups of equal size. To improve statistical balance, a four-stage matched-pair cluster randomization procedure was applied:

⁴ An assessment of the demographic and economic characteristics of the 500 villages under study reveals few substantive differences with those of a random sample of villages surveyed by the 2007-08 National Risk and Vulnerability Assessment.

1. Village Clusters: To minimize spillovers between treated and untreated units, villages located within 1 kilometer were grouped in clusters. Of the 500 villages, 107 were assigned to 41 clusters. The number of villages in each cluster ranged from two to six.
2. Matched Pairs: In each district, the 50 villages were divided into 25 pairs using an optimal greedy matching algorithm (King et. al. 2007), which matched villages on background characteristics provided that the villages were not in the same cluster.⁵
3. Assignment of Treatment: In each matched pair, one village was randomly assigned to receive the program, such that the clusters of villages were assigned the same treatment status.
4. Violations of Clustering Restrictions: In a few districts, the large number of clustered villages precluded the co-assignment of clustered villages to the same treatment status. For cases for which co-assignment without violation of clustering restrictions was not possible, the number of violations was minimized through simulation.

Villages in the treatment group received NSP and elected councils following the administration of a baseline survey in September 2007, with the remaining 250 control villages not receiving NSP or elected councils until spring 2012, at which point all data collection activities for the study had finished.

In addition to the variation in the existence of councils, we also introduce variation in the procedures of the food aid distribution (see Section III.3 below).

III.2 - Wheat Distribution and Data Collection

To identify the effects of elected councils on local governance, we organized a food aid distribution across the sample of 500 villages between June and October 2011, approximately three-and-a-half years after councils had been elected. The villages in the sample were generally representative of rural Afghan villages and came from all parts of Afghanistan except for the southern region, which was excluded from the study because of security considerations (Beath et al. 2010, Beath, Christia, and Enikolopov 2018).

⁵ The background characteristics included village size (based on data collected a few years earlier by Afghanistan's Central Statistics Organization) and a set of geographic variables (distance to river, distance to major road, altitude, and average slope).

Food aid (wheat) was delivered to village leaders, who were responsible for distributing it to vulnerable households. Wheat was donated by USAID, and WFP assisted with distribution logistics. Each village was given enough wheat for one sixth of the village households for half a month. The decision rule was not shared with villagers or leaders, who were only informed of the total amount of food aid designated to their village. The exact amount was determined based on WFP guidelines of 81 kg per household of six per month. Village allocations were rounded up to the closest multiple of 50 kg as this was the size of sacks in which wheat was distributed. The average amount of wheat distributed in each village was 1,100 kg.

The distribution and data collection necessitated three visits to each village:

First Visit: On the first visit, a distribution agent convened a short meeting with village leaders, at which they were informed of the amount to be delivered and asked to prepare a list of recipients (with corresponding amounts), to be collected when the aid was to be delivered three days later. As described in Section III.3, we induced randomized variation in how village leaders were selected for the meeting. At that point agents delivering wheat were asked to collect information on the number of men and women among village leaders and write down the title of the main person leading the discussion.

Second Visit: Three days later, independent contractors hired by WFP delivered the allocated wheat to village leaders and collected the recipient list. In order to limit any effect on distribution outcomes from monitoring the distribution process, wheat was delivered to village leaders identified during the first visit and there were no attempts to observe the delivery of the wheat to households selected by the village leaders. At that point agents delivering the wheat were asked to collect information on the title of the person signing for the wheat delivery and the number of people on the recipient list.

The first two visits are consistent with the way WFP conducts this type of food aid distribution. Village leaders are tasked with drawing up the list of potential beneficiaries and overseeing the food aid distribution process. There is no payment associated with this task for the village leaders or any budget to execute the distribution or pay assistants, and there is no clear and enforceable

means of holding the distributors accountable. This in turn raises some enforcement concerns that are discussed in more detail in the discussion section.⁶

Local Afghan realities precluded us from having monitors on the ground during the second visit to report on the outcomes and processes under study, as monitors are not part of the way wheat distributions are customarily carried out in Afghanistan, and there was therefore serious concern that their presence would alter leader and villager behavior (i.e. would function as an additional treatment rather than a measure).⁷ We therefore introduced a third visit, described below, which allowed us to collect the requisite data to assess the food aid distribution.

Third Visit: Ten days after the delivery, a team of enumerators made an unannounced visit to the village to administer household surveys designed to collect data on distribution outcomes.⁸ These surveys were directed to three groups of villagers: i) a random sample of village households; ii) a random sample of households listed as recipients by the village leaders (listed recipients); and iii) a random sample among households that the random sample of village households indicated had received wheat but that were not listed by village leaders (peer-reported recipients). As the surveys of wheat recipients are not representative of the average villager, we use information from these surveys only to measure recipient characteristics and we use only the first sample to construct village-level measures.

All three sets of questionnaires were administered to a male and a female respondent in the same household. Questionnaires, which were identical across the three groups, collected basic demographic and socioeconomic data on the respondent's household, as well as information on the wheat distribution and characteristics of recipient households. An average of 27 male and female surveys were conducted in each village, including an average of 14 surveys of randomly selected village households, 10 surveys of randomly selected listed recipients, and 5 randomly selected peer-reported recipients.

III.3 - Variation in Management of Distribution

⁶ All forms, guidelines, protocols and materials are available at: <http://www.nsp-ie.com/wheat.html>

⁷ Moreover, WFP made it clear that even if we were to identify positive results associated with introducing a monitor, such micro-level monitoring of food aid distributions is not something that the organization could ever logistically afford to introduce, and therefore not a meaningful policy intervention to be testing.

⁸ To prevent information about the survey from spreading across villages, we surveyed all the villages in a district as quickly as possible, while also surveying them in a sequence that would minimize the geographic spread of information about the survey.

As noted above, elected councils may affect local governance – and, by extension, aid distribution outcomes – either directly by affecting leadership composition and/or indirectly by changing the behavior of the village leadership. To isolate these two effects, randomized variation was induced in the procedures for the first visit to villages with elected councils. The experimental intervention primarily involved varying who was invited to the meeting and designated by the distribution agent as responsible for distribution.

In the beginning of 2011 all villages were classified based on their geographical accessibility (with 13 villages classified as having problems with accessibility and the remaining as not) and expected security situation (with 125 villages classified as potentially insecure and the remaining as not). Randomization of the distribution modalities was performed separately within groups with and without elected councils. Within each group villages were stratified based on geographical accessibility and expected security conditions.

Specifically, in half of NSP villages determined through stratified random assignment, male and female elected council members were invited to the meeting by the distribution agent and asked to select recipients and distribute aid (such villages are collectively referred to as, ‘mandated council’ villages). In the remaining half of NSP villages, ‘village leaders’ were invited to the meeting and asked to select recipients and distribute aid (such villages are referred to as ‘non-mandated council’ villages). Village leaders ordinarily include the village headman and members of the customary village council. Elected council members were not specifically invited, so they were included only in villages in which they were considered village leaders. This randomized variation is used to identify the effects of mandating the elected council to manage the distribution (see Figure 2).

One should note that council management of the distribution would be comprised of both the effect that stems from the council being elected and the effect from having women on the council, i.e. it gets at a bundled effect that also involves a gender dimension. We are therefore also interested in the effect of mandating female involvement in the distribution in non-NSP villages. While women are part of the elected council structure in NSP villages, as discussed above, there is no female participation in customary village councils. This effect could potentially be important for two additional reasons. First, widows comprise a vulnerable group of villagers who are potentially more likely to be identified by female village leaders. Second, women are likely to have better information on the food needs of different households as they are in charge of food preparation.

To identify the effect of mandating involvement of women, in half of the non-NSP villages (i.e. villages without elected councils) as determined through stratified random assignment, we invited both male village leaders as well as women that the village considers as prominent to the meeting and asked them to select recipients (these villages are referred to as ‘mandated female non-council’ villages). In the remaining half of non-NSP villages, village leaders were convened, without explicitly asking women to participate (these villages are referred to as ‘control villages’ since they serve as the control comparison group against which all treatments are assessed). This randomized variation is used below to identify the effects of mandating female participation in the distribution. One needs to note that women in elected councils have half the council seats reserved for them, which they in turn claim through elections. When we mandate that women participate along with the traditional leaders in overseeing the wheat distribution, it is an encouragement to include women who are not elected and as such a lot more likely to be relatives of the existing leaders (i.e. their wives and daughters). They are also not mandated to comply with our encouragement to include women and may choose not to do so at all. As such, this treatment may be perceived as a somewhat weaker treatment than the presence of women in the elected council.

