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Community monitoring interventions to curb corruption and increase access and quality of service delivery in low- and middle-income countries

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About this review

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Community monitoring interventions to curb corruption and increase access and quality of service delivery in low- and middle-income countries: a systematic review

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Plain language summary

Community Monitoring Interventions can reduce corruption and may improve services

Community monitoring interventions (CMIs) can reduce corruption. In some cases, but not all, there are positive effects on health and education outcomes. Further research is needed to understand contexts and designs for effective interventions.

What did the review study?

Corruption and inefficient allocation of resources in service delivery are widespread in low- and middle-income countries. Community monitoring interventions (CMIs) are intended to address this problem. The community is given the opportunity to participate in monitoring service delivery: observing and assessing providers' performance to provide feedback to providers and politicians.

This review assesses the evidence on the effects of community monitoring interventions on corruption and access and quality of service delivery outcomes. The review also considers the mechanism through which CMIs effect a change in corruption and service delivery outcomes, and possible moderating factors such as geographic region, income level or length of exposure to interventions.

What studies are included?

To assess the effect on corruption included studies had to have either an experimental or a quasi-experimental design. Qualitative studies were included to assess mechanisms and moderators.

The review assesses 15 studies of 23 different programmes' intervention effects. The studies were conducted in Africa (6), Asia (7) and Latin America (2). Most studies focused on programmes in the education sector (9), followed by health (3), infrastructure (2) and employment promotion (1).

What is the aim of this review?

This Campbell systematic review assesses the effectiveness of community monitoring interventions in reducing corruption. The review summarises findings from 15 studies, of which seven are from Asia, six from Africa and two from Latin America.

What are the main results of this review?

Community monitoring interventions can reduce corruption. They also improve use of health services, but no significant effect is found on school enrolments or dropouts. There is no improvement in health service waiting times, but there is an improvement in weight for age, though not child mortality. There are beneficial effects on education outcomes as measured by test scores.

Community monitoring interventions appear to be more effective in improving outcomes when they promote direct contact between citizens and providers or politicians, and when they include tools for citizens to monitor the performance of providers and politicians.

In all cases, findings are based on a small number of studies. There is heterogeneity in the findings with respect to health and education. Hence it is difficult to provide any strong, overall conclusions about intervention effectiveness.

What do the findings of this review mean?

The evidence identifies CMIs as promising. That is, there is evidence that they are effective. But the evidence base is thin, the interventions do no work in all contexts, and some approaches appear more promising than others.

Future studies should assess the effectiveness of different types of community monitoring interventions in different contexts, sectors and time frames to identify when and how such programmes may be most effective in improving outcomes. There is a need for adequate information and tools to assist citizens in the process of monitoring. Research about these mechanisms and their moderation of the effectiveness of CMIs should be a priority for further research in the area.

How up-to-date is this review?

The review authors searched for studies published until November 2013. This Campbell systematic review was published in November 2016.

Executive summary

Background

In many low- and middle-income countries (L&MICs) corruption and mismanagement of resources are prevalent in the public sector. Community monitoring interventions (CMIs) aim to address such issues and have become common in recent years. Such programmes seek to involve communities in the monitoring of public service providers to increase their accountability to users. However, their effectiveness in reducing corruption and improving access and quality of services remain unclear.

Objectives

This review aims to assess and synthesise the evidence on the effects of CMI interventions on access to and quality of service delivery and corruption outcomes in L&MICs. More specifically, the review aims to answer three main questions:

- What are the effects of CMIs on access to and quality of service delivery and corruption outcome measures in L&MICs relative to no formal community monitoring or CMIs with less community representation?
- What are the mechanisms through which CMIs effect a change in service delivery and corruption outcomes?
- Do factors such as geographic region, income level or length of exposure to interventions moderate final or intermediate outcomes?

Search methods

We searched for relevant studies across a broad range of online databases, websites and knowledge repositories, which allowed the identification of both peer reviewed and grey literature. Keywords for searching were translated into Spanish, French, and Portuguese and relevant non-English language literature was included. We also conducted reference snowballing and contacted experts and practitioners to identify additional studies. We used Endnote software to manage citations, abstracts, and documents. First stage results were screened against the inclusion criteria by two independent reviewers, with additional supervision by a third.

Selection criteria

We included studies of CMI in countries that were classified as L&MICs according to the World Bank definition at the time the intervention being studied was carried out. We included quantitative studies with either experimental or quasi-experimental design to address question 1. In addition, both quantitative and qualitative studies were eligible for inclusion to address questions 2 and 3.

Data Collection and Analysis

Two reviewers independently coded and extracted data on study details, design and relevant results from the included studies. Studies were critically appraised for potential bias using a predefined set of criteria. To prepare the data for meta-analysis

we calculated standardised mean differences and 95 per cent confidence intervals (CI) for continuous outcome variables and risk ratios and risk differences and 95% CI for dichotomous outcome variables. We then synthesised results using statistical meta-analysis. Where possible we also extracted data on intermediate outcomes such as citizen participation and public officials and service providers' responsiveness.

Results

Our search strategy returned 109,017 references. Of these 36,955 were eliminated as duplicates and a further 71,283 were excluded at the title screening stage. The remaining 787 papers were included for abstract screening and 181 studies were included for full text screening. Fifteen studies met the inclusion criteria for addressing question 1. Of these, ten used randomised assignment and five used quasi-experimental methodologies. An additional six sibling papers were also included to address questions 2 and 3. Included studies were conducted in Africa (6), Asia (7) and Latin America (2). The 15 studies included for quantitative analysis evaluated the effects of 23 different CMIs in the areas of Information Campaigns (10), Scorecards (3), Social Audits (5), and combined Information campaigns and Scorecards (2). Most studies focused on interventions in the education sector (9), followed by health (3), infrastructure (2) and employment promotion (1).

Corruption outcomes

Included studies on the effects of CMI on corruption outcomes were implemented in infrastructure, education and employment assistance programmes. The overall effect of CMI as measured by forensic economic estimates in two studies suggest a reduction in corruption (SMD=0.15, 95% CI [0.01, 0.29).

Three studies (comprising four interventions) measured perception of corruption as an outcome measure. A meta-analysis of two of these studies showed evidence for a reduction in the perception of corruption among the intervention group (risk difference (RD) 0.08, 95% CI [0.02, 0.13]). Another study, which was not included in the meta-analysis due to a lack of comparability in outcome, suggests an increase in perceptions of corruption in the intervention group (SMD -0.23, 95% CI [-0.38, -0.07]).

Access to services

A number of different outcome measures were included as proxies for access to service delivery. One study examined the effects of an information campaign and a combined information and scorecard campaign on health care utilisation. The information campaign showed no significant effect in the short term, but the information campaign and score card combined resulted in an increase in utilisation both in the short term (SMD 2.13, 95% CI [0.79, 3.47]) and the medium term (SMD 0.34, 95% CI [0.12, 0.55]).

The overall effects of two CMI interventions on immunisation outcomes suggest a positive effect in the short term (Risk Ratio (RR): 1.56, 95% CI [1.39, 1.73]).

However, the medium term effect reported from one of these interventions is smaller and less precise (RR 1.04, 95% CI [-0.52, 2.61]). Another study reporting on a range of measures of access to health services suggests an overall positive effect (RR 1.43, 95% CI [1.29, 1.58]).

Meta-analysis of four studies which evaluated the effects of CMI on school enrolment showed an overall positive effect, but the estimate cross the line of no effect (SMD 0.09, 95% CI [-0.03, 0.21]). The overall effect across on drop-out across four studies is no different from zero (SMD 0.0, 95% CI [-0.10, 0.10]).

Quality of services

For health related interventions child death and anthropometric outcomes were considered proxies for quality of service. A meta-analysis of two studies which examined the short term effects of a score card and a combined score card and information campaign using child deaths as an outcome is not clear (RR 0.76 [0.42, 1.11]). For the score card and information campaign intervention data was available on the medium term effects and the estimate is similarly imprecise (RR 0.79, 95% CI [0.57, 1.08]). The average effect on weight for age, based on the same two studies, suggests an overall beneficial effect (RR 1.20, 95% CI [1.02, 1.38]). For the combined score card and information campaign intervention with data on medium term effects the results suggest the benefits were sustained (RR 1.29, 95% CI [1.01, 1.64]). The same two studies also looked at waiting times for services and the results suggest no difference in this outcome (RR 0.99, 95% CI [.80, 1.17]).

In education interventions test scores were used as a proxy outcome measure for quality of service. The overall effect across six studies was 0.16 (SMD, 95%CI [0.04, 0.29]).

The limited number of studies included in our review, and the limited number of included studies with information on intermediate outcomes in particular limited our ability to answer our second and third research questions regarding the mechanisms through which CMIs effect change and whether contextual factors such as geographic region, income level or length of exposure to interventions moderate final or intermediate outcomes.

Nonetheless, some exploratory evidence is provided in response to these questions, which may inform further research in the area. Some likely important moderators of the effect of CMI are having an accountability mechanism for ensuring citizen participation, availability of information and tools for citizens engaged in the monitoring process and pre-existing beliefs regarding the responsiveness of providers to citizen's needs

Authors' conclusions

This review identified and analysed available evidence regarding the effects of CMIs on both access to and quality of service delivery and on corruption outcome measures in L&MICs. Overall, our findings were heterogeneous making it difficult to provide any strong, overall conclusions as to the effectiveness of CMIs.

However, the results suggest CMIs may have a positive effect on corruption measures and some service delivery measures.

We found the overall effect of CMIs on both forensic and perception based measures of corruption to be positive. In improving access to public sector services results were more variable. Effects on utilization of health services are not clear, but we observe an improvement in immunization rates. In the education sector, we did not find evidence of an effect on proxy access measures such as school enrollment and dropout.

We used child anthropometric measurements and deaths and waiting times for services as proxy measures for service quality in the health sector and test scores in the education sector. The evidence from two studies suggests improvements in weight for height, but no difference in child deaths or in waiting times for services. The results suggest an improvement of quality of services, as measured by improvements in test scores.

Despite limitations in our ability to synthesise evidence on the mechanisms which moderate the effects of CMIs, some important preliminary evidence was uncovered. Firstly, we identified a lack of accountability in ensuring the involvement of citizens in CMIs as an important potential bottleneck to effectiveness. Secondly, we identified the need for adequate information and tools to assist citizens in the process of monitoring. Further research on these mechanisms and their moderating effect on the effectiveness of CMIs should be a priority for further research in the area.

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1. Background

1.1 Description of the problem

Corruption and inefficient allocation of resources in service delivery are widespread in low- and middle-income countries (Pande and Olken, 2011). There is increasing evidence that corruption holds back countries' economic development and erodes their citizens' quality of life (Mauro, 1995; Svensson, 2005; Singer, 2013). Millions of people around the world encounter administrative corruption in their daily interactions with public services. Using a 0-100 scale on perceived levels of public sector corruption, only a third of the 176 countries covered in the Transparency International Corruption Index 2012 scored above 50. The World Bank Institute estimates that total bribes in a year amount to about one trillion USD (Rose-Ackerman, 2004), making corruption account for around three per cent of world GDP (Svensson, 2005). Bribes are used to influence the actions of public officials, either to performed their duties, distort the duties or to prevent them from performing their duties. For instance, under the presidency of Fujimori in Peru, there is direct evidence in the form of signed receipts that politicians and judges received bribes ranging from 3,000 to 50,000 USD and the media received as much as 1.5 million USD per month for turning a blind eye to government malfeasance (McMillan and Zoido, 2004).

In many countries, corruption is widespread throughout the public sector, not only among high level public officials. Gorodnichenko and Sabirianova (2007) estimate the aggregate amount of bribes collected by low and medium level public officials in Ukraine to be between 460 and 580 million USD, about one per cent of its GDP. Administrative corruption imposes a heavy burden on citizens' and firms' time and resources. Olken and Barron (2009) estimate that 13 per cent of the cost of a truck driver's trip in Indonesia is allocated to pay bribes to police officials that they encounter on their journey. In cases where the accountability relationship between bureaucrats, frontline providers and politicians is broken, unofficial payments can be the only way to incentive those frontline providers to perform their duties. Svensson (2003) finds that bribes represent eight per cent of firms' production costs in Uganda. Corruption creates discontent with public services, undermines trust in public institutions (Sacks and Larizza, 2012; Singer, 2013), and stifles business growth and investment. Khwaja and Mian (2005) find that politically connected firms receive substantially larger loans from government banks in spite of having a 50 per cent higher default rate.

Resources needed to improve equality of opportunities and provide services for citizens are lost every day as a result of corruption and inefficiency (World Bank, 2003), which in turn results in inadequate provision of key services. Often, it is the poor and the vulnerable who suffer the most from public sector corruption (Olken, 2006; Sukhtankar, 2011). A landmark study in Uganda found that only 13 per cent of the public funds that the central government had assigned to the school system reached the intended destination (Reinikka and Svensson, 2004, 2005, 2011). Similarly, leakages are also a problem in Tanzania, where elected officials are the recipients of more than half of the total amount of subsidised fertilizer's price

vouchers (Pan and Christiaensen, 2012). In Indonesia, village officials hide their corruption by deflating quantities, that is, they claim to procure enough rock, sand, and gravel to build a road that is 20cm thick, but instead build a road that is only 10cm or 15cm thick. Since the roads they build are thinner than official engineering guidelines, they will not last nearly as long and will need to be replaced sooner (Olken, 2007; 2009). In India, the lack of monitoring and accountability has resulted in high levels of public sector absenteeism, with one quarter of all the teachers in public schools and more than a third of nurses and doctors being absent from their duties (Chaudhury *et al*., 2006).[1] Corruption has also impacted on service delivery in Brazil. Municipalities where corruption in education has been detected have test scores that are 0.35 standard deviations lower than those without corruption, as well as higher rates of dropout and failure. Moreover, teachers in corrupt municipalities are 10.7 per cent less likely to receive pedagogical training and less likely to have a computer or science lab (Ferraz *et al*., 2012).

1.2 Description of the intervention

The idea that community members have incentives to monitor providers and demand better services (Stiglitz, 2002) led practitioners to believe that allowing communities to have monitoring power over providers could be beneficial for improving service delivery and reducing corruption in both the short and long term. In the short term, it could improve outcomes by identifying pockets of corruption and inefficiency in service delivery. In the long term it may contribute to changes in political norms and to establishing a transparent and accessible channel of communication for the community to provide feedback to providers and politicians on a regular basis.

This set the stage for a move to encourage governments in developing countries to become accountable to their own citizens, in an attempt to reform institutions from the bottom up. As a consequence, over the last two decades programmes aimed at encouraging community monitoring have been introduced in countries spanning continents and cultures including Albania, Argentina, Brazil, Cambodia, Cameroon, Colombia, Kenya, India, Indonesia, Malawi, Philippines, South Africa, and Uganda, among others (Reinikka and Svensson, 2004, 2005, 2011; Pan and Christiaensen, 2012; Tosi, 2010; Ferraz, Finan and Moreira, 2012; Capuno and Garcia, 2010; Ringold *et al*., 2012).

This idea was operationalised by the introduction of **community monitoring interventions (CMIs)**, often referred to as social accountability mechanisms. These programmes can be broadly defined as interventions where the community is given the opportunity to participate in the process of monitoring service delivery, where monitoring means being able to observe and assess providers' performance and provide feedback to providers and politicians.

[1] This is also the case of Sub Saharan Africa, where absence levels are above 20 per cent and in same countries even 50 per cent (Service Delivery Indicators, 2015).

The Association for the Empowerment of Workers and Farmers in India was the first organization to introduce a social accountability initiative, through social audits in the early 1990s (Maru, 2010).[2] Association workers read out government accounts and expenditure records at community meetings, and then invited villagers to testify to any discrepancies between official records and the villagers' personal experience. Since then, a range of different community monitoring initiatives has been implemented. The four major categories of such interventions are information campaigns, scorecards/citizen report cards, social audits, and grievance redress mechanisms. These four sub-categories of community monitoring share two common elements:

- a clear objective of reducing corruption and improving service delivery, and
- using encouragement of the community to monitor service delivery as a key intervention instrument.

Table 1 below summarises the key components of these interventions.

[2] The word 'audit' is derived from Latin, which means 'to hear'. In ancient times, emperors used to recruit persons designated as auditors to get feedback about the activities undertaken by the kings in their kingdoms. These auditors used to go to public places to listen to citizens' opinions on various matters, like behaviour of employees, incidence of tax and image of local officials (Centre for Good Governance, 2005).

Table 1: Interventions aimed to increase civic participation in monitoring public officials and providers.

Intervention	Description
Information Campaign	These are efforts to inform citizens about their rights to services, quality standards, and performance campaigns. In particular, it can include information on the importance of the service, on providers' performance, and on how to monitor providers.
Scorecard/ Citizen Report Cards	These involve quantitative surveys that assess users' satisfaction and experiences with various dimensions of service delivery. It often involves a meeting between the recipients of services and providers to discuss the findings of the survey and to develop a follow-up plan (Ringold *et al.*, 2012).
Social Audit	Social audits allow citizens receiving a specific service to examine and cross-check the information the provider makes available against information collected from users of the service (Ringold *et al.*, 2012).
Grievance Redress Mechanisms	These are mechanisms that provide citizens with opportunities to use information redress to influence service delivery and give feedback on government programmes and services, mechanisms including complaint hotlines, informal dispute resolution mechanisms, and courts (Ringold *et al.*, 2012).

Information campaigns are one of the most common interventions to encourage participation and interest in service delivery monitoring. They usually involve provision of information on the benefits of the service to be delivered (health, education, police, and so on) and the current state of the service in the community. The information could be provided door to door, in public gatherings aided by local leaders, through radio, newspapers or other means. Kefeer and Khemani (2011), for example, study the impact of having access to community radio programmes on the benefits of educational attainment in Benin. Information campaigns can also include information on how to monitor providers. For example, Banerjee *et al*. (2010) conduct a randomised evaluation of three interventions to encourage beneficiaries' participation in India's educational system. Prior to conducting the interventions, information was provided on the state of educational performance. They then a) provided information on existing institutions, Village Education Committees (VECs), to monitor schools, b) trained community members in a testing tool for children, and c) trained volunteers to hold remedial reading camps for disadvantage children.

Scorecards,[3] often referred to as citizen report cards, are another way in which to encourage citizen to participate in monitoring service delivery. The rationale is that by giving citizens a voice, they will be encouraged to demand better services. For example, Björkman and Svensson (2009) analyse the impact of a scorecard community monitoring intervention on primary health care in Uganda. For the intervention, a non-governmental organisation (NGO) facilitated village and service provider staff meetings in which members of the communities discussed baseline information on the status of health service delivery relative to other providers and the government standard. Community members were also encouraged to develop a plan identifying key problems and steps that providers should take to improve health service delivery. An important difference between information campaigns and scorecards is that the latter can include an interaction between citizens and providers, while the former does not include a forum for such interaction.

Social audits involve interactions not only between citizens and providers, but also with politicians, as for instance in Colombia's Citizens Visible Audit (CVA) (Molina, 2013b). As part of this program, infrastructure projects providing local public goods, such as water and sanitation infrastructure, schools and hospitals, included an additional CVA component. A social audit involves:

- dissemination of information through radio, newspapers and local TV about the CVA programme in the neighbourhoods where the project takes place;
- introduction of the infrastructure project to the community in a public forum. Citizens are told about their rights and entitlements, including the activities they can do to monitor the project and the responsibilities of the executing firm. A group of interested beneficiaries is established and trained to carry out community monitoring activities;
- periodical public forums, bringing together local authorities, neighbours, and representatives from the firm carrying out the specific project. The state of the project is explained in detail to the community, who can voice concerns and recommendations. Commitments are made by the firm, the local government, and project supervisor to solve the problems that may arise during the project. These commitments are monitored by the community, the facilitators from the central government and the project supervisor. If the problem persists, administrative complaints are submitted to the Supreme Audit Body in the central administration;

[3] Scorecards for health services were pioneered in Malawi in the early 2000s by Care International. This intervention followed the spirit of individual "citizen report cards," which were first introduced in Bangalore, India in 1993. The citizen report card revealed low levels of public satisfaction with the performance of service providers. The findings were widely publicised through the media, which created pressure among public officials to organize workshops and meeting with local civic groups and NGOs. Increased public awareness on government inefficiencies and other related concerns triggered the formation of more than 100 civic groups in different parts of India, as well as the launch of many campaigns for transparent public management (Bhatnagar, Dewan, Torres and Kanungo, 2003).

- regular monitoring of the project by the beneficiary group and collection of information on whether commitments are being honoured and any other new problem that may arise;
- presentation of the finalised project to the community before making the final payment to the executing firm, and sharing of the audit results with all interested and concerned stakeholders.

Social audits can also involve citizens as decision makers. In this case, citizens have the power to make actual decisions over the project. The extent of the decisions over which the community has control, however, varies. An example of a CMI where citizens had decision power is the Kecamatan Development Programme (KDP) in Indonesia (Olken, 2007). This programme funded projects in about 15,000 villages each year. Each village received an average of 8,800 USD, which was often used to surface existing dirt roads. To control the use of funds, checks were built into KDP. First, funds were paid to village "implementation teams" in three instalments. To receive the second and third payments, the teams had to make accountability reports at an open village meeting. Second, each project had a four per cent chance of being audited by an independent government agency. The study introduced two anti-corruption strategies: enhancing community participation and increasing government audits. To enhance community monitoring, invitations to the community meetings were randomly distributed throughout the village. It is important to note the community decides how to allocate the funds before monitoring the project, which differentiates it from studies on CMIs describe above.[4]

Grievance redress mechanisms (GRMs) provide people with opportunities to use information to influence service delivery. GRMs capture different mechanisms that provide citizens with opportunities to use information redress to influence service delivery and give feedback on government programmes and services. Such mechanisms include complaint hotlines, informal dispute resolution mechanisms, and courts (Ringold *et al*., 2012). An example described in Ringold (2012) is the design of Kenya's Hunger Safety Net Programme (HSNP), which includes GRMs at the community level. At the district level, the HSNP is designed to have a grievance front office to receive complaints. Complaints that cannot be addressed by the district office are forwarded to the national grievances coordinator.

1.3 How the intervention might work

For this systematic review, we define corruption as dishonest or fraudulent conduct by those in power. A big issue in the literature is the difficulty in measuring corruption accurately (Pande and Olken, 2011). As a consequence, each study measures it in a different way, reflecting the multi-faceted nature of corruption (Campos and Pradhan, 2007). We will review corruption estimates from both the forensic economic literature (Zitzewitz, 2012) as well as measures based on perceptions of corruption. An

[4] Furthermore, because these initiatives are put in place as a result of weak government presence, monitoring involves monitoring peers, which is different to traditional CMIs.

example from the forensic economic literature is Olken's study, (2007), where he measures corruption by comparing an estimate of what the project actually cost to what was reported on an item-by-item basis.

We refer to service delivery as the process through which basic services, such as education, health, and security are delivered to communities.[5] We will define service delivery outcomes as access to and quality of the service. For example, if the goal of the intervention is to facilitate household access to clean water, the percentage of access to clean water and water quality is the outcome of interest. If the goal is to monitor school performance, children's tests scores are the desired outcome.

Figure 1 presents a stylised theory of change we developed. Here we present a typical community monitoring program, clarifying the mechanisms through which the programme is expected to have an impact on corruption and service delivery. A typical CMI begins by attempting to make the project or service that it aims to monitor salient in the community. This is usually done though a communication campaign (building block 1) using as many mediums as possible, such as radio, newspapers, door to door campaigns, and local TV. The campaign's primary objective is to increase citizen knowledge of (a) the performance of the service to be monitored and/or (b) the importance of the service or project for the community.

Equipped with this information, citizens can engage in different activities. For instance, they might change their private actions, or contact fellow community members to collectively pressure providers and politicians to improve the quality of the service through monitoring activities (building block 2). To encourage citizens to monitor service providers, CMIs usually include activities to build the capacity of beneficiaries to monitor providers. For instance the CVA in Colombia provides information about the contractual obligations of the provider, ways for citizens to detect problems and to whom inquiries about the project should be directed to.

Empowered with information from building block 1 and/or 2, citizens are expected to solve the collective action problem and invest their time and effort to participate in monitoring service delivery (building block 3). Participation in monitoring activities could take many forms, depending on the specific CMI. For instance, social audits have public forums and scorecards and can include meetings between providers and citizens.

As an organised group, citizens can take turns to visit the place where the service or project takes place, such as a school, construction site or hospital, and collect information on its problems, for example absenteeism, use of low quality inputs in the construction process, unresponsive front-line providers. Citizens can then contact providers (building block 6) and/or elected officials (building block 4) to file complains about the service and provide information on the specific problems the service is

[5] For the purpose of this review, service delivery involves not only services, but also construction of necessary infrastructure to carry out those services. As a result, we will talk indistinctly between service delivery and project performance.

facing. In addition, citizens are expected to share the information collected by monitoring providers with their fellow neighbours that did not take part in monitoring activities (building block 5), to increase visibility of the community monitoring intervention and put pressure on providers and politicians. Finally, the independence and strength of the local media is assumed to impact upon the visibility of the project (Reinikka and Svensson, 2005; Ringold *et al*., 2012).

Citizens' participation in the programme may reduce the cost of monitoring front-line providers for politicians and managers. Citizens' monitoring activities also increase both visibility and citizens' ability to recognize whether elected officials are making an effort to reduce corruption and improve service delivery. As a result, there may be a greater incentive for politicians and policymakers to achieve better results and to put more pressure on providers to improve service delivery (building block 7). The threat of formal sanctions by politicians and/or informal sanctions by citizens is assumed to motivate service providers into exerting greater effort.

Many of these mechanisms are mediated by local norms and context. Participation in the CMI will be influenced by the strength of the community to act collectively. For example, communities with a history of grassroots participation are expected to organise more rapidly and more efficiently (Björkman and Svensson, 2010). History can play an important role in this crucial phase of the theory of change. In Africa, the history of slave trade left an imprint in cultural norms and beliefs which arguably diminished the trust among fellow citizens and reduced the strength of the community to act collectively (Nunn and Wantchekon, 2011). In Uganda, media attention was argued to be decisive to reduce corruption (Reinikka and Svensson, 2005) but that may not be the case in South Sudan or Zimbabwe today. Finally, this is a dynamic process, which makes understanding the specific history of service delivery, citizen engagement and political accountability in the community where the intervention took place, crucial.[6]

While the description above fits different type of CMI interventions, there are some features that are specific to each intervention. Below we describe two additional components of social audits and scorecards respectively. Scorecards have an added accountability mechanism through which citizens meet with service providers to discuss how to improve the service. This face-to-face interaction introduces intrinsic motivation arguments for the service providers, which may contribute to improving their performance. This will be moderated by whether it is credible for a given community to establish an informal system of rewards and sanctions. Additionally, the meeting could result in new ideas for providers and citizens on how to use and manage the service in a more efficient way.
Social Audits are CMIs with an additional component in the form of public forums, where representatives from the local government, the executing firm, the central government, and the community are present. It allows citizens to make their voice

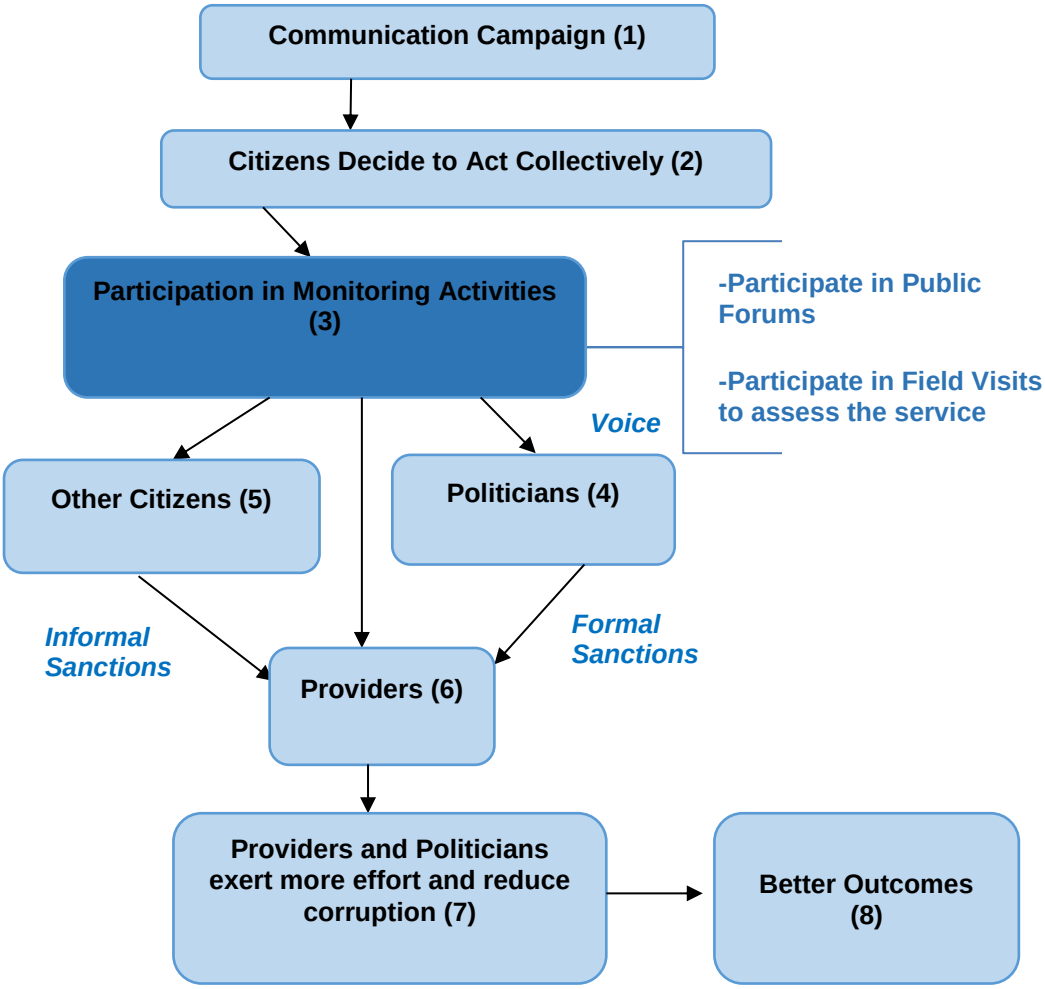
[6] For a review of the importance of context to understand the effectiveness of community monitoring interventions see Grandvoinnet, Ghazia and Raha (2015).

heard by local officials and providers, and reduces the time and effort citizens would need to invest to get an appointment with these officials. The public forums also reduce the cost for central government representatives to be heard by local officials. Finally, it reduces the cost of local officials to take actions to solve problems that arise during the implementation of the projects, such as lack of planning, lack of resources to finish the project, and acts of corruption. The symbolic act of the public forum may also signal to politicians and providers the importance of performing well on this project, as citizens are paying extra attention.

There are several empirical implications from this overall theory of change, which warrant testing:

- CMIs will increase the quantity and the degree to which citizens are involved in monitoring service providers.
- As a result of the CMIs, politicians and providers will exert more effort and improve their performance in relation to service delivery.
- CMIs will reduce the probability of corruption.
- CMIs will improve access and quality of the service provided.

Figure 1: Theory of Change for Community Monitoring



There are several assumptions underlying this theory of change, which must hold in order for it to accurately describe the process through which a CMI impacts on service delivery. Citizens need to participate in monitoring activities and politicians and providers need to be accountable. For citizens to participate, they need to have adequate information on how to monitor the project, be able to pay the opportunity cost of participation and coordinate their actions to monitor the project. Finally, citizens should believe the programme has the potential to be successful, be able to understand the information provided, pay attention and face a non-prohibitive opportunity cost to participate. Providers and politicians need to gain popularity, increased salary and/or social recognition, obtain re-election or avoid social disapproval or an indictment. If these assumptions are not met, the underlying programme theory of the CMI breaks down and this may prevent CMIs from having an impact on service delivery outcomes. In particular, whether or not they hold true can affect citizens' decision on whether to monitor government activity and the governments' willingness to facilitate citizen engagement and become more accountable. Below we present the bottlenecks as well as the empirical implications.

Civic participation failure

One potential concern with CMIs is that citizens will fail to participate in monitoring activities (building block 3). We have identified six potential bottlenecks[7] that could prevent citizens from participating in monitoring activities, which in turn reduces the potential impact of the programme (see Table 2). In particular, if community monitoring activities are not carried out, or carried out by only a few citizens, their ability to uncover problems and put pressure on the government to provide accountability can be significantly reduced.

[7] The term bottlenecks has been used in the literature (Lieberman *et al.*, 2013) to refer to constraints that limit the effectiveness of community monitoring programmes.

Table 2: Bottlenecks preventing citizens from participating in monitoring activities

Bottleneck	Description	Empirical Implications
Information Gaps	Scholars and policymakers have long argued that programmes often fell short of their expectations because of information problems (Ringold *et al.*, 2012). In the case of the CMIs there are two important potential deficiencies: (a) the information may not have been properly disseminated (building block 1), and/or (b) information on how to monitor the project was either not provided or not understood by the citizens (building block 2).	• If the information is not properly disseminated, citizens will not participate in monitoring activities • Citizens' probability of participation in monitoring activities will be a function of how well they understand how to monitor providers.
Lack of Attention Span or Rational Inattention	Even if information is provided, it may fail to have the anticipated outcome. A factor that conditions its success is what information is to be disclosed (content), and how it is to be presented (vehicle). In the case of CMIs, citizens' lack of attention span might prevent them from absorbing the information provided by the intervention and properly monitor providers. Citizen may also choose not to pay attention (Sims, 1998, 2003, 2006), often describe as rational inattention. As a consequence, introducing new information does not always lead to new beliefs or changes in behaviour.	If citizens' opportunity cost of paying attention to the information is high or they lack of attention span, their probability of participation will decrease[8].
High Opportunity	Citizens, and particularly the poor, simply may not have the time to get informed or give feedback on service delivery because of	If opportunity cost of participation is high, probability of

[8] In order to give salience to information practitioners use an array of instruments to attract the citizens' attention. We are not aware of any CMIs where these incentives were embedded in the theory of change and properly assessed. This appears to be a knowledge gap for CMIs.

Cost of Participation	more pressing priorities such as securing food and meeting other basic needs (Banerjee and Mullainathan, 2008).	participation will be lower.
Collective Action Failure	Scholars have emphasised the collective action problems that can arise in the presence of a non-excludable local public good (Olson, 1971), such as community monitoring. If community members believe fellow citizens will contribute to monitor the project, they may decide not to participate.	If citizens expect other citizens would free-ride on their efforts to monitor the project, the probability and intensity of participation will be lower.
Citizens' Beliefs can prevent participation	Citizens may refuse to take advantage of the opportunity to influence politicians and providers if they believe the chances of success are low. These beliefs can become a self-fulfilling prophecy where citizens refuse to participate and as a consequence providers have fewer incentives to improve performance (Molina, 2013a).	Citizens who perceived politicians and/or providers are responsive to them have higher probability of participation in community monitoring activities.
Elite Capture	Community monitoring may also be prone to be captured by local elites (Bardhan, 2002; Bardhan and Mookherjee, 2006; Olken, 2007). The rationale is that when decision making is part of the CMI, the elite would want to take advantage by capturing the monitoring process and appropriate the resources associated with the program.	If the CMI is captured by local elites, the participation will be limited to its supporters, which may affect the effectiveness of the program. It is an empirical question whether the elite capture could improve or worsen outcomes. See Atlas *et al.* (2013) for an example of different types of elites

Politicians and providers' accountability

Under this heading we present potential reasons for a lack of responsiveness on the part of the politicians and providers. The literature cites many reasons why politicians and providers may not be accountable to their citizens (building block 4 and 6). Below we identify three potential bottlenecks.

Table 3: Bottlenecks causing a lack of responsiveness from politicians and service providers

Bottleneck	Description	Empirical Implications
Unresponsive Politicians	Even in well-functioning democracies, citizens in a given community may not be pivotal for politician's electoral strategy (Downs, 1957; Hotelling, 1929; Persson and Tabellini, 2002). This means that citizens' support is not needed for politicians to win elections and/or stay in power. Additionally, especially in developing countries, often the political system does not work properly and institutions do not help translate the preference of the people into policy (Boix *et al.*, 2003; Acemoglu and Robinson, 2008). Keefer and Khemani (2004, 2005) argue that public service providers have weak incentives to improve performance quality because their jobs are protected by political agents – politicians have stronger incentives to provide secure public-sector jobs as teachers, health workers, and local bureaucrats, than to pressure these job-holders to improve service delivery.	If the community is not needed for the politicians to stay in power, we should find that politicians' performance does not increase as a result of the CMI, irrespective of what happens with citizen engagement in monitoring activities.
Unresponsive Providers	The literature on providers' motivations to deliver services no longer assumed them to be either public spirited altruists (knights) or passive recipients of state largesse (pawns). Instead, they are often considered to be in one way or another self-interested (knaves) (Le Grand, 2003). Communities in developing countries often have low state capacity, which limits the ability of governments to monitor self-	In communities where providers are not responsive to politicians, CMIs will only be effective if it changes providers' behaviour. If communities can impose a credible threat of informal social sanctions to unresponsive providers, the probability of a change in behaviour from providers will be higher, regardless of

interested providers (Besley and Persson, 2011). If this is the case, putting pressure on the government will be ineffective and only competition or informal sanctions from the community may have an effect on providers' performance.	whether they are responsive to politicians. If communities can choose providers, competition among them will foster better performance[9].

1.4 Why it is important to do this review

Community monitoring interventions have gained widespread acceptance as a tool to improve transparency and accountability by all the major players in the practitioners' world, that is, governments, NGOs, and the donor community. Increasing citizen participation in government decision making and policy formulation is the main objective behind the Open Government Partnership (OGP), a global consortium of governments. Through OGP, more than 50 countries around the world have already agreed upon different goals related to transparency and citizen participation. Moreover, international aid agencies increasingly require development projects to include 'beneficiary participation' components. Over the last decade the World Bank alone has channelled 85 billion USD to local participatory development (Mansuri and Rao, 2012).

The United Nations have set increasing citizen participation as their main strategy to achieve good governance and human rights (UN, 2008), and NGOs with a focus on increasing government accountability through citizen participation continue to expand around the globe, managing increasing amounts of resources. For instance, Transparency International has an annual budget of 36 million USD, which they use to advocate for increasing citizen engagement as a necessary step for development (Transparency International, 2013). Other examples of NGOs are Twaweza and the Affiliated Network for Social Accountability (ANSA). Twaweza engages in building citizen capacity to monitor governments and foster their accountability across East Africa and has an annual budget of 17 million USD. ANSA is currently operating in East Asia and the Pacific, South Asia, Africa, Middle East and at the global level to support civil society organisations in their efforts to monitor governments in service delivery and to build demand for public accountability.

Finally, through the newly created Global Partnership for Social Accountability (GPSA) a coalition of donors, governments and civil society organisations aim to improve development results by supporting capacity building for enhanced citizen feedback and participation to monitor service delivery. GPSA aims to reach overall funding of 75 to 125 million USD over the next seven years. To date, 15 countries

[9] In some parts of the world the state fails completely to provide services and to monitor illegal private service provision. Even under these environments, when citizens can choose providers overall providers' performance may increase.

have joined the GPSA: Bangladesh, Belarus, Colombia, Dominican Republic, Honduras, Indonesia, Kyrgyzstan, Malawi, Moldova, Mongolia, Mozambique, Philippines, Senegal, Tajikistan and Tunisia.

