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Social and economic impacts of *Tuungane*

Final report on the effects of a community-driven reconstruction programme in the Democratic Republic of Congo

March 2014

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Social and economic impacts of *Tuungane*: final report on the effects of a community-driven reconstruction programme in the Democratic Republic of Congo

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Finally, we note that this document draws extensively from text we produced for our report *Social and economic impacts of Tuungane* (Humphreys, M, De La Sierra, R S, and Van Der Windt, P, 2012. *Social and economic impacts of Tuungane. Final report on the effects of a community driven reconstruction program in eastern Democratic Republic of Congo,* New York: Columbia University).

Abstract

We assessed the impacts of a large randomised community-driven reconstruction (CDR) programme implemented in eastern Democratic Republic of Congo (DRC). The programme sought to improve welfare and to strengthen local cohesion and local governance capacity. We found that, although the programme was well implemented, the effects of the programme on the economic well-being and on the socio-political attitudes and behaviours of populations were weak at best. These null findings are broadly in line with results from other studies but contradict some of the strong claims made on behalf of the CDR model.

The programme: The evaluation examined the impact of the *Tuungane* programme.[1] *Tuungane* has been working since 2007 in 1,250 war-affected villages with a targeted beneficiary population of approximately 1.78 million people. Over this period, *Tuungane* organised the election of village committees as well as training in leadership, good governance and social inclusion. Elected committees worked with populations to select and implement development projects. The theory behind the intervention was that training, coupled with exposure to, and practice in, accountable governance could produce learning by doing and could bring about change in local accountability and social cohesion.

Assessing impact: This research, mounted in partnership with the International Rescue Committee (IRC) and CARE International, sought to measure whether these objectives were met. To measure causal effects we employed the method of randomised intervention. The *Tuungane* communities were randomly selected through public lotteries from a larger pool of potential participating communities. In addition, among a subsample of those selected, a random subset of communities implemented a variation of the programme in which community development committees were not required to have gender parity.

Innovations in measurement: The core behavioural measures were generated through the introduction of an entirely new and distinct unconditional cash transfer scheme (RAPID), in which both treatment and control villages were selected to receive block grants of US$1,000, which they could manage as they saw fit. We then examined differences in the ways treatment and control communities managed these new funds.

Findings on implementation: While the quality of implementation was not the focus of this research, our data confirmed that *Tuungane* was successful in implementing a large number of projects in the target areas, that the projects were in line with the preferences of the populations and that the populations reported high levels of exposure to project activities and satisfaction with the project outcomes.

Findings on impacts: On most measures we failed to find evidence that these positive experiences with the intervention led to behavioural changes. In general, we found for many local governance measures outcomes were strong in both control and treatment areas. For example, close to half of all RAPID committees were selected through electoral procedures, yet the likelihood of using elections was as great in non-*Tuungane* as in *Tuungane* areas. *Tuungane* communities included more women on committees to manage RAPID funds but the substantive difference was weak. There was also some

[1] *Tuungane* means 'Let's join together' in Swahili.

weak evidence for improvements in a survey-based measure of trust. There was little evidence of positive economic effects and some (generally scattered) evidence suggesting adverse effects.

Findings on gender provisions: We provided a small set of results on the effects of mandatory women's participation. We found that even without a requirement of gender parity, women comprised approximately 30 per cent of the committee members. This suggested that the gender parity requirement was not needed to ensure that there was a degree of representation. Rather, it increased the numerical strength of women on committees. There was evidence that the inclusion requirement resulted in different project selection, although differences did not reflect differences between the stated preferences of men and women in the population. We found no evidence of positive changes in attitudes towards the roles and responsibilities of women as a result of the gender parity requirement.

Implications: Our null findings are broadly in line with results from other studies that evaluated CDR projects but contradict some of the strong claims made on behalf of the CDR model. These findings suggest that there is need to reassess the effectiveness of the CDR model and the theory of change behind it.

Key questions raised by the evaluation include:

- Is the scale of CDR interventions well calibrated? Although the temporal and spatial coverage of this project was very great, the *per capita* investments were small.
- Is it possible, or appropriate, to change power structures without introducing material changes in power relations? The current theory relies largely on demonstration effects, but these may not be sufficient.
- Are CDR programmes pitched at the right level to effect change in governance structures? Governance problems may be more muted at local levels than at higher levels; indeed, that is one rationale for employing CDR programmes in the first place.

The results also suggest that external efforts to empower women by mandating their presence on village committees may not be effective. Finally, these results highlight the need to clarify the rationale for employing CDR models. We emphasise that, even if CDR does not transform structures of accountability, this does not imply that CDR is not an effective way to disburse funding in an accountable manner. Yet clarity regarding the most appropriate rationale is critical, since these different rationales can have different implications for the appropriate scale of funding made available through the CDR mechanism.

Contents

List of tables, figures and items

Abbreviations and acronyms

CDD	community-driven development
CDR	community-driven reconstruction
CDC	community development committee
DFID	Department for International Development (UK Government)
DRC	Democratic Republic of Congo
IDP	internally displaced person
IRC	international Rescue Committee
LLU	lowest-level units
NGO	non-governmental organisation
RAPID	Recherche-Action sur les Projets d'Impact pour le Développement (Action Research on Impact Projects for Development)
VDC	village development committee
watsan	water and sanitation

1. Introduction

Community-driven development and community-driven reconstruction (CDD and CDR) programmes have become major tools used to support local development. The World Bank, for example, reports the rise of this sector in its own portfolio, with an average US$1.3 billion a year in lending between 2000 and 2008. In 2008 alone CDD accounted for US$1.9 billion in loans (Mansuri and Rao 2013). However, the evidence for the effectiveness of this approach is weak. One review by Mansuri and Rao (2004) found mixed evidence that community-driven development programmes strengthen the targeting of beneficiaries, enhance service delivery and improve sustainability, and no evidence as yet that the participatory nature of CDR generates beneficial outcomes. An observational study by Khwaja (2004) found that community participation in some types of decisions is associated with weaker performance, while Olken (2007) found that (increased) community oversight did little to reduce corruption in Indonesia. Other work claims that development interventions are unlikely to alter fundamental structures or that, perversely, they may create dependency (Moyo 2009).

A new generation of rigorous impact evaluations of the CDR approach is now underway to address these questions. One of the first of this kind was conducted in the context of another DFID-funded IRC CDR programme in Liberia (Fearon, Humphreys and Weinstein, 2009). Other major evaluations have taken place in Afghanistan and Sierra Leone.

We describe here results from an assessment of the impacts of *Tuungane*, a major CDR programme implemented in eastern Democratic Republic of Congo (DRC). The programme, funded by the UK government and implemented by the International Rescue Committee (IRC) and CARE International between 2007 and 2011, was designed both to support economic recovery and to improve the quality of local governance and social cohesion. This research sought to measure whether these objectives were met.

In order to measure the effects of *Tuungane*, our research used a method of randomised intervention that allowed us to observe a set of non-*Tuungane* communities that were similar (in expectation) to the *Tuungane* communities in every respect except for the presence of the programme. Technically, these are the control communities. In all, 280 communities were assigned to treatment conditions (that is, exposure to the *Tuungane* project) through public lotteries, while the remaining 280 were not (the control communities)[2]. In each of these areas the programme organised the election of development committees to oversee the implementation of development projects. These committees, by design, comprised equal numbers of men and women. In a randomly sampled subset of areas, however, a variation of the programme was introduced in which community development committees were not required to have gender parity, enabling the assessment of the effects of gender parity on outcomes.

Our analysis produced, for the most part, little evidence of effects on social and economic outcomes. Whenever research produces null results there is a natural question about whether the non-findings reflect a reality of weak effects or reflect shortcomings in research design or implementation. We believe that, in general, the research design and implementation was strong in this case. However, we encourage readers to pay particular attention to the elements of the programme studied (only early economic

[2] Although, for reasons described below, not all of these 280 were part of the final frame.

projects were implemented), the measurement strategy (a reliance on a naturalistic behavioural exercise in which populations sought to solve a real collective action problem) and possible threats to the validity of results.

We discuss the context in which *Tuungane* was implemented in Section 2. Section 3 describes the programme strategy and Section 4 describes the implementation. Section 5 describes our evaluation strategy and Section 6 provides the main results, reporting the estimated impacts of the intervention on a series of social and economic outcomes. This section includes a short analysis of the effects of one element of programme design on attitudes regarding the rights and roles of women. Section 7 provides a set of robustness tests. Section 8 concludes and provides recommendations. The appendixes describe the research strategy and the strengths and weaknesses of the approach used.

2. Context

2.1 The community-driven development/community-driven reconstruction model and evidence from other contexts

Many development groups in post-conflict areas throughout the developing world implement the model of CDD or CDR. The World Bank reports the rise of this sector in its own portfolio, with US$1.3 billion a year in lending in 2000–2008 directed towards about 50 CDD projects. In 2008, CDD accounted for US$1.9 billion in loans. CDR programmes have been particularly popular in areas emerging from conflict, including Afghanistan, Liberia, Sierra Leone, Rwanda, Bosnia and Herzegovina, and Aceh. Advocates argue that the model is strong and effective. The World Bank claims, for example, that 'CDD operations produce two primary types of results: more and better-distributed assets, and stronger, more responsive institutions'.

Until recently, the popularity of the model notwithstanding, there has been little evidence of the impacts of CDR programmes (Mansuri and Rao 2004). Moreover, the basic principle behind CDR, that exposure to good governance practices over the course of a couple of years can alter social behaviour, runs largely counter to classic accounts of the determinants of social behaviour that emphasise structural and slow-moving features. For example, see discussions in Putnam (1993), Bowles and Gintis (2004) or Nunn (2008). More recently, however, there have been a number of studies examining the social and economic effects of these programmes. These studies have painted a mixed picture. In their study of a CDR programme in Liberia, Fearon *et al.* (2009) found little or no evidence for economic impacts but some positive evidence for an effect of CDR on the ability of communities to collectively solve problems. Casey *et al.* (2011) examined a CDR programme in Sierra Leone and found evidence of economic effects (many of which were on outputs rather than outcomes) but no evidence of any social effects. The economics investments they examined amount to about US$5 per person a year, which, while still small, is five times larger than the village development committee (VDC) investments examined here. Barron *et al.* (2009) also found economic effects but no social impacts of a CDR programme in Aceh. That programme had grants of about US$20 per capita a year in conflict affected regions. Final results from a major study in Afghanistan are not yet available, although initial results suggest that the introduction of committees in itself does not improve local governance, but that the existence of these committees can still provide a tool for implementing future programmes. An overall assessment of this emerging literature suggests uneven performance of the CDR model and that the claims made on its behalf are not strongly supported.

3. Description of intervention and theory of change

3.1 Goals of the programme

As stated in the original project description document, the aims of the *Tuungane* programme were:

> …to improve the stability and quality of life for communities in eastern DRC through structured, participatory, and inclusive collective action. By establishing and strengthening participatory local governance committees [the programme aims…] to improve the understanding and practice of democratic governance, improve citizens' relationships with local government, and improve social cohesion and thereby communities' ability to resolve conflict peacefully. The conduit to achieve these purposes will be village-level and community-level projects that themselves will contribute to socio-economic rehabilitation as DRC moves into a post-conflict and development period (IRC, 2006).

From these broad goals in 2007 the research team and IRC jointly formed a set of specific primary hypotheses.[3] A broader set of secondary hypotheses, relating to variations in implementation, heterogeneous effects, contextual factors, unintended consequences, behavioural outcomes and measurement strategies, are described in the final Outcomes and Data Sources (ODS) design document.[4] The primary hypotheses are shown in Table 1 below, alongside a reference to tables in this report that provide evidence in support of, or against, the hypotheses.

In this report we provide results on key measures to test these primary hypotheses as well as a set of related behavioural hypotheses that capture further dimensions of the quality of local governance. We organise our presentation by theme rather than by hypothesis number, first examining impacts on five dimensions of governance, then examining impacts on social cohesion and on welfare.

[3] http://www.columbia.edu/~mh2245/DRC/DRC_DESIGN.pdf
[4] http://cu-csds.org/wp-content/uploads/2011/03/20110304-ODS-REGISTRATION.pdf

Table 1 Primary hypotheses

#	Category	Hypothesis	Table(s)
H1	Cohesion	Individuals in *Tuungane* communities will exhibit higher levels of acceptance of others into their communities.	Table 25–27
H2	Cohesion	Individuals in *Tuungane* communities will exhibit higher levels of trust in other members of their communities.	Table 27
H3	Cohesion /participation	*Tuungane* communities will be more willing to contribute time and effort individually to collective goods.	Table 9
H4	Cohesion	*Tuungane* target communities will be more likely to work together to solve local development problems.	Table 28
H5	Accountability	Communities will be more proactive in seeking support from local government and NGOs for community initiatives and the private sector.	Table 18
H6	Cohesion	Villages in *Tuungane* communities will have a greater propensity to work collectively with other villages to address development challenges.	Table 29
H7	Participation	Individuals in *Tuungane* communities will report a greater sense of a right to take part in local decisions.	Table 4 Table 10 Table 11
H8	Participation	Individuals in *Tuungane* communities will report a greater sense of obligation to take part in local decisions.	Table 12
H9	Transparency	Individuals in *Tuungane* communities will report greater knowledge about local decision-making processes and outcomes.	Table 17 Table 19 Table 20
H10	Accountability	Individuals in *Tuungane* communities will report an increased willingness to hold traditional and political leaders accountable.	Table 13 Table 14 Table 15
H11	Participation	Individuals in *Tuungane* communities are more likely to believe that local leaders should be elected rather than selected through an alternative mechanism.	Table 5 Table 6
H12	Welfare	Access to community utilities and infrastructure, including those not directly supported by *Tuungane*, will be greater in *Tuungane* communities. [As evidenced by improved health and education indicators]	Table 35 Table 36 Table 37
H13	Welfare	Household income and asset holdings will be greater in *Tuungane* communities.	Table 30 Table 33 Table 34
H14	Welfare	Households will allocate a greater share of their time to productive activities in *Tuungane* communities.	Table 31 Table 32
H15	Welfare	Time devoted to productive activities not directly associated with *Tuungane* projects will increase.	Table 31 Table 32

Note: Primary hypotheses come from the 2007 design document. For further hypotheses see Outcomes and Data Sources (2011)[5].

[5] http://cu-csds.org/wp-content/uploads/2011/03/20110304-ODS-REGISTRATION.pdf

3.2 What did *Tuungane* do?

The strategy for achieving improvements in economic and social outcomes in this area was described by IRC as follows:

> [Programme] objectives were to be achieved through the establishment and strengthening of participatory local governance structures at two levels: village development committees (VDC) and community development committees (CDC). Public elections of the committees were followed by the identification, selection, and implementation of village and community level projects led by communities themselves (IRC, 2012).

The core strategy comprised the creation of project areas, the creation of development committees and the implementation of projects. We describe each of these elements in turn.

3.2.1 *Units of intervention*

The units of operation for the *Tuungane* project were village development committee (VDC) areas and community development committee (CDC) areas. *Tuungane* constructed these units for the purposes of the programme as follows. First, a set of approximately 5,500 lowest-level units (LLUs) – that is, natural settlements (sometimes villages, sometimes sub villages, sometimes *quartiers* (village neighbourhoods)) – were gathered together by IRC/CARE staff into VDCs, each with approximately 1,300 inhabitants based on proximity and affinity. Sets of VDCs were then aggregated into 560 CDCs, each with approximately 6,000 inhabitants.[6] CDCs were in turn aggregated into a set of lottery bin areas that contained between two and 30 CDCs, depending on logistic considerations. Approximately 50 per cent of the CDCs in each lottery bin were selected for treatment using a public lottery (see below). Thus, if a single CDC area was selected for treatment then all the VDC areas within that CDC area were selected as well.

For this research we sought to visit two LLUs in each CDC area, both of which would be surveyed, and in one of which behavioural measures would be taken.

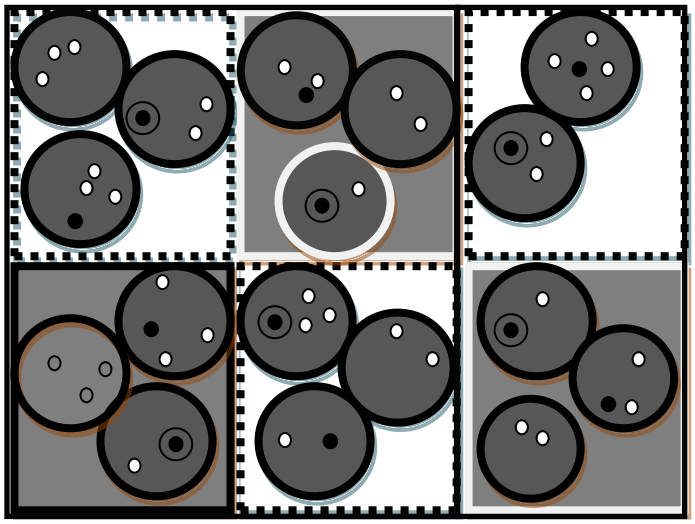
3.2.2 *Development committees*

The VDCs were formed through open and public elections of 10 representatives – two co-presidents, two co-treasurers, two co-secretaries and four ordinary members. By design, in about 75 per cent of areas, these committees were required to have one man and one woman elected to each position. In remaining areas this gender parity constraint was lifted (a feature we examine in Section 6). The CDCs were formed by VDC representatives, who selected two members from each of the VDCs.

Figure 1 provides an abstract illustration of the set of units and their relation to each other.

[6] More precisely, 600 CDC areas entered lotteries, 280 were selected for treatment and the remaining 320 were in control. For the purposes of this study we randomly selected 280 control CDCs at the lottery bin level for study in order to maximise balance in treatment and control units within each lottery bin.

Figure 1 Units and measures



Key:

	Lottery bin area: CDCs in a single lottery bin took part in a common public lottery for selection into the programme. There were 83 of these.
	Tuungane CDC: community development committee area. There were 280 of these.
	Control CDC: community development committee area. There were 280 of these.
	VDC: village development committee area. There were approximately 2,400 of these.
	LLUs: lowest level units. These were the smallest natural unit on which we had data. There were approximately 5,500 of these.
	LLUs that only received the survey but no behavioural measures. 560 of these were targeted.
	LLUs that received the survey *and* the (RAPID) behavioural measures. 560 of these were targeted.

3.2.3 *Social interventions and projects*

A series of sub interventions took place alongside the establishment and functioning of these groups.

The social interventions consisted, first, in the introduction of elections to create local committees who could select and oversee the implementation of development projects, as well as represent and liaise with populations. In each VDC area, committee members were tasked with sensitising populations on 'the importance of good leadership, and the meaningful inclusion of women and other vulnerable groups' (IRC 2012). VDC members also received two sets of trainings. First, a three-day training on their roles and

responsibilities, leadership and good governance, gender and vulnerability and the 'do no harm' principle; and second, a one-day training on financial management, and in particular on the necessity of documentation and the roles and responsibilities of the VDC members to ensure adequate financial management of the sub grant. Following consultations with the population, the VDC members then decided how to allocate an envelope of US$3,000 across projects, and then this decision was put to the population for an up-down vote.

VDCs also convened general assemblies (with an average of about four per VDC) to present expenditure reports to populations, with subsequent transfers being on condition of the approval by the population of the planned expenditure.

There were two types of economic interventions: those implemented at the VDC level (valued at US$3,000 per village), and those implemented at the CDC level (valued at between US$50,000 and US$70,000 per village).

Figure 2 shows the distribution of the most frequent VDC projects by Congolese administrative divisions, known as *chefferies* (chiefdoms) and demonstrates the large concentration of projects in the educational sector, such as schoolroom construction, in all areas, followed by transportation, such as constructing a small access road in South Kivu, water/sanitation, such as springs, latrines, wells and pumps in Maniema, and agriculture related projects, such as goat distributions or agricultural inputs in Haut Katanga.

Broadly, these projects were implemented to plan. IRC records only 26 instances of VDCs (2.1 per cent of the total) that were excluded from the project due to mismanagement of project funds, and 12 instances of CDCs (4 per cent of the total) where contracted enterprises, that had received advance payments, failed to carry out the work. Below we corroborate this general picture of implementation success from survey assessments.

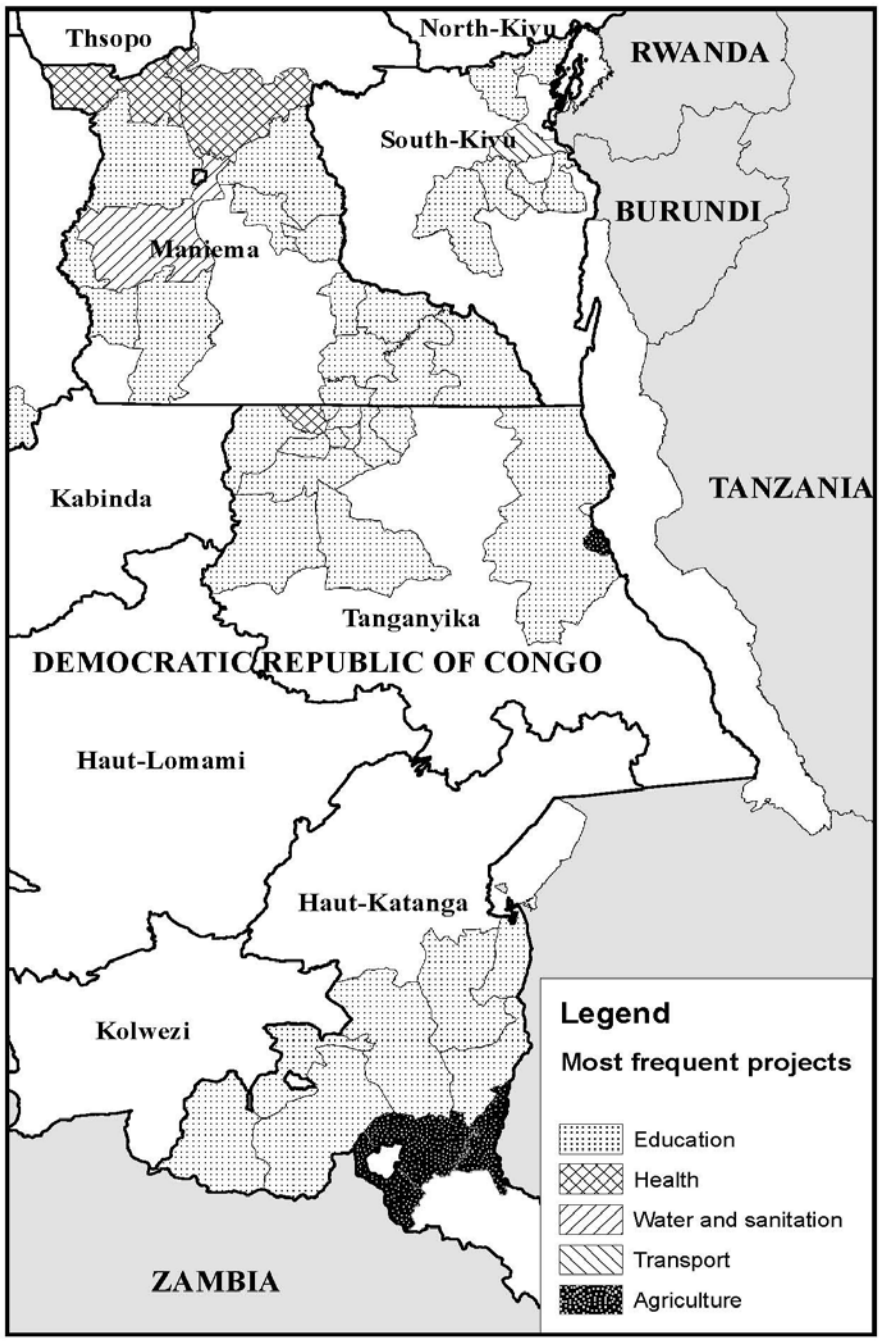
3.3 How large is *Tuungane*?

The *Tuungane* programme is one of the largest CDR programmes of its kind. The budget for the first phase (*Tuungane* I), examined here, was £29,685,253 (US$46,309,000). A second phase of the project (*Tuungane* II), now underway, has a value of £60,986,977 (US$95,139,684). *Tuungane* I worked for approximately four years in 1,250 war-affected villages with a beneficiary population of approximately 1,780,000 people (IRC 2012).

As illustrated in Figure 3, *Tuungane* covered large territories throughout eastern Democratic Republic of Congo (DRC), operating in four major regions: South Kivu Province, Maniema Province, and Haut Katanga and Tanganyika in Katanga province. The programmes were implemented, on average, in about four years, with the first village level (VDC) phase being implemented in about two years (see Figure 19 for an illustration of the timing of implementation across areas).

Although the aggregate numbers are very large by the standards of development projects in the DRC, the *per capita* investments are small. By IRC estimates, about 0.7 per cent of the population (12,510 of 1,780,000) was directly involved in VDC member trainings (and a fifth of these also took part in CDC trainings). Village-level projects numbering 1,811 were implemented at a value of US$3,707,624 over two years, which corresponds to approximately US$1 per person a year (3,707,624 divided by two times 1,780,000). A further US$14,354,403 was spent on larger community level (CDC)

Figure 2 VDC projects by territory and sector



Source: Referentiel Geographique Commun (administrative boundaries) and IRC (project choice).

projects. This larger investment (which was generally not completed at the time of measurement) corresponds to approximately US$4 per person a year over two years (14,354,403 divided by two times 1,780,000). The timing of these two phases across sites is illustrated in Figure 19. To put these numbers in perspective, the BRA-KDP programme in Aceh had investments targeted at around US$20 per capita a year and the Millennium Village initiative targets aid at US$120 per capita a year. Interventions in Western countries, such as the US stimulus plan, involve per capita investments that are orders of magnitude larger.

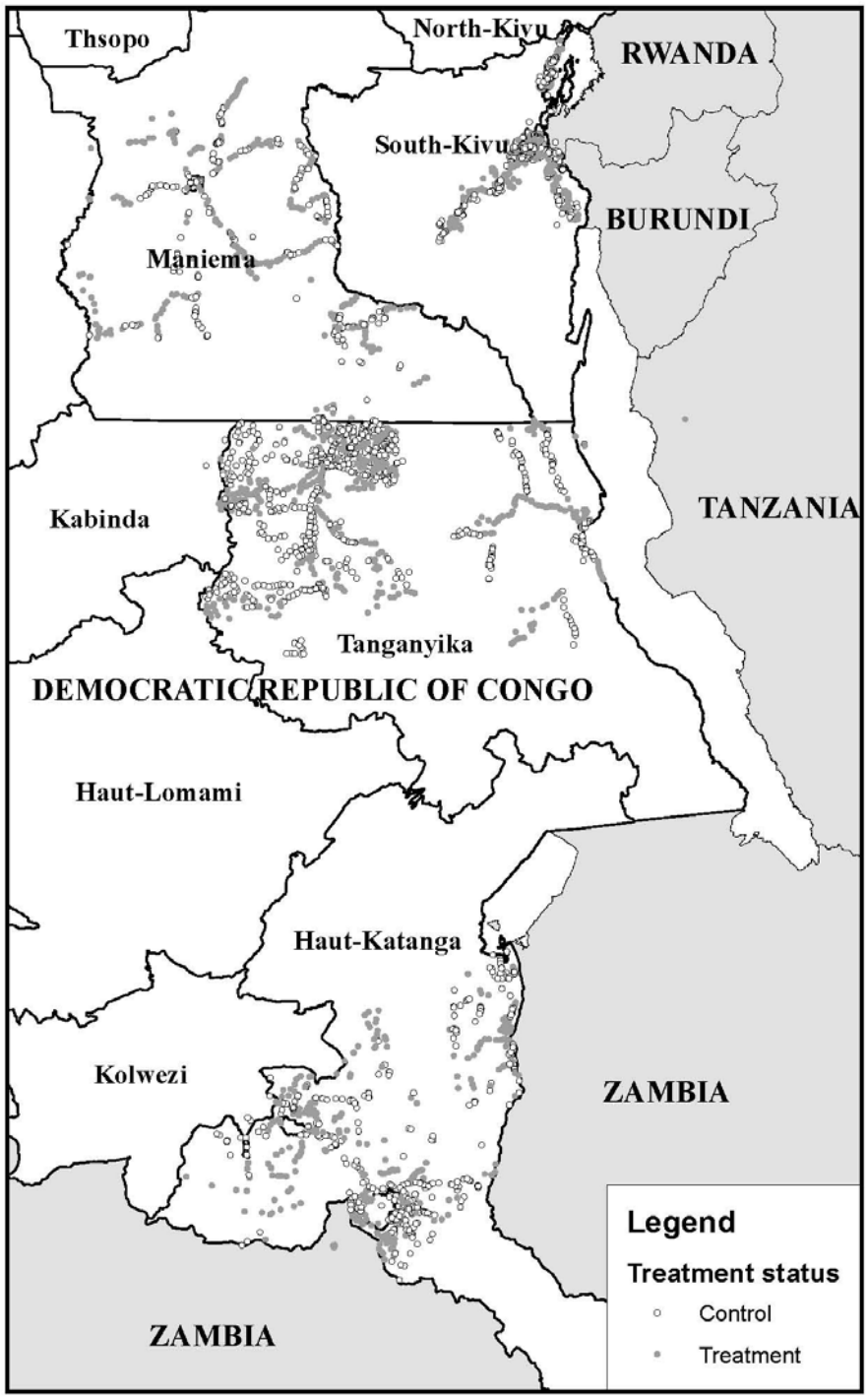
In the education sector, which was by far the largest sector, an estimated 420 schoolrooms were constructed and 1,348 were renovated, as part of the VDC projects. With an average of about 50 students per class,[7] these investments could improve the educational environments of perhaps 90,000 students a year. While this is an extraordinary accomplishment, the investment still provides direct benefits to less than 5 per cent of the population on the outside. In the health sector, approximately 160 clinics were built or rehabilitated that, if they serviced entire villages, could reach over 10 per cent of the population. With 5,000 mosquito nets distributed, there are direct gains to nearly 1 per cent of the population, assuming three people per net.

For all of these interventions there are possibilities of external effects, in terms of health, education and economic activity. For instance, because of transmission, improved health for some can have positive health effects for others in the communities and surrounding communities (see Miguel and Kremer 2004). Nevertheless, it bears emphasis that, by design, the direct interventions were small.

Finally, we note that although two to four years is a moderate to long period by the standards of many development interventions, it is, at least from the perspective of classic accounts of the development of social structures, still a short window to effect social change.

[7] Following the DRC government standards N MINEPSP/CABIN/0667/2007.

Figure 3 Geographic distribution of *Tuungane* projects



Source: Referentiel Geographique Commun (administrative boundaries) and Lottery data (treatment status).

Note: The figure shows the distribution of villages that form part of the *Tuungane* (treatment) group and the comparison (control) group. Villages were grouped together into smaller village level (VDCs) and larger community level collections (CDCs) and then a random collection of CDCs within each lottery bin area was selected to take part in the programme. This produced a clustering of *Tuungane* and comparison villages at the most local level and balance between *Tuungane* and control areas at the level of lottery bins.

4. Programme implementation

How was Tuungane implemented and perceived on the ground? How much awareness did it raise? How much did populations actually take part in Tuungane activities? How did they view the project?