This second stage randomization – with the first stage being having a council or not – is in effect an encouragement design as we are getting at the effect of inviting (depending on the sub-treatment) elected council members or women alongside customary councils to manage the aid distribution, rather than at the actual effect of managing the distribution itself. Though we will discuss how we assessed levels of compliance below, the reported effects should be seen as a low bound on the effect of actual management of the aid distribution because of imperfect compliance.

III.4 - Pre-Analysis Plan

In order to limit the risks of mining data and specifications, our analysis follows a pre-analysis plan that describes all the hypotheses, expected outcomes and exact indicators, outlines the appropriate econometric specifications, and references the use of mean effects.⁹ The pre-analysis plan was archived using The Experiments in Governance and Politics Network design registration tool on 17 January 2012 while data entry was being carried out, but before any data analysis had started. The time-stamped pre-analysis plan is available at <http://e-gap.org/design-registration/>. In Table

⁹ Recent papers that explicitly use a pre-analysis plans include Alatas et. al. (2012), Casey et. al. (2012), Finkelstein et. al. (2012), Humphreys et. al. (2012), Olken et. al. (2010), and Schaner (2011) among others.

A2 we indicate and explain cases where we deviate from the pre-analysis plan, which is largely due to a misspecification in the plan.

V. Hypotheses

The main assumption underlying the hypotheses informing this project is that the creation of gender-balanced, democratically-elected councils, improves governance by making leaders more responsive to the needs of ordinary villagers and less likely to divert public resources for private benefit. The effect may be driven either by elected councils directly assuming responsibility for local governance, or by indirectly affecting the behavior of customary leaders, or a combination of both. In theory, leaders elected by the people and for the people should be more representative and less client-driven than customary leaders and are therefore expected to be more accountable than their customary counterparts. Elected leaders are also on average better educated than customary leaders, suggesting higher levels of competence in both decision-making and implementation. In the context of a food aid distribution, it is expected that these effects will result in a higher proportion of aid reaching needy households (i.e., improved targeting), less embezzlement and nepotism (i.e., less diversion), and a more participatory decision-making process.

Although the creation of an elected council increases the probability that the council manages the distribution, it does not guarantee it. In particular, distributions in villages with elected councils may still be managed by the headman or by tribal elders. In order to separate the direct effect of council management from the indirect effects of council creation, we draw upon the randomized variation induced within villages *with* councils over whether the council was explicitly mandated to select the food aid recipients or not. Specifically, the difference between outcomes from ‘mandated council’ and ‘non-mandated council’ villages indicates the direction of the direct effect, while the difference between ‘mandated council’ villages and ‘non-council’ villages indicates the direction of the combination of the direct and indirect effect. As we expect that both the direct and indirect effects of elected councils will improve governance quality, mandating council management of the distribution is expected to improve targeting, reduce diversion of resources in the form of embezzlement or nepotism, and increase participation.

An additional institutional change induced by the mandating of council management of the distribution is the mandating of female participation, as both female and male elected council members are asked to participate in overseeing the distribution. To isolate the effect of mandating

female participation, we utilize the randomized variation induced within *non-council* villages as to whether or not women are explicitly invited to participate in the distribution. It is important to note that while NSP women whose participation we mandated in the food aid distribution have been elected to the village council, female participation among non-NSP villages involves women who are identified as leading women or women with authority in the community, but who have not been elected. We hypothesize that, by increasing the number of people involved in the selection and introducing a check on leader behavior from a group outside the customary leadership, mandated female participation in either form will improve targeting, reduce diversion, and increase participation.

The aforementioned hypotheses are formalized in tests spanning five dimensions, grouped in three categories: (i) targeting; (ii) diversion; and (iii) participation. The three hypotheses categories – and the constituent hypotheses – are presented below. Note that as the direction of the hypothesized effects is the same for all three interventions, the wording of the hypotheses below is identical, but they refer to three different interventions: (i) mandating that the elected councils oversee the management of the food distribution in a randomly-selected half of council villages; (ii) organizing a food distribution in villages with elected councils without explicit requirements on who manages the distribution; (iii) mandating female participation in the food distribution in villages with customary leadership.

IV.1 - Targeting

Targeting tests assess the extent to which the food aid provided to village leaders for distribution reaches the intended beneficiaries – that is, the most vulnerable households in the village. While some aspects of vulnerability can be captured by objective measures, the limitations of household surveys - as well as differences in how vulnerability is defined - imply that villagers' subjective assessments may sometimes be more accurate (Alatas et al. 2012) and that networks with higher levels of diffusiveness, differentially benefit from community-based targeting policies (Alatas et.al. 2016). For this reason we include both objective and subjective measures of targeting.

The quality of objective targeting is assessed by the characteristics of benefit recipients through observable measures of a household's economic welfare, such as asset ownership or whether the household is a member of a vulnerable group (e.g. widow-headed household or otherwise without an able-bodied, working age male member). Better targeting implies that aid recipients score lower

on measures of economic welfare compared to other villagers. The respective hypothesis is as follows:

Hypothesis 1: The interventions (creating elected councils without their mandated involvement in the distribution; mandating council management of the aid distribution; mandating female participation) will improve the targeting of provided benefits to vulnerable populations in the village, as measured by characteristics of benefit recipients.

To assess subjective targeting we ask community members directly whether they consider wheat recipients as vulnerable or not.

Hypothesis 2: The interventions will improve targeting by village leaders of provided benefits to vulnerable populations in the village, as assessed subjectively by villagers.

IV.2 - Diversion

Another important measure of food aid distribution outcomes is the extent to which aid provided for vulnerable households is diverted for the private benefit of village leaders. This may manifest itself either in the form of embezzlement or nepotism.

Embezzlement represents the direct transfer of aid to the households of village leaders, either those directly involved in the distribution or who otherwise form part of the village leadership. By increasing the accountability of village leaders and by improving checks and balances, the interventions should reduce the diversion of resources by village leaders. The respective hypothesis is as follows:

Hypothesis 3: The interventions reduce embezzlement by village leaders, as measured by reports that village leaders sold, retained, or revoked wheat, as well as by the difference between allocated and received amounts of wheat.

Nepotism represents the distribution of aid to relatives and friends of village leaders and is captured by the following hypothesis:

Hypothesis 4: The interventions reduce nepotism in distribution of benefits by village leaders, as measured by villagers' reports that wheat recipients were connected to village leaders.

IV.3 - Participation

The degree of participation in the distribution describes the extent to which ordinary villagers and marginalized groups (such as women) participate in the process of selecting recipient households. It also describes the transparency of the process – that is, whether villagers were informed of the distribution outcomes – and whether there were any disputes among villagers and/or the village leaders about the distribution. Increased transparency through information about aid beneficiaries rights has been found to reduce leakage and improve program efficiency (Banerjee et.al., 2014). Our expectations of how the interventions will affect such outcomes are captured in the following hypothesis:

Hypothesis 5. The interventions will result in more participatory decision-making processes, as measured by the reported number of people involved in the process, women’s involvement, existence of conflicts related to wheat distribution, and the public announcement of recipients.

V. Methodology

V.1 – Specifications

To test the effect of NSP we estimate the following regression:

$$Y_{vi} = \alpha + \beta T_v + \varphi_p + \varepsilon_{vi} \quad (1)$$

where Y_{vi} is the variable outcome for household i in village v ,¹⁰ T_v is the treatment dummy for village v ; φ_p is the village-pair fixed effect; and ε_{vi} is the error term. Following [Bruhn & McKenzie \(2009\)](#), village-pair fixed effects are included to account for the use of pair-wise village matches in the allocation of treatment. Standard errors are clustered at the village-cluster level to account for correlation of residuals within village-clusters due to non-independence of assignment.