From a theoretical perspective, as we highlighted above, there are no clear predictions as to what the impact of these programmes should be. Some authors have found reasons to expect CMIs to have a positive effect on improving service delivery and reducing corruption (Stiglitz, 2002), but others have argued that successful implementation of CMIs might prove more difficult than expected (Bardhan, 2002; Bardhan and Mookherjee, 2006; Olken, 2007; Molina, 2013a).

While a number of empirical studies have been conducted in recent years, we still lack a clear picture of the impact of community monitoring programmes. High quality primary studies find what at first appears to be contradicting evidence regarding the effect of CMIs on service delivery outcomes. Björkman and Svensson (2009) find that community scorecards in Uganda significantly increased the quality and quantity of primary health care provision. Banerjee *et al*. (2010), however, find the opposite result when testing the effect of an information campaign in India. They report that neither giving citizens information on how to use existing institutions to monitor schools nor training them in a testing tool to monitor children's learning had any statistical impact on children's learning performance.

There are several existing reviews of this literature. For instance, King, Samii and Snilstveit (2010) provide a systematic review of impact evaluations examining the effectiveness of community-driven development and curriculum interventions in improving social cohesion in sub-Saharan Africa. However, this is an outcomes driven review focusing on social cohesion outcomes, rather than focusing on corruption and service delivery outcomes of a broad range of CMIs. There are also several non-systematic reviews on related issues. Mansuri and Rao (2012) review the evidence on the effectiveness of local participatory programmes on an array of outcomes, including service delivery. The study focuses mostly on large-scale interventions such as Community Driven Development (CDD), and Community Driven Reconstruction (CDR). They find that, on average, results are below the expectations of these programmes, and suggest that the reason for this may be a failure to build cohesive and resilient organisations to pressure the government. In particular, they argue that both local and national contexts may be key factors in determining effectiveness, in part because not all communities have a stock of social capital that can be readily harnessed though a participatory intervention. Finally, they argue that induced participatory interventions work best when they are supported by a responsive state and when local accountability institutions are robust.

Moreover, Hanna *et al*. (2011) and Pande and Olken (2011) review studies of interventions aimed at reducing corruption. However, they do not provide comprehensive reviews of the literature on effects of community monitoring and use narrative methods of synthesis rather than meta-analysis. Ringold *et al*. (2012) review the effects of CMIs on human development, and while it is relatively comprehensive, it is a narrative review rather than a systematic review. It identifies

some key impediments to the successful implementation of CMIs: a) information asymmetries between citizens and providers, b) individuals may not use the opportunity to influence service providers, c) providers that are not amenable to change, and d) fragmented civil society and weak media.

Similarly, Devarajan *et al*. (2011) review interventions aimed at strengthening the role of civil society in service delivery and government accountability, focusing on Sub-Saharan Africa. The review, which is not systematic, finds preliminary evidence of the positive effects of building organic participation and building on existing political and civil society structures, on service delivery and government accountability. The findings are mediated by the local context, as in communities where clientelism and rent-seeking were widespread, civic participation failed to have an impact on service delivery and government accountability.

Gaventa and Barret (2012) perform a meta-case study of 100 interventions aimed at increasing citizen engagement in service delivery. However, the search for literature was limited to the studies undertaken by the Institute of Development Studies between 2000 and 2011 and the review adopts a vote counting approach with a broad range of study designs.

To date no systematic reviews have been conducted on the effects of CMIs on corruption and service delivery outcomes. The existing reviews provide some suggestive evidence of the effects of CMI, but come to different conclusions, in an area that is hotly debated and of key policy importance. Reports from USAID for instance acknowledge that the lack of systematic evidence limits our ability to make precise claims regarding the relationship between CMIs, corruption and service delivery outcomes (Brinkerhoff and Azfar, 2008).

Whether CMIs affect the behaviour of beneficiaries, providers and politicians, and in turn reduce corruption and improve service delivery outcomes is still an open empirical question. We also know little about the mechanisms through which these interventions have an effect (or lack thereof). The inconclusiveness reflected in the theoretical and empirical work described above highlights the need for systematic evidence on the subject. Our systematic review aims to shed light on this debate by providing a systematic and exhaustive literature search, together with a comprehensive and unbiased synthesis of the existing evidence.

2. Objectives

Our systematic review aims to assess and synthesise the evidence on the effects of CMI interventions on corruption and access to and quality of service delivery outcomes. We introduce a theoretical framework to understand the pathways of change of the CMIs interventions. Using this framework, we aim to uncover the facilitators and barriers for CMIs to successfully reduce corruption and improve access to and quality of service delivery. The review systematically collects and synthesises evidence from high quality impact evaluations of CMIs. Outcomes are synthesised along the causal chain, from intermediate outcomes such as participation in the monitoring activities through to public officials and providers'

responsiveness, to final outcomes such as corruption and access to and quality of the services provided.

The review aims to answer the following questions:

- What are the effects of CMIs on corruption and access to and quality of service delivery in L&MICs, relative to no formal community monitoring or CMIs with less community representation?
- What are the mechanisms through which CMIs have an effect (or lack thereof) on reducing corruption and improving service delivery outcomes?
- Do factors such as region, income level or length of exposure moderate the effects of CMI on intermediate and final outcomes?

3. Methods

Our review strives to answer these questions by synthesising evidence from both quantitative and qualitative studies. The review follows Campbell and Cochrane Collaboration approaches to systematic reviewing (Campbell Collaboration, 2015; Hammerstrøm *et al*., 2010; Higgins and Green, 2011; Shadish and Myers, 2004; Shemilt *et al*., 2008). The review is also informed by theory-based impact evaluation (White, 2009), using the theory of change (Figure 1) as the framework for the review, to guide the types of studies included, data collection and analysis. To ensure the review is adequately oriented towards both reporting effects and explaining the reasons for them, we synthesise effects along the causal chain, including qualitative evidence where appropriate, using the effectiveness plus approach (Snilstveit, 2012; Snilstveit *et al*., 2012). For the quantitative synthesis we use meta-analysis to pool study effects where studies are judged to be sufficiently similar to do so.

3.1 Criteria for including studies in the review [PICOs]

3.1.1 Participants

The review includes CMIs in either low- or middle-income countries at the time that the intervention was carried out. To assess whether a country is low, middle or high income we follow the World Bank classification method. For example, for interventions carried out in 2011, to qualify as a low income group gross national income (GNI) per capita should be 1,025 USD or less; middle income, 1,026 USD – 12,475 USD; and high income, 12,476 USD or more. We include all CMIs in low- and middle-income countries. The review excludes CMIs in high-income countries. For studies to be included, they need to collect and report on data at the individual or at the community level. Interventions targeting particular disadvantaged groups, or studies that conduct analysis across disadvantaged groups, are included in the review. This inclusion criterion was used for both quantitative and qualitative studies.

3.1.2 Interventions

We include community monitoring interventions where the community is given the opportunity to participate in the process of monitoring service delivery, where

monitoring means being able to observe and assess providers' performance and provide feedback to providers and politicians. To be included interventions need to:

- have a clear objective of reducing corruption and/or improving service delivery;
- use encouragement of the community to monitor service delivery as a key intervention instrument;
- fall into one of the following four intervention categories: information campaigns, scorecards/citizen report cards, social audits and grievance redress mechanism.

These interventions have a common theory of change that exactly addresses our objective of interest: whether programmes that encourage community monitoring reduce corruption and improve access to and quality of service delivery. Detailed descriptions of these interventions are provided below:

Information campaigns usually involves information on the benefits of the service to be delivered (health, education, police, etc.) and the current state of the service in the community. The information could be provided door to door, in public gatherings aided by local leaders, through radio, newspapers and other means. Kefeer and Khemani (2011), for example, study the impact of having access to community radio programmes on the benefits of educational attainment in Benin. Information campaigns may also include information on how to monitor providers.

Scorecards, or citizen report cards, also encourage citizen to participate in monitoring service delivery. The intervention takes the form of a quantitative survey that assesses users' satisfaction and experiences with various dimensions of service delivery. It often involves a meeting between the recipients of services and providers to discuss the findings of the survey and to develop a follow-up plan (Ringold *et al.*, 2012). For instance, Björkman and Svensson (2009), analyse the impact of a scorecard community monitoring intervention on primary health care in Uganda. A non-governmental organization (NGO) distributed a quantitative survey) and facilitated village and service provider's staff meetings in which members of the communities discussed the results. Community members were also encouraged to develop a plan identifying key problems and steps that providers should take to improve health service delivery. Scorecards may also include an interaction between citizens and providers, while information campaigns do not include a forum for such interaction.

Social audits involves group of citizens collecting information on the implementation of particular public services in relation to expected standards. This allow citizens receiving a specific service to examine and cross-check the information the provider makes available during a mandatory public hearing against information collected from users of the service (Ringold *et al.*, 2012). During the public hearing all relevant stakeholders are present, including citizens, providers, and politicians.

Grievance redress mechanisms (GRMs) provide people with opportunities to use information to influence service delivery. GRMs capture different mechanisms that provide citizens with opportunities to use information redress to influence service

delivery and give feedback on government programmes and services. Such mechanisms include complaint hotlines, informal dispute resolution mechanisms, and courts (Ringold *et al.*, 2012).

Other interventions may include community monitoring as part of a different intervention. For instance, Community Driven Development Interventions (CDDs), Community Driven Reconstruction Interventions (CDRs), participatory budgeting, and school based management will only be included if they have a clear community monitoring component. In that case, depending on the monitoring component, we will classify them as information campaigns, scorecards, social audits or grievance redress mechanism. The study from Olken (2007) in Indonesia is a case in point. The monitoring program, a social audit, is embedded in a larger intervention, a CDD. We include this type of interventions in our review.

However, there are other CDDs and CDRs where there is no monitoring component. For instance, Casey *et al.* (2012), who study the impact of a CDR programme in Sierra Leona, are excluded from our review. The reason is that the theory of change for these types of interventions is completely different than for CMIs. Even further, the objectives of these interventions are also different. A similar argument could be made about participative budgeting and school based management. As a result, we also exclude those interventions from our review when there is no community monitoring component.

Access to information laws provides a legal framework for the public provision of information (Ringold *et al.*, 2012). There are many laws that can potentially improve citizens' abilities to monitor service delivery, for instance, voting rights, laws that allow schools or hospitals to have user groups, the creation of the ombudsman figure, among many others. The theory of change for these interventions is different from the one we develop for CMIs and studies assessing such interventions on their own are excluded. Such interventions are not defined as community monitoring unless they include an additional component aimed at encouraging community monitoring. For instance, studies assessing the impact of information campaigns which aim to induce citizens to monitor the implementation of such laws fall under our definition of community monitoring, and thus are included in the review. These criteria are used for both quantitative and qualitative studies.

3.1.3 Comparisons: Treatment and Comparison Groups

Even for identical interventions we could have different estimands and/or different counterfactuals. We include interventions that estimate the impact among the following groups:

- Community Monitoring Interventions (CMI) as the treatment condition and no formal process of monitoring as the counterfactual. For example, see Björkman and Svensson (2009).
- CMIs where there is an encouragement for community to participate in monitoring as the treatment condition and CMI with no encouragement as the counterfactual. For example, see Olken (2007).

3.1.4 Outcomes

Primary outcomes

We include studies assessing the effects of CMI on the following primary outcomes to address review question (1), the effects of CMIs on access and quality of service delivery, and corruption outcomes in L&MICs.

Corruption outcomes

As we argued above, a big issue in the literature is the difficulty in measuring corruption accurately (Pande and Olken, 2011). In this review we synthesise two types of corruption measures, forensic estimates and perception measures. Below we provide specific examples:

- Forensic economic estimates: This refers to the application of economics to the detection and quantification of behaviour (Zitzewitz, 2012), in this case, corruption. In Olken (2007) corruption is measured by comparing the researcher's estimate of what the project actually costs[10] to what the village reported it spent on the project on an item by item basis.
- Perception measures: An imperfect way to deal with the fact that it is very difficult to detect and measure the extent of corruption, is to rely on citizen's perception measures of corruption.

Service delivery outcomes

For impacts on service delivery we look at two types of outcome: access and quality of the service. Below we provide specific examples:

Access to service: We use the percentage of the population that has access to the service to measure this outcome. For example, if the CMI involves an infrastructure programme to facilitate household access to clean water, the percentage of the population that has access to clean water is the primary variable of interest.

Quality of services

We will use measures of:

- *Improvement in prevalence condition.* For example, Björkman and Svensson (2009) capture the effect of the CMI on infant weight. Additional measures in the health care sector could be mortality rates as well as disease prevalence in general. In Banerjee *et al.* (2010), there is information on student's reading ability. Additionally, information on test scores would be in this category. For CMIs in the police sector, the outcome indicator could be victimisation rates for each type of crime. In infrastructure projects, we look at different outcomes depending on whether it is a school, a hospital, or a water and sanitation

[10] The cost is determined by the quantity of materials used and estimate of material prices and wages paid on the project.

program. In the last case we could measure the quality of the water that reaches households, as well as whether the service is working all the time or has interruptions. Finally, in Molina (2013b) the author looks at satisfaction with project performance as a measure of the impact of the social audit.

- *Average waiting time to get the service.* This is important for health care interventions as well as those in the security sector. See Björkman and Svensson (2009).

Studies that include at least one of these outcomes are included in the systematic review. Among those included studies, we collect and analyse data on a range of intermediate outcomes to address question (2), the mechanisms through which CMIs have an effect (or lack thereof) on improving service delivery outcomes and reducing corruption. This means that any study that has an intermediate outcome should also include at least one of the primary outcomes. Below we specify the intermediate outcomes of interest for this review.

Intermediate outcomes

These outcomes include changes in behaviour induced by the intervention, such as whether participants contribute to monitoring of the service or project and the behaviour and performance of providers and politicians. Below we provide specific examples that follow the logic of the theory of change presented above.

- *Citizen's participation in monitoring activities:* This could be measured by the percentage of citizens that contribute to the monitoring process. If measures of intensity of participation are available, we also collect them. In the context of the social audit in Colombia this would be the percentage of citizens that spend any time monitoring the project. The more time they spend, the higher the intensity of participation.
- *Providers' and politicians' performance:* This outcome could be measured in several ways. Traditionally, absenteeism rates are computed if a direct measure of effort and quality of their performance is not available.

3.1.5 Study types

To address review questions 1, 2 and 3, studies eligible for inclusion in the effectiveness synthesis include impact evaluations based on experimental design (where randomised assignment to the intervention is made at the individual or cluster level), quasi-experimental designs (including controlled before and after (CBA) studies with contemporaneous data collection and with two or more control and intervention sites, regression discontinuity designs and interrupted time series studies (ITSs)) and ex-post observational studies with non-treated comparison groups and adequate control for confounding.

For quasi-experimental studies and observational designs with comparison groups, eligible studies must use adequate methods of analysis to match participants with non-participants, or statistical methods to account for confounding and sample selection bias. Appropriate methods of analysis to match participants and non-

participants include propensity score matching (PSM) and covariate matching. Appropriate methods of analysis to control for confounding and selection bias include multivariate regression analysis using difference-in-differences (DID) estimation, instrumental variables (IV) or Heckman sample-selection correction models.

Studies that do not control for confounding using these methods, such as those based on reflexive comparison groups (pre-test post-test with no non-intervention comparison group), are excluded.

To address question (2) we extracted relevant data from studies meeting the criteria outlined above, and related documents for the interventions evaluated in the effectiveness studies.

3.2 Search methods for identification of studies

3.2.1 Electronic searches

We performed searches in the following databases and resources: International Bibliography of Social Science (IBSS), EconLit, Citas Latinoamericanas en Ciencias Sociales y Humanidades (CLASE), Plataforma Open Access de Revistas Científicas Electrónicas Españolas y Latinoamericanas (e-Revist@as), Red de Revistas Científicas de América Latina y el Caribe, España y Portugal (REDALyC), African Journals Online (AJOL), Scopus, the British Library for Development Studies (BLDS), PAIS (Public Affairs Information Service), Worldwide Political Science Abstracts (WPSA), International Political Science Abstracts (IPSA), JSTOR, CIAO (Columbia International Affairs Online), ABI/Inform (Ebsco), ELDIS, CAIRN and Google Scholar.

These databases cover a wide range of journals, including those from low- to middle-income countries that may be overlooked in global indexes. Initial searches were based on keywords derived from our research questions. All searches were stored to ensure replicability.

We searched using a combination of the group of keywords presented in the Table 4. The combination within each group is given by the Boolean operator OR, and between groups by AND (or equivalent operator for the database).

Table 4: Search keywords

Group 1: People	Group 2: Monitoring	Group 3: Results	Group 4: Government
communit*	monitor*	performance	representative*
civil*	particip*	effort*	local authorit*
civic*	empower*	attend*	bureaucra*
citizen*	control*	achievement*	councillor*
people	develop*	test score*	provider*
elector*	governanc*	absent*	politician*
grassroot*	superv*	disease prevalence	official*
social	report* card*	cost effectiv*	leader*
societ*	audit*	access*	govern*
local	informat* AND campaign*	deliver* service*	administration
resident*	scorecard*	performance service*	
neighbo*	score card*	provi* service*	
	accountab*	corrupt*	
	watchdog*	fraud*	
	democrati*	dishonest*	
	people power	brib*	
		mismanag*	
		leak*	
		missing fund*	
		client*	
		wait*	
		victim*	
		efficien*	
		inefficien*	
		quality	
		rent* seek*	

Keywords were translated to Spanish, French, and Portuguese.

The search strategy was adapted to the particularities of each database. Several of the databases had restrictions regarding the maximum number a keyword and/or wildcards used, or the number or reported results, and required dividing the searches into several combinations.

Whenever possible we searched for synonyms or used the option of searching for similar terms before every keyword. Depending on the maximum number of keywords allowed in the database we limited the searches with a L&MIC filter, to low- or middle-income countries.

We used ENDNOTE, and ZOTERO as an auxiliary tool, to record searches, and collect and organise references.

One example of the search strategy is available in Appendix A.

3.2.2 Other searches

We tried to avoid the bias against unpublished and non-English literature by searching in Google Scholar, REPEC-IDEAS, NBER, Global Development Network, Networked Digital Library of Theses and Dissertations Index to Theses, 3ie database and the ProQuest dissertation database using the set of keywords described above.

We also used the following methods to identify additional studies:

- Screening the references of included studies and existing reviews for eligible studies.
- Citation searches of all included studies using Social Sciences Citation Index, Scopus and Google Scholar.
- Searching in conference programmes and websites of key institutions; such as the World Bank, UNDP Governance Projects, Asian Development Bank, African Development Bank, Inter-American Development Bank, Open Government Partnership, Research centres and networks, as JPAL, MIT, IEN, Institute of Development Studies; International, Economic Commission for Latin America (ECLAC), Centro Interamericano para el Desarrollo del Conocimiento en la Formación Profesional (CINTERFOR), regional, national and local non-governmental organizations.
- Contact with subject-matter experts, and practitioners

3.2.3 Additional searches to address question 2

In order to analyse the mechanisms through which CMIs have (or not) an effect, we searched for sibling articles following Booth (2011), doing a citation tracking of all included studies to identify any sibling papers, and conducting targeted searches at implementing agencies websites, Google and databases using the intervention name.

3.3 Data Collection and Analysis

3.3.1 Selection of studies

Two independent review authors performed the searches and screened the first stage results against the inclusion/exclusion criteria. A third author supervised the process and solved any discrepancies. A record was kept of all decisions.

3.3.2 Data extraction and management

We extracted information on the study type, authors, date, publication status, type of publication and language. We also collected information about the intervention, country and area, dates of the intervention, available information, type of intervention, research design, outcomes reported, information transmission, interaction between community and service providers, and the community's power to make decisions. [11]

Two reviewers independently coded and extracted the data from the selected studies. Again, this process was supervised by a third author. A coding sheet with a description of the data collected is included in Appendix B.

3.3.3 Assessment of risk of bias in included studies[12]

Assessment of risk of bias in included studies of effects

Studies were critically appraised according to the likely risk of bias based on:

- quality of attribution methods (addressing confounding and sample selection bias);
- the extent of spillovers to services and projects in comparison groups;
- outcome and analysis reporting bias; and
- other sources of bias.

'Low risk of bias' studies are those in which clear measurement of and control for confounding was made, including selection bias, where intervention and comparison groups were described adequately (in respect of the nature of the interventions being received) and risks of spillovers or contamination were small, and where reporting biases and other sources of bias were unlikely.

Studies were identified as at 'medium risk of bias' where there were suspected threats to validity of the attribution methodology, or there were possible risks of spillovers or contamination, arising from inadequate description of intervention or comparison groups or reporting biases suspected.

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[11] We used a reduced version of the Coding sheet proposed in the Protocol, in which we have discarded the 'Capacity Building' block because we found several missing values for most of these fields.

[12] Our instrument was an abridged version of the one developed by Waddington, Snilstveit, Hombrados, Vojtkova, Phillips, Davies and White (2014).

'High risk of bias studies' are all others, including those where comparison groups were not matched or differences in covariates were not accounted for in multivariate analysis, or where there was evidence for spillovers or contamination to comparison groups from the same communities, and reporting biases were evident. Our evaluation criteria are presented in Appendix C.

At the same time, we also critically appraised the confidence in our classifications, the consistency among ratings by our coders by doing inter-rater assessment, we use an absolute agreement intra-class correlation, McGraw and Wong (1996).

Following de Vibe *et al*. (2012) and Waddington *et al*. (2014) we present a summary of the quality assessment of the included studies using a traffic light scheme graph to differentiate study quality across the four different components of our risk of bias assessment tool.

Quality appraisal of studies included to address review question 2

To address review question (2), we include a subset of the quantitative studies included in the review of question (1), specifically, those that measure not only primary outcomes but also intermediate outcomes, plus a set of sibling studies. Most of those sibling articles were previous versions of the final included paper, policy papers, or other authors' descriptions of the same intervention. The subset of studies already included to answer question (1) are already appraised, and we simply use again the same ratings, adjusting them if necessary when we take into account the new variables, and for the sibling articles we follow the same methodology, for those cases where the article is merely descriptive of the intervention, or a retelling of the main paper, we assigned the same appraisal to the design of the intervention than in the effects paper, namely whether they address the existence of spillovers, selection bias and confounding, and we assigned them their own values for the potential existence of outcome and analysis reporting bias, or any other sources of bias.

3.3.4 Measures of treatment effect[13]

We extracted comparable effect size estimates from included studies, together with 95 per cent confidence intervals. Whenever possible, we calculated standardised mean differences (SMDs) for continuous outcome variables, risk ratios (RRs) and risk differences (RD) for dichotomous outcome variables. Some studies, Björkman and Svensson (2009) and Bjiörkman, de Walque and Svensson (2013), already reported average standard effects; which are interpreted in the same way as SMDs; in those cases we used them directly. Treatment effects were calculated as the ratio of, or difference between, treated and control observations in a consistent way, such that outcome measures are comparable across studies. Thus, a SMD or RD greater

[13] This section draws heavily on Waddington, Snilstveit, Vojtkova and Hombrados (2012), IDCG (Campbell International Development Coordinating Group), Protocol and Review Guidelines (2012) as well as Waddington, Snilstveit, Hombrados, Vojtkova, Phillips, Davies, and White, (2014).

than zero (RR greater than 1) indicates an increase in the outcome of interest due to the intervention, as compared to the control group. A SMD or RD less than zero (RR between 0 and 1) indicates a reduction under the intervention as compared to the comparison. A SMD or RD equal to (or insignificantly different from) zero (RR equal to 1) indicates no change in outcome over the comparison. Whether these relative changes represent positive or negative impacts depend on meaning of the outcome in the context of the programme being evaluated. For example, while positive impacts on service delivery are measured as values greater than 1, positive impacts of CMIs on – in this case, reductions in – corruption are measured as values less than 1. We followed the statistical transformations for calculating risk ratios and standardised mean differences from matching-based and regression studies provided in Waddington *et al*. (2012).

Effect sizes for continuous outcomes

For studies using parallel group or matching strategies[14] the SMD and its standard error are computed as follows (Borenstein *et al*., 2009):

$$SMD = \frac{\bar{Y}_r - \bar{Y}_c}{S_p} \; SE = \sqrt{\frac{n_t + n_c}{n_c * n_t} + \frac{SMD^2}{2*(n_c + n_t)}}$$

where $\bar{Y}_r$ is the outcome in the treatment group, $\bar{Y}_c$ is the outcome in the control group, n_c is the sample size of the control group, n_t is the sample size in the treatment group and S_p is the pooled standard deviation.[15]

For studies using a regression analysis to address attribution of impact (cross sectional OLS regressions, instrumental variables, difference-in-difference multivariate regressions), SMD and its standard error are estimated as follows (Keef and Roberts, 2004):[16]

$$SMD = \frac{\hat{\beta}}{\hat{\sigma}} \; SE = \sqrt{\frac{SMD^2}{v-2} * (\frac{v}{t^2} + v * [c(v)]^2 - v + 2)}$$

[14] Note that for studies using a matching strategy the outcome level for the treatment group and control group used to estimate the effect size is the outcome level for each group after matching. If Kernel approach is used it is recommended to substitute $\bar{Y}_c$ in the formula with $\bar{Y}_r$-ATET (Average Treatment effect on the treated).

[15] There are two main categories of SMD, Cohen's *d* and Hedges *g*. The difference between them lies in the strategy to estimate the pooled standard deviation, S_p. For Cohen's *d*, S_p refers to the standard deviation of the dependent variable for the entire distribution of observations in the control and treatment group. For Hedges *g*, S_p is estimated as follows:

$$S_p = \sqrt{\frac{(n_c - 1) * S_c^2 + (n_t - 1) * S_t^2}{n_t + n_c - 2}}$$

Hedges *g* is preferable, though the use of *g* or *d* will depend on the availability of data.

[16] For studies with large n, c(v) is considered equal to 1. Otherwise, please see below footnotes for c(v) computation.

Where β refers to the coefficient of the treatment variable in the regression, $\hat{\sigma}$ is the pooled standard deviation[17], v is n-k degrees of freedom.

SMD effect sizes need to be corrected for sample bias by applying the following correction factor to the SMD calculations:

a) for studies using a parallel group or a statistical matching-based design (Ohlin, 1981):

$$SMD_{corrected} = SMD_{uncorrected} * \left[1 - \frac{3}{4 * (n_t + n_c - 2) - 1}\right]$$

b) for studies using a regression based approach (Keef and Roberts, 2004):[18]

$$SMD_{corrected} = SMD_{uncorrected} * c(v)$$

For continuous outcomes, whenever the data reported or obtainable from the authors was not sufficient to estimate SMD, it was necessary to estimate response ratios, which offer greater possibilities for estimation. Response ratios measure the proportionate change in the outcome between the intervention and the control group that is caused by the intervention (Hedges *et al*. 1999). The formula is the same as the formula for calculating risk ratios, as reported below (following Borenstein *et al*., 2009 and Keef and Roberts, 2004). [19]

Effect sizes for dichotomous outcomes

Treatment effects of dichotomous outcome variables are converted into Risk Ratios (RR) and 95% confidence intervals. RRs measure the ratio between two proportions – the dichotomous outcome level in the treatment group and the dichotomous outcome level in the control group. For studies using a parallel group or statistical matching-based strategy, the RR and its standard error are estimated as follows (Borenstein *et al*., 2009):

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[17] The calculation of the pool standard deviation from regression approaches vary for the two main types of SMD. While in the Cohen's d SMD $\hat{\sigma}$ is the standard deviation of the dependent variable both for all the individuals in the treatment and control group, in the Hedges g SMD $\hat{\sigma}$ is the standard deviation of the error term in the regression.

[18] Where $\frac{1}{c(v)} = \sqrt{\frac{v}{2}} * \frac{\Gamma(\frac{v}{2} - \frac{1}{2})}{\Gamma(\frac{v}{2})}$ where Γ() is the gamma function and v is the n-k degrees of freedom.

[19] When it is not possible to compute Sp, it is also possible to estimate the standard error for response ratios based on reported t statistics for differences in means between groups (e.g. PSM, regression), as $SE(R) = \exp(\frac{Ln(R)}{t})$, where ln(R) is the natural logarithm of the response ratio and t is the t-statistic of the significance of the effect, e.g. the t-statistic of the regression coefficient. For some maximum likelihood regression models such as Logit or Probit, the impact effect from this regression coefficient needs to be used. For difference-in-difference multivariate regression model the response ratio can be calculated as $RR = e^{\beta} * 100$.

$$RR = \frac{\bar{Y}t}{\bar{Y}c} \; SE = S_p^2 * (\frac{1}{n_t * (\bar{Y}_t)^2} + \frac{1}{n_c * (\bar{Y}_c)^2})$$

Where $\bar{Y}t$ is the mean outcome in the treatment group, $\bar{Y}c$ is the mean outcome in the control group, n_c is the sample size of the control group, n_t is the sample size in the treatment group and S_p is the pooled standard deviation.[20]

For regression-based studies, RR and its standard errors are estimated as follows:[21]

$$RR = \frac{\bar{Y}c + \beta}{\bar{Y}c} \; SE = \hat{\sigma} * (\frac{1}{n_t * (\bar{Y}_c + \beta)^2} + \frac{1}{n_c * (\bar{Y}_c)^2})$$

where β is the coefficient of the treatment effect, $\bar{Y}c$ is the mean outcome in the control group, n_c is the sample size of the control group, n_t is the sample size in the treatment group and $\hat{\sigma}$ is the pooled standard deviation.[22]

The RD is an absolute measure and sensitive to the baseline risk. RD and its standard errors are estimated as follows

$$RD = \frac{A}{A+B} - \frac{C}{C+D} \qquad SE = \sqrt{\frac{AB}{(A+B)^3} + \frac{CD}{(C+D)^3}}$$

where A is the number of cases with the event on the threated group, B the number of cases with no event on the threated, C the number of cases with the event on the controlled, and D the number of cases with no event on the controlled group.

This systematic review includes different study designs that assess the effects on different measures of the same outcome. For example, studies using a difference-in-differences approach would provide the impact of the programme on the growth rate of the outcome. Other studies that use a propensity score matching approach would provide the impact of the programme on the level of the outcome. Since the response ratio measures the proportional change in an outcome of an intervention, it does not seem unreasonable to combine the response ratios of studies measuring

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[20] There are different approaches to the estimation of the pooled standard deviation. The most commonly used is Hedges method: $S_p = \sqrt{\frac{(n_c-1)*S_c^2 + (n_t-1)*S_t^2}{n_t+n_c-2}}$ Cohen's method uses the standard deviation of the dependent variable as the pooled standard deviation.

[21] For some maximum likelihood regression models such as Logit or Probit (for dichotomous outcomes) and Tobit (for continuous outcomes), it is not possible to use the regression coefficient to estimate the RR. In such a case, β refers to the "impact effect" calculated from the regression coefficient for Logit, Probit or Tobit models.

[22] There are two main approaches to the calculation of the pooled standard deviation from regression-based studies. While in the Cohens approach $\hat{\sigma}$ is the standard deviation of the dependent variable both for all the individuals in the treatment and control group, in the Hedges approach $\hat{\sigma}$ is the standard deviation of the error term in the regression, Waddington *et al.* (2014)

impacts of an intervention on levels with studies assessing impacts on growth rates of outcomes.[23]

Average standardised treatment effect

Some of the studies report average standardised treatment effects following Kling *et al*. (2004)'s methodology. They combine several measures for the same outcome into a unique average standardised treatment effect (ASE), by estimating a seemingly unrelated regression system for K related outcomes:

$$Y = [I_K \otimes (T\ X\ )]\ \theta + \upsilon,$$

where I_K is a *K* by *K* identity matrix.

The average standardised treatment effect is estimated as

$$\widetilde{\beta} = \frac{1}{K}\sum_{k=1}^{K}\frac{\widetilde{\beta_k}}{\widetilde{\sigma_k}},$$

where $\widetilde{\beta_k}$ is the point estimate on the treatment indicator in the k^{th} outcome regression and and $\widetilde{\sigma_k}$ is the standard deviation of the control group for outcome *k* (Björkman de Walque and Svensson, 2013).

As the authors do not report a single effect for each of these *K* outcomes, we were not able to compute RR nor RD for them, so we simply report the ASE, as it is a standardised effect in itself, which is interpreted in the same way as SMDs.

Unit of analysis

For clustered designs, the assessment of the unit of analysis error is based on whether the unit of analysis is different from the unit of treatment assignment. If this is the case, the review assesses whether the authors take clustering into account in the analysis (for example using multilevel model, variance components analysis, cluster level fixed effects, and so on).

No adjustments were required as all studies included in the meta-analysis reported clustered standard errors.

Missing data

Many quasi-experimental studies used in impact evaluation in economics and political science do not report the information required to calculate standardised mean differences. In those cases, we contacted the authors to obtain it, and when needed, we compute response ratios, which measure the proportional change in an outcome in the situation in the intervention group relative to that in the comparison

[23] On the other hand it would not be meaningful to combine standardised mean differences or mean differences of studies measuring impact in corruption levels with studies measuring impact on growth rate of corruption. Indeed, the mean differences approaches might require included studies to use not only the same outcome but also the same measure of outcome, preventing the aggregation of results of studies that use study designs based on panel data (cross-sectional before versus after) and those based on cross-sectional data only.

group, giving a similar interpretation to a risk ratio. Borenstein *et al.* (2009) define this as $R= X_t / X_c$, where R is the response ratio effect size, X_t is the mean outcome in the treatment group and X_c is the mean outcome in the comparison group. The response ratio provides a measure of the relative change in an outcome caused by an intervention. In other words, the response ratio quantifies the proportionate change that results from an intervention.

3.3.5 Dependent effect sizes

For dependent effect sizes, where multiple outcome measures are reported by a sub-group, data is collected at multiple time points, or when the impacts of the programme on multiple outcomes measuring the same outcome category are reported, we combined groups from the same study prior to meta-analysis, in order to avoid problems of results-related choices, including one effect estimate per study and intervention in a single meta-analysis. Following Waddington *et al*. (2014), in which multiple outcomes were reported from alternate specifications, we selected the specification according to likely lowest risk of bias in attributing impact, or according to the authors' criteria[24]. In some cases, where studies have reported multiple effect sizes from different specifications, and we were not able to choose a preferred specification, we have calculated a synthetic effect size using appropriate formulae to recalculate variances according to Borenstein *et al*. (2009, chapter 24) and Higgins and Green, 2011, Chapter 16[25].

$$\overline{ES} = \frac{\sum_{i=1}^{m} ES_i}{m}$$

$$SE(\overline{ES}) = \sqrt{\frac{1}{m^2}\left(\sum_{i=1}^{m} V(ES_i) + \sum_{i \neq j} r_{ij} SE(ES_i)\, SE(ES_j)\right)}$$

The correlation between the effect sizes was calculated whenever possible using the databases for the papers, only Piper and Korda (2010) report the correlation matrix in the text, for the rest of the studies we assume correlations of 0.5. For those studies, we did a sensibility test, using extreme values for the correlations, and the conclusions on the effect sizes significance remained unchanged.

When studies used a single control group and several treatment groups the effects of each treatment are not independent of each other as the control group is the same for each intervention. To solve this problem, we follow the same procedure than to face the dependence produced by multiple outcomes, we computed a summary

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[24] For instance, in the case of Björkman and Svensson (2009), the effect of the intervention on some outcomes was computed in two ways: using a difference-in-difference estimator, and using an OLS estimator. In these cases, we chose the difference-in-difference estimator because, as the authors say, the OLS estimates are less precisely estimated.

[25] The same procedure was applied for computing an overall effect size from effect sizes arising from different regions, different age groups, different surveys, etc.

effect for the combination of interventions versus control, creating a composite variable which is simply the mean of the effect of each treatment versus control. The variance of this composite would be computed based on the variance of each effect size as well as the correlation between the two effects. This correlation can be estimated accurately based on the number of cases in each group as explained in Borenstein *et al*. (2009, chapter 25).

3.4 Data synthesis

The review synthesises quantitative data on effects to assess whether the intervention of interest works to improve service delivery outcomes and reduce corruption (objectives question 1), and mix of quantitative studies on intermediate outcomes with their companion sibling papers, which are useful to provide context and to explain the mechanisms behind the effects (objectives question 2). Finally, we conducted moderator analyses to assess which factors moderate effects on intermediate and final outcomes (objective question 3).

3.4.1 Review question (1): Effectiveness synthesis

We synthesised the evidence on effects using meta-analysis. Following Wilson *et al.* (2011), our a-priori rule for conducting meta-analysis required two or more studies, each with a computable effect size of a common outcomes construct (potentially measured in different ways), and similar comparison condition.

To account for the possibility of different effect sizes across studies, we used a random effects meta-analysis model, since the CMIs were carried out in different countries, with different contexts, for participants with different demographic characteristics, and with differences in intervention design and implementation. By accounting for the possibility of different effect sizes across studies in this way, random effects meta-analysis produces a pooled effect size with greater uncertainty attached to it, in terms of wider confidence intervals than a fixed effect model.

We used Stata software to estimate the meta-analyses, and effect sizes are reported using forest plots (Stata Corporation, College Station, TX, USA).

We estimated an aggregated meta-analysis for all types of interventions for each primary outcome (5). Initially, we anticipated running one meta-analysis for each outcome, and then decomposing into stratified meta-analyses for each CMI. However, given the low number of studies found, we decided that the breakdowns by intervention would be meaningless, except for a few outcomes[26]. We also decomposed the analysis by sector in which service was provided (e.g. education, health, infrastructure, etc.) and perform some sensitivity analyses, namely by study design and region. However, the results of these exercised should not be generalised given the low number of studies involved in them.

[26] We only decompose the analysis by CMIs for some measures of *Access to service* and *Improvement in prevalence condition*, where we found more papers to assess the effect size. For details, please see chapter **Error! Reference source not found.**.

Assessment of heterogeneity

We assessed heterogeneity of effects across studies, using the I^2 statistic to provide an overall estimate of the amount of variability in the distribution of the true effect sizes (Borenstein *et al.*, 2009).

3.4.2 Review question (2): CMIs mechanisms synthesis

For the synthesis of evidence relating to question 2, we used both qualitative and quantitative approaches.

For studies measuring intermediate and final outcomes, we used a narrative synthesis approach, where themes were identified based on the links and assumptions in the theory of change model described above. The low number of comparable effect sizes prevented us from running meta-regression analyses of the associations between intermediate and final outcomes.

3.4.3 Review question (3): Moderator analyses

For the synthesis of evidence relating to question 3, we attempted to use a quantitative approach. The a priori decision rule for performing meta-analysis following Wilson *et al*. (2011) required to consider two or more studies, in the end, given the restriction on the number of studies, we only were able to perform a modest analysis on the effect of the design of the CMI on the improvement in a prevalence condition. The coding sheet in Appendix B collects information about the differences within each intervention whenever possible. In particular, for information campaigns, it included a capacity building component where information on how to monitor providers is disseminated, and for scorecards and social audits, whether they involved facilitated meetings with providers and politicians. Finally, we studied whether length of exposure (measured as length of CMI programme implementation, and length of post-implementation follow-up period) had any impact on the effectiveness of the CMIs. Given the final low number and variation of the studies selected, we were only able to investigate in some extent geographical variation only for some primary outcomes.