To get an overall assessment we can turn to our survey data. In our survey (described in greater detail below[8]), we interviewed members of the broad population, chiefs and VDC members from 424 *Tuungane* villages. Our questions were designed to assess the degree of exposure of individuals and communities to *Tuungane* in the project areas as well as overall perceptions of the project. In addition, we have some information from control areas about knowledge and perceptions of *Tuungane*.

The box below shows 25 randomly sampled statements given by respondents when asked if they would like to make any general comments about how *Tuungane* worked in their village. This gives a flavour of the kinds of sentiments elicited. A very large share of responses simply express gratitude. Many take the form of requests for the continuation or the return of *Tuungane* to their villages. A few make complaints about the quality of projects or how committee members were not compensated. Overall, however, the responses are positive.

This positive pattern is reflected also in responses to more targeted questions. A general approval question asked respondents whether, overall, they felt the *Tuungane* project had been helpful, harmful, or neither helpful nor harmful to the village. The answers were overwhelmingly positive, with 81 per cent of the population reporting 'helpful' and only 2 per cent reporting 'harmful'. The reports from chiefs were essentially identical to those of the populations, while VDC committee members were more likely (91 per cent) to report the project 'helpful'. Men and women gave similar responses on this question.

Support for the specifics of the CDR model is more mixed. On the one hand, respondents reported liking the VDC and CDC structures and preferring decision making through these structures to decision making by the chiefs. Indeed, 41 per cent of the surveyed population reported that they would prefer community matters to be handled by the VDC rather than the chief (and just 11 per cent reported that they would prefer things handled by the chief, while the rest of the respondents were indifferent). Less surprisingly, 70 per cent of VDC members thought things would be better handled through the VDCs than through the chiefs.[9] Chiefs reported more support for chiefs compared to the other respondents, as might be expected, but even they largely supported the development committee approach. Thirty-six per cent of chiefs reported that these matters would be better handled by VDCs, while only 18 per cent said that chiefs should handle them. When asked if particular subgroups effectively controlled the process, the vast majority of respondents reported that the process was controlled by the community or by the committees, while only about 1 per cent reported that the chiefs controlled the process.

[8] Survey instruments are also available at http://cu-csds.org/wp-content/uploads/2011/03/20110304-ODS-REGISTRATION.pdf

[9] In this section we provide unweighted averages of survey responses. Responses are based on a target of five surveys in each surveyed village, villages were chosen proportionate to size, households were randomly sampled within villages and individuals were randomly sampled within households. The unweighted average responses can be interpreted as an estimate of the mean response of individuals in households in the population.

On the other hand, respondents also expressed strong support for an increased role for IRC and CARE in decision making, with majorities claiming that an increased role for outside organisations would have empowered populations (79 per cent), improved efficiency (84 per cent) and reduced corruption (72 per cent).

Complaints about the *Tuungane* process focused largely on the slowness of implementation (the main complaint for around 25 per cent of respondents). Fifteen per cent complained about limited information and 12 per cent about financial irregularities. A further 12 per cent felt that their views were not well represented, that the committees were too controlled by chiefs or that they did not otherwise have influence.

In their own words: a random sample of messages to *Tuungane* from survey respondents

- If possible *Tuungane* should do more in my village and also not forget other villages that did not have the chance to be selected.
- I thank *Tuungane* a lot for the work done for us. I hope *Tuungane* will do more than what has been done because so far the maternity clinic is not operational.
- We hope that *Tuungane* continues to help us and that it will come again.
- Let *Tuungane* help the marginalised to make them strong.
- The project helped us a lot. We hope that in the future it will bring us water.
- Take time to implement the project and reduce the number of steps.
- Medical aid, building of homes.
- *Tuungane* marginalised the chiefs entirely even though we are interested. It only listened to the members of the committee, which is a serious thing, and even the population does not know the first thing about *Tuungane*.
- Let *Tuungane* continue to help us.
- *Tuungane* did well.
- God bless *Tuungane* so that it will continue to help other villages.
- *Tuungane* helped us and we are very happy because we have leader transparency and cohesion and can go forward.
- The mill put in place by the project does not work any more, it broke and we cannot fix it for lack of money.
- I hope *Tuungane* continues to work in our village.
- For me the project does not do anything well. I think that the project itself should decide what we should do.
- I hope every village will have its project and that the committee members will get paid.
- We thank *Tuungane*. Soon the members of the committee should be paid to encourage them to work more on the projects.
- Huge thanks to the *Tuungane* project. I hope it will come again.
- I thank *Tuungane* for its work and its help.
- Construction of a market and gift of water mills and roads and bridges and pharmacies and roofing.
- We sincerely thank the *Tuungane* project for making our dream come true, which is the health centre. But we still need electricity.
- Bring another project and pay the members of the VDC.
- Speed up the process and send people who will put good governance into practice. Thank you.
- The CDC project was good but the one we have needed an engineer who let us down. Right now we don't know whether the money has been used or not or why the building is not yet finished.
- *Tuungane* has to monitor the work on the ground constantly to avoid corruption.

One concern about the *Tuungane* project that was noted by subjects related to relations between villages. When asked specifically whether the project generated jealousies with other villages (*de fortes jalousies*), about a third of respondents in *Tuungane* communities answered that it did. In non-*Tuungane* communities about 37 per cent of the general respondents, and 45 per cent of chiefs, who answered this question, reported inter-village jealousies, although it bears emphasis that only 15 per cent of the general population and 33 per cent of chiefs in non-*Tuungane* areas had heard of *Tuungane*.

Although we flag this concern regarding discord, overall the patterns suggest that the project was well received and that the approach to development was appreciated.

A series of more detailed questions allow us to make a more thorough assessment of the exposure of populations to the project and to have a sense of the extent to which the project succeeded in its goals of implementing participatory development.

The first question we address is the extent to which *Tuungane* is known among the population. As we can see from Item 1, *Tuungane* was known by name to almost two thirds of the population of the area. Rates were considerably higher among men (71 per cent) than among women (59 per cent). Interpretation of all subsequent questions should take account of this base knowledge rate since, if respondents do not know of *Tuungane,* it is assumed that they also do not know who implemented it, whether they took part in *Tuungane* elections and so on. Thus, the maximum values that any of these other measures of exposure can take is 65 per cent.

Item 1 Have populations heard of *Tuungane*?

	HK	MN	SK	TG	Total
Women	65%	13%	63%	44%	59%
	281	15	309	157	762
Men	71%	35%	82%	56%	71%
	299	20	319	184	822
All	68%	26%	72%	50%	65%
	580	35	628	341	1,584

Those who knew about *Tuungane* generally knew who implemented it, with 40 per cent of all respondents reporting that IRC implemented the project in IRC areas and CARE in CARE areas. Only 3 per cent guessed other groups.

Knowledge about the size of grants was somewhat weaker. Seventy-six per cent of VDC members reported the correct answer of US$3,000 for VDC projects, 48 per cent of chiefs (who had heard of *Tuungane*) guessed correctly and just 22 per cent of the general population guessed correctly. Knowledge of CDC envelopes was much weaker, with only 7 per cent of the population guessing in the US$30-US$90,000 range (the corresponding numbers for chiefs and VDC members are 23 per cent and 55 per cent).

We also asked respondents what they thought *Tuungane* was for. Was it primarily about bringing economic development through projects, or were the social components a core element? Respondents were asked to describe what they thought *Tuungane* was and then enumerators recorded whether the responses included references to projects, to

elections, to the idea of working together or to identifying community needs (all four, and other elements, could be contained in a single answer).

Item 2 shows how, across the board, the vast majority of those responding emphasised the projects. In most sites only a minority (25 per cent) mentioned elections or needs identification. Among VDC members (not provided in the table) there was much more of a focus on the elections, with about half of 515 VDC members answering this question referencing the elections in their answers. Chiefs were between these two groups, with a third of chiefs focusing on elections.

Item 2 What was *Tuungane*? Views from the field

District	Projects	Elections	Working together	Identifying needs
HK	69%	21%	65%	29%
	406	405	406	406
MN	85%	67%	87%	54%
	13	15	15	13
SK	85%	27%	32%	26%
	446	429	428	421
TG	58%	23%	41%	10%
	176	174	176	174
Total	74%	25%	48%	25%
	1,041	1,023	1,025	1,014

What level of participation in *Tuungane* activities did populations report? Item 3 summarises respondent reports of having attended some meetings associated with *Tuungane* (where those that have not heard of *Tuungane* are recorded as not having attended meetings). The table reports that 30 per cent of the population (36 per cent for men and 23 per cent for women) reported attending at least some meetings. More than half the chiefs interviewed reported attending some meetings and 94 per cent of VDC committee members reported attendance (again, rates are lower among women VDC members than among male VDC members). The median respondent (men and women) among those that attended any meetings attended two meetings, with the top 5 per cent claiming to have attended more than 10.[10] The median chief (among attending chiefs) reported attending four meetings, while the top 5 per cent attended 20 or more. The median VDC member reported attending nine meetings, while the top 25 per cent reported attending 15 or more. Attendance rates were highest in Haut Katanga and South Kivu and lowest in Maniema.

[10] The median respondent for a particular question is defined as the respondent whose answer to the question has an equal number of respondents that respond below and respondents that respond higher than their answer. The response by the median respondent is used as an indication of a typical response in a way that it is not sensitive to extreme responses given by others. For instance, taking the average response will be inflated if a minority of respondents provide a very large answer, and hence will no longer be representative of the response of the typical respondent.

Item 3 Attendance rates (share attending at least one meeting)

	Population	Chiefs	VDC members
Women	23%	44%	91%
	780	9	207
Men	36%	56%	97%
	822	264	227
Total	30%	56%	94%
	1,602	273	434

Note: Voting rates were similar to attendance rates, with a higher representation among men and generally greater engagement in Haut Katanga and South Kivu (see Item 4).

Item 4 Voting rates

	HK	MN	SK	TG	Total
Women	29%	6%	18%	13%	21%
	289	16	320	171	796
Men	35%	10%	43%	24%	35%
	303	20	327	197	847
Total	32%	8%	30%	19%	28%
	592	36	647	368	1,643

Contribution rates were somewhat lower than voting and meeting participation rates. Item 5 shows that overall about 14 per cent of respondents report contributing to projects in some form. These (reported) rates were highest in South Kivu and lowest in Maniema, and were somewhat higher among men than among women. Also, though not reported in the table, about 21 per cent of chiefs and 57 per cent of VDC committee members reported making contributions.

Item 5 Community contribution rates

	HK	MN	SK	TG	Total
Women	7%	0%	15%	6%	10%
	289	16	320	171	796
Men	13%	10%	26%	12%	18%
	303	20	327	197	847
Total	10%	6%	21%	9%	14%
	592	36	647	368	1,643

A key element of the CDR approach is that projects are selected by populations and not imposed by development organisations. To what extent do selected projects map onto the claims of populations regarding their preferred projects? For this analysis we note an important caveat that preferences expressed *ex post* (that is, after the implementation of the project) may not reflect the preferences that were salient at the time the decision was made. For this reason, the following analysis should be thought of as more of a measure of satisfaction with choice, reflecting the extent to which, in hindsight, populations feel the right projects were chosen.

Item 6 Project preferences and project choices

	Population preferences	Chief preferences	VDC member preferences	All	Actual reported frequency (of at least one project in sector)
Credit	2.6%	1.7%	1.6%	2.2%	1.5%
Health centres	21.2%	20.3%	20.5%	20.9%	25.1%
Seed distribution	3.1%	3.3%	1.8%	2.7%	4.0%
Schools	38.2%	38.4%	37.9%	38.1%	59.9%
Churches/mosques	0.4%	0.0%	0.4%	0.3%	0.2%
Pastoral farming	4.4%	2.5%	4.7%	4.2%	5.8%
Irrigation	0.9%	1.2%	1.2%	1.0%	0.5%
Wells/taps	12.5%	13.6%	14.3%	13.1%	23.5%
Roads	4.7%	5.8%	6.1%	5.2%	9.2%
Meeting halls	0.4%	1.7%	2.2%	1.1%	5.6%
Other	11.7%	11.6%	9.6%	11.1%	
Total	100.0%	100.0%	100.0%	100.0%	
Preferences met?	53%	51%	55%	54%	
N	1,004	242	512	1,758	

Item 6 reports preferences as well as beliefs regarding what projects were in fact implemented (thus, the final data is not based on project files but on respondent data; however, we can see that it broadly matches project reports). Broadly, there is a strong correlation between preferences for sectors and project selection with sectors, although we also see that some key sectors such as education, and water and sanitation (watsan) – for example, wells and taps – represent a much larger share in practice than they do in preferences.

How good is the mapping from group preferences to project choice? Before addressing this question we note four reasons why we might not expect a perfect mapping. First, feasibility constraints limit the ability to map group preferences to project choice. In particular, according to IRC, schools were often more feasible than other projects. Second, there can be substantial disagreement within villages regarding the optimal project. Third, multiple projects were often implemented in a given area and so top preferences may only partly capture the relevant information. Fourth, our data reflects the preferences of a sample of villagers only and so we expect some mismatch due simply to sampling error.

Item 7 Majority preference and project selection

	HK	SK	TG	Total
Pastoral farming		33%	0%	25%
		3	1	4
Health	60%	44%	40%	52%
	25	9	10	44
Irrigation		0%		0%
		2		2
Meeting	0%			0%
	1			1
Roads		80%		80%
		5		5
School	80%	93%	91%	87%
	45	30	23	98
Seeds	50%			50%
	2			2
Wells	33%	44%	50%	43%
	6	9	8	23
Total	68%	71%	69%	69%
	79	58	42	179

As an indication of the mapping, Item 7 reports the share of villages that expressed a majority preference for a given sector that had a project in that sector, broken down by district (Maniema is excluded due to a small number of observations for which there was internal agreement). We see again that those areas that agreed on educational outcomes generally received education projects and that these constitute a large share of all areas. Across the board there was nearly 70 per cent matching of preferences to projects.

Item 8 Use of the *Tuungane* projects

	General population	Chief	VDC member
HK	36%	57%	73%
	611	105	171
MN	18%	42%	82%
	68	12	17
SK	39%	58%	60%
	660	127	240
TG	27%	45%	71%
	374	73	108
Total	35%	54%	67%
	1,713	317	536

The last question we address is whether populations report making use of the *Tuungane* projects. Item 8 reports the share of households that say that at least one person in their household makes use of the project. Interestingly, VDC members and chiefs report

the highest usage rates, with VDC members reporting usage nearly twice that of general members of the population.

About 35 per cent of households report that some household members make use of the project. This suggests a relatively broad reach, especially given the relatively small budgets (in *per capita* terms) available through the project.

5. Strategy

After first discussing the calculation and presentation of the results, this section presents the *Tuungane* impacts on five dimensions of governance, and then examines impacts on social cohesion and on welfare. In addition this section provides the results of a variation in treatment and of a set of robustness tests.

5.1 Measurement strategy: description of RAPID programme and measures

Given the importance and scale of the current research, we sought strong outcome measures. In particular, in addition to survey measures, we gathered measures to record *behavioural* change in terms of outcomes of direct interest to policy formulation.

To assess behavioural change we introduced an entirely new intervention called RAPID (*Recherche-Action sur les Projets d'Impact pour le Développement*). As part of the RAPID process, 560 villages (half of which had participated in *Tuungane* and half of which had not) were selected to participate in an unconditional cash transfer programme in which they would receive grants of US$1,000 to be used on projects to benefit the village. In practice, the project funding announced at village entry was US$900, while in another visit US$1,000 was transferred to the selected group of representatives. This design feature allowed us to generate a measure of whether leaders share information on project size with the population.

Communities were asked to identify projects subject to minimal constraints. The key constraints were that some uses were ruled out if these were likely to result in harm (such as the purchase of arms) and the funds had to be spent out within a two month period, a somewhat artificial constraint that stemmed from our need to be able to assess the use of funds in a timely manner. There was general encouragement towards distributive projects for measurement purposes, but these were not required. There was no guidance of any form given as to who should manage the funds and how decisions should be made.

Item 18 shows the script used by the RAPID team when visiting villages. The RAPID project was then rolled out in four stages over the course of two to three months. The key features of these stages are described in Table 2.

Detailed measurement strategies were then employed to assess how funds were used in *Tuungane* treatment relative to control areas. The process provided key moments to gather information on *participation*, such as who turned up for meetings, who voiced preferences, who got to manage the funds (see, for example, Figure 6, Table 4, Table 8), accountability, such as measures put in place to hold project committees to account (Table 13), *efficiency*, such as the quality of accounting (Table 16), *transparency*, such as whether citizens knew the true grant size as given to committee members (see Table 19), and *capture*, such as who received what benefits from the grants (see, for example, Table 21 and Table 22).

Table 2 The RAPID process

Step	Description	Duration	Lead	Features
A	Team A schedules village meeting	2 days	Project team	The project team has an initial visit with the chief to ask that he convene a public meeting at which a minimum share of the village population is required to attend.
	Village meeting and project description forms		Project team	The RAPID project is described to the village. Measures of the quality of participation are taken at these meetings. The village is asked to take steps towards determining how to use the project funding. The population is told that at least US$900 will be made available.
B	Collection of forms	Brief visit	Project team	Measures of the village's decisions regarding *how* to use funding and *who* is entrusted to manage it are collected.
C	Disbursement of funds by IRC	Brief visit	IRC/ CARE	Funds are disbursed. The amount provided to villages will be US$1,000, US$100 more than the minimum guaranteed. This difference provides a means of measuring the extent to which financial information is communicated in communities beyond what is stipulated by the project structures.
D	Auditing	2 days	Audit team	Auditing is undertaken to examine capture, efficiency, transparency, and steps towards accountability that are taken.
	Follow-up surveys		Survey team	Measures are included in the final survey and a supplementary survey to determine the transparency of the process, the quality of participation in village decision making and the efficiency and equity of outcomes.

Note: The four steps in the RAPID process including information on strategy, responsibilities and measures gathered.

5.2 Calculation and presentation of results

The results presented in this report provide estimates of the effects of the *Tuungane* programme across a range of measures. For most measures we describe the estimated level of the measure across control communities. This can be interpreted as the expected outcome *in the absence of the programme*. We then provide the estimated effect of *Tuungane*, which is given by the difference in average outcomes in *Tuungane* and control areas.[11]

[11] As per the analysis plan these are weighted by *inverse propensity weights* to take account of the fact that different CDCs had a different propensity of being assigned to treatment depending on their location and, in addition, where relevant sampling weights are added in order to recover estimates of the population average treatment effects.

Because of the random assignment to treatment, this comparison gives unbiased estimates of the causal effect of the programme on outcomes of interest. For all estimates we also provide estimated standard errors, which capture the degree of uncertainty about the estimates of treatment effects. The smaller the standard errors are relative to estimated treatment effects, the more confident we can be in our results.[12] Given the size of the standard errors relative to coefficients, we record the level at which a finding is significant. When we say that the results are significant at the 95 per cent level this means that there is only about a 5 per cent chance that we would observe such positive effects if, in truth, the programme had no effects or had negative effects. These cases are generally indicated with ** markers in the tables. Ninety per cent confidence is marked * and 99 per cent confidence is marked ***.[13]

Note that given the hypotheses of the programme, these core tests are conducted as one-sided tests. We are interested in testing whether there is sufficient evidence to reject the hypothesis that the programme did not have any positive effect. A null result is interpreted as an inability to reject the null hypothesis of no or negative effect, at conventional significant levels. When a result is described as insignificant this means that the estimated effect size is too small for us to be confident that it did not arise by chance.[14] In cases where there are large negative effects, we mark these with a ! in the tables.[15]

Note that in this analysis we do not take account of any co-variates. This provides the simplest and most transparent set of results, and unbiasedness is still assured thanks to the randomisation. This approach does not, however, provide the most *precise* results. Introducing further co-variates (controls) may improve precision and reduce the risk of false negatives and, as indicated by our analysis plan, we intend to introduce these in later analyses. In our robustness tests, we provide results from the simplest analysis that controls for lottery bin fixed affects (as well as using propensity weights). This analysis should, in general, have little impact on estimated effects but should result in effects being more precisely estimated.

For ease of interpretation we present the results in a set of tables with a common structure. Figure 4 provides a summary explanation of how to read the typical table. Individual tables may differ from this canonical table – for example, by showing effects broken down by subgroup. Note that for all tables the source of data is indicated so that interested readers can consult the instruments employed (all instruments are available at: http://cu-csds.org/2011/03/drc-design-instruments-and-mock-report/*).*

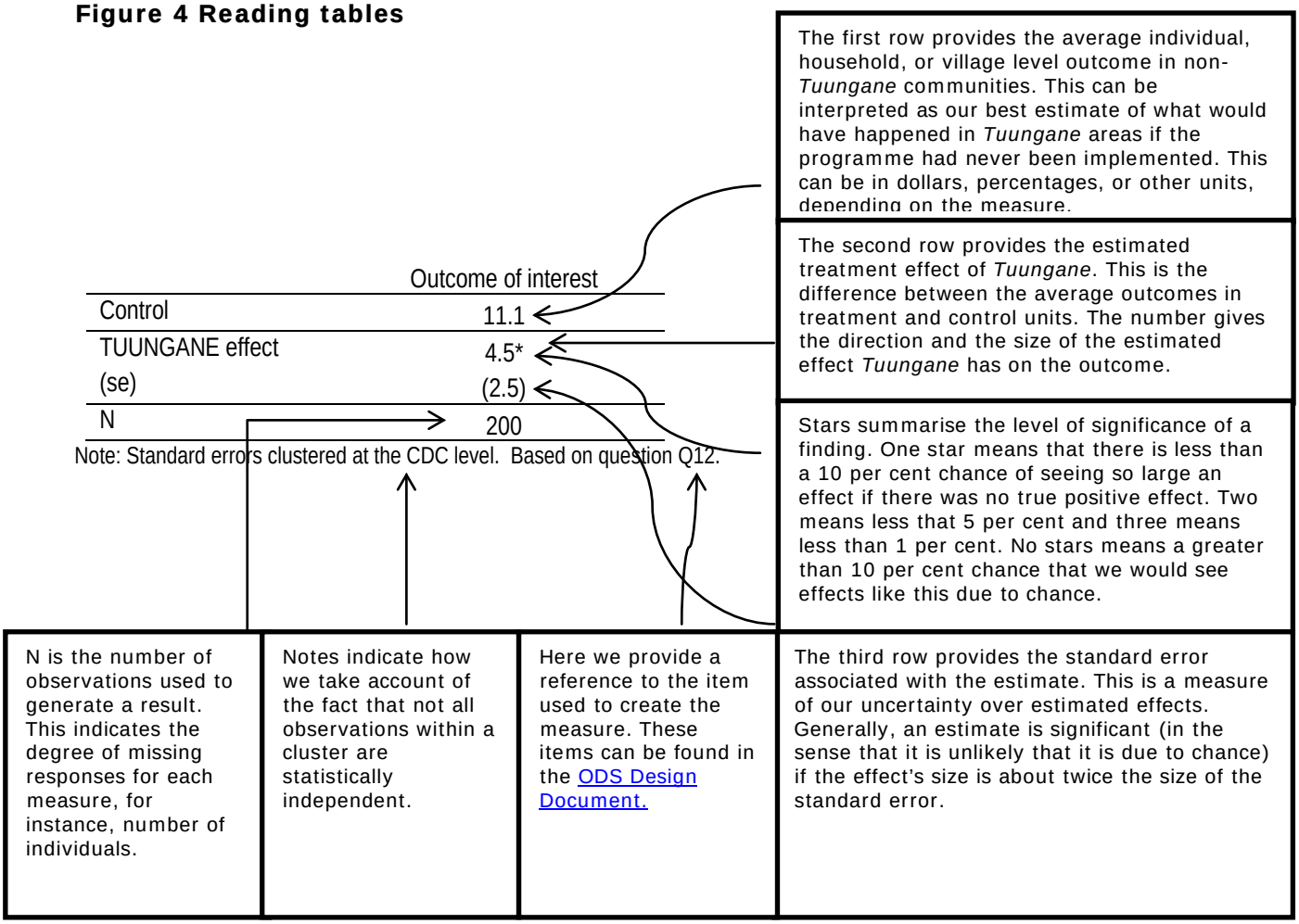
[12] The estimated treatment effects are regression estimates based on the weighted regression. We also indicate the level at which the errors are clustered, when applicable. The rationale for clustering is the following: say we are interested in a household level result – for example, household wealth. If households of the same village are similar on a wide variety of measures (they have a high 'intra-class correlation') then each household provides less unique information and this has to be taken into account in analysis. In our case, treatment was applied at the CDC level, a level higher than the individual or their village, and so we generally ensure that the standard errors are clustered at this level also.

[13] The 90 per cent, 95 per cent and 99 per cent levels are all conventional levels, although the 95 per cent level is the most common standard applied in the field.

[14] This does not however mean that the effect is substantively insignificant. A result may be substantively large but still be statistically insignificant. This is more likely to arise with smaller datasets, where the dispersion in the data is also large.

[15] We highlight negative results when these would be considered significant under a two-tailed test at the 95 per cent level.

Figure 4 Reading tables



5.3 Mean effects and average indices

For some analyses we have access to multiple related measures. For example, Table 9 has eight different types of public goods. In this case, distinct issues of interpretation may arise. For example, it may be that all measures trend positive but none is individually statistically significant. In such a case, it is possible that effects are jointly significant across the family of measures. Conversely, it may be that, by chance, one or other measure is significant in a family while most are not, or even trend in the wrong direction. In such cases, it is possible that there are no significant effects across the family of measures. In order to generate meaningful summary of multiple effects within each family, we follow the approach of Kling, Liebman and Katz (2007) and create standardised indices of outcomes on related items. This is done as follows. First we redefine each of the variables of interest in a family, so that higher values for each variable imply positive effects. Second, we rescale each of the redefined variables using the (weighted) mean and standard deviation of the control group units. The index is then the standardised average of the redefined rescaled variables.

For these measures the outcome in the control group is 0 by definition, and effects of the CDR programme are measured as units of a standard deviation of control areas. Loosely, that means that if an effect of 1 is observed then the average difference between treatment communities and control communities is as big as the average difference between any two units in the control group. There are many factors that generate the standard deviation of outcomes between communities in the control group.

If the treatment is able to increase outcomes of treated areas on average by the standard deviation of control groups, then the treatment alone plays a very large role in affecting the outcomes of communities that would otherwise not have been treated. On this scale a treatment effect of 0.2 or 0.4 would be a large effect.[16]

6. Main results

6.1 Results: governance

We examine five dimensions of governance: participation, accountability, efficiency, transparency and capture.

6.1.1 *Participation*

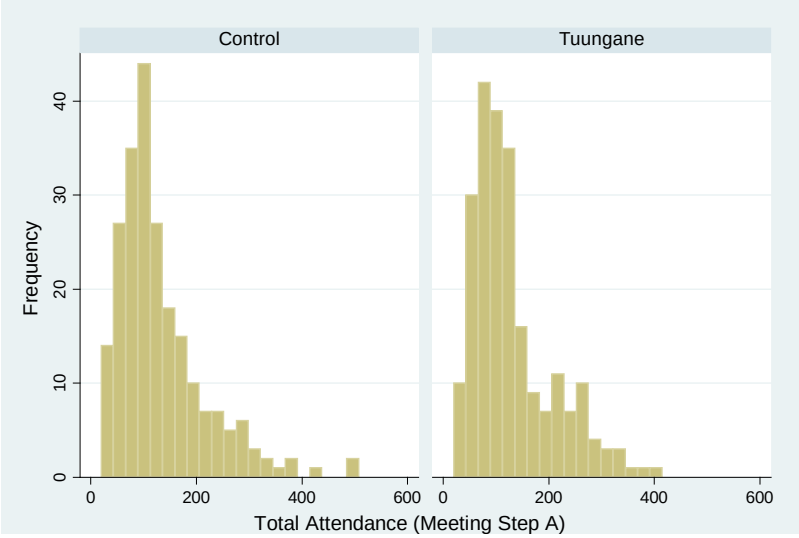
We define participation as the extent to which villagers are willing and able to be part of public decision making. The question of interest is whether *Tuungane* increased participation. The behavioural data collection is designed to provide multiple natural points to measure the quality of participation in public decision making, both in terms of who takes part and how they take part. We present here estimates of participation effects with a focus on behavioural measures of participation: RAPID meeting turnout; participation from dynamics of the discussion at RAPID meetings; community participation in the process of project and committee selection; participation into the RAPID committees; by gender; participation to the provision of public goods and perceptions about citizens' duty to participate. These measures capture the extent to which villagers participated in the process of RAPID, and the last measure captures the extent to which villagers feel they have a duty to participate.

RAPID meeting turnout

One of the first measures of participation collected during the behavioural exercise is the number of people that attend the initial meeting to learn about the RAPID project. For the project we asked for an attendance rate of 25 per cent or more. Given the opportunity costs of participating in a meeting of this form (no compensation was provided), we interpret attendance to indicate interest in civic participation (either on the part of the villager or on the part of the chief or other mobilisers).

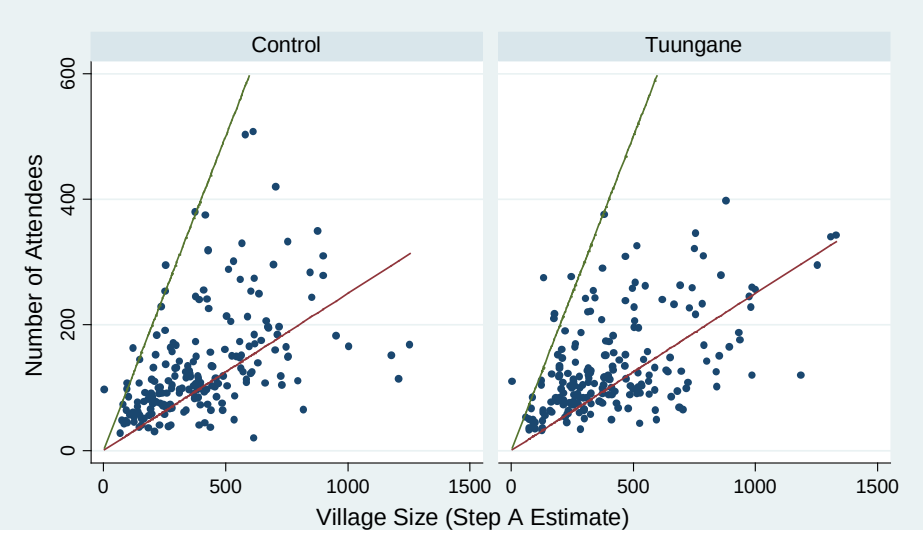
[16] To gain some intuition, say that in control 50 per cent of areas measured positive (scored 1) on some governance index, while 50 per cent scored 0, then the standard error in the control group would also be .5. If we estimated a treatment effect of, say, .4 on the standardised variable, this would correspond to average outcomes in the treatment areas of 70 per cent.

Figure 5 Distribution of number of meeting attendees



Note: Histogram shows the number of people attending Step A meetings in RAPID areas. Based on measures AM16 and AM17.

Figure 6 Meeting attendees as a function of village size



Note: Scatter plot of attendance against population size. The upper line shows the estimated village size, the lower line marks the 25 per cent threshold for attendance. Points above the lower line correspond to villages that exceeded the 25 per cent threshold. Points below are places that fell short. Based on measures: AM16 and AM17.