To test the effect of different distributional modalities we use villages where aid distribution was performed by traditional leaders as the baseline group and compare the outcomes in this group with the outcomes in (i) villages where elected councils were put in charge of the distribution; (ii) NSP villages where elected councils were not mandated to manage the distribution; and (iii) non-

¹⁰ Where indicators are constructed at the village level rather than the household level, the outcome is Y_v , rather than Y_{vi} .

NSP villages where female involvement was mandated. In particular, we estimate the following regression:

$$Y_{vi} = \alpha + \eta F_v + \lambda N_v + \mu M_v + \varphi_p + G_v + S_v + \varepsilon_{vi} \quad (2)$$

where Y_{vi} is the outcome of interest for observation i in village v ; F_v is a dummy variable that equals one if a non-NSP village v is assigned to mandated female involvement and zero otherwise; N_v is a dummy variable that equals one if village v is an NSP village in which the elected council was not mandated to manage the distribution and zero otherwise; M_v is a dummy variable that equals one if an NSP village v was assigned to have its distribution managed by the elected council and zero otherwise; φ_p is the village-pair fixed effect, G_v and S_v are dummy variables for geographical accessibility and expected security conditions used in stratifications, and ε_{vi} is the error term. Standard errors are clustered by village-cluster to account for correlation of residuals within village-clusters due to non-independence of assignment.

Note that in all specifications we are estimating intention to treat results. In the case of the effect of NSP these estimates are very close to average treatment effect, because of very high compliance rates (see Beath et al 2013). For the effect of distributional modalities these estimates are likely to be conservative, since we rely on an encouragement design and do not directly observe compliance with the prescribed distributional modality (see Subsection III.3 above).

Table A1 in the Appendix describes in detail all the indicators that we use to test each of the five hypotheses.

V.1 – Identifying Wheat Recipients

Hypothesis 1 and the first indicator for Hypothesis 4 prescribe the comparison of recipients with other villagers and thus necessitate the identification of recipients. However, as the distribution is not directly observed, there is no definitive means to identify recipients. We use the following three sets of information provided by village leaders and survey respondents to identify potential recipients: (i) lists of recipients prepared by village leaders; (ii) self-reports by male and female

survey respondents that their household received wheat; and (iii) households indicated as recipients by other male and female survey respondents.¹¹ This results in three categories of recipients:

- i. *Self-Reported Recipients*: Households in which either the male or female respondent self-reports that the household received wheat from the distribution (regardless of whether the household is designated by local leaders as a recipient);
- ii. *Listed Recipients*: Households which are listed as recipients by the local leader(s) (regardless of whether the household self-reports as such);
- iii. *Peer-Reported Recipients*: Households which first survey respondents (i.e., the random sample of villagers) designate as recipients, regardless of whether the household self-reports or is designated by village leaders.

To ensure that the analysis does not depend on different assumptions concerning which of the sub-samples best represents the actual group of recipients, all hypotheses tests that subsume information on the characteristics of recipients are conducted separately for the three sub-samples.

V.2 – Mean Effects

For each hypothesis, there are often several corresponding outcomes and, for some indicators, there are alternative definitions of wheat recipients. To examine the treatment effect on all indicators pertaining to each hypothesis and to account for multiple hypotheses testing, we estimate the overall average treatment effect. The average treatment effect is estimated by combining the effects on each of the constituent indicators (and each of the definition of recipient households) using the approach in Kling and Liebman (2004). This ‘mean effect index’ is constructed as the mean of the treatment effects for each of the individual outcomes (standardized to have a mean of zero and variance of one), with standard errors estimated using the variance-covariance matrix for the system of seemingly unrelated regressions for all individual outcomes.¹² Thus, the magnitude of the mean effect can be interpreted as the effect of an intervention measured in standard deviations.

VI. Data

VI.1 – Sample Attrition

¹¹ The sets of households within a village suggested by these sources could be partially intersecting.

¹² For further details, see Section IV.ii of the pre-analysis plan. This approach is also employed in [Casey, Glennerster, and Miguel \(2011\)](#).

Due to adverse security or other inaccessibility conditions, wheat was not delivered to 9 villages out of the original 500. Wheat was delivered to the village or to the nearest accessible location. The attrition and the mode of delivery were not correlated with the treatment status or with the group to which the village was assigned.

Most of the data used in the analysis comes from the surveys conducted after the wheat distribution. Table 2 presents information on the number of surveys completed in each type of village. Although the number of villages in which wheat could not be delivered was relatively low, there were numerous villages in which wheat was delivered but which could not be surveyed due to security or logistical problems. In total, male surveys were conducted in 400 villages and female surveys in 356 villages. Attrition was balanced across treatment groups with no statistical difference either between NSP and non-NSP villages, or between groups with different distribution modalities.

There was no significant difference between different groups of villages in the number of surveys per village that were administered to the random sample of village households and to listed recipients. However, the number of surveys of peer-reported recipients was somewhat higher in villages without elected councils (the difference is significant at the 10 percent level), which suggests that the official list of wheat recipients was more accurate in villages with elected councils. However, there were no statistically significant differences between different groups of villages in the amount of wheat distributed, number of villages in the lists of recipients, amount of wheat per recipient, or other observable characteristics of the villages (see Table A3 in the Online Appendix).

VI.2 – Additional Sources of Data

Information on which households were identified as vulnerable *ex-ante* is from the endline survey of the NSP impact evaluation, conducted ten days before the first visit for the food aid distribution. The survey was administered to the male head of household and his wife (or to another senior woman in the same household) separately in ten randomly selected households in each village. Respondents were asked to indicate five households that they considered to be the most needy in the village, the data for which is used to construct an indicator that measures the share of respondents that indicated a recipient household as needy.

Quantitative data collected on the third visit is complemented by qualitative accounts provided by enumerators. Such accounts suggest general compliance with the prescribed variation in the

identity of leaders responsible for aid distribution. In villages with no elected council, the village headman would generally oversee the distribution, along with village elders. In villages with elected councils, both the head of the elected council and the village headman were involved. This joint participation was only curtailed when the elected council was mandated to manage the distribution. Women were generally not involved in the process unless mandated, but when involved were perceived as being better at identifying poor or widowed households than male leaders. Enumerators indicated that ordinary villagers were rarely actively involved in the decision-making process. Villagers usually were just called to receive the wheat allocated to them and, even in the case that they were present during deliberations they would not actively participate in the decision-making. Enumerators suggested that leaders purposefully excluded villagers in order to avoid conflict and disagreement, but that villagers had a good sense of who was in charge of the distribution.

VI.2 – Compliance with Sub-Treatment Assignments

Although we do not directly observe who managed the process of the wheat distribution, we have some indirect evidence that suggests that on average villages were complying with the prescribed modalities. In particular, the food aid distribution agents collected basic information regarding the meeting with village leaders during the first visit and wheat delivery during the second visit. Generally, there were high levels of compliance with the different variations in the first visit procedures.

There were significantly more women and CDC members present at the first and second day visits in NSP villages compared to non-NSP villages (see Tables A4 and A5 in the Online Appendix). The number of representatives co-signing for the receipt of wheat was also slightly higher in NSP villages and non-NSP villages with mandated female participation, but the difference is not statistically significant. In NSP villages where there was no specific requirement that elected council members manage the distribution, 67 percent of meeting attendees identified themselves as members of the elected councils, whereas in villages where the council was mandated to manage the distribution, 90 percent of attendees identified as council members (see Table 1). The average number of women present in villages with required women participation was 5 women, which is close to the number of women that participated in NSP villages with mandated council participation. Across all groups of villages, an average of eight men attended the meeting.

VII. Results

VII.1 – Effect of NSP

Objective Targeting

First, we examine the effect of NSP on aid targeting to needy villagers as measured by observable characteristics. To test this, we examine whether the interventions increase the likelihood that a food aid recipient's measures of assets exceed the village median, reduce the probability that a recipient belongs to a vulnerable group, and increase the level of an omnibus indicator of economic status (which incorporates information on assets, vulnerability, education and the necessity to borrow money for food) for recipients relative to the median. As described in Section V, we use three alternate definitions to identify recipients: (i) self-reported recipients; (ii) listed recipients; and (iii) peer-reported recipients. Table 3 presents estimates of the effect of NSP on measures of objective targeting.