3.4.4 Integrated synthesis (review questions 1, 2 and 3)

We used the programme theory (Figure 1) as a framework for integrating the findings from synthesis of review questions (1), (2) and (3) with the aim of providing an integrated narrative synthesis along the causal chain addressing the objectives of the review.

3.4.5 Sensitivity analysis

Whenever the number of studies was high enough we perform sensitivity analysis in order to account per differences by study design, region and the existence of outliers.

3.4.6 Analysis of publication bias[27]

Additionally, whenever possible, we studied whether published and unpublished results tend to differ significantly, as a test for publication bias. Because statistical significance is often regarded as a requirement for publication, one symptom of publication bias is an unusually large number of published p-values just below the 0.05 threshold (Gerber and Malhotra, 2008a, 2008b). Another symptom is larger reported effects among studies with smaller samples; because smaller studies tend to have larger standard errors, their estimated effects need to be larger in order to achieve significance at the $p < 0.05$ level. We tested for possible publication bias using funnel plots and Egger *et al*.'s (1997) test. However, the low power of these tests due to the low number of studies prevented us from having conclusive findings.

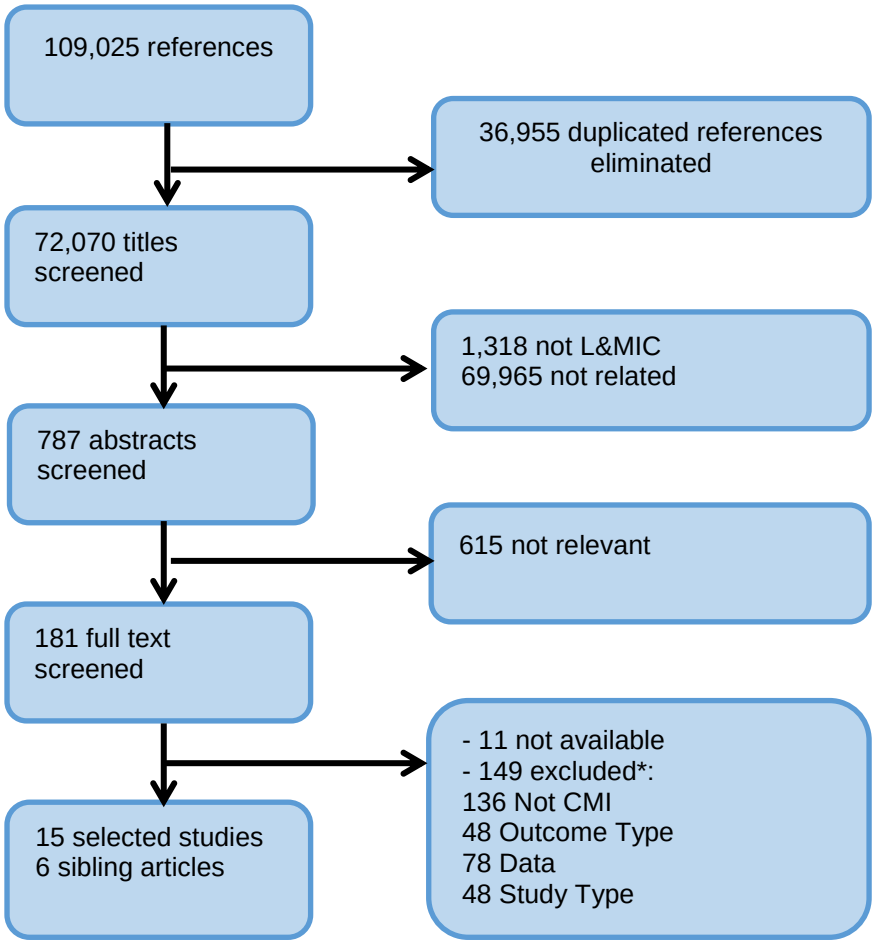
[27] A broader concept of publication bias would include not only published results, but also working papers as being affected by the same syndrome. Since we do not have access to those results which authors decided not to put on paper or circulate in the academic community (so-called file drawer problems), we will not be able to test for that type of publication bias.

4. Results

4.1 search results

The following figure shows a summary of the search and selection process.

Figure 2: Search and selection process



** Reasons for exclusion:*

- *Not CMI: the study does not assess a community monitoring intervention.*
- *Outcome type: the study does not have outcomes on corruption, service delivery or quality of services*
- *Data: the study does not collect data at the individual or the community level*
- *Study Types: the study does not follow any of the methodologies accepted, or it does not provide information on methodology.*

The search strategy presented in section 3.2 yielded 109,017 references, 36,955 of which were eliminated as duplicates, leaving 72,070 to be screened. Of the 72,070 potentially relevant papers identified, 65,044 were identified from databases, 7,009 from Google or Google scholar, citation tracking and bibliographic searches of reviews and 17 from contact with organisations and researchers. 71,283 were excluded at the title screening stage as they were irrelevant or not based in a low- or

middle income country, leaving 788 studies to be screened at abstract. Of these, 181 studies were assessed for inclusion at full text, 136 did not assess a community monitoring intervention, 48 did not have outcomes on corruption, service delivery or quality of services, 78 did not collect data at the individual or the community level, and the study does not follow any of the methodologies accepted, and 48 did not provide information on methodology.[28] Fifteen studies met the inclusion criteria, and six sibling studies were identified.

4.2 Characteristics of included studies

We included studies from three regions: six in Africa, seven in Asia and two in Latin America. Uganda and India had the largest presence of CMI impact evaluations, with four studies conducted in each country. We also identified two studies from Indonesia and one each from Benin, Liberia, Colombia, Pakistan and Mexico. Descriptive information on the 15 included studies assessing the effects of Community Monitoring Interventions (CMIs) is presented in Annex G.

The included studies evaluated the effects of 23 different CMI interventions. Information Campaigns were the most commonly studied intervention. Specifically, there were 10 examples of Information Campaigns (IC), three examples of Scorecards, five examples of Social Audits (SA), and two that combined Information campaigns and Scorecards[29]. We did not identify any studies on Grievance Redress Mechanisms. These programmes targeted different sectors, with most studies focusing on the education sector (9), followed by health (3), infrastructure (2) and promoting employment (1). **Error! Reference source not found.** includes additional information that describes each study.

The review includes studies assessing the effects of CMIs on all primary outcomes of interest. Improvement in prevalence condition was the outcome most commonly reported in the studies, followed by access to services. Eleven studies reported on improvement in prevalence condition, seven on access to service, three on perception of corruption, two on average waiting time to get the service and two on forensic economic estimates for corruption.

There are differences between the specific measures used to assess any one outcome. For example, in health, access could be measured as utilization, coverage or immunizations. Even in education, where different interventions measure pupil learning through test scores, the differences in the population of interest, age, type of test, etc. imply differences in the actual instrument.

For all outcomes, we have attempted to calculate effect sizes and 95 per cent confidence intervals. However, as reported in Table 14, in two studies insufficient

[28] Appendix F presents a list of the excluded studies along with the reasons for their exclusion.

[29] Appendix D describes these interventions in more detail.

information was provided in order to estimate standard errors and therefore statistical precision of the effect sizes.

The included studies used a range of study designs including randomised assignment (10), and quasi-experimental studies (5). In eight of the eight RCT the control group received no form of intervention, in the other two, the comparison group received or a simplified version of the intervention (Olken, 2007), or a combination of no treatment and a different treatment (Pradhan *et al.*, 2014). The quasi-experimental studies have more variation, from relying on the distance to a media outlet, to compare with a previous round of a social audit.

We can notice that we have a wide range of studies, from studies that were designed only to inform whether a given programme improved outcomes for the treatment group as compared to the control group, to more complex studies, with many treatment arms, that attempt to measure not only whether the intervention brought any positive effect but also to understand the pathway of change. As we anticipated in the protocol, we face different ways to measure the outcomes of interest.

Table 5 summarises some other important features of the included studies. Follow-up periods varied from less than one year to over 12 years, and most studies report clustered standard errors.

Table 5: Detailed descriptive information on included studies

Study	Intervention	Study design / Attribution method	Target and Control group	Follow-up time period[1]	Unit of analysis error assessment	Internal validity assessment
Afridi, F. and Iversen, V. (2013)	Social audit (Second audit) Social audit (Third audit)	Difference-in-differences (DID)	This is a panel data set that comprises of official data from three rounds of social audits, with an initial sample of 300 GPs in eight districts of Andhra Pradesh. It compares the results of the second (264 audits) and third audit (166) with those of the first one (284). 548 number of audits, from which 284 are first audit, and 264 are a second audit. (Clusters at GPs level: 300)	Five years	Low probability of relevant unit of analysis error: standard errors are clustered at GP level.	Low risk of bias
Andrabi, Das and Khwaja (2013)	Scorecard	Difference-in-differences (DID)	Treatment group: Scorecards. Control Group without scorecard 112 Villages were chosen randomly from among those with at least one private school according to a 2000 census of private schools. First, Grade 3 children in all primary schools were tested and then, in a randomly selected 50 per cent of the villages, were disseminated report cards with the results of school and	One year and two years	Low probability of relevant unit of analysis error.	Low risk of bias

Study	Intervention	Study design / Attribution method	Target and Control group	Follow-up time period[1]	Unit of analysis error assessment	Internal validity assessment
			child test scores for all schools (804) and tested children (12110)			
Banerjee *et al.* (2010)	Information campaign (IC) Treatment 1 Information campaign (IC) Treatment 2 Information campaign (IC) Treatment 3	RCT	85 villages as control group and 195 as target group. The final sample for the baseline survey consisted of 2,800 households, 316 schools, 17,533 children (ages 7–14) tested in reading and math, and 1,029 VEC member interviews from the 280 villages. In the endline survey, 17,419 children were tested, a sample that includes all but 716 of the children in the baseline and, thus, very little attrition from the baseline survey (the attrition is evenly spread across the various treatment and control groups).	One year	Low probability of relevant unit of analysis error: standard errors are clustered at village level.	Low risk of bias
Barr *et al*. (2012)	Scorecard Intervention 1: standard scorecard	RCT, Difference-in-differences (DID)	100 rural primary schools: 30 schools were assigned to each of the standard and participatory treatment arms, with the remaining	Two years and four	Low probability of relevant unit of analysis error: for some outcomes, authors use	Low risk of bias

Study	Intervention	Study design / Attribution method	Target and Control group	Follow-up time period[1]	Unit of analysis error assessment	Internal validity assessment
	Scorecard Intervention 2: participatory scorecard		40 serving as a control group. 3512 students, we assume it follows the same division as the schools	mon ths	DID which accounts for clustering at school level and include strata-years controls, but for they report strata control.	
Björkman and Svensson (2009)	Scorecard + information campaign	RCT	25 facilities/ communities randomly assigned as control group and 25 facilities/ communities randomly assigned as target group	One year	Low probability of relevant unit of analysis error: authors include district and facilities fixed effects and, when possible, they estimate DID. Standard errors are clustered by catchment areas.	Low risk of bias
Björkman, de Walque and Svensson (2013)	Scorecard + information campaign	Cross-section (regression),Difference-in-differences (DID), Seemingly unrelated regression	25 facilities/ communities randomly assigned as control group and 25 facilities/ communities randomly assigned as target group	Two years	Low probability of relevant unit of analysis error: authors include district and facilities fixed effects and, when possible, they estimate DID. Standard errors are clustered by catchment areas.	Low risk of bias
	Information Campaign Intervention (IC)		12 facilities/ communities randomly assigned as control group and 13 facilities/ communities randomly assigned as target group	One year		

Study	Intervention	Study design / Attribution method	Target and Control group	Follow-up time period[1]	Unit of analysis error assessment	Internal validity assessment
		(Kling *et al*., 2004)				
Gertler *et al.* (2008)	Scorecard	Difference-in-differences (DID)	Treatment schools are those schools that first received the AGE programme at the beginning of any school year between 1998-99 and 2001-02, and had AGE continuously ever since (N=2544). Those that had not received AGE before school year 2002-03 constitute the comparison group (N=3483).	Twelve years	Low probability of relevant unit of analysis error: standard errors are clustered at school level.	Low risk of bias
Keefer and Khemani (2011)	Information Campaign Intervention (IC)	Cross-section, Quasi-experimental	In the target group are the households and children in the villages which access to the radio. In the control are those in villages without access to the radio. 210 villages (4200 households) from 21 communes	Not applicable	Low probability of relevant unit of analysis error: standard errors are clustered at commune level.	Low risk of bias
Molina (2013b)	Social audit	Cross section-Matching	The random sample contains 390 households for the 13 projects in the treatment group and 410 for the 11 projects in the control group.	Not applicable	Low probability of relevant unit of analysis error: standard errors	Low risk of bias

Study	Intervention	Study design / Attribution method	Target and Control group	Follow-up time period[1]	Unit of analysis error assessment	Internal validity assessment
					are clustered at commune level.	
Olken (2007)	Social Audit - Invitations	RCT, Cross-section (regression)	Social Audit with Invitations vs. Social Audit. 199 villages (audit 94 , control 105)	OnOne year	Low probability of relevant unit of analysis error: standard errors are adjusted to allow for correlation within subdistricts. The estimations include engineering team fixed effects and fixed effects for each subdistrict (i.e., the stratifying variable for the participation experiments).	Low risk of bias
	Social Audit - Invitations + comments		Social Audit with Invitations plus comments vs. Social Audit 202 villages (audit 96, control 106)			
Pandey *et al.* (2007)	Information campaign (IC)	Difference-in-differences (DID) and Cross-section	105 villages (1045 households at the baseline), from which 55 (548 households) intervention and 50 (497 households) control	One year	Unclear: for some outcomes, authors use DID which accounts for clustering in the treatment allocation, but for other outcomes they report the results of a multivariate random-	Low risk of bias

Study	Intervention	Study design / Attribution method	Target and Control group	Follow-up time period[1]	Unit of analysis error assessment	Internal validity assessment
					effect regression for which the specification is not reported, although they state that random effects are at the village cluster level and standard errors are clustered at the village cluster level. They also argue that the regression adjusts for total population of the village cluster, district size, household caste, and highest education attained in the household.	
Pandey, Goyal and Sundararaman (2009)	Information campaign (IC) Second IC in one region	RCT	610 villages from which 340 intervention and 270 control	Two to four months	Low probability of relevant unit of analysis error: standard errors are clustered at village level.	Medium risk

Study	Intervention	Study design / Attribution method	Target and Control group	Follow-up time period[1]	Unit of analysis error assessment	Internal validity assessment
Piper and Korda (2010)	Information campaign (IC)	RCT	Groups were randomly selected and clustered within districts, such that several nearby schools were organised together. 117 schools, from which 59 are control. The intervention was targeted at grades 2 and 3.	One year	Low probability of relevant unit of analysis error: standard errors are clustered at school level.	Medium risk
Pradhan *et al.* (2014)	Training (T): IC Linkage (L): IC	Difference-in-differences (DID)	2 provinces, nine districts, 44 subdistricts and 520 schools. Training: treatment group: 230 schools, 1060 students; control group 190 schools and 2120 students. Linkage: treatment group: 240 schools and 893 students; control group: 180 schools and 2120 students. The authors also include a third treatment that we do not consider as it is not of the type of CMI considered in this review, the intervention introduced changes in the election of the committee. They also explore combinations of treatments given that some	One year and 10 months	Low probability of relevant unit of analysis error: all estimations include stratum fixed effects because assignment of treatment was within each stratum and the robust standard errors for regressions with test scores are clustered at the school level.	Low risk of bias

Study	Intervention	Study design / Attribution method	Target and Control group	Follow-up time period[1]	Unit of analysis error assessment	Internal validity assessment
			individuals in the control groups for each treatment had received the other treatments.			
Reinikka and Svensson (2011)	Information campaign (IC)	Difference-in-differences (DID), Instrumental Variable (IV)	Using distance to newspapers outlets the authors construct the treatment and control group. 218 schools for which survey data are available for the years 1991-95 and 2001, and a sample of 388 (218 + 170) schools for which survey data are available in 2001.	Not applicable	Low probability of relevant unit of analysis error: standard errors are clustered at school level.	Low risk of bias
Notes:	IC Information campaign, SA Social Audit; SC Scorecard; DID: Differences-in-differences, IV: Instrumental Variable; OLS: Ordinary Least Squares estimation 1/ Average years from intervention to endline survey.					

4.3 Sibling articles

We identified six additional documents related to the programmes analysed, and we describe them in Table 6.

Table 6: Related studies

Included Study	Additional Study	Study objectives	Country	Programme	Methods of data collection	Methods of analysis
Afridi, F. and Iversen, V. (2013)	Singh and Vutukuru (2009)	To evaluate the effectiveness of social audit as a tool to enhance accountability by measuring the impact of social audit on the implementation an employment guarantee programme.	India	National Rural Employment Guarantee Scheme, the flagship employment guarantee programme of the Government of India, in the state of Andhra Pradesh.	Case study. Quantitative data collected from the programme. A reporting format designed for the qualitative findings of each social audit carried out in each village. Interviews were conducted with Directors, Social Audits, Department of Rural Development, government of Andhra Pradesh	Mix of quantitative (DID) and qualitative methods.
Banerjee *et al.* (2010)	Banerjee *et al.* (2007)	To assess community participation in monitoring education services. To evaluate the impact of advocacy and public action information campaigns	India	Universalisation of elementary education (Sarva Shiksha Abhiyan (SSA)) and Pratham India Education	Data from a survey of parents, teachers, VECs, and children which was undertaken in the rural district of Jaunpur in the	Descriptive statistics using data from the survey.

Included Study	Additional Study	Study objectives	Country	Programme	Methods of data collection	Methods of analysis
		on local participation to improve school functioning and to strengthen learning outcomes of the children.		Initiative (Pratham).	eastern part of the state, during March-April 2005.	
Björkman and Svensson (2009)	Björkman and Svensson (2010)	To test whether social heterogeneity can explain why some communities managed to push for better health service delivery while others did not.	Uganda	Citizen report cards aimed at enhancing community involvement and monitoring in the delivery of primary health care, initiated in rural areas in Uganda in 2004.	The authors use a smaller subset of the data in Björkman and Svensson (2009). Specifically, they exploit detailed utilization data –on out-patients, delivery, antenatal care, and family planning – obtained directly from records kept by facilities for their own need (i.e. daily patient registers). The data set covers 50 primary health care providers in nine districts in Uganda of which half took part in the experiment (the remaining	Seemingly unrelated regression system.

Included Study	Additional Study	Study objectives	Country	Programme	Methods of data collection	Methods of analysis
					constitute the control group).	
Olken (2007)	Olken (2004)	To assess the effect of social audits and external audits on corruption in provision of public services (roads building).	Indonesia	The Kecamatan (Subdistrict) Development Project	The data come from four types of surveys, each designed by the author and conducted specifically as part of the project: a key-informant survey, covering baseline characteristics about the village and the village implementation team; a meeting survey, containing data on the attendees and a first-hand report of discussions at the accountability meetings; a household survey, containing data on household participation in and perceptions of the project; and a final field survey, used to measure corruption in the project.	Descriptive statistics and Ordinary- least-squares (OLS)

Included Study	Additional Study	Study objectives	Country	Programme	Methods of data collection	Methods of analysis
Olken (2007)	Olken (2005)	To examine the relationship between perceptions of corruption and a more objective measure of graft, in the context of a road building programme in rural Indonesia.	Indonesia	Kecamatan (Subdistrict) Development Project	Household survey, containing data on household perceptions of the project; a field survey, used to measure missing expenditures in the road project; a key-informant survey with the village head and the head of each hamlet, used to measure village characteristics; and a meeting survey, containing data on the village accountability meetings.	Probit model and Ordinary- least-squares (OLS).
Olken (2007)	Woodhouse (2005)	The paper aims to get a sense of the anatomy of corruption in KDP villages: of how the actors perceive their interests, what motivates them, what kinds of constraints they face, and what kinds of steps they take to resolve their	Indonesia	Kecamatan Development Programme (KDP)	interviews with people involved in corruption case, from ordinary villagers to local government officials (including those accused of corruption)	The report is based on an analysis of identified corruption cases in KDP, field visits to ten villages and three provinces, and on-site interviews with central KDP project staff, KDP field consultants,

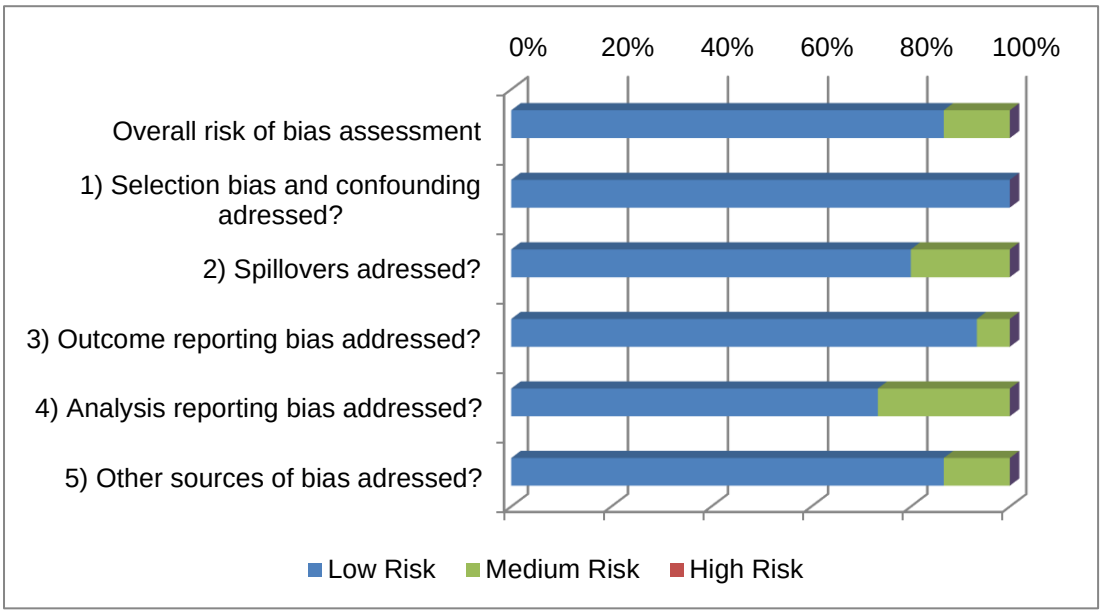
Included Study	Additional Study	Study objectives	Country	Programme	Methods of data collection	Methods of analysis
		problems. The underlying aim is to assess the kinds of anti-corruption measures that are likely to succeed in local projects that operate in a systemically corrupt environment and in an overall project whose size and breadth (20,000 villages nationwide) makes centralised control and monitoring of funds impossible. The paper also uses corruption as a lens through which to view snapshots of social and political change in Indonesian villages.				government officials, and villagers. It also makes use of information gathered during KDP supervision missions to provinces other than those visited for this report. The report especially makes use of the views of KDP's project historian and of staff from KDP's Complaints Handling Unit, who track and follow up corruption cases that get reported.

4.4 Assessment of risk bias

4.4.1 Assessment of risk of bias in included studies of effects

Taking into account the characteristics of each paper, it was possible to evaluate the internal validity and the risk of bias of the assessment of each programme. Seven studies were categorised as low risk of bias in attributing outcomes to the intervention, based on our five criteria of selection bias and confounding, spillovers, outcome reporting bias, analysis reporting bias, and other sources of bias (Andrabi *et al*., 2013; Banerjee *et al*., 2010; Barr *et al*., 2012; Björkman and Svensson, 2009; Olken, 2007; Pandey *et al*., 2007, and Pradhan *et al*., 2014). The remaining eight studies were classified as medium risk (Afridi and Iversen, 2013; Björkman *et al*., 2013; Gertler *et al*., 2008; Keefer and Khemani, 2011; Molina, 2013b; Pandey *et al*., 2009; Pandey, Goyal and Sundararaman, 2009, Piper and Korda, 2010, and Reinikka and Svensson, 2011). None were considered to have a high risk of bias. The summary report across risk of bias categories is provided in Figure 3.

Figure 3: Summary of quality appraisal across effectiveness studies



Thus, the overall risk of bias assessment is predominantly low, with 13 out of 15 papers having this level of risk, followed by two papers with medium risk of bias.[30] The inter-rater assessment, the absolute agreement intra-class correlation is 0.70 with a 95% CI [0.21, 0.95]

The full quality assessment for each study is reported in Appendix E. The table shows that included studies used a range of attribution methods. Most of them used randomised assignment in the study design. A minority of studies used quasi-

[30] This result was corroborated by having a third researcher analyse the ratings.

experimental approaches such as instrumental variables and matching (Keefer and Khemani, 2011; Molina, 2013b).

The majority of studies (11 out of 15) were adequately protected against performance bias as the units of treatment were located far from the control units. While in some cases the comparison group was selected from villages were the intervention was not carried out but were located near villages that had received the intervention[31], and in other cases, the comparison group received a different treatment or the same intervention with a different degree of intensity (for example, Afridi and Iversen, 2013; Olken, 2007), the authors took that into consideration while designing the intervention and selecting cluster for their standard errors.

We found just one case of potential outcome reporting bias, where the outcome reported was a new type of literacy test and the authors had not clearly justified the reason for using the measure over standard ones. There was no evidence in the remaining studies that outcomes were selectively reported and authors use "common" methods of estimation. Therefore, almost all studies are considered as having low risk of outcome reporting bias.

With regards to analysis reporting bias, in most of the included studies different measures for the same outcome are reported or different specification and estimation methods are applied, and in general there are no red alerts regarding other bias.

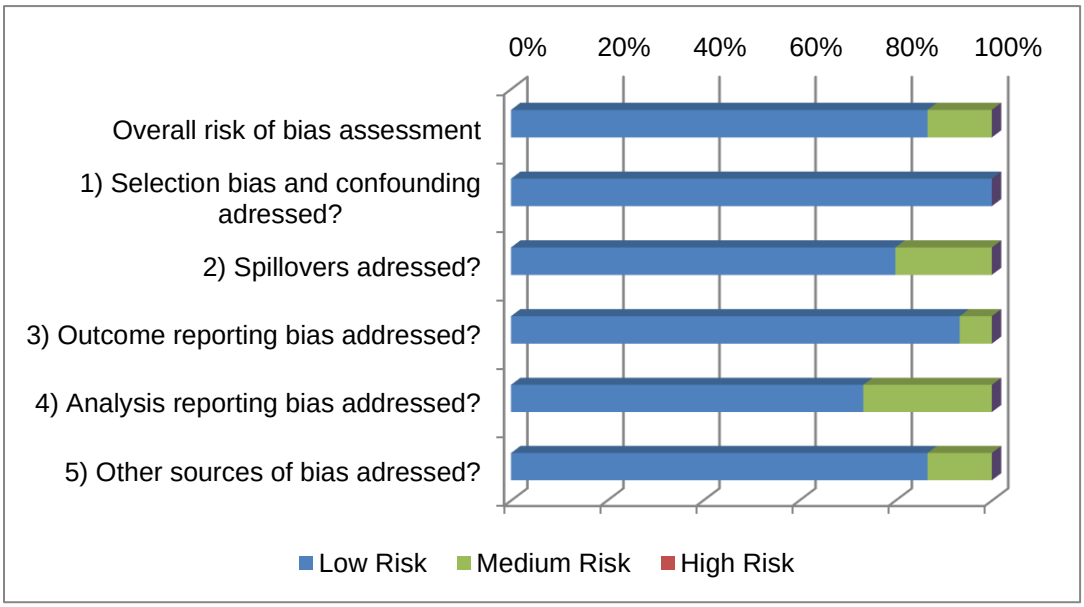
4.4.2 Assessment of risk of bias in included studies of effects to address review question (2)

For this section, we appraise 11 papers, five of them were already included in the previous subsection, and the other six are sibling studies. Four Olken (2004, 2005, 2007) and Woodhouse (2005) analyse the social audit evaluated in Olken (2007). We also included Banerjee *et al*. (2007, 2010), Björkman and Svensson (2010), Molina (2013b), Pandey *et al*. (2007), Pradhan *et al*. (2014), and Singh and Vutukuru (2009).

Thus, the overall risk of bias assessment is low, with ten out of 11 papers having this level of risk, and Woodhouse (2005) with a medium risk of bias. The full quality assessment for each study is reported in Appendix E. The inter-rater assessment, the absolute agreement intra-class correlation is good at 0.55, although is not statistically significant with a 95% CI [-0.19, 0.93].

[31] All randomised field experiments report no statistical difference between treatment and control groups.

Figure 4: Summary of quality appraisal across studies for question (2)



5. Results of synthesis of effects

In this section, we synthesize the quantitative data from our 15 studies on effectiveness using statistical meta-analysis to assess whether the included interventions worked to improve service delivery outcomes and reduce corruption (review question 1).

We report the results of meta-analyses for the effects of CMIs on the five primary outcomes, explained in detail in section 3.1.4. Initially, we expected to run one meta-analysis for each outcome, and then to decompose into separate analyses for each CMI. However we did not identify enough studies for each intervention sub-group to do so, except for a few outcomes.

The included studies use a range of different measures to assess primary outcomes and it would not be sensible to pool conceptually different outcomes in our analysis. To avoid this problem we grouped studies only when the outcome variables represent a similar broad outcome construct and the intervention is implemented in the same sector.

In some cases, studies report the effect of more than one intervention. In those cases, we chose the interventions that could be classified as one of our four categories. In cases where more than one intervention was relevant, we pooled their effects before integrate them into meta-analysis, taking into account the correlation of the treatment and control groups between study arms to address possible

dependency[32]. This is the case of Afridi and Iversen (2013), Banerjee *et al.* (2010), Barr *et al.* (2012), Olken (2007) and Pradhan *et al*. (2014); see **Error! Reference source not found.** for details.

In the case of Afridi and Iversen (2013) we took the two interventions carried out in India reported by authors and pooled their effects on the same outcome –a measure of corruption- assuming a correlation between treatments of 0.5. We computed the pooled effect size of both treatments and its corresponding standard error following Borenstein *et al*. (2009).

Banerjee *et al.* (2010) report three different interventions that were all classified as a CMI. We pooled their effects taking into account the correlations between them[33].

Barr *et al.* (2012) also explore the effect of two different scorecard interventions that are both relevant for our analysis. In this case, we computed correlations based on sample size, following Borenstein *et al*. (2009).

We also identified two CMIs in Olken (2007). Again, we computed correlations based on sample size. Although the author reports many possible measures of corruption, we chose the most representative for our analysis.

Finally, Pradhan *et al*. (2014) report three interventions in Indonesia and different combinations of them, but we only identified two of them as falling into one of our four categories of CMIs, namely, the 'Linkage' and 'Training' interventions. In this case, we were able to compute correlations using the dataset.

In case where we identified different measures for the same outcome, we followed a similar procedure. We computed a synthetic effect size, defined as the mean effect size in that study, with a variance that takes account of the correlation among the different outcomes (Borenstein *et al*., 2009). The details of the variables considered for each outcome are presented in the corresponding tables regarding effect sizes (see below).

Finally, when we found different follow up periods for comparable interventions, we compared them considering similar horizon time. This is the case for Björkman and Svensson (2009) and Björkman, de Walque and Svensson (2013), who report both the short and the medium term impact of an intervention in Uganda, and the short term impact of another intervention in the same place. In these cases, we only run meta-analysis for short term effects.

All effect sizes were computed as continuous outcomes, excepting those from Pandey *et al.* (2007), which were computed as binary outcomes.

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[32] In some cases, these correlations were available in the studies' databases, or where easily obtainable from tables reported in the papers. When not available, we assumed a value of 0.5, and checked whether the results changed substantially for extreme correlation values.

[33] We computed them using the author's dataset.

5.1 Corruption outcomes

We identified few studies assessing the effect of CMIs on corruption outcomes, both using forensic estimates (two studies, two interventions) and perception measures (three studies, four interventions). This limits our ability to extrapolate these results. In the case of service delivery, we differentiated access from quality.

5.1.1 Forensic economic estimates

We looked for different measures of corruption in the papers considered, with the aim of extracting measures based on the application of economics to the detection and quantification of behavior (Zitzewitz, 2012), in this case, corruption. With this purpose, we extracted all measures that we could identify for each intervention. Table 7 lists the measures reported in each case.[34] We identified two studies reporting forensic measures of corruption (Olken, 2007 and Reinikka and Svensson, 2011). Olken (2007) evaluates the impact of increasing citizen participation in social audits in Indonesia on corruption, with two different treatment arms testing different variations of social audits. Villagers were invited to participate in social audits (”accountability meetings”) in both treatment arms, but in one group the invitation was accompanied by an anonymous comment form, which could be submitted in a sealed box. The results of this exercise were summarised in the accountability meetings.[35] The study reports a forensic measure of corruption, which is the percentage of missing funds in roads and ancillary projects. The effect of social audits only was SMD 0.082, 95% CI [-0.10, 0.26] and the effect of social audits with anonymous comment form was SMD 0.08, 95% CI [-0.10, 0.25]. The combined effect for both treatment arms was SMD 0.08, 95% [-0.08 - 0.24].[36]

Reinikka and Svensson (2011) evaluate the effect of systematic publication of monthly financial transfers to schools in Uganda. They found that a school close to a newspaper outlet suffers less from the capture of funds as compared to a school away from a newspaper outlet (Reinikka and Svensson, 2011).[37] The SMD shows that schools where the intervention took place had 22 per cent less corruption than the others.

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[34] In both cases, we changed the sign of the effect size so it can be interpreted properly (that is, a positive effect size means that corruption has been reduced).

[35] The study also evaluates the effect of external audits, which did reduced corruption, but we did not include it in the meta-analysis because it does not fall into any of our four intervention categories.

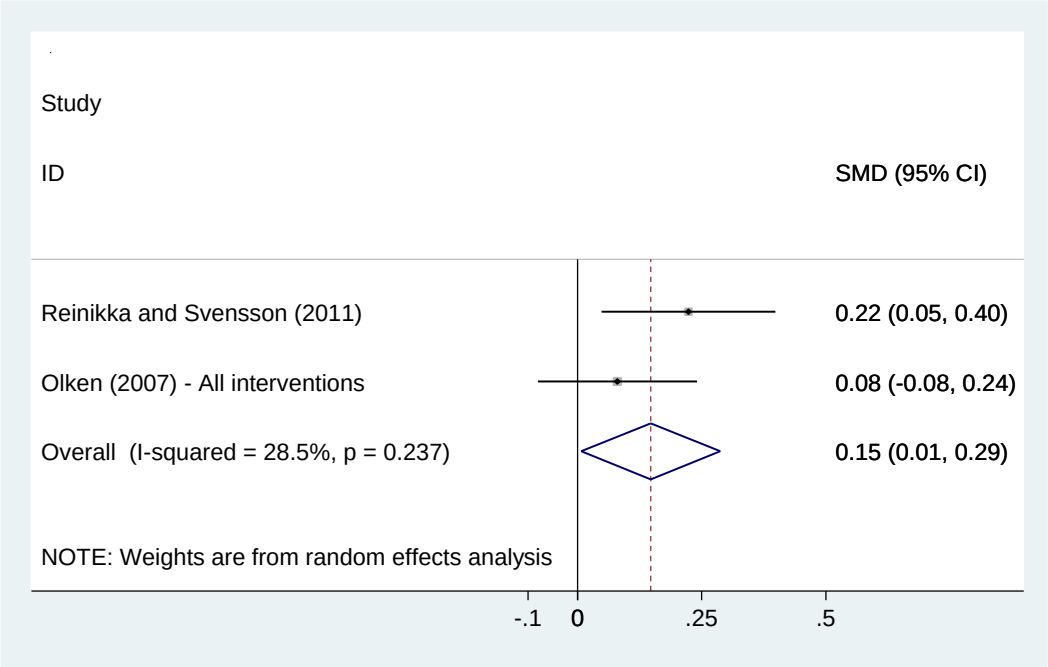
[36] This finding is consistent with those reported by the author, who argues that ‘increasing grassroots participation in monitoring had little average impact (Olken, 2007).

[37] Finally, we did not include Banerjee *et al.* (2010) as it is not a measure of corruption they use, but rather they look at whether the treatments to increase community monitoring generated additional nonteaching resources for the schools. They found that none of the interventions have any effect.

Table 7: Forensic economic estimates of corruption outcomes

Study	Variable definition	CMI Type	Effect Size	95% Confidence Interval		ES Type
Olken (2007) - Invitations	Percentage of missing funds as log reported value - log actual value (major items in roads and ancillary projects)	Social Audit	0.08	-0.10	0.26	SMD
Olken (2007) - Invitations + comments	Percentage of missing funds as log reported value - log actual value (major items in roads and ancillary projects)	Social Audit	0.08	-0.10	0.25	SMD
Olken (2007) - All interventions			*0.08*	*-0.08*	*0.24*	*SMD*
Reinikka and Svensson (2011)	Share of funding reaching school	IC	0.22	0.05	0.40	SMD
Meta-analysis			***0.15***	***0.01***	***0.29***	***SMD***

Figure 5: Forest plot for forensic economic estimates of corruption outcomes



The meta-analysis suggests that the overall effect of these interventions is positive, improving corruption outcomes in 15 per cent of cases, as is shown in Figure 5. Since Olken (2007) finds no statistically significant effects, this result is probably driven by Reinikka and Svensson (2011), who did find a positive and statistically significant effect.

1.1.1 Perception measures

Perception measures of corruption are more commonly available than forensic measures of corruption. While a less objective measure, it is difficult to detect and measure corruption objectively and because of that we included these more subjective measures.

Table 8 lists the outcome measures reported in the three studies (four interventions) that we have included in this category. We were able to compute RD for the first two studies and SMD for the third one, so we analysed them separately.