Figure 5 provides a histogram of the overall attendance levels. On average, approximately 130 adults participated in these first meetings (from villages with an average of 438 adult members), with attendance rising in population size but with proportions of village population decreasing in population size (see Figure 6). In general, attendance rates were higher among men than among women (approximately 55 per cent of attendees were male).

We note that, ultimately, some areas failed to meet attendance requirements, given our final data on population. Nevertheless, data collection proceeded in all areas. This

strategy avoids missing data on other measures. For some measures, however, interpretation should take account of this variation in initial attendance. For example, variation in knowledge of project values can reflect both variation in information flow but also variation in attendance at our initial meetings.

Table 3 Attendance

	Women	Men	All
Control	59.05	71.43	130.48
Tuungane effect	–2.49	0.5	–1.98
(se)	(3.67)	(4.28)	(7.40)
N (Number of villages)	455	455	455

Note: Based on questions AM16 and AM17.

Table 3 provides the effect of participation in *Tuungane* on attendance. From these results we see, on average, 2.5 fewer women showed up at village meetings in *Tuungane* communities as compared to non-*Tuungane* communities, and typically 0.5 more men, contributing to small negative (though not statistically significant) relation between *Tuungane* and village meeting attendance overall.

Discussion dynamics

A straightforward way to assess measure of participation is the extent to which individuals take part in public deliberations. To capture this feature we directly observed community discussion during the initial RAPID meeting to assess how many and which citizens were active in the conversation. The first meeting provided the opportunity for communities to learn more about the RAPID project and discuss what they would like to do with RAPID funding. Although the presence of the research team made this an inherently atypical village gathering, the meeting nevertheless provided an occasion for would-be participants to engage early and substantively in the RAPID process.

Table 4 Interventions

	Number of interventions	Number of male interventions	Number of female interventions	Proportion of interventions that are male	Proportion of interventions by elders	Proportion of interventions by the chief	Mean effects
Control	14.69	10.71	3.98	70.77	54.61	3.07	0
Tuungane effect	–0.44	–0.27	–0.17	0.21	1.42	0.5	–0.12
(se)	(0.51)	(0.41)	(0.25)	(1.46)	(2.08)	(0.54)	(0.09)
N	457	457	457	457	457	457	457

Note: Based on measure AD1.

As can be seen in Table 4, discussion interventions were dominated by men and by elders. Men accounted for 71 per cent of interventions (but 55 per cent of the

participants) and elders accounted for 55 per cent of interventions.[17] Chiefs intervened more than typical participants on average, but still accounted for only 3 per cent of interventions.

The effect of *Tuungane* on these outcomes tends to be negative. There are fewer interventions by both men and women and an overall rise in dominance of men, chiefs and elders. These differences, however, are very small and not significant at conventional levels.

Are committees and projects selected by a lottery or an election?
Examination of behaviour in the RAPID project allows us to assess the extent to which participation in *Tuungane* leads to greater adoption of participatory processes in the planning of public projects. Communities were required to select both a committee structure and a project as part of the terms of receiving RAPID funds, although there was no stipulation regarding how either of these was to be chosen.

We gathered information from multiple sources (citizens, committee members, RAPID project staff) on how the committees were formed. Below we report the summary judgement of our enumeration team after leading two simultaneous focus groups, one with members of the committee and a second with ordinary villagers during Step B of the RAPID process. This determination classifies the process as being electoral, through lottery, by consensus, imposed by the chief or elders, other or unknown. Our interest is in the use of elections and other participatory processes.

Overall, approximately 43 per cent of committees and 31 per cent of projects were coded as selected though election. Areas that selected committees using electoral approaches also selected projects in this way around 64 per cent of the time. Groups that did not select committees democratically, generally, also did not use elections when selecting projects.

Table 5 Selection mechanisms

		Project selected by election?		
		No	Yes	Total
Committee selected by election?	No	237	16	253 (57%)
	Yes	72	126	198 (43%)
	Total	309 (69%)	142 (31%)	451 (100%)

Note: Based on measures B32 and B33.

[17] An intervention is a distinct statement, question or argument made by an individual during a meeting. The length of interventions may vary considerably.

Table 6 Influence of *Tuungane* on selection mechanisms

	Committee		Project		Mean effects
	Selected by elections	Selected by elections or lottery or consensus	Selected by elections	Selected by elections or lottery or consensus	(All)
Control	40.81	71.35	30.52	73.23	0
Tuungane effect	4.5	4.7	–0.7	1.9	0.07
(se)	(4.75)	(4.24)	(4.38)	(4.21)	(0.09)
N	451	451	451	451	451

Note: Table 6 shows the prevalence of different types of selection procedures as well the effects of *Tuungane* on the choice of selection mechanism. Based on data from B32 and B33.

From Table 6 we see that less than half the areas report using election processes of some form to select committees (and 72 per cent when we also take into account selection by lottery or consensus). These rates are marginally higher in *Tuungane* areas than in treatment areas, though the effect is relatively small. In other words, elections were almost as common in areas without *Tuungane* as in areas with it. From the next two columns we can see how *Tuungane* areas in our sample are less likely to report using elections to select projects than are control areas, although this difference is not significant at conventional levels. Overall (final column) we see no trends in selection mechanisms associated with participation in *Tuungane*.

Who decides?

There was no constraint placed on the composition of the RAPID project committee other than the size (at least two members and no more than eight). In particular, communities were given no direction to select women, marginalised groups etcetera. An examination of the composition of RAPID committees thus provides an opportunity to assess whether the emphasis on gender inclusion in *Tuungane* altered behaviour at the village level.

Table 7 Male dominance in committees

		Number of women									
		0	1	2	3	4	5	6	7	8	Total
Number of men	0			0	0	0	0	0	0	0	0
	1		0	0	0	0	0	0	0		0
	2	0	0	1	0	0	0	1			2
	3	2	13	0	3	1	0	0			19
	4	38	7	7	7	24		0			83
	5	7	8	8	50	0		0			73
	6	12	13	96	0	0		0			121
	7	9	74	1	0	0		0			84
	8	70	0	0	0	0		0			70
	Total	138	115	113	60	25	0	1	0	0	452

Note: Table 7 shows the number of committees with different numbers of male and female members. Shaded grey areas are inadmissible committee sizes. Based on measure B13.

Table 7 shows the gender composition of RAPID committees. We see a strong tendency towards male domination of committees (higher numbers below the blue diagonal line than above). Of 452 committees, 28 had gender parity (the number of villages on the blue diagonal line), two had more women than men, and the rest had more men than women. Some 138 had only male members.

Table 8 *Tuungane* effect on committee composition

	Number of women	Number of men	Total size	Share women	Mean effects
Control	1.26	5.93	7.18	0.16	0
Tuungane effect	0.20**	–0.21	–0.01	0.03**	0.08
(se)	(0.12)	(0.14)	(0.14)	(0.02)	(0.10)
N	452	452	452	452	452

Note: Based on measure B13

From Table 8 we see that *Tuungane* is associated with a decrease in the number of men and an increase in the number of women included in the committee. This results in an overall increase in the share of women in the committees. Taken on its own, the share of women members increases by three percentage points and is significant at the 95 per cent level. Our overall index (last column) registers a positive but not significant effect. The mean effect is penalised, as it were, for the decline in male members and the marginal decline in overall size; hence, even though the effects on the share of women show a positive effect of *Tuungane* in female representation in committees, the mean effect on overall participation in committees is not significantly positive. Although statistically there is positive evidence here, it bears emphasis that the 0.2 effect on the number of women is small, corresponding to an additional woman selected on a seven-member committee (on average) in one in every five treatment areas.

Who contributes? Participation in public good provision

To assess household contributions to community projects, we ask respondents to recall recent collective action efforts with regard to public goods projects such as school rehabilitations, road clearing, organising security patrols or enhancing agricultural productivity. In each case we ask households whether they have taken part by contributing time or labour to these initiatives. The results are given in Table 9. The first row describes the share reporting that these activities have taken place, the second reports the share saying that they have participated in such a project and rows three and four report the *Tuungane* treatment effect on participation.

Table 9 *Tuungane* effect on public goods provision

	School	Clinic	Repairing road	Widening road	Well	Patrol	Agricultural productivity	Church	Mean effects
Share saying project exists	12.4	4.6	14.9	10.2	5.9	2.3	2.0	3.3	0.0
Household contribution in control	5.28	1.79	10.64	6.49	3.28	1.64	1.17	2.11	0.0
Tuungane effect	0.66	–0.23	–0.17	2.21*	0.02	–0.29	0.05	–0.27	0.01
(se)	(1.06)	(0.50)	(1.61)	(1.45)	(0.74	(0.56)	(0.45)	(0.53)	(0.05)
N	3,816	3,816	3,816	3,816	3,816	3,816	3,816	3,816	3,816

Note: Based on measure Q48. Numbers are in percentages.

A typical village reports one or two of the project types existing. The most common of these are road repairs (14.9 per cent) and schools (12.4 per cent), and the least common are security patrols and projects directed to increase agricultural productivity. Conditional upon the respondent reporting that the project exists, reported participation rates are high. However, because of the absence of projects in many areas, the total contributions (Table 9 reports these numbers in percentages) are low.

Overall, the evidence is mixed, with weak positive evidence for increased contributions to road widening but no clear effects on other items. The overall estimated effect of *Tuungane* is close to zero.

Rights and obligations in regards to decision making

The results in Table 4 indicate the extent to which individuals take part in local decision making (at the village level). To assess whether they feel they have broader rights to play a role in public decision making, we asked respondents to tell us what they thought were the main obligations of political leaders at the level above treatment, the *chefferie/secteur* (administrative units that rank below province and district). Our interest is in assessing the extent to which respondents see local government as having obligations that reflect citizens' rights to participate beyond the local effects that *Tuungane* might have in the communities in which it operates.

Table 10 provides the main results, in percentages. There is a general response (particularly among male respondents) that leaders are primarily required to distribute benefits. For most items there is little difference between *Tuungane* and non-*Tuungane* areas. The *Tuungane* effect on the mean effects index on duties of political leaders and of citizens to participate is negative, but very small, and not statistically distinguishable from zero.

Table 10 Duties of political leaders

	Duties of leaders to support citizens' participation				Other leaders' duties			Mean effects
	Accept elections	React to citizen complaints	React to citizen suggestions	Awareness raising	Avoid corruption	Consult populations	Distribute benefits	All
Female:								
Control	24.78	48.61	41.99	27.40	29.25	30.98	53.88	0
Tuungane effect:	–0.14	2.33	–1.93	–4.91	0.28	–2.3	–3.28	–0.06
(SE)	(2.80)	(3.09)	(3.24)	(2.69)	(2.90)	(2.80)	(3.12)	(0.07)
N	1,692	1,706	1,705	1,700	1,709	1,698	1,722	1,744
Male:								
Control	27.08	56.82	48.44	28.22	32.35	33.88	60.07	0
Tuungane effect:	0.57	1.4	1.76	–0.01	1.03	–2.88	1.53	0.02
(SE)	(2.57)	(2.93)	(3.24)	(2.72)	(2.66)	(2.70)	(2.81)	(0.06)
N	1,757	1,772	1,769	1,763	1,779	1,756	1,780	1,811
All:								
Control	26.58	53.14	45.85	28.87	31.95	33.45	57.68	0
Tuungane effect:	0.33	1.79	–0.03	–2.15	0.39	–2.5	–0.91	–0.02
(SE)	(2.14)	(2.34)	(2.64)	(2.26)	(2.19)	(2.30)	(2.40)	(0.06)
N	3,664	3,694	3,689	3,678	3,704	3,670	3,718	3,773

Note: Clustered at the CDC level. Female and male observations do not add up to the total number of observations because of small numbers of observations missing gender information. Numbers are in percentages. Source: DML Q78

Asking respondents simply if they felt that, in general, they were free to express their opinion in the village derives a second measure of rights. Broadly, as is shown in Table 11, populations in villages report a high level of freedom (84 per cent). But *Tuungane* is associated with a smaller, but not significant, score on this measure.

Table 11 Free to participate

	Share saying they are free to participate in decision making (%)
Control	84.20
Tuungane effect	–0.14
(se)	(1.52)
N	3,459

Note:
Standard errors clustered at the CDC level.
Numbers are in percentages. Based on DML Q41BIS.

Do citizens feel an obligation to take part? We address this question on the values of citizens using survey data. We ask respondents in an open manner what they feel are the main responsibilities of citizens. We then code their responses into a set of seven categories. These are then classified as actions that are meant to influence government and actions that are meant to support government. Our interest is whether the effects of *Tuungane* are stronger in the first set of (influence) categories, and whether individuals in *Tuungane* programmes are more likely to respond with an influence response than with a support response. Table 12 points towards marginal declines in the reported sense of a duty to participate, but no effects seen here are significant at conventional levels.

Table 12 Duties of citizens to participate

	Citizen duties to influence the government				…to support the government			Mean effects
	Participate in elections	Complain when things are not going well	Make suggestions to the government	Take part in meetings	Obey	Pay taxes	Give material support to government projects	All
Female:								
Control	31.18	27.24	20.59	22.48	54.65	41.26	16.84	0
Tuungane effect:	0.01	1.09	–2.06	1.72	0.24	–0.55	–1.7	–0.03
(SE)	(2.71)	(2.55)	(2.47)	(2.55)	(2.63)	(2.82)	(2.38)	(0.06)
N	1,706	1,683	1,692	1,686	1,715	1,703	1,687	1,744
Male:								
Control	36.82	30.67	24.31	27.13	67.78	54.51	22.20	0
Tuungane effect:	0.85	0.62	–2.02	0.99	1.52	–2.45	–0.26	0.01
(SE)	(2.77)	(2.48)	(2.46)	(2.60)	(2.61)	(2.96)	(2.48)	(0.06)
N	1,788	1,755	1,760	1,749	1,778	1,767	1,756	1,810
Total:								
Control	35.02	30.07	24.00	26.18	62.21	48.55	21.19	0
Tuungane effect:	0.07	0.78	–2.28	1.1	0.4	–1.42	–1.21	–0.02
(SE)	(2.12)	(1.98)	(2.06)	(2.15)	(1.92)	(2.26)	(2.03)	(0.05)
N	3,711	3,652	3,667	3,649	3,708	3,685	3,659	3,772

Note: Standard errors clustered at the CDC level. Female and male observations do not add up to the total number of observations because of missing gender information on some observations. Based on measure Q77.

6.1.2 *Accountability*

We define accountability as the willingness and ability of community members to sanction leaders for poor performance and the willingness of leaders to respond to citizen requests. We gather measures from multiple sources during and following the implementation of project RAPID to determine whether communities put in place and/or make use of any mechanisms of accountability to oversee the RAPID process.

Presence of accountability mechanisms

We examine the presence of accountability mechanisms that the village puts in place to oversee the use of *Tuungane* funding as a measure of a culture of accountability in villages. At no point during the RAPID process do we encourage or suggest to communities that they ought to put such measures in place. To find out whether they did implement such mechanisms of their own volition, we gather measures from three separate sources: (1) from a focus group meeting with RAPID committee representatives in Step D (for these results an item is marked if any one member reports it); (2) from two private interviews with two RAPID committee members, designed to take place simultaneously to minimise social desirability biases arising from the presence of other committee members; and (3) from private interviews with 10 randomly selected villagers (those receiving the household survey).

Three different measures are created:

1. Whether an external accountability measure (such as a distinct committee) has been put into place
2. Whether the committee has been required to report its actions to the community as a whole
3. Whether no mechanism has been put in place or the committee has been tasked with overseeing itself

The assumption is that effective accountability requires some form of oversight. Committee members can collectively gain benefits from their position and an external accountability mechanism might prevent them from doing so. In that sense, having the committee overseeing itself is analogous to having no mechanism (note that it is of course possible that no external accountability mechanism is put in place precisely because communities trust committees to function well). Table 13 provides a summary of results in percentages. In most cases, villages reported no oversight mechanisms (or the committee has been tasked with overseeing itself). External accountability mechanisms are especially unlikely to be present, especially when relying on information provided by random villagers. Each data source generally corroborated the numbers reported by the other two, although there are large differences in the responses by random villagers and the RAPID committee members.

Table 13 Presence of accountability mechanisms

	Focus group with RAPID committee members			Interview with two RAPID committee members			Interview with random villagers			Mean effects
	External	Community	None	External	Community	None	External	Community	None	All
Control	14.42	17.55	78.79	11.72	20.29	68.72	13.15	12.62	31.50	0
Tuungane effect:	1.74	–0.66	–5.15	–0.66	1.4	–3.9	–1.67	0.07	0.41	0.01
(se)	(3.84)	(4.09)	(4.35)	(3.02)	(3.74)	(4.19)	(1.80)	(1.81)	(2.70)	(0.10)
N	363	360	387	367	370	369	3,402	3,396	3,406	402

Note: RAPID committee member interviews and the village survey estimates have been clustered at the CDC level. Numbers are in percentages. Based on measures QR15, DA19 and DR31. Numbers may not sum to 100% as respondents could give multiple answers, including possibilities not listed here.

Table 13 also assesses whether *Tuungane* is associated with a greater or weaker propensity to put accountability mechanisms into place. We see here mixed results. Taking randomly selected villagers as the source of information, we find that *Tuungane* has a negative impact on placing external accountability mechanisms in place, and a positive, but small, impact on the RAPID committee being required to report its actions to the community as a whole. Also, according to the information provided by randomly selected villagers, *Tuungane* communities are more likely to have no mechanism in place (or the committee has been tasked with overseeing itself). The impact, however, is small and not statistically significant. However, the latter effect is reversed and significant when taken from the committee members. With this source of information, *Tuungane* communities are 6 per cent less likely to have no accountability mechanism in place, an effect that is significant at conventional levels and relatively large in magnitude.

Complaints

We also examined the culture of complaints within the village. To measure each respondent's propensity to complain, we asked them to indicate whether or not they agreed with the 13 statements listed below. Aggregating this data at the village level, we created an index of the average propensity of villagers within a particular village to issue complaints regarding problems they indicated as relevant.

We expected individuals in *Tuungane* communities to display an increased willingness to hold traditional and political leaders accountable as measured by their propensity to issue complaints (conditional on having something to complain about). Table 14 shows the kinds of complaints made regarding the RAPID process, in percentages. The most common complaints were related to lack of transparency of the process, with 37 per cent of the respondents in control communities claiming to have had too little information about the RAPID. Other complaints also come up regularly, with scores between 11 per cent and 26 per cent. In all, these results suggested a relatively vibrant overall propensity to complain. Overall, however, levels of complaint are no higher in *Tuungane* areas compared to control.

Table 14 Complaints (privately expressed)

	Control	*Tuungane* effect	se	N
The process took too long	10.68	1.19	(1.82)	3,687
The organisation (RAPID) did not behave well in villages	10.85	1.78	(1.89)	3,674
The projects selected were not the most important ones	19.85	1.36	(2.25)	3,671
The selected projects did not benefit a wide enough group	21.94	0.27	(2.54)	3,669
I had no real influence over the selection process	26.50	0.69	(2.43)	3,671
Disagreements were not well managed	17.49	0.8	(2.22)	3,670
The process was too complex	13.13	–0.35	(1.74)	3,668
There was not enough information about the process	37.49	–2.16	(2.91)	3,675
There was corruption (misuse of funds) in the village	14.51	0.13	(2.23)	3,667
The distribution of funds was not just	15.01	0.63	(2.14)	3,664
The project created divisions in the community	15.82	–0.83	(2.28)	3,666
The RAPID committee was too influenced by the chief	25.73	–0.55	(2.42)	3,669
The RAPID committee did not represent our concerns	15.81	1.09	(2.13)	3,671
Average propensity	0	0.02	(0.07)	3,703

Note: Based on measure QR26.

To capture the extent to which these complaints reflect a propensity to complain, given that there is something to complain about, we examined the effect of *Tuungane* on the complaints registered, controlling for the quality of project implementation[18]. For villages with similar levels of mismanaged funds (the measure for the quality of project implementation), we wanted to know how much more willing *Tuungane* villagers were to voice their concerns than their non-*Tuungane* counterparts, capturing changes in villagers' level of comfort with voicing opinions in private.

Table 15 first suggests that, indeed, villagers were more likely to complain in those situations where according to our data there was something to complain about. When a larger share of the funds was missing, more complaining took place. The relationship between funds missing and complaints was, however, considerably stronger in *Tuungane* areas than in non-*Tuungane* areas, an effect that is significant at the 95 per cent level (one-sided test). The full model suggests that in cases with few funds missing, *Tuungane* communities complain less than control communities, but that they react more when problems arise.

[18] Since the quality of implementation is post-treatment, readers should exercise caution in interpreting the results presented in Table 15. The interpretation we provide here holds under the assumption that quality of implementation is not affected by treatment for any units.

Table 15 Influence of *Tuungane* on citizen complaints regarding RAPID

	Index of private complaints
Tuungane effect × share of funds missing	0.68**
(se)	(0.37)
Share of funds missing	0.26
(se)	(0.21)
N	3,590

Note: Based on measure QR26. Estimates derive from regression, which includes a constant and an intercept for *Tuungane* as well as the interaction term.

6.1.3 *Efficiency*

We define efficiency to be the extent to which implementation makes good use of resources available. We hypothesise that, in general, projects will be implemented more efficiently in *Tuungane* areas.

Quality of accounting

Our first set of measures of efficiency is the existence and quality of accounting for grant fund expenditures by the RAPID committee. The committee is given an accounting form during the transfer of project funds (Step C) on which the committee is expected to indicate the total amount made available for the project (out of US$1,000), and to keep track of expenditures made. The presence of this form at the end of the project (when the research team visits in Step D) is an indicator of efficient project implementation.

An additional measure is the amount of money for which the committee has accounted. We have two measures of this: one is the total amount accounted for according to the RAPID committee's own accounting; the second is the total amount as calculated by the research team from adding up all the individual components in the committee's accounting. Surprisingly, the committee's total came to less than the research team's total.

A final measure is the amount of money that has been justified (in other words, that can be reconciled with receipts). We measure this by the share of amounts justified over the amount of money made available for the project. We separate between receipts and credible receipts to obtain an additional measure of credible justification.

Table 16 reports the overall patterns as well as the estimated effects of participation in *Tuungane* on the quality of accounting. Overall, RAPID committees in 82 per cent of the villages had the accounting form. Eighty-three per cent of RAPID funds were accounted for by the committees, a number provided by the research team after stepwise summation of all items presented by the committees. In addition, 56 per cent of the money the committee made available for the RAPID project (of the US$1,000) had been justified by receipts. Forty-six per cent had been justified with receipts deemed credible by the auditing team.

Table 16 Existence and quality of accounting

	Proportion of villages with accounting form present	Share of funds formally accounted for as calculated by the research team	Share of funds formally accounted for as calculated by the RAPID committee	Share of money justified	Share of money credibly justified	Mean effects
Control	81.90	83.00	77.74	56.07	46.04	0
Tuungane effect:	2.37	–6.52**	–1.06	4.46	3.43	0.03
(se)	(3.85)	(3.26)	(3.92)	(4.45)	(4.42)	(0.11)
N	394	400	344	356	404	404

Note: Unless otherwise indicated, numbers are in percentages. Based on measures DA27, DA28, DA31, DA32, DA33 and DA34.

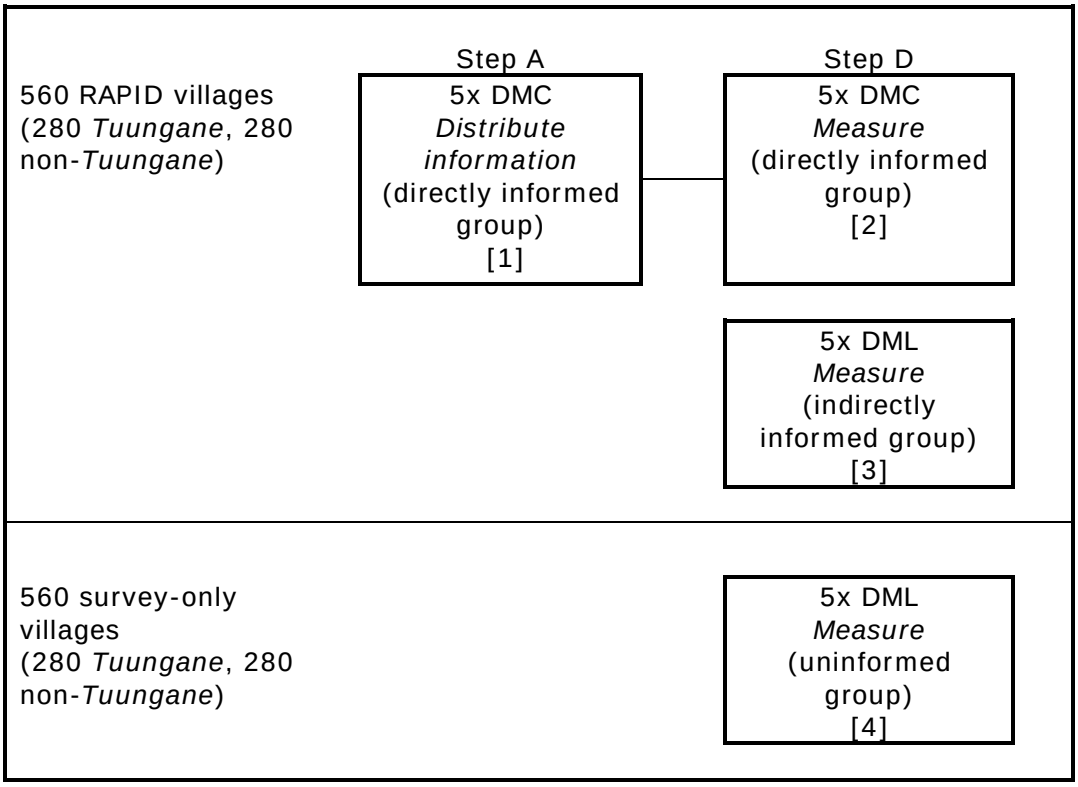
We find that RAPID committees in *Tuungane* areas are more likely to have the accounting form present and that more money is (credibly) justified. These results are reasonably large but given the variation on these outcomes the effect is not large enough to reach significance at conventional levels. We find mixed results regarding the share of funds formally accounted for.

Speed of information transmission

By examining the extent of effective transmission of information within villages, a second behavioural measure of the extent to which the community can function efficiently outside of the RAPID process is generated.

We sought to assess the ease of information transmission between villagers as follows. In half the communities a random sample of five villagers (DMC villagers) was provided with public health information on hygiene and diarrhoea during Step A. In Step D a new random sample of five villagers in all areas (DML villagers) are asked, in private, about each individual element provided to the DMC villagers in Step A, and receive a test score for the percentage of questions they answered correctly. Comparison of the scores derived in the interviews of Step D for the DML villagers from RAPID villages and DML villagers from non-RAPID villages (in other words, members of villages in which no villager was provided the information before the survey) allowed us to assess the rate of information flow. We obtained the *Tuungane* effect on information transmission by comparing the RAPID effect on test scores of DML villagers in *Tuungane* and non-*Tuungane* areas. Figure 7 illustrates this.

Figure 7 Health information



The main measure here is how successfully the public health message given during Step A [1] has spread through the village. We measure this using an index that records the average success rate of villagers' answers to health questions in Step D. Figure 8 shows the distribution of village scores on this measure.

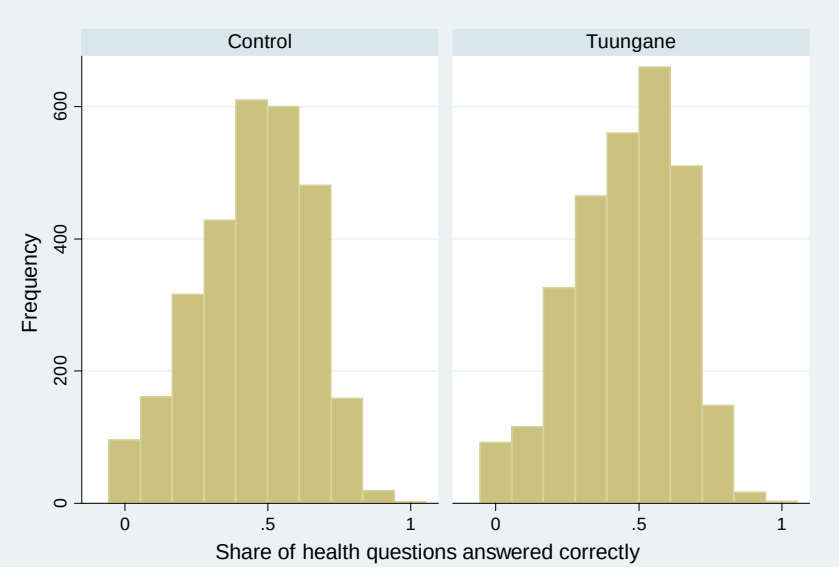
We expect the level of knowledge to be higher in RAPID villages [3 in Figure 8] than in survey-only villages [4] because, in the latter, no information was distributed. To be clear, we only looked at people who did not directly receive the information in Step A (we did not perform analysis on data collected for [2] except to confirm that learning indeed took place). Moreover, we expect *Tuungane* to have positive effects on social cohesion and, hence, on the ability of communities to address collective action problems. We thus expect the speed of transmission to be faster in *Tuungane* areas than in non-*Tuungane* areas. In other words, we expect to see a positive interaction between *Tuungane* and RAPID.

Table 17 illustrates the results and gives the effects of *Tuungane*, RAPID and their combined effect. As expected, being a RAPID (versus a non-RAPID) village has a powerful positive effect on the level of health knowledge because that is where the health information was distributed. Information is truly being transmitted. This fact speaks to the reliability of the measures used here.

The table suggests that there is more baseline information in *Tuungane* areas relative to control areas but there is no evidence that the speed of transmission is faster in *Tuungane* areas than in non-*Tuungane* areas. As can be seen from the bottom row,

which reports the interaction between *Tuungane* and RAPID, the estimated *Tuungane* effect is small in magnitude and is negative in sign. The interpretation of the table is as follows. The first row gives the average test score on the health items provided in Step A in RAPID villages, for the DML villagers that are in non-RAPID, non-*Tuungane* villages (in other words, where no health intervention took place). The corresponding test score in non-RAPID villages is higher by 1.7 per cent in *Tuungane* villages and the difference is significant at conventional levels. One can read the *Tuungane* effect in the second row. This does not speak to the impact of *Tuungane* on information transmission (since it is still in non-RAPID villages, where no information was systematically provided to anyone prior to the visit) but on the impact of *Tuungane* on knowledge.

Figure 8 Health knowledge



Note: Based on answers to QS1 – QS5.

Table 17 Level of knowledge of health facts

	Level of health knowledge
Control	38.64
Tuungane effect on knowledge	1.74*
(se)	(1.34)
RAPID effect	9.72***
(se)	(1.10)
Tuungane effect on knowledge transmission	–1.33
(se)	(1.58)
N	3855

Note: Standard errors clustered at the CDC level. Numbers are in percentages. Based on answers to QS1 – QS5.

The scores in RAPID villages (but still not *Tuungane* villages) are higher than non-RAPID (non-*Tuungane*) villages by around 10 per cent, and the result is strongly significant, suggesting that being in a village in which five DMC villagers received information increases the average information levels available to all. Finally, the last row provides the relevant numbers for the impact of *Tuungane* on the speed of information transmission. It indicates that the RAPID effect is smaller by 1.33 in *Tuungane* villages than in non-*Tuungane* villages, suggesting that *Tuungane* villages do marginally worse at information transmission (perhaps in part due to marginally higher baseline levels of information), but the difference is not statistically significant.