In NSP villages both listed and peer-reported recipients were more likely to fall below the median in terms of economic status by 2 percentage points. In addition, listed-recipients were 2 percentage points more likely to belong to a vulnerable group. The mean effect shows that objective targeting was significantly better in NSP villages, although the effect is modest in size – 3 percent of a standard deviation. Overall, results indicate that targeting, as measured by objective characteristics, is improved in NSP villages.

Subjective Targeting

Next, we examine whether NSP improves aid targeting to vulnerable populations in the village as assessed subjectively by other villagers. The measures used to test this hypothesis are the proportion of respondent households that were identified *ex-ante* by a random sample of villagers as being among the five neediest households in the village; the proportion of households that are *ex-post* reported by a random sample of villagers to be vulnerable; and whether villagers generally perceive that the distribution benefited vulnerable households.¹³

The results presented in Table 4 indicate that there are no significant effects of NSP either on the mean effect for subjective targeting, or on any of the individual measures of subjective targeting.

¹³ The latter variable is the first principal components of binary indicators reporting whether, in the view of the respondent, all deserving households received wheat; no recipients are non-vulnerable; wheat was distributed primarily to vulnerable households; and an ordinal measure assessing the fairness of the distribution.

Embezzlement

To measure the effect of NSP on embezzlement of food aid by village leaders we use the following measures: the incidence of reports from respondents of wheat being retained, sold, or revoked by village leaders; and by the difference between the amounts allocated to recipients on the list prepared by the leaders as compared to the amounts recipients reported to have been received by respondents. The last measure has an advantage of being less likely to be subject to survey bias, as it relies on quantitative comparisons rather than perceptions of respondents.¹⁴

The results in Table 5 indicate that there is only a marginally significant positive effect of NSP on the difference between the amounts allocated to recipients on the list prepared by the leaders as compared to the amounts received by respondents. However, the mean effect of NSP on measures of embezzlement is not statistically significant and is negative in sign. Thus, the results suggest that there was no difference in the amount of embezzlement between NSP and non-NSP villages.

Nepotism

To estimate the effect of NSP on nepotism we look at whether a recipient self-identifies as a relative of the village leadership;¹⁵ reports by other respondents as to whether a recipient is linked to the village leadership; perceptions of non-recipients as to whether the distribution primarily benefited households connected to influential villagers; and at whether wheat was reported to have been given to village leaders not directly involved in the distribution.

The results presented in Table 6 indicate that there are no significant effects of NSP either on the mean effect of nepotism, or on any of the individual measures of nepotism.

Participation

Finally, we examine whether NSP led to more participatory decision-making processes. To test this hypothesis, we look at whether recipient selection was made by more than one individual; whether villagers were consulted; the number of people involved in recipient selection; whether the respondent was involved; whether women were involved; whether there were any conflicts related to the distribution; and whether the identity of recipients was publicly announced.

¹⁴ Survey bias (also known as Clever Hans bias) for this measure is still possible if aid recipients in some treatment groups are more likely to over-report (or under-report) the amount of wheat received as compared with non-recipients.

¹⁵ We subtract the mean in the random sample of respondents in the village to take into account that the creation of the councils itself may lead to an increase in the number of village leaders and, as a result, to an increase in the number of villagers connected to village leaders (including non-recipients).

The results presented in Table 7 indicate that in NSP villages there were more conflicts related to the distribution. However, the mean effect of NSP on measures of participation is not statistically significant and is negative in sign, suggesting that on average participation was the same in NSP and non-NSP villages.

VII.1 – Effect of Distributional Modalities

Objective Targeting

In mandated council management villages, both self-reported and listed recipients were more likely to belong to a vulnerable group (by 3 and 5 percentage points respectively), relative to recipients in control villages in which female participation was not mandated (see Table 8). In addition, listed and peer-reported recipients were more likely to fall below the median in terms of economic status (by 5 and 3 percentage points respectively). The mean effect shows that objective targeting was significantly better in villages with mandated elected council participation (as compared to control villages without mandated female participation), although the effect is modest in size – 6 percent of a standard deviation.

In non-mandated council villages, listed recipients were more likely to fall below the median on measures of assets (by 3 percentage points) and economic status (by 6 percentage points), whereas peer-reported recipients were 4 percentage points more likely to fall below the median in economic status. However, the mean effect for NSP villages without mandated council management is not statistically significant. Within NSP villages the difference between villages with mandated and non-mandated council management are not statistically significant except for the effect on the probability of listed recipients to belong to a vulnerable group, which is positive for mandated council management and not significant for non-mandated council management.

In female mandated non-council villages, listed and peer-reported recipients were more likely to fall below the median in terms of economic status (by 6 and 4 percentage points respectively) and listed recipients were 3 percentage points more likely to score below the median on the measure of assets. The mean effect for the mandated female involvement, however, is not statistically significant.

Overall, the results indicate that targeting, when measured by objective characteristics, is improved if the distribution is mandated to be managed by elected councils rather than by customary leaders.

The results for individual indicators also provide some evidence that the creation of elected councils and the mandated participation of women both improve objective targeting, but the mean effects for these two interventions are not statistically significant. The difference between mandated and non-mandated council management are also not statistically significant.

Subjective Targeting

Mandating the management of the distribution by elected councils has a weakly significant positive impact on whether the distribution had benefited vulnerable households (and this effect is different from NSP villages without mandated council management), but does not affect any other measures of subjective targeting (see Table 9). Thus, the mean effect of mandating council management is also not statistically significant. There are no significant effects of the existence of elected councils *per se* in terms of either individual indicators or mean effects. Mandating female involvement in non-council villages decreases the probability that recipients are *ex-post* perceived as vulnerable by 3 percentage points, but the mean effect for this intervention is not statistically significant.

Overall, the results indicate that there are no significant effects of any of the three interventions on subjective targeting.

Embezzlement

There are no statistically significant differences for any of the embezzlement measures between mandated council villages and control villages where female participation was not mandated (see Table 10). This applies to both individual and mean effect indicators.

However, in villages where elected councils exist but were not mandated to manage the distribution, respondents were 5 percentage points more likely to report that village leaders retained wheat. In addition, the presence of an elected council not mandated to manage the distribution increased the discrepancy between the amount of wheat that was allocated to respondents and the amount of wheat that they received by 3 kg, which corresponds to slightly more than a day's worth of wheat for a household of six according to World Food Organization calculations. The mean effect index indicates that, relative to standard non-council villages, embezzlement was higher in non-mandated council villages by 9 percent of a standard deviation. There is also a statistically significant difference between NSP villages with and without mandated council management in the

retention of wheat by village leaders, as well as in the mean effect (although the latter is significant only at the 10 percent level).

In mandated female non-council villages, respondents were 6 percentage points more likely to report that village leaders retained some wheat and 3 percentage points more likely to report that village leaders sold some wheat. The statistically significant mean effect indicates that embezzlement was higher in female mandated non-council villages by 10 percent of a standard deviation.

Overall, the results indicate that there is no difference in embezzlement in villages where either the elected council or the customary leaders are explicitly designated as responsible for aid distribution. However, both the creation of an elected council without a clear assignment of responsibility for the aid distribution and mandated female participation increases the probability that the village leadership embezzles food aid.

Nepotism

Across the individual indicators, there are only two weakly statistically significant results (see Table 11). Mandating that the council manages the distribution reduces, by 3 percentage points, the share of self-identified recipients who report being connected to village leaders, whereas the existence of an elected council without it being mandated to manage wheat distribution increased by 3 percentage points the number of villagers who indicate that wheat was distributed primarily to households connected to influential villagers. For both of these outcomes the difference between NSP villages with and without mandated council management is statistically significant. The mean effects on the extent of nepotism in aid delivery, however, are not statistically significant for any of the three interventions. Thus, as in the case with subjective measures of targeting, there is no clear difference in nepotism between different groups of villages.

Participation

The fifth hypothesis posits that the interventions result in more participatory decision-making processes. To test this hypothesis, we look at whether the recipient selection was made by more than one individual; whether villagers were consulted; the number of people involved in the recipient selection; whether the respondent was involved; whether women were involved; whether there were any conflicts related to the distribution; and whether the identity of recipients was

publicly announced. **Error! Reference source not found.** presents estimates of the effect of the different interventions on participation.