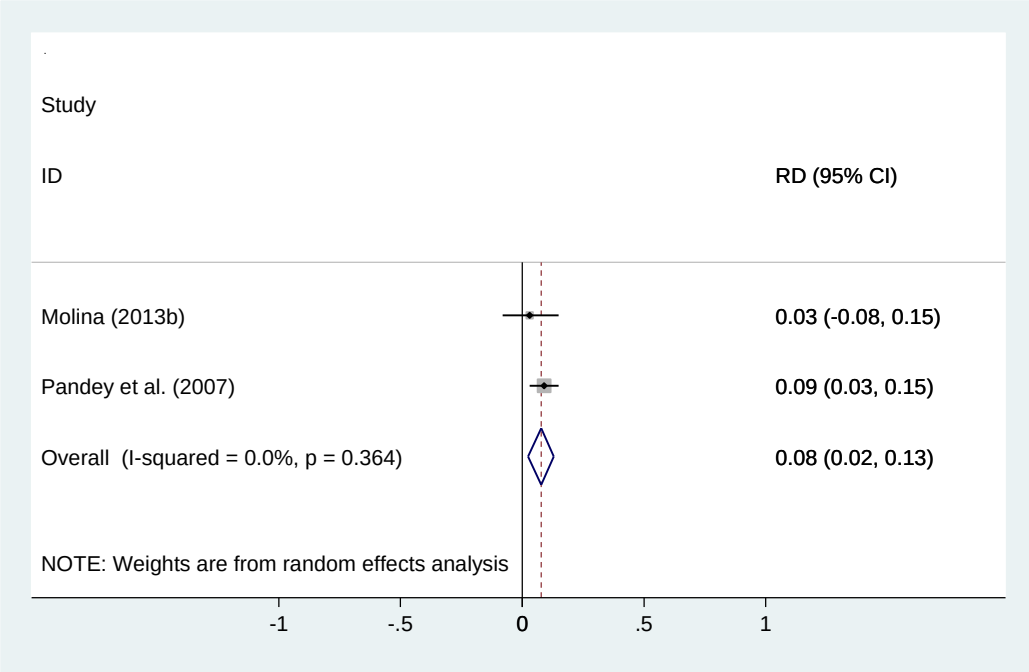
Table 8: Perception measures of corruption outcomes

Study	Variable definition	CMI Type	Effect Size	95% Confidence Interval		ES Type
Molina (2013b)	Adequacy in the Administration of Resources	Social Audit	0.03	-0.08	0.15	RD
Pandey *et al.* (2007)	Percentage of household reporting	IC	0.09	0.03	0.15	RD

	that development work has been performed in the village					
Meta-analysis			***0.08***	***0.02***	***0.13***	***RD***
Afridi and Iversen (2013) - Second audit	Total number of irregularities (reversed sign)	Social Audit	-0.22	-0.39	-0.05	SMD
Afridi and Iversen (2013) - Third audit	Total number of irregularities (reversed sign)	Social Audit	-0.23	-0.43	-0.04	SMD
Afridi and Iversen (2013) - All interventions			***-0.23***	***-0.38***	***-0.07***	***SMD***

We identified two studies assessing the effect of CMI on corruption perception measures for which comparable effect size were available. Molina (2013b) found evidence that a social audit improved the perception of the administration of resources in Colombia, while Pandey *et al*. (2007) suggest that an information campaign carried out in India increased the probability of households reporting that development work took place in the villages, which can be interpreted as a reduction in corruption. The overall effect is RD 0.08, 95% CI [0.02, 0.13], as can be seen from the forest plot presented in Figure 6. The confidence intervals are overlapping and the test of homogeneity does not suggest between study variability. Both studies report a reduction in the perception of corruption among beneficiary households.

Figure 6: Forest plot for corruption outcomes – Perception measures. Risk Differences



Afridi and Iversen (2013) assess the effect of social audits in India. They estimate the effect on reported irregularities in Gram Panchayats with one audit as compared to those with two and three audits respectively. The effect after two audits was SMD -0.22, 95% CI [-0.39, -0.05] and after three audits SMD -0.23, 95% CI [-0.43, -0.04]. The overall average effect across both groups was SMD -0.23 [-0.38 - 0.07], suggesting a worsening in corruption outcomes of 23 per cent in Gram Panchayats after these interventions, and a stronger effect with two or more social audits, as compared to those with one audit only. The authors explain that “maladministration and corruption could be underreported in initial audit rounds when beneficiaries may be more sceptical of and have less confidence in the integrity of the audit process. Alternatively, beneficiaries may, initially, be less aware of their MGNREGA entitlements. In both instances we would expect the number of complaints to surge even if the quality of programme delivery remained the same. Similarly, if the quality of social audits improves through audit team learning, which is plausible, but not a given (…), the growing sophistication of audit teams should increase the number of reported harder to detect irregularities and the number of such complaints filed by the audit team” (Afridi and Iversen, 2013).

5.2 Service delivery outcomes

In the case of service delivery, we differentiated access from quality. We also perform separate analysis by sector and by outcome.

5.2.1 Access to services

In this section we begin with health services and present the results for utilization, immunization and other measures of access, followed by results for enrolment and dropout rates in the education sector.

Health

We identified two studies that assessed at the impact of CMIs on utilization. Björkman and Svensson (2009) evaluate the same intervention, a combination of a scorecard with an information campaign both in the short and in the medium term,[38] using the same group of 50 facilities/communities that were randomly assigned to treatment and control group.

In addition, Björkman, de Walque and Svensson (2013) assess a new intervention, an information campaign with new treatment and control groups in which 25 new facilities were randomly assigned to a treatment group (13 units) and control group (12 units). This intervention differs from the previous one since it does not include a scorecard with relevant information about the health service provision.

In these studies, authors report an 'average standardised treatment effect' following Kling *et al*. (2004)'s methodology, that can be combined into one meta-analysis given their homogeneity and given that they are comparable as they all imply better access to health services. Table 9 presents the effect size for each intervention regarding utilization of health services.[39]

[38] Actually, the medium term impact of the first intervention is assed in Björkman, de Walque and Svensson (2013). However, to avoid confusion, we designate the latter as the main reference for the second intervention and Björkman and Svensson (2009) for the first intervention.

[39] We were not able to compute neither SMD nor RR for these outcomes due to lack of information.

Table 9: Utilisation Outcomes

Study	Variable definition	CMI Type	Effect Size	95% Confidence Interval		ES Type
Björkman and Svensson (2009) - Short Term	Utilization/coverage (pooled from average number of patients visiting the facility per month for out-patient care, average number of deliveries at the facility per month, share of visits to the project facility of all health visits, averaged over catchment area and share of visits to traditional healers, averaged over catchment area).	Scorecard + IC	2.13	0.79	3.47	ASE
Björkman, de Walque and Svensson (2013) - Short Term	Utilization/coverage (idem before)	IC	0.04	-0.41	0.49	ASE
Meta-analysis			***0.99***	***-1.05***	***3.02***	***ASE***
Björkman and Svensson (2009) - Medium Term	Utilization/coverage (idem before)	Scorecard + IC	0.34	0.12	0.55	ASE

Looking at the short run, both interventions show an increase in the access to health services, although for the second one the result is statistically no significant. This suggests that effects are stronger when the information campaign is coupled with a scorecard, which is consistent with the authors' findings, who hint that without information, the process of stimulating participation and engagement had little impact on health workers' performance or the quality of health care (Björkman, de Walque

and Svensson, 2013). The overall effect is positive but no statistically significant, and the I-squared suggests a large amount of between study variability (I^2 = 88.0%, p=0.004).

Looking at the medium term, the information campaign combined with the scorecard has a positive and statistically significant effect, improving the access to services.

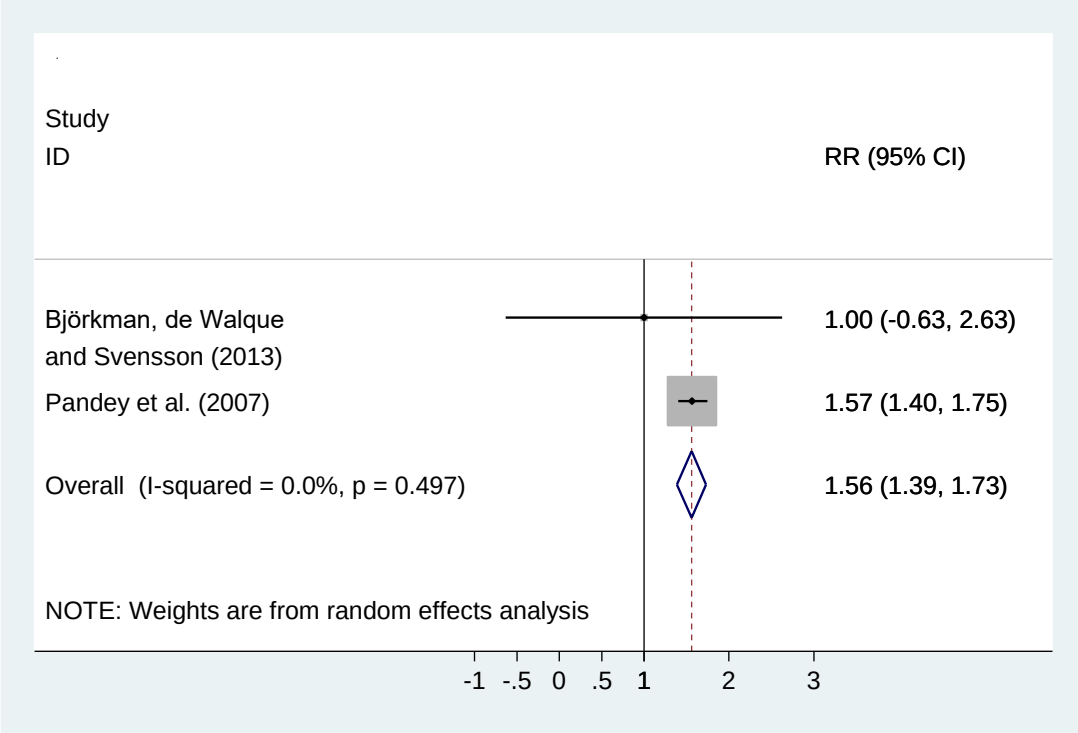
Regarding immunization outcomes, Table 10 reports the short run effects found by Björkman, de Walque and Svensson (2013) and Pandey *et al*. (2007). It also reports the medium term effects for the intervention assessed in Björkman and Svensson (2009) but it is not incorporated into meta-analysis given the different time horizons.

Table 10: Immunisation outcomes

Study	Variable definition	CMI Type	Effect Size	95% Confidence Interval		ES Type
Björkman, de Walque and Svensson (2013)	immunization (pooled from newborn, children less than 1-year, 1-year old, 2-year old 3-year old and 4-year old, whether the child has received at least one dose of measles, DPT, BCG, and Polio)	IC	1.00	-0.63	2.63	RR
Pandey *et al.* (2007)	Vaccinations received by infants	IC	1.57	1.40	1.75	RR
Meta-analysis			***1.56***	***1.39***	***1.73***	***RR***
Björkman and Svensson (2009) - Medium Term	immunization (idem Björkman, de Walque and Svensson, 2013)	IC	1.04	-0.52	2.61	RR

Björkman, de Walque and Svensson (2013) assess the impact of the CMI on immunization for different age groups, while Pandey *et al*. (2007) compute the percentage of households where infants have received vaccinations. Overall effect is RR 1.56, 95% CI [1.39, 1.73], implying that the effect of the CMI was positive and improved access to services by 56 per cent as can be seen in Figure 7.

Figure 7: Forest plots for immunisation



The medium term impact of the intervention reported by Björkman and Svensson (2009) is positive but statistically not significant[40].

Pandey *et al.* (2007) also report on different measures of access to health services, specifically the percentage of households getting health services such as visits by nurse midwives, prenatal examinations, tetanus vaccinations, and prenatal supplements received by pregnant women. We computed risk ratios for these outcomes, and the results are reported in Table 11. All risk ratios are above unity, with an overall effect RR 1.43, 95% CI [1.29, 1.58] implying that the intervention improved access to services in 43 per cent.

[40] The short term impact of this intervention is also not statistically significant, but it is not reported in the table since we were not able to compute RR.

Table 11: Other access to service outcomes

Study	Variable definition	CMI Type	Effect Size	95% Confidence Interval		ES Type
Pandey *et al.* (2007)	Visits by nurse midwife	IC	1.03	0.94	1.14	RR
	Prenatal examinations		1.63	1.45	1.83	RR
	Tetanus vaccinations		1.57	1.39	1.77	RR
	Prenatal supplements received by pregnant women		1.45	1.29	1.64	RR
	Vaccinations received by infants		1.57	1.40	1.75	RR
Meta-analysis			***1.43***	***1.29***	***1.58***	***RR***

Education

We identified four studies evaluating effects on enrolment in six different treatment arms. Table 12 presents the effect sizes from all treatment arms. Before combining the studies into a meta-analysis, we created synthetic effect sizes for the study with multiple treatment arms (Banerjee *et al*., 2010) to avoid combining effects based on dependent samples. Figure 8 presents the forest plot for the meta-analysis of enrolment rates. The overall average effect of CMI on enrolment is SMD 0.09, 95% CI [-0.03, 0.21]. However, it can be noted that this result is driven by the inclusion of one study for which the SMD is substantially higher than the others (Andrabi, Das and Khwaja, 2013). Also, the I-squared suggests a large amount of between study variability (I^2 = 73.6%, p=0.010). To address this issue, we performed another meta-analysis excluding this study. Figure 9 presents the results. When excluding this study, the overall effect is 0.05, 95% CI [-0.03, 0.13].

Table 12: Enrolments outcomes

Study	Variable definition	CMI Type	Effect Size	95% Confidence Interval		ES Type
Andrabi, Das and Khwaja (2013)	Enrolment	Scorecard	0.58	0.17	0.99	SMD
Banerjee *et al.* (2010) - Mobilization	Enrolment	IC	0.059	-0.138	0.257	SMD
Banerjee *et al.* (2010) - Mobilization + information	Enrolment	IC	0.05	-0.14	0.25	SMD
Banerjee *et al.* (2010) - Mobilization + information + "Read India"	Enrolment	IC	-0.008	-0.199	0.183	SMD
Banerjee et al. (2010) - All interventions			***0.04***	***-0.13***	***0.20***	***SMD***
Gertler *et al. (*2008)	Enrolment	Scorecard	0.003	-0.048	0.054	SMD
Reinikka and Svensson (2011)	Enrolment	IC	0.12	0.02	0.22	SMD
Meta-analysis			***0.09***	***-0.03***	***0.21***	***SMD***

Figure 8: Forest plot for Enrolment outcomes

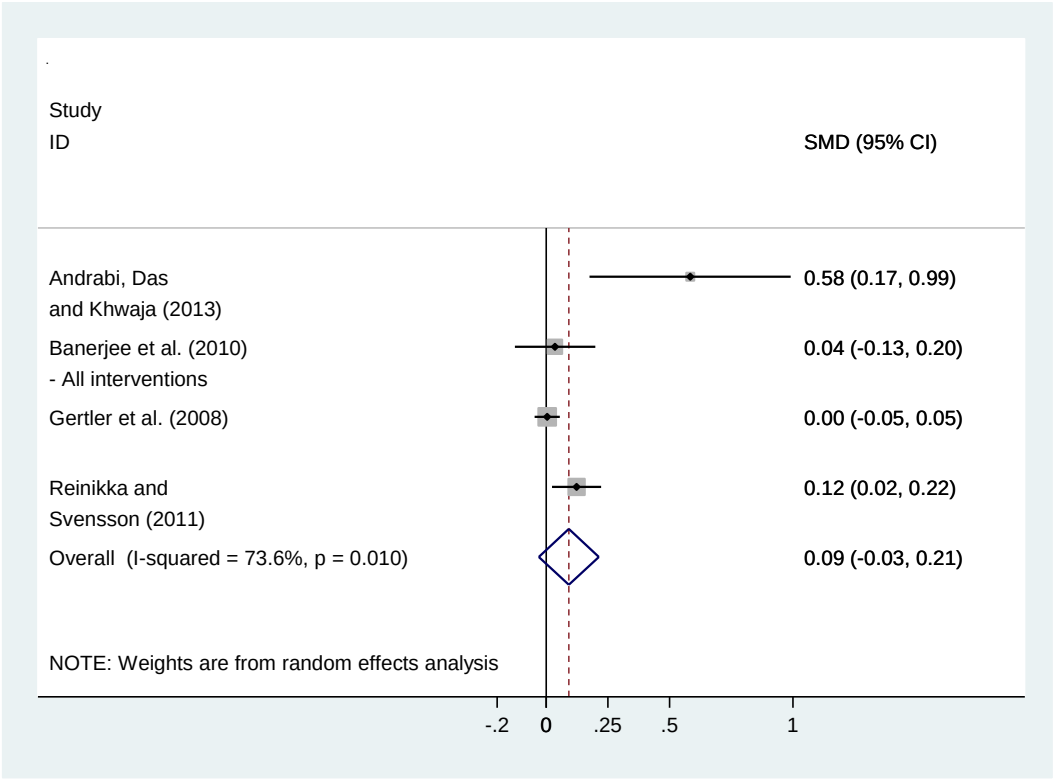
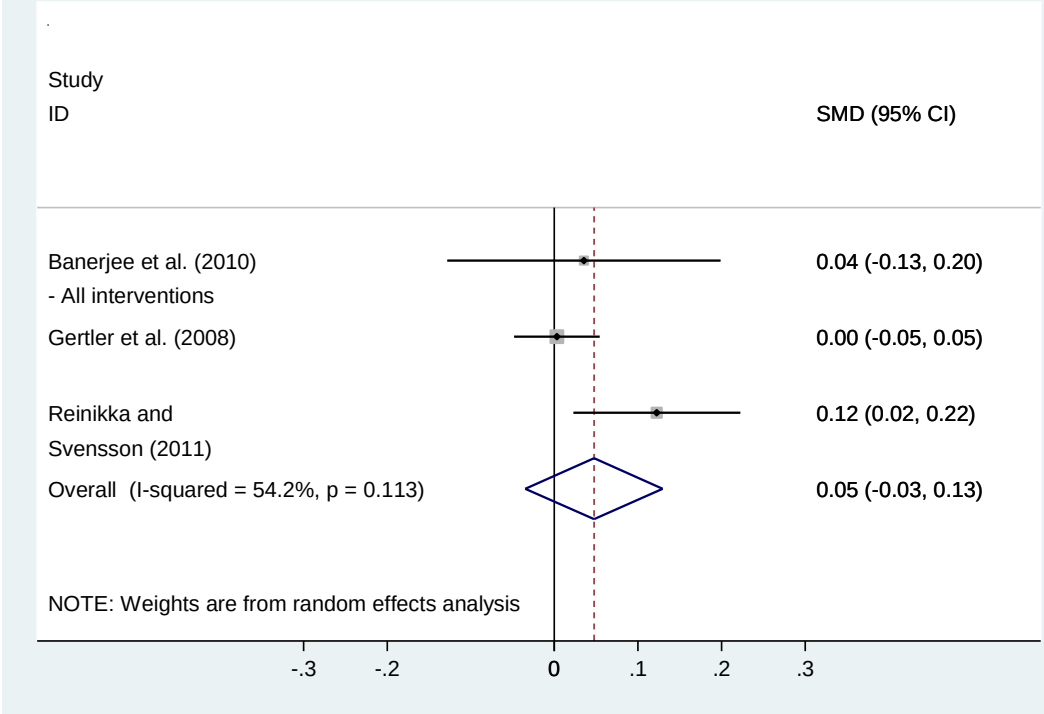
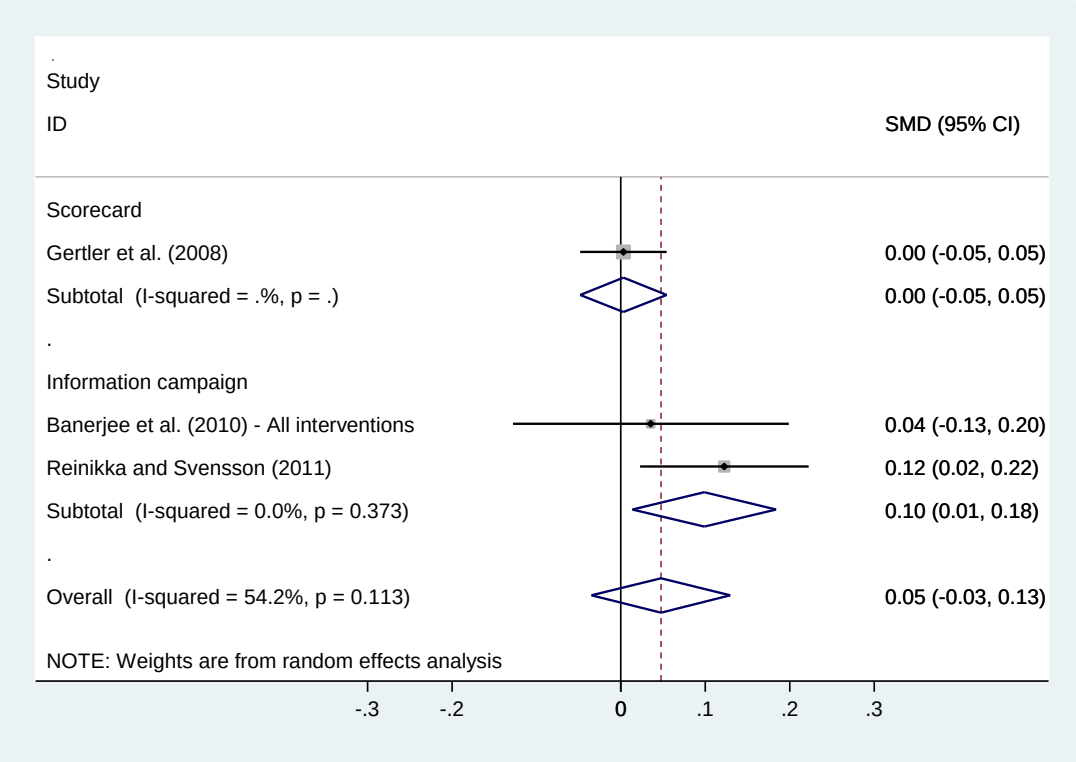


Figure 9: Forest plot for Enrolment outcomes – Outliers excluded



We also performed sensitivity analysis. The results are shown in Figure 10. When considering only Scorecards (Gertler *et al*., 2008), the overall effect is SMD 0.003, 95% CI [-0.05, 0.05], positive but not statistically significant. On the other hand, information campaigns show an overall effect SMD 0.10, 95% CI [0.01, 0.18], suggesting that these interventions have increased enrolment rates in 10 per cent.

Figure 10: Forest plot for Enrolment outcomes – Sensitivity analysis – Outliers excluded



We also identified four studies that measure dropout at schools in seven treatment arms. Table 13 presents the results.

Table 13: Dropout outcomes

Study	Variable definition	CMI Type	Effect Size	95% Confidence Interval		ES Type
Andrabi, Das and Khwaja (2013)	Dropout rate	IC	0.220	-0.159	0.600	SMD
Banerjee *et al.* (2010) - Mobilization	Dropout rate	IC	0.028	-0.006	0.063	SMD
Banerjee *et al.* (2010) - Mobilization + information	Dropout rate	IC	0.02	-0.01	0.06	SMD
Banerjee *et al.* (2010) - Mobilization + information + "Read India"	Dropout rate	IC	0.046	0.011	0.081	SMD
Banerjee et al. (2010) - All interventions			*0.032*	*0.003*	*0.061*	*SMD*
Gertler *et al. (*2008)	Dropout rate	IC	-0.09	-0.14	-0.04	SMD
Pradhan *et al.* (2014) - Training	Dropout rate	IC	0.12	-0.08	0.31	SMD
Pradhan *et al.* (2014) - Linkage	Dropout rate	IC	-0.03	-0.23	0.16	SMD
Pradhan et al. (2014) - All interventions			*0.041*	*-0.124*	*0.207*	*SMD*
Meta-analysis			***0.00***	***-0.10***	***0.10***	***SMD***

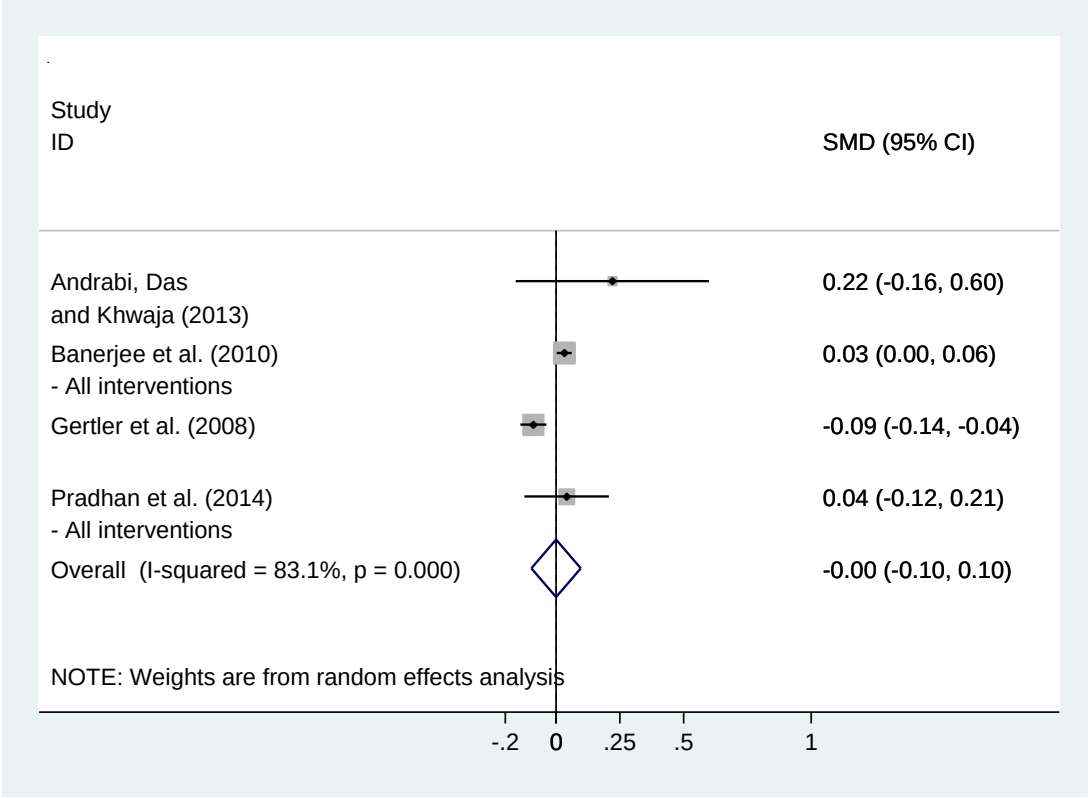
For some interventions, the results suggest an increase in children out of school in the villages receiving CMI compared to those that did not receive the programme. Considering the study of Banerjee *et al*. (2010), the effect range from SMD 0.02, 95% CI [-0.01, 0.06] for the treatment arm with mobilization and information, to SMD 0.046, 95% [0.011, 0.081] for the treatment arm with mobilization and information in addition to “Read India” – reading camps held by trained volunteers, with a combined effect of SMD 0.032, 95% CI [0.003, 0.06]. The authors argue that this result is due to “children dropping out of private or NGO schools (results omitted to save space). It

may be that parents consider the reading classes to be an adequate alternative to a private school". The CMI also resulted in an increase in dropout rates in the cases of Andrabi, Das and Khwaja (2013) and for the Training intervention in Pradhan *et al*. (2014). On the other hand, the Linkage intervention in Pradhan *et al*. (2014) and the study of Gertler *et al*. (2008) find a reduction in dropout rates after interventions.

Before performing the meta-analysis, we calculated a synthetic effect size for the two treatment arms included in Pradhan *et al*.'s (2014) study from Indonesia to avoid issues with dependent effect sizes in the meta-analysis. We did the same with the three interventions reported by Banerjee *et al*. (2010).

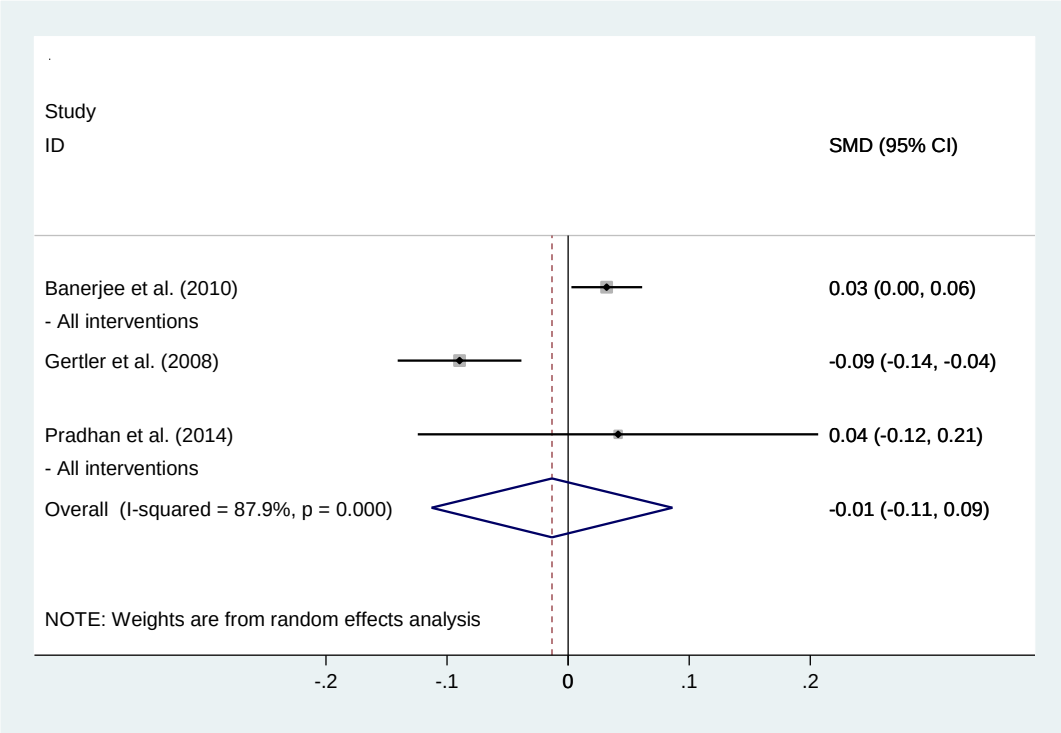
Taking into account all the interventions, the overall effect of these CMIs is SMD 0.00, 95% CI [-0.10, 0.10], suggesting that the effect is not significant. However, the I-squared in Figure 11 suggests a large amount of between study variability (I^2 = 83.1%, p=0.000).

Figure 11: Forest plot for Dropout outcomes



When we exclude the study of Andrabi, Das and Khwaja (2013), which reports an effect considerably larger than the others, the overall effect of CMIs is SMD -0.01, 95% CI [-0.11, 0.09], suggesting a 1 per cent reduction in dropout rates in those communities where CMI have taken place, although the effect is still not significant and the homogeneity test still reveals a large amount of between study variability (I^2 = 87.9%, p=0.000).

Figure 12: Forest plot for Dropout outcomes – Outliers excluded



5.2.2 Quality of services

In this section, we present the analysis of effects on quality of services by sector and outcome, starting with health, and followed by education.

Health

For health related outcomes, we consider measures of child death and anthropometric outcomes.

We identified two studies with measurements of child mortality, Björkmann and Svensson (2009) and Björkman, de Walque and Svensson (2013). Table 14 shows that the short term evaluation for the two interventions had an overall effect of RR 0.76, 95% CI [0.42, 1.11], suggesting that child death had been reduced by 24 per cent after CMIs, however the effect is not statistically significant. Similar conclusions apply for the medium term effect of the information campaign combined with a scorecard, were the effect is a reduction in 21 per cent in mortality.

Table 14: Child death

Study	Variable definition	CMI Type	Effect Size	95% Confidence Interval		ES Type
Björkman and Svensson (2009) - Short Term	child death (under five mortality rate)	Scorecard + IC	0.65	0.42	1.02	RR
Björkman, de Walque and Svensson (2013) - Short Term	child death (infant mortality rate)	IC	1.05	0.61	1.81	RR
Meta-analysis			***0.76***	***0.42***	***1.11***	***RR***
Björkman and Svensson (2009) - Medium Term	child death (under five mortality rate)	Scorecard + IC	0.79	0.57	1.08	RR

We can also interpret an improvement in anthropometric measures as an improvement in the quality of health services provided. Table 15 reports the impact of the same two CMIs on weight-for-age scores.

Table 15: Weight for age

Study	Variable definition	CMI Type	Effect Size	95% Confidence Interval		ES Type
Björkman and Svensson (2009) - Short Term	Weight for age (children 0-18 months)	Scorecard + IC	1.20	1.00	1.43	RR
Björkman, de Walque and Svensson (2013) - Short Term	Weight for age (children 0-11 months)	IC	1.22	0.92	1.60	RR
Meta-analysis			***1.20***	***1.02***	***1.38***	***RR***
Björkman and Svensson (2009) - Medium Term	Weight for age (children 0-18 months)	Scorecard + IC	1.29	1.01	1.64	RR

In the short term, CMIs have increased weight for age scores by 20 per cent, suggesting that quality of health services has improved. The positive impact seems to be stronger in the medium term, resulting in a 29 per cent improvement. Homogeneity test suggests no variability between studies (I^2 = 0.01%, p=0.928).

In addition to these measures of health services' quality, these studies also report on another measure, namely average waiting time in medical facilities. The effects range from RR 0.91 95% CI [0.81, 1.01], to RR 1.10 95% CI [0.81, 1.15], and are displayed in Table 16.[41] Meta-analysis for the short term interventions suggests a negligible effect –reducing waiting time in 1 per cent, and this is not significant. However, there is a large between study heterogeneity that might be driven the results (I^2 = 70.8%, p=0.064).

Table 16: Average waiting time to get the service outcome variables

Study	Variable definition	CMI Type	Effect Size	95% Confidence Interval		ES Type
Björkman and Svensson (2009) - Short Term	Waiting time in medical services*	Scorecard + IC	0.91	0.81	1.01	RR
Björkman, de Walque and Svensson (2013) - Short Term	Waiting time in medical services*	IC	1.10	0.81	1.15	RR
Meta-analysis			***0.99***	***0.80***	***1.17***	***RR***
Björkman and Svensson (2009) - Medium Term	Waiting time in medical services*	Scorecard + IC	1.06	0.95	1.19	RR

* Difference between the time the user left the facility and the time the user arrived at the facility, subtracting the examination time.

[41] It is important to note why we think this is a quality measure and not an access measure. Access is related to getting the service. However, you can get the service and the fact that you had to wait makes it less valuable and of lesser quality.

Education

We included six studies assessing the effect of CMI on the quality of education as measured by test scores.[42] As can be seen from Table 17, three of these studies include multiple treatment arms. We calculated synthetic effect sizes combining the different treatment arms before including these studies in the meta-analysis. The overall average effect of CMI on student outcomes across these six studies is SMD 0.16, 95% CI [0.04, 0.29],[43] suggesting that CMIs improved test scores by 16 per cent.

[42] When different test scores where reported (e.g. language and math test scores), we previously pooled them following the procedure explained before.

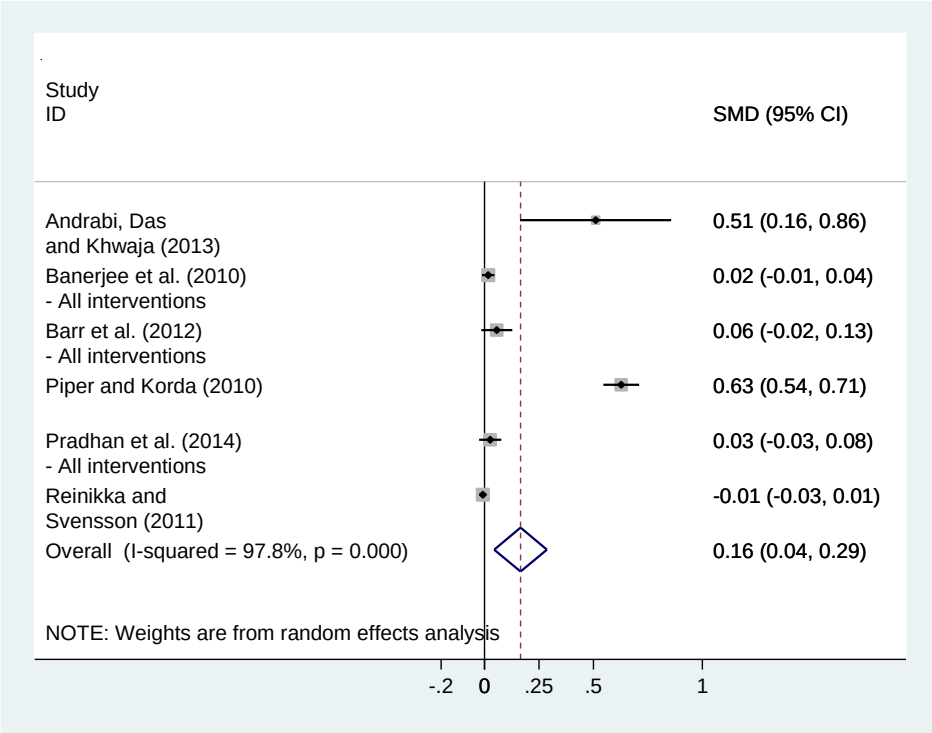
[43] It should be noted that we are excluding some studies for which we were not able to compute standardised effects (Table 11) but which found significant effects of CMIs on our outcomes of interest. For example, Keefer and Khemani (2011) found that the information campaign resulting from communities' access to radios enhanced literacy tests.

Table 17: Test scores

Study	Variable definition	CMI Type	Effect Size	95% Confidence Interval		ES Type
Andrabi, Das and Khwaja (2013)	test score	Scorecard	0.510	0.163	0.857	SMD
Banerjee *et al. (*2010) - Mobilization	test score	IC	0.01	-0.02	0.04	SMD
Banerjee *et al.* (2010) - Mobilization + information	test score	IC	0.010	-0.018	0.037	SMD
Banerjee *et al.* (2010) - Mobilization + information + "Read India"	test score	IC	0.03	0.00	0.05	SMD
Banerjee et al. (2010) - All interventions			*0.02*	*-0.01*	*0.04*	*SMD*
Barr *et al. (*2012) - Standard scorecard	test score	Scorecard	0.03	-0.05	0.11	SMD
Barr *et al. (*2012) - Participatory scorecard	test score	Scorecard (Participatory)	0.078	-0.002	0.158	SMD
Barr et al. (2012) - All interventions			*0.056*	*-0.015*	*0.127*	*SMD*
Piper and Korda (2010)	test score	IC	0.63	0.54	0.71	SMD
Pradhan *et al.* (2014) - Training	test score	IC	-0.02	-0.09	0.04	SMD
Pradhan *et al.* (2014) - Linkage	test score	IC	0.07	0.02	0.13	SMD
Pradhan et al. (2014) - All interventions			*0.03*	*-0.03*	*0.08*	*SMD*
Reinikka and Svensson (2011)	test score	IC	-0.01	-0.03	0.01	
Meta-analysis			***0.16***	***0.04***	***0.29***	***SMD***

The assessment of homogeneity suggests a large amount of variability between studies. This is further supported by the forest plot in Figure 13. The effect sizes range from SMD -0.01, 95% CI [-0.03, 0.01] in Uganda (Reinikka and Svensson, 2011) to SMD 0.63, 95% CI [0.54, 0.71] in Liberia (Piper and Korda, 2010). The confidence intervals of these two studies do not overlap.

Figure 13: Forest plot for Test scores



We tried excluding the possibly outlier papers (Andrabi, Das and Khwaja, 2013 and Piper and Korda, 2010). The results are presented in Figure 14. Overall effect is SMD 0.01, 95% CI [-0.01, 0.03].