Seeking support from external actors

A third way that *Tuungane* communities might be more effective is the extent to which they are proactive in seeking external support. To address this question we asked whether, in the previous six months, communities contacted either the government or NGOs to lobby for interventions in their areas. Such lobbying behaviour reflects a number of features, most importantly their ability to organise (efficiency) and their sense of a right to demand action on their behalf.

The results are given in Table 18, in percentages of respondents who respond positively. The data shows only negligible amounts of lobbying overall. Lobbying NGOs, however, appears the dominant form of lobbying. The table broadly suggests an increase in lobbying NGOs (statistically significant) but a decrease in lobbying government for health services, and a decrease in lobbying government for other, and the average effect measures zero. This evidence runs contrary to the aspiration to connect communities more strongly to government.

Table 18 Seeking support from external actors

	Government for health	Government for education	Government for other	NGO	Mean effects
Control	5.24	4.63	3.60	5.16	0
Tuungane effect	–1.37	1.18	–1.41	2.15**	0.01
(se)	(0.88)	(1.01)	(0.73)	(1.21)	(0.05)
N	3,781	3,768	3,469	3,729	3,794

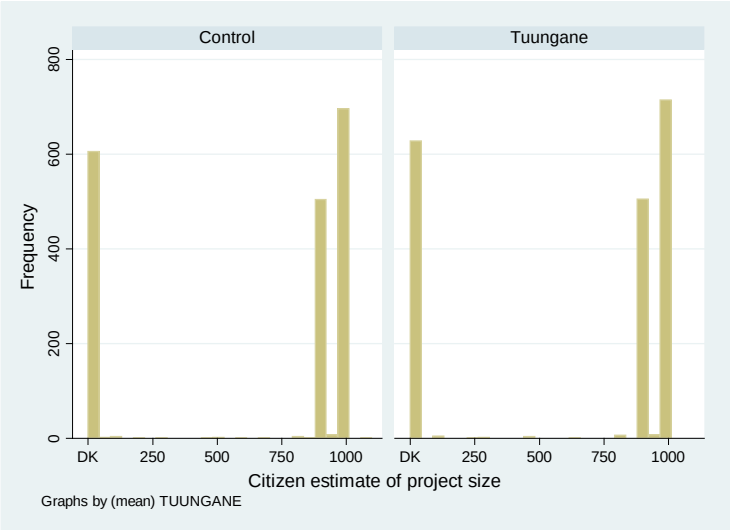
Note: Clustered at the CDC level. Numbers are in percentages. Based on measures Q65, Q66, Q67 and Q68.

6.1.4 *Transparency*

Knowledge about RAPID

To measure transparency, we examined the extent to which basic information (beyond what we make known to villages) on RAPID project finances was known in villages. As part of the RAPID process, the enumerators told communities in public that (at least) US$900 will be made available through the RAPID project. In fact, a total of US$1,000 is transferred to the project committee in every RAPID village, in private. Thus, one of our primary measures of transparency is whether villagers know about the full amount received and not simply the amount that was communicated to them by the RAPID enumerators in the initial village meeting.

Figure 9 Villager knowledge of project funds



Note: Based on measure QR2.

As seen in Figure 9, the most common guess (given by more than 38 per cent of all subjects and by 56 per cent of those subjects that gave an answer) reported the correct answer of US$1,000 as the total amount of project funds available to the community. This is striking since it implies that information about the incremental US$1,000 reaches villagers through their own representatives. This suggests relatively high levels of transparency. A slightly smaller share of the respondents said US$900 – reflecting the amount that was told to the population by the RAPID team. Most other respondents did not venture to guess the final amount at all (captured in the DK column of Figure 9).

To assess the effects of *Tuungane* we created a first measure of whether a villager provided a correct response and a second measure of how far the individual's guess was from US$1,000 (conditional on them guessing an amount). These measures were recorded at the individual level and treatment effects were estimated with clustering at the CDC level.

Table 19 shows how *Tuungane* communities score marginally better on both measures. They are more likely to guess US$1,000 (first column) and are more likely to be close to US$1,000 (second column), although neither result is significant at conventional levels.

Table 19 Knowledge of project amount

	Correct estimates? (%)	Distance from US$1,000?
Control	38.43	106.63
Tuungane effect:	1.33	–5.28
(se)	(3.19)	(15.85)
N	3,719	2,608

Note: Based on measure QR2.

Willingness to seek information

If taking part in the *Tuungane* intervention has made communities more transparent, then it is likely that valuable information about public resources has become more accessible. We measure this by the willingness of randomly selected villagers to obtain relevant information about the management of public resources for which they are beneficiaries. Villagers were presented with the opportunity to seek information about the revenues of the last period for either the main school attended by this village or the main health centre (the precise units are identified by our teams at each site). They were offered US$1 as compensation for attempting to retrieve the information and an additional dollar upon success. Our enumerators checked the veracity of the information, and condition the second payment on accuracy.

Our first interest is in the willingness of the villagers to participate in this exercise. However, if they were not willing, we recorded the reason for their refusal. If they were willing we recorded whether they returned with the information and whether the information was accurate. We have data in from 1,415 respondents on this measure. Of these, 39 per cent agreed to participate. The people that refused gave various reasons: that it is not appropriate to ask for this information (76); that the respondent did not have time (75); that the game is strange to them (50); that the husband of the respondent refuses or would refuse the collection of this information (13); and other reasons (192). Overall, this suggests broad challenges to accessing basic financial information.

Table 20 Willingness to seek information

	Willingness to participate (%)
Control	37.77
Tuungane effect:	3.71
(se)	(3.28)
N	1,409

Note: Based on measure QI3.

Table 20 indicates that there is no strong evidence for an estimated effect of *Tuungane* on willingness to participate. In other words, although villagers in *Tuungane* areas are marginally more willing to seek fiscal information from service providers than villagers in *Tuungane* control areas, the difference is small and not statistically significant.

6.1.5 *Capture of project outcomes/equity*

The final measure of governance we explored was the extent to which the outcome of collective decision making is subject to capture. Are project benefits equitably distributed or are benefits concentrated among elites or particular subgroups? While most of our measures of governance focus on processes, the capture measures focus directly on behavioural outcomes.

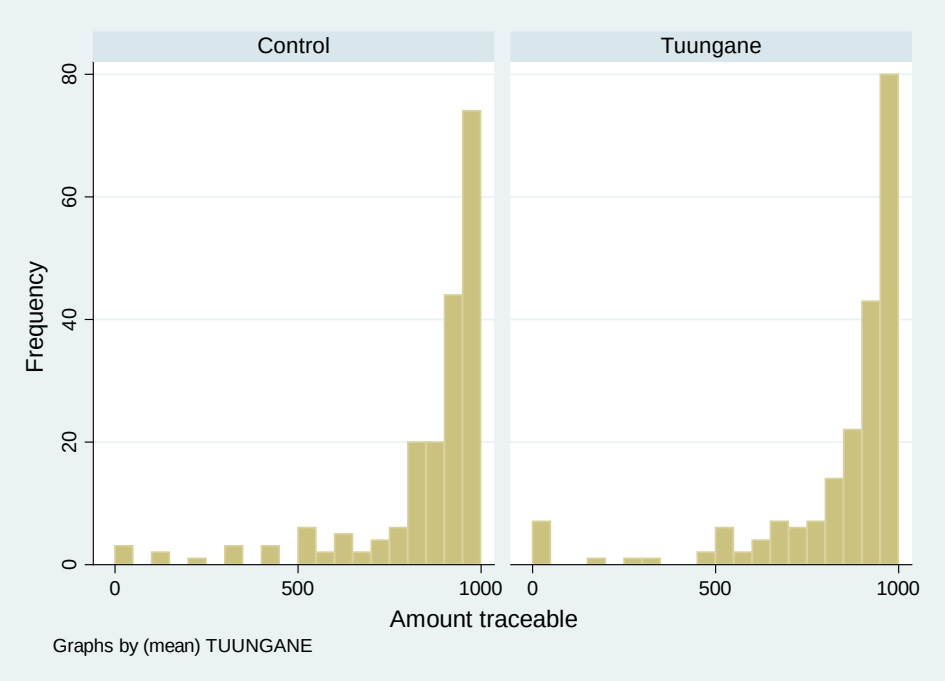
RAPID: financial irregularities (from audit)

Our most important measure of capture is the amount of the US$1,000 grant that our auditors were unable to account for during their two-day community audit. The auditors were trained to rule out as many strategies as possible that committees could use to divert funds. They operated using a checklist of 32 possible strategies that the committees could use, including exchange rate manipulations, quantity manipulations,

quality manipulations and quality over-reporting. Auditors were asked to verify prices in the market whenever possible and they used group discussions to assess the actual price to minimise the risk of over-reporting at any step. When auditors were constrained by time, they were trained to obtain information, only in the last resort, from a selected group of villagers, who were known by all to be the group visiting markets most often (women). In addition, they interviewed a random sample of the list of beneficiaries provided by the RAPID committee and evaluated how much was transferred to them, obtaining proofs when possible. This also provided us with an estimate of how many ghost beneficiaries were added to the list.

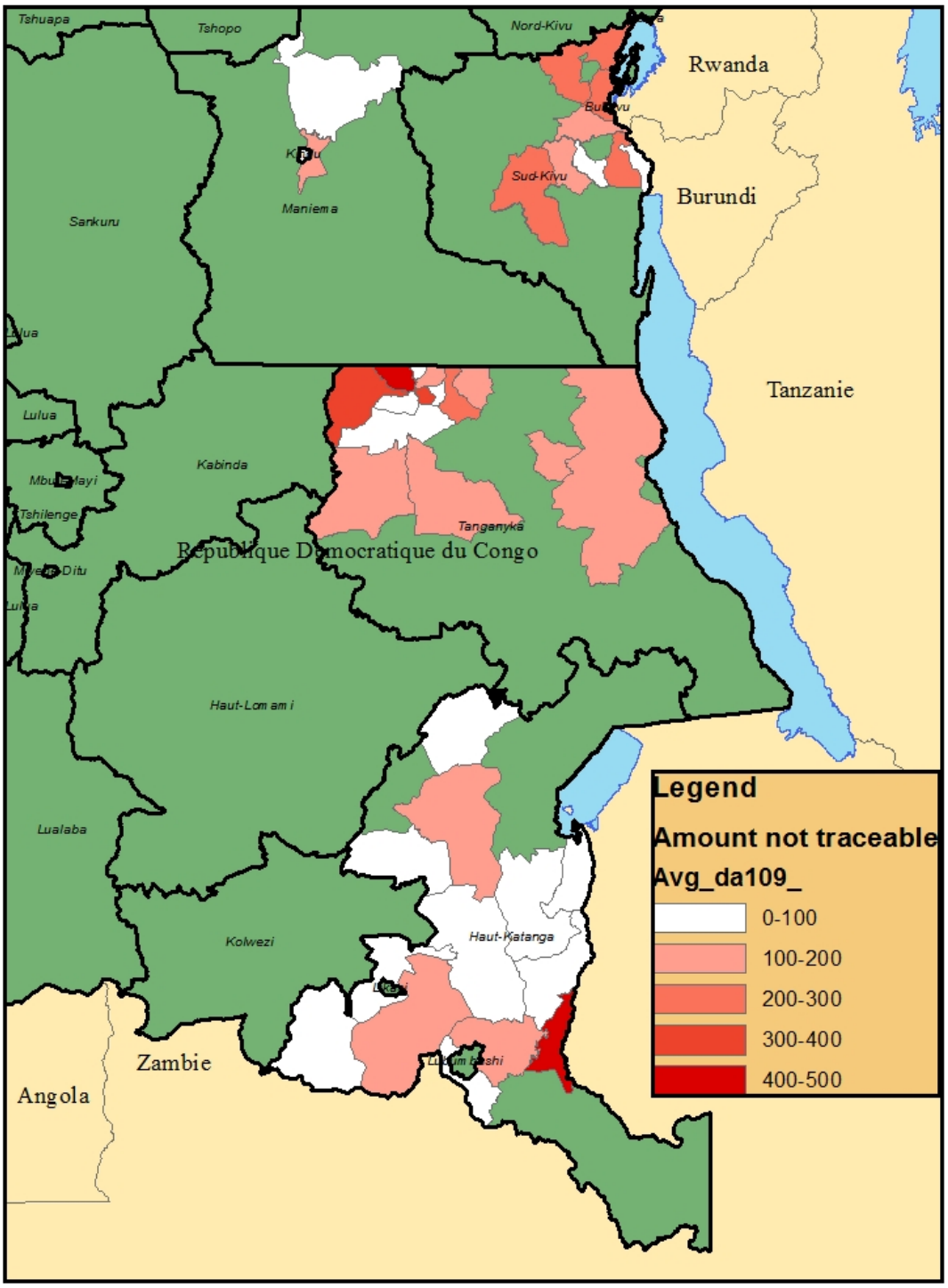
Figure 10 shows the distribution of the amount of RAPID funds that could be traced by the RAPID audit teams on the 398 villages for which reliable audit data exists. On average, US$850 of the US$1,000 could be verified by the teams.

Figure 10 Amount verifiable



Note: Based on measure DA109.

Figure 11 Amount not traceable, by *chefferie*



Source:Referentiel Geographique Commun (administrative boundaries)

Figure 11 shows the distribution of the RAPID funds that could not be traced by the audit teams, by *chefferie* (chiefdom). While the difference between districts could be due to the effectiveness of different audit teams from the RAPID project (which were allocated by districts), the map shows how Haut Katanga RAPID funds can be traced to a greater extent than in other areas, particularly South Kivu and Tanganyika. Note that audit

teams were allocated randomly to communities within districts to avoid biases, following the Step D randomisation.

Table 21 Traceability of money

	Amount traceable (US$)
Control	850.39
Tuungane effect:	1.58
(se)	(20.58)
N	398

Note: based on measure DA109.

As seen in Table 21, we found a positive but not significant relationship between *Tuungane* and traceability of funds. In other words, our auditors found it marginally easier to account for funds in *Tuungane* areas. The effect, however, is small and not significant.

RAPID: number of beneficiaries

A second measure of capture is the extent to which benefits are distributed broadly or narrowly in villages. Table 22 shows the average number of household beneficiaries per project in percentages. We restricted the analysis here to villages in which at least one respondent was a recipient of private transfer to eliminate villages with projects that did not involve cash transfers.

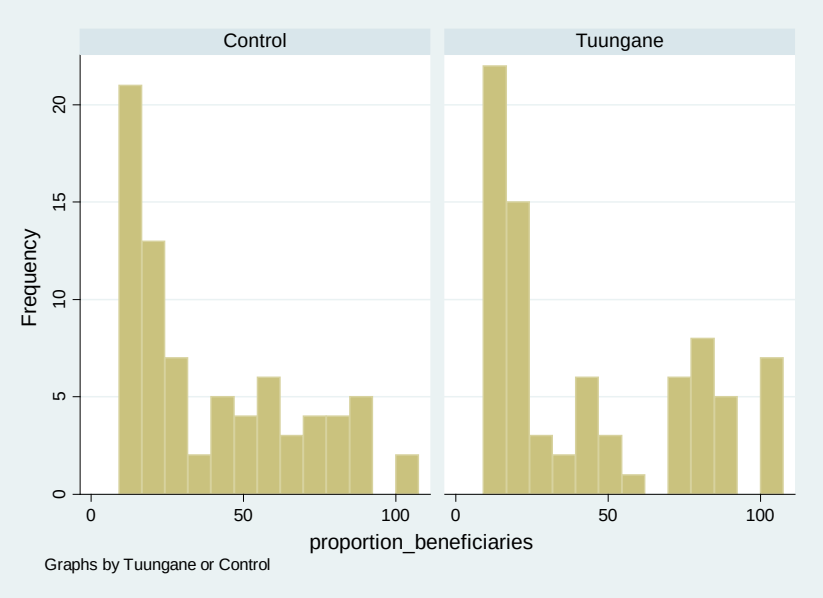
On average, 40 per cent of the households in the villages with projects of private distribution claimed to have received private transfers from the RAPID project. Among the villages with projects of private distribution, there were, on average, four percentage points more beneficiaries in *Tuungane* villages but the difference is not significant. The evidence from the RAPID committee is similarly weak (and points in the opposite direction).

Table 22 Proportion of respondents who received transfers from RAPID

	Villages with evidence of private transfers, from household surveys	Villages with evidence of private transfers, from RAPID committee
Control	39.94	16.65
Tuungane effect:	4.01	–2.04
(se)	(5.65)	(3.44)
N	154	303

Note: Numbers are in percentages. Based on measure QR3.

Figure 12 Proportion of beneficiaries



Note: Share of population that received benefits in treatment and control areas. Distributions are broadly similar although a larger share report 100% benefits in treatment areas.

RAPID: inequality of the distribution of benefits

What of the overall inequality of distributions, conditional on receipt of some benefits? Given the small sample size, we focused our attention on the dispersion of the benefits. This is best captured by a Gini coefficient, but for interpretation purposes we focused on a simple standard deviation. The standard deviation represents the average difference in the amount received between two randomly selected villagers. Hence, the larger the standard deviation, the larger the degree of disparity of the distribution of benefits. The standard deviation allows us to keep track of how large the difference in transfers between villagers is.

Table 23 Mean deviation of benefits distributed

	Villages with evidence of existing distribution, from household surveys (US$)	Villages with evidence of existing distribution, from RAPID committee (US$)
Control	8.72	3.26
Tuungane effect:	0.39	–0.23
(se)	(1.60)	(0.80)
N	128	301

Note: Amounts in US$. Based on measure QR3.

Table 23 provides the *Tuungane* effect on the mean distance from the mean transfer offered by RAPID, in dollars. As with the evidence on the number of beneficiaries, results here suggest similar outcomes in *Tuungane* and non-*Tuungane* areas.

RAPID: dominance of preferences of chiefs and men (power)

A fundamental measure of capture is the extent to which actual decisions reflect the

preferences of different sorts of villagers. We focused on the dominance of the preferences of the chief and the preferences of men over preferences of a random sample of other villagers. Hence, we produce a measure of chiefly power by comparing the stated preferred project realisation by the chief in a private meeting during our first visit and the actual project realisation, and comparing the predictive power of the chiefs' preferences to those of the population. A similar approach is used to generate a measure of male power, using data on all sampled villagers excluding chiefs.[19]

To operationalise the measure, we provided a 0–1 score for each individual, whereby if his *ex ante* preferences coincided with the actual project realisation, he got a score of 1, and otherwise a score of 0. The interpretation in the analysis will be the probability to successfully have his preferences represented in the project realisation. The hypothesis that *Tuungane* villages will exhibit lower levels of capture of outcomes by the chief, conditional on the villagers preferences, should result in *Tuungane* having a negative effect on the ability of the chief's *ex ante* preferences to predict the project realisation over and above the preferences of ordinary citizens.

Table 24 presents first the mean of the binary variable for all villagers, which should be interpreted as the proportion of villagers (including the chief) for whom the project realisation coincides with their stated preferences. The two columns indicate that we collected the individuals' preferences at two stages before observing the outcome, before and after the village meeting, which took place on the second day of the first visit. We included both points in time because these represent very different quantities. During the village discussion, villagers interact and substantive deliberation may potentially produce agreement. Not taking into account the preferences after the village meeting risks confounding influence over power, since the chief could have greater knowledge of the village needs and convince the villagers during the meeting.

The first row in Table 24 reports the likelihood that we can correctly predict project selection using a villager's preferences (chief included) as given before and after village discussions, by noting whether the private wish of the villager coincides with the actual project realisation. In general we expect the chief's preferences to be more predictive of outcomes than those of citizens. This difference forms the basis of our measure of dominance. We see that in control areas the chief's prior preferences are 4 per cent more likely than those of a randomly selected villager to coincide with actual projects. The chief's post-meeting preferences are not, however, more likely to predict correctly. The *Tuungane* effect reported in the table is the degree to which this chiefly dominance is reduced in *Tuungane* areas. We see in the two measures weak and inconsistent evidence for a *Tuungane* effect.

The lower rows report the same relations for male dominance. We see again that male preferences predict outcomes more accurately than those of women. We find, however, that men are no less dominant in Tuungane areas.

[19] Note that the male dominance measure was added after the initial report but before analysis of any data.

Table 24 *Tuungane* effect on dominance of chiefs and of men

	Relative to pre-group meeting citizen preferences	Relative to post-group meeting citizen preferences
Chief dominance in control areas	0.04	0
Tuungane effect on chief dominance	–0.01	0.02
(se)	(0.03)	(0.03)
N	2,401	5,316
Male dominance in control areas	0.04***	0.07***
	(0.02)	(0.02)
Tuungane effect on male dominance	–0.03	0
(se)	(0.03)	(0.03)
N	2,096	4,891
Male or chief dominance in control areas	0.04***	0.07***
Tuungane effect on male or chief dominance	–0.03	0
(se)	(0.03)	(0.03)
N	2,096	4,891

Note: Based on data from AC-17, AV-14-bis, B-23, AP1-13 and AP2-14.

6.2 Results: social cohesion

6.2.1 *RAPID: distribution of benefits across social categories*

To test the hypothesis that *Tuungane* improves social cohesion, we looked at the access of identifiable categories to benefits available to the communities. Participation in the RAPID process provided a unique opportunity to detect changes in the access of target social categories to the benefits of the programme. This was particularly straightforward to measure when communities chose to use the RAPID funds for direct distribution of small assets or consumption goods. Since we collected socio-economic data of a random sample of respondents in RAPID villages (10 per village), as well as their benefits from the RAPID project, we could measure the impact of *Tuungane* on cohesion by the difference in *per capita* amounts received by marginalised social categories (relative to the average amount received in the village) in *Tuungane,* against non-*Tuungane* communities. The difference is interpreted as the average treatment effect on the access of those categories to benefits of public projects in their respective communities.

Table 25 displays the average level of private transfer. The average transfer is of US$2.38 per household and ranges from US$0 to US$50.

Table 25 Distribution of benefits

	Private benefits (all villages)	Private benefits (villages in which at least one person reported some private benefits)
Mean	US$ 2.38	US$7.61
Standard Deviation	(8.85)	(14.5)
Max	US$100	US$100
Min	US$0	US$0
N	3,763	1,174

Note: Average benefits reported received by respondents (household). Based on measure QR3.

Table 26 restricts attention to migrants (respondents not born in the village) and focuses on villages where at least one respondent reported receiving benefits. In particular, it provides the estimated *Tuungane* effect on the *per capita* benefit earned by a villager who is not born in the village. Its interpretation is the number of additional dollars that migrants receive as direct transfers from the RAPID project if they happen to be in *Tuungane* communities.[20]

Migrants receive nearly US$0.70 less than non-migrants in control communities, which we refer to as a migrant penalty (and this effect is statistically significant). In the third line of Table 26 we see that *Tuungane* reverses this effect (with migrants receiving relatively more in *Tuungane* areas). The *Tuungane* effect on the penalty is relatively large but, given the high variation on this measure, is not statistically significant.

Table 26 Distribution of benefits to migrants

Average non-migrants in control	2.03
	(0.51)
Migrant penalty in control areas (amount migrants receive *less* than others, on average)	0.69*
	(0.53)
Tuungane effect on the migrant penalty	–1.83
(se)	(1.56)
N	1,893

Note: Numbers in dollars. Based on measures QR3 and SP1.

6.2.2 *Trust: willingness to lend money to other village members*

The survey also provides multiple measures of social cohesion. As a measure of trust, respondents were asked to report whether (and to what extent) there was a person from a given category to whom they would be willing to lend money to go to market. Average responses ranged from 0.37 for ex-combatants to 0.97 for individuals of the same family.

[20] A more precise test would be to restrict attention to migrants who arrived before the launch of the *Tuungane* programme, since we have not yet ruled out the possibility that *Tuungane* attracted new migrants of a different type (total numbers of new migrants are about the same in *Tuungane* and non-*Tuungane* areas), or changing the patterns of integration of new migrants, while not improving the access to benefits of the rest of migrants. Low sample size prevents us from conducting this analysis in the current report.

The quantity of interest is the effect *Tuungane* has on the probability that a randomly selected villager responded 'yes' to any of the questions across categories. Results reported in Table 27 indicate that *Tuungane* had a positive and weakly significant effect overall. The strongest effects are increased levels of trust in ex-combatants, a result consistent with findings from the Liberia research (Fearon *et al.* 2011).

Table 27 Trust

	Family member	From this village	From another village	Co-ethnic from another village	Non co-ethnic from other village	Ex-combatant	Mean effects
Control	0.97	0.93	0.73	0.68	0.52	0.37	0
Tuungane effect	0.01	0.01	0.01	0.02	0.02	0.04**	0.07*
(se)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.05)
N	3,855	3,855	3,855	3,855	3,855	3,855	3,855

Note: Based on measure Q43.

6.2.3 *Presence of cleavages in the village*

Divisions can occur along many lines. The endline survey asked respondents to report on the kinds of divisions that exist in their villages, as well as the effects of *Tuungane* on the prevalence of these divisions. Results in Table 28 suggest a general willingness to report on multiple types of division (note that the possible lines of division listed were not read out to respondents for this question). Gender, generational and religious divisions are the least reported and the most prominent reports are of class, religious and native/migrant divisions.

Table 28 Presence of cleavages

	Rich-poor	Male-female	Young-old	Natives-migrants	Religion	Ethnic groups	Different elites	Other	None	Mean effects
Control	21.98	12.53	12.08	18.57	11.98	13.51	10.87	9.70	26.49	0
Tuungane effect:	–1.65	0.26	–0.83	0.2	–1.1	0.59	1.22	0.03	–0.22	–0.01
(se)	(1.90)	(1.39)	(1.42)	(1.63)	(1.50)	(1.56)	(1.49)	(1.40)	(2.20)	(0.05)
N	3,776	3,776	3,776	3,776	3,776	3,776	3,776	3,776	3,776	3,776

Note: Clustered at the CDC level. Based on DML Q38.

Table 28 suggests that on several counts (rich-poor, young-old, religion) reported divisions are weaker in *Tuungane* areas. However, the effects are not significant for any of the measures. The mean effects are also negative and not significant.

6.2.4 *Inter-village cooperation*

So far our focus has been on cohesion within villages. But as part of *Tuungane,* villages also work together within VDCs and within CDCs. Possibly, then, *Tuungane* may also promote cohesion at broader levels. To assess inter-village cohesion we asked individual respondents to do a thought experiment. We asked them to imagine that an NGO could choose whether to invest US$800 in a project in their own community or to invest US$500 in their own community plus US$500 in a randomly sampled village in their *chefferie* (we indicated for each respondent a set of particular villages). Our interest is in the extent to which individuals are willing to support actions that have broader benefits to the wider community even at a cost to their own village.

Table 29 Inter-village cooperation: willingness to cooperate

	Men	Women	Total
Control	49.31	51.67	50.17
Tuungane effect:	1.27	0.69	0.98
(se)	(3.18)	(3.86)	(3.06)
N	1,928	1,512	3,482

Note: Standard errors clustered at the CDC level. Numbers are in percentages. Based on question SP14.

Table 29 provides the results, as the percentages of respondents who were willing to share with neighbouring villages. On average, about 50 per cent of men reported a willingness to share the projects between villages, and slightly more for women. *Tuungane* areas score moderately better than control areas. The difference is, however, not statistically significant.

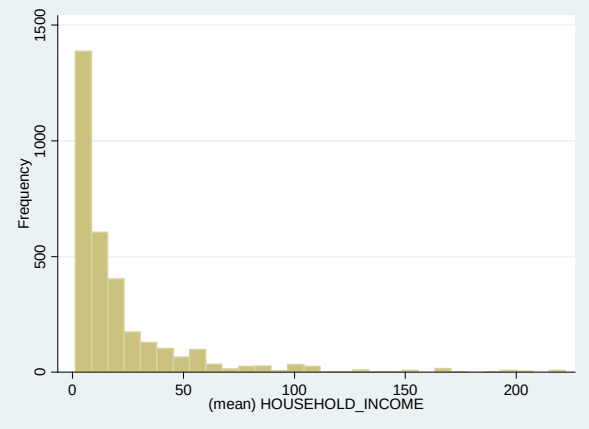
6.3 Results: welfare

We hypothesised that participation in *Tuungane* would improve economic productivity, augment household assets and improve access to services. We assess these hypotheses by examining the amount of time devoted to productive activities and the household assets of random samples of villagers, including the quality of their homes. Alongside this, we estimate welfare outcomes including the incidence of sickness (which would be reduced if *Tuungane* resulted in better welfare outcomes overall and better health facilities in particular) and school attendance (which would be increased if participation in *Tuungane* improved access to education or positively altered household decision making with respect to education).

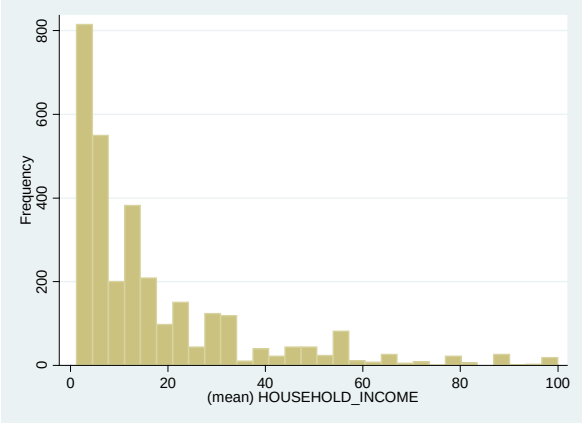
6.3.1 *Income*

The first measure of welfare we report is the household's reported income generation over the previous two weeks.

Figure 13 Household income in the last two weeks (US$)



Note: Full sample [n=3,161]. Based on measure QE9.



Note: Less than US$100 [n=3,094]. Based on measure QE9.

Table 30 Household income

	Reported household income (over two weeks) (in US$)
Control	22.77
Tuungane effect:	–3.09 !
(se)	(1.28)
N	3,155

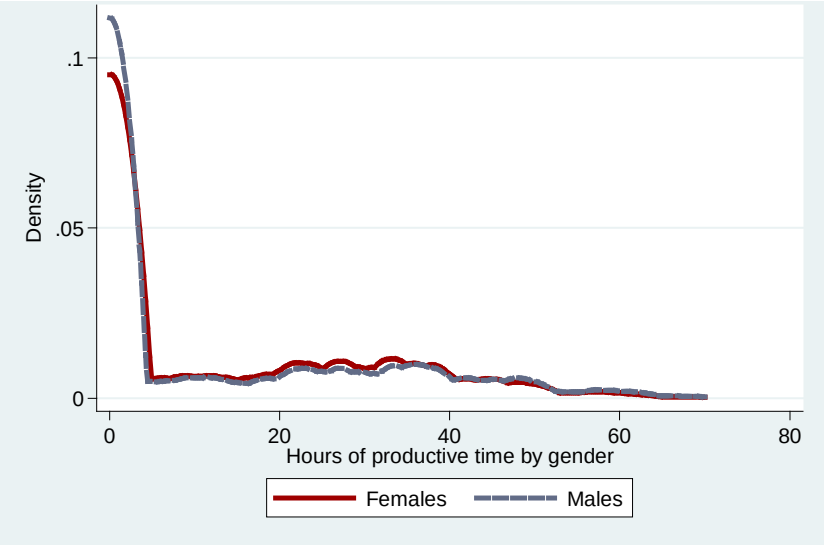
Note: Standard errors clustered at the CDC level. Amounts in US$. Based on measure QE9.