Mandating that the elected council manages the distribution, increases the number of people involved in the selection by 13 percentage points, while also increasing the incidence of conflicts related to the distribution by one percentage point. The results for the mean effects index, however, indicate that there is no general effect of mandating management by the elected council on participation outcomes.

The presence of an elected council reduces, by 9 percentage points, reports of villagers either being involved or consulted in the selection process and also reduces, by 4 percentage points, the probability that a respondent is involved in the selection process. The mean effect index indicates that participation was lower, by 7 percent of a standard deviation, in villages in which a council exists but was not mandated to participate in the distribution.

Mandating female involvement reduces, by 9 percentage points, reports that villagers were either involved or consulted in the selection process. The mean effect results indicate no effect of mandating female participation on levels of villager participation in the process.

The results thus indicate that the decision-making processes in mandated female non-council and mandated council villages were broadly similar to those in villages where the customary leaders managed the distribution. However, participation was significantly lower in villages where elected councils exist, but were not mandated to manage the distribution.

VII.3 – Additional Results

In this subsection we report some additional results that were not included in the pre-analysis plan, but could nevertheless be helpful in interpreting the results and highlighting the underlying mechanisms.

Controlling for the Economic Effect of NSP

NSP is a bundled treatment involving not only the creation of democratically elected councils, but also the provision of block grants to finance development projects. Thus, the observed difference between villages with customary governance structures and villages with elected councils may potentially be explained by the effects of the influx of resources provided by NSP, rather than by the differences in institutions. The results become even stronger controlling for measures that

capture changes in income, consumption, and assets that come as a result of the program (see Tables A6-A15). In addition, this explanation cannot account for the effects of making councils responsible for the aid distribution and for mandated female participation, since in this case we compare villages where the amount of resources provided by NSP does not vary. Thus, the observed effects on governance quality are likely to be driven by the creation of councils, rather than by the provision of resources.

Effect Of NSP on Perceptions of Local Governance.

To examine the effect of NSP on perceptions of local governance, we exploit information collected during the midline survey (May – October 2009) and endline survey (May – October 2011) for the NSP impact evaluation. In particular, we estimate the following regression:

$$Y_{tvi} = \alpha + \beta_1 \cdot (T_v \times \tau_1) + \beta_2 \cdot (T_v \times \tau_2) + \gamma \cdot \tau_2 + \varphi_p + \varepsilon_{tvi} \quad (2)$$

where Y_{tvi} is the outcome of interest for household i in village v in the midline (1) or endline (2) Survey $t \in \{1,2\}$, T_v is the village treatment dummy, τ_t is the dummy for t , φ_p is the village-pair fixed effect, and ε_{tvi} is the error term. Thus, β_1 corresponds to the impact of NSP at midline and β_2 corresponds to the impact at endline. As in the main regressions, we include village-pair fixed effects and cluster standard errors at the village-cluster level.

The results of this analysis (see Table A16 in the Online Appendix) indicate that NSP had no significant effect on the perceptions of local governance quality in the midline survey, at the time when newly elected councils were assigned the responsibility for managing development projects funded by NSP. However, there is strong evidence of a negative impact on perceptions of local governance at the time of the endline survey, which was conducted after the completion of development projects and at a point when the role of the elected councils was more ambiguous.

Number of Women Involved

We also examine how the effects depend on the number of women involved in the decision-making process, as measured by the number of women present during the first day. The results indicate that targeting based on subjective measures was somewhat better in NSP villages where a higher number of women was involved in the process (see Table A18 in the Online Appendix). The positive effect of women's involvement on subjective measures of targeting was observed in

all sub-treatment groups except for non-NSP villages without mandated women involvement (see Table A23 in the Online Appendix). Higher involvement of women was however also associated with higher embezzlement and nepotism in non-NSP villages, but the effect was exactly the opposite or nonexistent in NSP villages with elected councils (see Table A19 in the Online Appendix). An increase in embezzlement due to higher participation on women was observed in NSP villages without mandated management by the elected council and in non-NSP villages with mandated women involvement, but was the opposite in sign and significant in NSP villages with mandated management by the elected and in non-NSP villages without mandated women involvement (see Table 24 in the Online Appendix). The effect on nepotism was mostly driven by nepotism increasing in the number of women involved in management in non-NSP villages with mandated women involvement (see Table 25 in the Online Appendix).

VIII. Discussion

Our findings indicate that the creation of elected councils through NSP caused an improvement in objective measures of aid targeting, but had no significant effect on subjective measures of targeting, embezzlement, nepotism, or villagers' participation. More detailed analysis indicates that the positive effect of aid was driven primarily by the villages where management of the distribution by elected councils was explicitly mandated. These results suggest that democratically elected local bodies can deliver local aid more equitably than customary institutions, so long as they are clearly put in charge. This finding is consistent with Bardhan (2002,) who argues that for institutional reforms to work, they have to be effectively in control of the underlying power structure.

Villages where elected councils existed, but were not explicitly put in charge of managing the distribution had higher levels of embezzlement and lower levels of participation, but there was no difference in targeting or nepotism as compared to villages where customary leaders managed the distribution. These results suggest that a lack of clear assignment of responsibilities can have a negative effect on the quality of governance, which in turn can reverse the positive effects of directly electing councils, in agreement with Bowles and Gintis (2004), who find that if democratic accountability is weak, the impact of formal institutions will remain constrained.

Mandating female participation had effects broadly similar to those found when councils exist but are not mandated to manage the distribution. Specifically, levels of embezzlement were higher, but there was no difference in terms of aid targeting, nepotism, or villagers' participation in the

decision-making process. These results may appear inconsistent with the positive effects of women's involvement and leadership in local politics (Chattopadhyay and Duflo, 2004, Beaman et al., 2009; Bhavnani, 2009; Beath et al., 2013) but the distinction between that literature and our intervention is that in this instance the women involved were not elected. Mandated female participation in non-NSP villages resulted in increased involvement of traditional female leaders, who are ordinarily related by marriage or blood to male village leaders. In these type of villages embezzlement was increasing with the number of women involved in the decision-making process (see previous section). However, this negative effect was not observed in villages with mandated management of aid distribution by elected councils, in which an increase in the number of women involved in the decision-making process was likely to be driven by elected female leaders. Thus, our results, taken together with the aforementioned literature on the positive effects on development of women's leadership suggest that requiring women's participation in the decision-making process improves outcomes only if it refers to women leaders elected through democratic means.

Our findings suggesting that elected councils offer more equitable governance outcomes are largely consistent with Grossman (2014), who finds that elected leaders exhibit higher levels of responsiveness than appointed leaders, as communities with appointed leaders are less likely to develop the monitoring capacity required to constrain predatory elite behaviors. Our results are also in agreement with Martinez-Bravo et al (2017), although the main mechanisms that drive the results seem to be different. Martinez-Bravo et al (2017) provide suggestive evidence that in the context of democratization in China the effect of elections is driven primarily by re-election incentives, rather than by changes in the quality of office-holders. In Afghanistan, however, re-election incentives of the council members were very weak, which suggests that the effect of elections in this context was driven primarily by selection. Taken together, these results provide evidence that elections can improve governance by addressing both adverse selection and moral hazard problems (Besley, 2006).

The results also indicate that the creation of multiple institutional structures without a defined hierarchy or distinct responsibilities can lead to institutional competition that degrades governance quality instead of creating additional checks and balances that enhance efficiency. This finding contradicts the initial hypotheses of the study, but is broadly consistent with the theoretical predictions of Shleifer and Vishny (1993), who argue that institutional competition and collusion

are important determinants of corruption. In particular, uncoordinated competition for rents between different officials leads to the highest level of corruption. If all village leaders involved in aid distribution have an opportunity to retain a certain amount of wheat for themselves, then an increase in the number of people, responsible for the wheat distribution would increase the total amount of wheat retained by village leaders. As long as the creation of elected councils results in an increase in the number of leaders involved in the decision-making process, this would increase the total amount of rents extracted by village leaders.¹⁶ This can be direct rent extraction through village leaders “taking cuts” from the various resources or indirect by creating more players that in turn inhibit responsiveness and make it hard to attribute blame. Clear assignment of responsibilities for aid distribution, however, restricts the number of people who can extract rents, which leads to lower levels of embezzlement.