Figure 14: Forest plot for Test scores – Outliers excluded

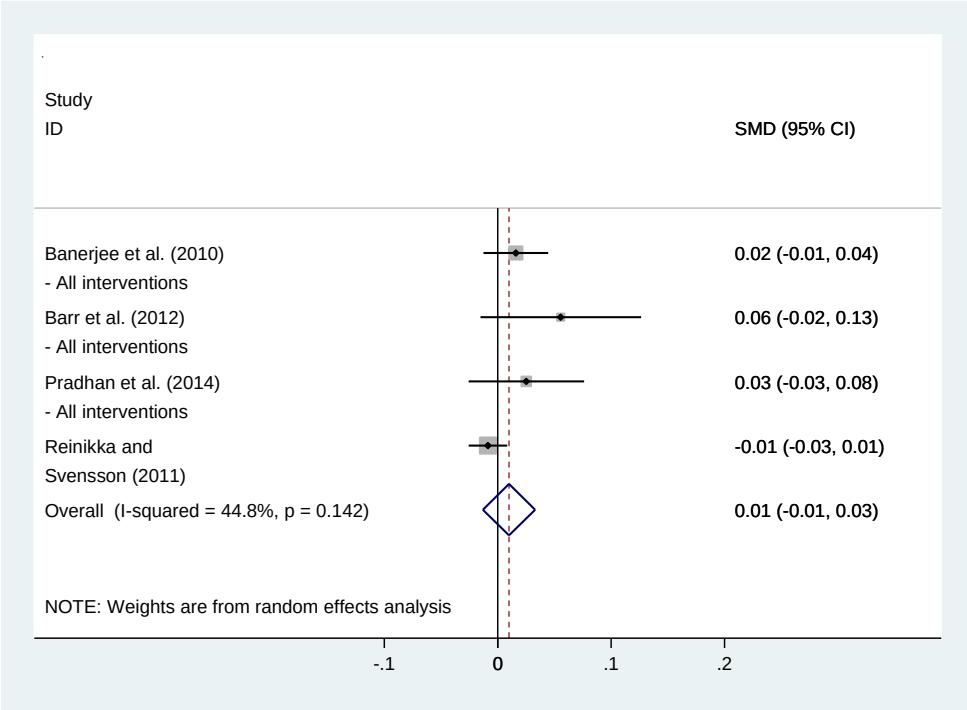
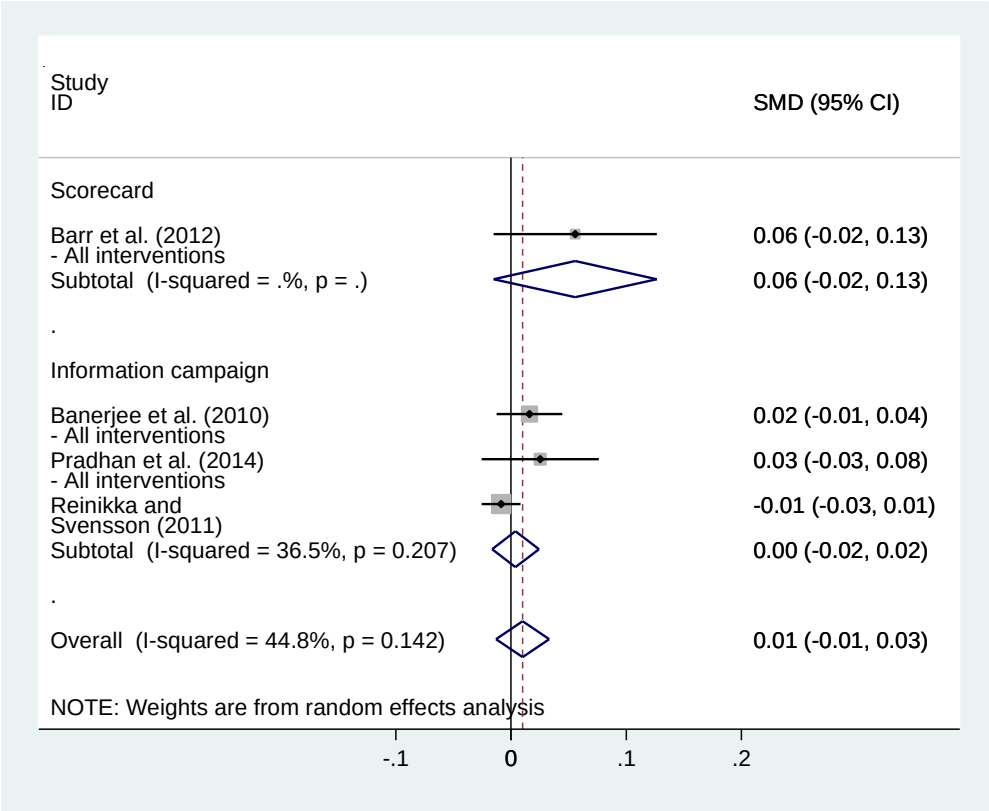


Figure 15 presents sensitivity analysis by CMI type, excluding the possible outliers. Overall effect for information campaigns is SMD 0.004, 95% CI [-0.017, 0.024].

Figure 15: Forest plot for Test scores – Sensitivity analysis - Outliers excluded



5.3 Studies not included in meta analyses

We also identified other measures of the quality of services, but we were unable to compute effect sizes for them. In all cases, the reason was that the information required to compute effect sizes was lacking. Table 18 lists the variables for which we were not able to compute effect sizes.

Table 18: Excluded studies

Study	Variable definition	Available information	Missing information
Keefer and Khemani (2011)	Proportion of children tested in the village public school who could read sentence and paragraphs (ASER literacy test)	coefficients and its p-values, total n	The standard deviation of the dependent variable or the standard deviation of the error term in the regression
Pandey, Goyal and Sundararaman (2009)	Percentage of children who could pass different learning tests, including reading and writing competences and mathematics abilities	Change in treatment - change in control, p-values	Does not provide total size nor the size of the control or the comparison group

5.4 Moderator analysis

In this subsection we had hoped to explore whether our findings differ by intervention characteristics such as design and implementation or the length of exposure to the treatment (review question 3). Unfortunately, the lack of outcome data available from different studies prevented us from undertaking many moderator analyses for the primary outcomes.[44] We were only able to perform some sensitivity analysis by type of intervention for some outcomes, namely enrolment rates and test scores, as reported in the previous sections.

We also performed sensitivity analysis by study design (namely, RCT versus Non RCT). Most of them coincide with the previous analysis due to the way we have aggregated outcomes (namely, corruption measures, utilization, immunization, child death, weight for age and average waiting time to get the service). The other outcomes for which we obtained different results are presented in Table 19.

[44] To address review question 3, we also tried to identify whether information campaigns with a capacity building component where information on how to monitor providers is disseminated differ from those without it, or whether scorecards or social audits that involve facilitated meetings with providers and politicians have better impacts than those that does not or -for all CMIs- whether citizens act not only as monitors but also as decision makers in the project. For details, please see the “Results of Barriers and Facilitators’.

Table 19: Moderator analysis by study design – Outliers excluded

Sub-group	Effect size	95% confidence interval		Num. Estimates	I-squared	Type
Access to service						
Enrolment						
Study design						
RCT	0.055	-0.061	0.171	2	77.1%	SMD
					(p= 0.037)	
Others	0.035	-0.128	0.199	1	n/a	SMD
Dropout						
Study design						
RCT	0.032	0.003	0.061	2	0.0%	SMD
					(p= 0.913)	
Others	-0.09	-0.14	-0.04	1	n/a	SMD
Quality of service						
Test scores						
Study design						
RCT	0.022	-0.001	0.046	3	0.0%	SMD
					(p=0.591)	
Others	-0.01	-0.03	0.01	1	n/a	SMD
Note: n/a not applicable						

While analysing enrolment rates, neither those studies designed as RCT nor the other studies seem to have found statistically significant effects after CMIs. In the case of dropout, the overall effect of studies designed as a RCT shows an increase in dropout rates, while the other study finds a reduction in it. Finally, when we evaluate the impact of the interventions on test scores, again the overall effects are statistically non-significant, although RCT studies show a positive aggregated effect and the remaining study shows a reduction in this measure. However, the caveat regarding the low amount of studies considered remains relevant and these findings cannot be generalised.

We also undertook some moderator analysis by region.[45] Again, in many cases, results coincide with the analysis in the previous section (namely, for corruption measures, utilization, child death, weight for age and average waiting time to get the service). Other cases are presented in Table 20.

Table 20: Moderator analysis by study region – Outliers excluded

Sub-group	Effect size	95% confidence interval		Num. Estimates	I-squared	Type
Access to service						
Immunisation						
Region						
Africa	0.998	-0.631	2.627	1	n/a	RR
Asia	1.565	1.403	1.746	1	n/a	RR
Enrolment						
Region						
Africa	0.122	0.023	0.222	1	n/a	SMD
Asia	0.035	-0.128	0.199	1	n/a	SMD
Latin America	0.003	-0.048	0.054	1	n/a	SMD
Dropout						
Region						
Asia	0.032	0.003	0.061	2	0.0%	SMD
					(p= 0.913)	
Latin America	-0.09	-0.14	-0.04	1	n/a	SMD
Quality of service						
Test scores						
Region						
Africa	0.014	-0.046	0.074	2	66.5%	SMD
					(p=0.084)	
Asia	0.02	-0.01	0.04	2	0.0%	SMD
					(p=0.756)	
Note: n/a not applicable						

[45] The idea behind this exercise is to explore whether results vary according to key contextual factors, such as geographical region or income level.

Regarding immunization, the study of Asia finds a positive effect of this outcome, while for Africa the effect is statistically not significant. Looking at enrolment rates, we can distinguish a study carried out in Africa, another one in Asia and the third one in Latin America. While the three of them show a positive impact of CMI, only the first one is statistically significant. In the case of dropout, overall effect of studies from Asian countries shows an increase in dropout rates, while the intervention in Latin America has reduced this outcome. Finally, there is no evidence of a differential impact in test scores of CMIs from Africa or Asia.

5.5 Publication bias

We assess publication bias by reporting funnel graphs and the results of the Egger's Test, which evaluates the null hypothesis that there is publication bias present. A funnel plot is a scatter plot of treatment effect against a measure of study size. It assumes that the largest studies will be near the average, and small studies will be spread on both sides of the average. Variation from this assumption can indicate publication bias. Egger *et al*. (1997) proposed a test for asymmetry of the funnel plot. This is a test with the null hypothesis that the intercept from a linear regression of normalised effect estimate (estimate divided by its standard error) against precision (reciprocal of the standard error of the estimate) is equal to zero.

On a first stage, we analyse this issue by outcome and type of effect size, considering those reported in the previous section. In many cases, we had only two observations for each case, so we were not able to perform Egger's Test. Here we present the results of those outcomes for which we could perform this test, namely enrolment, dropout rates test scores. However, it should be taken into account that the power of this method to detect publication bias will be low with such a small numbers of studies.

For enrolment rates, the p-value of Egger's Test is 0.160 and the number of studies is 4. The results of the Egger's Test suggest that there is not publication bias. However, the caveat regarding the low power of the test holds, given the low number of observations. Funnel plot is reported in Figure 16.

Figure 17 reports the funnel plot for dropout rates. In this case, the p-value of Egger's Test is 0.975 and the number of studies is 4. Again, there is no evidence of publication bias, but the power of the test is low.

Finally, Figure 18 presents the funnel plot for test scores. The p-value of Egger's Test is 0.156, considering six studies.

Figure 16: Funnel plot showing pseudo-95% confidence limits for Enrolment rates

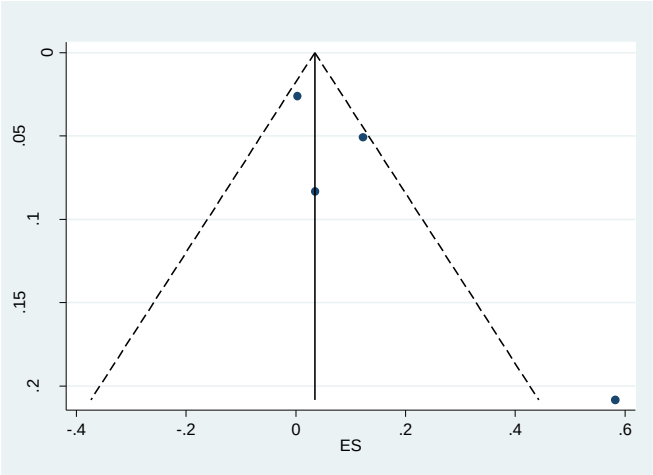


Figure 17: Funnel plot showing pseudo-95% confidence limits for Dropout rates

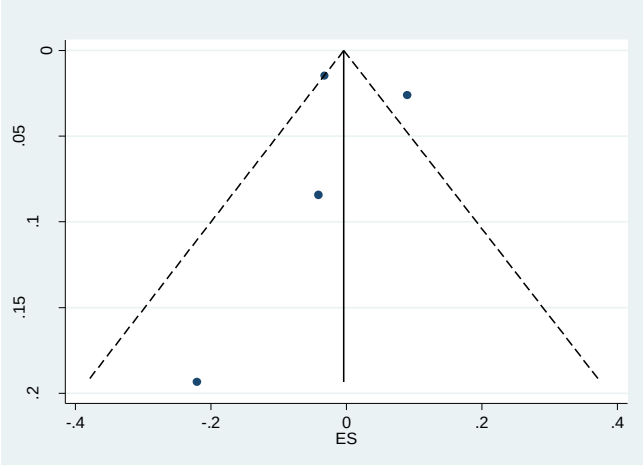
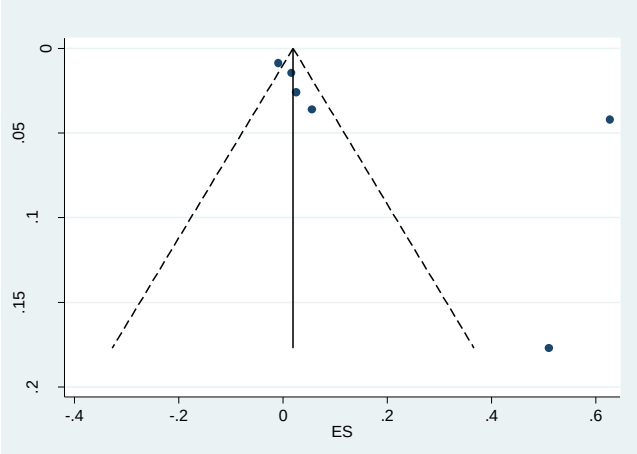


Figure 18: Funnel plot showing pseudo-95% confidence limits for Test scores



In a second stage, we pooled all effect size (reversing the sign when needed, so they all measure positive effects in the same direction) and preformed the same analysis by type of effect size. Figure 19 and Figure 20 present the cases for RR and SMD, respectively.

Figure 19: Funnel plot showing pseudo-95% confidence limits for RR

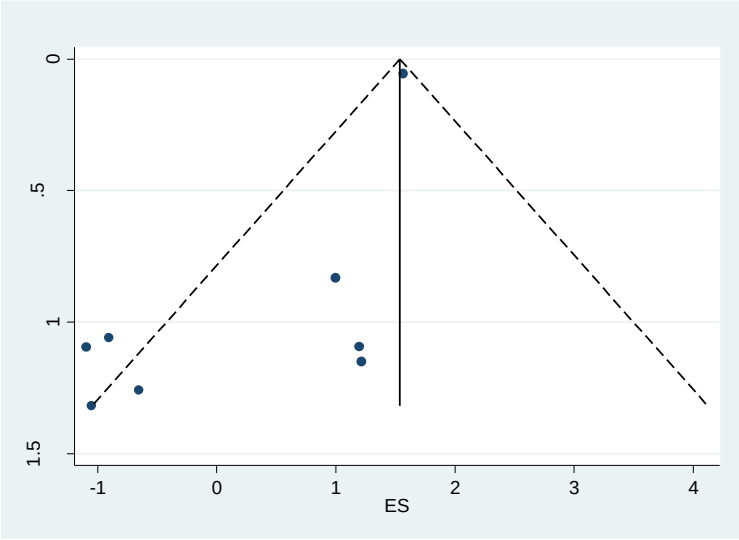
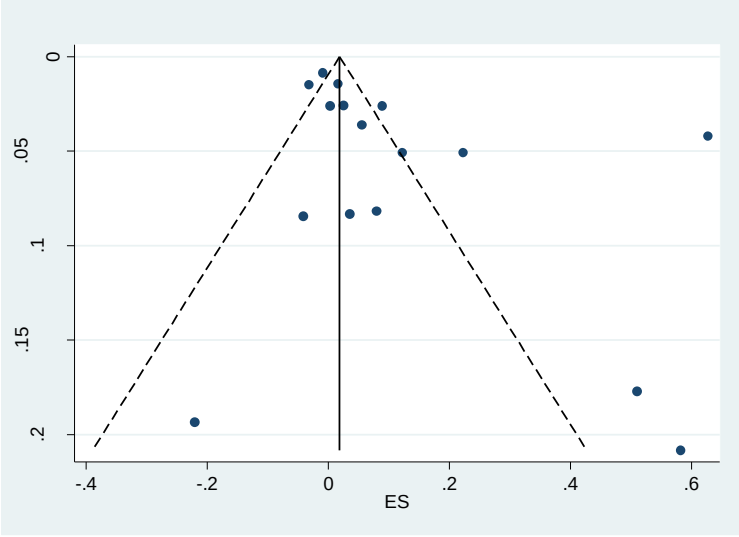


Figure 20: Funnel plot showing pseudo-95% confidence limits for SMD



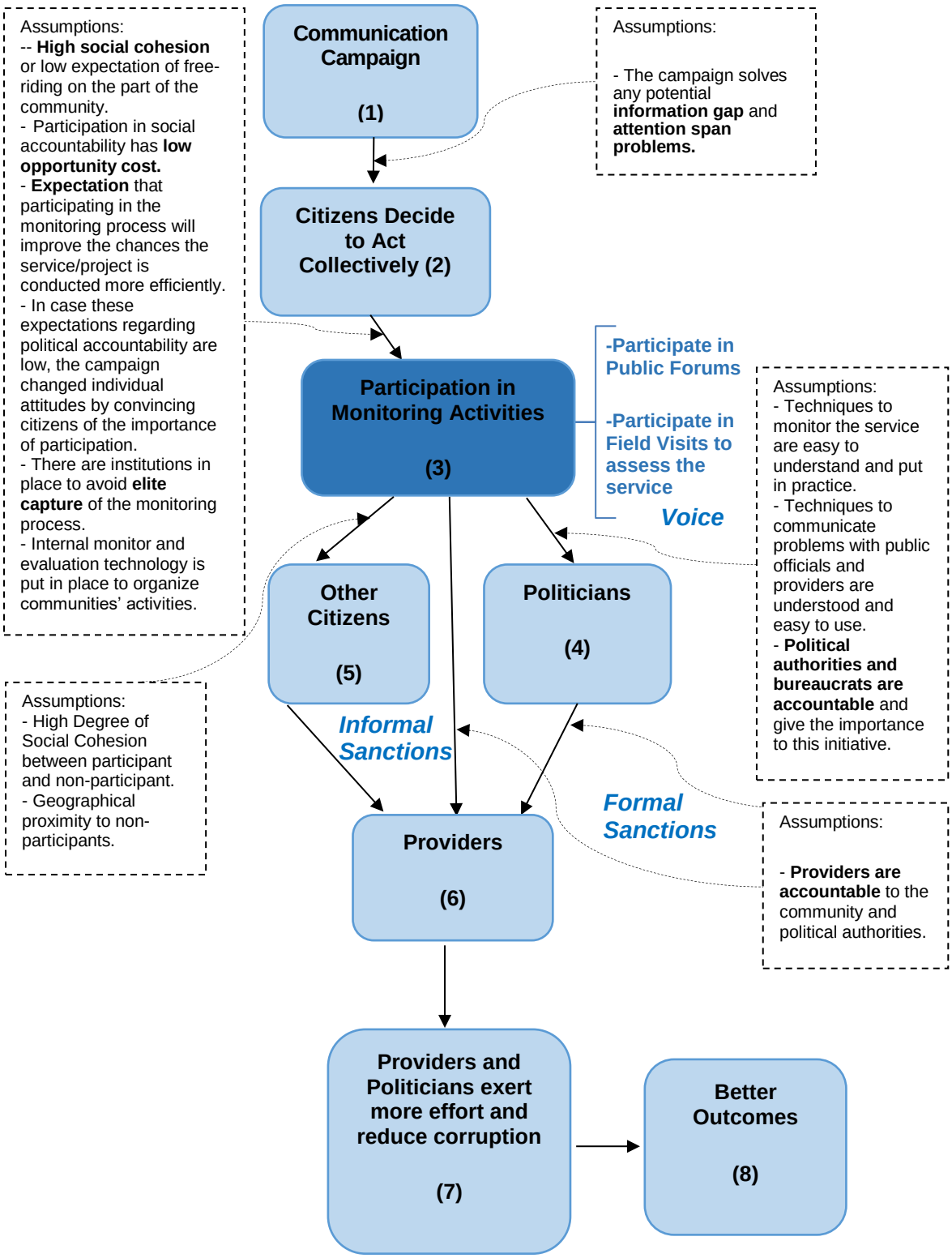
In the case of effect sizes calculated as RR (immunization, child death, average waiting time to get the service and weight for age), the p-value of Egger's Test is 0.007, suggesting some evidence of probable publication bias. For SMD (enrolment, dropout, test scores and forensic economic estimates of corruption), the p-value of Egger's Test is 0.058, suggesting again some evidence of probable publication bias. However, the low power of these tests prevents us for extracting conclusions with any degree of reliability.

6. Results of mechanisms synthesis

In this section, we synthesise evidence on the mechanisms through which CMIs may have an effect (or lack thereof) on corruption and service delivery outcomes. Asking why programmes succeed or fail involves identifying causal pathways. Sometimes pathways are explicit and other times finding pathways means looking for implicit assumptions and arguments. There are a range of possible pathways from the CMIs process to improvement in service delivery, and assessing these pathways can assists us in answering how and why interventions work or not.

The theory of change presented in Figure 21 highlights the implicit necessary assumption for the different pathways. It allows us to articulate the expected mechanisms through which the CMIs may have effect, and the underlying set of assumptions involved for each stage of the process. As described above, the synthesis is based on data available in the studies included to address our primary research question, as well as any sibling papers identified for these studies. Because the limited number of studies, which are also not a random sample of CMI programmes, the findings presented here should be considered preliminary and should be further assessed in future studies.

Figure 21: Theory of change



6.1 Citizens' participation in monitoring activities

Citizens' participation is a key component of most CMI interventions, and a potential concern with CMIs is that citizens may fail to participate in monitoring activities (building block 3). In section 1.3 we identified six potential bottlenecks that could prevent citizens from participating in monitoring activities, which in turn reduces the potential impact of the programme. In particular, if community monitoring activities are not carried out, or carried out by only a few citizens, the likelihood they uncover problems and put pressure on the government to provide accountability can be significantly reduced. Several of the included studies (or their sibling papers) provide data on citizens' participation in monitoring activities. A summary of the potential relevant variables is presented in Appendix H.

Our theory of change provides some potential reasons why participation may fail to materialize. In particular, citizens could have (i) inadequate information on how to monitor the project, (ii) high opportunity cost of participation, (iii) pessimistic beliefs about politicians/providers responsiveness, or (iv) believe that other citizens will decide not to participate. Below we discuss the findings from the included interventions that failed to increase participation on each of these potential explanations.

(i) Inadequate information on how to monitor the project

The question here is whether inadequate information on how to monitor the programme is what is behind the *citizen participation failure*. This can be expressed as two questions: (a) whether citizens are inadequately informed and (b) whether having the necessary information would increase participation.

Banerjee *et al*. (2010) study was designed to answer these questions. They begin to answer that question with a previous study on the same intervention. Banerjee *et al*. (2007) found that parents, teachers and the VEC (equivalent to parent-teacher association) members did not seem to be fully aware of how low the actual level of students' performance was. At the same time, they did not appear to have given much thought to the role of local committees, and/or to the possibility of local participation in improving outcomes. Many parents did not know that a VEC existed, sometimes even when they were supposed to be members of it. Moreover, the VEC members were unaware of the key roles they had been assigned in the educational system. Public participation in improving education was negligible, and people's ranking of education on a list of village priorities was low.

We know that citizens were inadequately informed about the VEC programme as well as about the quality of education their children received. The question now is whether proving that information would increase participation. To answer this question Banerjee *et al*. (2010) test three different interventions. In the first one, Pratham field staff was sent to villages to inform and mobilize parent on how to monitor schools using the VEC. In the second one, they explained how to monitor schools as well as why it was important. To make this salient, Pratham staff taught the community how to conduct a simple test to evaluate student performance and

compile a report card. The results of the report card (which revealed that the quality of education was very low) were used highlight the importance of monitoring schools and student progress. The third intervention requested randomly chosen communities to come up with volunteers to be trained by Pratham staff on techniques to teach children how to read to then run after-class remedial reading classes for them.

The results show that the first two interventions had no impact on increasing citizen participation monitoring schools but the third one did managed to get volunteers and improve children reading levels in these villages. The question then is how to interpret these results. One interpretation is that the information was not narrow enough in intervention 1 and 2 and as a result, participation did not increase (Banerjee and Duflo, 2011). Another interpretation is that citizens preferred to circumvent the state and the existing institutions for school monitoring (Banerjee *et al*. 2010). That interpretation would suggest citizens having pessimistic beliefs as to whether their participation in the VEC system would improve outcomes as the reason why the programme failed to increase participation.

Banerjee *et al*. (2010) was not the only one to attempt to answer this question, though it had the best identification strategy. In Molina (2013b), the author found that in some communities, citizens were not aware of the existence of the project they were supposed to monitor. In other communities citizens knew about the project, but they did not have access to information on how to monitor it. This prevented citizens from those communities from taking an active role in social audit community forums and community monitoring activities in general. While the author found a lack of information to be an obstacle, this was neither the only nor most important issue identified in this study, as discussed below.

(ii) High opportunity cost of participation

It is difficult to test this hypothesis as it is not easy to measure individual opportunity costs and the included papers only report indirect evidence on this. Molina (2013b) assesses under what conditions citizens decided to monitor the project in the context of “Auditorias Visibles”, a social audit in Colombia. The author finds that participants are not statistically different from non-participants in employment status, income level or whether they work at home or not. From this he infers that opportunity cost cannot explain the variation in citizens’ participation in monitoring the project.

Andrabi, Das and Khwaja (2013) find that better educated parents participate more actively in monitoring activities, which could indicate that the opportunity cost of participation is lower for them.

(iii) Pessimistic beliefs about politicians/providers responsiveness

Citizens may refuse to take advantage of the opportunity to monitor the government and service providers if they believe that the chances of politicians and providers being responsive are low. Molina (2013b) found that perceiving oneself as being able

to influence local government is crucial for deciding whether to spend time in community monitoring activities.

Results coming from and evaluation in India (Banerjee *et al*., 2010) provide suggestive evidence on the importance of citizens' perceptions of providers' responsiveness on social accountability interventions.[46] Only the intervention that did not involve government action, but rather trained volunteers to help children learn to read, had a significant impact on citizens' participation and a positive effect on children's reading skills (3-8%).

In the case of the social audit programme in India Afridi and Iversen (2013) suggest cases of maladministration and corruption may have been underreported in the initial audit rounds because of citizens pessimistic beliefs about the integrity of the audit process. They suggest this may be one possible explanation for the lack of a decrease in the aggregate number of complaints. This is supported by findings from Singh and Vutukuru (2009), analysing the initial stages of the same intervention. They describe a situation with little political enthusiasm during the pilot phase of the social audits, but with subsequent high level political support generating huge increases in the turnouts at the local social audit meetings.

Interactions between citizens and providers of the services could change citizens' perceptions of low accountability. Evidence in Barr *et al*. (2012) assess whether facilitated contacts with providers had an effect on citizen participation in CMIs. The authors argue that the impacts of the participatory treatment exceed those of the standard treatment primarily because of increased willingness to contribute to public goods, rather than differences in the information content of the scorecards. They find that the willingness to contribute to public goods is statistically higher for participants of the participatory treatment. However, the identification strategy prevents the authors from discriminating between two potential theories. The participatory treatment could influence outcomes in the school (test scores) and in the lab (voluntary contribution games) either by affecting preferences or by affecting beliefs about the willingness of providers to contribute to public goods.

Woodhouse (2005) analyses the beginning of the KDP programme studied later in Olken (2007). The author finds that when villagers possessed information about their rights and, crucially, when the potential perpetrators of corruption knew that villagers

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[46] There is additional qualitative and quantitative evidence that could be understood using these insights. Gaventa and Barrett (2012) perform a meta-case study of 100 interventions aim to increase citizen engagement in service delivery. For the 828 outcomes from the 100 reviewed case studies, only 153 came from interventions where the final goal was to strengthen the responsiveness and accountability of the state to provide services. Results indicate that 55 per cent of those 153 outcomes were positive and 45 per cent were negative. Negative results were associated with failure of citizens to participate, due in part to fear of backlash against those who speak out and a sense of tokenism in the participation mechanism.

There are also other quantitative papers that could be interpreted using this lens. For example, Keefer and Khemani (2011), as discussed above.

had this information, it raised the perceived cost of corrupt behaviour and reduced the cost of fighting it.

These pessimistic beliefs could also be the cause of elite capture, as the indirect evidence from Olken (2007) suggest. The intervention showed that issuing anonymous comment forms to villagers reduced missing expenditures only if the comment forms were distributed via schools in the village, completely bypassing village officials who may have been involved in the project. Olken (2006) find that those closer to the project, either by distance or participation, are less likely to report corruption in the project, probably reflecting the fact that those who benefit from the project do not want to be on record stating the existence of corruption as they might be concerned that this would create problems for the project that might result in it not being completed.

(iv) Beliefs that other citizens would not participate

Björkman and Svensson (2010), a follow up to Björkman and Svensson (2009), suggest that citizen participation may be threatened by differences within the community. They find that 'income inequality, and particularly ethnic fractionalization, adversely impact collective action for improved service provision'. This means that in communities where there was higher income inequality, and ethnic fractionalization the programme failed to increase participation. However, the available data prevent the authors from being able to answer whether this failure in collective action is due to lack of trust among community members, lack of trust of the community in the service providers and representatives, or both. This is important as if the lack of trust is with representatives we would place this as evidence of alternative (iii).

Molina (2013b) found no relation between measures of fractionalization indexes, a measure of trust in fellow neighbours and the variation in average time spent in monitoring activities. However, the low number of communities in the study limits the information we can extract from this finding.

6.2 Politicians' and providers' accountability

In the case of providers and politicians, they need to gain popularity, increase/maintaine salary and/or social recognition for their responsiveness. If these assumptions are not met, the underlying programme theory of the social accountability information breaks down and this may prevent them from having an impact on service delivery. In particular, whether or not they hold true can affect citizens' decision on whether to monitor government activity and the governments' willingness to facilitate citizen engagement and become more accountable. The literature cites many reasons why politicians and providers may not be accountable to their citizens (as we described in the building blocks 4 and 6 of our theory of change). In section 1.3 we identified three potential bottlenecks, the existence of unresponsive politicians, clientelism and unresponsive providers.

We looked for measures of providers' or politicians' performance in the studies included in the meta-analyses. The results are presented in Appendix I. Although we

refer to ‘Providers’, depending on the service under analysis, it may also involve politicians. We do not discuss findings separately for these two groups because of the low number of studies measuring politicians’ performance.

According to our theory of change, the programme may fail to generate positive treatment effects if there is not enough demand for change (i.e. participation is low), or even in the presence of this demand politicians can for some reason disregarded it (i.e. the politicians does not need their support, clientelism, among others). Even when there is demand and politicians are committed to improve service delivery, the providers might not be responsive.

From the included studies we find that in the cases where citizens decided not to monitor service provision, providers responded by not changing their behaviour. This is consistent with our theory of change. We would not expect providers to change behaviour when the citizens do not participate in monitoring activities. However, in this subsection we are interested in understanding why demand was lacking. Is it because (a) politicians would be unresponsive to demand, (b) providers would be unresponsive to increased pressure from politicians, or (c) citizens believe politicians/providers would be unresponsive. It is important to note the difference between (a) and (b) with (c). While in (a) and (b) politicians and providers respectively are not responsive, in alternative (c) they are responsive but citizens believe they are not. As a result they do not participate in monitoring and politicians and providers act as if there is no demand.

Molina (2013) provides suggestive evidence that when the community increases its demands by increasing citizen participation in the social audit, the politicians respond by performing better, as evaluated by the citizens. This would suggest politicians are actually responsive in this case.

We discussed the results from Banerjee *et al*. (2010) above. The data does not allow us to infer why citizens decided not to participate in the program, neither what would have happened if citizens actually participated. On the other hand, Keefer and Khemani (2011) found that better learning outcomes were not due to better performance by providers, but rather changes in households’ behaviour:

‘…government inputs into village schools, and household knowledge of government policies related to education, are all unrelated to village access to community radio. Instead, greater access to community radio leads to significantly greater private investment by households in the education of their children. This shows a case where monitoring did not increase, neither provider effort but quality of service provision improved. This is because among households with children, those that listen to more community radio because of their access to a larger number of

community radio stations, are more likely to buy books and to make informal or private tuition payments to schools'.[47]

This suggests that understanding why citizens decide to circumvent the existing institutions to monitor service providers and instead use the private sector to invest in their children's human capital is an underexploited area of research.

So far we have used evidence coming from interventions that failed. However, we can also extract information from studies that had positive outcomes. Björkman and Svensson (2009) found the scorecard intervention improved the type of equipment used to treat patients, reduced the average waiting time and the absence rate of staff at the nurseries, and also improved the management of the nurseries, that is, cleaner floors and rooms, staff politeness, among others. Furthermore, using data collected through visual checks by the enumerators during the post-intervention facility survey, the authors find evidence that the programme increased the opportunity the health facility gave the community to monitor them through various methods. In particular, the CMI increase the probability that the health facility had: (i) A suggestion box for complaints and recommendations; (ii) Numbered waiting cards for its patients; (iii) A poster informing about free health services; and (iv) A poster on patients' rights and obligations.

The authors suggest these improvements in the management of the health facilities and the behaviour of health facility staff resulted in better health outcomes for the targeted population. Changes in increased intrinsic motivation due to the interaction between the community and providers appears to be the key behind the improvement in the behaviour of service providers.

Evidence from studies with both a programme with facilitated contact with providers, and one without it support this finding. These studies have a better identification strategy to answer the question of whether facilitated contact improved provider responsiveness. Barr *et al*. (2012) is one of such studies. They found that only the participatory treatment had a positive and statistically significant effect in reducing teachers' absence rate in schools, compared to the standard treatment that did have no effect. Pradhan *et al*. (2014), also found suggestive evidence of impact on teachers' effort, though the statistical significance of the results is not present for all teacher effort outcomes. Again, facilitated contact between users and providers may enhance motivation for citizens to concern on service outcomes and for providers to perform better. As authors argue, 'these effects are driven by reported increases in the village council's collaboration with the school and the school principal's satisfaction of the extent of the village council's attention to education in the village

[47] It can be thought that households were persuaded by the public interest programming on the radio to increase their private investments (i.e. buying inputs such as books, hiring tutors, etc.) in the education of their children. However, it should be emphasize that this is one of many potential interpretations of the paper, as they do not have data to test the reason behind parents' decision to circumvent public sector institutions and use private solutions to increase their children's learning.

[…] Instead of being a passive fundraising vehicle only, the joint planning meetings between the school committee and the village council translated into co-sponsored education initiatives'.

Björkman, de Walque and Svensson (2013) designed a follow up study to Björkman, de Walque and Svensson (2009) to attempt to assess whether information on how to monitor providers and facilitated contact with providers alone is enough to increase participation and improve outcomes, or if there is a need to add objective information on how the facility is performing to influence the dialogue. They find that without the information on the facility performance the process of stimulating participation and engagement had little impact on health workers' performance or the quality of health care. They interpret this finding as the need for objective information to influence the discussion and the content of the action plan the community develops in conjunction with the health facility to improve outcomes. When that objective information on health workers effort and performance is not available, the action plans get "captured" by health workers and the real issues are not addressed.

These findings suggest the details of intervention design are important in driving changes in citizen participation, the performance of service providers and politicians, and ultimately service delivery outcomes. The theory of change has many bottlenecks and the included studies show that different interventions suffer from more than one bottleneck. But more importantly, the binding constraint is not always the same and does not have the same degree of importance. The evidence in this section, though important, should be interpreted as preliminary, as there is almost no paper with a rigorous identification strategy to answer mechanism questions. In order to investigate whether this is the actually the case more research is needed in the area.

7. Discussion

7.1 Synthesis

In this review we aimed to summarize empirical evidence on the effects of CMIs on corruption or service delivery outcomes (review question 1), assess the channels through which these effects occur (review question 2) and whether contextual factors and intervention design features moderate effects on intermediate and final outcomes (review question 3). In this section we integrate the findings from the synthesis structured around the intervention components and the intermediate and final outcome categories. Many of these links have been drawn in the previous section, but here we summarize all findings.

Table 21 summarises the findings for review question 1. The results for both forensic estimates and perception outcomes suggest a positive effect of CMIs on reducing corruption on average. In the case of service delivery, we differentiated access from quality outcomes. For access we divided the analysis by sector and outcome. Effects on utilization of health services are not clear, but we observe an improvement in immunization rates. In the education sector, we did not find evidence of an effect on

proxy access measures such as school enrolment and dropout. On service quality measures, studies looked at child death and weight for age for the health sector, and test scores for education. Evidence from two studies suggests improvements in weight for height, but no difference in child deaths or in waiting times for services. On average waiting time to get a service results from two interventions show a reduction in waiting time in the short term, but this is not sustained in the medium term. Finally, CMI may improve test scores in some contexts. Overall, our findings are heterogeneous and all results are based on few studies. The results should therefore be interpreted with caution.

Table 21: Summary of effectiveness of CMIs

Primary Outcome	Variable definition	Number of Interventions	Effect size	95% Confidence Interval	
Forensic economic estimates of corruption		3	0.15 (SMD)	0.01	0.29
Perception measures of corruption		2	0.08(RD)	0.02	0.13
		2	-0.23 (SMD)	-0.38	-0.07
Access to service	Utilization (short term)	2	0.99 (SMD)	-1.05	3.02
	Utilization (medium term)	1	0.34 (SMD)	0.12	0.55
	Immunization (short term)	2	1.56 (RR)	1.39	1.73
	Immunization (medium term)	1	1.04 (RR)	-0.52	2.61
	Enrolment	6	0.09 (SMD)	-0.03	0.21
	Dropout rate	7	-0.00 (SMD)	-0.10	0.10
Improvement in prevalence condition	Child death (short term)	2	0.76 (RR)	0.42	1.11
	Child death (medium term)	1	0.79 (RR)	0.57	1.08
	Weight for age (short term)	2	1.20 (RR)	1.02	1.38
	Weight for age (medium term)	1	1.29 (RR)	1.01	1.64
	Test score	10	0.16 (SMD)	0.04	0.29
Quality of service	Average waiting time to get the	2	0.99 (RR)	0.80	1.17

Primary Outcome	Variable definition	Number of Interventions	Effect size	95% Confidence Interval	
	service (short term)				
	Average waiting time to get the service (medium term)	1	1.06 (RR)	0.95	1.19

* Statistically significant at 95% confidence level

Understanding the effect of the programme on intermediate outcomes, such as citizens' participation in monitoring activities and providers and politicians' performance, seems crucial. If an intervention fails to increase citizens' participation in those activities, and does not improve service providers' or politicians' performance, it will be almost impossible for the intervention to have an impact on final outcomes. The limited evidence available on mechanisms suggests that interventions that have modified these intermediate outcomes have been those that include a set of tools for citizens to monitor providers or politicians, and facilitate contacts between citizens, providers and politicians. These interventions appear to be the ones that have the bigger impacts on providers' responsiveness (lower absence rates, more teachers' effort, better school inputs) and more participation of communities in monitoring activities (more time spent in monitoring, more in-kind and monetary donations).

There are many reasons why interventions may fail in increasing citizens' participation in monitoring activities. In some cases, it is related to insufficient or even no information provision to citizens for controlling service delivery (Banerjee *et al*., 2007; Björkman, de Walque and Svensson, 2013). It could also be a result of citizen's low expectations of leaders, officials, or service providers' accountability or about the chances of success (Molina, 2013b; Banerjee *et al*., 2010, Khemani, 2007). In addition, the nature of the service provided may be related with the incentives of citizens to actively participate in monitoring activities (Olken 2004, 2007). This relates to the collective action failure, where some citizens may free-ride on their efforts to monitor the project.