Table 30 indicates that an average household in control areas generated around US$21 in the two weeks before the survey took place. The table also indicates the impact of *Tuungane*. The average household in *Tuungane* areas reports less income than non-*Tuungane* areas (about US$3 less over the past two weeks), an effect that is significant at the 95 per cent level on a two-tailed test.

6.3.2 *Productivity*

A second measure of welfare is a person's level of productivity. We measure an aspect of productivity by the total amount of hours a person spent over the last seven days on activities that generated income for the household.

Figure 14 Productive hours, by gender



Note: Distributional plot for males and females< 100 hours [n=18,729 for females and n=18,910 for males]. This figure is based on measures QF7 and QF18.

Table 31 Respondent productivity (hours)

	Female	Male	Total
Control	16.80	16.53	16.67
Tuungane effect:	–0.14	–0.42	–0.28
(se)	(0.60)	(0.61)	(0.53)
N	8,992	8,616	17,608

Note: Based on measures QF7 and QF18.

Table 31 shows the average number of hours in the last seven days spent on activities that generated income for the household, and *Tuungane* effects for all respondents aged between 12 and 60 reporting hours worked as below 100 a week. The average number of hours worked does not differ significantly among men and women. Though more men work, those women that work do so for longer periods of time.

It was expected that *Tuungane* respondents would allocate more of their time to productive activities. As Table 31 indicates, the average adults in *Tuungane* communities devote less time to productive activities than in control communities. The difference in productivity of *Tuungane* villagers' productivity is, however, not significant at conventional levels.

Table 32 presents our estimate of an average household agricultural production in our sample, for those households whose main source of income is agriculture and for whom survey teams could obtain data on agricultural production. This is estimated from measures of the quantities of production of all major crops by all households of the village (excluding the chief and VDC members), and average prices at the *chefferie* level, as reported by the price received by each selected household in the *chefferie*.

Results suggest that the average *Tuungane* household produces about eight more in dollar value per year (or 4 per cent more than a non-*Tuungane* household), although these gains are measured here with great uncertainty and are not significant at conventional levels.

Table 32 Household annual agricultural production

	Household production
Control	185.02
Tuungane effect:	8.11
(se)	(15.50)
N	3,773

Note: Amounts in US$. Based on measures CH18 and CH20. All estimations are based on the local exchange rate provided in CQ18.

6.3.3 *Household assets*

To evaluate asset holdings, we asked each respondent about a range of items that the family may own, including livestock, household furnishings and equipment and technology. These measures correlate highly, suggesting that they jointly reflect an underlying attribute (wealth) reasonably well.[21]

In the analysis in Table 33, we show the effect of *Tuungane* on ownership of each of these items, as well as on an index of asset holdings formed using principal components analysis. The results suggest no effects of *Tuungane* on asset holdings overall.

[21] The Cronbach's alpha score for these items is high at 0.77.

Table 33 Household assets

Type of assets	Control	*Tuungane* effect	(se)	N
Assets	0	–0.01	(0.05)	4,761
Goats or sheep	1.14	–0.06	(0.08)	5,609
Poultry	2.83	–0.05	(0.17)	5,603
Cattle	0.06	0	(0.01)	5,597
Pigs	0.19	–0.02	(0.03)	5,605
Houses	1.58	0.05	(0.06)	5,609
Rooms	3.18	0.06	(0.08)	5,603
Tins or flasks	2.45	–0.13	(0.11)	5,587
Chairs	2.18	–0.07	(0.11)	5,591
Beds	0.47	–0.01	(0.06)	5,615
Foam mattress	0.52	–0.03	(0.04)	5,610
Straw mattress	1.70	0.08	(0.07)	5,592
Bucket	0.86	–0.04	(0.07)	5,608
Basins	1.31	0	(0.04)	5,599
Petrol lamps (or equivalent)	0.58	0.03	(0.04)	5,623
Radios	0.55	–0.01	(0.03)	5,612
Bikes	0.48	–0.03	(0.04)	5,626
Machetes	3.06	0.04	(0.08)	5,614
Pans	4.78	–0.12	(0.13)	5,395
Dressers	0.09	0.01	(0.01)	5,623
Canoes or boats	0.04	0	(0.01)	5,618
Cellphones	0.29	–0.02	(0.03)	5,602

Note: The assets index is centred on 0 for control with unit standard deviation.

6.3.4 *Quality of housing*

The quality of respondents' walls served as another indicator for household wealth. We recorded whether walls were made of mud, plastic, non-baked bricks, bamboo, stone, semi-durables, baked bricks, concrete, metal or cardboard. Multiple responses were possible for any given household. Approximately 27 per cent of respondents had homes made of mud; the most common material was non-baked bricks, which were used in 64 per cent of homes. Much smaller shares used bamboo (14 per cent) and baked brick (6 per cent) or other quality material. We constructed a measure to indicate that a high-quality wall is made out of baked bricks, concrete or metal.

Table 34 Wall quality (share households with high-quality walls)

	High-quality wall
Control	7.6
Tuungane effect:	–1.8
(se)	(1.01)
N	5,612

Note: Based on measure QE10. Amounts in percentages.

Table 34 shows the effect of *Tuungane* on the quality of walls. Contrary to expectations, we found a negative relationship between *Tuungane* and wall quality (roughly reducing the share by two percentage points). This result is not statistically significant in a two-tailed test. To provide confidence that this measure captures relevant features of household wealth, we examined how the measure related to chief status, finding that chiefs are seven percentage points more likely than the rest of the population to have high-quality walls.

6.3.5 *School attendance*

As a measure of effects on access to education we took the number of days attended to school in the last two weeks for all children between five and 17. We constructed this measure for boys, girls and both combined. Attendance rate averages 50 per cent for this group (five days out of 10), with no difference between boys and girls.

Table 35 suggests a negative relationship between *Tuungane* and school attendance. These estimates, however, are not significant at conventional levels.

Table 35 Days of attendance at school (last two weeks)

	Girls	Boys	Total
Control	5.01	5.03	5.02
Tuungane effect on attendance	–0.21	–0.04	–0.12
(se)	(0.34)	(0.35)	(0.30)
N	1,469	1,607	3,076

Note: Based on measures QF7 and QF14.

6.3.6 *Sickness*

We measured the incidence of sickness in a household by obtaining information about different types of sickness that took place over the previous two weeks for children younger than seven years old. These were fever, cough, cough plus sweat and faster breathing, diarrhoea and diarrhoea plus blood. A final measure was whether, in the previous two weeks, a person in the household was seriously ill, defined as being unable to go to work or to school. Table 36 shows the results on incidence of sickness in the population. Overall, the incidence of sickness within a household over the previous two weeks for children aged seven or younger is high. As Table 36 indicates, nearly one in three of these children had had a fever and one in four a cough over the last two weeks. About one in six (adult) household members was reported as seriously ill, making it impossible for them to work or to go to school. Table 36 indicates a weak positive correlation between *Tuungane* and poor health but the relation is not significant at conventional levels.

Table 36 Sickness during the last two weeks

	Children					Adults	Mean effects
	Fever	Cough	Cough plus sweat and faster breathing	Diarrhoea	Diarrhoea plus blood	Seriously ill (cannot go to work or go to school)	(Children and adults)
Control	30.69	26.29	10.98	9.74	3.07	16.66	0.00
Tuungane effect:	1.32	1.08	0.49	0.00	–0.42	0.74	0.05
(se)	(1.63)	(1.65)	(1.04)	(0.81)	(0.81)	(1.27)	(0.04)
N	10,375	10,381	10,354	10,278	10,278	9,525	4,195

Note: Data is clustered at the CDC level. Numbers are in percentages. Based on measures QF20 and QF21.

Table 37 Rate of accessing services (per month use)

	Wells	Primary school	Secondary school	Health centre	Maternity clinic	Road	Bridge	Irrigation	Meeting centre	Church	Gov't services	Mean Effects
Control	14.92	11.37	6.13	2.14	0.36	3.61	5.50	0.59	0.38	5.58	0.24	0
Tuungane effect:	0.29	0.46	–0.26	0	0.01	–1.23	0.48	0.01	0	–0.12	–0.07	0
(SE)	(1.13)	(0.58)	(0.53)	(0.16)	(0.04)	(0.64)	(0.53)	(0.11)	(0.05)	(0.29)	(0.06)	(0.05)
N	3,319	3,595	3,629	3,439	3,705	3,295	3,364	3,784	3,364	3,561	3,356	3,852

Note: Standard errors are clustered at the CDC level. Numbers are in number of times used per month. Data drawn from responses to DMLQ.

6.3.7 *Rate of accessing services*

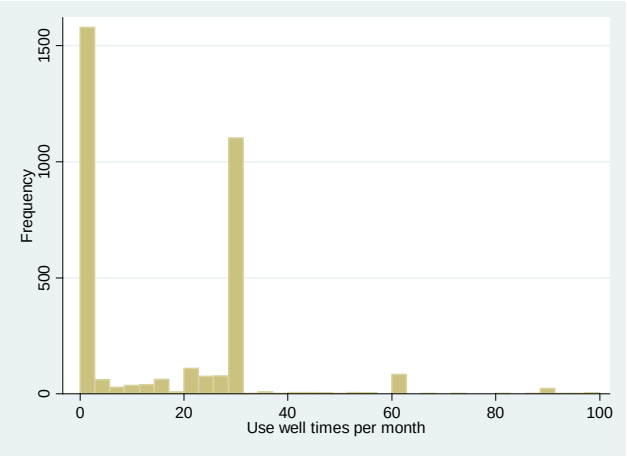
A final welfare measure we examined was the rate at which a household accesses services such as schools, hospitals, bridges and so on.

Table 37 shows average access to these services, ranging from access on average once every two days (wells) to less than once per month (government services). These average numbers mask the variation between respondents that, as seen from Figure 15, can be considerable. While, on average, children attend primary school 12 days per month, this figure comes from near full-time attendance for one group of children of households and nearly complete absence for another group.[22]

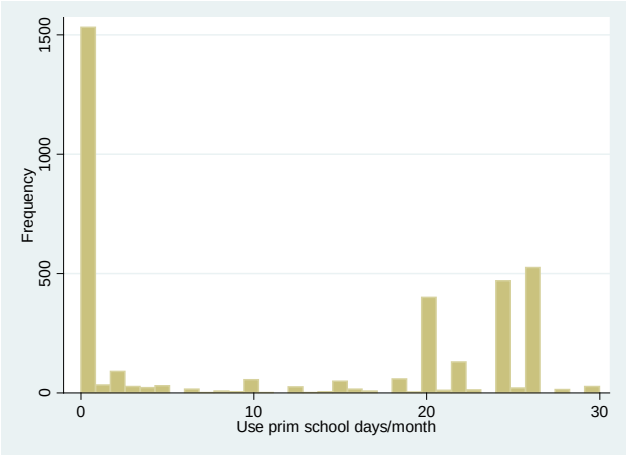
Table 37 also shows the effects of *Tuungane* on access to services. *Tuungane* seems to have had a negative relationship with access to roads – with a p=0.054 it is almost statistically significant at the 95 per cent (two-tailed test) – although in the context of the multiple measures given here this single adverse finding is plausibly due to chance. The overall pattern suggests no difference in access to services attributable to *Tuungane*.

[22] In later analysis we can condition on households with children of school-going age.

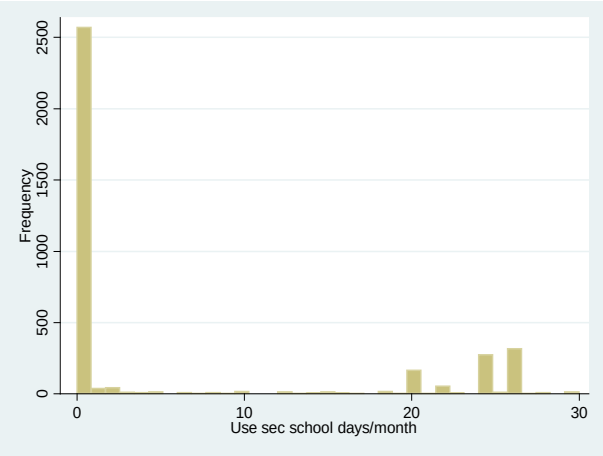
Figure 15 Access to services



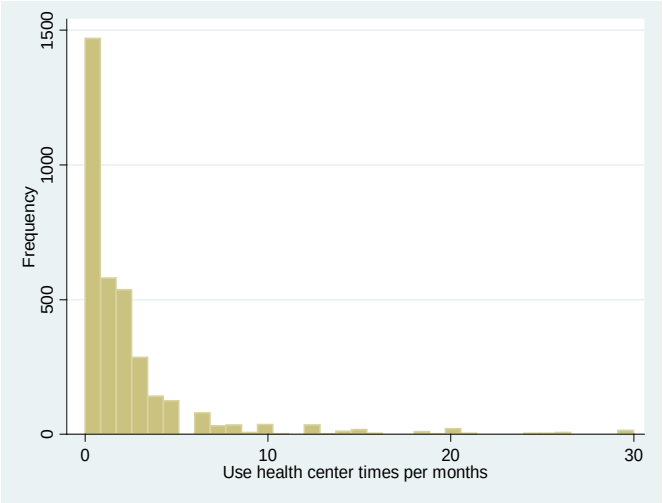
Use of wells (per month) N=3,330



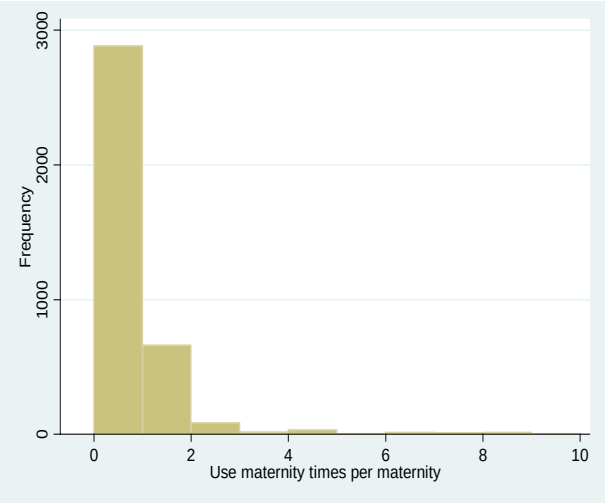
Use of primary school (days per month) N=3,609



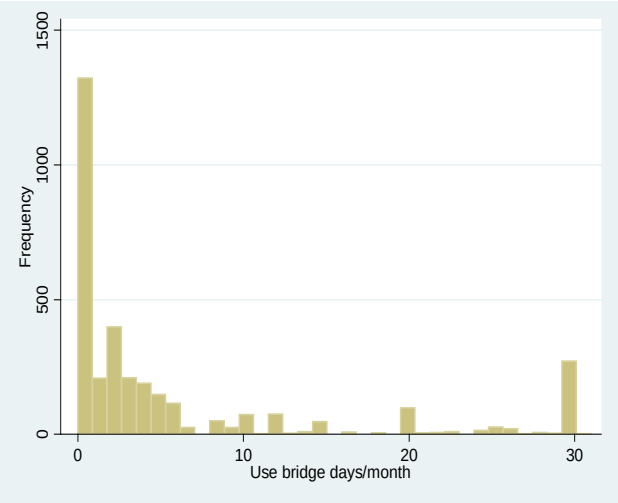
Use of secondary school (days per month) N=3,643



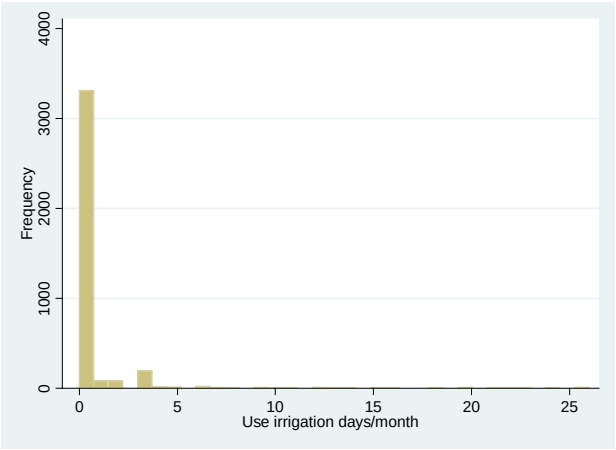
Use of health centre (per month) N=3,454



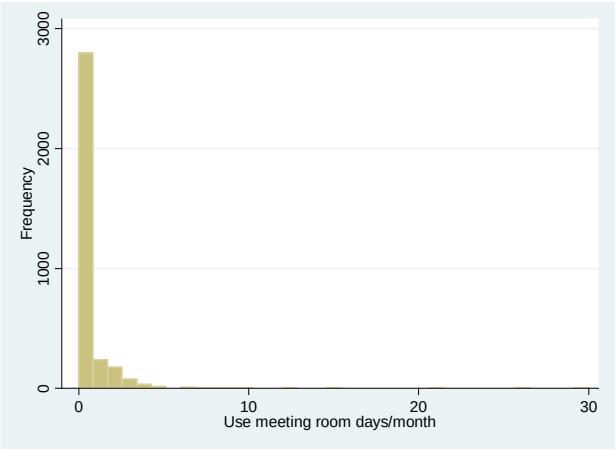
Use of maternity clinic (per maternity) N=3723



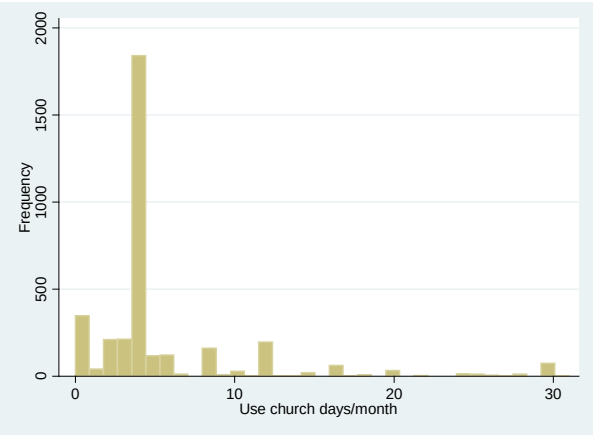
Use of bridge (days per month) N=3,380



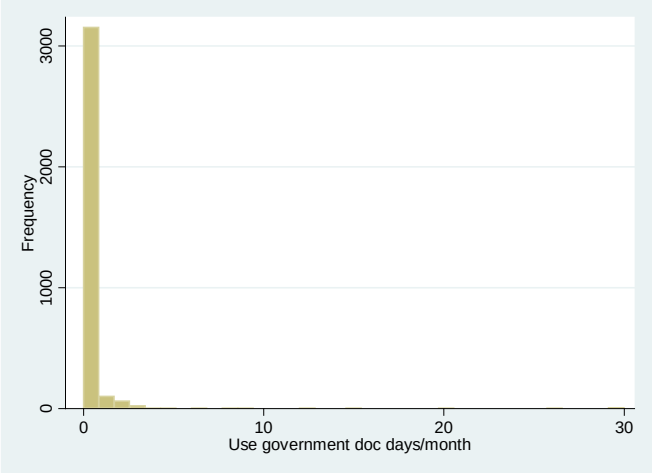
Use of irrigation (days per month) N=3,800



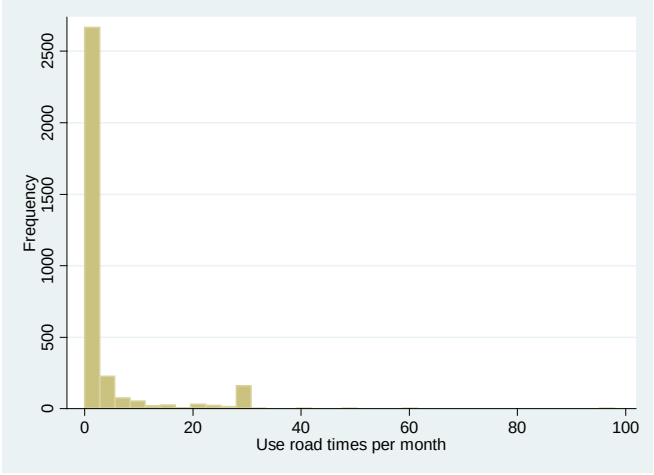
Use of meeting room (days per month) N=3,377



Use of church (days per month) N=3,573



Use of government document services (days per month) N=3,365



Use of road times (per month) N=3,310

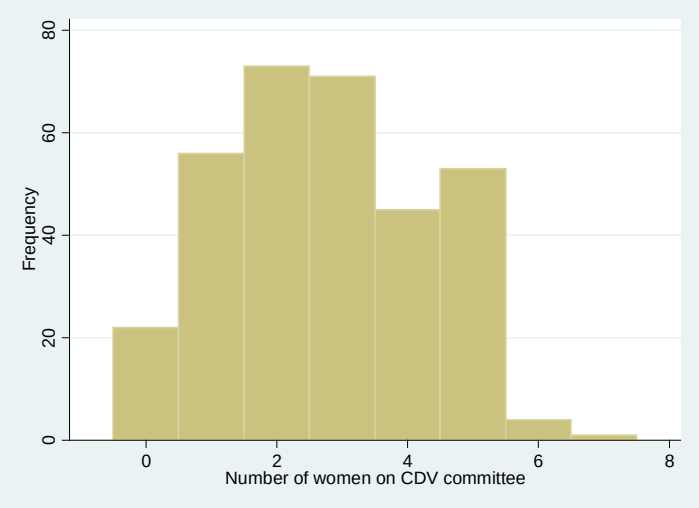
Note: Based on responses to measure Q63.

6.4 Results: effects of gender parity variation

A core element of *Tuungane* is a focus on women and the rights and roles of women in collective decision making. As described in the initial project document (IRC, 2006), women are thought to be particularly disenfranchised by conflict and there was a hope that socioeconomic projects that focus at the village level would lead to greater involvement of women. Many other elements of the programme emphasised these themes, with trainings focused on the needs of women and an institutional provision that development committees established in villages would be gender balanced.

A key element of the design of *Tuungane* I was that the requirement of gender parity in development committees was lifted for a random subsample of VDCs. The random lifting of this requirement allowed us to assess how effective this requirement was in changing attitudes towards women as decision makers. In areas in which the requirement was lifted, communities were still free to select women and indeed many of them did. The distribution of the number of committee members (out of 10) that were women in areas in which parity was not imposed is shown in Figure 16 below. In the median case, communities selected three women, rather than the five required in the cases with imposed parity. In only 7 per cent of cases (mostly in Maniema) were no women selected in the non-parity areas. This suggests that while the requirement increases women's representation, it generally does not do so in a situation where women would otherwise not be represented.

Figure 2 Share of women on VDC committees in areas without a gender parity requirement



Note: Share of 10 committee members that are women in areas that did not have a gender parity requirement.

Many of the results we have examined have provided a breakdown of effects along gender lines that allow us to assess whether benefits of *Tuungane* were particularly strong among women. In this section, we first described results on project selection and, second, results on a set of simple attitudinal measures that directly assessed views towards the rights and roles of women.

Past studies have found that women in leadership positions can lead to changes in the types of projects selected by local governments (Chattopadhyay and Duflo 2004). We asked the same question here, focusing on the set of VDCs that had gender lotteries. Do areas in which there is a mandated gender parity select projects differently to those in

which there is not? Item 9, below, shows the average share of projects selected in each sector in each village across major categories, broken down by whether a parity condition was in place or not. We see broad patterns that are similar in areas with and without gender parity. However, the removal of gender parity is associated with an approximately 9 per cent shift away from the selection of education projects and towards watsan.[23]

Item 9 Project selection according to gender parity

	Health	Education	Transport	Watsan	Agriculture	Other
Parity condition level	0.13	0.58	0.07	0.11	0.06	0.06
Effect of removing parity requirement	–0.01	–0.09	0	0.09***	0.01	0
(se)	(0.03)	(0.06)	(0.03)	(0.03)	(0.03)	(0.02)
N (Number of villages)	591	591	591	591	591	591

Note: Effect of removing the parity requirement on the average share of projects focused in different sectors. Data reported only for VDCs that were eligible for standard errors clustered on CDC (the level at which randomisation took place).
*** significant at 99% (two-sided test). Based on project data and includes villages that were and were not surveyed by the research teams.

We turn now to effects on attitudes. In the endline survey we asked up to five respondents in each village to position themselves on four issues related to women's rights and roles, specifically:

- Equality of rights: in DRC women should have the same rights as men (or alternatively, according to Congolese custom, women have always been subject to men and they should remain so);
- Right to complain: if a man mistreats his wife she has a right to complain (or alternatively, according to Congolese custom, women should not complain even if mistreated by their husbands);
- Decision making: women should have the same opportunities as men to occupy socio-administrative positions in the village (or alternatively, men are better leaders and should occupy these positions); and
- Leadership: women have knowledge to contribute and so should be eligible to serve as presidents of development committees in the village (or alternatively, only men should serve as presidents of development committees).

For each statement, or its alternative, respondents could agree strongly, agree weakly, disagree, disagree strongly, or register no position. Based on these responses, we created a score ranging from 1 for those strongly agreeing with the empowering position and 0 for those strongly agreeing with the opposite position.

Item 10, below, reports the effects of *Tuungane* on these items, independent of the specific institutional requirements for parity. We see that, in control areas, scores average weakly positive across the board. Participation in *Tuungane* has no impact on these average scores, however.

[23] The table reports significant effects on just one coefficient which has an associated p value of 0.007. We can nevertheless reject the null of [should 'of' read 'or'?] no effect on all sectors using the Bonferroni adjustment approach. Under this approach, to maintain a 5 per cent family-wise error rate we should test individual hypotheses at p=0.008. Note that this finding is different from that found in Chattopadhyay and Duflo (2004), where women's leadership was associated with a greater investment in watsan.

Item 10 *Tuungane* and attitudes to women and governance

	Equality of rights	Right to complain	Decision making	Leadership	Mean effects
Control average score	0.59	0.64	0.69	0.70	0
Tuungane effect	0.01	0	0	–0.01	0.01
(se)	(0.02)	(0.01)	(0.01)	(0.01)	(0.07)
N (Number of villages)	815	815	815	815	815

Note: Based on questions QG 8–11.

Item 11, below, shows that removing the gender parity requirement is associated with gains in attitudes towards women and governance overall. Conversely, the imposition of parity requirements has adverse effects on these outcomes. However, these adverse effects are not statistically significant at conventional levels.

Item 11 Gender parity and attitudes to women and governance

	Equality of rights	Right to complain	Decision making	Leadership	Mean effects
Parity condition level	0.54	0.63	0.69	0.69	0
Effect of removing parity requirement	0.04	0.0	0.0	0.01	0.1
(se)	(0.04)	(0.03)	(0.03)	(0.03)	(0.15)
N (Number of villages)	186	186	186	186	186

Note: Based on questions QG 8–11.

7. Further analysis

7.1 Robustness

Overall, we have reported a large number of null results. In light of this, and from concern that analysis decisions resulted in false negatives, we have undertaken a series of robustness tests to examine the extent to which the results (and non-results) are sensitive to various features of our specification. The seven robustness tests we implemented are as follows:

1. We generated a set of results in which all analysis was undertaken at the village level (rather than at the individual level as is sometimes the case). Variables were aggregated to the village level using individual sampling weights. The village level analysis was done using propensity weights only. This limited the extent to which extreme weights can influence cross-village comparisons.
2. We generated all results at the village level, introducing controls for lottery bins. Introducing these controls between bin variance should lead to more precise estimates of effects.
3. We generated all results (at the village level) but clustering by CDC for treatment units and village level for the control units. This approach is less conservative but can be defended on the grounds that the CDCs had no meaning for control groups.
4. We generated results (at the village level) using propensity weights adjusted to assess VDC level sample average treatment effects, rather than population average

treatment effects. These weights have lower variance and may provide more precise estimates.[24]

5. We generated village estimates at the CDC level (at this level there is no clustering of treatment assignment) and used White's heteroskedasticity-robust variance estimator to give a conservative approximation to the exact randomisation variance of the difference in means (see Aronov and Samii 2012).
6. We generated village level results using a treatment variable that uses IRC's classification of treatment in cases in which databases disagreed.
7. We examined the effect of distance to nearest *Tuungane* villages as an alternative treatment (conditional on lottery bin and shortest distance to any village), in order to capture spillover effects.

Item 12, below, summarises the results that would have been found significant if each alternative model had been selected as the base model.[25] Overall, we find that the results are strikingly robust. Over most models, estimates changed only moderately. A number of the variations result in reduced standard errors. No estimates in this table shift from positive significant to negative significant across specifications. The positive findings on share of women on committees, willingness to complain when funds are misused and trust in others generally maintained significance across specifications. The weak positive effect on contributions to road widening is stronger in alternative specifications. The tendency to observe a shift towards reaching out to NGOs and away from other groups is observed in other specifications also. In two specifications there is also some evidence of reduced social divisions. In a number of specifications there is also suggestive evidence of adverse effects, particularly on household income, quality of homes and citizen attitudes regarding duties to engage with government. In all, there is positive evidence in some specifications for 21 items (one-sided test) and negative evidence for 14 items (two-sided test).

Overall, these shifts on some variables occur in a context where there is no shift to significance on the vast majority of variables examined, with 165 variables registering no effects significant at the 95 per cent level in any specification. Thus, for example, while some specifications find positive effects on some assets, no specifications find effects on the overall asset index. While one specification finds positive effects on one health outcome, no specifications find significant effects on health items overall. While in later work we will seek more precise estimates by employing prespecified controls (as per our analysis plan), our conclusion now is that the collection of negative findings reported here is not highly sensitive to the simple model employed.

[24] Consider two lottery bins, one with three units and one with four units. Say that in the four-unit area two units were selected for treatment, and that in the three-unit bin one unit was selected for treatment and one of the control units was randomly sampled to participate in the study. Then under the base specification, units in the first bin have propensity weights of 2, while units in the second bin have propensity weights of 3. Under the robustness check, both sets would have weights of 2 (with no attempt to make inferences to the unsampled control unit in this bin).
[25] Full results are available from the authors.

Item 12 Robustness

		0	1	2	3	4	5	**6**	**7**
Table	Column	Base	Village	Bin controls	Alt. cluster	Alt. propensity	Robust SEs	Alt. treatment	Distance
8: Committee composition	Number of women	+	+	+	+	+	+	+	+
	Number of men							-	
	Share women	+	+	+	+	+	+	+	+
	Means effects								+
9: Public good provision	Widening road (% contributing)	+		+	+	+			+
10: Duties of political leaders	Awareness raising (women)	-							
	Avoid corruption (men)								+
	Consult populations (women)								-
12: Duties to participate	Suggestions to the government (women)								-
	Make suggestions to government (all)			-			-		
	Material support to government projects (women)			-					-
	Material support to government projects (all)								-
14: Complaints (private)	There was not enough information about the process								-
15: Complaints (private)	Conditional on implementation	+	+	+	+	+	+	+	
16: Accounting	Amount (evaluation team)	-	-	-	-	-	-	-	
	Share justified								+
18: External support	Government/health			-					
	Government other	-	-	-	-	-			
	NGO support	+		+				+	
24: Dominance	Male (post-decision)		+	+	+	+	+	+	
	Men or chief (post-decision)			+		+		+	
27: Trust	Villagers			+	+		+		
	Co-ethnics			+				+	+
	Ex-combatant	+	+	+		+		+	
	Mean effects	+		+	+			+	+
28: Cleavages	Religions							+	
	Other								+
	None							+	
30: Income	Over two weeks (US$)	-	-	-	-	-	-	-	
33: Assets	Lamps								+
	Dressers							+	+
34: Walls	High wall quality	-		-	-		-		
36: Sickness	Diarrhoea and blood							+	
37: Access services	Bridges			+					
	Roads	-		-	-				

Note: + indicates positive effects, significant at at least 95% (one-sided tests). This indicates negative estimated effects, but significant at 95% (two-tailed test). For the remaining 165 variables there are no relations that are individually significant.