The results are also consistent with Persson, Roland and Tabellini (1997), who argue that separation of powers is an effective way of preventing abuse of power, but only if it provides checks and balances - that is, if the two sets of distinct institutions have different interests and are required to reach joint agreement over decisions. If, on the other hand, different institutions make independent claims over resources, then this leads to inefficiencies and diluted accountability. In both settings, making only one institution (or official) responsible for a policy can improve outcomes by preventing inefficient institutional competition. Our results reinforce this argument by showing that the separation of powers can lead to sub-optimal outcomes and that these inefficiencies can be resolved by clear assignment of responsibilities.

This interpretation of the results is also consistent with anecdotal evidence from the reports by enumerators, who confirmed that embezzlement increased when both the elected council and customary leaders were involved in the distribution, as both sets of leaders believed they were entitled to a portion of the food aid as a fee for the service of managing the distribution.¹⁷ While villagers privately opposed this because it deprived poor households of what was due to them, the power wielded by local elites discouraged villagers from opposing the practice in public.

¹⁶ One way to address this assumption empirically is to look at the effect of the number of council members or the overlap between council members and traditional leaders on the outcomes of aid distribution. Both of these measures, however, are highly endogenous and, as a result, such analysis does not provide any consistent results (see Tables A27-A36 in the Online Appendix).

¹⁷ This practice is a specialized form of the informal taxation that often occurs in Afghan villages, whereby local leaders receive an annual payment in kind (e.g., agricultural produce or livestock) from villagers.

The result that the creation of democratically-elected councils by NSP has a negative effect on local governance when institutional roles are ill-defined is consistent with survey-based evidence on the effect of NSP on the perceptions of local governance described in the previous section. It is possible that the negative effect on perception-based measures might not reflect an actual worsening of government quality. Such changes could, for instance, be caused by a relaxation in cultural constraints to voicing dissatisfaction due to the increased involvement of villagers in local decision-making. Similarly, observations of the work of elected councils may have altered the basis against which customary leaders are evaluated. However, the results on embezzlement that are based on the comparison of allocated and received amounts of wheat are harder to reconcile with these explanations, suggesting that certain aspects of governance appear to have deteriorated in treatment villages over the course of the study.

One important difference between customary leaders and elected council members is the difference in their time horizons as decision-makers (Olson, 2000). For customary leaders their position is usually life-long, so they are likely to have longer time-horizons than the members of the elected council, who hold this position only temporarily and do not have clear reelection incentives. A longer time-horizon is likely to put additional limits on rent-seeking behavior, which suggests that traditional leaders are less prone to corruption than members of the elected council. However, as long as elected councils make traditional leaders less sure that they will retain their position in the future and, thus, shorten their time-horizon, we should observe more embezzlement in villages in which councils were created. Accordingly, while differences in decision makers' time horizons can explain the negative indirect effect of council creation, they cannot explain the lack of any negative direct effect, i.e. when the council is mandated to be in charge.

Improvements in targeting that come as a result of making the councils responsible for the food aid distribution may be driven by a reduction in shirking of the leaders responsible for the distribution. Determining which households should receive wheat requires a certain degree of effort and when responsibilities are not clearly assigned, leaders can free ride on each other, which results in worse targeting. However, the results on embezzlement cannot be explained by differences in the levels of shirking and instead indicate that the creation of elected councils without clear assignment of responsibilities leads to an increase in rent-seeking.

Our results expand upon and offer additional nuance to findings in the literature on the transplantation of institutions. Most existing works argue that informal pre-existing structures are resilient to change (Easterly, 2006) and institutions that develop internally are much more likely to be effective than institutions that are externally imposed as the latter are rarely attuned to the country-specific context in which they are operating (Hayek 1960; Berkowitz, Pistor, and Richard 2003). Our results, which support knowledge of the role of underlying existing institutional structures (per Humphreys et. al. 2014), indicate, that externally imposed democratic institutions can have a positive effect, but only if they are specifically assigned full responsibility for certain tasks. This finding is consistent with Acemoglu et al. (2011), which argues that exogenously imposed reforms may be successful if sufficiently effective in displacing existing power-holders from *de facto* authority.

IX. Conclusion

This paper exploits randomized variation in the structure of local governance institutions to examine the effect of the creation of democratically elected councils on the quality of local governance in Afghan villages. In that regard, we use the outcomes of a food aid distribution as a measure of local leader performance. To explore mechanisms underlying the effects of council creation, we induced randomized variation in whether councils are explicitly mandated to manage the distribution. We also induced randomized variation in whether women are required to participate in the distribution in those villages that do not have elected councils.

Our findings indicate that the aid distribution was better in villages where elected village councils were created less than four years before the distribution took place. More detailed analysis demonstrates, that the quality of targeting is improved without any adverse effects on embezzlement or nepotism only if elected councils are explicitly mandated to manage the distribution. However, if councils exist but there is no clear assignment of responsibility for managing the distribution, the positive effect on targeting is muted, embezzlement is higher and decision-making processes are less inclusive. At the same time, in villages without elected councils, required participation in aid distribution of prominent village women increased embezzlement, without improving aid targeting.

Our results show that, subject to a clear assignment of responsibilities, local elected institutions improve the quality of local governance, which is in line with findings by Martinez-Bravo et al.

(2017) and Grossman (2014). The results also show that the existence of parallel institutions can lead to underperformance if responsibilities are not clearly delineated, which is consistent with the theoretical literature that suggests that competition for rents between different institutions can lead to inefficient outcomes that might be improved by clear assignment of responsibilities (Shleifer and Vishny, 1993; Persson, Roland and Tabellini 1997, Berry 2009).

One of the main limitations in providing direct evidence on the mechanisms behind the effects that we identify in our study is that we did not seek to directly observe the decision-making process, because the act of monitoring in this context would have undoubtedly affected the behavior of village leaders. One avenue for future research, would be to develop data collection modalities capable of directly observing decision-making processes while limiting the extent to which such observation alters the incentives for decision-makers, which in turn should further enhance our understanding of the mechanisms through which institutional reforms can enhance governance outcomes overall.

Our findings on the potential negative effects of creating parallel institutions and superficial involvement of women in aid initiatives are important from both an academic and policy perspective. These are among the most frequent institutional interventions in development and they seem to have potential to either improve or harm local governance depending on the particular way in which they are implemented. Thus, our results stress one more time the need for careful design of such interventions and the analysis of how implanted institutions interact with pre-existing governance structures.

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Figure 1: Variation in Distribution Procedures