Within community differences may result in heterogeneous participation (Björkman and Svensson, 2010). As Banerjee and Mullainathan (2008) argue, certain groups, especially the poor, are less likely to participate in monitoring activities because they have more pressing priorities. All these factors may also influence the degree of providers and politicians' responsiveness, since it is influenced by citizens' participation. We would not expect providers to change behaviour when the citizens do not solve the collective action problem and participate in monitoring activities. For the latter to improve their performance, they need to gain popularity, increased/maintained salary and/or social recognition for their responsiveness.

Other reasons why providers and politicians may not be accountable are related to institutional settings. If citizens' support is not needed for politicians to stay in power, it is likely that CMIs will not improve their performance. In addition, if citizens can impose sanctions to unresponsive providers, CMIs are more likely to improve providers' performance.

With this in mind, Björkman and Svensson (2010) argue that their results 'have implications for both the design and evaluation of interventions aimed at strengthening beneficiary control in public service delivery programmes. On programme design, interventions should be adjusted to the local socio-political situation. As little is known about how this is to be done, our results open up an important agenda for research: How to enhance collective action in socially heterogeneous communities. On evaluation, ideally the researchers should design the evaluation protocol so as to be able to assess the impact conditional on the socio-political environments'.

Other studies have emphasised the need of adapting interventions to local contexts. Masouri and Rao (2012) argue that both local and national context may be a key factor in determining effectiveness. In turn, Devarajan, Khemani and Walton (2011, 2013) find that interventions' effectiveness is mediated by the local context, as in communities where clientelism and rent-seeking is widespread, civic participation fails to have an impact on service delivery and government accountability.

7.2 Implications for policy and practice

Overall, our findings are heterogeneous and based on few studies, and should therefore be interpreted with caution. However, the results suggest CMIs can have a positive effect on corruption measures and some service delivery measures.

Considering the potential bottlenecks that may arise given the local context is important to design complementary policies to enhance the effect of CMIs. For example, in India citizens did not know how to get involved in community monitoring in the education sector, but even after receiving information they decided not to participate. In such cases, policy design should focus on either improving the accountability of those institutions to motivate citizens to participate or focus the interventions on policy options that do not require involvement of state institutions, such as remedial education programmes run by local citizens. The review also highlights the need to provide accessible information for citizens on how to monitor providers. Finally, there is some preliminary evidence that combining objective information on service delivery outcomes together with facilitated interactions between citizens and service providers in particular has improved outcomes.

7.3 Implications for research

We identified a relatively small number of impact evaluations that assess the effects of CMIs on service delivery and corruption outcomes in L&MICs. We also found few studies that address the channels through which effects materialise. This might be

due to the difficulty of performing such experimental evaluations with appropriate identification strategies, especially in the case of causal mediation.

To improve future systematic reviews there is a need for not only more impact evaluations on this topic but more coordination among researchers on the design of the interventions and outcome measurement tools. In particular studies assessing replications of several almost identical interventions in different contexts, using similar study designs and measurement tools would improve our ability to reach more generalizable findings about intervention effects.[48] Even if this degree of coordination is not possible,[49] there is a need to encourage better reporting of the necessary data to compute effect sizes to avoid having to exclude studies from formal meta-analysis due to lack of data.

New studies should embed the theoretical underpinning of the programme when designing new interventions. For instance, what is their theory of change? Who are the 'providers' and 'politicians' that the 'community' needs to hold accountable? What are the sources of change in incentives that these interventions aim to address? How can these be nudged and supported through more data and new information?

Understanding the micro determinants of intermediate outcomes is crucial for translating academic research to policy. For example, how to influence beliefs about providers' responsiveness, citizen participation in monitoring activities, providers and politicians' responsiveness is an area for future research.

Another issue that arose from this review is how to enhance collective action in socially heterogeneous communities. As Björkman and Svensson (2010) argue, 'ideally the researchers should design the evaluation protocol so as to be able to assess the impact conditional on the socio-political environments'.

Additionally, we still know very little on how the information-for-accountability diffuses among citizens' social networks. Using social network mapping to understand the diffusion of these interventions would be important.

Complementary to this, there are very few studies that compare social accountability interventions with other supervision strategies. Comparing as well as combining bottom-up accountability mechanisms with top down accountability mechanisms, such as improving monitoring capacity by the regulator (e.g. new technology that allows the regulator to monitor providers), impose higher penalties or increase audit probability (as in Olken, 2007) should also be part of the research agenda.

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[48] Berk Ozler made this suggestion in a World Bank seminar on why we found very different conclusions from other systematic reviews of interventions to improve learning outcomes.

[49] Researchers may not have incentives to put effort into working on the same intervention as other researchers.

7.4 Limitations

Due to the low number of included studies, results from meta-analysis should be interpreted carefully. Interventions considered to address review question (1) may be not representative since they took place mainly in Africa and Asia, in rural communities within specific contexts, so the same interventions may have different effects elsewhere. Moreover, in some cases, studies assess the same intervention with a different time scope.

Finally, it seems reasonable that this type of interventions, more than others like vaccinations and the like, are more sensible to the political economy of the society. As such, external validity of the findings is even more difficult to achieve.

7.5 Deviation from protocol

We ran an aggregated meta-analysis for all types of interventions for each primary outcome (5). Initially, we anticipated running one meta-analysis for each outcome, and then decomposing into stratified meta-analyses for each CMI. However, given the low number of studies found, we decided that the breakdowns by intervention would be meaningless, except for a few outcomes. We also decomposed the analysis by sector in which service was provided (e.g. education, health, infrastructure, etc.) and perform some sensitivity analyses, namely by study design and region. However, the results of these exercised should not be generalised given the low number of studies involved in them.

Also, we did not run parametric meta-analysis for different degrees of quality among studies as well as uncertainty about the bias associated to each study, in the spirit of Gerber and Green (2012) Bayesian framework due to low number of studies.

Appendix A: Search strategy – an example

Econlit (Ovid) Search – 20 October 2013

1. (communit* or civil* or civic* or citizen* or people or elector* or grassroot* or social or societ* or local or resident* or neighbo*).ti,ab.
2. (monitor* or particip* or empower* or control* or develop* or governanc* or superv* or "report* card*" or audit* or (informat* adj3 campaign*) or scorecard* or "score card*" or accountab* or watchdog* or democrati* or "people power").ti,ab.
3. (performance or effort* or attend* or achievement* or "test score*" or absent* or (disease adj3 prevalence) or "cost effectiv*" or access* or ((deliver* or performance or provi*) adj3 service*) or corrupt* or fraud* or dishonest* or brib* or mismanag* or leak* or (missing adj3 fund*) or client* or wait* or victim* or efficien* or inefficien* or quality or (rent* adj3 seek*)).ti,ab.
4. (representative* or "local authorit*" or bureaucra* or councillor* or provider* or politician* or official* or leader* or govern* or administration).ti,ab.
5. (D720 or D730 or H110).cc.
6. 4 or 5
7. (Africa or Asia or Caribbean or West Indies or South America or Latin America or Central America).ti,ab,hw.
8. (Afghanistan or Albania or Algeria or Angola or Antigua or Barbuda or Argentina or Armenia or Armenian or Azerbaijan or Bangladesh or Barbados or Benin or Byelarus or Byelorussian or Belarus or Belorussian or Belorussia or Belize or Bhutan or Bolivia or Bosnia or Herzegovina or Hercegovina or Botswana or Brazil or Bulgaria or Burkina Faso or Burkina Fasso or Upper Volta or Burundi or Urundi or Cambodia or Khmer Republic or Kampuchea or Cameroon or Cameroons or Cameron or Camerons or Cape Verde or Central African Republic or Chad or Chile or China or Colombia or Comoros or Comoro Islands or Comores or Mayotte or Congo or Zaire or Costa Rica or Cote d'Ivoire or Ivory Coast or Croatia or Cuba or Djibouti or French Somaliland or Dominica or Dominican Republic or East Timor or East Timur or Timor Leste or Ecuador or Egypt or United Arab Republic or El Salvador or Eritrea or Ethiopia or Fiji or Gabon or Gabonese Republic or Gambia or Gaza or Georgia Republic or Georgian Republic or Ghana or Gold Coast or Grenada or Guatemala or Guinea or Guam or Guiana or Guyana or Haiti or Honduras or India or Maldives or Indonesia or Iran or Iraq or Jamaica or Jordan or Kazakhstan or Kazakh or Kenya or Kiribati or Korea or Kosovo or Kyrgyzstan or Kirghizia or Kyrgyz Republic or Kirghiz or Kirgizstan or Lao PDR or Laos or Latvia or Lebanon or Lesotho or Basutoland or Liberia or Libya or Lithuania or Macedonia or Madagascar or Malagasy Republic or Malaysia or Malaya or Malay or Sabah or Sarawak or Malawi or Nyasaland or Mali or Marshall Islands or Mauritania or Mauritius or Agalega Islands or Mexico or Micronesia or Middle East or Moldova or Moldovia or Moldovian or Mongolia or Montenegro or Morocco or Ifni or Mozambique or Myanmar or Myanma or Burma or Namibia or Nepal or Netherlands Antilles or New

Caledonia or Nicaragua or Niger or Nigeria or Northern Mariana Islands or Oman or Muscat or Pakistan or Palau or Palestine or Panama or Paraguay or Peru or Philippines or Philipines or Phillipines or Phillippines or Papua New Guinea or Portugal or Romania or Rumania or Roumania or Russia or Russian or Rwanda or Ruanda or Saint Lucia or St Lucia or Saint Vincent or St Vincent or Grenadines or Samoa or Samoan Islands or Navigator Island or Navigator Islands or Sao Tome or Senegal or Serbia or Montenegro or Seychelles or Sierra Leone or Sri Lanka or Ceylon or Solomon Islands or Somalia or Sudan or Suriname or Surinam or Swaziland or South Africa or Syria or Tajikistan or Tadzhikistan or Tadjikistan or Tadzhik or Tanzania or Thailand or Togo or Togolese Republic or Tonga or Trinidad or Tobago or Tunisia or Turkey or Turkmenistan or Turkmen or Uganda or Ukraine or Uruguay or USSR or Soviet Union or Union of Soviet Socialist Republics or Uzbekistan or Uzbek or Vanuatu or New Hebrides or Venezuela or Vietnam or Viet Nam or West Bank or Yemen or Yugoslavia or Zambia or Zimbabwe or russia).tw.

9. ((developing or less* developed or under developed or underdeveloped or middle income or low* income or underserved or under served or deprived or poor*) adj (countr* or nation? or population? or world or state*)).ti,ab.
10. ((developing or less* developed or under developed or underdeveloped or middle income or low* income) adj (economy or economies)).ti,ab.
11. (low* adj (gdp or gnp or gross domestic or gross national)).tw.
12. (low adj3 middle adj3 countr*).tw.
13. (lmic or lmics or third world or lami countr*).tw.
14. transitional countr*.tw.
15. developing countries.hw.
16. or/7-15
17. 1 and 2 and 3 and 6 and 16 (Result 3435 hits)

Appendix B: Coding sheet

Variable	Description	Values	
Study information:			
Id	Unique identifier code	Numeric	
Author	Name of Authors	String	
Year	Year of the document	Yyyy	
Publication	Type of publication	1	Article
		2	Chapter in a book
		3	Conference presentation
		4	Government or institutional report
		5	Mimeo
		6	Working paper
Status	Publication status	1	Published or forthcoming in refereed journal or book
		2	Published or forthcoming in non refereed journal or book
		3	Unpublished
		4	Unknown
Source	Document found using…	1	Citation
		2	Electronic database
		3	Handsearch
		4	Unknown

Variable	Description	Values	
		5	Website
Language		1	English
		2	French
		3	Portuguese
		4	Spanish
		5	Other
Intervention:			
Country	Country	String	
Region	region (EAP, LAC, MENA, SA, SSA)	String	
Sector	Sector in which the intervention was carried out, e.g. health, police, education, infrastructure, etc.	String	
Urban	Urban/Rural	0	Rural
		1	Urban
Fragility	Fragility of the community according underlying political systems, social norms, etc.	String	
Start	Year when the intervention started	Yyyy	

Variable	Description	Values	
duration	Intervention period (from MM/YY to MM/YY)	String	
quantitative	Is it a quantitative or qualitative study?	0	Qualitative
		1	Quantitative
data	Is the intervention data available?	0	No
		1	Yes
contrafactual	Treatment and comparison groups	1	CMI vs no formal process of monitoring
		2	CMI with encouragement to participate vs. CMI without encouragement to participate in monitoring
design	Research design	1	impact evaluations based on experimental design
		2	quasi-experimental designs
		3	contemporaneous data collection
		4	two or more control and intervention sites
		5	regression discontinuity designs
		6	interrupted time series studies

Variable	Description	Values	
		7	ex post observational studies with non-treated comparison groups and adequate control for confounding
		8	reflexive comparison groups
		9	project completion reports and process evaluations
		#	Other
units	Units of observations	String	
Variables for Sample size	Number of clusters, number of individuals for each sample size (treatment, exposed, and comparison group)	Numeric	
Variables for Sample attrition	Attrition for each sample size (treatment, exposed, and comparison group)	Numeric	
spillover	Geographical separation of treatment and comparison	String	
assignment	Information reported on method of allocating individuals to groups		
additional_int	Description of whether there is an additional intervention provided: e.g. CDD, CDR, school management.		
control_group	Description of comparison group		

Variable	Description	Values	
target_group	Description of targeted group.		
Type of intervention			
campaign	information campaigns	0	No
		1	Yes
scorecard	Scorecards/Report Cards	0	No
		1	Yes
audits	social audits	0	No
		1	Yes
Grm	Grievance Redress Mechanism	0	No
		1	Yes
desc_int	Description of the Intervention	String	
Outcomes and Effect Size			
outcome#	Outcome as stated in the study	String	
o_type#	Is the outcome a …	1	Measure of corruption
		2	Measure of access or quality of service delivered
		3	Forensic measure
		4	Time measure
		5	Measure of access
		6	Perception measure
		7	

Variable	Description	Values	
		8	Measure of citizen participation in monitoring activities?
		9	Measure of elite capture
		10	Measure of providers' performance
			Other measure
0_main#	Is it a primary outcome?	0	No
		1	Yes
o_estimate#	Estimate extracted from the study.	String	
0_smd#	Effect size for standardised mean differences	Numeric	
0_rr#	Effect size for risk ratios	Numeric	
o_estimand#	Description of treatment effect estimated: ITT, ATET, ATE, LATE and whether the estimates is adjusted for cluster if possible or unadjusted analysis	String	
o_other#	Other relevant information for the outcome	String	
Information Transmission:			
it_description	How was devised the information transmission mechanism?	String	
it_presentation	How was the message presented?	String	

Variable	Description	Values	
Interaction between Community and Service Providers:			
int_nmeetings	Number of meetings (0 to N)	numeric	
int_participation	Attendance rate to meetings (%)	numeric	
Int_distance	Distance to the meeting	numeric	
Community Power to Make Decisions:			
pow_decision	Which type of decisions can the community make?	string	
Critical Appraisal			
ci_aims	Are the aims of the study clearly stated?	0	No
		1	Yes
ci_framework	Is there a clear link to relevant literature/theoretical framework?	0	No
		1	Yes
ci_context	Is there an appropriate description of the context?	0	No
		1	Yes
ci_theory	Is there a clear link to the theoretical framework and previous literature?	0	No
		1	Yes
ci_data		0	No

Variable	Description	Values	
	Is there an appropriate description of the methods of data collection?	1	Yes
ci_methods	Is there an appropriate description of the methods of analysis?	0	No
		1	Yes
ci_design	Was the research design appropriate?	0	No
		1	Yes
ci_controls	Does it control for potential confounding variables?	0	No
		1	Yes
ci_findings	Are the findings supported by the data?	0	No
		1	Yes
ci_ethics	Are there ethical concerns related to the research?	0	No
		1	Yes
		9	Unclear
ci_perform	Was the study adequately protected against performance bias?	0	No
		1	Yes
		9	Unclear
ci_report	Was the study free from outcome and analysis reporting biases?	0	No
		1	Yes
		9	Unclear
ci_otherbias		0	No

Variable	Description	Values	
	Was the study free from other sources of bias?	1 9	Yes Unclear
ci_quality	What is the overall quality of the study?	1	Low
		2	Medium
		3	High
Qualitative/quantitative information:			
Barriers to and enablers of final and intermediate outcomes: information gaps, attention spans, social capital, opportunity cost of participation, description of the interactions, etc.			

We used this narrower version of the Coding sheet proposed in the Protocol, in which we have discarded the ‘Capacity Building’ block because we found several missing values for most of these fields.

Appendix C: Critical appraisal of studies[50]

1) Selection bias and confounding

a) For Randomised assignment (RCTs), Score "YES" if:

- a random component in the sequence generation process is described (e.g. referring to a random number table)[51];
- and if the unit of allocation was at group level (geographical/ social/ institutional unit) and allocation was performed on all units at the start of the study,
- or if the unit of allocation was by beneficiary or group and there was some form of centralised allocation mechanism such as an on-site computer system;
- and apart from receiving different treatments, subjects in different experimental conditions should be handled in an identical fashion.
- and the same standards where used to measure outcomes in the two groups, possible those tasked with measuring outcomes are blind to experimental condition.
- and if the unit of allocation is based on a sufficiently large sample size to equate groups on average.
- baseline characteristics of the study and control/comparisons are reported and overall
- similar based on t-test or ANOVA for equality of means across groups[52],
- or covariate differences are controlled using multivariate analysis;
- and the attrition rates (losses to follow up) are sufficiently low and similar in treatment and control, or the study assesses that loss to follow up units are random draws from the sample (e.g. by examining correlation with determinants of outcomes, in both treatment and comparison groups);
- and problems with cross-overs and drop outs are dealt with using intention-to-treat analysis or in the case of drop outs, by assessing whether the drop outs are random draws from the population;

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[50] We drew almost entirely on Waddington *et al*. (2012) in developing this tool.

[51] If a quasi-randomised assignment approach is used (e.g. alphabetical order), you must be sure that the process truly generates groupings equivalent to random assignment, to score "Yes" on this criteria. In order to assess the validity of the quasi-randomization process, the most important aspect is whether the assignment process might generate a correlation between participation status and other factors (e.g. gender, socio-economic status) determining outcomes; you may consider covariate balance in determining this (see question 2).

[52] Even in the context of RCTs, when randomisation is successful and carried out over sufficiently large assignment units, it is possible that small differences between groups remain for some covariates. In these cases, study authors should use appropriate multivariate methods to correcting for these differences.

- and, for cluster-assignment, randomization should be done at the cluster level. If this is not the case, authors should control for external cluster-level factors that might confound the impact of the programme (e.g. institutional strength, provider's competition, media independence, and community fixed effects) through either matching or multivariate analysis.

Score "UNCLEAR" if:

- the paper does not provide details on the randomization process, or uses a quasi- randomization process for which it is not clear has generated allocations equivalent to true randomization.
- insufficient details are provided on covariate differences or methods of adjustment;
- or insufficient details are provided on cluster controls.

Score "NO" if:

- the sample size is not sufficient or any failure in the allocation mechanism or execution of the method could affect the randomization process[53].

b) For discontinuity assignment (regression discontinuity design)

Score "YES" if:

- allocation is made based on a pre-determined discontinuity on a continuous variable (regression discontinuity design) and blinded to participants or,
- if not blinded, individuals reasonably cannot affect the assignment variable in response to knowledge of the participation decision rule;
- and the sample size immediately at both sides of the cut-off point is sufficiently large to equate groups on average.
- the interval for selection of treatment and control group is reasonably small,
- or authors have weighted the matches on their distance to the cut-off point, and the mean of the covariates of the individuals immediately at both sides of the cut-off point (selected sample of participants and non-participants) are overall not statistically different based on t-test or ANOVA for equality of means,
- or significant differences have been controlled in multivariate analysis;
- and, for cluster-assignment, authors control for external cluster-level factors that might confound the impact of the program (e.g. weather, infrastructure, community fixed effects, etc.) through multivariate analysis.

[53] If the research has serious concerns with the validity of the randomisation process or the group equivalence completely fails, it is recommended to assess the risk of bias of the study using the relevant questions for the appropriate methods of analysis (cross-sectional regressions, difference-in-difference, etc.) rather than the RCTs questions.

Score “UNCLEAR” if:

- the assignment variable is either non-blinded or it is unclear whether participants can affect it in response to knowledge of the allocation mechanism.
- there are covariate differences across individuals at both sides of the discontinuity which have not been controlled for using multivariate analysis, or if insufficient details are provided on controls,
- or if insufficient details are provided on cluster controls.

Score “NO” if:

- the sample size is not sufficient or
- there is evidence that participants altered the assignment variable prior to assignment.

c) For identification based on an instrumental variable (IV estimation)

Score “YES” if:

- An appropriate instrumental variable is used which is exogenously generated: e.g. due to a ‘natural’ experiment or random allocation. This means there is evidence that both assumptions holds: any effect of the instrument on the outcome must occur via the effect of the instrument on the treatment (exclusion restriction) and the instrument is correlated with the variable is instrumenting.
- Following Staiger and Stock (1997) and Stock and Yogo (2005) the F-statistic in the first stage regression should exceed 10[54] (or if an F test is not reported, the authors report and assess whether the R-squared (goodness of fit) of the participation equation is sufficient for appropriate identification);
- the identifying instruments are individually significant ($p \leq 0.01$); for Heckman models, the identifiers are reported and significant ($p \leq 0.05$);
- where at least two instruments are used, the authors report on an over-identifying test ($p \leq 0.05$ is required to reject the null hypothesis of all instruments are uncorrelated with the structural error term.); and none of the covariate controls can be affected by participation and the study convincingly assesses qualitatively why the instrument only affects the outcome via participation[55].

[54] We will include studies where the first stage the F-statistics is below 10, but confidence intervals for the IV regression are computed following Chernozhukov and Hansen (2008) method and are statistically significant at the 95 per cent significance.

[55] If the instrument is the random assignment of the treatment, the reviewer should also assess the quality and success of the randomisation procedure in part a).

- and, for cluster-assignment, authors particularly control for external cluster-level factors that might confound the impact of the programme through multivariate analysis.

Score “UNCLEAR” if:

- the exogeneity of the instrument is unclear (both externally as well as why the variable should not enter by itself in the outcome equation).
- relevant confounders are controlled but appropriate statistical tests are not reported or exogeneity[56] of the instrument is not convincing,
- or if insufficient details are provided on cluster controls (see category f) below).

Score “NO” otherwise.

d) For assignment based non-randomised programme placement and self-selection (studies using a matching strategy or regression analysis (excluding IV), studies which apply other methods)

Score “YES” if:

- Participants and non-participants are either matched based on all relevant characteristics explaining participation and outcomes, or
- all relevant characteristics are accounted for.[57] [58]

Score “UNCLEAR” if:

- it is not clear whether all relevant characteristics (only relevant time varying characteristics in the case of panel data regressions) are controlled.

Score “NO” if:

- relevant characteristics are omitted from the analysis.

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[56] An instrument is exogenous when it only affects the outcome of interest through affecting participation in the programme. Although when more than one instrument is available, statistical tests provide guidance on exogeneity (see background document), the assessment of exogeneity should be in any case done qualitatively. Indeed, complete exogeneity of the instrument is only feasible using randomised assignment in the context of an RCT with imperfect compliance, or an instrument identified in the context of a natural experiment.

[57] Accounting for and matching on all relevant characteristics is usually only feasible when the programme allocation rule is known and there are no errors of targeting. It is unlikely that studies not based on randomisation or regression discontinuity can score “YES” on this criterion.

[58] There are different ways in which covariates can be taken into account. Differences across groups in observable characteristics can be taken into account as covariates in the framework of a regression analysis or can be assessed by testing equality of means between groups. Differences in unobservable characteristics can be taken into account through the use of instrumental variables (see also question 1.d) or proxy variables in the framework of a regression analysis, or using a fixed effects or difference-in-differences model if the only characteristics which are unobserved are time-invariant.

In addition:

d1) For non-randomised trials using panel data
(including DID) models,

Score “YES” if:

- the authors use a difference-in-differences (or fixed effects) multivariate estimation method;
- the authors control for a comprehensive set of time-varying characteristics; 59
- and the attrition rate is sufficiently low and similar in treatment and control, or the study assesses that drop-outs are random draws from the sample (e.g. by examining correlation with determinants of outcomes, in both treatment and comparison groups);
- and, for cluster-assignment, authors control for external cluster-level factors that might confound the impact of the programme through multivariate analysis.

Score “UNCLEAR” if:

- insufficient details are provided,
- or if insufficient details are provided on cluster controls.

Score “NO” otherwise, including if the treatment effect is estimated using raw comparison of means in statistically un-matched groups.

d2) For statistical matching studies including propensity scores (PSM) and covariate matching,[60]

Score “YES” if:

- matching is either on baseline characteristics or time-invariant characteristics which cannot be affected by participation in the program; and the variables used to match are relevant (e.g. demographic and socio-economic factors) to explain both participation and the outcome (so that there can be no evident differences across groups in variables that might explain outcomes) (see fn. 6).

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[59] Knowing allocation rules for the programme – or even whether the non-participants were individuals that refused to participate in the programme, as opposed to individuals that were not given the opportunity to participate in the programme – can help in the assessment of whether the covariates accounted for in the regression capture all the relevant characteristics that explain differences between treatment and comparison.

[60] Matching strategies are sometimes complemented with difference-in-difference regression estimation methods. This combination approach is superior since it only uses in the estimation the common support region of the sample size, reducing the likelihood of existence of time-variant unobservable differences across groups affecting outcome of interest and removing biases arising from time-invariant unobservable characteristics.

- In addition, for PSM Rosenbaum's test suggests the results are not sensitive to the existence of hidden bias.
- and, with the exception of Kernel matching, the means of the individual covariates are equated for treatment and comparison groups after matching;
- and, for cluster-assignment, authors control for external cluster-level factors that might confound the impact of the programme through multivariate or any appropriate analysis.

Score "UNCLEAR" if:

- relevant variables are not included in the matching equation, or if matching is based on characteristics collected at endline,
- or if insufficient details are provided on cluster controls.

Score "NO" otherwise.

d3) For regression-based studies using cross sectional data (excluding IV)

Score "YES" if:

- the study controls for relevant confounders that may be correlated with both participation and explain outcomes (e.g. demographic and socio-economic factors at individual and community level) using multivariate methods with appropriate proxies for unobservable covariates (see fn. 6),
- and a Hausman test[61] with an appropriate instrument suggests there is no evidence of endogeneity,
- and none of the covariate controls can be affected by participation;
- and either, only those observations in the region of common support for participants and non-participants in terms of covariates are used, or the distributions of covariates are balanced for the entire sample population across groups;
- and, for cluster-assignment, authors control particularly for external cluster-level factors that might confound the impact of the programme through multivariate analysis.

Score "UNCLEAR" if:

- relevant confounders are controlled but appropriate proxy variables or statistical tests are not reported,
- or if insufficient details are provided on cluster controls.

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[61] The Hausman test explores endogeneity in the framework of regression by comparing whether the OLS and the IV approaches yield significantly different estimations. However, it plays a different role in the different methods of analysis. While in the OLS regression framework the Hausman test mainly explores endogeneity and therefore is related with the validity of the method, in IV approaches it explores whether the author has chosen the best available strategy for addressing causal attribution (since in the absence of endogeneity OLS yields more precise estimators) and therefore is more related with analysis reporting bias.

Score “NO” otherwise.

d4) For study designs which do not account for differences between groups using statistical methods, score “NO”.

2) Spill-overs: was the study adequately protected against performance bias?

Score “YES” if:

- the intervention is unlikely to spill-over to comparisons (e.g. participants and non- participants are geographically and/or socially separated from one another and general equilibrium effects are unlikely) [62].

Score “UNCLEAR” if:

- spill-overs are not addressed clearly.

Score “NO” if:

- allocation was at individual or household level and there are likely spill-overs within households and communities which are not controlled for in the analysis;
- or if allocation at cluster level and there are likely spill-overs to comparison clusters.

3) Selective reporting: was the study free from outcome and analysis reporting biases?

Score “YES” if:

- there is no evidence that outcomes were selectively reported (e.g. all relevant outcomes in the methods section are reported in the results section).
- authors use ‘common’ methods[63] of estimation and the study does not suggest the existence of biased exploratory research methods[64].

[62] Contamination, that is differential receipt of other interventions affecting outcome of interest in the control or comparison group, is potentially an important threat to the correct interpretation of study results and should be addressed via PICO and study coding.

[63] ‘Common methods’ refers to the use of the most credible method of analysis to address attribution given the data available.

[64] A comprehensive assessment of the existence of ‘data mining’ is not feasible particularly in quasi-experimental designs where most studies do not have protocols and replication seems the only possible mechanism to examine rigorously the existence of data mining.

Score “NO” if:

- some important outcomes are subsequently omitted from the results or the significance and magnitude of important outcomes was not assessed.
- authors use uncommon or less rigorous estimation methods such as failure to conduct multivariate analysis for outcomes equations where it is has not been established that covariates are balanced.[65]

Score “UNCLEAR” otherwise.

4) Other: was the study free from other sources of bias?

Important additional sources of bias may include: concerns about blinding of outcome assessors or data analysts; concerns about blinding of beneficiaries so that expectations, rather than the intervention mechanisms, are driving results (detection bias or placebo effects)[66]; concerns about courtesy bias from outcomes collected through self-reporting; concerns about coherence of results; data on the baseline collected retrospectively; information is collected using an inappropriate instrument (or a different instrument/at different time/after different follow up period in the comparison and treatment groups).

Score “YES” if:

- the reported results do not suggest any other sources of bias.

Score “UNCLEAR” if:

- other important threats to validity may be present

Score “NO” if:

- it is clear that these threats to validity are present and not controlled for.

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[65] For PSM and covariate matching, score “YES” if: where over 10 per cent of participants fail to be matched, sensitivity analysis is used to re-estimate results using different matching methods (Kernel Matching techniques). For matching with replacement, no single observation in the control group is matched with a large number of observations in the treatment group. Where not reported, score “UNCLEAR”. Otherwise, score “NO”.

For IV (including Heckman) models, score “YES” if: the authors test and report the results of a Hausman test for exogeneity ≤ 0 ($p < 0.05$ is required to reject the null hypothesis of exogeneity), the coefficient of the selectivity correction term (Rho) is significantly different from zero ($p<0.05$) (Heckman approach). Where not reported, score “UNCLEAR”. Otherwise, score “NO”.

For studies using multivariate regression analysis, score “YES” if: authors conduct appropriate specification tests (e.g. reporting results of multicollinearity test, testing robustness of results to the inclusion of additional variables, etc). Where not reported or not convincing, score “UNCLEAR”. Otherwise, Score “NO”.

[66] All interventions may create expectations (placebo effects), which might confound causal mechanisms. In social interventions, which usually require behaviour change from participants, expectations may form an important component of the intervention, so that isolating expectation effects from other mechanisms may be less relevant.

Appendix D: Description of interventions

Study	Description of the intervention	Outcomes measurement
Afridi and Iversen (2013)	The first step in conducting the social audit is a notification with reference to RTI (Right to Information) obligations, requesting unrestricted access to muster rolls and other relevant MGNREGA project documents would be sent to the relevant sub-district or mandal office (ibid.). A team, comprising state and district auditors will, upon their arrival in the mandal headquarter, first recruit and then, in a two-day workshop, intensively train Village Social Auditors about MGNREGA rights and regulations, about how to conduct the social audits and about how to obtain information under RTI legislation (ibid.). The social audit teams will then, over a period of about a week, implement social audits in all GPs of the mandal. In each GP, official labour expenses will be verified by visiting labourers listed in the worksite logs ('muster-rolls'). Complaints by individuals, groups and the audit team are recorded and attested using a standardised audit report template. For verification of material expenditure, the audit team is mandated to undertake worksite inspections. Once the audits of all GPs have been completed, a mandal level "public hearing" to discuss the audit findings is organised with mandatory attendance for all implementing officials. Complaints will be read out, testimonies verified while accused officials will be given an opportunity to defend themselves. After the "public hearing" a decision taken report (DTR) is created by the officer presiding over the public	The GP audit reports have two components: a standard audit report card which records the date of the audit along with the demographic characteristics of the GP, and more importantly, the impressions of the audit team about process performance since the last audit including an estimate of financial misappropriations. These impressions and estimates are based largely on the second component of the audit report – the list of complaints filed during the verification process by individuals, groups of individuals or by the members of the audit team itself. These complaints are recorded during the door-to-door verification of labour expenditures and the visits by the technical members of the audit team to project sites to verify expenditures on the materials component of the MGNREGA projects. During the public hearing the responsibility for each complaint is pinned on one or multiple MGNREGA functionaries.

Study	Description of the intervention	Outcomes measurement
	hearing. In this report the responsibility for each confirmed malfeasance is pinned on a programme functionary.	
Andrabi, Das and Khwaja (2013)	Villages were sampled from three districts: one each in the north, center and south. Within these districts, villages were chosen randomly from among those with at least one private school according to a 2000 census of private schools; this frame captures the educational environment for 60 per cent of the province's population. In each of the three districts in the study, the authors experimentally allocated half the villages (within district stratification) to the group that would receive report cards. Since the report card intervention affects the entire educational marketplace and the authors were interested in exploring how the overall market would respond, the intervention was carried out at the village rather than the school level. For a well-defined market-level experiment, the authors required "closed" markets where schools and children (including those who switch schools across years) could be tracked over time.	In 2003, the first year of the survey, the authors completed a census of 80,000 households in the sample villages, and since 2004 the project has conducted additional survey rounds consisting of school, teacher, child, and parent surveys, in addition to annual testing of the same children that were in Grade 3 in 2003. School surveys were administered to all schools in the sample. Through these surveys the authors collected information on infrastructure, prices and costs, as well as the availability of other facilities in the neighborhood of the school. In addition, in every school, they administered teacher surveys to Grade 3 teachers and the head teacher (the head teacher questionnaire included questions on management practices, along with other modules.). Finally, for a sample of 10 randomly selected children in every tested grade (6,000 children), a short questionnaire was administered to collect information on parental literacy, family structure, and household assets. In classes with less than 10 children, all children were chosen. The household questionnaire, with an extended focus on education investments, was fielded for 1,800 households in the sample villages and stratified to over-sample students eligible by age for (the tested) Grade 3. The dataset is matched across schools, children, and households, allowing the authors to follow children and teachers even when

Study	Description of the intervention	Outcomes measurement
		they switch schools or drop out. The 12,110 children the authors tested in the 804 public and private schools in Grade 3 in 2004 were retested in 2005 in whatever grade they were enrolled in at the time.
Banerjee *et al.* *(*2010)	The evaluation took place in 280 villages in the Jaunpur district in the state of UP, India. All three interventions adopted the same basic structure to share information on education and on the resources available to villagers to improve the quality of education. The interventions started with small-group discussions carried out in each hamlet over at least two days. The intervention culminated in a general village meeting typically attended by the Pradhan (village head) and the school headmaster. The intervention teams tried to facilitate the discussion in this meeting so that local key actors of the village (the school teachers or Pradhans) provided general information about the provisions and resources available at the village level, as well as village-specific information on the existence of VECs, its membership, what resources it receives, and the different roles it can play. Pratham facilitators were provided a fact sheet covering information about the public education system and VECs, and checked whether all these facts were shared at the village meeting. If something was missing, they would raise it themselves. In the following weeks, facilitators visited each VEC member and gave him or her a written pamphlet on the roles and responsibilities of the VEC, which they also discussed with the VEC member.	The outcomes were measure through two surveys. The baseline survey consists of 2,800 households, 316 schools, 17,533 children (ages 7–14) tested in reading and math, and 1,029 VEC member interviews from the 280 villages, and in the endline survey, 17,419 children were tested, a sample that includes all but 716 of the children in the baseline and, thus, very little attrition from the baseline survey (the attrition is evenly spread across the various treatment and control groups). The main outcome that authors measure is Learning, through reading and math tests. They also measure some intermediate outcomes, such as knowledge of VEC members about their role; VEC activism; what VEC members know about the education situation in the village; parental awareness and involvement with the school; parental knowledge about the education situation in the village; the priority given to education in village discussions; school resources, and student educational status.

Study	Description of the intervention	Outcomes measurement
Barr *et al.* *(*2012)	The first step is the selection and training of individuals to participate in the use of the scorecard and to be part of the scorecard committee. There are two variants on the scorecard approach. The standard scorecard contains questions on themes of pupils involvement, provision for teachers, teacher presence and activities, materials and facilities, school finances, community involvement, health and wellbeing, and security and discipline. Under each theme, members of the SMC are provided with both quantitative indicators and a five-point scale to register their satisfaction with progress relative to the goals of the community. In schools allocated to the participatory scorecard, SMC members received the same training in the principles of monitoring and the development of objectives and indicators of progress. They then were led in the definition of their own goals and measures, starting from only a simple framework for a scorecard. Once training was completed, the scorecard process was carried out in the same way in both treatment arms. In each term for the duration of the study, this process consisted of three steps. First, members of the scorecard committee would visit the school individually at least once during the term and complete their own copy of the scorecard. Second, at the end of the term, there would be a reconciliation process, in which scorecard committee members would meet, initially in small groups according to their roles, and subsequently as a whole, in order to agree upon a single set of scorecard results for the term and to discuss specific goals and means for improvement in relation to this information. Third, the results of this	First, they collected data on student learning achievements, together with survey-based and directly observed measures of school characteristics, at baseline and follow-up. To this end, they worked with officials from the Uganda National Examinations Board, who administered the National Assessment of Progress in Education (NAPE) exams at baseline to a representative sample of 20 pupils each in Primary 3 and Primary 6. These are the two years for which NAPE instruments are available. Because pupils in P6 had graduated primary school by the time of our follow-up survey, the authors focus analysis on the sample of Primary 3 pupils, who they tracked at follow-up. The exams administered to each sampled student consisted of both a literacy and numeracy component. In addition, at follow-up they conducted unannounced visits in both treatment and control schools to measure absenteeism; these were conducted separately from survey and testing activities.