7.2 Heterogeneous effects

Tuungane might have had a different impact on different types of people, in different types of villages, and so on. We try to understand whether this is the case by sub setting the dataset into different groups and analysing whether there are differences in the impact of *Tuungane* within these groups. Item 13, below, lists the hypotheses.

Item 13 Groups for heterogeneous effects

#	H0: There is no differential *Tuungane* impact for…	Origin	2007	3ie	Measures
1	villages, based on project area (MN, TN, HK, SK)	ODS	H34		
2	villages, based on levels of conflict	ODS	H30	H2	(*) Baseline conflict measures
3	villages, based on wealth levels	ODS	H31	H1	(*) Baseline welfare data
4	villages, based on levels of cohesion or decision-making norms	ODS	H32	.	(*) Baseline social cohesion data
5	villages, based on levels of returnees and new migrants	ODS	H33	.	(*) In and out migration from the village prior to 2007 (CQ 136-CQ 140, SP 1SP 2)
6	villages, based on previous support from international organisations	ODS	H35	.	All outcome measures. Household surveys; Q44, Q45, Q48. (*) Chief survey: CQ31 [condition on organisations starting operations pre 2008]. Baseline data on operations of other organisations
7	villages, based on level of isolation	ODS	.	H3	Survey measures: QE13, GIS (AS-10)
8	villages, based on time-length of the CDV project	KIN	.	.	CDV time-length will come from IRC tracking data
9	villages, based on time period between end CDV project and evaluation	DFID	.	.	CDV end-dates will come from IRC tracking data
11	villages, based on the level of turnover in the CDV committee	HK	.	.	DV12: For how long have you been a member of the CDV? (years)
12	villages, based on type of project	HK	.	.	Type of project will come from IRC tracking data
13	villagers, based on elite status (rich and educated)	HK	.	.	QF13: NIVEAU SCOLAIRE. QE 9 How much cash income did this household earn over the last week?
14	villagers, based on gender	HK	.	.	QF7 SEXE

Notes: The column Origin indicates from where this hypothesis originates; ODS indicates the final evaluation's design document; KIN refers to the meeting between the IRC and the University of Columbia team in Kinshasa in early 2012; DFID refers to the meeting with DFID in Kinshasa; and HK refers to the meeting between the IRC and the University of Columbia team in Lubumbashi, Haut Katanga. The columns 2007 and 3Ie list the related hypothesis number in the original design document from 2007 and the 3ie application. Finally, Measures indicates which measures will be used, and (*) indicates the main measure of interest. We emphasise that all tests for heterogeneous effects have been reported here.

All tests in this section are two-sided. That is to say, we do not hypothesise what different effects we expect the *Tuungane* programme to have for these different groups. The rows include the possible outcome measures, exactly the same as those used in the final evaluation report. The columns will indicate the different subgroups. A black star indicates that we find evidence in support of the hypothesis at the 95 per cent confidence interval in a two-sided test. A red star indicates evidence for adverse effects. Note, that due to sub grouping the dataset, we decrease the number of observations and therefore the precision of our estimates.

7.2.1 *The heterogeneous impact of Tuungane by village characteristics*

First of all, we looked at whether *Tuungane* had a different impact based on the type of village. Item 14 and Item 15 provide the results. Because each regression has a different number of observations, based on things such as the number of missing data points, the level of analysis, etcetera, the top row in the tables provides the number of observations for the village level regression of attendance at meetings (Table 3) to give an indication of the size of the different subgroups (in practice, however, there may be some variation in these numbers since data was missing).

By province: *Tuungane* operated in four different provinces: Haut Katanga, South Kivu, Maniema and Tanganyika. These provinces are different in many regards and it is possible that *Tuungane* had different effects across provinces.

Item 14 presents the results for the four provinces. First, we find that the measure that originally was the only significant positive effect of *Tuungane* above the 5 per cent significance level – willingness to complain conditional on poor project implementation (Table 15) – is no longer significant when provinces are separated in the analysis (a result that can be due to a loss in statistical power). Second, we find that none of the positive effects are consistently positive across areas. For example, the positive impact of *Tuungane* on trust in others (Table 27) and agricultural productivity (Table 32), while only slightly significant in the main findings, are driven by South Kivu. The negative impact on household income seems to be driven by Haut Katanga and does not hold for the other three provinces. Overall, if we find any evidence, it points in a negative direction with, for example, *Tuungane* seeming to have had a negative impact on levels of participation in Tanganyika.

By level of violence: The next two columns in Item 14 (violence high and low) present results when looking at the impact of *Tuungane* in areas that are characterised by high levels of violence and those that are not. We define an area of high violence as those *chefferie* where a village has, on average, been attacked twice or more between 1995 and 2007. This information is obtained from the chief survey we conducted in 2007 in eastern Congo (CQ39).[26] We find little evidence for an impact by *Tuungane* for these different subgroups. The effect of *Tuungane* on reported trust in other groups (Table 27) is now positive in low-violence areas. And *Tuungane* had a negative impact on household income (Table 30) in low-violence areas.

By level of wealth: The final two columns in Item 14 (wealth high and low) are also calculated at the *chefferie* level, where wealthy *chefferies* are those where the villages, on average, had one or more schools in 2007. Low wealth level *chefferies* are those where this average is lower than one. Also, this information is based on the baseline chief survey (CQ65). We find an adverse impact of *Tuungane* on wealth in poorer areas: the effect of *Tuungane* on household

[26] In each CDC we had two evaluation villages: a RAPID village and a survey-only village. The panel dataset (i.e. where we did both a baseline survey and an endline survey) we only have for the RAPID villages. We therefore constructed the violence variables at the chiefdom level.

income (Table 30) and wall quality (Table 34) is statistically significant at the 95 per cent confidence level. Finally, in richer areas we find evidence that *Tuungane* is associated with worse health outcomes. We do not find any statistically significant positive impact of *Tuungane*.

By level of cohesion: *Tuungane* is a community-driven development programme and we might expect levels of social cohesion before the start of the programme to be important for the success of *Tuungane*. We obtained information about the level of cohesion from the baseline chief survey (CQ81) and measured social cohesion as those *chefferies* where, on average, a village had two or more community projects in the six months preceding the survey. We define those areas with low levels of cohesion as those *chefferies* where villages, on average, had less than that. We again do not find evidence of any statistically significant positive impact of *Tuungane* (see Item 15). On the other hand, we find a negative relation between *Tuungane* and wealth in low social cohesion areas as measured by household income (Table 30) and wall quality (Table 34). Moreover, in more cohesive villages we find evidence that *Tuungane* leads to worse health outcomes.

Presence of new inhabitants: We also looked at whether *Tuungane* had a differential impact in villages with a population of displaced people prior to the programme. This data was obtained from the chief survey (CQ136-139) and is a combination of the entry of internally displaced people (IDPs), returned IDPs, refugees and repatriated refugees between 2005 and 2007. *Tuungane* is expected to reduce cooperation barriers due to segregated groups and therefore is expected to have a stronger effect in villages with a population of displaced people, who are believed to be less integrated than natives. Item 15 presents the results. While we find fewer financial irregularities (Table 21) in villages with new inhabitants, we also find that *Tuungane* had a negative impact on household income (Table 30) in such villages.

By NGO presence: *Tuungane* might be particularly successful in areas not exposed to NGO activity. In our 2007 baseline survey, we obtained from the chief survey whether an NGO was operating in the village between 2005 and 2007 (CQ68). We define *chefferies* with a high NGO presence as those where, on average, more than 50 per cent of the village chiefs interviewed report prior exposure to NGO work. We find no impact of *Tuungane* in both types of areas.

By level of isolation: Finally, we expected the effect of *Tuungane* to vary with the degree of isolation of villages. For example, more isolated villages are more difficult to research and the treatment might be less effective. We define an isolated village as a village that is more than a two-hour walk (one way) from the nearest chief of the *chefferie* during the rainy season (QE13K). We find that *Tuungane* has a strong positive effect on the spreading of the health message especially in isolated areas. However, we again find evidence of a negative impact of *Tuungane* on wealth. In less isolated areas household income (Table 30) is lower in *Tuungane* areas compared to non-*Tuungane* areas. In isolated areas the quality of the wall (Table 34) is lower in *Tuungane* areas compared to non-*Tuungane* areas.

Item 14 Heterogeneous treatment by province, violence and wealth level

		by province				violence		wealth	
Item #	Measure	Haut Katanga	South Kivu	Maniema	Tanganyika	high	low	high	low
		N=137	N=147	N=65	N=106	N=206	N=192	N=247	N=151
Participation	Attendance at meetings (Table 3)	☆	☆	☆	☆	☆	☆	☆	☆
	Number of interventions in group discussions (Table 4)	☆	☆	☆	#	☆	☆	☆	☆
	Dominance of men, elders or chiefs in discussions (Table 4)	☆	☆	☆	#	☆	☆	☆	☆
	RAPID committee and projects selected through participatory approach (Table 6)	☆	☆	#	☆	☆	☆	☆	☆
	Committee composition mean effects (Table 8)	☆	☆	☆	☆	☆	☆	☆	☆
	Public goods provision in village (Table 9)	☆	☆	☆	☆	☆	☆	☆	☆
	Duties of political leaders (Table 10)	☆	☆	☆	☆	☆	☆	☆	☆
	Freedom to participate (Table 11)	☆	☆	☆	☆	☆	☆	☆	☆
	Obligations for participation (Table 12)	☆	☆	☆	☆	☆	☆	☆	☆
Accountability	Presence of mechanisms (Table 13)	☆	☆	☆	☆	☆	☆	☆	☆
	Willingness to complain (not conditional on project quality) (Table 14)	☆	☆	☆	☆	☆	☆	☆	☆
	Willingness to complain conditional on poor project implementation (Table 15)	☆	☆	☆	☆	☆	☆	☆	☆
Efficiency	Existence and quality of accounting (Table 16)	☆	☆	☆	☆	☆	☆	☆	☆
	Efficiency of information flow (health message) (Table 17)	☆	☆	☆	☆	☆	☆	☆	☆
	Village support sought from external actors (Table 18)	☆	☆	☆	☆	☆	☆	☆	☆
Transparency	Knowledge of project amounts (Table 19)	☆	☆	☆	☆	☆	☆	☆	☆
	Willingness to seek information (Table 20)	☆	☆	☆	☆	☆	☆	☆	☆
Capture	Financial irregularities (Table 21)	☆	☆	☆	☆	☆	☆	☆	☆
	Number of beneficiaries (Table 22)	☆	☆	☆	☆	☆	☆	☆	☆
	Inequality of the distribution of benefits (Table 23)	☆	☆	#	☆	☆	☆	☆	☆
	Dominance of special villagers' preferences (Table 24)	☆	☆	☆	☆	☆	☆	☆	☆
Cohesion	Trust in others (Table 27)	☆	★	☆	☆	☆	★	☆	☆
	Presence of social cleavages in the village (Table 28)	☆	☆	☆	☆	☆	☆	☆	☆
	Inter-village sharing (Table 29)	☆	☆	☆	☆	☆	☆	☆	☆
Welfare	Household income (Table 30)	#	☆	☆	☆	☆	#	☆	#
	Productivity (hours) (Table 31)	☆	☆	☆	☆	☆	☆	☆	☆
	Agricultural productivity (Table 32)	☆	★	☆	☆	☆	☆	☆	☆
	Household assets (Table 33)	☆	☆	☆	☆	☆	☆	☆	☆
	Quality of housing (Table 34)	☆	☆	☆	☆	☆	☆	☆	#
	School attendance (Table 35)	☆	☆	☆	☆	☆	☆	☆	☆
	Sickness (Table 36)	☆	☆	☆	☆	☆	☆	#	☆
	Frequency of use of village services (Table 37)	☆	☆	☆	☆	☆	☆	☆	☆

Note: A hypotheses receives a black star if we find evidence of a positive (adverse) estimated effect of *Tuungane* (95% significance in a two-tailed test).

Item 15 Heterogeneous treatment by cohesion, new inhabitants and NGO presence and isolation

		Cohesion		New inhabitants		NGOs		Village isolated	
Item #	Measure	High	Low	Yes	No	High	Low	Yes	No
		N=186	N=212	N=119	N=231	N=105	N=293	N=199	N=199
Participation	Attendance at meetings (Table 3)	☆	☆	☆	☆	☆	☆	☆	☆
	Number of interventions in group discussions (Table 4)	☆	☆	☆	☆	☆	☆	☆	☆
	Dominance of men, elders or chiefs in discussions (Table 4)	☆	☆	☆	☆	☆	☆	☆	☆
	RAPID committee and projects selected through participatory approach (Table 6)	☆	☆	☆	☆	☆	☆	☆	☆
	Committee composition mean effects (Table 8)	☆	☆	☆	☆	☆	☆	☆	☆
	Public goods provision in village (Table 9)	☆	☆	☆	☆	☆	☆	☆	☆
	Duties of political leaders (Table 10)	☆	☆	☆	☆	☆	☆	☆	☆
	Freedom to participate (Table 11)	☆	☆	☆	☆	☆	☆	☆	☆
	Obligations for participation (Table 12)	☆	☆	☆	☆	☆	☆	☆	☆
Accountability	Presence of mechanisms (Table 13)	☆	☆	☆	☆	☆	☆	☆	☆
	Willingness to complain (not conditional on project quality) (Table 14)	☆	☆	☆	☆	☆	☆	☆	☆
	Willingness to complain conditional on poor project implementation (Table 15)	☆	☆	☆	☆	☆	☆	☆	☆
Efficiency	Existence and quality of accounting (Table 16)	☆	☆	☆	☆	☆	☆	☆	☆
	Efficiency of information flow (health message) (Table 17)	☆	☆	☆	☆	☆	☆	★	☆
	Village support sought from external actors (Table 18)	☆	☆	☆	☆	☆	☆	☆	☆
Transparency	Knowledge of project amounts (Table 19)	☆	☆	☆	☆	☆	☆	☆	☆
	Willingness to seek information (Table 20)	☆	☆	☆	☆	☆	☆	☆	☆
Capture	Financial irregularities (Table 21)	☆	☆	★	☆	☆	☆	☆	☆
	Number of beneficiaries (Table 22)	☆	☆	☆	☆	☆	☆	☆	☆
	Inequality of the distribution of benefits (Table 23)	☆	☆	☆	☆	☆	☆	☆	☆
	Dominance of special villagers' preferences (Table 24)	☆	☆	☆	☆	☆	☆	☆	☆
Cohesion	Trust in others (Table 27)	☆	☆	☆	☆	☆	☆	☆	☆
	Presence of social cleavages in the village (Table 28)	☆	☆	☆	☆	☆	☆	☆	☆
	Inter-village sharing (Table 29)	☆	☆	☆	☆	☆	☆	☆	☆
Welfare	Household income (Table 30)	☆	#	☆	#	☆	☆	☆	#
	Productivity (hours) (Table 31)	☆	☆	☆	☆	☆	☆	☆	☆
	Agricultural productivity (Table 32)	☆	☆	☆	☆	☆	☆	☆	☆
	Household assets (Table 33)	☆	☆	☆	☆	☆	☆	☆	☆
	Quality of housing (Table 34)	☆	#	☆	☆	☆	☆	#	☆
	School attendance (Table 35)	☆	☆	☆	☆	☆	☆	☆	☆
	Sickness (Table 36)	#	☆	☆	☆	☆	☆	☆	☆
	Frequency of use of village services (Table 37)	☆	☆	☆	☆	☆	☆	☆	☆

Note: A hypotheses receives a black star if we find evidence of a positive (adverse) estimated effect of *Tuungane* (95% significance in a two-tailed test).

7.2.2 *The impact of Tuungane by individual characteristics*

In this section we look at the differential impact of *Tuungane* depending on the type of individual. The results are presented in Item 16, below. The top row provides the number of observations for the individual level regression of public goods provision in village (Table 9), to give an indication of the size of the different subgroups. Again, this number will change somewhat for different measures due to lack of data. Because we look at individual outcomes, Item 16 does not include those measures that are taken at the village level, such as attendance at meetings (Table 3).

By wealth ownership: With its goal of inclusion, *Tuungane* might have had an impact especially on those that are poor. We investigated this by identifying three groups: rich, poor and moderate. We base these groups on data from question QE9. Our poor people category includes those people who are classified by the World Bank as living in extreme poverty, specifically those living on less than US$1.25 (in purchasing power parity) per day.[27] 'Rich' people are defined as those who are the 20 per cent richest in the sample, and the 'moderate' group consists of all those people in between these two categories. We do not find that *Tuungane* has had an impact on poor people. Within this subgroup we only find that, compared to control communities, people in villages that received *Tuungane* (Table 27) exhibit higher levels of trust. Among those people of moderate wealth we find that *Tuungane* leads to an increased level of information flow (Table 17). Finally, we find no difference between the richer individuals living in *Tuungane* areas and the richer individuals living in control areas.

By level of education: We obtained information about the individual's level of education from QF13, and defined this to be low if the individual has obtained primary school or lower (including not schooled at all). We find no evidence of effects of *Tuungane* on those individuals with low education. Moreover, for those individuals with high education we find evidence that *Tuungane* had adverse wealth effects. Highly educated individuals in *Tuungane* areas have less overall assets (Table 33) and worse wall quality (Table 34) than those not living in *Tuungane* areas. *Tuungane* also had a negative impact on household income (Table 30) for this group, but this result is only significant at the 10 per cent level.

By gender: We make use of the gender of the respondent (QF7), despite the fact that many outcome measures are at the household level. Again, if anything, we find a negative impact of *Tuungane* on levels of wealth. Among women, *Tuungane* seems to have had a negative impact on household levels of income. Among men, *Tuungane* is associated with negative effects on health outcomes.

[27] DRC's purchasing power parity is equal to 214.27 (World Bank 2008). That is, a Congolese person would pay 214.27 Congolese francs for a set of goods that would have a value of US$1 for a similar set of goods in the United States. Because we ask the total amount of money earned by the household in the week preceding the interview, the cut-off value is: 214.27 x 1.25 x 7 = 1874.86 Congolese francs.

Item 16 Heterogeneous treatment by individual level characteristics

		Individual wealth			Education		Gender	
Item #	Measure	rich	moderate	poor	high	low	male	female
		N=686	N=1347	N=1508	N=766	N=685	N=1859	N=1780
Participation	[Report on] Public goods provision in village (Table 9)	☆	☆	☆	☆	☆	☆	☆
	Duties of political leaders (Table 10)	☆	☆	☆	☆	☆	☆	☆
	Freedom to participate (Table 11)	☆	☆	☆	☆	☆	☆	☆
	Obligations for participation (Table 12)	☆	☆	☆	☆	☆	☆	☆
Accountability	Willingness to complain (not conditional on project quality) (Table 14)	☆	☆	☆	☆	☆	☆	☆
	Willingness to complain conditional on poor project implementation (Table 15)	☆	☆	☆	☆	☆	☆	☆
Efficiency	Efficiency of information flow (health message) (Table 17)	☆	★	☆	☆	☆	★	☆
	Village support sought from external actors (Table 18)	☆	☆	☆	☆	☆	☆	☆
Transparency	Knowledge of project amounts (Table 19)	☆	☆	☆	☆	☆	☆	☆
	Willingness to seek information (Table 20)	☆	☆	☆	☆	☆	☆	☆
Capture	Dominance of special villagers' preferences (Table 24)	☆	☆	☆	☆	☆	☆	☆
Cohesion	Trust in others (Table 27)	☆	☆	★	☆	☆	☆	☆
	Presence of social cleavages in the village (Table 28)	☆	☆	☆	☆	☆	☆	☆
	Inter-village sharing (Table 29)	☆	☆	☆	☆	☆	☆	☆
Welfare	[Reports on] household income (Table 30)	☆	☆	☆	☆	☆	☆	#
	Productivity (hours) (Table 31)	☆	☆	☆	☆	☆	☆	☆
	Agricultural productivity (Table 32)	☆	☆	☆	☆	☆	☆	☆
	[Reports on] household assets (Table 33)	☆	☆	☆	#	☆	☆	☆
	[Reports on] quality of housing (Table 34)	☆	#	☆	#	☆	☆	☆
	[Reports on] school attendance (Table 35)	☆	☆	☆	☆	☆	☆	☆
	[Reports on] sickness (Table 36)	☆	☆	☆	☆	☆	#	☆
	Frequency of use of village services (Table 37)	☆	☆	☆	☆	☆	☆	☆

Note: A hypothesis is marked with # if there is evidence of adverse effects of *Tuungane* (95% significance in a two-tailed test).

8. Conclusions and recommendations

We have described a wide-reaching examination of the first stages of the *Tuungane* programme and its effects across a range of outcomes. Our research confirms that the programme succeeded in implementing a large number of projects in the *Tuungane* areas, that the projects were largely in line with the preferences of populations and that populations reported very high levels both of exposure to project activities and satisfaction with the outcomes of the project.

However, the evidence for impact is very weak. A summary of results is provided in Table 38, indicating where positive effects existed as well as the significance of findings and pointers to evidence of possibly adverse effects. On most measures we failed to find evidence that the positive experience with *Tuungane* led to behavioural changes. In general, we found that, for many local governance measures, outcomes were relatively strong in both control and treatment areas. For example, close to half of all committees were selected through electoral procedures. Yet the likelihood of using elections was nearly as great (and statistically no different) in non-*Tuungane* and in *Tuungane* areas. Levels of transparency were also similar in both groups. As part of RAPID, villagers were told that at least US$900 would be made available in funding. In fact US$1,000 was provided to project leaders. On our return we found, however, that 40 per cent of the general population knew that the final figure was US$1,000, not US$900. This figure was, again, almost identical in *Tuungane* and non-*Tuungane* areas. Of the US$1,000 allocated to communities, an average of US$150 was not traceable by our audit teams. Again, this rate was nearly identical in treatment and control areas. One area where we did find effects is in the gender composition of RAPID committees. These had stronger women's representation in *Tuungane* areas (see Table 8). The substantive size of this effect is weak, however. There is also evidence that citizens in *Tuungane* areas are more likely to complain when funds are misused by leaders, suggesting some fostering of bottom up accountability (see Table 15).

In examining social cohesion we find weak positive evidence on a survey-based measure of trust (see Table 27) but no effects on other measures. Estimates of welfare effects are weak across the board and trend negative on some items. This pattern of null findings is found also when we conduct robustness tests employing a set of variations in approaches to estimation that are likely to provide more precise estimates.

We also provided a small set of results that made use of the variation in design introduced in *Tuungane* I to assess the importance of requiring gender parity as part of the formation of community committees. Our first finding here is that, even without the requirement, villages select about 30 per cent women members. This may reflect attitudes towards the role of women or it may reflect features of the general emphasis on gender inclusion as part of *Tuungane*. We believe the latter explanation accounts, at least in part, for this pattern, since our examination of a similar choice as part of RAPID, outside of the *Tuungane* process, suggested considerably lower shares of women in RAPID committees, compared with *Tuungane* committees in areas without the gender parity requirement. The presence of women on committees in areas without mandated parity suggests that, at least within the context of the CDR programme, the requirement is not needed to ensure some representation, but rather it serves to increase the number of women. Its effects then are quantitative rather than qualitative. It generally does not introduce a new practice in local communities that might motivate replication by the population.

There is, on the other hand, some evidence that the inclusion requirement affects project selection. In particular, we see a greater focus on education rather than watsan in parity areas. While there are many factors that induce communities to choose particular projects against others (feasibility, for instance), the difference in itself can only be explained by the requirement and not by contextual factors or feasibility constraints, since the parity requirement was randomly introduced and the same constraints are, on average, present in parity and non-parity areas. However, whereas the difference in project choice does not reflect differences between the stated preferences of men and women in the general population, it is (weakly) reflected in differences in stated preferences between male and female VDC leaders. We find, however, no positive evidence of changes in attitudes towards the roles and responsibilities of women either as a result of *Tuungane* or, specifically, as a result of the gender requirement, and indeed the evidence points weakly towards adverse effects on this item. Taken together, this evidence does not suggest that the imposition of gender parity requirements is an effective way to strengthen the position of women in this context.

Table 38 Summary table

Item #	Measure	Evidence of adverse effect?	Support for hypothesis?
Participation	Attendance at meetings (Table 3)	★	★★★★
	Number of interventions in group discussions (Table 4)	★	★★★★
	Dominance of men, elders or chiefs in discussions (Table 4)	★	★★★★
	RAPID committee and projects selected through participatory approach (**Table 6**)	★	★★★★
	Committee composition mean effects (Table 8)	★	★★★★
	Public goods provision in village (Table 9)	★	★★★★
	Duties of political leaders (Table 10)	★	★★★★
	Freedom to participate (Table 11)	★	★★★★
	Obligations for participation (Table 12)	★	★★★★
Accountability	Presence of mechanisms (Table 13)	★	★★★★
	Willingness to complain (not conditional on project quality) (Table 14)	★	★★★★
	Willingness to complain conditional on poor project implementation (Table 15)	★	★★★★
Efficiency	Existence and quality of accounting (Table 16)	★	★★★★
	Efficiency of information flow (health message) (Table 17)	★	★★★★
	Village support sought from external actors (Table 18)	★	★★★★
Transparency	Knowledge of project amounts (Table 19)	★	★★★★
	Willingness to seek information (Table 20)	★	★★★★
Capture	Financial irregularities (Table 21)	★	★★★★
	Number of beneficiaries (Table 22)	★	★★★★
	Inequality of the distribution of benefits (Table 23)	★	★★★★
	Dominance of special villagers' preferences (Table 24)	★	★★★★
Cohesion	Distributions to migrants (Table 26)	★	★★★★
	Trust in others (Table 27)	★	★★★★
	Presence of social cleavages in the Village (Table 28)	★	★★★★
	Inter-village sharing (Table 29)	★	★★★★
Welfare	Household Income (Table 30)	#	★★★★
	Productivity (hours) (Table 31)	★	★★★★
	Agricultural productivity (Table 32)	★	★★★★
	Household assets (Table 33)	★	★★★★
	Quality of housing (Table 34)	★	★★★★
	School attendance (Table 35)	★	★★★★
	Sickness (Table 36)	★	★★★★
	Frequency of use of village services (Table 37)	★	★★★★
Gender	Attitudes towards women (Item 11)	★	★★★★

Note: A hypothesis receives one black star if the overall estimated effect goes in the expected direction but the effect is not statistically significant at conventional levels (that is, we cannot rule out that it is due to chance), and two, three or four stars if these effects are also significant at the 90%, 95% or 99% level (one-tailed test). Flags for adverse effects are provided in cases where a negative result would be considered significant in a two-tailed test at the 95% level.

Thus, overall the results on the impacts of the programme on the wellbeing and the attitudes and behaviours of populations are surprisingly negative. While there is in general little evidence of adverse effects, the evidence for positive effects is scattered and generally weak.

One possible reason for these null effects is that there are indeed no effects, or very weak effects, across the board. On the other hand, *Tuungane* might have had a different impact on different types of people and in different types of villages. To examine such heterogeneous effects, this report analysed the impact of *Tuungane* within a large set of different groups at individual and village levels. We find little evidence for any positive effects by *Tuungane* in any of these subgroups. If anything, we find evidence for an adverse impact by *Tuungane* on wealth. In particular, our measures for household income (Table 30) and quality of housing (Table 34) – two widely used indicators for levels of wealth – are negative and statistically significant in respectively seven and five of the 23 subgroups.

Another theoretical possibility is that the null effects are due simply to low statistical power. Underpowered studies (with small numbers of observations) are more likely to produce false negatives. Power is particularly weak in the presence of measurement error. While in future analyses we intend to increase precision by introducing prespecified controls, overall we do not believe that the problem here is one of power. This is a large study and our design is able to estimate relatively small effects. In many cases, moreover, the non-significant results (and the significant results) are associated with substantively small estimates.

Other design features may matter, however. Item 17 provides a characterisation of two other families of explanation. One focuses on the research design and the other on the intervention design. We discuss these in turn.

First: explanations related to the level of analysis. In particular, it is possible that the primary effects of *Tuungane* are on leaders in communities – for example, those who took part in trainings directly. If this is the case, the research is not well calibrated to capture these effects. Another possibility is that these effects operate through the particular institutions created by *Tuungane*, such as the various committees established. If this were the case then our focus on natural units would miss these effects. However, as described in the section What programme compontents are studies?, the *ex ante* expectation was that the programme would have general effects.

Item 17 Accounting for null effects

	Research	Intervention
Level	Research focused on general populations but perhaps the treatment had strongest impacts on community leaders only. Perhaps treatment had impacts at VDC or CDC level but not village level.	Treatment should target governance at levels higher than local communities.
Outcomes	Research measured governance in unstructured environments, but perhaps treatment affected governance in more structured environments only. Perhaps measurement of outcomes is flawed.	The treatment did not effectively address fundamentals – such as the material distribution of power.
Scale	Measures were taken too soon after completion of VDC projects. Perhaps block randomisation strategy led to risks of spillovers.	The treatment was too small and too short.

A second possibility is that the research focuses on the wrong outcomes. Perhaps, for example, the programme improves the ability of communities to function effectively in partnership with NGOs or in other more structured environments. This is possible but, since the interest was in examining more fundamental effects, the *ex ante* plan focused intentionally on unstructured settings in which communities largely determine the ways in which they resolve collective problems. It is also possible that we suffer from weak measures. This possibility has to be assessed on a measure by measure basis but we do note that, while our measures do not find evidence of treatment effects, they do capture other patterns one might expect. Thus, for example, our measures for wealth show that leaders are wealthier than others, our measures for empowerment suggest that men have more influence than women, and our measures for complaints and capture suggest that citizens are more likely to complain in villages in which we assess capture.

Third, there may be issues related to temporal and geographic scale. The timing of the research was plausibly too soon for economic effects to kick in. We think this less of a problem for the social effects, however, which we might expect to decline over time.[28] Finally, as discussed in the section Threats to validity, the geographic pattern of treatment assignment could produce vulnerabilities to spillover effects. As discussed in that section, we think this unlikely to be important in this context. Initial analysis presented in Section 7, Further analysis, also suggests that this might not be critical although, given our design, it is a feature that can be examined more directly in future analysis. A second family of explanations focuses on the intervention itself. Again, these could operate in terms of level of intervention, the outcomes selected and the scale of operation.

One possibility is that the basic approach may be effective but that this particular intervention was simply too small in scale. Although the temporal and spatial coverage of the *Tuungane* project is great relative to comparable interventions, the *per capita* investments, both in terms of finances and training, are small. They are considerably smaller than those in other CDR programmes for which positive economic effects have been found, and are negligible compared to interventions in developed areas. Similarly, four years may be too short a span to have a noticeable effect on social norms and behaviour.