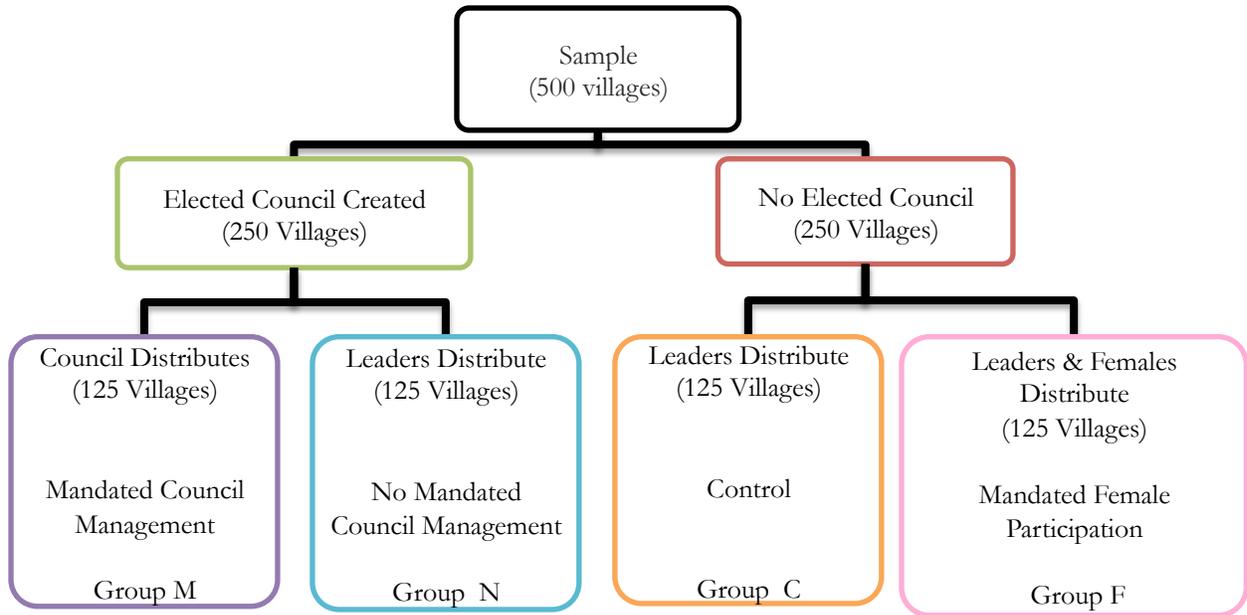


Table 1: Wheat Delivery Process

	Non-Council Villages		Council Villages	
	Unconstrained	Female	Unconstrained	Council
	<i>Group C</i>	<i>Group F</i>	<i>Group N</i>	<i>Group M</i>
Number of villages	125	125	125	125
Mean Number of Men Present at the First Visit	7.9	8.1	8.1	7.9
The Person Leading Discussion at the First Visit Identified as a CDC Member	2%	3%	67%	90%
The Person Signing for Wheat at the Second Visit Identified as a CDC Member	16%	11%	61%	67%
Women Present at First Visit Meeting	6	91	8	100
Mean Number of Women Present at First Visit	0.2	4.7	0.4	5.2

Table 2: Survey Coverage

	Non-Council Villages		Council Villages	
	Unconstrained	Female	Unconstrained	Council
	<i>Group C</i>	<i>Group F</i>	<i>Group N</i>	<i>Group M</i>
Panel A: Number of Respondents				
Random Male Surveys	1539	1387	1337	1450
Random Female Surveys	1397	1276	1206	1257
Listed Recipients Male Surveys	1021	975	968	951
Listed Recipients Female Surveys	895	893	845	808
Peer-Reported Rec. Male Surveys	301	305	247	271
Peer-Reported Rec. Female Surveys	302	299	255	262
Panel B: Survey Attrition (village level)				
Missing Random Male Surveys	20	27	28	25
Missing Random Female Surveys	31	36	38	39
Missing Listed Recipients Male Surveys	28	28	30	31
Missing Listed Recipients Female Surveys	38	37	40	45
Missing Peer-Reported Rec. Male Surveys	64	62	76	68
Missing Peer-Reported Rec. Female Surveys	65	67	75	75

Table 3: Effect of NSP on Objective Measures of Targeting

Outcomes	Mean Value in No Councils	Councils Exist (1)	Obs.
		Self-Reported Recipients	
Household Assets Below Median	0.779	0.008 [0.010]	5,480
Vulnerability Status	0.114	0.015 [0.010]	6,030
Omnibus Indicator of Economic Status Below Median	0.779	0.013 [0.010]	5,356
		Listed Recipients	
Household Assets Below Median	0.881	0.007 [0.011]	4,310
Vulnerability Status	0.118	0.017* [0.010]	4,655
Omnibus Indicator of Economic Status Below Median	0.881	0.024** [0.010]	4,212
		Peer-Reported Recipients	
Household Assets Below Median	0.836	-0.003 [0.013]	4,231
Vulnerability Status	0.110	0.012 [0.011]	4,569
Omnibus Indicator of Economic Status Below Median	0.837	0.018* [0.011]	4,130
<i>Mean Effects Index</i>		0.033**	

Notes: All estimates are relative to values in non-CDC villages without mandated women's participation. Regressions include controls for matched-pair fixed effects, and indicators of geographical accessibility and expected security situation. Robust standard errors adjusted for clustering at the village-cluster level in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4: Effect of NSP on Subjective Measures of Targeting

Outcomes	Mean Value in No Councils	Councils Exist	Obs.
		(1)	
Share of Respondents <i>Ex-Ante</i> Identifying Self-Reported Recipient as Vulnerable	0.043	0.001 [0.002]	10,268
Share of Respondents <i>Ex-Ante</i> Identifying Listed Recipient as Vulnerable	0.064	-0.004 [0.003]	13,085
Share of Respondents <i>Ex-Ante</i> Identifying Peer-Reported Recipient as Vulnerable	0.056	0.001 [0.004]	13,728
Proportion of Recipients Reported <i>Ex-Post</i> to be Vulnerable	0.968	0.001 [0.006]	5,989
Distribution Perceived to Have Benefited Vulnerable Households	-0.044	0.078 [0.063]	6,364
<i>Mean Effects Index</i>		0.010	

Notes: All estimates are relative to values in non-CDC villages without mandated women's participation. Regressions include controls for matched-pair fixed effects, and indicators of geographical accessibility and expected security situation. Robust standard errors adjusted for clustering at the village-cluster level in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: Effect of NSP on Embezzlement

Outcomes	Mean Value in No Councils	Councils Exist	Obs.
		(1)	
At Least Some Wheat Retained by Village Leader(s)	0.091	-0.014 [0.012]	6,129
At Least Some Wheat Sold by Village Leader(s)	0.019	-0.003 [0.005]	6,994
At Least Some Wheat Revoked by Village Leader(s) following Distribution	0.014	-0.002 [0.007]	5,639
Difference between Amount Allocated and Amount of Received	2.602	1.422* [0.800]	9,610
<i>Mean Effects Index</i>		-0.008	

Notes: All estimates are relative to values in non-CDC villages without mandated women's participation. Regressions include controls for matched-pair fixed effects, and indicators of geographical accessibility and expected security situation. Robust standard errors adjusted for clustering at the village-cluster level in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6: Effect of NSP on Nepotism

	Mean Value in No Councils	Councils Exist	Obs.
		(1)	
Proportion of Recipients Connected to Village Leaders (Self-Identified Recipients)	-0.032	-0.005 [0.011]	7,179
Proportion of Recipients Connected to Village Leaders (Listed Recipients)	-0.047	0.011 [0.013]	5,200
Proportion of Recipients Connected to Village Leaders (Peer-Reported Recipients)	-0.048	0.001 [0.014]	5,191
Recipient Self-Identifies Household as Related to Village Leaders	0.240	-0.000 [0.023]	6,016
Wheat Distributed Primarily to HHs Connected to Influential Villagers	0.064	-0.001 [0.009]	7,076
Wheat Distributed to Leaders Not Involved in Decision-Making Process	0.931	-0.000 [0.008]	6,545
<i>Mean Effect Index</i>		0.002	

Notes: All estimates are relative to values in non-CDC villages without mandated women's participation. Regressions include controls for matched-pair fixed effects, and indicators of geographical accessibility and expected security situation. Robust standard errors adjusted for clustering at the village-cluster level in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 7: Effect of NSP on Participation

	Mean Value in No Councils	Councils Exist	Obs.
		(1)	
Selection of Recipients by More than One Person	0.824	0.005 [0.014]	7,318
Villagers Selected or Were Consulted	0.232	0.004 [0.024]	5,891
Logarithm of Number of People Involved in Selection	1.774	0.030 [0.035]	6,950
Respondent Involved in Selection	0.111	-0.003 [0.007]	9,393
Women Involved in Selection	0.031	-0.002 [0.006]	8,378
No Conflicts Related to Distribution	0.981	-0.009** [0.004]	7,526
Identity of Recipients Publicly Announced	0.651	0.022 [0.019]	7,650
<i>Mean Effects Index</i>		0.002	

Notes: All estimates are relative to values in non-CDC villages without mandated women's participation. Regressions include controls for matched-pair fixed effects, and indicators of geographical accessibility and expected security situation. Robust standard errors adjusted for clustering at the village-cluster level in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 8: Effect of Distribution Modalities on Objective Measures of Targeting