Study	Description of the intervention	Outcomes measurement
	`consensus scorecard' would be disseminated, by sending it to the District Education Office and by discussing it at the next parent teacher association meeting.	
Björkman and Svensson (2009)	A set of information obtained from pre-intervention surveys, including utilization, quality of services, and comparisons vis-à-vis other health facilities, was assembled in report cards. Each treatment facility and its community received a unique report card, translated into the main language spoken in the community, summarizing the key findings from the surveys conducted in their area. The process of disseminating the report card information, and encouraging participation, was initiated through a series of meetings: a community meeting; a staff meeting; and an interface meeting. Staff from various local NGOs (CBOs) acted as facilitators in these meetings. The community meeting was a two-afternoons event with approximately 100 invited participants from the community.	Outcomes were measured through surveys addressed to health care providers and users. Utilization/coverage was measured by the average number of patients visiting the facility per month for out-patient care, average number of deliveries at the facility per month, average number of antenatal visits at the facility per month, average number of family planning visits at the facility per month, share of visits to the project facility of all health visits, averaged over catchment area and share of visits to traditional healers and self-treatment of all health visits, averaged over catchment area. Immunization was measured as the number of children receiving at least one dose of measles, DPT, BCG, and Polio. Waiting time was measured as the difference between the time the citizen left the facility and the time the citizen arrived at the facility, subtracting the examination time. Baseline surveys were collected in 2004, and Follow-up in 2006.
Björkman, de Walque and Svensson (2013)	The first intervention was the same as described in Björkman and Svensson (2009). The second one was similar but it did not include the information component, it only included the meetings.	Outcomes were measured in a similar way as described in Björkman and Svensson (2009). For the first intervention (Participation and information) baseine surveys were collected in 2006, and endline surveys in 2009. For the second

Study	Description of the intervention	Outcomes measurement
		intervention, baseline surveys were collected in 2006, and Follow-up in early 2009.
Gertler *et al. (*2008)	AGE is part of a broader school reform designed to improve the supply and quality of education in schools in highly disadvantaged communities. The Compensatory Programme consists of: (i) infrastructure improvement, (ii) provision of school equipment, (iii) provision of materials for students (e.g. notebooks, pens, etc), (iv) pedagogical training for teachers, (v) performance based monetary incentives for teachers, and (vi) AGE. AGE finances and support the schools' parent associations. The monetary support varies from $500 to $700 per year depending on school size. The use of funds is restricted and subject to annual financial audits for a random sample of schools. Amongst other things, the parents are not allowed to spend money on wages and salaries for teachers. Most of the money goes to infrastructure improvements and small civil works. In return, parents must commit to greater involvement in school activities, participate in the infrastructure work, and attend training sessions delivered by state educational authorities. In these sessions, parents receive training in the management of the funds and in participatory skills to increase their involvement in the school. Parents also receive information on the role of the school as an educator, on the role of the schools' parent association, on their children educational achievements and on how to help their children learn.	Data on school level grade repetition, failure and drop out as well as other characteristics comes from the Mexican School Census (Censo Escolar).

Study	Description of the intervention	Outcomes measurement
Keefer and Khemani (2011)	It is a sort of IC, exploting a scenario where some communes have access to radio stations and some others have not. Variation in radio access is exogenous, driven by the nature of media markets in northern Benin. Community broadcasters have limited signal strength, so small geographical differences between villages are sufficient to yield large differences in access.	The main outcome is the proportion of children (from second grade) tested in the village public school who could read sentences and paragraphs. They also measure education inputs and households' education investments. The data are from a March 2009 survey of more than 4,000 households and 210 villages, and a literacy test given to 2,100 children in second grade (on average, eight to nine years old) in village schools in Benin. The survey was undertaken in 32 of the 77 communes in Benin, all located in the northern part of the country
Molina (2013b)	The SA implies to give information about the projects through the media and a public forum, in which citizens are told about their rights and entitlements, including the activities they can do to monitor the project and the responsibilities of the executing firm. A group of beneficiaries composed of interested citizens is constituted and trained to carry out community monitoring activities. Additionally periodical public forums are held, bringing together local authorities, neighbors, and representatives from the firm that carries out the specific project. In these public forums, the state of the project is explained in detail to the community, which in turn might voice its suggestions and recommendations. Commitments are made by the firm, the local government, and project supervisor to solve the problems that may arise during the construction of the project. These commitments are monitored by the community, the facilitators from the central government (DNP) and the project	The author carries out a retrospective evaluation and uses indicators derived from a household survey instrument about the projects. For each project with the CVA programme he looks for similar projects without the program, within the same sector (education, health, water and sanitation), with similar spatial concentration of its population, similar initial estimated timeline of the project and similar resources. Additionally he selected projects that were carried out in a non-contiguous community from the same municipality to guarantee same administrative procedures and same responsible local government. Using this methodology, he find matches for 10 CVA projects out of the universe of 400 CVA projects. He expand the search for similar projects in similar municipalities to add three additional pairs to the final sample.

Study	Description of the intervention	Outcomes measurement
	supervisor. If a commitment is not honored, facilitators and supervisors intervene to let the local government know about this. If the problem persists, administrative complaints are submitted to the Supreme Audit Body in the central administration. Before making the final payment to the executing firm, the finalised project is presented to the community. The audit results are shared with all interested and concerned stake-holders.	Two different random samples were collected: (a) a sample of individuals from treated and control projects that may or may not have participated in community monitoring activities and (b) a sample of participants in the public forums. For (a) he use a household survey of 28 infrastructure projects, 13 of which were treated with the CVA programme and 15 were control projects. Each project was located it in the cartographical map and sampled randomly from the surrounding areas. The random sample contains 30 households for all 13 projects in the treatment group and 11 in the control group. For the two CVA projects that have two controls each, each sample contains 20 households. The total sample is 390 treated and 410 control households. For (b), the contact information collected for each community forum for each CVA project is used. He uses a random sample of 10 participants in each of the 13 treated projects.
Olken (2007)	In the invitations treatment, either 300 or 500 invitations were distributed throughout the village several days prior to each of the three accountability meetings. The village head, who normally issues written invitations for the meetings, therefore has the potential to stack the attendance of the accountability meeting in his favor by issuing invitations only to his supporters. By distributing a large number of invitations, the village head's ability to control who attends the meeting was substantially reduced. Given the size of a typical village, approximately one in every two households in	Corruption is measured by comparing the researcher's estimate of what the project actually costs to what the village reported it spent on the project on an item by item basis. A team of engineers and surveyors was assembled who, after the projects were completed, dug core samples in each road to estimate the quantity of materials used, surveyed local suppliers to estimate prices, and interviewed villagers to determine the wages paid on the project. From these data, was constructed an independent estimate of the amount each

Study	Description of the intervention	Outcomes measurement
	treatment villages received an invitation. The invitations were distributed either by sending them home with school children or by asking the heads of hamlets and neighborhood associations to distribute them throughout their areas of the village. The number of invitations (300 or 500) and the method of distributing them (schools or neighborhood heads) were randomised by village. The purpose of these extra randomizations—the number of invitations and how they were distributed—was to generate additional variation in the number and composition of meeting attendees, to distinguish size effects from composition effects.	project actually cost to build and then compare this estimate with what the village reported it spent on the project on a line-item by line-item basis. The difference between what the village claimed the road cost to build and what the engineers estimated it actually cost to build is the key measure of missing expenditures used as outcome in the article. Since the village must account for every rupiah it received from the central government, stolen funds must show up somewhere in the difference between reported expenditures and estimated actual expenditures.
	In the invitations plus comment forms treatment were distributed exactly as in the invitations treatment, but attached to the invitation was a comment form asking villagers' opinions of the project. The idea behind the comment form was that villagers might be afraid of retaliation from village elites, and thus providing an anonymous comment form would increase detection of corruption. The form asked the recipient to answer several questions about the road project and then to return the form—either filled out or blank—to a sealed drop box, placed either at a village school or at a store in the subvillage. The form had three closed-response questions (i.e., requesting answers of the form good,satisfactory, or poor) about various aspects of the project and two freeresponse questions, one asking about the job performance of the implementation team and one asking about any other project-related issues. The comment forms were collected from the drop boxes two days before each	

Study	Description of the intervention	Outcomes measurement
	meeting and summarised by a project enumerator. The enumerator then read the summary, including a representative sample of the open-response questions, at the village meeting.	
Pandey *et al.* *(*2007)	The information campaign was conducted in two rounds in each village cluster, separated by a period of two weeks. Each round consisted of two to three meetings, as well as distribution of posters and leaflets. Residents were informed in advance about the dates and locations of meetings, and separate meetings were held in low- and mid- to high-caste neighborhoods. Each meeting lasted about an hour and consisted of a 15-minute audiotaped presentation that was played twice, opportunities to ask questions, and distribution of leaflets. People were notified that the information was collected from the government and distributed in the public interest by the research team and a nongovernmental organization based in Uttar Pradesh, Sahbhagi Shikshan Kendra.	The outcomes were measured through two surveys. In baseline survey, both parents from each household were asked several questions about access to health and social services. Health services questions included whether a nurse midwife had come to the village in the past four weeks; whether there was a pregnant woman in the household within the past 12 months and, if so, whether she had received a prenatal examination, tetanus shots, and prenatal supplements (iron/folic acid tablets); and whether there was an infant younger than one year in the household and, if so, whether he or she had received any vaccinations. Social services questions included how many children went to primary school in the village for the previous academic year and how much in school fees they were charged, whether a village council meeting had occurred in the past six months, and whether development work was performed in the village. Baseline survey participants were interviewed again 12 months later.
Pandey, Goyal and Sundararaman (2009)	The authors collaborated with the Nike Foundation [...] in the development of campaign tools. The tools consisted of a short film of six minutes, a poster, a wall painting, a take-home calendar and a learning assessment booklet. The tools were the same in all	- Teacher attendance and activity. Four unannounced visits were made, one every two or three weeks, to record attendance and activity. Activity is a measure of whether a teacher is actively engaged in teaching when the team arrives.

Study	Description of the intervention	Outcomes measurement
	states except that the information communicated was state specific. The film, poster and calendar focused on the following information: details of roles and responsibilities of school oversight committees; rules for selection of members of these committees; rules for committee meetings; number of mandatory meetings, minimum attendance requirements for meetings; record keeping of minutes; organization and funding of school accounts; right to information regarding the school including right to obtain copies of any school record; where to complain about any problems; and benefits that students in primary grades are entitled to, such as a cash stipend, textbooks, mid-day meals, school uniforms. The film and poster contained key information while the calendar contained all of the information in detail. The learning assessment booklet outlined the minimum levels of language and mathematics skills that children are expected to acquire by grade, based on the minimum level of learning framework recognised by the Government of India [...] In addition to the information campaign treatment in each of the three states, there was a second treatment carried out only in Karnataka. This was an additional two minute capsule at the end of the film that showed average wages for different levels of schooling to increase awareness about the economic benefits of schooling. The information campaign was conducted in the same way as Pandey *et al*. (2007).	It is scored one if the teacher is teaching, writing on the board, supervising written work, teaching by rote or another method; and scored zero if the teacher is absent, chatting, sitting idle/standing outside classroom, keeping order but not teaching, doing non-teaching work. Teacher attendance and activity variables are constructed as averages over the four visits and interpreted as fraction of visits a teacher was present (or engaged in teaching). Both variables take values between zero and one. - Students were tested in school on competency and curriculum-based language and mathematics tests that lasted approximately 20 minutes. The language test included reading and writing competencies while the mathematics test contained addition, subtraction, multiplication and division. - Interviews of parents of sample students on their knowledge about school oversight committees, whether the students had received entitlements for current school year; textbooks, school uniform, stipend, whether the mid-day meal was served daily in the past week and whether parents had raised school-related issues. In MP and UP, female students in educationally backward blocks – and in Karnataka, all students – are entitled to a school uniform annually.

Study	Description of the intervention	Outcomes measurement
		- Interviews of oversight committee members about their knowledge and participation in oversight.
Piper, B. and Korda, M. (2010)	The EGRA Plus Liberia intervention was itself based on a three-stage intervention strategy. First, a baseline reading assessment was implemented in a nationally representative set of Liberian primary schools. This assessment not only served as the baseline for all the impact evaluations, but also informed the intervention itself, taking student achievement evidence as the first step in assessing teacher training needs, and developing teacher professional development courses to respond to the critical learning areas for improving student achievement. Second, RTI, in collaboration the Ministry of Education and supported by Liberian Education Trust, implemented a teacher professional development programme that included intensive, week-long capacity-building workshops. These workshops gave teachers an opportunity to learn techniques for high-quality instruction in early grade reading. Teachers also received ongoing professional development support and regular feedback regarding their teaching. The intervention was buttressed with activities designed to foster community action and stakeholder participation, particularly around the production and dissemination of EGRA findings reports at various stages in the EGRA Plus intervention. The project also encouraged meetings between school managers and community members. The third major intervention activity was an additional two rounds of	The reading tests evaluated :letter naming fluency, number of names of letters identify in a minute, phonemic awareness, number of sounds identified in a minute, familiar word fluency, familiar words that children could identify in one minute, unfamiliar word fluency, number of unfamiliar words indentify in one minute, reading comprehension, listening comprehension. All of them were measured by test scores.

Study	Description of the intervention	Outcomes measurement
	EGRA, which allowed for a longitudinal research design. This design allowed researchers and the Ministry of Education to identify whether and how the interventions had a significant impact on student achievement, as well as which causal mechanisms were responsible for the project's success.	
Pradhan *et al.* *(*2014)	Training (T): Information campaign (IC) about different topics, such as their lack of knowledge about the decree; and capacity, such as how to engage the community, how to play a role in school management, and how to promote student learning services, and village governance requirements. A two day, district-level training attended by four school committee members (principal, teacher, parent, and one village representative) covered planning, budgeting and steps the school committee could take to support education quality. The budget session focused on a plan for spending the block grant. The training also included a visit to a 'model' school committee that had been successful in applying school-based management practices.	The paper evaluates the effects of four treatments (grant, election, linkage ad training) independently and combined with each other on public primary rural schools indicators. The baseline survey took place in January 2007, midline in April 2008, and the endline survey in October 2008. Tests in mathematics and Indonesian, designed by the Ministry, were administered to all students in grade four at baseline and grade six at endline. They matched students on the basis of student names written on the test sheets and school ID. They were able to match 10,941 students, which is equal to 87 per cent of the tests administered at baseline in grade four, and 88 per cent of the tests administered at endline in grade six in the 517 schools that participated in both rounds. Broadly, these intermediate outcomes relate to awareness of school committees, school-based management, parent, community and teacher inputs to education and perceptions of student learning. They interviewed parents, teachers, students, school committee members, and principals. Administrative data and interviewer observations on infrastructure and teacher activities at the start of visit were also recorded. To track the teachers of
	Linkage (L): meetings between the school committee and the village council, discussing potential measures to address education issues in the village. The first facilitated meeting was between the school principal and the school committee members to identify measures for improving education quality that they would then propose to the village council. These measures were discussed in a subsequent meeting with village council representatives and other village officials, and the results of the meeting were documented in	

Study	Description of the intervention	Outcomes measurement
	a memorandum of understanding, signed by the head of the school committee, the head of the village council and the school principal.	the students tested, the teacher sample was restricted to teachers teaching grade four at baseline and grade six at endline. They then randomly selected three students from their classes, and these students' parents, for interview.
	The authors also include a third treatment that we do not consider as it is not of the type of CMI considered in this review, the intervention introduced changes in the election of the committee. They also explore combinations of treatments given that some individuals in the control groups for each treatment had received the other treatments	
Reinikka and Svensson (2011)	Towards the end of 1997, the Ugandan government began to publish systematic public information on monthly transfers of capitation grants to districts in the national newspapers. The newspaper campaign came in response to evidence of extensive capture and corruption in the education sector –in 1995 schools received on average only 24 per cent of the total yearly capitation grant from the central government (Reinikka and Svensson, 2004). The campaign was intended to enhance head teachers'and parents'ability to monitor the local administration and to voice complaints if funds did not reach the schools.	As a measure of the entitled number of students, the paper take the average of the number of enrolled students (in grades P1–P3 and P4–P7) from the public expenditure tracking surveys and the number of enrolled students according to district records. Also derive a measure of cognitive skills from the Primary Leaving Exam records. Standardised test scores in Math, English, Science, and Social Studies aggregated into a score averaged across grade 7 students in the school.

Appendix E: Results of critical appraisal of studies

Studies to address review question 1

Study	Study design (analysis method)	Selection bias and confounding addressed?	Spillovers addressed?	Outcome reporting bias addressed?	Analysis reporting bias addressed?	Other sources of bias addressed?	Overall risk of bias assessment
Afridi and Iversen (2013)	Longitudinal (DID)	Yes: to assess whether programme implementation improves with repeated social audits within the same mandal, over time, while controlling for other trends that could potentially impact the quality of programme delivery and corruption in the programme.	Unclear: the authors do not mention spillovers concerns in this article, but the persons attending the meetings seem to be local.	Yes: The authors use fixed effects to solve part of the problem.	Yes: different specification models are reported.	Yes: No evidence of other bias.	Low risk
Andrabi, Das and Khwaja (2013)	RCT	Yes: Villages were sampled from three districts: one each in the north, center and south. Within these districts, villages were chosen randomly from among those with at least one private school according to a 2000 census of private schools; this frame captures the educational environment for 60 per cent of the	Yes: Using the facts that children do not travel long distances to school, and villages are geographically separated by farmland (or forests and	Yes: No evidence of outcome reporting bias.	Yes: when available, different measures for the same outcome are reported and different specification and estimation	Yes: No evidence of other bias.	Low risk

Study	Study design (analysis method)	Selection bias and confounding addressed?	Spillovers addressed?	Outcome reporting bias addressed?	Analysis reporting bias addressed?	Other sources of bias addressed?	Overall risk of bias assessment
		province's population. In each of the three districts in our study, we experimentally allocated half the villages (within district stratification) to the group that would receive report cards. Since the report card intervention affects the entire educational marketplace and we were interested in exploring how the overall market would respond, the intervention was carried out at the village rather than the school level. For a well-defined market-level experiment, we required "closed" markets where schools and children (including those who switch schools across years) could be tracked over time.	wasteland), the authors were able to define closed markets for the purpose of the intervention as follows. They constructed boundaries around the sampled villages that were within a fifteen minute walking distance from any house in the village. All institutions offering formal primary education within this boundary were covered by our		methods are applyed. The standard errors are reported in all cases.		

Study	Study design (analysis method)	Selection bias and confounding addressed?	Spillovers addressed?	Outcome reporting bias addressed?	Analysis reporting bias addressed?	Other sources of bias addressed?	Overall risk of bias assessment
			study and are considered to be the “village” schools.				
Banerjee *et al.* *(*2010)	RCT	Yes: The evaluation took place in 280 villages in the Jaunpur district in the state of UP, India. Districts in India are divided into administrative blocks. In each block, on average, there are about 100 villages. Four of these blocks were randomly selected to participate in the study, and the study villages were then randomly selected within each block. The survey and the study are thus representative of Jaunpur district (and its 3.9 million population) as a whole. Each of these interventions was implemented in 65 villages, randomly selected out of the 280	Unclear: Authors do not mention the distance between control and treatment villages, but they use clustering by village in their analysis	Yes: No evidence of outcome reporting bias.	Yes: The only empirical difficulty is that there are a large number of outcomes that could have been affected by the interventions. To avoid "cherry picking" - emphasizing the results that show large effects, the authors	Yes: No evidence of other bias.	Low risk

Study	Study design (analysis method)	Selection bias and confounding addressed?	Spillovers addressed?	Outcome reporting bias addressed?	Analysis reporting bias addressed?	Other sources of bias addressed?	Overall risk of bias assessment
		villages in the baseline between September 2005 and December 2005. A fourth group of 85 villages formed the control group. Monitoring data suggests that the interventions were well implemented. All treated villages held at least one meeting, with some holding more than one, for a total of 215 village-level meetings in the 195 villages.			present results on all of the outcomes on which they collected data, and calculate the average standardised effect over the family of outcomes.		
Barr *et al.* *(*2012)	RCT	Yes: The allocation was done using a stratified random assignment, with sub-counties used as strata to balance the competing aims of comparability within strata and concerns over potential for contamination across study arms. Of five study schools per subcounty, two were assigned to control, and the remaining three were divided between the two	Yes: they stratified the sample by sub-counties	Yes: No evidence of outcome reporting bias.	Yes: different specification and estimation methods are applied. The p-values are reported in all cases.	Yes: No evidence of other bias.	Low risk

Study	Study design (analysis method)	Selection bias and confounding addressed?	Spillovers addressed?	Outcome reporting bias addressed?	Analysis reporting bias addressed?	Other sources of bias addressed?	Overall risk of bias assessment
		treatments. Consequently, each district contains either seven or eight schools of each treatment type.					
Björkman and Svensson (2009)	RCT	Yes: The experiment involved 50 public dispensaries, and health care users in the corresponding catchment areas, in nine districts covering all four regions in Uganda. For the experimental design, the facilities were first stratified by location (districts) and then by population size. From each group, half of the units were randomly assigned to the treatment group and the remaining 25 units were assigned to the control group.	Yes: There are reasons to believe spillovers will not be a serious concern. The average (and median) distance between the treatment and control facility is 30 kilometers and in a rural setting it is unclear to what extent information about improvements in treatment facilities has	Yes: No evidence of outcome reporting bias.	Yes: when available, different measures for the same outcome are reported and different specification and estimation methods are applyed. The standard errors are reported in all cases.	Yes: No evidence of other bias.	Low risk

Study	Study design (analysis method)	Selection bias and confounding addressed?	Spillovers addressed?	Outcome reporting bias addressed?	Analysis reporting bias addressed?	Other sources of bias addressed?	Overall risk of bias assessment
			spread to control communities. The authors do not find evidence in favor of the spillover hypothesis (the results of the tests are available in a supplemental appendix).				
Björkman, de Walque and Svensson (2013)	RCT	Yes: Of the 75 rural communities and facilities, 50 facilities/communities were included in the first-phase of the project (the participation and information intervention) and 25 facilities/communities were added in 2007 (the participation intervention). For each intervention, the units	Yes: although the authors do not mention spillovers concerns in this article, it is a continuation of Björkman and Svensson (2009), where the issue is addressed.	Yes: No evidence of outcome reporting bias.	Yes: when available, different measures for the same outcome are reported and different specification and estimation	Yes: No evidence of other bias.	Low risk

Study	Study design (analysis method)	Selection bias and confounding addressed?	Spillovers addressed?	Outcome reporting bias addressed?	Analysis reporting bias addressed?	Other sources of bias addressed?	Overall risk of bias assessment
		(facility/community) were first stratified by location (districts) and then by population size. From each block, half of the units were randomly assigned to the treatment group and the remaining health facilities were assigned to the control group.			methods are applyed. The standard errors are reported in all cases.		
Gertler et. al. (2008)	Longitudinal (DID)	Yes: The paper use the phased rollout of the AGE to identify treatment and comparison groups, with the treatment group being schools getting AGE early and the comparison group being those who got AGE later.	Unclear: there is a probability, although we think is low, of contamination between municipalities.	Yes: The authors use fixed effects to solve part of the problem.	Yes: different specification models are reported together with their corresponding p-values of significance tests	Yes: No evidence of other bias.	Low risk
Keefer and Khemani (2011)	Quasi-experimental Cross-	Yes: The fragmentation of the Benin radio offers a quasi natural experiment, the authors report no statistically significant association	Yes: Signals from multiple communes spill over to villages in	Unclear: the outcome selected is a new type of	Yes: different specification models are reported	Yes: No evidence of other bias.	Low risk

Study	Study design (analysis method)	Selection bias and confounding addressed?	Spillovers addressed?	Outcome reporting bias addressed?	Analysis reporting bias addressed?	Other sources of bias addressed?	Overall risk of bias assessment
	section (regression)	between village characteristics and access to radios.	adjoining communes, leading to substantial variation in access to neighboring commune-based radio across villages within the same commune. The authors perform several tests to show that these variations are uncorrelated with village-specific characteristics.	literacy test, but it is not justified the reason for using this measure.	together with their corresponding p-values of significance tests		
Molina (2013)	Cross sectional	Yes: use a household survey of 28 infrastructure projects, 13 of which were treated with the CVA	Yes: for the matching, the author selected	Yes: No evidence of outcome	Yes: different specification and estimation	Yes: No evidence of other bias.	Low risk

Study	Study design (analysis method)	Selection bias and confounding addressed?	Spillovers addressed?	Outcome reporting bias addressed?	Analysis reporting bias addressed?	Other sources of bias addressed?	Overall risk of bias assessment
	(regression)-Matching	programme and 15 were control projects. Each project was located it in the cartographical map and sampled randomly from the surrounding areas. The random sample contains 30 households for all 13 projects in the treatment group and 11 in the control group. For the two CVA projects that have two controls each, each sample contains 20 households. The total sample is 390 treated and 410 control households. They use a random sample of 10 participants in each of the 13 treated projects.	projects that were carried out in non- contiguous communities with the same characteristics.	reporting bias.	methods are applied. The p-values are reported in all cases. The paper also reports the risk difference.		
Olken (2007)	RCT	Yes: randomization into the invitations and comment form treatments was independent of randomization into the audit treatment. In both cases, the treatments were announced to	Yes: The author was a concern that the audit treatment might be likely to spill over from one	Yes: No evidence of outcome reporting bias.	Yes: are reported three different specifications: no fixed effects, fixed	Yes: No evidence of other bias.	Low risk

Study	Study design (analysis method)	Selection bias and confounding addressed?	Spillovers addressed?	Outcome reporting bias addressed?	Analysis reporting bias addressed?	Other sources of bias addressed?	Overall risk of bias assessment
		villages after the project design and allocations to each village had been finalised, but before construction or procurement of materials began.10 Thus the choice of what type of project to build, as well as the project's design and planned budget, should all be viewed as exogenous with respect to the experiments.	village to another, since officials in other villages might worry that when the auditors came to the subdistrict, their villages might be audited as well. On the other hand, the participation treatments were much less likely to have similar spillover effects, since the treatment was directly observable in the different villages early on. Therefore, the		effects for each engineering team that conducted survey, and stratum fixed effects. The adjusted standard errors are reported in all cases.		

Study	Study design (analysis method)	Selection bias and confounding addressed?	Spillovers addressed?	Outcome reporting bias addressed?	Analysis reporting bias addressed?	Other sources of bias addressed?	Overall risk of bias assessment
			randomization for audits was clustered by subdistrict (i.e., either all study villages in a subdistrict received audits or none did), whereas the randomization for invitations and comment forms was done village by village. The calculations of the standard errors are adjusted to take into account the potential correlation of outcomes in				

Study	Study design (analysis method)	Selection bias and confounding addressed?	Spillovers addressed?	Outcome reporting bias addressed?	Analysis reporting bias addressed?	Other sources of bias addressed?	Overall risk of bias assessment
			villages within a subdistrict.				
Pandey *et al.* *(*2007)	RCT	Yes: Of the 70 districts in this state, authors focused on 21 central, central eastern, and southern districts in which they have previously conducted surveys. Districts consist of approximately 14 blocks and each block consists of about 65 village clusters. From a comprehensive list of blocks and village clusters, a random number generator was used to randomly select one block within each district and then randomly select five village clusters within each block. They then randomly assigned districts to intervention and control arms.	Yes: By randomly selecting only five village clusters of about 1000 in each district, authors spread the selection of 105 village clusters over 21 districts to minimize any potential for contamination between intervention and control villages. Although the districts were adjacent to one another, no two	Yes: No evidence of outcome reporting bias.	Unclear: the study reports the results of a multivariate random-effect regression but there is no discussion on the specification model.	No: according to the authors, there is a possible recall bias.	Low risk

Study	Study design (analysis method)	Selection bias and confounding addressed?	Spillovers addressed?	Outcome reporting bias addressed?	Analysis reporting bias addressed?	Other sources of bias addressed?	Overall risk of bias assessment
			blocks were adjacent to each other and the village clusters were far apart. Travel between them would be difficult.				
Pandey, Goyal and Sundararaman (2009)	RCT	Yes: GPs from three states were randomly allocated to receive or not receive the information campaign. A GP is a cluster of approximately one to three adjacent villages and is the smallest unit of local government. In each state, four districts were chosen purposefully, matched across states by literacy rates. Within a district, 50 GPs were selected from two randomly chosen blocks. A random number generator was used to randomly	Yes: Treatment and control GPs were evenly spread across the two blocks to reduce any potential contamination between intervention and control villages.	Yes: No evidence of outcome reporting bias.	Unclear: the analytical model is not specified	No: some estimates are performed at teacher's or student's level but the number of observations is not reported	Medium risk

Study	Study design (analysis method)	Selection bias and confounding addressed?	Spillovers addressed?	Outcome reporting bias addressed?	Analysis reporting bias addressed?	Other sources of bias addressed?	Overall risk of bias assessment
		select the blocks and then GPs within the blocks. One-half of the GPs within each block were then randomly assigned to the intervention arm and the remaining half to the control arm. Treatment and control GPs were evenly spread across the two blocks to reduce any potential contamination between intervention and control villages. In one state (Karnataka) the design was identical except an additional set of treatment villages was added that received a slightly different treatment called information and advocacy campaign.					
Piper and Korda (2010)	RCT	EGRA Plus: Liberia was designed as a randomised controlled trial. Groups of 60 schools were randomly selected into treatment,	Yes: Although the authors do not mention spillovers concerns in this	Yes: No evidence of outcome	Yes, the model is well specified, and they study the	Unclear: according to the authors, there is a	Medium risk

Study	Study design (analysis method)	Selection bias and confounding addressed?	Spillovers addressed?	Outcome reporting bias addressed?	Analysis reporting bias addressed?	Other sources of bias addressed?	Overall risk of bias assessment
		and control groups. These groups were clustered within districts, such that several nearby schools were organised together.	article, they use cluster within districts	reporting bias.	correlations between variables	possible recall bias.	
Pradhan *et al.* *(*2014)	RCT	Yes: From the 44 sub-districts of six districts, they selected 520 villages and randomly selected one school from each of these villages. The resulting sample of 520 schools was then stratified into three groups using their average test scores. Within each stratum, schools were randomly assigned into the nine treatments and comparison groups. They dropped schools with extremely good or bad average sixth grade examination scores in mathematics or Indonesian. To gauge the extent of the external validity problem due to this selection criterion, they checked	Yes: To avoid spillovers between treatment and comparison schools within a village, they sampled one school per village.	Yes: No evidence of outcome reporting bias.	Unclear: the study reports the results but there is no discussion on the specification model.	Yes: attrition occurred both in terms of schools and students. They do find that students with lower baseline scores have a statistically significantly higher probability of not being matched,	Low risk

Study	Study design (analysis method)	Selection bias and confounding addressed?	Spillovers addressed?	Outcome reporting bias addressed?	Analysis reporting bias addressed?	Other sources of bias addressed?	Overall risk of bias assessment
		the average scores for the selected schools and the full sample. The median is also not that different.				but the size of the effect is small. They believe that most of the matching problems arise from problems in writing names.	
Reinikka and Svensson (2011)	Longitudinal (DID), Instrumental Variable (IV)	The identification strategy builds on two assumptions. First, prior to 1998 –before the government began to systematically publish data on disbursement –schools, 'knowledge about the grant programme was largely a function of own effort and ability. Second, schools/communities closer to a newspaper outlet will be more	Yes: they use a distance variable as an instrument for exposure, and assess its validity.	Yes: The authors use fixed effects to solve part of the problem.	Yes: different methodologies are used to check the issue.	Yes: No evidence of other bias.	Low risk

Study	Study design (analysis method)	Selection bias and confounding addressed?	Spillovers addressed?	Outcome reporting bias addressed?	Analysis reporting bias addressed?	Other sources of bias addressed?	Overall risk of bias assessment
		exposed to information disseminated through newspapers. Controlling for time and school …fixed effects, our strategy is thus to use distance timing as an instrument for exposure (access to information). We assess the validity of this instrument procedure next.					

Studies to address review question 2

Study	Study design (analysis method)	Selection bias and confounding addressed?	Spillovers addressed?	Outcome reporting bias addressed?	Analysis reporting bias addressed?	Other sources of bias addressed?	Overall risk of bias assessment
Banerjee *et al*. (2007)	Descriptive statistics using data from the baseline survey of	Yes: The evaluation took place in 280 villages in the Jaunpur district in the state of UP, India. Districts in India are divided into administrative blocks. In each block, on average, there are about 100 villages. Four of these blocks	Unclear: Authors do not mention the distance between control and treatment villages, but they use	Yes: No evidence of outcome reporting bias.	Yes: No evidence of bias	Yes: No evidence of other bias.	Low risk

	Banerjee *et al*. (2010)	were randomly selected to participate in the study, and the study villages were then randomly selected within each block. The survey and the study are thus representative of Jaunpur district (and its 3.9 million population) as a whole. Each of these interventions was implemented in 65 villages, randomly selected out of the 280 villages in the baseline between September 2005 and December 2005. A fourth group of 85 villages formed the control group.	clustering by village in their analysis				
Banerjee et. al (2010)	RCT	Yes: The evaluation took place in 280 villages in the Jaunpur district in the state of UP, India. Districts in India are divided into administrative blocks. In each block, on average, there are about 100 villages. Four of these blocks were randomly selected to participate in the study, and the study villages were then randomly selected within each block. The survey and the study are thus representative of Jaunpur district (and its 3.9 million population) as a whole. Each of these interventions was	Unclear: Authors do not mention the distance between control and treatment villages, but they use clustering by village in their analysis	Yes: No evidence of outcome reporting bias.	Yes: The only empirical difficulty is that there are a large number of outcomes that could have been affected by the interventions. To avoid "cherry picking" - emphasizing the results that show large	Yes: No evidence of other bias.	Low risk

		implemented in 65 villages, randomly selected out of the 280 villages in the baseline between September 2005 and December 2005. A fourth group of 85 villages formed the control group. Monitoring data suggests that the interventions were well implemented. All treated villages held at least one meeting, with some holding more than one, for a total of 215 village-level meetings in the 195 villages.			effects, the authors present results on all of the outcomes on which they collected data, and calculate the average standardised effect over the family of outcomes.		
Björkman and Svensson (2010)	Seemingly unrelated regression system.	Yes: They use a smaller subset of the data in Björkman and Svensson (2009).	Yes: as in Björkman and Svensson (2009) there are reasons to believe spillovers will not be a serious concern.	Yes: No evidence of outcome reporting bias.	Yes: when available, different measures for the same outcome are reported and different specification and estimation methods are applyed. The standard errors are reported in all cases.	Yes: No evidence of other bias.	Low risk

Molina (2013)	Cross sectional (regression)-Matching	Yes: use a household survey of 28 infrastructure projects, 13 of which were treated with the CVA programme and 15 were control projects. Each project was located it in the cartographical map and sampled randomly from the surrounding areas. The random sample contains 30 households for all 13 projects in the treatment group and 11 in the control group. For the two CVA projects that have two controls each, each sample contains 20 households. The total sample is 390 treated and 410 control households. They use a random sample of 10 participants in each of the 13 treated projects.	Yes: for the matching, the author selected projects that were carried out in non-contiguous communities whith the same characteristics.	Yes: No evidence of outcome reporting bias.	Yes: different specification and estimation methods are applied. The p-values are reported in all cases. The paper also report the risk difference.	Yes: No evidence of other bias.	Low risk
Olken (2004)	Descriptive statistics and Ordinary-least-squares (OLS)	Yes: uses the same strategy as Olken 2007	Yes: uses the same strategy as Olken 2007	Yes: No evidence of outcome reporting bias.	Yes: No evidence of analysis reporting bias.	Yes: No evidence of other bias.	Low risk
Olken (2005)	Probit model and Ordinary-least-squares (OLS).	Yes: uses the same strategy as Olken 2007	Yes: uses the same strategy as Olken 2007	Yes: analyses how the results of Olken (2007)	Yes, it reports alternatives	Yes: No evidence of other bias.	Low risk

				would change by using a perception measure			
Olken (2007)	RCT	Yes: randomization into the invitations and comment form treatments was independent of randomization into the audit treatment. In both cases, the treatments were announced to villages after the project design and allocations to each village had been finalised, but before construction or procurement of materials began.10 Thus the choice of what type of project to build, as well as the project's design and planned budget, should all be viewed as exogenous with respect to the experiments.	Yes: The autor was a concern that the audit treatment might be likely to spill over from one village to another, since officials in other villages might worry that when the auditors came to the subdistrict, their villages might be audited as well. On the other hand, the participation treatments were much less likely to have similar spillover effects, since the treatment was directly observable in the different villages	Yes: No evidence of outcome reporting bias.	Yes: are reported three diferents specifications: no fixed effects, fixed effects for each engineering team that conducted survey, and stratum fixed effects. The adjusted standard errors are reported in all cases.	Yes: No evidence of other bias.	Low risk

			early on. Therefore, the randomization for audits was clustered by subdistrict (i.e., either all study villages in a subdistrict received audits or none did), whereas the randomization for invitations and comment forms was done village by village. The calculations of the standard errors are adjusted to take into account the potential correlation of outcomes in villages within a subdistrict.				
Pandey et. al (2007)	RCT	Yes: Of the 70 districts in this state, authors focused on 21 central, centraleastern, and southern districts in which they have previously conducted	Yes: By randomly selecting only five village clusters of about 1000 in each	Yes: No evidence of outcome reporting bias.	Unclear: the study reports the results of a multivariate	No: according to the authors, there is a	Low risk

		surveys. Districts consist of approximately 14 blocks and each block consists of about 65 village clusters. From a comprehensive list of blocks and village clusters, a random number generator was used to randomly select one block within each district and then randomly select five village clusters within each block. They then randomly assigned districts to intervention and control arms.	district, authors spread the selection of 105 village clusters over 21 districts to minimize any potential for contamination between intervention and control villages. Although the districts were adjacent to one another, no two blocks were adjacent to each other and the village clusters were far apart. Travel between them would be difficult.		random-effect regression but there is no discussion on the specification model.	possible recall bias.	
Pradhan et. al (2013)	RCT	Yes: From the 44 sub-districts of six districts, they selected 520 villages and randomly selected one school from each of these villages. The resulting	Yes: To avoid spillovers between treatment and comparison schools	Yes: No evidence of outcome reporting bias.	Unclear: the study reports the results but there is no	Yes: attrition occurred both in terms of	Low risk

	sample of 520 schools was then stratified into three groups using their average test scores. Within each stratum, schools were randomly assigned into the nine treatments and comparison groups. They dropped schools with extremely good or bad average sixth grade examination scores in mathematics or Indonesian. To gauge the extent of the external validity problem due to this selection criterion, they checked the average scores for the selected schools and the full sample. The median is also not that different.	within a village, they sampled one school per village.	discussion on the specification model.	schools and students. They do find that students with lower baseline scores have a statistically significantly higher probability of not being matched, but the size of the effect is small. They believe that most of the matching problems arise from problems in writing names.