A second possibility is that the intervention was pitched at the wrong level. While there seems little doubt that governance problems are endemic in DRC, it is less clear that the key problems are operating at the village level. In the areas under study, traditional leaders often enjoy considerable legitimacy. Moreover, their authority does not imply that local decision making is authoritarian. Fearon *et al*. (2009) note a similar argument for social cohesion. Social divisions are often important in developing areas but the divisions of political significance are often between community divisions, which are largely unaddressed by CDR Interventions, and not within community divisions. If this is right it suggests a need to make the level of intervention a more central consideration in the design of these projects. For interventions working at the most grassroots level, this argument suggests that it may be more fruitful to try to build on existing local governance capacity, rather than seeking to change it.

[28] One possibility that we have not examined is that social effects depend on the realisation of economic effects.

A third possibility is that the outcomes – changes in governance structures – are not susceptible to changes from interventions of this form. Existing structures can be resilient and while behaviour may change temporarily to meet the conditions of development actors, more profound change may require changes to fundamentals, such as the power bases of different actors or the distribution of wealth within communities.

More research can and should be done (including with existing data) to shed light on which of these explanations is most important in this context and to check further the robustness of the null results found here.

While recognising the need for continued research, however, we feel the null effects reported here provide a challenge to the CDR model, at least to the extent to which it is employed as a vehicle for social change rather than as a vehicle for delivering development aid. We have examined a major intervention that coupled strong implementation with a robust research design. The design employed an optimal approach for assessing causal effects and made use of innovative behavioural measures that directly captured the outcomes that were deemed most important *ex ante*. Despite these features the evidence of impact is very weak. The lack of evidence of effect is particularly striking for the social outcomes. Coupled with null findings emerging from other studies, these results suggest the need for a reassessment of the appropriateness of the CDR model and the scale and settings in which it is likely to be effective.

Appendix A: Sample

The *Tuungane* project is implemented at two levels: at village development committee areas (VDCs), and at community development committee areas (CDCs). First, a set of lowest-level units (LLUs) – natural settlements with fewer than 2,000 inhabitants (thus, sometimes villages, sometimes sub-villages, sometimes *quartiers*) – is gathered together by IRC/CARE staff into VDCs with approximately 2,000 inhabitants. Sets of VDCs are then aggregated into 560 CDCs with approximately 6,000 inhabitants in each CDC. CDCs are in turn aggregated into a set of lottery bin areas that contain anything from two to 30 CDC areas, depending on local geography. Approximately 50 per cent of the CDCs in each lottery bin are selected for treatment using a public lottery approach. Thus, if a single CDC area is selected for treatment, so are all the VDC areas within that CDC area.

In total there are over 5,000 LLU areas (lowest-level units in sampling frame) and the research team is unable to gather data on all of these. The team's strategy is to collect data on two LLUs in every CDC (both treatment and control) for a total of 1,120 study LLUs. LLUs are selected using systematic random sampling, with probability proportional to size from the set of LLUs, in order to ensure that each selected LLU is drawn from distinct VDCs whenever possible. Survey data is collected in each of these 1,120 LLUs. In addition, behavioural data (described in more detail below) is gathered in 560 of these LLUs (one from each CDC area).

Figure 1 provides an illustration of the set of units and their relation to each other. The distribution of measure types is shown in Table 39.

Table 39 Distribution of measures across units

		Haut Katanga		Sud Kivu		Maniema		Tanganyika		Total
		T	C	T	C	T	C	T	C	
LLUs surveyed	Behavioural measures taken	74	74	75	74	74	73	57	59	560
LLUs surveyed	No behavioural measures taken	74	74	75	74	74	73	57	59	560

Note: T stands for treatment (*Tuungane*) and C for control.

Appendix B: Survey instrument(s)

All survey instruments can be found in our Outcomes and Data Sources document. In this section we discuss in more detail the measurement strategy, including the survey instruments used.

The reliability of the lessons learned from this research depends not just on the strategy used to attribute effects to the programme, but also on the strategy to measure outcomes. Since community-driven reconstruction (CDR) programmes seek to affect social outcomes, they confront specific measurement challenges. In particular, it can often be difficult to determine just from responses to survey questions whether there have been real changes in attitudes and behaviour. Recent evaluations of CDR programmes have thus found the use of behavioural measures to be a stronger and less ambiguous method of measurement than relying solely on survey measures. These considerations motivated the use of the RAPID project as a mechanism to assess direction effects on behaviour.

The use of an intervention as a measurement strategy gave rise to a number of considerations. One was how best to handle the consent process, given that the intervention was both a real project and a tool for research. For this we opted for an approach in which we identified the link with research at the outset. Consent was sought first for the project component, conditional on the ability of audit data to be used for research purposes. Consent was then sought at the village for gathering more general measures (such as observation of meetings), allowing in principle for villages to accept the project but refuse individual and village level measurement elements. For individual surveys, consent was sought on an individual basis in the usual way. The text used to describe the process is shown in Item 18.

Item 18 Description of RAPID to communities (script used by teams during general assembly)

I work for RAPID and I want to talk with you about a project that we are introducing in this village. RAPID stands for Research-Action through Projects for Development Impacts. The project provides development funding from the British government and is coordinated with researchers from Columbia University in New York and from the universities of Bukavu and Lubumbashi. The aim of the project is to provide development aid to your community while at the same time contributing to scientific research to better understand your priorities and needs.

Your village and other villages were selected in a lottery involving all the villages in this territory for the programme. The programme will provide a grant of at least US$900 (perhaps more) in international funding to implement a quick impact project. In this project we will let the community decide how best to use the funds.

Your chief [name] gave us permission to hold this meeting as a prerequisite for participation in the project. The aims of this meeting are to inform you of the programme, to provide you the opportunity as members of the village to ask us any questions about the project and to offer a forum for discussion on development priorities in this village and use of these funds.

There are a few requirements for participation in this project and it is important to us that you understand them:

1. First, we want the community to decide how to use the project funds. Following this meeting, your village will have seven days to decide how to use the funds. The total funding guaranteed for this community is at least US$900. It is up to you as a village to decide the best use of funds. There are no restrictions on the use of funds, except they must be used to benefit the community and be spent out by you in the next 50 days. For this reason we encourage you to use the funds to

assist members of the community through projects such as purchasing and distributing seeds, tools, large participatory work or other projects that support the wellbeing of this community. These funds may also be distributed to community members to use at their discretion. We prohibit the use of these funds to purchase any item whose purpose is to harm others.

2. Second, we are asking the community to identify people to represent the village for this project. These individuals will be responsible for carrying out the accounting of the use of these funds. It is up to the community to decide who these people will be over the next seven days. You are free to choose any person or persons that you feel are most appropriate to act as representatives
3. Third, we ask you to complete this form [show the form] to return it on [date]. It is the project description form. [Show form BP1]. I will leave it with you today to complete over the next six days. The information in the form will contain the decisions you have made for the project. A representative of Project RAPID will return in six days to collect this form. We will not be able to make the grant payment if you do not complete this form.
4. Fourth, among the questions I ask you to fill out on the form are: Who are the individuals who will be responsible for managing these funds? Which project has the community chosen? What is the budget of such a provisional project?
5. Fifth, we ask that, in two months, representatives of the community for the Project RAPID provide us with an accurate accounting of the usage of funds, with evidence. This is to facilitate our understanding of the priorities of your village, as part of our research.
6. Finally, in accepting this project you also accept that the use of Project RAPID funds will be subject to an audit. What will this look like? We will send teams to implement an audit in certain villages participating in the programme. If this village is audited, we will examine what the village has done with project funds. The findings will contribute to our study of the needs of eastern DRC.

Information on the disbursement of funds will be provided when collecting project description forms from the representatives chosen by the community for the management of funds. Following receipt of these funds, your village will spend out these funds for your chosen project over the next seven weeks (49 days), as is compatible with the project.

Do you have questions about this process? Would you like to participate in this project?

As we said before, there is a research component linked with this project. It is important for us that you have a good understanding of what is involved in this research so that you can use that understanding to either agree or refuse to take part in it. As this project is implemented we will seek to hold a series of interviews with members of this community. These interviews will all be anonymous. The aim of these is to understand the community's priorities. It is important that you understand that if you choose to be interviewed your responses will be kept anonymous.

Another part of our research will be on decision making during community meetings. Collecting measures during discussions helps us to understand more about this community and its priorities. Again, we will only do this if the community agrees to this and, in all cases, the recording of information will be done in a way that conserves anonymity.

Before asking for your consent we want to note that this research does not bring risks, but nor does it bring direct benefits for you. By improving our understanding of community priorities in eastern Congo this research seeks to contribute to an improvement in the quality of development aid throughout the area.

Do you consent to us collecting this data to help with this research?

To protect villages we, unusually, agreed in advance that our partners, the IRC, CARE and DFID would not receive detailed village-identifiable information on the performance of communities.

A second consideration was how to identify teams in order to avoid inducing social desirability biases. For this we took efforts so that the research was not associated with the IRC or CARE International. The teams introduced themselves to the villages as affiliated with the Official University of Bukavu (in Maniema and South Kivu) or the University of Lubumbashi (Haut Katanga and Tanganyika), and the project RAPID was implemented by their respective universities in cooperation with Columbia University in

New York City and was funded by the British government. Although we sought to minimise any connection with IRC and CARE, we also adopted a policy of no deception. If respondents asked directly about IRC or CARE involvement, team members acknowledged their involvement, emphasising their role in disbursing funds. The IRC and CARE employees, who would visit villages to distribute the project funds, were assigned to areas in which they had not worked previously so that they would not be identified as staff by populations.

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Your chief [name] gave us permission to hold this meeting as a prerequisite for participation in the project. The aims of this meeting are to inform you of the programme, to provide you the opportunity as members of the village to ask us any questions about the project and to offer a forum for discussion on development priorities in this village and use of these funds.

There are a few requirements for participation in this project and it is important to us that you understand them:

7. First, we want the community to decide how to use the project funds. Following this meeting, your village will have seven days to decide how to use the funds. The total funding guaranteed for this community is at least US$900. It is up to you as a village to decide the best use of funds. There are no restrictions on the use of funds, except they must be used to benefit the community and be spent out by you in the next 50 days. For this reason we encourage you to use the funds to assist members of the community through projects such as purchasing and distributing seeds, tools, large participatory work or other projects that support the wellbeing of this community. These funds may also be distributed to community members to use at their discretion. We prohibit the use of these funds to purchase any item whose purpose is to harm others.
8. Second, we are asking the community to identify people to represent the village for this project. These individuals will be responsible for carrying out the accounting of the use of these funds. It is up to the community to decide who these people will be over the next seven days. You are free to choose any person or persons that you feel are most appropriate to act as representatives
9. Third, we ask you to complete this form [show the form] and return it on [date]. It is the project description form. [Show form BP1]. I will leave it with you today to complete over the next six days. The information in the form will contain the decisions you have made for the project. A representative of Project RAPID will return in six days to collect this form. We will not be able to make the grant payment if you do not complete this form.
10. Fourth, among the questions I ask you to fill out on the form are: Who are the individuals who will be responsible for managing these funds? Which project has the community chosen? What is the budget of such a provisional project?
11. Fifth, we ask that in two months representatives of the community for the Project RAPID provide us with an accurate accounting of the usage of funds, with evidence. This is to facilitate our understanding of the priorities of your village, as part of our research.
12. Finally, in accepting this project you also accept that the use of Project RAPID funds will be subject to an audit. What will this look like? We will send teams to implement an audit in certain villages participating in the programme. If this village is audited, we will examine what the village has done with project funds. The findings will contribute to our study of the needs

of eastern DRC.

Information on the disbursement of funds will be provided when collecting project description forms from the representatives chosen by the community for the management of funds. Following receipt of these funds, your village will spend out these funds for your chosen project over the next seven weeks (49 days), as is compatible with the project.

Do you have questions about this process? Would you like to participate in this project?

As we said before, there is a research component linked with this project. It is important for us that you have a good understanding of what is involved in this research so that you can use that understanding to either agree or refuse to take part in it. As this project is implemented we will seek to hold a series of interviews with members of this community. These interviews will all be anonymous. The aim of these is to understand the community's priorities. It is important that you understand that if you choose to be interviewed your responses will be kept anonymous.

Another part of our research will be on decision making during community meetings. Collecting measures during discussions helps us to understand more about this community and its priorities. Again, we will only do this if the community agrees to this and, in all cases, the recording of information will be done in a way that conserves anonymity.

Before asking for your consent we want to note that this research does not bring risks, but nor does it bring direct benefits for you. By improving our understanding of community priorities in eastern Congo this research seeks to contribute to an improvement in the quality of development aid throughout the area.

Do you consent to us collecting this data to help with this research?

Instruments

During these steps a wide array of instruments was used to gather the measures.

Item 19 gives an overview of the major instruments (names refer to their acronym in French). These instruments are referenced under each table of results in this report. All instruments can be found in the project ODS document.

The instruments used a variety of approaches to gather data. Some forms (such as AM and AD1, and AD2) recorded direct observations by enumerators of events in the villages, such as the form of discussion taking place. Some, such as the auditing forms, gathered data from investigations such as inspection of projects or assessments of prevailing prices and accounting forms. Some recorded enumerator conclusions from open-ended discussions with village decision makers, while others recorded subject responses to closed questions asked either of focus group (DA) or, more commonly, individuals (DML). In some cases, multiple approaches were used to gather the same information, such as focus groups with community decision makers led by one enumerator and one on one interviews held at the same time with decision makers and random villagers by other enumerators.

The ODS document provides a mapping of each of these instruments and items to the core hypotheses. In this report we introduce each particular item as it is used. However, a number of innovations in measurement that will be used for later analyses bear special mention.

Item 19 Data collection instruments

Step	Form	Description	RAPID	Survey only
A	ASS	The research was conducted in villages with populations of 200-2,000. If a village was too large a random sub-unit was selected. Form ASS was used to do this.	Yes	No
A	ACA	Recording of consent of the village chief prior to implementation of RAPID activities.	Yes	No
A	AL	Within each RAPID village 10 people were randomly selected for the survey from a list (AL) that included all the households of the village. AL was created with the chief and other informed people.	Yes	No
A	AS	AS provides information about which households from AL were selected and their location – including GPS coordinates.	Yes	No
A	ALM	Within each household one member was randomly selected for the survey. ALM listed the members of the household from which one person would be randomly selected.	Yes	No
A	AV	Household survey gathering data on the development priorities for the five households selected in AL.	Yes	No
A	AM	Information about the village meeting – including measures such as attendance rates.	Yes	No
A	AD1 and AD2	Information on discussion dynamics during the general assembly.	Yes	No
A	AVR	A summary of the RAPID project that was left in each village.	Yes	No
A	BP1	The form used by villages to collectively communicate their preference for a project, who would be part of the RAPID committee *etcetera*.	Yes	No
A	APM	Random number table to select 10 village meeting participants for the survey AP1.	Yes	No
A	AP1 and AP2	Short surveys conducted with 10 randomly selected participants, plus the three most vocal, the three most influential (as judged by enumerators) participants and the chief.	Yes	No
A	AC	A more in-depth survey with the chief also took place in each RAPID village.	Yes	No
A	AA	Enumerators form to ensure completion of tasks and to summarise village dynamics.	Yes	No
B	B	A survey to learn about the composition of the RAPID committees. This included how the committee members are related to each other and information about the suggested project. In addition, both the Provincial Supervisor and the Animator conducted focus groups (the Animator with several randomly selected villagers and the Supervisor with the committee).	Yes	No
B	BP2 and BF	The committee filled in a form indicating the project and how they would spend the US$1,000. An official document (BF) was signed.	Yes	No
	B-TF	Formal receipt of transfer of US$1,000.		
C	C	This form kept track of who was present during the distribution of the money.	Yes	No
D	DSS, DL, DS, DLM	Analogies of ASS, AL, AS and ALM for non-RAPID villages. That is, survey-only villages that were not part of the RAPID project.	No	Yes
D	DML	Long endline survey: implemented in five households in each village.	Yes	No
D	DMC	Short endline survey: implemented with the five households that took part in Step A.	Yes	Yes
D	DINFO	In two out of five households that received the DML survey the respondent was asked whether he or she would – given minimal incentives – collect information about the annual budget for the local school or the health centre. Enumerator information on the local school/health centre is recorded in DINFO.	Yes	No

D	DCDV	Two members of the *Tuungane* VDC committee were interviewed in *Tuungane* treatment villages.	Yes	Yes
D	DC	An in-depth interview was conducted with the chief of each village.	Yes	Yes
D	DR	In-depth surveys were also conducted with two randomly selected RAPID committee members.	Yes	No
D	DA	Auditor form. DA includes information from interviews with focus group of RAPID committee members, information from visiting 10 randomly selected beneficiaries, the visit to the local market to learn whether the prices correspond to those in the accounting forms *etcetera*.	Yes	No
D	DP	These forms recorded whether the respondents selected for the behavioural question returned and whether he or she provided the correct information about the school or health centre.	No	Yes
D	DM	During Step D a general assembly took place and modes of participation were recorded.	Yes	No
D	TEL	Form given to respondents with codes and phone numbers to which they could report their opinion about their *chefferie* and territory leaders – and about the RAPID implementation (in RAPID villages).	Yes	Yes

First, during the meeting with the RAPID committee at Step B, the RAPID teams informed the committees that RAPID would implement an audit on the use of the funds. At this stage we introduced variations in communications on how audit data would be used – whether results of the audit would be shared with the village population and/or with the international community. With this variation we seek to assess whether interventions such as *Tuungane* result in elites that are more accountable to their populations or, instead, whether they create elites that are more accountable simply to the international community.

Second, during Step A of the RAPID process we provided a random sample of households with a set of simple facts about diarrhoea and how to avoid it. For a subset of these we emphasised the public benefits of hand washing. In Step D we visited both RAPID villages (in which some households had received information) and survey-only villages (where no information was disseminated). In all villages we conducted the DML survey with individuals that did *not* receive the direct dissemination, in order to assess the degree of health information transmission. This will allow an examination of transmission rates and how these depend on the extent to which benefits are seen as public or private.

Third, after the household surveys in Step D we distributed information to respondents on how to send SMS messages to voice their opinions about their *chefferie* and *territoire* level governance, and about the RAPID project in RAPID villages. Subjects were informed that any message sent would be received by the RAPID project, would be considered anonymous and would be collated and shared with the programme, *chefferie* and *territoire* leaders. Our expectation was that if *Tuungane* has an impact on governance we would receive more such messages from *Tuungane* areas. Unfortunately, after several months we did not receive any message and we discontinued this component.

Another behavioural measure gathered during the course of survey implementation asked a subset of respondents to collect information about the budget of the local school or the health centre. The hypothesis is that, if there is greater transparency in the village, there should be greater willingness for villagers to seek this information. We report on this measure below.

Finally, as we implemented the Step D surveys, we introduced a set of variations to help assess data quality. Two variations respond to the concern that quality declines during the course of survey implementation. To assess this, one variation altered the ordering of questions asked and a second introduced a mandatory pause midway through. A second concern is that results are contaminated by social desirability bias. To investigate this we introduced a variation in which we asked respondents 'Do you agree with the idea that elections are the best way to choose community representatives for positions with technical responsibilities?' For one subgroup the question was preceded by the statement 'Many NGOs in the region think that elections are not the best way to choose community representatives when it comes to an appointment with technical responsibilities'. Another subgroup was told 'Many NGOs in the region are of the opinion that elections are always the best way to choose community representatives for technical posts'. Comparison of answers will allow us to assess the degree to which respondents seek to provide answers that they think NGOs want to hear.

A third concern relates to ordering effects within questions. On one set of question we asked respondents to give their position on a dimension anchored by two competing statements (one consistent and one inconsistent with programme values). To assess ordering effects we randomly vary the order in which anchors were presented. A final concern is reluctance to answer sensitive questions on corruption and violence. To address these issues we introduced a set of list experiments that provides a means to estimate population effects while protecting individual subjects.

In the field

The data collection effort was a massive undertaking implemented over the course of more than a year in a region in DRC the size of France. We provide a brief account of the logistics of data undertaking.

Research teams: Multiple teams were engaged in implementing RAPID and gathering outcome data. Each province had two teams for Step A, each consisting of a project facilitator and an assistant. 'A' teams were responsible for introducing the project to the village chief and to the village during a general assembly and for conducting a set of surveys (to be discussed in more detail below). One B team in each area visited the villages a week after team A, was responsible for meeting the committee, and conducted focus groups to learn how both the committee and the RAPID project were chosen. This team included the provincial supervisor who had a satellite phone in order to call the IRC or CARE International headquarters so that a C team could visit the village to distribute the RAPID project funds. Disbursement was done by IRC or CARE staff but without identifying themselves as such. Approximately 48 days later, after the implementation of the project, both RAPID and non-RAPID villages were visited by D teams. D teams in RAPID villages included three enumerators and one auditor. The latter did a detailed investigation into how RAPID grants were spent and, where applicable, located beneficiary populations. The survey-only villages consisted of only two enumerators. In addition to A, B, C and D teams, each province had two super-assistants – one responsible for Steps A to C and one for Step D. Super-assistants visited teams to collect and back up data, photos and GPS coordinates, and ensured quality control. These staff were hired and directed by leads at the universities of Bukavu and Lubumbashi. Finally, there were two regional evaluation coordinators, hired by the IRC – one based in Bukavu (South Kivu) and one in Lubumbashi (Haut Katanga). They were responsible for supervision of implementation, and monitoring the data collection and its quality. These coordinators were in daily contact with the Columbia University research team and worked closely with the research and evaluation coordinator of the IRC. Between June and December 2010 two of the authors were based in DRC to launch the project.[29]

Conditions: Because of the lack of public transport the evaluation purchased 20 motorbikes to provide faster and more reliable transportation to villages. But, in practice, teams often had to walk or push bikes over great distances. Since teams often did not reach major centres for weeks, we relied on PDAs to gather and move data. Each team member had a PDA and each team leader had a backup PDA, and all were equipped with solar chargers and large quantities of AA batteries. The harsh conditions produced great costs to enumerators, with high incidence of sickness, including malaria

[29] In addition to those seven months, one author was present during March-April 2011 and again in January-February 2012, another in June-July 2011, and a colleague from the London School of Economics was dispatched to the area between July-December 2011. These visits took place to guarantee the quality of the research.

and cholera. Although safety regulations were in place in all areas, one of the teams was involved in a tragic accident in which a child died.

Security: High levels of insecurity, especially in South Kivu, also mark the area where the research took place. The security of the teams was a major concern throughout and teams were not allowed to visit a village before receiving security clearance from the IRC's security team. The latter had contact with the major actors such as the United Nations peacekeeping forces, the DRC government and others. Despite the precautions undertaken we did encounter some security issues. Thirty-one villages were not visited due to security risks. One team was ambushed and had to hand over their equipment and one IRC staff member was abducted (and subsequently released unharmed) during the implementation of Step C. In particularly risky areas of South Kivu, Step C was undertaken through accounts in local credit offices (COOPECs), rather than having cash delivered by a field agent.

Appendix C: Power calculations

Our initial power calculations assumed a model of the form:

$$Y_{ij} = \alpha + \beta T_j + u_j + \varepsilon_{ij}$$

where T_j is an indicator for whether or not an observation in village j receives treatment, $u_j \sim N(0, \tau^2)$ is a village specific error and $\varepsilon_{ij} \sim N(0, 1)$ is an individual specific error. We estimated the power associated with identifying a treatment effect for different values of β for τ^2 set at 1/3, a conservative estimate based on Liberian data. Since we normalised the variance of the individual specific errors to 1, all other quantities are interpreted in units of this error. The results shown below provided power estimates for different effect sizes for the main effect, for variation in treatment, as a function of the number of respondents per community and for first wave data.

We estimated that the statistical power associated with our survey strategy was likely to be sufficient to pick up even relatively small effects for the main analysis. Power is weaker for the variation in treatment analysis but is still likely to be sufficient for identifying medium to large effects. As can be seen from the top two panels, there are substantial gains from moving from one to two clusters per site (on the order of a 33 per cent difference in minimum effect size for which we have 90 per cent power). Power for subgroup analysis is weaker (power for subgroup analysis from two clusters per location is expected to be similar to that for main analysis for one cluster per location).

The analysis also highlighted the gains from moving from two to five respondents per village (and the relatively weak additional gains from moving to eight respondents).

Our ultimate target was to gather data on 1,120 villages, for which we would have RAPID data for 560 and survey data for the full 1,120. Different targets were set for different items but the most common data (the household survey) was to be gathered from five households in survey-only villages and for 10 households in RAPID villages. Given that there were 560 RAPID villages and 560 non-RAPID survey-only villages this makes a total of 8,400 households (for some items gathered only in RAPID, or only in survey-only areas, the targets were 2,800).

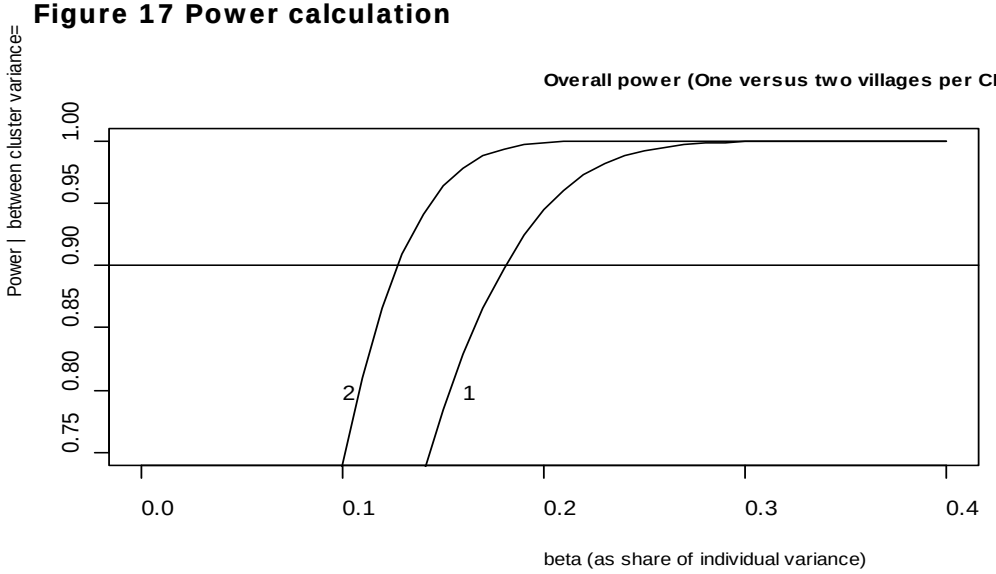
In the event, the survey teams successfully collected final (Step D) data on 72 per cent of villages and 62 per cent of individuals (see Item 20 and Item 21), with somewhat higher numbers gathered for Steps A and B. The full complement of targeted data was not gathered for a number of reasons.

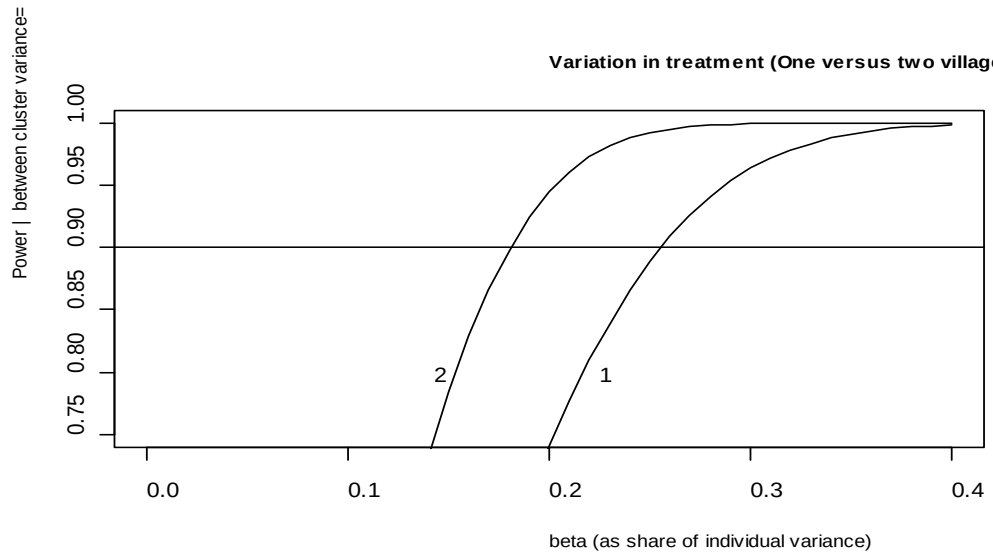
Item 20 Data availability and targets: village count

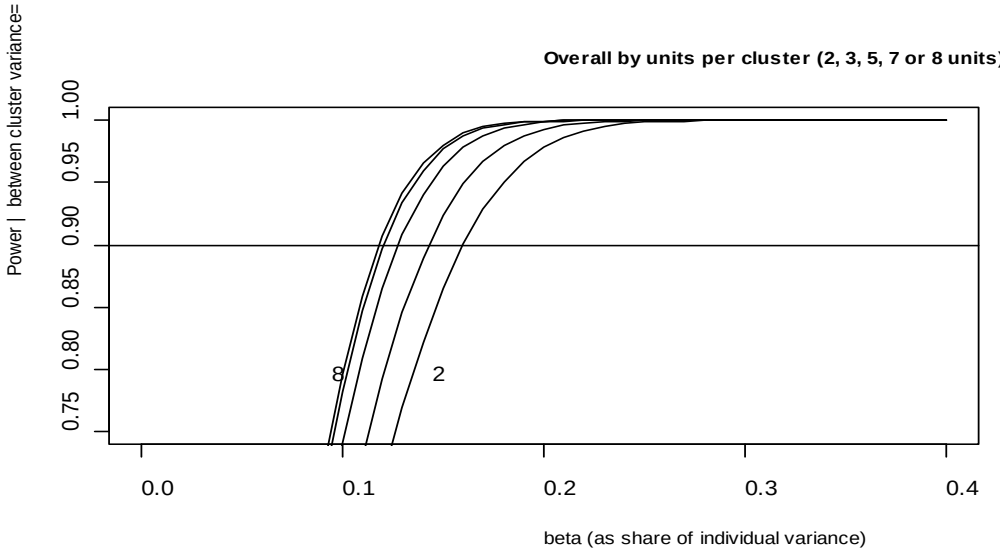
	Survey only	RAPID + Survey	All
Control	199 (280)	204 (280)	403 (560)
Tuungane	195 (280)	208 (280)	403 (560)
All	394 (560)	412 (560)	806 (1,120)

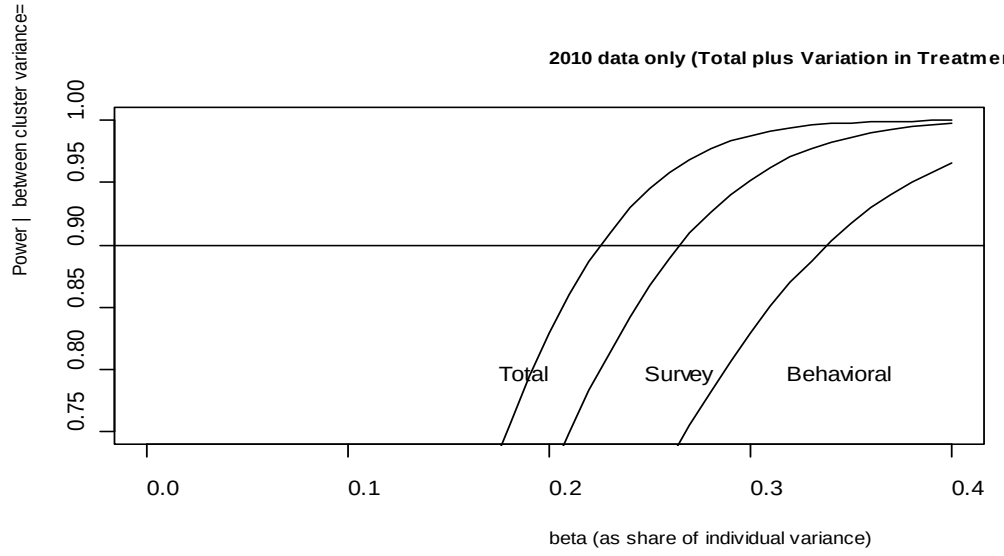
Note: Table shows number of villages for which final survey was implemented. Numbers in parentheses are the target number of villages (completion rates are higher for Step A and B measures).