Outcomes	Mean Value in No Councils without Mandated Female Part. <i>Group C</i>	Councils Exist, Mandated Council Management <i>Group M</i>	Councils Exist, No Mandated Council Management <i>Group N</i>	No Councils, Mandated Female Participation <i>Group F</i>	Obs.	p-value for equality of coefficients in (1) and (2)
		(1)	(2)	(3)		
Self-Reported Recipients						
Household Assets Below Median	0.779	-0.002 [0.014]	-0.002 [0.019]	-0.020 [0.020]	5,480	0.992
Vulnerability Status	0.105	0.034** [0.015]	0.021 [0.020]	0.027 [0.019]	6,030	0.448
Omnibus Indicator of Economic Status Below Median	0.779	0.010 [0.014]	0.003 [0.018]	-0.014 [0.020]	5,356	0.700
Listed Recipients						
Household Assets Below Median	0.874	0.015 [0.020]	0.031* [0.017]	0.033* [0.017]	4,310	0.399
Vulnerability Status	0.112	0.046*** [0.017]	-0.012 [0.018]	0.001 [0.018]	4,655	0.001
Omnibus Indicator of Economic Status Below Median	0.865	0.047*** [0.017]	0.055*** [0.016]	0.056*** [0.018]	4,212	0.655
Peer-Reported Recipients						
Household Assets Below Median	0.833	0.004 [0.020]	-0.002 [0.021]	0.010 [0.021]	4,231	0.797
Vulnerability Status	0.102	0.020 [0.016]	-0.000 [0.020]	-0.004 [0.021]	4,569	0.282
Omnibus Indicator of Economic Status Below Median	0.827	0.033** [0.016]	0.037* [0.019]	0.036* [0.021]	4,130	0.830
<i>Mean Effects Index</i>		0.063***	0.034	0.033		0.261

Notes: All estimates are relative to values in non-CDC villages without mandated women's participation. Regressions include controls for matched-pair fixed effects, and indicators of geographical accessibility and expected security situation. Robust standard errors adjusted for clustering at the village-cluster level in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 9: Effect of Distribution Modalities on Subjective Measures of Targeting

Outcomes	Mean Value in No Councils without Mandated Female Part. <i>Group C</i>	Councils Exist, Mandated Council Management <i>Group M</i>	Councils Exist, No Mandated Council Management <i>Group N</i>	No Councils, Mandated Female Participation <i>Group F</i>	Obs.	p-value for equality of coefficients in (1) and (2)
		(1)	(2)	(3)		
Share of Respondents <i>Ex-Ante</i> Identifying Self-Reported Recipient as Vulnerable	0.043	-0.001 [0.004]	0.005 [0.005]	0.002 [0.005]	10,268	0.175
Share of Respondents <i>Ex-Ante</i> Identifying Listed Recipient as Vulnerable	0.069	0.002 [0.006]	0.001 [0.006]	0.010 [0.007]	13,085	0.885
Share of Respondents <i>Ex-Ante</i> Identifying Peer-Reported Recipient as Vulnerable	0.058	-0.002 [0.007]	0.008 [0.007]	0.005 [0.008]	13,728	0.142
Proportion of Recipients Reported <i>Ex-Post</i> to be Vulnerable	0.972	-0.013 [0.008]	-0.012 [0.012]	-0.031*** [0.012]	5,989	0.920
Distribution Perceived to Have Benefited Vulnerable Households	-0.024	0.192* [0.103]	-0.155 [0.108]	-0.098 [0.141]	6,364	0.013
<i>Mean Effects Index</i>		0.000	-0.018	-0.041		0.655

Notes: All estimates are relative to values in non-CDC villages without mandated women's participation. Regressions include controls for matched-pair fixed effects, and indicators of geographical accessibility and expected security situation. Robust standard errors adjusted for clustering at the village-cluster level in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 10: Effect of Distribution Modalities on Embezzlement

Outcomes	Mean Value in No Councils without Mandated Female Part. <i>Group C</i>	Councils Exist, Mandated Council Management <i>Group M</i>	Councils Exist, No Mandated Council Management <i>Group N</i>	No Councils, Mandated Female Participation <i>Group F</i>	Obs.	p-value for equality of coefficients in (1) and (2)
		(1)	(2)	(3)		
At Least Some Wheat Retained by Village Leader(s)	0.087	-0.012 [0.020]	0.051** [0.020]	0.065*** [0.024]	6,129	0.006
At Least Some Wheat Sold by Village Leader(s)	0.014	0.012 [0.008]	0.008 [0.012]	0.028** [0.011]	6,994	0.683
At Least Some Wheat Revoked by Village Leader(s) following Distribution	0.018	-0.015 [0.010]	-0.007 [0.013]	-0.018 [0.012]	5,639	0.582
Difference between Amount Allocated and Amount of Received	1.882	1.703 [1.293]	3.175** [1.479]	2.004 [1.554]	9,610	0.364
<i>Mean Effects Index</i>		0.005	0.090*	0.109**		0.097

Notes: All estimates are relative to values in non-CDC villages without mandated women's participation. Regressions include controls for matched-pair fixed effects, and indicators of geographical accessibility and expected security situation. Robust standard errors adjusted for clustering at the village-cluster level in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 11: Effect of Distribution Modalities on Nepotism

	Mean Value in No Councils without Mandated Female Part. <i>Group C</i>	Councils Exist, Mandated Council Management <i>Group M</i>	Councils Exist, No Mandated Council Management <i>Group N</i>	No Councils, Mandated Female Participation <i>Group F</i>	Obs.	p-value for equality of coefficients in (1) and (2)
		(1)	(2)	(3)		
Proportion of Recipients Connected to Village Leaders (Self-Identified Recipients)	-0.034	-0.029* [0.016]	0.022 [0.018]	0.002 [0.022]	7,179	0.015
Proportion of Recipients Connected to Village Leaders (Listed Recipients)	-0.055	0.013 [0.020]	0.023 [0.024]	0.015 [0.026]	5,200	0.683
Proportion of Recipients Connected to Village Leaders (Peer-Reported Recipients)	-0.049	0.002 [0.020]	0.016 [0.023]	0.017 [0.027]	5,191	0.597
Recipient Self-Identifies Household as Related to Village Leaders	0.225	0.046 [0.035]	-0.017 [0.033]	0.037 [0.048]	6,016	0.172
Wheat Distributed Primarily to HHs Connected to Influential Villagers	0.066	-0.014 [0.014]	0.031* [0.016]	0.015 [0.020]	7,076	0.019
Wheat Distributed to Leaders Not Involved in Decision-Making Process	0.930	-0.007 [0.013]	-0.011 [0.013]	-0.018 [0.015]	6,545	0.798
<i>Mean Effect Index</i>		0.005	0.030	0.032		0.491

Notes: All estimates are relative to values in non-CDC villages without mandated women's participation. Regressions include controls for matched-pair fixed effects, and indicators of geographical accessibility and expected security situation. Robust standard errors adjusted for clustering at the village-cluster level in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 12: Effect of Distribution Modalities on Participation

	Mean Value in No Councils without Mandated Female Part. <i>Group C</i>	Councils Exist, Mandated Council Management <i>Group M</i>	Councils Exist, No Mandated Council Management <i>Group N</i>	No Councils, Mandated Female Participation <i>Group F</i>	Obs.	p-value for equality of coefficients in (1) and (2)
		(1)	(2)	(3)		
Selection of Recipients by More than One Person	0.816	0.030 [0.022]	-0.009 [0.030]	0.016 [0.029]	7,318	0.158
Villagers Selected or Were Consulted	0.236	0.010 [0.035]	-0.109** [0.045]	-0.099** [0.047]	5,891	0.016
Logarithm of Number of People Involved in Selection	1.759	0.130** [0.053]	-0.106* [0.062]	-0.008 [0.071]	6,950	0.001
Respondent Involved in Selection	0.122	0.012 [0.015]	-0.038*** [0.012]	-0.016 [0.014]	9,393	0.001
Women Involved in Selection	0.019	0.007 [0.010]	-0.002 [0.010]	0.010 [0.012]	8,378	0.470
No Conflicts Related to Distribution	0.983	-0.013** [0.006]	0.001 [0.009]	0.004 [0.008]	7,526	0.092
Identity of Recipients Publicly Announced	0.665	0.026 [0.029]	0.016 [0.033]	-0.001 [0.042]	7,650	0.774
<i>Mean Effects Index</i>		0.044	-0.072*	-0.022		0.007

Notes: All estimates are relative to values in non-CDC villages without mandated women's participation. Regressions include controls for matched-pair fixed effects, and indicators of geographical accessibility and expected security situation. Robust standard errors adjusted for clustering at the village-cluster level in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.