Singh and Vutukuru (2009)	Mix of quantitative (DID) and qualitative methods.	Yes: compare the performance of Karnataka, a neighbouring state, which has not taken up social audit, to Andhra Pradesh, in the overall implementation of the program; and the reasons behind the successful scale up of social audits in Andhra Pradesh. A difference of difference estimator was used to estimate the effect of social audit using the person-days of work generated and the proportion of timely payments in mandals (sub-district level) where social audit had been conducted and mandals where it had not been conducted in the years 2006-07 and 2007-08.	Yes: They selected one control mandal for each treatment mandal, where social audit was not conducted in 2006-07. The control mandal was chosen to be in the same district and was a geographically adjacent mandal. The control mandal was chosen by listing all the adjacent mandals, and then looking at the date in which the social audit has been conducted in that mandal. The control mandal was designated as the mandal where the social audit had been conducted after September	Yes: No evidence of outcome reporting bias.	Yes: The difference of difference estimator was used because it gave us the best chance of isolating the impact of social audits. By comparing the difference in performance before and after the social audits between the treated and control mandals and by ensuring that the control mandals are similar in all other aspects to the treatment mandals, we	Yes: An interstate comparison is actually of little added significance in view of the major challenges the programme faces in Karnataka, compared with the relative stability and robust growth of the programme in Andhra Pradesh.	Low risk

			2007. This would imply that the mandal had no social audit in 2006-07 (no treatment) and social audit, if conducted in the year 2007-08, had been conducted in the second half of the financial year, so that the size of the programme in 2007/08 can be assumed to be substantially without the treatment.		could isolate the impact of social audit on the program.		
Woodhouse (2005)	Analysis of identified corruption cases, field visits, and on-site interviews.	Unclear, although the author exploits several alternative sources of information	Unclear	Yes: No evidence of outcome reporting bias.	Unclear	Yes: No evidence of other bias.	Medium risk

Appendix F: Reasons for exclusion

Authors (year)	CMI criterion: Does the study assess a community monitoring intervention?	Outcome types: Does the study have outcomes on corruption, service delivery or quality of services?	Data: Does the study collect data at the individual or the community level?	Methodology criterion: Does the study report at least some information on all of the following: research question; procedures for collecting data; sampling and recruitment?	Reason for exclusion
	[1=yes, 2=no, 9=unclear]	[1=yes, 2=no, 9=unclear]	[1=individual, 2=community, 9=unclear]	[1=yes, 2=no, 9=unclear]	
Abdullah, R. (2006)	2	1	2	9	Relevance and Methodology
Adamolekun, L. (2002)	2	2	2	2	Relevance and Methodology
Adserà, A., *et al*. (2003)	2	1	2	2	Relevance and Methodology
Anazodo, R. O., *et al*. (2012)	2	1	2	2	Relevance and Methodology

Authors (year)	CMI criterion: Does the study assess a community monitoring intervention?	Outcome types: Does the study have outcomes on corruption, service delivery or quality of services?	Data: Does the study collect data at the individual or the community level?	Methodology criterion: Does the study report at least some information on all of the following: research question; procedures for collecting data; sampling and recruitment?	Reason for exclusion
Asaduzzaman, M. (2011)	2	1	9	9	Relevance and Methodology
Basheka, B. C. (2009)	2	9	1	9	Relevance and Methodology
Bassey, A. O., *et al*. (2013)	2	1	9	2	Relevance and Methodology
Beasley and Huillery (2012)	1	2	1	1	Methodology
Bhatnagar, S. C. (2002)	2	2	9	2	Relevance
Bisht, B. S. and S. Sharma (2011)	1	2	1	1	Relevance and Methodology

Authors (year)	CMI criterion: Does the study assess a community monitoring intervention?	Outcome types: Does the study have outcomes on corruption, service delivery or quality of services?	Data: Does the study collect data at the individual or the community level?	Methodology criterion: Does the study report at least some information on all of the following: research question; procedures for collecting data; sampling and recruitment?	Reason for exclusion
Blunt, P. (2009)	2	1	9	1	Relevance and Methodology
Boyd, T. M. (2005)	2	1	2	9	Relevance and Methodology
Brixi, H. (2009)	1	1	1	2	Methodology
Bussell, J. L. (2010)	2	1	9	1	Relevance
Calavan, Barr and Blair (2009)	2	2	2	9	Relevance and Methodology
Cano Blandón, L. F. (2008)	2	2	9	2	Relevance
Capuno, J. J. and M. M. Garcia (2010)	2	2	1	1	Relevance and Methodology

Authors (year)	CMI criterion: Does the study assess a community monitoring intervention?	Outcome types: Does the study have outcomes on corruption, service delivery or quality of services?	Data: Does the study collect data at the individual or the community level?	Methodology criterion: Does the study report at least some information on all of the following: research question; procedures for collecting data; sampling and recruitment?	Reason for exclusion
Carasciuc, L. (2001)	2	1	1	1	Relevance and Methodology
Caseley, J. (2003)	2	9	9	9	Relevance and Methodology
Caseley, J. (2006)	2	9	9	9	Relevance and Methodology
Claudio, O. L. (1996)	2	9	9	9	Relevance and Methodology
Devas, N. and U. Grant (2003)	2	2	9	2	Relevance and Methodology
Dibie, R. (2003)	2	9	9	9	Relevance and Methodology

Authors (year)	CMI criterion: Does the study assess a community monitoring intervention?	Outcome types: Does the study have outcomes on corruption, service delivery or quality of services?	Data: Does the study collect data at the individual or the community level?	Methodology criterion: Does the study report at least some information on all of the following: research question; procedures for collecting data; sampling and recruitment?	Reason for exclusion
Digman, E. R. (2006)	2	9	9	9	Relevance and Methodology
Dorado, D. (2009).	2	2	9	9	Relevance and Methodology
Eckardt, S. (2008)	2	9	9	1	Relevance and Methodology
Ferraz, C. and F. Finan (2011)	2	1	2	1	Relevance and Methodology
Ferraz, C., *et al*. (2012)	2	1	2	1	Relevance and Methodology
Francken, N., *et al*. (2006)	2	1	2	1	Relevance and Methodology

Authors (year)	CMI criterion: Does the study assess a community monitoring intervention?	Outcome types: Does the study have outcomes on corruption, service delivery or quality of services?	Data: Does the study collect data at the individual or the community level?	Methodology criterion: Does the study report at least some information on all of the following: research question; procedures for collecting data; sampling and recruitment?	Reason for exclusion
Goldfrank, B. (2002)	2	9	1	9	Relevance and Methodology
Goodspeed, T. J. (2011)	2	1	2	1	Relevance and Methodology
Gray-Molina, Pérez de Rada and Yañez (1999)	2	1	1	1	Relevance and Methodology
Hentic, I. and G. Bernier (1999)	2	9	9	9	Relevance and Methodology
Huss, R. (2011)	2	1	2	9	Relevance and Methodology

Authors (year)	CMI criterion: Does the study assess a community monitoring intervention?	Outcome types: Does the study have outcomes on corruption, service delivery or quality of services?	Data: Does the study collect data at the individual or the community level?	Methodology criterion: Does the study report at least some information on all of the following: research question; procedures for collecting data; sampling and recruitment?	Reason for exclusion
Iati, I. (2007)	2	2	2	2	Relevance and Methodology
Israr, S. M. and A. Islam (2006)	2	9	9	9	Relevance and Methodology
Jarquin, E. and F. Carrillo-Flores (2000)	2	9	9	9	Relevance and Methodology
Kakumba, U. (2010)	2	9	9	9	Relevance
Kaufmann, D., *et al*. (2002)	2	1	2	1	Relevance and Methodology
Khagram, S. (2013)	2	1	9	9	Relevance and Methodology

Authors (year)	CMI criterion: Does the study assess a community monitoring intervention?	Outcome types: Does the study have outcomes on corruption, service delivery or quality of services?	Data: Does the study collect data at the individual or the community level?	Methodology criterion: Does the study report at least some information on all of the following: research question; procedures for collecting data; sampling and recruitment?	Reason for exclusion
Khalid, S.-N. A. (2010)	2	9	2	1	Relevance
Kohl, B. (2003)	1	1	2	2	Methodology
Kolybashkina, N. (2009)	2	1	2	1	Relevance and Methodology
Kubal, M. R. (2001)	2	1	2	2	Relevance and Methodology
Kumnerdpet, W. (2010)	2	2	1	1	Relevance
Kurosaki, T. (2006)	2	2	1	1	Relevance
Lamprea, E. (2010)	2	1	2	1	Relevance and Methodology

Authors (year)	CMI criterion: Does the study assess a community monitoring intervention?	Outcome types: Does the study have outcomes on corruption, service delivery or quality of services?	Data: Does the study collect data at the individual or the community level?	Methodology criterion: Does the study report at least some information on all of the following: research question; procedures for collecting data; sampling and recruitment?	Reason for exclusion
Lassibille *et al.* *(*2010)	1	2	1	1	Methodology
Li, L. (2001)	2	1	1	9	Relevance and Methodology
Lieberman, Posner and Tsai (2013)	1	2	1	1	Methodology
Loewenson, R. (2000)	2	2	2	1	Relevance and Methodology
Lopez, J. A. F. (2002)	2	2	2	1	Relevance and Methodology
Lulle, T. (2004)	2	2	2	1	Relevance and Methodology

Authors (year)	CMI criterion: Does the study assess a community monitoring intervention?	Outcome types: Does the study have outcomes on corruption, service delivery or quality of services?	Data: Does the study collect data at the individual or the community level?	Methodology criterion: Does the study report at least some information on all of the following: research question; procedures for collecting data; sampling and recruitment?	Reason for exclusion
Mackay, K. and S. Gariba (2000)	2	1	2	1	Relevance and Methodology
MacLean, M. J. (2005)	2	2	9	1	Relevance
MacPherson, E. (2008)	2	1	2	1	Relevance and Methodology
Mahmood, Q., *et al*. (2012)	1	2	1	1	Relevance
Mahmud, S. G., *et al*. (2007)	1	2	2	1	Relevance and Methodology
Malinowitz, S. (2006)	2	1	2	9	Relevance and Methodology

Authors (year)	CMI criterion: Does the study assess a community monitoring intervention?	Outcome types: Does the study have outcomes on corruption, service delivery or quality of services?	Data: Does the study collect data at the individual or the community level?	Methodology criterion: Does the study report at least some information on all of the following: research question; procedures for collecting data; sampling and recruitment?	Reason for exclusion
Manor, J. (2004)	2	2	2	2	Relevance and Methodology
Marulanda, L. (2004)	2	2	2	1	Relevance and Methodology
Matančević, J. (2011)	2	1	1	1	Relevance and Methodology
Mbanaso, M. U. (1989)	2	1	2	1	Relevance and Methodology
McAntony, T. S. (2009)	2	1	2	1	Relevance and Methodology
McDonald, J. (2006)	2	1	2	2	Relevance and Methodology

Authors (year)	CMI criterion: Does the study assess a community monitoring intervention?	Outcome types: Does the study have outcomes on corruption, service delivery or quality of services?	Data: Does the study collect data at the individual or the community level?	Methodology criterion: Does the study report at least some information on all of the following: research question; procedures for collecting data; sampling and recruitment?	Reason for exclusion
McNulty, S. (2013)	1	2	2	1	Relevance and Methodology
Mela, U. A. (2009)	2	1	1	1	Relevance and Methodology
Miarsono, H. (2000)	2	1	1	1	Relevance and Methodology
Mitchinson, R. (2003)	2	1	2	2	Relevance and Methodology
Mohammadi, S. H., *et al*. (2011)	2	2	1	1	Relevance and Methodology
Mohmand, S. K. and A.	2	1	1	9	Relevance and Methodology

Authors (year)	CMI criterion: Does the study assess a community monitoring intervention?	Outcome types: Does the study have outcomes on corruption, service delivery or quality of services?	Data: Does the study collect data at the individual or the community level?	Methodology criterion: Does the study report at least some information on all of the following: research question; procedures for collecting data; sampling and recruitment?	Reason for exclusion
Cheema (2007)					
Molyneux, S., *et al*. (2012)	2	1	1	1	Relevance and Methodology
Montambeault, F. c. (2011)	2	2	1	1	Relevance
Morrison, K. M and M. M. Singer (2006)	2	2	1	1	Relevance
Mosquera, J., *et al*. (2009)	2	1	1	1	Relevance
Mubangizi, B. C. (2009)	2	1	2	9	Relevance and Methodology
Muriisa, R. K. (2008)	2	1	2	1	Relevance and Methodology

Authors (year)	CMI criterion: Does the study assess a community monitoring intervention?	Outcome types: Does the study have outcomes on corruption, service delivery or quality of services?	Data: Does the study collect data at the individual or the community level?	Methodology criterion: Does the study report at least some information on all of the following: research question; procedures for collecting data; sampling and recruitment?	Reason for exclusion
Muwanga, N. K. M. S. (2000)	2	1	2	2	Relevance and Methodology
Narayanan, S. (2010)	2	1	2	1	Relevance and Methodology
Nengwekhulu, R. H. (2009)	2	1	2	9	Relevance and Methodology
Nguemegne, J. P. (2009)	2	1	9	1	Relevance and Methodology
Nguyen, P. (2010)	2	1	1	1	Relevance and Methodology
Nguyen, T. V. (2008) Chapter 2	2	2	1	1	Relevance

Authors (year)	CMI criterion: Does the study assess a community monitoring intervention?	Outcome types: Does the study have outcomes on corruption, service delivery or quality of services?	Data: Does the study collect data at the individual or the community level?	Methodology criterion: Does the study report at least some information on all of the following: research question; procedures for collecting data; sampling and recruitment?	Reason for exclusion
Nsingo, S. A. M. and J. O. Kuye (2005)	2	1	2	9	Relevance and Methodology
Nurick, R. (1998)	2	2	2	2	Relevance and Methodology
O'Leary, D. (2010)	2	1	1	2	Relevance and Methodology
OECD(2007)	2	1	2	2	Relevance and Methodology
Ohemeng, F. L. K. (2010)	2	1	2	2	Relevance and Methodology
Olken, B. A. and R. Pande (2012)	2	1	2	1	Relevance and Methodology

Authors (year)	CMI criterion: Does the study assess a community monitoring intervention?	Outcome types: Does the study have outcomes on corruption, service delivery or quality of services?	Data: Does the study collect data at the individual or the community level?	Methodology criterion: Does the study report at least some information on all of the following: research question; procedures for collecting data; sampling and recruitment?	Reason for exclusion
Olmedo, M. S. G. (2005)	2	2	2	2	Relevance and Methodology
Olowu, D. (1985)	2	1	2	9	Relevance and Methodology
Omar, M. (2009)	2	1	2	2	Relevance and Methodology
Pandey, P. (2010)	2	1	9	1	Relevance and Methodology
Pape-Yalibat (2003)	1	2	9	2	Methodology
Paredes-Solís, S., *et al*. (2011)	2	1	2	1	Relevance and Methodology

Authors (year)	CMI criterion: Does the study assess a community monitoring intervention?	Outcome types: Does the study have outcomes on corruption, service delivery or quality of services?	Data: Does the study collect data at the individual or the community level?	Methodology criterion: Does the study report at least some information on all of the following: research question; procedures for collecting data; sampling and recruitment?	Reason for exclusion
Parker, A. N. (1998)	2	1	1	1	Relevance and Methodology
Pascaru, M. and C. Ana Butiu (2010)	2	2	2	1	Relevance and Methodology
Pathak, R. D., *et al*. (2009)	2	1	1	2	Relevance and Methodology
Paul, S. (2002)	2	1	1	2	Relevance and Methodology
Payani, H. (2000)	2	1	2	2	Relevance and Methodology
Paz Cuevas, C. (1999)	2	2	2	2	Relevance and Methodology

Authors (year)	CMI criterion: Does the study assess a community monitoring intervention?	Outcome types: Does the study have outcomes on corruption, service delivery or quality of services?	Data: Does the study collect data at the individual or the community level?	Methodology criterion: Does the study report at least some information on all of the following: research question; procedures for collecting data; sampling and recruitment?	Reason for exclusion
Peirce, M. H. (1998)	2	1	2	2	Relevance and Methodology
Peters, D. H., *et al*. (2007)	2	1	1	1	Relevance
Petrova, T. (2011)	2	2	2	1	Relevance and Methodology
Plummer, J. and P. Cross (2006)	2	1	2	2	Relevance and Methodology
Priyadarshee, A. and F. Hossain (2010)	2	1	1	2	Relevance and Methodology
Quiroga, G. d. (1999)	2	2	2	2	Relevance and Methodology

Authors (year)	CMI criterion: Does the study assess a community monitoring intervention?	Outcome types: Does the study have outcomes on corruption, service delivery or quality of services?	Data: Does the study collect data at the individual or the community level?	Methodology criterion: Does the study report at least some information on all of the following: research question; procedures for collecting data; sampling and recruitment?	Reason for exclusion
Rajshree, N. and B. Srivastava (2012)	2	1	2	2	Relevance and Methodology
Reaud, B. (2011)	2	1	2	9	Relevance and Methodology
Recanatini, F., *et al*. (2008)	2	1	1	9	Relevance and Methodology
Remme, J. H. F. (2010)	2	2	2	1	Relevance and Methodology
Rincón González and Mujica Chirinos (2010)	2	2	1	9	Relevance

Authors (year)	CMI criterion: Does the study assess a community monitoring intervention?	Outcome types: Does the study have outcomes on corruption, service delivery or quality of services?	Data: Does the study collect data at the individual or the community level?	Methodology criterion: Does the study report at least some information on all of the following: research question; procedures for collecting data; sampling and recruitment?	Reason for exclusion
Ringold, D., *et al*. (2012)	9	1			Methodology
River-Ottenberger, A. X. (2004)	2	1	2	2	Relevance and Methodology
Rose, J. (2010)	2	1	2	9	Relevance and Methodology
Ross Arnold, J. (2012)	2	1	2	1	Relevance and Methodology
Ruzaaza, G., *et al*. (2013)	2	1	2	1	Relevance and Methodology
Sangita, S. (2007)	2	1	2	1	Relevance and Methodology

Authors (year)	CMI criterion: Does the study assess a community monitoring intervention?	Outcome types: Does the study have outcomes on corruption, service delivery or quality of services?	Data: Does the study collect data at the individual or the community level?	Methodology criterion: Does the study report at least some information on all of the following: research question; procedures for collecting data; sampling and recruitment?	Reason for exclusion
Sawada, Y. (1999)	2	2	9	1	Relevance and Methodology
Schatz, F. (2013)	2	1	2	1	Relevance and Methodology
Shah, A. (1999)	2	1	2	2	Relevance and Methodology
Shah, A. (2008)	2	1	2	2	Relevance and Methodology
Siddiquee, N. A. (2008)	2	1	2	2	Relevance and Methodology
Singh, G., *et al*. (2010)	2	1	1	9	Relevance and Methodology

Authors (year)	CMI criterion: Does the study assess a community monitoring intervention?	Outcome types: Does the study have outcomes on corruption, service delivery or quality of services?	Data: Does the study collect data at the individual or the community level?	Methodology criterion: Does the study report at least some information on all of the following: research question; procedures for collecting data; sampling and recruitment?	Reason for exclusion
Smith, J. A. and J. M. Green (2006)	2	1	2	2	Relevance
Smulovitz, C. and E. Peruzzotti (2000)	2	1	2	2	Relevance and Methodology
Souza, C. (2001)	2	2	2	2	Relevance and Methodology
Speer, J. (2012)	2	1	1	2	Relevance and Methodology
Stromberg, J. (1975)	2	1	2	2	Relevance and Methodology

Authors (year)	CMI criterion: Does the study assess a community monitoring intervention?	Outcome types: Does the study have outcomes on corruption, service delivery or quality of services?	Data: Does the study collect data at the individual or the community level?	Methodology criterion: Does the study report at least some information on all of the following: research question; procedures for collecting data; sampling and recruitment?	Reason for exclusion
Subirats, J. (2000)	2	2	2	2	Relevance and Methodology
Swindell, D. and J. M. Kelly (2000)	2	1	9	1	Relevance and Methodology
Tarpen, D. N. (1984)	2	1	2	2	Relevance and Methodology
Teixeira, M. A. C. (2011)	2	2	1	1	Relevance and Methodology
Thomas, C. J. (1996)	2	2	1	1	Relevance
Thompson, I. N. M. (2005)	1	2	1	1	Relevance

Authors (year)	CMI criterion: Does the study assess a community monitoring intervention?	Outcome types: Does the study have outcomes on corruption, service delivery or quality of services?	Data: Does the study collect data at the individual or the community level?	Methodology criterion: Does the study report at least some information on all of the following: research question; procedures for collecting data; sampling and recruitment?	Reason for exclusion
Tolosa, H. A. M. *et al.* *(*2012)	2	2	1	1	Relevance
Tosi, F. G. (2012)	2	1	2	2	Relevance and Methodology
Tsai, L. L. (2005)	2	1	1	1	Relevance and Methodology
Tshandu, Z. (2005)	2	1	2	2	Relevance and Methodology
Tshishonga, N. (2011)	2	1	1	9	Relevance and Methodology
Unger, J. P., *et al*. (2003)	2	2	1	1	Relevance

Authors (year)	CMI criterion: Does the study assess a community monitoring intervention?	Outcome types: Does the study have outcomes on corruption, service delivery or quality of services?	Data: Does the study collect data at the individual or the community level?	Methodology criterion: Does the study report at least some information on all of the following: research question; procedures for collecting data; sampling and recruitment?	Reason for exclusion
Vannier, C. N. (2010)	2	1	2	2	Relevance and Methodology
Varatharajan, D., *et al*. (2004)	2	1	1	9	Relevance and Methodology
Vyas-Doorgapersad, S. (2009)	2	1	2	9	Relevance and Methodology
Wampler, B. (2008)	2	2	2	1	Relevance and Methodology
Yang, K. (2005)	2	2	1	1	Relevance and Methodology
Yen, N. T. K. and P. V. Luong (2008)	1	2	2	2	Methodology

Authors (year)	CMI criterion: Does the study assess a community monitoring intervention?	Outcome types: Does the study have outcomes on corruption, service delivery or quality of services?	Data: Does the study collect data at the individual or the community level?	Methodology criterion: Does the study report at least some information on all of the following: research question; procedures for collecting data; sampling and recruitment?	Reason for exclusion
Zafarullah, H. (1997)	2	2	2	2	Relevance and Methodology
Zhag, X., *et al*. (2002)	2	1	1	2	Relevance and Methodology

Appendix G: The 15 included impact evaluations assessing the effects of CMIS

Study	Country	Intervention	Outcome data						
			Type (and number) of interventions	Forensic economic estimates corruption	Perception of corruption	Access to service	Changes in prevalence condition	Average waiting time	Population /Units	Sector
Afridi, F. and Iversen, V. (2013)	India	It assesses the impact of audits on irregularities in the implementation of the he Mahatma Gandhi National Rural Employment Guarantee Act (MGNREGA 2005) in Andhra Pradesh. In the implementation of the public work projects, 'social' audits have been made mandatory. The ones responsible for implementation of such audits are the Gram Sabhas, meetings of the residents of village councils. The Act thus empowers intended beneficiaries to scrutinize programme expenditures and to monitor and keep track of programme delivery.	Social audit (Two later rounds compared with the first one)		number of irregularities				Gram Panchayats (GPs)	Promotion of Employment

Study	Country	Intervention	Type (and number) of interventions	Outcome data					Population /Units	Sector
				Forensic economic estimates corruption	Perception of corruption	Access to service	Changes in prevalence condition	Average waiting time		
Andrabi, Das and Khwaja (2013)	Pakistan	It studies the impact of providing report cards with school and child test scores on changes in test scores, prices, and enrolment in markets with multiple public and private providers. Scorecards were delivered in person in discussion groups, during which parents were given a sealed envelope with their child's report card, which they could open and discuss with others, or with members of the LEAPS team. Every group started with a 30-minute open discussion on what influences test score results, followed by the card distribution. The team was careful to not offer any advice to parents or schools. The goal of the meetings was to	Scorecard			enrolment	test score		Schools and children	Education

Study	Country	Intervention	Type (and number) of interventions	Outcome data					Population /Units	Sector
				Forensic economic estimates corruption	Perception of corruption	Access to service	Changes in prevalence condition	Average waiting time		
		provide the report cards and explain what the information meant but not to advocate or discuss any particular plan of action.								
Banerjee *et al.* (2010)	India	The authors conducted a randomised evaluation of three interventions to encourage beneficiaries' participation to India. Treatment 1: providing information on existing institutions. Teams facilitated the meeting, got discussions going, and encouraged village administrators to share information about the structure and organization of local service delivery. After the meetings, distributed pamphlets describing	Information campaign (IC) Treatment 1 Information campaign (IC) Treatment 2 Information campaign (IC) Treatment 3			enrolment	test score		Villages	Education

Study	Country	Intervention	Outcome data						
			Type (and number) of interventions	Forensic economic estimates corruption	Perception of corruption	Access to service	Changes in prevalence condition	Average waiting time	Population /Units	Sector
		the various roles and responsibilities of VEC members and training of individual VEC members. Treatment 2: training community members in a testing tool for children. It also provided this information and, in addition, the teams trained community members to administer a simple reading test for children, and invited them to create “report cards” on the status of enrolment and learning in their village. Treatment 3: training volunteers to hold remedial reading camps. It started with the team conducting treatment 2 in the village, then recruiting volunteers per village, and giving them a week’s training in a pedagogical technique for								

Study	Country	Intervention	Outcome data						
			Type (and number) of interventions	Forensic economic estimates corruption	Perception of corruption	Access to service	Changes in prevalence condition	Average waiting time	Population /Units	Sector
		teaching basic reading skills developed and used by Pratham throughout India. These trained volunteers then held reading camps in the villages.								
Barr *et al.* (2012)	Uganda	This paper combines field and laboratory experimental evidence to study the impacts and mechanisms of community-based monitoring interventions in rural, government primary schools in Uganda. Treatment 1: The first step is the selection and training of individuals to participate in the use of the scorecard and to be part of the scorecard committee. Then they visit the school and complete the scorecard. Finally,	Treatment 1: standard scorecard Treatment 2: participatory scorecard				test score		Children	Education

Study	Country	Intervention	Outcome data						
			Type (and number) of interventions	Forensic economic estimates corruption	Perception of corruption	Access to service	Changes in prevalence condition	Average waiting time	Population /Units	Sector
		during a reconciliation process, scorecard committee members would meet, in small groups and later as a whole, in order to agree upon a single set of scorecard results for the term and to discuss specific goals and means for improvement in relation to this information. These meetings were facilitated by the CCTs. After, the results of this 'consensus scorecard' would be disseminated, by sending it to the District Education Office and by discussing it at the next parent teacher association meeting. Treatment 2: The process is the same as the standard scorecard with the exception that they were								

Study	Country	Intervention	Outcome data						
			Type (and number) of interventions	Forensic economic estimates corruption	Perception of corruption	Access to service	Changes in prevalence condition	Average waiting time	Population /Units	Sector
		allow creating their own goals and measures.								
Björkman and Svensson (2009)	Uganda	This paper presents a randomised …field experiment on community-based monitoring of public primary health care providers. Each treatment facility and its community received a unique report card summarizing the key findings from pre-intervention surveys conducted in their area, including utilization, quality of services, and comparisons vis-à-vis other health facilities. The process of disseminating the report card information and encouraging participation was initiated through a series of meetings: a community meeting;	Scorecard			utilization/ coverage, immunization	mortality rate, weight for age	average waiting time to get the service	Health facilities and health care users	Health

Study	Country	Intervention	Type (and number) of interventions	Outcome data					Population /Units	Sector
				Forensic economic estimates corruption	Perception of corruption	Access to service	Changes in prevalence condition	Average waiting time		
		a staff meeting; and an interface meeting.								
Björkman, de Walque and Svensson (2013)	Uganda	This paper presents the results of two field experiments on local accountability in primary health care. The first experiment is a longer run version of Björkman and Svensson (2009).	Scorecard + information campaign			utilization /coverage	mortality rate, weight for age	average waiting time to get the service	Communities/health facilities and households.	Health
		The second one, the participation intervention included three types of meetings: a community meeting; a health facility meeting; and an interface meeting, with representatives from the community and the staff attending. The objective was to encourage community members	Information Campaign Intervention (IC)							

Study	Country	Intervention	Outcome data						
			Type (and number) of interventions	Forensic economic estimates corruption	Perception of corruption	Access to service	Changes in prevalence condition	Average waiting time	Population /Units	Sector
		and health facility staff to develop a shared view on how to improve service delivery and monitor health provision in the community; i.e., to agree on a joint action plan or a community contract. In total, the process of reaching an agreement took five days. After the meetings, the communities themselves had the responsibility to monitor the implementation of the issues outlined in the joint action plan.								
Gertler *et al.* (2008)	Mexico	The authors examine a programme that involves parents directly in the management of schools located in highly disadvantaged rural communities. The program, known as AGE,	Scorecard			enrolment	repetition rate		nonindigenous primary schools in rural areas	Education

Study	Country	Intervention	Type (and number) of interventions	Outcome data					Population /Units	Sector
				Forensic economic estimates corruption	Perception of corruption	Access to service	Changes in prevalence condition	Average waiting time		
		finances parent associations and motivates parental participation by involving them in the management of the school grants.								
Keefer and Khemani (2011)	Benin	This paper study the effect on literacy rates in children in villages exposed to signals from a larger number of community radio stations. They exploited the large number of very local radio stations in north Benin to argue that variation in radio access across villages within the same commune is accidental, and exogenous to village characteristics.	Information Campaign Intervention (IC)				test score[1]		Households and children in second grade	Education

Study	Country	Intervention	Type (and number) of interventions	Outcome data					Population /Units	Sector
				Forensic economic estimates corruption	Perception of corruption	Access to service	Changes in prevalence condition	Average waiting time		
Molina (2013b)	Colombia	The paper provides evidence on the effect of social audits on citizens' satisfaction with infrastructure projects as well as subjective measures of the efficiency of the execution process. The SA implies giving information about the projects through the media and public forums. A group of beneficiaries composed of interested citizens is constituted and trained to carry out community monitoring activities. Commitments are made by the firm, the local government, and project supervisor to solve the problems that may arise during the construction of the project. These commitments are monitored by the community, the	Social audit		perception of adequacy in the administration of resources				Projects and households	Infrastructure

Study	Country	Intervention	Type (and number) of interventions	Outcome data					Population /Units	Sector
				Forensic economic estimates corruption	Perception of corruption	Access to service	Changes in prevalence condition	Average waiting time		
		facilitators from the central government (DNP) and the project supervisor. In between public forums, the beneficiary group monitors the project and collects information on whether commitments are being honoured and any other new problem that may arise. Before making the final payment to the executing firm, the finalised project is presented to the community. The audit results are shared with all interested and concerned stake-holders.								
Olken (2007)	Indonesia	This paper presents a randomised field experiment on reducing corruption in village road projects. Invitations are send to participate in Social Audits (“accountability	Treatment 1: Social Audit - Invitations	Per cent missing funds major items in roads and					Villages	Infrastructure

Study	Country	Intervention	Type (and number) of interventions	Outcome data					Population /Units	Sector
				Forensic economic estimates corruption	Perception of corruption	Access to service	Changes in prevalence condition	Average waiting time		
		meetings"), to encourage direct participation in the monitoring process of a road project and to reduce elite dominance of the process. The invitations were distributed either by sending them home with school children or by asking the heads of hamlets and neighbourhood associations to distribute them throughout their areas of the village.		ancillary projects						
		Invitations to participate in SA were distributed along with anonymous comment form, providing villagers an opportunity to relay information about the project without fear of retaliation. The form asked the recipient to answer several questions about	Treatment 2: Social Audit - Invitations + comments							

Study	Country	Intervention	Type (and number) of interventions	Outcome data					Population /Units	Sector
				Forensic economic estimates corruption	Perception of corruption	Access to service	Changes in prevalence condition	Average waiting time		
		the road project and then to return the form before the accountability meetings—either filled out or blank—to a sealed drop box, placed either at a village school or at a store in the sub-village. The results were summarised at the meetings.								
Pandey *et al.* (2007)	India	The objective of the paper is to determine the impact of informing resource-poor rural populations about entitled services. An information campaign was conducted consisting in two rounds of two or three public meetings in each intervention village, plus the distribution of posters and leaflets to disseminate information on	Information campaign (IC)		development work in villages	Visits by nurse midwife; prenatal examinations, tetanus vaccinations, and prenatal suppleme			Households	Health

Study	Country	Intervention	Type (and number) of interventions	Outcome data					Population /Units	Sector
				Forensic economic estimates corruption	Perception of corruption	Access to service	Changes in prevalence condition	Average waiting time		
		entitled health services, entitled education services, and village governance requirements.				nts received by pregnant women; vaccinations received by infants.				
Pandey, Goyal and Sundararaman (2009)	India	This study evaluates the impact of a community-based information campaign on school performance. The IC consisted in the development of tools such as a short film of six minutes, a poster, a wall painting, a take-home calendar and a learning assessment booklet focused on information about	Treatment 1: Information campaign (IC) Treatment 2: Additional IC in one of the regions				test score[1]		Teacher and students in villages.	Education

Study	Country	Intervention	Outcome data						
			Type (and number) of interventions	Forensic economic estimates corruption	Perception of corruption	Access to service	Changes in prevalence condition	Average waiting time	Population /Units	Sector
		school oversight committees; organization and funding of school accounts; right to information regarding the school including right to obtain copies of any school record; where to complain about any problems; benefits that students in primary grades are entitled to and minimum levels of language and mathematics skills that children are expected to acquire by grade. In addition, there was a second treatment carried out only in one of the three regions involved on the first one to increase awareness about the economic benefits of schooling. It also advocated the audience to								

Study	Country	Intervention	Outcome data						
			Type (and number) of interventions	Forensic economic estimates corruption	Perception of corruption	Access to service	Changes in prevalence condition	Average waiting time	Population /Units	Sector
		become involved in monitoring outcomes in the school.								
Piper and Korda (2010)	Liberia	The authors study a targeted reading intervention focused on improving the quality of reading instruction in primary schools and its impact on student achievement. The control group did not receive any interventions. In the treatment group, reading levels were assessed; teachers were trained on how to continually assess student performance; teachers were provided frequent school-based pedagogic support, resource materials, and books; and, in addition, parents and	Information campaign				test score		Schools	Education

Study	Country	Intervention	Type (and number) of interventions	Outcome data					Population /Units	Sector
				Forensic economic estimates corruption	Perception of corruption	Access to service	Changes in prevalence condition	Average waiting time		
		communities were informed of student performance.								
Pradhan *et al.* (2014)	Indonesia	This paper investigates the role of school committees in improving education quality in public schools in Indonesia. Two of the interventions, are CMI: Training: IC about different topics, such as their lack of knowledge about the decree; and capacity, such as how to engage the community, how to play a role in school management, and how to promote student learning, services, and village governance requirements.	Treatment 1: Training. Information Campaign (IC)				test score		Schools	Education
		Linkage: IC with facilitated contact with providers: meetings between	Treatment 2: Linkage:							

Study	Country	Intervention	Type (and number) of interventions	Outcome data					Population /Units	Sector
				Forensic economic estimates corruption	Perception of corruption	Access to service	Changes in prevalence condition	Average waiting time		
		the school committee and the village council, discussing potential measures to address education issues in the village.	Information Campaign							
Reinikka and Svensson (2011)	Uganda	This paper exploits an information campaign done by the government. The government published systematic public information on monthly transfers of capitation grants to districts in the national newspapers.	Information campaign (IC)	Share of grants received		enrolment	test score		Schools	Education

Notes: [1] We had to exclude these papers from the meta-analysis for lack of information. See **Error! Reference source not found.**

Appendix H: Citizens' participation – potential relevant variables

Study	Variable definition	Effect of the Intervention on Participation	Effect of the Intervention on Service Provision	Suggested Reason for these Results
Banerjee *et al.*(2010) - Mobilization	Number of school inspections reported, Visited school to monitor or complain, Parents visit the school	0	0	Expectations about providers
Banerjee *et al. (*2010) - Mobilization + information	Number of school inspections reported, Visited school to monitor or complain, Parents visit the school	0	0	Expectations about providers
Banerjee *et al. (*2010) - Mobilization + information + "Read India"	Number of school inspections reported, Visited school to monitor or complain, Parents visit the school	0/+	0/+	Expectations about providers
Molina (2013b)	Citizen Participation in the public forums and time spent monitoring service provision	Mixed. In projects where participation was higher, treatment effect was also higher	+/-	Information asymmetry and expectations about providers
Olken (2007) - Invitations	Measures of participation (Attendance, Attendance of Non-elite, Number who talk, Number non-elite who talk)	+	0/+	Elite Capture

Study	Variable definition	Effect of the Intervention on Participation	Effect of the Intervention on Service Provision	Suggested Reason for these Results
Olken (2007) - Invitations + comments	Measures of participation (Attendance, Attendance of Non-elite, Number who talk, Number non-elite who talk)	+	0/+	Elite Capture
Pandey *et al. (*2007)	Percentage of household reporting that have had village council meetings in the previous six months	0/+	0/+	Idiosyncratic Reasons related to context
Pradhan *et al. (*2014) - Linkage	Number of times parents come to school to meet a teacher, meetings between principal and teachers, meeting with school committee.	0/+	0/+	Expectations about providers
Pradhan *et al. (*2014) - Training	Number of times parents come to school to meet a teacher, meetings between principal and teachers, meeting with school committee.	0	0	Expectations about providers

Notes: 0 = No significant effect; + Positive effect; +/- Mixed effects.

Appendix I: Providers' and politicians' performance outcome variables

Provider's and Politician's (PP) performance					
Study	**Variable definition**	**Effect of the Intervention on**			**Suggested Reason for these Results**
		Monitoring	**PP performance**	**Service Provision**	
Andrabi, Das and Khwaja (2013)	Whether the school spent money on teaching aids (textbooks), whether the class teacher for the tested school went from below matric to above matric qualification and changes in school schedule (break/recess time)	+	+	+	Parents' pressure for improving schools' investments
Banerjee *et al*. (2010) - Mobilization	Textbooks, indoor classes, seats, maps, charts, boundary wall, electricity, water, toilet	0	0	0	Low Participation
Banerjee *et al*. (2010) - Mobilization + information	Textbooks, indoor classes, seats, maps, charts, boundary wall, electricity, water, toilet	0	0	0	Low Participation
Banerjee *et al*. (2010) - Mobilization + information + "Read India"	Textbooks, indoor classes, seats, maps, charts, boundary wall, electricity, water, toilet	0/+	0	0/+	Low Participation Way of influencing learning outcomes without engaging

					with the school system
Barr *et al*. (2012) - Standard scorecard	Teacher presence rates	0	0	0	Low Participation
Barr *et al*. (2012) - Participatory scorecard	Teacher presence rates	+	+	+	Intrinsic Motivation
Björkman and Svensson (2009) - Short term	Absence rate, equipment used, management of clinic (first component from a principal components analysis of the variables Condition of the floors of the health clinic, Condition of the walls, Condition of furniture, and Smell of the facility), health information (whether the household has received information about the importance of visiting the health facility and the danger of self-treatment; importance of family planning (whether the household has received information about family planning, and share of months in 2005 in which stock-cards indicated no availability of drugs.	No info	+	+	Intrinsic Motivation

Björkman and Svensson (2009) - Medium term	Absence rate, equipment used, condition of clinic (first component from a principal components analysis of the variables Condition of the floors of the health clinic, Condition of the walls, Condition of furniture, and Smell of the facility) and share of months in 2009 in which stock-cards indicated no availability of drugs.	No info	0	0/+	Intrinsic motivation Hard to institute permanent changes in behaviour
Björkman, de Walque and Svensson (2013) - Short term	Absence rate, equipment used, condition of clinic (first component from a principal components analysis of the variables Condition of the floors of the health clinic, Condition of the walls, Condition of furniture, and Smell of the facility) and share of months in 2009 in which stock-cards indicated no availability of drugs.	No info	0	0	Lack of information difficults providers' accountability
Keefer and Khemani (2011)	Share of teachers that are absent, average pupil-teacher ratio across classrooms, number of available textbooks per enrolled pupil,	No info	0	0/+	Increase in private tutors

	proportion of active classrooms with teachers and level of PTA activity.				
Molina (2013b)	Providers and politicians performance	0/+	0/+	0/+	Citizens participation in monitoring providers
Pradhan *et al*. (2014) - Linkage	Number of teachers and their work effort (hours worked per day in past week on teaching activities)	0/+	0/+	0/+	Intrinsic Motivation
Pradhan *et al*. (2014) - Training	Number of teachers and their work effort (hours worked per day in past week on teaching activities)	0	0	0	Low participation
Notes: 0 = No significant effect; + Positive effect; +/- Mixed effects.					

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