Figure 17 Power calculation









The most significant source of missing data resulted from data loss in Maniema, one of the three provinces. Political tensions in the run up to the elections led to the expulsion of the Maniema teams shortly after launch of Step D. This led to the loss of 89 per cent of the RAPID villages and 89 per cent of the survey-only villages for all measures based on Step D or involving a combination of steps (the data loss was greater for Step D than for Step A and Step B data, which were more advanced at the time of the expulsion)[30]. This loss covered entire lottery bin areas, affecting treatment and control units alike. While it affects the range of areas to which our results can speak, as well as our statistical power, we do not think that this loss is plausibly related to the treatment status of units and could induce bias.

A second significant source of missing data is the inaccessibility of some regions for safety and security reasons. Such losses account for 36 village losses outside Maniema, with balance between RAPID and survey-only villages. However, since these also affect clusters of regions containing both treatment and control areas in nearly equal amounts, we believe they are not plausibly related to treatment status.

The third loss of data is due to various failures in the field that can range from loss, damage or theft of PDAs, water damage to paper surveys or enumerator error in the implementation of surveys or particular questions. Given the difficulty of the environment, this third category is relatively small, affecting a total of 7 per cent of surveys in surveyed villages. This loss is also not related to treatment status. The geographic distribution of missing data due to these three sources is illustrated in Figure 18.

Item 21 Data availability and targets: survey count

	General population	Chief	VDC member	Total
Control	2,741 (4,200)	362 (560)	0 (0)	3,103 (4,760)
Tuungane	2,732 (4,200)	350 (560)	482 (1,120)	3,564 (5,880)
All	5,473 (8,400)	712 (1,120)	482 (1,120)	6,667 (10,640)

Note: Table shows number of individual for which final survey was implemented (and for whom there is non-missing gender information). Numbers in parentheses are the target number of villages.

The fourth source of data loss is non-response on particular items by subjects, although again we have not found evidence that absence of data is associated with treatment status.

A final concern is that non-responses lead to bias of various forms. An examination of household survey data suggests that 2,200 of the 5,473 households, visited for the endline survey, were replaced by neighbouring houses. The major reasons were that: 617 were empty; 712 did not have any individual of the indicated gender; 95 refused for any member of the household to be interviewed and 25 did so in a way considered

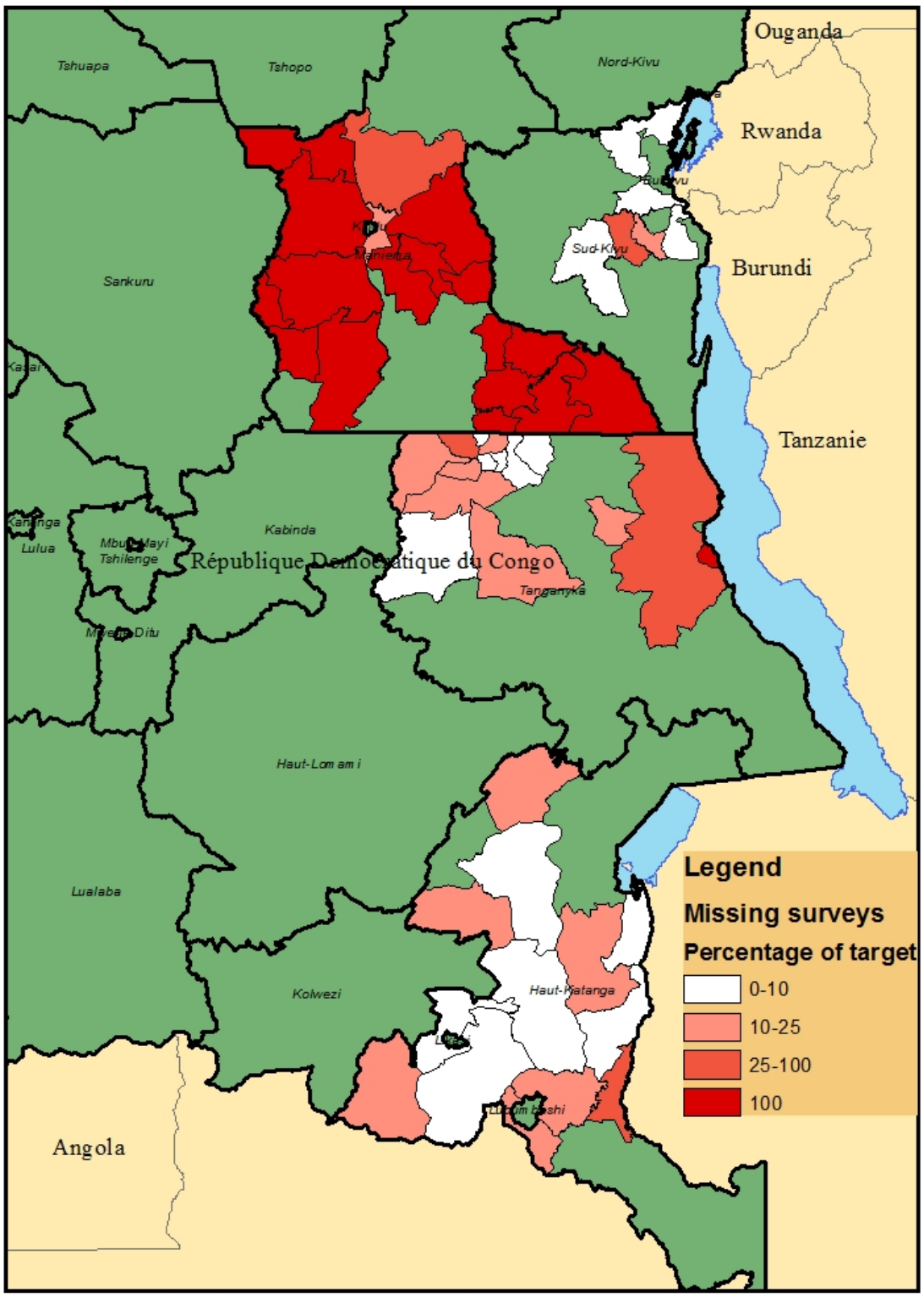
[30] A total of 62/147 RAPID villages received step A, a total 7/147 RAPID villages received step D. The same number of survey-only villages received step D.

hostile by enumerators; and 360 households were not found by the survey teams. These non-responding households were split nearly evenly between treatment and control units, suggesting that household missingness is not correlated with treatment. The implication of this need to replace households is that individual level results should be interpreted as reflecting the attitudes of individuals in accessible households.[31]

Overall, we think it unlikely that the data loss introduced biases, and our calculations and results suggest that the decision to move to more clusters and a larger respondent set largely protected us from the loss in power due to missing observations.

[31] And more precisely of accessible individuals in accessible households.

Figure 18 Distribution of missing data by *chefferie*



Source: Referentiel Geographique Commun (administrative boundaries)

Appendix D: Study design and methods

The use of randomised interventions for assessing impacts[32]

At the heart of any good assessment of impact is the strategy to answer the question 'what would have happened in this programme area if there had not been a programme?'. Simple before and after comparisons are unfortunately not enough because, even if there are improvements over time in the programme areas, it will be difficult to know whether these improvements are actually due to the project. One might find that since the programme began, things in the programme communities improved and thus conclude that the programme must be the cause. Yet further investigation could show that conditions improved in all communities during the time the programme was running, programme and non-programme communities alike. This might be because of something completely unrelated to the programme, such as improving economic conditions or a change in government. For this reason too, simply talking to beneficiaries of a project is not enough. Though the perspective of beneficiaries is fundamentally important, gathering these perspectives does not provide sufficient information to know how beneficiaries fared relative to non-beneficiaries.

The key design challenge for a strong evaluation is to identify a good control. A good control group should be in all ways identical to the treatment group except for the fact that it did not receive treatment. This is called balance.

Balance is hard to achieve. For example, in some cases project designers choose particular sites as treatment sites precisely because of some positive features, such as the receptiveness of populations. In such cases, the principle of balance is violated and the evaluation will not be able to tell whether differences in outcomes are really due to the programme or whether they are instead due to fundamental differences between populations. Factors like the receptiveness of populations in this example are confounding factors, factors that are correlated with both the likelihood that a unit receives treatment and the outcomes for that unit. Confounding factors complicate the researcher's ability to assess the effectiveness of the treatment because they make it difficult to tell if differences between treated and control group outcomes are due to the treatment or to the confounding factor.

One might address the problem of balance by trying to think of all the confounding factors and compare only those treated and control cases that are very similar on these factors. In that way one could plausibly claim that the only major difference between the treated and control cases is the fact that one received the treatment and the other did not. This technique is called matching. While the matching approach is superior to one that does not factor in the systematic differences between treated and control cases, the approach suffers from important shortcomings. First, the analyst must think of *all* relevant confounding factors that may affect the outcome of the treatment, a daunting task and one that is constrained by the depth of knowledge and the imagination of the researcher. Second, the analyst must obtain measures of all of these confounding factors. Unfortunately, in many cases these factors may be unobserved so that measures are unobtainable.

[32] This section draws on text from Columbia CSDS, 2010. *Evaluation Strategies.* http://www.csds.columbia.edu/documents/CSDS_Evaluation_Strategies.pdf

The optimal way, from a learning perspective, to identify a good control group is through the method of randomised intervention. Essentially, the process of randomised intervention works as follows: if there are 100 people who will receive some treatment and 200 people who are eligible to receive the treatment, then 100 people are chosen randomly from the group of 200 eligible people and assigned the treatment. All 200 people, however, are tracked. The fact that the 100 are chosen randomly means that, in expectation, there is no systematic difference between those that did and those that did not receive the treatment – the only systematic difference lies in the treatment itself. Since our treated cases are drawn randomly from the whole sample, treated and control cases are just as likely to bear any particular confounding characteristic and, if the sample size and number of treated cases is large enough, the treated and control cases will on average be very similar. The beauty of randomisation, combined with a large enough sample, is that it renders our treated and control cases similar on average, even on factors that are unobserved and even on factors that the researcher might not have thought of but that could have been relevant confounds nonetheless.

Often when researchers present evidence for the effect of a programme, critics ask, 'But did you control for this or that?' or 'But how do you take account of all of the unique features of each unit?' The great advantage of a randomised evaluation approach is that one has always controlled for everything in the sense that there are no third factors that are systematically related to treatment. As a result, the findings do not depend on the idiosyncrasies of the treated units but on what they have in common – exposure to the treatment[33].

Randomisation in the *Tuungane* project

The randomised selection of communities into *Tuungane* took place through a series of public lotteries. As described above, in each area potential communities were identified in advance as areas in which *Tuungane* might work and were grouped into constructed communities – the CDCs. Then collections of neighbouring CDCs were gathered together into lottery bins from which project communities were to be drawn. In general, lottery bins corresponded to *chefferies* or *secteurs* (for the sake of simplicity, we generally use the term *chefferies* for both units) or parts of *chefferies*.[34] Representatives from all the potential project communities came together for the lottery, were told briefly about the project and were able to witness the actual selection of communities (generally done by drawing names out of a hat).

We thus should not expect differences between control and treatment groups. To test this, Item 23 lists the average for a set of key variables (that are plausibly related to outcomes even though we do not expect them to be related to treatment) by treatment status. We emphasise that these variables were prespecified. There are indeed no differences across these two groups, which are consistent with what is to be expected given the random assignment.

[33] Technically, randomisation ensures that there is balance of this form in expectation. Especially when one has a small number of units there can be a risk that third variables are correlated with treatment and outcomes by chance. It is quite possible that the treatment works differently in different areas. If this is true, the randomisation approach nevertheless succeeds in returning the average treatment effect.

[34] *Chefferies* and *secteurs* are administrative units that rank below province and district and *territoire*; in our study area there are 33 of these units.

Item 22 Balance

Variable	*Tuungane*	Control	Difference	N
Distance from major urban centre	8.99	8.99	0.00	804
Population size of village	488.35	469.08	18.37	457
Prior level of poverty	3.19	3.41	0.22	710
Exposure to conflict	2.43	2.44	.00	992
Existence of prior NGO activity	.43	.42	–.01	992
Gender	0.51	.50	.00	5,539
Age	39.26	39.73	.47	5,409
Education	4.35	4.35	.00	3,978
Migration status	.46	.47	.01	3,733

Note: Based upon the following measures: QE13E, AC11, QC23-27, CQ39 (2007 baseline survey), CQ68 (2007 baseline survey), QF7, QF9, QF13, SP1. Exposure to conflict and existence of prior NGO activity have been aggregated to the *chefferie* level. Comparing treatment and control communities, taking into account weights and clustering, gives the same result.

Public lotteries have a set of normative advantages as well as some statistical advantages and limitations. The chief normative advantage is that they provide a limited form of informed consent on the part of communities, both those that benefit from the programme and those that do not. Control communities learn that they could have been a part of the programme and all communities learn that there is a learning component to the interventions. A second, more programmatic advantage is that there is transparency over the selection process and reduced concerns that one or other project was being unfairly favoured. A research advantage of selecting communities in this way is that, within each area, there is good geographic balance. In terms of the number of treated and control areas, this minimises the chances that treatment communities all end up clustered in one area and control communities in another. More complex procedures to ensure balance are, however, difficult to implement with public lottery schemes of this form. The flip side of this balance is a somewhat reduced ability to estimate spillover effects, since clusters of treatment and control villages are contiguous, and there is limited variation in geographical distance between treatment and controls. A final concern might be that awareness of the intervention among control communities could lead to jealousy, which could in principle lead to biased results if those communities started performing more strongly or more weakly as a result of not being chosen.

Our survey data allows us to assess the extent to which individuals understood the selection process. We asked a set of survey respondents (who had heard of *Tuungane*) in treatment and control areas how they thought communities were chosen (see Item 23). In treatment areas, 59 per cent of those responding reported that the villages were chosen by chance. Divine intervention was the next most common answer. Few gave traditional explanations such as favouritism by government or NGOs.

We asked the same question about *Tuungane* of individuals in non-*Tuungane* areas. Patterns in control areas were largely similar, although in these areas the vast majority of respondents had not heard of *Tuungane* or had no explanation for why it went to other places and not to them. A small number of respondents (eight) offered alternative explanations. These were:

- these villages have many representatives in government
- these villages have indigenous populations that can influence where programmes go
- someone spoke on behalf of those villages to the NGOs

- people in the project office were able to influence outcomes
- there are people from those villages who work in *Tuungane* and they were able to influence the choice
- I think someone asked for the project to come
- because of their representatives in government
- someone of good faith made it come.

A couple of respondents emphasised that it was pure chance:
- our luck wasn't good
- it was just chance.

Seventy-five per cent reported that they thought the process fair (83 per cent among chiefs and 72 per cent among the population). In control areas, 86 per cent of chiefs thought the process fair, compared to 81 per cent of the general public and 93 per cent of VDC members.

Item 23 Beliefs about selection process

Beliefs about how villages were selected:	*Tuungane* areas	Control areas
Random	59%	36%
God	28%	14%
Village reputation	5%	5%
Chief influence	3%	5%
Government influence	1%	2%
NGO decision	6%	7%
Village needs	5%	5%
Number responding:	1,760	447

A final feature of the public lotteries approach has implications for analysis. In practice, targets (number of CDCs to be selected for the programme) were set for each bin and in general these targets were close to 50 per cent. Nevertheless, the exact targets vary between bins, sometimes because of integer problems (in some three-village bins, just one village was selected, in others, two) and sometimes because of the programmatic needs to have larger numbers of treated CDCs in different regions. The result is that not every unit has the same propensity to enter the programme. In other words, units in different bins were selected with different probabilities (but units in a given bin were selected with the same probabilities). Comparing raw outcomes in treatment and control CDCs would produce a biased estimate of the effect of treatment, since treatment CDCs for bins where many communities were selected into treatment would be over represented, distorting the comparison of outcomes. We take account of this fact by applying inverse propensity score weights to every unit, reflecting the inverse of the share of units from each lottery bin that were targeted for treatment, and hence the inverse of the probability that this particular unit was selected.[35]

[35] If p_j is the probability of being assigned to treatment then the inverse propensity weight is $1/p_j$ for treatment units and $1/(1-p_j)$ for control units. In practice, targets were set so that there were

What programme components are studied?

As described in the section What did Tuungane do?, the social interventions took place at the village (VDC) level, while much of the economic investment activity took place subsequent to these social interventions and at the community (CDC) level. The research focuses squarely on the first component, the VDC projects, and the timing of the research, illustrated in Figure 19, below, reflects this focus. There are principled and pragmatic reasons for focusing on the VDC component. The principled reason is that, at this level, all the major social interventions took place and these components are broadly seen as the key innovative components of the *Tuungane* design. By implementing research after the implementation of the social interventions but before the implementation of the major CDC projects, the concerns of a complex treatment (conflating economic and social interventions, though still present) are mitigated somewhat. The practical reason is that the CDC projects were to be followed almost immediately by a new round of *Tuungane* II interventions, including new social interventions, and it would not have been possible to implement data collection between these rounds of projects.

The first implication of this for interpretation of the results, particularly the welfare results, is that the study does not capture effects of the CDC projects, which financially account for a very large share of the *Tuungane* I envelope. The second implication is that the project was, in some sense, live during the measurement phase. Thus, insofar as respondents felt that the measurement was associated with the programme (as described below, care was taken to minimise this risk), this may result in a social desirability bias in responses.

As far as possible we sought to time the research to be a set interval after the project start in a particular lottery bin (that is, the date of the lottery). Logistic concerns made it impossible to do this exactly but the final timing, as shown, in Figure 19, below, is largely consistent with this goal. To do this perfectly the data collection would have had to take place over more than two years. The implication for interpretation is that we seek to assess the impact of *Tuungane* three years out from project onset (the median gap between lotteries and the onset of research is 1,081 days with a standard deviation of 192 days).

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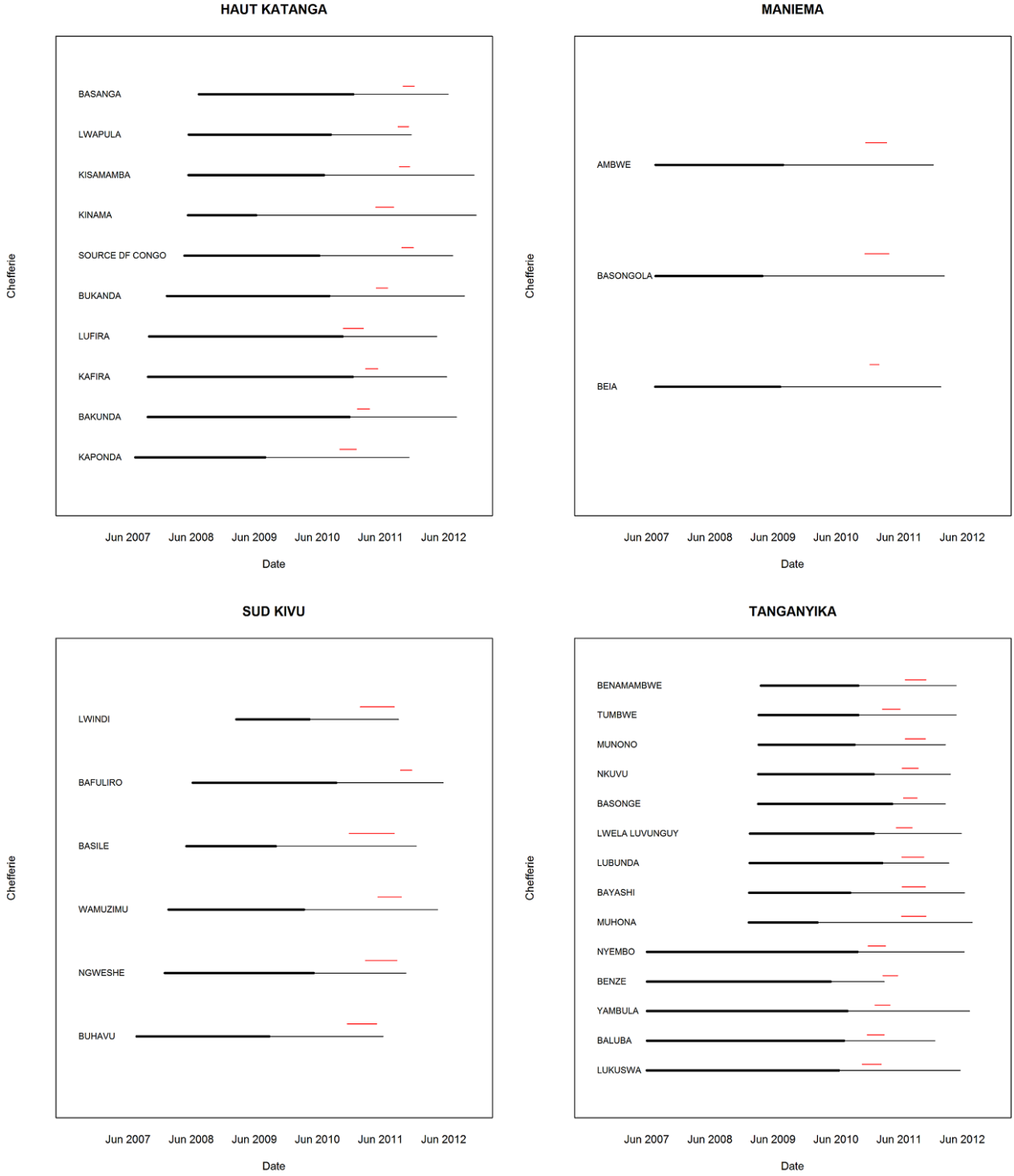
often more control units than treated units and so, to maximise efficiency in data collection, we under sampled from control CDCs and modified weights accordingly. Thus if, for example, there were three units in a bin and one were assigned to treatment, the propensity score weights would be 3 on the treated unit and 3/2 on the control units. If, in practice, we randomly selected only one control unit for research then the weights on control units would be 2 x 3/2 = 3, resulting in identical weights for the one treatment and one control unit in our sample.

Figure 19 Timing of research implementation relative to project implementation



Note: The thick black line shows the period of implementation of the first stage of *Tuungane*, including the implementation of VDC projects. The continuing thinner black line shows the period of implementation of CDC projects. In each case lines are marked by the start and end dates of the median VDC in a *chefferie* (or, in urban areas, *secteur*). The black lines show the median duration of the research, from the median start date in a *chefferie* to the median end date.

A second feature of the design bears emphasis. The research sought to examine social effects of *Tuungane* and for this we need to specify the level at which we believe social effects operate. In practice, it is not possible to examine effects at the level of VDC or CDC areas if only for the practical reason that these units have no meaning in the control areas. More substantively, outside of the context of the *Tuungane* programme, these units have no meaning in treatment areas either and so looking for effects at this level has unclear external validity. Instead we sought to measure effects primarily within LLUs at the level of small natural settlements (although some measures gather information on relations with other villages or with higher levels of government). The principle behind seeking effects at this level, or at levels other than the VDC and CDC, is that the programme works, not simply through the creation of particular institutions for particular groups (say a committee), but more abstractly through changing the values and practices of individuals, which then has an effect in multiple fora.

Ex ante hypotheses and changes to analysis

The results provided here are true hypothesis tests in the sense that hypotheses were developed *ex ante* (in 2007) and specified without reference to evidence on treatment effects. Moreover, the core analysis provided here was developed and coded by the research team without accessing actual data on treatment and circulated in advance in a mock report.[36] This differs significantly from an approach in which researchers craft precise questions inductively as they examine the data. The latter approach is much more likely to yield significant results, although the reliability of results generated in this way is weaker.[37]

This final report differs from the mock report in five ways, which we describe below:

At the request of IRC, two extra analyses are included. First, in Table 24, estimates of male dominance were added alongside estimates of chief dominance and, second, an assessment of the gender variation effect on attitudes towards women and governance was added as Section 6.4 Results: effects of gender parity variation. In neither case was the request based on knowledge of outcomes on these items.

Second, we altered the test on the effect of *Tuungane* on the propensity to complain conditional on funds missing. In our analysis plan we sought to estimate the marginal effect of *Tuungane* after accounting for the effect of the share of funds missing on complaints (technically we looked for the marginal effect of *Tuungane*, controlling for funds missing). In the final analysis we sought to examine how *Tuungane* affects the propensity to complain in light of funds missing (technically we looked for the interactive effect of *Tuungane* and funds missing). This approach, we feel, is more faithful to the hypotheses being examined; however, we note that significant results were found under the revised approach but not under the original approach.

Third, the index on health information flows examined in Table 17 was changed to focus only on items that were provided to peers (excluding items provided uniquely to chiefs). This was to reflect the intention of the original measure but produces no substantive effect on results.

[36] See: http://cu-csds.org/projects/postconflict-development-in-congo/20110307_drc_registration/.
[37] See Ioannidis (2005) on *Why Most Published Research Findings Are False*, and Humphreys *et al.* (2012).

Fourth, for a number of complex tables we added summary analyses, generally mean effects analysis, as described in the Mean effects and average indices (in Section 5). These make for easier interpretation of the multiple results described in given tables. In particular, in situations where positive results are found on some items and not on others, the average effect gives a determination of whether the average effect is significant. This practice, used in Kling *et al.* (2006), has also been adopted in the analyses of other CDR programmes, notably in Casey *et al.* (2011), Fearon *et al.* (2011) and Beath *et al.* (2011).

Fifth, the final summary table (Table 38) reports summary significance of tables based on the significance of mean effects measures or indices, wherever these exist. This allows for simpler assessment of overall patterns. Thus, if some items register as significant but the average effect does not, the summary table reports no overall significant effect.

Independence of the research team

The IRC provided considerable support to this research and in particular shared databases, contracted with the universities of Lubumbashi and Bukavu to implement data gathering and provided funding for field costs for the Columbia University team (conversely Columbia University also provided funding through a sub-grant to IRC to support a share of the research costs). Two consultants hired by the IRC worked closely with the Columbia University team on oversight, sharing databases. The IRC and CARE staff implemented Step C of RAPID. Moreover, the IRC provided input at multiple points into the research design and analysis plan. Despite the overall engagement of the IRC and CARE, the research team was substantively independent. First, at no point during the research did any of the Columbia University team receive remuneration from the IRC or its partners for their work on this research. Second, although the IRC provided input into the analysis plan, they did so with no information on estimated effects. Third, although the IRC has provided comments on this report, by advance agreement, the IRC has had no editorial control over what is included and how results are interpreted.

Threats to validity

We identify a number of possible threats to the validity of the results presented here.

Attrition and missing responses

We discussed this in Appendix C.

Noncompliance

A second threat to the validity of the interpretations offered here is noncompliance, in the sense that areas that were selected by lottery to form part of *Tuungane* did not (and areas that were not selected, in fact, did form a part). From our survey data approximately one in seven chiefs either deny that *Tuungane* took place in a *Tuungane* area, or claim that it did take place when, according to records, it did not. These are expected to be lower than the direct effect of participating in *Tuungane,* since it includes villages that were selected and did not participate. For 229 possibly ambiguous cases, including all those with discrepancies between our data and chief reports, we asked IRC to confirm whether the project did or did not take place in these areas. IRC records of where *Tuungane* did take place matched our records of where *Tuungane* ought to have taken place in 77 per cent of cases. This suggests that the discrepancy is due either to weak impact, poor recall by chiefs or enumeration error. The check leaves 51 cases out of 806 of possible noncompliance and/or database error. For this analysis we use our

database measure of units selected by lottery, which, assuming our database is correct, can be interpreted as intent to treat effects (albeit with a very high compliance rate). In robustness tests we report results under the assumption that our databases are incorrect, that the IRC data is correct, and there is no failure of compliance. Our results are generally unaffected by this check.

Heterogeneity

As seen in Figure 19, there is heterogeneity both in the timing and length of project implementation, and the timing and length of data collection relative to project implementation. Broadly, the research schedule sought to follow the timing of the start data of implementation of *Tuungane* in each area, although the research schedule was more compressed. While the timing of project initiation spanned approximately two years (with the first lottery date being in July 2007 and the last in April 2009), the data gathering spanned approximately one year (with the first village that was visited with Step A of RAPID in October 2010 and the last villages visited for Step A in October 2011). Thus, in general, and by design of the research, areas that launched late also had a shorter lag between start and measurement. The median gap was 1,185 days, and 90 per cent of cases had a gap between 871 and 1,202 days. These timing decisions, however, all took place at the level of lottery bins, all units in lottery bin areas were first exposed to the project at the same time (although projects started at different times) and were visited by the research team at the same time, thus ensuring strong balance in timing issues between treatment and control areas at the bin level. The implication of this heterogeneity is that the results should be seen as the average of a set of experiments that varied in time to measurement.

Spillover effects

The final concern we note is that *Tuungane* may produce spillover effects across communities. If part of the effect of *Tuungane* was to improve outcomes in control areas, then this added contribution of the project would lead to estimates of smaller, rather than larger, programme effects.[38] Three features, however, suggest that this is not likely. First, assuming indirect effects are weaker than direct effects, strong treatment effects would result in smaller but still positive estimated effects. Second, in this case communities are comprised of clusters of villages, meaning that most treated villages are surrounded by treated villages and control villages by control villages. Third, populations in control areas reported very low levels of knowledge about *Tuungane*. In later analyses we hope to exploit features of the randomisation in order to further assess the plausibility of such spillovers. Finally, we have undertaken an analysis in which, rather than estimating the effect of participation in *Tuungane*, we estimate the effect of distance from the nearest *Tuungane* village (conditional on remoteness) and find that results are changed little.[39]

[38] Note that there are possible related concerns that control areas were directly treated by elements of *Tuungane*. For example, a small component of *Tuungane* involved radio programming that could be received by both control and treatment areas. Properly speaking, this is not a spillover but rather the application of a component of the programme across areas. Insofar as such programming has effects, the implication is that our results should be interpreted as the effects of the village level activities over and above any general effects that result from radio programming.
[39] In further work we can use a design-based, rather than modelling, approach, to seek further evidence on spillover effects.

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In recent years, community-driven reconstruction (CDR) programmes have aimed to promote local development in post-conflict settings. Despite their growing popularity, there is little rigorous evidence to indicate their effectiveness. To help plug this evidence gap, this impact evaluation used a randomised-controlled trial design to assess the impact of *Tuungane*, a CDR programme in the Democratic Republic of Congo. The findings suggest a need to reassess the effectiveness of the CDR model and focus on more fundamental questions. The International Rescue Committee, the implementing agency, has subsequently launched a programme of research to further investigate the implicit and explicit theoretical assumptions of CDR programming and intends to apply this learning to future programme and evaluation design.

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